

Enforcement of Civil Rights Law in Private Workplaces: The Effects of Compliance Reviews and Lawsuits Over Time

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Has federal antidiscrimination law been effective in moving women and minorities into management? Early studies show that government affirmative action reviews improved the numbers, and rank, of blacks, but evidence of what has happened since 1980 is sparse. There is little evidence that civil rights lawsuits improved the employment status of women or African Americans. We examine establishment-level effects of compliance reviews and lawsuits on the percentage of women and blacks in management. We find that compliance reviews, which alter organizational routines, had stronger and more lasting effects than lawsuits, which create disincentives to discriminate. We also find that deregulation was more consequential for compliance reviews than for lawsuits: Compliance reviews initiated in the 1980s were less effective than

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those initiated in the 1970s. Not so for lawsuits. Compared to lawsuits, compliance reviews appear to have a greater capacity to elicit lasting organizational change, but their effects are mediated by the regulatory environment.

INTRODUCTION

The passage of the Civil Rights Act of 1964 marked an historic change in federal employment policy. Employment had traditionally been viewed through the lens of contract law as based on an implicit or explicit contract between employer and employee. Employers were free to hire whom they pleased and to terminate the contract when they pleased. The Wagner Act curtailed this freedom, guaranteeing the right to collective bargaining and the protection of unionists against retaliatory termination. Title VII of the Civil Rights Act and the executive orders mandating "affirmative action" among federal contractors significantly altered the role of the federal government in the employment relationship. They made it illegal for employers to discriminate on the basis of race, ethnicity, sex, or religion, which suggested that employers could no longer hire whomever they pleased and fire whenever they pleased.

The Civil Rights Act and the executive orders requiring affirmative action carried two principal sorts of enforcement. Civil rights law was enforced through complaints conciliated by the Equal Employment Opportunity Commission (EEOC) or resolved through lawsuits. Employers came to see costly lawsuits as the main threat posed by the law. Affirmative action orders were overseen by an arm of the Department of Labor, the Office of Federal Contract Compliance Programs (OFCCP), which had the power to conduct compliance reviews of contractors. The OFCCP could not sue, but it could debar contractors and ask employers to provide back pay to workers or groups that had not been fairly treated.

These enforcement mechanisms differ in character. Lawsuits discourage discrimination by creating a disincentive. For the neoclassical economist who believes that incentives shape behavior, the lawsuit may look like the ideal enforcement mechanism. Compliance reviews seek to alter personnel routines that might result in discrimination (Anderson 1996). Federal officials survey employment practices and, if they find practices inconsistent with affirmative action orders, request changes. For the organizational sociologist who believes that routines shape behavior, the compliance review, designed to alter employment practices, may look like the ideal enforcement mechanism.

The effect of *regime change* on these two mechanisms of enforcement also differs, for the courts are insulated in the short run from changes in administration. Presidents can alter enforcement by changing the level of regulatory enthusiasm more easily than they can alter enforcement by influencing case law. They can, for instance, alter the intensity of investigations, such as Department of Labor compliance reviews. The Reagan administration's policy

of deregulation had, by some accounts, instantaneous effects when it came to administrative law but weaker and slower effects when it came to judicial enforcement (Leonard 1989).

Does civil rights enforcement through lawsuits and compliance reviews lead to increases in the numbers of women and minorities in good jobs, such as managerial jobs? We have surprisingly little knowledge. A handful of studies found that employers subject to affirmative action law, and those that underwent compliance reviews, were more likely to expand their employment of African American men in the 1970s, but that the effect diminished in the 1980s (Leonard 1990). Two additional studies found effects of lawsuits on the representation of women and minorities in management (Leonard 1984a; Skaggs 2001).

