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THE EFFECTS OF RACE AND SEX DISCRIMINATION LAWS

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ABSTRACT

The question of the effects of race and sex discrimination laws on relative economic outcomes for blacks and women has been of interest at least since the Civil Rights and Equal Pay Acts passed in the 1960s. We present new evidence on the effects of these laws based on variation induced first by state anti-discrimination statutes passed prior to the federal legislation, and then by the extension of anti-discrimination prohibitions to the remaining states with the passage of federal legislation. This evidence improves upon earlier time-series studies of the effects of anti-discrimination legislation. It is complementary to more recent work that revisits this question using data and statistical experiments that provide "treatment" and "comparison" groups. We examine the effects of race and sex discrimination laws on employment and earnings, in each case focusing on outcomes for black females, black males, and white females relative to white males.

Overall, we interpret the evidence as corroborating the general conclusion that race discrimination laws positively impacted the relative employment and earnings of blacks, although the evidence is less dramatic than that reported in other research, and there are some cases (in particular, earnings effects for black males) and periods for which we find little positive impact. We find some evidence that sex discrimination/equal pay laws boosted the relative earnings of black and white females. Finally, we find that sex discrimination/equal pay laws reduced the relative employment of both black women and white women.

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I. Introduction

Public policies to narrow the gaps in labor market outcomes between whites and minorities, and between men and women, have a long and controversial history. Two pieces of federal legislation stand out as perhaps the most significant such policies. The first was the Equal Pay Act of 1963, which requires equal pay for equal work, noting some exceptions but explicitly prohibiting a worker's sex as one of them. The second was Title VII of the Civil Rights Act of 1964, making it illegal to discriminate in hiring, discharge, compensation, etc., on the basis of race, color, religion, sex, or national origin. Two other important landmarks in the evolution of these policies at the federal level were amendments to Title VII embodied in the Equal Employment Opportunity Act (EEOA) of 1972, which expanded coverage and increased the enforcement powers of the Equal Employment Opportunity Commission (EEOC), and Executive Orders 10925 (1961), 11246 (1965), and 11375 (1967), which laid the groundwork for affirmative action, although the phrase has its origins in Title VII.[1]

There are three broad controversies regarding these public policies. The first concerns the need for any government attack on labor market discrimination, hinging on the questions of whether the observed group differences in labor market outcomes reflect discrimination, and whether competition in labor markets and product markets will undermine discriminatory behavior.[2] The second concerns the fairness and efficiency of affirmative action, which many (e.g., Steele, 1990; Carter, 1991) regard as distinct from non-discrimination policies in advocating preferential treatment of particular groups.[3] The third, with which this paper is concerned, is the effectiveness of these policies, asking, in particular, whether anti-discrimination policies contributed to relative improvements in labor market outcomes for

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[1]For detailed discussions of these legislative and executive policies, and of the numerous court cases that have shaped the evolution of anti-discrimination policy, see, for example, Epstein (1992) and Bloch (1994). There were some earlier Executive Orders issued by President Roosevelt in 1941 outlawing racial discrimination in the defense industry and in training for defense production (see Collins, 2000).

[2]Much of this controversy is based on Becker (1971). For a recent exchange and review of evidence see Heckman (1998) and Darity and Mason (1998). For evidence on the role of competition in eliminating discrimination, see Ashenfelter and Hannan (1986), Black and Brainerd (1999), and Hellerstein, et al. (1997).

[3]For a detailed review of the literature on affirmative action, see Holzer and Neumark (2000).

minorities and women (and presumably continue to contribute if discrimination persists).

Much of the previous research on the impact of anti-discrimination legislation on race and sex differences in labor market outcomes consists of time-series studies. This research–reviewed in the next section–essentially asks whether concurrent with the passage of anti-discrimination laws there was a jump or acceleration in the relative economic status of the groups protected by these laws. The fundamental problem with these time-series analyses of the impact of federal anti-discrimination laws is that the laws have nearly universal applicability, which prevents identification of an appropriate comparison group that can be used to control for changes in the relative outcomes under study unrelated to the policy innovation. For example, if the black-white wage gap was narrowing prior to the passage of Title VII of the Civil Rights Act, then testing whether Title VII narrowed the gap requires a comparison of changes in the black-white gap for workers covered by Title VII and workers not covered, in the same period. Researchers have of course considered other ways to bring complementary evidence to bear, including efforts to identify control groups, and use of auxiliary data.

This paper takes an alternative approach to the problem of inferring the effects of anti-discrimination policies to those taken previously. In particular, prior to the enactment of the federal legislation, many states enacted similar laws or practices barring discrimination in wages and employment. Because these laws or practices were passed at different times in different states, a more natural control group is provided. Specifically, the experimental design afforded by the variation across time and over states allows us to assess the impact of state anti-discrimination statutes while using for comparison data for the same time span from states that did not enact such statutes.[4] Similarly, additional identifying information comes from the later imposition of federal legislation in states that did not yet have their own laws established, in comparison to those that did. Although this approach has some limitations, at a minimum our empirical strategy provides important complementary evidence to that in the existing

[4]Landes (1968) employs a similar experimental design to investigate state anti-discrimination laws. However, the date of his study precludes inclusion of the federal legislation, and his study also ignores sex discrimination laws. Other differences between our research and his are discussed in Section II below.

literature on the impact of federal anti-discrimination laws. A recent survey of research on the effects of race and gender in the labor market by Altonji and Blank (1999) concurs. They summarize their review of existing evidence on the effects of federal anti-discrimination policy as follows:

> [D]espite major public and private resources devoted to anti-discrimination policy, the research literature on the results of these efforts is sparse. While we recognize the difficulties of studying nationally enacted legislation, in many cases there are differences over time or across regions in the implementation of such legislation, or there is variation in related state-specific legislation. Such research ... is likely to provide useful information, particularly in a world where existing anti-discrimination measures in ... the labor market are at the center of a major public debate about the appropriate response to ongoing racial differentials (p. 3250).

II. Existing Research

The primary focus in the previous time-series studies of the impact of anti-discrimination legislation is on examining evidence regarding alternative explanations of black economic progress in the late 1960s. Summarizing the empirical facts, Cain (1986) roughly characterizes black-white earnings ratios for all working men as stable from 1948 to 1965, growing from 1966 to 1974, and stable again from 1975 to 1982. Heckman (1990) paints a slightly different picture, noting that relative improvements for blacks were concentrated in the period 1965-1975, but began before 1964. The primary question addressed in the time-series studies is, as Heckman (1990) puts it, "Does continuous or discontinuous change characterize the recent economic history of black Americans?" (p. 242). That is, is there a discontinuity in the relative progress of blacks that is most consistent with an important role for federal anti-discrimination efforts, or does black economic progress simply reflect longer-term trends, perhaps obscured in some periods (and hence giving the impression of more rapid change in the 1960s and early 1970s) because of other changes. The latter view is put forth most forcefully by Smith and Welch (1989), who conclude that "slowly evolving historical forces ... education and migration–were the primary determinants of long-term black economic improvement. At best affirmative action has marginally altered black wage gains around this long-term trend" (p. 519).[5]

[5]For a more recent look at the evolution of relative earnings by race (focused to some extent on issues other than anti-discrimination policies), see Bound and Freeman (1992). Their paper also points to a jump in economic outcomes for blacks after 1965, and helps establish that the apparent stagnation or decline in black relative progress beginning in the mid-1970s is attributable to a decrease in the price of

The empirical studies on this question are reviewed by Brown (1982), Heckman and Payner (1989), and others. These studies share a common goal of sorting out the question of continuous vs. discontinuous change, asking whether the relative progress of blacks in this period could or could not be explained by changes in migration rates, changes in the relative educational attainment of blacks, and other supply shifts (leading the lowest-wage blacks to leave the labor force), business cycle fluctuations, other observable factors, or other trends that may be difficult to relate to observables but that pre-dated federal legislation (see Butler and Heckman, 1977; Donohue and Heckman, 1991; Freeman, 1973 and 1981; Culp, 1986; Vroman, 1974; Brown, 1984; Fosu, 1992; and Smith and Welch, 1977).

Many, but not all, of these studies reach conclusions consistent with the view that federal anti-discrimination efforts did have a positive effect on black economic progress. Nonetheless, ambiguities regarding the time-series evidence remain. Summarizing the evidence and the conflicting views in the literature, Brown (1982) takes a relatively agnostic view, noting that "separating anti-discrimination (demand-curve), supply-curve, and truncation effects places extreme demands on time-series data, due to collinearity among the variables and the shortage of "strong" variables affecting only relative supplies to serve as instruments" (pp. 44-5). Smith and Welch's more unambiguously negative view of the impact of federal efforts was noted above.[6] In contrast, Heckman (1990) takes a strong stand regarding the positive impact of federal policy (based on evidence in Donohue and Heckman (1991), and a review of the earlier evidence), arguing that "there is ample evidence of discontinuous change in the improvement of black status during the crucial period 1965-1975" (p. 242).

There is a smaller body of research on the impact of federal anti-discrimination policy on sex differences in labor market outcomes. Eberts and Stone (1985) use panel data to examine relative rates of

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less-skilled relative to more-skilled labor, as blacks were overrepresented among the less skilled.

[6]In earlier work, Welch (1976) took a more nuanced view, noting, in reference to the Civil Rights Act of 1964 and Executive Orders 11246 (1965) and 11375 (1967), that "There is a real question of the impact of these statutes. It is clear that, with respect to blacks, both earnings and occupational status have improved relative to whites in the period since 1964. But relative wages were rising before then, and the extent of recent gains that can be attributed to antidiscriminatory mandates is unclear" (p. S106).

promotion of males and females in public schools before and after the EEOA. They find declining evidence of discrimination in promotions of teachers in two states in the latter part of the 1970s, and conclude that the EEOA contributed to that decline. Beller (1979) estimates models for earnings of men and women using Current Population Survey data for 1974, 1971, and 1967, incorporating measures of Title VII investigations and settlements by region (large states and state groups), but not including a time trend or year dummy variables. She finds relatively weak evidence that these measures reduced the sex wage gap prior to the EEOA, but stronger evidence of this effect after the passage of the EEOA. O'Neill (1985) studies the evolution of the sex gap in wages, but without an explicit focus on policy effects.[7]

The core problem with time-series analyses of the impact of federal anti-discrimination laws is that these laws have nearly universal applicability, which prevents identification of an appropriate comparison group that can be used to control for changes in the outcomes under study unrelated to the policy innovation.[8] In the face of this methodological problem, researchers have taken three different approaches. One approach, which is not directly related to the research in this paper, is to bring to bear other auxiliary evidence on the impact of federal anti-discrimination efforts, which Brown (1982) labels "procedural analyses." The second, which is also embodied in the time-series literature reviewed above, is to attempt to introduce control variables into the time-series regressions to capture trends or changes other than the policy change. While this is an appropriate strategy, it is not necessarily fully convincing (as suggested by Brown's quote above). The third approach, which attempts to address the fundamental problem with the

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[7]Britain implemented equal pay legislation (the Equal Pay Act) and equal employment opportunities legislation (the Sex Discrimination Act) at the end of 1975. There is a literature on sex discrimination legislation in Britain that parallels the U.S. literature on race discrimination legislation, in trying to tease out the effects of the legislation from time-series data (Zabalza and Tzannatos, 1985 and 1988; and Pike, 1985).

[8]These concerns are echoed by Chay (1998), who points out that "the timing of the legislation (in the mid-1960s) corresponds with the timing of many other significant changes in the U.S. labor market. In addition, the nature of these laws, and in particular their nearly universal coverage, makes it difficult to control for changes that would have occurred even in the absence of the legislation" (pp. 608-9). Similarly, Hahn, et al. (1999) note that the lack of consensus over the impact of Title VII and related laws "probably stems in part from the difficulty of assessing the impact of laws that have near universal coverage" (p. 14).

time-series analyses and which characterizes the newer research on the effects of anti-discrimination legislation, is to exploit other sources of identifying information.

Donohue and Heckman's (1991) study can be interpreted in this light. They argue that black economic progress over the 1965-1975 period was generated mainly in the South, at the same time that federal policy was directed toward the South.[9] This can be interpreted as testing for the effectiveness of federal efforts by comparing changes in relative black economic status in two regions–the South, where enforcement was more vigorous, and the rest of the country, which because it experienced less vigorous enforcement serves as a crude control group.[10] As long as underlying trends did not differ between the South and elsewhere, this may more reliably identify at least the qualitative effect of federal anti-discrimination laws.[11]

Although federal anti-discrimination laws expanded relatively quickly to near-universal coverage, Title VII coverage was initially extended only to firms with at least 100 employees, with the minimum workforce size falling to 25 by 1968. The 1972 EEOA extended coverage to employers with as few as 15 employees. Chay (1998), Carrington, et al. (1998), and Hahn, et al. (1999) have exploited these coverage differences to develop tests of the effectiveness of federal anti-discrimination laws based on black vs. white differences in employment, earnings, etc., across firms or establishments of different sizes. For example, Chay (1998) exploits the extension of coverage to smaller establishments with the EEOA, as well as the existence of state laws that in some cases had already extended coverage beyond that mandated by

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[9]For example, 50 percent of all charges filed by the EEOC during 1966-1972 were filed against firms and establishments in that region.

[10]Heckman and Payner (1989) take this type of analysis to an even finer level, looking at changes in relative black employment and wages in one Southern state (South Carolina), and noting that the sharpest improvements came in the textile industry, which was targeted by the EEOC. Their study also does a thorough job of exploiting data across counties in South Carolina to demonstrate that relative improvements for blacks in textiles cannot be adequately explained without relying on a role for government anti-discrimination efforts.

[11]One suggested source of differences in underlying changes regards closing of the gap in school quality between blacks and whites in the South (Card and Krueger, 1992).

the EEOA, to obtain treatment and control groups.[12] In particular, he uses Current Population Survey (CPS) data on establishment size, coupled with information on state laws, to identify those industries and regions that should have been most affected by the EEOA, and finds that in these industry-region cells black men had the largest gains relative to white men in terms of employment, earnings ratios, and occupational status. Chay concludes that the EEOA had positive effects on the labor market status of blacks. By identifying regions over which the effects of policy should vary in systematic ways associated with the "strength" of the treatment, Chay's study is more similar in approach to ours than to the existing time-series literature.[13,14]

In our view, Chay's (1998) study utilizes the most compelling research design in the literature on the effects of federal anti-discrimination laws (complemented nicely by the richer, more historical studies by Heckman and his co-authors). It does, however, lead to some large estimated policy effects, and therefore a related but different empirical study is warranted.[15] Furthermore, it is somewhat limited in

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[12]He provides a detailed discussion of the changes in coverage entailed by the EEOA, as well as other relevant legislation including the Civil Rights Acts of 1866 and 1870; the latter did not have any size exemption but, Chay argues, had little impact in this era.

[13]This work also has some parallels to Leonard's research (e.g., 1984) on affirmative action, in which he compares minority and female employment growth in establishments that are federal contractors (and hence subject to affirmative action requirements) to growth in non-contractor establishments.

[14]Carrington, et al. (1998) instead look at the overall effect of federal anti-discrimination efforts by asking whether black and female representation at large firms increased during the period in which these efforts were introduced. Much of the time-series evidence appears consistent with this. However, while acknowledging the existence of state fair employment practices that would have made the impact of federal efforts (by race) vary across states, they conduct no analysis of potential treatment and control states other than a South/non-South breakdown (as these practices were not implemented in Southern states).

