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Layoffs and litigation

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and

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We study a possible link between two recent U.S. labor market trends: increased wrongful termination litigation and more frequent mass layoffs. We argue that if workers are more likely to sue when fired than when dismissed as part of a layoff, then increases in the expected costs to firms of such suits should induce substitution toward layoffs and away from individual firings. Our empirical analysis supports this assertion, showing that shortly after the passage of the Civil Rights Act of 1991, the methods of displacement changed differently by race but changes to the overall level of displacement were consistent across races.

1. Introduction

■ Firms face major challenges when implementing workforce reductions. Although the costs of continuing to employ too many or unproductive workers are potentially crippling, costs associated with displacing workers can also be significant. Many observers argue that these costs of displacing workers, which can include legal expenses, severance payments, unemployment insurance taxes, and outplacement assistance, have escalated in recent years due to substantial increases in employment discrimination litigation.[1]

In this article we explore how changes in the legal environment may affect firms' choices on how to displace unproductive employees. We focus in particular on a distinction between ''layoffs''—situations where a relatively large number of employees are displaced—and ''firings''—where relatively few employees are displaced. We emphasize this distinction because legal scholars have argued that firms face greater exposure to employment discrimination litigation when dismissing a worker for cause than when dismissing a worker as part of a mass layoff. Donohue and Siegelman (1993) assert that ''it is much more difficult to prove discrimination when 100 workers are

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[1] Donohue and Siegelman (1991) document the growth in employment discrimination litigation throughout the 1970s and 1980s. Using the same data source, we found that the rate of growth in suits increased in the early 1990s.

laid off in a sales slump than when a single worker is fired for some alleged malfeasance'' (p. 747) and ''an individual-specific discharge underlies most suits'' (p. 748). In addition, the incidence of class action suits—cases in which a plaintiff alleges that an entire group of workers has been unlawfully dismissed—has dropped dramatically in recent years. Donohue and Siegelman (1991) note that ''[w]hile individual suits have blossomed, the class action, once a key aspect of the fight for civil rights in the work place, has withered'' (p. 984).[2]

We reason that if a given worker is more likely to sue for unlawful termination when fired for cause than when dismissed as part of a layoff, then laws offering additional employment protections tip the firing/layoff balance in the direction of layoffs. That is, if the likelihood of a lawsuit conditional on firing a given worker is larger than the likelihood of a suit conditional on dismissing the worker as part of the next layoff, then increases in the costs associated with lawsuits will result in a larger increase in the marginal cost of firings compared to the marginal cost of layoffs. We therefore expect firms to substitute away from firings and toward layoffs in response to new employment protection legislation. This reasoning suggests a possible link between two trends that have affected the U.S. labor market over the last decade: increases in wrongful termination litigation and increases in the frequency and size of layoffs at otherwise healthy firms. Of course, many layoffs result from closings of plants or entire firms, and we do not suggest that firms shutter entire operations to avoid the costs of displacing a few workers. However, Farber (1997) documents that although the rate of total involuntary separations did not change substantially over the late 1980s and early 1990s, there was a decade-long increase in the number of workers citing ''position abolished'' or ''other'' (that is, some cause not related to economic conditions) as the cause of their displacement. This finding lends credibility to the widely held belief that there has been an increase in the number of selective downsizings, where some workers are discontinued while others stay at the firm.

To test our assertion that firms substitute toward layoffs when litigation costs increase, we look for evidence of changes in employers' choices over displacement methods around the time of the passage of the Civil Rights Act of 1991 (CRA91). 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 it includes racial minorities, females, anyone over the age of 40, and those with disabilities. Previous federal employment discrimination legislation typically limited plaintiff recovery to lost wages, but CRA91 gives employees with discrimination claims the right to sue for up to $300,000 in punitive damages. Also, 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. Finally, CRA91 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. If CRA91 increases the costs of firing protected workers without comparably affecting the costs of firing unprotected workers, then we expect to observe substitution toward layoffs and away from firings for protected, but not for unprotected, workers.[3]

[2] Our own analysis of federal court records indicates that between 1989 and 1995, far fewer than 1% of employment discrimination suits (representing less than 1% of the total damages claimed) sought class action status. This status was denied in about two-thirds of these cases.

[3] In an earlier version of this article, we showed that if the marginal cost of displacing a protected worker is a concave function of the total number of (protected and unprotected) workers displaced, then firms may reduce individual firings for both types. The direct effect of the law on firings for protected workers can, however, be expected to be larger than this indirect effect on unprotected workers.

