Litigation Costs and Returns to Experience

By Paul Oyer and Scott Schaefer*

We develop a model linking maximum damage awards available to plaintiffs in wrongful termination lawsuits, workers' propensity to sue as a function of experience, and returns to experience. Using Equal Employment Opportunity Commission data on protected-worker discrimination complaints and labor-market data from the Current Population Survey, we examine how returns to experience among protected workers changed around the passage of the Civil Rights Act of 1991. We show that employers' reactions to employment protections may induce redistributive effects. Furthermore, these effects operate not merely across groups of differing protected status, but also within groups of identical protected status. (*JEL* D21, J31, J71, K31)

Recent work studying the effects of antidiscrimination protections has focused on the possibility that laws aimed at protecting jobs and preventing employment discrimination may actually work against protected workers by raising the expected costs of employing these workers. Theoretical and empirical analyses of such legislation usually treat members of protected groups as homogeneous and examine whether, and if so how, the protections redistribute employment outcomes across groups of workers (see e.g., Thomas DeLeire, 2000; Daron Acemoglu and Joshua D. Angrist, 2001). Ignored in this literature, however, is the possibility that antidiscrimination protections may redistribute employment outcomes among members of protected groups, making some protected workers better off and others worse off. In this paper, we examine this question by studying the relationship between litigation costs and returns to experience for members of protected groups.

We begin by developing a stylized model of the labor-market effects of employment discrimination legislation. In our model, firms and workers are initially symmetrically uninformed regarding workers' abilities, and public signals regarding ability are revealed over time. Such learning gives rise to for-cause firings, in which workers whose abilities are revealed to be low are terminated. However, some managers discriminate against certain workers and fire them even if ability is not revealed to be low. As the legal system can imperfectly distinguish between for-cause and discriminatory firings, both sets of workers face the option of filing an unlawful termination lawsuit. Increases in maximum damage awards associated with employment discrimination lawsuits affect wages by making workers who are likely to sue more costly to employ.

The key issue in linking increases in litigation costs to changes in returns to experience is therefore whether the increase in litigation costs is greater for experienced or inexperienced workers. If, for example, inexperienced workers are more likely to be fired for cause (as is the case in our learning model), then these workers may be more likely to sue conditional on being employed. Increases in litigation costs may therefore make inexperienced workers relatively more costly to employ, and employers will discount wage offers accordingly. Potentially offsetting this effect, however, is the fact that back (and some future) wages often comprise one part of damage awards. The prospect

* Oyer: Graduate School of Business, Stanford University, 518 Memorial Way, Stanford CA 94305; Schaefer: Kellogg School of Management, Northwestern University, 2001 Sheridan Road, Evanston, IL 60208. We thank Joshua Angrist, David Besanko, David Butz, William Carrington, Jim Dana, Kathleen Engel, Jane Goodman-Delahunty, Rachel Hayes, Thomas Klein, Canice Prendergast, Chris Taber, two referees, and participants in seminars at the University of Chicago, Princeton University, Northwestern University, Stanford University, the 1999 AEA Meetings, the 1999 NBER Summer Institute, and the 2000 SOLE/EALE Meetings for comments. We thank Daron Acemoglu, Josh Angrist, and Chris Mazingo for providing a research extract of EEOC complaints data. We received generous financial support from the Institute for Policy Research at Northwestern University (Oyer) and the Searle Fund (Schaefer).

of higher damages may make experienced workers more likely to sue conditional on being fired, which may imply that increases in litigation costs will reduce firms' demand for these workers. Developing these effects further, we demonstrate that the effect of increases in litigation costs on returns to experience depends crucially on (i) how employees' propensity to file employment-discrimination litigation conditional on employment varies with experience, and (ii) how the *increase* in propensity to sue (stemming from the increase in litigation costs) conditional on employment varies with experience.

The passage of the Civil Rights Act of 1991 (CRA91) provides an opportunity to study this relationship empirically. This Act contains a number of provisions that increased the expected costs to firms of displacing protected employees. The group protected by CRA91 is broad, and includes racial minorities, females, and those with disabilities. While previous federal employment discrimination legislation typically limited plaintiff recovery to lost wages, CRA91 allows employees to sue for up to $300,000 in punitive damages. By extending the Civil Rights Act of 1866, CRA91 allows employees claiming unlawful termination on the basis of *race* to sue for unlimited punitive damages. CRA91 also gives either side in a suit the right to a jury trial; this presumably favors plaintiffs as juries are thought to be partial to claims of individuals over those of firms.

We proceed by identifying relationships between the propensity to sue as a function of age and changes in the returns to experience among protected workers around the time of the passage of CRA91. Using data on complaints filed with the Equal Employment Opportunity Commission (EEOC), we find that wrongful termination complaints drop sharply with age for women, but rise steadily with age for blacks. We tie this finding into an analysis of the returns to experience for these protected groups. Using data from the 1988–1996 annual demographics file of the Current Population Survey (CPS), we find that CRA91 had relatively minor aggregate employment and wage effects. However, the law does appear to have affected returns to experience in the way suggested by our model. We show that returns to experience increased for women, but not for blacks, shortly after the passage of the Act. This finding is consistent with the experience/propensity-to-sue relationships found in the EEOC data. Together, these findings offer a pattern that fits with our model of litigation costs and returns to experience. We take these results as evidence that antidiscrimination protections (and employment protections in general) have redistributive effects. These effects appear to operate not merely *across* groups of differing protected status, but also *within* groups of identical protected status.

Although our empirical analysis focuses solely on CRA91, our model applies equally well to any erosion of employment-at-will. While CRA91 did aid the growth in employment discrimination litigation, the number of such suits had been increasing steadily for at least two decades before the passage of CRA91. Our analysis therefore suggests that this increasing tide in litigation (or, more generally, continued erosions in employment-at-will) may have been a contributing factor in the observed increase in returns to experience over that period.[1]

I. The Civil Rights Act of 1991 and the Legal Environment

The Civil Rights Act of 1991, which took effect on November 21, 1991, strengthened several prior pieces of employment-discrimination legislation, including the Civil Rights Act of 1866, the Civil Rights Act of 1964 (Title VII), the Age Discrimination in Employment Act (ADEA), and the Americans with Disabilities

[1] John J. Donohue and Peter Siegelman (1991) document the growth in employment-discrimination litigation throughout the 1970's and 1980's. Lawrence F. Katz and Kevin M. Murphy (1992) and John Bound and George Johnson (1992) show that the returns to experience increased for all workers over this period. Bound and Richard B. Freeman (1992) show this effect was stronger for blacks than for whites, while June O'Neill and Solomon Polachek (1993) and Francine D. Blau and Lawrence M. Kahn (1997) show the effect was stronger for women than for men. David Neumark and Wendy A. Stock (1999) consider the effects of employment protections on returns to experience over the 1980's, but from a very different perspective. They study age-discrimination laws and suggest that, by providing an enforcement mechanism for implicit contracts, these laws increase the steepness of wage profiles for all workers. Unlike ours, their analysis does not explicitly link wage profiles to litigation costs imposed by protected workers.

Act (ADA).[2] The Act also counteracted several 1989 Supreme Court interpretations of earlier antidiscrimination legislation, notably *Wards Cove Packing Co. v. Atonio* and *Patterson v. McLean Credit Union*. CRA91 contained three provisions that may have affected the willingness of displaced employees to file race- and gender-based discrimination lawsuits: it increased damage awards available to plaintiffs, allowed plaintiffs to ask for jury trials, and made it somewhat easier for plaintiffs to use statistical evidence to prove discrimination. We discuss each provision in turn.

CRA91 affected available damage awards by both amending Title VII and extending the CRA of 1866. CRA91 amends Title VII, which had limited damage awards to back pay only, by allowing plaintiffs who allege intentional race- or gender-based discrimination to sue for punitive and compensatory damages. Maximum damages under CRA91 vary by employer size, ranging from $0 for firms with fewer than 15 employees to $300,000 for firms with more than 500 employees. In addition, CRA91's extension of the CRA of 1866 removed all limits on damages in cases of *racial* discrimination in termination. The 1866 Act, which forbids discrimination on the basis of race in the "making and enforcement of contracts," allows plaintiffs to sue for unlimited punitive and compensatory damages. While the Supreme Court's *Johnson v. Railway Express Agency* (1975) and *Runyan v. McCrary* (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 quoted above is somewhat ambiguous on this point.) Throughout the 1980's, different federal courts applied somewhat different interpretations on 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, and thus could claim only back wages as damages. CRA91 explicitly extends the CRA of 1866 to the termination of contracts, thereby removing all limits on potential damage awards in race-based cases. Because suits filed under the CRA of 1866 go directly to federal court (instead of going through the EEOC as Title VII claims must) and there is no detailed data source on the number of such cases, it is impossible to fully quantify the effects of *Patterson* and CRA91 on the litigiousness of black men.

CRA91 also gives plaintiffs who seek punitive damages the right to a jury trial. This may increase the costs of displacing workers for two reasons: (i) juries are perceived to favor claims of individuals rather than corporations, and (ii) jury trials increase the legal costs associated with defending against employment-discrimination lawsuits.

Finally, CRA91 strengthened a plaintiff's ability to use statistical evidence to prove unlawful discrimination on the basis of "disparate impact." A series of influential 1970's Supreme Court rulings (starting with *Griggs v. Duke Power Co.*) 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. The 1989 *Wards Cove* decision made it more difficult to prove disparate impact by requiring plaintiffs to identify a particular employment practice leading to the disparate impact. This decision also partially reversed *Griggs* by requiring plaintiffs to show that the employment practice being challenged was not "necessary" to the defendant's business (see Abram, 1993). CRA91 weakens this standard somewhat, allowing plaintiffs to use statistical evidence in cases where the plaintiff can show the employer's decision-making process cannot be separated into specific practices.[3]

[2] Robert K. Robinson et al. (1992) provide a more detailed description of the Act's provisions, while Thomas G. Abram (1993) assesses its likely impact. We focus our discussion and our empirical analysis on provisions of CRA91 affecting race- and gender-based discrimination cases. Changes to the ADEA as a result of CRA91 were relatively minor.

[3] It does not appear that plaintiffs have increased claims of discrimination on the basis of disparate impact since the Act's passage (see Abram, 1993; Glen D. Nager and Julia M. Broas, 1994). Because plaintiffs must show disparate treatment in order to earn punitive and compensatory damages, and because CRA91 greatly increased the potential

CRA91 appears to have had a significant effect on the litigiousness of displaced employees. Our analysis of lawsuits filed in federal court shows that the number of cases alleging employment discrimination more than doubled from 1991 to 1995. Similarly, the number of race-based (gender-based) complaints filed with the EEOC increased by 13 percent (46 percent) from 1991 to 1994 and the monetary benefits awarded in cases resolved by the EEOC increased by 47 percent (87 percent) over that period. The large increase in gender-based complaints relative to race-based complaints probably reflects the fact that CRA91 gave women their first chance to seek punitive and compensatory damages and the fact that race-based suits seeking redress under the CRA of 1866 bypass the EEOC and proceed directly to federal court.

