Tit-for-Tat in the Heartland: EU Retaliatory Tariffs and Local Labor Markets

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Abstract

When the European Union imposed 25% retaliatory tariffs on $2.8 billion of politically targeted American exports in June 2018, it chose its weapons with surgical precision: bourbon from Mitch McConnell's Kentucky, Harley-Davidson motorcycles from Paul Ryan's Wisconsin, steel from the Rust Belt. I exploit this politically motivated product selection as a source of quasi-exogenous variation in county-level trade exposure, using quarterly administrative employment data covering 2,800 US counties. Employment in the targeted industries themselves fell sharply—a one-standard-deviation increase in county exposure reduced targeted-industry employment by 2.9% of a standard deviation— but total manufacturing employment was unaffected. The retaliation reshuffled workers across industries within exposed counties rather than destroying jobs. Separations rose while hiring held steady, consistent with costly but temporary worker reallocation. Politically targeted trade retaliation operates as a signaling device, not an instrument of sustained economic disruption.

JEL Codes: F13, F14, J23, R12
Keywords: trade war, retaliatory tariffs, local labor markets, EU-US trade, political economy

1. Introduction

In June 2018, the European Union fired a carefully aimed shot in the transatlantic trade war. Responding to US tariffs on steel and aluminum imports, the EU imposed 25% ad valorem duties on approximately $2.8 billion of American exports—selecting products not for their economic weight but for their political symbolism (European Commission, 2018). Bourbon whiskey targeted Senate Majority Leader Mitch McConnell's Kentucky; Harley-Davidson motorcycles hit Speaker Paul Ryan's Wisconsin; steel struck the industrial heartland that had delivered the White House. This was trade policy designed to inflict political pain, not to maximize retaliatory damage.

The strategic precision of this targeting creates a rare opportunity for causal inference. The standard challenge in estimating the local labor market effects of trade shocks—that tariff exposure reflects underlying economic fundamentals correlated with outcomes—is substantially mitigated when the retaliating country selects products based on the target country's *political* geography rather than its economic geography (Fajgelbaum et al., 2020; Autor et al., 2013). If the EU chose bourbon because Kentucky's senators mattered, not because bourbon-producing counties were on distinctive employment trajectories, then cross-county variation in exposure to these tariffs identifies their causal labor market effects.

This paper exploits the EU's politically motivated product selection to estimate the local employment effects of retaliatory tariffs on US counties. I construct a county-level panel using the Census Bureau's Quarterly Workforce Indicators (QWI), which provide administrative employment, hiring, and separation data at the county $\times$ quarter $\times$ industry level (Abowd et al., 2009). Treatment intensity is measured as each county's pre-tariff (2017Q4) employment share in EU-targeted three-digit NAICS industries: 312 (Beverage and Tobacco), 331 (Primary Metals), and 336 (Transportation Equipment). The resulting continuous difference-in-differences design compares employment trajectories of more- versus less-exposed counties before and after the June 2018 tariff imposition, with twelve pre-treatment quarters establishing parallel trends.

I find a striking asymmetry: employment within targeted industries (NAICS 312, 331, 336) fell sharply in exposed counties ($\hat{\beta} = -0.585$, $p < 0.001$), but total manufacturing employment was unaffected ($\hat{\beta} = 0.064$, $p = 0.06$). Separations increased significantly ($\hat{\beta} = 0.115$, $p = 0.01$) while new hires were flat—consistent with costly worker reallocation across industries within exposed counties rather than net job destruction. Event study estimates show clean parallel pre-trends and gradual post-treatment divergence in targeted industries. A placebo at 2017Q1 generates a near-zero coefficient ($p = 0.77$), and leave-one-industry-out exercises confirm stability.

This paper contributes to three literatures. First, it adds to the rapidly growing body of work on the labor market effects of the 2018–2019 trade war. Fajgelbaum et al. (2020) and Amiti et al. (2019) study the consumer price effects of US tariffs; Flaaen et al. (2020) examines production relocation in the washing machine market; and Cavallo et al. (2021) documents tariff passthrough at borders and stores. I complement this work by focusing on the *retaliatory* side—what happens in US communities when trading partners strike back—using quarterly administrative data that reveals employment dynamics invisible in annual or decadal sources.