[15]His employment estimates indicate that black employment shares grew 0.5 to 1.1 percentage points more per year at newly-covered than previously-covered employers. These effects are larger by a factor of about 10 than those found in the affirmative action literature (Leonard, 1990; Donohue and Heckman, 1991), but perhaps not large relative to evidence regarding the federal role in increasing access of blacks to manufacturing employment in South Carolina (Heckman and Payner, 1989), although this latter evidence pertains to an isolated industry in one state that was particularly strongly targeted by the EEOC. His earnings estimates indicate that the black-white earnings gap narrowed by .11 to .18 log points more at newly covered than previously covered employers. These estimates are roughly in line with those obtained in a time-series analysis of the effect of the Civil Rights Act (with the possible flaws discussed earlier) by Freeman (1973, p. 101).

restricting attention to policy effects in firms or establishments in a particular size range, and to black vs. white males. Indeed, we would argue that many of the newer studies (by Chay, 1998; Carrington, et al., 1998; Hahn, et al., 1999; and Heckman and Payner, 1989), in providing more compelling evidence of the positive impact of federal anti-discrimination policies on relative labor market outcomes for blacks, have also, as a result of their research designs, generated more narrowly-tailored evidence, and hence evidence that does not necessarily justify inferences regarding overall effects of these policies. The evidence we present, based on variation across states and time in the introduction of anti-discrimination legislation, is of a more aggregated nature, and in that sense closer in spirit to the earlier time-series studies in providing estimates of overall policy effects, but while also building on the general empirical strategy of the newer research that develops better "experiments" for evaluating the effects of anti-discrimination legislation.

Finally, the one study that in many ways comes closest to ours is a much earlier paper (Landes, 1968), which looks at the impact of state fair employment practices on relative earnings, unemployment, and occupational distributions of black and white men. For its time, this is a sophisticated study relying on both cross-sectional and "difference-in-difference" estimates of the effects of state-level Fair Employment Practices Acts (FEPAs). Landes concludes that state fair employment practice laws boost relative weekly wages of black men (by about .03), conditional on relative employment and including controls for schooling and the urban-rural makeup of the overall population and blacks relative to whites.[16] However, he does not find a stronger effect in states for which these laws have been on the books longer, or in states with higher enforcement budgets (per firm),[17] which he speculates is because states with earlier legislation or higher enforcement budgets had more serious discrimination problems. With respect to employment

[16]Here, we concentrate on Landes' estimated effects on relative wages and earnings. He also considers the effects of these laws on relative unemployment rates, which we view as somewhat problematic since the participation decision is a choice variable, as pointed out subsequently in research on minimum wages (Mincer, 1976). Landes recognizes this issue (p. 524), which at a minimum makes it difficult to interpret any effects of FEPAs on relative unemployment rates. Finally, he also estimates effects of FEPAs on occupation-weighted relative average earnings figures, while noting that these are highly correlated with relative earnings, and hence that "little additional information will be gained by a separate investigation of relative occupation distribution" (p. 529).

[17]He was unable to obtain information on penalties.

effects, the descriptive statistics and regressions suggest that in states that enacted fair employment practice laws, the relative increase in black employment was faster than in other states, between both the 1940 and 1950 Censuses, and the 1950 and 1960 Censuses. However, in his aggregate regressions (footnote 31), these differences are not statistically significant.

Aside from using micro-data, our research differs in three important ways. First, we are able to look at the extension of federal legislation to states that already had fair employment practices, similar to the identification strategy underlying Chay's work. Second, Landes tries to estimate the effects of anti-discrimination statutes on the relative demand function, specified as an equation for relative earnings with the ratio of non-white to white male workers as a control variable. The latter is obviously endogenous, which leads us instead to estimate reduced form equations for earnings and employment.[18] Such equations are arguably more relevant from a policy perspective, especially if there are also supply responses, and are more consistent with the approach taken in the time-series studies reviewed above; on the other hand, Landes' approach (if identified) is more pertinent to testing hypotheses regarding employers' behavioral responses to anti-discrimination laws. Third, whereas Landes looked only at fair employment practices related to race discrimination, we also study the effects of laws concerned with sex discrimination, estimating the effects of these, and, potentially as importantly, estimating the effects of race discrimination laws controlling for the enactment of laws targeting sex discrimination that were not infrequently passed in the same states in similar periods.

III. Race and Sex Discrimination Laws

This paper exploits cross-state variation in anti-discrimination legislation at the state level. Thus, a major part of the research project was assembling information on this legislation, and on related legislation potentially impacting women, in particular. Information on laws regarding race discrimination is summarized in Table 1. As the table indicates, the most common type of legislation is the enactment of

[18]Landes alternately refers to the relative numbers of workers (p. 509) and labor force members (p. 514). It is therefore not clear whether he refers to employment or participation. Either way this variable is endogenous, although the problem is presumably more severe if it is defined in terms of employment.

a mandatory FEPA (e.g., Alaska, California, and Massachusetts). FEPAs establish two types of behavior as violations of the acts: refusing employment or discharging a non-white because of that person's race; and discriminating against non-whites in terms or conditions of employment, including compensation.[19] However, as Table 1 also reveals, there is heterogeneity in the FEPAs, including variation in exemptions for employers below certain size thresholds (e.g., Indiana, Minnesota, and Pennsylvania), differences in enforcement mechanisms (e.g., New Jersey, New York, and Wisconsin),[20] and whether compliance is mandatory or voluntary (e.g., Colorado, Kansas, and Oregon).

State FEPAs operate in a similar fashion to Title VII of the Civil Rights Act. A 1973 Bureau of National Affairs report described the workings of FEPAs as follows:

> The enforceable state FEP laws share a common pattern. They rely upon civil, rather than criminal, proceedings and vest responsibility for enforcement in an administrative agency. They stress education and conciliation, using public hearings and court proceedings as a last resort. Under the state FEP laws, an individual may file a complaint with the commission. If the commission, after investigating, finds no probable cause to support the complaint, it dismisses the complaint. Most commissions, however, still may study the employer's general employment pattern and attempt to eliminate any discriminatory practice found. If the commission finds probable cause to believe the complaint, or finds evidence of other discriminatory practices, it attempts to adjust the matter through conciliation. If conciliation fails, the dispute becomes public for the first time, and a hearing is held. This results either in dismissal of the complaint or issuance of an order requiring the accused to cease and desist from discriminating and to take affirmative remedial action. Such orders may be enforced or reviewed in the courts (pp. 68-9).

Note that this 1973 report refers to state FEPAs as operative even after the Civil Rights Act, because Title VII directed federal authorities to first defer processing of discrimination charges to states or their political subdivisions that had anti-discrimination laws. These deferrals were limited and more stringently regulated as a result of the EEOA. Lockard (1968) provides a similar description of state FEP laws, and notes that in some states a complainant could appeal in court a finding of no probable cause. He also provides details on the conciliation and hearing process, and describes some of the actions a Commission might take in response to a finding of discrimination, including requiring back pay, the posting of signs announcing non-discriminatory policies, the requirement of future reporting on hiring

[19]Such laws also refer to creed, color, and national origin, but here we focus on race.

[20]See Chay (1995, Appendix Table 1) for more details on enforcement as of 1972.

practices (and changes in those practices), etc. He indicates that commissions had the authority to follow up on whether orders were being obeyed, although he suggests that follow-up investigations were rare.

In the empirical work, we do not pay attention to all of the variation in state legislation documented in Table 1, including some of that displayed in the table but not mentioned above. However, as shown in Table 3, where we report our "coding" of the state FEPAs (in the "race" columns), we do account for some differences in state legislation. Specifically, we initially distinguish mandatory FEPAs (MRD), voluntary FEPAs (VRD), laws only targeting race discrimination in wages (MRDW), and mandatory FEPAs with no commission for enforcement (MRDWE). The table also distinguishes laws passed in the year corresponding to the Census earnings data (i.e., years ending in a nine). In the empirical analysis, we allow for such laws to have potentially weaker effects, because they were in effect only part of the year or because of slow adjustment.

Table 2 summarizes information on laws regarding sex discrimination in the labor market. As the table shows, in almost every case through the 1960s, these were equal pay laws, and did not explicitly refer to discrimination in hiring, discharge, etc. The only exceptions are a voluntary FEPA-type provision in Oregon as of 1950, and then, as of 1970, the extension of FEPAs to refer to sex in numerous states. As before, these are "coded" in Table 3.

The state laws for women raise a couple of issues. First, given that the state laws cover wage discrimination, while the federal laws are considerably more extensive, we have to consider what additional information we obtain from the federal legislation. As discussed below, it turns out that the federal legislation most likely gives us additional information on the effects of equal pay laws, but no information on the other components of the federal legislation; as there are no state laws paralleling the full federal legislation as of 1960, we cannot use the data through 1970 to identify the effects of the federal legislation from a comparison between states that did and did not have similar laws in effect in earlier decades.

A second issue is the potentially confounding effects of "protective" laws restricting women's

work. At the end of the 19th and beginning of the 20th century, most states adopted laws that in some way or other restricted women's work, most commonly by specifying maximum hours or prohibiting employment in particular occupations or at night. These are reported in Appendix Table A1. As the last column of the table shows, state attorneys general or other state bodies sometimes issued opinions in the 1960s noting that these laws were inconsistent with Title VII of the Civil Rights Act, and we assume that in all states such laws were by and large non-binding by 1970. Nonetheless, if these laws were changing over the 1940-1960 period, any effects of such changes could be difficult to sort out from changes in anti-discrimination laws. As the table shows, however, with the exception of Delaware, there was no activity regarding these laws in this period. As Goldin (1990) notes, protective legislation was largely put in place by the 1920s, and was viewed by social reformers for nearly the next half-century as "more valuable to women than was legislation to ensure equality" (p. 6). As the table also documents, only following the Civil Rights Act of 1964 did the dismantling of protective legislation occur. Thus, we need not be concerned with confounding effects of changes in anti-discrimination laws with effects of changes in protective laws.[21]

As just noted (and explained further below), our evidence regarding sex discrimination laws is most likely informative only regarding equal pay laws. There is a legitimate question as to how interesting such evidence is, given that federal policy essentially enacted equal pay legislation and prohibitions of employment discrimination simultaneously. In our view, it is of considerable policy interest. The current debate over affirmative action (see Holzer and Neumark, 2000) may eventually weaken laws barring employment discrimination in hiring. Although the policy debate is focused on affirmative action per se, numerical guidelines also play a critical role in anti-discrimination enforcement efforts (Bloch, 1994). While we do not anticipate a return to a world where sex discrimination is regulated only by equal pay laws, we can envision one in which this becomes a relatively much more important part of the arsenal of

[21]Goldin (1990) documents how needs for female workers during World War II helped to bring about the demise of marriage bars, but presents no information suggesting that protective legislation eroded during the war. Aldrich (1989) reports that maximum hours restrictions for women generally remained in effect during the war.

anti-discrimination efforts. In addition, we think it useful to understand the separate effects of the two components of federal anti-discrimination efforts.

A final question concerns whether state laws have any "teeth." Of course to a large extent this question will be answered empirically, by estimating the effects of these laws on relative economic outcomes of blacks and women. However, there is information suggesting that state laws regarding race discrimination were far from irrelevant. Landes (1968) presents some descriptive information on violations "cleared up" in four states–New York, New Jersey, Connecticut, and Massachusetts–in the period between 1945 and 1961. The number of cases ranges from about 500 to 700 for the three smaller states, to nearly 3,300 for New York. These cases, in turn, are a subset of complaints for which state commissions found probable cause to support the complaint. Lockard (1968) presents data on the number of FEP cases closed over the 1940s, 1950s, and 1960s in 10 states, with the numbers ranging from about 150 to 6,000, depending on the state; he also reports that in 64 percent of cases no probable cause was found. Similarly, a report written by the New York State Commission Against Discrimination (1958) details efforts to counter discrimination in the hotel industry in New York City, including information on complaints received, and the settling of those complaints that were sustained (i.e., judged as valid) through conference and conciliation.[22] Another issue is the reliance of most states on individual complaints, although some states (such as California) had started to open up inquiries in the absence of individual complaints (Lockard, 1968, pp. 88-9). On the other hand, through the 1960s (until the EEOA of 1972) federal law also was complaint-based. Turning to evidence on effectiveness, Lockard (1968) cites a study by the New Jersey Division Against Discrimination showing that among 54 firms that had been involved in discrimination cases that had resulted in adjustments in behavior, total employment was up 22 percent, but minority employment was up by 107 percent. Undermining the claims of effectiveness, though, Lockard argues that budgets for enforcement were low (although providing no basis for comparison), and notes

[22]The report notes that in New York at that time, if efforts at conciliation failed, a public hearing was held before members of the commission. If the respondent was found to have been engaged in discrimination, a cease and desist order enforceable by the state Supreme Court (which is under the state Court of Appeals) would be issued.

cases where top administrative positions were left empty (1968, pp. 82-4). Of course, one could argue that the EEOC has similarly experienced periods of weak enforcement of anti-discrimination laws at the federal level.

We have been able to find less information regarding enforcement of state laws requiring equal pay for men and women. According to a U.S. Department of Labor report discussing state equal pay laws (U.S. Bureau of Labor Standards, 1967), "The labor administrators of the respective States are generally responsible for enforcement of State equal pay laws. However, experience in the equal pay States indicates that once a law is enacted, with its subsequent publicity which serves an educational purpose, most employers comply voluntarily with its provisions" (p. 249). Although this may reflect nothing other than wishful thinking, the report does cite two cases–one in Michigan, and one in California–in which female employees who had suffered from wage discrimination were given financial awards under their states' equal pay laws.

Ultimately, then, we cannot definitively establish the level of enforcement of state anti-discrimination efforts, especially in comparison with later federal efforts. But it appears clear that there was indeed some anti-discrimination enforcement at the state level prior to the advent of federal anti-discrimination legislation.[23]

IV. Empirical Strategy

Basic Approach Using State-Level Variation

Our empirical strategy relies on a difference-in-difference-in-difference estimator, which estimates how changes in the dependent variables between sets of demographic groups differ between states that did and did not enact various anti-discrimination laws. We use data from the 1940, 1950, 1960, and 1970 Decennial Censuses of Population. Denote by Y_{ist} the dependent variable (alternatively employment and log earnings) for individual i in state s in year t. Denote by X_{ist} a vector of control variables, including age

[23]We should note that Chay's (1998) analysis also relies on the effectiveness of state laws. If these laws were ineffective, there would be no predicted difference between his treatment and control groups; if the state laws were very weak, the predicted difference would be correspondingly smaller.

and its square, residence in an urban area (SMSA), years of schooling, and marital status. Let I_s be a set of dummy variables corresponding to states, and I_t be a set of dummy variables corresponding to years. Let BF_{ist}, BM_{ist}, and WF_{ist} denote dummy variables for black females, black males, and white females. Restrict attention only to the two predominant types of laws, mandatory race discrimination laws (MRD_{st}) and sex wage discrimination laws (WDL_{st}). Finally, for the moment consider estimation for the 1940, 1950, and 1960 Censuses, hence using only the information on state anti-discrimination laws. We will make the simplifying assumption that state laws are largely the same across states; as Tables 1 and 2 show, this is not entirely true, but we do not have the requisite information (such as employer size) to exploit more detailed information on state laws.

We alternately look at black females, black males, and white females relative to white males. Thus, for example, when studying relative outcomes for black females and white males, we estimate specifications of the form:

$$(1) \qquad Y_{ist} = \alpha + \beta_{BF}BF_{ist} + \gamma_{BF}BF_{ist}\cdot MRD_{st} + \delta_{BF}BF_{ist}\cdot WDL_{st} + X_{ist}\beta_X + I_s\beta_s + I_t\beta_t + I_s\cdot I_t\beta_{st} + \theta_{BF,s}BF_{ist}\cdot I_s + \eta_{BF,t}BF_{ist}\cdot I_t + \epsilon_{ist} .$$

In this specification, the coefficient β_{BF} identifies the differences in Y between black females and white males common to all states and years (or more specifically for the reference year and state omitted from the set of year and state dummy variables). The coefficients on the state and year dummy variables, β_s and β_t, identify differences across states and years for white males, while the coefficients on the interactions between BF and the state and year dummy variables, $\theta_{BF,s}$ and $\eta_{BF,t}$, allow for state-specific demographic differences in levels of Y across states (common to all years), and across years (common to all states); the latter, for example, allows for different aggregate trends in earnings or employment of black females vs. white males. γ_{BF} then identifies the shift in the difference in Y between black females and white males associated with the enactment of race discrimination laws in particular states and years, and δ_{BF} identifies the shift in this difference associated with the enactment of sex wage discrimination laws in particular states and years.

