

Induced Discrimination and Firm Size: Information vs Incentive Effects

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ABSTRACT. This paper characterizes the differences in incentives to respond adversely to the Civil Rights Act of 1991 CRA91 across firm size. I then empirically test the firm responses and compare them to the incentives. I find that responses across firm size match the incentive differences. Thus firms appear to be rational as well as fully informed. The most pronounced adverse response comes from large firms. Firms with more than 100 employees reversed a trend of increasing employment at a rate of 0.28 weeks worked per year prior to CRA91 to a trend of decreasing employment at a rate of -0.21 weeks per year. Since this group employs 63.7% of the workforce, this has a profound impact on opportunities for protected groups.

JEL CLASSIFICATION: J7, L2.

1. Introduction

Small businesses are considered to be a vital force in the United States economy, providing a mechanism for innovation, job creation and labor force mobility (US SBA, 1998, 1999, 2001), and serving as the backbone of full and free competition (Public Law 85–536). But it is also felt that small firms may be disadvantaged relative to large firms in accessing physical and financial markets and in accessing the policy making process (US SBA, 2001). The record certainly indicates that small firms incur some form of disadvantage. Studies consistently show high failure rates for new and small firms relative to their older and larger counterparts. Given the higher failure rates much of the economic contribution of small firms may come from firms who enter for a brief period then

exit. It is unclear whether such transient participation is a good thing or bad thing for social welfare though at least one study is trying to tackle that question (Knott and Posen, 2004). Despite the fact that small firm contributions may come from transient firms and despite the fact that the economy enjoys the benefits in the presence of whatever disadvantages small firms face, the general view is that more should be done to promote the development of small firms (US SBA, 2001). In this sense the SBA Office of Advocacy is essentially an affirmative action program to compensate for the inherent disadvantages of small firms, just as the EEOC is a program to compensate for the inherent employment disadvantages of minorities and women.

One particular access concern, and the focus of this paper, is whether small firms benefit from new technologies and practices to the same extent as large firms. This question is typically studied in the context of the diffusion literature. The goal of that literature is to examine the extent to which new technologies are fully adopted because full exploitation maximizes the social benefit inherent in the new technologies. Thus the question of small firm adoption of new technology is important because of its implications for full exploitation. The question is examined both theoretically and empirically. The key tenets of the diffusion literature with respect to firm size are that small size confers two disadvantages: an information disadvantage and an incentive disadvantage. The information disadvantage arises from the epidemiological characteristics of knowledge transmission. Small firms are more numerous and thus more difficult to reach directly. In addition they have fewer professional managers and less affiliation with professional networks, and thus are more difficult to reach indirectly.

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The incentive disadvantage stems from the fact that most innovations have fixed costs of adoption and benefits that are a function of firm scale. Accordingly, innovations that have positive returns for large firms, may generate insufficient returns to merit small firm adoption.

Empirical work on diffusion tends to support the view that small firms are less likely to adopt new technologies. These tests however have not been able to decompose the main firm size effect into its information and incentive components. The decomposition is important because the information problem is in principle easier to remedy than the incentive problem.

This paper takes advantage of a natural experiment to isolate the effects of the information disadvantage in the adoption of practices by small firms. This experiment is possible because of the introduction of a new policy whose response is costless to adopt. The policy is the Civil Rights Act of 1991 (CRA91) and the response is induced hiring discrimination. The features of the Act facilitating its use as a natural experiment are the facts that its implementation date is public, its penalties are tied to firm size, and the response is costless.

Admittedly CRA91 is an odd setting to examine diffusion phenomena, because the expected response is adverse to the policy's goals. However, the test is worthwhile for two reasons. First, the test has implications for the diffusion literature and for SBA policy as mentioned above. Additionally, the test is interesting in its own right because it quantifies the adverse impact of CRA91 across firm size.

The paper proceeds as follows. Section 2 briefly reviews the firm size problem in the diffusion literature. Section 3 provides background of EEOC regulation, CRA91, and the induced discrimination response. Section 4 characterizes the incentive differences in CRA91 across firm sizes. Section 5 characterizes the information diffusion problem. Section 6 presents an aggregate test of induced discrimination using data from the Current Population Survey (CPS). Section 7 presents results. Section 8 offers a brief discussion.

2. Diffusion and firm size

Diffusion processes have two components: a communications component that considers hetero-

geneity in access to information (Rogers, 1995), and an incentives component that considers heterogeneity in the returns to adoption given full information. Looking first at the incentive component there are two perspectives. Mansfield (1968) outlines a decision theoretic approach where a firm's returns to adoption are considered in isolation. A firm compares its fixed cost to adopt the technology against the reduction in unit costs arising from the use of the new technology. Since the derived benefit from adoption is the total cost savings minus the fixed cost of implementation, adoption is generally a function of output scale. Heterogeneity in firm scale thus leads to differential adoption times as the price of technology drops. Large firms find adoption profitable at early stages with high prices. Smaller firms must wait until the price drops or their scale increases.

Later work (Reinganum, 1981; Fudenberg and Tirole, 1985; Gotz, 1999) incorporates the behavior of rivals in the adoption decision. Prior adoption by rivals may increase the returns to adoption if there are network externalities. This would be the case for example with electronic data interchange (EDI), where the value of a firm's adopting the technology grows with the number of suppliers and buyers who have also adopted the technology. Alternatively prior adoption may decrease the returns to adoption if there are competitive effects. This would be the case for example with implementing new product features. If those features have negative sloping demand, and if other firms have already incorporated those features in their products, then the price premium a late adopter can capture for the new features is lower than the premium available to earlier innovators.

