Racially Disparate Effects of the Japan Trade Shock*

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Abstract. We evaluate the effects of the Japan trade shock on manufacturing employment and whether they were racially disparate. Most importantly, we test whether differential occupational exposure drove racially disparate effects. Using detailed establishment-level data and a shift-share instrumental variables design, we find that the shock caused substantial decreases in overall manufacturing employment and in Black manufacturing operator employment. We find that two-thirds of the decrease in Black operator employment (relative to that of white operators) was due to disparate occupational exposure. Disparate exposure was associated with local anti-Black prejudice. The Japan shock decreased Black income in affected areas, and had persistent effects on Black poverty and joblessness. Taken together, these results show that aggregate sector-level trade shocks can hit minority workers particularly hard when they are concentrated in exposed occupations.

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1 Introduction

Racial inequality in the United States is pervasive and persistent: The Black-white wage gap closed from 50 log points in 1940 to 40 log points in 1970, but did not narrow further in subsequent decades (Bayer and Charles, 2018). Wilson (1978, 1999) argues that the manufacturing decline was responsible for stalled Black progress and the rise of the urban Black underclass.[1] In particular, prior to the manufacturing decline, a disproportionate share of high-paying jobs held by Black workers was in manufacturing production jobs: Black men were twice as likely as white men to work as production operators in 1970, and those jobs fell higher in the Black income distribution than in the white income distribution.[2]

In this paper, we study whether the overrepresentation of Black workers in production operator employment left them disproportionately vulnerable to the adverse effects of a particular demand shock to manufacturing: the Japanese Trade Shock of the 1960s-'80s. After World War II, Japan's economy boomed and its manufacturing base expanded substantially (Irwin, 2017). Around the same time, in 1955, Japan entered the General Agreement on Tariffs and Trade, leading to gradual reductions in trade restrictions between the United States and Japan through the 1950s-'70s (Irwin, 2017). These developments, combined with other economic forces, led to a five-fold increase in Japanese import penetration to the United States, concentrated overwhelmingly in the automobile and electronics industries. Like the later China shock, this increase in Japanese imports increased competitive pressure on American manufacturers (Autor, Dorn and Hanson, 2013) — leading to a reduction in demand for production workers.

The central aim of this paper is to evaluate whether the overrepresentation of Black workers as production operators left them disparately exposed to Japanese import competition. To answer this central question, we first evaluate the overall effect of the Japan shock on manufacturing employment, and we test for racially disparate effects on employment and income. We then build on our analysis by discussing the role of several

[1] This hypothesis was empirically corroborated in recent work by Gould (2021) and Batistich and Bond (2023). The latter does so in the context of the Japan shock. As discussed later, our paper builds on theirs by showing the mechanism through which the Japan shock adversely affected Black Americans. We directly show the overrepresentation of Blacks in manufacturing left Black workers disparately exposed to the Japan shock. In so doing, we uncover a mechanism through which historic occupational segregation can increase racial inequality: by disparately exposing Black workers to sectoral shocks.

[2] Moreover, Northern Black manufacturing operator share of total employment decreased from 20% to 8% over the 1970-1990 period. The Black-whte gap in the share of total employment that were manufacturing operators decreased from 7pp to 1pp over the 1970-1990 period.

potential mechanisms, including anti-Black prejudice, in shaping the estimated disparate effects.

We leverage a rich administrative dataset, collected by the Equal Employment Opportunity Commission. This detailed dataset contains establishment-level, cross-tabulation counts of employment by race, gender, and occupation. Its broad coverage, including 40 percent of overall employment and 60-70 percent of manufacturing employment, provides a detailed look at trends occurring in the economy as a whole and in the manufacturing sector. The granularity of this dataset also allows us to create a firm-level measure of disparate exposure — accounting for differences among companies in company size and industry. We use this measure to look for disparate racial employment trends, both within-firm and across local labor markets, as well as to quantify the role played by disparate exposure.

Following a number of recent shift-share papers in the literature on trade and worker outcomes (Autor, Dorn and Hanson, 2013; Batistich and Bond, 2023; Kahn, Oldenski and Park, 2022), this study leverages a shift-share instrumental variables strategy. This shift-share approach maps exogenous industry-level trade shocks to local labor markets. It leverages the observation that different local labor markets may be differentially exposed to the shock, depending on their manufacturing industry mix. Because US employment trends can be affected by changes both in Japanese-specific factors, such as comparative advantage and trade costs, and in US-specific factors, such as changes in productivity, we use an instrument to purge any US-specific factors from the shift-share measure of trade exposure. The instrument is the change in Japanese imports to several high-income countries with comparable economic structures to the United States, including Great Britain, Australia, and France. If the within-industry trade exposure common to both the US and comparison countries is driven exclusively by Japan-specific factors – and not US-specific factors – then the instrument identifies the causal effects of Japanese trade.

We find that the Japan trade shock caused large decreases in manufacturing employment. Our results imply that the trade shock reduced manufacturing employment by about 3 percentage points, as a share of initial employment. This effect was twice as large for Black workers as for white workers. More than 80 percent of the overall decrease in Black manufacturing employment was among production operators; the shock reduced Black manufacturing operator employment by 2.4 percentage points, as a share of initial employment. By contrast, the decline in white manufacturing employment was 1.2 percentage points, of which 0.5 percentage points was decreased operator employment.

The racially disparate employment changes of the shock translated into racially disparate changes in income. We find the shock had a significant negative effect on Black incomes. The shock had the greatest negative effect toward the middle of the income distribution, driven by Blacks losing middle-income operator jobs. Our estimates suggest that the Japan shock reduced Black median income by 3 percent, and increased the Black-white income gap by 2 percentage points at the median of the respective populations.

After establishing the racially disparate effects of the Japan shock on income and employment, we explore whether the disparate employment impact is driven by disparate exposure — that Black workers were more likely to work as production operators at baseline – or by disparate impact conditional on exposure (DICE) – that the trade shock led Black workers to be laid off disproportionately, relative to baseline occupational composition of Black and white workers. Using a commuting zone (CZ)-level decomposition, we find that about two-thirds of the increase in the racial employment gap is driven by disparate exposure. We then run a firm-level Oaxaca decomposition to further understand the types of companies that drive the disparate impact. We study several mechanisms, including anti-Black prejudice, that may drive our findings.

In particular, we use firm-level variation to preliminarily explore the relationship between facets of occupational segregation and disparate exposure. Then as now, the American economy featured high levels of occupational segregation – defined as unequal distribution of racial groups across jobs (Jardina et al., 2023). The manufacturing sector, in particular, exhibited high occupational segregation. Skilled trades have historically seen high levels of segregation, in part because of various discriminatory mechanisms that prevented Black workers from advancing to higher-paying jobs (Waldinger and Bailey, 1991). Because the occupational segregation of the period took the form of excluding minorities from higher-paid work, occupational segregation may have disparately exposed Black workers to the reduction in demand for middle-pay work resulting from the Japan shock.[3] We explore this possibility by focusing on the association between disparate exposure and prejudice.

Racial prejudice may also affect disparate exposure, as occupational choices are socially determined. To explore the role of prejudice, we use two main measures of discrim-

[3]In general, a high level of occupational segregation does not necessarily imply that sectoral shocks will widen racial inequality. (Nor does occupational segregation even imply Blacks will earn less than whites.) It does imply one racial group will be adversely affected by sectoral decreases in demand for a particular type of work – but not necessarily the minority group. When occupational segregation results from (possibly society-wide) barriers to minority advancement – as it historically has – then it may leave Black workers vulnerable to aggregate shocks.

ination: the county-level vote share of George Wallace in the 1968 election and an index of racial resentment we construct from the 1972 and 1973 rounds of the General Social Survey. We find that disparate occupational exposure is greater in counties with above average levels of prejudice. We further find that firms in counties with more prejudice had a higher concentration of Black workers in operator employment. While not definitive, both conclusions suggest an association between prejudice and the disproportionate occupational exposure of Black workers to the Japan shock.

This paper contributes to three literatures. First is the literature on the racial wage gap (Batistich and Bond, 2023; Bayer and Charles, 2018; Card and Krueger, 1992; Derenoncourt, 2022; Gould, 2021; Neal and Johnson, 1996). This research documents how the wage gap narrowed dramatically until the 1970s, when the trend stalled. Studies cite such factors as education disparities, incarceration, and discrimination (Bayer and Charles, 2018; Lang and Spitzer, 2020). We advance this literature by studying a specific factor that depressed Black wages, namely trade shocks, and we quantify the impact.

Second, this paper adds to the large and growing literature on the effects of trade shocks on labor markets (Acemoglu et al., 2016; Autor, Dorn and Hanson, 2013; Batistich and Bond, 2023; Borusyak, Hull and Jaravel, 2021; Dix-Carneiro and Kovak, 2017; Kahn, Oldenski and Park, 2022; Pierce and Schott, 2016; Topalova, 2010). We show how aggregate sector-level trade shocks increase racial inequality when minority workers are concentrated in more exposed occupations.

We build on the work of Batistich and Bond (2023), who were first to use a shift-share strategy to reveal the adverse impact of the Japan trade shock on Black manufacturing employment and, more broadly, Black economic status. Leveraging a high-resolution, firm-level administrative dataset, we study the effect of shocks both within and across firms, directly contrasting impacts on workers by race. This allows us to explore why the trade shock had a disproportionate impact on Blacks, which we show stemmed from their overrepresentation in production jobs displaced by import competition. We further demonstrate that the trade shock had a large overall negative impact on manufacturing employment, in addition to a disparate impact on Blacks – an outcome that confounding pre-trends masked in the Batistich and Bond (2023) analysis. Finally, we show that both discriminatory racial attitudes are associated with the disparate exposure of Black workers to the shock.

Third, this paper speaks to the smaller literature on the consequences of occupational segregation (Aneja and Xu, 2022; Chafetz, Reskin and Padavic, 2006; Derenoncourt and

Montialoux, 2021; Hamilton, Austin and Darity Jr, 2011; Hellerstein and Neumark, 2008; Jardina et al., 2023; Waldinger and Bailey, 1991). This literature generally focuses on state action (Aneja and Xu, 2022) or on mechanisms that occur at the individual firm level, such as those initially sorting Black workers into lower-status jobs and legitimating unfair treatment in the workplace (Chafetz, Reskin and Padavic, 2006). We contribute to this literature by showing that occupational segregation can exacerbate racial inequality by disparately exposing minority workers to aggregate sectoral shocks.

The rest of the paper proceeds thus. Second 2 details the background. Section 3 describes the data. Section 4 details the empirical strategy. Section 5 presents the results. Section 6 concludes.

2 Background

2.1 The Rise of Japanese Trade to the U.S.

Japan's industrial base was decimated by World War II. Attacks by the Allied forces destroyed 40 percent of its industrial plants. In subsequent decades, the nation experienced an incredible economic turnaround, known as the "Japanese Economic Miracle." Real GDP growth was shy of 10 percent annually over the 1950-70 period. Japan implemented industrial policies, including protectionist tariffs and government subsidies, to shield its nascent, flourishing manufacturing base.

As its industrial capacity grew, so did Japan's need to find world wide; this economic necessity precipitated a shift in stance away from protectionism and toward free trade. In 1955, Japan entered the General Agreement on Tariffs and Trade (GATT). The multilateral treaty was forged in 1947 to promote trade relations among member countries. By the time it was replaced by the World Trade Organization in 1955, the GATT had 128 members.

Japan's entrance into the GATT led to many rounds of tariff reductions, including the Kennedy Round (1967) and the Tokyo Round (1979), which combined reduced tariffs on more than $340 billion in world trade. These tariff reductions helped drive Japan's export surge to the United States. Figure 1 documents that imports to the US increased nearly five-fold from 1962 (the first year for which there is product-level UN Comtrade data - see Section 3.3) to 1988, about the peak of Japanese import penetration to the United States. In our analysis, we will study the 1966-88 period when possible.[4]

[4]When using data from the decennial Census, we will study 1970-90, the closest decadal years. We begin

Two additional drivers of Japanese trade exposure occurred concurrently in the United States. First, in the early 1970s, the world faced an oil supply crisis, driven by geopolitical instability. Skyrocketing oil prices spurred American consumers to substitute toward more fuel-efficient Japanese automobiles. Second, following a decade of persistently high inflation, Federal Reserve Chairman Paul Volcker dramatically increased interest rates from 1979-1981. Increasing interest rates strengthens the US dollar, which increases the price of American exports relative to imports. Consequently, consumer demand for imported goods increased – notably those from Japan.

Underscoring these facts, Table 1 displays the manufacturing sectors most affected by the Japanese trade shock. In dollar terms, the auto indsutry felt the impact most, with imports from Japan soaring in the wake of the oil crisis. In terms of industry absorption[5] [6], the industries most exposed to the shock were industrial trucks/tractors/trailers, household video and audio equipment, and electronic capacitors; and the sectors most exposed to the Japan shock were electronics, transportation, and industrial machinery.

2.2 Production Operators Were High-Paying Jobs for Black Americans

Figure 2 shows occupation shares among Black and white men in manufacturing at large firms. This figure is constructed in our main dataset, compiled by the Equal Employment Opportunity Commission. Panel A plots occupation shares for Black men in the years 1966, 1973, 1979, and 1987; Panel B plots occupation shares for white men in the same years. This figure shows that just over half of all Black men at large manufacturers were production operators in 1966, with nearly all the remainder craft workers or laborers. By contrast, only a third of all white men at large manufacturers were production operators in 1966 – the operator share among Blacks was 50 percent higher than that among whites. Moreover, nearly 20 percent of white workers were managerial or professional in 1966, compared to under 2 percent of Black workers.

For Black men during the study period, the largest share of high-paying jobs were in

our analysis in 1966 (rather than 1962) because the EEOC first collected employment data then. See Section 3.1.

[5]Industry absorption is the main measure of the shock we use throughout the paper. It is defined as the change in imports, divided by value of industry shipments minus net exports. Its interpretation is the size of the product demand shock facing the industry. See Section 4.1

[6]The automobile industry was not in the top 25 industries by industry absorption. Since the identification of our design occurs at the industry level, this implies that the high dollar value change in automobile imports does not make the automobile industry an outlier, and thus does not complicate inference. See Section 4.2.

middle-skill occupations. Indeed, Figure 3 demonstrates the importance of production operator jobs for Black men in the period. Relative to white men, a larger fraction of the high-paid jobs held by Black men in the 1960s and 1970s were production operator jobs in the manufacturing sector. In 1970, production operators in the manufacturing sector accounted for over 15 percent of workers in the top quintile of the Black male income distribution. By contrast, that figure was under 4 percent for white men. Figure 3 also shows that, even after accounting for Black men's overrepresentation in production operator employment, these operator jobs fell higher in the Black income distribution than in the white. Black men in the fifth through ninth deciles of the Black income distribution were more likely to work as production operators than the Black average. By contrast, white men in the second through sixth deciles of the white income distribution were more likely to work as production operators than the white average. [7]

These production operator jobs began to disappear over the course of the subsequent decades. Figure 4 shows the rise and fall of manufacturing operator employment for Black Americans in the North. From 1920 to 1970, the manufacturing operator share of employment among northern Black men increased from 5 percent to 20 percent, nearly quadrupling. This increase was driven by large numbers of Black Americans moving North during the Great Migration, coupled with the post-World War II manufacturing boom. Yet, over the subsequent decades – as the Japan shock hit – manufacturing began its decades-long decline, sharply hitting operators. The Black manufacturing operator share of total employment decreased from 20 percent to 8 percent; the Black-white disparity in the production operator share of the total employment decreased from 7 percentage points to 1 percentage point.

3 Data

3.1 EEO-1 Data

Our main labor market statistics come from an administrative dataset collected by the Equal Employment Opportunity Commission: the EEO-1 data. The EEOC requires all

[7] Of course, in this period, the white income distribution stochastically dominates that of Blacks. In unreported results, we show that, conditional on overall income level (and even education level), Black workers were overrepresented in operator employment. Therefore, the conclusion of Figure 3 – that Black men were overrepresented in manufacturing employment, and that these were good jobs for Black Americans – holds true even when conditioning on income levels in the overall distribution. This conclusion is reaffirmed in Appendix B, where we implement a commuting zone-level version of the Bayer and Charles (2018) decomposition and project it onto the Japan shock. See Appendix B.

firms with more than 100 employees to file a report with the commission.[8] EEO-1 reports contain establishment-level, cross-tabulation counts by gender, race/ethnicity (for five groups: white, Black, Hispanic, Asian/Pacific Islander, Native American/Alaskan Native), and occupation (nine coarse categories: managers, professionals, technicians, sales workers, office/clerical workers, craft workers, operatives, laborers, and service workers). Multi-establishment firms must submit (a) a report for their headquarters, (b) one report for each establishment with more than 100 employees, (c) one combined report for all establishments with fewer than 100 employees, and (d) one consolidated report for all of their employees. Single-establishment firms submit one report. The EEO-1 data contains one record per report.

The EEO-1 dataset contains information on establishment industry - three-digit Standard Industry Classification (SIC) codes in most years in our sample, but four-digit SIC codes in later years. It contains fine-grained geographical information – by county for nearly all establishments in our sample, and either ZIP codes and/or addresses for most establishments in later years. The dataset was collected annually starting in 1966, but the data is missing for several of the early years. The data is available in 1966, 1971-1973, 1975, and every year since 1978.[9] we study the effects of the Japan shock starting in 1966, the first year the EEO-1 data was available.

3.2 Other Labor Market Data

The granularity of the EEO-1 data permits a detailed look at occupational employment within-firms and across various geographies, including counties and local labor markets. Because this dataset does not cover small firms, and does not contain wage information, we supplement the EEOC data with a number of auxiliary datasets.

Census. we use two separate databases from the U.S. Census Bureau: a microdata sample from the decennial census and extracts from the Census Tabulation Files (CTF). When the desired crosstabs are available, we use the CTF; in all other cases, we use the microdata extracts to run our analysis.

For the microdata sample, we use a 1% sample from the Census Bureau, taken from the Integrated Public Use Microdata Series (IPUMS). The main analysis data covers the

[8] Prior to 1983, the EEOC required all firms with more than 50 employees to file reports with the commission. To keep a consistent sample before and after the regulatory changes in 1983, we limit our dataset to establishments that have fewer than 100 employees.

[9] Because we run a decadal specification, the sporadic availability of earlier years of data is not a concern for our purposes.

period 1970-1990, though we extend the dataset backward to study pre-trends (to 1950), and backward and forward (to 1920 and 2010) to run descriptives, placebo checks, and checks for persistence in treatment effects. We use the microdata sample to study income, poverty, educational attainment, and family structure.

For the Census Tabulation Files, we use datasets extracted from Social Explorer. These data extracts are available from 1950 to 1990. We use the CTF to study income and poverty.

ASM. We use the Annual Survey of Manufacturers to obtain aggregate industry-level variables. We obtain the data from the NBER-CES Manufacturing Database. This dataset contains annual, industry-level aggregated production statistics from 1958 to the present.

We use this dataset for two purposes. First, we leverage it when creating our measure of industry absorption, which includes information on the value of industry shipments. Second, we study the effect of the shock on a number of industry-level variables, including capital, employment, and payroll. Third, we use this dataset to check for pre-trends in industry-level variables, including capital, payroll, employment, and industry shipments.

3.3 Trade Data

The main trade dataset comes from the UN Comtrade database. This database contains annual bilateral trade flows between countries. We use trade flow data from 1962, which is the first year that the data is available, until 1990. We follow Batistich and Bond (2023) in creating a crosswalk from the trade data (at the Standard International Trade Classification Revision 1 level) to the industry level (SIC 1987 codes). The result is a dataset of Japanese trade flows at the industry level that we match to the aforementioned labor market data. See Appendix A for details.

3.4 Coverage and Summary Statistics

Figure 5 shows the coverage of the EEOC dataset, defined as the number of employees covered in the EEO-1 dataset relative to the number of employees covered in the County Business Patterns, during the sample period that we study. The data includes about 40 percent of US employment; manufacturing is overrepresented in the EEO-1 data, as over 65% of manufacturing employment is covered in the EEO-1 data. The coverage is relatively stable over time.