Finally, note that the inclusion of the interactions between the state and year dummy variables ($I_s \cdot I_t$) in equation (1) implies that we do not identify the effects of these laws on Y for the reference group of white males. The focus is therefore on relative changes among demographic groups, paralleling the earlier time-series analyses in focusing attention on the effects of anti-discrimination policies on relative earnings and employment. We could identify the absolute effects on whites if we omitted $I_s \cdot I_t$, but this restriction would rule out shifts in Y across states and years for reasons other than anti-discrimination laws, a rather severe restriction over such long time horizons.[24] Of course, if we allowed interactions between $I_s \cdot I_t$ and the race-sex indicator variables, we would be unable to identify any policy effects.

As stated above, γ_{BF} and δ_{BF} in equation (1)–and the corresponding parameters γ_{BM}, γ_{WF}, δ_{BM}, and δ_{WF} when equation (1) is estimated for black males or white females, relative to white males–are difference-in-difference-in-difference estimators, identified from differences in the changes in relative economic position among demographic groups between states that did and did not enact various anti-discrimination laws. To see this, work with a simpler specification in which there is only one law (L), denote the minority group under study B, and ignore the variables in X. The specification then becomes:

$$(2) \qquad Y_{ist} = \alpha + \beta B_{ist} + \gamma B_{ist} \cdot L_{st} + I_s \beta_s + I_t \beta_t + I_s \cdot I_t \beta_{st} + \theta_s B_{ist} \cdot I_s + \eta_t B_{ist} \cdot I_t + \epsilon_{ist} .$$

The difference between the changes in relative economic position among blacks (B=1) and whites (B=0) between states that did (L=1) and did not (L=0) enact various anti-discrimination laws can then be written as follows:

$$(3) \qquad \{E(Y_{is't'}|B_{is't'}=1,L_{s't'}=1) - E(Y_{ist'}|B_{ist'}=1,L_{st'}=0)\}$$
$$- \{E(Y_{is't}|B_{is't}=1,L_{s't}=0) - E(Y_{ist}|B_{ist}=1,L_{st}=0)\}$$
$$- [\{E(Y_{is't'}|B_{is't'}=0,L_{s't'}=1) - E(Y_{ist'}|B_{ist'}=0,L_{st'}=0)\} - \{E(Y_{is't}|B_{is't}=0,L_{s't}=0) - E(Y_{ist}|B_{ist}=0,L_{st}=0)\}] .$$

In this specification, the first difference in curly brackets is the difference in (the conditional expectation of) Y between blacks in states that passed a race discrimination law (state s'), after the law was passed (year t'), and Y for blacks in states that did not pass a law (s), in the same year (t'). This

[24]The inclusion of $I_s \cdot I_t$ also provides an alternative to correcting the standard errors to allow for non-independence within state-year cells.

simple-difference estimate of the effect of L can be computed from a regression with one year of data, using only observations on the minority group B. Clearly this regression can include neither state dummy variables, year dummy variables, nor their interactions with each other or with B.

This cross-sectional, simple-difference estimator may give us a spurious result if, for example, Y tends to be higher in the states indicated by s' than those indicated by s (i.e., if blacks in the states that pass anti-discrimination laws tend to earn more than blacks in states that do not pass laws). To control for this possibility, equation (3) shows that the difference-in-difference-in-difference estimator next subtracts off the second expression in curly brackets, which is the difference in Y for blacks across states that later do pass these laws and states that do not, prior to the passage of the race discrimination law, yielding a difference-in-difference estimator. This difference-in-difference estimate of the effect of L can be computed from a regression with multiple years of data, but still using only observations on the minority group B. This regression allows the inclusion of state dummy variables and year dummy variables, but not their interactions with each other or with B.

The difference-in-difference estimator, though, can give spurious results if the trend in Y is different in states s' and s (e.g., if earnings in states that passed laws were growing at a different rate than in other states). We control for this possibility by subtracting off the entire expression for white males, given by the third expression in equation (3) (in square brackets). This estimator, of course, parallels equation (2) in adding the state-year interactions, and the interactions of state and year dummy variables with B. This difference-in-difference-in-difference estimator then finds an effect of the race discrimination law only if Y changed differently over time for blacks in states that did and did not pass race discrimination laws, and only if this differential change differs between blacks and whites. Finally, using equation (2) to substitute for the eight conditional expectations in (3)[25] reveals that this difference-in-difference-in-difference estimator is precisely γ in equation (2) (or, similarly, γ_{BF} and δ_{BF} in equation (1)).

As this discussion suggests, equation (1) is a relatively unrestrictive specification, identifying the

[25]For example, $E(Y_{is't'}|B_{is't'}=1,L_{s't'}=1) = \alpha + \beta + \gamma + \beta_{s'} + \beta_{t'} + \beta_{s't'} + \theta_{s'} + \eta_{t'}$.

effects of changes in race and sex discrimination laws while allowing for fixed state differences across the races or sexes, different trends for each race-sex group, and arbitrary shifts across states and years in the absolute levels of the dependent variable. In the presentation of the empirical results, we will show how relaxing the specification to allow this flexibility (going from the simple-difference to the difference-in-difference, and then to the difference-in-difference-in-difference estimator) affects the inferences. Note, however, that the specification does not interact the race-sex indicators with the control variables in X, and hence does not identify how the policy effects vary with other observable characteristics. The specification we estimate, then, is equivalent to asking how race and sex discrimination laws shift the race and sex dummy variables in regressions pooled across demographic groups with indicators for those groups.

Adding Information on Federal Legislation

When we add data from the 1970 Census, all states and years are coded as having MRD_{st} and WDL_{st} equal to one for that year.[26] Suppose we simply add the 1970 data, and estimate equation (1) with these variables set to one. If state policies and federal policies were identical, the interpretation of this specification would be straightforward, as the 1970 data simply provide more information on the effects of uniform race and sex discrimination laws.

However, there are likely to be some differences in practice between the federal and state laws and policies, stemming from differences in things such as sizes of employers covered, enforcement, awareness of the law, etc. Moreover, federal laws are marked by more fundamental changes, including the beginnings of federal affirmative action, and, with respect to sex, Title VII (and the effective repeal of protective legislation), which went well beyond the equal pay laws that had prevailed at the state level.

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[26]One could argue that prior to the EEOA, federal legislation was not effective, and should perhaps be "coded" as characterized by weak enforcement. This is not a completely untenable position a priori, but in our view is inconsistent with some of the existing evidence. In particular, if the law was non-binding until 1972, then there is no reason Chay (1998) should have found differential effects for small and large employers beginning in 1972. Similarly, Heckman and Payner (1989) and Donohue and Heckman (1991) report gains for blacks in South Carolina and the rest of the South in the mid-1960s, and also provide evidence of EEOC activity prior to 1972 targeting the South.

These changes suggest that we have to consider federal laws that are broader than state laws, and ask what the addition of the 1970 data contributes in this case.

The first implication of federal laws being broader than states laws is that, because equation (1) includes interactions between the race-sex indicators and the year dummy variables, we cannot identify common effects of the federal policies across all states–for example, the extension of the protection against sex discrimination in employment in Title VII. Instead, what we identify from the policy variation induced by the federal legislation are the differential changes in states that went from having no policy to being bound by federal policy, compared with states that went from being bound by state policy to being bound by federal policy. The difference between these is the effect of the component of the federal policy that is similar to that in states that had previously enacted anti-discrimination policies. For example, despite the fact that federal legislation regarding sex discrimination enacted between 1960 and 1970 included both the Equal Pay Act and Title VII, the 1970 data provide us with identifying information regarding the Equal Pay Act–paralleling earlier state equal pay laws–because this component of federal anti-discrimination policies represents the part that does not change in common across all states.

To see this point more explicitly, consider equation (2) with the data now augmented to include the 1970 Census. To set the stage, consider first the case in which the federal and state legislation are identical, so the only difference is that L_{st} is set to one for all observations from the 1970 Census. To see that we get identifying information regarding γ from the extension of federal legislation to states that did not have race discrimination laws, we return to equation (3), letting t denote 1960, t' denote 1970, s denote a state that did not have a race discrimination law in 1960, and s' denote a state that did. To draw out this point, it is easiest to first rearrange the terms in equation (3) as follows:

$$(4) \quad [\{E(Y_{is't'}|B_{is't'}=1,L_{s't'}=1) - E(Y_{is't}|B_{is't}=1,L_{s't}=0)\} - \{E(Y_{is't'}|B_{is't'}=0,L_{s't'}=1) - E(Y_{is't}|B_{is't}=0,L_{s't}=0)\}]$$
$$- [\{E(Y_{ist'}|B_{ist'}=1,L_{st'}=0) - E(Y_{ist}|B_{ist}=1,L_{st}=0)\} - \{E(Y_{ist'}|B_{ist'}=0,L_{st'}=0) - E(Y_{ist}|B_{ist}=0,L_{st}=0)\}]$$

which expresses the difference-in-difference-in-difference estimator as a comparison between treatment and control states, subtracting the relative black/white change in Y for the control group from the same

change for the treatment group. Then returning to the case of the federal laws, this equation becomes:

$$(5) \quad [\{E(Y_{is't'}|B_{is't'}=1,L_{s't'}=1) - E(Y_{is't}|B_{is't}=1,L_{s't}=1)\} - \{E(Y_{is't'}|B_{is't'}=0,L_{s't'}=1) - E(Y_{is't}|B_{is't}=0,L_{s't}=1)\}]$$
$$- [\{E(Y_{ist'}|B_{ist'}=1,L_{st'}=1) - E(Y_{ist}|B_{ist}=1,L_{st}=0)\} - \{E(Y_{ist'}|B_{ist'}=0,L_{st'}=1) - E(Y_{ist}|B_{ist}=0,L_{st}=0)\}] \ ,$$

which differs from equation (4) in that states s' have a race discrimination law in effect for both periods (first state, then federal), while states s have one only in the latter period t'. Intuitively, the "treatment group," which is represented by the second expression in square brackets in equation (5), is now the set of states where the law first takes effect in t' (those states that had no state law), while the control group is those states that initially had their own law, superceded in t' by the federal law. It is again easy to substitute for the eight conditional expectations in (5) using equation (2), to show that this difference-in-difference-in-difference estimator is $-\gamma$, and that γ is identified from the expressions for states s, as we would expect, so the 1970 data on these states provide additional identifying information on γ.[27]

Now return to the case at hand, where the federal law is more broad than the state law, so that the federal law effectively becomes binding in all states once it is passed, and supercedes the state law. We define a single dummy variable LS for state anti-discrimination laws exclusively, and a dummy variable for federal laws, LF. LS is set to zero when LF is set to one.[28] Augmenting equation (2) to include these variables, we have:

$$(6) \quad Y_{ist} = \alpha + \beta B_{ist} + \gamma^S B_{ist}\cdot LS_{st} + \gamma^F B_{ist}\cdot LF_t + I_s\beta_s + I_t\beta_t + I_s\cdot I_t\beta_{st} + \theta_s B_{ist}\cdot I_s + \eta_t B_{ist}\cdot I_t + \epsilon_{ist} \ .$$

Prior to 1970, LF_t is zero for all observations, so the discussion surrounding equation (3) applies to the identification of γ^S with the data through 1960. As above, continue to let t denote 1960, t' denote 1970, s denote a state that did not have a race discrimination law in 1960, and s' denote a state that did. Then the difference-in-difference-in-difference in equation (3) using the 1960 and 1970 data becomes:

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[27]This case is most analogous to Chay's (1998), although he considers the extension of federal law that "caught up" with laws existing in some states.

[28]We cannot simply define a dummy variable LF for the federal legislation set equal to one in 1970 for all states, without linking its definition to LS. This would be equivalent to simply trying to estimate the effect of some unspecified federal policy on relative black-white outcomes, which of course cannot be identified given that we include year dummy variables and their interaction with race.

(7) $[\{E(Y_{is't'}|B_{is't'}=1,LS_{s't'}=0,LF_{s't'}=1) - E(Y_{is't}|B_{is't}=1,LS_{s't}=1,LF_{s't}=0)\}$

$- \{E(Y_{is't'}|B_{is't'}=0,LS_{s't'}=0,LF_{s't'}=1) - E(Y_{is't}|B_{is't}=0,LS_{s't}=1,LF_{s't}=0)\}]$

$- [\{E(Y_{ist'}|B_{ist'}=1,LS_{st'}=0,LF_{st'}=1) - E(Y_{ist}|B_{ist}=1,LS_{st}=0,LF_{st}=0)\}$

$- \{E(Y_{ist'}|B_{ist'}=0,LS_{st'}=0,LF_{st'}=1) - E(Y_{ist}|B_{ist}=0,LS_{st}=0,LF_{st}=0)\}]$.

The first difference expression in square brackets measures the difference in the black-white gap in states that had an explicit state law in year t, and the federal law in year t'. The second difference in square brackets is the difference for states that did not have an explicit state law in year t, but were subject to the federal law in year t'. As the first difference captures the effect of imposing the federal law in lieu of the state law (i.e., the effect of the federal law "minus" the effect of the state law), while the second difference captures the effect of imposing the federal law where there was no state law, the difference between them again identifies the effect of the state law. This can be seen readily by using equation (6) to substitute for the eight conditional expectations in equation (7). Eliminating redundant terms, equation (7) reduces to:

(8) $[\{\gamma^F - \gamma^S\}] - [\gamma^F] = -\gamma^S$.

Thus, returning to the case of sex discrimination laws, we cannot identify the effect of the imposition of federal laws unrelated to equal pay, since these are imposed on all states simultaneously, and we have allowed different trends for women and men. But the 1970 data do provide us with more information on the impact of the component of federal legislation that is equivalent to the state equal pay laws that had been implemented prior to the Act. This makes intuitive sense, in that we identify with the 1970 data the differential impact of anti-discrimination laws in 1970–precisely the extension of the "state-like" equal pay component of the federal law to states that previously had no law. Conversely, this specification provides no information on the effects of federal legislation, which is apparent because B·LF in equation (6) is perfectly collinear with the interaction between B and the 1970 dummy variable.

This argument is predicated on the behavior targeted by the policy having a similar effect across states, and the application of federal law being applied with equal vigor across all states. We noted earlier

that Donohue and Heckman (1991) document that federal policy efforts were targeted towards the South, perhaps because more discrimination was occurring there, in which case we might expect larger effects from the 1970 "experiment." As a consequence of this consideration, we explore the robustness of our results to excluding Southern states from the analysis.