The second factor affecting the diffusion of innovation is information effects. Information effects are typically treated as a social process (Rogers, 1995) through which individuals and firms become aware of an innovation and obtain information about the costs and benefits of its adoption. There are two mechanisms through which information is spread: mass media (broadcast model), and direct contact through other firms or individuals (contagion model). There is heterogeneity across firms affecting their awareness in both models. In the broadcast model the

timing pertains to media habits; in the contagion model the timing pertains to a firm's interconnectedness to a network. Factors affecting interconnectedness are the number of network affiliations, the frequency of attendance at network events, and physical and personal proximity to other members in the network.

Diffusion studies have shown that small firm adoption of new technology often lags that of large firms both because of information effects and incentive effects. The information disadvantages of small firms arise from fewer interfaces with the external environment (Almeida et al., 2003). The incentive effects arise from insufficient scale to recoup the cost of the new technology (Karshenas and Stoneman, 1993). However because the information effects and incentive effects occur jointly in these studies, it is difficult to isolate the relative contributions of the separate effects.

This paper takes advantage of a natural experiment to isolate the information effects in the adoption of practices by small firms. This experiment is possible because of the introduction of an "innovation" which is costless to adopt, and whose benefits are easily characterized. Thus we can control for incentive effects, to isolate the information effects. The "innovation" is hiring discrimination, which appears to be an unlikely innovation. Therefore before discussing the experiment, I discuss the "innovation" itself.

3. CRA91 and induced discrimination

The term "employment discrimination" refers to the set of five laws administered by the EEOC. These include the Equal Pay Act of 1963 prohibiting gender-based wage discrimination, Title VII of the Civil Rights Act of 1964 prohibiting employment discrimination based on race, color, religion, sex and national origin, the Age Discrimination in Employment Act of 1967 (ADEA) prohibiting employment discrimination against persons 40 years of age or older, the Americans with Disabilities Act of 1990 (ADA), prohibiting employment discrimination against qualified individuals with disabilities, and the Civil Rights Act of 1991 modifying the prior laws to allow compensatory and punitive damages in cases of employer "malice or reckless indifference to the rights of aggrieved individuals."

Past empirical work has tended to show that the Civil Rights laws and EEOC enforcement have improved economic conditions for protected groups (see for example Freeman, 1981; Heckman and Payner, 1989; Leonard, 1996). However recent work has expressed concerns that the laws have actually induced discrimination (Posner, 1987; Donohue and Siegelman, 1991; Abram, 1993; Acemoglu and Angrist, 1998; Oyer and Schaeffer 2000, 2002). Acemoglu and Angrist (1998) find that the ADA has induced discrimination of the disabled because employers incur physical costs of accommodating the disabled, thus making disabled employees more expensive than non-disabled employees. Similarly, Oyer and Schaeffer (2002) find that employers sort away from blacks and women in their hiring in order to avoid later employment litigation. This paper focuses on the latter effect using introduction of CRA91 as a natural experiment.

CRA91 presents an interesting natural experiment in which to test employer response to equal employment legislation. CRA91 was enacted in 1992 to expand the rights of plaintiffs in federal discrimination suits. The chief provisions of interest here are (1) the right to a jury trial, (2) sufficiency of statistical evidence to demonstrate disparate treatment of protected groups, and (3) the introduction of punitive damages. What makes it an interesting experiment is not only a clear historical change in incentives, but also differential incentives across firm sizes. Maximum punitive damages are tied to firm size, and statistical reporting of workforce composition via EEO-1 reports is only required of firms with greater than one hundred employees. Better still, there is a control group of firms exempt from EEOC regulation. Thus I can test more convincingly whether the changes in firm behavior are due to CRA91 or some other exogenous shock.

Two features of CRA91 facilitate induced discrimination of protected group employees. The first of these is the higher cost of employment litigation by protected groups. The provision allowing compensatory and punitive damages has a primary effect on the implicit wage of protected groups through the increased cost of a suit. Additionally, the higher allowed damages produce a secondary effect of stimulating additional EEOC charges. This secondary effect is demon-

strated in Figure 1 which traces the employer hazard rate before and after CRA91. The hazard rate is expressed as a ratio of the number of charges to total protected group employment. The combination of increased cost per charge (higher damages) and increased likelihood of charges has substantially increased the cost of employing individuals from protected groups. In fact a new line of insurance, Employment Practices Liability Insurance (EPLI), emerged in 1990 in anticipation of increased employer exposure from CRA91.

Since the implicit wage for employees is money wages plus employment litigation costs, and since protected group employees have higher expected damages and higher suit probabilities arising from CRA91, employees from protected groups become more costly than non-protected employees. Thus all else equal, employers rationally prefer non-protected employees to avoid litigation costs, just as they prefer to hire non-disabled employees to avoid accommodation costs.

Induced discrimination can be avoided if money wages for protected groups are allowed to adjust to correct for the higher termination costs. However, wage discrimination is unlawful under EEOC regulation. An alternative adjustment mechanism is to increase the hiring costs of non-protected employees. This is accomplished through anti-discriminatory hiring provisions in the EEOC regulations. In principle, hiring non-protected employees generates legal costs from lawsuits by rejected candidates from

protected group. However, such lawsuits are rare. Hiring charges account for less than 10% of all EEOC charges (Figure 2). The lower incidence of hiring charges may be due to greater difficulty in demonstrating hiring discrimination (since the applicant is unlikely to know anything about competing applicants), or from the fact that economic damages from hiring discrimination are lower, since the applicant has no specific investment in the prospective employer. Thus while increased costs of hiring non-protected employees may in principle offset the higher employment costs of those from protected groups, this appears not to occur in practice.