While our analysis focuses on suits alleging wrongful termination, CRA91 applies broadly to hiring, termination, and many on-the-job activities. Our model, with slight modifications, would apply equally well to litigation surrounding on-the-job activities, but explicit consideration of hiring-based protections would yield quite different results. As Richard A. Posner (1987) and Donohue and Siegelman (1991) (among others) have argued, the labor-market implications of hiring protections are very different from those of protections against discrimination in termination or on-the-job activities. Termination-based protections increase the costs associated with hiring a protected employee, as the increased costs are felt only if the employee is hired and then terminated. Hiring-based protections, on the other hand, increase the costs to employers associated with *failing* to hire a protected employee. We limit our analysis in this way because of the dramatic shift (documented by Donohue and Siegelman, 1991) in the 1980's away from hiring-based employment-discrimination litigation and toward termination-based suits. Our own examination of EEOC data from the 1990's reveals a continuation of this trend. Of all gender- and race-based complaints filed with the EEOC between 1992 and 1996, 58 percent claimed wrongful termination, 5.6 percent were for discriminatory hiring, with the rest based on on-the-job practices such as unequal pay, denial of promotion, or harassment.[4]

II. A Model of Litigation Costs and Returns to Experience

In this section, we develop a stylized model of the relationship between employment-discrimination litigation costs and the returns to experience for protected workers, and use it to examine how changes in the legal environment of the type associated with the Civil Rights Act of 1991 affect returns to experience. Our model indicates that the effect of CRA91 on returns to experience depends crucially on (i) how employees' propensity to file employment-discrimination litigation conditional on employment varies with experience, and (ii) how the CRA91-induced *increase* in propensity to sue conditional on employment varies with experience. These insights suggest a link between returns to experience and the age profile of workers filing unlawful termination claims with the EEOC. This link then forms the basis of our empirical analysis below.

A. *Workers and Firms*

We consider a discrete-time, overlapping-generations setting in which potential workers live for two periods. A worker can be employed in both periods of his life by any of M infinitely lived firms. We refer to workers in their first and second periods of life as inexperienced and experienced, respectively. Firms and potential workers are risk neutral and discount the future at rate β. We normalize the measure of each cohort of workers to one.

At the beginning of the first period of a worker's life, the worker receives job offers from firms, compares these offers to his reservation wage (which we assume to be zero), and decides whether to accept a job. A worker who accepts employment may be fired for either of two reasons. First, the worker may be discriminated against by his supervisor. We assume that while firms hire with no intention of dis-

sizes of these awards, relatively few suits filed since 1991 have alleged disparate impact.

[4] These figures actually overstate the importance of hiring cases as approximately 10 percent of hiring-based complaints also charge wrongful termination and may not have been registered had it not been for termination.

criminating against protected employees, it is costly to prevent biases of supervisors from affecting employment outcomes for individual workers. We argue that supervisors can easily claim a dismissal is performance based, when in reality the supervisor is simply biased against certain types of employees. A worker in the first period of life is discriminated against and fired with probability δ_1.[5]

Second, a worker can be fired "for cause" if his productivity is revealed to be sufficiently low. To model this, we assume workers have either high or low ability, and that workers and firms are initially symmetrically uninformed about worker ability. A worker employed in period $i \in \{1, 2\}$ of his life generates signal ψ_i, which is jointly observed by the worker and the firm and takes a value from the set $\{L, H\}$. We suppose

$$\text{Prob}[\psi_i = L|\text{low ability}] = \alpha$$

$$\text{Prob}[\psi_i = L|\text{high ability}] = 0.$$

In words, conditional on a worker being of low ability, the signal ψ_i is indicative of this with probability α. We fix the fraction of low-ability workers in each cohort at $1 - \phi_1 > 0$, and let the marginal productivity of low-productivity workers be zero.[6] Wages are sufficiently high so that any worker for whom $\psi_1 = L$ is fired immediately. To simplify our exposition, we assume that if the worker is discriminated against, then he is fired prior to the realization of ψ_1. Hence, the probability that an inexperienced worker is fired due to discrimination is given by δ_1, while the probability he is fired for cause is $\alpha(1 - \phi_1)(1 - \delta_1)$. Wages are paid at the end of the period, so that a worker who is fired (for either reason) earns no wages in the period he is fired.[7]

Workers who are not fired continue to work throughout the first period of life, and are paid the period t inexperienced-worker wage w_{1t}. Workers who are not fired in the first period of life participate in the labor market again in the second period, while workers who are fired in the first period do not work in the second. If an experienced worker accepts a job, then he is discriminated against and fired with probability δ_2. If the worker is not discriminated against, then a second signal of ability, ψ_2, is generated. As was the case in the first period, workers who are revealed to be of low ability are fired, while any worker who is retained earns wage w_{2t}.

A fired worker may elect to file suit against his former employer. A worker fired in period $i \in \{1, 2\}$ draws a personal cost of suing s from a density characterized by the cumulative distribution function G_i.[8] The worker sues if the expected damage award conditional on filing a suit exceeds s. We assume each fired worker perfectly observes the reasons underlying his dismissal, but that courts observe these reasons imperfectly. Hence, workers fired for cause sometimes win employment-discrimination lawsuits, while workers who were discriminated against sometimes lose. We let q_d represent the probability a worker who was discriminated against wins a lawsuit, and let q_c (where $q_c < q_d$) be the probability that a worker fired for cause wins. Fired workers may sue for wages in the period they are fired (w_{it}) and, after CRA91, some punitive and compensatory damages (Δ). Conditional on winning a lawsuit, a worker earns damages of amount $w_{it} + \Delta$. Workers fired for cause therefore sue with probability $G_i[q_c(w_{it} + \Delta)]$, while workers who were discriminated against sue with probability $G_i[q_d(w_{it} + \Delta)]$.

A firm's revenue in a given period depends

[5] Of course, firms may invest in monitoring or training programs that reduce the likelihood protected workers are discriminated against. One may view CRA91 and similar legislation as intended to force firms to make such investments. We discuss implications of endogenizing firms' choices over how much discrimination to permit below. See Debra A. Barbezat and James W. Hughes (1990) for a simple model of endogenous discrimination.

[6] The assumptions that workers are either high or low ability and that low-ability workers have zero productivity are not crucial here. Comparable results can be obtained from a model in which employees' abilities are continuous and firms cannot adjust wages downward to match productivity. We can also extend our model to allow the uncertainty to relate to the quality of the match between the worker's skills and employer's needs.

[7] Alternatively, we could allow workers to be fired after fraction ρ of the first period has elapsed. These workers would then earn wages ρw_{1t}. This would make the model more complex, without offering additional insights. More realistic assumptions could be applied elsewhere without changing our main findings—we could, for example, allow the realizations of discrimination and ability to occur simultaneously, or allow workers who are fired in the first period to work in the second period.

[8] Our modeling of workers' litigation choices is similar to that of Acemoglu and Angrist (2001).

on the number of high-ability experienced and inexperienced workers it employs. Denote the measure of the set of inexperienced (experienced) workers employed by firm m in period t as I_{mt} (E_{mt}). A firm employing I_{mt} inexperienced workers in period t will find that fraction ϕ_1 have high ability. Because low-ability workers have a higher likelihood of being fired in their first period of employment, experienced workers are more likely to have high ability than inexperienced. We apply Bayes' rule and find the probability an experienced worker has high ability is $\phi_2 = \phi_1/(1 - \alpha(1 - \phi_1))$. We denote firm m's revenue in period t as $R(\phi_1 I_{mt} + \gamma\phi_2 E_{mt})$, where R is increasing and strictly concave. We allow $\gamma \geq 1$ to capture the possibility that experience improves productivity.

In making employment decisions, firms consider all employment-related costs, including wages and the potential costs stemming from litigation. Consider the I_{mt} inexperienced workers hired by firm m in period t. Fraction $\delta_1 + \alpha(1 - \phi_1)(1 - \delta_1)$ of these workers are fired during period t, while the remainder are retained through the period and paid wage w_{1t}. Fraction $G_1[q_c(w_{1t} + \Delta)]$ of the fired-for-cause workers file suits for unlawful termination, while fraction $G_1[q_f(w_{1t} + \Delta)]$ of discriminated-against workers file. We assume the cost to a firm of defending against a suit is k, so the total expected cost to the firm from a suit (which includes expected damages plus direct costs of preparing a defense) is given by $q_j(w_{1t} + \Delta) + k$, where $j \in \{c, d\}$ depending on the actual reason for termination.

Firms take current and future wages as exogenous and choose employment levels to maximize the net present value of profits. Assuming an interior optimum, the firm's period t employment decisions are characterized by the following first-order conditions:

$$
\begin{aligned}
(1)\quad & \phi_1 R' \\
&= [1 - \delta_1 - \alpha(1 - \phi_1)(1 - \delta_1)]w_{1t} \\
&\quad + \delta_1 G_1[q_d(w_{1t} + \Delta)][q_d(w_{1t} + \Delta) + k] \\
&\quad + \alpha(1 - \phi_1)(1 - \delta_1)G_1[q_c(w_{1t} + \Delta)] \\
&\quad \times [q_c(w_{1t} + \Delta) + k]
\end{aligned}
$$

$$
\begin{aligned}
(2)\quad & \gamma\phi_2 R' \\
&= [1 - \delta_2 - \alpha(1 - \phi_2)(1 - \delta_2)]w_{2t} \\
&\quad + \delta_2 G_2[q_d(w_{2t} + \Delta)][q_d(w_{2t} + \Delta) + k] \\
&\quad + \alpha(1 - \phi_2)(1 - \delta_2)G_2[q_c(w_{2t} + \Delta)] \\
&\quad \times [q_c(w_{2t} + \Delta) + k].
\end{aligned}
$$

In a steady-state equilibrium, all workers except those who were fired in their first period are employed, so we require that $MI_{mt} = 1$ and $ME_{mt} = 1 - \alpha(1 - \phi_1)(1 - \delta_1)$.[9]

B. *Factors Affecting Returns to Experience*

We now address factors affecting returns to experience—that is, $w_{2t} - w_{1t}$—in this labor market. The first-order conditions in equations (1) and (2) equate the marginal productivity of each cohort to the marginal cost of employing a worker in that cohort. Three factors may lead to differences in wages paid to experienced and inexperienced workers: (i) differences in productivity (if $\gamma > 1$), (ii) differences in the fraction of high-ability workers, and (iii) differences in the expected costs of litigation. We discuss each in turn, and then ask how changes in employment-discrimination law may affect the returns to experience.