Table 2 presents summary statistics about the EEOC data and the Census data. We see

that the following types of employment characteristics are overrepresented in the EEOC data relative to the Census data: manufacturing, operator, Northeast, and Black.[10] There are no notable differences in the gender balance between the EEOC data and the Census data. The granularity of the EEOC data, coupled with its broad coverage of manufacturing workers, means that the EEOC data provides a detailed lens for uncovering the effects of trade on workers.[11]

4 Empirical Strategy

4.1 Shift-Share Instrumental Variables Strategy

The study leverages a shift-share instrumental variables strategy.[12] This strategy has been commonly employed in the literature that estimates the effects of trade shocks on workers (Autor, Dorn and Hanson, 2013; Batistich and Bond, 2023; Kahn, Oldenski and Park, 2022). The shift-share strategy enables us to map industry-level trade shocks into local labor markets, since many outcomes of interest are area-level. For example, if a machinist is laid off from an automotive manufacturing plant as a result of Japanese import competition and finds work in a non-exposed manufacturing industry close by his house, he may be no worse off. Thus, the most appropriate way to study the effects of trade on workers is at the local labor market level, as a shift-share strategy enables.

Shift-share strategies combine two sources of variation. The **shifts** in our paper are the exports from Japan to the United States. Following Acemoglu et al. (2016), we use industry absorption as our measure of industry trade exposure, using data from UN Comtrade and the Annual Survey of Manufacturers.[13] This measure of industry absorption for in-

[10]The overrepresentation of manufacturing employment in the EEOC data is a mixed bag. One the one hand, it allows for a more detailed look at what occurs in manufacturing. On the other hand, without any adjustment, the dataset may provide an skewed picture of what is occuring in the economy. For this reason, in our main regression specifications, we normalize the change in employment counts (measured in the EEOC) by the Census population. So long as there are not systematic offsetting employment increases at firms not represented in the EEOC database, our estimated results will be smaller than the true reduction in employment.

[11]In unreported results, we also detect a negative effect of the Japan shock on the operator share of manufacturing employment in the Census data. This result is true for Black workers and overall. This finding rules out the hypothesis that our results are entirely driven by our focus on large firms (i.e. that the shock shifted employees one-for-one from large firms to small firms).

[12]We run a complementary specification at the industry level. Because, in the early years of our data, we only have three-digit SIC industries, these regressions are less powered (N=135) than our CZ-level shift-share specification. See Appendix B for details.

[13]We construct the industry measure of imports using product-level data coming from the UN Comtrade database. For more information about how we assign products to industries, see Appendix A.

dustry i at time t is defined as

$$\Delta IP_{i,t}^{US} = \frac{z_{US,i,t+1}^{Jpn} - z_{US,i,t}^{Jpn}}{vshp_{i,66} + z_{US,i,66}^{world} - x_{i,66}^{world}} \tag{1}$$

where $z_{US,i,t}^{Jpn}$ represents imports to the US from Japan in industry i at time t, $vshp_{i,66}$ is total value of industry shipments in industry i in 1966, $x_{i,66}^{world}$ are total industry exports from the US to the World in industry i in 1966, $z_{US,i,66}^{world}$ are total industry imports to the US from the World in industry i in 1966. In other words, the industry-level shifts are the change in imports normalized by the baseline industry size. Industry absorption represents a measure of the industry's product demand shock resulting from Japanese import competition.

The **shares** in our paper are the baseline industry employment mix within each local labor market, taken from the Census microdata. For commuting zone c, industry i, and time t, the shares are defined as

$$s_{c,1950}^{i} = \frac{E_{c,i,50}}{E_{c,50}} \tag{2}$$

The shares can be interpreted as the predicted change in employment resulting from a one-unit increase in industry import absorption. That is, if Japanese manufacturers in industry i increased their exports to the U.S. by 100 percent of the value of initial U.S. industry size (value of industry shipments minus net exports), then the share above describes how much employment in commuting zone c would change, as a fraction of initial CZ employment.

Note that, summing over industries i, the shares above add up to the total manufacturing share in the commuting zone. These shares are thus *incomplete* (Borusyak, Hull and Jaravel, 2021). In our regressions, we control for the overall manufacturing share in the commuting zone. We do this to avoid merely comparing between more- and less-manufacturing-intensive places; instead, we isolate the across-CZ variation in more- and less-Japan-exposed industries within the manufacturing sector.

Our main CZ-level measure of import penetration is the product of the industry shifts and the CZ-industry shares, summed over industries:

$$\Delta IP_{c,t}^{US} = \sum_{i} s_{c,1950}^{i} \Delta IP_{i,t}^{US} \tag{3}$$

This change in import penetration can be interpreted as follows: If Japanese companies in every manufacturing industry increased their exports to the U.S. by 100 percent of the value of initial US industry size (value of industry shipments minus net exports), then $\Delta IP_{c,t}^{US}$ results describe how much employment in commuting zone c would change, as a share of initial CZ employment.

The logic underlying the shift-share is simple: CZs are more exposed to the Japan shock when, at baseline, a higher fraction of their employment is concentrated in more Japan-exposed industries. For example, take Detroit and Baltimore. Both cities had a large working-class Black population in 1950, with many from the South, and both had high levels of manufacturing. Yet Baltimore's manufacturing base was concentrated in steel, fabrics, and food processing, while Detroit's was dominated by automobiles and electronics. Therefore, Detroit's employment was more vulnerable to the surge in import of autos and electronics from Japan; this is the variation captured by the shift-share strategy.

Following the recent shift-share literature on trade and worker outcomes, we use an instrument for Japanese exports to the United States. This is because imports to the US from Japan are influenced by two sets of factors: changes in US manufacturers' behavior and changes in Japanese manufacturers' behavior. If US manufacturers become more productive, for example, then imports will fall and employment may rise; this would cause a negative empirical relationship between imports and employment, yet the fall in imports would not drive the rise in employment. We aim to isolate the change in imports due to changes in Japanese exporter behavior.

To do this, we will construct a shift-share measure for six high-income countries (Australia, Denmark, Finland, New Zealand, Spain, and Switzerland) and average them together:

$$\Delta IP_{i,t}^{OC} = \frac{z_{OC,i,t+1}^{Jpn} - z_{OC,i,t}^{Jpn}}{vshp_{i,62} + z_{US,i,62}^{world} - x_{i,62}^{world}} \tag{4}$$

and the instrument is therefore defined as

$$\Delta IP_{c,\Delta t}^{OC} = \sum_{i} s_{c,1950}^{i} \Delta IP_{i,t}^{OC} \tag{5}$$

In the first stage, our model specification regresses import absorption to the United States onto the import absorption to other countries, thus isolating the exogenous Japan shock. The second stage then regresses the outcome onto the shock. The regression equa-

tions are

$$\Delta y_{c,t} = \alpha_1 + \beta_1 \Delta IP_{c,t}^{US} + \delta_1 X_c + \epsilon_{1,c,t} \tag{6}$$

$$\Delta IP_{c,t}^{US} = \alpha_2 + \beta_2 \Delta IP_{c,t}^{OC} + \delta_2 X_c + \epsilon_{2,c,t} \tag{7}$$

We weight all regressions by initial CZ employment and cluster standard errors by commuting zone. We run this specification both stacked (for 1966-78 and 1978-88), and for the long difference (1966-88). When running the stacked specification, we include a separate intercept term for each period.[14] All specifications control for the manufacturing share; for the stacked specification, we also interact the manufacturing share with the period indicator variable.[15]

The first stage is strong; the F-statistic is approximately 35. See Appendix B for details.

4.2 Shift-Share Instrumental Variables Identification

The shift-share instrumental variables design combines two sources of variation: industry-level shocks and local area-industry shares. This strategy may be identified in one of two ways. Either the shares may be exogenous, an assumption first detailed by Goldsmith-Pinkham, Sorkin and Swift (2020), or the shocks may be exogenous, an assumption first detailed by Borusyak, Hull and Jaravel (2021). We argue that our design is identified by the exogenous shocks assumption.

The exogenous shares assumption states that the local area-industry shares are as-good-as randomly assigned. The identification assumption is therefore equivalent to a parallel-trends assumption, comparing places that are higher in Japan-exposed industries with places that are lower in exposure. In this strategy, the local area-industry shares are used as instruments for the import penetration to the United States, and the industry-level shifters are taken as predetermined.

The exogenous shares assumption generally does not work in the trade setting. The main variation that we exploit is one across industries. In certain industries, Japan increased its comparative advantage with strategic investments in productive capacity. Moreover, trade costs declined in certain industries, as protective tariffs and other trade barriers fell with multilateral trade treaties. Thus, the variation in changes in Japanese productive capacity is fundamentally at the industry-level, not the area-level.

[14] We do not include an indicator variable for each commuting zone, as it is not necessary for valid inference. The results are qualitatively similar when including an indicator variable.

[15] Our main result is robust to including additional controls: the Black share of manufacturing, region controls, operator share of manufacturing.

Moreover, the exogenous shares assumption is doubly problematic in our setting because Black workers moved Northward during the Great Migration, in part to take manufacturing jobs. In the northern United States, according to the 1970 Census, in the Northern U.S., Southern-born Black workers were twice as likely to work as production operators as those born in the North. Clearly, using manufacturing shares as an instrument cannot work when Black workers moved to the local area to take manufacturing jobs.

Instead, our design is best thought of as identified by the exogenous shocks assumption. The second-stage (area-level) regression is numerically equivalent to a reweighted industry specification, and the identification occurs at this (reweighted) industry-level. The observed industry-level measure of import penetration to comparison countries serves as an instrument for the reweighted import penetration to the United States. Thus, the identification assumption is an as-if-random shocks assumption, comparing industries that are higher in Japanese import penetration with those that are lower in import penetration. This strategy takes the shares as pre-determined.

In the trade setting, an additional assumption is necessary for identification (Autor, Dorn and Hanson, 2013). The within-industry common component of import growth in the United States and comparison countries must be due to changes in the comparative advantage structure within Japan or falling trade costs facing its exporters. The design may be complicated by two factors. First, simultaneous shifts in demand in the United States and comparison countries would weaken the first stage and/or bias the results towards zero. Second, contemporaneous shifts in productivity among manufacturers in the United States and comparison countries may bias the result upward or downward.

In Appendix B, we show that the industries most exposed to the Japan shock were expanding prior to the shock. Thus, we need an additional assumption for identification: that the shock is uncorrelated with any simultaneous changes in previous trends. We assume, then, that the pre-trend in employment would have continued apace in the absence of the Japanese trade shock. As we show in Section 5.1.3, the value of industry shipments, a measure of the size of the industry, showed the same correlation with the Japan shock in the pre-period and during the time period of the shock. Therefore, because the siize of the industry remained unchanged, we argue that it is plausible to assume that the shock was uncorrelated with changes in the trend.

5 Results

5.1 Overall Impact of the Japan Shock on Black and white Workers

5.1.1 Employment effects, by occupation/race

Figure 6 reports the effect of the Japan trade shock on CZ employment by race and occupation, among men only, using our main EEO-1 employment dataset. This figure runs 27 separate regressions: one for each race and coarse occupation category. Note that, because the occupation categories are mutually exclusive and exhaustive categories within each race, the nine regression coefficients for each occupation add up to the total within each race. Additionally, because some workers are neither Black nor white, a population-weighted (or employment-weighted) average of the Black and white coefficients will not yield the coefficients for the All series. Nonetheless, because there were few manufacturing workers of other races at baseline, such a population-weighted average would closely resemble the All series; for this reason, we omit this "Other Race" series from the chart. The dependent variable is the change in occupational employment, normalized by initial population; we use a stacked specification detailed by equations (6) and (7).

Figure 6 suggests that the Japan shock reduced overall manufacturing employment by approximately 3 percentage points, as a share of initial CZ population. About half of this decrease stemmed from reduced production operator employment–approximately 1.5 percentage points. Craft workers and managerial/professional workers also saw decreases. White manufacturing employment declined by approximately 1 percentage point as a share of initial CZ employment; the decrease was split between craft, operator, manager/professional, and technician employment.

Figure 6 also shows that Black workers bore the brunt of the shock. Black manufacturing employment fell just over 3 percentage points, as a share of initial CZ employment. The racial difference in impact is statistically significant; we reject the null that the employment reductions are equal for Blacks and whites at the five percent level. Production operators accounted for about 80 percent of the reduction in Black employment. The 2.4 percentage point drop in Black operator employment compares with a 0.4 percentage point decline for their white counterparts. This difference is statistically significant.

The sizes of these magnitudes are substantial. The reduction in manufacturing employment over the time period, for both Black and white workers, was approximately 5 percentage points (Gould, 2021). These findings suggests that 20 percent of the de-

crease in white manufacturing employment over the period, and approximately half of the decrease for Black workers, can be explained by the Japan trade shock.[16] As a second benchmark, consider that 20 percent of Black workers in the northern United States were manufacturing operators in 1970. Thus, our results imply a nearly 25% reduction in the share of Black workers that were production operators as a result of the Japan shock.

Moreover, both of the previous effects may be underestimates. Our results are estimated for large firms, the only companies present in the EEOC data. Thus, our outcome variables are changes in employment at large firms, normalized by *total* initial CZ population. Assuming no systematic offsetting employment increase at firms in the EEOC database versus those not in the database, our estimated results will be smaller than the true reduction in employment.

Taken together, these results confirm that Japanese trade was a major driver of reduced manufacturing employment during the period. These reductions are racially disparate – nearly twice as large for Blacks than whites – and the disparate outcome for Blacks is driven almost exclusively by production operator employment.

5.1.2 On the robustness of the main result

We now investigate the robustness of our employment results to different specifications and across different subgroups. In all of the figures in this section, the dependent variable is the change in occupational employment normalized by initial population; we use a specification detailed by equations (6) and (7). Unless otherwise noted, the regressions are stacked.

For the first robustness check, we note that the Japan shock should have the largest displacement effects on the groups most likely to work in manufacturing. Thus, the impact on men should be greater than that on women, since manufacturing is a male-dominated occupation. We confirm this in Figure 7, which shows the effects of the Japan shock by gender.[17] The effects of the shock are more pronounced among men than women. The shock reduced manufacturing employment for both Black and white men; but spared white women. Manufacturing employment did fall among Black women, but this reduction was half that of Black men.

In the next two figures, we explore the time dimension of the shock. Because the

[16] Modulo the missing intercept critique, for which there is no general solution, to our knowledge.

[17] Figure 7 only displays the results for manufacturing operators and all manufacturing employees. Appendix B presents results for all nine occupations in the database.

change in US industry import absorption was nearly twice as high in the 1980s as it was in the 1970s, we would expect to see a lager displacement in the 1980s. We confirm this in Figure 7, which separately displays the employment effects of the Japan shock by period (1966-78 and 1978-88). The qualitative takeaway from both periods are similar; the regression on the earlier period is less precise than the regression on the later period. In both specifications, we see decreases in Black manufacturing employment and Black manufacturing operator employment. The coefficients on overall and white manufacturing and white manufacturing operator employment are significant in the later period, but not in the earlier period. The quantitative magnitudes from both periods are statistically indistinguishable and are of similar magnitude. But because the shock was twice as large in the later period as in the earlier period, the employment reductions for both races were twice as large then as well.

We also explore the differences between a stacked decadal specification and long difference specification. Because both specifications measure the same economic phenomena over the same ultimate time period, the results should be broadly consistent across specifications. We confirm this in Figure 7, which compares a stacked specification (for time periods 1966-78 and 1978-88) with a long difference specification (1966-88). The qualitative takeaway from both specifications is similar, although the long difference specification is less precise. In both specifications, we see decreases in Black employment and Black operator employment. The coefficients on overall and white manufacturing employment are insignificant in the long difference specification. For each coefficient, the difference between it and the comparable coefficient in the stacked specification is not statistically significant.

Next, noting that trade shocks directly affect manufacturing, we would expect the displacement effects to be greater there than in the non-manufacturing sector. Figure 7 confirms this to be the case. The shock had comparably little effect on non-manufacturing employment, either overall or by race. However, the shock did cause statistically significant decreases in non-manufacturing operator employment across races. Compared with the impact on the manufacturing sector, the magnitude was one sixth for Blacks and half for whites. These figures imply that the Japan trade shock reduced total employment.

Last, Figure 7 displays the effects estimated by ordinary least squares (OLS), compared with the main estimation strategy of instrumental variables (IV). This exercise helps probe the size of the bias that the instrument solves, as well as checks that the results are not qualitatively inconsistent. The magnitudes of the OLS regressions are substan-

tially smaller than the magnitudes of the IV regressions. The difference between the OLS and the IV estimates are the size of the endogeneity bias that the exogenous instrument purges. The upward bias of the OLS estimate suggests that endogenous factors, such as technological changes, are positively correlated with the Japan shock, thus increasing the estimate. By purging these factors, the IV estimates reveal the true negative effects of the Japan shock. Note that the coefficient on Black operators and overall Black employment are negative for both OLS and IV, so the conclusion that the Japan shock reduced Black employment does not depend on the instrument.

5.1.3 Aggregate effects, controlling for pre-trends

As discussed in Section 4.2, the Japan shock hit industries that were growing. This suggests that a comparison of employment changes in more- versus less-Japan-exposed industries might understate the true employment effects of the Japan shock, if these industries would have grown in the absence of the shock.[18] To explore this possibility, we control for pre-trends in our regression. Because the EEO-1 data begins in 1966, we do not have pre-trends measured in our data. Therefore, we turn to the publicly-available database from the Annual Survey of Manufacturers for this exercise.

In Figure 8, we present the effects of the Japan trade shock on industry-level outcomes, using an industry-level specification and data from the Annual Survey of Manufacturers. We stack the periods 1966-88 and 1958-66, defining the endogenous variable and instrument to be zero in the pre-period, and including industry and period fixed effects. Thus, the specification is the industry-level analogue to the stacked specification (6) and (7), but the endogenous variable and instrument are zero in the pre-period, and the matrix of controls includes fixed effects.

This figure shows us that the Japan shock hit industries that had been on a growth trajectory; overall employment, production employment, payroll, and production payroll were all increasing prior to 1966. If we do not account for these pre-trends, we do not have the statistical power to detect a negative relationship between the shock and employment growth. When we do account for the pre-trends, we see a negative relationship between employment and the shock.

Batistich and Bond (2023) found that the Japan shock had no effect on manufacturing overall, but did cause a shift in demand towards higher-skilled workers. They argue that

[18] The identification assumption under which this is true is discussed in Section 4.2 – namely, the shock needs to be uncorrelated with changes in the pre-existing trends.

in response to the shock exposed industries increased capital expenditures, which in turn raised demand for skilled labor. Figure 8 examines this interpretation. The Japan shock hit industries where employment was growing in the pre-period; relative to that rapid growth rate, employment growth slowed significantly in more Japan-exposed industries. Moreover, in Appendix B, we show that the Japan shock disproportionately hit more capital-intensive industries – complicating the interpretation that the Japan shock caused industries to increase their capital intensity. In light of these pre-trends, we interpret the evidence as the Japan shock reducing manufacturing overall, with no appreciable increases in demand for skilled employment. (That is, the shock increased the *relative* demand for skilled employment, but not the *absolute* demand.)

5.1.4 Effects on Black incomes and communities

At present, there are large disparities between Black and white Americans in income, education, poverty, family structure, and a number of other economic outcomes. In the 2010 Census – the last decadal Census year before the COVID-19 pandemic – Black earnings were about 40 percent lower; marriage rates were 22 percentage points lower; and poverty was 17 percentage points higher.[19] These disparities reflect long-lasting inequities that held in the late 20th century as well (Bayer and Charles, 2018; Gould, 2021). Wilson (1999) argues the decline of blue-collar manufacturing led to a number of adverse effects on Black communities, including on poverty, joblessness, and family formation. Moreover, the previous three subsections of this paper demonstrated that the Japan shock reduced manufacturing operator employment – and Figure 3 demonstrates that the production operator jobs were a large fraction of the high-paying jobs held by Black men in the period. In this subsection, we test the Wilson (1999) hypothesis by testing for contemporaneous effects of the Japan trade shock across the Black income distribution and the persistent effects on Black joblessness and poverty.

