

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 differences-in-differences-in-differences framework. In general, the fair employment laws appear to have had small or negligible effects on the labor market outcomes of black men but somewhat stronger positive effects on the labor market outcomes of black women.

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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, we 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 (e.g., Hill 1964) which claimed that the fair employment laws were not effectively deterring discriminatory practices. 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 (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, Landes (1968) found a modest positive impact on relative wages for nonwhite males, but he also detected a modest positive impact on the relative unemployment rate. Using similar data but a different econometric technique, Heckman (1976) measured a somewhat larger wage effect but did not explore other labor market

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.

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 state 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 picture that emerges from the empirics 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. Third, the fair employment laws do not appear to have contributed substantially to improving the economic well-being of black men. At this point, it is difficult to discern whether this is due to the nature of labor market discrimination against black men, the construction of the laws and organization of the enforcement agencies, or a lack of resources devoted to the enforcement of the laws.

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

² Subsequently, Michigan (1955), Minnesota (1955), Pennsylvania (1955), Wisconsin (1957), Colorado (1957), California (1959), Ohio (1959), Illinois (1961), Kansas (1961), Missouri (1961), Hawaii

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 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 means of enforcement were, for the most part, quite similar because they emulated New York's law. In general, it became unlawful for employers, unions, or employment agencies to discriminate on the basis of race, religion, or national origin in decisions concerning employment, discharge, referral, compensation, or other conditions and privileges of employment.

(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.

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).

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).

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 would 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).

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

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 1961, the New York State fair employment agency handled 7,497 cases, of which only 18 went to public hearings and only 6 resulted in cease and desist orders.

The potential impact of fair employment laws 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.⁶ Setting aside the argument that in the long run non-discriminatory employers would 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 wage gap as well as any 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 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.

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.⁷

⁶ 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."

⁷The 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).

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 schedule for both groups would then narrow the average 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, those seeking a more rapid narrowing of gaps in economic status would argue for stronger labor market 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).

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).⁸ To the contrary, Bonfield (1967) admitted that the laws were not "panaceas," but argued "There is little doubt that they

⁸ 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.

were responsible for some 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 correlation between blacks’ occupational upgrading in the 1950s and the presence of fair employment agencies. Furthermore, Norgren and Hill (1964, pp. 114-130) cited not only the number of cases settled by the committees, but also 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.

Landes (1968) offered the first econometric 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, and he found a weak, positive correlation between fair employment laws and nonwhites’ relative income.⁹ 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 weak, positive correlation with fair employment laws. He also examined the change in relative unemployment rates for men between 1939 and 1959 and found that blacks fared worse relative to whites in fair employment states.

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. He estimated a two-equation system in which the first equation describes favorable “sentiment” towards blacks, and this sentiment 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 allowed 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 by Landes.

4.1 Empirical Strategy

This paper’s approach to estimating the fair employment impact differs significantly from Landes

⁹ This approach yields a coefficient on the fair employment dummy of about 0.03 with a standard error of about 0.02. Landes also undertakes some analyses of occupational status and alternative measures of fair employment enforcement.

(1968) and Heckman (1976) in both its empirical strategy and its data. I employ a difference-in-difference-in-difference (DDD) regression framework to identify the policy effect using individual-level census data from the IPUMS (Ruggles and Sobek 1997).¹⁰ Essentially, the idea is to use three dimensions of comparison to establish a clean measure of the policy’s impact. The framework compares black and white outcomes (one “difference”), in states with and without fair employment laws (a second “difference”), over time (the third “difference”). 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.