First, note that $R' > 0$ is the marginal productivity of a high-ability inexperienced worker, while $\gamma R'$ is the marginal productivity of a high-ability experienced worker. If γ is strictly greater than one, then experience results in greater productivity and hence higher wages.

Second, the firm's expectation of a worker's ability depends on the worker's experience. Firms have no information regarding ability levels of inexperienced workers; hence the probability that an inexperienced worker has high ability is ϕ_1. However, experienced workers remain in the labor force only if they *were not* fired in their first period of employment.

[9] Note that in order for both types of workers to be employed, it must be that wages are such that the right-hand side of (1) is equal to $\phi_1/\gamma\phi_2$ times the right-hand side of (2).

Because low-ability workers are more likely to be fired in the first period than high-ability workers (as long as $\alpha > 0$), the share of experienced workers with high ability is greater than ϕ_1. This means greater demand and higher wages for these workers.

Third, experienced and inexperienced workers differ in the expected costs they impose on the firm from employment-discrimination litigation. For a worker in period i of his life, the expected cost to the firm from litigation on the part of that worker is

$$(3)\quad \delta_i G_i[q_d(w_{it} + \Delta)][q_d(w_{it} + \Delta) + k] + \alpha(1 - \phi_i)(1 - \delta_i)G_i[q_c(w_{it} + \Delta)] \times (q_c(w_{it} + \Delta) + k).$$

A number of potentially opposing effects are in place here. Expected litigation costs are increasing in w_{it}, as workers earning higher wages earn higher damage awards and are, as a result, more likely to sue conditional on being displaced. Because returns to experience are positive, this effect works in the direction of higher expected litigation costs for experienced workers. However, litigation costs are also decreasing in ϕ_i, the likelihood a worker has high ability. Because inexperienced workers are more likely to have low ability and hence more likely to be fired for cause, this effect works in the direction of higher expected litigation costs for inexperienced workers. Expected litigation costs also depend on δ_i, the likelihood a worker is discriminated against, and G_i, the distribution of personal costs of litigating. If $\delta_i > \delta_j$ or if $G_j > G_i$ (in the sense of first-order stochastic dominance), then these effects work in the direction of higher expected litigation costs for workers in period i of life. As we have no a priori expectation as to how δ and G vary with experience, we conclude that these two effects could work in the direction of higher litigation costs for either experienced or inexperienced workers.

C. *Effects of Changes in the Legal Environment*

We next attempt to incorporate the effects of CRA91 into our model. While damages available to pre-1991 plaintiffs were limited to back pay (implying $\Delta = 0$), post-1991 plaintiffs can earn both punitive and compensatory damages. We therefore model CRA91 as increasing Δ.[10] This increase in potential damage awards clearly raises the cost of employing both inexperienced and experienced workers. In order to determine how the returns to experience are affected, we ask where the cost increase is larger, as wages for this group will be depressed relative to the other.

To examine this issue, we differentiate expected litigation costs in (3) with respect to Δ, and ask how the resulting expression varies with i. The derivative is given by

$$(4)\quad \delta_i q_d G_i[q_d(w_{it} + \Delta)] + (\delta_i q_d g_i[q_d(w_{it} + \Delta)] \times [q_d(w_{it} + \Delta) + k]) + \alpha(1 - \phi_i)(1 - \delta_i)q_c G_i[q_c(w_{it} + \Delta)] + (\alpha[1 - \phi_i][1 - \delta_i]q_c g_i[q_c(w_{it} + \Delta)] \times [q_c(w_{it} + \Delta) + k])$$

where g_i is the probability density function associated with G_i. Increases in Δ affect firms' expected litigation costs in two ways. First, employees who sue successfully impose higher costs on the firm in the form of higher damage awards. Mathematically, this effect is embodied in the first and third terms of (4), which are the probability an employee is displaced and sues times the derivative of the expected cost to the firm conditional on being sued. Second, the prospect of higher damage awards induces more displaced workers to file suit. The second and fourth terms of (4) are the product of the likelihood of displacement, the increase in the likelihood of suing conditional on displacement, and the expected cost to the firm conditional on being sued.

Clearly, the increase in expected litigation costs associated with CRA91 could be larger for

[10] We can obtain similar results focusing on the provision of CRA91 that allows either side to seek a jury trial. As juries are perceived to favor the claims of individuals over those of corporations, we model this as an increase in both q_d and q_c, the likelihoods that suits are successful.

either experienced or inexperienced workers. The higher wage for experienced workers implies that any increase in the likelihood of suing conditional on displacement is more costly for these workers. However, the higher likelihood of displacement for inexperienced workers means that the increase in damages is more costly for these workers.

While our model does not yield an unambiguous comparative static regarding the link between CRA91 and returns to experience, it does allow us to make two observations. First, it is apparent from (4) that one key determinant of the link between CRA91 and returns to experience is how the propensity to sue conditional on employment varies with experience. If inexperienced workers are considerably more likely to file suit conditional on being employed—that is, if

$$\delta_i G_i[q_d(w_{it} + \Delta)] + \alpha(1 - \phi_i)(1 - \delta_i)G_i[q_c(w_{it} + \Delta)] \tag{5}$$

is decreasing in i—then the increase in damages associated with CRA91 is more costly for these workers. If inexperienced workers are more likely to be discriminated against or face lower personal costs of filing suit, then employers will discount wages for inexperienced workers relative to experienced after 1991, and the returns to experience should increase.[11]

Second, another key determinant of this link is how the *increase* in propensity to sue conditional on employment varies with experience. If the increase in suits by inexperienced workers is greater than that for experienced—that is, if $\delta_i g_i[q_d(w_{it} + \Delta)] + \alpha(1 - \phi_i)(1 - \delta_i) g_i[q_c(w_{it} + \Delta)]$ is decreasing in i—then this also favors a larger increase in litigation costs for inexperienced workers, and an increase in returns to experience. Our model therefore suggests that in order to understand how CRA91 affects returns to experience, we must first examine how rates of employment-discrimination litigation vary with age among protected workers, and how the response of litigation rates to the Civil Rights Act of 1991 varied with age.

D. *Extensions*

Before turning to our empirical analysis, we briefly describe implications of two simple enrichments of our model. First, our analysis suggests two ways a firm may respond to increases in potential costs of employment-discrimination litigation. It may adjust its demand for protected workers (leading to the changes in wages we illustrate above), but it may also invest in monitoring or training programs that reduce the likelihood protected workers are discriminated against. Our model emphasizes the first, and suggests that CRA91 should negatively impact the employment of protected workers. However, firms may also adjust their monitoring efforts in a way that introduces an offsetting positive effect on employment. If we let δ_i be a decreasing function of the firm's level of monitoring, then passage of CRA91 would cause firms to revisit monitoring decisions. Increased monitoring could cause discrimination-based terminations to fall after the passage of the Act, which would yield conflicting effects on overall levels of protected-worker employment. However, as long as increased monitoring does not completely offset firms' exposure to increased litigation costs, our predictions regarding changes in relative wages are not affected.

Second, while we have, for ease of presentation, assumed labor supply is completely inelastic, removal of this restriction does yield one additional implication. Under the assumptions that (i) labor supply is somewhat elastic and (ii) the increase in expected litigation costs associated with an increase in Δ is larger for inexperienced workers, then it is possible to construct examples in which w_{2t} increases in response to the increase in Δ. This effect arises as firms substitute away from inexperienced workers because of the high potential costs of litigation, and bid up the wages of experienced workers. This observation implies that average wages *within a given period* may not be greatly affected by increases in Δ. However, this does not mean that protected workers are not harmed; because wages are redistributed from younger to older workers, the present value of a worker's lifetime earnings falls. This suggests studies examining the effects of employment protec-

[11] As we discuss below, there is evidence to suggest that protected workers' perceptions of employment discrimination vary considerably with age.

tions on *average wages* without also considering how protections may *redistribute wages* among protected workers may miss part of the effect.

III. Age Patterns in Wrongful Termination Complaints

Our model suggests that the effect of CRA91 on returns to experience is partially determined by the relationship between experience and the propensity to sue. We therefore begin our empirical analysis by examining the age distribution of employees making discrimination claims. Using data from the EEOC and the CPS, we measure the share of employed protected workers who file wrongful termination complaints, and compute how this share varies with age.[12]

Our EEOC data set lists a range of facts regarding each complaint, including the date the complaint was first filed, the "basis" of the complaint (e.g., race, gender, disability), and the "issue" (e.g., hiring, discharge, harassment). The data also include demographic information such as the plaintiff's state of residence, gender, race, and (for 70 percent of plaintiffs) age. We analyze gender-based cases brought by women and race-based cases brought by black men that were first filed with the EEOC between 1988 and 1995.[13] To eliminate age-based cases and concentrate on workers likely to be attached to the labor force, we look exclusively at plaintiffs aged 20 to 40 at the time of complaint. Also, because our model focuses explicitly on termination-based litigation, we consider only termination-based complaints. There were a total of 113,283 gender-based cases brought by white women aged 20 to 40 and 118,779 race-based cases brought by black men aged 20 to 40. Of these, a total of 149,489 (64.4 percent) were wrongful termination cases and comprise our final sample.[14]

We use the Annual Demographic File of the March CPS to estimate the number of employed white women and employed black men of each age between 20 and 40 in each year between 1988 and 1995 (where a worker is employed if he or she reported working at least 1,000 hours during the year). We create counts of workers by age/year/protected group, and use these counts and the number of complaints in each age/year/group cell to determine, by cell, the percentage of employees who file a complaint with the EEOC. These complaint rates indicate the approximate probability that a person of a given age who works at least 1,000 hours in a given year files a wrongful termination complaint.

Figures 1(a) and 1(b) show the complaint rates by age for white women and black men, respectively, during 1990 and 1993. We chose these years as representative pre-CRA91 and post-CRA91 years; the age/complaint patterns are similar in every year from 1988–1995, so examining these two years is sufficient. The complaint rate is much higher for black men than for white women. Each year, the EEOC received a gender-based wrongful termination claim from approximately one out of every 2,500 to 3,500 employed white women, but the proportion is one out of 400 to 600 for black men. Also, as suggested in Section I, the rate of complaint for both groups is noticeably higher in 1993 than in 1990. The increase in complaints is more dramatic for women than for blacks, which could be related to the attention drawn to gender-based discrimination by the 1991 Clarence Thomas confirmation hearings. Alternatively, the smaller increase in the black EEOC complaint rate may be due to the fact that

[12] Except when filing under the CRA of 1866, all workers seeking redress using CRA91 must start by filing a complaint with the EEOC.

[13] Approximately 18 percent of gender-based cases are brought by men. Approximately 80 percent of race-based cases are brought by blacks, 10 percent by whites, and the rest are split among Asians, Native Americans, and others. Some complaints allege more than one basis (that is, a person may claim both age and gender discrimination), but over 95 percent of the complaints in the age and basis groups that we analyze claim a single basis. Our results are not altered if we include complaints with multiple bases, or if we examine all nonhiring-based complaints.