The final case we have to consider is when the state law is more broad than the federal law. One example would be state anti-discrimination laws that do not exempt employers based on size (the type of variation exploited by Chay (1998)). In this case, the federal law only extends a different type of statute to states without their own legislation, while in the other states the federal law is non-binding. Thus, we set LF in equation (6) equal to one in year t', only for states (s) that had no law in year t. For the states (s') that had a law, we leave LS equal to one (including in year t'), and set LF equal to zero. Then equation (7) becomes:

$$
\begin{aligned}
(9) \quad & [\{E(Y_{is't'}|B_{is't'}=1,LS_{s't'}=1,LF_{s't'}=0) - E(Y_{is't}|B_{is't}=1,LS_{s't}=1,LF_{s't}=0)\} \\
& \quad - \{E(Y_{is't'}|B_{is't'}=0,LS_{s't'}=1,LF_{s't'}=0) - E(Y_{is't}|B_{is't}=0,LS_{s't}=1,LF_{s't}=0)\}] \\
& - [\{E(Y_{ist'}|B_{ist'}=1,LS_{st'}=0,LF_{st'}=1) - E(Y_{ist}|B_{ist}=1,LS_{st}=0,LF_{st}=0)\} \\
& \quad - \{E(Y_{ist'}|B_{ist'}=0,LS_{st'}=0,LF_{st'}=1) - E(Y_{ist}|B_{ist}=0,LS_{st}=0,LF_{st}=0)\}] \ .
\end{aligned}
$$

Intuitively, in this case we should identify the effect of the federal legislation, since nothing changes in the states s' (which therefore serve as a control group), while the federal legislation is enacted in the states s. This is easily verified by using equation (6)–with LS and LF modified as above–to substitute into equation (9), which yields $-\gamma^F$. So in this case the extra year of data provides no more information on the effects of the state legislation, but does provide information on the effects of the federal legislation.

Thus, the information we get from the addition of data for 1970 depends on whether we should characterize the federal legislation as in general stronger or weaker than the state legislation. There is no way to definitively answer this question, and given the variation in state laws, one might even argue that we cannot characterize state laws as a whole versus federal laws. We believe, though, that there is a

stronger case for viewing the federal legislation as broader and more encompassing, in which case the 1970 data provide additional information on the effects of state laws. In this case, we can use information through 1960, using dummy variables only for state laws, and through 1970, using dummy variables for federal and state laws as defined in equation (6), to identify the effects of state anti-discrimination laws or the components of them that are incorporated into federal law. If one is skeptical regarding this characterization of the federal versus state legislation, the evidence based on the data through 1970 can be ignored. We do not, however, report evidence based on the assumption that the state laws were stronger and more broad than the federal laws. Rather, the preceding discussion is intended to clarify under what conditions the 1970 data give us more information on state anti-discrimination laws, and to clarify what policy effects we are estimating.

Limitations of the Analysis

Aside from these substantive issues of experimental design and identification, there are some potential reservations regarding our empirical analysis. First, to the extent that there are state-specific factors unrelated to the policy changes driving relative economic status by race or sex, our identification strategy is no better than that in the time-series studies. But we regard it as unlikely that the problem of contaminated estimates from state-specific changes is as serious as the related problem at the aggregate level, at which there are some rather clear changes that coincided with the federal legislation and subsequent anti-discrimination activity.

Second, one might expect state statutes to be somewhat more endogenous than federal ones. As an example, Chay (1998) suggests that state laws are "more likely to be passed in states where prejudice is relatively low" (p. 612). This might result in the data through 1960 providing weaker evidence of beneficial effects of anti-discrimination laws than the data through 1970, when the laws were extended to more discriminatory states where the impact of the laws might consequently be larger.[29] On the other hand, as noted earlier, Landes (1968) suggested that the endogeneity bias may run in the opposite

[29]Our regression models allow the race or sex gap to vary by state, but not the policy effect to vary by state.

direction, with states with more serious discrimination problems passing laws earlier and directing more resources toward their enforcement. We address this issue to the limited extent possible, after describing the empirical results.

Third, the Census data we use have relatively limited information on workers' characteristics that may be productivity related. A lack of information on experience and tenure is likely the biggest problem. For example, O'Neill (1985) suggested that comparisons of men's and women's wages in the 1970s were contaminated by declining experience of women stemming from a large number of new entrants. Similarly, Cunningham and Zalokar (1992) suggested that black-white comparisons for women in earlier years were problematic because of unmeasured experience differentials, as "white women's labor force participation rates increased steadily after 1940, likely bringing inexperienced white female workers into the labor market ..., whereas black women's labor force participation rates increased much more slowly" (pp. 544-5). This is an inherent limitation of Census data. On the other hand, it points to an advantage of the state-level analysis. As long as unmeasured changes of women relative to men, for example, are similar across states, they are picked up in the interactions between the year and demographic group dummy variables, and do not affect the estimated effects of anti-discrimination laws; in contrast, the earlier time-series studies cannot control for spurious correlations between policy changes and changes in unmeasured variables.

Overall, while one can point to limitations of our analysis, we believe that our empirical strategy, at a minimum, provides important complementary evidence to that in the existing literature on the impact of anti-discrimination laws on the relative economic status of blacks and women.[30]

Data

Aside from the numerous sources used to assemble the information on anti-discrimination laws, we rely on individual-level data from the 1940-1970 U.S. Censuses of Population. For all of the equations

[30]Failure to find evidence of these effects can reflect either ineffectiveness of the anti-discrimination laws, or no prior discrimination. The first interpretation of the null hypothesis is the common one in the literature on anti-discrimination laws.

we estimate, the sample is restricted to those who are black or white, and aged 18 to 70. We drop those in the armed forces, self-employed workers, and unpaid workers. Also, when we study earnings, we exclude agricultural and private household workers in order to focus on more standard, formal work arrangements, and those for which earnings are well-defined.[31] When we study earnings, we further restrict the sample to individuals who are closer to full-time, full-year workers, excluding those who worked less than 27 weeks per year or 30 hours per week. We do this in part because the Census data provide us with limited information with which to correct for weeks and hours variation in studying earnings. We drop those with hourly wages–based on half-time, half-year work–below $1 (in 1980 dollars). Appendix Table A2 provides information on some of these constraints. The restriction to non-agricultural workers has more bite for all workers (except white females) early in the sample, and especially for black males. The exclusion of private household workers cuts out a substantial portion of the sample of black women–over 68 percent in 1940.[32] While we think these restrictions are useful in carrying out the empirical analysis, their large effects for some groups of workers suggest that they may influence the results. Below, therefore, we also report results when agricultural and private household workers are included in the sample.

Descriptive statistics for the four demographic groups, for the dependent variables in each Census year, are reported in Table 4. The table reveals the generally much higher employment rates of men than of women, although the race differential differs for men and women (with the employment rate higher for black than for white women, but lower for black than white men). The rise in the absolute and relative employment rate of women over the sample period is clear. The earnings means reveal well-known differentials, with a large gap favoring men over women, and sizable race differences. The sex difference is relatively stable over time, whereas the race difference narrows over time, especially for women.

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[31]Cunningham and Zalokar (1992) note that in earlier decades in-kind pay was common for private household workers and farm laborers.

[32]Goldin (1990, p. 74) and Cunningham and Zalokar (1992, p. 540) provide roughly similar figures.

Theoretical Expectations

Prior to presenting the evidence, we briefly outline some theoretical expectations, which provide a basis for interpreting the results. As outlined in Section II, the existing literature looks at the impact of legislation protecting a particular demographic group (such as black males), on economic outcomes of this group relative to the majority group (typically white males). In the case of race discrimination laws, the predicted effects are relatively clear. These laws prohibit discrimination in hiring, dismissals, terms of employment, etc. For the most part, they should therefore boost the employment of the protected minority–their direct, intended effect. There are two possible exceptions. One is if the principal effect–because of the nature of past discrimination, or difficulties of enforcing prohibitions of different types of discrimination–is to enforce equal pay for equal work. In a Becker-type model of employer discrimination, the pay gap reflects discriminatory tastes, and an equal pay constraint acts like a relative price increase for the protected group, which could reduce their employment.[33] We regard this as unlikely with respect to race because of the broader provisions of race discrimination laws. The second exception is if the principal effect of race discrimination laws turns out to be increased difficulties of dismissing protected workers, a cost-increasing effect that could in principle deter hiring. Overall, though, we regard the predicted effect of race discrimination laws on relative black employment as most plausibly positive. On the other hand, the sex discrimination laws primarily concern equal pay, without employment protection. Because these laws therefore increase the relative price of female labor, they are predicted to reduce relative female employment.[34]

As explained in the previous section, we also estimate the effects of race discrimination laws on relative female-male outcomes, and of sex discrimination laws on relative black-white outcomes. Even if

[33]In this model, as black employment falls, the marginal employer is less discriminatory, so the wage gap shrinks.

[34]In their separate analyses of the Americans with Disabilities Act (ADA), which also effectively raised the relative price of the affected group through the Act's "reasonable accommodation" provision, Acemoglu and Angrist (1998) and DeLeire (2000) find negative employment effects of the ADA for disabled workers.

we assume that workers in different demographic groups–at least conditional on the control variables included in our regressions–are substitutes, there are no clear predictions for the "cross" effects. For example, the increase in the relative price of female labor owing to sex discrimination laws should boost demand for white male and black male workers, all else the same, but we do not know how it will affect the relative demand for these two groups of male workers. We therefore regard the cross-effects as ambiguous.

In principle, of course, the general equilibrium effects of race and sex discrimination laws can go in a number of directions, and evidence against the predictions laid out here may suggest more complex effects of these laws. In addition, complementarity relationships among workers of different types could undermine the directions of these expected effects. In the meantime, though, the "first-order" predictions of the effects of these laws on employment are listed in the first column of Table 5.

With regard to earnings, the simpler prediction is for the effects of sex discrimination laws. These laws are expected to boost relative earnings of women within jobs, although they could conceivably affect the distributions of women and men across jobs (if, for example, discriminatory tastes are stronger regarding the employment of women in particular occupations or industries).[35] With the Census data, the best we can do to mimic the "within-job" test is to include industry and occupation dummy variables.[36] If race discrimination laws only covered employment, but not compensation, they might lower black relative earnings; for example, in the Becker employer discrimination model, higher employment likely extends the employment of blacks to a more discriminatory marginal employer. However, because these laws also regulate terms of employment, we expect them to have similar effects on pay as equal pay laws, and hence to raise relative black earnings. Again, the predictions are summarized in Table 5, in this case in the second column.

V. Results

[35]We look at workers who are mainly full-time and full-year, to pick up primarily wage variation rather than hours or employment variation.

[36]We use two-digit codes.

Evidence from a "Quasi-Time-Series" Experiment

Prior to turning to estimates of the specifications described in the previous section, we report estimates that use the Census data but mimic the existing, earlier time-series studies. In particular, we ignore information on state anti-discrimination laws, and instead simply try to infer the effects of federal anti-discrimination legislation from changes in employment and earnings of black females, black males, and white females, relative to white males, during the period when this legislation was implemented. This is done by estimating our equations using the 1940-1970 Census data, including only the individual-level controls, state and year dummy variables, and interactions between year indicators and a dummy variable for the minority group considered. Paralleling the time-series studies, the effects of federal legislation are inferred from the estimated coefficients of the variable interacting the year dummy variable capturing the 1960s (i.e., Year = 1970) with an indicator for the minority group considered. This latter interaction measures the change in, for example, black female employment relative to white male employment between 1960 and 1970. This can be compared with any changes in these relative employment rates across earlier decades, with a discrete jump or an acceleration providing evidence of an effect of the federal legislation.

The estimated coefficients of the interactions between the minority or female group indicators and the year dummy variables are reported in Table 6. The year dummy variables are defined to equal one in and after the indicated year, in which case the interaction measures the change in relative outcomes in that year, relative to the previous Census year. Looking first at employment, for black females we see a modest decrease in the relative employment rate from 1940 to 1950, an increase of .038 from 1950 to 1960, and a larger increase of .070 from 1960 to 1970. This evidence is consistent with black female employment increasing in relative terms prior to the federal legislation, but accelerating in the decade in which it was passed. The results for white females are similar (except for the absence of a decline from 1940 to 1950). In contrast, the results for black males reveal if anything declines in relative employment up to 1960, and

no increase from 1960 to 1970.[37]

Turning to earnings, for black females and black males the evidence indicates rather large swings, with large increases in relative earnings from 1940 to 1950, some declines from 1950 to 1960, and then increases (of .110 for black females, and .041 for black males) from 1960 to 1970. The 1940 to 1950 change reflects the "Great Compression" of general wage inequality as well as black-white earnings differences, documented elsewhere (Margo, 1995; Goldin and Margo, 1992).[38] For white women, in contrast, there is evidence of only a tiny increase in relative earnings from 1960 to 1970.

The evidence in Table 6 makes two points. First, for the most part relative economic outcomes for blacks and women improved from 1960 to 1970, which is consistent with beneficial effects of federal anti-discrimination policies. On the other hand, changes were in some cases occurring in earlier decades, and the earlier changes were sometimes larger. Altogether, these findings make it difficult to draw strong conclusions regarding the effects of federal anti-discrimination policies enacted in the 1960s.[39] Even if the changes in all other decades were zero, and in the expected direction from 1960 to 1970, alternative explanations would be possible. But the estimates reported in Table 6 are far from that scenario, which, along with the earlier time-series evidence, emphasizes that it is difficult to draw inferences regarding the effects of anti-discrimination policies solely from changes over time in relative outcomes for protected groups, which helps to motivate our analysis of state-level anti-discrimination laws.

Effects of State Anti-Discrimination Laws: Employment

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[37]This does not contradict earlier time-series evidence. Most of this evidence (e.g., Vroman, 1974; Brown, 1982; and Freeman, 1973) focuses on relative earnings. In addition, graphs of changes in relative black-white employment over the 1960s do not indicate large shifts, and the attrition from the labor force of less-skilled blacks over this period has been documented (Donohue and Heckman, 1991).

[38]According to Margo (1995), this compression was due to numerous factors, including a decline in residual wage inequality favoring lower-skill workers, other changes in the wage structure favoring lower-skill workers, migration, shifts in demand stemming from World War II, and changes in school quality. Our regression estimates should reflect most of these, except perhaps the effects of cross-state migration, since we include state dummy variables.

[39]As Margo (1995) states, if anti-discrimination legislation played a strong role in racial wage convergence in the 1960s, "how did black workers manage such impressive relative wage gains in the 1940s, well before the modern civil rights movement bore its fruit?" (p. 470).

Evidence of the effects of state anti-discrimination laws on relative employment and earnings are reported in Tables 7 and 8, respectively. In each table, we first report the simple-difference estimates for the sample of all states, using Census data for 1960,[40] followed by the difference-in-difference estimates using Census data for 1940 and 1950 as well. We then report the difference-in-difference-in-difference estimates for these years, which we regard as the most reliable causal estimates because that they are based on a less restrictive specification that is far less likely to reflect spurious associations. After that, we explore the implications of excluding the Southern states, and adding the 1970 Census data to incorporate additional information on the effects of state laws from the extension of federal anti-discrimination laws to all states.

Estimates of the employment effects of state anti-discrimination laws are reported in Table 7. The simple-difference estimates, computed for the minority or female group exclusively, and reported in column (1), suggest that race discrimination laws boosted the employment of black females but not black males, and lowered slightly the employment of white females. In contrast, sex discrimination laws are associated with lower employment of black females and males, with perhaps implausibly large effects, and not significantly associated with employment of white females. However, the simple-difference estimates reflect only the cross-sectional association between state laws and employment rates for these groups. As a first step toward teasing out a causal effect of state anti-discrimination laws, column (2) reports difference-in-difference estimates, which ask how changes in employment rates for these groups are associated with the implementation of anti-discrimination laws. The difference-in-difference estimates are quite different in some respects. The strong negative coefficients on sex discrimination laws in the equations for black female and black male employment disappear, replaced by positive coefficients, although much larger for black males than for black females. On the other hand, there is now stronger evidence of a positive employment effect of race discrimination laws for black males. Looking at white females, there is stronger evidence of a negative employment effect of race discrimination laws, but also

[40]As noted in the footnotes to Table 7, states with very few observations on blacks were omitted; we still refer to "all" states for expositional ease.

now evidence of a modest negative employment effect from sex discrimination laws (regarding equal pay).

