

Sorting, Quotas, and the Civil Rights Act of 1991: Who Hires When It's Hard to Fire?*

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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. In this paper, we analyze the effects of CRA91 on firms’ labor demand. We model employer behavior when firms vary exogenously in their susceptibility to discrimination suits and when firms can reduce their 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. We examine these effects empirically using data from the Current Population Survey, the EEOC and the Census Bureau. We find that trends in employment shares of protected workers changed after CRA91 in a manner consistent with the sorting effect. We also find that EEOC complaint rates vary substantially across industries and are negatively correlated with protected-worker employment shares. Industries with high gender-discrimination EEOC complaint rates decreased their female employment shares after CRA91. Our results uncover no evidence that CRA91 lead to widespread quota hiring and no evidence that CRA91 helped integrate industries that excluded protected workers.

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1 Introduction

The early 1990's 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 pieces of new legislation were enacted. The Americans with Disabilities Act (ADA) offered broad new protections to physically challenged employees. The Civil Rights Act of 1991 (CRA91), while not defining any new protected group, significantly increased the rights of plaintiffs in discrimination complaints to the Equal Employment Opportunity Commission (EEOC) and in Federal Civil Court. Many opponents of CRA91 warned that the bill would lead to hiring "by quota."¹ Critics focused on CRA91's strengthening of a plaintiff's ability to use statistical evidence to prove unlawful discrimination by demonstrating a "disparate impact" on protected employees, 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 examine some of these claims, and to provide a theoretical and empirical assessment of the effects of CRA91 on firms' hiring practices

There are at least two reasons to think that critics' predictions of widespread quota-based hiring may have been overstated. First, CRA91 significantly increased damage awards plaintiffs can earn in employment discrimination cases. Title VII plaintiffs could recover only back wages before 1991, but CRA91 amends this law to allow plaintiffs to earn both compensatory and punitive damages. The Act also extends the Civil Rights Act of 1866, which allows plaintiffs to sue for unlimited damages in cases of *racial* discrimination, to cover on-the-job activities and termination of employment contracts. In order to be eligible for the larger damage awards stemming from CRA91, however, plaintiffs must show "disparate treatment" — that is, intentional discrimination on the part of an employer. According to Nager and Broas (1994), it appears most post-1991 plaintiffs have been willing to undertake the more stringent burden of proof associated with disparate treatment in order to try to earn these higher damages, which means the provisions of CRA91 relating to disparate impact have had relatively minor effects. Second, as Donohue and Siegelman (1991) have documented, there has been a dramatic shift over the last quarter century away from hiring-

¹ 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."

based employment discrimination litigation and toward firing-based suits. A model by Ayres and Siegelman (1996) shows 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 actually have the opposite effect. Based on these arguments, one might conclude that there is little reason to think that CRA91 would lead employers to increase employment of protected workers.

We show, 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 result 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* decision, implies that the costs of employing protected workers may be concave — the hiring of the marginal protected worker *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. The derivation of this *quota effect* amounts to a potential rehabilitation of the idea that CRA91 could lead employers to hire “by the numbers.”² Our analysis does not, however, suggest that this is the only effect of the Act. Also present in our model is a *sorting effect* that reflects the idea that firms may be heterogeneous in their costs of employing protected workers (see Epstein (1992)). In our model, firms with higher susceptibility to employment discrimination litigation (and therefore presumably lower pre-CRA91 protected-worker employment shares) are more strongly impacted by the increases in potential damage awards, and thus may reduce protected-worker employment in response to the Act. Our model yields these two opposing effects, but offers no guidance as to which is stronger. We then turn to an empirical analysis, using employment data from the 1980 and 1990 Censuses and the 1983-1996 Current Population Surveys, to assess the relative strengths of the sorting and quota effects. Since this data is at the employee level and the employer is not iden-

² Our analysis is intended to show how increases in termination-side damage awards could lead to this effect, even in the absence of increased litigation on the basis of disparate impact. Our model does not rule out 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.

tified, we use protected-worker concentration by industry to proxy for firm-level protected-worker shares. If the sorting effect predominates, then we expect those industries where protected workers' employment shares are lowest prior to the law to reduce their employment of protected workers in the period after the law. If, on the other hand, the quota effect predominates, then we expect those industries with low pre-CRA91 protected-worker employment shares to have higher shares after the law. Our empirical 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 those industries that had low shares and inter-industry variation in employment shares had been on the decline. After the Act, both of these trends ended. We then examine the relationship between protected worker employment shares and the frequency of EEOC complaints. We find evidence for two necessary conditions for sorting to be an important phenomenon – industries vary substantially in their propensity to draw EEOC complaints and, for some classes of protected workers, there is a strong negative relationship between industries' EEOC complaint rates and the level of protected-worker employment shares. We also find that, at least for women, industries with the highest pre-CRA91 litigation rates cut back on their female employment shares after the Act. Finally, we examine patterns in employee turnover to study how this sorting took place. We find that, relative to unprotected workers, there was no change in protected-worker turnover following CRA91. Thus, it appears that the sorting effect operated by shifting employees into new industries largely when they would have been switching jobs anyway. On balance, our results suggest that the Civil Rights Act of 1991 may have had quite different effects from earlier employment discrimination legislation. Prior research shows modest but significant improvements in employment outcomes for protected workers subsequent to changes in the law (see Leonard (1990), Donohue and Heckman (1991), and Chay (1998)). If one goal of CRA91 was to integrate industries where minorities 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.”

2 The Civil Rights Act of 1991 – The Law and the Debate

A contentious Congressional debate regarding employment discrimination raged throughout most of 1990. Two bills were eventually passed: first the Americans with Disabilities Act and then a civil rights bill. After signing the ADA, President Bush vetoed the Civil Rights Act of 1990,

citing its encouragement of quota-based hiring. The administration and Congress then sought a compromise. Arguments over what would or would not lead to quotas raged, filling the editorial and news pages throughout much of 1991. In the end, after the Clarence Thomas confirmation hearings brought employment discrimination back into the headlines, Congress passed, and Bush signed, the Civil Rights Act of 1991. CRA91 contained most of the provisions of the bill Bush had vetoed (with one important exception, noted below). Bush's Deputy Assistant Attorney General later wrote that, during the 1991 negotiations over what became CRA91, the Bush administration was "enormously successful (and accurate) in characterizing the proposed legislation as 'a quota bill' and a 'lawyer's bonanza'" (Clegg (1994)). CRA91 amended the Civil Rights Acts 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 discriminatory intent on the part of an employer.³ A series of 1970's Supreme Court rulings (most notably *Griggs v. Duke Power*, 1971) 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*, 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.⁴ CRA91 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.⁵ Three provisions of the Act

³ See Robinson, Allen, Terpstra and Nasif (1992) for a more detailed discussion of these and other aspects of CRA91.

