THE LABOR MARKET IMPACT OF STATE-LEVEL
ANTI-DISCRIMINATION LAWS, 1940-1960

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Abstract: By the time Congress passed the 1964 Civil Rights Act, 98 percent of non-southern blacks (40 percent of all blacks) were already covered by state-level "fair employment" laws which prohibited labor market discrimination. This paper assesses the impact of fair employment legislation on black workers' income, unemployment, labor force participation, and occupational and industrial distributions relative to whites using a difference-in-difference-in-difference framework. In general, the fair employment laws adopted in the 1940s appear to have had larger effects than those adopted in the 1950s, and the laws had relatively small effects on the labor market outcomes of black men compared to those of black women.

1. Introduction

By the time the United States Congress passed the Civil Right Act of 1964, approximately 98 percent of non-southern blacks (40 percent of all U.S. blacks) were already covered by state-level "fair employment" legislation which prohibited employment discrimination on the basis of race, color, creed, and national origin. Understanding the labor market impact of these state-level laws is important for at least two reasons. First, prior to 1964, the state fair employment laws were arguably the most significant legislative achievements of the Civil Rights Movement, but economic analyses of the impact of the Civil Rights Movement have been heavily weighted toward the study of federal legislation (e.g., Smith and Welch 1989, Donohue and Heckman 1991, Chay 1998). Thus, a more balanced and accurate description of the Civil Rights Movement's contribution to African-American economic progress, particularly through the promotion of anti-discrimination legislation, requires an assessment of the state laws' effects. Second, some critics of current affirmative action programs recommend pursuing "color blind" employment standards which sound quite similar in spirit and form to the fair employment standards (e.g., Thernstrom and Thernstrom 1997, p. 540). By studying the impact of the state fair employment laws, one can discern whether or not such legal standards facilitated economic gains for blacks in the past.[1] Certainly, debates about the future of race-related policy ought to be informed about the successes and failures of alternative legal arrangements.

By the early 1960s, the state agencies charged with enforcing the fair employment laws had become targets of harsh criticism from civil rights groups which claimed that the fair employment laws were not effectively deterring discriminatory practices (e.g., Hill 1964). This conviction helped underpin not only the continuing drive for federal anti-discrimination legislation, but also the drive to strengthen enforcement powers, to widen the scope of discrimination's definition, and ultimately, to pursue "affirmative action" rather than just "fair employment" policies (see Moreno 1997).

But were the state fair employment laws really such failures? To my knowledge, only two papers have offered econometric estimates of their impact. Using aggregate state-level census data,

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[1] The claim by Thernstrom and Thernstrom (1997, p. 427; citing Belz 1991, p. 40) that "it cannot be argued that color-blind policies had failed; they had not been tried" is incorrect. Such policies had been pursued by state governments for years before affirmative action programs were implemented.

Landes (1968) found a modest positive impact on the wages of nonwhite males relative to whites, but he also detected a modest positive impact on the relative unemployment rate. Using similar data but a different econometric technique, discussed below, Heckman (1976) measured a somewhat larger wage effect but did not explore other labor market outcomes.

This paper extends the previous work in a number of ways and provides a richer account of when, where, for whom, and how much these laws mattered. First, the empirical investigation is based on individual-level census data rather than on state-level averages. This allows control for a variety of individual characteristics (age, education, metropolitan residence, and so on) which are strongly correlated with labor market outcomes, but only roughly accounted for in studies depending on aggregate data. Second, the paper employs a differences-in-differences-in-differences framework within which "experimental" and "control" states can be more carefully chosen to identify a fair employment effect than in earlier work. Third, the paper assesses a variety of labor market outcomes, including income, employment status, labor force participation, occupational and industrial distribution, and migration. Finally, unlike most other studies of the effects of civil rights policy, this paper examines labor market outcomes for both men and women.

The story that emerges from the empirics provides new insights, but it is neither simple nor complete, and so the following general conclusions are made with appropriate caution. First, the states that adopted the laws in the 1940s appear to have improved the relative income of black workers (male and female) by more than the states that adopted laws in the 1950s. Second, the fair employment effects for black women, in terms of income, unemployment, labor force participation, and occupational status, appear to have been substantially more positive than for men in both the 1940s and the 1950s. Although further research will be necessary to understand precisely why these differences across decades and gender groups exist, I do investigate a number of hypotheses, particularly for the 1940s, and suggest some others worthy of exploration.

2. A Brief History of State Fair Employment Laws

By 1964, 22 non-southern states had adopted fair employment laws which were enforced by government administrative agencies and backed by state courts. New York was the first, passing a

fair employment statute in 1945, and was followed in quick succession by New Jersey (1945), Massachusetts (1946), Connecticut (1947), Oregon (1949), Rhode Island (1949), New Mexico (1949) and Washington (1949).[2] These laws have deep historical roots. In fact, the seed of the idea that the government should not discriminate in employment is embedded in many state constitutions that ban religious tests for public officials. Over time, this principle expanded to cover other kinds of government employees and other forms of discrimination, including race (for details, see Bonfield 1967). The fair employment legislation of the post-1940 period, however, took a sizable step beyond earlier curbs on discrimination. The laws applied broadly to private employment, even when there was no direct connection to government funds, and independent agencies with the power to issue cease-and-desist orders were charged with the laws' enforcement. Thus, the scope of coverage and the method of enforcement of anti-discrimination policies changed distinctly in this period.

Although several states adopted fair employment legislation before the federal government did, the most important precursor to the state anti-discrimination efforts was created at the federal level during World War II (Ruchames 1953, Reed 1991). In 1941, under pressure from A. Philip Randolph's March on Washington Movement, Franklin Roosevelt issued an executive order declaring that "there shall be no discrimination in the employment of workers in defense industries or government because of race, creed, color, or national origin" and established the Fair Employment Practice Committee (FEPC) to field and resolve complaints of discrimination. The federal anti-discrimination measures were never passed into law during the war, however, and afterwards, the FEPC was disbanded.

For the next two decades, congressional bills prohibiting discrimination in employment were frequently detained in committee. On the few occasions when the bills made it to the floor for debate, filibusters preempted their passage in the Senate. When the 1964 Civil Rights Act finally did pass, it was only after the Senate mustered a two-thirds majority vote for cloture on the southern

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[2] Subsequently, Michigan (1955), Minnesota (1955), Pennsylvania (1955), Wisconsin (1957), Colorado (1957), California (1959), Ohio (1959), Illinois (1961), Kansas (1961), Missouri (1961), Hawaii (1963), and Indiana (1963) adopted fair employment laws before the 1964 Civil Rights Act (Landes 1968). Several of the remaining states passed laws after 1964.

filibuster.[3]

While frustrated at the federal level, the Civil Rights Movement's legislative agenda moved forward in the form of state-level anti-discrimination initiatives (Lockard 1968, Collins 2000a). The details of the fair employment laws' provisions varied somewhat across states, but the prohibitions and methods of enforcement were, for the most part, quite similar because they emulated New York's law. The "standard package" of enforcement powers wielded by the state fair employment agencies included the power to receive and investigate complaints of discrimination, to eliminate any unlawful discrimination by conference and persuasion, and if necessary, to issue cease-and-desist orders to non-compliant firms and unions (Norgren and Hill 1964, pp. 94-98).[4] Typically, in response to a discrimination complaint, a representative of the fair employment agency would contact the accused establishment to collect information about both the alleged act of discrimination and the establishment's general employment policies. On the basis of the investigator's report, a fair employment commissioner would decide whether there was sufficient evidence of discrimination to press forward. If so, the commissioner would meet with a firm or union representative to seek "conciliation" – essentially a formal agreement to discontinue discriminatory practices, to reverse the act of discrimination against the person who complained, and to submit to subsequent reviews. To this point in the process, the charge of discrimination and the findings of the fair employment agency would not be publicized, and relatively few cases went beyond this stage.[5] If, however, conciliation could not be achieved, the agency would order a public hearing before a panel of fair employment commissioners. If "probable cause" of discrimination was upheld at the hearing, then a cease-and-desist order, backed by state courts, would be issued (Norgren and Hill 1964, pp. 102-113).

[3] Beginning with proposed anti-lynching legislation in 1938 and ending with the Civil Rights Act of 1964, eleven consecutive efforts failed to secure cloture on Senate filibusters holding up civil rights-related legislation (Congressional Quarterly Almanac 1964, p. 368).

[4] It should be noted that in some ways the reach and strength of the state agencies exceeded that of the EEOC as established by Title VII of the 1964 Civil Rights Act. In general, the state laws covered a broader class of employers than the federal law did, including smaller employers and those who were not engaged in interstate commerce. Furthermore, the state agencies could issue cease-and-desist orders, but the EEOC could not (Bonfield 1967, pp. 1082-1088).

[5] For example, from 1945 through 1960, New York State's anti-discrimination agency "adjusted" 3,262 employment cases. Only 18 cases went to public hearings, and only three cases resulted in cease-and-desist orders (Norgren and Hill 1964, pp. 116, 107).