Using data from the 1987–1993 Surveys of Income and Program Participation, we assess the effect of CRA91 on firms' human resources management practices. We compare job displacement rates among black men between the ages of 21 and 39, all of whom gained significant legal protections from CRA91, to those of white men between 21 and 39. We find that, relative to whites, rates of overall involuntary displacement of black men were unaffected by the passage of CRA91. However, while black men were significantly more likely to be fired than white men during the pre-CRA91 period, this difference disappeared in the post-CRA91 period. We document that the share of black involuntary displacements coming in the form of firings dropped by around one-third after the passage of the act, while the share of white involuntary displacements coming in the form of firings was unchanged. We obtain similar results when we exploit age and state of residence as two additional sources of variation in protected status among black men. Our empirical results are consistent with the hypothesis that firms substituted toward layoffs and away from firings for protected workers in response to the Civil Rights Act of 1991.

In focusing on human resource management decisions made at the level of the firm, our analysis departs from the large literature studying the relationship between employment discrimination law and labor market outcomes for protected employees.[4] While our analysis shares some empirical methods with the program evaluation literature, our aim is to use changes in employment discrimination law to develop evidence on how displacement costs affect human resource management practices within firms. In asserting that firms' choices over methods of displacement may be affected by the legal environment and emphasizing the distinction between layoffs and firings, we believe our analysis to be unique.

2. The Civil Rights Act of 1991

■ 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). Since the emphasis of our article is on the costs of displacing workers, we focus on three provisions of the law that may significantly affect these costs.[5] First, the law allows employees who claim intentional race or gender discrimination to sue for punitive damages. Before CRA91, 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 up to $300,000 for firms with more than 500 employees. Second, CRA91 explicitly extends the Civil Rights Act of 1866, which allows plaintiffs alleging *racial* discrimination to sue for unlimited punitive damages, to cover both on-the-job activities and termination of employment. Earlier Supreme Court decisions 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. Third, CRA91 gives plaintiffs who seek punitive damages the right to a jury trial. Since juries are perceived to favor claims of individuals over those of corporations, this change may have increased expected damage awards.

We argue that these changes in the legal environment increased the cost to firms of firing protected workers without comparably affecting the costs of firing unprotected

[4] See surveys by Leonard (1990) and Donohue and Heckman (1991). For recent analyses of the ADA, see DeLeire (forthcoming) and Acemoglu and Angrist (1998).

[5] See Robinson, et al. (1992) for a more detailed description of CRA91.

workers. The provisions allowing for jury trials and larger damage awards affect firms' costs of displacing workers in two ways. First, both provisions presumably increase the expected restitution a firm must pay if it is found to have terminated an employee unlawfully. Second, the prospect of larger damage awards raises the expected payoff to a displaced employee from filing a lawsuit, thereby increasing displaced employees' propensity to sue.[6] As displaced employees sue more frequently, it becomes more likely that the firm pays damages and other, indirect costs of employment discrimination litigation.[7] If, as argued by Donohue and Siegelman (1993), employment discrimination claims are more difficult to prove when an employee is terminated as part of a layoff, then we expect the changes associated with CRA91 to lead to substitution away from firings and toward layoffs for protected workers, with no such substitution for unprotected workers.

3. Analysis

■ **Data.** Our data are taken from the 1987 through 1993 Surveys of Income and Program Participation (SIPP). This survey, conducted by the Census Bureau, selects households at random and interviews every member of the household between three and eight times, depending on the survey year. Interviews are conducted every four months and solicit information on up to two jobs per person per interview. While our analysis incorporates many standard economic and employment variables, our focus is on two SIPP interview questions. The first asks (for each of two jobs, if the person held multiple jobs) if the respondent stopped working for the employer during the preceding four-month reference period. The second question, asked only of those who did leave a job, asks whether the person was laid off, retired, discharged, left because the job was temporary, or voluntarily quit. We refer to those who were laid off or discharged as having suffered ''involuntary separation'' and those who were discharged as having been ''fired.''[8]

This method of identifying firings is subject to potential measurement error. Survey respondents may not interpret the terms ''laid off'' and ''discharged'' similarly or may be unwilling to admit to being fired. We check this method of distinguishing firings and layoffs in three ways. First, since employees fired for cause are not generally eligible for unemployment insurance, we compare the unemployment insurance take-up rates across groups. We find that employees who reported being fired were much less likely to collect unemployment insurance than those laid off. Second, since layoffs are often temporary but firings are likely to be permanent, we examine the rates at which employees return to work for the same employer. As expected, we found those laid off were much more likely to return to an employer. Third, Boisjoly, Duncan, and Smeeding (1998) perform follow-up interviews to carefully distinguish between layoffs and for-cause firings with workers from the Panel Study of Income Dynamics (PSID) who reported losing jobs. The proportion of firings in our data closely matches their findings. Despite these checks, however, we still expect some measurement error in

[6] CRA91 does appear to have had a substantial effect on the litigiousness of displaced employees. The number of employment discrimination lawsuits filed in federal courts more than doubled from 1991 to 1995, and the Equal Employment Opportunity Commission saw a 43% increase in complaints filed from 1991 to 1994 (Johnson, 1997).

