Litigation Costs and Returns to Experience*

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Abstract

We model the relationship between maximum damage awards available to plaintiffs in wrongful termination lawsuits and the returns to experience among protected workers. Firms learn workers' abilities over time, so younger workers are more likely to be terminated. As a result, increases in maximum damage awards may increase the returns to experience by making younger workers more expensive to employ. This effect is mitigated, however, if older workers are more likely to sue conditional on being fired. This reasoning suggests a relationship between damage awards, workers' propensity to sue as a function of experience, and returns to experience. The passage of the Civil Rights Act of 1991 (CRA91) provides an opportunity to assess this relationship empirically. We use data on Equal Employment Opportunity Commission (EEOC) filings to compare rates at which blacks and women file complaints, and examine changes in returns to experience for these groups using the Current Population Survey. The EEOC data indicate that wrongful termination complaint rates among women drop sharply with age, while complaint rates for blacks increase slowly with age. We find an increase in the returns to experience for women following the passage of CRA91, while the returns to experience for blacks are unchanged. Our analysis suggests that employers' reactions to employment protections may induce redistributive effects, and that these effects operate not merely *across* groups of differing protected status, but also *within* groups of identical protected status.

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1 Introduction

Recent work studying the effects of anti-discrimination 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, for example, DeLeire (1997) and Acemoglu and Angrist (1998)). Ignored in this literature, however, is the possibility that anti-discrimination 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 relationship between litigation costs and returns to experience. The central features of this model are that employers and workers are initially symmetrically uninformed regarding workers' abilities, and that information regarding ability is revealed over time. Since abilities of more experienced workers are known with greater precision and firms seek to fire those workers whose abilities are far below expectations, workers become less likely to be displaced as they gain experience. Thus, if damage awards available to plaintiffs in wrongful termination cases increase, inexperienced workers may become more expensive to employ relative to experienced, since inexperienced workers are most likely to be terminated. This effect suggests that employers may react to increased litigation costs by steepening their age/wage profiles.

A potentially offsetting effect is that, conditional on being fired, experienced workers may be more likely to sue. Since back (and some future) wages often comprise one component of damage awards, experienced workers may have a greater incentive to sue. We therefore argue that the relationship between litigation costs and returns to experience is partially determined by how the propensity to sue varies with experience. We show that if the likelihood of filing a suit conditional on being employed decreases with experience, then increases in minimum damage awards will increase the return to experience. If, on the other hand, the propensity to sue increases with experience, then the effect of increases in damage awards on returns to experience is ambiguous. 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 since juries are thought to be partial to claims of individuals over those of firms. Finally, the Act made it marginally easier for displaced employees to prove discrimination on the basis of statistical evidence.

We proceed by looking for relationships between the propensity to sue as a function of age and changes in the return 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 significantly increased the returns to experience for women, but not for blacks. 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 anti-discrimination protections do appear to have redistributive effects, and that these effects 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 may have been a contributing factor in the observed increase in returns to experience over that period.[1]

[1] Donohue and Siegelman (1991) and Oyer and Schaefer (1999) document the growth in employment discrimination litigation from 1970-1994. Katz and Murphy (1992) and Bound and Johnson (1992) show that the returns to experience increased for all workers during the 1970s and 1980s, while Bound and Freeman (1992) show that this effect during the 1980s was stronger for blacks than for whites. Neumark and Stock (1999) also consider the effects of discrimination laws on returns to experience over the 1980s, but from a very different perspective. They examine effects of 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.

2 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 Acts of 1866 and 1964 (Title VII), the Age Discrimination in Employment Act (ADEA), and the Americans with Disabilities Act (ADA).[2] CRA91 also counteracted several 1989 Supreme Court decisions on discrimination cases, notably *Wards Cove Packing Co. v. Atonio* and *Patterson v. McLean Credit Union.*

CRA91 allows employees who claim intentional race- or gender-based discrimination to sue for punitive damages. Damage awards under Title VII were limited to back pay. Maximum compensatory and punitive 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. The Act also explicitly extends the Civil Rights Act of 1866, which allows plaintiffs alleging *racial* discrimination to sue for unlimited punitive damages, to cover both on-the-job activities and termination of employment. Earlier Supreme Court decisions (in particular, *Patterson*) had limited the applicability of the CRA of 1866 to the formation of employment relationships. Hence, CRA91 effectively removed all limits on damage awards in cases of racial discrimination in termination. Together, these features of CRA91 significantly increase the damage awards available to plaintiffs and make litigation a more attractive option for displaced workers. 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.

CRA91 also strengthened a plaintiff's ability to use statistical evidence to prove unlawful discrimination on the basis of "disparate impact." A series of influential 1970s 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 significantly more difficult for plaintiffs to prove disparate impact by requiring plaintiffs to demonstrate that a particular employment practice led to the differing effect on protected groups. CRA91 weakens this standard, allowing plaintiffs to use statistical evidence in cases where the plaintiff can show the employer's decision-making process cannot be separated into specific prac-

[2] Geslewitz (1992) provides a more detailed description of the Act's provisions, while 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. DeLeire (1997) and Acemoglu and Angrist (1998) evaluate the labor market effects of the ADA.

tices.[3]

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 complaints filed with the EEOC increased by 13% from 1991 to 1994 and the monetary benefits awarded in race-based cases resolved by the EEOC increased by 47% over that period. The number of gender-based complaints with the EEOC and the total benefits awarded increased by 46% and 87%, respectively. In addition, the Act appears to have affected firms' decisions as to how to displace employees; Oyer and Schaefer (1999) document changes in employers' firing practices around the time of the Act.

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 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, since 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 1980s away from hiring-based employment discrimination litigation and toward termination-based suits. Our own examination of EEOC data from the 1990s reveals that this trend has continued. Of all gender- and race-based complaints filed with the EEOC between 1992 and 1998, 58% claimed wrongful termination, 5.6% were for discriminatory hiring, with the rest based on on-the-job practices such as unequal pay, denial of promotion, or harassment.[4]

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[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) and Nager and Broas (1994)). Since plaintiffs must show disparate treatment in order to earn punitive and compensatory damages, and since CRA91 greatly increased the potential sizes of the these awards, relatively few suits filed since 1991 have alleged disparate impact.

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

3 A Model of Litigation Costs and Returns to Experience

We first develop a stylized model of the relationship between increased employment-discrimination litigation costs and the returns to experience for protected workers. The model features a number of simplifying assumptions, but the main intuition underlying the relationship between litigation costs and returns to experience carries over to a richer, more realistic model. Consider a discrete-time, overlapping-generations framework 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. Our central assumption is that firms and workers are symmetrically uninformed regarding worker ability and that, over time, information regarding a worker's ability is publicly revealed.