Thus, the state fair employment agencies had two levers to pull when faced with opposition. First, the agencies could attempt to publicly embarrass the firm, union or employment agency. Second, the agencies could resort to their cease-and-desist power, and of course, the rarity of such directives does not imply that their threat was without effect. Essentially, both of these levers could increase the cost of discrimination for employers, but the state laws also might have lowered the perceived cost of integration. For example, the state agencies might have facilitated integration by giving employers an excuse to hire blacks despite the wishes of white employees or local employment norms, or by drawing on their accumulated experience to suggest how integration could be accomplished without risking a disruption of work (Cartwright 1948, p. 310; Lockard 1968, p. 88).[6]

3. The Nature of Discrimination and the Impact of State Fair Employment Laws

The potential impact of fair employment legislation depends heavily on the nature of labor market discrimination. In a basic Becker (1957) model of *employer* discrimination, black workers obtain employment by offering their services at a lower wage rate than equally productive whites in order to compensate the employer for the psychic costs presumed to accompany blacks' employment. Unless or until non-discriminatory employers drive discriminatory ones out of the market, the labor market equilibrium entails a wage gap between equally productive black and white workers. If blacks and whites have a similar distribution of reservation wages, the wage gap may also imply a gap in employment rates. Therefore, in this context, an effectively enforced fair employment law which simultaneously bans discrimination in hiring, promotion, and wages would tend to diminish the observed wage gap as well as any existing gap in employment rates.

Although employer groups (e.g., state chambers of commerce) were among the most vocal and organized opponents of state fair employment laws (Kesselman 1948, Lockard 1968), it does not

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[6] In discussing the impact of the 1964 Civil Rights Act on racial employment patterns in the South, Donohue and Heckman argue that "in certain ways the South was ripe for change. There is evidence that some Southern employers were eager to employ blacks if given the proper excuse" (1991, p. 1639). Perhaps the same was true of northern employers prior to the passage of state fair employment laws.

necessarily follow that this model is the most accurate representation of labor market discrimination in the 1940s and 1950s. When explaining their opposition to, or non-compliance with, fair employment standards, employers sometimes expressed concern about how the white employees, customers, or community would react to racial integration or to the promotion of blacks into previously all-white occupations.[7]

If *employees*, rather than employers, are the repositories of discriminatory attitudes in a Becker model with competitive input and product markets, then racial segregation within or between workplaces results, but not a wage gap. In this context, fair employment legislation might force integration, but there would be no detectable impact on relative wages. However, if there are wage premiums, perhaps associated with unions, in a particular set of occupations, firms, or industries that happen to employ whites disproportionately, then a racial wage gap would exist for otherwise observationally similar workers.[8] Then, by forcing integration, an effective fair employment law would tend to diminish the racial wage gap.

Unlike the Becker models, models of statistical discrimination do not begin by assuming that some group has a taste for discrimination; rather, discrimination follows naturally from imperfect information about individual workers' productivity. For example, Lundberg and Startz (1983) demonstrate that when information for one group is less reliable than information for another group, optimizing firms will construct different wage offer schedules for the different groups. Optimizing workers, in turn, respond to these offer curves when making human capital investments. In general, workers from the group with less reliable information receive lower wage offers, even after controlling for observed human capital levels. Requiring firms to maintain the same wage offer

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[7] In 1952, Pennsylvania's Governor's Commission on Industrial Race Relations surveyed 1,229 firms (it is unclear how they were selected). Ninety percent of the firms were found to practice some form of racial or religious discrimination, with the frequency of discriminatory practices increasing with occupational skill level. As to why they discriminated, by far the most frequent responses were "tradition" and "company policy," followed by "alleged union restrictions." Many fewer firms cited "alleged employee reaction" or "alleged customer reaction."

[8] The relationship between unions and African-American economic progress in the 1940s and 1950s was a complicated one. On one hand, unions (especially CIO) helped promote fair employment legislation, but on the other hand unions (especially AFL) had long histories of racial exclusion and were often the most resistant targets of the state fair employment agencies. See Northrup (1944), Ashenfelter (1972), and Collins (2000a).

schedule for both groups would then narrow the wage gap in two ways: by eliminating gaps between workers with similar human capital levels in the short run, and by encouraging convergence in human capital levels across groups over the long run.

Finally, racial differences in human capital, occupational attainment, industrial distributions, and income levels might persist and be re-enforced through a variety of feedback mechanisms, path-dependencies, network effects, and social norms (e.g., Myrdal 1944, Arrow 1973, Wright 1987, Whatley 1990, Loury 1998). If so, then even in a race-neutral labor market, the erosion of economic disparities derived from past discrimination could be a very slow process. In this context, although fair employment laws might diminish the frequency of overt acts of discrimination, they might not be expected to quickly narrow racial gaps in labor market outcomes. Consequently, groups or policymakers seeking a rapid narrowing of racial gaps would argue for stronger intervention than is provided by fair employment standards. Indeed, arguments along these lines unfolded in political and judicial discourses during the 1960s as policy drifted away from the original fair employment standards embodied in the state laws and in Title VII of the 1964 Civil Rights Act (Belz 1991, Epstein 1992, Moreno 1997). On the other hand, if the adoption of anti-discrimination legislation contributes to a rapid shift in firms' employment policies, or bolsters boycott movements, or alters social norms regarding discrimination, then the effect of a fair employment law could be far larger than suggested by, for example, the number of firms investigated by fair employment agencies.

4. Measuring the Fair Employment Effect

In 1964, Herbert Hill, the labor secretary of the NAACP, alleged that the state fair employment agencies acted with "timidity and a general reluctance to broadly and rapidly enforce antidiscrimination statutes"and that ultimately the "state FEPC laws have failed" (1964, pp. 68, 23).[9] However, several studies have argued to the contrary. Bonfield (1967) admitted that the laws were not "panaceas," but asserted that "There is little doubt that they were responsible for some

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[9] Hill's argument is based on three varieties of evidence: blacks' earnings and employment levels still lagged those of whites by wide margins even in states with FEP laws; the state commissions dismissed a substantial portion of discrimination complaints for lack of evidence; and several supporting anecdotes.

substantial employment gains by minority group members during this period and for the eradication of significant amounts of discrimination by employers, labor organizations, and employment agencies" (p. 1076). Liggett (1969) found evidence of a positive correlation between blacks' occupational upgrading in the 1950s and the presence of fair employment agencies. Norgren and Hill (1964, pp. 114-130) cited both the number of cases settled by the committees and positive changes in the occupational and industrial distributions of black workers in fair employment states. As Lockard (1968, p. 94) pointed out, however, these comparisons are rough, and they fall short of a convincing measure of the policy's impact.[10]

Landes (1968) offered the first regression-based estimates of the fair employment laws' impact. With each state serving as an observation, he ran a cross-section regression of nonwhite/white average weekly wages in 1959 on a fair employment law dummy variable and some pertinent state characteristics. This revealed a positive, though weak, correlation between fair employment laws and nonwhites' relative income.[11] Recognizing that the examination of a single year's cross section is a dubious route to the identification of a policy effect, Landes then examined the change in nonwhite/white male annual income between 1949 and 1959, and again he found a modest, positive correlation with the presence of fair employment laws (pp. 534-536). He also examined the change in relative unemployment rates for men between 1939 and 1959 and found that over the 1950s, blacks fared worse (compared to whites) in fair employment states than in non-fair employment states (pp. 538-540).

Stigler (1973) and Heckman (1976) pointed out that the presence of a fair employment law in 1959 ought not be viewed as an exogenous state characteristic, especially in cross-section estimation. Heckman went on to use the Landes paper to motivate the development of an econometric approach to dealing with such endogeneity. Then, he estimated by maximum likelihood a two-equation

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[10] Lockard goes on to make comparisons of blacks' occupational improvement between 1950 and 1960 across 20 non-southern cities. "They by no means support the thesis that the existence of FEP is a decisive force in determining Negro employment opportunities" (1968, p. 95)

[11] This approach yields a coefficient on the fair employment dummy of about 0.03 with a standard error of about 0.02 (Landes 1968, p. 516). Using annual earnings or income as the dependent variable delivers much smaller coefficients (results available from author upon request). Landes undertakes similar analyses of occupational status and explores alternative measures of fair employment enforcement.

system in which the first equation describes favorable "sentiment" towards blacks; this sentiment, in turn, determines the presence or absence of a fair employment law. The second equation is similar to the cross-section estimated by Landes for nonwhite/white male weekly wages in 1959, but Heckman's approach accounts for correlation between the error terms of the two equations. This adjustment led to an estimate of the laws' effect that is more than double the size of that from Landes's 1959 cross-section (Heckman 1976, p. 256-264).

