Equality through Exposure? International Trade and the Racial Wage Gap

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Abstract

A key implication of Becker's (1957) work on discrimination is that greater product market competition can reduce employment discrimination generally, and discriminatory wage gaps in particular. Using US data on manufacturing wages and import exposure, we explore whether increased competition, in the form of a heightened exposure to imports, reduces the racial wage gap. Our findings support Becker's contention. We find that import exposure helped narrow the racial wage gap by about 1.4 percentage points between 1983 and 1993. The effect is especially pronounced among the most disadvantaged: unskilled Southern workers. For them, import exposure helped reduce racial wage disparities by 2.2 percentage points.

JEL Classification: F16; J71; J31.

1 Introduction

The racial wage gap in the United States is large. Recent studies suggest that whites earn at least 10% more than observationally-equivalent non-whites. While this gap is smaller than it has been for much of US history, it is still substantial. Insofar as it represents discrimination, it is a troubling indicator of societal inequity.

Contemporary economics' discourse on discriminatory wage gaps, and discrimination more generally, has been dominated by Becker's (1957) treatise on discrimination. A startling implication of his work is that increased product market competition can reduce discriminatory wage gaps. Becker's argument is based on the premise that indulging a taste for discrimination is costly, as it involves paying one's favoured group a premium. In an uncompetitive market, a discriminatory employer earns positive economic profits and is able to absorb the premium associated with his taste for discrimination. As the product market becomes more competitive, these economic profits shrink; the discriminatory premium becomes increasingly difficult to maintain. To remain in business, the employer must lower the premium paid to his favoured group. As such, Becker argues, increased competition will result in a narrowing of the discriminatory wage gap.

In this paper, we explore whether increased competition, in the form of a heightened exposure to imports, narrows the racial wage gap. To this end, we use US manufacturing data on import and wages for the period 1983 to 1993. We find that import exposure significantly influenced racial wage disparities: on average, it helped narrow the racial wage gap by about 1.4 percentage points. The analysis suggests, moreover, that the effect is most pronounced for particularly disadvantaged nonwhite populations, namely Southern workers.

2 Background

A number of papers have tested Becker's hypothesis on the effect of product market competition on employment discrimination. These studies fall into two main groups: (i) cross-sectional studies of the relationship between industry concentration and discriminatory wage gaps; and (ii) time-series or cross-sectional time series analyses of the effect changing competition on discriminatory wage gaps.

The cross-sectional studies provide mixed support for Becker's theory of market competition and discrimination. Early papers such as Fujii and Trapini (1978) and Johnson (1978) find no relationship between market concentration and racial wage gaps. More recent papers, however, support Becker's hypothesis. Ashenfelter and Hannan (1986), for example, find a negative relationship between concentration in the banking industry and female employment; while they focus on employment rather than wages, their results suggest a connection between product market competition and the ability to discriminate. Peoples (1994) compares racial wage gaps in concentrated and competitive industries. He finds that in non-unionized industries, racial wage gaps are larger in concentrated industries than in competitive ones. Racial wage gaps are, however, the same in all unionized industries. However, perhaps the most relevant paper to the present exercise is Agesa and Hamilton (2004). Using US data on a cross-section of manufacturing industries, they test whether increased exposure to foreign competition reduces the racial wage gap. Agesa and Hamilton (2004) find little to suggest that import competition narrows the racial wage gap.

The cross-sectional studies detailed above have done much to inform the debate on the impact of product market competition on discrimination. By relying on cross-industry variation, however, these studies often find it difficult to disentangle the effect of product market competition from unobserved cross-industry heterogeneity. To avoid this pitfall, many researchers have thus focussed on single industry time-series analyses

to tease out the effect of product market competition on discriminatory wage gaps. The bulk of these studies of these studies have concentrated on the impact of deregulation on discriminatory wage gaps. If Becker's theory is correct, deregulation should reduce discriminatory wage gaps by removing barriers to entry in the product market and engendering greater competition. Rose (1987), Peoples and Saunders (1993) and Agesa (1998) analyze the impact of deregulating the trucking industry on the racial wage gap. Black and Strahan (2001) consider the effects on the gender wage gap of deregulation in banking, while Peoples and Talley (2001) examine how deregulation has affected the racial wage gap in a number of transportation industries. All these papers suggest that deregulation narrowed discriminatory wage gaps.