3.1 Workers

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 chooses whether to work or not. If the worker accepts a job, he begins working and a public signal regarding the worker's ability is immediately revealed. Workers are of either high or low ability, and the fraction of high-ability workers in each cohort is fixed at ϕ_1. The public signal, s_1, takes a value from the set $\{L, H\}$. We suppose

$$\begin{aligned} \text{Prob}[s_1 = L \,|\, \text{low ability}] &= \alpha \\ \text{Prob}[s_1 = L \,|\, \text{high ability}] &= 0. \end{aligned}$$

In words, conditional on a worker being of low ability, he is revealed as such in the first period of his life with probability α. We assume the marginal productivity of a low-ability worker is always zero, and that wages are sufficiently high that workers revealed to have low ability are immediately fired. We also assume wages are paid at the end of the period, so that a fired worker earns no wages in the period he is fired. Workers who are not fired after the realization of s_1 continue to work all throughout the first period of life, are paid the period t inexperienced-worker wage w_{1t}, and participate in the labor market again during the second period of life.[5] At the beginning of

[5] A perhaps more realistic assumption is that low-ability workers are fired after fraction ρ of the first period has elapsed. Fired workers then earn wages ρw_{1t} and forfeit the remainder $(1 - \rho)w_{1t}$. Proceeding under this assumption yields a model that is notationally more complex than the model we present, without offering any

the second period of life, the worker again participates in the labor market. If the worker accepts a job, then a second signal of ability, s_2, is publicly revealed. We assume s_2 behaves identically to s_1, so that the probability $s_2 = L$ when the employee's ability is low (high) is α (0). As was the case in the first period, workers who are revealed to be of low ability are fired, while any worker who is not revealed to be of low ability is retained and paid wage w_{2t}.[6] A fired worker has the option of filing suit against his former employer. A worker fired in period $i \in \{1, 2\}$ of his life draws a personal cost of suing c from a density characterized by the cumulative distribution function G_i. The worker sues if the expected damage award conditional on filing a suit exceeds c. We tailor our analysis to fit the pre- and post-CRA91 legal environments by allowing fired workers to sue for wages in the period they are fired and some punitive and compensatory damages. A worker who files a suit against his employer wins with exogenous probability q and, conditional on winning, earns damages of amount $w_i + d$. A fired worker therefore sues with probability $G_i[q(w_i + d)]$.[7]

3.2 Firms

We assume a firm's revenue in a given period depends 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. Among experienced workers, a higher fraction have high ability, since $\alpha(1 - \phi_1)$ low-ability workers are screened out of the labor market after this cohort's first period of employment. Since $(1 - \alpha)(1 - \phi_1)$ low-ability workers remain in the labor market into the second period of life, the probability that an experienced worker has high ability is given by $\phi_2 = \phi_1/(1 - \alpha(1 - \phi_1))$. 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 use $\gamma \geq 1$ to capture the possibility that experience improves a worker's 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 $\alpha(1 - \phi_1)$ of these workers are fired during period t, while fraction $1 - \alpha(1 - \phi_1)$ are retained through the period. To the retained workers, the firm pays wage w_{1t}. Fraction $G_1[q(w_{1t} + d)]$ of the fired workers file suits for unlawful

additional insights.

[6] The assumption that workers are either high or low ability and that low-ability workers have zero productivity is made for ease of exposition. Similar results can be obtained from a model in which employees' abilities are continuous and firms cannot adjust wages downward to match productivity.

[7] Our modeling of the mechanisms by which workers litigate is similar to that of Acemoglu and Angrist (1998).

termination. We assume the marginal 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 $q(w_{1t}+d)+k$. 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:

$$\phi_1 R' = \Big(1-\alpha(1-\phi_1)\Big)w_{1t} + \alpha(1-\phi_1)G_1[q(w_{1t}+d)]\Big(q(w_{1t}+d)+k\Big) \tag{1}$$

$$\gamma\phi_2 R' = \Big(1-\alpha(1-\phi_2)\Big)w_{2t} + \alpha(1-\phi_2)G_2[q(w_{2t}+d)]\Big(q(w_{2t}+d)+k\Big). \tag{2}$$

In a steady-state equilibrium, all workers except those who have been shown to have low ability are employed, so we require that $MI_{mt}=1$ and $ME_{mt}=1-\alpha(1-\phi_1)$.

3.3 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 workers who have high ability, and (iii) differences in the expected costs of litigation. We discuss each in turn, and then ask how changes in employment discrimination law 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* shown to be of low ability in their first period of employment. While some experienced workers are low ability (as long as $\alpha<1$), the share of experienced workers with high ability is $\phi_2>\phi_1$. Since low-ability workers have zero marginal productivity, the fact that experienced workers are more likely to have high ability means greater demand and higher wages for these workers. Third, experienced and inexperienced workers differ in expected costs associated with 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

$$\alpha(1-\phi_i)G_i[q(w_{it}+d)]\Big(q(w_{it}+d)+k\Big). \tag{3}$$

Breaking this expression into three parts, we identify three distinct effects of experience on expected litigation costs. The first term in (3), $\alpha(1-\phi_i)$, represents a *displacement effect*: since $\phi_1>\phi_2$, inexperienced workers are more likely to be displaced, and hence more likely to have the option of filing suit. The displacement effect therefore works in direction of higher litigation costs for inexperienced workers. We refer to the second term, $G_i[q(w_{it}+d)]$, as a *litigation effect*: conditional on being fired, the probability a worker sues depends on both the wages that can be recovered and the the distribution, G, of costs of filing a suit. Wages are typically higher for experienced workers, but we have no expectation as to how G may vary with experience. This litigation effect may work in the direction of either higher or lower litigation costs for inexperienced workers. The third term, $q(w_{it}+d)+k$, reflects a *damages effect*. Since damage amounts are positively related to wages, this effect works in the direction of lower litigation costs for inexperienced workers. In general, therefore, the relationship between expected litigation costs and experience is indeterminate: learning implies that inexperienced workers are more likely to be fired, but higher wages mean larger expected damage awards for experienced workers. We next ask how the Civil Rights Act of 1991 may affect returns to experience. Whereas prior to the law, damage awards were limited to back pay and minimal compensatory damages, post-1991 plaintiffs can earn both punitive and compensatory damages. We model this change as an increase in d.[8] An increase in d clearly raises the cost of employing both inexperienced and experienced workers, which reduces wages for both cohorts. In order to determine how the return to experience is affected, we ask where the cost increase is larger, as wages for this group will be depressed relative to the other group. For a worker employed in period i of his life, the derivative of expected litigation cost with respect to d is

$$\alpha(1-\phi_i)\Big(qg_i[q(w_{it}+d)]\Big(q(w_{it}+d)+k\Big)+qG_i[q(w_{it}+d)]\Big), \tag{4}$$

where g_i is the probability density function associated with the cumulative distribution function G_i. An increase in d affects both the litigation effect and the damages effect: employees who are fired become more likely to litigate, and employees who litigate earn larger expected damages.