4. Discrimination incentives and firm size

What is of particular interest in this study are the structural differences in CRA91 treatment for firms of different sizes. These differences are interesting for two reasons. First, the incentive differences across size provide a more direct test that firm behavior is in fact a response to CRA91 rather than some other change in economic conditions. Second, by comparing differences in response to differences in incentives across firm size I can assess whether firm responses reflect full information or whether smaller firms have information disadvantages that suppress their response.

There are three firm size distinctions. The most important distinction is that firms with fewer than 15 employees are exempt from EEOC regulations (the exemption was originally for firms of fewer than 25 employees, but this threshold was reduced to 15 employees for all but the ADA with the 1972 amendments to Title VII, and was reduced for ADA in 1994). The second distinction is that punitive damage caps introduced by CRA91 are tied to firm size, as shown in Table I. The caps rise in discrete steps from \$50,000 for firms with 15–100 employees, to \$300,000 for firms with over 500 employees. I ignore for now the question of whether the damage caps for each firm size are the appropriate amounts, and focus on the issue of whether firms respond to their respective damage caps in similar fashion.

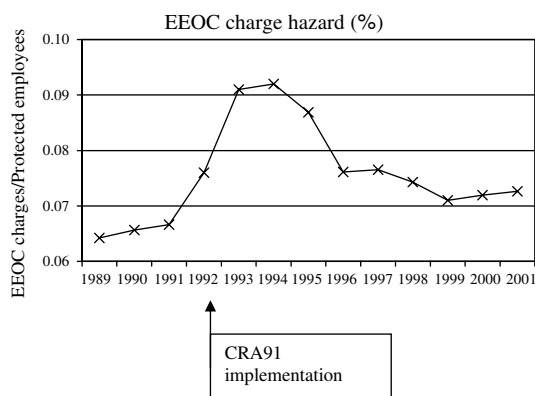


Figure 1. Annual hazard rate of EEOC charges.

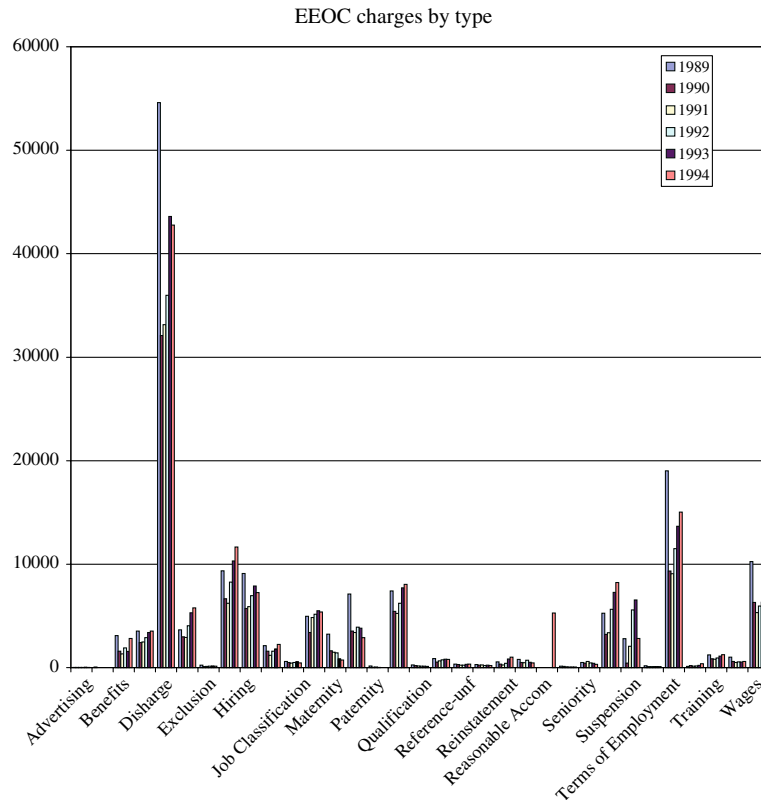


Figure 2. EEOC Charges by type over time.

The final distinction between large and small firms is reporting requirements. This distinction is important because only firms with more than 100 employees (50 employees if the firm is a government contractor) are required to report labor force composition to the EEOC via EEO-1 reports. These composition statistics are used to substantiate charges of disparate impact by aggrieved employees, as well as to stimulate inde-

pendent charges by the EEOC. Prior to CRA91 plaintiffs had to link charges of disparate impact to specific practices of the employer. CRA91 reduced the plaintiff's burden by making statistical evidence of disparate impact sufficient to demonstrate discrimination. In addition to their role in substantiating claims of discrimination generally (potentially causing an across the board increase in litigation), year-to-year changes in

TABLE I
Differences in EEOC treatment of firms by firm size

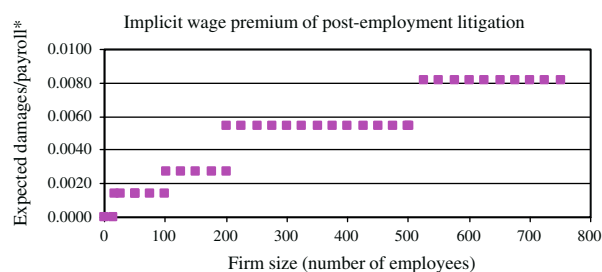
Number of employees	Percent of firms in class (1995)	Percent of employment in class (1995)	Maximum compensatory and punitive damages	EEO-Reporting
< 15	84.20%	14.10%	Exempt	Not required
15–100	14.10%	22.20%	\$50,000	Not required ^a
101–200			\$100,000	Required
201–500			\$200,000	Required
> 501	0.30%	47.50%	\$300,000	Required

^a except for firms over 50 employees with government contracts.