In our study, we seek to provide a more thorough picture of the effects of compliance reviews and lawsuits over time. We analyze data collected annually by the EEOC on workplace composition. We surveyed over 800 work establishments covered by EEOC data to obtain historical information about the experiences these workplaces had with Title VII lawsuits and OFCCP compliance reviews, as well as information about their employment practices. We explore the effects of these government activities on the share of white women, black women, and black men in management positions in American firms between 1971 and 2002. As the occupational category “management” in the EEOC data covers all positions above first-line supervisors, our analysis explores whether the enforcement of antidiscrimination regulations has helped move women and minorities into at least low-level management positions. We find that lawsuits and compliance reviews did have positive effects on all three groups, but the effects of compliance reviews declined in the 1980s following deregulation. Moreover, early compliance reviews had effects that lasted into the 1990s, likely because such reviews caused permanent changes in organizational routines. Compliance reviews initiated after deregulation had more modest and more short-lived effects. Lawsuits had more stable, if weaker, effects over time.

REGULATION AND DIVERSITY: PREVIOUS RESEARCH FINDINGS

Compliance reviews typically point to areas that require attention or improvement. The OFCCP can choose to review any federal contractor it likes, and the agency claims to target employers who, based on their EEO-1 reports, have significantly underperformed in the employment of women and minorities, given the local labor market and the employment patterns of others in the industry (Anderson 1996, 298). Leonard (1984b), however, shows that compliance reviews do not necessarily target the organizations that have the greatest problems.

The goal of the compliance review is to inventory employer practices, to assess annual affirmative action plans and the progress being made in achieving affirmative action goals, and to encourage the employer to adopt approaches to recruitment, hiring, and promotion that are in keeping with these goals. When the OFCCP discovers that an employer is not acting in compliance with its own affirmative action plan, the OFCCP attempts conciliation. The latter can cover new recruitment, hiring, and promotion practices, and can include back pay for groups of protected employees. Before 1978, these reviews were conducted by the contracting agencies under the oversight of the Office of Federal Contractor Compliance (OFCC), but in 1978 the Carter administration centralized responsibility for compliance reviews in the OFCCP.

Lawsuits produce another sort of regulatory effect. They create an immediate incentive to prevent discrimination because they make real the threat of financial loss. Even unsuccessful lawsuits are often costly in terms of legal expense and lost business due to negative publicity. Skaggs (2001) describes the effect in terms of resource dependence theory, but the upshot is very much that lawsuits create an immediate incentive to stop discriminating in order to avoid further suits. We expect lawsuits to be effective, but not as effective as compliance reviews, which are designed to create permanent changes in organizational routines.

Civil rights law and affirmative action edicts have been in force, in something approximating the present form, since the mid-1960s, yet we know little about how effective they have been (Donohue and Heckman 1991). Most of what we do know comes from studies comparing contractor and noncontractor firms designed to sort out the effects of affirmative action status. Six studies show that contractor establishments saw greater growth of minority employment through the 1970s than did noncontractor establishments (Ashtenfelter and Heckman 1976; Goldstein and Smith 1976; Heckman and Payner 1976; Heckman and Wolpin 1976; Leonard 1984b, 1984c). These studies use data collected by the EEOC, which are the same federal data that we use. Each study examined a large sample of employers over a four-to-six-year period ranging from 1966 to 1980. In general, black employment rose more quickly in contractor establishments than in noncontractor establishments. For example, Leonard (1984b) finds that between 1974 and 1980, among contractor firms, black male employment rose from 5.8 percent to 6.7 percent and black female employment rose from 3 percent to 4.5 percent, while white male employment fell from 58.3 percent to 53.3 percent. In noncontractor firms, black male employment rose from 5.3 percent to 5.9 percent, black female employment rose from 4.7 percent to 5.9 percent, and white male employment fell from 44.8 percent to 41.3 percent. Contractors had more black men and fewer black women on their payrolls at the outset, but they added more of each group over time. During the same period, contractors also added more black men and women to their managerial ranks as compared to noncontractors (Leonard 1990, 53). In contrast to black men

and women, white women saw negligible gains from affirmative action during this period (Leonard 1989, 65).