To assess the effect of the Act on returns to experience, we ask whether the expression in (4) is larger for experienced or inexperienced workers. We begin by noting that the first term in (4) — the probability a worker is fired — is unambiguously larger for inexperienced workers. The second term in (4) — the rate at which the expected costs of firing a worker increases as d increases — could be larger for either experienced or inexperienced workers. To see this, note that if G_1 and G_2 are identical uniform distributions on $[0,\overline{c}]$, then this term will be larger for experienced workers. Since experienced workers are more likely to sue in this case, the increase in potential

[8] We can obtain similar results focusing on the provision of the CRA that allows either side to seek a jury trial. Since juries are perceived to favor the claims of individuals, we model this as an increase in q, the likelihood a suit is successful.

damages is more costly for these workers. Also, since experienced workers earn higher wages (and therefore higher damages if they win a suit), the increase in the propensity to sue conditional on being fired that is generated by the increase in d is more costly for experienced workers. However, if inexperienced workers are more likely to sue conditional on being fired (so that $G_1[q(w_{1t}+d)] > G_2[q(w_{2t}+d))]$, or if the increase in the propensity to sue stemming from an increase in d is larger for inexperienced workers ($g_1[q(w_{1t}+d)] > g_2[q(w_{2t}+d)]$), then the second term in (4) may be larger for inexperienced workers.[9] While we do not obtain unambiguous comparative statics regarding the relationship between CRA91 and returns to experience, our model does point out several relationships that we can examine in the data. For example, it is clear from our discussion that if $\alpha(1-\phi_i)G_i[q(w_{it}+d)]$ — that is, the likelihood an employee sues conditional on being employed — decreases with experience, then (4) will be decreasing in i (as long as g_2 is not significantly greater than g_1) and passage of CRA91 will be associated with an increase in the return to experience. Similarly, if $\alpha(1-\phi_i)g_i[q(w_{it}+d)]$ — the increase in the propensity to sue conditional on being employed associated with increased damage awards — decreases with experience, then returns to experience should increase. Our analysis therefore suggests that in order to understand how CRA91 affects returns to experience, we must first examine how complaint rates vary with age among protected workers, and how the response of complaint rates to the Act varies with age.

We have, for ease of presentation, assumed labor supply is perfectly inelastic, but removal of this restriction does yield one additional implication. Under the assumptions that (i) labor supply is somewhat elastic, (ii) experienced and inexperienced workers are substitutes in the production process, and (iii) the increase in expected litigation costs associated with an increase in d is larger for inexperienced workers, then it is possible to construct examples in which w_{2t} increases in response to the increase in d. 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* — that is, $(w_{1t}+w_{2t})/2$ — may not be greatly affected by increases in d. However, this does not mean that protected workers are not harmed; since wages are redistributed from younger to older workers, the present value of lifetime earnings for a given worker, $w_{1t}+\beta w_{2t+1}$, falls. This suggests that studies examining the effects of employment protections on *average wages* without also considering how protections may *redistribute wages* may be missing part of the effect.

[9] As an example of distributions of G_1 and G_2 that could yield this outcome, suppose G_i is uniform on $[0,\bar{c}_i]$. If $\bar{c}_1 < \bar{c}_2$, then by construction $g_1[q(w_{1t}+d)] > g_2[q(w_{2t}+d)]$, while if $\bar{c}_1$ is sufficiently small relative to $\bar{c}_2$, then $G_1[q(w_{1t}+d)] > G_2[q(w_{2t}+d)]$. One reasonable justification for this set of assumptions regarding G_1 and G_2 is that the opportunity cost of a more experienced worker's time is higher, on average.

4 Determinants of 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 full-time employed protected workers who file wrongful termination complaints, and compute how this share varies with age.[10] 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% 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.[11] 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 20 to 40 year-old white women and 118,779 race-based cases brought by 20 to 40 year-old black men. Of these, a total of 149,489 (64.4%) were wrongful termination cases and comprise our final sample.[12] We use the Annual Demographic File of the March CPS to estimate the number of full-time employed white women and full-time employed black men of each age between 20 and 40 in each year between 1988 and 1995 (where a worker is full-time if he or she reported working at least thirty hours in the

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

[11] Approximately 18% of gender-based cases are brought by men. Approximately 80% of race-based cases are brought by blacks, 10% 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% of the complaints in the age and basis groups that we analyze claim a single basis. Our results are not affected by excluding complaints with multiple bases.

[12] There are at least two limitations to our analysis of EEOC data. First, the age of the plaintiff is missing for approximately 30% 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 when we have to discard over a quarter of our data. 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 wrongful termination suits, so it is unclear how representative EEOC complaints are of all post-CRA91 race-based discrimination complaints.

week before the survey). We create counts of full-time 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 full-time employee of a given age and in a given year files a wrongful termination complaint. Figures 1(a) and 1(b) show the complaint rates by age for full-time 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 2,000 to 2,500 full-time white women, but the proportion is one out of 300 to 500 for black men. Also, as suggested in Section 2, 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 or the fact that CRA91 made it possible for plaintiffs to bypass the EEOC in filing race-based suits. Alternatively, the black complaint rate may not have increased as much since it was much higher to begin with.

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 30s. The complaint rates in 1990 and 1993 were 0.362 and 0.487, respectively, per 1000 for white women in their 20s, but only 0.267 and 0.378 per 1000 for white women in their 30s. Over the full 1988-91 (1992-95) period, the yearly complaint rate was 0.311 (0.473) for 25-year-old white women and 0.234 (0.399) for 35-year-old white women. This pattern 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 2.36 and 2.72, respectively, per 1000 for black men in their 20s, but 2.69 and 3.04 for those in their 30s. Similarly, over the full 1988-91 (1992-95) period, the EEOC received 1.99 (2.17) complaints per 1000 25-year-old black men per year and 2.71 (2.96) per 1000 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 3, 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 full-time 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.[13]

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. Since propensity-to-sue does not trend downward with age for black men, our analysis does not suggest a definitive prediction for this group.