⁴ 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.

⁵ Many observers (see Clegg (1994)) 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 Civil Rights Act allowed plaintiffs to use statistical evidence to demonstrate disparate impact without identifying any specific employment practices.

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 compensatory and punitive 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 Civil Rights Act of 1866, which allows plaintiffs alleging *racial* discrimination to sue for unlimited punitive and compensatory damages, to cover both on-the-job activities and termination of employment. Earlier Supreme Court decisions (notably *Patterson v. McLean Credit Union*, 1989) had limited the applicability of the CRA of 1866 to the formation of employment relationships. Hence, CRA91 effectively removed all limits on damage awards in cases of unlawful racial discrimination in termination. Third, CRA91 gives plaintiffs who seek punitive damages the right to a jury trial. Since juries are perceived to favor claims of individuals over those of corporations, this change probably increased the expected size of damage awards to plaintiffs.⁶

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 (1994) point out that plaintiffs must show disparate treatment in order to earn punitive and compensatory damages, and conclude (based on their experience as attorneys) that it appears plaintiffs have been willing to take on the more stringent burden of proof associated with showing disparate treatment in order to earn the higher damage awards associated with these claims. Similarly, Donohue and Siegelman (1991) document a dramatic shift toward firing-based employment discrimination claims throughout the 1970’s and 1980’s. Our own examination of discrimination claims filed with the EEOC during the 1990’s reveals a continuation of this trend, with termination-based complaints outnumbering hiring-based complaints by approximately ten to one. This shift towards firing-based suits is particularly significant when viewed in light of a model developed by Ayres and Siegelman (1996). They consider a setting in which the variability of productivity is higher among black workers than white. If employers use probationary hiring to gain information about workers’ productivities, then we would

⁶ The only relevant empirical comparison of jury and judge verdicts of which we are aware is in Selmi (1998), who finds that juries are twice as likely as judges to find for the plaintiff in federal employment discrimination cases. This finding should, however, be interpreted cautiously since there are selection issues both with pre-trial settlement of cases and with the choice of a judge or a jury.

expect to observe a higher fraction of blacks fired than whites. However, if employers must fire in proportion to the racial makeup of their workforces, then employers respond by reducing 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."

3 A Model of Firing Costs and Hiring Behavior

The post-1991 shift toward firing-based, disparate treatment claims may seem to rule out the argument that CRA91 would force employers to increase employment of protected workers. In this section, we explore this assertion further by constructing a two-sector model of a labor market. Our purpose is to examine how increases in damage awards available to plaintiffs in wrongful termination litigation affect the employment of protected employees in each sector, and to use our findings to motivate our empirical analysis. The novel features of our model are that firms vary exogenously in their susceptibility to employment discrimination litigation, and that the employment of the marginal protected employee reduces the cost of employing inframarginal protected employees. We show that these two assumptions give rise to a *sorting effect* — whereby protected-worker employment rises in firms that are less susceptible to litigation — and a *quota effect* — where protected-worker employment rises in firms that are more susceptible to litigation and hence have lower pre-CRA91 protected-worker employment shares.

3.1 Workers and Firms

Workers live forever, are risk neutral, and discount the future at rate β . There are two types of firms: m firms that are *more susceptible* to employment discrimination litigation and n that are *less susceptible*.⁷ The firms are infinitely lived and discount the future at rate β . Firms convert labor into value according to the production function $F(L)$, where L is the number of workers employed by the firm. We assume F is twice differentiable, increasing and concave. If a firm employs a particular

⁷ Firms may differ in their potential exposure to litigation for many reasons. 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.

worker in period t , then with probability s that firm-worker pair receives an adverse match-specific shock at the end of period t . In the event of an adverse shock, the worker's productivity at that firm falls to zero in all subsequent periods. We assume that wages are sufficiently high that the firm chooses to fire any worker whose productivity falls to zero.⁸ A fired worker then chooses whether to sue the firm for employment discrimination. A fired worker draws his personal cost of filing a lawsuit, c , from a uniform distribution on $[c_0, c_1]$. These costs include any personal costs of preparing a suit as well as any fees paid for legal representation. The worker compares this cost to the expected benefit from filing a suit, which is simply the probability of winning a suit (denoted by p) times the expected damage award conditional on winning (denoted K). An employee therefore sues if $pK > c$, so the probability of a lawsuit being filed conditional on a worker being fired is given by $(pK - c_0)/(c_1 - c_0)$. A firm that is sued pays damages of expected amount pK and also pays J (which includes attorneys' fees and other costs of litigation) to defend against the suit. A firm's expected cost of terminating an employee is therefore given by $(pK + J)(pK - c_0)/(c_1 - c_0)$. Assuming a period t wage of w_t , we can write a firm's objective function as

$$\max_{\{L_t\}} \sum_{t=0}^{\infty} \beta^t \left(F(L_t) - w_t L_t - s L_{t-1} \frac{(pK + J)(pK - c_0)}{c_1 - c_0} \right).$$

By concavity, the firm's optimal choice of L_t is characterized by a first-order condition:

$$F'(L_t) = w_t + \beta s \frac{(pK + J)(pK - c_0)}{c_1 - c_0}. \quad (1)$$

We use the implicit function theorem to define the solution as $L(w_t, pK, J)$ and note that $L(\cdot, \cdot, \cdot)$ is decreasing in each of its arguments. Labor supply depends on both wages and the potential damage awards stemming from employment discrimination litigation. A worker who takes a job in period t earns wage w_t . If the worker is fired and files a suit, he earns expected damages pK and pays costs c . The worker's expected payoff from being employed in period t , which we denote as y_t , is therefore

$$\begin{aligned} y_t &= w_t + \beta s \int_{c_0}^{pK} \frac{(pK - c)}{(c_1 - c_0)} dc \\ &= w_t + z(pK). \end{aligned} \quad (2)$$

⁸ This assumption, which is identical to one made by Acemoglu and Angrist (1998), is used as a simple way of generating severance of employment relationships in equilibrium. By assuming shocks are match specific, we are able to abstract from issues relating to future employers' inferences regarding the worker characteristics.