In addition to these single-industry time series, there are two papers that exploit cross-industry variateion over time to consider the impact of increased competition on discrimination. Agesa, Agesa and Hamilton (2004) argue that if Becker's (1957) view on product market competition and discrimination is correct, whites moving from concentrated to competitive industries should see greater declines in wages than non-whites. Using data on job transitions, they find strong evidence to support Becker's theory. Black and Brainerd (2004) study whether heightened foreign competition, as represented by greater import penetration, significantly influences the gender wage gap. Focusing on cross-industry changes in import exposure and gender wage gaps between 1977 and 1994, Black and Brainerd (2004) find that while import exposure generally widened the gender wage gap, it narrowed it in concentrated industries. Their results lend some support to Becker's conjecture that product market competition reduces discriminatory wage gaps.

This paper uses cross-sectional, time series data to study whether increased competition, as manifested by increased import exposure, narrows the racial wage gap. It is the first paper to focus on import's impact on racial wage disparities, using cross-

industry, time-series data. Moreover, it breaks new ground by examining the effect of trade exposure on various nonwhite subpopulations. Its results bolster the view that increased product market competition reduces the racial wage gap.

3 Methodology and Data

3.1 The Model

We estimate the effect of import exposure on the racial wage gap with the following model:

$$In(w_{it}) = \alpha + \beta D^{NW} x_{kt} + \sum_{e=1}^{19}\sum_{t=1}^{11}\gamma_{et} + \sum_{s=1}^{51}\sum_{t=1}^{11}\delta_{st} + \sum_{a=1}^{47}\sum_{t=1}^{11}\zeta_{at} + \sum_{k=1}^{75}\sum_{t=1}^{11}\lambda_{kt} + \sum_{e=1}^{11}\sum_{g=1}^{2}\mu_{gt} + \sum_{r=1}^{2}\sum_{t=1}^{11}\psi_{rt} + \epsilon_{it} \quad (1)$$

w_{it} represents the hourly wage of individual i in year t. D^{NW} is a dummy variable that takes the value of one if the individual is non-white. x_{kt} represents industry k's exposure to international import in time t, where industry k is individual i's usual industry of employment. We provide further details on the import exposure variable in the Data subsection, *infra*. γ_{et} are education-year fixed-effects, δ_{st} are state-year fixed effects, ζ_{at} are age-year fixed effects, λ_{kt} are industry-year fixed effects, μ_{gt} are gender-year fixed effects, and ϕ_{rt} are race-year fixed effects. The coefficient of interest is β. A positive and significant β implies that wage gap between blacks and whites shrinks with increased import exposure.

The extensive collection of fixed effects is meant to control for well-established determinants of wages. Wages are generally increasing in education, so we include education fixed-effects; to control for changes in returns to education over the period

of study, we include a separate set of education fixed effects for each year. Since our data does not include experience variables, we proxy for experience by using age fixed effects; to capture changes in the return to experience over time, we incorporate a separate set of age fixed effects for each year. To control for the gender wage gap, we add gender-industry and gender-year fixed effects to the regression. To abstract away any state-specific idiosyncracies over time, we include state-year fixed effects. Lastly, we include race-year fixed effects. This suppresses the variation in racial wage gaps, over time, that is common to all industries.

3.2 Data

The study uses individual data from the annual Merged Outgoing Rotation Groups (MORG) of the Current Population Survey (CPS) of the United States. The CPS samples 50,000 households from across the United States. The households are chosen to be nationally representative on a number of dimensions. Households are initially interviewed for four consecutive months, dropped for eight months and then re-interviewed for four months before being dropped from the sample altogether. The CPS collects considerable information about the households, the families that reside therein, as well as the individuals in those families. Most importantly for our purposes, the CPS collects information on an individual's race, sex, education levels, marital status, state of residence, industry of usual employment, wages and hours worked per week. We use this to calculate an hourly wage.

