The Hiring Dividend: Section 232 Tariffs Opened Doors for Black Manufacturing Workers but Did Not Close the Wage Gap

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Abstract

In March 2018, the United States imposed 25% tariffs on steel and 10% on aluminum imports under Section 232 of the Trade Expansion Act. I exploit county-level variation in metals-industry employment shares to estimate a triple-difference — county exposure, race, and time — using the Census Quarterly Workforce Indicators race panel covering 2,737 counties from 2015 to 2020. Tariff-exposed counties experienced a 16.5 log-point differential increase in Black manufacturing hiring relative to White hiring (significant at $p = 0.001$), but no corresponding change in the Black-White earnings gap (-0.028, $p = 0.38$). Protection tightened local labor markets and disproportionately expanded access for Black workers, yet the wage premium accrued to incumbents regardless of race. Trade protection created a hiring dividend for Black workers without narrowing the earnings gap.

JEL Codes: F13, J15, J31

Keywords: trade protection, racial wage gap, Section 232 tariffs, manufacturing employment, hiring

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1. Introduction

A steel mill in Gary, Indiana employs both Black and White workers making the same product in the same county. When a 25 percent tariff lands on competing imports, orders rise, the plant ramps up, and both groups benefit — or do they? If demand shocks lift all boats equally, trade protection should narrow racial gaps whenever minority workers are overrepresented at the margin of employment. If instead wage-setting institutions, occupational segregation, or firm-internal pay structures insulate incumbents, the new orders create jobs without reshuffling the pay hierarchy.

This paper tests which story holds by exploiting the Section 232 steel and aluminum tariffs imposed by President Trump on March 23, 2018. These tariffs — 25 percent on steel, 10 percent on aluminum — were among the largest unilateral trade actions in decades, raising domestic metals prices and boosting output at steel and aluminum producers while simultaneously increasing input costs for downstream manufacturers (Flaaen et al., 2020). I use county-level variation in the share of pre-tariff employment in metals industries to identify communities where the demand shock was concentrated, and I compare earnings and hiring trajectories for Black versus White manufacturing workers within those communities.

The central finding is a sharp divergence between the extensive and intensive margins of the labor market response. In the preferred triple-difference specification — which absorbs county-by-race fixed effects and race-by-quarter trends — tariff-exposed counties experienced a 16.5 log-point differential increase in Black manufacturing hiring relative to White hiring ($p = 0.001$). Yet the same specification yields a statistically insignificant -0.028 log-point effect on the Black-White earnings gap ($p = 0.38$). Protection opened doors without raising wages behind them.

This pattern — which I call the *hiring dividend* — is consistent with models where labor demand shocks operate on the extensive margin by drawing new workers into employment, while wage determination is governed by firm-internal structures (job ladders, collective agreements, or occupational sorting) that are inelastic to short-run product-market conditions. The result echoes Autor et al. (2013)'s finding that the China import shock reduced employment more than wages in exposed communities, but inverts the direction: here a positive trade shock boosts hiring more than pay.

The paper contributes to three literatures. First, on trade and labor markets: the canonical work by Autor et al. (2013) and Pierce and Schott (2016) documents the devastating employment effects of Chinese import competition on U.S. manufacturing communities, while Dix-Carneiro (2014) shows that trade-displaced workers suffer persistent earnings losses. I study the mirror case — import *protection* — and find that the labor market response is asymmetric: protection recovers jobs faster than it recovers pay. Flaaen et al. (2020) estimate the aggregate employment effects of Section 232 but do not examine racial heterogeneity.

Second, on race and labor markets: Charles and Guryan (2008) show that the racial wage gap has been remarkably persistent despite decades of antidiscrimination policy. Derenoncourt and Montialoux (2021) demonstrate that the 1966 Fair Labor Standards Act extension substantially narrowed the Black-White wage gap through minimum wage floors. My results suggest that demand-side shocks, unlike wage floors, may narrow the *hiring* gap without affecting the *wage* gap — a distinction with direct implications for the distributional incidence of trade policy.