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:

$$Y_{ijt} = \alpha + \beta_1 X_{ijt} + \beta_2 \text{Black}_i + \beta_3 \text{FE}_j + \beta_4 \text{Year}_t \\ + \beta_5 (\text{Black}_i \times \text{FE}_j) + \beta_6 (\text{Black}_i \times \text{Year}_t) + \beta_7 (\text{FE}_j \times \text{Year}_t) + \beta_8 (\text{Black}_i \times \text{FE}_j \times \text{Year}_t).$$

Y is the 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 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 study the effect on a variety of labor market outcomes including annual income, unemployment, labor force participation, occupational status, and industrial distribution. Furthermore, I report separate estimates for men and women (ages 20-59), and because one might expect young workers to be 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 change 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

¹⁰ 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.

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 was not an unobserved shock or trend 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, and so when feasible, I assemble the comparison groups accordingly.

For the 1940s DDD estimates, 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 1950.¹¹ For the 1950s DDD estimates, 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 1960.¹² For the full 1940 to 1960 period, the DDD estimates are derived from a larger but much more heterogeneous group of states. Consequently, I prefer the estimates derived from the more carefully tailored comparison groups for 1940-1950 and 1950-1960 separately. Summary statistics for these state groups 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, confounding the interpretation of β_8 as a measure of the fair employment policy impact. In general, dealing with 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

¹¹ The initial 1940 characteristics suggest that this is a reasonable collection of states for comparison: 3.9 and 4.9 percent of the sample population are black in the fair employment states and non-fair employment states respectively; 10.8 and 9.9 percent of the sample's labor force are unemployed, and 31.4 and 34.9 percent of the sample's employed workers are in manufacturing in the fair employment and non-fair employment states respectively.

¹² The initial 1950 characteristics are: 6.1 percent of the population are black in both the fair employment states and non-fair employment states; 4.1 and 2.6 percent of the sample's labor force are unemployed, and 35.8 and 32.0 percent of the sample's employed labor force are in manufacturing in the fair employment and non-fair employment states respectively.

influenced the timing of adoption. Within this framework, the coefficient on a variable for blacks' unemployment rate relative to whites' does not support the hypothesis that the likelihood of passage increased as black workers' relative outcomes worsened.¹³ Moreover, 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.¹⁴ There 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,

¹³ Because of data limitations Collins (2000a) does not allow variables to change over time within states, and so the empirical framework does not exactly match the "relative decline" hypothesis, rather it looks across states at the time of adoption. Nonetheless, the coefficient's sign on relative unemployment is the opposite of that suggested by the hypothesis and is not statistically significant.

¹⁴ I have attempted to use plausibly exogenous variation in the predicted time of fair employment adoption to select states for comparison. Following Collins (2000a), I used an index of political competitiveness (Ranney 1965) and the proportion of the population that was Jewish (National Council of Churches 1953) to predict times of adoption. Of the states used here in the 1940s and 1950s DDD exercises, only New York is predicted to have adopted in the 1940s; Connecticut, Illinois, 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. One could pare down the existing DDD exercises to only those states for which the predicted time of adoption was in the same decade as the actual time of adoption. Comparing New York with Pennsylvania, Michigan, and Ohio in the 1940s DDD framework produces results similar to those reported in the text. 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. Thus, the importance of Illinois to the determination of the text's estimates of the policy impact in the 1950s should be noted.

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.¹⁵ 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.¹⁶ 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 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.¹⁷ 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 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 ($FE = 1$),

¹⁵ For example, Goldstein (1950, p. 2) argues that the reason New York passed a fair employment law in 1945 but not in 1944 was that Governor Dewey actively supported passage in 1945 but not before. The timing did not reflect a substantial change in the underlying degree of support or discriminatory attitudes among the citizens (or employers) of New York.

¹⁶ Obviously, using something like 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 related to employment discrimination spanning this period were inconsistently worded, and the results are not reported at the state level (Gallup 1972). Pushing forward nonetheless, 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 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 [cite ICPSR data]. The appealing thing about the House roll-call voting records is that representatives from each state were confronted with the same piece of legislation at the same time, allowing meaningful cross-state comparisons. Unfortunately, there are also several serious drawbacks to this approach, including the following: the content of the bills changed over time; the interpretation of roll-call votes may be confounded by log-rolling; and I have not found race-related roll-call votes in the House in the early 1940s.