[14] There are at least two limitations of this EEOC data. First, the age of the plaintiff is missing for approximately 30 percent of the observations. While we have no reason to believe there is any systematic difference between the ages of the complete sample and the missing age sample, we want to be careful about drawing conclusions from a sample limited in this way. Second, as discussed above, race-based complaints can be filed under the CRA of 1866 directly in federal court. CRA91 made this a viable option in termination suits, so it is unclear how representative EEOC complaints are of all post-CRA91 race-based discrimination complaints.

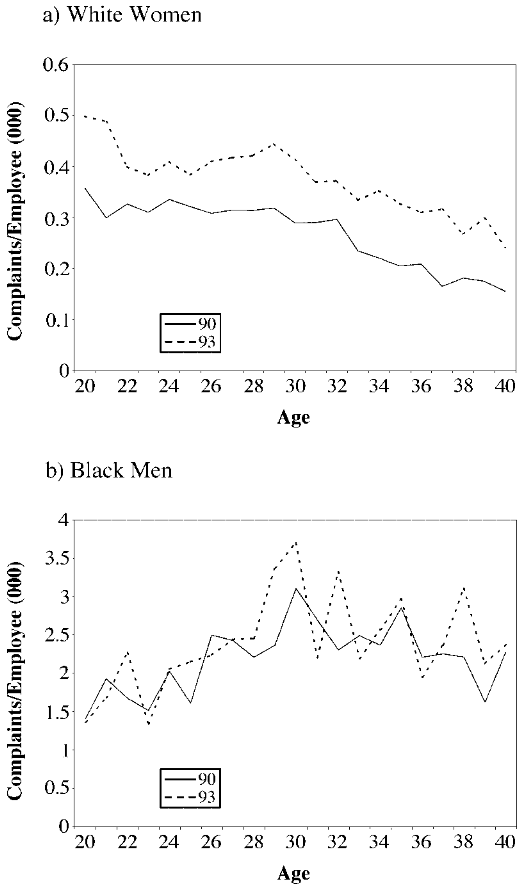

FIGURE 1. EEOC COMPLAINTS PER 1,000 EMPLOYED WORKERS BY AGE FOR WHITE WOMEN AND BLACK MEN

CRA91 allows race-based termination cases to bypass the EEOC.

The age patterns in EEOC complaints are very different for the two groups. As depicted in Figure 1(a), complaint rates decline steadily and steeply for white women in their 30's. The complaint rates in 1990 and 1993 were 0.320 and 0.420, respectively, per 1,000 for white women in their 20's, but only 0.222 and 0.328 per 1,000 for white women in their 30's. Over the full 1988–1991 (1992–1995) period, the yearly complaint rate was 0.290 (0.389) for 25-year-old white women and 0.192 (0.331) for 35-year-old white women. This pattern of decreasing complaint rates with age does not hold, however, for black men. As shown in Figure 1(b), complaint rates increase slowly and steadily as black men age. In 1990 and 1993, the complaint rates were 2.02 and 2.18, respectively, per 1,000 for black men in their 20's, but 2.38 and 2.56 for those in their 30's. Similarly, over the full 1988–1991 (1992–1995) period, the EEOC received 1.74 (1.79) complaints per 1,000 25-year-old black men per year and 2.38 (2.47) per 1,000 35-year-old black men per year.

We expect the returns to experience for a given protected group to increase as a result of the passage of CRA91 if the increase in litigation-related costs of employment is smaller for more experienced workers. In Section II, we modeled these costs explicitly and argued that if (i) the likelihood of filing a complaint conditional on being employed decreases with experience, or (ii) the increase in the propensity to sue conditional on being employed associated with increased damage awards decreases with experience, then the increase in litigation-related costs of employment may be decreasing with experience. For white women, it appears that the first of these conditions holds. There is no evidence that the increase in the propensity to sue associated with CRA91 varies with age for this group, but it is apparent that, conditional on employment, younger women are more likely to file complaints with the EEOC. For black men, it appears that neither condition holds. Older black men are more likely to file with the EEOC, and it does not appear that the increase in propensity to sue associated with CRA91 varied by age.

These differences in the age patterns of EEOC complaints for white women and black men lead us to expect different patterns in returns to experience as a result of CRA91. Our finding that young white women are more likely to file employment-discrimination litigation than older white women leads us to expect the passage of CRA91 to result in an increase in the returns to experience for women. Because propensity to sue trends upward with age for black men, our analysis does not offer a definitive prediction for this group.

Though this issue is not central to our analysis, Figure 1 does raise the question of why the age trend in EEOC complaints differs so markedly across these two groups. Our model in Section II suggests three potential answers to this question. First, the age pattern of complaints may differ across these groups if the age pattern of job displacement differs across groups. If the rate of displacement drops more sharply with age for white women than for black men, then we would expect the rate of complaints to drop more sharply with age as well. Using our CPS data, however, we found the age pattern of displacement rates to be very similar across these groups.

Second, the age pattern of complaints may differ across groups if the rates of actual discrimination vary differently with age across groups. If $\delta_1 > \delta_2$ for white women but not for black men, then displacements among young white women will be disproportionately discrimination-based compared to displacements among older white women. This could then generate the pattern we observe, as discriminated-against employees are more likely to file suit than employees fired for cause. This may occur if, for example, employers exaggerate the effect of childbearing on productivity and discriminate against women of childbearing age (see Michael Selmi, 2000). In addition, Heather Antecol and Peter Kuhn (2000) find that women's perceptions of discrimination vary considerably with age. Using data from a Canadian survey of displaced workers, they show that women aged 25 to 34 are significantly more likely to feel they were victims of gender-induced harm in the workplace than women aged 35 to 44.

Third, the age pattern in complaints may differ across groups if the personal costs of filing suit vary differently with age across these two groups. While we were unable to find any research in economics exploring determinants of employees' litigation decisions, this area has been explored by scholars in the sociology of law. Michele Hoyman and Lamont Stallworth (1986) and Phoebe A. Morgan (1999) find that women are more likely to seek redress through the legal system when they have significant parental responsibilities. If younger women are more likely to have dependent children compared to older women and thus more likely to file with the EEOC, then the pattern we observe could be explained. Alternatively, gender-based differences in government assistance programs and social norms may cause women who become disenchanted as they age to find nonparticipation to be a more viable option than would black men. Hence, older women who, conditional on working, would be likely to file complaints may instead elect not to seek employment. Our EEOC data do not contain enough demographic information to allow us to validate or refute these potential explanations for differing age trends in complaint rates.

IV. Empirical Analysis of Labor-Market Outcomes

A. *Data and Methodology*

We examine effects of CRA91 on labor-market outcomes for protected workers using data from the 1988 through 1996 Annual Demographic File of the March CPS. The March CPS, like all CPS monthly surveys, asks about current employment status including hours worked last week, full-time/part-time status, industry, and occupation. The March CPS also gathers detailed information about the respondent's employment in the previous calendar year, such as weeks and hours of work, and wages.

We focus on three measures of employment outcomes: whether the respondent worked full time, how many hours the respondent worked, and what hourly wage the respondent earned. First, we define survey respondents who worked at least 35 hours on all jobs in the week before the CPS interview as "currently employed." Second, we measure the total hours worked in the calendar year before the interview as the product of the number of weeks the person reported working in the previous year and the reported hours per week. Third, we calculated the hourly wage for the previous year by dividing the reported total wages for the year by the total hours worked.[15] Note that the years we discuss refer to the year about which the respondent was asked and not necessarily the survey year. Employment in year t refers to employment status reported in March of year t, while year t wages and hours refer to the wages and hours reported retrospectively for year t in March of year $t + 1$.

We limit our analysis to CPS respondents between the ages of 25 and 39. This restriction focuses on those with a strong labor-force attachment, minimizes the effects of schooling,

[15] The maximum annual earnings in the CPS is $99,999, so our wage variable is right-censored. However, this only affects 1.5 percent of all observations in our wage regressions. The rate of top-coding varies by year, peaking in the 1995 CPS (1994 earnings) where 2.4 percent of the observations in our wage regressions are top-coded.

TABLE 1—SUMMARY STATISTICS FROM THE 1988–1996 MARCH CPS

	Entire sample (1)	Pre-CRA91 (2)	Post-CRA91 (3)	White men (4)	White women (5)	Black men (6)
Total observations	254,320	150,275	104,045	118,091	122,968	13,261
Percentage currently employed	67.2	67.4	67.0	79.9	55.2	65.9
Hours/week	40.75	40.65	40.89	44.47	36.47	41.42
	(11.56)	(11.03)	(11.68)	(10.37)	(11.58)	(9.47)
Hourly wage	11.65	11.03	12.54	13.15	10.18	10.16
	(7.2)	(6.8)	(7.7)	(7.48)	(6.63)	(9.47)
Age	32.16	32.03	32.35	32.19	32.16	31.92
	(4.23)	(4.24)	(4.23)	(4.23)	(4.23)	(4.32)
Education	13.5	13.4	13.6	13.6	13.5	12.8
	(2.3)	(2.4)	(2.2)	(2.4)	(2.2)	(2.2)
State unemployment rate	5.9	5.9	5.8	5.9	5.9	6.1
	(1.6)	(1.7)	(1.5)	(1.6)	(1.6)	(1.5)

Notes: Data are from 1988–1996 March CPS, limited to black men and non-Hispanic white men and women aged 25–39. Column (2) [Column (3)] includes observations before 1992 [after 1992]. Hours/week and wages are averages over all nonzero observations. Standard deviations are in parentheses. The state unemployment rates are reported as percentages.

and keeps the comparison group (white men) clean by removing ADEA-protected workers.[16] We define 1987–1991 observations as "pre-CRA91" and 1992–1996 observations as "post-CRA91."[17] Summary statistics in Table 1 show that just over two-thirds of survey respondents reported full-time employment and the average work week was 41 hours. From columns (2) and (3) of the table, it appears there are no noteworthy differences between the "pre-CRA91" and "post-CRA91" samples. Columns (4)–(6) show that employment rates, hours worked, and wages are higher for white men than for either of the protected groups.

Our primary methodology is to measure how the differences between protected- and unprotected-worker employment-outcome measures changed from the period shortly before CRA91 to the period shortly after. That is, we measure the "difference-in-differences" of, for example, black and white wages before and after the law. The critical assumption needed for our analysis is that, in the absence of CRA91, wages would not have changed in a way that differed by race or gender over the 1988–1995 period. At least two non-CRA91 factors may have contributed to changes in employment outcomes for protected workers over this period, and we will consider these factors in framing and interpreting our results. First, omitted variable bias could result from other policy changes, particularly the minimum wage increases in 1990 and 1991. Second, there were underlying trends in the returns to skill and the income distribution, and the effects of these trends may have differed across protected and unprotected groups.