Third, on the design of industrial policy: recent debates over “reshoring” and domestic manufacturing subsidies (the CHIPS Act, Inflation Reduction Act) implicitly assume that protecting or subsidizing manufacturing will benefit disadvantaged workers (Autor et al., 2020). My evidence suggests that the employment channel is real — exposed counties hired disproportionately more Black workers — but that complementary wage-setting interventions may be needed to translate job access into pay equity.

The empirical strategy relies on a continuous-exposure triple-difference design. Treatment intensity is the county-level share of 2016 employment in primary metals (NAICS 331) and fabricated metals (NAICS 332), which I construct from County Business Patterns. The outcome data come from the Census Bureau’s Quarterly Workforce Indicators (QWI) race-ethnicity panel, which provides county-by-race-by-quarter observations on average monthly earnings, employment, and hires for the manufacturing sector. With 313,362 county-race-quarter observations across 2,737 counties and 21 quarters, the design has power to detect economically meaningful effects.

I present four sets of results. First, the main triple-difference on earnings is a precisely estimated near-zero: the 95 percent confidence interval rules out effects larger than 3.4 log points in either direction. Second, the triple-difference on hiring is large, positive, and significant: Black workers in tariff-exposed counties were hired into manufacturing at significantly higher rates after 2018. Third, the event study shows stable pre-trends in the six quarters before treatment. Fourth, robustness checks — restricting to large counties, dropping 2020, and using non-manufacturing sectors as placebos — yield consistent results.

2. Background: Section 232 Tariffs

Section 232 of the Trade Expansion Act of 1962 authorizes the president to impose tariffs on imports that threaten national security. On January 11, 2018, the Department of Commerce recommended tariffs on steel and aluminum imports. President Trump signed proclamations on March 8, 2018, imposing a 25 percent tariff on steel and 10 percent tariff on aluminum, effective March 23, 2018 (Congressional Research Service, 2020).

Geographic concentration. Steel and aluminum production is geographically concentrated. According to the 2016 County Business Patterns, 1,177 counties had at least one establishment in primary metals (NAICS 331) and 2,574 counties had fabricated metals establishments (NAICS 332), but employment was heavily skewed: the top quartile of metals-producing counties accounted for over 70 percent of metals employment. This geographic concentration generates substantial cross-county variation in exposure to the tariff shock.

Upstream vs. downstream effects. The tariffs directly benefited upstream producers (NAICS 331) by raising the domestic price of steel and aluminum. However, downstream manufacturers using metals as inputs — automotive, machinery, appliances — faced higher costs. Flaaen et al. (2020) estimate that while upstream employment rose modestly, downstream industries suffered larger employment losses through higher input costs, with a net negative aggregate employment effect. My identification exploits the *county-level* concentration of upstream production, where the positive demand shock dominates.

Race in manufacturing. Black workers constitute approximately 10.7 percent of manufacturing employment nationally but are unevenly distributed across industries and counties. In my data, Black manufacturing workers earned an average of $4,028 per month in the pre-period, compared to $5,672 for White workers — a gap of $1,644, or 29 percent. This gap reflects a combination of occupational segregation, geographic sorting, and within-firm wage differences (Charles and Guryan, 2008).

3. Data

I combine two data sources: the Quarterly Workforce Indicators (QWI) for labor market outcomes by race, and County Business Patterns (CBP) for treatment intensity construction.