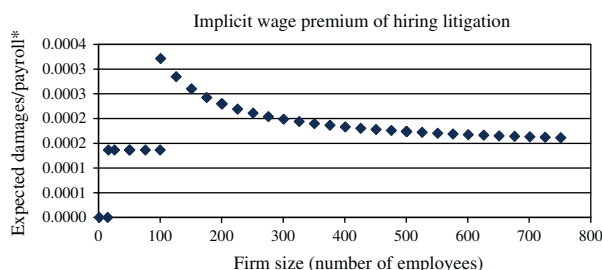
EEO-1 reports provide implicit evidence of hiring discrimination.

Figure 3 conveys the impact of the CRA91 on the litigation risks of firms, and accordingly their incentive to discriminate. I characterize two effects: the implicit wage effect (Figure 3a) and the detection effect (Figure 3b). To characterize the implicit wage effect I first compute the annual probability (hazard) that a firm of a given size will face an employment suit. To do so I apply a constant employee-level suit hazard of 0.00073 (from Figure 1) to the number of employees in each firm size. This firm-level suit hazard is combined with the maximum damages in the size

classification to determine the expected annual damages. Next I express the expected damages in terms of implicit wages by normalizing the expected damages by the annual payroll. It is clear from Figure 3a that the impact of CRA91 on implicit wages is an increasing step function of firm size. Thus the incentive to discriminate against protected groups at hiring increases with firm size. One thing to note is that I have assumed that the charge hazard is constant across firm sizes. There is reason to believe that the actual suit incidence is higher in small firms because such firms are less likely to have Human Resources (HR) professionals (Figure 4). Accord-



- $(\text{suit hazard} \times \text{Max damages}) / (\text{employees} \times \text{median wage})$



- hiring suit hazard is estimated at $0.10 \times \text{employment suit hazard}$ (assumes 10% turnover and 10 candidates per slot), and reduced suit frequency (10% of employment suits)
- the impact of EEO-1 on detection probability is arbitrary. It is set at .01 just to make point about step function change

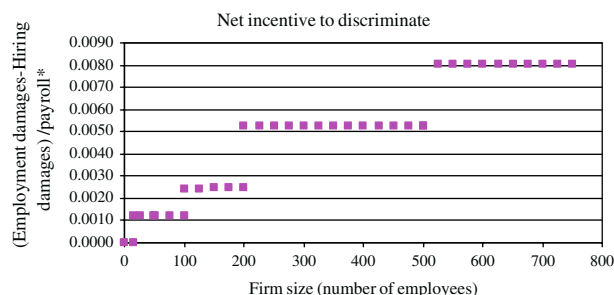


Figure 3. Impact of CRA91 across firm size.

ingly, they are unlikely to have formal training programs to deter discriminatory behavior, or formal counseling programs for employees who feel they have been aggrieved.

Firms may be dissuaded to act on the implicit wage incentives if there are offsetting disincentives to hiring discrimination. While CRA91 did not affect hiring discrimination directly, its provision making statistical evidence sufficient to substantiate claims has an indirect impact. In particular, rejected candidates can utilize work force composition data from the EEO-1 reports to substantiate claims of hiring discrimination. While these reports could always be used, until CRA91 they were not sufficient. Plaintiffs needed to link evidence of disparate impact to a particular firm policy. These reports can be used on claims other than hiring discrimination. I argue however that their primary impact post-CRA91 was on hiring discrimination suits, since there are few alternative forms of evidence to substantiate such claims given the limited contact between the employer and the candidate.

The disincentives to hiring discrimination are captured in Figure 3b. The figure takes into account the arrival rate of job candidates. For simplicity I assume 10% turnover and 10 rejected candidates per turnover. This is merely a plausible justification for the 10% relative incidence of pre-employment versus post-employment charges shown in Figure 2. I attempt to incorporate the effects of EEO-1 reporting by applying a 10% premium for firms with greater than 100 employees.

Figure 3c shows the combined impact of higher implicit wages of CRA91 less the offsetting effect of hiring discrimination suits. The figure demonstrates that returns to hiring discrimination are substantial and increasing in firm size. Since the cost to adopt a practice of hiring discrimination is negligible, these returns are also the net incentives to discriminate. One nice feature of the incentive differences across firm size is that they exhibit large discrete steps. Thus the experimental response should be easier to detect.

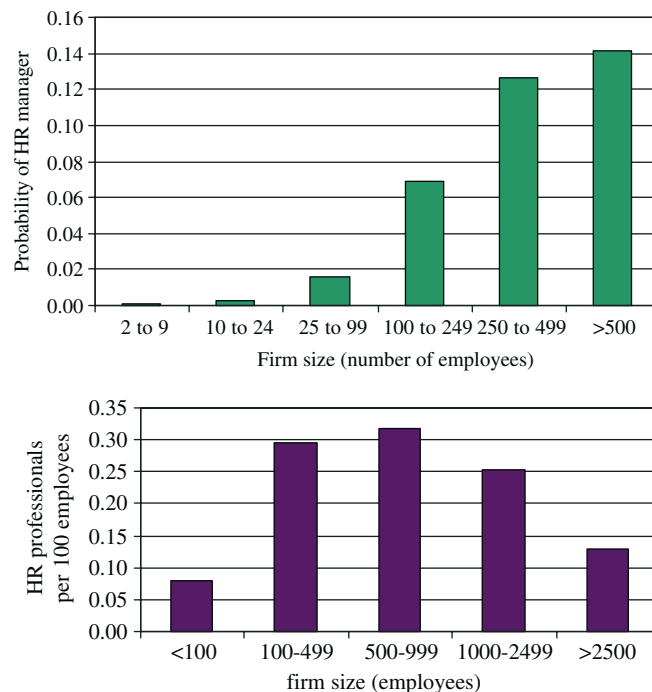


Figure 4. Prevalence of HR professionals across firm size.