Three studies show that federal OFCCP compliance reviews had significant effects on the growth of black employment and the movement of black men and women into better jobs, over and above the effect of being a federal contractor (Goldstein and Smith 1976; Leonard 1984b, 1984c). These studies use time-series methods, modeling change in employment of blacks or controlling for previous levels, and each includes five or six other control variables: size, region, industry, white collar, corporate structure, and growth. Leonard (1984b) finds that during the period 1974–1980, compliance reviews advanced black male employment by 7.9 percent and black female employment by 6.1 percent. Here, too, the effects on white women were negligible (Leonard 1984b, 451). Compliance reviews had a more modest impact on occupational upgrade, with significant positive effects only for black men. In another study, Leonard looks at which sorts of contractor activities contributed to the growth of female and minority employment between 1974 and 1980. He looks at the effects of conciliation agreements, show-cause hearings, and affirmative action employment goals. He finds that affirmative action goals were the only significant predictor of increases in female or minority employment, although employers never fulfilled the employment goals they had established (Leonard 1985b, 9). The effect of being a contractor on black employment growth disappeared in the early and mid-1980s, coincident with the Reagan administration's new policy of deregulation (Leonard 1990, 58). The only study that uses EEO-1 data to examine the effectiveness of the contractor compliance program after the 1980s was conducted by Rodgers and Spriggs (1996), who utilize a series of cross-sectional models for the years 1979, 1982, 1983, 1984, 1988, 1989, 1990, and 1992. They find that the effect of being a contractor on African Americans' employment was larger in 1992, three years after the end of the Reagan administration, than it was in 1982 (a 1.36 percentage point increase for a mean of 11.7 percent versus a 0.83 percentage point increase for a mean of 10.3 percent, respectively).

How might affirmative action plans and compliance reviews have influenced the hiring and promotion of blacks? Harry Holzer and David Neumark (1998) offer some evidence based on a survey of employers. Employers with recruitment systems designed to pursue affirmative action: (a) utilize more elaborate recruitment and screening practices; (b) are more willing to hire disadvantaged applicants; (c) receive more applications from women and minorities; (d) are more likely to provide training to employees; and (e) are more likely to use formal performance evaluations to assess employees. Konrad and Linnehan (1995) find that employers that have faced compliance reviews are more likely to use diversity-conscious personnel policies and to take positive steps to recruit and hire women and minorities. Thus, it looks like compliance reviews may encourage employers to change some of their hiring tactics and to be more receptive to hiring people from underrepresented groups.

Evidence to illustrate the effects of lawsuits is even sparser than evidence to show the effects of compliance reviews. Because of the near universal coverage of Title VII of the Civil Rights Act (only employers with fewer than fifteen workers are exempt), almost no reliable research has been done on the effectiveness of this act (Donohue and Heckman 1991). Leonard (1984a) analyzes two panels of cross-sectional EEO-1 data from 1966 and 1978 and finds that the number of Title VII class action suits per corporation in an industry-state cell is associated with a significant improvement in the employment of blacks, especially black women and especially in professional and managerial occupations. Here, too, as with the case of affirmative action enforcement, the effect on white women is negligible. He concludes that Title VII litigation created pressures for employment, as well as occupational advancement during the 1970s (Leonard 1984a, 151). This finding is consistent with Donohue and Siegelman's (1991) argument that lawsuits were relatively rare but relatively effective in the 1970s, and with time they became both more common and less effective. They attribute the reduction in efficacy to two causes. On the one hand, early lawsuits put an end to the most egregious forms of discrimination, affecting employers who were actually sued and those who took lessons from lawsuits against others. By the 1980s, the worst practices had been eliminated, leaving less room for improvement. In our models, we include a dummy variable for each year during the period under study in order to absorb the variance in managerial diversity coming from secular changes in the nature of discrimination. On the other hand, lawsuits were increasingly about firing rather than hiring. Because members of protected groups are more likely to sue for discrimination in firing under Title VII, Donohue and Siegelman suggest that later discrimination suits actually provided a disincentive to employers to hire women and minorities.