4.1 Empirical Strategy

This paper's approach to estimating the fair employment impact differs significantly from Landes (1968) and Heckman (1976) in both its empirical strategy and its data. I employ a difference-in-difference-in-difference (DDD) regression framework and individual-level census data from the IPUMS to identify the policy impact (Ruggles and Sobek 1997).[12] Essentially, the idea is to use three dimensions of comparison to establish a clean measure of the policy's impact. The framework compares native-born black and white outcomes (one "difference"), in states with and without fair employment laws (a second "difference"), over time (the third "difference").[13] Putting them all together, one emerges with an estimate of the difference between the change in the racial outcome gap in states that adopted laws and those that did not, controlling for each worker's characteristics.

Each of the DDD regressions pools data from two census cross-sections (1940-1950, 1950-1960, or 1940-1960) and takes the following general form:

$$Y_{ijt} = \alpha + \beta_1 X_{ijt} + \beta_2 Black_i + \beta_3 FE_j + \beta_4 Year_t$$
$$+ \beta_5 (Black_i \times FE_j) + \beta_6 (Black_i \times Year_t) + \beta_7 (FE_j \times Year_t) + \beta_8 (Black_i \times FE_j \times Year_t).$$

Y is a labor market outcome variable; i indexes individuals, j indexes the two groups of states (those with and those without fair employment laws), and t indexes the two census years; X is a vector of

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[12] See Besley and Case (1994) and Meyer (1995) for discussions of this approach. See Card (1992), Gruber (1994), and Moehling (1999) for applications to minimum wages, maternity benefits, and child labor respectively.

[13] The fair employment coefficients are very similar if the foreign born are included in the samples; they are also similar if the specification includes interaction terms between race and personal characteristics.

personal characteristics (age, education, marital status, metropolitan residence); *Black* is a dummy variable equal to one for blacks; *FE* is a dummy variable equal to one for states that adopted a fair employment law between the first and second census date; and *Year* is a dummy variable equal to one for the later census year in the comparison.

I measure the fair employment effect on a variety of labor market outcomes including annual income, unemployment, labor force participation, occupational status, and industrial distribution. Unlike previous studies of fair employment, I report separate estimates for men and women (ages 20-59). Also, in case young workers are more strongly affected by such legislation than older workers (perhaps because they are more mobile between industries and occupations), I provide separate estimates for young men and women (ages 20-34).

In this framework, β_2 reflects time-invariant and state-invariant differences between blacks and whites; β_3 reflects time-invariant and race-invariant differences between fair employment states and non-fair employment states; β_4 captures race-invariant and state-invariant changes over time; β_5 captures time-invariant differences between blacks and whites in fair employment states relative to non-fair employment states; β_6 reflects state-invariant changes in blacks' labor market outcomes relative to whites'; and β_7 reflects race-invariant changes over time in the fair employment states relative to non-fair employment states. The coefficient of particular interest (β_8) is on the triple interaction of $Black_i \times FE_j \times Year_t$ which measures the change in blacks' outcomes relative to whites' outcomes in fair employment states relative to non-fair employment states.

To interpret β_8 as a clean measure of the fair employment policy effect, one must assume that there were not unobserved shocks or trends that differentially affected fair employment states compared to non-fair employment states *and* differentially affected blacks compared to whites. This assumption is most likely to hold when the groups of states being compared are geographically proximate and/or industrially similar and therefore subject to common shocks and trends. So, when feasible, I assemble the comparison groups accordingly, and I return to these issues in discussing the results. Moreover, one must assume that there are not offsetting migration responses to the passage of fair employment legislation. In perfectly integrated labor markets, an improvement in employment opportunities in one place will provoke an arbitraging migration flow; if so, then β_8 will

not detect any fair employment effect. I explore the data for migration responses later in the paper.

The first two sets of DDD estimates (1940-1950 and 1950-1960) focus on the Northeast and Midwest. For the 1940s, I use New York, New Jersey, Connecticut, and Massachusetts as the fair employment states. All of these states adopted fair employment laws between 1945 and 1947. The control states are Pennsylvania, Ohio, and Michigan, all of which adopted fair employment laws after 1950. The initial 1940 characteristics suggest that this is a reasonable collection of states for comparison. Blacks comprise 3.9 percent and 4.9 percent of the sample population in the fair employment and non-fair employment states respectively. Unemployed workers comprise 10.8 percent (fair employment) and 9.9 percent (non-fair employment) of the state-groups' labor forces. Manufacturing workers comprise 31.4 percent (fair employment) and 34.9 percent (non-fair employment) of the state-groups' employed workers.

For the 1950s, Pennsylvania, Michigan, and Minnesota, all of which adopted laws in 1955, are the fair employment states, and this group is compared with Illinois, Indiana, and Iowa, none of which adopted laws before 1960.[14] The initial 1950 characteristics are as follows. Blacks comprise 6.1 percent of the sample's population in both fair employment and non-fair employment states. Unemployed workers comprise 4.1 percent (fair employment) and 2.6 percent (non-fair employment) of the state-groups' labor forces. Manufacturing workers comprise 35.8 percent (fair employment) and 32.0 percent (non-fair employment) of the state-groups' employed workers.

For the full 1940 to 1960 period, the DDD estimates are derived from a larger and more heterogeneous sample of non-southern states. Ordinarily, a broader sample would be helpful for purposes of measurement, but in this case the benefits are not clear. The non-fair-employment states differ systematically (geographically, industrially, racially, politically) from the fair-employment states. This strains the DDD assumption that there are no unobserved and confounding trends or shocks.[15] Consequently, throughout the paper, I emphasize the estimates derived from the

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[14] I considered including Ohio in the non-fair employment group, but it did adopt a law in 1959 and so the timing is a problem. Nonetheless, reviewing preliminary regressions, the results do not appear to be affected by Ohio's inclusion.

[15] There are 18 states in the 1940-1960 DDD estimates. The coverage of the metro status variable in the IPUMS microdata requires dropping several non-southern states. I also dropped states that adopted laws in the late 1950s because it was unclear where they should be grouped. See the notes to

comparison groups for 1940-1950 and 1950-1960 separately, though I do provide the full 1940-1960 estimates for the sake of comparison. Summary statistics for are reported in appendix tables A1-A3.

The quality of the estimates derived from these cross-state comparisons depends on the exogeneity of the timing of fair employment adoption with respect to changes in the relative labor market outcomes of blacks and whites. It is not difficult to imagine, however, that the laws were endogenous in one way or another. For example, perhaps states passed the laws in response to a relative decline in the economic fortunes of African Americans, thereby confounding the interpretation of β_8 as a measure of the fair employment policy impact. In general, dealing with such an endogeneity-of-law problem requires some understanding of the factors that determine the timing of a law's adoption. In the case of fair employment laws, Collins (2000a) estimates duration models to measure how a variety of economic, demographic, and political variables influenced the timing of adoption. Within this framework, the coefficient on a variable for blacks' unemployment rate relative to whites' in 1950 does not support the hypothesis that the likelihood of passage increased as black workers' relative outcomes worsened. Moreover, the numerous historical accounts of the campaigns for fair employment adoption do not suggest that changes in the economic well-being of blacks relative to whites played a role in determining the timing of adoption (Cartwright 1948, Goldstein 1950, Bonfield 1967, Lockard 1968, Gray 1970).

A more plausible, but less easily tested, confounding hypothesis is that the states that passed fair employment laws were subject to larger declines in discriminatory attitudes over time than other states, and that these unobserved changes made their own contribution, separate from the legislation, toward relative improvements in black labor market outcomes. Thus, the policy effect estimates reported below may tend to have a positive bias because they reflect both the effect of the law *per se* on outcomes as well as the effect of weakening, but unobserved, discriminatory attitudes.[16] There

Table 1 for details.

[16] I have attempted to use plausibly exogenous variation in the predicted time of fair employment adoption to select states for comparison. I used an index of state-level inter-party political competition and the proportion of the population that was Jewish to predict times of adoption (Ranney 1965, National Council 1956). Then, for comparison, I pared down the existing DDD exercises to states for which the predicted time of adoption was in the same decade as the actual time of adoption. Only New York is predicted to have adopted in the 1940s. Comparing New York with Pennsylvania, Michigan, and Ohio in the 1940s DDD framework produces results similar to those reported in the text. Connecticut, Illinois,

are, however, several conceptual problems with this hypothesis. First, even supposing that popular and political support for a piece of legislation are directly linked and that the same *level* of support has to be met for passage in each state, it does not follow that a given state's passage necessarily implies that the *change* in support in that state was different from the *change* in support in states that did not pass a law. Second, although one might hope that a democratic system would effectively link popular support with legislative outcomes, it is abundantly clear that this link is neither direct nor especially strong because of the committee structure of legislative bodies, the dynamics of interest-group competition, the potential for log-rolling across legislative issues, and so on. Finally, supposing that differing times of adoption really do reflect differential changes in political support, it does not follow that such changes have an influence on labor market outcomes which is independent of their influence on the legislation's passage. One reason that state laws often serve as useful "quasi-experiments" is that changes in law may be abrupt compared to changes in the underlying factors that influence a state's political economy (Meyer 1995). Nevertheless, because there is no fully satisfactory way to construct a measure of change in discriminatory attitudes, the hypothesis cannot be completely and empirically refuted.[17] Therefore, the potentially positive bias the hypothesis suggests should be kept in mind when interpreting the following results.