The remaining columns of the table report difference-in-difference-in-difference estimates, which provide estimates relative to white males, allowing arbitrary differences across states and over time for the minority or female workers we study, and arbitrary state trends. In these estimates, we also report the estimated coefficient on the dummy variable indicating the minority group, for reference purposes. For example, the estimates for black female relative employment indicate that after controlling for the other variables in the employment equation, their employment rate falls short of that of white males by .293.[41] Referring to column (3), and looking first at the direct effects, we find no evidence of relative employment effects of race discrimination laws for black females, while the effect for black males is positive but only significant at the ten-percent level. The estimated effect of sex discrimination laws is to reduce the relative employment of females of both races. Turning to the cross-effects, the evidence suggests that race discrimination laws lower relative employment of white females, while sex discrimination laws boost relative employment of black males, both of which are consistent with substitution between black males and white females.

The final columns alternately drop Southern states, and add data for 1970 (when the federal legislation extended anti-discrimination laws to all states). Some of the results are rather sensitive to this. In particular, the cross-effects of sex discrimination laws on black males, and of race discrimination laws on white females, are not robust, so no firm conclusions can be drawn. When the data for 1970 are added, the evidence of positive employment effects of race discrimination laws for black females and black males becomes stronger, and is significant and of similar magnitude whether or not Southern states are included. As we explained earlier, this should not be attributed to stronger federal than state laws, but rather to the effects of the components of state legislation that are also captured in the later federal legislation although identified from a different set of states–namely those states that had not passed their own statutes prior to the federal legislation. If these latter states had more discriminatory environments, this may explain the

[41]In each case this is for the omitted reference group of observations in New York in the most recent Census year used.

larger effects. On the other hand, the fact that the estimate is the same for black males whether or not Southern states are included, and larger for black females when the Southern states are excluded, runs against this interpretation.

One result that is robust across all of the alternative samples is the negative effect of sex discrimination laws on the relative employment of both black females and white females. Across the eight estimates in columns (3)-(6), the estimated effect is always negative, and is significant in seven out of eight cases, although the effect is stronger when the Southern states are included.

Effects of State Anti-Discrimination Laws: Earnings

Table 8 reports the results for earnings. As can be seen in column (1), the simple-difference estimates indicate that earnings of all three protected groups are much higher in states that had such laws as of 1960. In contrast, in the difference-in-difference estimates these apparent positive effects disappear. Instead, the estimates in column (2) suggest that race and sex discrimination laws lowered earnings of all three protected groups, as evidenced by both the direct effects and the cross-effects of these laws.

The difference-in-difference-in-difference estimates, however, provide a different view, and one that is more consistent with other work. First, race discrimination laws are estimated to have a positive effect on the relative earnings of black females, a relationship that appears regardless of whether Southern states are excluded or data from 1970 are included, although for the non-Southern states including 1970 the estimated effect becomes insignificant. For black males, however, there is no consistent evidence of positive (or negative) effects of race discrimination laws on relative earnings, which is a rather different conclusion from the time-series evidence.

Turning to the effects of sex discrimination laws, for white females the evidence is mixed, depending on whether Southern states or data from 1970 are included. The most consistent evidence is for the non-Southern states, indicating positive relative earnings effects of sex discrimination laws whether or not data from 1970 are included. On the other hand, for black females there is significant evidence of positive earnings effects only when data are used through 1970 and Southern states are included.

Note that despite the robust evidence of negative employment effects of sex discrimination laws on the relative employment of black females and white females in Table 7, Table 8 does not provide robust evidence of positive earnings effects for these groups, which was thought to be the trigger for the employment effects. In fact, for white females the stronger employment effects were apparent when Southern states were included, whereas the evidence of positive earnings effects emerges only when the Southern states are excluded. We are not strongly troubled by this lack of correspondence of the results, however. First, we believe the earnings estimates for women may suffer from non-trivial selection biases, as in the early years studied in this paper employment rates of women were very low; unfortunately, we do not believe there is a compelling solution to this problem, as the Census does not provide very rich data with which to identify a selection model. Second, the effect of sex discrimination laws regarding wages should be primarily within jobs within establishments, and we can only crudely approximate this experiment with the inclusion of occupation and industry dummy variables. Indeed, in research using matched employer-employee data based on the 1990 Census, Bayard, et al. (2000) find that a substantial component of sex wage differentials arises within occupations within establishments, rather than simply across industries and occupations. Thus, the data we study in this paper may be perfectly consistent with sizable reductions in within-establishment, within-occupation sex wage differentials, coupled with a failure to find strong evidence that women's wages increased relative to those of men in broadly similar occupations and industries.[42]

A further puzzle with respect to white women's earnings is that race discrimination laws also appear to boost the relative earnings of white females, with magnitudes sometimes only a bit smaller than

[42]As noted above, we excluded agricultural workers and private household workers from the sample, to focus on the formal sector where pay is unlikely to be in-kind. As Appendix Table A2 showed, this restriction drops a high fraction of black females in the early part of the sample period. We re-estimated the models for earnings including these workers. (Occupation and industry are undefined for the non-employed, so the employment models impose no occupation or industry restrictions.) The results were qualitatively unchanged, except that for black females there was no longer significant evidence of positive relative earnings effects, and the estimates using data for 1940-1960 were close to zero. This is not entirely surprising, as we would not expect anti-discrimination laws to have much effect in this sector, and we would expect to see little effect reflected in pay if a considerable share of it is in-kind. (See Appendix Table A3.)

the effects of race discrimination laws on the relative earnings of black females. Two possible interpretations are that white females are complementary with black men and women–although this seems far from obvious–or that race discrimination laws led to an overall demand shift against white male workers. But in only two of the four specifications in Table 7 was there evidence that race discrimination laws boosted white female relative employment. Alternatively, the positive effects of race discrimination laws on black women's relative earnings may have spilled over to white women's relative earnings.

As Tables 1-3 make clear, a number of states implementing prohibitions on race discrimination were also states that barred sex discrimination, although the overlap is far from perfect. This raises the question of whether the estimated effects of race discrimination laws would differ if the information on sex discrimination laws were omitted (as, e.g., in Landes, 1968), and vice versa. We re-estimated all of the difference-in-difference-in-difference estimates including information only on one type of law or the other. (See Appendix Table A4.) In general, the estimates were qualitatively similar. However, the one instance in which this mattered was with respect to the estimated effects of sex discrimination laws on relative earnings of white women. In particular, with the race discrimination laws omitted, the estimated effects of sex discrimination laws on white women's relative earnings were uniformly positive and statistically significant, which suggests a less ambiguous conclusion regarding the effects of sex discrimination laws on the relative earnings of white females. Nonetheless, given that race discrimination laws appear to boost the relative earnings of white females (Table 8), this contrast suggests that it is necessary to control for both types of legislation in inferring the effects of sex discrimination laws on women's relative earnings.

Given the existing literature, the absence of any evidence of positive earnings effects of race discrimination laws for black males is perhaps the most surprising result. One possible explanation is that such an effect is obscured because we use individuals of all ages (18-70). It seems plausible that older black men were less affected by these laws than younger black men, in part because of lower ability to take advantage of increased opportunities (including, but not limited to, human capital investment). In addition, if enrollment rates responded positively to race discrimination laws, otherwise higher-earning blacks at

relatively young ages may have selected out of the sample (temporarily), also potentially obscuring wage-enhancing effects of race discrimination laws. To address both of these issues, we re-estimated the core difference-in-difference-in-difference models using samples restricted to those aged 25-34, in the process eliminating older individuals, as well as younger ones most likely to be enrolled in school.

These estimates are reported in Table 9. Most of the estimates are qualitatively similar to the corresponding ones in Tables 7 and 8, with some differences. Highlighting some of these, and looking first at employment, the estimated positive effects of race discrimination laws for black women are stronger when the 1970 data are used, whereas the effects of sex discrimination laws in the data through 1960 are still negative, but statistically weaker. For black men, the previous statistically significant evidence of positive employment effects using the data through 1970 no longer appears. Turning to earnings, the positive effects for black women are stronger. In contrast, the estimated earnings effects for black men, which to a large extent motivated this analysis, are similar to those in Table 8, and indicate no positive effects. Thus, this result is not attributable to the inclusion in the analysis of older individuals or those for whom enrollment decisions may play a role; rather, the lack of positive relative wage effects for black men appears to be a robust finding in our statistical experiment.

Interpreting Variation in Effects of Race Discrimination Laws Across Time/Regions

The empirical approaches in this paper and much of the existing research on the effects of race and sex discrimination legislation take the advent of such legislation as exogenous. As noted earlier, the potential endogeneity of this legislation–perhaps more so at the state than at the federal level–raises some questions about the interpretation of the results. In particular, the positive effects of race discrimination laws on employment of black men and women appear only when data are used through 1970, or in other words only when the effects of anti-discrimination legislation are identified in part from the advent of the federal legislation. One possible interpretation of this is that states that passed their own laws prior to 1960 were those in which relative employment of blacks was already relatively high, so that state laws to some extent ratified the status quo or at least had smaller effects because there was less discrimination to

begin with, whereas federal laws served as more severe constraints on behavior and hence led to larger increases in the relative employment of blacks.

To address this question, we look at information on the relationship at the state level between employment rate gaps by race as of the first year of the sample (1940) and passage of a race discrimination law prior to 1960.[43] We begin by looking at employment rate gaps between black females and white males, in the first column of Table 10. In the sample means, we see that the employment rate gap was slightly smaller (.407 vs. .430) in states that passed their own race discrimination laws prior to 1960. To account for skill differences, we next compute the regression-adjusted employment gaps by state in 1940, using the same control variables as in Table 7, and use this as an independent variable in a probit for passage of a race discrimination law prior to 1960. The estimated relationship–reported as the effect of a one percentage point change in the gap on the probability of passage of a state law–is negative (similar to the means) but insignificant.

The second panel repeats this analysis for black males vs. white males. In this case the gap (in the means, or regression-adjusted) is larger, rather than smaller, in states that end up passing race discrimination laws prior to 1960.[44] Thus, the evidence regarding employment does not support the argument that race discrimination laws prior to the federal legislation had less impact because these state laws were implemented in states with less discrimination initially, at least insofar as the employment gaps capture the extent of discrimination.

Further evidence against the argument that endogenous policy plagues the results comes from looking at earnings.[45] In Table 8, only for black women did we find evidence that race discrimination laws

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[43]Collins (2000) studies the determination of the timing of adoption of state laws prohibiting race discrimination, but focusing on factors related to demographics, politics, and labor organization, rather than the extent of prior discrimination.

[44]The results were similar if we compared states that did and did not pass a race discrimination law prior to the federal legislation, rather than prior to 1960.

[45]Earnings gaps may more accurately capture discrimination than employment gaps. As an example, if labor supply is perfectly inelastic, Beckerian employer discrimination will result in pay gaps but no employment differences.

boosted relative earnings. Although the signs of the estimated earnings effects were positive regardless of whether the data through 1970 were used, the magnitudes were smaller using the data through 1970. Yet the results in the second column of Table 10, which look at the relationship between earnings gaps in 1940 and passage of state laws prior to 1960, suggest that if anything earlier state laws were passed in states with smaller initial earnings gaps. Thus, the effects of race discrimination laws (for black women) were stronger in the subset of states with smaller initial earnings gaps, the opposite of what would be expected if laws initially passed in less discriminatory environments in which they had less impact.

Note that the difference between the earnings and employment results–with the effects of race discrimination laws through 1960 stronger for earnings, and weaker for employment–indicates that a simple story based on the effects of earlier state legislation being identified from non-Southern states, and the effects of federal legislation being identified from Southern states, cannot fully explain the discrepancies in the results. As we noted in Section IV, if enforcement of federal legislation was directed at the South, and was more vigorous than enforcement of earlier state legislation, then the effects when incorporating data through 1970 would be expected to be stronger.[46] We find this for employment, but not earnings. However, if the stronger enforcement of federal legislation in Southern states was focused primarily on employment, and not earnings, this could help explain the results. Although Donohue and Heckman (1991) do not explicitly address the question of employment vs. earnings discrimination, they certainly argue that the Title VII ushered in a strong federal attack on employment discrimination in the South. However, particularly strong federal efforts in the South do not appear to be the entire explanation of the differences in results, as we still find evidence of positive effects of race discrimination laws (using the data through 1970) when we exclude the Southern states.

VI. Summary and Conclusions

The question of the effects of race and sex discrimination laws on relative economic outcomes for

[46]Note that this scenario differs from the one discussed at more length in Section IV, in which federal legislation is broader but enforcement is the same everywhere; we showed that that scenario does not lead to stronger estimated effects from the incorporation of information on federal legislation.

blacks and women has been of interest at least since the Civil Rights and Equal Pay Acts passed in the 1960s, and remains of interest today as some components of this legislation, particularly those that led to affirmative action, are reassessed. We present new evidence on the effects of these laws based on variation induced first by state anti-discrimination statutes, and then by the extension of discrimination prohibitions to the remaining states through the passage of federal legislation. Our evidence improves upon earlier time-series studies of the effects of federal anti-discrimination legislation, and is complementary to more recent work that revisits this question using data and statistical experiments that provide "treatment" and "comparison" groups. We examine the effects of race and sex discrimination laws on two outcomes–employment and earnings–in each case focusing on outcomes for black females, black males, and white females relative to white males.

With regard to race discrimination laws, our evidence yields some similarities and some differences in comparison to the newer research on race discrimination laws improving on the time-series analyses by using quasi-experimental designs–most notably Chay (1998) and Donohue and Heckman (1991). Our evidence indicates that race discrimination laws boost earnings of black females relative to white males. However, the evidence does not establish positive relative earnings effects for black males. This absence of evidence of earnings effects for black males contrasts with Chay's findings that the 1972 EEOA's expansion of coverage to employers with 15-24 workers boosted black male earnings among such employers.

The evidence regarding the effects of race discrimination laws on the relative employment of black males and females indicates positive effects only when data through 1970 (and hence the effects of federal legislation) are included, with no effect apparent using only the data through 1960 (and hence only the variation induced directly by state anti-discrimination laws). The differences between the results for employment excluding and including the 1970 data can be explained in part by federal enforcement targeted particularly at employment discrimination in the Southern states that, by and large, were the states that did not earlier implement state laws prohibiting race discrimination, consistent with the arguments

proffered by Donohue and Heckman (1991). At the same time, we still find evidence of positive relative employment effects of race discrimination laws (using the data through 1970) when we exclude the Southern states, indicating that federal efforts in the South are not the whole story. Overall, we interpret the evidence as corroborating the general conclusion that race discrimination laws positively impacted the relative employment and earnings of blacks, although the evidence is less dramatic than that in other research, and there are some cases (in particular, earnings effects for black males) and periods for which we find little positive impact. It remains an important question for future research to attempt to better understand the differences in results from using state vs. federal variation to estimate the effects of race discrimination laws, as well as the differences from alternative approaches to using state-level variation.

While we devote as much attention to sex discrimination laws as to race discrimination laws, in past research sex discrimination laws have received less attention. We expect sex discrimination laws to boost the relative earnings of females. There is some evidence of this for both black and white females, although not in every specification. Finally, we find that sex discrimination/equal pay laws reduced the relative employment of black women and white women–a result that is consistent with theory but has received relatively little attention previously. Indeed, this latter evidence is quite robust. This does not imply that laws prohibiting wage discrimination based on sex do not on net help women, but rather emphasizes that such laws may impose tradeoffs between higher wages and lower employment. Such considerations may become increasingly important if there is some retrenchment of affirmative action in the U.S., which would likely weaken policies combating employment discrimination.[47] That is, the combined evidence suggests that efforts to boost the employment and earnings of groups perceived as experiencing labor market discrimination may require restrictions on both prices and quantities.

[47]As explained in Holzer and Neumark (2000), numerical guidelines currently play a role in enforcement of Title VII, so it is difficult to draw a sharp distinction between Title VII, at least as it is now enforced by the courts, and affirmative action.