Skaggs (2001) uses supermarket EEO-1 reports for 1983 to 1998 to examine the effects of lawsuits on workplace diversity. She finds that in "progressive" federal court districts, supermarkets that experience discrimination lawsuits subsequently move more women and Latinos, but not African Americans, into management positions. There is little effect in other court districts, suggesting that employers are sensitive not only to the risk of lawsuit but also to variation in the legal environment across federal court districts. Following lawsuits, competitor supermarkets also move more women, but not African Americans, into management. However, the proportion of women in management in supermarkets that experience discrimination lawsuits, and of women and African Americans in competitor supermarkets, *declined* following a settlement or award associated with litigation, supporting the possibility of retaliation, as was suggested by Donohue and Siegelman (1991).

We follow in the tradition of these studies, examining how effective compliance reviews and lawsuits are in altering management composition in a large national sample of employers, for the period 1971 to 2002. We control for the kinds of organizational factors and labor market characteristics

to which sociologists and economists typically point. And, having both lawsuits and compliance reviews in the model, we control for the effect of each. We ask: Do organizations that experience compliance reviews, or lawsuits, subsequently see increases in the representation of women and African Americans in management? Does the regulatory environment under which the compliance review, or lawsuit, first occurs make a difference?

ENFORCEMENT AND ORGANIZATIONAL ROUTINES

Sociologists and economists have taken different paths to understanding discrimination and its remediation. Economists have been interested in the costs of discrimination (Becker 1964) and in how public policy can create disincentives to discrimination (Leonard 1990), as well as in the role of human capital in determining life chances. Economists have been skeptical of the effects of federal policy. As Donohue and Heckman (1991, 1604) note,

[while there is] evidence of sustained improvement in black relative earnings . . . [t]he case for a government policy effect violates . . . widely held canons of current professional standards. First, it suggests that something other than “basic economic forces” accounts for an important economic phenomenon.

Sociologists were not constrained by this canon, but early studies of inequality in employment did not explore public policy or organizational routines. Using individual-level data to understand inequality in education, occupation, and income, sociologists consistently found effects of race and gender on attainment, which suggests discrimination (Blau and Duncan 1967; Featherman and Hauser 1978; Bielby 1981). But it was not until the early 1980s that the organizational literature on hiring and promotion routines began to converge with the stratification literature to generate “structural” analyses of stratification, in which sociologists analyzed the organizational processes that lead to different employment outcomes for men and women, blacks and whites (Baron and Bielby 1980; Baron 1984).

We build on the sociological insight that organizational structures and routines explain much of the difference in attainment by gender and race. Structural discrimination theorists explore how recruitment and hiring routines may sort women and blacks into jobs with poor opportunities for training and promotion (Reskin 2000; Reskin and McBrier 2000; Elvira and Town 2001). If organizational practices and routines are key to understanding why some groups get ahead and others do not, then public policies that target practices and routines may help to equalize opportunity among groups. We test this hypothesis alongside the hypothesis that financial incentives change organizational behavior. Our thinking is that significant changes to organizational

practices and routines may have lasting effects on inequality. We expect incentives to have effects as well, but we expect that these effects would be less likely to be sustained.

ENFORCEMENT AND ADMINISTRATIVE REGIMES

The Reagan Administration's Experiment in Deregulation

Our longitudinal data allow us to pursue a twofold research question. First, we observe establishments before and after compliance reviews and lawsuits. Second, we look at the effects of compliance reviews and lawsuits under different federal regulatory approaches. The Reagan administration pursued a broad policy of deregulation under the theory that regulation was stifling business and was not achieving the desired goals. The theory of deregulation suggested that intensive, fine-grained regulation of business led firms to opt out of compliance altogether or to focus on esoteric short-term goals and lose sight of underlying goals. Either response, deregulation theorists argued, would lead to regulatory failure. Goals, such as improved safety, a cleaner environment, and reduced discrimination, would be elusive under intensive regulation. At the heart of the theory was the idea that markets could better regulate organizations than could the government.