[7] Dertouzos and Karoly (1992) estimate that the indirect costs of employment law, including costs stemming from additional corporate counsel, internal dispute resolution, human resource specialists, and more formal performance review processes, are up to one hundred times greater than the costs of actual damages and settlements.

[8] While data sources other than the SIPP have been used to study trends in displacement, no other source elicits information on methods of displacement from such a large sample of workers.

these responses. This difficulty affects the interpretation of our findings only if changes in the measurement error differed by race around the time of CRA91. While we cannot rule this out, we see no reason to think that this would be the case.[9]

To focus on two groups of workers that were significantly differently affected by CRA91, we restrict attention throughout most of our analysis to non-Hispanic white men and black men between the ages of 21 and 39 who hold full-time jobs. Black men make up our group of workers highly affected by CRA91, as these are the employees with the most to gain from CRA91's amendments to the Civil Rights Act of 1866. Since the ability of non-Hispanic white men to recover damages for wrongful termination was not directly affected by the law, we use these workers as a comparison group.[10] We drop workers under age 21 because these workers are unlikely to have a strong attachment to employment. Dropping workers over 40 maximizes the differences in the effects of CRA91 on the two groups—since workers over 40 were protected by the ADEA prior to 1991, there is no variation in protected status by race for older workers. However, classifying older workers as affected by CRA91 is not necessarily appropriate because the ADEA provided stronger protection than Title VII (see Donohue and Siegelman (1991)) before CRA91 and weaker protection after CRA91. Also, our assertion that substituting layoffs for individual firings can shield employers from litigation is less likely to hold for these workers. Donohue and Siegelman (1993) note that older workers are far more successful than younger workers in suing for wrongful termination after layoffs. Women were dropped because the propensity to leave a job voluntarily differs significantly by gender in the age range we consider, which means women are less comparable to the unprotected comparison group. Also, the frequency of white female firings was already extremely low before CRA91, so even if CRA91 lowered the firing rates of women, the effect would necessarily be small and hard to detect.[11]

The basic unit of observation in our data is a job in a four-month period. Each person can therefore account for up to two observations in any given period and, over the course of up to eight SIPP interviews, can account for up to sixteen observations.[12] Summary statistics are in Table 1.

□ **Trends in displacement, 1987–1994.** We first analyze general trends in job displacement between 1987 and 1994. We define three categories of displacement. An observation is classified as a ''job end'' if the respondent reports having stopped working at the job during the four-month SIPP reference period. An observation is classified as ''involuntary separation'' if the respondent reports having left the job due to layoff

[9] In fact, we might expect CRA91 to make protected workers more likely to report being fired if discrimination claims are easier to prove after firings. Such a trend would bias our analysis against finding a relative reduction in firings among these workers.

[10] With the exception of the disabled and those who allege reverse discrimination, white men aged 21–39 should be completely unprotected by CRA91. Since less than 5% of workers in the 21–39 age group are disabled (Acemoglu and Angrist, 1998) and reverse discrimination suits are rare, we expect this to have little effect on our analysis.

[11] We reproduced our study using white women as the affected group and white men as the comparison group. While CRA91 did lower the firing rates of women more than men, the difference is not statistically significant.

[12] To compensate for the fact that multiple observations are contributed by each individual, all probit standard errors are adjusted to allow for correlation in observations from a given individual. To be sure that the varying number of observations per individual is not driving our results, we reran our analysis using just one observation per person (using the last observation or a randomly selected observation). Parameter estimates were similar in magnitude to those presented below, although, due to the limited sample, the standard errors were higher.

TABLE 1 Summary Statistics from the 1987–1993 SIPP

	Entire Sample (1)	Pre-CRA91 (2)	Post-CRA91 (3)
Total observations	198,642	96,401	102,241
Number of jobs	61,205	34,629	34,190
Number of individuals	43,573	26,396	25,723
Black	10.1%	10.1%	10.1%
White, non-Hispanic	77.8%	78.7%	76.8%
Mean age	30.4	30.2	30.5
	(5.2)	(5.2)	(5.2)
Mean weekly hours	43.9	44.1	43.7
	(8.7)	(8.8)	(8.5)
Union	16.2%	17.0%	15.2%
Some college or more	52.4%	51.8%	53.0%
State unemployment	6.2%	5.7%	6.8%
	(1.6%)	(1.5%)	(1.5%)

Notes: Data from 1987–1993 Surveys of Income and Program Participation, limited to men aged 21 to 39 holding full-time jobs. An observation is a job in a four-month SIPP interview period. Column 2 (column 3) covers interviews before October 1991 (after April 1992). Percentages are weighted (by SIPP sampling weights) averages across observations. Standard deviations are in parentheses.

or discharge. An observation is classified as a ''firing'' if the respondent reports having been discharged from the job.