5 Empirical Analysis of Labor Market Outcomes

5.1 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 30 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.[14] 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,

[13] Examining the set of all non-hiring-based EEOC complaints yields similar age trends.

[14] The maximum annual earnings in the CPS is $99,999, so our wage variable is right-censored. However, this only affects 1.5% 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% of the observations in our wage regressions are top-coded.

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

	Entire Sample	Pre-CRA91	Post-CRA91	White Men	White Women	Black Men
	(1)	(2)	(3)	(4)	(5)	(6)
Total Observations	254,320	150,275	104,045	118,091	122,968	13,261
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	5.9%	5.9%	5.8%	5.9%	5.9%	6.1%
	(1.6%)	(1.7%)	(1.5%)	(1.6%)	(1.6%)	(1.5%)

Data from 1988-96 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 non-zero observations. Standard deviations in parentheses.

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, and keeps the comparison group (white men) clean by removing ADEA-protected workers.[15] We define 1987-1991 observations as "pre-CRA91" and 1992-1996 observations as "post-CRA91."[16] Summary statistics in Table 1 show that roughly 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

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

[16] 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.

returns to experience before and after the law. The critical assumption needed for our analysis is that, in the absence of CRA91, returns to experience would not have changed in a way that differed by race or gender over the 1988-95 period. 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.

5.2 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.[17] We measure the effect of CRA91 by estimating

$$y_{it} = \alpha + \alpha_p d_p + \sum_{t=87}^{95} \beta_t (d_t * d_p) + \boldsymbol{\delta} \mathbf{x}_i + \varepsilon_{it}, \tag{5}$$

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 (5) to

$$y_{it} = \alpha + \alpha_p d_p + \beta_{\text{post}} (d_{\text{post}} * d_p) + \boldsymbol{\delta} \mathbf{x}_i + \varepsilon_{it} \tag{6}$$

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. We then 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, 5-year age categories, one-half percentage point intervals in state unemployment rates, and interactions between state unemployment and protected status indicators. If log wage is the dependent variable, then $\mathbf{x}_i$ includes age and age squared, as well as indicators

[17] The Act did affect the legal status of disabled white men. However, since fewer than five percent of workers in the age group we study are disabled (Acemoglu and Angrist (1998)), we do not expect this to have a significant impact on our analysis.

for year, state, education, state unemployment rate and unemployment rate/protected status interactions. Some specifications also include year/protected status interactions to allow for separate trends in employment outcomes for protected and unprotected workers.

Panel A of Table 2 and Figure 2 display the results of estimating Equations (5) and (6) 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 (5), while the table reports coefficients from estimation of Equation (6). 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 (about 700 fewer per year), and that they earn less (about 27% 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% more for women than men over the pre-CRA91 period to the post-CRA91 period, while annual hours worked rose by 13 hours more for women than for men.

These effects cannot be attributed to CRA91, however. When we control for a female-specific trend in hours or wages (as in Columns 2, 4 and 6), the pre/post difference becomes negligible. The post-CRA91 effect is positive and significant in the employment probit, even when controlling for a time trend in female labor force participation, but Figure 2(a) makes it apparent that the beginning of 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 3 and Figure 2 indicate that CRA91 had at most a mild negative effect on employment for white women and no measurable effect on hours or average wages.

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 does not suggest a very strong overall effect of CRA91 on average black employment outcomes. There is no indication that black employment changed as a result of CRA91, as the black/post interaction term in the first two columns of Panel B of Table 2 is not remotely significant. Also, Figure 3(a) reveals no distinguishable difference between pre- and post-CRA91 employment levels. Columns 3 and 4 and Figure 3(b), however, suggest that CRA91 lowered black hours worked by about one per week. After trending up through 1991, black hours worked dropped relative to whites starting in 1992. This effect is statistically significant when controlling for a linear trend in hours worked and represents about a 2% decline in black hours worked. The last two columns of the table and Figure 3(c) show similar, though only marginally significant, effects of CRA91 on black wages.

Table 2: Changes in Protected Workers' Employment Outcomes Post-CRA91

Panel 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	-0.7647	-1.0302	-707.91	-1332.63	-0.2720	-1.1404
	(0.0205)	(0.4163)	(11.68)	(250.83)	(0.0088)	(0.1877)
	[-0.2649]	[-0.3509]				
Female*Post-CRA91	0.0673	0.0538	13.21	-17.68	0.0420	-0.0010
	(0.0117)	(0.0242)	(7.24)	(14.35)	(0.0054)	(0.0107)
	[0.0236]	[0.0189]				
Female*Linear Trend		0.0030		6.95		0.0097
		(0.0047)		(2.79)		(0.0021)
		[0.0011]				
Observations	241,059	241,059	241,059	241,059	156,552	156,552
Log-Likelihood/R^2	-140,226	-140,226	0.1800	0.1800	0.2475	0.2476

Panel 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	-0.4506	-1.8987	-432.98	-1616.99	-0.2247	-0.0540
	(0.0424)	(0.9310)	(22.35)	(528.10)	(0.0182)	(0.3973)
	[-0.1456]	[-0.6566]				
Black*Post-CRA91	0.0067	-0.0674	-4.83	-62.46	- 0.0010	0.0073
	(0.0262)	(0.0543)	(15.23)	(29.86)	(0.0120)	(0.0237)
	[0.0019]	[-0.0197]				
Black*Linear Trend		0.0163		13.15		-0.0019
		(0.0105)		(5.86)		(0.0047)
		[0.0046]				
Observations	131,352	131,352	131,352	131,352	100,578	100,578
Log-Likelihood/R^2	-65,340	-65,339	0.0738	0.0738	0.2133	0.2133

Data from 1988-1996 March CPS, limited to black men and non-Hispanic white men and women aged 25-39. Panel A (Panel B) compares white women to white men (black men to white men). Full-Time Employment equals 1 if individual reports working 30 or more hours in the week before the CPS interview. Columns 1-2 are probits and include indicators for high school and college graduation, 5-year age categories, state, year, one-half percentage point intervals in state unemployment rate, and interactions between state unemployment and black or female. Bracketed terms are probability derivatives or estimated change in probability that the dependent variable takes value 1. Hours Worked is the product of avg. weekly hours and number of weeks worked over the preceding year. Columns 3-4 are OLS and include the same controls as in Columns 1-2. Log Wage is the log of avg. hourly wage for the year. Columns 5-6 are OLS limited to individuals working 1500 or more hours during the specified year and include controls for experience, experience squared, education, and indicators for state, year, and one-half point state unemployment rate intervals and interactions between state unemployment and black or female. Coefficients on "Female" and "Black" are for the pre-CRA91 period with state unemployment rate of six percent. Standard errors are in parentheses.