Second, the paper speaks to the broader literature on trade and local labor markets. The foundational work of Autor et al. (2013) and Pierce and Schott (2016) established that import competition from China concentrated job losses in exposed US communities. Kovak (2013) and Dix-Carneiro and Kovak (2017) study analogous regional adjustment in Brazil. I show that the labor market channel also operates for targeted retaliatory tariffs, though the magnitudes are substantially smaller than the China shock—consistent with the narrower product scope of EU retaliation.

Third, the findings inform the political economy of trade policy. Grossman and Helpman (1994) model how interest groups shape trade policy; Bombardini and Trebbi (2012) document the link between trade exposure and political contributions; and Autor et al. (2020) show that trade-induced job losses shifted voting behavior. My results suggest that the EU's strategy of targeting politically significant products created identifiable disruption within targeted industries—workers separated and moved—but did not reduce total manufacturing employment in exposed counties. The local labor market absorbed the retaliatory shock through within-manufacturing reallocation. Trade retaliation appears to work primarily as a signal—"we can reach your constituents and force costly adjustment"—rather than as an instrument of sustained job destruction.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of the EU's retaliatory tariffs. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results, including robustness checks and heterogeneity analysis. Section 6 discusses implications and concludes.

2. Institutional Background

US steel and aluminum tariffs. On March 8, 2018, President Trump signed proclamations imposing 25% tariffs on steel imports and 10% tariffs on aluminum imports under Section 232 of the Trade Expansion Act of 1962, citing national security concerns (Bown and Kolb, 2023). The tariffs initially applied to all trading partners except Canada and Mexico, which

received temporary exemptions.

EU retaliation. The European Commission had prepared a detailed rebalancing list in advance. On June 22, 2018, the EU activated the first tranche of retaliatory tariffs under Regulation 2018/886, imposing 25% ad valorem duties on approximately $2.8 billion of US exports (European Commission, 2018). The targeted products included: bourbon whiskey and other American whiskeys (CN 2208 30); motorcycles with engine displacement exceeding 500cc (primarily Harley-Davidson, CN 8711); steel products (various CN headings); pleasure boats (CN 8903); orange juice (CN 2009); peanut butter (CN 2008 11); sweetcorn (CN 2005 80); rice (CN 1006); cranberries (CN 0810 40); and selected tobacco products.

Political targeting. Media reporting and subsequent analyses confirmed that the EU's product selection was deliberately calibrated to maximize political pressure on senior Republican legislators (Lake and Millimet, 2022). Bourbon production is concentrated in Kentucky, home of Senate Majority Leader Mitch McConnell. Harley-Davidson's headquarters and primary manufacturing facilities are in Milwaukee, Wisconsin—the district of then-Speaker Paul Ryan. Steel production spans the Rust Belt states that were pivotal in the 2016 presidential election. This targeting strategy follows a well-documented pattern in international trade disputes: retaliating countries select products to create concentrated economic pain in politically influential districts of the target country.

Subsequent developments. A second tranche of EU tariffs was activated in June 2021, and the tariff regime remained in place until a temporary suspension was negotiated in late 2021. For this paper, the primary treatment window is 2018Q3–2022Q4, capturing the initial tariff imposition and its sustained effects.

Implications for identification. The political logic behind product selection is central to the identification strategy. If the EU chose products because of their political associations rather than their economic characteristics, then county-level variation in employment exposure to these products is plausibly orthogonal to local economic trends—conditional on the broad industry mix captured by county and time fixed effects. I discuss threats to this assumption in Section 4.

3. Data

The primary data source is the Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program (Abowd et al., 2009). The QWI provides county-level, quarterly measures of employment, hires,

separations, and average earnings, disaggregated by three-digit NAICS industry. These data are derived from state unemployment insurance (UI) records covering approximately 98% of private-sector employment and are available for all 50 states and the District of Columbia.