In the first three columns of Table 2, we compute the rates of the various types of turnover and compare the pre- and post-CRA91 periods. Column (3) shows the incidence of job ends, involuntary separations, and firings to be significantly lower in the post-CRA91 period. The share of job ends dropped from 8.79% before CRA91 to 7.37% after. Involuntary separation dropped from 2.79% to 2.43%, and firings dropped from .56% to .48%. Since one of our aims is to assess firings as a share of involuntary separations, we present this fraction in the bottom row of the table.

These raw estimates may overstate the actual trend somewhat, owing to a change in the SIPP sampling methodology. The SIPP increased the number of ''waves'' (that is, the number of times each respondent is interviewed) in its survey from three (1989), six (1988), or seven (1987) to eight from 1990 to 1993. If individuals who are likely to change jobs are also relatively likely to leave the survey, then earlier SIPP waves have more high-turnover respondents. To minimize the impact of this and other factors that may affect turnover, we estimate a series of probit models using the indicators ''job ends,'' ''involuntary separation,'' ''fired,'' and ''fired given involuntary separation'' as dependent variables. Explanatory variables include indicators for one-digit occupation and industry codes, state, union status, age under 25 years, more than high school education, SIPP wave, and one-half percentage point intervals in the state unemployment rate in the first month of the interview reference period.[13] We also add an

[13] While many employment hazard models condition on job tenure, we are unable to do this because the SIPP does not regularly collect tenure information. We partially control for this by analyzing only observations with at least one year of job tenure, as discussed below.

TABLE 2 Pre/Post-CRA91 Comparisons of Job Displacement

	Pre-CRA91 (1)	Post-CRA91 (2)	Difference (3)	Post-CRA91 Probit, Full Sample (4)	Post-CRA91 Probit, 1+ Years Tenure (5)
Job ends	8.79%	7.37%	−1.42%	−.088	−.053
	(.09%)	(.08%)	(.12%)	(.014)	(.023)
				[−1.201%]	[−.485%]
Involuntary separation	2.79%	2.43%	−.36%	−.085	−.048
	(.05%)	(.05%)	(.07%)	(.019)	(.032)
				[−.422%]	[−.165%]
Fired	.56%	.48%	−.08%	−.048	−.040
	(.02%)	(.02%)	(.03%)	(.031)	(.055)
				[−.056%]	[−.031%]
Fired given involuntary separation	20.17%	19.69%	−.48%	.035	.013
	(.77%)	(.81%)	(1.12%)	(.061)	(.120)
				[.859%]	[.318%]

Notes: SIPP weights are used. Columns 4 and 5 report probit coefficients on a post-CRA91 indicator. Bracketed terms are corresponding estimated changes in probability. Columns 1–4 (column 5) use 198,642 (75,917) observations, except in the last row, where there are 5,143 (1,262) observations. Standard errors are in parentheses. Probit standard errors are adjusted for correlation of observations for any given individual.

indicator for whether the observation is in the post-CRA91 period. In column (4) of Table 2, we report the coefficient and standard error on the post-CRA91 indicator. Controlling for other factors that affect turnover, we still find lower job end and involuntary separation rates after the law. The trend toward lower firing rates, however, is no longer significant.

This drop in turnover rates from the late 1980s into the early 1990s has been identified by other studies. Using both the SIPP and the PSID, Gottschalk and Moffitt (1998) show that while monthly turnover rates fell over this period, annual turnover rates remained constant. They suggest that this pattern may be due to a reduction in the fraction of respondents who change jobs multiple times each year. This is consistent with evidence provided by Neumark, Polsky, and Hansen (1999), who demonstrate that expected tenure in very new jobs increased over this period.

To check the effect on our analysis of changes in expected tenure for new jobs, we next limit our sample to jobs that had been held for at least one year at the beginning of the SIPP four-month reference period. While the core SIPP does not ask about job tenure, the fact that respondents are surveyed every four months over a period of several years allows us to identify observations of jobs that have been held for at least one year. This restriction eliminates two-thirds of the sample, including many jobs that had been held for more than a year before the SIPP interviews started. In this subsample, the probit coefficients (shown in column (5) of Table 2) on the post-CRA91 indicator are much lower, and the coefficient on involuntary separation is no longer significant. So, while job displacement did fall from 1987 to 1994, the effect appears to be due in part to a reduction in the number of frequent job changers.