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Table 1
State Race Discrimination Laws

Years: State:	1900-1939	1940-1949	1950-1959	1960-1969
AL				
AK			1953- enacts mandatory FEPA. Exempt: employers with <10 employees 1957- amends FEPA to include all employers	
AR				
AZ				1961- enacts mandatory FEPA covering public contracts only 1965- amends FEPA to include all employment
CA		1949- passes act prohibiting inclusion of any race-related questions on any application form	1959- enacts mandatory FEPA	1965- extends FEPA to include apprenticeships
CO			1951- enacts voluntary FEPA. Mandatory for public employers 1957- enacts mandatory FEPA for all types of employment	1963- amends FEPA to include apprenticeships and other training programs
CT		1943- establishes Interracial Commission to compile/investigate claims of discrimination 1947- broadens powers of Commission by enacting mandatory FEPA. Exempt: employers with <6 employees	1951- State Court rules for first time on FEPA breach by ice cream parlor	1967- extends FEPA coverage by changing exemption to employers with <3 employees
DC				1965- enacts ordinance against discrimination in employment based on race, religion, creed, or national origin
DE				1960- enacts mandatory FEPA. Creates the Division Against Discrimination in the Labor Commission
FL				
GA				
HI			1959- enacts law prohibiting race discrimination in wage rates	1963- enacts mandatory FEPA. Effective in Jan. 1964
IA			1955- Legislature adopts resolution directing Governor to appoint a commission to study the problem of discrimination and recommend remedies	1963- enacts mandatory FEPA
ID				1961- enacts mandatory FEPA. Largely directed to discrimination in public accommodations, but includes provision making it a misdemeanor to discriminate in hiring and discharge 1969- approves mandatory FEPA with greater emphasis on employment. Creates Commission on Human Rights. Exempt: employers with <5 employees
IL	1935- enacts law which prohibits discrimination on account of race or color in employment in public works			1961- enacts mandatory FEPA. Exempt: employers with ≤100 employees. Beginning Jan. 1963, employers with ≤75 employees exempt. Beginning Jan. 1965, employers with ≤50 employees exempt. 1967- removes numerical exemptions to FEPA
IN		1945- enacts voluntary FEPA	1953- amendment defining unfair labor practices passed. Exempts employers with <6 employees from FEPA	1963- voluntary FEPA becomes mandatory

Table 1 (continued)

Year: State:	1900-1939	1940-1949	1950-1959	1960-1969
KS	1935- enacts law prohibiting discrimination on account of race or color in employment in public works	1941- passes law prohibiting discrimination because of race or color by labor unions 1949- authorizes a commission to study discrimination practices in employment	1953- enacts voluntary FEPA. Creates Antidiscrimination Commission	1961- FEPA becomes mandatory for employers with >7 employees. 1965- passes amendments to FEPA. Extends coverage to employers with >3 employees
KY				1960- enacts voluntary FEPA 1966- voluntary FEPA becomes mandatory
LA				
MA		1946- enacts mandatory FEPA. Exempt: employers with <6 employees. Establishes independent FEPA commission to administer law	1950- changes name of FEPA Commission to Commission Against Discrimination; broadens its power	1966- amends FEPA to permit keeping of records relating to race, color, national origin by employers or labor organizations
MD				1965- enacts mandatory FEPA. Includes training programs
ME				1963- passes law requiring that standards for apprenticeship agreements contain non-discrimination provisions 1965- enacts mandatory FEPA, no commission
MI			1955- enacts mandatory FEPA. Exempt: employers with <8 employees	
MN			1955- enacts mandatory FEPA. Exempt: employers with <8 employees	1965- removes numerical exemption for FEPA 1967- broadens scope of FEPA by creating a Department of Human Rights, which is given authority not available to previous commission
MO				1961- enacts mandatory FEPA. Exempt: employers with <50 employees 1965- amends FEPA to include apprentice programs. Coverage is extended to employers with ≥25 employees
MS				
MT				1965- enacts mandatory FEPA, no commission
NB		1941- enacts law stating "it is against public policy" for a representative of labor in collective bargaining to discriminate with regard to race or color. Law does not specify method of enforcement. 1949- authorizes commission to study discriminatory practices in employment		1961- enacts mandatory FEPA for public contracts only 1965- amends FEPA to include all employment
NC				
ND				
NH				1965- enacts mandatory FEPA. Passes prohibitions against discrimination in employment agencies
NJ	1938- establishes the Good Will Commission to promote racial understanding	1945- enacts mandatory FEPA. Exempt: employers with <6 employees 1949- amends existing FEPA by combining it with existing Civil Rights Law. Places administration of both under a new agency, the Commission on Civil Rights		1966- removes FEPA numerical exemption for employers
NM		1949- enacts mandatory FEPA		1969- repeals Civil Rights Act and Equal Employment Opportunity-FEPA. Passes in its place the Human Rights Act with administration vested in the newly created Human Rights Commission

Table 1 (continued)

Year: State:	1900-1939	1940-1949	1950-1959	1960-1969
NV				1960- passes law stating any apprentice program that discriminates shall be suspended for 1 year from state apprentice program 1961- enacts voluntary FEPA covering only public contracts 1965- voluntary FEPA becomes mandatory and includes all forms of employment
NY	1935- enacts law prohibiting discrimination on account of race or color in employment in public works	1942- empowers Industrial Commissioner to investigate, issue cease and desist orders, and criminally prosecute any employers holding war production contracts, unions, or employment agencies that violate Civil Rights Law by discriminating 1945- enacts mandatory FEPA. Exempt: employers with <6 employees 1947- forms committees to investigate employment discrimination	1950- passes two amendments to FEPA. One prohibits issuance of a license to conduct an employment agency when the name of the agency expresses racial discrimination. The other stipulates that government contracts must contain provisions prohibiting discrimination in the hiring of employees by the contractor 1957- passes agreement to abolish discrimination in apprenticeship programs	1962- amends FEPA to include apprenticeships 1964- amends FEPA more specifically to prohibit selection of persons for apprentice/training programs based on anything other than personal qualifications 1965- amends FEPA to exempt firms with <4 employees 1968- replaces State Commission for Human Rights with the Division of Human Rights, headed by a commissioner. The new division may investigate complaints of discrimination and take legal action against the offenders 1969- declares not an unlawful discriminatory practice for employers, employment agencies, labor organizations, or joint labor-management committees to carry out plans to increase employment of members of a minority group that has statewide unemployment rates disproportionately higher than that of general population
OH			1959- enacts mandatory FEPA	1967- passes amendment invalidating hiring hall agreements that obligate public works contractors to use union labor, unless the union has in effect anti-discrimination procedures for referring qualified employers
OK				1963- enacts voluntary FEPA 1968- voluntary FEPA becomes mandatory
OR		1947- enacts voluntary FEPA 1949- voluntary FEPA becomes mandatory	1957- amends FEPA by authorizing the State Attorney General and any person claiming to be discriminated against to file a complaint	1969- amends FEPA to include employers with >1 employee, state agencies, political subdivisions, and municipalities
PA	1935- enacts law prohibiting discrimination on account of race or color in employment in public works		1955- enacts mandatory FEPA. Exempt: employers with <12 employees	
RI		1949- enacts mandatory FEPA		
SC				
SD				1968- establishes the Commission on Human Rights Relations, which is authorized to hear complaints alleging violation of rights because of race, color, or creed, and to recommend legislation
TN				
TX				
UT		1945- establishes committee to investigate discrimination because of race, color, creed, and to recommend legislation		1965- enacts mandatory FEPA
VA				
VT				1963- enacts mandatory FEPA, no commission
WA		1949- enacts mandatory FEPA	1957- enacts law that makes it an unfair employment practice to advertise or inquire in such a way as to express any discrimination	1969- passes law requiring joint apprentice programs that receive state assistance to include, when available, members of minority races in a ratio at least equal to the ratio such races bear to population of the city

Table 1 (continued)

Year: State:	1900-1939	1940-1949	1950-1959	1960-1969
WI		1945- passes law empowering State Labor Department to hear cases of discrimination in employment and to make recommendations to parties or publicize findings	1951- enacts voluntary FEPA 1957- voluntary FEPA becomes mandatory	
WV				1961- enacts voluntary FEPA 1967- voluntary FEPA becomes mandatory
WY				1965- enacts mandatory FEPA

To the best of our knowledge, all state FEPAs cover both public and private employers, labor organizations, and employment agencies. Additions or exemptions to this standard are noted.

Sources: Bureau of National Affairs (1973), Norgren and Hill (1964), U.S. Bureau of Labor Standards (1967), U.S. Bureau of Labor Statistics, Monthly Labor Review (all years).

Table 2
State Sex Discrimination Laws

Year: State:	1900-1939	1940-1949	1950-1959	1960-1969
AL				
AK		1949- enacts EPL		1969- amends FEPA to include sex discrimination clause. Exempt: employment agencies, labor organizations
AR			1955- enacts EPL	
AZ				1962- enacts EPL 1965- enacts FEPA including sex discrimination clause
CA		1949- enacts EPL		1965- strengthens EPL enforcement 1968- extends EPL to include both men and women
CO			1955- enacts EPL	1969- amends FEPA to include sex discrimination clause. Exempt: employment agencies, labor organizations
CT		1949- enacts EPL	1953- amends EPL to permit employers to consider length of service and merit ratings as factors in determining wage/salary rates	1967- amends FEPA to include sex discrimination clause
DC				1965- enacts anti-discrimination ordinance including sex discrimination clause
DE				
FL				1969- enacts EPL
GA				1966- enacts EPL 1968- limits EPL to "employers in intrastate commerce"
HI			1959- enacts EPL	1963- enacts FEPA including sex discrimination clause
IA				
ID				1967- amends FEPA to include sex discrimination clause 1969- enacts EPL. Approves mandatory FEPA with greater emphasis on employment. Creates Commission on Human Rights. Exempt: employers with <5 employees
IL		1944- enacts EPL. Applies only to manufacturing		
IN				1967- adds EPL to existing minimum wage law
KS				
KY				1966- enacts EPL
LA				1968- creates a Women's Division within the Department of Labor. Establishes the Commission on the Status of Women, which conducts studies and develops recommendations regarding employment
MA		1945- enacts EPL		1965- amends FEPA to include sex discrimination clause
MD				1965- enacts FEPA including sex discrimination clause. Law also prohibits sex discrimination in wage rates. Exempt: labor organizations and employment agencies 1966- enacts EPL
ME		1949- enacts EPL		1965- EPL becomes applicable to both men and women
MI	1919- enacts EPL. Covers only manufacturing	1940- EPL upheld as constitutional by State Supreme Court.		1962- EPL strengthened to include all employers 1966- amends FEPA to include sex discrimination clause
MN				1969- enacts EPL. Amends FEPA to include sex discrimination clause
MO				1963- enacts EPL 1965- amends FEPA to include sex discrimination clause. An additional amendment prohibits sex discrimination in apprenticeship and employment agencies
MS				
MT	1919- enacts EPL			

Table 2 (continued)

Year: State:	1900-1939	1940-1949	1950-1959	1960-1969
NB			1957- adopts resolution endorsing equal pay for equal work. Urges employers to adopt	1965- enacts FEPA including sex discrimination clause. Law also prohibits sex discrimination in wage rates. 1967- enacts EPL
NC				
ND				1965- enacts EPL
NH		1947- enacts EPL		
NJ			1952- enacts EPL	
NM				1969- repeals Civil Rights Act and Equal Opportunity-FEPA and enacts Human Rights Act, which includes a clause on sex discrimination
NV				1967- amends FEPA to include sex discrimination clause. Law also prohibits sex discrimination in wage rates. 1969- adds EPL to minimum wage law
NY		1944- enacts EPL		1965- amends FEPA to include sex discrimination clause. Exempt: employment agencies. State EPL is made to conform to Federal EPL. 1967- amends FEPA to include employment agencies
OH			1959- enacts EPL	
OK				1965- enacts EPL 1968- enacts FEPA including sex discrimination clause. Effective beginning in 1969
OR		1947- enacts voluntary FEPA including clause on sex discrimination	1955- enacts EPL	1969- amends mandatory FEPA to include sex discrimination clause
PA		1947- enacts EPL. Includes provision prohibiting sex discrimination in wage rates		1968- extends EPL to include state employees and respective political subdivisions. Exempt: those subject to the minimum wage provisions of the FLSA 1969- amends FEPA to include sex discrimination clause
RI		1946- enacts EPL		1965- strengthens EPL by deleting exemption for individuals under union contract
SC				
SD				1966- enacts EPL
TN				
TX				
UT				1965- enacts FEPA including sex discrimination clause. Law also prohibits sex discrimination in wage rates
VA				
VT				1963- enacts FEPA that prohibits sex discrimination in wage rates, no commission
WA		1943- enacts EPL		
WI				1961- amends FEPA to include sex discrimination clause. Law also prohibits sex discrimination in wage rates.
WV				1965- enacts EPL
WY			1959- enacts EPL	1965- enacts FEPA including sex discrimination clause

EPL refers to Equal Pay Law.

Sources: Besner (1970), Bureau of National Affairs (1973), Lockard (1968), U.S. Bureau of Labor Standards (1967), U.S. Bureau of Labor Statistics, Monthly Labor Review (all years), U.S. Women's Bureau (1965, 1969, 1970).

Table 3: Coding of Race and Sex Discrimination Legislation

Census year:	1940		1950		1960		1970	
State:	Race	Sex	Race	Sex	Race	Sex	Race	Sex
AL								
AK				WDL(9)	MRD	WDL	MRD	WDL,MSD(9)
AR						WDL		WDL
AZ							MRD	WDL,MSD
CA				WDL(9)	MRD(9)	WDL	MRD	WDL
CO					MRD	WDL	MRD	WDL,MSD(9)
CT			MRD	WDL(9)	MRD	WDL	MRD	WDL,MSD
DC							MRD	MSD
DE							MRD	
FL								WDL(9)
GA								WDL
HI					MRDW(9)	WDL(9)	MRD	WDL,MSD
IA							MRDWE	
ID							MRD(9)	WDL(9),MSD
IL				WDL		WDL	MRD	WDL
IN			VRD		VRD		MRD	WDL
KS					VRD		MRD	
KY							MRD	WDL
LA								
MA			MRD	WDL	MRD	WDL	MRD	WDL,MSD
MD							MRD	WDL,MSD
ME				WDL(9)		WDL	MRDWE	WDL
MI		WDL		WDL	MRD	WDL	MRD	WDL,MSD
MN					MRD		MRD	WDL(9),MSD(9)
MO							MRD	WDL,MSD
MS								
MT		WDL		WDL		WDL	MRDWE	WDL
NB						VWDL	MRD	WDL,MSD
NC								
ND								WDL
NH				WDL		WDL	MRD	WDL
NJ			MRD		MRD	WDL	MRD	WDL
NM			MRD(9)		MRD		MRD	MSD(9)
NV							MRD	WDL(9),MSD
NY			MRD	WDL	MRD	WDL	MRD	WDL,MSD
OH					MRD(9)	WDL	MRD	WDL
OK							MRD	WDL,MSD
OR			MRD(9)	VSD	MRD	WDL,VSD	MRD	WDL,MSD(9)
PA				WDL	MRD	WDL	MRD	WDL,MSD(9)
RI			MRD(9)	WDL	MRD	WDL	MRD	WDL
SC								
SD								WDL
TN								
TX								
UT							MRD	WDL,MSD
VA								
VT							MRDWE	WDL
WA			MRD(9)	WDL	MRD	WDL	MRD	WDL
WI					MRD		MRD	WDL,MSD
WV							MRD	WDL
WY						WDL(9)	MRD	WDL,MSD

Key: MRD - mandatory race discrimination law. These states have mandatory FEPAs. MRDW - mandatory law prohibiting race discrimination in wage rates only. MRDWE - mandatory race discrimination law with weak or no enforcement authority. (These states have a FEPA but no commission to enforce it.) VRD - voluntary race discrimination law. (These states have voluntary FEPAs, which have no enforcement authority.) Four states (IL, KS, NY, and PA) passed a law in 1935 preventing race discrimination in public works only, and other states did so later. Laws covering public sector employees only are not covered in these tables. MSD - mandatory sex discrimination law, including state mandatory FEPAs. WDL - wage discrimination law. (Laws include EPLs, and FEPAs with clauses forbidding sex discrimination in wage rates.) VSD - voluntary sex discrimination law. (States with voluntary FEPAs are included.) VWDL - voluntary wage discrimination law, including voluntary EPLs. Those codes ending in '(9)' mean the relevant law was passed in a year ending with a '9' (e.g., 1969).