To test for racially disparate effects on income, we follow the method from Chetverikov, Larsen and Palmer (2016).[20] In this method, we regress changes in quantiles of the unconditional income distribution (by race) onto the Japan shock; we use a long-difference specification detailed by equations (6) and (7). We run this exercise in the Census micro-

[19]The result on poverty is widened a bit by the Great Recession. In 2019, this figure was 12pp - still a sizable disparity.

[20]Because there are no individual-level controls in our setting, the CLP method simplifies to regressing the unconditional quantiles of the commuting zone income distribution onto the commuting-zone-level shock, with controls. Standard errors clustered at the commuting-zone-level.

data, and the years we study are 1970-90, the nearest decadal Census years to the years of study for our main EEO-1 regressions.

Figure 9 displays the results. Panel A shows the effects of the Japan shock on incomes, by percentile of the Black income distribution. At the median of the income distribution, the Japan shock caused a reduction in annual income of approximately $100 in the average CZ; given that Black average annual incomes were around $3,050 in 1970, this implies that the Japan shock reduced Black incomes by about 3 percent. Moreover, the greatest shift in the distribution was near the middle of the distribution: at the bottom of the distribution (5th percentile) and near the top (90th+ percentile), the Japan shock caused no change in Black incomes. These results suggest that the Japan shock hollowed out the Black middle class.[21]

By contrast, the Japan shock had a smaller impact on white incomes. As can be seen in Panel B, the shock reduced annual income by less than $30 at the median of the white income distribution – less than 1 percent. There were no disparate changes in the bottom, middle, or top of the distribution.

For the persistent effects of the Japan shock, we study outcomes from the initial decadal Census around the beginning of the Japan shock (1970) until the last pre-pandemic decadal Census (2010).[22] We use a long-difference specification detailed by equations (6) and (7), with one modification: Our outcomes are defined over the period 1970-2010, which we regress on our long difference shock (1970-90).

Figure 10 shows the effects of the Japan shock on Black and white poverty. On average, the shock increased the poverty rate by one-tenth of a standard deviation for Black Americans. The impact was consistent across definitions of the poverty rate: income less than 50 percent, 100 percent or 150 percent of the poverty line. White Americans saw no significant effects of the Japan shock on persistent poverty. Our estimates imply that 2.6 percentage points, or 15 percent, of the 2010 gap in Black-white poverty can be explained by the Japan shock of the 1970s and 1980s.

Figure 10 also displays effects on joblessness. We find persistent and significant negative effects of the Japan shock on the fraction of Black Americans in the labor force; we also find effects on the fraction of Black Americans employed, though this coefficient is

[21]These income results are in accordance with Batistich and Bond (2023). They study medians only; we study the effects across the distribution.

[22]Using a causal analysis centered on the Japan shock, Batistich and Bond (2023) confirm the Wilson (1999) hypothesis about the manufacturing decline for the 1970-1990 period coinciding with the Japan shock.

significant only at the 10 percent level, not the 5 percent level. Our estimates suggest that the Japan shock caused a one-tenth of a standard deviation decrease in the fraction of Black Americans in the labor force. Approximately 3.4 percentage points, or 33 percent, of the 2010 gap in Black-white labor force participation is due to the Japan shock.

In addition to confirming the Wilson (1999) hypothesis, this exercise demonstrates how historically important economic events cast a long shadow. Though the Japan shock occurred during the 1900s, it is partially responsible for the high level of inequality present in the economy today by its role in expanding the urban Black underclass.

5.2 Understanding Disparate Impact: Disparate Exposure

5.2.1 Measuring the Contribution of Disparate Exposure

The previous section establishes that the Japan shock had a larger effect on Black employment than on white employment. We demonstrated that Blacks were more likely to work in production operator employment – a type of manufacturing job disproportionately vulnerable to the Japan shock. This section quantifies the role of the baseline concentration of Black workers in operator employment ("disparate exposure") in driving disparate employment effects.

We measure disparate exposure using an Oaxaca decomposition. It separates racially disparate changes in employment, normalized by initial population, into two components. First are the changes due to racial differences in the baseline occupational shares. Second are the changes due to racial differences in the proportional employment changes conditional on the initial share. We will perform this decomposition at both the CZ- and the firm-level.

For each commuting zone c, race r, and time t, we can decompose the change in employment normalized by population as

$$\frac{E^r_{c,t+1} - E^r_{c,t}}{P^r_{c,t}} = \underbrace{\frac{E^r_{c,t+1} - E^r_{c,t}}{E^r_{c,t}}}_{\text{proportional change}} \times \underbrace{\frac{E^r_{c,t}}{P^r_{c,t}}}_{\text{baseline share}} \equiv g^r_{c,t} s^r_{c,t} \tag{8}$$

Therefore, as seen before, the Black-white difference in employment can be decomposed as

$$\frac{E^b_{t+1} - E^b_{c,t}}{P^b_{c,t}} - \frac{E^w_{t+1} - E^w_{c,t}}{P^w_{c,t}} = \underbrace{\frac{(s^b_{c,t} - s^w_{c,t})(g^b_{c,t} + g^w_{c,t})}{2}}_{\text{disparate exposure}} + \underbrace{\frac{(g^b_{c,t} - g^w_{c,t})(s^b_{c,t} + s^w_{c,t})}{2}}_{\text{DICE}} \quad (9)$$

The "disparate (occupational) exposure" term is the compositional effects term in the Oaxaca decomposition; it is the portion of the total racially disparate employment change that can be explained by racial differences in the baseline occupational shares.[23] The "disparate impact conditional on exposure" (DICE) term is the within-occupation term of the Oaxaca decomposition; it is the residual disparate impact unaccounted for by racial differences in baseline occupation shares. Put differently, it is the portion of disparate impact explained by racial differences in the proportional employment changes induced by the shock.[24]

Note that, because the compositional effects term only includes occupation shares, any additional covariate that disparately exposes Black workers to the Japan shock accrues to the DICE term. Thus the DICE term in the decomposition includes all forms of disparate treatment, as well as forms of disparate exposure that are not captured by baseline occupation shares.

5.2.2 Overall CZ decomposition

We now turn to the empirical implementation of our Oaxaca decomposition. We first run this for all commuting zones, and then account for firm-level variation in exposure. We regress each component of the decomposition onto the shock and recover the coefficients. We use a stacked specification detailed by equations (6) and (7).

Figure 11 presents the results of the Oaxaca decomposition for each of the nine occupations in the database. We see that two-thirds of the excess decrease in Black operator employment (relative to white) is due to disparate exposure; one-third of the decrease is due to DICE. None of the other coefficients are noteworthy: Black laborer employment declines slightly more than white, and white craft employment declines slightly more

[23] There is a second, more substantial interpretation of "disparate exposure": it is the racial difference in a shift-share counterfactual change in employment by race, where the shifts are aggregate employment changes in race, and the shares are the baseline racial composition within each occupation. This interpretation underscores the shift-share nature of the Oaxaca decomposition.

[24] In classic legal terminology, the portion of disparate impact due exclusively to race, and not proximately explained by any other observable factors, is called "disparate treatment". Our measure of disparate exposure only captures racial differences in occupational composition – other forms of non-occupational disparate exposure are unobserved. Thus we use the more neutral term "DICE" to indicate that all non-occupational disparate exposure accrues to the residual term.

than black, but both of these changes are one-fourth the size of the racially different decrease in operator employment.

Both the disparate exposure term and the DICE term are statistically significant from zero (and from each other). The significance of the disparate exposure term rejects the null hypothesis that there is no disparate occupational exposure, thus establishing that the large concentration of Black workers in operator employment at baseline increased the effects of the Japan shock on Black workers. But the DICE term is significant as well. As noted previously, DICE includes disparate treatment as well as all other forms of disparate exposure that are not caused by occupational differences. Though most of the disparate impact is explained by disparate exposure, the minority that is due to DICE suggests that disparate treatment or non-occupational disparate exposure may matter, tool.

5.2.3 Firm-level Decomposition

The previous result demonstrates the centrality of disparate exposure in driving aggregate disparate impact. Yet it misses an important fact. Occupational segregation, other forms of disparate exposure, and disparate treatment can vary across firms, depending on the structure of the workplace (e.g., rules surrounding promotion and firing, and variation in the mix of occupations required for production). Thus, in this section, we leverage the granularity of our data and introduce firm-level variation into our decomposition. This decomposition forms the basis for further exploration of firm-level mechanisms, including prejudice, in subsequent sections.

In a commuting zone c, the racial difference in employment changes, normalized by population, is the difference present across each firm f:

$$\frac{\Delta E^t_{b,c}}{P^t_{b,c}} - \frac{\Delta E^t_{w,c}}{P^t_{w,c}} = \sum_f \left(\frac{\Delta E^t_{b,f}}{P^t_{b,c}} - \frac{\Delta E^t_{w,f}}{P^t_{w,c}} \right) \tag{10}$$

As with to the CZ-level decomposition, for each firm f, we can rewrite the firm employment changes for each race r as

$$\frac{\Delta E^t_{r,f}}{P^t_{r,c}} = \underbrace{\frac{\Delta E^t_{r,f}}{E^t_{r,f}}}_{\text{proportional change}} \times \underbrace{\frac{E^t_{r,f}}{P^t_{r,c}}}_{\text{baseline share}} \tag{11}$$

Thus, using the standard Oaxaca decomposition, we can decompose the total disparate impact into disparate exposure and DICE:

$$\frac{\Delta E^t_{b,c}}{P^t_{b,c}} - \frac{\Delta E^t_{w,c}}{P^t_{w,c}} = \underbrace{\sum_f \frac{1}{2}\left(\frac{\Delta E^t_{b,f}}{E^t_{w,f}} + \frac{\Delta E^t_{w,f}}{E^t_{w,f}}\right)\left(\frac{E^t_{b,f}}{P^t_{b,c}} - \frac{E^t_{w,f}}{P^t_{w,c}}\right)}_{\text{disparate exposure}} + \underbrace{\sum_f \frac{1}{2}\left(\frac{\Delta E^t_{b,f}}{E^t_{b,f}} - \frac{\Delta E^t_{w,f}}{E^t_{w,f}}\right)\left(\frac{E^t_{b,f}}{P^t_{b,c}} + \frac{E^t_{w,f}}{P^t_{w,c}}\right)}_{\text{DICE}} \quad (12)$$

We regress each component of the decomposition onto the shock, and recover the coefficients. We use a stacked specification detailed by equations (6) and (7).

In this section, we restrict our attention to firms that are operational and present in the data in the baseline time period. Both firms that exit and firms that operate in both periods are included in the decomposition.[25] We also restrict our attention to firms that have at least one Black operator and one white operator, so that we can calculate proportional changes for the Oaxaca decomposition.

Figure 12 displays the results, across an ordinary least squares and instrumental variables specification, as well as with fewer controls and the full set of controls. Across specifications, the fraction of total disparate impact due to disparate exposure is greater than two-thirds; with the full set of controls, the total disparate exposure explains nearly the full effect. The fact that the fraction explained by disparate exposure is similarly high reaffirms the conclusion from the previous exercise: that baseline concentration of Black workers in production operator employment disparately exposed them to the Japan shock.

Also worth noting is that, in this specification, DICE is insignificant with the full set of controls (though borderline significant when only controlling for the manufacturing share); this suggests that some of the DICE in the previous figure is explained by firms that either did not exist at baseline (entrants) or firms that did not have at least one Black and white operator.

[25]Note that there is zero DICE for exiting firms, since the proportional employment changes are -100 percent for both Black and white workers.

5.2.4 Prejudice and disparate exposure

Disparate exposure explains over two-thirds of the negative effects of the Japan shock on Black workers, and the remaining is a residual, likely explained by a mixture of other dimensions of disparate exposure we are not measuring and disparate treatment due to racism and other social factors (e.g., among operators doing the same job, Blacks may be more likely to be laid off under some circumstances). Moreover, even the extent of disparate exposure, which accounts for the bulk of our results, is socially determined, since historic patterns of discrimination in education and hiring likely impacted where Black workers were employed.

To shed preliminary light on these factors, we study whether prejudice against Black people was associated with the disparate exposure driving the racially disparate effects of the Japan shock that we estimate. In itself, anti-Black prejudice is a set of racist beliefs, while discrimination is an outcome that can result from anti-Black prejudice or more systemic factors. We note that we do not directly measure discrimination. Nonetheless, if prejudice explains a substantial fraction of the disparate exposure, that would suggest discrimination may be a driving factor.

We employ two measures of prejudice in our analysis. First, we use the 1968 vote share for George Wallace, an explicitly segregationist candidate.[26] Second, we construct an index using a battery of questions on racial resentment from the 1972-73 General Social Survey.[27] We construct both measures at the county- and CZ-levels.

We first explore the relationship between local area anti-Black prejudice and firm-level baseline occupational differences. We run a regression of the firm-level share of operators who are Black onto the county-level Wallace share, sequentially adding controls. Because the levels of manufacturing and the levels of support for Wallace ranged widely by region, we run these regressions separately for the North and the South. Figure 13 shows a positive association between the relative proportion of operators that are Black and the county-level Wallace share, conditional on firm size and region. While not definitive, this figure suggests an association between local area prejudice and baseline racial occupational differences.

In the second exercise, we run an Oaxaca decomposition analogous to equation (12).

[26] Using the Wallace vote share at the CZ-level, Batistich and Bond (2023) argue that discrimination played no role in driving racially disparate effects of the Japan shock. We improve on their design by using county-level variation in Wallace votes, coupled with firm-level variation in employment outcomes.

[27] See Appendix A for details on the construction of this measure.

For both of our prejudice measures, we standardize the county-level measures by subtracting the CZ-level average measure, and dividing by the CZ-level averages. We split the sample in half, based on if the firm is in a county with the normalized index greater than or less than zero. We regress each component of the decomposition onto the shock and recover the coefficients. We use a stacked specification detailed by equations (6) and (7).

Figure 14 reports the results. As in Figures 12, due to controls, there is no DICE in this figure – almost all of the disparate impact is driven by disparate exposure. For both measures of prejudice, approximately two-thirds of total observed disparate exposure is present in the high prejudice counties. These results suggest that prejudice is associated with disparate exposure to the Japan shock. Yet this relationship is not necessarily causal: We cannot rule out that the Japan shock itself caused a racist backlash. We leave clarifying the causal effects of prejudice to companion work.

6 Conclusion

The Black-white wage gap substantially closed prior to 1970, after which progress stalled. That shift coincided with a decline in manufacturing. We show that manufacturing operator jobs were a disproportionate share of the high-paying jobs held by Black Ameircans in the 1970s – jobs that disappeared rapidly over the ensuing decades.

This paper tests whether their overrepresentation as production operators left Black workers vulnerable to the Japan trade shock of the 1960s-1980s. The United States began to import an increasing number of automobiles, electronics, and other products in the decades after World War II, leading to a reduction in demand for production employees. As the quintessential middle-skilled occupation of the mid-20th century, production operator employment was the most vulnerable to Japanese trade. We find that the Japan shock had large racially disparate effects on employment and income. Our estimates imply that the decline in Black operator employment decreased Black manufacturing employment by 10 percent, accounting for approximately half of the overall reduction of Black manufacturing employment over the period.

We then test whether Black workers were disparately exposed to the Japan shock by virtue of being disproportionately concentrated in production operator employment. We find that over two-thirds of the racially disparate decrease in production operator employment was due to disparate occupational exposure. Further, we provide evidence that

the firm-level disparate exposure to the Japan shock was associated with local area racial prejudice.

More broadly, our results point to a potential channel through which occupational segregation can shape racial inequality: namely, by disparately exposing Black workers to aggregate sectoral shocks. In the 1970s, like the American economy broadly, the manufacturing sector featured a high level of occupational segregation. Blacks had few paths to advancement into higher-paid work for myriad reasons, including skills and education gaps, discrimination, and other deep-rooted factors. This historical occupational segregation left Black workers disparately vulnerable to the Japan shock. Even when sectoral shocks affect all workers, they can still have long-lasting effects on inequality when Black workers are concentrated in the types of work most exposed to the disruption.

We conclude with thoughts for future directions. This paper leaves open the question of what caused the occupational segregation present in the manufacturing sector at the time of the Japan shock. We provide suggestive evidence of an association between local area racial prejudice and disparate occupational exposure. Future work may disentangle the role played by various other factors, including labor market institutions and education, in causally driving the occupational segregation present in manufacturing at the time.

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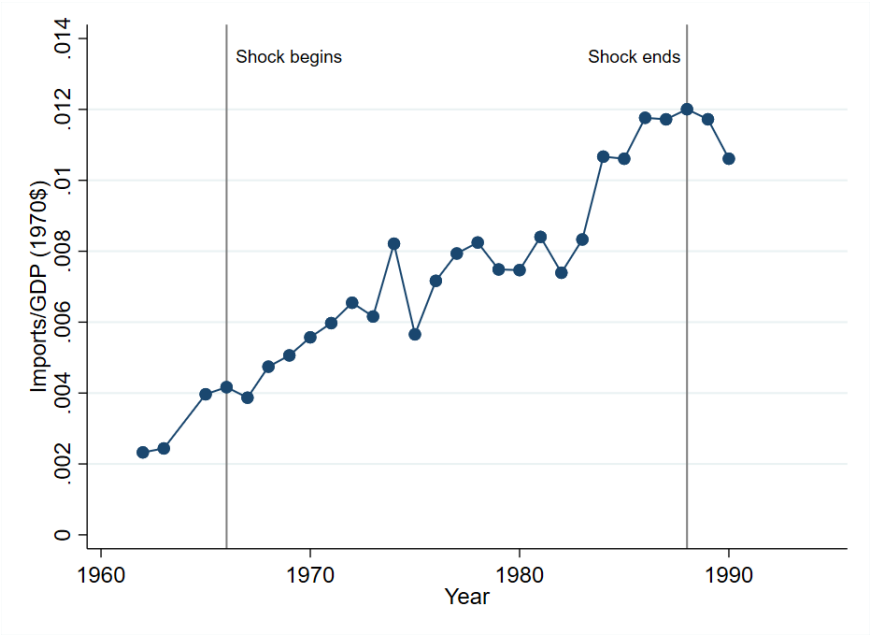
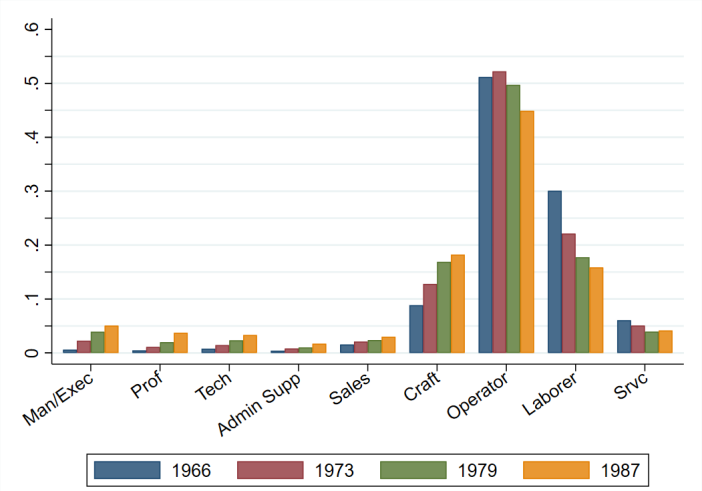


Figure 1: Japanese Import Penetration, Over Time

Notes: This figure shows the level of US imports from Japan per GDP, from 1962 to 1990. Gray lines denote the time periods we study: 1966-88. Trade data taken from UN Comtrade.