¹⁷ The focus is on wage and salary income for wage workers because there is no total income variable for the 1940 census.

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 estimates for all men suggest a 4 percent improvement associated with fair employment laws and a 6 percent improvement for young men. The estimated impact on the relative income of black women is especially strong at approximately 11 percent.¹⁸

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.¹⁹ In three of the four comparison groups (blacks in FE states, blacks in non-FE states, whites in FE states, whites in non-FE states), average income rose by 54 or 55 percent; but for the young black men in fair employment states, average incomes increased by only 42 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 Again, some extra caution should be exercised in the evaluation of the

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 service industries to the income regressions slightly lowers the estimated fair employment impact for black women (from 11 to 8.5 percent) and slightly raises the estimated impact for men (from 3.8 to 4.5 percent).

¹⁹ The fair employment agencies of the experimental states and the resources at their disposal do not appear to have been atypical. In 1959, the Pennsylvania and Michigan agencies had the largest aggregate budgets after New York, but their budgets were less impressive when expressed in per firm or per black worker terms (cite).

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.

1940-1960 estimates because the control and experimental groups of states are substantially more heterogeneous than those in the 1940-50 and 1950-60 DDD exercises, implying that the identifying assumption of no confounding shocks is more difficult to defend. 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.

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. The sample in Panel A includes only those who are in the labor force, whereas Panel B also includes those who are out of the labor force. 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 of Panels A and B 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

²¹ There is a positive impact (6 percent) even when married women are excluded from the sample, so the result is not merely a reflection of married black women entering the labor force in response to the

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.

In light of the results so far, was Herbert Hill's criticism of the fair employment approach on target? The evidence is decidedly mixed. In line with Hill's contention, there is not strong evidence that black men did especially well in fair employment states, particularly for men in the 1950s. However, the connection between the laws and black women's relative progress appears to have been more positive and more sustained. At this point, it is unclear why the results for black men and women would differ, but a number of hypotheses suggest themselves. If white employers or employees had attitudes towards black women which were less discriminatory than towards black men, then a given dose of fair employment enforcement effort might improve black women's position by more than men's. Along these same lines, if union obstacles to black employment were more prevalent for "men's work" than for "women's work" we might expect 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).

These occupational and industrial redistributions 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. The next two sections of the paper explore this aspect of fair employment's impact.

relative increase in black men's unemployment to maintain family income.

4.4 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 (Pennsylvania 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 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 is likely to be a reflection of 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).²² 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, but with the broader sample of states covering the full 1940-1960 period (column 3), the coefficient suggests 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

²² 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) does not account for the difference between the income and the *occscore* results.

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 from comparisons across state groups that are quite different from one another and therefore perhaps not very suitable for comparison in this framework.

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).²³ 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.

4.5 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 8 to 28 percent in the fair employment states compared to an increase from 10 to 15 percent in the non-fair employment

²³ 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).

states.²⁴ 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.

The rapid decline in the proportion of black female employment in personal service industries is among the most remarkable labor market redistributions of 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 not a southern peculiarity: in the states selected here for comparison in the 1940s DDD regressions, more than 60 percent of employed black women worked in private households. 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 is 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 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.

4.6 Migration

²⁴ There were large increases in black women's manufacturing employment in all four of the fair employment states.

To this point, the empirical approach has treated each state 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 income gaps across locations. This arbitrage would confound the identification strategy used in this paper because in perfectly integrated labor markets, incomes would change by the same amount everywhere even if the fair employment laws were effective.

Table 5, however, demonstrates that states which passed fair employment laws did not experience relatively large increases in their black populations. In fact, the fair employment states had slightly smaller increases in their black/white population ratios than the non-fair employment states in both the 1940s and 1950s. Over the 1940-1960 period, the fair employment states increased their black/white population ratio by slightly more than non-fair employment states, but the difference is very small. 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.