Table 4: Sample Means of Outcomes, 1940-1970

	Employment		Log Earnings (nominal)	
	Means	N	Means	N
	(1)	(2)	(3)	(4)
Black females				
1940	.290	37,507	6.12	2,131
1950	.308	13,635	7.17	1,691
1960	.368	49,758	7.65	8,071
1970	.442	61,596	8.21	17,159
Black males				
1940	.696	25,880	6.46	10,630
1950	.699	10,221	7.49	5,528
1960	.672	39,474	8.00	20,606
1970	.681	48,299	8.53	27,731
White females				
1940	.204	310,520	6.72	44,082
1950	.247	109,354	7.55	20,510
1960	.307	382,173	7.98	84,987
1970	.396	440,761	8.40	123,905
White males				
1940	.718	237,208	7.16	133,934
1950	.752	83,803	7.98	54,178
1960	.781	305,356	8.51	212,261
1970	.786	362,164	8.94	252,371

Data from the 1940, 1950, 1960, and 1970 Decennial Censuses of Population are used. There are fewer observations for 1950 because in that year state identifiers are available only for "sample line" records, not all person records. The sample is restricted to those aged 18-70. When we study earnings, we exclude agricultural and private household workers, and we exclude from all analyses those in the armed forces, self-employed workers, and unpaid workers. States with fewer than 5 workers for any of the demographic groups we study, in any year, were excluded from the analysis. The small cells occurred for black females or black males. The excluded states are (Alaska, Arizona, Colorado, Hawaii, Idaho, Iowa, Maine, Minnesota, Montana, Nebraska, Nevada, Hew Hampshire, New Mexico, North Dakota, Oregon, Rhode Island, South Dakota, Utah, Vermont, Washington, West Virginia, Wisconsin, and Wyoming).

Table 5: Expected Effects of Race and Sex Discrimination Laws

	Employment	Earnings
Black females		
Race discrimination law	+	+
Sex discrimination law	−	+
Black males		
Race discrimination law	+	+
Sex discrimination law	?	?
White females		
Race discrimination law	?	?
Sex discrimination law	−	+

See text for arguments underlying these expected effects. ? signifies ambiguous prediction.

Table 6: Effects of Race and Sex Discrimination Laws on Employment and Log Earnings, "Quasi-Time-Series Experiment," 1940-1970, Regression Estimates

	Employment	Log Earnings
	(1)	(2)
Black females		
× Year ≥ 1950	-.014	.185
	(.004)	(.014)
× Year ≥ 1960	.038	-.070
	(.004)	(.011)
× Year = 1970	.070	.110
	(.003)	(.006)
R^2	.229	.772
N	1,151,027	681,796
Groups	BF, WM	BF, WM
Black males		
× Year ≥ 1950	-.033	.161
	(.005)	(.007)
× Year ≥ 1960	-.043	-.031
	(.005)	(.007)
× Year = 1970	.001	.041
	(.003)	(.004)
R^2	.186	.777
N	1,112,405	717,239
Groups	BM, WM	BM, WM
White females		
× Year ≥ 1950	.003	-.016
	(.002)	(.004)
× Year ≥ 1960	.032	-.076
	(.002)	(.004)
× Year = 1970	.086	.006
	(.001)	(.002)
R^2	.277	.765
N	2,231,339	926,228
Groups	WF, WM	WF, WM

See notes to Table 4. The specifications include controls for age and its square, residence in an SMSA, years of schooling, and marital status (currently married or divorced/widowed/separated). In addition, the specifications include dummy variables for race/sex and year and state dummy variables. The results were similar including state by year interactions and interactions between race/sex and state. The earnings specifications also include dummy variables for two-digit industry and occupation, and for hours and weeks worked. The results were similar when all states were included.

Table 7: Effects of Race and Sex Discrimination Laws on Employment, Regression Estimates

	Simple difference	Difference-in-difference	Diff.-in-diff.-in-diff.	Diff.-in-diff.-in-diff.	Diff.-in-diff.-in-diff.	Diff.-in-diff.-in-diff.
	(1)	(2)	(3)	(4)	(5)	(6)
States	All	All	All	Non-South	All	Non-South
Years	1960	1940-1960	1940-1960	1940-1960	1940-1970	1940-1970
Black females						
× Race discrimination law	.054	.003	-.011	.005	.035	.056
	(.008)	(.009)	(.009)	(.010)	(.007)	(.008)
× Sex discrimination law	-.093	.016	-.032	-.021	-.040	-.012
	(.007)	(.008)	(.008)	(.010)	(.006)	(.008)
Black female	...	...	-.293	-.319	-.292	-.341
			(.008)	(.013)	(.008)	(.011)
R^2	.081	.101	.228	.201	.231	.209
N	49,758	100,900	727,267	526,900	1,151,027	825,040
Groups	BF	BF	BF, WM	BF, WM	BF, WM	BF, WM
Black males						
× Race discrimination law	.009	.022	.018	.003	.018	.018
	(.009)	(.010)	(.010)	(.011)	(.008)	(.008)
× Sex discrimination law	-.062	.068	.031	.011	-.019	-.004
	(.008)	(.009)	(.009)	(.011)	(.007)	(.009)
Black male	...	...	-.133	-.104	-.091	-.099
			(.009)	(.014)	(.009)	(.012)
R^2	.163	.130	.173	.173	.188	.185
N	39,474	75,575	701,942	519,999	1,112,405	812,185
Groups	BM	BM	BM, WM	BM, WM	BM, WM	BM, WM
White females						
× Race discrimination law	-.010	-.018	-.019	.005	-.000	.012
	(.003)	(.002)	(.003)	(.003)	(.003)	(.003)
× Sex discrimination law	.003	-.004	-.039	-.016	-.021	-.013
	(.002)	(.002)	(.003)	(.003)	(.003)	(.003)
White female	...	...	-.383	-.447	-.331	-.371
			(.003)	(.003)	(.004)	(.004)
R^2	.119	.172	.303	.295	.276	.269
N	382,173	802,047	1,428,414	1,098,467	2,231,339	1,688,116
Groups	WF	WF	WF, WM	WF, WM	WF, WM	WF, WM

Data from the 1940, 1950, 1960, and 1970 Decennial Censuses of Population are used. See notes to Table 4 for sample restrictions. States with fewer than 5 workers for any of the demographic groups we study, in any year, were excluded from the analysis; see notes to Table 4. The results for white women were very similar when these states were included. The dummy variables for race and sex discrimination laws are coded as 0.5 if the law passed in the Census year. Table 3 also indicates a law barring wage discrimination by race in Hawaii as of 1960, but since Hawaii only became a state in 1959, a separate effect of this type of law is unidentified. All specifications include controls for age and its square, residence in an SMSA, years of schooling, and marital status (currently married or divorced/widowed/separated). In addition, the difference-in-difference specifications include dummy variables for year and state, and the difference-in-difference-in-difference specifications add state by year interactions, and interactions between race/sex and year and race/sex and state. In columns (1) and (2), only black females, black males, or white females are included in the sample, so the discrimination law dummy variables are not actually interactions with the demographic group indicators. The estimated coefficients for black female, black male, and white female are for the omitted reference group consisting of observations in New York in the most recent Census year. In this table and the others that follow, the results were very similar when dummy variables were also included for voluntary or non-enforced race and sex discrimination laws, and the estimated coefficients for these variables were generally insignificant. The excluded Southern states are Delaware, Maryland, Virginia, North Carolina, South Carolina, Georgia, Florida, Kentucky, Tennessee, Alabama, Mississippi, Arkansas, Louisiana, Oklahoma, and Texas; Washington, D.C. is also excluded, and West Virginia was dropped based on sample size restrictions discussed in the notes to Table 4.

Table 8: Effects of Race and Sex Discrimination Laws on Log Earnings, Regression Estimates

	Simple difference	Difference-in-difference	Diff.-in-diff.-in-diff.	Diff.-in-diff.-in-diff.	Diff.-in-diff.-in-diff.	Diff.-in-diff.-in-diff.
	(1)	(2)	(3)	(4)	(5)	(6)
States	All	All	All	Non-South	All	Non-South
Years	1960	1940-1960	1940-1960	1940-1960	1940-1970	1940-1970
Black females						
× Race discrimination law	.063	-.044	.055	.073	.035	.025
	(.019)	(.028)	(.026)	(.027)	(.017)	(.017)
× Sex discrimination law	.227	-.058	-.033	.010	.041	.020
	(.018)	(.025)	(.023)	(.026)	(.015)	(.020)
Black female	...	...	-.393	-.468	-.370	-.352
			(.024)	(.034)	(.017)	(.024)
R^2	.490	.734	.759	.763	.745	.753
N	8,071	11,893	412,266	323,655	681,796	520,267
Groups	BF	BF	BF, WM	BF, WM	BF, WM	BF, WM
Black males						
× Race discrimination law	.004	-.045	.006	-.016	-.003	-.017
	(.012)	(.013)	(.013)	(.014)	(.011)	(.011)
× Sex discrimination law	.231	-.027	-.011	-.008	.009	-.005
	(.011)	(.012)	(.012)	(.014)	(.009)	(.012)
Black male	...	...	-.166	-.150	-.146	-.120
			(.013)	(.019)	(.012)	(.016)
R^2	.433	.776	.764	.763	.750	.752
N	20,606	36,764	437,137	333,492	717,239	535,341
Groups	BM	BM	BM, WM	BM, WM	BM, WM	BM, WM
White females						
× Race discrimination law	-.017	-.013	.047	.030	.031	.022
	(.005)	(.005)	(.006)	(.006)	(.005)	(.005)
× Sex discrimination law	.124	-.014	-.005	.012	-.010	.012
	(.005)	(.005)	(.006)	(.005)	(.005)	(.005)
White female	...	...	-.393	-.406	-.369	-.400
			(.006)	(.006)	(.006)	(.007)
R^2	.435	.731	.757	.765	.738	.748
N	84,987	149,579	549,952	433,442	926,228	709,627
Groups	WF	WF	WF, WM	WF, WM	WF, WM	WF, WM

See notes to Tables 4, 6, and 7. Additional controls include dummy variables for two-digit industry and occupation, and for hours and weeks worked. In this table, we also exclude individuals working fewer than 27 weeks or fewer than 30 hours per week, and those whose wages would have been less than $1 per hour in 1980 dollars, based on half-time, half-year work; this wage restriction is approximately equally binding on white males in each of the Census years we study. The earnings data were adjusted to use consistent top codes across the years, and to use midpoints of reported earnings intervals where required.

Table 9: Difference-in-Difference-in-Difference Estimates of Effects of Race and Sex Discrimination Laws on Employment and Log Earnings, Ages 25-34

		A. Employment		
States	All	Non-South	All	Non-South
Years	1940-1960	1940-1960	1940-1970	1940-1970
Black females				
× Race discrimination law	-.017	.005	.058	.093
	(.015)	(.017)	(.013)	(.014)
× Sex discrimination law	-.017	-.012	-.030	-.001
	(.014)	(.018)	(.011)	(.015)
R^2	.220	.173	.213	.176
N	176,720	116,043	268,512	175,269
Black males				
× Race discrimination law	-.027	-.041	-.005	-.005
	(.017)	(.019)	(.013)	(.015)
× Sex discrimination law	.046	.024	.002	.007
	(.015)	(.020)	(.012)	(.016)
R^2	.079	.079	.082	.080
N	169,153	113,653	257,793	171,207
White females				
× Race discrimination law	-.042	-.022	.001	.017
	(.007)	(.007)	(.006)	(.007)
× Sex discrimination law	-.061	-.043	-.034	-.034
	(.006)	(.008)	(.006)	(.007)
R^2	.380	.370	.360	.356
N	336,143	233,354	503,091	344,045
		B. Log Earnings		
Black females				
× Race discrimination law	.076	.096	.072	.064
	(.042)	(.044)	(.029)	(.029)
× Sex discrimination law	-.071	-.028	.014	.014
	(.037)	(.045)	(.026)	(.035)
R^2	.781	.787	.802	.810
N	115,302	81,372	184,758	126,820
Black males				
× Race discrimination law	.017	-.024	.005	-.013
	(.024)	(.026)	(.019)	(.020)
× Sex discrimination law	-.021	-.016	.004	.008
	(.021)	(.027)	(.017)	(.023)
R^2	.787	.787	.808	.810
N	122,033	83,918	194,047	130,674
White females				
× Race discrimination law	.048	.038	.030	.025
	(.011)	(.012)	(.010)	(.011)
× Sex discrimination law	.011	-.006	-.005	-.009
	(.011)	(.012)	(.009)	(.011)
R^2	.785	.795	.806	.816
N	147,816	105,071	235,651	162,413

Specifications and samples are identical to those in columns (3)-(6) of Tables 7 and 8.

Table 10: Relationships Between Race Gaps in Employment and Earnings in 1940, and Subsequent State Legislation

	Employment rate	Log earnings
Black females		
Race discrimination law		
Sample means		
Gap relative to white males, 1940	.423	.902
States with race discrimination law prior to 1960	.407	.783
States with no race discrimination law prior to 1960	.430	.950
Probit estimates, probability of passage of state race discrimination law prior to 1960 as function of regression estimate of gap in 1940	-.004 (.010)	-.016 (.006)
Black males		
Race discrimination law		
Sample means		
Gap relative to white males, 1940	.043	.549
States with race discrimination law prior to 1960	.079	.392
States with no race discrimination law prior to 1960	.028	.613
Probit estimates, probability of passage of state race discrimination law prior to 1960 as function of regression estimate of gap in 1940	.036 (.014)	-.036 (.016)

Sample means are unweighted state means for the 28 states included in the analysis. The regression estimates of the gaps are based on specifications including controls for age and its square, residence in an SMSA, years of schooling, marital status (currently married or divorced/widowed/separated), race and sex group, state, and interactions of state dummy variables with race and sex; the latter are used as regression estimates of the wage or employment gap. The earnings regressions also include dummy variables for two-digit industry and occupation and controls for hours and weeks worked. For the probit estimates, partial derivatives of the probability are reported. Estimates are unweighted, to correspond to sample means. In the probit estimates, the gaps were multiplied by 100, so the reported coefficients measure the impact of a one percentage point change in the gap. The standard errors of the probit estimates are not corrected for estimation error in the gaps.