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.

5.3 CRA91 and Returns to Experience

While average wages for protected groups appear to be unchanged as a result of CRA91, our analysis suggests that the Act may alter the return 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 1990s. In our stylized model of Section 3, 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 is also revealed from the frequency and length of non-employment 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 work force experience. Unfortunately, the CPS does not contain enough historical employment information about individual respondents to allow us to measure actual experience. We can, however, use historical CPS data to derive a proxy for actual experience. Applying a method similar to one used by Gladden and Taber (1998), 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 diploma, 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.[18] To examine the effects of CRA91 on returns to experience, we estimate

$$w_{it} = \alpha + \alpha_p d_p + \sum_{t=87}^{95} \beta_t (d_t * d_p * e_{it}) + \boldsymbol{\delta}\mathbf{x}_i + \varepsilon_{it}, \qquad (7)$$

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.[19] As above, some specifications include interactions between a linear

[18] The standard errors in our individual-level regressions using cell average experience are likely to be somewhat understated since cell-average experience is actual experience plus unobserved noise. See Moulton (1986).

[19] To insure that our results do not reflect the interaction between experience and other variables, we re-ran our analysis with controls for experience interacted with education and with the state unemployment indicators. This

year 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

$$w_{it} = \alpha + \alpha_p d_p + \beta_{\text{post}}(d_{\text{post}} * d_p * e_{it}) + \boldsymbol{\delta}\mathbf{x}_i + \varepsilon_{it}. \tag{8}$$

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

5.3.1 Women

We estimate Equations (7) and (8) 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 (8), while in Figure 4(a), we plot the β_t coefficients obtained when estimating Equation (7). In Column 1, we use potential experience and obtain an estimate of β_{post} of 0.0031, which is significant at better than the two 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% higher after CRA91 than before. Plots of the individual year effects in Figure 4(a) 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 cell-average experience (see Columns 3 and 4) also show that women's returns to experience increased following CRA91. The estimate reported in column 4 indicates that the premium to being a woman whose cell averaged 5 years of experience more than another group increased by 4% after CRA91. Given that cell-average experience increases more slowly with age than potential experience, the estimates using both measures are fairly consistent.

From the results in Section 5.2, 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

had a trivial effect on our estimates.

Table 3: Changes in Protected Workers' Returns to Experience Post-CRA91

Panel A: White women compared to white men

Experience Measure	Potential	Potential	Cell Average	Cell Average
	(1)	(2)	(3)	(4)
Female*Experience*Post	0.0031	0.0045	0.0062	0.0084
	(0.0011)	(0.0022)	(0.0023)	(0.0043)
Female*Experience*Trend		-0.0003		-0.0005
		(0.0004)		(0.0009)
Observations	156,552	156,552	155,727	155,727
R^2	0.2540	0.2540	0.2533	0.2533

Panel B: Black men compared to white men

Experience Measure	Potential	Potential	Cell Average	Cell Average
	(1)	(2)	(3)	(4)
Black*Experience*Post	-0.0042	-0.0012	-0.0043	-0.0034
	(0.0025)	(0.0048)	(0.0038)	(0.0074)
Black*Experience*Trend		-0.0007		-0.0002
		(0.0009)		(0.0014)
Observations	100,578	100,578	99,557	99,557
R^2	0.2155	0.2155	0.2151	0.2151

Dependent variable is the log of avg. hourly wage for the year. Data from the 1988-96 March CPS, limited to black men, non-Hispanic white men and women aged 25-39 and working 1500 hours or more in the specified year. Panel A (Panel B) compares white women to white men (black men to white men). Control variables 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 indicators and experience, experience squared, and protected status. Experience is defined as age − education − 6 in columns 1-2, and as the average experience for 1965-1995 March CPS respondents with the same gender, race, education, and year of birth in columns 3-4. Standard errors are in parentheses.

to result in increasing returns to potential experience over the time period we study. While 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 return to experience as if the CRA has been passed in 1989. If increasing female labor market participation leads mechanically to increasing returns to experience, then we would expect to see a pre/post change using 1989 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 focus on data from the 1988-1992 March CPS.[20] We analyze

[20] If the effects of CRA91 were immediate and involved only a discrete shift with no effect on the trend in returns

these data as though a law that might have affected women's returns to experience was enacted at the end of 1989; that is, we treat 1987-1989 as a simulated "pre" period and 1990-1991 as a "post" period. A finding of positive relative changes in return to experience over these "pre" and "post" periods would make it hard to justify connecting the results in Panel A of Table 3 and Figure 4(a) with CRA91. Using both potential and cell-average experience measures, we obtain (but do not report) "post-1989" coefficients that do not significantly differ from zero and are much smaller than the post-CRA91 effects in Panel A of Table 3. The estimated linear trend in women's returns to experience is negative and insignificant using both experience measures. This result is also apparent from examining Figure 4, which shows no upward trend in returns to experience for white women prior to the passage of CRA91. This finding indicates that increasing female returns to experience does not follow mechanically from increasing female labor force participation.

As a second check, we gather data from the NLSY.[21] 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.[22] 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, since 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.

We estimate Equations (7) and (8) 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 (7)) are displayed in Figure 4(b). While each year effect is measured with considerable sampling variance, the overall pattern is similar to obtained from CPS

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, since 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 analysis.

[21] Because we use the NLSY only for comparison purposes, we do not provide background information on this data source. Farber and 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.

[22] In fact, there is reason to suspect that using potential experience is problematic since, in a standard wage regression with our entire CPS sample, the returns to experience are significantly higher for men than for women. This difference vanishes when we use our measure of actual experience from the NLSY.

data in 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 estimates in Table 3 suggest that, all else equal, a 30-year-old woman's wage was 1.5% 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. Since Dertouzos and Karoly (1992) estimate that the costs of damage awards themselves reflect only about 1% 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% 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.

5.3.2 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 (7) and (8) 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 four of our estimates of β_{post} in the table are negative — indicating a reduction 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.[23]

Despite the smaller sample, the NLSY does offer some support for the assertion that returns to experience increased for blacks. When we estimate Equation (8) 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 (7) 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. In general, 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.