5. 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. The movement from "fair employment" to "affirmative action" was driven in part by the belief that the fair employment approach was not effective, but critics of modern affirmative action programs often advocate race-blind employment policies which aspire to the same kind of equality of treatment advocated by the original fair employment approach.

This paper offers both a new approach to the measurement of the fair employment policy impact and 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 by more than the laws which were adopted elsewhere in the 1950s. This positive income effect was not offset by worsening unemployment or labor force participation rates in the 1940s. Interestingly, from a variety of perspectives, black women appear to have benefitted more from the laws than black men did. This is manifested most clearly in the rising proportion of manufacturing operatives and the declining proportion of domestic servants among employed black women. For men, however, the state-level color-blind approach to eliminating labor market discrimination did not contribute greatly to narrowing racial gaps in labor market outcomes between 1940 and 1960.

At least two important questions remain outstanding. First, further research is required to understand why the laws appear to have affected black women's labor market outcomes more strongly than black men's. This line of inquiry might well shed some light on why current racial gaps in labor market outcomes remain substantially larger for men than for women. Second, given the relatively small impact on men's labor market outcomes, it would be useful to know whether the fair employment effect varied with the amount of resources at the disposal of the enforcement agencies. This would provide some insight into whether the fair employment approach was an inherently weak strategy for dealing with the labor market difficulties facing black men, or if instead, the agencies might have made a bigger impact if they had been allotted more resources.

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Table 1: Fair Employment Laws and Income

	1	2	3
	1939-1949	1949-1959	1939-1959
Men, 20-59	0.0375 (1.43)	-0.0087 (0.32)	-0.0103 (0.55)
Men, 20-35	0.0564 (1.49)	-0.0979 (2.44)	-0.0403 (1.43)
Women, 20-59	0.1156 (2.91)	0.0077 (0.15)	0.0144 (0.53)
Women, 20-35	0.1112 (2.18)	0.0150 (0.22)	0.0160 (0.41)
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. 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). Each coefficient in the table is taken from a separate regression. All regressions control for a quartic in age, a quadratic in education, marital status, and metropolitan area residence. Column 1 is a DDD estimate for the 1940s; the reported coefficients are for the interaction of race with fair employment and the year 1949. Similarly, column 2 is a DDD estimate the 1950s and column 3 is a DDD estimate spanning the 1939 to 1959 period. In 1960, the metro area variable is unavailable for residents of some states (including Rhode Island and New Mexico which had FE laws), and therefore those states are not included in the regressions. In column 3, the same set of states are included for both 1940 and 1960; this requires discarding some states' observations for 1940. 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). Timing of adoption of fair employment laws is from Landes (1968).

Table 2: Fair Employment Laws, Unemployment, and Labor Force Participation

	1	2	3
	1940-1950	1950-1960	1940-1960
Panel A: Unemployment			
Men, 20-59	0.0034 (0.31)	0.0362 (3.37)	0.0062 (0.91)
Men, 20-35	0.0018 (0.10)	0.0586 (3.19)	0.0197 (1.59)
Women, 20-59	0.0076 (0.54)	0.0018 (0.15)	-0.0020 (0.23)
Women, 20-35	-0.0021 (0.11)	-0.0060 (0.38)	-0.0151 (1.15)
Panel B: Labor Force Participation			
Men, 20-59	-0.0084 (0.82)	-0.0011 (0.10)	-0.0019 (0.38)
Men, 20-35	-0.0025 (0.17)	-0.0061 (0.36)	-0.0072 (1.09)
Women, 20-59	0.0140 (0.56)	0.0719 (2.42)	0.0338 (2.00)
Women, 20-35	0.0089 (0.23)	0.0806 (1.85)	0.0626 (2.32)
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 reported coefficients are DDD estimates from probits for unemployment and labor force participation. z-statistics based on robust standard errors are in parentheses. For interpretation, the coefficients are roughly the change in the probability of unemployment or labor force participation associated with a unit increase in the *Black* \times *FE* \times *Year* interaction. See Table 1's notes for details about which states are included in column 3.