Panel A: Black



Panel B: White

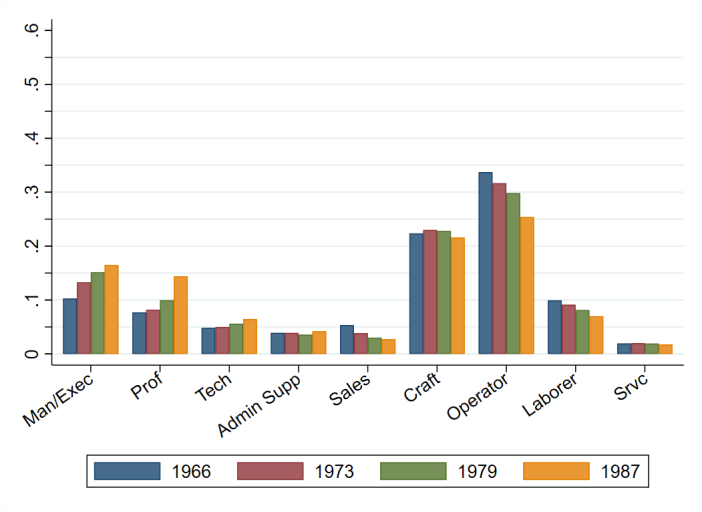


Figure 2: Occupation Shares By Race, Manufacturing

Notes: This figure the occupational shares of Black men and white men in manufacturing at large firms, for selected years. This figure is calculated in the EEO-1 data; see text for details.

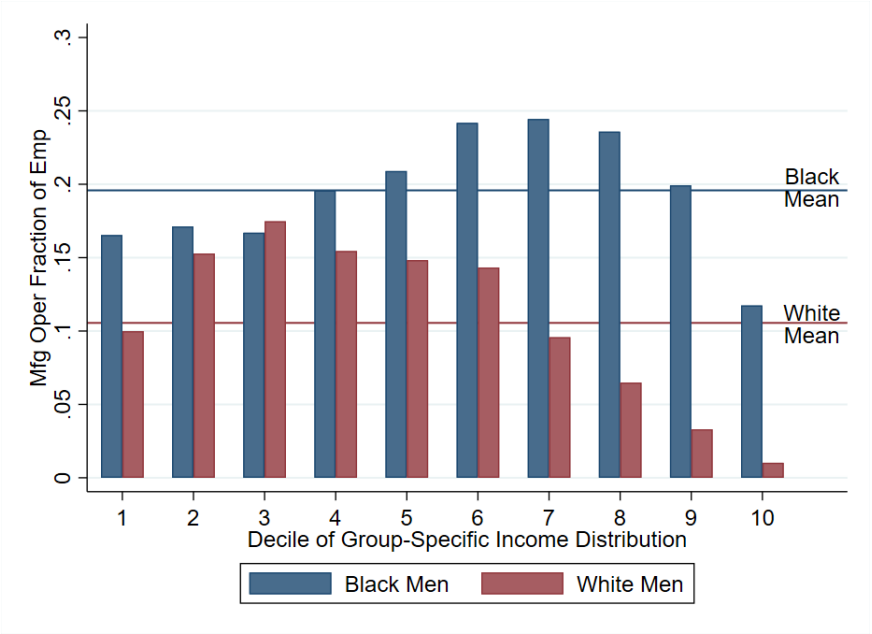


Figure 3: Production Operator Jobs Were High Fraction of Black Well-Paying Jobs

Notes: This figure displays the fraction of workers in each racial group (Black/white) who are operators and in manufacturing, by decile of the Black/white-specific income distribution. Horizontal lines indicate the average likelihood of working as a manufacturing operator. The sample in this figure is limited to the Northeast and Midwest Census regions. This figure is calculated in the 1970 Census.

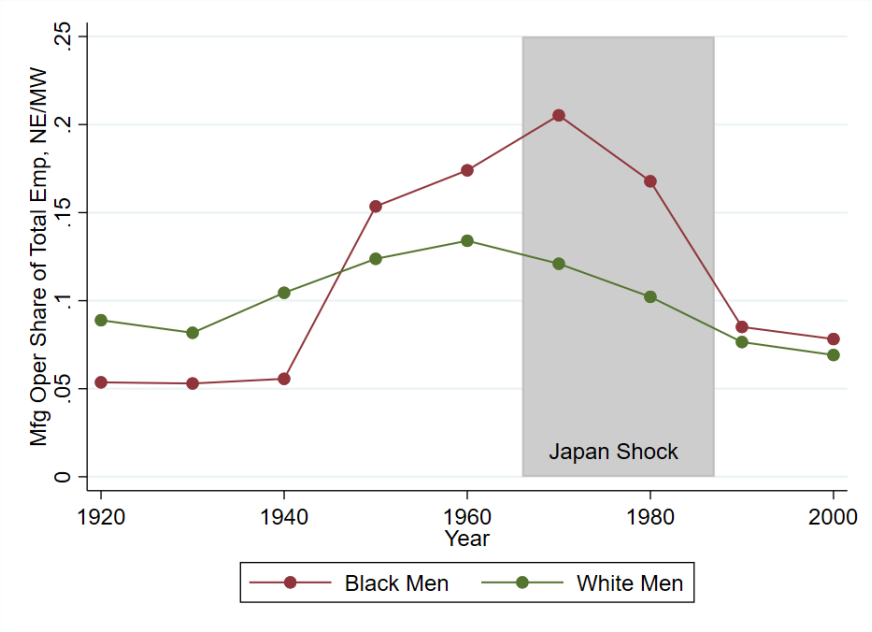


Figure 4: Trends in Northern Manufacturing Employment, by Race

Notes: This figure displays the fraction of workers in each racial group who are operators in manufacturing. The sample in this figure is limited to the Northeast and Midwest Census regions. This figure is calculated in the Census data, in the years 1920-2000. The time period of the Japan shock is shaded in gray.

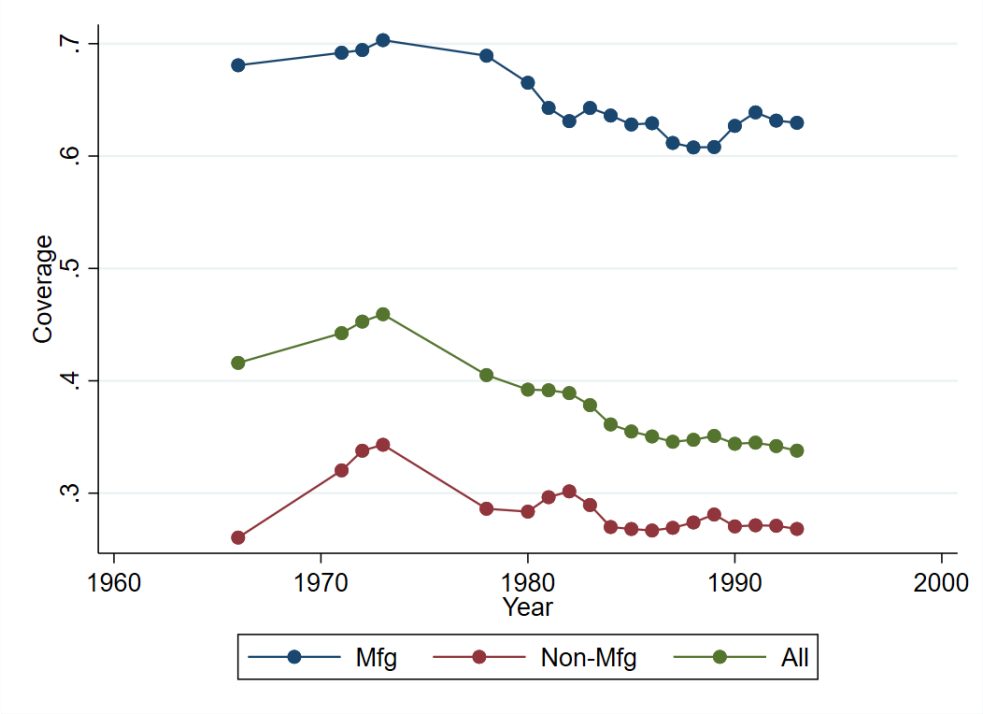


Figure 5: EEO-1 Data Coverage, by Sector

Notes: This figure displays the fraction of the workforce covered by the EEOC data, over time. This figure displays the count of EEO-1 employment, divided by the County Business Patterns employment, by sector (manufacturing vs. non-manufacturing vs. all).

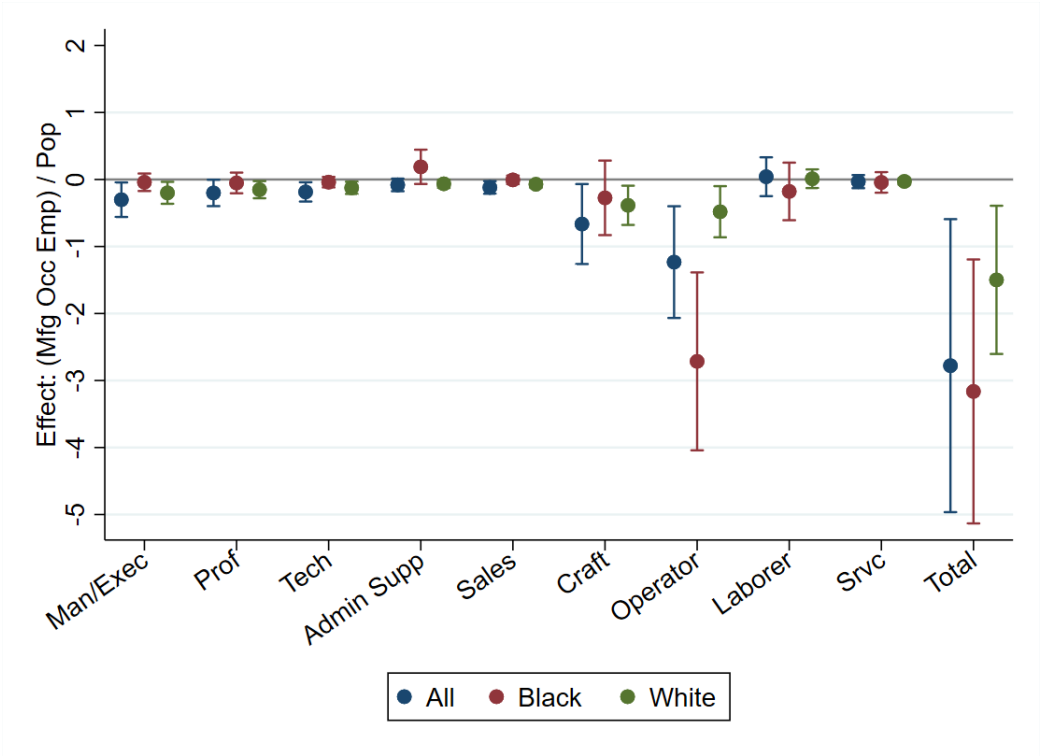


Figure 6: Main Result: Employment Effects of Japan Shock

Notes: This figure displays the coefficients from a regression of the change in manufacturing occupational employment, divided by initial population onto the Japan shock. These regressions are run in the EEOC data, over the time periods 1966-78 and 1978-88 (stacked specification). Regressions weighted by 1950 CZ employment. This figure includes men only. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

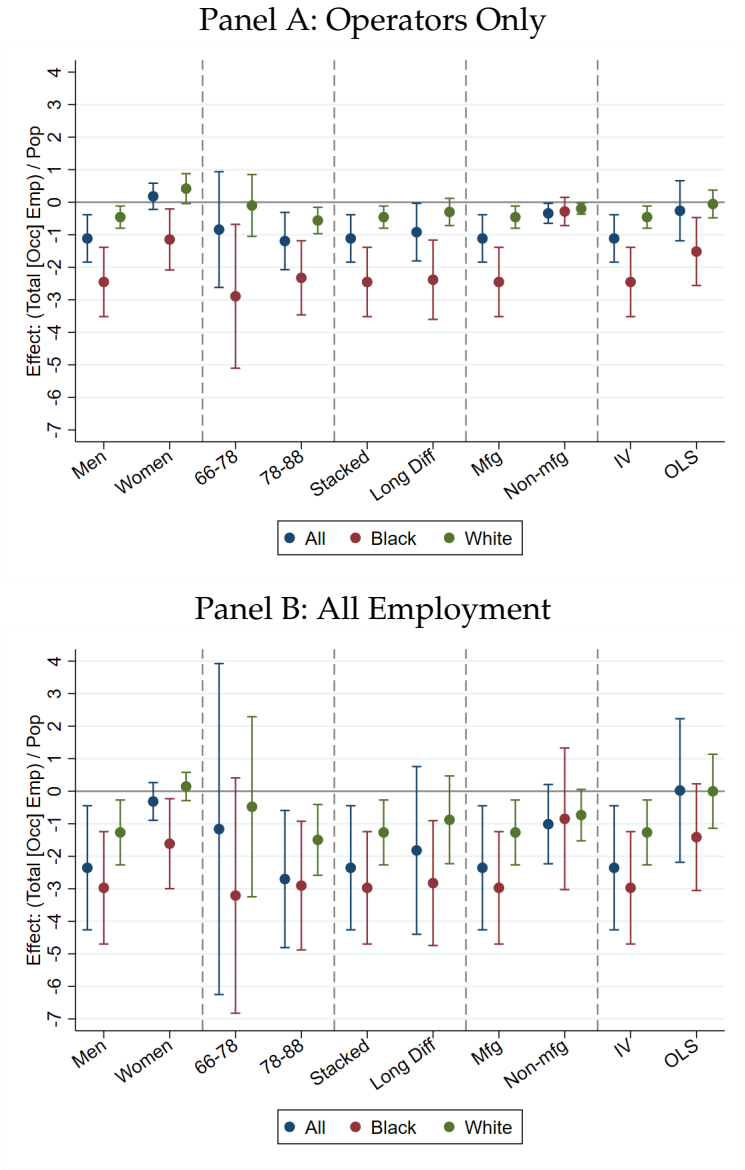


Figure 7: Employment Effects of Japan Shock – Robustness

Notes: This figure displays the coefficients from a regression of the change in manufacturing occupational employment, divided by initial population onto the Japan shock. These regressions are run in the EEOC data. The first set of regressions are run separately by gender; the second is the separately by period; the third is stacked vs long difference; the fourth is manufacturing versus non-manufacturing; the fifth is IV vs OLS. Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

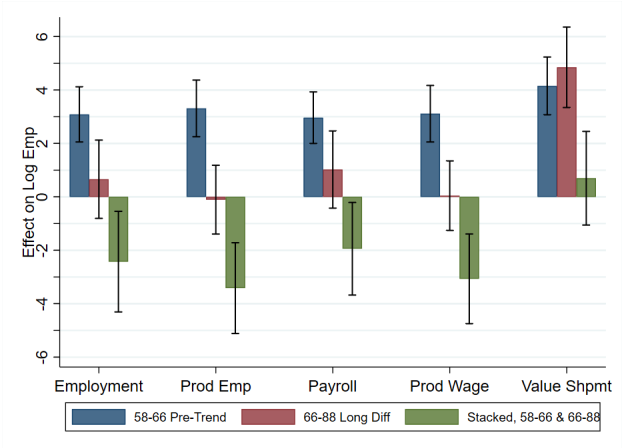


Figure 8: Industry-Level Effects of Japan Shock, Accounting for Pre-Trends

Notes: This figure displays the coefficients from a regression of the change in industry-level outcome (employment, payroll, etc.) onto the industry-level Japan shock. These regressions are run in the ASM data, over the time period 1966-1988 (for the long difference) and 1958-1966 (for the pre-trend specification). For the stacked specification, we define the shock/instrument to be zero in the pre-period, and include time period and industry fixed effects. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

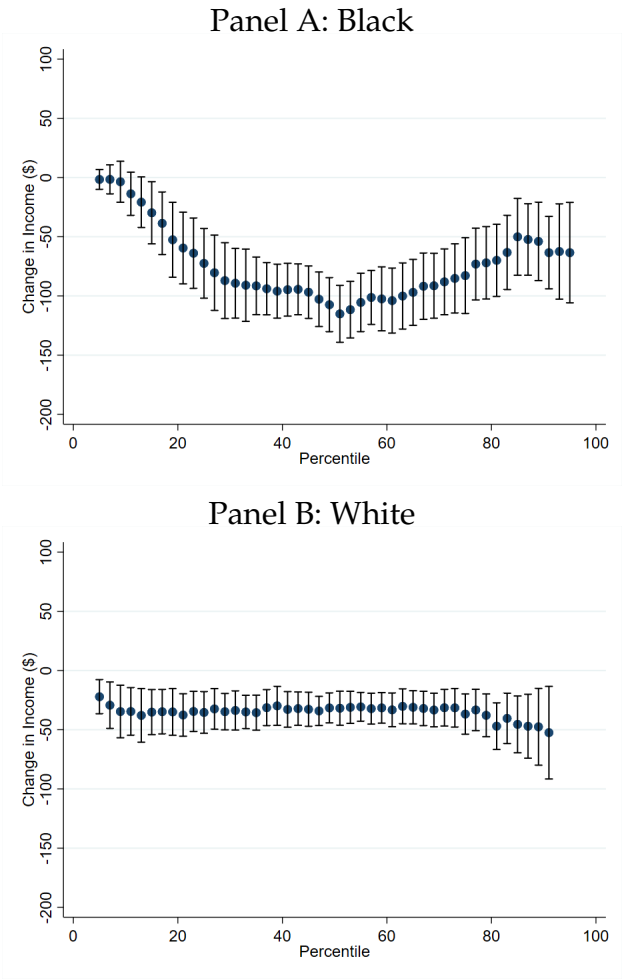


Figure 9: Distributional Effects of the Japan Shock, by Race (In Dollars)

Notes: This figure implements the Chetverikov, Larsen and Palmer (2016) unconditional quantile IV regression method to study the effects of the Japan shock on Black (Panel A) and white (Panel B) incomes. These regressions are run in the Census data; each panel is run over the time period 1970-90 (long difference). Regressions weighted by 1950 CZ employment. This figure includes men only. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ. Incomes are deflated to 1970 dollars.