4.2 Annual Income

To maintain a comparable sample and set of variables over time, I use annual wage and

Massachusetts, New Jersey, and Pennsylvania are predicted to have adopted between 1950 and 1955; Ohio, Michigan, and Minnesota are predicted to have adopted between 1955 and 1960; and Indiana and Iowa are predicted to have adopted after 1960. However, comparing Pennsylvania, Michigan and Minnesota with Indiana and Iowa (but not Illinois) in the 1950s DDD framework 1950s is difficult because of the relatively small number of sample-line blacks in the 1950 microdata sample for Indiana and Iowa.

[17] Of course, using the change in the wage gap between blacks and whites to gauge changes in discriminatory attitudes is not helpful in this context. The Gallup Poll questions on employment discrimination which span this period are inconsistently worded, and the results are not reported at the state level (Gallup 1972, pp. 528, 748, 1083). Instead, I attempted to gauge changes in support by using roll-call voting records in the House of Representatives for the six states which enter the 1950-1960 DDD framework. A comparison of the proportion of votes favoring consideration of a fair employment bill in 1950 with votes favoring the Civil Rights Act of 1964 revealed no evidence that the change in sentiment was larger in the states that passed fair employment legislation (Rosenthal and Poole 1992).

salary income for workers who were 20 to 59 years old, who were not in school, who worked at least 40 weeks in the relevant year, whose earnings were at least half of the level implied by the minimum wage for a full-time worker, and who were employed, but not self-employed, at the time of the census. I focus on wage and salary income for non-self-employed workers because total income is not available in the 1940 census and because the laws pertain to employment relationships. These sample restrictions obviously exclude the unemployed, those tenuously attached to the labor force, the self-employed, and those out of the labor force all together. Later in the paper, when estimating the laws' effects on employment and labor force participation, I will use a much less restricted sample.

Column 1 of Table 1 reports DDD estimates for laws adopted in the 1940s, where New York, New Jersey, Massachusetts, and Connecticut are grouped together and designated the "experimental" states ($FE = 1$), and Pennsylvania, Ohio, and Michigan are the "control" states ($FE = 0$). The regression equations are similar in form to the one reported above, and the personal characteristics (X) include age (quartic), years of education (quadratic), metropolitan residence, and marital status. The coefficient of interest (reported in Table 1) is for the triple interaction of the race, fair employment, and 1949 dummies ($Black \times FE \times 1949$). It should be noted that the federal FEPC operated in all these states during World War II, and so distinguishing a separate state FEPC effect might be difficult. Nevertheless, the results in column 1 are fairly supportive of the notion that state fair employment laws had a positive impact on the relative income of black workers in the 1940s. Though not strongly statistically significant, the estimate for all men suggests a 5 percent improvement associated with fair employment laws, and that for young men suggests a 7 percent improvement. The estimated impact on the relative income of black women is especially strong at approximately 12 percent.[18] In the fair employment states in 1940, the black-white gap in average log income was 0.48 for men and 0.49 for women (see Appendix Table A1), and so, the estimated

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[18] For the most part, these results are not driven by changes in the industrial distribution of black workers. Adding indicator variables for manufacturing and personal service industries to the income regressions slightly lowers the estimated fair employment impact for black women (from 11.7 to 8.2 percent) and slightly raises the estimated impact for men (from 5.3 to 5.8 percent).

fair employment impact is non-trivial relative to the size of the initial gaps.[19] It appears even more

impressive relative to the change in the income gaps between 1940 and 1950 (narrowing by 0.14 for

men and 0.24 for women in the fair employment states).

Column 2 of Table 1 presents DDD estimates of the effect of state laws adopted in the 1950s.

In this case, I designate the three states that adopted laws in 1955 (Pennsylvania, Michigan, and

Minnesota) as the "experimental" states and three states that did not adopt laws before 1960 as the

"control" states (Illinois, Indiana, Iowa). For the laws adopted in the 1950s, there is no support for

the hypothesis that the laws made a substantial contribution to the improvement of black workers'

relative income. In fact, young black men in the fair employment states appear to have fared

substantially worse relative to their white counterparts than young black men in the non-fair

employment states over the course of the decade. In three of the four comparison groups of young

men (blacks in non-FE states, whites in FE states, whites in non-FE states), average nominal income

rose by 60 to 63 percent; but for the young black men in fair employment states, average incomes

increased by only 46 percent.

Column 3 presents a DDD estimate covering the full twenty year period for a broader sample

of states than in columns 1 and 2.[20] Workers residing in non-southern states which had adopted fair

employment laws before 1957 are assigned to the experimental group. Those residing in non-

southern states which did not adopt the laws by 1960 are assigned to the control group. Those

residing in states which adopted the laws between 1957 and 1960 are excluded because it is not clear

that the laws would have had time to affect outcomes (Wisconsin, Colorado, California, and Ohio).

As with the estimates for the 1950s, the coefficient estimates for the full twenty year period detect

virtually no evidence of a positive fair employment effect on African Americans' relative income.

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[19] The "adjusted" initial gaps in 1940 for the fair employment states (accounting for differences in age, education, and so on) are reflected in the regressions underlying Table 1 and are 0.37 for men and 0.28 for women.

[20] Because of limitations on the metropolitan status variable in 1960, some states are entirely and automatically dropped from the regressions in that year (generally, states with very small metropolitan populations). I have trimmed the 1940 sample accordingly so that the same states are included in 1940 and 1960. See the notes to Table 1 for details.

4.3 Unemployment and Labor Force Participation

Table 2 examines the impact of fair employment laws on the likelihood of unemployment (Panel A) and labor force participation (Panel B) using DDD specifications of probit models. Column 1 reports the DDD estimates for the fair employment effect in the 1940s. For both men and women, there appears to have been no effect on the relative likelihood of unemployment or labor force participation. Thus, the income gains for *employed* black workers recorded for the 1940s in Table 1 were not offset by losses in employment, nor were they related to withdrawal from the labor force by other workers. That is, during the 1940s, the fair employment laws did not induce a tradeoff between blacks' relative income and relative employment.

Again, however, the story that emerges for black men in the new fair employment states of the 1950s is not nearly as positive as that for the 1940s. Column 2 of Panel A suggests a substantial positive effect on black male unemployment, though there is no adverse labor force participation effect in Panel B. Column 3 does not find evidence of a significant impact on unemployment or labor force participation for all men between 1940 and 1960, but in terms of unemployment, young black men again tended to fare relatively poorly in fair employment states.

The story for black women in the 1950s is quite different from that for men. There is no apparent adverse effect on black women's unemployment and a substantial positive impact on labor force participation.[21] Furthermore, the results from Table 1 indicate that this relative expansion of black women's employment in the fair employment states of the 1950s did not come at the expense of lower relative earnings. Over the full twenty year period, and with the broadest possible sample of states (column 3), the results for black women's employment are not quite as strong as in column 2, but they tell a similar story: a positive fair employment effect on labor force participation with no offsetting decline in income (Table 1) or increase in unemployment.

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[21] There is a positive impact (7.3 percent) on labor force participation even when married women are excluded from the sample. Thus, the result is not simply a reflection of married black women entering the labor force in response to the relative increase in black men's unemployment, perhaps to maintain family income.

4.4 Discussion of Income and Employment Results

In light of the results so far, was Herbert Hill's criticism of the fair employment approach on target? The evidence is decidedly mixed. Contrary to Hill's argument, the laws appear to have had a non-trivial impact on the racial income gap during the 1940s, especially for women. In line with Hill's contention, however, there is no evidence that black workers did especially well in the fair employment states studied in the 1950s.

The differing results from the 1940s and 1950s comparisons could be related to a number of factors, but the two most compelling, and to some extent testable, hypotheses concern changes in the tightness of labor markets over time, and differences in the strength of enforcement efforts across the fair employment states. If the wartime boom had substantially different amplitudes across the fair employment and non-fair employment states during the 1940s, and if blacks gained more than proportionately from it, and if those gains persisted through reconversion, then the apparent relative gains by blacks in the 1940s could reflect the differing booms across state groups, rather than the impact of state fair employment laws. Similarly, it is possible that differential business cycle conditions across the state groups in the 1950s could influence the 1950s DDD exercise.

At first blush, and focusing on the endpoints of the decade, the premise of a wartime boom that was substantially uneven across state groups seems unlikely. For example, the unemployment rate for white men, a reasonable barometer of business cycle conditions, fell by six percentage points in both the fair employment and the non-fair employment states during the 1940s (Table A1). Likewise, a differential business cycle is not apparent in the 1950s comparisons. Over the course of the 1950s, the unemployment rate for white men rose by 1.5 percentage points in the fair employment states and by 1.3 percentage points in the non-fair employment states (Table A2).