Denote by $\eta(y_t)$ the quantity of labor supplied as a function of the benefits to workers from employment. We assume $\eta(\cdot)$ is increasing and continuous. To capture differences in firms' susceptibility to employment discrimination litigation, we assume that firms differ in their propensity to lose a lawsuit. For the m firms that are more susceptible to suits, we assume the probability of losing is p_h , while firms that are less susceptible lose with probability $p_l < p_h$.

3.2 Equilibrium

An equilibrium level of job-related benefits y_t is a fixed point of the mapping

$$T(y_t) = y_t + mL(y_t - z(p_h K), p_h K, J) + nL(y_t - z(p_l K), p_l K, J) - \eta(y_t).$$

This mapping is the sum of job-related benefits y_t and the excess demand for workers at this level of benefits. At the equilibrium, excess demand is zero, so $T(y_t) = y_t$. It is straightforward to show that the equilibrium level of job-related benefits y_t is decreasing in p_h, p_l and K . To do this, it is sufficient to show that the mapping T is continuous and decreasing in each of these parameters. Note that since η and L are continuous (by assumption and by the theorem of the maximum, respectively), we have that the mapping T is continuous. To show that T is decreasing in p_h, p_l and K , we need to show that L is decreasing in these parameters. From Equation (1), we have the firm's first-order condition:

$$F'(L_t) = w_t + \beta s \frac{(pK + J)(pK - c_0)}{c_1 - c_0}.$$

Rearranging Equation (2) generates an expression for w_t :

$$w_t = y_t - \beta s \frac{(pK - c_0)}{(c_1 - c_0)} (pK - E[c | c < pK]). \quad (3)$$

Combining Equations (1) and (3) yields

$$F'(L_t) = y_t + \beta s \frac{(pK - c_0)}{(c_1 - c_0)} (E[c | c < pK] + J).$$

Note that the expected transfer, pK , affects the equilibrium only by affecting the probability that the worker sues. Since damages are pure transfer, only the costs of filing (c) and defending against (J) employment discrimination lawsuits affect employment. Since the marginal cost of employing a worker (the right-hand side of this equation) is increasing in pK , labor demanded by a firm is decreasing in pK . This implies that T is decreasing, and hence that employment levels and equilibrium benefits to workers are decreasing in p_h, p_l and K .

3.3 The Sorting Effect

We next enrich this model by explicitly considering the potential effects of the Civil Rights Act of 1991. As discussed above, the law potentially affected damage awards in three ways: by allowing Title VII plaintiffs to sue for limited punitive and compensatory damages, by allowing plaintiffs who allege unlawful termination on the basis of racial discrimination to sue for unlimited damages, and by allowing either side in a dispute to request a jury trial. We model the first and second of these points as increases in potential damage awards K and model the third as an increase in p . Our objective is to assess how these changes affect the allocation of labor among firms that are more and less susceptible to litigation. Our primary result is to offer conditions under which increases in damage awards (K) and likelihood of lawsuit success (p_l and p_h) raise the marginal cost of employing protected workers by more at firms that are more susceptible to employment discrimination litigation. Assuming the elasticity of labor demand is identical across the two sectors, we should therefore observe protected workers being sorted into the industries where they are least costly to employ.⁹ To derive this *sorting effect*, we examine the marginal cost of employing a protected worker. If the likelihood of losing a suit is given by p_i (where $i \in \{h, l\}$), then the marginal cost of employing a protected worker is given by

$$y_t + \beta s \frac{(p_i K - c_0)}{(c_1 - c_0)} (E[c | c < p_i K] + J).$$

Differentiating both with respect to K yields the rate at which the marginal cost of employing a protected worker increases as damage awards increase:

$$p_i \beta s \frac{(p_i K + J)}{(c_1 - c_0)}.$$

There are two ways in which K affects the marginal cost of employing a protected worker. Increases in K increase the expected damage award conditional on losing a suit. In addition, the prospect of higher damages induces employees to sue more frequently, since the likelihood that $pK > c$ is increased. Note that since $p_l < p_h$, we have that the marginal cost of employing protected workers increases by more at the firms that are more susceptible to litigation. Since the overall job-related benefits (y_t) to workers necessarily goes down in response to increases in K , the aggregate

⁹ The idea that minority workers will be sorted into industries where the mutual gains from employment are highest is, of course, not new here. See, for example, Epstein (1992), Chapter 2. Our innovation is to emphasize the role of susceptibility to litigation in inducing differential costs of employment of protected workers across firms, and modeling how the increases in damage awards associated with CRA91 affect equilibrium in the labor market.

employment level must fall. If the elasticity of labor demand is comparable across sectors, then 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 law’s passage.¹⁰ 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.¹¹

3.4 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 assumption by briefly outlining the judicial tests applied to disparate treatment cases, initially set out by the Supreme Court in *McDonnell Douglas Corp. v. Green*, 1973.¹² As described in Epstein (1992), *McDonnell Douglas* sets out a “tripartite standard of prima facie case, justification, and pretext.” 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.”¹³ Next, the burden shifts to the defendant, who must “articulate some legitimate, nondiscriminatory reason” for the employment decision.¹⁴ Finally, the plaintiff is “afforded a fair opportunity to show that the

¹⁰ A sufficient condition on F for this result to hold is $F''' \leq 0$, which states that the marginal product of labor (F') is a concave function of the number of workers a firm employs. This condition implies that the reduction in L corresponding to a fixed increase in the marginal cost of employing a worker is larger when L is smaller.

¹¹ We can obtain similar results when considering the effect of jury trials on the probability of plaintiff success. As long as the possibility of a jury trial does not increase p_l by more than it increases p_h , employment of protected workers will still be shifted toward firms that are less susceptible to litigation.

¹² The standards set out in *McDonnell Douglas* have been clarified, but not significantly altered, in subsequent rulings. See *Furnco Construction Corp. v. Waters et al.*, 1978 and *Texas Department of Community Affairs v. Burdine*, 1981.

¹³ See *Texas Department of Community Affairs v. Burdine*, 1981.

¹⁴ See *McDonnell Douglas Corp. v. Green*, 1973.