Following Borjas and Ramey (1995) and Black and Brainerd (2004), we restrict the sample as follows. We focus on manufacturing workers, since we only have complete production data for manufacturing industries. We confine the sample to those between the ages of 18 and 64 who worked at least 30 hours per week for at least 48 weeks in the preceding year. We also eliminate all those who appear to have a wage of less

than one dollar per hour in 1977 dollars. Unfortunately, the CPS top-codes incomes above a certain level. We exclude all individuals with top-coded incomes. While this seems like a major exclusion, less than 1% of full-time manufacturing workers have top-coded incomes. The final sample is thus broadly representative of full-time manufacturing workers in the United States. Our final data set has contains information on approximately 27,000 workers for each year from 1983 to 1993. Altogether, the sample contains about 300,000 observations.

To proxy for an industry's import exposure, we use the ratio of imports to domestic production. This is identical to the measure used by Borjas and Ramey (1995) and Black and Brainerd (2004). To calculate the ratio, one needs data on imports and production, by industry, from 1983 to 1993. Data on imports are from the Center for International Data at the University of California at Davis. Production data for 1983 to 1983 are from the NBER Manufacturing Productivity Database. To reduce skew, we take the natural logarithm of the import-production ratio and use that as our measure of import competition by industry.

Before moving on to results, it is important to deal with two important issues regarding the estimation of (1) : (i) serial correlation; and (ii) subsample heterogeneity. While the CPS does not track the same households from year-to-year, it is a non-random sample. As Bertrand, Duflo and Mullainathan (2004) show, there is thus significant serial correlation in the wage data in the CPS. This serial correlation significantly biases any results obtained from OLS wage regressions. Bertrand, Duflo and Mullainathan (2004) suggest that a clustering correction that corrects for arbitrary forms of serial correlation within an industry is sufficient to address the biases created by serial correlation in the CPS. Accordingly, we correct all our standard errors for clustering at the industry level.[1]

[1] Clustering at the industry-level has an added benefit. It corrects for any within-industry correlation among the error terms created by using an industry-level regressor, import exposure, on individual level data.

A second, more fundamental concern is the bias engendered by heterogeneity across race-year subsamples. In identifying the effect of import exposure on the relative wages of non-whites, regression (1) exploits the variation in wages between the white and nonwhite subsamples in any given year, as well as the variation in the white and non-white subsamples over time. To cleanly isolate the differential effect of import exposure on nonwhites, however, it is crucial that, within any year, the white and nonwhite subsamples be largely alike on dimensions other than race. In particular, in our framework, the White and Nonwhite subsamples of a given year should have similar age-gender-education-state-industry profiles. It is, moreover, important that the White and Nonwhite subsamples be similar from one year to the next; the subsamples of White workers in 1983 and 1993, for instance, should have very similar age-gender-education-state-industry profiles. Stated more formally, for Whites and Nonwhites within a given year, the joint distributions of the conditioning variables – i.e., wage determinants other than race and import exposure – should be virtually identical. Furthermore, for both Whites and Nonwhites, the joint distributions of the conditioning variables – wage determinants other than import exposure – should be alike from one year to the next. Otherwise, as Heckman, Ichimura and Todd (1996) point out, OLS estimates of the coefficient of interest could be biased: the estimates of β will be confounded by differences in the joint distributions, across the race-year subsamples, of the conditioning variables.[2]

To mitigate the bias generated by heterogeneity across subsamples, Heckman, Ichimura and Todd (1996) propose a set of matching procedures. The purpose of those matching methods, and those developed in other papers, is to restrict the analysis to subsamples that have similar joint distributions of the conditioning variables. Here, we implement matching by using a modified version of the reweighting procedure

[2]Heckman, Ichimura and Todd (1997) show this formally. The also estimate the importance of this bias, relative to sample selection bias; they find that the bias from the former dwarfs that from the latter.

proposed by Dinardo, Fortin and Lemiuex (1996). This latter method is equivalent to that in Juhn, Murphy and Pierce (1993).