□ **Trends in displacement by race, 1987–1994.** In this section we examine how the displacement trends studied above vary by race. We estimate the differences in displacement trends across races and over time and use these estimates to assess the impact of CRA91 on firms' choices of displacement methods. We compute two estimates. First, we obtain a ''raw'' difference-in-differences (d-in-d) estimator by calculating the amount by which black displacement is lower after CRA91 than before, minus the amount by which white displacement is lower after CRA91 than before. If we let y_g^p be the observed displacement rate for workers in group $g \in \{black, white\}$ and period $p \in \{pre\text{-}CRA91, post\text{-}CRA91\}$ then our raw d-in-d estimator is given by $(y_b^{post} - y_b^{pre}) - (y_w^{post} - y_w^{pre})$. We can compute this estimator for each of our three categories of displacement.

While the raw estimator is easy to interpret, it ignores observable individual-specific factors that may affect displacement. To control for these factors, we also use a probit specification to measure any differential in changes in black and white displacement coinciding with CRA91.[14] We assume the probability that an observation results in displacement is a function of individual-specific and economywide factors. As control variables, we use indicators for race, calendar year, one-digit occupation and industry codes, state, union status, age under 25 years, more than high school education, SIPP wave, and one-half percentage point intervals in the state unemployment rate. The independent variable of interest is an interaction between a black indicator and an indicator for whether the observation is in the post-CRA91 period. The coefficient on this variable is our probit-adjusted estimate of the effect of CRA91.

The critical assumption needed for our analysis is that, in the absence of CRA91, employers would not have changed their displacement policies in a way that differed by race during the period we study. As an example of how this assumption could be violated, suppose employers reduced their opposition to civil rights legislation around 1991 because they were becoming generally less discriminatory (and, therefore, likely to bear lower costs from CRA91 enforcement). If this is the case, then our methods would attribute a change in the relative propensity of blacks and whites to be displaced to CRA91 when the causality actually runs the other way. Although we cannot disregard this and other explanations for why CRA91 was enacted, we believe the assumption underlying our approach is reasonable. First, note that businesses did lobby heavily against CRA91. Also, President George Bush vetoed a 1990 Civil Rights bill that would have given plaintiffs even more rights, but estimates are much weaker using 1990 as the pre/post division. Finally, various observers have pointed to the summer 1991 Clarence Thomas hearings as an important event in ensuring the passage of CRA91. We think it is reasonable to treat the Thomas publicity as exogenous for our purposes.

Tables 3 and 4 display the raw and probit-adjusted estimates, respectively, for each category of displacement. In Figures 1–4, we graph year-to-year changes in these displacement rates for both blacks and whites. As the estimates in the first row of Table 3 show, the rate of job ends fell for both blacks and whites in the post-CRA91 period. Black displacement fell by 1.66%, while white displacement fell by 1.34%. Both estimates are statistically significant, and these trends are readily apparent from examination of Figure 1. We do not find, however, that the changes in job end rates around the time of CRA91 differed by race. Our raw estimate is −.31%, which is not significantly different from zero. The regression in column (1) of Panel A, Table 4 confirms this result; the probit-adjusted estimate of the relative-to-whites change in black job ends after CRA91 is −.014%, which is also insignificantly different from zero.

We reach a similar conclusion when examining rates of involuntary separation. Raw estimates show that this form of displacement became somewhat less common for both races in the period after the enactment of CRA91. Figure 2 and the raw estimator both suggest that the reduction in involuntary separation was larger for

[14] All results are robust to the choice of logit or probit specification.

TABLE 3 Pre/Post-CRA91 Comparison of Job Displacement by Race

Variable	Pre-CRA91 (1)	Post-CRA91 (2)	Pre/Post Difference (3)	Variable	Pre-CRA91 (4)	Post-CRA91 (5)	Pre/Post Difference (6)
White job ends	8.77% (.10%)	7.43% (.09%)	−1.34% (.14%)	White fired	.51% (.03%)	.48% (.02%)	−.04% (.04%)
Black job ends	8.77% (.31%)	7.12% (.29%)	−1.66% (.43%)	Black fired	1.01% (.11%)	.50% (.08%)	−.52% (.14%)
Black/white difference	.00% (.33%)	−.31% (.31%)	−.31% (.45%)	Black/white difference	.50% (.11%)	.02% (.08%)	−.48% (.14%)
White involuntary separations	2.64% (.06%)	2.27% (.05%)	−.36% (.08%)	White fired given invol. separation	19.38% (.87%)	20.89% (.95%)	1.51% (1.28%)
Black involuntary separations	3.62% (.06%)	2.79% (.05%)	−.83% (.08%)	Black fired given invol. separation	27.99% (.87%)	17.78% (.95%)	−10.21% (1.28%)
Black/white difference	.98% (.21%)	.52% (.19%)	−.46% (.29%)	Black/white difference	8.61% (2.77%)	−3.11% (2.83%)	−11.72% (3.96%)