Zeroing in on the wartime experience allows for different perspectives on the boom's amplitude. The cumulative value of war contracts (through June 1944) was approximately $1850 per person in the fair employment states and $2110 per person in the non-fair employment states.[22] Thus, the implied difference in the wartime boom across the two groups of states is relatively small,

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[22] War contract data are from a <u>Summary of War Supply and Facility Contracts by Industrial Area</u> (1944, Table 2). Population figures are from <u>Historical Statistics</u> (Series A195-209).

and it would appear to work to the advantage of the non-fair employment states in the DDD calculation.

However, it is possible that African Americans in the fair employment states benefitted from more frequent federal FEPC intervention during the war. Accurately interpreting caseload data is notoriously difficult, and so the following figures should be viewed as suggestive evidence, rather than proof positive, of differing degrees of federal intervention. From July 1, 1943 to June 30, 1944, 1.2 cases were docketed per 1,000 blacks in the fair employment states, compared to 0.8 cases in the non-fair employment states.[23] Largely on the basis such caseload data's correlation with black/white employment ratios, Collins (2001) argues that intervention by the federal FEPC did improve blacks' employment opportunities in war-related industries, at least outside the South.

An alternative hypothesis for the differing magnitudes of the impacts across decades is that the state agencies' enforcement efforts might have varied across fair employment states, even when the form of the legislation itself looked very similar. Given the relatively high-esteem in which the fair employment literature holds the New York State commission, this hypothesis seems entirely plausible. By 1964, the New York Commission was the largest, best funded, and most experienced of the state commissions (Lockard 1968, p. 86; Norgren and Hill 1964, p. 100). However, the fair employment states used in the 1950 DDD exercises were not starved for resources. In fact, in 1959, Pennsylvania and Michigan had the largest fair employment budgets after New York, though the gap was large (Landes 1966, Table 4). Furthermore, although caseload data might not accurately reflect the impact of a law (for example, if firms conform to the law before being prodded by the enforcement agency), the data do provide a measure of the agencies' level of activity. Between 1956 and 1963, Pennsylvania's agency satisfactorily adjusted 0.106 cases per thousand African Americans per year (on average). From the mid-1940s to 1963, New York and New Jersey satisfactorily

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[23] The federal FEPC docketed most of its cases during the 1943-44 period for which state-level caseload data are available from the agency's microfilmed records (John Davis files; see Friend 1970). The state-level data do not report the proportion of complaints coming from blacks relative to other groups, but the FEPC's First Report indicates that Jews filed about 16 percent of the complaints in New York, a much larger proportion than elsewhere (Appendix E, Table 2C). Thus, black caseload gap would be narrowed somewhat by subtracting cases submitted by non-African Americans.

adjusted 0.061 and 0.066 cases per thousand blacks per year.[24] Shifting from a "per black person" to

a "per firm" basis does not reverse this finding regarding the activity of the agencies.

Two points should be taken from the state agencies' caseload data. First, it would be difficult

to argue on the basis of the existing caseload data that the lack of impact in the 1950s (compared to

the 1940s) was due to a relative lack of effort. Second, for the laws to have had a substantial impact

on African-American labor market outcomes, there would have to have been substantial spillover

effects coming from the enforcement efforts (that is, more than a single black worker would have to

be made better off from each "adjusted" case), or there would have to have been a substantial

number of firms and unions voluntarily complying with the laws. Further research on such issues,

perhaps at the firm level, will be very helpful but lies beyond the scope of this paper.

Given the decline in unemployment rates over the 1940s and the rise in unemployment rates

over the 1950s, it is tempting to assert that the different fair employment effects across the decades

were influenced by different business cycle conditions. Although it is certainly plausible that

antidiscrimination laws have bigger impacts when labor demand is high, it should be kept in mind

that the state fair employment laws were passed *after* the war, and therefore were in effect during the

period of reconversion (as unemployment was rising).[25]

The differing results for black men and women that emerge from the DDD estimates are also

worthy of further consideration. Gender discrimination was not covered in the original fair

employment laws, and so it is not obvious why black women might benefit more from their passage

than men. Nonetheless, a number of hypotheses do suggest themselves. If white employers or

employees had attitudes towards black women which were less discriminatory than their attitudes

towards black men, then a given dose of fair employment enforcement effort might improve black

women's relative position by more than men's. Along these same lines, if union obstacles to black

employment gains were more prevalent for "men's work" than for "women's work" we might expect

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[24] Calculations are made using the caseload data and population figures reported by Lockard (1968, pp. 91, 86). Comparable data are not reported for Massachusetts and Michigan. Minnesota (0.193) and Connecticut (0.308) have higher figures, but these have little impact on the weighted average within the state groups.

[25] New York State did have a "Committee on Discrimination in Employment" that attempted to discourage discrimination during the war (Reed 1991, pp. 31, 209, 211).

black women to gain ground faster than black men. Finally, given the initial prevalence of domestic service work among black women, occupational and industrial movement for black women might have been associated with relatively large income gains compared to occupational and industrial movement for black men (Cunningham and Zalokar 1992).

5.0 Occupational and Industrial Redistribution

The occupational and industrial redistributions of workers during this period are interesting in their own right. First, to a large extent the campaigns for fair employment legislation and the activities of the state agencies were geared towards moving black workers into occupations and industries which had long-standing barriers to their entry. Thus, in a sense, studying the reallocation provides a more direct view of the fair employment impact than the study of income and employment levels. Second, these movements might have economically important effects which are not reflected in income or unemployment differentials. For example, the redistribution could lead to improvements in non-pecuniary job characteristics, to the extension of networks both up the occupational hierarchy and across the industrial spectrum, and to a wider range of job choices which has value whether or not it leads to higher average incomes.

5.1 Occupational Status

Table 3 investigates changes in occupational status from three vantage points. In panel A, I use the natural log of the IPUMS *occscore* variable as a simple index of each individual's occupational status. The *occscore* measure is based on the median income earned in 1950 by all workers within each three-digit occupational category. In panel B, I estimate the fair employment effect on the likelihood of employed workers being craftsmen or operatives. The movement of black workers into the semi-skilled or skilled operative and craftsman categories was an important aspect of African-American economic progress between 1940 and 1960 (Maloney 1994, Margo 1995, Collins 2000b). The regressions in panel B shed light on whether or not this redistribution was promoted by state-level anti-discrimination laws. Finally, in panel C, I estimate the policy effect on the likelihood of workers holding clerical occupations. The movement of black workers out of blue-

collar and into white-collar work is significant not only because income gains might accompany such moves, but also because white-collar work generally has fewer disamenities than blue-collar work. Furthermore, a relative expansion of white-collar opportunities for black workers in fair employment states might signal an effective campaign against long-standing social norms that excluded blacks from such occupations (Governor's Commission 1953, Whatley 1990, Sundstrom 1994). Of course, occupational and industrial redistributions often go hand in hand, and so the paper's next section examines the industrial side of the story.

The most striking results from panel A of Table 3 are the large relative gains in occupational status by black women working in fair employment states in the 1940s, 1950s, and over the full 1940 to 1960 period. In general, these gains are substantially larger than those found for actual income, which likely reflects movement out of occupational categories where black women made up a significant proportion of the workers (e.g., household service) into occupational categories where the *occscore* value is more influenced by men's earnings (e.g., operatives).[26] In comparison with the results for women, the estimated fair employment impacts for men in columns 1 and 2 of panel A are less impressive, even if taking account of the fact that the initial gap in occupational status for men was smaller than for women. With the broader sample of states covering the full 1940-1960 period (column 3), however, the coefficient does suggest a substantial degree of relative improvement.

Panel B reports estimates of the policy effect on the likelihood of employment as an operative or a craftsman. In the 1940s (column 1), the effect for men is positive but small and statistically weak. The effect for women, on the other hand, is very strong, a finding that will also be reflected in the next section's study of the probability of manufacturing employment. The results for the 1950s (column 2) faintly echo those for the 1940s – there is no evidence of a positive fair employment impact on black men's employment in the operative and craftsman categories, but there is support for a positive effect on black women's employment in those categories. The 1940-1960 results suggest a strong impact for both men and women, but again, the 1940-1960 estimates emerge

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[26] The difference between the income and the *occscore* results is not explained by excluding those who worked less than 40 weeks, were self-employed, or earned less than one half of the implied minimum annual earnings from the *occscore* regressions (to make the sample more similar to that used for the income regressions).

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from comparisons across state groups that are different from one another in many dimensions, and the DDD framework might be inadequate for dealing with those differences.