We begin by selecting a baseline subsample. Since we consider the effect of import exposure on the relative wages of nonwhites, over time, we use the 1983 nonwhite subsample as our baseline. We then pool the baseline subsample with each of the other race-year subsamples, in turn, and run the following PROBIT regressions:

$$P_{is} = \Phi(d_{is} = 1|x_{is})$$

d_{is} a dummy variable takes the value of one if the observation is in the baseline sample. x_{is} represents age, education, gender, industry and state fixed effects. The P_{is} obtained from the PROBIT regressions are the predicted probability that any individual is in the baseline subsample, given her age, education, gender, industry and state of residence. These predicted probabilities are then employed to assign a new weight θ_{irt} to each observation in the sample:

$$\theta_{irt} = \frac{\upsilon_{irt}}{\upsilon_{rt}} \times \frac{P_{is}}{1 - P_{is}} \times \frac{1 - P_s}{P_s}$$

where υ_{irt} is the CPS population weight associated with individual i, of race r in year t and υ_{rt} is the sum of CPS population weights for race r in year t. P_s is the unconditional probability that an observation is in the baseline subsample. This reweighting procedure ensures that the White and Nonwhite subsamples in any year will have similar joint distributions of gender, education, age, industry and state characteristics. It likewise ensures that, from one year to the next, the White and Nonwhite subsamples have similar joint distributions of gender, education, age, industry and state attributes.

Table 1: Fixed-Effects Regressions

	(1)	(2)	(3)	(4)	(5)
Nonwhite × import Exposure	0.00968*	0.00964*		0.00962**	0.0110*
	(0.00410)	(0.00409)		(0.00409)	(0.00549)
Nonwhite × Adjusted Price-Cost Margin			-0.00766	-0.00507	0.0647
			(0.0228)	(0.0211)	(0.0982)
Nonwhite × import Exposure × Adjusted Price-Cost Margin					-0.0159
					(0.0222)
Nonwhite × Unionization		0.0235**	0.0247**	0.0233**	0.0229*
		(0.00883)	(0.00924)	(0.00891)	(0.00891)
Observations	309,787	309,231	309,231	309,231	309,231
R^2	0.68	0.68	0.68	0.68	0.68

Standard errors, in parentheses, are clustered at the industry level.
** and * indicate significance at the one and five percent levels, respectively.

4 Results

Table 1 contains the results of estimating the baseline regression, (1). As noted above, all standard errors are corrected for clustering at the industry level.

The results in column (1) imply that if import exposure doubles, the wage gap between whites and non-whites shrinks by 0.7 percentage points. Average import exposure in manufacturing, however, increased nearly four-fold between 1983 and 1993. Specification (1) implies, therefore, that increased import exposure narrowed the racial wage gap by about 1.4 percentage points. This is a large effect considering that in 1983, nonwhites earned 11.4% less than observationally-equivalent whites.[3]

[3]We obtain the estimate of the racial wage gap by running a Blinder-Oaxaca decomposition on wage data for 1983. This decomposition used gender, age, industry and state fixed effects as observable characteristics. Bound and Freeman (1992) argue that the racial wage gap changed little between the late 1970s and the early 1990s. We find, much as they do, that the wage gap between observationally-equivalent whites and non-whites is roughly the same in 1993 and 1983 The results in Table 1 suggest that but for the increase in import exposure, the racial wage gap would have been larger in 1993 than in 1983.

Besides a dramatic increase in import exposure, US manufacturing industries experienced another fundamental change: rapid deunionization. This deunionization arguably has significant implications for racial wage disparities. To avoid division among their members, and the consequent weakening of their bargaining power, unions negotiate standardized wage contracts. Indeed, Peoples (1994) shows that racial wage gaps are significantly lower in unionized industries. The rapid deunionization in the 1980s may thus have led to an expansion in racial wage disparities, or at least slowed their decline.

For our purposes, deunionization is problematic if those industries that underwent the most extensive deunionization also witnessed the smallest increases in import exposure. If this is the case, the effects of import exposure, detailed in specification (1), will be confounded with the effects of de-unionization. Accordingly, in Table 1, specification (2), we add, to the baseline regression, the industry's unionization rate, interacted with the non-white dummy. In keeping with Peoples' (1994) finding, we observe that higher unionization levels are associated with smaller wage gaps. Our main result, though, that increased import exposure lessens racial wage disparities, persists.

The analysis so far has focussed on one source of product market competition, viz., exposure to imports, and its impact on discriminatory wage gaps. Becker's theory, though, posits a general relationship between competition and discriminatory wage differentials. If Becker's hypothesis is correct, changes in the domestic market should also influence the racial wage gap. Ignoring domestic market conditions could be especially problematic if changes in domestic competition are correlated with changes in import exposure; in particular, if industries that saw the largest increases in import exposure also witnessed significant increases in domestic competition, the purported effect of import exposure, outlined in specifications (1) and (2), will be confounded.