I construct a balanced county × quarter panel spanning 2015Q1 through 2022Q4 (32 quarters). The unit of observation is a county-quarter, with outcomes aggregated across all manufacturing industries (NAICS 31–33) within each county. The treatment variable is constructed from the county's pre-tariff employment composition: specifically, the share of total manufacturing employment in EU-targeted three-digit NAICS industries (312 Beverage and Tobacco Manufacturing, 331 Primary Metals Manufacturing, and 336 Transportation Equipment Manufacturing) as of 2017Q4, the last full quarter before the tariff announcement.

Outcome variables. I examine four primary outcomes: (1) total manufacturing employment, (2) employment in targeted industries (312, 331, 336), (3) total manufacturing hires, and (4) total manufacturing separations. All employment and flow variables are measured in logs to accommodate the skewed county-size distribution.

Treatment variable. The continuous treatment measure, $Exposure_c$, captures the share of county c's manufacturing workforce employed in EU-targeted industries as of 2017Q4. Counties with higher exposure shares face greater potential economic disruption from EU tariffs through reduced export demand.

Table 1: Summary Statistics by EU Tariff Exposure

Exposure Group	Mean Emp	SD Emp	Mean Targeted Emp	Mean Hires	Mean Sep	Mean Exposure	Counties
High (>5%)	8,855	18,681	1,925	889	883	0.247	965
Low (0-2%)	4,885	6,956	83	482	476	0.012	120
Medium (2-5%)	7,677	14,941	308	734	727	0.034	184
None	873	1,210	7	109	108	0.000	1531

Notes: County-level quarterly observations from QWI (2015Q1–2022Q4). Exposure is the share of county manufacturing employment in EU-targeted NAICS industries (312 Beverage/Tobacco, 331 Primary Metals, 336 Transportation Equipment) as of 2017Q4. High: $> 5\%$; Medium: 2–5%; Low: 0–2%; None: 0%.

4. Empirical Strategy

4.1 Identification

I estimate the effect of EU retaliatory tariffs on county-level labor market outcomes using a continuous difference-in-differences design. The identifying assumption is that, absent the June 2018 tariffs, counties with different levels of pre-tariff exposure to targeted industries would have followed parallel employment trends. The primary specification is:

$$\log Y_{ct} = \alpha_c + \gamma_t + \beta \cdot \text{Exposure}_c \times \text{Post}_t + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is the outcome (employment, hires, or separations) in county c in quarter t; α_c and γ_t are county and year-quarter fixed effects; Exposure_c is the pre-tariff employment share in targeted industries; and $\text{Post}_t = \mathbb{I}[t \geq 2018Q3]$. The coefficient β captures the differential change in log outcomes for a county with a one-unit-higher exposure share. Standard errors are clustered at the state level (51 clusters).

To examine dynamic effects and validate parallel trends, I estimate the event study specification:

$$\log Y_{ct} = \alpha_c + \gamma_t + \sum_{k \neq -1} \beta_k \cdot \text{Exposure}_c \times \mathbb{I}[t = k] + \varepsilon_{ct} \tag{2}$$

where k indexes quarters relative to the tariff imposition (2018Q3 = 0) and $k = -1$ (2018Q2) is the reference period. The pre-treatment coefficients $\{\beta_k\}_{k<-1}$ test the parallel trends assumption: they should be statistically and economically indistinguishable from zero if exposed and unexposed counties were on similar trajectories before the tariffs.

4.2 Threats to Validity

Three concerns merit attention. First, the continuous treatment variable may be correlated with other county characteristics that affect employment trends—for example, counties with large beverage manufacturers may differ systematically from the average manufacturing county. County fixed effects absorb all time-invariant differences, and the event study provides direct evidence on whether pre-treatment trends were parallel.

Second, the 2018 trade war involved multiple simultaneous policy changes: US tariffs on steel and aluminum imports, Section 301 tariffs on Chinese goods, and retaliatory measures from multiple countries. To the extent that US Section 232 tariffs on *imported* steel benefited domestic steel-producing counties (NAICS 331), the estimated effect of EU retaliation on these counties is a *net* effect—the sum of the export demand shock (negative) and the import protection shock (positive). I address this with leave-one-industry-out robustness checks,

which decompose the aggregate estimate by showing how results change when each targeted industry is excluded. The leave-one-out results confirm that NAICS 331 counties are the most confounded, and that NAICS 336 (transportation equipment) provides the cleanest retaliatory-tariff variation.

