SORTING, QUOTAS, AND THE CIVIL RIGHTS ACT OF 1991: WHO HIRES WHEN IT'S HARD TO FIRE?*

PAUL OYER   *and*   *SCOTT SCHAEFER*
Stanford University   *Northwestern University*

ABSTRACT

The Civil Rights Act of 1991 (CRA91) was enacted after a rancorous debate about whether it was a "quota" hiring bill or a necessary means of opening labor markets. We analyze the effects of CRA91 on the composition of firms' workforces. We consider employer behavior when firms vary in their susceptibility to discrimination suits and when firms can reduce exposure to discrimination claims by employing more protected workers. These forces lead to a sorting effect, which causes firms that are more susceptible to discrimination litigation to substitute away from protected workers, and a quota effect, which causes firms with fewer pre-CRA91 protected workers to substitute toward these workers. Using data from various sources, we find evidence consistent with CRA91 having had a sorting effect. We find no evidence that CRA91 led to widespread quota hiring and no evidence that CRA91 helped integrate industries that had employed relatively few protected workers.

I. INTRODUCTION

THE early 1990s saw a rebirth of congressional interest in employment discrimination legislation. After months of national debate, a presidential veto, and a nationally televised hearing focused on sexual harassment in the workplace, two major new laws were enacted. The Americans with Disabilities Act (ADA) offered broad new protections to physically challenged employees. The Civil Rights Act of 1991[1] (CRA91) expanded the rights of plaintiffs in discrimination complaints to the Equal Employment Opportunity Commission (EEOC) and in federal civil court. Many opponents of CRA91

[1] Civil Rights Act of 1991, Pub. L. No. 102-166, 105 Stat. 1071 (codified in various sections of 42 U.S.C (Supp. III 1992)).

warned that the bill would lead to hiring "by quota."[2] Critics focused on CRA91's "disparate-impact" provisions, whereby the act made it significantly easier for plaintiffs to use statistical evidence to prove unlawful discrimination even if there was no discriminatory intent on the part of the employer. Proponents argued that CRA91 was necessary to open opportunities to women and minorities in businesses that had traditionally been unwelcoming. Our objective in this paper is to delve deeper into the question of how CRA91 may have affected firms' hiring practices and to examine these effects empirically.

There are at least two reasons to think that critics' predictions of widespread quota-based hiring may have been overstated. First, while CRA91 did increase maximum damage awards, plaintiffs must show "disparate treatment"—that is, intentional discrimination on the part of an employer—in order to be eligible for the increased awards. According to Glen Nager and Julia Broas, it appears that most post-1991 plaintiffs have been willing to undertake this more stringent burden of proof to try to earn these higher damages.[3] Second, as John Donohue and Peter Siegelman have documented, there has been a dramatic shift over the past quarter century away from hiring-based employment discrimination litigation and toward firing-based suits.[4] Ian Ayres and Siegelman show that while application of disparate-impact liability to firms' hiring decisions will cause firms to hire more protected workers, application of this principle to firms' firing decisions can have the opposite effect.[5] On the basis of these arguments, one might conclude that there is little reason to think CRA91 would lead employers to increase employment of protected workers.

We argue, however, that the combination of increases in disparate-treatment damages and employment protections that apply primarily to firms' firing decisions can lead firms that employed few protected workers prior to CRA91 to increase hiring of protected workers. The key assumption underlying this reasoning is that a firm is less likely to be successfully sued by a given protected employee when the firm employs a larger number of protected employees. This assumption, which is motivated by the judicial standards for disparate-treatment cases set out in the Supreme Court's 1973 *McDonnell Douglas Corp. v. Green*[6] decision, implies that the costs of employing protected workers may be concave: the hiring of the marginal protected worker

---

[2] For example, on the day after the bill was signed into law, the *Wall Street Journal* ran an editorial headlined "It's a Quota Bill."

[3] Glen D. Nager & Julia M. Broas, Enforcement Issues: A Practical Overview, 54 La. L. Rev. 1473 (1994).

[4] John J. Donohue III & Peter Siegelman, The Changing Nature of Employment Discrimination Litigation, 43 Stan. L. Rev. 983 (1991).

[5] Ian Ayres & Peter Siegelman, The Q-Word as Red Herring: Why Disparate Impact Liability Does Not Induce Hiring Quotas, 74 Tex. L. Rev. 1487 (1996).

[6] McDonnell Douglas Corp. v. Green, 411 U.S. 792 (1973).

reduces the costs of employing inframarginal protected workers. Hence, firms with low protected-worker employment shares prior to the act may find it in their interests to increase their employment of protected workers. This quota effect amounts to a potential rehabilitation of the idea that CRA91 could lead employers to hire "by the numbers."

However, an opposing effect may arise if firms vary in their susceptibility to employment discrimination litigation. Richard Epstein argues that firms with greater susceptibility to litigation face higher expected costs of employing protected workers and hence employ fewer protected workers in equilibrium.[7] Increases in expected costs of litigation by protected workers exacerbate this effect; thus, our sorting effect predicts that firms employing few protected workers in the pre-CRA91 period should further reduce employment of protected workers after the law.

We attempt to sort out the relative importance of the quota and sorting effects by conducting an empirical analysis of employment outcomes for protected workers around the time of the passage of CRA91. We collect employment data from the 1980 and 1990 Censuses of Population and Housing and the 1983–96 Current Population Surveys (CPS) and construct various measures of susceptibility to employment discrimination litigation using data from the EEOC, census, CPS, and the Survey of Income and Program Participation. Because our data are at the employee level and the employer is not identified, we use protected-worker concentration by industry to proxy for firm-level protected-worker shares. Our empirical model relates post-CRA91 changes in protected-worker employment shares to pre-CRA91 levels of protected-worker employment shares and measures of susceptibility to litigation. If CRA91 had a strong sorting effect, then we would expect those industries that are most susceptible to discrimination claims and where protected workers' employment shares are lowest prior to the law to reduce their employment of protected workers after the law. If, on the other hand, CRA91 had a strong quota effect, then we expect industries with low pre-CRA91 protected-worker employment shares and relatively high discrimination exposure to increase employment shares after the law.

Our results suggest that, for CRA91, the sorting effect was stronger than the quota effect. We find a significant trend break in protected-worker employment shares around the time of CRA91. Before CRA91, black and female employment shares had been increasing in industries where those groups had been least represented. In addition, interindustry variation in protected-worker employment shares had been on the decline. Both trends ended after the act. We also present evidence on patterns in employee turnover to learn how this sorting took place. We detect no change in protected-worker turnover, relative to unprotected workers, following CRA91. Thus, it appears the sorting effect

---

[7] Richard A. Epstein, Forbidden Grounds: The Case against Employment Discrimination Laws (1992).

operated by shifting employees into new industries largely when they would have been switching jobs anyway.

On balance, our results suggest that CRA91 may have had quite different effects from earlier employment discrimination legislation. Prior researchers have documented modest but significant improvements in employment outcomes for protected workers subsequent to changes in the law: for example, Jonathan Leonard,[8] John Donohue and James Heckman,[9] and Kenneth Chay.[10] If one goal of CRA91 was to integrate industries where protected groups were underrepresented prior to the law, we find little evidence to indicate the law was successful. This finding also implies that there is no support for the widely discussed fear that CRA91 was a "quota bill."

II. The Civil Rights Act of 1991: The Law and the Debate

A contentious congressional debate regarding new antidiscrimination legislation raged throughout most of 1990. Two bills were eventually passed: the ADA and then a civil rights bill. After signing the ADA, President Bush vetoed the civil rights bill, citing its encouragement of quota-based hiring. As the administration and Congress sought a compromise, arguments over what legislative measures would or would not lead to quotas filled editorial and news pages. After the summer 1991 Clarence Thomas confirmation hearings brought employment discrimination back into the headlines, Congress passed, and Bush signed, CRA91. The Civil Rights Act of 1991 contained most of the provisions of the vetoed bill (with one important exception, noted below). Roger Clegg, a deputy assistant attorney general in the Bush administration, later wrote that during the 1991 negotiations over what became CRA91, the administration was "enormously successful (and accurate) in characterizing the proposed legislation as 'a quota bill' and a 'lawyer's bonanza.'"[11]

The Civil Rights Act of 1991 amended the Civil Rights Acts (CRAs) of 1866 and 1964, the Age Discrimination in Employment Act (ADEA), and the ADA. While many of the law's provisions had the potential to affect employment relationships, we focus our discussion on two main points: changes in the ability of plaintiffs to use statistical evidence to prove discrimination and increases in damage awards available to plaintiffs who prove

[8] Jonathan S. Leonard, The Impact of Affirmative Action Regulation and Equal Employment Law on Black Employment, 4 J. Econ. Persp. 47 (1990).