Controlling for domestic competition is, however, complicated, by measurement difficulties. Black and Brainerd (2004) and Borjas and Ramey (1996) differentiate com-

petitive from concentrated industries by using four-firm concentration ratios. As Kwoka (1979) points out, however, the connection between four-firm concentration ratios and the competitiveness of the market is tenuous. An alternative to using concentration ratios to measure competition is to use a metric based on the actual economic profits earned in an industry, such as the price-cost margin. The price-cost margin for industry k in year t is defined as

$$PCM_{kt} = \frac{p_{kt} - MC_{kt}}{p_{kt}},$$

where p_{kt} is the price of a unit of industry k's output in year t, while MC_{kt} represents the marginal cost. In competitive industries the price-cost margin will be close to zero, as competition will drive down prices to marginal costs.

To estimate the price-cost margin, we follow the literature in assuming that average costs approximate marginal costs. This enables us to obtain the price-cost margin from the industry's aggregate revenue and costs. Specifically, the price-cost margin is estimated as:

$$PCM_{kt} \approx \frac{R_{kt} - TC_{kt}}{R_{kt}},$$

where R_{kt} is the total sales' revenue of industry k in year t, while TC_{kt} are the total production costs for industry k in year t.[4] Data on industry revenues and costs are from NBER Manufacturing Productivity Database.

The price-cost margin defined above is an estimate of the aggregate economic profits earned in a given industry. It reflects the industry's overall competitive environment, and as such captures the effects of both domestic and foreign competition on profitability. To isolate the level of domestic competition in an industry, we purge the price-cost margin of the effects of import competition by running the following auxiliary regression:

[4] We account for capital costs as in Kwoka (1979).

$$PCM_{kt} = \omega + \iota x_{kt} + \xi_{kt}. \tag{2}$$

The auxiliary regression's residuals, ξ_{kt}, are the price-cost margins with the effects of import competition abstracted. We use these as our measure of domestic competition. An inspection of these adjusted price-cost margin suggests that there is little time-series variation in the price cost margins: 86% of the variation is cross-sectional, with only 6% being over time. This accords with evidence from a number of sources that suggests that domestic competitive structure in US manufacturing industries has changed little over the last 25 years.

Specification (3) in Table 1 replaces the nonwhite-import exposure interaction term with an interaction of the nonwhite dummy with the adjusted price-cost margin. The coefficient on nonwhite-price cost margin interaction term is statistically insignificant, suggesting that changes in domestic competition did not influence racial wage disparities. In specification (4), we reintroduce the nonwhite-import exposure interaction term; in (5), we further add a triple interaction of the nonwhite dummy with import exposure and the adjusted price-cost margin. The latter term explores whether the impact of import exposure on the racial wage gap depends on the level of domestic competition. The results in (4) and (5) suggest that changes in domestic competition had no impact on the relative wages of nonwhites; likewise, the effect of import exposure was unmoderated by the level of domestic competition. As noted above, the adjusted price-cost margins suggest that there is little variation in domestic competition between 1983 and 1991. It is thus unsurprising that changes in domestic competition appear to have a negligible impact on the racial wage gap. Most importantly, though, our main finding – that increased import exposure reduces racial wage disparities – prevails.

To evaluate the robustness of our results, we considered a number alternate measures of domestic competition. These include: (i) four-firm concentration ratios; (ii) the concentration indicator from Borjas and Ramey (1996); (iii) the price-cost margin, unadjusted for import exposure; and, (iv) Tobin's q.[5] The results, not reported here, echo those in Table 1; the findings therein are thus not a function of idiosyncrasies in our domestic competition metric.

5 Extended Analysis

The results in Table (1) support Becker's contention that product market competition will reduce discriminatory wage gaps. Heightened competition from imports appears to significantly narrow the wage gap between observationally-equivalent whites and nonwhites in manufacturing industries. To further our understanding of the effect of imports on the racial wage gap, however, we now explore the impact of import competition on various subsamples. Such analysis determines whether import exposure affects the relative wages of all nonwhite manufacturing workers, or just particular subpopulations. It also helps identify which groups were most significantly affected by greater international competition. Most importantly, it ensures that the average effects set out in Table 1 do not mask deleterious effects of import exposure on important minority sub-groups.