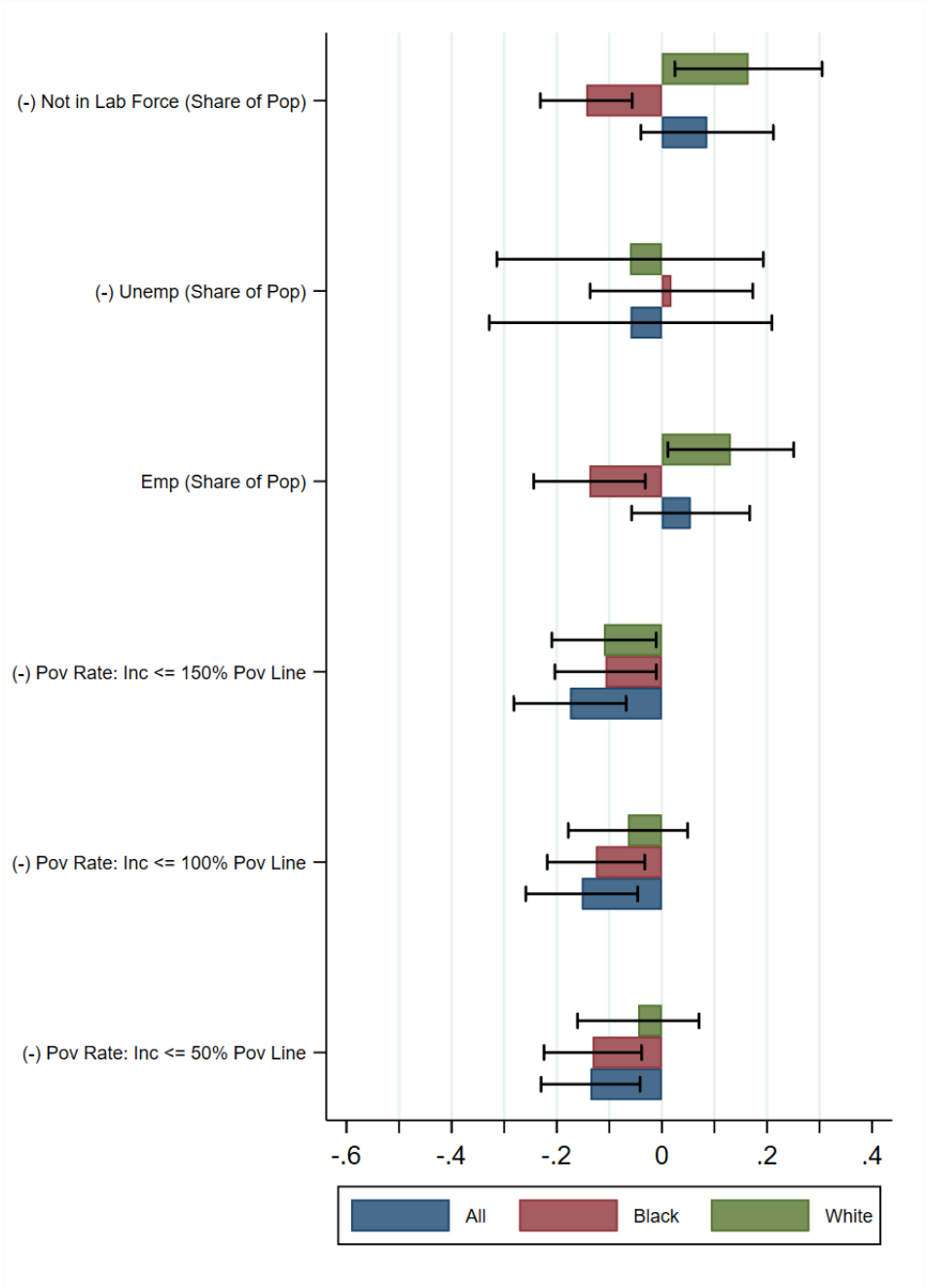


Figure 10: Persistent Effects of the Japan Shock on Joblessness and Poverty

Notes: This figure displays the coefficients from a regression of the change of the outcome variable onto the Japan shock. Each regression dependent variable is normalized by its standard deviation. These regressions are run in the Census data, over the time period 1970-2010 (long difference specification). Regressions weighted by 1950 CZ employment. This figure includes men only. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

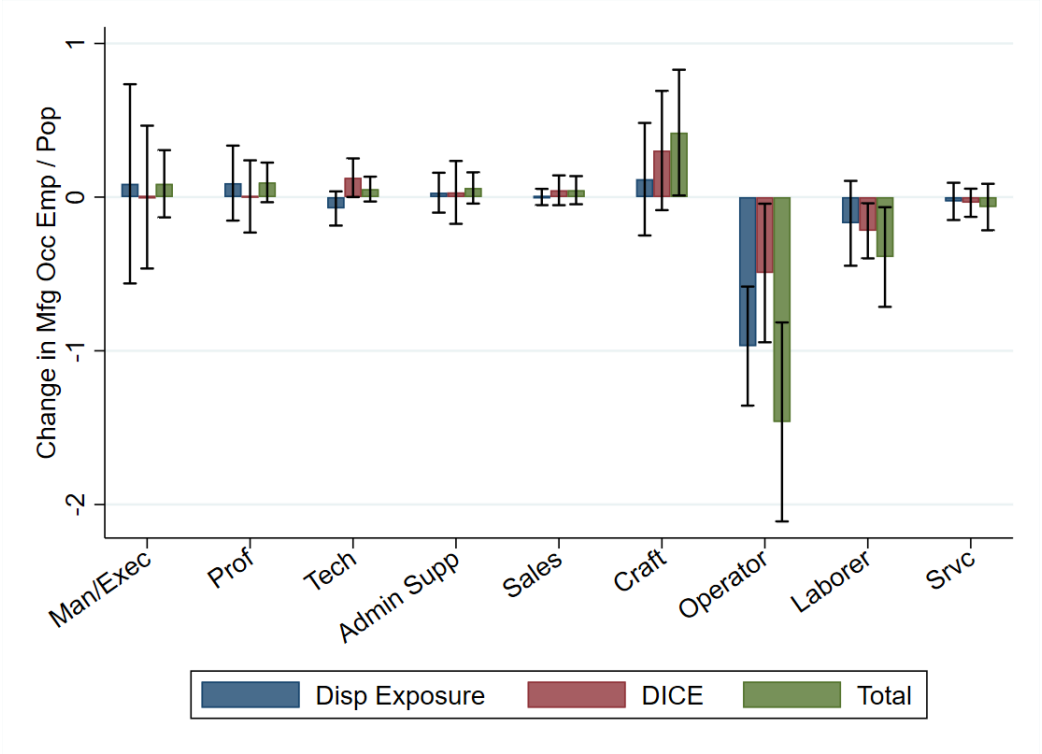


Figure 11: Disparate Exposure Drives Japan Shock Effects

Notes: This figure displays the coefficients from a regression of the terms from an Oaxaca decomposition of the Black-white difference in manufacturing occupational employment onto the Japan shock. These regressions are run in the EEOC data, over the time periods 1966-78 and 1978-88 (stacked). Specification detailed in the text. Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

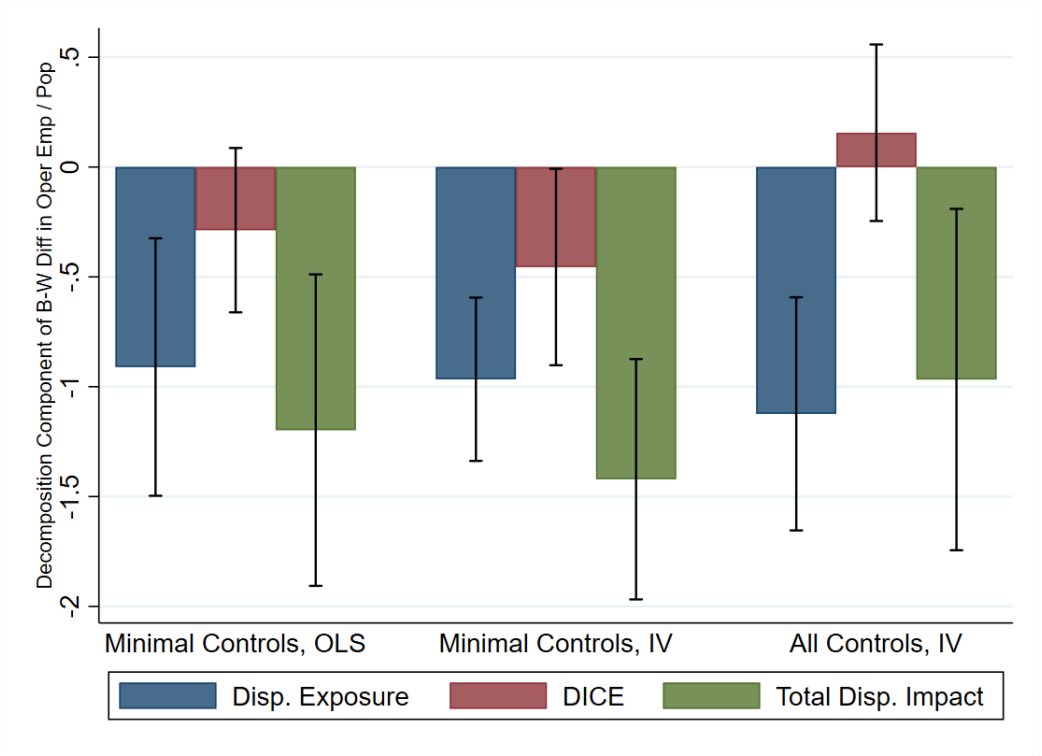


Figure 12: Disparate Exposure Drives Japan Shock Effects: Firm-Level

Notes: This figure displays the coefficients from a regression of the terms from an Oaxaca decomposition of the Black-white difference in manufacturing occupational employment onto the Japan shock. The sample is restricted to firms that existed at baseline, with at least one Black operator and one white operator. These regressions are run in the EEOC data, over the time periods 1966-78 and 1978-88 (stacked). Specification detailed in the text. Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

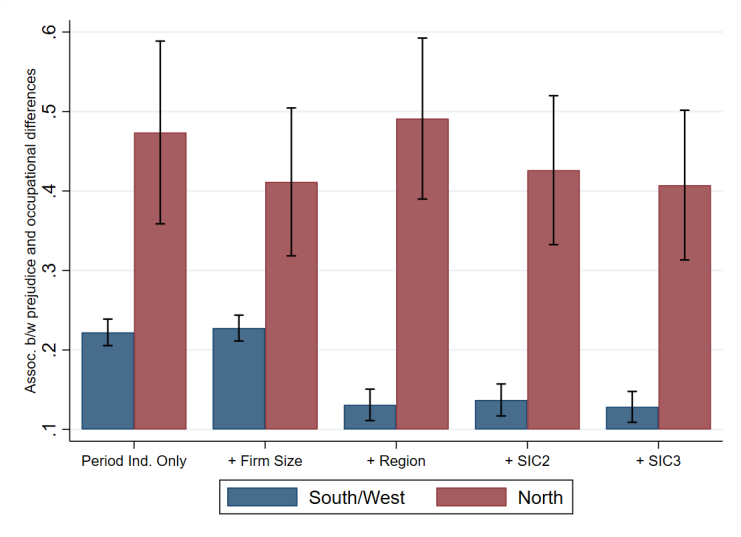


Figure 13: Association Between Racial Differences in Baseline Operator Employment Occupational Differences and Local Area Prejudice

Notes: This figure plots the results of a firm-level regression of the fraction of operators who are Black onto the county-level vote share for George Wallace in 1968. This is run separately for the North and the other regions. Both the occupational differences variable and the Wallace share are standardized. The firm size controls are indicator variables for firms of size 0-50, 50-100, 100-250, 250-500, 500-1000, and 1000+ employees. All regressions include surviving and exiting firms, and are weighted by initial firm employment.

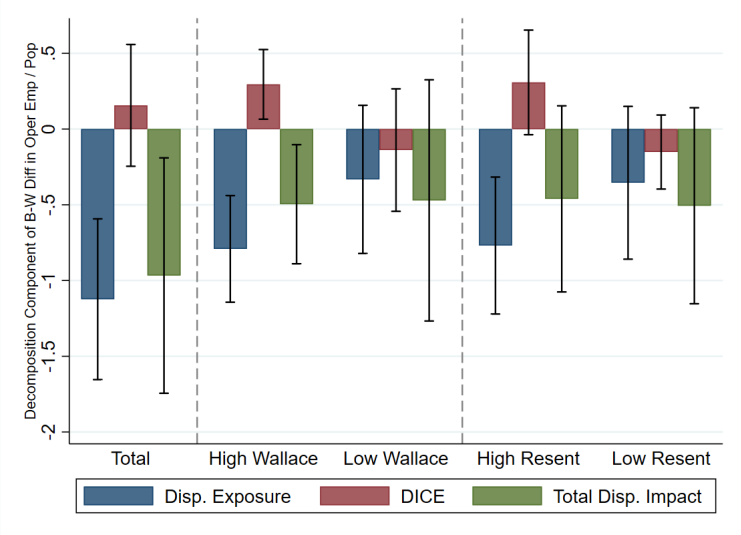


Figure 14: Association Between Local Area Prejudice and Disparate Exposure to the Japan Shock

Notes: This figure displays the coefficients from a regression of the terms from an Oaxaca decomposition of the Black-white difference in manufacturing occupational employment onto the Japan shock. The sample is restricted to firms that existed at baseline (survivors and exitors), with at least one Black operator and one white operator. The right four columns refer to firms in counties with higher Wallace votes (normalized total ≥ 0) or resentment scores (normalized total ≥ 0). These regressions are run in the EEOC data, over the time periods 1966-78 and 1978-88 (stacked). Specification detailed in the text. Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

Panel A: Japan Shock By Industry (4 Digit SIC)

Industry	Change in Absorption
Industrial Trucks, Tractors, Trailers, Stackers	1.806
Household Video and audio equipment	1.523
Electronic Capacitors	1.521
Electrical Equipment, NEC	0.784
Motorcycles, Bicycles, and Parts	0.769
Optical Instruments and Lenses	0.710
Musical Instruments	0.680
Photographic Equipment and Supplies	0.560
Radio Television Broadcasting and Communications equipment	0.539
Printed Circuit Boards	0.515
Computer Peripheral Equipment, NEC	0.473
Surgical and Medical Instruments	0.422
Products of Petroleum and Coal, NEC	0.371
Electrical Machinery, Equip, Supplies, NEC	0.329
Pottery Products, NEC	0.311
Scales and Balances, Except Laboratory	0.305
Medicinals and Botanicals	0.285

Panel B: Japan Shock By Sector (2 Digit SIC)

Sector	Change in Absorption
Electronics	0.231
Measuring Instr	0.134
Misc	0.075
Indstr/Comm Machinery	0.073
Transportation	0.033
Primary Metals	0.025
Fabricated Metals	0.020
Chemicals	0.016
Stone/Clay/Concrete/Glass	0.011
Petroleum Refining	0.006
Furniture	0.006
Paper	0.005
Apparel	0.005
Textile	0.004
Printing	0.002
Rubber, Misc Plastic	-0.001
Food	0.000
Lumber/Wood	-0.011
Leather	-0.011

Table 1: Japanese Trade Exposure by Industry (by Industry Absorption)

Notes: This table displays the change in industry absorption, from 1966 to 1988. Industry absorption is defined as change in US imports from Japan, divided by the initial value of industry shipments plus total imports minus total exports. All variables are deflated to 1970 dollars. Panel A shows Japan Shock exposure by four-digit SIC industry; Panel B shows Japan Shock exposure by two-digit SIC industry.

	EEOC 1971	EEOC 1989	Census 1970	Census 1990
Race				
Black	0.096	0.127	0.078	0.075
White	0.860	0.777	0.887	0.829
Hispanic	0.035	0.065	0.016	0.039
Native	0.003	0.004	0.005	0.010
Asian	0.006	0.026	0.005	0.023
Gender				
Men	0.654	0.542	0.641	0.553
Women	0.346	0.458	0.359	0.447
Occupation				
Op/Lab	0.323	0.231	0.164	0.112
Not Op/Lab	0.677	0.769	0.836	0.888
Sector				
Mfg	0.497	0.350	0.208	0.176
Non-Mfg	0.503	0.650	0.792	0.824
Region				
Midwest	0.266	0.265	0.274	0.297
Northeast	0.298	0.222	0.126	0.135
South	0.278	0.326	0.333	0.337
West	0.158	0.182	0.162	0.231

Table 2: Distribution of Workforce across Demographic/Economic Characteristics: EEOC vs. Census Data

Notes: This table displays summary statistics, for both the EEOC (EEO-1) and Census datasets. The Census numbers are calculated on the sample of workers who are employed.

A Data appendix

A.1 Mapping Products to Industries

We follow Autor, Dorn and Hanson (2013); Batistich and Bond (2023) in mapping product-level trade data to 4-digit SIC industries. We utilize data from the UN Comtrade database. We download product-level data at the SITC Revision 1 level, for each year of study. As in Batistich and Bond (2023), We then use product-level data between 1992-1995 to calculate a crosswalk between SITC Revision 1 codes and Harmonized System (HS) codes, using Japan-specific wieghts to construt the crosswalk. This allows us to map the product-level data from the 1960s-1980s, which predates HS codes, to HS codes. Then, we use a crosswalk from Autor, Dorn and Hanson (2013) to map HS-level trade data into industries.

A.2 Construction of General Social Survey Index

We take measures from the 1972 and 1973 waves of the General Social Survey, and project them to counties, to create a county-level measure of prejudice. We use the answers to the following questions related to racial resentment:

- Do you think there should be laws against marriages between (Negroes/Blacks/African-Americans) and whites?
- How strongly would you object if a member of your family wanted to bring a (Negro/Black) friend home to dinner? Would you object strongly, mildly, or not at all?
- Agree/disagree: (Negroes/Blacks/African-Americans) shouldn't push themselves where they're not wanted.
- Agree/disagree: White people have a right to keep (Negroes/Blacks/African-Americans) out of their neighborhoods if they want to, and (Negroes/Blacks/African-Americans) should respect that right
- If your party nominated a (Negro/Black/African-American) for President, would you vote for him if he were qualified for the job?
- Has the government spent too much/not enough money on (Negroes/Blacks)?

The survey also asks voters about whether they voted for Wallace in the 1968 election. We average the resentment scores across white individuals, for self-identified Wallace voters

and not, by region. For four of the six questions, the question was only asked to white individuals. We then use county-level variation in the mix of Wallace voters, plus county-level variation in the racial composition of the county, to construct a county-level GSS measure.

B Empirical Appendix

B.1 Bayer-Charles (2018) Decomposition

This section of the paper discusses whether occupational changes, which were important for understanding racially disparate employment effects, are also important for understanding racially disparate income effects. I implement a decomposition from Bayer and Charles (2018). This method decomposes the gap in median earnings into the amount due to the Black and White income distributions diverging ("positional divergence") versus the amount due to Black and White people being in different places in the overall income distribution at baseline ("distributional divergence"). For a quantile q and a race-specific income CDF f_t^r, the decomposition is:

$$(f_{t+1}^b(q) - f_t^b(q)) - (f_{t+1}^w(q) - f_t^w(q)) = \underbrace{(f_{t+1}^b(q) - f_{t+1}^w(f_t^{w,-1}(f_t^b(q)))) - (f_{t+1}^w(q)) - f_t^w(q))}_{\text{distributional divergence}} + \underbrace{(f_{t+1}^w(f_t^{w,-1}(f_t^b(q))) - f_t^b(q))}_{\text{positional divergence}} \quad (1)$$

In words, the first term of the decomposition holds fixed the baseline relationship between Black and White income percentiles, allowing the Black and White distribution to shift over time; the second term of the decomposition holds fixed the Black and White distributions, allowing the mapping between the Black and White income percentiles (i.e. the relative position of Black and White people) to shift over time. I perform this decomposition for each commuting zone, and regress the results onto the shock.

Figure 15 shows the decomposition for selected percentiles. We see that the majority of the decomposition is due to positional divergence - not a function of Black workers being located differently in the baseline income distribution. This is a point of uniqueness for the Japan shock - other macroeconomic shocks that have widened inequality since the 1980s are generally thought to do so through distributional divergence. That the effects

of the Japan shock work shift the Black and White distributions differently is a function of the Japan shock disparately affecting the demand for the type of work in which Black workers were overrepresented.

This conclusion is reaffirmed in Figure 16, which plots the baseline operator fraction of employment by race, within each education level and income level. For both college and noncolleg workers, and across income levels, we see that Black workers were overrepresented in production operator employment. This suggests that the overrepresentation of Black workers in operator employment is not solely a function of Black workers being differentially positioned in the baseline aggregate income distribution.

B.2 Pre-trend test

This section probes the identification of the shift-share design. (Borusyak, Hull and Jaravel, 2021). As discussed in the text, (Borusyak, Hull and Jaravel, 2021) provides a method for reweighting the commuting zone-level regression to a numerically equivalent industry-level regression; the identification for the CZ-level design thus lays in variation across industries in the (reweighted) shock. There are two assumptions under which the design is valid. First is that the shocks are as good as randomly assigned, conditional on controls. Second is that there are many uncorrelated shocks.

First, we study the relationship between the Japan shock and pre-trends. The first exercise implements a pre-trend test recommended by (Borusyak, Hull and Jaravel, 2021). This test regresses the industry-level covariates onto the industry-level shock, leveraging the insight that the exogenous variation that identifies this design occurs at the industry-level. We take several variables from the Annual Survey of Manufacturers, and regress their log-changes from 1966-1988 onto the shock. Figure 17 shows a positive relationship between the shock and all of the industry-level variables of interest, including employment, value of shipments, capital, payroll, and value added. This figure shows that the Japan shock hit industries that were growing.

The second exercise looks at the relationship between the Japan shock and the trends in commuting zone level employment. This test regresses the 1950-1970 changes in employment onto the Japan shock. Figure 18 displays the results. For the manufacturing employment trends, the pre-trends are insignificant at the 5% level: however, the pre-trend coefficients for all workers, Black workers, and white workers are all positive and significant at the 10% level. This suggests a weak association between the pre-trend and the Japan shock at the commuting zone level. Note this relationship is weaker in part due

to the lower resolution of small Census sample.