Notes: SIPP weights are used. There are 175,074 observations overall, with 4,418 observations of involuntary separation. Standard errors are in parentheses.

blacks; however, this result largely disappears when we control for time trends, economywide factors, and individual-specific factors. Our probit-adjusted estimator from column (2) of Panel A, Table 4 is −.132%, which is not significantly different from zero. Thus, while it would seem that CRA91 would make it more difficult for firms to displace black workers without comparably affecting whites, we can find no conclusive evidence that the law affected rates of overall or involuntary displacement differentially by race. This is similar to Acemoglu and Angrist's (1998) finding that the ADA had minimal effect on turnover and displacement.

FIGURE 1

JOB ENDS

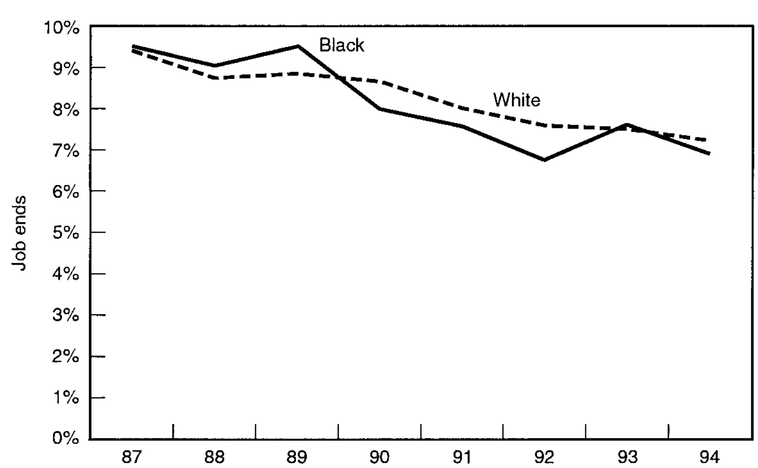


TABLE 4 Probit-Adjusted Pre/Post-CRA91 Comparison of Job Displacement by Race

Variable	Job Ends (1)	Involuntary Separation (2)	Fired (3)	Fired Given Involuntary Separation (4)
Panel A: full sample				
Black	−.012	.137	.184	.131
	(.028)	(.036)	(.056)	(.104)
	[−.166%]	[.740%]	[.255%]	[3.392%]
Black∗post-CRA91	−.001	−.028	−.206	−.397
	(.040)	(.050)	(.086)	(.167)
	[−.014%]	[−.132%]	[−.185%]	[−8.141%]
Log-likelihood	−46,992	−19,598	−5,451	−1,907
Observations	175,074	175,074	175,074	4,418
Panel B: 1+ years tenure				
Black	−.062	.119	.285	.430
	(.061)	(.077)	(.118)	(.267)
	[−.531%]	[.433%]	[.289%]	[12.07%]
Black∗post-CRA91	−.001	−.073	−.372	−.767
	(.075)	(.097)	(.167)	(.381)
	[−.010%]	[−.224%]	[−.179%]	[−12.97%]
Log-likelihood	−12,229	−5,250	−1,442	−445
Observations	67,196	67,196	67,196	1,091

Notes: SIPP weights are used. Bracketed terms are estimated change in probability. Panel B is limited to observations where job has been held at least one year when the period began. Standard errors (in parentheses) are adjusted for correlation of observations for any given individual.