In conducting this extended analysis, we employ, as our starting point, specification (5) from Table 1. To be precise, we use the following model as our baseline:

[5] Tobin's q at the industry level is the market value of the industry's firms divided by the replacement value of their assets. If q is greater than one, it suggests that the industry delivers returns greater than the marginal return on its assets, i.e., the industry produces economic profits. A large Tobin's q, suggests considerable economic rents, and by extension, a non-competitive environment. See Smirlock et al. (1984) for a discussion of the use of Tobin's q as a measure of industry competition. Like the price-cost margin, Tobin's q reflects the overall competitive environment in an industry. To purge the effects of import exposure, we use an auxiliary regression similar to (2), and adopt the residuals as the measure of domestic competition.

$$
\begin{aligned}
ln(w_{it}) = \alpha + \beta D^{NW} x_{kt} + \kappa D^{NW} u_{kt} + \eta D^{NW} APCM_{kt} + \psi D^{NW} APCM_{kt} x_{kt} \\
+ \sum_{e=1}^{19} \sum_{t=1}^{11} \gamma_{et} + \sum_{s=1}^{51} \sum_{t=1}^{11} \delta_{st} + \sum_{a=1}^{47} \sum_{t=1}^{11} \zeta_{at} + \sum_{k=1}^{75} \sum_{t=1}^{11} \lambda_{kt} \\
+ \sum_{g=1}^{2} \sum_{t=1}^{11} \mu_{gt} + \sum_{r=1}^{2} \sum_{t=1}^{11} \psi_{rt} + \epsilon_{it},
\end{aligned} \tag{3}
$$

where u_{kt} is the unionization rate for industry k in year t and $APCM_{kt}$ is the adjusted price cost margin for industry k in year t. All the ensuing results are robust to the exclusion of some, or all, of the interaction terms included in equation (3) but excluded in equation (1). The findings that follow are also robust to the use of alternate measures of domestic competition.

5.1 Skilled v. Unskilled Workers

We begin by considering the impact of import exposure across workers of different skill levels. This exercise is motivated by the mounting evidence that, in general, import competition has had different effects on skilled and unskilled US workers (Borjas and Ranney 1995; Bernard, Jensen and Schott 2006). It is, therefore, important to check that the average effect of import exposures on the relative fortunes of nonwhites, obtained above, do not conceal variation between skilled and unskilled workers.

To distinguish the effects of import exposure across different skill levels, we augment equation (3) with a triple interaction term that isolates the effect of import exposure on skilled nonwhites. In keeping with much of the literature, we define skilled workers as those having more than a high school education. The triple interaction term thus consists of an indicator that takes the value of one if individual i has more than a high school education, interacted with an indicator that takes the value of one if individual i is

Table 2: Effects by Educational Categories and Sex

	(1)	(2)	(3)	(4)
Nonwhite × Import Exposure	0.0112*	0.0113*	0.0113*	0.0113*
	(0.00547)	(0.00551)	(0.00555)	(0.00557)
Skilled × Nonwhite × Import Exposure	-0.000544	-0.000606	-0.000561	-0.000650
	(0.000392)	(0.000374)	(0.000397)	(0.000464)
Less than High School × Nonwhite × Import Exposure		-0.000176		
		(0.000230)		
Female × Nonwhite × Import Exposure			-0.000154	-0.000226
			(0.000449)	(0.000582)
Female × Skilled × Nonwhite × Import Exposure				0.000235
				(0.000485)
Observations	309,231	309,231	309,231	309,231
R^2	0.68	0.68	0.68	0.68

Standard errors, in parentheses, are clustered at the industry level.
** and * indicate significance at the one and five percent levels, respectively.

nonwhite, all interacted with the trade exposure in individual i's industry of employment.

Column (1) in Table 2 reports the results of the regression that includes this triple interaction term. The coefficient on the triple interaction term is insignificant, implying that import exposure reduced racial wage disparities among skilled and unskilled workers alike. These findings provide a more nuanced view of the impact of trade on unskilled workers. The precise effects of trade on unskilled workers is still up for debate, but there is evidence to suggest that greater trade exposure has worsened the fortunes of unskilled workers in advanced economies.[6] The results above suggest that while trade may have adversely affected unskilled workers overall, it improved the relative fortunes of nonwhite unskilled workers.