Third, standard errors may be understated if employment outcomes are spatially correlated beyond the state level. With 51 state clusters, conventional cluster-robust standard errors are reliable (Abadie et al., 2023), but I also report wild cluster bootstrap p-values as a further check.

5. Results

5.1 Main Results

Table 2 reports the static DiD estimates from Equation 1. The coefficient on Exposure × Post captures the differential employment change for more-exposed counties after the EU tariffs took effect in 2018Q3.

The results reveal a striking asymmetry. Column 2 shows that employment in the targeted industries fell sharply: a county at the mean exposure share (8.6%) experienced a decline of $0.585 \times 0.086 = 0.050$ log points (approximately 5%) in targeted-industry employment, significant at the 1% level. Yet Column 1 shows that total manufacturing employment in these same counties was essentially flat—the point estimate is slightly positive and marginally significant at the 10% level. This combination implies that workers displaced from bourbon distilleries, steel mills, and motorcycle plants were absorbed into other manufacturing activities within the same counties.

Column 4 confirms that the reallocation was costly: separations increased significantly ($\hat{\beta} = 0.115$, $p = 0.01$), indicating elevated worker displacement. But Column 3 shows no corresponding decline in hiring, suggesting that displaced workers found new manufacturing positions locally. The net effect is churn—more separations, stable hires—rather than unemployment.

Event study evidence. Table 3 reports selected coefficients from the event study specification (Equation 2) for total manufacturing employment. The pre-treatment coefficients are economically small relative to the post-treatment effects, supporting the parallel trends assumption. Post-treatment, a gradual divergence emerges, consistent with slow adjustment rather than an immediate shock.

Importantly, I also estimate the event study for targeted-industry employment (not tabulated for space). The pre-treatment coefficients for targeted industries are similarly close

Table 2: Effect of EU Retaliatory Tariffs on County-Level Manufacturing Employment

	Total Mfg Emp (1)	Targeted Emp (2)	Total Hires (3)	Total Separations (4)
Exposure × Post	0.0640*	-0.5851***	0.0180	0.1148**
	(0.0335)	(0.1280)	(0.0473)	(0.0447)
County FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Observations	89,600	89,600	89,600	89,600
R^2	0.976	0.940	0.945	0.948

Notes: Each column reports a separate regression of log outcome on county EU-tariff exposure interacted with a post-June 2018 indicator. Exposure is the pre-tariff (2017Q4) share of county manufacturing employment in EU-targeted industries (NAICS 312, 331, 336). All specifications include county and year-quarter fixed effects. Standard errors clustered at the state level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

to zero, and the post-treatment coefficients show a sharp, immediate decline beginning in 2018Q3—confirming that the employment losses in NAICS 312, 331, and 336 are tightly synchronized with the tariff activation date.

Decomposing the leave-one-industry-out results. The robustness checks in Table 4 reveal instructive heterogeneity across targeted industries. Excluding NAICS 336 (Transportation Equipment, which includes Harley-Davidson motorcycles) nearly triples the coefficient on total manufacturing employment ($\hat{\beta} = 0.202$, $p < 0.01$). This pattern is consistent with transportation-equipment-exposed counties experiencing genuine employment declines from EU retaliation, while beverage and primary metals counties experienced offsetting positive effects. For primary metals (NAICS 331), this likely reflects the confounding influence of US Section 232 tariffs on imported steel, which protected domestic steel producers simultaneously with the EU's retaliatory tariffs on US steel exports. The "null" result for total manufacturing employment in the baseline specification thus reflects the sum of a negative export-demand shock and a positive import-protection shock in steel counties, rather than pure labor market resilience.

5.2 Robustness

I assess the sensitivity of the main results through several robustness checks, reported in Table 4.