[9] John J. Donohue III & James J. Heckman, Continuous versus Episodic Change: The Impact of Civil Rights Policy on the Economic Status of Blacks, 29 J. Econ. Literature 1603 (1991).

[10] Kenneth Y. Chay, The Impact of Federal Civil Rights Policy on Black Economic Progress: Evidence from the Equal Employment Opportunity Act of 1972, 51 Indus. & Lab. Rel. Rev. 608 (1998).

[11] Roger Clegg, Introduction: A Brief Legislative History of the Civil Rights Act of 1991, 54 La. L. Rev. 1459 (1994).

discriminatory intent on the part of an employer.[12] A series of 1970s Supreme Court rulings (most notably *Griggs v. Duke Power*)[13] had allowed plaintiffs to show unlawful discrimination by demonstrating that an employer's practices led to a "disparate impact" on protected groups even if there was no discriminatory intent on the part of the employer. A 1989 Court decision, *Wards Cove Packing Co. v. Atonio,*[14] made it significantly more difficult for plaintiffs to prove disparate impact. After *Wards Cove*, plaintiffs could not show disparate impact using statistical evidence alone—plaintiffs were required instead to demonstrate that a particular employment practice led to the differing effect on protected groups.[15] The Civil Rights Act of 1991 relaxes this standard somewhat by allowing the use of statistical evidence without identification of a particular employment practice, provided the plaintiff can show the employer's decision-making process cannot be separated into specific practices.[16]

　　Three provisions of the act affect the potential size of damage awards. First, the law allows employees who claim "disparate treatment"—that is, intentional discrimination on the part of an employer—under Title VII to sue for punitive and compensatory damages. Prior to CRA91, damage awards had been limited to back pay. Maximum punitive and compensatory damages vary by employer size, ranging from $0 for firms with fewer than 15 employees up to $300,000 for firms with more than 500 employees. Second, CRA91 explicitly extends the CRA of 1866, which allows plaintiffs alleging racial discrimination to sue for unlimited punitive and compensatory damages, to cover termination of employment. Earlier Supreme Court decisions (notably *Patterson v. McLean Credit Union*)[17] had limited the applicability of the CRA of 1866 to the formation of employment relationships.[18] Hence,

---

[12] See Robert K. Robinson *et al.*, Equal Employment Requirements for Employers: A Closer Review of the Effects of the Civil Rights Act of 1991, 43 Lab. L. J. 725 (1992), for a more detailed discussion of these and other aspects of CRA91.

[13] Griggs v. Duke Power Co., 401 U.S. 424 (1971).

[14] Wards Cove Packing Co. v. Atonio, 490 U.S. 642 (1989).

[15] This ruling also reduced the employer's burden of proof in defending against disparate impact claims. Whereas prior to *Wards Cove* employers were required to show the "necessity" of the questioned employment practice, this ruling required employers to document only a legitimate business justification.

[16] Many observers (see, for example, Clegg, *supra* note 11) point to congressional displeasure with *Wards Cove* and other 1989 Court decisions as a primary motivation for the drafting of new civil rights legislation. Indeed, the vetoed 1990 CRA allowed plaintiffs to use statistical evidence to demonstrate disparate impact without identifying any specific employment practices.

[17] Patterson v. McLean Credit Union, 485 U.S. 617 (1989).

[18] The 1866 act forbids discrimination on the basis of race in the "making and enforcement of contracts." While the Johnson v. Railway Express Agency, 421 U.S. 454 (1975), and Runyan v. McCrary, 427 U.S. 160 (1976), decisions clearly interpreted this act as applying to employment contracts, these decisions did not clarify whether it also applies to the ending of employment contracts. (Note that the language of the act is somewhat ambiguous on this point.) Throughout the 1980s, different federal courts applied somewhat different interpretations on

CRA91 effectively removed all limits on damage awards in cases of unlawful racial discrimination in termination. Third, CRA91 gives plaintiffs seeking punitive damages the right to a jury trial. Because juries are perceived to favor claims of individuals over those of corporations, this change may have increased expected plaintiff damage awards.[19]

While critics of the act asserted that the threat of hiring-based disparate-impact lawsuits would force firms to hire "by quota," the act does not appear to have led to increases in either hiring-based lawsuits or disparate-impact claims. Nager and Broas point out that plaintiffs must show disparate treatment in order to earn punitive and compensatory damages and conclude (on the basis of their experience as attorneys) that plaintiffs have been willing to take on this more stringent burden of proof in order to earn the higher damage awards.[20] Similarly, Donohue and Siegelman document a dramatic shift toward firing-based employment discrimination claims throughout the 1970s and 1980s.[21] Our own examination of discrimination claims filed with the EEOC during the 1990s reveals a continuation of this trend, with termination-based complaints outnumbering hiring-based complaints by approximately 10 to one. This shift toward firing-based suits is particularly significant when viewed in light of a model developed by Ayres and Siegelman.[22] They consider a setting in which the variability of productivity is higher among black workers than white workers. If employers use probationary hiring to gain information about workers' productivities, then a higher fraction of blacks are fired compared to whites. If, however, employers must fire in proportion to the racial makeup of their workforces, then employers respond by reducing the hiring of black employees. Hence, Ayres and Siegelman conclude that if disparate-impact protections are applied to firms' firing decisions, then firms may have an incentive to shift their mix of employees away from protected workers, further limiting pressures to hire "by quota."

---

this point, which meant that some plaintiffs alleging racial discrimination in firing were allowed to proceed under the CRA of 1866, while others could sue under Title VII only. In the 1989 *Patterson* decision, the Supreme Court ruled that the CRA of 1866 did not apply to the termination of employment contracts. Thus, between 1989 and 1991 plaintiffs in such cases could sue under Title VII only.

[19] Eric Helland & Alexander Tabarrok, Runaway Judges? Selection Effects and the Jury, 16 J. L. Econ. & Org. 306 (2000), examines the widely held notion that juries are more sympathetic than judges are toward plaintiffs. They find that while award sizes and plaintiff-win rates in personal injury cases do vary according to whether judges or juries decide cases, much of this difference can be explained by selection effects.

[20] Nager & Broas, *supra* note 3.

[21] Donohue & Siegelman, *supra* note 4.

[22] Ayres & Siegelman, *supra* note 5.

III.   POTENTIAL EFFECTS OF THE CIVIL RIGHTS ACT OF 1991

A.   The Sorting Effect

Epstein, among others, has noted that minority workers are likely to be sorted into industries where the mutual gains from employment are highest.[23] Among the factors that can influence these mutual gains are the extent to which employers discriminate, the extent to which employees perceive discrimination, and the costs associated with employment discrimination litigation. Many factors are likely to make firms and industries differ significantly in their susceptibility to discrimination litigation, leading to differential costs of employment of protected workers. For example, firms with more centralized human resource management systems may be better able to assess litigation risks associated with personnel decisions. Alternatively, some firms may benefit from catering to customers or suppliers who prefer dealing with certain types of employees.

Suppose firms A and B are identical in every way, except that firm A is less susceptible to employment discrimination litigation. Given that even prior to CRA91 the potential costs of discrimination litigation were substantial, we would expect firm A to employ a higher fraction of protected workers than firm B before CRA91.[24] The passage of CRA91 increases the marginal cost of employing a protected worker at both firms, but this increase should be larger at firm B than at firm A. As a result, the law increases firm B's resistance to hiring protected workers more than it increases firm A's. This sorting effect suggests that firms that are more susceptible to litigation will have both smaller employment of protected workers prior to the passage of CRA91 and a larger reduction in employment of protected workers after the act.[25] Hence, the sorting effect predicts that CRA91 will result in further polarization or sorting of the workforce, with protected workers becoming more concentrated in the firms and industries where they are least costly to employ.

---

[23] Epstein, *supra* note 7.

[24] In order for differing susceptibilities to litigation to persist in equilibrium, there must be some cost to firm B of simply adopting firm A's practices. We argue that the presence of complementarities between certain product market and human resource strategies may yield persistent differences among firms' susceptibilities to litigation.

[25] In an earlier version of this paper, we demonstrated that the only assumptions needed for this result are that firms vary in susceptibility to litigation and have comparable labor demand elasticity. See Paul Oyer & Scott Schaefer, Sorting, Quotas, and the Civil Rights Act of 1991: Who Hires When It's Hard to Fire? (working paper, Northwestern Univ. 1999). Also, while our discussion here emphasizes termination-based cases, it is straightforward to show that suits centering on on-the-job treatment (harassment or discriminatory promotion practices, for example) will have similar labor market effects. Our empirical analysis encompasses this point, as our measure of EEOC complaint rates (constructed below) includes all non-hiring-based complaints.