B. *Wage and Employment Effects of CRA91*

We first estimate the effects of CRA91 on the three employment-outcome variables (employment, hours worked, and wages) separately for both protected groups. The comparison group throughout is non-Hispanic, white men under 40 years old. White men file discrimination claims far less frequently than members of either protected group, so we expect the Act to have a much smaller effect on these workers' employment outcomes.[18] We measure the effect of CRA91 by estimating

[16] We expanded the upper limit of the age range to 50 and obtained essentially the same results.

[17] This is not a perfect division between pre- and post-CRA91. The 1991 measures of wages and hours include 40 days of post-CRA91 time and the data for these observations were recorded in March 1992, after the law was enacted. However, our results were basically unaffected when we redid our analysis without the 1992 survey observations.

[18] The Act did affect the legal status of disabled white men. However, fewer than 5 percent of workers in the age group we study are disabled (Acemoglu and Angrist, 2001).

$$(6)\quad y_{it} = \alpha + \alpha_p d_p + \sum_{t=87}^{95} \beta_t (d_t \times d_p) + \boldsymbol{\delta}\mathbf{x}_i + \varepsilon_{it}$$

where y_{it} is hours worked or log hourly wages in year t for person i, d_p is a protected indicator variable, d_t is an indicator for year t, and $\mathbf{x}_i$ is a vector of control variables. Examining how the β_t coefficients differ for pre- and post-CRA91 years tells us how employment outcomes were affected by the law. To get at the pre/post effect more directly, we can adjust equation (6) to

$$(7)\quad y_{it} = \alpha + \alpha_p d_p + \beta_{\text{post}}(d_{\text{post}} \times d_p) + \boldsymbol{\delta}\mathbf{x}_i + \varepsilon_{it}$$

where d_{post} is an indicator for "post-CRA91." When we measure the effect of CRA91 on employment status, y is an indicator variable equal to one if the respondent reports full-time employment, and we estimate the coefficients with a probit specification. If employment or hours worked is the dependent variable, the vector of control variables, $\mathbf{x}_i$, includes indicators for year, state, high school and college graduation, five-year age categories, half percentage point intervals in state unemployment rates, marital status, and interactions between state unemployment and protected status indicators and between marital status and female. If log wage is the dependent variable, then $\mathbf{x}_i$ includes experience, experience squared and education, as well as indicators for year, state, state unemployment rate, and unemployment rate/protected status interactions. Some specifications also interact a linear time trend with protected status for separate trends in employment outcomes for protected and unprotected workers.

Panel A of Table 2 and Figure 2(a) display the results of estimating equations (6) and (7) using white women under 40 as the protected group and white men under 40 as the comparison group. In the figure, we plot the β_t coefficients from estimation of equation (6), while the table reports coefficients from estimation of equation (7). Our findings confirm the well-established facts that women participate in the labor market at significantly lower rates than men, that they work fewer hours, and that they earn less (about 27 percent less on a regression-adjusted basis). However, as the table and figure show, there was a distinct trend towards increasing female labor-force participation, hours, and wages during the period we study. Log wages rose by 4 percent more for women than men over the pre-CRA91 period to the post-CRA91 period, while women's employment grew 2 percent more than men's.

These effects cannot be attributed to CRA91, however. In columns (2), (4), and (6) we control for female-specific trends in employment, hours, and wages, which leaves only small (and insignificant) pre/post differences in the hours and wage regressions. While this difference remains significant in the employment regression, careful examination of Figure 2(a) shows the growth in female employment was well underway by March 1991, predating CRA91 considerably. Visual inspection of Figures 2(b) and 2(c) confirms that any effects of CRA91 are minor compared to the ongoing growth in women's hours and wages relative to those of men. Overall, Table 2 and Figure 2 indicate that CRA91 had minor effects on average employment, hours, and wages for white women.

The results look different for black men, but this is primarily due to the different underlying labor-market trends. Panel B of Table 2 confirms the significant difference between black and white employment outcomes, even controlling for other observable characteristics. The evidence suggests a mild negative effect of CRA91 on average black employment and hours. The black/post-interaction term is negative for the employment probit [column (2)] and negative and significant for the hours regression [column (4)]. Figures 3(a) and 3(b) also suggest that black employment and hours were affected by CRA91—after trending up through 1991, black employment and hours worked dropped relative to whites starting in 1992. The effect is noteworthy but not large, representing about a 2-percent decline in black hours worked. The last two columns of the table and Figure 3(c) do not indicate that black wages changed as a result of CRA91. Overall, the evidence in Table 2 and Figure 3 is consistent with CRA91 having had a mild negative effect on the employment prospects of black men.

TABLE 2—CHANGES IN PROTECTED WORKERS' EMPLOYMENT OUTCOMES POST-CRA91

A. *White Women Compared to White Men:*

Dependent variable	Full-time employment (1)	Full-time employment (2)	Hours worked (3)	Hours worked (4)	Log wages (5)	Log wages (6)
Female	−0.2253	−0.2132	−240.59	−519.67	−0.2704	−1.1530
	(0.0216)	(0.4096)	(12.43)	(246.21)	(0.0087)	(0.1875)
	[−0.0845]	[−0.0800]				
Female × post-CRA91	0.0538	0.0544	5.20	−8.59	0.0416	−0.0021
	(0.0115)	(0.0238)	(7.11)	(14.68)	(0.0054)	(0.0107)
	[0.0201]	[0.0203]				
Female × linear trend		−0.0001		3.11		0.0098
		(0.0046)		(2.74)		(0.0021)
		[−0.0001]				
Observations	241,059	241,059	241,059	241,059	156,552	156,552
Log-likelihood/R^2	−143,857	−143,857	0.2109	0.2109	0.2485	0.2486

B. *Black Men Compared to White Men:*

Dependent variable	Full-time employment (1)	Full-time employment (2)	Hours worked (3)	Hours worked (4)	Log wages (5)	Log wages (6)
Black	−0.3517	−1.8710	−371.14	−1,758.56	−0.2254	−0.0217
	(0.0421)	(0.9185)	(21.98)	(518.85)	(0.0182)	(0.4186)
	[−0.1199]	[−0.6075]				
Black × post-CRA91	0.0052	−0.0645	−2.39	−65.14	−0.0026	0.0073
	(0.0259)	(0.0537)	(14.96)	(29.33)	(0.0120)	(0.0237)
	[0.0016]	[−0.0206]				
Black × linear trend		0.0153		15.41		−0.0023
		(0.0103)		(5.76)		(0.0046)
		[0.0048]				
Observations	131,352	131,352	131,352	131,352	100,578	100,578
Log-likelihood/R^2	−70,501	−70,501	0.1060	0.1060	0.2147	0.2147

Notes: Data from 1988–1996 March CPS, limited to black men and non-Hispanic white men and women aged 25–39. Panel A (B) compares white women to white men (black men to white men). Full-time employment = 1 if individual reports working ≥ 35 hours in week before CPS interview. Columns (1)–(2) are probits and include indicators for high-school and college graduation, five-year age categories, state, year, marital status, half-percentage-point intervals in state unemployment rate, and protected status/state unemployment interactions and marital status/female interactions. Bracketed terms are probability derivatives or estimated change in probability that the dependent variable takes value 1. Hours worked is the product of average weekly hours and number of weeks worked over the preceding year. Columns (3)–(4) are ordinary least squares (OLS) and include the same controls as in columns (1)–(2). Log wage is the log of average hourly wage for the year. Columns (5)–(6) are OLS limited to individuals working 1,500 or more hours during the specified year. Controls include experience, experience squared, education, and indicators for state, year, and half-percentage-point state unemployment rate intervals, and protected status/state unemployment interactions. Coefficients on "Female" and "Black" are for the pre-CRA91 period with state unemployment rate of 6 percent. Standard errors are in parentheses.

C. *CRA91 and Returns to Experience*

While average wages for protected groups appear to be unchanged as a result of CRA91, our analysis in Section II suggests that the Act may alter the returns to experience and thus redistribute wages within groups of protected workers. From our analysis of the age distribution of EEOC claims, we expect this effect to be stronger for white women than for black men. We expect no change in returns to experience for unprotected workers as a result of the Act, so examining the relative changes in returns to experience for protected and unprotected workers allows us to control for other factors affecting returns to experience during the early 1990's.

In our stylized model, employers learn over time about the productivity of workers. In actual work environments, it is unclear whether this learning by employers occurs only when employees are actually on the job or if information