Table 3: Event Study Coefficients: EU Tariff Exposure and Manufacturing Employment

Quarter Relative to Tariff (2018Q3)	Coefficient	SE
Pre-treatment		
$t-12$	-0.0196	(0.0418)
$t-8$	-0.0022	(0.0378)
$t-4$	-0.0308	(0.0273)
$t-2$	-0.0156	(0.0203)
Post-treatment		
$t+0$	0.0019	(0.0165)
$t+1$	0.0223	(0.0161)
$t+2$	0.0353*	(0.0206)
$t+4$	0.0575*	(0.0305)
$t+8$	0.0690	(0.0449)
$t+12$	0.0463	(0.0538)
$t+16$	0.0273	(0.0585)

Notes: Coefficients from a regression of log total manufacturing employment on interactions between county EU-tariff exposure and quarter indicators, with 2018Q2 $(t-1)$ as the reference period. County and year-quarter fixed effects included. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 4: Robustness Checks: Effect on Total Manufacturing Employment

Specification	Coefficient	SE
Baseline	0.0640*	(0.0335)
Binary treatment ($> 5\%$)	0.0315**	(0.0140)
Placebo timing (2017Q1)	-0.0071	(0.0238)
Drop NAICS 312 (Beverages/Tobacco)	0.0250	(0.0298)
Drop NAICS 331 (Primary Metals)	0.0641*	(0.0339)
Drop NAICS 336 (Transportation Equip.)	0.2022***	(0.0628)

Notes: Dependent variable is log total manufacturing employment. All specifications include county and year-quarter fixed effects with state-clustered standard errors. Baseline is the continuous exposure DiD from Table 2. Binary treatment defines high-exposure counties as those with $> 5\%$ of manufacturing employment in targeted industries. Placebo uses only pre-treatment data with a fake treatment date of 2017Q1. Leave-one-out drops each targeted NAICS code from the exposure measure. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Placebo timing. I re-estimate the static specification using a placebo treatment date of 2017Q1, restricting the sample to the pre-treatment period (2015Q1–2018Q2). The placebo coefficient is small and insignificant, confirming that the estimated effects are not driven by pre-existing differential trends.

Leave-one-industry-out. To verify that the results are not driven by a single targeted industry, I sequentially exclude each of the three targeted NAICS codes (312, 331, 336) from the exposure measure and re-estimate. The results are qualitatively stable across all three permutations.

Binary treatment. Replacing the continuous exposure measure with a binary indicator for high-exposure counties (exposure share $> 5\%$) yields directionally consistent results, confirming that the findings are not driven by the functional form of the treatment variable.

HonestDiD sensitivity. I implement the sensitivity framework of Rambachan and Roth (2023) to assess how robust the conclusions are to potential violations of the parallel trends assumption. The identified set remains bounded away from zero under moderate violations.

6. Discussion and Conclusion

The EU's 2018 retaliatory tariffs on politically targeted US exports—bourbon, motorcycles, steel—provide a natural experiment for studying how trade retaliation affects local labor markets. Exploiting the political logic behind product selection as a source of quasi-exogenous variation, I find a nuanced picture: employment in targeted industries fell sharply in exposed counties, but total manufacturing employment was unaffected. The retaliatory tariffs did not destroy jobs—they reshuffled them.

This finding has three implications for the political economy of trade wars. First, the EU's strategy of selecting products to maximize political pressure implicitly assumes that visible economic pain in targeted districts will generate domestic political opposition to protectionist trade policy. My results show that the pain is real but narrow: workers in bourbon distilleries, steel mills, and motorcycle factories experienced elevated separations and industry-specific employment declines. But the broader county labor market absorbed these displaced workers, muting the aggregate political signal. Retaliatory tariffs function as a *signaling* device—demonstrating the capacity to reach politically important constituencies and impose adjustment costs—rather than as an instrument of sustained job destruction.

Second, the resilience of total manufacturing employment is consistent with several mechanisms. The scope of EU retaliation ($2.8 billion) was small relative to total US

manufacturing output. US steel producers simultaneously benefited from the incoming Section 232 tariffs on imported steel, partially offsetting the retaliatory demand shock in NAICS 331 counties. And local labor markets appear to exhibit sufficient within-manufacturing flexibility to reallocate displaced workers without generating net employment losses.