Given the higher termination costs of protected group employees since CRA91, the unlawfulness of firms to adjust implicit wages through real differences in money wage, and the limited likelihood for hiring discrimination to be detected in firms with fewer than one hundred employees, I expect to see evidence of induced discrimination following implementation. Moreover I expect to see that the responses to CRA91 across firm size will correspond to the incentive differences depicted in Figure 3.

5. Information diffusion and firm size

The expectation that firm responses to CRA91 will match the incentives in Figure 3 rests on an assumption that firms are fully informed. It is possible however that firm responses will lag the change in incentives, and that the length of the lags will be a function of firm size. Because CRA91 is fairly recent, it is possible that smaller firms are not fully aware of its implications. This stems from a lower likelihood of having direct experience with employment litigation (constant hazard over smaller employee base), and from the fact that small firms are less likely to have HR professionals. HR professionals interact with one another through conferences and practice journals, and accordingly stay abreast of best practice. In smaller firms HR functions are likely performed by multi-functional administrators without professional affiliations. Without such affiliations it is possible that CRA91 information is not fully diffused among small and medium firms, and accordingly the induced discrimination response may not fully realized. In fact, conversations with an EPLI broker indicate that such insurance is difficult to sell to medium-sized firms because they do not recognize the litigation threat.

If small firms suffer such information disadvantages relative to large firms then I expect their response to be muted or lagged relative to larger firms. Accordingly, I would expect the cross-sectional response to be more pronounced than that depicted in Figure 3. This is the central hypothesis I test in this paper.

6. Aggregate test of induced discrimination

I test for aggregate evidence of induced discrimination arising from CRA91 using Person

Records from the Census Bureau CPS data for the years 1989–1997. I examine weeks worked by individual, i , in firm size, s_j , in year t , y_{ijt} , as a function of an interaction between protected group status, p_i , firm size, s_j , and year, d_t . I control for differences in human capital, x_{it} , structural differences in employment for firms of different sizes, σ_j , structural differences in employment across industry, ϕ_k , and changes in aggregate conditions, δ_t .

$$y_{it} = \gamma_{ijt} p_i s_j d_t + \beta x_{it} + \sigma_j + \phi_k + \delta_t + \varepsilon_{it} \quad (1)$$

The CPS classifies firms into four size categories: exempt (under 10 employees), small (10–24 employees), medium (25–99 employees), and large (100 or more employees). Unfortunately, this classification does not coincide with the CRA91 firm size distinctions shown in Table I. Table I indicates that firms with fewer than 15 employees are *exempt* from EEOC legislation. *Small* firms (those with the lowest punitive damage caps) are 15–100 employees. There are two classes of *medium* firms (101–200 and 201–500 employees). *Large* firms are 501 employees and above.

Comparing the two classification systems indicates that CPS small firms (10–24 employees) mixes CRA91 *exempt* and *small* firms. CPS medium firms (25–99 employees) is a subset of CRA91 *small* firms. CPS large firms (>100) encompasses all CRA91 *medium* and *large* firm.¹ The match between classification systems is closer for EEO-1 reporting. The impact of EEO-1 reporting is borne entirely by all CPS large firms and also borne by CPS medium firms with government contracts and more than 50 employees.

In an effort to determine whether the discrimination response is induced rather than inherent in employer preferences, I examine levels of discrimination for all groups over time. In particular I compare levels before and after the increased sanctions of CRA91. In addition I compare the responses across firm size to see if they match incentive differences across size. I expect to find lower protected group employment in medium and large firms post CRA91 and I expect the employment change to be more pronounced in large firms than in medium firms.

From the total set of 1,335,151 person-year observations from the CPS person records, I restricted attention to adults in the private sector (deleting children, armed forces and public administration). The final data set comprises 494,285 person-year observations. Descriptive statistics for the CPS data are reported in Table II.

7. Results

Table III reports ordinary least squares estimates of equation 1. The coefficients of interest are the γ_{ijt} for the protected group, firm size, year interactions. I report results for all protected groups as a single class, and for each protected group separately. These results are most easily interpreted by plotting the coefficients for protected classes by firm size over time (Figure 5).

The results provide evidence that firms did indeed respond to CRA91. Discrimination was decreasing (protected group employment increasing) in medium and large firms until survey year 1993 (for employment in 1992, the first year of CRA91 implementation), and increasing thereafter. The coefficient for protected employees in medium-sized firms is -2.24 in 1997 vs -1.71 for 1993. Thus employees from protected groups worked on average 0.53 less weeks in 1996 than they did in 1992. A test of the restriction that the coefficient in 1997 is higher than that in 1993 is significant. The large firm effect is even more

pronounced. The coefficient for 1997 is -2.15 versus -1.28 for 1993, a difference of 0.87 weeks worked. In contrast, results for the exempt firm control group tend to indicate that discrimination is decreasing over the 9 year period. The coefficient for exempt firms (< 10 employees), though noisy, decreases in absolute value from 2.18 in 1989 to 1.70 in 1997. Since discrimination is decreasing in exempt firms, and increasing in firms affected by CRA91, there is preliminary support for the hypothesis that CRA91 is inducing discrimination.