B.   The Quota Effect

We next consider the possibility that the hiring of the marginal protected worker may affect the expected costs stemming from employment discrimination litigation on the part of inframarginal protected workers. We argue that this gives rise to a quota effect, which encourages firms with relatively few protected employees to hire more. We justify this argument by briefly outlining the judicial tests applied to disparate-treatment cases, initially set out by the Supreme Court in *McDonnell Douglas Corp. v. Green*.[26] As described by Epstein, *McDonnell Douglas* sets out a "tripartite standard of prima facie case, justification, and pretext."[27] A plaintiff who alleges disparate treatment must first establish a prima facie case of discrimination by documenting that "the most common nondiscriminatory reasons for the plaintiff's rejection do not apply."[28] Next, the burden shifts to the defendant, who must "articulate some legitimate, nondiscriminatory reason" for the employment decision.[29] Finally, the plaintiff is "afforded a fair opportunity to show that the (defendant's) stated reason for the (plaintiff's) rejection was in fact pretext."[30] In attempting to show pretext, the plaintiff may use evidence that does not directly relate to the particular employment decision that generated the dispute. In particular, the plaintiff may introduce evidence regarding the defendant's "general policy and practice with respect to minority employment" and may also use "statistics as to (the defendant's) employment policy" to show a "general pattern of discrimination."[31] This pretext standard thus gives plaintiffs broad scope to examine an employer's general employment practices with regard to protected workers. We therefore argue that by employing more protected workers, an employer can make it more difficult for displaced protected workers to prove unlawful discrimination.

This discussion suggests that the probability that a worker wins an employment discrimination lawsuit is a decreasing function of the number of protected workers employed by the firm. If this effect is important, then hiring the marginal protected worker reduces the likelihood that suits by inframarginal workers are successful, reducing both the likelihood that inframarginal protected workers sue and the expected costs to the firm conditional on being sued. This may make hiring additional protected workers more attractive to firms that are susceptible to litigation. If, in addition, the reduction in litigation costs imposed by inframarginal workers associated

[26] The standards set out in *McDonnell Douglas*, 411 U.S. 792, have been clarified, but not greatly altered, in subsequent Supreme Court rulings.

[27] Epstein, *supra* note 7.

[28] See Texas Dep't Community Affairs v. Burdine, 450 U.S. 248 (1981).

[29] *McDonnell Douglas*, 411 U.S. at 792.

[30] 411 U.S. at 804.

[31] 411 U.S. at 805.

with the hiring of the marginal protected worker is larger at firms that employ fewer protected workers, then we would expect this quota effect to lead to employment gains among protected workers in industries where such workers had been less represented. This effect may therefore work in the opposite direction from the sorting effect. While the sorting effect suggests that firms with higher susceptibility to employment discrimination litigation and lower pre-CRA91 protected-worker employment shares should further reduce protected-worker employment after the law, the quota effect suggests the reverse.

These two effects have very different implications for wages and employment of protected workers. If the sorting effect dominates, then CRA91 increases the marginal cost of employing protected workers, leading to reductions in wages and employment. Conversely, if the quota effect is important, then CRA91 reduces the cost of employing the marginal protected worker, leading to wage and employment gains.[32] As our discussion here offers no prediction as to whether the sorting or quota effect is stronger, we next attempt to assess this question empirically.

IV. EMPIRICAL ANALYSIS

A. Data and Methodology

The objective of our empirical analysis is to examine labor market trends in the pre- and post-CRA91 periods for evidence consistent with the sorting or quota effect. The sorting effect suggests that firms with lower susceptibility to employment discrimination litigation and higher pre-CRA91 protected-worker employment shares should increase their employment of protected workers after the law, while the quota effect suggests the reverse.[33] We therefore proceed by examining relationships between (i) post-CRA91 changes in protected-worker employment shares, (ii) pre-CRA91 levels of protected-worker employment shares, and (iii) various measures of susceptibility to employment discrimination litigation.

Because firm-level data on protected-worker employment shares are unavailable, we proxy for firm-level employee demographics with data at the industry level. We gather data on employment of protected workers from the

---

[32] Evidence in Paul Oyer & Scott Schaefer, Litigation Costs and Returns to Experience, 92 Am. Econ. Rev. (forthcoming 2002), provides some initial support for the sorting effect: to the extent CRA91 affected protected-worker wages or employment at all, the effect was negative.

[33] Our logic does not preclude the possibility that disparate-impact hiring liability could lead firms with low pre-CRA91 protected-worker employment shares to increase employment of protected workers after the law. In our empirical analysis, we seek to examine employment data for evidence of quotas or sorting, but we make no effort to determine whether any observed quota effects arise from changes in the disparate-impact standards or increases in disparate-treatment damage awards.

1983–96 Annual Demographic Files of the Current Population Survey (CPS). The CPS asks respondents monthly about current employment status including hours worked, full-time/part-time status, industry, and occupation. The Annual Demographic Files, which are gathered by the CPS each March, consist of a specific set of more detailed questions on employment outcomes over the past year. The March CPS gathers information such as weeks and hours of work, rates of pay, and number of jobs. We divide our full 1983–96 sample into three chronological subsamples—a "historical" sample (1983–86), a "pre-CRA91" sample (1988–91), and a "post-CRA91" sample (1993–96). The key variable of interest to us is the change in industry-level protected-worker employment shares from the pre-CRA91 sample to the post-CRA91 sample. Differences in employment shares from the historical sample to the pre-CRA91 sample allow us to examine and control for any preexisting trends in protected-worker employment.[34]

To measure levels of protected-worker employment shares, we use the 5 percent samples of the 1980 and 1990 censuses. This second source of industry protected-worker employment share information insures that any measurement error in our explanatory variables is uncorrelated with the error in our dependent variable (see below). We use the 1990 census as a predictor of pre-CRA91 to post-CRA91 protected-worker employment share changes and the 1980 census as a predictor of protected-worker employment share changes from our historical to pre-CRA91 samples. Data limitations cause a timing inconsistency between our historical sample (taken from the 1983–86 CPS) and our levels measure (from the 1980 census). Unfortunately for our purposes, March CPS industry classifications changed in 1983, so we cannot use pre-1983 CPS information. Also, as the census 5 percent sample is gathered only every 10 years, we are not able to match this source more contemporaneously with the CPS.

We focus our analysis on two groups of protected workers. We use the black employment share—that is, number of black workers in the industry divided by the number of workers in the industry—and female employment share as measures of the composition of workers in a given industry. While CRA91 also affected the rights of older workers and nonblack minorities, we omit consideration of these protected groups. We drop older workers because the effects of CRA91 on the ADEA were relatively minor and because demographic changes over the period we study would make it dif-

---

[34] If workers who switch jobs could be assumed to be representative of the overall set of workers (with the possible exception of protected status), then we could restrict attention in our study to flow of employees into an industry as an indicator of how the industry is adjusting to a post-CRA91 equilibrium. However, as shown by Henry S. Farber, The Analysis of Inter-Firm Worker Mobility, 12 J. Lab. Econ. 554 (1994), many new jobs are short-lived; hence, the change in an industry's stock of workers may not be accurately reflected by the flow into the industry. We did conduct an analysis looking only at new jobs and found similar, though much less precisely estimated, results.

ficult to separate the effect of CRA91 from other factors affecting employment of older workers. Nonblack minorities are omitted because rates of discrimination complaints are much lower among these workers than among blacks and because of the effects of immigration on changes in the composition of this group. Our final sample of protected individuals consists of blacks and women between the ages of 20 and 59 who reported working at least 20 hours in the week prior to being surveyed. We focus on industries for which the CPS surveyed at least 100 workers (protected or unprotected) who met these age and hours criteria in each of our chronological subsamples. While there are 243 three-digit industries represented in our CPS data, only 196 meet this selection criterion for both the pre- and post-CRA91 samples. Our number of industries narrows to 194 when we consider the historical sample as well.