FIGURE 2

INVOLUNTARY SEPARATIONS

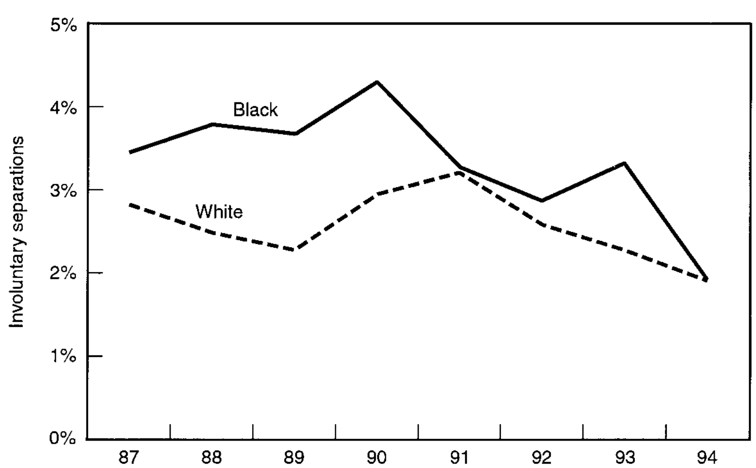
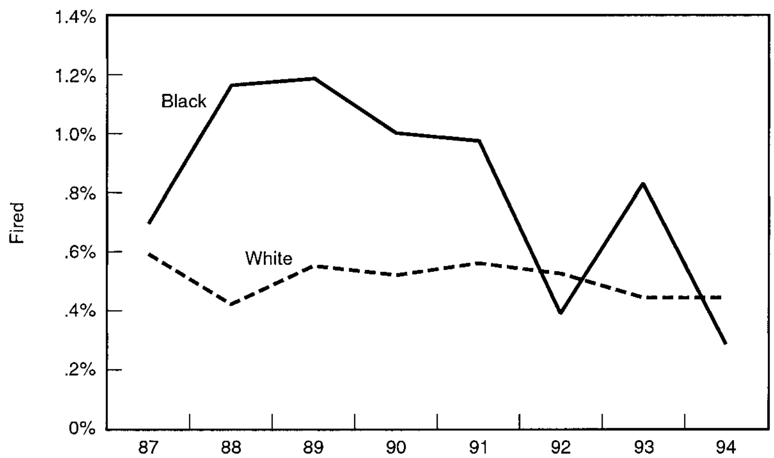


FIGURE 3

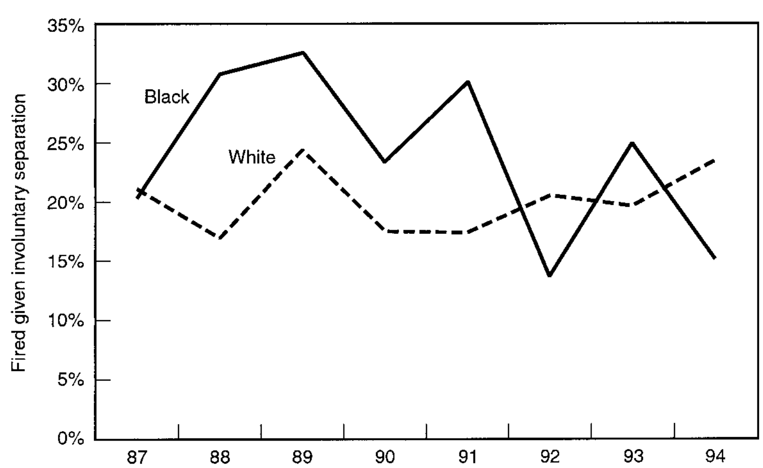
FIRINGS



We obtain much more striking results when examining trends in firings. As Figure 3 shows, the rate at which blacks were fired during the pre-CRA91 period was roughly twice as high as that of whites. While about 1% of jobs held by blacks ended in a firing during the relevant four-month SIPP reference period, only about one-half of 1% of jobs held by whites ended in this way. The graph then depicts a dramatic closing of this racial firing gap that was almost exactly coincident with the passage of CRA91. Black firings dropped sharply, while the rate of white firings remained roughly constant. Both the raw and probit-adjusted estimates are strongly negative and are significant at the 1% and 2% levels, respectively. Figure 4 shows that, among black workers who suffered involuntary separation, the proportion who were fired went down by more than a third after the law. During the same period, the share of involuntarily separated

FIGURE 4

FIRINGS GIVEN INVOLUNTARY SEPARATION



whites who were fired actually increased (though not significantly). The raw and probit-adjusted coefficients measuring fired given involuntary separation are significant at the 1% and 2% levels, respectively.

In Panel B of Table 4, we check whether changes in job tenures over the period we study may be affecting our results. As in column (5) of Table 2, we limit our sample to jobs that we can verify had been held at least one year at the beginning of the SIPP four-month reference period. Using this subsample, we find the estimates whether raw d-in-d (not reported) or probit-adjusted to be qualitatively very similar to those reported for the full sample. Changes in rates of job ends and involuntary separation did not differ by race, but black firings became significantly less common relative to white firings in the period after the enactment of CRA91.

These estimates are strongly consistent with the hypothesis that firms adjusted their discharge policies for protected workers away from individual firings and toward layoffs in response to the passage of CRA91. The share of black involuntary separations coming as firings dropped 10.21% after the passage of CRA91, while the share of white involuntary separations coming as firings rose 1.51%. The evidence suggests that although rates of job displacement were not affected by the passage of CRA91, firms' choices over *methods* of displacement were affected.