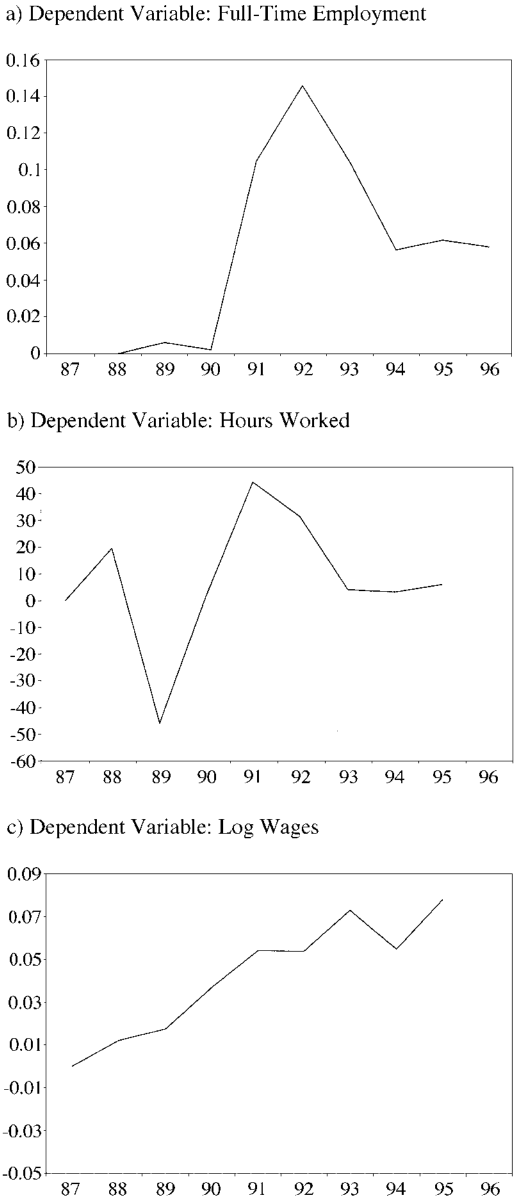

FIGURE 2. ESTIMATES OF β_t FROM EQUATION (6): WHITE WOMEN COMPARED TO WHITE MEN

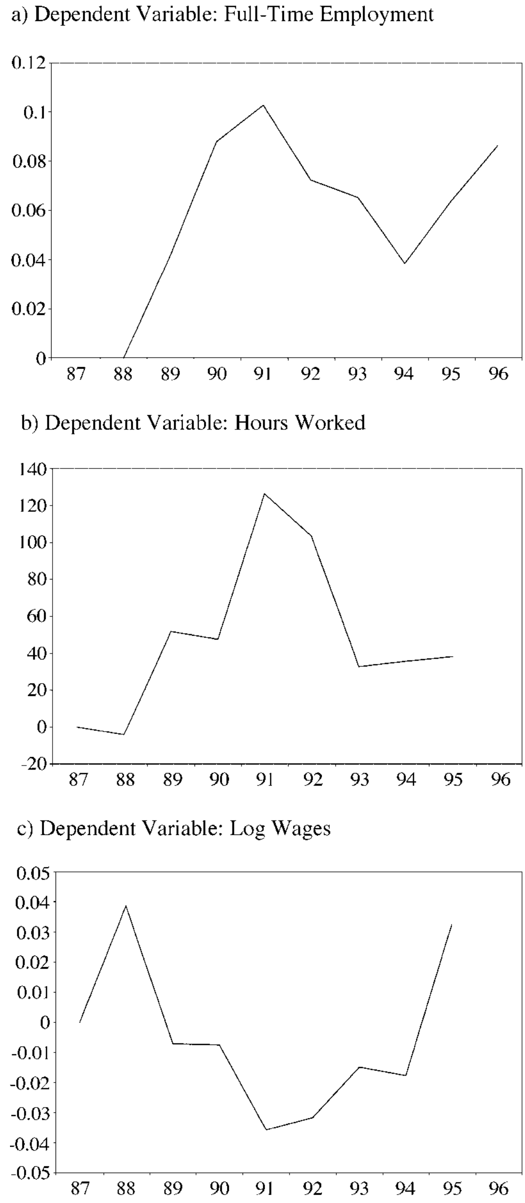

FIGURE 3. ESTIMATES OF β_t FROM EQUATION (6): BLACK MEN COMPARED TO WHITE MEN

is also revealed from the frequency and length of nonemployment spells. To allow for both possibilities, we would ideally like to use both "potential experience" (which we define as age − years of education − 6) and "actual experience" as measures of workforce experience. Unfortunately, the CPS does not contain enough historical employment information about individual respondents to allow us to measure actual experience.

We therefore use historical CPS data to derive two proxies for actual experience. Our first proxy applies a method similar to that used by Tricia Gladden and Christopher Taber (2000).

We gather labor-force participation rates from the 1964–1995 March CPS and divide respondents into cells along four dimensions: gender, race (white or black only), education (less than high school, high-school graduate, some college, college graduate), and year of birth. We then sum average work experience across years for each cell, and use this as a proxy for actual experience gathered by workers in this cell. We refer to this proxy for a worker's actual experience as "Cell Average I."

A problem with this measure, however, is that the individuals we observe to be working 1,500 or more hours in a year (and who therefore comprise our sample) are likely to have different experience profiles than the average CPS respondent in a given cell. If there is persistence in individual employment status, then average actual experience across all individuals in a cell is likely to be lower than the average actual experience of individuals in a cell who are currently employed.[19] We devise a second proxy for actual experience, which we refer to as "Cell Average II," in response to this concern. To construct this proxy, we assume that the individuals in any given gender-race-education-birth-year cell who work the most hours in year t are also those who worked the most hours in year $t - 1$, $t - 2$, etc. While the first cell-average actual experience proxy assumes each individual's labor-force participation is completely independent from year to year, the second assumes the correlation of across-year participation is maximized. (See the Appendix for a detailed description of the construction of these measures.) Neither measure is perfect, but we believe they represent reasonable lower and upper bounds, respectively, on average actual experience for employed individuals in each cell.[20]

To examine the effects of CRA91 on returns to experience, we estimate the following:

$$(8) \quad w_{it} = \alpha + \alpha_p d_p + \sum_{t=87}^{95} \beta_t (d_t \times d_p \times e_{it}) + \boldsymbol{\delta}\mathbf{x}_i + \varepsilon_{it}$$

where w_{it} and e_{it} are log hourly wages and experience (either potential experience or the cell-average proxy for actual experience), respectively, of individual i in year t. Our vector of control variables ($\mathbf{x}_i$) is described in Table 3.[21] As above, some specifications include interactions between a linear time trend, protected status, and experience to allow the trend in returns to experience to differ for protected and unprotected workers. The coefficient β_t represents the amount by which the returns to experience for the protected group exceeds that of the comparison group in year t, where the first year's β_t is normalized to zero. Higher values of β_t after CRA91 would imply that the increase in returns to experience was larger for protected workers. To assess pre/post differences more directly, we also estimate

$$(9) \quad w_{it} = \alpha + \alpha_p d_p + \beta_{\text{post}}(d_{\text{post}} \times d_p \times e_{it}) + \boldsymbol{\delta}\mathbf{x}_i + \varepsilon_{it}.$$

As above, we limit the analysis to survey respondents between the ages of 25 and 39.

Women.—We estimate equations (8) and (9) with white women as the protected group and white men as the comparison group. In Panel A of Table 3, we list the results from estimation of equation (9), while in Figure 4(a), we plot the β_t coefficients obtained when estimating equation (8).

In column (1), we use potential experience and obtain an estimate of β_{post} of 0.0031, which is significant at better than the 2-percent level. The magnitude of this estimate implies that, relative to white men, the wage premium for 30-year-old women relative to 25-year-old women was 1.5 percent higher after CRA91 than before. Plots of the individual year effects

[19] Much of the analysis of labor-force attachment has focused on women. James J. Heckman and Robert J. Willis (1977), Claudia Goldin (1990), and Kathryn Shaw (1994) document considerable persistence in individual women's employment status.

[20] Using OLS to regress individual-level wages on cell-average experience would produce understated standard errors because cell-average experience is actual experience plus unobserved noise. We therefore follow Brent R. Moulton (1986) and run GLS assuming a random group effect.

[21] To insure that our results do not reflect the interaction between experience and other variables, we reran our analysis with controls for experience interacted with education and with the state unemployment indicators. This had a trivial effect on our estimates.

TABLE 3—CHANGES IN PROTECTED WORKERS' RETURNS TO EXPERIENCE POST-CRA91

A. *White Women Compared to White Men:*

Experience measure	Potential (1)	Potential (2)	Cell average I (3)	Cell average I (4)	Cell average II (5)	Cell average II (6)
Female × experience × post	0.0031	0.0045	0.0110	0.0100	0.0010	0.0115
	(0.0011)	(0.0022)	(0.0024)	(0.0048)	(0.0014)	(0.0027)
Female × experience × trend		−0.0003		0.0002		−0.0024
		(0.0004)		(0.0009)		(0.0005)
Observations	156,552	156,552	156,292	156,292	156,161	156,161
R^2	0.2540	0.2540	0.2527	0.2528	0.2498	0.2499

B. *Black Men Compared to White Men:*

Experience measure	Potential (1)	Potential (2)	Cell average I (3)	Cell average I (4)	Cell average II (5)	Cell average II (6)
Black × experience × post	−0.0042	−0.0012	−0.0011	−0.0046	−0.0044	−0.0024
	(0.0025)	(0.0048)	(0.0043)	(0.0084)	(0.0033)	(0.0062)
Black × experience × trend		−0.0007		0.0008		−0.0004
		(0.0009)		(0.0016)		(0.0012)
Observations	100,578	100,578	100,183	100,183	99,918	99,918
R^2	0.2155	0.2155	0.2143	0.2143	0.2136	0.2136

Notes: Dependent variable is the log of average hourly wage for the year. Data from the 1988–1996 March CPS, limited to black men, non-Hispanic white men, and women aged 25–39 and working 1,500 hours or more in the specified year. Panel A (B) compares white women to white men (black men to white men). Controls include experience, experience squared, education, indicators for year, state, and state unemployment rate, interactions between protected status and unemployment rate, experience, and experience squared, and interactions between year and experience, experience squared, and protected status. In columns (1)–(2), regressions use OLS and experience is defined as age − education − 6. In columns (3)–(6), regressions use GLS and experience is defined as the average experience for 1965–1995 March CPS respondents with the same gender, race, education, and year of birth. Cell Average I and II measures are computed using different assumptions about the persistence of labor-force participation. See Appendix for details. Standard errors are in parentheses.

in Figure 4(a) (with 1991 normalized to zero) indicate that this premium started to increase in 1992, which coincides with the passage of the Act. To verify that this effect is not merely the continuation of an upward trend in female returns to experience, we estimate a specification that includes a linear trend and present results in column (2). Our estimate of the effect of CRA91 remains significant, and grows slightly in magnitude. The corresponding estimates using either cell-average experience [see columns (3)–(6)] also show that women's returns to experience increased following CRA91. The estimates reported in columns (4) and (6) indicate that the premium to being a woman whose cell averaged five years of experience more than another group increased by 4.2 percent and 5.8 percent, respectively, after CRA91. Given that cell-average experience increases more slowly with age than potential experience, the estimates using the potential and cell-average measures are fairly consistent.

From the results in subsection B, it is apparent that during the time period we study, labor-force participation rates were increasing for women relative to men. This trend in participation implies that a post-CRA91 woman of a given potential experience level is likely to have more actual labor-force experience than a pre-CRA91 woman of the same potential experience. If employers offer higher wages to women with more actual experience, then we might expect this participation trend to result in increasing returns to potential experience over the time period we study.[22] While our control for a trend in women's returns to experience and our results using cell-average experience suggest that this effect is not driving our results, we offer two additional robustness checks here. First, we perform a "placebo" analysis where we assess the pre/post difference in returns to

[22] O'Neill and Polachek (1993) document increases in women's relative returns to potential experience in the 1980's, and show this can be attributed to increased actual experience resulting from increased labor-force participation.

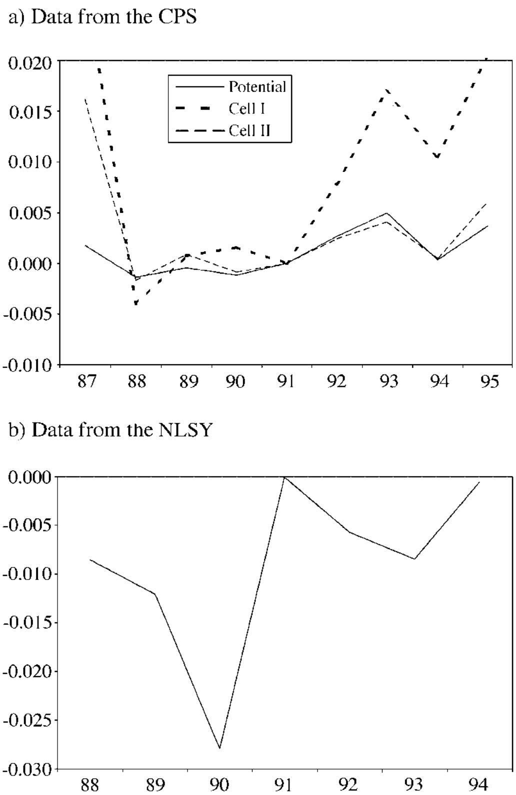

FIGURE 4. ESTIMATES OF β_t FROM EQUATION (8): WHITE WOMEN COMPARED TO WHITE MEN

experience as if the CRA has been passed at the end of 1987. If increasing female labor-force participation leads mechanically to increasing returns to experience, then we would expect to see a pre/post change using 1987 as the break point. Second, we use the National Longitudinal Survey of Youth (NLSY) to obtain a small sample of workers for whom we are able to measure actual workforce experience.