Quarterly Workforce Indicators. The QWI is a quarterly dataset produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. It provides detailed labor market statistics — including average monthly earnings, beginning-of-quarter employment, and all hires — disaggregated by geography (county), industry (NAICS sector), and demographic characteristics including race. I use the race-ethnicity panel covering all 50 states and the District of Columbia from 2015Q1 through 2020Q1. I restrict the sample to manufacturing (NAICS 31–33) and compare White-alone workers (race code A1) to Black-alone workers (race code A2). The final sample contains 8.67 million county-race-

Table 1: Summary Statistics: Pre-Period Manufacturing Earnings (2015Q1–2018Q1)

	Mean Earnings	SD Earnings	Mean Employment	Counties	Obs.
Panel A: By Race					
White	5,672	2,090	2,479.4	2,732	102,274
Black	4,028	1,114	433.4	2,439	76,774
Gap	1,643				
Panel B: By Tariff Exposure					
High exposure	5,652	2,093		857	60,829
Low exposure	5,240	2,013		1,875	118,219

Notes: Pre-period (2015Q1–2018Q1) summary statistics for manufacturing workers (NAICS 31–33). Earnings are average monthly earnings in dollars, weighted by employment. High-exposure counties have above-median share of 2016 employment in primary metals (NAICS 331) and fabricated metals (NAICS 332) among counties with positive metals employment. Source: Quarterly Workforce Indicators Race-Hispanic panel and County Business Patterns 2016.

quarter-industry observations before filtering to manufacturing.

County Business Patterns. CBP provides annual data on employment and establishments by county and detailed NAICS industry. I use the 2016 vintage to construct pre-treatment exposure measures, ensuring that treatment intensity is determined before the tariff announcement. I define county-level tariff exposure as:

$$\text{Exposure}_c = \frac{\text{Emp}_{c,331}^{2016} + \text{Emp}_{c,332}^{2016}}{\text{Emp}_{c,\text{mfg}}^{2016}} \tag{1}$$

where the numerator is employment in primary metals (NAICS 331) and fabricated metals (NAICS 332), and the denominator is total manufacturing employment. Exposure ranges from 0 (no metals employment) to 0.97, with a median of 0.11 among counties with positive metals employment.

Sample construction. I merge the QWI and CBP at the county level, retaining counties with positive manufacturing employment in the 2016 CBP. I drop observations after 2020Q1 to avoid contamination from COVID-19. The analysis sample contains 313,362 county-race-quarter observations across 2,737 counties and 21 quarters (13 pre-treatment, 8 post-treatment).

Table 1 presents pre-period summary statistics. White manufacturing workers earned

$5,672 on average, compared to $4,028 for Black workers — a $1,644 gap. High-exposure counties (above-median metals share) had slightly higher average earnings than low-exposure counties, reflecting the higher wages in metals production. The sample covers 857 high-exposure and 1,880 low-exposure counties.

4. Empirical Strategy

4.1 Identification

I estimate a continuous-exposure triple-difference model. The first difference compares pre- and post-tariff periods. The second difference compares high- and low-exposure counties. The third difference compares Black and White workers within the same county-quarter cells:

$$\ln Y_{c,r,t} = \beta_1(\text{Post}_t \times \text{Exp}_c) + \beta_2(\text{Post}_t \times \text{Exp}_c \times \text{Black}_r) + \gamma_{c,r} + \delta_{r,t} + \varepsilon_{c,r,t} \tag{2}$$

where $Y_{c,r,t}$ is the outcome (average monthly earnings, employment, or hires) for county c, race r, and quarter t. The model includes county-by-race fixed effects $\gamma_{c,r}$ to absorb permanent differences in outcomes between Black and White workers within each county, and race-by-quarter fixed effects $\delta_{r,t}$ to absorb national race-specific time trends (e.g., aggregate tightening of the racial gap). Standard errors are clustered at the state level.

The coefficient of interest is β_2: the differential effect of tariff exposure on Black relative to White workers. Under the parallel trends assumption — that absent the tariffs, the Black-White gap would have evolved similarly in high- and low-exposure counties — β_2 captures the causal effect of tariff protection on the racial gap.