Under the new theory of deregulation, the government would establish general goals, advise the public of the performance of firms, and allow firms latitude in compliance. Strict standards and intrusive inspections would be replaced by sunshine policies, designed to shine light on organizational failures, and by market forces, which would give workers and customers the opportunity to stop doing business with (or working for) dangerous, polluting, discriminatory organizations. The theory was backed by an intellectual movement among public policy experts. In the 1960s and 1970s, economists at the Federal Communication Commission, the Civil Aeronautics Board, and the Interstate Commerce Commission turned their sights on industry regulation and its effects. They found regulation to be intrusive, ineffective, and inefficient (see McCraw 1975). A theory about the effects of deregulation began to form, with cogent arguments about why regulation might be counterproductive (Mitnick 1978; Viscusi and Zeckhauser 1979; Diver 1980). Kip Viscusi's *Risk by Choice* (1983) chronicled the ludicrous micromanagement of the Occupational Safety and Health Administration (OSHA), and the utter absence of progress on safety and health in the 1970s, when OSHA was implementing detailed standards on everything down to the height of stair railings. The title, *Risk by Choice*, captures the driving idea that knowledgeable consumers and employees will choose acceptable levels of risk, and thus the market can bear more of the brunt of regulation. Bardach and Kagan's *The Problem of Regulatory Unreasonableness* (1982) charted the inefficiencies

produced by excessive environmental regulation. The idea of voluntary compliance dates to the nineteenth-century British experiments with sunshine commissions, designed (following Jeremy Bentham's arguments in *Principles of the Civil Code* [1840]) not to establish standards for factories, railroads, and other private enterprises, but to make the public aware of the performance of these enterprises and of the risks to employees and customers. The market was to do the rest (MacDonagh 1958; Lubenow 1971).

Ronald Reagan campaigned on a platform of promoting economic growth via deregulation. Once in office, Reagan sought to turn over more control for compliance with antidiscrimination laws to employers themselves (Leonard 1990, 1996; Anderson 1996). Under the theory of deregulation, more realistic and less intrusive compliance goals would increase the number of employers seeking to comply and would permit employers to comply as they saw fit rather than becoming fixated on esoteric and irrelevant compliance criteria established by bureaucrats. Reagan's newly appointed EEOC head, Clarence Thomas, directed the agency's general counsel not to approve conciliation agreements that included employment goals and timetables, giving employers more latitude (Blumrosen 1993, 270; Skrentny 1996). The EEOC sponsored fewer of the conciliation agreements that directed employers to make specific changes (Blumrosen 1993; Yakura 1995).

The Department of Labor's OFCCP increased the number of compliance reviews significantly, but reduced the use of sanctions (Leonard 1989). It also reduced the size of the staff by half, with the overall effect that compliance reviews were more rapid and less intrusive than they had been. OFCCP staffing remained low. In 1996, it was at 835, still only about half of the 1978 level of 1,700 (DuRivage 1985, 364; Edelman 1992, 301; Anderson 1996; Skrentny 1996). Reagan's OFCCP chief, Ellen Shong, eliminated the agency's more aggressive enforcement measures, such as the practice of targeting visible industries for compliance reviews (DuRivage 1985). Data on agency activity show a broad pattern of deregulation. Contractor debarments declined from thirteen in fiscal years 1977–1980 to four in fiscal years 1981–1985. The number of persons receiving back pay declined from 4,336 in 1980 to 499 in 1986. Conciliation agreements following violations declined from 49 percent in 1980 to 33 percent in 1985 (Anderson 1996, 300; Blumrosen 1993, 274). These changes in enforcement led Leonard to conclude that “an administration lacking the will to enforce affirmative action beyond rubber-stamped compliance reviews has resulted in an affirmative action program without practical effects since 1980” (Leonard 1989, 74). Meanwhile, Reagan's Department of Justice began to file *amicus* briefs supporting the challengers of federally mandated affirmative action plans. In two of those cases, *Firefighters Local Union No. 1784 v. Stotts* (1984) and *Wygant v. Jackson Board of Education* (1986), the Court ruled against plans that suspended seniority rules to retain minority workers during layoffs.