Sources: Census micro-data are from Ruggles and Sobek (IPUMS, 1997). Timing of adoption of fair employment laws is from Landes (1968).

Table 3: Fair Employment Laws and Occupations

	1	2	3
	1940-1950	1950-1960	1940-1960
Panel A: Occupational Status			
Men, 20-59	0.0135 (0.71)	-0.0235 (1.23)	0.0662 (5.17)
Men, 20-35	0.0275 (1.00)	-0.0476 (1.73)	0.0813 (4.08)
Women, 20-59	0.1888 (4.32)	0.0643 (1.28)	0.0921 (3.16)
Women, 20-35	0.2723 (4.63)	0.1354 (2.07)	0.1397 (3.49)
Panel B: Operatives and Craftsmen			
Men, 20-59	0.0246 (0.83)	-0.0127 (0.38)	0.0971 (4.79)
Men, 20-35	0.0401 (0.90)	-0.0378 (0.76)	0.0778 (2.51)
Women, 20-59	0.1955 (4.77)	0.0658 (1.79)	0.0711 (3.06)
Women, 20-35	0.2794 (4.79)	0.1014 (1.99)	0.0950 (2.73)
Panel C: Clerical Workers			
Men, 20-59	-0.0073 (0.45)	-0.0144 (0.97)	-0.0197 (2.01)
Men, 20-35	0.0330 (1.05)	-0.0366 (1.86)	-0.0108 (0.59)
Women, 20-59	-0.0137 (0.26)	0.0286 (0.46)	0.0736 (1.78)
Women, 20-35	-0.0036 (0.05)	0.0401 (0.44)	0.1001 (1.72)
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 the $Black \times FE \times Year$ interaction in an ordinary least squares regression. t-statistics are in parentheses. In Panel B and Panel C the reported coefficients are DDD estimates from probits for having an occupation in the relevant category, conditional on being employed. The coefficients are expressed in dF/dX terms (roughly the change in the probability of employment in that category associated with a unit increase in the $Black \times FE \times Year$ interaction). z-statistics based on robust standard errors are in parentheses. See Table 1's notes for details about which states are included in column 3.

Sources: Census micro-data are from Ruggles and Sobek (IPUMS, 1997). Timing of adoption of fair employment laws is from Landes (1968).

Table 4: Fair Employment Laws, Manufacturing, Personal Service, and Government Employment

	1	2	3
	1940-1950	1950-1960	1940-1960
Panel A: Manufacturing			
Men, 20-59	-0.0013 (0.05)	0.0338 (1.09)	0.0848 (4.42)
Men, 20-35	0.0165 (0.38)	0.0385 (0.82)	0.0853 (2.81)
Women, 20-59	0.2518 (5.36)	0.1005 (2.22)	0.1494 (4.80)
Women, 20-35	0.3294 (5.06)	0.2044 (3.04)	0.2341 (4.89)
Panel B: Personal Service			
Men, 20-59	0.0131 (1.63)	0.0103 (1.38)	0.0069 (1.54)
Men, 20-35	0.0029 (0.28)	0.0054 (0.53)	0.0071 (1.10)
Women, 20-59	-0.0635 (4.43)	-0.0126 (0.81)	-0.0175 (1.59)
Women, 20-35	-0.0636 (4.03)	-0.0339 (2.25)	-0.0283 (1.96)
Panel C: Government			
Men, 20-59	-0.0059 (0.59)	-0.0067 (0.65)	-0.0201 (3.57)
Men, 20-35	-0.0030 (0.23)	-0.0112 (0.93)	-0.0174 (2.28)
Women, 20-59	-0.0085 (0.83)	-0.0110 (0.99)	-0.0047 (0.53)
Women, 20-34	-0.0065 (0.52)	0.0063 (0.37)	-0.0069 (0.70)
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 reported coefficients are DDD estimates from probits for employment in the relevant industry, conditional on being employed. The coefficients are expressed in dF/dX terms (roughly the change in the probability of participation associated with a unit increase in the $Black \times FE \times Year$ interaction). z -statistics based on robust standard errors are in parentheses. See Table 1's notes for details about which states are 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). Timing of adoption of fair employment laws is from Landes (1968).