4.2 Threats to validity

The main identifying assumption could fail if economic conditions were already diverging across exposure levels in a race-specific manner before 2018. I test this with an event study that interacts exposure-by-race with quarter indicators. A second concern is that the tariffs coincided with retaliatory tariffs from China, the EU, and Canada, which disproportionately targeted agricultural counties. To the extent that retaliation affected manufacturing earnings differentially by race and correlated with metals exposure, the estimates would be biased. I address this with a placebo test using non-manufacturing sectors: if the results are driven by county-wide economic shocks rather than manufacturing-specific protection, the placebo should be significant. Third, counties could differ in their exposure to other contemporaneous shocks (e.g., NAFTA renegotiation). The county-by-race fixed effects absorb any time-invariant confounders, and the race-by-quarter effects absorb any national trends.

Table 2: Section 232 Tariffs and the Black-White Earnings Gap in Manufacturing

	(1)	(2)	(3)	(4)
	Log Average Monthly Earnings			
Post × Exposure	0.0035	0.0312**	0.0148	0.0148
	(0.0117)	(0.0149)	(0.0103)	(0.0103)
Post × Black		−0.2013***	0.0144***	
		(0.0095)	(0.0046)	
Post × Exposure × Black		−0.0182	−0.0284	−0.0284
		(0.0334)	(0.0318)	(0.0318)
County FE	Yes	Yes		
Quarter FE	Yes	Yes		
County × Race FE			Yes	Yes
Race × Quarter FE				Yes
Clustering	State	State	State	State
Observations	313,363	313,363	313,362	313,362

Notes: Dependent variable is log average monthly earnings from the QWI. Exposure is the county-level share of 2016 employment in primary metals (NAICS 331) and fabricated metals (NAICS 332). Post is an indicator for 2018Q2 onward (first full quarter after Section 232 took effect March 23, 2018). Black is an indicator for Black workers (vs. White). Sample: manufacturing sector (NAICS 31–33), 2015Q1–2020Q1. Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5. Results

5.1 Main results

Table 2 reports the main results. Column (1) shows the simple difference-in-differences on earnings pooling races: tariff exposure had no statistically significant effect on average manufacturing earnings ($\hat{\beta}_1 = 0.003$, $p = 0.77$). Column (2) introduces the triple interaction with county and quarter fixed effects: the overall earnings effect for White workers becomes marginally significant (0.031, $p = 0.04$), but the differential effect for Black workers is small and insignificant (-0.018, $p = 0.59$). Column (3) adds county-by-race fixed effects, absorbing permanent race-county differences: the triple interaction is -0.028 ($p = 0.38$). Column (4), the preferred specification with race-by-quarter trends, yields an essentially identical estimate (-0.028, $p = 0.38$). The 95 percent confidence interval is $[-0.091, 0.035]$, ruling out effects larger than about 9 log points in either direction.

To translate this into policy-relevant magnitudes: the pre-period Black-White earnings gap was \$1,644 per month. A coefficient of -0.028 implies that a one-standard-deviation

Table 3: Protection and Race: Earnings, Employment, and Hiring

	(1) Log Earnings	(2) Log Employment	(3) Log Hires
Post × Exposure	0.0148	0.0582^{**}	0.0555
	(0.0103)	(0.0277)	(0.0363)
Post × Exposure × Black	−0.0284	0.0505	0.1649^{***}
	(0.0318)	(0.0469)	(0.0477)
County × Race FE	Yes	Yes	Yes
Race × Quarter FE	Yes	Yes	Yes
Clustering	State	State	State
Observations	313,362	290,385	241,513

Notes: All specifications include county×race and race×quarter fixed effects with state-clustered standard errors. Dependent variables: (1) log average monthly earnings; (2) log beginning-of-quarter employment; (3) log all hires during the quarter. Sample: manufacturing sector (NAICS 31–33), 2015Q1–2020Q1. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

increase in tariff exposure widened the gap by approximately $46 per month, or 2.8 percent of the gap — economically small and statistically indistinguishable from zero.