What is of particular interest here is whether firms behave as if they have full information – do the responses across firm size match the incentive differences. To understand this I first characterize firm response in terms of the difference in employment slopes² before and after CRA91 for each CPS firm size classification. This response is captured in the right-hand scale in Figure 6. I compare this response to the incentive differences across CRA91 size classifications from Figure 3 (the left hand scale of Figure 6). The figure indicates first that firm responses do indeed track the incentive differences. Protected group employment in medium-sized firms exhibited a reversal in employment growth of 0.23 weeks worked per year, while large firms exhibited a reversal in employment growth of 0.49 weeks worked per year.

Another thing noteworthy from the figure pertains to the disincentives to hiring discrimination

TABLE II
Descriptive statistics for CPS data person records-private sector adults number of observations = 494285

	Mean	Std Dev	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Weeks worked	43.89	15.22	1.00															
2. Hours per week	36.97	13.81	0.55	1.00														
3. Protected group	0.76	0.43	-0.04	-0.13	1.00													
4. Female	0.47	0.50	-0.07	-0.23	0.53	1.00												
5. Black	0.09	0.28	-0.05	-0.04	0.17	0.04	1.00											
6. Native American	0.01	0.09	-0.03	-0.01	0.05	0.00	-0.03	1.00										
7. Asian	0.03	0.17	0.00	0.00	0.10	0.01	-0.05	-0.02	1.00									
8. Hispanic	0.13	0.33	-0.04	-0.02	0.21	-0.04	-0.09	-0.01	-0.06	1.00								
9. Over 40	0.36	0.48	0.11	0.06	0.42	0.00	-0.01	-0.01	0.00	-0.07	1.00							
10. Disabled	0.03	0.18	-0.14	-0.11	0.10	0.00	0.01	0.01	-0.02	-0.02	0.07	1.00						
11. Experience	21.78	13.01	0.16	0.11	0.34	0.00	-0.01	-0.02	0.00	-0.07	0.83	0.09	1.00					
12. Education	10.72	3.06	0.14	0.15	-0.08	0.02	-0.05	-0.03	0.03	-0.26	-0.03	-0.05	0.01	1.00				
13. Exempt	0.19	0.39	-0.01	-0.03	-0.02	0.01	-0.05	0.00	-0.01	0.02	-0.01	0.01	-0.01	0.00	1.00			
14. Small	0.12	0.33	0.03	0.04	-0.02	-0.02	-0.03	0.00	0.00	0.02	-0.02	-0.01	-0.02	0.00	-0.18	1.00		
15. Medium	0.15	0.36	0.06	0.08	-0.01	-0.02	-0.01	0.00	0.00	0.02	0.00	-0.01	0.00	-0.02	-0.20	-0.16	1.00	
16. Large	0.50	0.50	0.21	0.19	0.01	0.00	0.04	-0.01	0.00	-0.06	0.03	-0.04	0.03	0.05	-0.48	-0.37	-0.42	1.00

TABLE III
Aggregate test of induced discrimination dependent variable = weeks worked in prior year