(defendant’s) stated reason for the (plaintiff’s) rejection was in fact pretext.” 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.” This pretext standard thus gives plaintiffs broad scope to examine an employer’s general minority employment practices. We therefore argue that by employing more protected workers, an employer can make it more difficult for displaced protected workers to prove unlawful discrimination. To model this, we assume the probability a worker wins an employment discrimination lawsuit is a decreasing function of the number of protected workers employed by the firm. To formalize this, we assume p_h and p_l are decreasing and convex functions of L with the property that

$$p_h(L) - p_l(L) = C > 0,$$

for all L . This means p_h and p_l have identical shapes, but different levels. One immediate consequence of these assumptions regarding the p functions is to render the firms’ objective functions non-concave. This limits our ability to use the implicit-function theorem to characterize firms’ labor demand, as we did in the previous section. We focus instead on the question of how firms’ marginal costs of employing protected workers change when K increases. To begin our analysis, we note that the cost to a firm of employing L_t workers in period t is given by

$$w_t L_t + \beta s L_t \frac{p_i(L_t)K - c_0}{c_1 - c_0} (p_i(L_t)K + J)$$

where $i \in \{h, l\}$. Taking the derivative with respect to L_t , we have

$$w_t + \beta s \left\{ \frac{p_i(L_t)K - c_0}{c_1 - c_0} (p_i(L_t)K + J) + L_t p'_i(L_t) \left(\frac{1}{c_1 - c_0} (p_i(L_t)K + J) + \frac{p_i(L_t)K - c_0}{c_1 - c_0} K \right) \right\}. \quad (4)$$

We discuss each of the four terms of this expression in turn. If a firm hires an additional worker, it must pay the worker’s wage in period t . This is the first term in the expression. The second term reflects the costs associated with the filing of a lawsuit by the marginal worker. A firm fires the marginal worker with probability s and incurs expected costs of litigation $p_i(L_t)K + J$ if that worker files a suit, which happens with probability $(p_i(L_t)K - c_0)/(c_1 - c_0)$. The third and fourth terms represent the effect of the marginal worker on the costs of litigation imposed by *inframarginal* workers. When the firm hires an additional protected worker, this reduces the likelihood that inframarginal workers sue (third term) *and* reduces the likelihood that such a suit is

successful if it is brought (fourth term). Since $p'_i(L_i) < 0$, both terms are non-positive meaning that these inframarginal effects reduce the marginal cost of hiring an additional protected employee.

Since firms' hiring decisions are made by comparing these marginal costs to the employee's marginal productivity, we consider how the marginal cost of employment varies as K increases. Differentiating the expression in (4) with respect to K , we obtain

$$p_i(L) \frac{2Kp_i(L) + J - c_0}{c_1 - c_0} + Lp'_i(L) \frac{4Kp_i(L) + J - c_0}{c_1 - c_0}. \quad (5)$$

The first term in (5) represents how the direct effect of hiring an additional protected worker changes as K increases. Since there is some chance that the marginal worker will sue, increases in K increase the direct costs. Since, in equilibrium, L is larger for the firms that are less susceptible to litigation, we have that $p_l < p_h$. The first term in (5) is therefore larger for more susceptible firms, which gives rise to the sorting effect discussed above. The second term represents how the effect of the marginal protected worker on inframarginal protected workers varies as K increases. Since $p'_i < 0$, we have that an increase in K increases the rate at which hiring an additional protected worker reduces the costs of employing inframarginal protected workers. Since firms that are more susceptible to employment discrimination litigation employ fewer protected workers in equilibrium, we have that $p'_l < p'_h$ (by convexity of p). The magnitude of this effect is therefore larger for firms that are more susceptible to litigation, which means that the reduction in the marginal cost of employing protected workers may be larger in the more susceptible sector. This *quota effect* therefore works in the opposite direction from the sorting effect. While the sorting effect may induce firms with lower pre-CRA91 protected-worker employment shares to cut back further on protected-worker employment, we expect the quota effect to have the opposite effect. If the quota effect dominates, then we expect firms with low pre-CRA91 protected-worker employment shares to increase their employment of protected workers subsequent to the law. Note also that the quota effect has very different implications for wages and employment of protected workers. While the sorting effect increases the marginal cost of employing protected workers and leads to reductions in wages and employment, the quota effect reduces the marginal cost of employing protected workers and leads to wage and employment gains.¹⁵ Our model offers no prediction as to which effect is stronger, so we next attempt to assess this question empirically.

¹⁵ The evidence in Oyer and Schaefer (1999b) provides some initial support for the sorting model. That paper shows that, to the extent CRA91 affected protected worker wages or employment it all, the effect was negative.

4 Empirical Analysis

4.1 Data and Methodology

The main objective of our empirical analysis is to examine labor market trends in the pre-CRA91 and post-CRA91 periods for evidence consistent with the sorting or quota effects. We seek to determine whether firms with low pre-CRA91 protected-worker employment shares increased or decreased their employment of protected workers subsequent to the law.¹⁶

Since firm-level data on protected-worker employment shares is unavailable, we proxy for firm-level employee demographics with data at the industry level. We compute our primary dependent variable, industry-level change in protected-worker employment share, from the 1988-1996 Annual Demographic Files of the Current Population Survey (CPS). The CPS is a monthly survey that asks respondents 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 offers information such as weeks and hours of work, rates of pay, and number of jobs. To analyze changes in both the stock and flow of employment shares, we examine both jobs the employee has held for a long period of time and new jobs. 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 a firm/industry as an indicator of how the firm/industry is adjusting to a post-CRA91 equilibrium. However, because many new jobs are short-lived (see Farber (1994)), the change in an industry's stock of workers may not be accurately reflected by the flow into the industry. We define a CPS respondent as holding a new job if the person reports having worked for two or more employers during the previous calendar year.

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

¹⁶ As we noted above (see footnote 2), our model 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 from 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.