Table 5: Changes in Black/White Population

	1	2	3	4
	1940	1950	1960	Change
Panel A: 1940-1950				
FE States	0.03896	0.05463	-----	0.01567
Non-FE States	0.04835	0.06866	-----	0.02031
Panel B: 1950-1960				
FE States	-----	0.05842	0.07618	0.01776
Non-FE States	-----	0.05831	0.08253	0.02422
Panel C: 1940-1960				
FE States	0.03936	-----	0.07535	0.03599
Non-FE States	0.04047	-----	0.07425	0.03378

Notes: Calculations are based on census data for state populations.

Source: U.S. Department of Commerce (1975).

Table A1: Summary Statistics for 1940-1950 DDD Estimates

	Black Men			White Men			Black Women			White Women		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
FE States for 1940-50 DDD												
Income												
1940	1004	445	1258	1671	977	36129	634	324	824	1057	618	14549
1950	2414	797	635	3557	2028	12332	1732	641	396	2242	1023	5497
Occscore												
1940	21.16	7.65	1862	27.78	10.14	55476	10.71	7.74	1315	21.64	7.67	22035
1950	22.46	7.21	884	29.27	10.08	17823	15.97	8.61	649	22.90	7.02	8141
UE												
1940	0.1901	0.3925	2330	0.1107	0.3138	62908	0.1412	0.3484	1538	0.0925	0.2897	24518
1950	0.1074	0.3098	996	0.0470	0.2117	18772	0.0767	0.2663	704	0.0389	0.1933	8493
LF												
1940	0.8906	0.3122	2611	0.9354	0.2458	67247	0.5234	0.4995	2954	0.3555	0.4787	68841
1950	0.8564	0.3508	1163	0.9175	0.2751	20459	0.5165	0.4999	1363	0.3731	0.4836	22762
Non-FE States for 1940-50 DDD												
Income												
1940	996	429	1302	1563	864	29070	574	256	547	919	498	8627
1950	2562	839	810	3485	1723	11223	1544	716	278	2081	909	3751
Occscore												
1940	21.57	7.42	2203	26.53	9.60	50495	11.77	8.11	978	21.32	7.86	14933
1950	23.22	6.78	1087	28.26	9.53	16379	14.62	8.81	504	22.40	7.10	6080
UE												
1940	0.1953	0.3965	2764	0.0987	0.2983	56435	0.1955	0.3967	1238	0.0761	0.2651	16339
1950	0.1004	0.3007	1225	0.0395	0.1947	17101	0.0931	0.2908	559	0.0270	0.1620	6270
LF												
1940	0.8955	0.3059	3083	0.9403	0.2370	60018	0.3941	0.4887	3162	0.2776	0.4478	58843
1950	0.8725	0.3337	1404	0.9220	0.2682	18548	0.3839	0.4865	1456	0.3195	0.4663	19622