	All protect		Female		Over 40		Disabled		Black		Native Amer		Asian		Hispanic	
	Coef.	St err.	Coef.	St err.	Coef.	St err.	Coef.	St err.	Coef.	St err.	Coef.	St err.	Coef.	St err.	Coef.	St err.
Protect*Exempt*1989	-2.18	0.16	-1.31	0.16	-1.43	0.19	-8.72	0.52	-1.10	0.39	-7.79	1.05	0.67	0.67	0.90	0.30
Protect*Exempt*1990	-1.99	0.15	-1.53	0.16	-1.55	0.18	-7.30	0.50	-1.17	0.39	-2.19	1.17	1.29	0.59	1.73	0.26
Protect*Exempt*1991	-1.69	0.15	-1.20	0.15	-1.62	0.18	-7.80	0.51	-0.56	0.39	-5.07	1.06	1.02	0.58	1.93	0.25
Protect*Exempt*1992	-1.76	0.18	-0.53	0.19	-2.08	0.22	-7.82	0.60	-1.55	0.49	-5.65	1.37	-0.34	0.75	1.90	0.34
Protect*Exempt*1993	-1.46	0.18	-0.33	0.20	-1.85	0.23	-7.74	0.61	-1.17	0.50	-2.95	1.42	0.67	0.74	1.22	0.34
Protect*Exempt*1994	-1.86	0.19	-0.98	0.20	-1.65	0.23	-7.39	0.63	-2.18	0.52	-9.27	1.28	0.26	0.77	1.28	0.34
Protect*Exempt*1995	-1.28	0.19	-0.47	0.20	-1.35	0.22	-7.49	0.61	-0.14	0.53	-6.11	1.26	-0.06	0.74	2.51	0.33
Protect*Exempt*1996	-1.45	0.20	-0.67	0.21	-1.52	0.23	-6.38	0.68	-1.15	0.55	-0.61	1.41	0.00	0.72	2.26	0.33
Protect*Exempt*1997	-1.70	0.20	-1.39	0.21	-1.39	0.23	-7.43	0.66	-1.27	0.56	-1.12	1.32	-0.24	0.76	2.53	0.33
Protect*1989*Small	-1.92	0.19	-1.66	0.21	-1.27	0.24	-6.47	0.70	-0.98	0.46	-3.81	1.54	0.09	0.77	0.76	0.38
Protect*1990*Small	-2.16	0.18	-1.99	0.20	-1.48	0.23	-8.70	0.69	-1.72	0.45	-6.76	1.53	1.23	0.70	1.00	0.32
Protect*1991*Small	-1.55	0.19	-1.35	0.20	-1.43	0.23	-8.72	0.73	-1.66	0.45	0.16	1.29	1.90	0.73	1.03	0.31
Protect*1992*Small	-1.82	0.21	-1.15	0.23	-2.22	0.28	-7.34	0.85	-2.72	0.59	-1.68	1.67	-1.00	0.87	0.84	0.38
Protect*1993*Small	-1.85	0.21	-1.17	0.23	-1.88	0.27	-6.58	0.82	-2.19	0.61	-5.72	1.56	-0.10	0.79	0.05	0.39
Protect*1994*Small	-1.81	0.22	-1.43	0.24	-1.82	0.28	-7.12	0.86	-2.04	0.61	-0.13	1.69	0.76	0.88	1.52	0.40
Protect*1995*Small	-1.94	0.22	-2.17	0.24	-1.70	0.28	-7.77	0.96	-0.46	0.60	-4.39	1.42	0.01	0.87	1.61	0.40
Protect*1996*Small	-2.25	0.22	-2.23	0.25	-1.85	0.29	-8.01	0.87	-1.85	0.65	-0.84	1.50	0.28	0.84	1.28	0.39
Protect*1997*Small	-1.41	0.22	-1.11	0.25	-1.40	0.28	-6.95	0.89	-2.08	0.58	-2.72	1.73	1.53	0.86	2.41	0.38
Protect*1989*Medium	-2.10	0.19	-1.49	0.20	-1.16	0.23	-6.27	0.73	-0.83	0.43	-1.66	1.73	0.68	0.79	0.00	0.39
Protect*1990*Medium	-2.37	0.18	-1.90	0.19	-1.29	0.22	-6.60	0.70	-1.59	0.40	-2.86	1.33	0.11	0.74	0.64	0.34
Protect*1991*Medium	-2.07	0.18	-1.49	0.19	-1.53	0.22	-5.26	0.68	-2.40	0.42	-2.14	1.51	0.90	0.70	0.83	0.35
Protect*1992*Medium	-1.96	0.18	-1.04	0.20	-1.94	0.23	-8.16	0.72	-2.24	0.45	-2.66	1.51	0.85	0.69	0.50	0.31
Protect*1993*Medium	-1.71	0.18	-1.09	0.20	-1.80	0.22	-7.43	0.73	-1.05	0.43	-5.57	1.21	0.17	0.72	0.25	0.30
Protect*1994*Medium	-1.85	0.19	-1.33	0.21	-1.45	0.23	-5.47	0.74	-2.03	0.46	1.32	1.41	0.61	0.67	0.72	0.33
Protect*1995*Medium	-2.12	0.19	-1.79	0.21	-1.72	0.23	-6.72	0.77	-1.81	0.47	-2.87	1.41	0.22	0.72	1.21	0.33
Protect*1996*Medium	-1.90	0.20	-1.40	0.22	-1.40	0.24	-5.97	0.79	-1.43	0.49	-3.75	1.51	-0.30	0.77	1.23	0.34
Protect*1997*Medium	-2.24	0.20	-2.14	0.22	-1.39	0.24	-8.43	0.81	-2.11	0.48	-4.76	1.37	0.24	0.73	1.26	0.32
Protect*1989*Large	-2.24	0.13	-1.79	0.13	-1.03	0.15	-6.83	0.50	-1.15	0.26	-4.54	1.06	0.10	0.51	-0.01	0.29
Protect*1990*Large	-2.31	0.13	-1.86	0.13	-1.07	0.15	-7.45	0.47	-1.00	0.24	-6.38	1.10	-0.29	0.46	-0.13	0.25
Protect*1991*Large	-1.91	0.13	-1.44	0.13	-1.23	0.15	-7.03	0.48	-1.28	0.24	-3.27	0.94	0.76	0.44	0.63	0.25
Protect*1992*Large	-1.43	0.12	-0.70	0.11	-1.44	0.13	-6.11	0.39	-0.60	0.21	-1.95	0.81	0.17	0.38	0.59	0.20
Protect*1993*Large	-1.28	0.12	-0.42	0.12	-1.33	0.13	-6.95	0.39	-1.06	0.22	-0.13	0.77	0.17	0.37	0.24	0.20
Protect*1994*Large	-1.69	0.12	-1.07	0.12	-1.20	0.13	-6.48	0.40	-1.34	0.22	-2.75	0.76	-0.11	0.38	0.38	0.20
Protect*1995*Large	-1.82	0.13	-1.11	0.12	-1.68	0.13	-6.67	0.41	-1.12	0.2	-2.46	0.74	-0.37	0.36	0.61	0.20
Protect*1996*Large	-2.08	0.13	-1.57	0.12	-1.50	0.14	-5.65	0.43	-0.99	0.23	-2.57	0.78	-0.23	0.38	0.67	0.21
Protect*1997*Large	-2.15	0.13	-1.61	0.12	-1.59	0.13	-5.89	0.42	-1.35	0.22	-1.59	0.71	-1.04	0.37	0.50	0.20
Education	0.47	0.01	0.50	0.01	0.50	0.01	0.49	0.01	0.50	0.01	0.50	0.01	0.50	0.01	0.53	0.01
Experience	0.17	0.00	0.15	0.00	0.19	0.00	0.16	0.00	0.15	0.15	0.15	0.00	0.15	0.00	0.15	0.00