— and female employment share as measures of the composition of workers in a given industry. While CRA91 did affect the rights of older workers and non-black 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 difficult to separate the effect of CRA91 from other factors affecting employment of older workers. Non-black 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 the composition of this group. We further restrict attention to industries for which the CPS surveyed at least 100 workers in both the pre-CRA91 and post-CRA91 periods. While there are 243 industries represented in our data, only 200 meet this selection criterion. When looking at new jobs only, we limit the analysis to the 131 industries with at least 40 pre-CRA91 and 40 post-CRA91 observations. To measure pre-CRA91 protected-worker employment shares, we use the 5% sample of the 1990 Census. This second source of employment share data insures that any measurement error in our explanatory variables is uncorrelated with the error in our dependent variable (see below). Our final data source is a record of employment discrimination complaints filed with the EEOC. We use this data source as a way of measuring the susceptibility of a particular industry to employment discrimination litigation. All litigants who charge employment discrimination under Title VII must start their pursuit of redress with a complaint to the EEOC.¹⁷ The data include the industry (2-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 drop the complaints that are focused on hiring only (less than 6% of the total cases).¹⁸ We analyze gender-based cases brought by women and race-based cases brought by blacks. This deletes the relatively small number of reverse discrimination cases.

Tables 1 and 2 summarize the CPS and Census data. The first column of Table 1 provides summary statistics for all respondents to the CPS Annual Demographic File from 1988-1996 who were employed in one of the 200 industries in our sample. The second column lists the same

¹⁷ Claims under the Civil Rights Act of 1866 do not go through the EEOC. However, we analyze EEOC data from before CRA91 that involve non-hiring complaints. In the years before CRA91 (and especially after *Patterson*), only hiring cases could be filed under CRA of 1866.

¹⁸ While we explicitly modeled only termination costs, the model applies equally well to any on-the-job (that is, post-hiring) complaint.

Table 1: Summary Statistics from the 1988-1996 March CPS

	Entire Sample	“New” Jobs
Total Observations	595,575	89,188
Black	8.7%	7.5%
Minority	23.7%	20.9%
Female	47.0%	45.7%
40 or older	38.1%	23.2%
Mean Hourly Earnings	\$11.12 (82.22)	\$9.28 (14.19)
Mean Age	37.4 (10.4)	33.0 (9.8)
Mean Weekly Hours	35.9 (17.8)	34.6 (18.7)
Years Education	13.1 (3.5)	13.2 (3.3)
State Unemployment	5.97% (1.6%)	5.87% (1.6%)

NOTES: Data, which include employed people between the ages of 20 and 59, are from the 1988-1996 Annual Demographic File of the Census of Population Survey. The survey is administered in March of each year. Earnings are the average for the previous calendar year. Hours worked refers to all jobs in the week before the survey. People were classified as holding “new jobs” if they reported working at more than one job in the previous calendar year. Standard deviations in parentheses.

information for those respondents who took a new job in one of the 131 industries in our new job sample. The job switchers are younger, less well paid, and somewhat more likely to be white men than the sample as a whole. The groups are comparable in terms of education and hours worked. Also, there is not a noteworthy difference between state unemployment rates for the two groups, indicating that local economic conditions are not the primary determinant of turnover.

4.2 Changes in Industry Employment Shares Before and After CRA91

We begin our analysis by examining the pre-CRA91 to post-CRA91 change in protected-worker employment shares by industry. Our primary objective is to assess the relative strengths of the sorting and quota effects. The sorting effect suggests that increases in the costs of displacing employees would lead to greater concentration of protected workers in the industries where they

Table 2: Summary Statistics from Industries

	CPS Pre-CRA91	CPS Post-CRA91	1990 Census
Total Observations	200	200	200
Black Share	8.2%	8.1%	7.8%
Female Share	41.9%	40.8%	37.4%
Mean Age			39.2 (2.0)
Mean Annual Earnings			\$24,588 (7,034)
Number of Observations in Industry	1,165 ^a (2,057)	1,165 ^a (2,057)	20,502 (34,629)

NOTES: Data are averages for the 200 Census Bureau Industry classifications for which there are at least 100 observations for both the 1988-1991 CPSs and the 1993-1996 CPSs. CPS data averages across all employed people 20 to 59. Census data averages across all employees who worked more than 25 weeks in 1989 and at least 30 hours during the week prior to the survey. Figures superscripted ^a are the minimum of the sum of the observations in the 1988-1991 CPSs and the sum of the observations in the 1993-1996 CPSs. Standard deviations in parentheses.

are least costly to employ, while the quota effect would predict that industries with low pre-CRA91 protected-worker concentrations would try to reduce exposure to lawsuits by increasing their employment of these workers. To see if either pattern emerges from the data, we relate the change in protected-worker employment shares to the level of protected-worker employment shares prior to the law. For a protected class k and an industry i , we define the pre-CRA91 to post-CRA91 CPS change in the protected-worker employment share as

$$\text{CPS Share Change}_{ik} = \frac{p_{ik,93-96}}{n_{i,93-96}} - \frac{p_{ik,88-91}}{n_{i,88-91}} \quad (6)$$

where p_{ikt} 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 .¹⁹ It would be a straightforward calculation to measure the relationship

¹⁹ We weight our regressions by the number of employees in the industry (n) 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.

between (6) and pre-CRA91 employment share. However, since pre-CRA91 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 towards finding evidence of the quota effect. That is, since the post-CRA91 measure does not have the same sampling error as the pre-CRA91 estimate, there would appear to 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.²⁰ For protected-worker class k and industry i , we define the pre-CRA91 Census level of protected-worker employment share as

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

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

$$\text{CPS Share Change}_{ik} = \alpha_k + \beta_k \text{Census Share Level}_{ik} + \epsilon_{ik}. \quad (7)$$

We run each regression twice: first for all workers and then using only new jobs in the CPS sample. Panel A of Table 3 displays results for the regressions using all workers, while Figure 1 shows the relationships graphically. Panel B of Table 3 shows the results when we limit the sample to those CPS respondents whose job was new at the time of the sample. None of the coefficients are statistically significant, though the estimates are reasonably precise. This would seem to imply that CRA91 had neither a strong sorting nor a strong quota effect on employment of blacks or women.

However, any interpretation of Table 3 should put employment shares of blacks and women in historical context. If CRA91 changed the trend in sorting or quotas, then the innocuous effects suggested by Table 3 could be understated. To address this possibility, we ran regressions using the specification in Equation (7), but looking at a period before CRA91. We now look at the change in employment shares from 1983-1986 to 1988-1991 and use the 1980 Census as the measure of initial employment share.²¹ The results are displayed in Table 4. For both blacks and women, there was

²⁰ In fact, since the sample size of the Census is huge, sampling error should be almost non-existent.