Taken together, these two exercises show that the shock hit industries that were growing. As discussed in the text, we therefore need a stronger assumption for identification: the shock must be uncorrelated with contemporaneous changes in the trend.

Next, we probe the assumption that there are many uncorrelated shocks. This exercise follows Batistich and Bond (2023). We estimate a hierarchical random effects model, regressing the residual from the industry-level first-stage regression onto indicators for 2-digit SIC and 3-digit SIC industries. Table 3 reports the results. The intraclass correlations (for both 2-digit SIC and 3-digit SIC) are insignificant, which corrobrates that the many uncorrelated shocks assumption is appropriate in this setting.

B.3 First Stage

Table 4 reports the F-statistic for our main regressions. The area-level first stage is calculated using the procedure using Borusyak, Hull and Jaravel (2021). This procedure reweights the commuting zone-level regression to a numerically equivalent industry specification, and regresses the reweighted endogenous variable onto the industry-level instrument in the first stage. The table reports the first stage for the area level (using the reweighing procedure above) and the industry level (which uses an industry-level endogenous instrument and the instrument). The F-statistic is approximately 61 at the industry-level, and approximately 35 at the CZ-level. This table indicates that the first stage is strong.

Figure 19 displays the first-stage relationship between United States import absorption and import absorption by comparison countries over the 1966-1988 time period. Each marker in the figure is a 4-digit SIC industry, and the size of the marker indicates the baseline industry employment size. This figure shows there is a strong positive relationship between both variables, visually reinforcing the conclusion from Table 4: the first stage is strong.

B.4 Ruling out firm exit in driving disparate exposure

In this section, we test whether firm exit drives the observed result that disparate occupational exposure drives the disparate impact of the Japan shock on Black workers. While we only see large firms in our dataset, we proxy for firm exit – we define a firm as an exitor if we see them at baseline, but not at the end of our long difference period.

All other firms present at baseline are designated as survivors. Because all firm-level decompositions in this section use firm-level variation in disparate exposure, we restrict our attention to firms that existed at baseline; entering firms are not present in the decomposition. We construct decomposition terms analogous to the decompositiosn in the text, and use a similar regression strategy. In unreported results, we find that, in Japan-exposed CZs, firms were likelier to close. Moreover, in Japan-affected CZs, conditional on firm size, firms that were more operator-intensive were likelier to close. Figure 20 reports the contribution of firm exit to the aggregate employment effects estimated. This figure suggests that exitors contributed next to nothing to the aggregate changes in employment, nor racially disparate impacts, nor disparate exposure. All of the estimated effects are driven by surviving firms only.

B.5 Ruling out job suburbanization driving disparate exposure

In this section, we rule out job suburbanization shaping the disparate exposure that drive the racially disparate impact of the Japan trade shock on Black workers. Job suburbanization is a crucial component of the spatial mismatch hypothesis: the idea that there is a mismatch between where Black workers live and suitable job opportunities for them (Kain, 1968; Wilson, 1978). In the Northern U.S., Black workers were overwhelmingly concentrated in central cities; the mid-twentieth century saw a large increase in job surburbanization for a number of reasons, including white flight and the construction of the interstate highway system (Miller, 2023; Weiwu, 2023). If the Japan shock accelerated the hallowing out of central cities by shifiting employment out to the suburbs, then the spatial mismatch hypothesis predicts this would disparately impact Black workers.

While we cannot directly test the spatial mismatch hypothesis, we test whether the Japan shock accelerated job suburbanization. We test this possibility using the decomposition analogous to the firm-level decomposition in the text. Using data from Weiwu (2023), for each of the 384 Metropolitan Statistical Areas, I identify the county containing the primary central business district. I then split the sample for firms in those 384 counties (”central counties”), versus firms in the other counties (”non-central counties”). As an example for how this works: in the Baltimore Metropolitan Statistical Area, Baltimore city, which is an independent district, is identified as the central county; all of the surrounding suburban counties, including Baltimore, Anne Arundel, Howard, etc. are all included in the non-central county category. I regress each component of the decomposition onto the shock, and recover the coefficients. I use a stacked specification detailed in the main text.

Figure 20 displays the results. We find that the Japan shock increased (relative) job suburbanization, in the sense that it decreased the fraction of manufacuring jobs in central counties. Despite the fact that approximately one-third of manufacturing jobs were in non-central counties, almost all of the racially disparate effects I estimate are in central counties only. We find no evidence that jobs shifted away from center cities, away from where Black workers could access it. Thus, the shock should be primarily understood as affecting employment opportunities in central counties, with no effect on employment opportunities in the suburbs. [1]

B.6 Association between firm size and disparate exposure

In this section, we show an association between the size of firms and the disparate exposure that drives the racially disparate effect of the Japan shock on Black workers. Larger firms were more racially diverse than their smaller counterparts – a fact that remains true to the present (Miller and Schmutte, 2023). Yet, in unreported results, I find the racial composition in smaller vs larger manufacturing appears driven by the occupational mix: larger firms have more production operators, and fraction of operators that are Black does not vary with firm size. Thus, if firms with more operators are likeliest to be more affected, then larger firms may be more affected – a hypothesis I explore below.

Figure 21 presents the results for larger and smaller firms. I split the sample in half by firms with higher vs lower number of employees. Because my data covers primarily large firms, the critical value on which I cut is just over 1000 employees. Almost all of the disparate exposure occured at larger firms; this is due to almost all of the disparate impact occuring at larger firms. While not definitive, this result suggests that the racial diversity may increase the disparate impact (i.e. The converse implies that small firms, which have fewer Black workers, will contribute less to the total displacement than larger firms. This is sensible, given that larger firms are more racially diverse than smaller firms.)

B.7 Baseline education and occupation disparities

This section explores the extent to which Black workers' observed lower education levels at baseline explain the high baseline level of operator employment among Black workers.

[1] This result halfway confirms what Batistich and Bond (2023) find. They find no increased job suburbanization and no increased spatial mismatch. We have sufficient power to detect (relative) suburbanization, but we affirm their conclusion that there is comparatively little racially disparate increase in employment outside of central counties.

We perform an Oaxaca decomposition, at the national level. Noting that the average likelihood of working in operator employment conditional on education can be recovered from race-specific individual-level regressions of operator employment on indicators for educational attainment:

$$o_i = \alpha_{r(i)} + \beta_{r(i)} e_i + \epsilon_i \tag{2}$$

one can then easily apply the canonical Oaxaca decomposition. (i.e. isomorphic the canonical gender wage gap example, where here occupation = wage). This is a national level decomposition - I do not project this onto the CZ shock.

Figure 22 reports the results. I decompose the Black-White difference in operator/laborer share into difference due to racial disparities in education levels (+ industry or region, depending on the specification) and difference due to within-education (+ industry/region) differences in likelihood of working as an operator. Across specifications, approximately 3/5-2/3 of the difference in operator share is due to the within-education level difference. This both suggests that education disparities are important for explaining differences in occupational choices across races, and that the majority is left unexplained by educational disparities. Note, however, that mismeasurement in education levels will load onto the unexplained component of the decomposition.

B.8 Relationship between Wallace share and racial resentment

Figure 23 presents the relationship between the racial resentment-related measures and self-reported Wallace support in the 1972-73 General Social Survey. Wallace voters were significantly more likely to express views consistent with resentment, such as expressing an objection to hoting a Black person in their home for dinner.

B.9 Relationship between Wallace share and racial occupational differences

Figure 24 presents the CZ-level relationship between the Wallace share and educational attainment/occupational difference. Panel A shows a negative association between Wallace support and the Black share of workers that were managers and professionals; Panel B shows a negative association between Wallace support and the Black share of workers that graduated from high school. Both panels support the finding in the text that prejudice is positively associated with baseline occupational differences; places with more anti-Black prejudice had lower educational attainment and lower advancement into managerial positions. This suggests a reason why anti-Black prejudice may relate to disparate

occupational exposure.

B.10 Industry-Level Analysis

Following Acemoglu et al. (2016), we run an industry-level specification. This strategy regresses industry-level employment changes onto the endogenous change in United States import penetration, instrumenting with the change in import penetration to comparison countries. Note that the identification for the main strategy in the paper (the shift-share instrumental variables strategy) relies on the exogenous shocks assumption of **?**. Therefore, we can use the industry-level instrument to study changes in outcomes at the industry-level, since the industry shocks are exogenous.

The estimating equations are:

$$\Delta y_{i,t} = \alpha_1 + \beta_1 \Delta IP_{i,t}^{US} + \delta_1 X_i + \epsilon_{1,i,t} \tag{3}$$

$$\Delta IP_{i,t}^{US} = \alpha_2 + \beta_2 \Delta IP_{i,t}^{OC} + \delta_2 X_i + \epsilon_{2,i,t} \tag{4}$$

where $\Delta y_{i,t}$ is the change in outcome for industry i in time period t, $\Delta IP_{i,t}^{US}$ is the change in import penetration to the United States, and $\Delta IP_{i,t}^{OC}$ is the change in import penetration to the comparison countries. We weight by initial employment and cluster standard errors at the two-digit Standard Industry Classification (SIC) level.

We first study the proportional changes in employment induced by the Japan shock. We run a series of industry-level regressions, where the dependent variable is the change in log employment by race and occupation. We run 30 separate regressions. Figure 25 reports the results. This figure shows a decrease in log employment for operator employment, overall and by race. On average, the Japan shock reduced the rate of employment growth by 0.05 log points, or 5 percent. This reduction is not racially disparate. The estimated effects on log total employment are noisy. The Japan shock reduced employment growth for Black workers, but there is no significant reduction overall or for whites.

Importantly, the proportional reduction in Black operator employment is not statistically difference from the reduction in employment overall, or in six of the remaining eight other occupation categories. Thus, the disproportionate change in Black operator employment is due to the initial concentration of Black workers as operators, rather differences in the proportional employment reductions by occupation.

That conclusion is reaffirmed in Figure 26, which presents regressions of the occupation shares within each race onto the shock. The reduction in the operator share of

employment among Black workers is significantly greater than the changes for other occupations.

B.11 Robustness of Main Result

This section expands on the robustness of the main estimated effects of the Japan shock on employment. Figures 27-31 show different variants of the main shift-share regression of employment onto the Japan shock. Figure 27 shows effects separately by gender; Figure 28 shows effects separately by period; Figure 29 shows effects estimated in a long difference vs stacked specificaiton; Figure 30 shows effects for manufacturing vs non-manufacturing; Figure 31 shows results estimated by OLS vs IV. While the figures in the main text only include operator and overall employment, these figures also include separate regressions for the remaining eight occupational categories (managers, professionals, technicians, administrative support, sales, craft, laborers, service workers). There is little effect of the Japan shock on any of these eight occupations, for any of the five robustness checks that we run. This reaffirms the conclusion that the Japan shock disparately affected operator employment – a conclusion that is robust across subgroups and specifications.

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	ICC	SE
3 digit SIC	0.012	0.009
2 digit SIC	0.011	0.024

Table 3: Testing the Many Uncorrelated Shocks Assumption

This table reports the results of a hierarchical random effects model. This estimation procedure regresses the (residualized) shock onto random coefficients for 2- and 3-digit SIC industry indicators. Intraclass correlations (ICC) and standard errors (SE) are reported. The model is weighted by 3-digit SIC codes.

	(1)	(2)
Shock	3.01	2.45
	(0.21)	(0.47)
F-statistic	61	35.7
Level	industry	CZ

Table 4: First Stage

This table reports the first stage coefficients at the industry (4-digit SIC) and commuting zone levels. The commuting zone regression is reweighted using the procedure from Borusyak, Hull and Jaravel (2021). F-statistics are reported for both the industry design and commuting zone design.

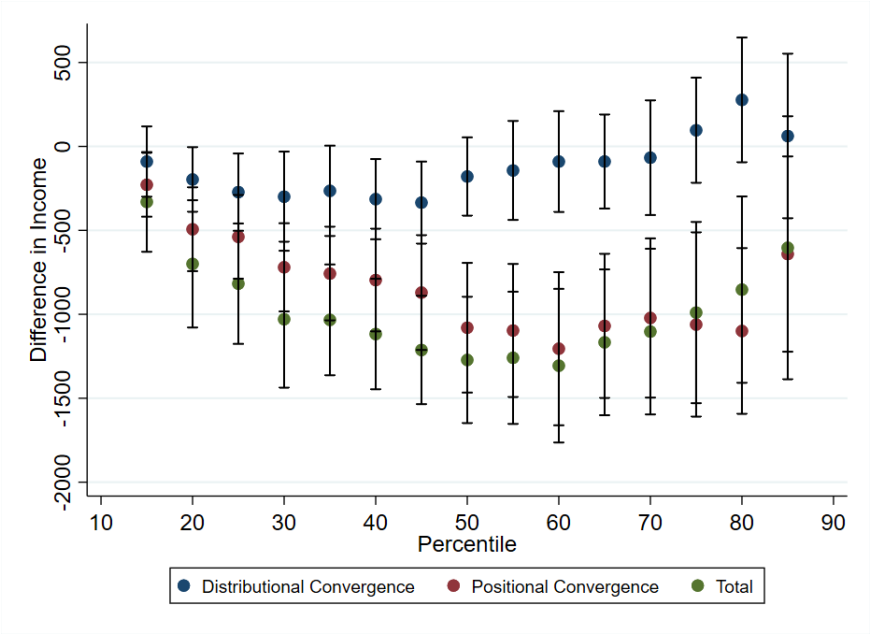


Figure 15: Bayer-Charles Decomposition of Income Effects of Japan Shock

Notes: This figure displays the coefficients from a regression of the terms of the Bayer and Charles (2019) decomposition onto the Japan shock. These regressions are run in the Census data; the time period is run over the time period 1970-90 (long difference). Regressions weighted by 1950 CZ employment. This figure includes men only.

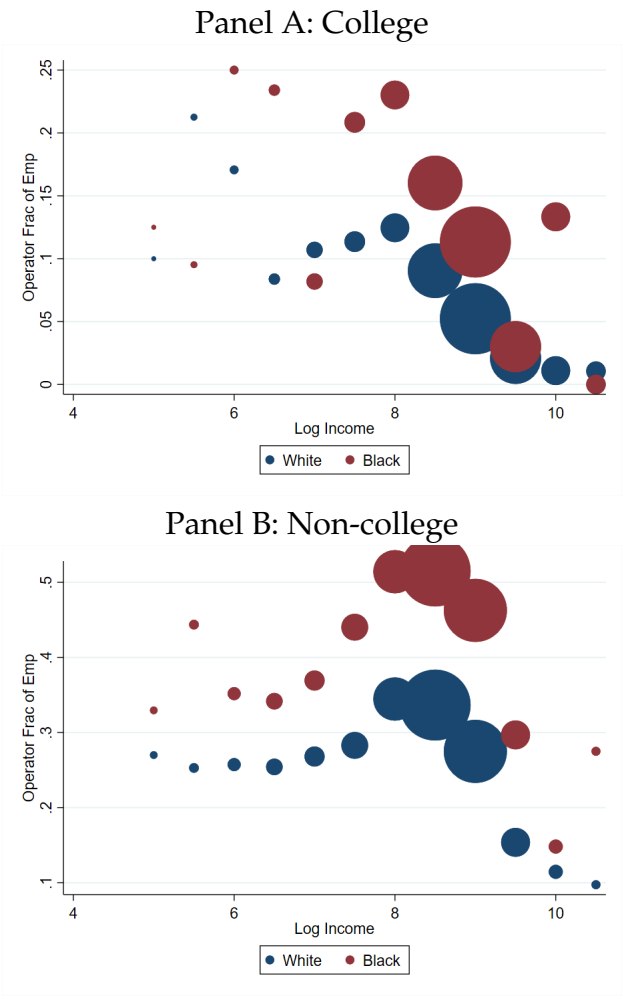


Figure 16: Black Workers Likelier to Work as Operators, Within Income Level

Notes: This figure displays the average likelihood of working as a production operator/laborer, by race and income level, for college workers (Panel A) and non-college workers (Panel B) in 1970. This figure includes men only.

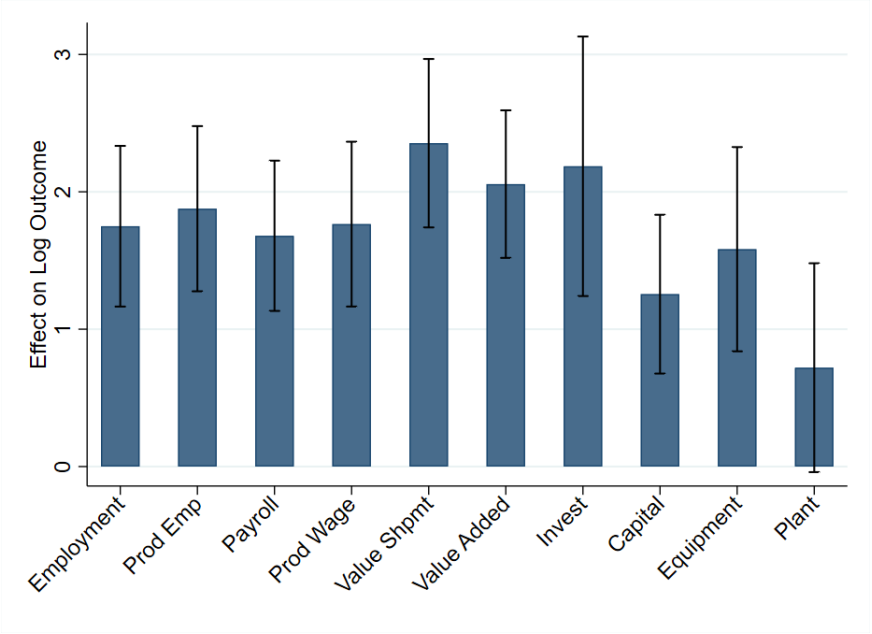


Figure 17: Pre-trends

Notes: This figure displays the industry-level relationship between the Japan shock and industry covariates. Covariates are log-changes measured from 1958-1966. All regressions weighted by initial employment. This figure is run in the Annual Survey of Manufacturers data.

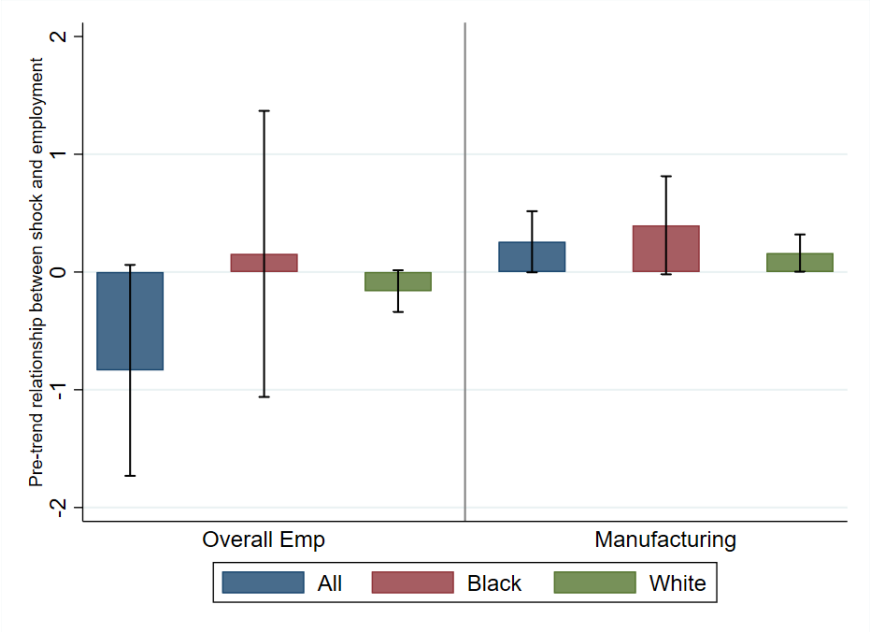


Figure 18: Pre-trends

Note: This figure displays the industry-level relationship between the Japan shock and CZ employment. The dependent variable is the 1950-1970 change in employment, normalized by initial population. Regressions weighted by 1950 employment. This figure restricts to men only.