□ **Other sources of variation.** We next look for more evidence on the potential effects of CRA91 by exploiting additional sources of variation. We first use variation in fair employment practice (FEP) law across states, arguing that CRA91 should have a larger effect in states with FEP laws that are less favorable to employees. While federal antidiscrimination protections are more favorable to workers than those offered by most state statutes, California is a noteworthy exception. Stieber (1985) identifies California and Michigan as the states with ''the most legislative activity designed to prohibit unjust discharge'' (p. 561), while Donohue and Siegelman (1991) report that ''the more generous remedies under California's wrongful discharge law probably encourage litigants to sue in state rather than federal court'' (p. 1022).[15]

Under the assertion that CRA91 had a larger effect outside California than inside, we can further isolate our affected group to be black men outside California after CRA91. This difference-in-difference-in-difference (DDD) estimator has two major advantages over d-in-d. First, it removes less-affected employees from the affected group. Second, the assumptions underlying the estimation are less stringent; even if unobserved factors affect turnover in California differently from other states, our DDD estimates are unbiased as long as between-state trend differences are not race-specific. These advantages come at the considerable cost of increasing the noise in the estimate by three to five times. Given the magnitudes of the effects estimated above, we cannot expect a statistically significant result, so this DDD estimate is strictly meant as a check on previous findings.

We find that, relative to whites, black firings rose inside but fell outside of California over the pre- to post-CRA91 periods. Inside California, the relative change in black firings was .14% (standard error .57%), while outside the change was $-.53\%$ (.14%). Our DDD estimator for firings is therefore $-.67\%$ (.59%), suggesting that the relative-to-whites black firing rate dropped more outside of California. Rates of firings given involuntary separation show a similar pattern. The relative change in the share of black involuntary displacements coming as firings was -12.67% (4.04%) outside

[15] Michigan's employee protection legislation weakens the presumption of employment-at-will, rather than providing particularly strong antidiscrimination coverage. As a result, CRA91 had more impact in Michigan than in California.

California, but only −3.98% (20.23%) inside. Our DDD estimate for firings given involuntary separation is therefore −8.69% (20.63%). These results are, as expected, not statistically significant, but the signs are consistent with our expectation of more substitution toward layoffs and away from firings for blacks outside of California.

We next exploit variation in protected status by age. The ADEA, which allows for jury trials and some compensatory damages, covered men over 40 prior to 1991. We therefore expect any effects of CRA91 to be more strongly present for younger black men than for older black men. Expanding our sample to include all black and non-Hispanic white men between the ages of 21 and 55, we again compute a DDD estimator. The relative-to-whites change in firing rates was −.48% (.14%) for young black men and −.03% (.10%) for older black men, resulting in a DDD estimator of −.45% (.17%).[16] The relative-to-whites change in the share of involuntary terminations coming as firings was −11.71% (3.96%) for young black men but .53% (5.20%) for older black men. This yields a DDD estimator of −12.25% (6.54%). The addition of older men increases our sample size to 278,001 observations (6,131 involuntary separations), and it raises the significance of these DDD estimates to 1% and 7% for firings and firings given involuntary separation, respectively. These DDD estimates provide further evidence to support our assertion that CRA91 had a particularly strong effect on the firing rates of young black men.[17]

4. Conclusion

■ If a worker is more likely to file a lawsuit alleging unlawful termination when fired than when dismissed as part of a mass layoff, then changes in the legal environment that increase the expected costs of defending against such suits should induce firms to substitute toward layoffs and away from individual firings. We test this reasoning by examining changes in firms' choices over methods of displacement around the time of the passage of CRA91. We find that the share of black involuntary displacements coming in the form of individual firings dropped by around one-third after CRA91, while firings as a share of white involuntary displacement was unaffected. We conclude that there is evidence to suggest that firms substituted away from firings and toward layoffs as a means for displacing protected workers. By focusing on firms' choices over methods of displacement, our analysis differs from most earlier research on the labor market effects of employment protections.

One limit of our analysis is that by focusing narrowly on firms' choices over methods of displacement, we have ignored the possibility that firms may adjust both wages and hiring practices in response to changes in the legal environment.[18] Future research could use CRA91 to explore how firms adjusted these (and other) human resource policies in response to new employment discrimination legislation.

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[16] Probit-adjusted estimates using variation in state FEP law and age are comparable to these raw DDD estimates.

[17] An alternative explanation for why CRA91 may have had a larger impact on young workers arises if eliminating unproductive workers by using layoffs protects employers from litigation by younger workers but not older workers. There is some evidence to support this assertion; see our discussion of Donohue and Siegelman (1993) in Section 3.

[18] See Hamermesh (1993) for a review of the effects of displacement costs on labor demand.

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