We gather data from several sources to devise measures of industries' susceptibility to employment discrimination litigation. Our most direct measure is derived from a record of employment discrimination complaints filed with the EEOC. Litigants who charge employment discrimination under Title VII must start their pursuit of redress with a complaint to the EEOC. For each complaint, our EEOC data list the industry (two-digit) of the firm, the issue of the complaint (for example, termination, hiring, or harassment), the basis of the complaint (for example, age, race, or gender), and demographic information about the person filing the complaint. We analyze gender-based nonhiring complaints filed by women and race-based nonhiring complaints filed by blacks.[35] This deletes the relatively small number of reverse discrimination cases. Within each industry, we measure the propensity of workers to file EEOC complaints as the ratio of complaints filed between 1987 and 1991 to workers employed in 1990. That is, we start with the number of non-hiring-based complaints filed between 1987 and 1991 by members of protected group k employed in industry i and then divide by the number of workers in protected group k employed in industry i as reported by the 1990 census.[36]

We construct a set of less direct measures of susceptibility to litigation from the CPS, the census, and the Survey of Income and Program Partici-

[35] This discussion is subject to one qualification. Because race-based claims filed under the CRA of 1866 do not go through the EEOC, our measure of complaints omits such lawsuits. However, in the years before CRA91 (and especially after *Patterson*), termination-based cases could not be filed under the CRA of 1866. Our measure of EEOC complaint rates may therefore understate firms' exposure to race-based suits alleging discrimination in on-the-job treatment. Restricting our complaints measure to termination-based cases leads to moderate increases in the statistical significance of the EEOC complaint rate coefficients in Tables 6 and 7.

[36] We also measured the propensity to file complaints by computing the ratio of complaints filed in 1990 to workers employed in 1990. This shorter window of EEOC complaints increases the randomness in the arrival rate of complaints but eliminates measurement problems if employment growth rates and protected-worker employment shares changed differentially among industries. Our results are not sensitive to this measurement choice.

pation (SIPP). We use the 1990 census to calculate each industry's average employee education. All else equal, one might expect better educated people to have better access to legal assistance and, therefore, to be more likely to sue. This hypothesis is supported by a positive correlation between industry-level average education and EEOC complaint rates. We use wage data from the CPS to devise a measure of rents earned by employees in each industry. We first estimate a standard log wage regression containing controls for education, experience, experience squared, and indicator variables for gender, race, state, and year, and then compute rents as the average residual from this regression for employees within an industry. Workers earning larger rents may suffer more substantial losses from displacement and thus may be more likely to file complaints.

We use the 1987–93 SIPP to measure various characteristics of employers in each industry. We assess the prevalence of large employers by computing the share of workers employed by firms with more than 100 employees. While CRA91 allows litigants to impose greater damages on larger employers, these firms are perhaps also more able to bear the fixed costs of setting up human resource policies that make discrimination claims less likely. Hence, we do not have a solid expectation as to how size affects susceptibility to claims. We also devise a measure of decentralization, under the hypothesis that a more decentralized firm may be less able to monitor human resource decisions made by local managers and hence may be more susceptible to complaints. We define a worker as being at a small office of a larger firm if the employee reports working at a facility with 25 or fewer employees within a firm of 100 or more. For each industry, we compute the share of employees who work at a small office of a larger firm.

We use the 1980 and 1990 censuses to measure industry employment growth during the 1980s. It is difficult to predict whether high-growth industries would tend to be more or less susceptible to discrimination claims. We might expect low-growth industries to be relatively likely to incur complaints because of higher rates of involuntary separation. If terminated workers in low-growth industries have poor outside employment prospects, then the opportunity costs of litigating may be low. Also, as women's labor force participation has increased steadily for decades, women may be disproportionately affected by last-in, first-out layoffs in declining industries. On the other hand, higher growth companies may find it easier to adjust the composition of their workforces. Growing firms may also be perceived as having "deeper pockets," thereby attracting more claims and more sympathy for the claims of disgruntled employees.

As additional controls for changes in industry-level employment shares, we compute the fraction of workers in each industry who reside in states with strong and weak Fair Employment Practice (FEP) laws. If CRA91 affected overall employment levels of protected workers, then these effects would likely be stronger in states with weak employment protections, as

TABLE 1

INDIVIDUAL-LEVEL SUMMARY STATISTICS

	1988–96 March CPS		1988–96 March CPS
Total observations	496,379	Female (%)	45.1
Black (%)	8.4	Minority (%)	23.2
40 or older	39.0	Mean age	37.7
			(10.2)
Mean hourly earnings ($)	11.25	Mean weekly hours	42.3
	(22.32)		(11.2)
Years of education	13.4	State unemployment (%)	5.93
	(2.6)		(1.6)

NOTE.—Data are from the 1988–96 Annual Demographic File of the Current Population Survey (CPS) and include respondents between the ages of 20 and 59 who reported working at least 20 hours in the week prior to being surveyed. The CPS is conducted in March of each year. "Mean hourly earnings" is the respondent's average hourly earnings over the previous calendar year. "Mean weekly hours" refers to hours worked at all jobs in the week before the survey. Standard deviations are in parentheses.

CRA91-related changes in damage awards available to those employees are greatest. Hence, an industry whose employees are concentrated in states with weak state-level employment protections may show reductions in protected-worker employment share independent of any quota or sorting effects. Southern states have a history of particularly weak FEP laws, and early civil rights legislation has been shown to have been particularly effective there.[37] We therefore define the 13 states that passed secession ordinances in 1861 as weak FEP states.[38] Employment lawyers identify California, Illinois, Massachusetts, Minnesota, and New York as having particularly strong FEP laws and historically liberal courts, so we define these as strong FEP states. For each industry, we compute (using the 1990 census) the share of workers in that industry who reside in weak and strong states.[39]

Tables 1–5 summarize the CPS, census, and EEOC data that we use for the core of our analysis. Table 1 provides summary statistics for individual-level CPS data in our pre- and post-CRA91 samples. Table 2 summarizes the industry averages in the pre- and post-CRA91 periods, as well as the industry averages from the 1990 census. The table suggests there was a slight drop in average protected-worker employment shares after CRA91. This change is attributable to shifts of protected workers to larger industries, as opposed to a sharp drop in aggregate protected-worker employment. As our related work shows, CRA91 may have had mildly negative employment

[37] See Chay, *supra* note 10.

[38] These states are Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North and South Carolina, Tennessee, Texas, and Virginia.

[39] Note this argument also suggests that any sorting or quota effects may be more (less) pronounced in states with weak (strong) employment protections. We experimented with interacting our weak and strong-state share variables with 1990 census share level and EEOC complaint rate but found no conclusive evidence on this point.

TABLE 2

SUMMARY STATISTICS FROM 196 INDUSTRIES

	CPS Pre-CRA91	CPS Post-CRA91	1990 Census
Mean black employment share (%)	7.8	7.8	8.0
Mean female employment share (%)	39.8	38.9	39.3
Mean age			38.0
			(1.6)
Mean annual earnings ($)			24,582
			(6,960)
Median number of observations of individuals employed in industry	551	464	9,511

NOTE.—Figures are averages across the 196 three-digit Census Bureau industry classifications for which there are at least 100 Current Population Survey (CPS) respondents in both the pre- and post-CRA91 samples. Includes respondents to the census and CPS aged 20–59 who reported working at least 20 hours in the week prior to being surveyed. Standard deviations are in parentheses.

effects on blacks and females, but female labor force participation increased steadily throughout the entire period we study.[40]

Table 3 summarizes industry-level EEOC complaint rates and the relationship between complaint rates and protected-worker employment shares. Complaint rates are approximately 10 times higher for blacks than for women, and there is substantial variability in both groups' complaint rates across industries. The last row of the table displays the correlation between the industry complaint rate and the industry protected-worker employment share. For both women and blacks, this correlation is large, negative, and statistically significant; that is, in industries where women and blacks have relatively low representation, they file a relatively high number of complaints per employee.[41] In addition, the complaint rates for a given industry are highly correlated across the two protected groups (correlation coefficient of .74).

The finding of a negative correlation between protected-worker employment share and the pre-CRA91 EEOC complaint rate is consistent with the assumptions underlying both the sorting and quota effects. To see this, note that if industries differ in their susceptibility to discrimination suits, then we would expect industries with higher susceptibility (and thus perhaps a higher rate of complaints) to employ fewer protected workers. On the other hand, if the higher levels of protected-worker employment reduce a litigant's likelihood of prevailing, then higher employment shares would deter workers from filing complaints.

Tables 4 and 5 explore in more detail the cross-industry differences in

[40] Oyer & Schaefer, *supra* note 32.

[41] If there is considerable error in the EEOC's classification of industry codes, we would expect to find negative correlation because the misclassified observations would be disproportionately allotted to smaller industries. However, there is no correlation for other protected groups not specifically analyzed here (older workers and all minorities), which suggests that EEOC measurement error alone is not driving the correlation.