Although much of the fair employment literature, including this paper, focuses on access to blue-collar occupations, it is evident that discrimination in white-collar jobs was very common at mid-century. For example, a study by Pennsylvania's Governor's Commission on Industrial Race Relations (1953) found that 88 percent of 1,137 firms providing information discriminated against racial or religious minorities in "office occupations" (compared to 37 percent in unskilled occupations).[27] Nonetheless, Sundstrom (2000) highlights the magnitude and economic importance of black women's movement into the clerical sector between 1940 and 1980. The role of state-level fair employment laws in facilitating any such movement between 1940 and 1960 is explored in Panel C. For women, there is no evidence of a fair employment effect in the states selected for comparison for the 1940s, there is only weak support for a positive impact in the 1950s framework, but somewhat stronger support emerges from the 1940-1960 regressions in column 3. For men, there is no evidence of a positive fair employment impact on the likelihood of employment in clerical occupations.

5.2 Industrial Distribution

To a large extent, black inroads into manufacturing before the 1940s came during World War I or as replacement workers when whites went on strike (Whatley 1990, 1993). Historically, manufacturing jobs were viewed as "good" jobs (relative to the alternatives) by black workers, but it is not known whether the state-level fair employment laws eased blacks' entry into the sector. Panel A of Table 4 reports a strong positive effect on women's likelihood of manufacturing employment in all three columns, especially for the 1940s when the proportion of employed black women in manufacturing jumped from 7 to 27 percent in the fair employment states compared to an increase

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[27] Surveys conducted in five cities in 1940 by the Women's Bureau of the Department of Labor found that about 50 percent of firms had policies against hiring black clerical workers (Goldin 1990, p. 147).

from 10 to 15 percent in the non-fair employment states.[28] The vast majority of women in manufacturing industries were classified as operatives, and so the results here are closely connected to those for female operatives in the previous table. The estimated effect for men is very small in the 1940s, slightly stronger in the 1950s, and stronger yet in the full 1940-1960 DDD framework. Thus, on the whole, there is a good deal of evidence consistent with the hypothesis that state-level anti-discrimination laws facilitated the movement of black workers, especially women, into manufacturing jobs.[29]

The rapid decline in the proportion of black female employment in personal service is among the most remarkable features of labor market reallocation in the post-1940 period. Sundstrom notes that between 1940 and 1980, the proportion of black female workers employed as domestic servants fell from 58 percent to 6 percent (2000, p. 1). The high proportion of black women in such jobs in 1940 was certainly not a southern peculiarity: in the states selected here for comparison in the 1940s DDD regressions, over 70 percent of employed black women worked in personal service industries (in 1940), primarily in household service. Did fair employment laws accelerate the decline blacks' employment in personal service industries? For women in the 1940s there is evidence consistent with a substantial fair employment effect, but the evidence in much weaker in columns 2 and 3, and there is no such evidence for men's employment in service.

Finally, panel C reports estimates of the policy effect on (non-military) government employment, a sector which experienced a substantial increase in its proportion of African-American workers after 1940. *A priori*, the effect of fair employment legislation on blacks' employment in government is ambiguous. On one hand, we might expect the government to follow fair employment policies more strictly than the private sector, thereby drawing a relatively high proportion of blacks into government employment. On the other hand, effective fair employment legislation might have opened better paying opportunities for blacks outside of government, and anti-discrimination policy

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[28] There were large increases in black women's manufacturing employment in all four of the fair employment states.

[29] Along these lines, an industrial breakdown of caseload data for New York State from 1945-1961 indicates that about 34 percent of employer-targeted complaints (and cases resolved) pertained to manufacturing, by far the single largest category (Higbee 1966, pp. 364, 372).

covering government employment sometimes preceded the passage of fair employment laws (Bonfield 1967). In panel C of Table 4, the estimated effects on black women's likelihood of government employment are negligible in all cases. For men, the estimated effects are negligible in both the 1940s and 1950s DDD frameworks, but somewhat stronger (and negative) in the 1940-1960 estimates. In any case, the state-level fair employment laws do not appear to have crowded blacks into government employment.

6. Migration

To this point, the empirical approach has treated each state-group as a separate labor market and attempted to identify a fair employment effect by making comparisons across those labor markets. Clearly, however, African-Americans were very mobile between 1940 and 1960, the two greatest decades of the Great Migration. To the extent that migration patterns are sensitive to differences in economic opportunities, mobile labor may arbitrage away gaps across locations, confounding the DDD identification strategy.

However, states that passed fair employment laws did not experience relatively large increases in their black populations. In fact, the fair employment groups of states had slightly smaller increases in their black/white population ratios than the non-fair employment states in both the 1940s and 1950s. Thus, it seems unlikely that the potential labor demand-side impact of fair employment laws was offset by a relatively strong migration response to improved opportunities in fair employment states. More formally, using the familiar DDD framework, panel A of Table 5 shows that, in general, the relative probability of being an interstate migrant (i.e., resident in a state that is not one's state of birth) is not positively associated with the adoption of fair employment laws.

A different test of the migration-response hypothesis is reported in panel B of Table 5. I assembled a panel of state-level data for each census period from 1900 to 1960, covering all important destinations for black migrants. Then, I regressed the net black immigration rate (net black in-migration expressed relative to the state's total population) for each state-decade observation on a series of state and period fixed effects, plus a dummy variable which is equal to one for state-decade observations with fair employment laws in place. The state fixed effects should

account for time-invariant state characteristics (such as distance from the South, and persistent differences in industrial structure and transportation networks). The time fixed effects should account for state-invariant time-series influences (such as declining transportation costs and macroeconomic demand shocks). The fair employment dummy variable then reflects any change in the black migration rate that coincides with the initiation of fair employment laws.

First, I run the regressions for the industrial states of the Northeast and the Midwest, the most important receiving states of black migrants throughout the period. Then, I add to the sample several states which received substantial inflows of black migrants in the 1940s and 1950s. In short, there is no evidence that the fair employment laws elicited a substantial migration response from African Americans.

7. Conclusion

State-level fair employment laws were among the earliest legislative achievements of the Civil Rights Movement, and by the time the Civil Rights Act of 1964 was passed, nearly all African-Americans outside the South were already covered by this anti-discrimination legislation. The symbolic and political importance of these laws is reasonably clear: for the first time, state governments promised to protect minority workers from discriminatory treatment in the private sector. The economic importance of the laws, however, has long been in dispute, and it has been nearly 25 years since economists have weighed the evidence. The dearth of economic inquiry into these laws is unfortunate because an accurate understanding of their labor market impact could provide a better understanding of the Civil Rights Movement's contributions to black economic progress, and could be informative for current discussions of affirmative action policy.

This paper takes a new approach to the measurement of the fair employment policy impact and offers a broader view of labor market outcomes than previous studies. The earliest fair employment laws, which were adopted in the 1940s, appear to have improved the relative income of black workers. They certainly did not eliminate the racial income gap between otherwise observationally similar workers, but these income gains were not trivial either. Moreover, the positive relative income effect was not offset by worsening relative unemployment or labor force

participation rates in the 1940s.

The laws which were adopted during the 1950s, however, appear to have had a much less positive impact than those in the 1940s even though, in general, the laws and enforcement agencies were similar in form. Evidence from the caseload volume does not support the view that the state agencies created in the 1950s were less active than their predecessors. But in any case, by the time the Civil Rights Act passed in 1964, the perception among activists that the fair employment approach was ineffective had firm and viable roots in the disappointing experience of the previous decade's new fair employment laws, particularly with respect to men's outcomes. Given this point of view, a view that focuses on labor market outcomes, the drive for, and continuing commitment to, more aggressive labor market intervention is not surprising.

Nonetheless, from a variety of perspectives, black women appear to have benefitted more from the laws than black men did. This is an especially intriguing result given the eventual collapse in the racial wage gap among women and the gap's persistence among men. In the 1940-1960 period, black women's advance is manifested most clearly in the rising proportion black women working as manufacturing operatives and the sharply declining proportion working as domestic servants. In moving beyond the findings reported in this paper, more detailed comparisons of black women's labor market experiences with black men's experiences may shed considerable light on how the contours of racial discrimination in labor markets have been reshaped over time.

<div style="text-align: center;">References</div>

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Table 1: DDD Estimates of Fair Employment Laws' Effect on Annual Income

	1	2	3
	1939-1949	1949-1959	1939-1959
Men, 20-59	0.0526	-0.0043	-0.0048
	(1.92)	(0.16)	(0.25)
Men, 20-35	0.0660	-0.0923	-0.0440
	(1.71)	(2.30)	(1.55)
Women, 20-59	0.1172	-0.0065	0.0166
	(2.87)	(0.13)	(0.60)
Women, 20-35	0.1153	-0.0037	0.0081
	(2.23)	(0.05)	(0.21)
Non-FE States	MI, OH, PA	IA, IL, IN	Non-South w/o law
FE States	CT, MA, NJ, NY	MI, MN, PA	All w/ law in 1955

Notes: The dependent variable is log annual wage and salary income for those who worked at least 40 weeks in the relevant year, who were not self-employed but were employed at the time of the census, and who were not in school. Each coefficient in the table is taken from a separate regression. The DDD coefficients are from ordinary least squares regressions which include the set of characteristics and interactions described in the text. t-statistics based on robust standard errors are in parentheses. Top-coded income is multiplied by 1.4. Samples exclude those earning less than half of the minimum wage on a full time basis (in 1940 the cutoff is $250; in 1950, $400; in 1960, $1000). Observations are weighted by the IPUMS person weights in 1940 and 1960 and the IPUMS sample line weights for 1950. In column 3, the same set of states are included for both 1940 and 1960; maintaining this comparability required discarding some states' observations for 1940 because the metro area variable is unavailable for some states (including Rhode Island and New Mexico which had FE laws). The "experimental group" in column 3 includes: Connecticut, Massachusetts, Michigan, Minnesota, New Jersey, New York, Oregon, Pennsylvania, and Washington. The "control group" includes: Idaho, Illinois, Indiana, Iowa, Kansas, Missouri, Nebraska, Vermont, and Wyoming. Ohio, California, Wisconsin, and Colorado are excluded from column 3 because they adopted laws in the late 1950s. Hawaii and Alaska are excluded throughout.
Sources: Census micro-data are from Ruggles and Sobek (IPUMS, 1997).