Appendix Table A1
State Protective Laws for Women

Year: State:	1860-1939	1940-1959	1960-1969
AL	1893- passes law prohibiting women from employment in selected occupations. Occupations include M		
AK			
AR	1893- passes law prohibiting women from employment in selected occupations. Occupations include M 1915- passes enforceable maximum hours law. Industries include Mf, Mc, O		
AZ	1912- passes law prohibiting women from employment in selected occupations. Occupations include M 1913- passes enforceable maximum hours law. Industries include Mc, O 1927- amends hours law to include Mf		1968- employment that complies with FLSA requirements exempt from maximum hours law
CA	1911- passes enforceable maximum hours law. Industries include Mf, Mc, O 1916- Industrial Welfare Commission issues order prohibiting women from employment in selected occupations. Occupations include H 1918- Industrial Welfare Commission issues order prohibiting night work. Industries include Mf 1919- amends night work orders to include O		1960s- amendments to hours laws reduce scope/loosen restrictive nature of hours laws
CO	1885- passes law prohibiting women from employment in selected occupations. Occupations include M 1912 -passes enforceable maximum hours law, effective in 1913. Industries include Mf, Mc, O		
CT	1887- passes enforceable maximum hours law. Industries include Mf, Mc 1909- passes law prohibiting night work. Industries include Mc 1913- amends night work law to include Mf 1917- amends maximum hours law to include O 1917- amends night work law to include O		1960s- amendments to hours laws reduce scope/loosen restrictive nature of hours laws
DC	1914- U.S. Congress passes enforceable maximum hours law for District of Columbia. Industries include Mf, Mc, O		
DE	1913- passes enforceable maximum hours law. Industries include Mf, Mc, O 1917- passes law prohibiting night work. Industries include Mf, O	1955- repeals night work law	1965- repeals maximum hours law
FL			
GA			
HI			
IA			
ID	1913- passes enforceable maximum hours law. Industries include Mf, Mc, O		
IL	1872- passes law prohibiting women from employment in selected occupations. Occupations include M 1893- passes enforceable maximum hours law. Industries include Mf 1895- State Court declares hours law unconstitutional. 1909- passes enforceable maximum hours law. Industries include Mf, O 1911- amends maximum hours law to include Mc		
IN	1899- passes law prohibiting night work. Industries include Mf 1905- passes law prohibiting women from employment in selected occupations. Occupations include M		
KS	1915- passes law prohibiting women from employment in selected occupations. Occupations include H 1917- Industrial Welfare Commission issues enforceable maximum hours order. Industries include Mc, O 1917- Industrial Welfare Commission issues order prohibiting night work. Industries include Mc 1918- amends night work order to include O 1919- amends night work order to include Mf 1919- amends maximum hours order to include Mf		
KY	1912- passes enforceable maximum hours law. Industries include Mf, Mc, O		
LA	1886- passes enforceable maximum hours law, allowing an average maximum per day. Industries include Mf 1908- passes enforceable maximum hours law, enforcing a maximum limit on hours per day rather than an average. Industries include Mf, Mc, O 1908- passes law prohibiting women from employment in selected occupations. Occupations include H		
MA	1879-passes enforceable maximum hours law. Industries include Mf 1890- passes law prohibiting night work, effective in 1891. Industries include Mf 1900- amends maximum hours law to include Mc 1913- amends maximum hours law to include O		1960s- amendments to hours laws reduce scope/loosen restrictive nature of hours laws
MD	1902- passes law prohibiting women from employment in selected occupations. Occupations include M 1912- passes enforceable maximum hour law. Industries include Mf, Mc, O		1969- employment that complies with FLSA requirements exempt from maximum hours law

Appendix Table A1 (continued)

Year: State:	1860-1939	1940-1959	1960-1969
ME	1887- passes enforceable maximum hours law. Industries include Mf 1915- amends maximum hours law to include Mc, O		
MI	1885- passes enforceable maximum hours law, allowing an average maximum per day. Industries include Mf 1893- amends hours law to include only girls under 21 1907- hours law again regulates work of all women, regardless of age. Law enforces a maximum limit on hours per day, rather than an average. Industries include Mf, Mc 1909- maximum hours law now includes O. 1919- passes equal pay law including a provision prohibiting women from employment in selected occupations. Occupations include H		1967- repeals maximum hours law. However, State Labor Department is given authority to adopt rules on special working conditions for women 1969- State Attorney General invalidates protective laws for employment that are covered by Federal Civil Rights Act
MN	1909- passes enforceable maximum hours law. Industries include Mc, Mf 1913- amends hours law to include O 1913- passes law prohibiting women from employment in selected occupations. Occupations include H 1919- sets a basic maximum hour per week law for all industries		
MO	1881- passes law prohibiting women from employment in selected occupations. Occupations include M 1891- amends law prohibiting women from working in selected occupations by including H 1909- passes enforceable maximum hours law. Industries include Mf, Mc, O		
MS	1914- passes enforceable maximum hours law. All industries included		1969- State Attorney General opinion/administrative ruling given regarding state protective laws
MT	1913- passes enforceable maximum hours law. Industries include Mf, Mc, O		
NB	1899- passes enforceable maximum hours law. Industries include Mf, Mc, O 1899- passes law prohibiting night work. Industries include Mf, Mc, O		1969- repeals maximum hours law
NC	1915- passes enforceable maximum hours law. Industries include Mf 1933- amends maximum hours law to include Mc 1935- amends maximum hours law to include O		1967- maximum hours law exemption for employment conforming to FLSA standards
ND	1919- passes enforceable maximum hours law. Industries include Mf, Mc, O 1920- passes law prohibiting employment of women in selected occupations. Occupations include H 1920- passes law prohibiting night work. Industries include Mc, O		1969- State Attorney General recognizes that prosecution of protective laws may be difficult given federal ban on sex discrimination in Title VII of the Civil Rights Act
NH	1887- passes enforceable maximum hours law. Industries include Mf 1913- amends hours law to include O, Mc		
NJ	1892- passes enforceable maximum hours law. Industries include Mf 1912- amends hours law to include Mc, O 1917- Bureau of Hygiene and Sanitation issues orders prohibiting employment of women in selected occupations. Occupations include H 1937- passes law prohibiting night work. Industries include Mf, Mc		
NM	1921- passes enforceable maximum hours law. Industries include Mf, Mc, O		1969- maximum hours law not applicable if employee voluntarily agrees to more hours in writing and is paid overtime rates
NV	1917- passes enforceable maximum hours law. Industries include Mf, Mc, O		
NY	1896- passes law prohibiting employment of women in selected occupations. Occupations include H 1899- passes enforceable maximum hours law. Industries include Mf 1901- amends maximum hours law to include O 1906- amends law prohibiting women from working in selected occupations by including M 1913- passes law prohibiting night work. Industries include Mf, Mc 1913- amends maximum hours law to include Mc 1917- amends night work law to include O		
OH	1909- passes law prohibiting employment of women in selected occupations. Occupations include H 1911- passes enforceable maximum hours law. Industries include Mf, O 1913- amends maximum hours law to include Mc 1919- passes law prohibiting night work. Industries include O 1919- amends law prohibiting women from working in selected occupations by including M		1969- Department of Industrial Relations issues release saying it will not prosecute violations of state women's laws that are in conflict with federal anti-discrimination law

Year: State:	1860-1939	1940-1959	1960-1969
OK	1907- passes law prohibiting employment of women in selected occupations. Occupations include M 1915- passes enforceable maximum hours law. Industries include Mf, Mc, O		1969- State Attorney General invalidates state protective laws
OR	1903- passes enforceable maximum hours law. Industries include Mf, O 1907- amends maximum hours law to include Mc 1913- includes prohibition of night work in Industrial Welfare Commission orders for city of Portland. Includes rest of state in 1914 orders. Industries include Mc 1914- amends night work prohibition to include Mf, O 1919- includes prohibition of employment of women in selected occupations in Industrial Welfare Commission orders. Occupations include H		1967- repeals maximum hours law. However, State Labor Department is given authority to adopt rules on special working conditions for women
PA	1885- passes law prohibiting women from employment in selected occupations. Occupations include M 1897- passes enforceable maximum hours law. Industries include Mf, Mc, O 1913- passes law prohibiting night work. Industries include Mf 1915- Industrial Welfare Commission issues orders to prohibit employment of women in H		1969- State Attorney General invalidates state protective laws.
RI	1885- passes enforceable maximum hours law. Industries include Mf 1913- amends maximum hours law to include Mc		
SC	1911- passes enforceable maximum hours law. Industries include Mc 1914- passes law prohibiting night work. Industries include Mc		1967- repeals night work law
SD	1913- passes enforceable maximum hours law. Industries include Mf, Mc, O		1969- State Attorney General rules that the maximum hours law is superseded by the sex discrimination ban in Title VII of the Civil Rights Act
TN	1907- passes enforceable maximum hours law. Effective Jan. 1908. Industries include Mf 1915- amends maximum hours law to include Mc, O		1969- maximum hours law exempt for employment conforming to FLSA standards
TX	1913- passes enforceable maximum hours law. Industries include Mf, Mc, O		
UT	1896- passes law prohibiting employment of women in selected occupations. Occupations include M 1911- passes enforceable maximum hours law. Industries include Mf, Mc, O		
VA	1890- passes enforceable maximum hours law. Industries include Mf 1912- amends maximum hours law to include Mc 1912- passes law prohibiting employment of women in selected occupations. Occupations include M 1914- amends maximum hours law to include O		1966- maximum hours law exemption for employment conforming to FLSA standards
VT	1912- passes enforceable maximum hours law. Industries include Mf 1917- amends maximum hours law to include O		
WA	1891- passes law prohibiting employment of women in selected occupations. Occupations include M 1901- passes enforceable maximum hours law. Industries include Mf, Mc, O 1913- passes act prohibiting employment of women in H 1920- Industrial Welfare Commission issues order prohibiting night work. Industries include O		
WI	1911- passes enforceable maximum hours law. Industries include Mf, Mc, O 1911- passes law prohibiting employment of women in selected occupations. Occupations include M 1917- Industrial Welfare Commission issues order prohibiting night work. Industries include Mf, O		
WV	1887- women prohibited from employment in selected occupations. Occupations include M		
WY	1890- passes law prohibiting employment of women in selected occupations. Occupations include M 1915- passes enforceable maximum hours law. Industries include Mf, Mc, O		

The following are the abbreviations used to denote the prohibited industries or occupations in each state. M-Mining. May include, but not be limited to, the following: work in or around mines, quarries, coal breakers, coke ovens, or smelters. H-Heavy Lifting/Dangerous Occupations. May include, but not be limited to, the following: lifting "any excessive burden", cleaning moving machinery, work on moving abrasives, work in core making rooms, manufacture of nitro compounds, handling of any dry substance with specified amount of lead, employment in work environments that are not sufficiently lighted, ventilated, or sanitary, messenger service, bell boy, trucking, gas/electric meter reader, taxi cab driver, elevator operator, guard on streets or subways, work in pool hall/bowling alley, delivery service, or "employing women under any conditions detrimental to their health or welfare." The following are the abbreviations used for the industries covered by maximum hours and night work laws. Mf-Manufacturing, as defined by "all processes in the production of commodities." May include, but not be limited to, work in the following: factories, packing and canning establishments, mechanical establishments, millinery workrooms (and other sorts of workrooms), upholstery, dressmaking, alteration, parts of mercantile establishments dealing with production, mills, or textiles. Manufacturing in the South consists solely of textiles. Mc-Mercantile establishments, as defined by "establishments operated for purpose of trade in purchase or sale of any goods or merchandise." May include, but not be limited to, the following work: sales, newspaper

Appendix Table A1 (continued)

reporting, pharmacists, or any person involved in the sale or purchase of a commodity. O-Other, which consists of miscellaneous occupations. May include, but not be limited to, the following work: laundry and dry-cleaning, telephone and telegraph, restaurant, hotel, hair-dressing salon, photo gallery, bowling alley, billiard room, shoe-shine establishment, printing establishment, office, bakery, domestic labor, medical personnel, farm labor, elevator operator, conductor or guard on a street or subway,

messenger, transportation, place of amusement, employee in mine or quarry. Note that the definitions of the above industries may differ among states. The FLSA, or Fair Labor Standards Act was passed in 1938 and, among other things, requires that a rate of time-and-a-half must be paid for hours worked over the maximum.

Sources: Goldin (1990), Smith (1932a, 1932b), U.S. Bureau of Labor Standards (1953, 1965, 1967, 1969), U.S. Bureau of Labor Statistics, Monthly Labor Review (all years), Walstedt (1976).

Appendix Table A2: Effects of Sample Restrictions

	Lower wage cutoff	Exclude agricultural workers	Exclude private household workers
Black females			
1940	.048	.030	.688
1950	.006	.015	.389
1960	.005	.010	.313
1970	.011	.008	.135
Black males			
1940	.019	.211	.041
1950	.007	.069	.008
1960	.003	.069	.007
1970	.007	.035	.003
White females			
1940	.007	.003	.095
1950	.003	.002	.025
1960	.002	.003	.018
1970	.004	.004	.010
White males			
1940	.003	.054	.002
1950	.003	.027	.001
1960	.001	.019	.000
1970	.002	.015	.000

Proportions of observations dropped as a result of imposition of noted sample restriction in isolation are reported. Because of overlapping criteria, total proportion of observations dropped is smaller than the sum of the figures reported in this table.

Appendix Table A3: Difference-in-Difference-in-Difference Estimates of Effects of Race and Sex Discrimination Laws on Log Earnings, Including Agricultural and Private Household Workers

States Years	All 1940-1960	Non-South 1940-1960	All 1940-1970	Non-South 1940-1970
Black females				
× Race discrimination law	-.006	-.002	.023	.018
	(.020)	(.021)	(.015)	(.015)
× Sex discrimination law	-.047	.005	.033	.029
	(.018)	(.021)	(.013)	(.018)
N	436,008	335,526	712,345	535,481
Black males				
× Race discrimination law	.014	-.011	.011	-.013
	(.014)	(.015)	(.011)	(.011)
× Sex discrimination law	-.021	-.012	.006	-.004
	(.012)	(.014)	(.009)	(.012)
N	456,259	343,203	741,461	547,707
White females				
× Race discrimination law	.044	.025	.030	.022
	(.006)	(.006)	(.005)	(.005)
× Sex discrimination law	-.006	.016	-.011	.014
	(.006)	(.005)	(.005)	(.005)
N	570,627	448,884	952,605	728,764

___Specifications are identical to those in columns (3)-(6) of Tables 7 and 8.

Appendix Table A4: Difference-in-Difference-in-Difference Estimates of Effects of Race and Sex Discrimination Laws on Employment and Log Earnings, Entering Race and Sex Discrimination Laws Separately

States Years	All 1940-1960	Non-South 1940-1960	All 1940-1970	Non-South 1940-1970
A. Employment				
Black females				
× Race discrimination law	-.032	-.000	.004	.053
	(.007)	(.010)	(.005)	(.007)
× Sex discrimination law	-.038	-.020	-.020	.006
	(.006)	(.010)	(.005)	(.008)
Black males				
× Race discrimination law	.038	.006	.003	.017
	(.008)	(.011)	(.006)	(.008)
× Sex discrimination law	.040	.012	-.009	.001
	(.007)	(.010)	(.005)	(.009)
White females				
× Race discrimination law	-.040	-.001	-.014	.006
	(.003)	(.003)	(.003)	(.003)
× Sex discrimination law	-.048	-.014	-.021	-.009
	(.003)	(.003)	(.002)	(.002)
B. Log Earnings				
Black females				
× Race discrimination law	.034	.075	.068	.031
	(.021)	(.026)	(.012)	(.016)
× Sex discrimination law	-.004	.025	.063	.031
	(.019)	(.025)	(.011)	(.019)
Black males				
× Race discrimination law	-.001	-.017	.004	-.018
	(.011)	(.014)	(.008)	(.011)
× Sex discrimination law	-.008	-.011	.007	-.011
	(.010)	(.014)	(.007)	(.012)
White females				
× Race discrimination law	.044	.034	.024	.026
	(.005)	(.005)	(.004)	(.005)
× Sex discrimination law	.019	.021	.008	.019
	(.005)	(.005)	(.004)	(.005)

Specifications and samples are identical to those in columns (3)-(6) of Tables 7 and 8, except that the race and sex discrimination variables are entered separately; estimates from each specification are reported.