TABLE 3

EQUAL EMPLOYMENT OPPORTUNITY COMMISSION COMPLAINT SUMMARY STATISTICS

	Black	Female
Mean 1990 census share level (%)	8.38	39.19
Minimum (%)	1.44	5.66
Maximum (%)	22.3	91.4
1987–91 EEOC complaints per 1,000 workers	29.78	3.24
Minimum	3.06	.14
Maximum	140.60	12.70
Correlation between 1987–91 complaint rate and 1990 census share level	−.2359*	−.3702*

NOTE.—Figures are averages for the 72 two-digit standard industry classification industries for which we have Equal Employment Opportunity Commission (EEOC) and census data. Employment shares are number of protected workers in an industry divided by number of total workers in the industry computed using the 1990 census. Complaint rates are number of non-hiring-based discrimination complaints filed by members of a given protected group over the period specified divided by the estimated number of protected workers in that industry in 1990 computed using the 1990 census.

* $p < .05$.

employment shares and EEOC complaint rates for blacks and women, respectively. Panel A of each table lists the three-digit industries with highest and lowest protected-worker employment shares in the pre-CRA91 period, while panel B of each table lists the two-digit industries with highest and lowest EEOC complaint rates. Industries with high black employment shares include transit, household service, and textiles. Blacks are not highly represented in mining industries, and, notably, metal mining tops the list of industries with highest black EEOC complaint rates. Women dominate caregiving fields, household service, beauty shops, and libraries and are poorly represented in natural resource extraction, auto repair, railroads, and construction. The inverse relationship between employment share and complaint rate is again apparent, as railroads, forestry, and mining all show high rates of female EEOC complaints, while household service shows a low rate.

B.   Trends in Protected-Worker Employment Shares

Before turning to a formal analysis of the effects of CRA91, we present a few simple graphs to illustrate the primary changes in protected-worker employment shares around the time of the law's passage. In Figures 1 and 2, we plot the changes in protected-worker employment shares over the period we study. For each protected group, we split the set of three-digit industries into thirds on the basis of employment share in the 1990 census. We then examine how average protected-worker employment share varies over time within each group of industries. To illustrate the construction of Figures 1 and 2, we take, as an example, industries in the middle third of black employment share. We compute the average black employment share among this set of industries in the historical period to be 6.67 percent. Black em-

TABLE 4

INDUSTRIES WITH LARGEST AND SMALLEST EMPLOYMENT SHARE LEVELS AND EQUAL
EMPLOYMENT OPPORTUNITY COMMISSION COMPLAINT RATES: BLACK

A. EMPLOYMENT SHARES			
Highest 1990 Employment Share		Lowest 1990 Employment Share	
Industry	Share (%)	Industry	Share (%)
Taxi service	22.5	Chiropractic offices	1.1
Yarn, thread, and fabric mills	22.4	Miscellaneous vehicle dealers	1.3
Household service	22.3	Coal mining	1.4
Bus and urban transit	21.5	Retail nurseries	1.7
Knitting mills	20.3	Metal mining	1.8
B. EEOC COMPLAINT RATES			
Highest 1987–91 EEOC Complaint Rate		Lowest 1987–91 EEOC Complaint Rate	
Industry	Rate (%)	Industry	Rate (%)
Metal mining	14.1	Engineering and management	.3
Forestry	12.6	Crop production	.4
Local government	6.6	Household service	.5
Unions and associations	6.5	Agricultural services	.6
Oil and gas extraction	6.2	Public finance	.7

NOTE.—Panel A: Data are from the 1990 census; industries are three-digit census classifications. Panel B: Data are from the Equal Employment Opportunity Commission (EEOC) and 1990 census; industries are two-digit standard industry classification codes.

ployment share among these industries rose to 7.29 percent in the pre-CRA91 period but then fell to 6.98 percent in the post-CRA91 period. On our graph, we normalize the historical share to 100 and then plot how this share changes over the pre- and post-CRA91 periods. If our quota effect is dominant, then we expect industries with lowest protected-worker employment shares to register the largest increases in protected-worker employment. If the sorting effect is dominant, then we expect industries with highest protected-worker employment shares to achieve the highest gains.

Figure 1 focuses on blacks and depicts a clear trend break around the time of CRA91. From the historical to pre-CRA91 periods, black employment shares were increasing fastest in industries with medium and low black representation. From the pre- to post-CRA91 periods, however, average black employment shares fell for the low and medium black employment share industries and rose for the high black employment share industries. Figure 2 shows a similar pattern for women. From the historical to pre-CRA91 periods, industries with medium and low female employment shares were expanding their shares faster than industries with high female employment shares, but this trend also reversed after the act. Figures 1 and 2 are therefore consistent with CRA91 having a sorting effect, as the law seems to have reversed a trend toward increasing integration of industries that had previously employed few protected workers.

TABLE 5

INDUSTRIES WITH LARGEST AND SMALLEST EMPLOYMENT SHARE LEVELS AND EQUAL
EMPLOYMENT OPPORTUNITY COMMISSION COMPLAINT RATES: FEMALE

A. EMPLOYMENT SHARES			
Highest 1990 Employment Share		Lowest 1990 Employment Share	
Industry	Share (%)	Industry	Share (%)
Day care	96.1	Logging	5.5
Household service	91.4	Coal mining	5.7
Beauty shops	89.1	Railroads	8.2
Nursing homes	88.5	Auto repair	8.3
Libraries	82.2	Construction	9.1
B. EEOC COMPLAINT RATES			
Highest 1987–91 EEOC Complaint Rate		Lowest 1987–91 EEOC Complaint Rate	
Industry	Rate (%)	Industry	Rate (%)
Metal mining	1.3	Engineering and management	.01
Railroads	1.2	Household service	.05
Forestry	1.0	Apparel	.07
Local government	1.0	Livestock production	.07
Oil and coal refining	.9	Public finance	.08

NOTE.—Panel A: Data are from the 1990 census; industries are three-digit census classifications. Panel B: Data are from the Equal Employment Opportunity Commission (EEOC) and 1990 census; industries are two-digit standard industry classification codes.

Another way to examine heterogeneity of industries' protected-worker employment shares is to look at the variation of shares across industries. If CRA91 led protected workers to become more concentrated in certain industries, then there should have been a trend toward greater interindustry variance in employment shares beginning in about 1992. Restricting our sample to the 156 industries for which there are at least 50 observations in every CPS between 1983 and 1996, we compute the standard deviation of protected-worker employment shares for each year and for both of our protected groups. Figures 3 and 4 display the results. Interindustry variation in black employment shares trended down steadily from 1983 to the time of CRA91. Shortly after the act, however, the trend reversed sharply. Figure 4 shows a similar, if less pronounced, pattern in female employment shares. Figures 3 and 4 offer further evidence consistent with the view that CRA91 led protected workers to become more concentrated in industries where they were already well represented.

C.   Changes in Employment Shares before and after the Civil Rights Act of 1991

We next apply econometric methods to see if the pre- to post-CRA91 changes in protected-worker employment shares indicated by Figures 1–4
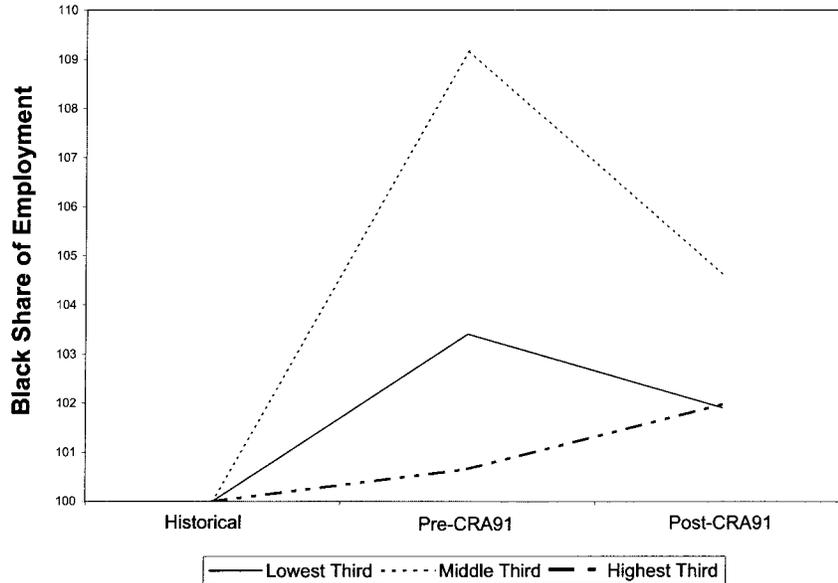
FIGURE 1.—Time trends in protected-worker employment shares: black

are of any significance. Our regression equation relates post-CRA91 changes in protected-worker employment shares to pre-CRA91 employment-share levels and measures of susceptibility to employment discrimination litigation. For a protected class k and industry i, we define the pre-CRA91 to post-CRA91 CPS change in the protected-worker employment share as

$$\text{CPS Share Change}_{ik} = \frac{p_{ik,93-96}}{n_{i,93-96}} - \frac{p_{ik,88-91}}{n_{i,88-91}},$$

where p_{ik_t} is the number of workers in a protected class k employed in industry i surveyed by the CPS in years t, and n_{it} is the total number of workers surveyed by the CPS in years t who were employed in industry i.[42]