Table 2: DDD Estimates of Fair Employment Laws' Impact on Unemployment
and Labor Force Participation

	1	2	3
	1940-1950	1950-1960	1940-1960
Panel A: Unemployment			
Men, 20-59	0.0052	0.0388	0.0077
	(0.46)	(3.51)	(1.12)
Men, 20-35	0.0040	0.0583	0.0245
	(0.22)	(3.17)	(1.93)
Women, 20-59	0.0122	-0.0008	-0.0007
	(0.83)	(0.07)	(0.08)
Women, 20-35	0.0077	-0.0085	-0.0134
	(0.37)	(0.54)	(1.01)
Panel B: Labor Force Participation			
Men, 20-59	-0.0097	-0.0003	-0.0039
	(0.92)	(0.03)	(0.76)
Men, 20-35	-0.0109	-0.0059	-0.0090
	(0.71)	(0.35)	(1.32)
Women, 20-59	0.0057	0.0774	0.0323
	(0.21)	(2.56)	(1.85)
Women, 20-35	-0.0048	0.0812	0.0585
	(0.12)	(1.85)	(2.14)
Non-FE States	MI, OH, PA	IA, IL, IN	Non-South w/o law
FE States	CT, MA, NJ, NY	MI, MN, PA	All w/ law in 1955

Notes: The DDD coefficients are from probit regressions and expressed in dF/dx terms (evaluated at sample means): that is, the estimated fair employment effect on the probability of being unemployed (Panel A) or participating in the labor force (Panel B). z-statistics based on robust standard errors are in parentheses. The regressions control for the characteristics and interactions described in the text. Observations are weighted by the IPUMS person weights in 1940 and 1960 and the IPUMS sample line weights for 1950. See Table 1's notes for a list of states included in column 3.
Sources: Census micro-data are from Ruggles and Sobek (IPUMS, 1997).

Table 3: DDD Estimates of Fair Employment Laws' Impact on Occupational Distribution

	1 1940-1950	2 1950-1960	3 1940-1960
Panel A: Occupational Status			
Men, 20-59	0.0284 (1.43)	-0.0207 (1.08)	0.0689 (5.27)
Men, 20-35	0.0326 (1.16)	-0.0456 (1.65)	0.0780 (3.88)
Women, 20-59	0.2177 (4.88)	0.0654 (1.29)	0.1028 (3.49)
Women, 20-35	0.2790 (4.65)	0.1275 (1.94)	0.1467 (3.65)
Panel B: Operatives and Craftsmen			
Men, 20-59	0.0219 (0.71)	-0.0146 (0.44)	0.0997 (4.84)
Men, 20-35	0.0438 (0.96)	-0.0388 (0.78)	0.0777 (2.49)
Women, 20-59	0.2295 (5.53)	0.0662 (1.83)	0.0799 (3.57)
Women, 20-35	0.2753 (4.66)	0.0986 (1.96)	0.1044 (3.07)
Panel C: Clerical Workers			
Men, 20-59	0.0050 (0.25)	-0.0137 (0.89)	-0.0168 (1.51)
Men, 20-35	0.0465 (1.36)	-0.0370 (1.86)	-0.0104 (0.54)
Women, 20-59	-0.0254 (0.42)	0.0345 (0.54)	0.0768 (1.72)
Women, 20-35	0.0257 (0.31)	0.0517 (0.56)	0.1110 (1.84)
Non-FE States	MI, OH, PA	IA, IL, IN	Non-South w/o law
FE States	CT, MA, NJ, NY	MI, MN, PA	All w/ law in 1955

Notes: In Panel A, the natural log of the IPUMS occscore variable is the dependent variable, and the DDD coefficients are from ordinary least squares regressions. t-statistics based on robust standard errors are in parentheses. In Panel B and Panel C, the DDD coefficients are from probit regressions and expressed in dF/dx terms (evaluated at sample means): that is, the estimated fair employment effect on the probability of working in a particular occupational category, conditional on being employed. z-statistics based on robust standard errors are in parentheses. The regressions include the set of controls and interactions described in the text. Observations are weighted by the IPUMS person weights in 1940 and 1960 and the IPUMS sample line weights for 1950. See Table 1's notes for a list of states included in column 3.

Sources: Census micro-data are from Ruggles and Sobek (IPUMS, 1997).

Table 4: DDD Estimates of Fair Employment Laws' Impact on Industrial Distribution

	1 1940-1950	2 1950-1960	3 1940-1960
Panel A: Manufacturing			
Men, 20-59	0.0126 (0.42)	0.0405 (1.29)	0.0896 (4.60)
Men, 20-35	0.0360 (0.81)	0.0438 (0.93)	0.0904 (2.96)
Women, 20-59	0.2800 (5.79)	0.0949 (2.10)	0.1577 (5.07)
Women, 20-35	0.3261 (4.89)	0.1935 (2.88)	0.2394 (5.00)
Panel B: Personal Service			
Men, 20-59	0.0094 (1.32)	0.0085 (1.21)	0.0042 (1.09)
Men, 20-35	0.0039 (0.39)	0.0061 (0.59)	0.0072 (1.17)
Women, 20-59	-0.0599 (4.51)	-0.0116 (0.75)	-0.0195 (1.87)
Women, 20-35	-0.0588 (3.90)	-0.0304 (1.98)	-0.0261 (1.86)
Panel C: Government			
Men, 20-59	-0.0003 (0.02)	-0.0058 (0.53)	-0.0215 (3.48)
Men, 20-35	-0.0002 (0.01)	-0.0112 (0.91)	-0.0188 (2.37)
Women, 20-59	-0.0102 (0.87)	-0.0118 (1.00)	-0.0062 (0.63)
Women, 20-34	-0.0075 (0.56)	0.0056 (0.32)	-0.0075 (0.68)
Non-FE States	MI, OH, PA	IA, IL, IN	Non-South w/o law
FE States	CT, MA, NJ, NY	MI, MN, PA	All w/ law in 1955

Notes: The DDD estimates are from probit regressions and are expressed in dF/dx terms (evaluated at sample means): that is, the estimated fair employment effect on the probability of working in a particular industry, conditional on being employed. z-statistics based on robust standard errors are in parentheses. The regressions include the set of controls and interactions described in the text. Observations are weighted by the IPUMS person weights in 1940 and 1960 and the IPUMS sample line weights for 1950. See Table 1's notes for a list of states included in column 3. Personal service industries include service in private households, hotels, laundries, and so on.
Sources: Census micro-data are from Ruggles and Sobek (IPUMS, 1997).

Table 5: Migration Response to Fair Employment Laws

Panel A: Probits for Out-of-State Birth (DDD estimates)

	1940-1950	1950-1960	1940-1960
Men, 20-59	-0.0140	0.0072	-0.0449
	(0.54)	(0.23)	(2.60)
Men, 20-35	-0.0172	-0.0136	-0.0352
	(0.52)	(0.35)	(1.49)
Women, 20-59	-0.0064	0.0501	-0.0838
	(0.19)	(1.12)	(3.73)
Women, 20-35	-0.0325	-0.0063	-0.0554
	(0.83)	(0.12)	(1.88)

Panel B: OLS for State-Level Black Migration Rates, 1900-1960

	Industrial Sample: ln Migration Rate	Industrial Sample: Migration Rate	Broader Sample: ln Migration Rate	Broader Sample: Migration Rate
Fair Employment Coefficient	-0.0983	-3.4160	0.0913	-2.4570
	(0.42)	(1.30)	(0.38)	(0.84)
States	10	10	17	17
Observations	60	60	96	102
Mean of Dependent Var.	10.04	1.86	9.18	1.67

Notes: In Panel A., the reported coefficients are DDD estimates from probits for having a state of birth which is different from the current state of residence. The coefficients are expressed in dF/dx terms (evaluated at sample means): that is, the estimated fair employment effect on the probability of being an inter-state migrant. Regressions include the set of characteristics and controls described in the text. z-statistics based on robust standard errors are in parentheses. Observations are weighted by the IPUMS person weights in 1940 and 1960 and the IPUMS sample line weights for 1950. In Panel B, estimates of the fair employment coefficient from OLS regressions of the black migration rate on a complete set of time and state indicator variables and a fair employment indicator (equal to one when a law is in place before the end of the decade). t-statistics are in parentheses. The "Industrial Sample" corresponds to the industrial Northeast and Midwest: CT, IN, IL, MA, MI, NJ, NY, PA, OH, and WI. The "Broader Sample" adds AZ, CA, CO, MN, NM, OR and WA . In the broad sample, the net migration rate is sometimes zero, implying that the log of the rate is undefined. Trimming the last column's sample to match the previous column's (from 102 to 96 observations) does not have much impact on the results.
Sources: For Panel A, census micro-data are from Ruggles and Sobek (IPUMS, 1997). For Panel B, migration and population data for 1900-1950 are from Eldridge and Thomas (1964), and for 1950-1960 are from U.S. Department of Commerce (1975).