Reagan administration officials proposed other changes in line with their theory of deregulation. Though these proposals failed, they sent the message

that the administration favored maximizing the discretion of private sector employers. For example, a proposed change to the OFCCP's Revised Order 4 (47 FR 17770, April 23, 1982) would have reduced the number of companies required to submit affirmative action plans by three-quarters, from about 16,767 to about 4,143 (Kelly and Dobbin 2001). Reagan's cabinet also debated revising Johnson's Executive Order 11246 to stipulate that contractors were not required to develop numerical goals and timetables (Detlefsen 1991, 151; see also Belz 1991).

During the 1980s, OFCCP compliance reviews became more frequent, but they became less intrusive and less likely to result in sanctions. Despite an increase in the number of reviews, there was a decline in the total number of sanctions (debarments, back pay awards). We expect the change to affect workforce composition, largely because the compliance reviews of the 1970s led employers to change their routines, and these more cursory reviews should not have had that effect. The literature in economics on responses to litigation suggests additional reasons. Ronald Edwards (1991) tested regulation theories from economics in a survey of corporate reactions to OFCCP and EEOC interventions. Since the early work of Gary Becker (1968) on the deterrent effects of incarceration, economists have theorized about the effects of two factors—the likelihood of apprehension and the severity of punishment—on corporate regulatory compliance. Edwards found that the severity of the punishment played a more central role in motivating managers to comply with antidiscrimination laws than did the certainty of detection. In the case at hand, the deregulation experiment appears to have increased the likelihood of oversight and detection (by increasing the number of compliance reviews), but decreased the severity of punishment. If the higher severity of punishment is what drove the effects of compliance reviews in the 1970s, rather than changes in personnel routines, we would expect to see similar effects for lawsuits and compliance reviews, as both would be presumed to operate through the same deterrence mechanism.

In sum, as compared to other arenas of employment law (e.g., maximum hours, minimum wage), compliance with civil rights and affirmative action law was difficult to judge. No litmus test was set out in Title VII or in the executive orders, no set of practices that would establish a firm's compliance. Instead, there was the vague mandate to eliminate discrimination. To encourage compliance, Title VII permitted lawsuits, and the presidential orders mandating affirmative action established compliance reviews for contractors. Employees, or the EEOC, could sue employers. The OFCCP could initiate compliance reviews of contractors. Compliance reviews were more intrusive, more likely to result in sanctions, and more likely to result in suggested changes in corporate employment practices during the 1970s. The policy of deregulation pursued in the 1980s should, we expect, reduce the efficacy of compliance reviews. We do not expect to see the same reduction in the efficacy of lawsuits, because the role of the judiciary changed slowly with the appointment of

new judges committed to deregulation. Evidence from a study of regulatory and voluntary approaches to compliance with clean water standards suggests that regulation will be more effective than deregulation, although voluntary standards may create a stronger sense of duty to comply (May 2005).

Predictions

Our expectations are twofold: First, compliance reviews alter employer hiring and promotion routines, and hence will have sustained effects on increasing the diversity of the managerial workforce. We expect lawsuits to show more modest long-term effects. Lawsuits act as disincentives to discriminate, rather than leading to changes in personnel routines, and we expect the effects of those disincentives to erode with time. Second, we expect the effects of compliance reviews to decline after 1981, with deregulation, but we expect the effect of lawsuits not to be sensitive to regime changes, because deregulation was not as consequential for the judicial branch as it was for the administrative branch.

Hypothesis 1: Compliance reviews, designed to alter organizational personnel routines, will have more lasting effects than lawsuits, which create incentives to avoid discrimination that may erode over time.

Hypothesis 2: Compliance reviews conducted after deregulation will have more modest effects than compliance reviews conducted earlier. This will not be the case for lawsuits, which are less sensitive to regime change.