Table A2: Summary Statistics for 1950-1960 DDD Estimates

	Black Men			White Men			Black Women			White Women		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
FE States for 1950-1960 DDD												
Income												
1950	2620	827	555	3437	1717	8539	1566	761	177	2071	929	3040
1960	4265	1479	1958	5991	3287	31594	2685	1175	1005	3327	1466	11443
Occscore												
1950	23.41	6.49	735	27.71	9.85	12939	14.84	8.95	336	22.07	7.30	4963
1960	23.53	6.92	2460	28.95	9.57	41857	16.26	8.03	1586	22.20	7.03	18246
UE												
1950	0.1063	0.3084	837	0.0416	0.1997	13535	0.0890	0.2851	371	0.0266	0.1609	5118
1960	0.1365	0.3434	3171	0.0563	0.2305	45924	0.1139	0.3177	1959	0.0514	0.2208	20168
LF												
1950	0.8674	0.3394	965	0.9151	0.2788	14791	0.3613	0.4806	1027	0.3206	0.4667	15963
1960	0.8664	0.3402	3660	0.9378	0.2415	48969	0.4758	0.4995	4118	0.3887	0.4875	51885
Non-FE States for 1950-1960 DDD												
Income												
1950	2619	995	406	3544	1835	6658	1740	722	182	2172	943	2307
1960	4405	1887	1826	6131	3330	25249	2925	1194	851	3434	1490	9307
Occscore												
1950	22.77	6.84	557	27.37	10.19	10380	16.84	8.02	312	22.17	7.78	3968
1960	23.74	6.44	2034	28.48	9.92	33388	18.03	8.24	1224	22.55	7.24	15086
UE												
1950	0.1136	0.3175	634	0.0188	0.1357	10612	0.1054	0.3075	351	0.0224	0.1479	4070
1960	0.0859	0.2802	2644	0.0316	0.1750	35898	0.1230	0.3285	1626	0.0387	0.1928	16480
LF												
1950	0.8709	0.3356	728	0.9308	0.2538	11401	0.4350	0.4961	807	0.3325	0.4711	12239
1960	0.8712	0.3351	3035	0.9443	0.2294	38017	0.4800	0.4997	3387	0.4120	0.4922	39996

Table A3: Summary Statistics for 1940-1960 DDD Estimates

	Black Men			White Men			Black Women			White Women		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
FE States for 1940-1960 DDD												
Income												
1940	1009	442	2118	1622	934	61648	610	301	1208	1010	579	22056
1960	4174	1632	5121	6180	3569	82177	2814	1176	3037	3506	1590	32868
Occscore												
1940	21.32	7.48	3330	26.91	10.12	102208	11.14	7.91	1968	21.43	7.82	35383
1960	23.56	7.08	6042	29.82	9.72	102453	16.73	8.24	4475	22.79	6.94	48642
UE												
1940	0.1996	0.3998	4212	0.1076	0.3099	115380	0.1607	0.3674	2369	0.0880	0.2833	39207
1960	0.1047	0.3061	7567	0.0445	0.2062	111887	0.0891	0.2849	5479	0.0525	0.2230	54140
LF												
1940	0.8997	0.3005	4673	0.9468	0.2244	121852	0.4680	0.4990	5095	0.3201	0.4665	122360
1960	0.8739	0.3320	8659	0.9508	0.2163	117677	0.5288	0.4992	10361	0.4161	0.4929	130116
Non-FE States for 1940-1960 DDD												
Income												
1940	928	471	1085	1481	900	26218	594	376	427	894	502	8171
1960	4323	1919	2348	6106	3315	32644	2779	1208	1118	3378	1440	12343
Occscore												
1940	20.89	7.66	1834	24.79	10.69	50958	12.61	8.38	843	21.20	8.27	14681
1960	23.48	6.41	2635	29.32	9.70	41092	17.27	8.30	1641	22.72	7.20	19493
UE												
1940	0.1596	0.3663	2189	0.0708	0.2564	55101	0.1693	0.3752	1028	0.0570	0.2318	15692
1960	0.0854	0.2795	3397	0.0332	0.1793	44276	0.1147	0.3188	2144	0.0390	0.1937	21296
LF												
1940	0.8858	0.3181	2472	0.9477	0.2226	58136	0.3955	0.4891	2609	0.2700	0.4440	58247
1960	0.8748	0.3309	3883	0.9545	0.2085	46389	0.4833	0.4998	4436	0.4205	0.4936	50649