²¹ March CPS industry classifications changed in 1983, so we cannot use pre-1983 CPS.

Table 3: Change in Protected-Worker Employment Shares from the 1988-91 to 1993-96 Periods

	Black	Female
<i>Panel A: All Employees</i>		
1990 Census Share Level	-0.0015 (0.0331)	0.0075 (0.0154)
R^2	0.001	0.001
<i>Panel B: CPS New Hires Only</i>		
1990 Census Share Level	0.0103 (0.0557)	0.0139 (0.0249)
R^2	0.001	0.002

NOTES: Dependent variable is CPS Share Change from the 1988-91 period to the 1993-96 period. Each panel A regression has 200 observations and each panel B regression has 131 observations, where an observation is a Census Bureau industry code. Industries were only included in the all employees (new hires) regressions if there were at least 100 (40) people who reported working in that industry in both the pre- and post-CRA91 CPS samples. New hires are workers who reported more than one employer for the previous calendar year. Standard errors in parentheses. Regressions are weighted by the minimum number of employees used to generate the two CPS shares.

a clear trend during the 1980's for those industries with fewer of each of these types of workers to hire more. That is, during the 1980's, the industries that had been the most segregated along racial and gender lines were becoming less so. Given the results in Table 3, however, it is clear that this trend did not continue after CRA91. This change in trend after CRA91 provides evidence that is consistent with CRA91 leading to a sorting effect. At the very least, Tables 3 and 4 and Figure 1 allow us to conclude that CRA91 did not lead to widespread hiring by quota. As a robustness check, we relax the assumption implied by our Equation (7) 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 non-monotonic relationship may provide evidence in favor of the quota effect — evidence that would be missed by the monotonic specification. Under this hypothesis, we might expect to see increases in protected-worker employment shares at both the left and right extremes of Figure 1. This, however, is not supported by the graphs, nor was it

Table 4: Change in Protected-Worker Employment Shares from the 1983-86 to 1988-91 Periods

	Black	Female
1980 Census Share Level	-0.1123 (0.0195)	-0.0147 (0.0075)
R^2	0.142	0.019

NOTES: Dependent variable is CPS Share Change from the 1983-86 period to the 1988-1991 period. Each regression has 203 observations, where an observation is a Census Bureau industry code. Included industries had at least 100 people who reported working in that industry in both the 1983-1986 and 1988-1991 CPS samples. Standard errors in parentheses. Regressions are weighted by the minimum number of employees used to generate the two CPS shares.

supported by less restrictive regressions we ran. Overall, we find no evidence that the firms and industries with low protected-worker employment shares felt any pressure to increase their shares as a result of CRA91. To further probe the evidence of sorting and the trends in employment shares, we turn from mean protected-worker employment shares to the variance in employment shares. If CRA91 led protected workers to become more concentrated in certain industries, then there should have been a trend toward greater inter-industry variance in employment shares beginning in about 1992. We calculated the standard deviation of each protected-worker classification in each year using the 170 industries for which there are at least 50 observations in every CPS from 1983 to 1996. As shown in Figure 2(a), inter-industry variation in black employment shares trended down steadily from 1983 to the time of CRA91. However, shortly after the Act, the trend began to reverse sharply. Figure 2(b) shows a similar, if less sharp, trend for female employment-share variation. These figures provide further evidence consistent with CRA91 leading to employees concentrating at firms that are least likely to be affected by the increased litigation costs associated with the Act.

4.3 Employment Shares and Discrimination Complaints Rates

We further explore employers' responses to CRA91 by examining the relationship between protected-worker employment shares and the propensity of workers to file discrimination complaints with the EEOC. While in the previous subsection we were able to conduct our analysis at the 3-digit industry level, the EEOC classifies the industry of a plaintiff's employer at the 2-digit level. Hence, our analysis here is limited to the coarser industry definitions. To measure the propensity of workers to file complaints with the EEOC, we compute the ratio of complaints filed in 1990 to workers employed

in 1990. That is, we start with the number of non-hiring based complaints filed in 1990 by members of protected group k employed in industry i and then divide by the number of number of workers in protected group k employed in industry i surveyed by the 1990 Census.²² Table 5 presents a summary of the industry-level EEOC complaint rates and their relationship with protected-worker employment shares. Complaint rates vary dramatically by protected group and industry. Black complaint rates are noticeably higher than those for women, with over 0.5% of employees filing an EEOC complaint in 1990.²³ Some industries have complaint rates less than 0.1% (for example, agriculture and engineering), while others have rates over 2% (mining). Complaint rates are almost ten times lower for women. Again, there is substantial variation in womens' complaints across industries, from almost no complaints in textiles and law, to very high rates (3 per 1,000) in metals.

The last row of Table 5 displays the correlation between the industry complaint rate in 1990 period and the industry protected-worker employment share in 1990. 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 large number of complaints.²⁴ We also note that the complaint rates for a given industry are highly correlated across the two protected groups (correlation coefficient of 0.74). 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 in our model. To see this, note that if industries differ in their susceptibility to discrimination suits, then we would expect industries with higher susceptibility (and perhaps a higher rate of complaint) to employ fewer protected workers.

²² We also measured the propensity to file complaints by computing the ratio of complaints filed between 1987 and 1991 to workers employed in 1990. This longer window of EEOC complaints reduces the randomness in the arrival rate of complaints, but introduces measurement problems if employment growth rates and protected-worker employment shares changed differentially among industries. Our results are not sensitive to this measurement choice.

²³ To get the mean complaint rate or employment share for the whole sample requires weighting by the number of workers in the industry. Weighting has very little effect on the summary statistics, so the figures displayed in Table 5 are unweighted.

²⁴ If there is considerable error in the EEOC's classification of industry codes, we would expect to find negative correlation because the mis-classified 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 5: EEOC Complaints Summary Statistics

	Black	Female
Mean 1990 Census Share Level	8.40%	37.18%
Minimum	1.38%	5.86%
Maximum	24.1%	90.8%
1990 EEOC Complaints per 1000 Workers	5.69	0.71
Minimum	0.63	0.03
Maximum	23.53	3.16
Correlation between 1990 Complaint Rate and 1990 Census Share Level	-0.2282*	-0.3818***

NOTES: Data are averages for the 72 2-digit SIC codes for which there are 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 5% sample of the 1990 Census. Complaint rates are number of non-hiring 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. * and *** indicate significance at the 10% and 1% levels, respectively.