5.3.3 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 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 litigation arising from termination would increase the returns to education for younger protected workers relative to older protected workers.[24] We offer a simple test of this assertion by examining how the

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[23] When looking at black employment over the 1988-96 period, it is important to consider the effects of the 1990 and 1991 federal minimum wage increases. We 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.

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

returns to education changed around the time of CRA91. We estimate

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

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 return 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 1980s and 1990s.[25]

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 return to a year of education increased by 1.5% relative to older blacks. Similarly, columns 3 and 4 indicate that returns to education increased by 0.7% 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 this is not overwhelmingly significant from a statistical perspective, it does provide some evidence consistent with employers' use of education as a signal of termination probability.

6 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 cost of displacing 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 the costs of displacing a worker may vary with individuals' litigiousness. If the likelihood of an extreme realization of a worker's productivity decreases as the worker gains experience and if people do not become more prone to file wrongful termination claims

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

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
	(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

Dependent variable is the log of avg. 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 1500 hours in the preceding year. These OLS regressions include controls for potential experience, experience squared, and education, indicator variables for state, year, and half-percentage state unemployment rate categories and year/black or female interactions. Standard errors are in parentheses.

as they age, then the firm's expectation of termination-related costs associated with a particular worker will be decreasing in the worker's experience. Exogenous increases in costs of termination will then increase the returns to experience in the labor market. 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. Our analysis suggests that a law that has fairly negligible average effects on protected groups can have important effects on the age/wage profiles for these workers.

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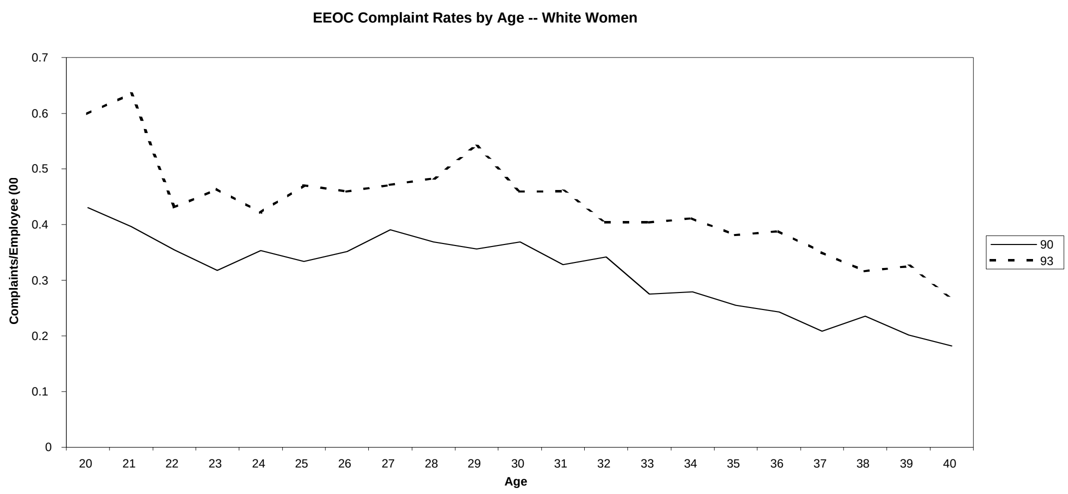

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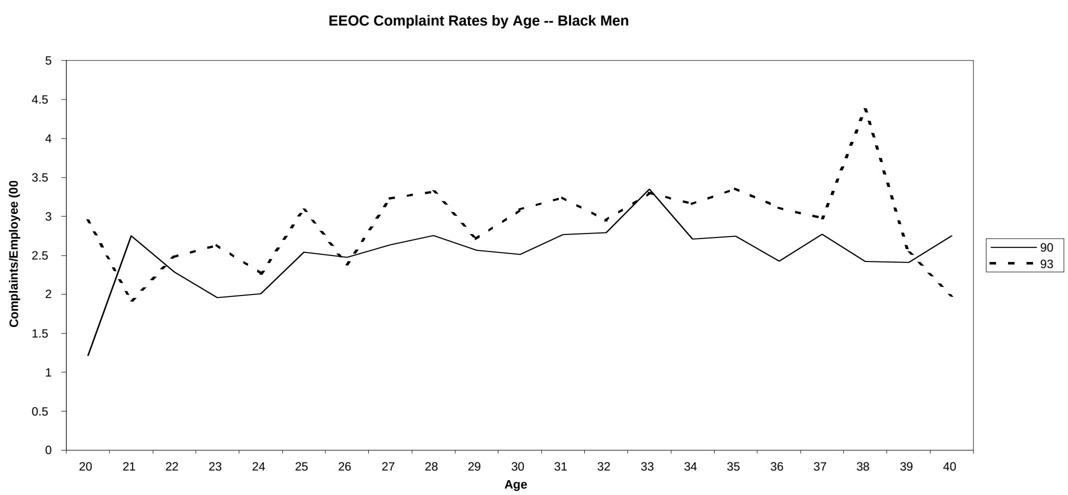

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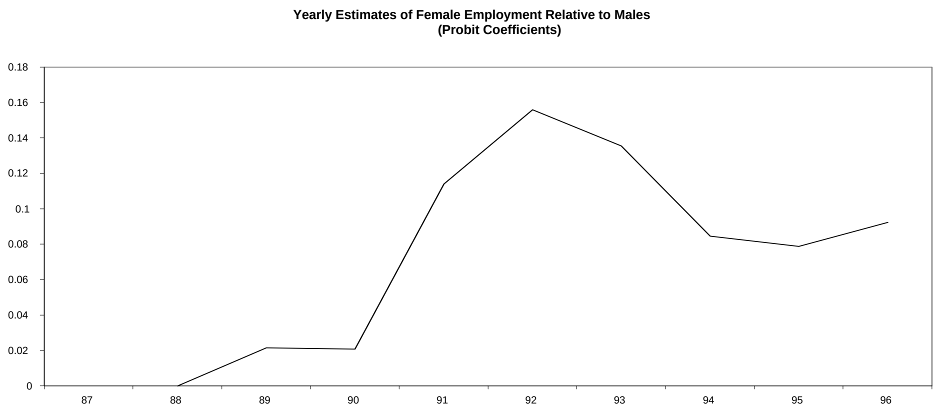