To perform our placebo analysis, we expand our data to include the 1986–1991 March CPS.[23] We analyze these data as though a law that might have affected women's returns to experience was enacted at the end of 1987; that is, we run the regressions presented in Panel A of Table 3 where the "pre" and "post" periods are 1985–1987 and 1988–1990, respectively. If this analysis yields positive changes in women's returns to experience, then we would question whether the effects we document are attributable to CRA91. We find, using all three measures of experience, no evidence that there was an increase in women's relative returns to experience around 1987. The analysis yields "post-1987" coefficients (which we do not report) that are negative and statistically insignificant. The estimated linear trend in women's returns to experience is positive and significant using each of the two cell-average experience measures, and negative and insignificant using potential experience. Furthermore, in regressions that combine post-CRA91 data with 1985–1990 data, we find that the "post-1991" effect is statistically distinguishable from the "post-1987" effect. That is, we can reject the hypothesis that the increase in women's returns to experience using 1991 as a break point is equal to that using 1987 as a break. These findings suggest that the positive and significant "post" coefficients presented in Table 3 are not following mechanically from increasing female labor-force participation.

As a second check, we gather data from the NLSY.[24] The main advantage of this survey for our purposes is that we can follow individuals through their careers and measure actual labor-market experience more accurately. This comes at the cost of a much smaller sample. The NLSY sampled fewer than 12,000 individuals during the years we study, while the CPS surveyed each resident of 60,000 different households. Also, because the NLSY is administered to the same people each year, the sample ages each year and we have to drop the youngest pre-CRA91 observations and the oldest post-CRA91 observations in order to measure the returns to experience over comparable ranges of experience.

[23] If the effects of CRA91 were immediate and involved only a discrete shift with no effect on the trend in returns to experience, then we could use either the pre-CRA91 or the post-CRA91 period to see how wages might have been changing in the absence of the law. However, because CRA91 may affect wages with lag and may induce some trend shifts, we need to concentrate on the period before the law to remove its effects from the placebo analysis.

[24] Because we use the NLSY only for comparison purposes, we do not provide background information on this data source. Henry S. Farber and Robert Gibbons (1996) offer details on using the NLSY to measure actual experience. Descriptions of our experience measure and sample restriction criteria are available from the authors upon request.

We estimate equations (8) and (9) using 9,447 individual-year wage observations for white men and women between the ages of 27 and 31. The NLSY-estimated pre/post change in female returns to experience is 0.0066 log points, which is similar in magnitude to the estimates obtained using cell-average experience in Table 3. However, this coefficient is not significantly different from zero. The annual effects [β_t from equation (8)] are displayed in Figure 4(b). While each year effect is measured with considerable sampling variance, the overall pattern is similar to that obtained from CPS data in Figure 4(a). While we cannot place a great deal of confidence in any of our NLSY-based estimates, what evidence there is suggests that the increase in female returns to experience around the time of CRA91 is not the result of a mismatch between actual and potential experience.

Finally, we ask whether the magnitudes of our estimates of women's relative increase in returns to experience are reasonable given the magnitudes of expected litigation costs. This is naturally an imprecise discussion because the exact estimates of costs stemming from employment-discrimination litigation are not available. The estimate in Table 3, column (1), suggests that, all else equal, a 30-year-old woman's wage was 1.5 percent higher than a 25-year-old woman's after CRA91 than it would have been before CRA91. The median annual wage for 25-year-old white women in our sample employed in full-time jobs in 1994 was $17,000. Our estimate suggests, therefore, that the increase in annual expected costs of litigation and litigation prevention associated with CRA91 were $200–$300 higher for a 25-year-old woman than for a 30-year-old woman. Applying James N. Dertouzos and Lynn A. Karoly's (1992) estimate that the costs of damage awards themselves reflect only about 1 percent of the total costs of potential litigation, this translates to $2 or $3 of actual damages.

To compare this figure to amounts actually awarded, consider that the EEOC awarded $44 million in gender-discrimination claims in 1994, which was nearly double the 1991 amount. Federal courts awarded over $200 million in damages in all employment-discrimination cases in 1994, although most of these cases were originally filed or involved discrimination before CRA91 was enacted. New employment-discrimination suits filed in federal court in 1994 demanded over $5 billion in damages, a 165-percent increase from 1990. We are unable to determine the shares of these figures that stem from gender-based cases, however. When state fair employment practice judgements and state court damages are added, the impact of CRA91 could be enough to explain the $200–$300 differential.

Another way to consider the costs of discrimination suits is to look at prices for Employment Practices Liability Insurance (EPLI). This product, which has grown significantly in recent years but still has fairly low market penetration, indemnifies companies against the legal costs and damages resulting from discrimination and other employment-practices litigation. From conversations with two insurance agents, we learned that the approximate cost of EPLI is $62 per employee per year, although this figure varies considerably with firm size, firm type, and the buyer's internal human resource policies. The contribution of CRA91-protected workers to this cost is presumably significantly higher. Also, insurers are unwilling to provide EPLI at the quoted rates unless the employer develops a set of internal procedures to minimize the probability of litigation. Thus, the expected cost of employment-practices litigation is probably at least a few hundred dollars per year per protected worker. Overall, these back-of-the-envelope calculations lead us to think that the experience effects measured in Table 3 are comparable to what we might have expected.

Blacks.—We now analyze the effects of CRA91 on returns to experience for black men. Panel B of Table 3 and Figure 5(a) display the results from estimation of equations (8) and (9) using CPS potential and cell-average experience measures. We find no evidence to indicate that CRA91 affected returns to experience for black men relative to white men. All of our estimates of β_{post} in the table are negative—indicating a drop in returns for experience for blacks—but none is statistically distinguishable from zero. Addition of a black trend in returns to experience does not affect the results.[25]

[25] When looking at black employment over the 1988–1996 period, it is important to consider the effects of the 1990 and 1991 federal minimum wage increases. We

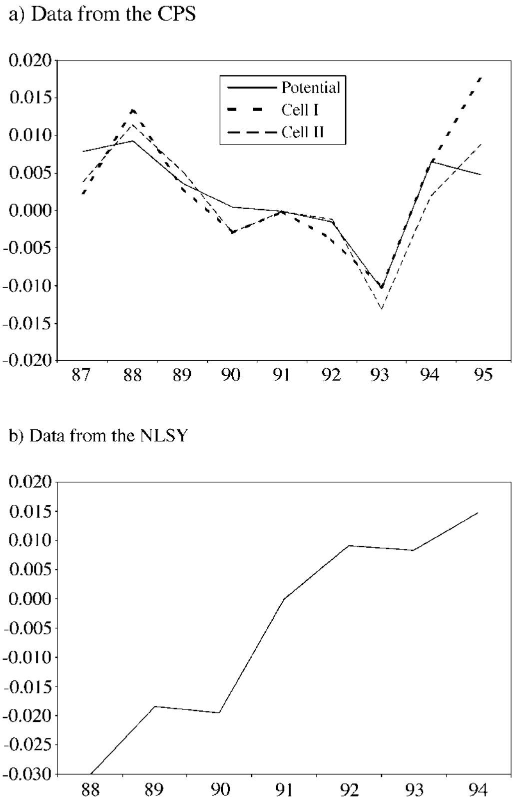

FIGURE 5. ESTIMATES OF β_t FROM EQUATION (8): BLACK MEN COMPARED TO WHITE MEN

Despite the smaller sample, the NLSY does offer some support for the assertion that returns to experience increased for blacks. When we estimate equation (9) with our NLSY sample, we obtain a coefficient of 0.026 for β_{post}. This point estimate of the post-CRA91 effect is larger than any of the estimates in Table 3, although it is quite imprecise. Plotting the β_t parameters from estimation of equation (8) using NLSY data [see Figure 5(b)], we see that the relative change in returns to experience for black men was actually largest *just prior* to the passage of CRA91.

We conclude that there is little evidence to suggest that returns to experience for black men increased as a result of the passage of CRA91. We interpret this as consistent with our model, given the age patterns of black male EEOC claims. However, we cannot entirely rule out two alternative interpretations. First, it is possible that CRA91 simply did not have a significant effect on the legal environment faced by black plaintiffs. As we described above, different federal courts applied different interpretations of the CRA of 1866 in the years leading up to *Patterson.* If, before *Patterson,* employers behaved as though the CRA of 1866 could be applied to termination-based cases and were slow to change their actions after the decision, then we would expect the 1991 Act to have had little effect on blacks. This is inconsistent, however, with the fact that other studies examining CRA91 have documented effects on employment outcomes of black men (see Oyer and Schaefer, 2000).

Second, recall that CRA91 explicitly allows black men to use the CRA of 1866 in termination-based suits, and that such claims do not need to go through the EEOC. Because data on suits filed directly in federal court are unavailable, it is possible that our Figure 1(b) (which makes use of EEOC data alone) is not an accurate representation of the age distribution of claims in the post-CRA91 period. This is problematic for our analysis if the age distribution of EEOC plaintiffs differs significantly from the age distribution of federal court plaintiffs in the post-CRA91 period. If this were the case, however, one might expect the age pattern of EEOC complaints in the years after the *Patterson* decision but before CRA91 (when terminated black workers had no recourse without the EEOC) to be markedly different from that after CRA91. We find the pre- and post-CRA91 age distributions of black EEOC plaintiffs to be quite similar.

An Extension to Education as a Signal.—Finally, we briefly consider another source of information for employers—educational achievement of potential employees. Suppose educational attainment signals productivity in the manner suggested by A. Michael Spence (1973), and that this signal becomes less important as the worker ages because potential employers gain alternative sources of information (such as work history) regarding the employee's productivity. Then we would expect that an increase in potential costs of

experimented with Tobit specifications and with left-censoring wages at a constant minimum wage throughout the period we study. This had no substantive effect on our results. Also note that while we ignored the minimum wage in the previous section, a much smaller fraction of white women earn minimum wage compared to black men.