On the other hand, if the higher levels of protected-worker employment within an industry reduce a litigant's likelihood of prevailing, then higher employment shares would deter workers from filing complaints. While these two effects cannot be distinguished when looking only at pre-CRA91 data, we can try to disentangle them by relating the pre-CRA91 complaint rate to post-CRA91 changes in employment shares. If CRA91 led to additional sorting, then we expect industries with high pre-CRA91 complaint rates to further reduce protected-worker employment shares. If quota pressures dominate, then we expect industries with high complaint rates to increase protected-worker employment shares. Table 6 repeats the regressions in Table 3, except that we include our two measures of complaint rates as explanatory variables and, due to the nature of the EEOC data, we run the regressions at the 2-digit SIC level. As in Table 3, the coefficients on 1990 employment share are small and insignificant, and, therefore, unaffected by the sample change. The coefficients on the 1990 EEOC complaint rate are inconsistent across the two groups of protected worker. The regression for women leads to a very high point estimate, suggesting that an increase of 1 complaint per 1,000 workers corresponds to a 2.16% decrease in the fraction of females working in a given industry. The coefficient is statistically different from zero. This suggests that industries with the highest rates of EEOC complaints cut back on their female employment after CRA91 and provides

Table 6: Protected-Worker Employment Shares and EEOC Complaint Rates

	Black	Female
1990 EEOC Complaint Rate	0.5592 (0.3535)	-21.64 (8.828)
Share from 1990 Census	-0.0388 (0.0310)	-0.0018 (0.0185)
R^2	0.061	0.091

NOTES: Dependent variable is CPS Share Change. Each regression has 72 observations where an observation is a 2-digit SIC code. Standard errors in parentheses. Regressions are weighted by the number of employees used to generate the CPS share.

further evidence consistent with the sorting effect. The positive (though small and insignificant) coefficient in the black regression does not support this conclusion.

4.4 Reshuffling Employees after CRA91

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.²⁵ Another concern raised by our results above is that such laws lead workers to move towards firms where, due 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 anti-discrimination rules would presumably not lead employers to terminate protected workers.²⁶ 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 1988-1996 March CPS and look for a trend in protected-

²⁵ For empirical evidence on how recent discrimination legislation has affected protected-worker employment, see DeLeire's (1997) and Acemoglu and Angrist's (1998) studies of the American with Disabilities Act and Oyer and Schaefer's (1999b) analysis of CRA91.

²⁶ See, for example, Oyer and Schaefer (1999a). Using the Survey of Income and Program Participation, 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 their results with a different dataset and broadens the analysis to include women.

Table 7: Change in Protected-Worker Turnover

	Black	Female
Protected Group	-0.1505 (0.0353)	-0.1115 (0.0152)
Protected Group by Year	See Figure 3	See Figure 3
Log Likelihood	-78,899	-138,163
Observations	166,720	304,892

NOTES: Dependent variable is 1 if the person reports having held more than one job during the calendar year before the March CPS survey. The comparison group in each regression is non-Hispanic white men under 40. The coefficients refer to black men under 40 (column 1) and white women under 40 (column 2). Each probit includes indicator variables for high school degree, college degree, five year age categories, half-percent categories of state unemployment rate, and year. “Protected group” coefficient corresponds to an indicator variable that equals 1 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 3. Standard errors in parentheses.

worker turnover. More specifically, we investigate whether there was a temporary spike upwards in protected-worker turnover following the passage of CRA91 as workers sorted to the best matches. We estimated the year-to-year relative turnover rates of protected workers using probits where the dependent variable is 1 if the person held more than one job during the calendar year. The explanatory variables of interest are individual year effects, the protected-worker effect, and the interaction of the year effects and protected status. The sample for each probit is limited to people who are in one protected group only (that is, either black men or white women) and a comparison group of non-Hispanic white men under 40, because these workers are unambiguously not covered by CRA91. Column 1 (2) of Table 7 includes black men (white women) under 40 and non-Hispanic white men under 40, but no Hispanics or women (blacks).²⁷ 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 Figure 3 and Table 7, however, there is no evidence that CRA91 had any effect on the relative propensity of protected workers to change jobs. Figure 3 shows the probit coefficients for the year-protected group interaction terms in each

²⁷ Our probits also include controls for a number of relevant factors, such as age, state unemployment rate, education. We use a series of dummy variables for each of these effects, so as to impose fewer restrictions. See notes to Table 7 for details.

probit, 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. Table 7 shows that the turnover rates of young white men differ from those of blacks and women, but Figure 3 indicates that these differences did not change in any systematic way as a result of CRA91. This suggests that, to the extent that CRA91 and its corresponding increase in potential litigation costs 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.

5 Conclusion

While earlier antidiscrimination legislation appears to have had a modestly positive impact on employment outcomes for protected workers, it is not clear that further extensions of such laws necessarily lead to further improvements in employment outcomes. In this paper, we have examined the effects of the Civil Rights Act of 1991, which, unlike much prior legislation, focuses on preventing discrimination in *termination* of employment relationships rather than hiring. We developed a model of labor market effects of changes in costs of displacing employees. Our model has two distinguishing features: First, we assumed that firms exogenously vary in their susceptibility to employment discrimination legislation. Second, we allowed the hiring of the marginal protected employee to affect the firm's cost of employing *inframarginal* protected employees. Applying our model to study CRA91, we derived 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. Our model suggested that both effects could arise from the law and offered no prediction as to which will dominate. Using data from the Census, Current Population Surveys, and EEOC filings, we then tried to assess the relative strengths of the sorting and quota effects. Our primary finding was that there was a distinct change in the employment shares of blacks and women after CRA91 and that this change was consistent with the sorting effect dominating the quota effect. While in the years leading up to CRA91, industries with relatively few women and blacks had been increasing their share of these workers, this trend stopped and began to reverse shortly after CRA91. Our EEOC complaint results are

also somewhat consistent with the sorting effect, as industries with the highest complaint rates lowered their concentration of women shortly after CRA91. Overall, our results suggest that the widely discussed fear that CRA91 would lead firms to hire by quota were not justified. However, to the extent that CRA91 was intended to open new opportunities to the groups it protects, we found no evidence that it succeeded.