It would be a straightforward calculation to measure the relationship between the CPS share change and pre-CRA91 CPS employment share. However, because the pre-CRA91 CPS share is an estimate based on a finite (though fairly large) sample, using the same pre-CRA91 measure for both the change in share and the initial share would likely bias the result toward finding evidence of the quota effect. That is, because the post-CRA91 measure

---

[42] We weight our regressions by n_{it} because the CPS share calculations are estimates from a limited sample. In economic terms, this makes the unit of observation a person rather than a firm. However, we also did the analysis without weights and found that this did not materially affect the conclusions.
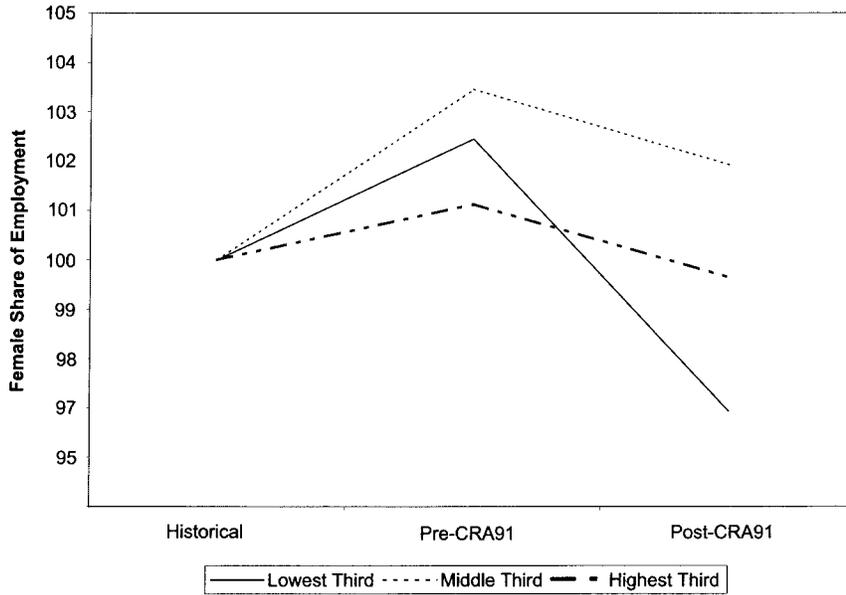
FIGURE 2.—Time trends in protected-worker employment shares: female

does not have the same sampling error as the pre-CRA91 estimate, there would likely be mean reversion in the shares even if this did not accurately reflect employer behavior. We therefore use the 1990 census as our measure of pre-CRA91 employment share because the sampling errors of the census and the pre-CRA91 CPS samples should be uncorrelated.[43] For protected-worker class k and industry i, we define the 1990 census level of protected-worker employment share as

$$1990 \text{ Census Share Level}_{ik} = \frac{q_{ik}}{m_i},$$

where q_{ik} is the number of workers in a protected class k employed in industry i surveyed by the 1990 census, and m_i is the total number of workers surveyed by the 1990 census who were employed in industry i.

For both protected classes k (black and female), we run regressions of the form

$$\text{CPS Share Change}_{ik} = \alpha_k + \beta_k \text{Census Share Level}_{ik} + \delta_k \mathbf{x}_{ik} + \epsilon_{ik}, \quad (1)$$

where $\mathbf{x}_{ik}$ is a vector of measures of industry i's susceptibility to employment

[43] In fact, because the census sample is very large, sampling error should be almost nonexistent.
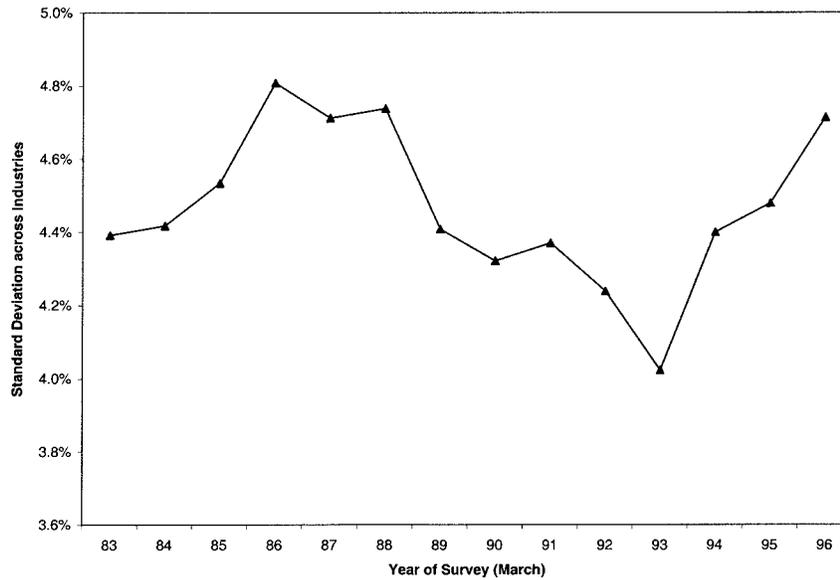
FIGURE 3.—Changes in interindustry variability of protected-worker employment shares: black.

discrimination litigation. As discussed above, our measures of susceptibility include EEOC complaint rates, average worker education, average worker wage residual, average firm size, small-office share, industry employment growth, strong-state share, and weak-state share. Note that this regression does not employ the historical data and thus ignores employment share trends before CRA91.[44] We therefore analyze changes at the time of the act and later compare them to changes that were occurring before CRA91.

We display regression results in Table 6. None of the coefficients on the pre-CRA91 employment share are statistically significant, though the estimates are reasonably precise. Some of the measures of susceptibility to lawsuits do appear to be related to employment share changes, but these do not point uniformly in the direction of either the sorting or quota effect. In columns 2 and 5, we find the coefficients on the EEOC complaint rates to differ in sign across the two protected groups. For blacks, our coefficient is positive and statistically significant at the 10 percent level. The point estimate suggests that an increase of one complaint per 1,000 workers corresponds to a post-CRA91 increase of .016 percent in black employment share. For

---

[44] While we could construct a first-difference specification here, the fact that there is little variation in our **x** variables over time would mean these variables would essentially be differenced out using such an approach.
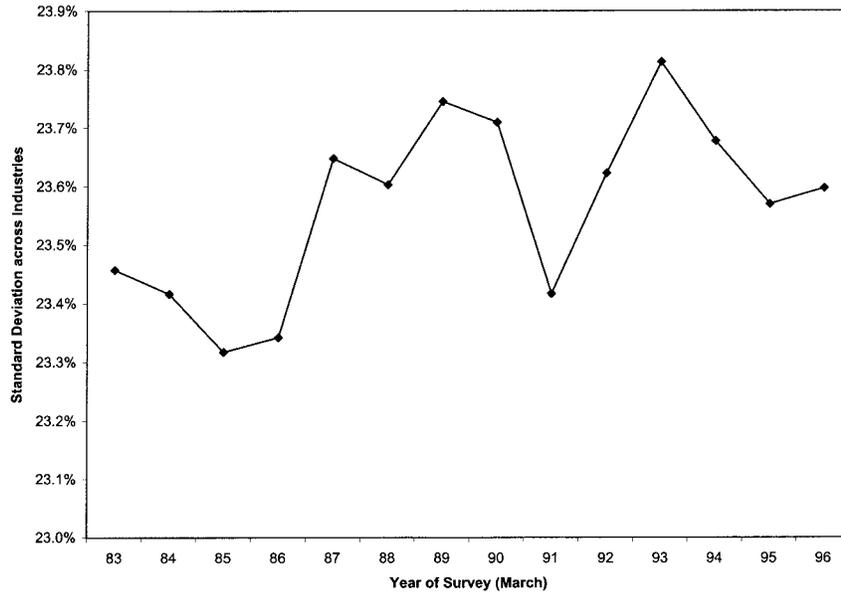
FIGURE 4.—Changes in interindustry variability of protected-worker employment shares: female.

women, we obtain an estimate that is quite large in magnitude, but negative; here, an increase of one complaint per 1,000 workers corresponds to a .345 percent post-CRA91 decrease in female employment share. However, this estimate is only statistically significant at the 15 percent level.