Table A1: Summary Statistics for 1940-1950 DDD Estimates

	Black Men		White Men		Black Women		White Women	
	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean
Log Income								
1940	6.814	6.815	7.290	7.217	6.364	6.277	6.858	6.703
	(0.432)	(0.428)	(0.549)	(0.535)	(0.392)	(0.370)	(0.501)	(0.493)
1950	7.734	7.787	8.073	8.058	7.374	7.242	7.646	7.553
	(0.354)	(0.378)	(0.461)	(0.432)	(0.424)	(0.467)	(0.406)	(0.428)
Unemployed								
1940	0.188	0.194	0.110	0.099	0.132	0.195	0.093	0.078
1950	0.112	0.100	0.047	0.040	0.079	0.093	0.039	0.026
Labor Force Participant								
1940	0.893	0.898	0.937	0.942	0.536	0.396	0.387	0.292
1950	0.849	0.873	0.918	0.923	0.518	0.386	0.384	0.328
Log Occscore								
1940	2.974	3.015	3.270	3.206	2.154	2.265	3.030	2.976
	(0.396)	(0.347)	(0.374)	(0.395)	(0.567)	(0.609)	(0.409)	(0.462)
1950	3.060	3.101	3.328	3.284	2.587	2.494	3.099	3.053
	(0.332)	(0.301)	(0.345)	(0.347)	(0.618)	(0.622)	(0.331)	(0.381)
Craftsmen and Operatives								
1940	0.257	0.276	0.384	0.433	0.129	0.167	0.253	0.250
1950	0.419	0.457	0.424	0.508	0.342	0.212	0.250	0.258
Clerical								
1940	0.051	0.025	0.125	0.087	0.026	0.019	0.332	0.256
1950	0.091	0.049	0.097	0.076	0.066	0.070	0.386	0.322
Manufacturing								
1940	0.141	0.279	0.315	0.363	0.069	0.101	0.318	0.296
1950	0.270	0.456	0.342	0.409	0.267	0.146	0.343	0.336
Personal Service								
1940	0.147	0.100	0.032	0.020	0.779	0.683	0.125	0.159
1950	0.102	0.058	0.024	0.018	0.464	0.502	0.059	0.073
Government								
1940	0.051	0.028	0.056	0.037	0.016	0.018	0.034	0.036
1950	0.068	0.041	0.062	0.042	0.036	0.056	0.038	0.036

Note: Standard deviations are in parentheses for variables that are not dummies.
Source: IPUMS (Ruggles and Sobek, 1997).

Table A2: Summary Statistics for 1950-1960 DDD Estimates

	Black Men		White Men		Black Women		White Women	
	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean
Log Income								
1950	7.813	7.812	8.039	8.061	7.249	7.372	7.544	7.600
	(0.368)	(0.348)	(0.446)	(0.472)	(0.485)	(0.442)	(0.435)	(0.438)
1960	8.295	8.323	8.590	8.619	7.800	7.903	8.024	8.057
	(0.372)	(0.373)	(0.446)	(0.450)	(0.440)	(0.404)	(0.422)	(0.426)
Unemployed								
1950	0.106	0.115	0.042	0.019	0.088	0.104	0.026	0.022
1960	0.136	0.085	0.057	0.032	0.113	0.124	0.052	0.038
Labor Force Participant								
1950	0.866	0.871	0.917	0.931	0.363	0.438	0.329	0.332
1960	0.867	0.871	0.938	0.944	0.476	0.479	0.390	0.411
Log Occscore								
1950	3.113	3.085	3.255	3.230	2.505	2.684	3.029	3.025
	(0.293)	(0.285)	(0.378)	(0.410)	(0.631)	(0.563)	(0.414)	(0.446)
1960	3.118	3.130	3.309	3.286	2.645	2.763	3.041	3.054
	(0.290)	(0.283)	(0.337)	(0.365)	(0.565)	(0.546)	(0.382)	(0.392)
Craftsmen and Opereratives								
1950	0.487	0.388	0.485	0.419	0.260	0.328	0.244	0.221
1960	0.442	0.388	0.457	0.426	0.202	0.222	0.197	0.178
Clerical								
1950	0.050	0.067	0.074	0.075	0.063	0.096	0.316	0.332
1960	0.063	0.100	0.071	0.071	0.127	0.146	0.313	0.357
Manufacturing								
1950	0.461	0.440	0.372	0.318	0.181	0.273	0.306	0.296
1960	0.409	0.356	0.407	0.358	0.155	0.180	0.269	0.264
Personal Service								
1950	0.050	0.063	0.020	0.018	0.511	0.389	0.078	0.083
1960	0.045	0.044	0.014	0.015	0.332	0.260	0.066	0.065
Government								
1950	0.035	0.049	0.040	0.040	0.045	0.039	0.035	0.034
1960	0.064	0.086	0.044	0.036	0.070	0.072	0.038	0.032
Inter-State Migrant								
1950	0.825	0.836	0.203	0.255	0.779	0.859	0.184	0.261
1960	0.744	0.770	0.203	0.270	0.724	0.771	0.201	0.269

Note: Standard deviations are in parentheses for variables that are not dummies.
Source: IPUMS (Ruggles and Sobek, 1997).

Table A3: Summary Statistics for 1940-1960 DDD Estimates

	Black Men		White Men		Black Women		White Women	
	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean	FE Mean	Non-FE Mean
Log Income								
1940	6.651 (0.506)	6.580 (0.512)	7.053 (0.650)	6.939 (0.673)	6.258 (0.407)	6.215 (0.456)	6.667 (0.563)	6.573 (0.539)
1960	8.180 (0.432)	8.218 (0.434)	8.552 (0.508)	8.556 (0.495)	7.775 (0.438)	7.772 (0.451)	7.973 (0.480)	7.936 (0.476)
Unemployed								
1940	0.199	0.159	0.107	0.070	0.157	0.170	0.089	0.058
1960	0.106	0.085	0.044	0.033	0.090	0.115	0.051	0.038
Labor Force Participants								
1940	0.901	0.887	0.951	0.949	0.472	0.396	0.340	0.275
1960	0.872	0.874	0.952	0.955	0.524	0.483	0.414	0.421
Log Occscore								
1940	2.992 (0.375)	2.974 (0.374)	3.219 (0.407)	3.102 (0.476)	2.200 (0.585)	2.330 (0.622)	3.001 (0.440)	2.960 (0.489)
1960	3.115 (0.300)	3.118 (0.284)	3.351 (0.321)	3.323 (0.341)	2.661 (0.582)	2.711 (0.558)	3.082 (0.341)	3.068 (0.366)
Craftsmen and Operatives								
1940	0.272	0.232	0.394	0.319	0.150	0.166	0.245	0.190
1960	0.420	0.376	0.435	0.441	0.223	0.199	0.188	0.174
Clerical								
1940	0.037	0.028	0.073	0.075	0.024	0.036	0.299	0.281
1960	0.079	0.094	0.080	0.073	0.131	0.128	0.361	0.363
Manufacturing								
1940	0.216	0.207	0.316	0.213	0.083	0.104	0.295	0.219
1960	0.341	0.336	0.383	0.354	0.189	0.155	0.277	0.253
Personal Service								
1940	0.124	0.117	0.026	0.021	0.742	0.656	0.142	0.177
1960	0.052	0.048	0.017	0.016	0.331	0.287	0.056	0.070
Government								
1940	0.041	0.033	0.047	0.037	0.015	0.018	0.035	0.035
1960	0.064	0.088	0.056	0.043	0.053	0.063	0.039	0.036
Inter-State Migrant								
1940	0.817	0.760	0.248	0.272	0.829	0.760	0.212	0.262
1960	0.735	0.746	0.248	0.308	0.725	0.740	0.245	0.299

Note: Standard deviations are in parentheses for variables that are not dummies.
Source: IPUMS (Ruggles and Sobek, 1997).