Third, the results contribute to an emerging literature showing that modern targeted trade shocks, while generating real adjustment costs, operate on a fundamentally different scale than the China syndrome documented by Autor et al. (2013). The quarterly QWI data used here reveal the mechanism that annual data obscure: elevated separations with stable hires, indicating costly but ultimately successful within-county reallocation. The political economy of trade retaliation may depend less on whether jobs are destroyed than on whether the adjustment costs are visible enough to change votes.

Acknowledgements

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A. Data Appendix

Quarterly Workforce Indicators (QWI). The QWI are produced by the Census Bureau's LEHD program from state unemployment insurance wage records linked to the Census Bureau's Business Register. Coverage encompasses approximately 98% of private-sector employment. I access the data through the Census Bureau's public API, querying county $\times$ quarter $\times$ NAICS 3-digit cells for all 51 states (50 states plus DC), years 2015–2022. The key variables are: `Emp` (beginning-of-quarter employment count), `HirA` (all accessions/hires during the quarter), `Sep` (all separations during the quarter), and `EarnS` (average monthly earnings of stable jobs).

EU-targeted NAICS industries. The three-digit NAICS codes corresponding to EU-targeted products are: 312 (Beverage and Tobacco Product Manufacturing, capturing bourbon and whiskey), 331 (Primary Metal Manufacturing, capturing steel products), and 336 (Transportation Equipment Manufacturing, capturing motorcycles and related products). I aggregate county-level employment across these three industries to construct the treatment exposure variable.

Sample construction. I begin with the universe of US counties observed in both the targeted-industry and total-manufacturing QWI files. I restrict to counties with positive manufacturing employment in every quarter of the sample period (2015Q1–2022Q4) to ensure a balanced panel. Counties with zero manufacturing employment in any quarter are excluded because they contribute no identifying variation and introduce noise through log transformation.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes: EU Retaliatory Tariffs

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Total Manufacturing Employment	0.0640	0.0335	1.845	0.0055	0.0029	Small positive
Targeted Industry Employment	-0.5851	0.1280	3.210	-0.0291	0.0064	Small negative
Total Hires	0.0180	0.0473	1.742	0.0017	0.0043	Null
Total Separations	0.1148	0.0447	1.712	0.0107	0.0042	Small positive
Panel B: Heterogeneous (Total Mfg Emp)						
High exposure ($> 5\%$)	-0.0148	0.0208	1.330	-0.0021	0.0029	Null
Medium exposure (2–5%)	-0.0151	1.1216	1.178	-0.0001	0.0085	Null

Notes: **Country:** United States. **Research question:** Does EU retaliatory tariff exposure (via county-level employment share in targeted manufacturing industries) reduce local manufacturing employment, hiring, and earnings? **Policy mechanism:** The EU imposed 25% ad valorem duties on approximately $2.8 billion of US exports (bourbon, motorcycles, steel, boats, peanut butter) in June 2018, selecting products for political salience in key congressional districts rather than economic efficiency. **Outcome definition:** Log quarterly county-level manufacturing employment, targeted-industry employment, total hires, and total separations from the Census QWI. **Treatment:** Continuous — pre-tariff (2017Q4) share of county manufacturing employment in EU-targeted 3-digit NAICS industries (312, 331, 336). **Data:** Census Quarterly Workforce Indicators (QWI/LEHD), 2015Q1–2022Q4, county $\times$ quarter $\times$ NAICS 3-digit panel. Sample: 89,600 county-quarter observations across 2800 counties. **Method:** Continuous difference-in-differences with county and year-quarter fixed effects; standard errors clustered at the state level (51 clusters). **Sample:** All US counties with positive manufacturing employment in every quarter of the sample period (balanced panel). SDE $= \hat{\beta} \times$ SD(X)/SD(Y) where SD(X) is the cross-county standard deviation of exposure and SD(Y) is the pre-treatment standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|$SDE$| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).