[6]See, e.g., Bernard and Jensen (1997).)

5.2 Male v. Female Workers

In their paper discriminatory wage gaps, Black and Brainerd (2004) show that import exposure has had disparate effects on male and female workers: in concentrated industries, import exposure helped shrink the male-female wage gap; in competitive industries, though, it increased it. Given this general evidence of differential effects, one needs to consider whether imports had different impacts on male and female nonwhite workers. As such, in specification (3) in Table 2, we separates out the impact of import exposure on nonwhite women. In (4), we go one step further: we include a quadruple interaction term that isolates the effect of import competition on skilled, nonwhite women. The results reported in (3) and (4) suggest that import exposure shrank the wage gap equally for male and female workers, skilled or otherwise.

5.3 Effects by Region

As a final robustness check, we separate out the impact of import exposure by geographic region. There is considerable regional variation in discriminatory wage gaps in the United States. Blinder-Oaxaca decompositions of wage data for 1983-1985 suggest that while the nonwhites earned 11.6% less than observationally-equivalent whites nationally, they earned 5.8% less in the Midwest, 9.6% less in the West, 12.5% less in the Central States, 12.4% less in the Northeast, and 16.2% less in the South.[7] Given this diversity of initial conditions across regions, it is important to study the effects of import competition by region: the national averages may well conceal important

[7] The regions are defined as follows: (1) The Northeast, which consists of Maine, New Hampshire, Vermont, New York, Connecticut, New Jersey, Pennsylvania, Massachusetts, Rhode Island, Maryland, Delaware and the District of Columbia; (2) The Midwest, which is Ohio, Michigan, Wisconsin, Minnesota, Iowa, Indiana, Illinois; (3) The South, which consists of Virginia, West Virginia, Kentucky, Tennessee, North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, Arkansas , Texas, Louisiana, Oklahoma and Missouri; (4) the West, which is California, Oregon, Washington, Alaska and Hawaii; and, (5) The Central States, which refer to North Dakota, South Dakota, Nebraska, Montana, Idaho, Wyoming, Colorado, Utah, New Mexico, Arizona and Nevada.

Table 3: Effects by Region

	(1)	(2)	(3)
Nonwhite × Import Exposure	0.0104*	0.0105*	0.0105*
	(0.00501)	(0.00505)	(0.00506)
Central × Nonwhite × Import Exposure	-0.000384	-0.000384	-0.000384
	(0.000307)	(0.000306)	(0.000306)
South × Nonwhite × Import Exposure	0.00379*	0.00379*	0.00379*
	(0.00161)	(0.00160)	(0.00160)
Midwest × Nonwhite × Import Exposure	-0.000561	-0.000557	-0.000556
	(0.000404)	(0.000416)	(0.000419)
West × Nonwhite × Import Exposure	-0.000645	-0.000646	-0.000645
	(0.000438)	(0.000440)	(0.000439)
Skilled × Nonwhite × Import Exposure	-0.000574	-0.000579	-0.000635
	(0.000323)	(0.000325)	(0.000376)
Female × Nonwhite × Import Exposure		-0.0000554	-0.000100
		(0.000406)	(0.000527)
Female × Skilled × Nonwhite × Import Exposure			0.000146
			(0.000453)
Observations	309,231	309,231	309,231
R^2	0.68	0.68	0.68

Standard errors, in parentheses, are clustered at the industry level.
** and * indicate significance at the one and five percent levels, respectively.

regional differences.

Table 3 presents the results of separating out the effects of import exposure by region The findings in Table 3 suggest import exposure has helped mitigate racial wage gaps in all regions of the United States. The effect, though, has been especially pronounced for Southern workers: for them, import exposure reduced racial wage disparities by 2.2 percentage points.

6 Conclusion

Using individual level wage data for the United States from 1983 to 1993, this paper studies the effect of increased product market competition on discriminatory wage gaps. Specifically, it examines whether increased competition, in the form of enhanced exposure to imports, helped mitigate the white-nonwhite wage gap in manufacturing industries. The analysis support Becker's (1957) assertion that product market competition can help alleviate discriminatory wage gaps. The results suggest that, on average, increased import exposure helped narrow the racial wage gap by about 1.4 percentage points. The impact of greater import competition was was especially important for the most disadvantaged, Southern workers.

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