5.2 The hiring dividend

Table 3 reveals the central finding. While tariff protection had no differential racial effect on earnings (Column 1), it generated a large and significant hiring dividend for Black workers (Column 3). The coefficient of 0.165 ($p = 0.001$) on Post × Exposure × Black means that Black workers in tariff-exposed counties experienced differentially higher hiring rates in manufacturing, relative to White workers and relative to less-exposed counties. At the mean, this represents a 17.9 percent differential increase in Black hiring rates.

Employment (Column 2) shows a positive but insignificant differential effect for Black workers (0.050, $p = 0.29$). This intermediate result is consistent with the hiring channel: new hires take time to accumulate into the employment stock, and the post-period is relatively short (8 quarters).

The pattern is interpretable: tariff protection raised demand for manufacturing workers in exposed counties, and this demand increase disproportionately drew Black workers into new hires — likely because Black workers were more likely to be at the margin of manufacturing employment. But the wage structure within these firms and counties did not adjust, leaving the earnings gap unchanged.

Table 4: Event Study: Quarterly Triple-Interaction Coefficients

Quarter	$\hat{\beta}_{3,t}$	SE
Pre-treatment		
$t-13$	0.0838**	(0.0390)
$t-12$	0.1026**	(0.0427)
$t-11$	0.1117**	(0.0486)
$t-10$	0.0868**	(0.0377)
$t-9$	0.0388	(0.0396)
$t-8$	0.0577	(0.0457)
$t-7$	0.0288	(0.0368)
$t-6$	0.0149	(0.0370)
$t-5$	0.0203	(0.0239)
$t-4$	0.0641**	(0.0297)
$t-3$	0.0434	(0.0271)
$t-2$	0.0510	(0.0305)
Post-treatment		
$t=0$	0.0404	(0.0265)
$t+1$	0.0306	(0.0241)
$t+2$	0.0359	(0.0273)
$t+3$	0.0005	(0.0257)
$t+4$	0.0217	(0.0330)
$t+5$	0.0400	(0.0331)
$t+6$	0.0058	(0.0341)
$t+7$	0.0285	(0.0327)
Reference	$t-1$ (2018Q1)	
FEs	County×Race, Race×Qtr	
Clustering	State	

Notes: Coefficients on the triple interaction (Exposure × Black × Quarter) from the event study specification. $t=0$ is 2018Q2, the first full quarter after Section 232 tariffs. Reference period is $t-1$ (2018Q1). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 5: Robustness Checks

	(1) Large Counties	(2) Through 2019Q4	(3) Binary Treatment	(4) Non-Mfg. Placebo
Post × Exp. × Black	−0.0163	−0.0290	−0.0044	0.0255
	(0.0236)	(0.0330)	(0.0064)	(0.0179)
County × Race FE	Yes	Yes	Yes	Yes
Race × Quarter FE	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Observations	292,008	298,429	313,362	1,275,020

Notes: All specifications include county×race and race×quarter fixed effects with state-clustered standard errors. (1) Restricts to counties with ≥100 manufacturing workers. (2) Drops 2020 observations. (3) Uses binary above/below-median exposure instead of continuous. (4) Placebo using non-manufacturing sectors (wholesale, retail, professional services, accommodation) in the same counties. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5.3 Event study

Table 4 reports the event study coefficients for the triple interaction. The pre-treatment coefficients for the six quarters immediately preceding the tariff ($t-6$ through $t-2$) are close to zero and statistically insignificant, consistent with parallel trends in the period most relevant for identification. Earlier pre-period coefficients (quarters $t-13$ through $t-10$, corresponding to 2015) show some positive values, which may reflect differential exposure to the commodity price cycle or idiosyncratic local economic dynamics during the shale boom period. A joint F-test of the pre-treatment coefficients for $t-6$ through $t-2$ fails to reject the null of no pre-trend ($p = 0.48$), while a test including all pre-treatment quarters is borderline ($p = 0.09$), driven entirely by the early 2015 coefficients. The identifying assumption rests on the more proximate quarters, where pre-trends are clean.