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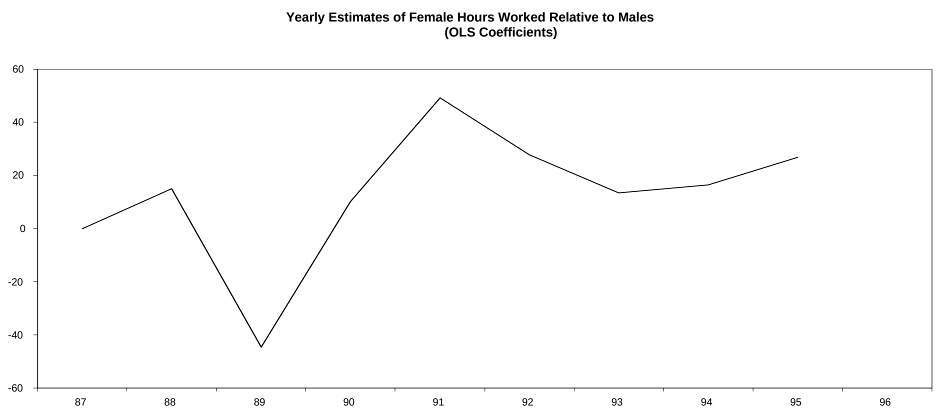

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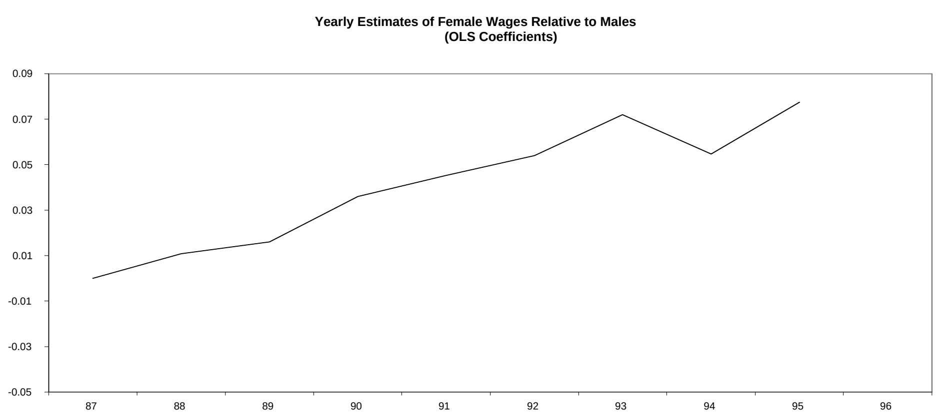

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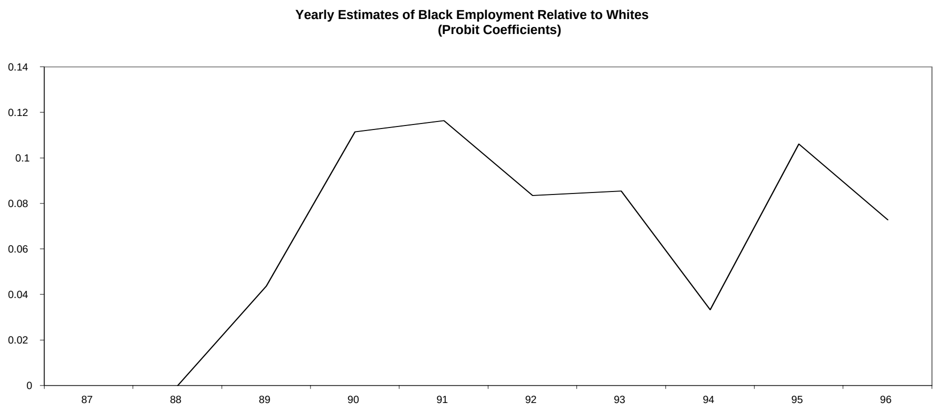

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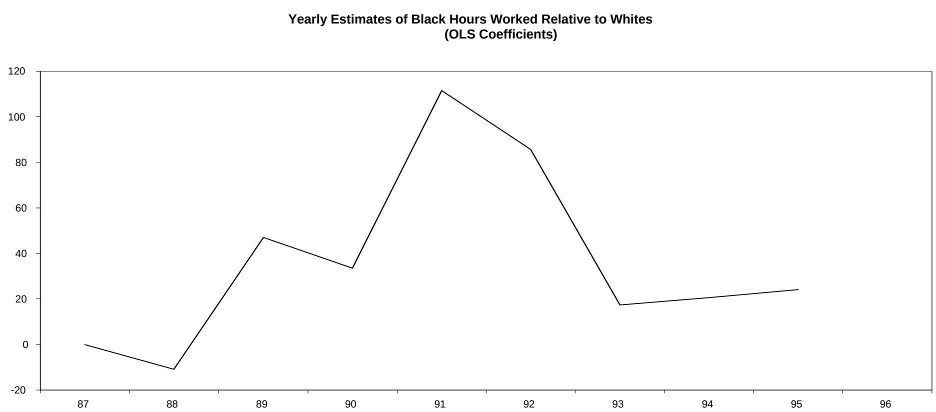

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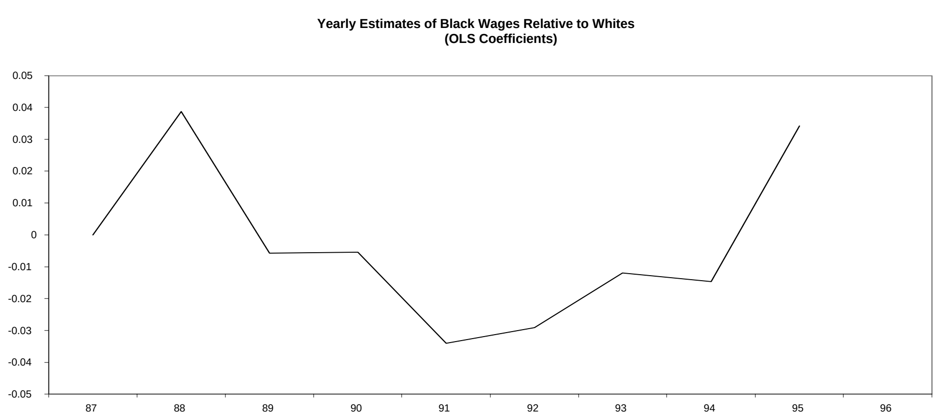