Exempt	44.29	0.11	43.48	0.09	43.54	0.09	43.31	0.08	43.09	43.06	0.09	0.08	43.01	0.09	42.74	0.09
Small	45.47	0.12	44.81	0.10	44.62	0.10	44.34	0.09	44.22	0.09	44.12	0.09	44.08	0.09	43.89	0.09
Medium	46.19	0.11	45.35	0.09	45.12	0.09	44.86	0.09	44.79	0.09	44.66	0.09	44.63	0.09	44.50	0.09
Large	46.47	0.09	45.64	0.08	45.46	0.08	45.24	0.08	45.15	0.08	45.05	0.08	45.03	0.08	44.94	0.08
R ²	0.94		0.94		0.94		0.94		0.94		0.94		0.94		0.94	

All models include industry and year effects.
Number of observations = 494285.

arising from EEO-1 reporting. The estimates in Figure 3 suggested that EEO-1 was likely to have a negligible impact on the tendency to discriminate, and the results in Figure 5 bear that out. The response of large firms to CRA91 is more that double that of medium firms.

Finally, the main hypothesis in the paper is that smaller firms might be less likely to respond to CRA91 due to information inefficiencies. These inefficiencies might arise from broadcast effects: it is difficult to reach all 800,000 non-exempt firms with fewer than five hundred employees. Alternatively the inefficiencies might arise from contagion effects: smaller firms are less likely to belong to the networks and associations that fuel these effects. The figure indicates there is little evidence of such information inefficiency. If anything the opposite appears to be the case. The medium firm response is above the incentives, while the large firm response is below the incentives of firms with more than 500 employees (representing 75% of the employment in that group)³. This may also be an indication that EEO-1 reporting has some impact on suppressing the incentives to discriminate.

8. Conclusion

The goal of civil rights legislation is to create equal opportunity for employees, regardless of race, color, religion, sex, national origin, age and disability. While there have been strides in the employment of protected groups over the last 30 years, I show that the higher damages introduced by CRA91 have increased the implicit wages of protected group employees relative to non-protected employees. The higher implicit wages create incentives for firms to discriminate against protected groups when hiring. I present evidence that firm behavior changed significantly before and after CRA91, and that the magnitude of response differed across firm size. I find that medium and large firm response to CRA91 is consistent with the temporal change in incentives as well as the differences in incentives across firm size. Not only do both size classes reverse their pattern of protected group hiring, they do so in a magnitude consistent with the incentive differences across size. Moreover, there is no

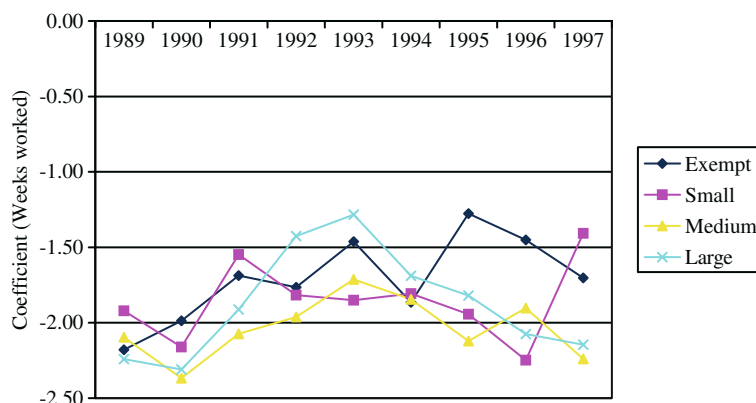


Figure 5. Employment coefficients for protected groups over time.

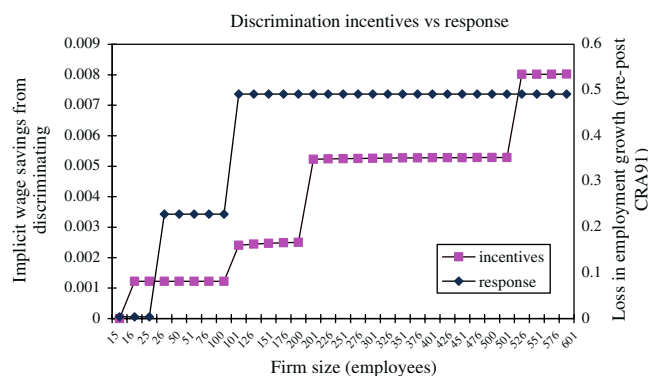


Figure 6. CRA91 incentives versus response across firm size.

significant response to CRA91 from firms in the exempt control group.

These results demonstrate that firms' behaviors match their economic incentives. This has three implications. First, the incentives implicit in the policy are very powerful. Second, firms respond rationally to these incentives. Third, firms appear to be fully informed. Diffusion of the induced discrimination behavior appears to be immediate and complete across firm size. Thus there appears to be no firm size disadvantage in the diffusion of information regarding CRA91.

While the immediate and complete diffusion is "bad news" in this instance because it increases discrimination, this response is due entirely to incentives implicit in the policy that are adverse to its stated goals. CRA91 is inducing rather than suppressing hiring discrimination. The fully

felt response is good news however for better conceived policies. It suggests that mechanisms are in place such that policies with powerful incentives and minimal adoption costs can diffuse almost immediately throughout the population of approximately 950,000 firms with 15 or more employees.

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Notes

¹ Except those firms with exactly 100 employees who are excluded from CRA91 medium firms but included with CPS large firms.

² Mean annual change in weeks worked prior to CRA91 minus the mean annual change post CRA91.

³ While the scales for incentives and responses are different, if both scales are linear, we can make this comparison.

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