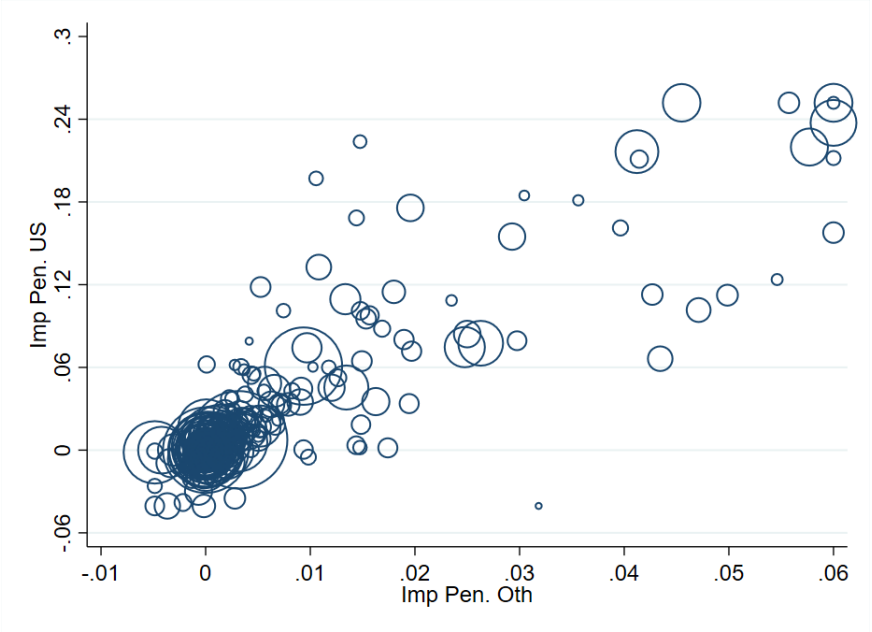


Figure 19: First Stage Relationship Between Imports to US and Imports to Comparison Countries

Notes: This figure shows the first-stage relationship between US industry absorption and industry absorption by comparison countries. Each data point represents a 4-digit industry; the size of the marker reflects the 1966 employment in the industry.

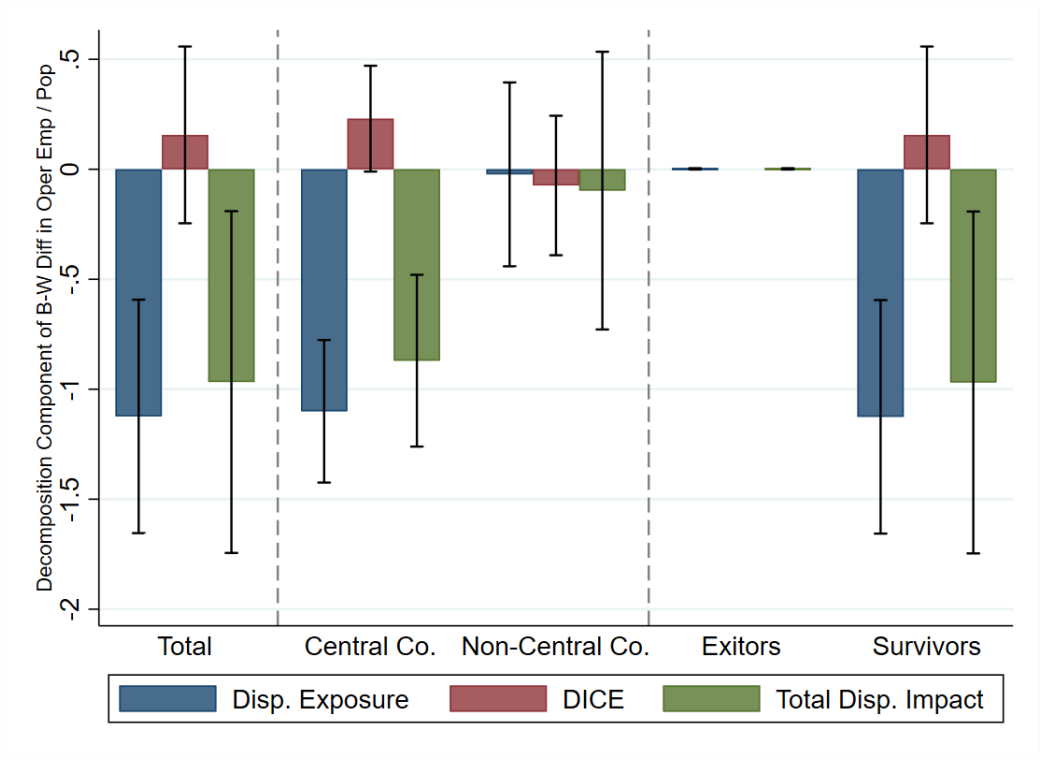


Figure 20: Disparate Exposure Drives Japan Shock Effects: Firm-Level

Notes: This figure displays the coefficients from a regression of the terms from an Oaxaca decomposition of the Black-White difference in manufacturing occupational employment onto the Japan shock. The sample is restricted to firms that existed at baseline, with at least one Black operator and one White operator. The right four columns refer to firms in central counties (containing central CBD in a MSA) vs not, and firms that are exitors vs survivors. These regressions are run in the EEOC data, over the time period 1966-1978 and 1978-1988 (stacked). Specification detailed in the text. Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

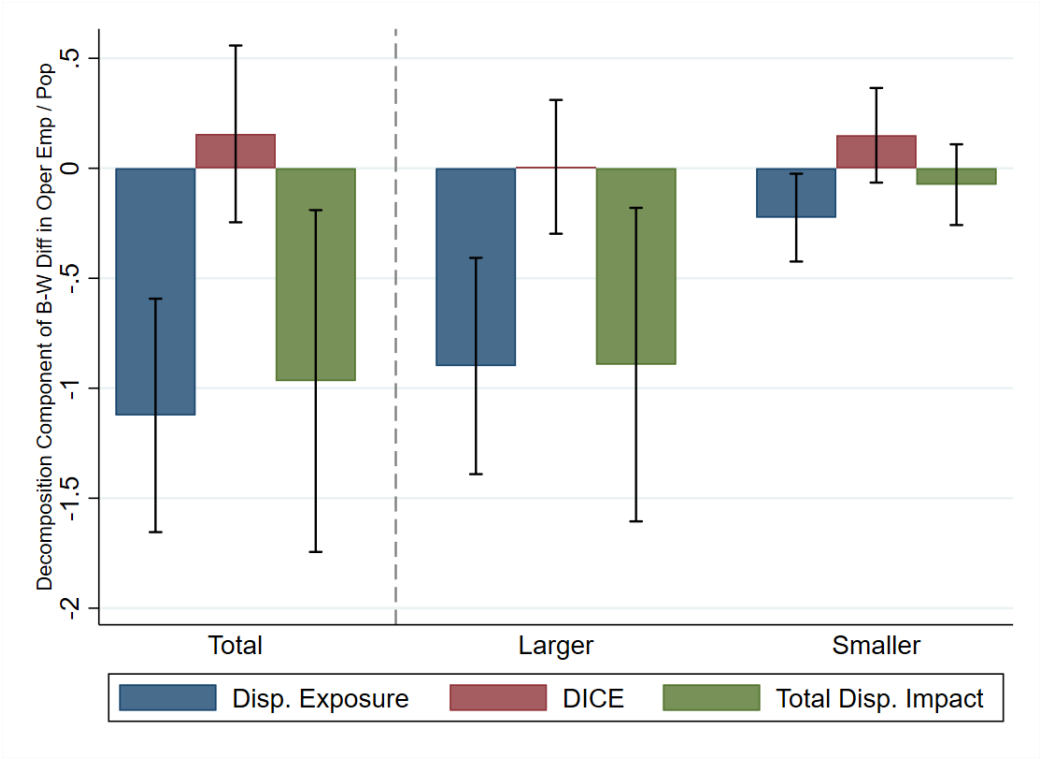


Figure 21: Disparate Exposure Drives Japan Shock Effects: Firm-Level

Notes: This figure displays the coefficients from a regression of the terms from an Oaxaca decomposition of the Black-White difference in manufacturing occupational employment onto the Japan shock. The sample is restricted to firms that existed at baseline, with at least one Black operator and one White operator. The right two columns refer to firms that are larger ($\geq$ 1000 employees) versus smaller. These regressions are run in the EEOC data, over the time period 1966-1978 and 1978-1988 (stacked). Specification detailed in the text. Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

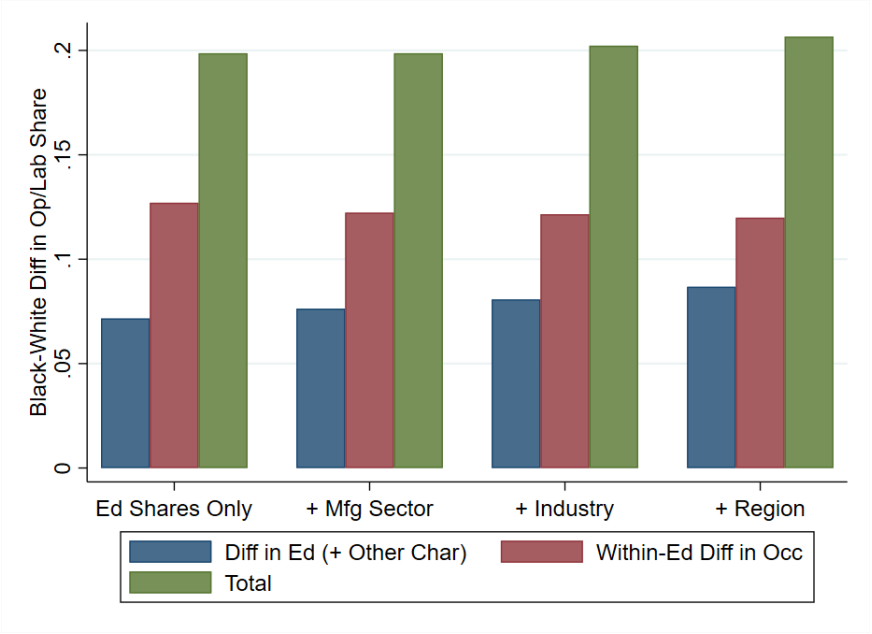


Figure 22: Role of Educational Disparities in Explaining Occupational Disparities

This figure describes the Oaxaca decomposition of Black-White differences in operator/laborer employment into differences in educational attainment (+ industry/region) and differences in likelihood of being an operator conditional on education level (+ indsutry/region). The included categories are indicated along the x-axis. Totals are slightly different across categories due to rounding. This is a nationwide decomposition; there is no regression component. Sample includes men only.

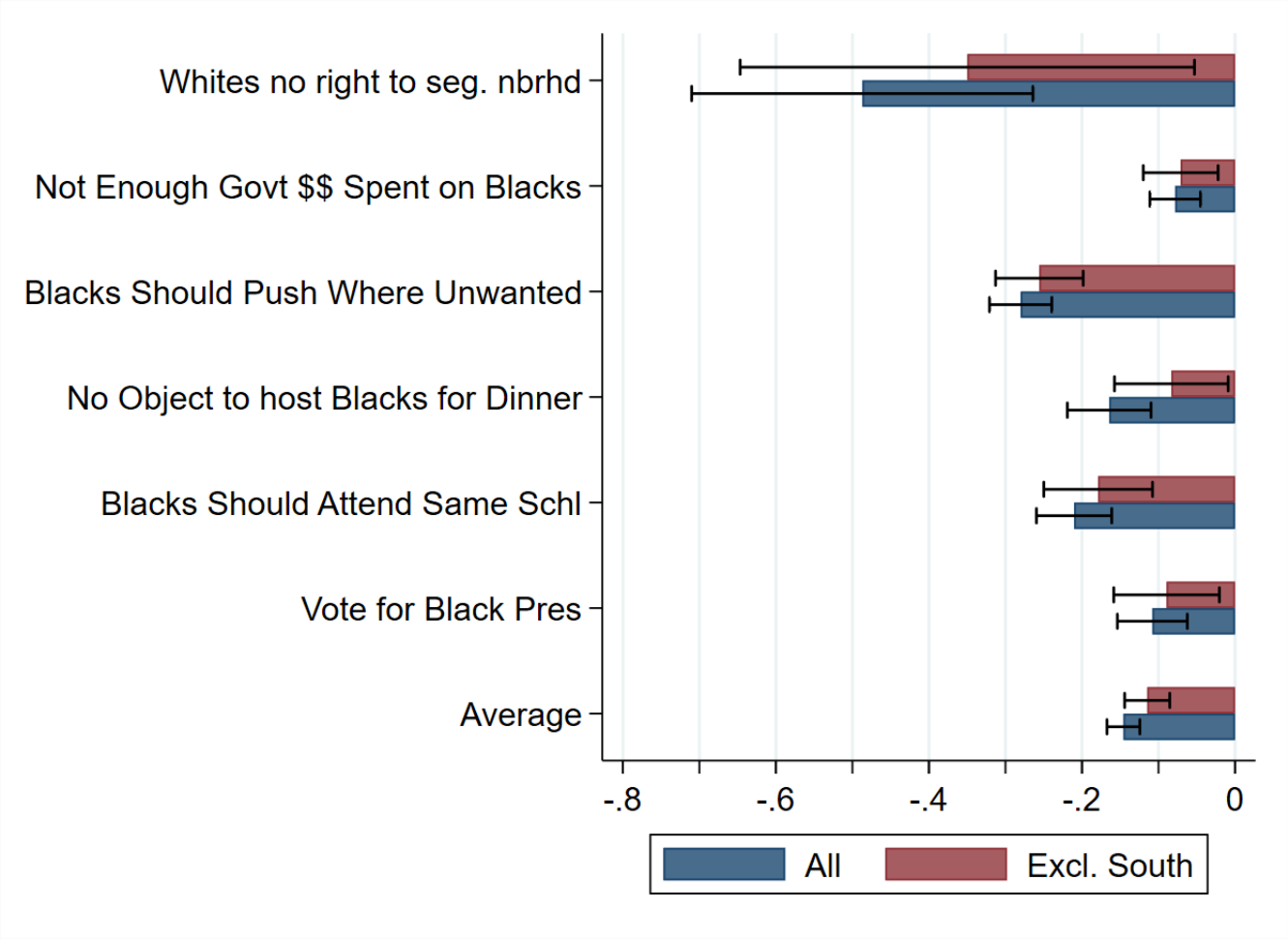
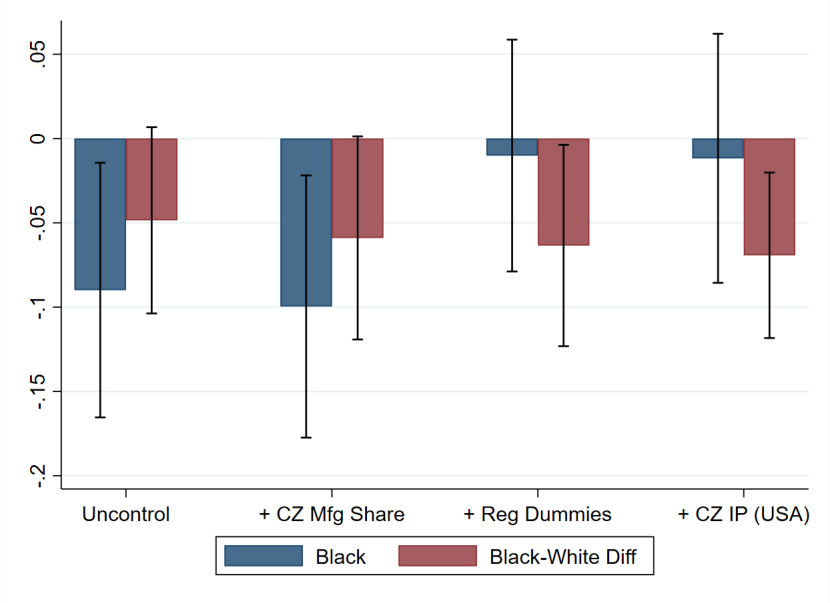


Figure 23: Racial Resentment Predicts Wallace Vote

Notes: This figure describes the relationship between survey responses to race-related questions and self-reported support for George Wallace. This data source for this figure is the 1972-1973 waves of the General Social Survey. Regressions weighted by GSS weights.

Panel A: Manager/Professional Share



Panel B: High School+ Share

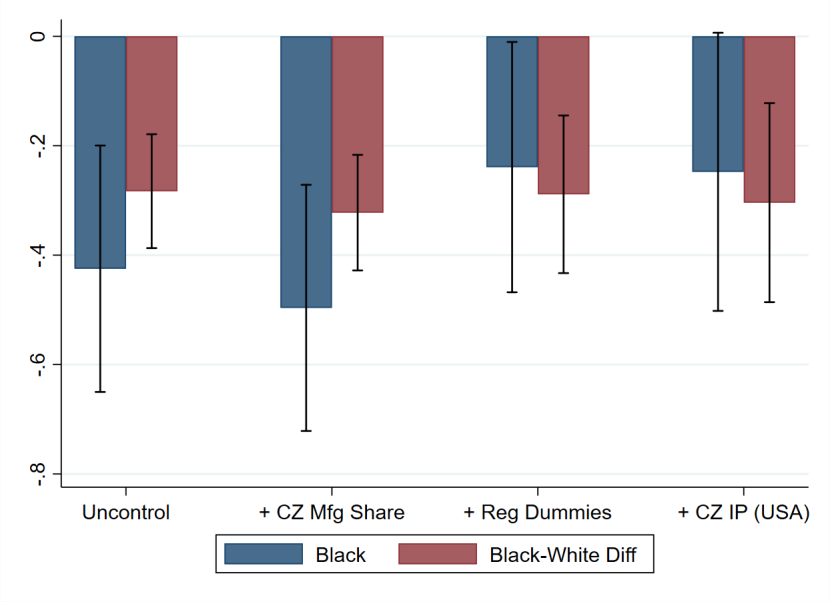


Figure 24: Wallace Vote Predicts Educational Attainment/Occupational Difference

Notes: This figure describes the CZ-level relationship between the Wallace share and occupational differences/educational attainment. The outcome in Panel A is the manager/professional share; the outcome in PAnel B is the share with at least a high school degree. The blue series regresses educational attainment/occupational shares for Blacks only; the red series is Black-white differences. Controls sequentially added as indicated. Regressions weighted by baseline employment, and robust standard errors used.