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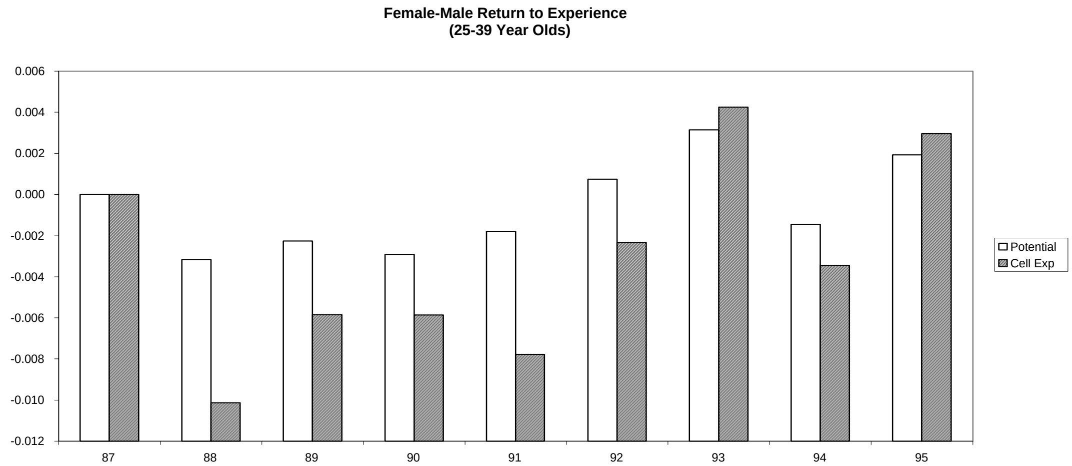

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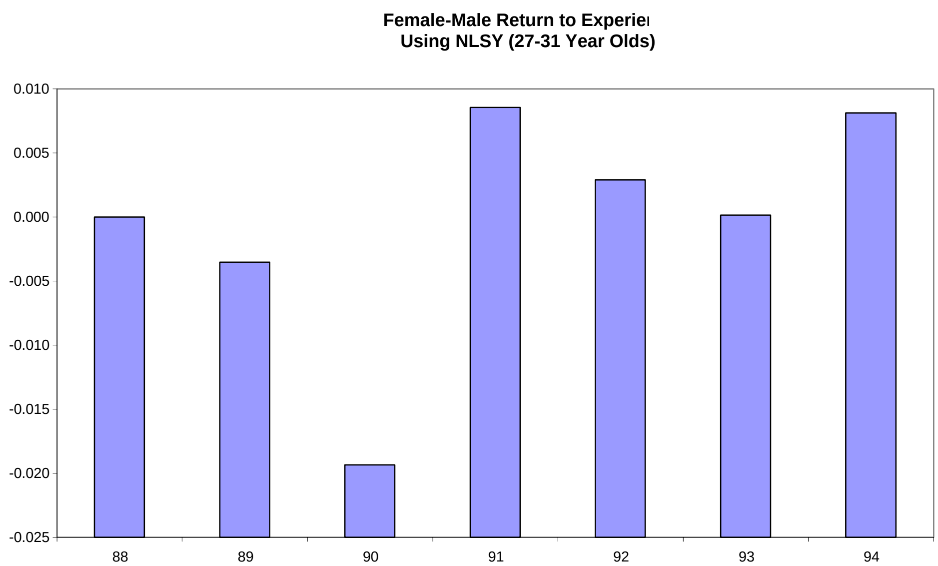

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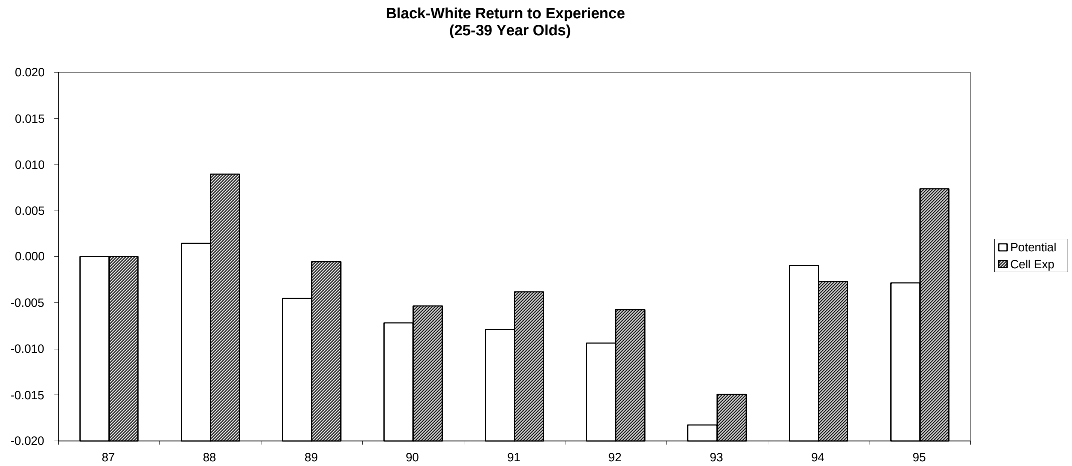

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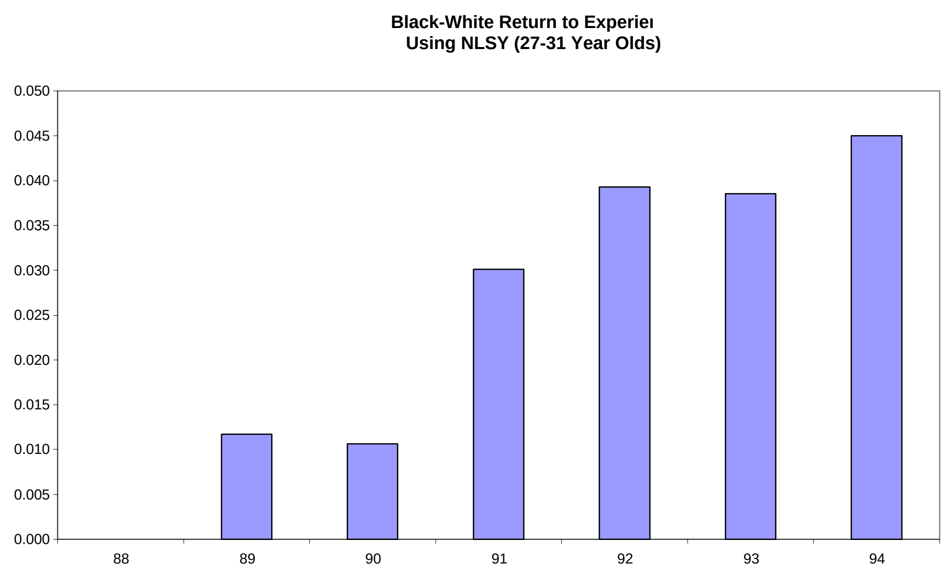

Figure 5b