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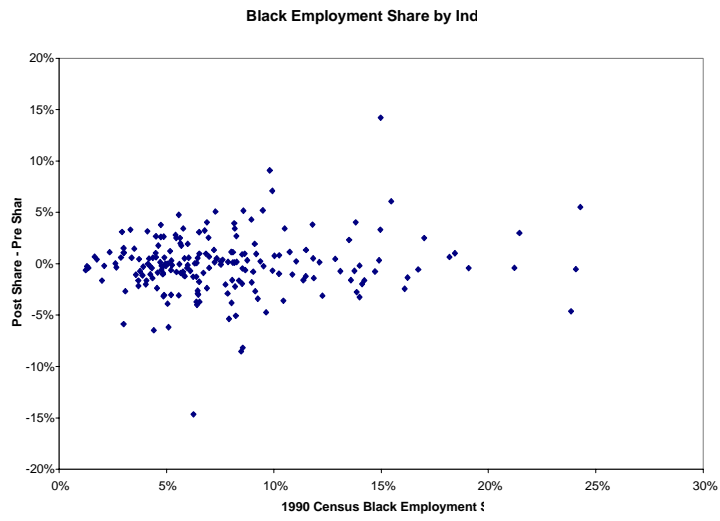


Figure 1a

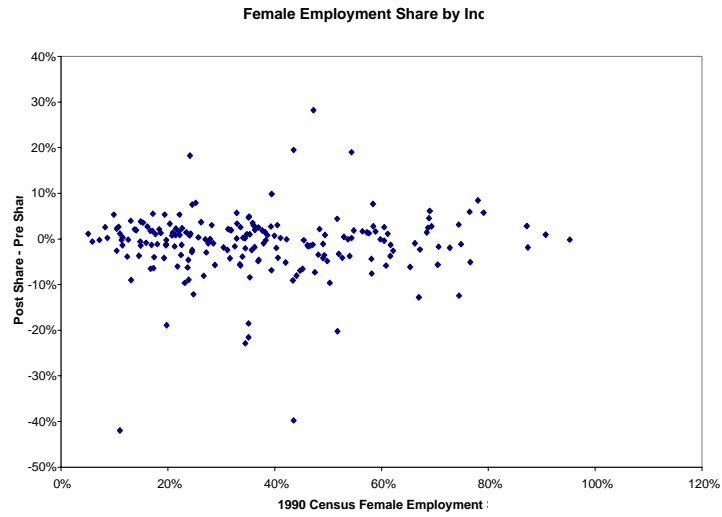


Figure 1b

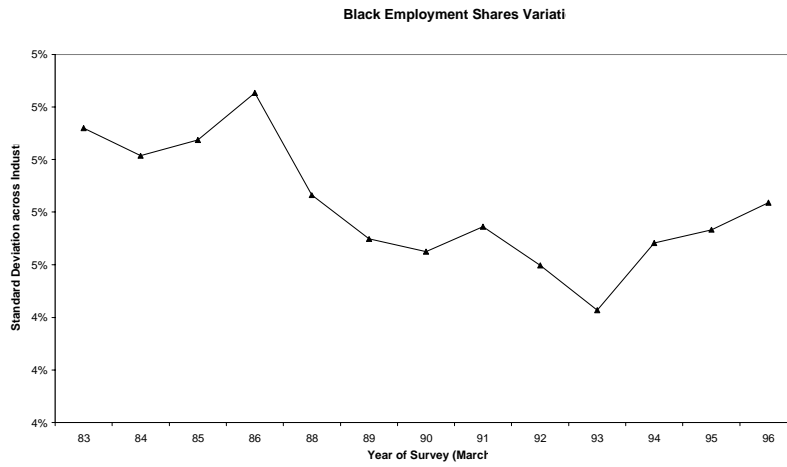


Figure 2a

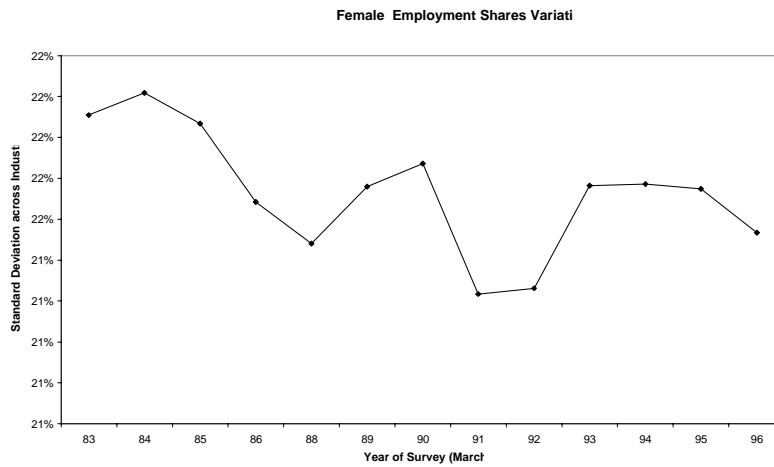


Figure 2b

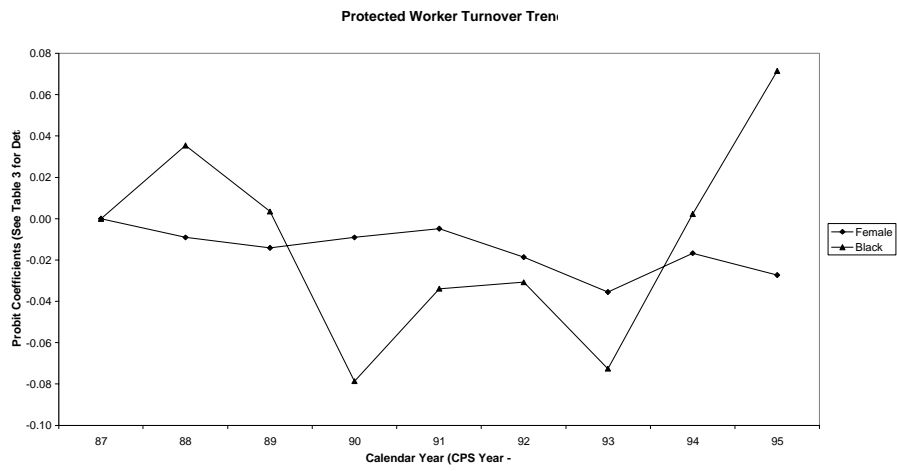


Figure 3