TABLE 4—CHANGES TO PROTECTED WORKERS' RETURNS TO EDUCATION POST-CRA91

Protected group:	Blacks	Blacks	Women	Women
Age range:	25–32	33–39	25–32	33–39
Experience measure	(1)	(2)	(3)	(4)
Protected × education × post	0.0069	−0.0077	−0.0003	−0.0070
	(0.0082)	(0.0077)	(0.0034)	(0.0034)
Observations	51,861	48,717	81,812	74,740
R^2	0.1735	0.1961	0.2075	0.2658

Notes: Dependent variable is the log of average hourly wage for the year. Data from the 1988–1996 March CPS, limited to black men, non-Hispanic white men, and women aged 25–39. Sample limited to individuals working at least 1,500 hours in the preceding year. These OLS regressions include controls for potential experience, experience squared, education and indicators for state, year, and half-percentage-point state unemployment rate categories and year/protected status interactions. Standard errors are in parentheses.

litigation arising from termination would increase the returns to education for younger protected workers relative to older protected workers.[26]

We offer a simple test of this assertion by examining how the returns to education changed around the time of CRA91. We estimate

$$w_{it} = \alpha + \beta_{\text{post}}(d_{\text{post}} \times d_p \times s_{it}) + \boldsymbol{\delta}\mathbf{x}_i + \varepsilon_{it}$$

where s is years of schooling and other variables are defined as above, separately for CPS respondents between the ages of 25 and 32 and for those between 33 and 39. The coefficient β_{post} estimates how the relative-to-white-men returns to education for protected workers changed after the passage of CRA91. This specification allows us to control for overall, age-group-specific, and protected-group-specific trends in returns to education. These controls are particularly important here, as there is ample evidence to suggest there have been significant changes in the returns to education overall and among certain demographic groups during the 1980's and 1990's.[27]

If the reasoning outlined above is correct, then we would expect β_{post} to be higher for younger workers than for older workers. We present results in Table 4. For both blacks and women, the point estimates are consistent with the assertion that CRA91 increased returns to education for young protected workers. The estimates in columns (1) and (2) suggest that, for young black men, the returns to a year of education increased by 1.5 percent relative to older blacks. Similarly, columns (3) and (4) indicate that returns to education increased by 0.7 percent for young white women relative to older white women. Not surprisingly, given that we are using four sources of variation simultaneously, these difference are not statistically significant at conventional levels. The p-values testing the hypotheses that the young/old difference in β_{post} is zero are 0.16 and 0.19 for women and blacks, respectively. While our finding here is not strong, it does provide some evidence consistent with employers' use of education as a signal of termination probability.

V. Conclusion

This paper studies how the Civil Rights Act of 1991 affected employment outcomes for members of protected groups. While most prior work on the effects of employment-discrimination litigation examines *average* effects on protected workers, we focus on how the law affected the *distribution* of wages and employment across members of protected groups. We develop a simple model to study the effects on labor markets when the costs of potential litigation on the part of displaced employees increases. The novel features of our model are that workers' characteristics are assumed to become more easily observable as workers become more experienced and that firms engage in both for-cause and discriminatory firings. We find the effect of increases in litigation costs on returns to experience depends crucially on (i)

[26] Our argument here is similar to Farber and Gibbons (1996), except that we consider possible costs of bad estimates of employees' productivity.

[27] See, for example, Katz and Murphy (1992).

how employees' propensity to file employment-discrimination litigation conditional on employment varies with experience, and (ii) how the *increase* in propensity to sue (stemming from the increase in litigation costs) conditional on employment varies with experience.

We test this assertion using a data set of complaints filed with the EEOC and using the Annual Demographics File from the CPS. We find that women become less likely to file wrongful termination claims as they age. Consistent with our model, we also find an increase in women's returns to experience when CRA91 increased expected costs of employment-discrimination litigation. Black men, on the other hand, become more likely to file a wrongful termination complaint as they age, so our model does not provide a definitive prediction about their returns to experience. We detect no change in black returns to experience around the time of the passage of CRA91. Our analysis suggests that a law that has fairly negligible average effects on protected groups can have important redistributive effects within these groups.

Appendix: Construction of "Cell Average" Proxies for Actual Experience

This Appendix provides additional description of the construction of our "Cell Average I" and "Cell Average II" proxies for actual experience used in Table 3. We first partitioned our wage regression sample (from Table 2) into cells by birth year, gender, race, and education. We use two categories for race (black and non-Hispanic white) and four for education (did not finish high school, high-school graduate, some college, and college graduate). We then gather information from the 1964–1995 March CPS on how individuals in each cell accumulated work experience over time. For each CPS, we examine all respondents who are in one of our cells and for whom age is greater than the minimum of 18 and that respondent's education plus six. For each such respondent, we compute work experience in that year as the minimum of one and hours worked divided by 2,080.

To calculate our Cell Average I measure, we first compute the average of this work experience measure across individuals within a given cell in each CPS year. We then sum these averages across CPS years to compute the cumulative average experience for members of each cell. As an example, consider white female high-school graduates born in 1960. Beginning in 1979 (because this is when individuals in this cell turned 19), we compute average work experience across all such individuals who responded to the CPS. For all members of this cell who appear in our data for the year 1990, we use the sum of average work experience of individuals in the cell from 1979 through 1989 as our Cell Average I measure of work experience.

To calculate our Cell Average II measure, we begin with the wage regression sample from Table 2. For each year (1987–1995) and for each cell, we compute the share of all CPS respondents who worked at least 1,500 hours and thus qualified for our wage regression sample. To compute the actual experience proxy for that year and cell, we then go to the historical CPS data. The procedure is best illustrated by an example. Suppose that of the white female high-school graduates born in 1960 who responded to the March 1991 CPS, 60 percent worked 1,500 or more hours in 1990. We again begin in 1979 and compute the average experience in that year of the 60 percent of white female high-school graduates born in 1960 who worked the most hours. We repeat this calculation for the years 1980 through 1989. We sum these average experience figures across years 1979–1989 to obtain our Cell Average II measure of work experience for white female high-school graduates whom we observe to be employed in 1990.

REFERENCES

Abram, Thomas G. "The Law, Its Interpretation, Levels of Enforcement Activity, and Effect on Employer Behavior." *American Economic Review*, May 1993 (*Papers and Proceedings*), *83*(2), pp. 62–66.

Acemoglu, Daron and Angrist, Joshua D. "Consequences of Employment Protection: The Case of the Americans with Disabilities Act." *Journal of Political Economy*, October 2001, *109*(5), pp. 915–57.

Antecol, Heather and Kuhn, Peter. "Gender as an Impediment to Labor Market Success: Why Do Young Women Report Greater Harm?" *Journal of Labor Economics*, October 2000, *18*(4), pp. 702–28.

Barbezat, Debra A. and Hughes, James W. "Sex Discrimination in Labor Markets: The Role of Statistical Evidence: Comment." *American Economic Review*, March 1990, *80*(1), pp. 277–86.

Blau, Francine D. and Kahn, Lawrence M. "Swimming Upstream: Trends in the Gender Wage Differential in 1980's." *Journal of Labor Economics*, January 1997, Pt. 1, *15*(1), pp. 1–42.

Bound, John and Johnson, George. "Changes in the Structure of Wages in the 1980's: An Evaluation of Alternative Explanations." *American Economic Review*, June 1992, *82*(3), pp. 371–92.

Bound, John and Freeman, Richard B. "What Went Wrong? The Erosion of Relative Earnings and Employment among Young Black Men in the 1980's." *Quarterly Journal of Economics*, February 1992, *107*(1), pp. 201–32.

DeLeire, Thomas. "The Wage and Employment Effects of the Americans with Disabilities Act." *Journal of Human Resources*, Fall 2000, *35*(4), pp. 693–715.

Dertouzos, James N. and Karoly, Lynn A. "Labor Market Responses to Employer Liability." RAND Report: R-3989-ICJ, 1992.

Donohue, John J. and Siegelman, Peter. "The Changing Nature of Employment Discrimination Litigation." *Stanford Law Review*, May 1991, *43*(5), pp. 983–1033.

Farber, Henry S. and Gibbons, Robert. "Learning and Wage Dynamics." *Quarterly Journal of Economics*, November 1996, *111*(4), pp. 1007–47.

Gladden, Tricia and Taber, Christopher. "Wage Progression Among Low Skilled Workers," in David E. Card and Rebecca M. Blank, eds., *Finding jobs: Work and welfare reform*. New York: Russell Sage Foundation, 2000, pp. 160–92.

Goldin, Claudia. *Understanding the gender gap*. Oxford: Oxford University Press, 1990.

Heckman, James J. and Willis, Robert J. "A Beta-logistic Model for the Analysis of Sequential Labor Force Participation by Married Women." *Journal of Political Economy*, February 1977, *85*(1), pp. 27–58.

Hoyman, Michele and Stallworth, Lamont. "Suit Filing by Women: An Empirical Analysis." *Notre Dame Law Review*, October 1986, *62*(1), pp. 61–82.

Katz, Lawrence F. and Murphy, Kevin M. "Changes in Relative Wages, 1963–1987: Supply and Demand Factors." *Quarterly Journal of Economics*, February 1992, *107*(1), pp. 35–78.

Morgan, Phoebe A. "Risking Relationships: Understanding the Litigation Choices of Sexually Harassed Women." *Law and Society Review*, April 1999, *33*(1), pp. 67–91.

Moulton, Brent R. "Random Group Effects and the Precision of Regression Estimates." *Journal of Econometrics*, August 1986, *32*(3), pp. 385–97.

Nager, Glen D. and Broas, Julia M. "Enforcement Issues: A Practical Overview." *Louisiana Law Review*, July 1994, *54*(6), pp. 1473–86.

Neumark, David and Stock, Wendy A. "Age Discrimination Laws and Labor Market Efficiency." *Journal of Political Economy*, October 1999, *107*(5), pp. 1081–125.

O'Neill, June and Polachek, Solomon. "Why the Gender Gap in Wages Narrowed in the 1980s." *Journal of Labor Economics*, January 1993, Pt. 1, *11*(1), pp. 205–28.

Oyer, Paul and Schaefer, Scott. "Layoffs and Litigation." *RAND Journal of Economics*, Summer 2000, *31*(2), pp. 345–58.

Posner, Richard A. "The Efficiency and the Efficacy of Title VII." *University of Pennsylvania Law Review*, December 1987, *136*(2), pp. 513–21.

Robinson, Robert K.; Allen, Billie Morgan; Terpstra, David E. and Nasif, Ercan G. "Equal Employment Requirements for Employers: A Closer Review of the Effects of the Civil Rights Act of 1991." *Labor Law Journal*, November 1992, *43*(11), pp. 725–34.

Selmi, Michael. "Family Leave and the Gender Wage Gap." *North Carolina Law Review*, March 2000, *78*(3), pp. 707–82.

Shaw, Kathryn. "The Persistence of Female Labor Supply: Empirical Evidence and Implications." *Journal of Human Resources*, Spring 1994, *29*(2), pp. 348–78.

Spence, A. Michael. "Job Market Signaling." *Quarterly Journal of Economics*, August 1973, *87*(3), pp. 355–74.