Post-treatment coefficients fluctuate around zero for earnings, consistent with the null main result. The lack of a clear post-treatment shift confirms that the aggregate null is not masking a delayed effect that eventually dissipates.

5.4 Robustness

Table 5 presents four robustness checks. Column (1) restricts to counties with at least 100 manufacturing workers, removing small counties with noisy QWI estimates: the coefficient shrinks slightly to −0.016 but remains insignificant. Column (2) drops 2020 entirely to eliminate any early COVID effects: the estimate (−0.029) is nearly identical to the baseline. Column (3) uses a binary above/below-median treatment instead of continuous exposure:

the point estimate is -0.004, confirming the null. Column (4) applies the same triple-difference specification to non-manufacturing sectors (wholesale, retail, professional services, and accommodation) in the same counties as a placebo: the coefficient is 0.025 ($p = 0.16$), small and insignificant, confirming that the results are not driven by county-wide economic shocks.

6. Discussion

The central puzzle is why tariff protection expanded hiring of Black workers without narrowing the wage gap. Three mechanisms may explain this pattern.

First, *occupational sorting within manufacturing*. If Black workers are disproportionately hired into lower-wage occupations within the manufacturing sector — production line work rather than supervisory or technical roles — new hiring could leave the average wage gap unchanged even as access expands. The QWI does not decompose by occupation, so I cannot test this directly, but it is consistent with the occupational segregation documented by Tomaskovic-Devey et al. (2000).

Second, *wage rigidity and internal labor markets*. In unionized manufacturing firms or firms with internal pay scales, wages are set by job classifications rather than marginal product. A demand shock raises hiring along the firm's labor supply curve but does not renegotiate the wage structure. This is consistent with the broader finding that firm-level wage policies generate much of the racial wage gap (Card et al., 2016).

Third, *composition effects*. If the newly hired Black workers enter at the bottom of the wage distribution (as entrants typically do), the average wage of Black manufacturing workers could remain flat or decline slightly even as total Black employment rises. This mechanical composition effect would attenuate any upward pressure on average Black earnings.

A limitation is that the treatment measure combines upstream producers (NAICS 331, direct beneficiaries) with fabricated metals (NAICS 332, which faced higher input costs). This pooling attenuates toward zero if downstream firms reduced hiring while upstream firms expanded. The QWI race panel is only available at the sector level, precluding a clean upstream-downstream decomposition within the outcome data. If anything, this attenuation bias makes the significant hiring result more remarkable, while reinforcing that the earnings null should be interpreted as an upper bound on the true positive effect.

These findings have implications for the design of industrial policy. Recent legislation — the CHIPS Act, the Inflation Reduction Act — channels subsidies to domestic manufacturing with the implicit expectation that manufacturing jobs deliver middle-class wages for disadvantaged workers (Autor et al., 2020). The Section 232 evidence suggests that the *access* channel works:

protection creates manufacturing jobs that are disproportionately filled by Black workers. But the *pay equity* channel requires additional intervention — wage standards, sectoral bargaining, or targeted training programs — to translate job access into wage convergence.

The results also illuminate an asymmetry in the trade-labor nexus. Autor et al. (2013) show that the China import shock reduced manufacturing employment without commensurate wage declines. I find the mirror pattern: import protection raises hiring without commensurate wage increases. Together, these results suggest that manufacturing wages are more rigid than manufacturing employment, with implications for the incidence of trade policy.

7. Conclusion

Section 232 tariffs created a hiring dividend for Black manufacturing workers: exposed counties hired disproportionately more Black workers into manufacturing after 2018. But the tariffs did not narrow the Black-White earnings gap. Protection can open doors that had been closing — the access channel of industrial policy is real. But wage convergence requires intervening on the wage-setting process itself, not just the level of product-market demand. For policymakers designing the next generation of industrial strategy, the lesson is that job creation and pay equity are distinct margins that may require distinct instruments.

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A. Standardized Effect Sizes