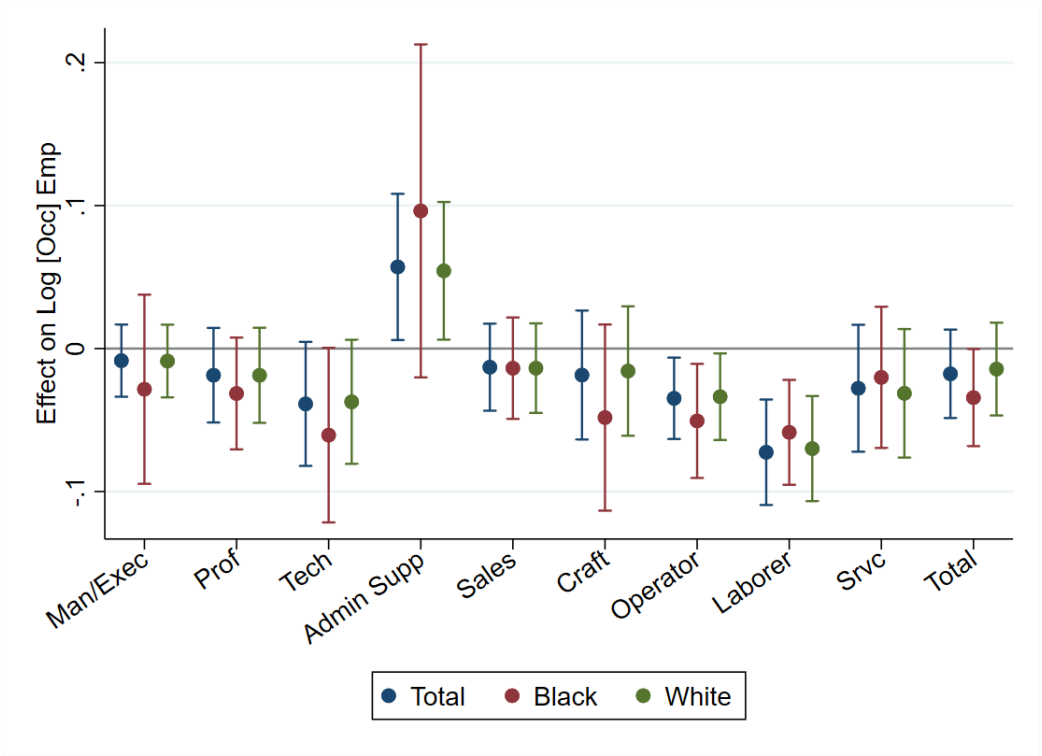


Figure 25: Employment Effects of the Japan Shock – Industry Level

Notes: This figure displays the coefficients from a regression of the change in log manufacturing occupational employment. These regressions are run in the EEOC data, over the time period 1966-1988 (long difference specification). Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

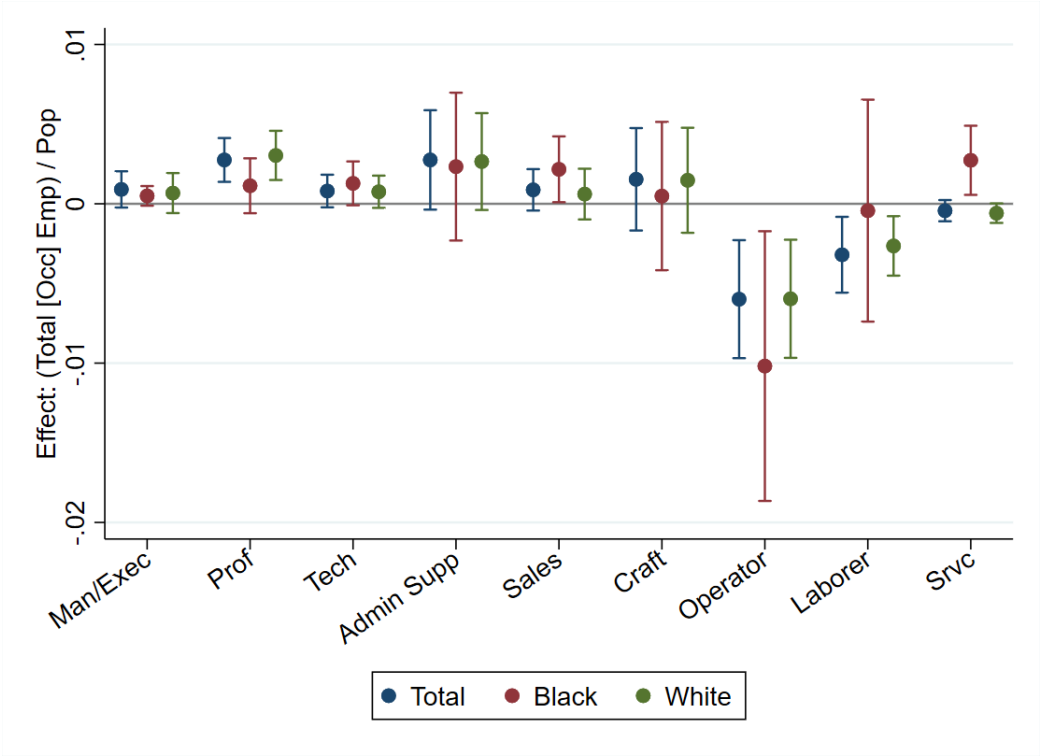


Figure 26: Employment Effects of the Japan Shock – Industry Level

Notes: This figure displays the coefficients from a regression of the change in the share among each racial group in the given occupation. These regressions are run in the EEOC data, over the time period 1966-1988 (long difference specification). Regressions weighted by 1950 CZ employment. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

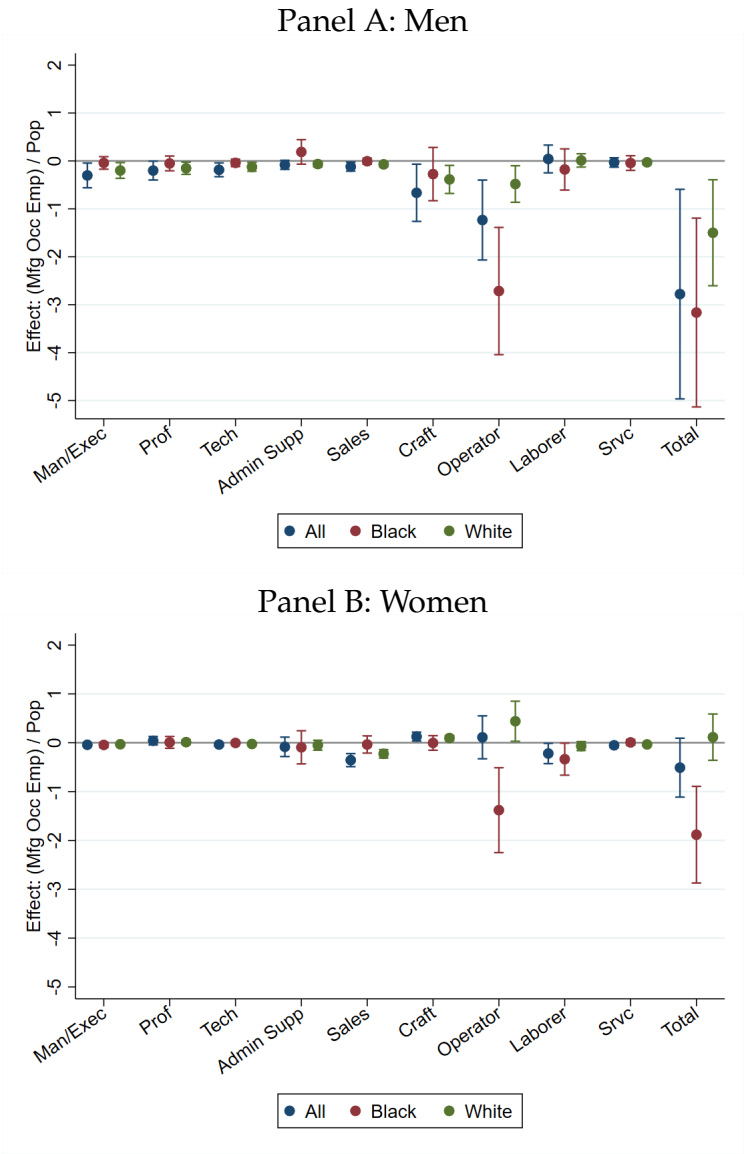


Figure 27: Employment Effects of Japan Shock, by Gender

Notes: This figure displays the coefficients from a regression of the change in manufacturing occupational employment, divided by initial population onto the Japan shock. These regressions are run in the EEOC data, over the time periods 1966-78 and 1978-88 (stacked specification). Regressions weighted by 1950 CZ employment. Panel A includes men only; Panel B includes women only. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

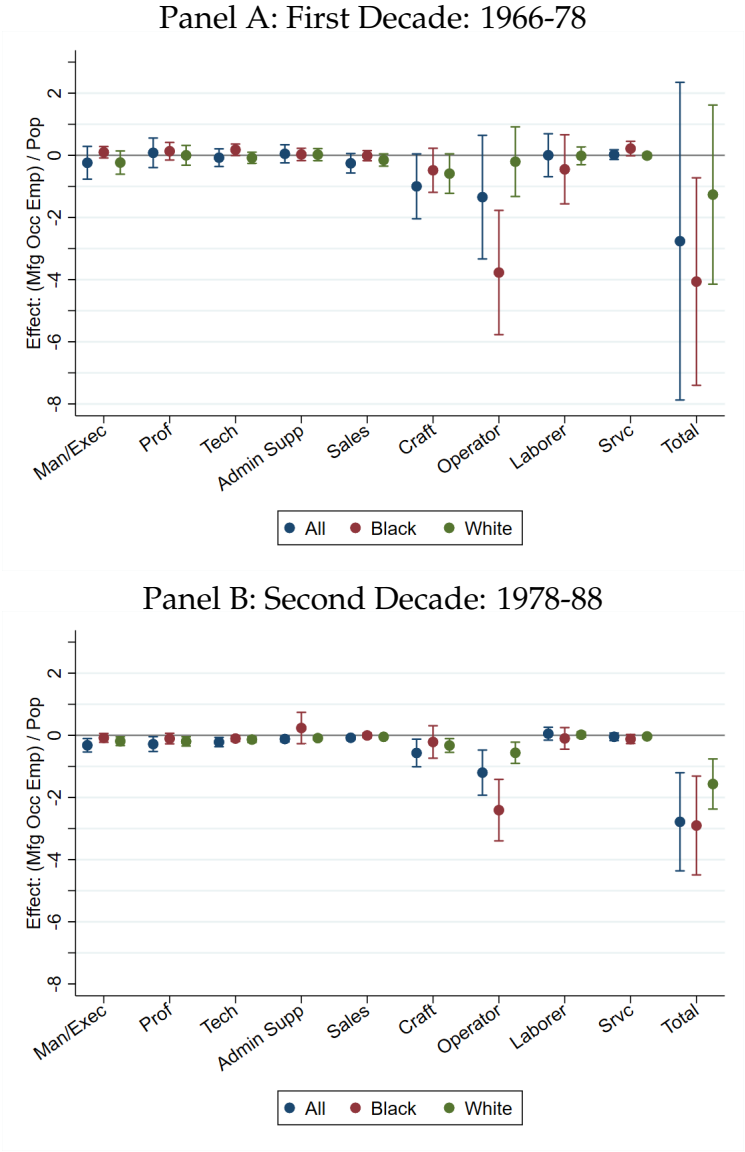


Figure 28: Employment Effects of the Japan Shock, By Period

Notes: This figure displays the coefficients from a regression of the change in manufacturing occupational employment, divided by initial population onto the Japan shock. These regressions are run in the EEOC data; Panel A is run over the time period 1966-78; Panel B is run over the time period 1978-88. Regressions weighted by 1950 CZ employment. This figure includes men only. We divide the endogenous variable by its mean *pooled over both periods*; thus, the coefficients can be interpreted as the effect size on the average CZ. The value of the shock is twice as large in the second period as the first.

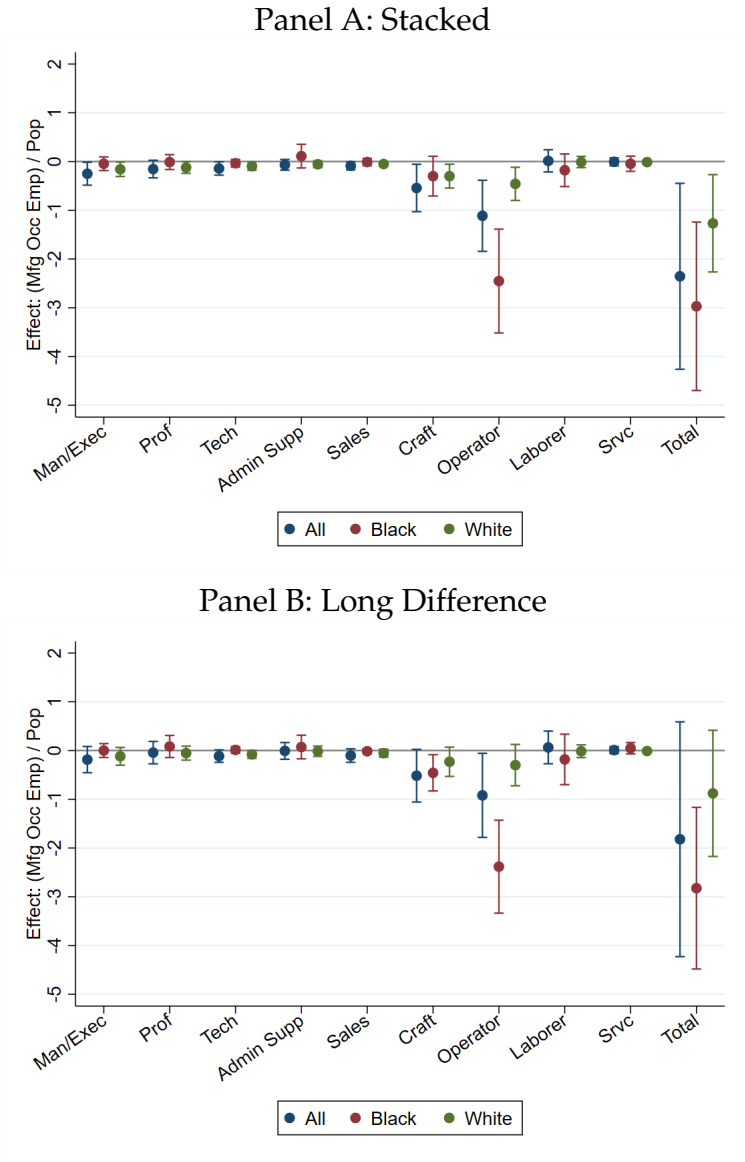
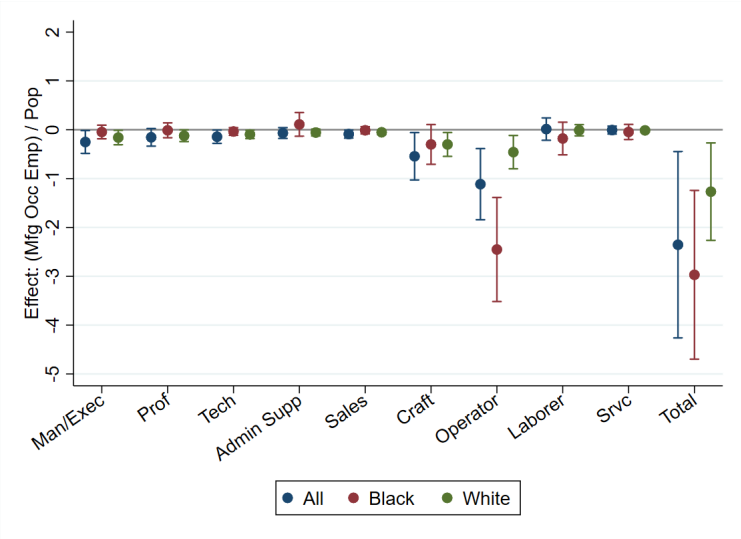


Figure 29: Employment Effects of the Japan Shock, Different Specifications

Notes: This figure displays the coefficients from a regression of the change in manufacturing occupational employment, divided by initial population onto the Japan shock. These regressions are run in the EEOC data; Panel A is run over the time periods 1966-78, and 1978-88 (stacked specification); Panel B is run over the time period 1966-1988. Regressions weighted by 1950 CZ employment. This figure includes men only. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

Panel A: Manufacturing Sector



Panel B: Non-Manufacturing Sector

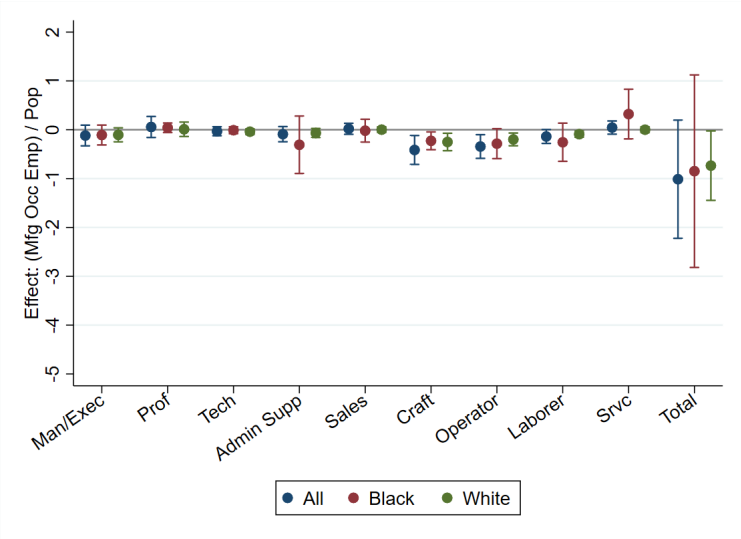


Figure 30: Employment Effects of the Japan Shock, by Sector

Notes: This figure displays the coefficients from a regression of the change in manufacturing occupational employment (Panel A) and non-manufacturing occupational employment (Panel B), divided by initial population onto the Japan shock. These regressions are run in the EEOC data, over the time periods 1966-78 and 1978-88 (stacked specification). Regressions weighted by 1950 CZ employment. This figure includes men only. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.

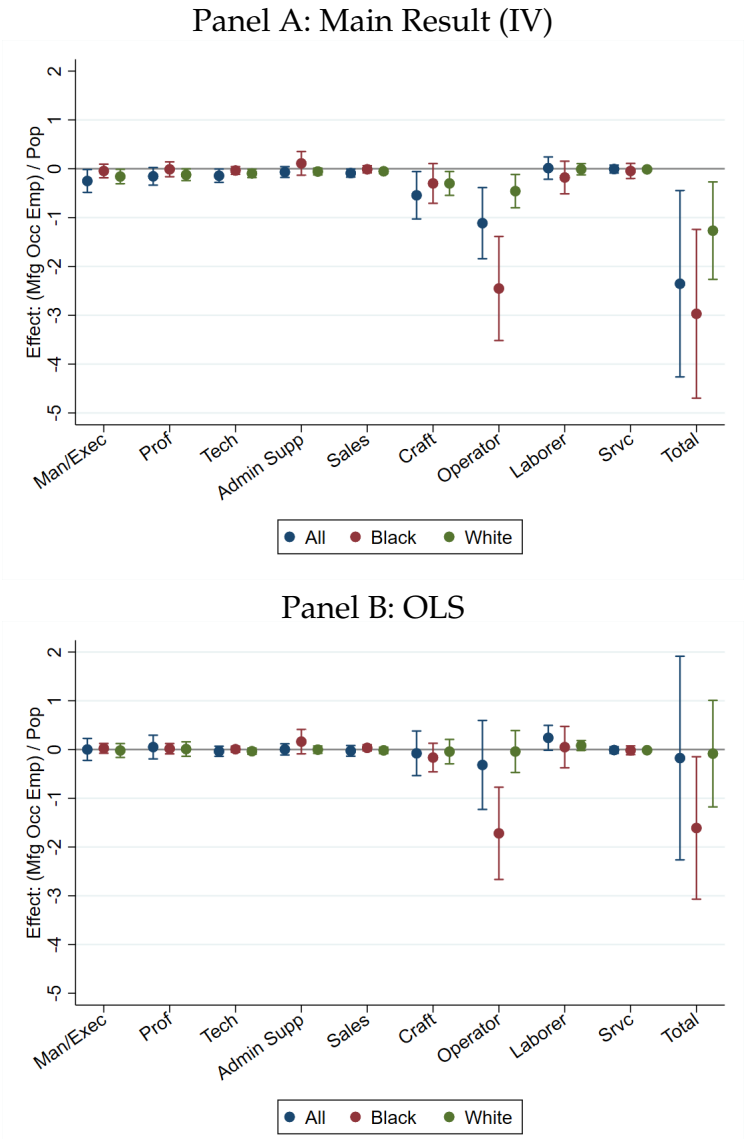


Figure 31: Employment Effects of the Japan Shock, by Estimation Method

Notes: This figure displays the coefficients from a regression of the change in manufacturing occupational employment divided by initial population onto the Japan shock. Panel A is estimated by IV; Panel B is an OLS estimate of the second stage. These regressions are run in the EEOC data, over the time periods 1966-78 and 1978-88 (stacked specification). Regressions weighted by 1950 CZ employment. This figure includes men only. We divide the endogenous variable by its mean; thus, the coefficients can be interpreted as the effect size on the average CZ.