Few of the susceptibility measures in columns 3 and 6 enter significantly. Most notable among these is the coefficient on average worker education for women. This coefficient is positive and significant at better than the 1 percent level. While this finding could be interpreted as providing evidence in favor of a quota effect (as industries with many potentially litigious women hire additional women to reduce exposure to lawsuits), it may also reflect that women, as they have become more attached to the labor force and their relative pay has increased, have increased their penetration in high-education industries. We also find that black employment shares dropped in fast-growing industries, while female employment shares dropped in industries located in states with strong state-level employment protections. Overall, Table 6 appears to imply that CRA91 had neither a strong sorting nor a strong quota effect on employment of blacks or women.

However, as shown by Figures 1–4, any interpretation of Table 6 is incomplete without consideration of pre-CRA91 trends in protected-worker employment shares. If CRA91 changed trends in employment shares, then

TABLE 6

CHANGES IN PROTECTED-WORKER EMPLOYMENT SHARES: PRE- TO POST-CRA91 PERIODS

	BLACK			FEMALE		
PROTECTED GROUP	(1)	(2)	(3)	(4)	(5)	(6)
1990 census share level	−.0083	−.0530	.0295	.0148	.0107	.0020
	(.0339)	(.0327)	(.0430)	(.0147)	(.0184)	(.0192)
EEOC complaint rate		.1677			−3.445	
		(.0857)			(2.370)	
Average worker education			.0004			.0103
			(.0013)			(.0035)
Average worker wage residual			.0080			−.0013
			(.0100)			(.0261)
Large-firm share			−.0099			−.0161
			(.0098)			(.0202)
Small-office share			.0380			−.0592
			(.0249)			(.0593)
Employment growth			−.0081			.0008
			(.0033)			(.0078)
Strong-state share			.0330			−.1456
			(.0284)			(.0679)
Weak-state share			−.0261			−.0906
			(.0257)			(.0597)
Observations	196	71	196	196	71	196
R^2	.0003	.0911	.0597	.0052	.0535	.0775

NOTE.—EEOC = Equal Employment Opportunity Commission. Dependent variable: Current Population Survey (CPS) share change from pre-CRA91 to post-CRA91 periods. In columns 1, 3, 4, and 6, an observation is a three-digit Census Bureau industry code. In columns 2 and 5, an observation is a two-digit standard industry classification code. Industries were included only if there were at least 100 people who reported working in that industry in both the pre- and post-CRA91 CPS samples. Standard errors are in parentheses.

the innocuous effects suggested by Table 6 could be understated. To address this possibility, we applied the specification in equation (1) to the period before CRA91. We regress the historical to pre-CRA91 CPS change in protected-worker employment share on protected-worker employment shares from the 1980 census and the same vector of **x** variables used in Table 6.[45]

We display results in Table 7. For both blacks and women, there was a clear trend toward higher protected-worker employment shares in industries that had low protected-worker employment shares in 1980. That is, during the 1980s, the industries that had been the most segregated along racial and gender lines were becoming less so. Combining the regressions in Tables 6

[45] That is, our **x** vector includes education, wage residual, strong and weak-state measures from the 1990 census, large-firm and small-office measures from the 1987–93 SIPP, industry employment growth from the 1980–90 censuses, and EEOC data from 1987 through 1991. Unfortunately, limits on our EEOC data set prevent us from making use of claims filed during our historical period. We did run alternative specifications relying primarily on data from the 1980 census and obtained similar results.

TABLE 7

CHANGES IN PROTECTED-WORKER EMPLOYMENT SHARES:
HISTORICAL TO PRE-CRA91 PERIODS

	BLACK			FEMALE		
PROTECTED GROUP	(1)	(2)	(3)	(4)	(5)	(6)
1980 census share level	−.0485	−.0752	−.0550	−.0115	−.018	−.0153
	(.0218)	(.0255)	(.0261)	(.0082)	(.0101)	(.0105)
EEOC complaint rate		−.0308			−.7344	
		(.0903)			(1.320)	
Average worker education			.0003			.0049
			(.0011)			(.0019)
Average worker wage residual			.0080			.0144
			(.0080)			(.0147)
Large-firm share			.0048			−.0023
			(.0074)			(.0114)
Small-office share			.0190			.0666
			(.0197)			(.0321)
Employment growth			.0031			.0045
			(.0026)			(.0042)
Strong-state share			−.0175			−.0726
			(.0216)			(.0363)
Weak-state share			.0251			−.0270
			(.0193)			(.0319)
Observations	194	71	194	194	71	194
R^2		.0250	.1135	.0685	.0100	.0462	.1014

NOTE.—Dependent variable: Current Population Survey (CPS) share change from historical to pre-CRA91 periods. In columns 1, 3, 4, and 6, an observation is a three-digit Census Bureau industry code. In columns 2 and 5, an observation is a two-digit standard industry classification code. Industries were included only if there were at least 100 people who reported working in that industry in both the pre- and post-CRA91 CPS samples. Standard errors are in parentheses.

and 7, we find that the changes in the coefficients on census share levels are statistically significant for both blacks and females in our three-digit Census Bureau industry code regressions. This marked change in trend after CRA91 provides evidence that is consistent with CRA91 leading to a sorting effect. The most noteworthy change in the susceptibility coefficients also provides evidence to support the sorting effect. While women had been gaining share in more decentralized industries prior to CRA91, this trend ended after CRA91. In a combined regression, the difference in coefficient on decentralization is significant at the 7 percent level. We also find the trend toward lower black employment shares in growing industries did not predate CRA91. Trends toward higher female employment shares in high-education industries and lower female employment shares in industries subject to strong state-level employment protections were evident prior to CRA91.

As a robustness check, we relax the assumption implied by our equation (1) that the relationship between CPS share change and the census share level is monotonic. To see why this is potentially important, suppose the

quota effect is felt strongly by only those industries with the very lowest pre-CRA91 protected-worker employment shares. If the sorting effect dominates for all other firms, then our linear specification may still show larger gains in protected-worker employment shares for firms with larger pre-CRA91 levels. Allowing for a nonmonotonic relationship may provide evidence in favor of the quota effect—evidence that would be missed by the monotonic specification. This, however, was not supported by less restrictive regressions we ran.

Finally, note that the primary source of variation we use to analyze the effect of CRA91 is the share of workers from a given protected group and that factors other than legislation may impact these shares. Ideally, we would have liked to construct a control group of firms that were unaffected by CRA91. We then could have interacted our share measures with a control-group indicator variable. Such an approach would remove the effects of any broad trends affecting the labor market. However, because CRA91 affected all firms with 15 or more employees, we cannot create an ideal control group. Though our results were unaffected when we tried to approximate a control group by looking at state variation,[46] we cannot completely dismiss the possibility that some other structural change (such as, for example, the adoption of information technology) led to an end of labor market desegregation beginning in approximately 1991.[47]

D.   Reshuffling Employees after the Civil Rights Act of 1991

A common criticism of employment discrimination legislation is that, by making it harder to displace some workers, these laws lower employment among the very workers they were meant to protect.[48] Another concern raised by our results above is that such laws lead workers to move toward firms where, owing to the legislation, they are less costly to employ. If this leads to the loss of firm-specific human capital, then these movements result in some inefficiency. Any inefficient turnovers would likely have to come from voluntary quits by the workers, since imposing antidiscrimination rules would

[46] See note 39 *supra*.

[47] We believe we can dismiss the business cycle as a cause of our results, however, given the documented characteristics of the early 1990s recession. Robert W. Fairlie & Lori G. Kletzer, Race and the Shifting Burden of Job Displacement: 1982–1993, 119 Monthly Lab. Rev. 13 (1996), show that black job displacement was high relative to white displacement during the early 1980s recession, but not during the 1991–93 downturn.

[48] For empirical evidence on how recent discrimination legislation has affected protected-worker employment, see Thomas DeLeire, The Wage and Employment Effects of the Americans with Disabilities Act, 35 J. Hum. Resources 693 (2000); Daron Acemoglu & Joshua Angrist, Consequences of Employment Protection: The Case of the Americans with Disabilities Act, 109 J. Pol. Econ. 915 (2001); and Oyer & Schaefer, *supra* note 32.

TABLE 8

CHANGE IN PROTECTED-WORKER TURNOVER

	Black (1)	Female (2)
Protected group[a]	−.2767	−.1954
	(.0639)	(.0270)
Log likelihood	−78,888	−138,687
Observations	166,720	304,892

NOTE.—The dependent variable is one if the person reports having held more than one job during the calendar year before the March Current Population Survey survey. The comparison group in each regression is non-Hispanic white men under 40. Each logit includes indicator variables for high school degree, college degree, 5-year age categories, half-percent categories of state unemployment rate, and year. Standard errors are in parentheses.

[a] The protected-group coefficient corresponds to an indicator variable that equals one if the observation is a member of the relevant group. This indicator is also interacted with calendar year and the coefficients are displayed in Figure 5.

presumably not lead employers to terminate protected workers.[49] If, for example, the laws led to decreased relative wages at firms that expected a higher burden from the new laws, then workers might choose to leave. To see how CRA91 affected the turnover rates of protected workers, we use the individual-level data from the 1988–96 March CPS and look for a trend in protected-worker turnover. More specifically, we investigate whether there was a temporary increase in protected-worker turnover following the passage of CRA91 as workers sorted to the best matches.

To address this issue, we estimate the year-to-year relative turnover rates of protected workers using logits where the dependent variable is one if the person held more than one job during the calendar year.[50] The explanatory variables of interest are individual year effects, the protected-worker effect, and the interaction of the year effects and protected status. For each logit, we limit our sample to CPS respondents who are in one protected group only (that is, either black men or white women). As a comparison group, we select non-Hispanic white men under 40 because these workers are unambiguously not covered by CRA91. The logit in column 1 of Table 8 include black men under 40 and non-Hispanic white men under 40, while column 2

[49] See, for example, Paul Oyer & Scott Schaefer, Layoffs and Litigation, 31 Rand J. Econ. 345 (2000). Using the SIPP, they found that CRA91 led to a decrease in black men being involuntarily separated (and a particularly large decrease in black men being fired for cause) relative to white men. They also found no effect of CRA91 on overall black male turnover, so this section revisits that result with a different data set and broadens the analysis to include women.

[50] The CPS asks how many employers the respondent had in the previous year and instructs, "If more than one at same time, only count it as one employer" (see CPS documentation). Hence, if a respondent simply held two jobs at one time, this would not show up as turnover in this data.
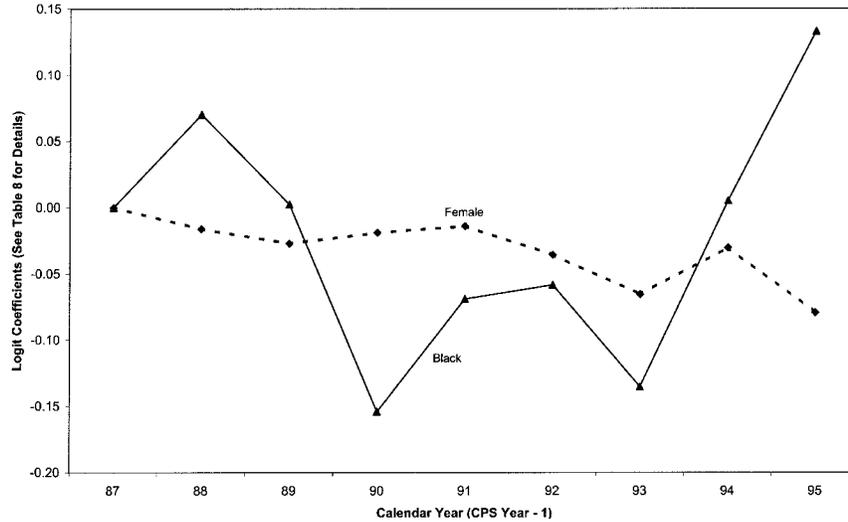
FIGURE 5.—Turnover trends for protected workers

compares non-Hispanic white women under 40 to non-Hispanic white men under 40.[51] If CRA91 led to increased turnover as workers sorted, we would expect the protected-worker effects to increase in 1992 and return to pre-CRA91 levels after a period of adjustment.

As shown in Table 8 and Figure 5, however, there is no evidence that CRA91 had any effect on the relative propensity of protected workers to change jobs. Figure 5 shows the logit coefficients for the year–protected group interaction terms in each logit, with 1987 (that is, the March 1988 CPS) serving as the excluded category. None of the year effects shortly after CRA91 are significant, and inspection of the graph makes it clear that there is no indication that protected-worker turnover changed post-CRA91. While Table 8 shows that the turnover rates of young white men differ from those of blacks and women, Figure 5 indicates that these differences did not change in any systematic way as a result of CRA91. This suggests that, to the extent that CRA91 led to changes in the distribution of protected workers among firms, the change was achieved through turnover that would have occurred even without CRA91, rather than workers changing jobs in response to a CRA91-induced change in firm/worker match quality.

---

[51] Our specifications also include controls for a number of relevant factors, such as age, state unemployment rate, and education. We use a series of dummy variables for each of these effects, so as to impose fewer restrictions. See note to Table 8 for details.

V.   CONCLUSION

In this paper, we analyze the effects of CRA91 on the composition of firms' workforces. Unlike most prior work on the effects of employment protections, our analysis explicitly incorporates the notion that firms may be heterogeneous in their responses to new legislation and that this fact may result in redistribution of protected workers among employers. We identify two ways in which increases in costs of displacing workers may affect the composition of firms' workforces. We discuss how CRA91 could lead to a sorting effect—where increases in damage awards cause protected employees to be sorted to the firms where they are least costly to employ—and a quota effect—where firms with low concentrations of protected employees hire more in order to reduce their exposure to lawsuits from inframarginal protected employees.

Using data from the census, CPS, and EEOC filings, we try to assess the relative strengths of the sorting and quota effects. Our primary finding is that there was a distinct change in the trends in the employment shares of blacks and women after CRA91 and that this change was consistent with the sorting effect. In the years leading up to CRA91, industries with relatively few women and blacks had been increasing their share of such workers, but this trend stopped shortly after CRA91. Overall, our results suggest that the widely discussed fear that CRA91 would lead firms to hire "by quota" was not justified. However, to the extent that CRA91 was intended to open new opportunities to the groups it protects, we find no evidence that it succeeded.

BIBLIOGRAPHY

Acemoglu, Daron, and Angrist, Joshua. "Consequences of Employment Protection: The Case of the Americans with Disabilities Act." *Journal of Political Economy* 109 (2001): 915–57.

Ayres, Ian, and Siegelman, Peter. "The Q-Word as Red Herring: Why Disparate Impact Liability Does Not Induce Hiring Quotas." *Texas Law Review* 74 (1996): 1487–1526.

Chay, Kenneth Y. "The Impact of Federal Civil Rights Policy on Black Economic Progress: Evidence from the Equal Employment Opportunity Act of 1972." *Industrial and Labor Relations Review* 51 (1998): 608–23.

Clegg, Roger. "Introduction: A Brief Legislative History of the Civil Rights Act of 1991." *Louisiana Law Review* 54 (1994): 1459–71.

DeLeire, Thomas. "The Wage and Employment Effects of the Americans with Disabilities Act." *Journal of Human Resources* 35 (2000): 693–715.

Donohue, John J., III, and Heckman, James J. "Continuous versus Episodic Change: The Impact of Civil Rights Policy on the Economic Status of Blacks." *Journal of Economic Literature* 29 (1991): 1603–43.

Donohue, John J., III, and Siegelman, Peter. "The Changing Nature of

Employment Discrimination Litigation." *Stanford Law Review* 43 (1991): 983–1033.

Epstein, Richard A. *Forbidden Grounds: The Case against Employment Discrimination Laws.* Cambridge, Mass.: Harvard University Press, 1992.

Fairlie, Robert W., and Kletzer, Lori G. "Race and the Shifting Burden of Job Displacement: 1982–1993." *Monthly Labor Review* 119 (1996): 13–23.

Farber, Henry S. "The Analysis of Inter-firm Worker Mobility." *Journal of Labor Economics* 12 (1994): 554–93.

Helland, Eric, and Tabarrok, Alexander. "Runaway Judges? Selection Effects and the Jury." *Journal of Law, Economics, and Organization* 16 (2000): 306–33.

Leonard, Jonathan S. "The Impact of Affirmative Action Regulation and Equal Employment Law on Black Employment." *Journal of Economic Perspectives* 4 (1990): 47–63.

Nager, Glen D., and Broas, Julia M. "Enforcement Issues: A Practical Overview." *Louisiana Law Review* 54 (1994): 1473–86.

Oyer, Paul, and Schaefer, Scott. "Sorting, Quotas, and the Civil Rights Act of 1991: Who Hires When It's Hard to Fire?" Working paper. Evanston, Ill.: Northwestern University, 1999.

Oyer, Paul, and Schaefer, Scott. "Layoffs and Litigation." *Rand Journal of Economics* 31 (2000): 345–58.

Oyer, Paul, and Schaefer, Scott. "Litigation Costs and Returns to Experience." *American Economic Review* 92 (2002): forthcoming.

Robinson, Robert K., et al. "Equal Employment Requirements for Employers: A Closer Review of the Effects of the Civil Rights Act of 1991." *Labor Law Journal* 43 (1992): 725–34.