OTHER FACTORS AFFECTING MANAGERIAL COMPOSITION

In order to assess the net effect of Title VII lawsuits and OFCCP compliance reviews, we include in the analysis measures of other regulatory activities, as well as additional sources of change in management composition that are rooted in organizational structure and demography, and the legal and economic environments. We provide a brief discussion motivating the inclusion of each of these factors below.

Other Regulatory Activities

EEOC Charges

Title VII originally gave the new Equal Employment Opportunity Commission no power of enforcement. The commission was to investigate

complaints of discrimination and seek voluntary conciliation where it found discrimination (Shaeffer 1973, 6). Individuals had the right to bring suit, but the EEOC had no authority to bring suit or to otherwise impose sanctions. The attorney general could bring suit in cases where a “pattern or practice” of resistance to Title VII was identified, but this power was rarely used (Shaeffer 1973, 7). The EEOC could draw up guidelines for nondiscrimination, but these guidelines had no legal status. Congress amended the powers of the EEOC so that from 1972 it could bring pattern and practice suits itself. Title VII enforcement came down to EEOC charges and conciliation, on the one hand, and lawsuits initiated by individuals or by the EEOC itself, on the other. We control for the effects of EEOC charges so we can assess the net effects of lawsuits.

Affirmative Action Status

The OFCCP required every employer with at least fifty workers and a government contract worth \$10,000 to provide regular reports on workforce composition. Employers with \$50,000 in contracts were also to write affirmative action plans. The agency encouraged employers to take steps to end discrimination and specified procedures for debarring employers from future contracts (DuRivage 1985, 362). As discussed above, studies from the late 1960s and the 1970s showed that black men and women saw larger employment gains and occupational advances among contractors than among noncontractors (Leonard 1989, 1990). Apart from compliance reviews, the main enforcement activity of the OFCCP was established in a 1971 order requiring contractors to write affirmative action plans. This meant that contractors are to annually evaluate their own workforces, specify goals for the fair representation of women and minorities based on their own labor market analyses, and sketch timetables for achievement of those goals (Shaeffer 1973, 66; Hammerman 1984, 12). The 1971 order specified that goals could not be used as quotas (U.S. Department of Labor 1992), and Leonard (1985b) indeed finds that employers virtually never meet the goals they set. This pattern suggests that, contrary to common perceptions of affirmative action, goals do not operate as quotas.

Affirmative action status does not fully predict whether an employer has an affirmative action plan. Employers are not required to file these plans with the OFCCP, and some employers simply do not create plans (Bureau of National Affairs 1986). On the other hand, noncontractors sometimes voluntarily prepare affirmative action plans, either in preparation for applying for a government contract or to symbolize their openness to diversity (Reskin 1998). Our survey data show that 7 percent of the covered employers never wrote such plans, and 20 percent of establishments that were never contractors wrote plans. We therefore include in the analysis two separate measures for

affirmative action status: first, whether an employer had at least fifty workers and a government contract worth \$50,000 or more; and second, whether an employer wrote annual affirmative action plans.

Organizational Characteristics

Grievance Procedure

Formal grievance mechanisms were one of the first innovations that employers adopted to intercept and remedy discrimination complaints internally (Edelman 1990). These internal systems could preclude lawsuits; hence, we include a measure for their presence in our analysis.

Formalization of Personnel Policies

Formal personnel systems are expected to limit managerial discretion and thereby curtail discrimination. Reskin and McBrier (2000) find formalization to increase managerial diversity. Others contend that bureaucratic practices can exacerbate inequality by creating separate career trajectories for different groups (Baron and Bielby 1985; Baldi and McBrier 1997; Elvira and Zatzick 2002). The effect of formalizing personnel policies on managerial diversity could thus go either way.

Diversity and Work-Family Programs

Previous studies have found that diversity programs increase workforce diversity (Baron, Mittman, and Newman 1991; Holzer and Neumark 1998; Edelman and Petterson 1999; Kalev, Dobbin, and Kelly 2006). Firms that adopted work-family programs, designed to make it easier for employees to handle work and family demands, may experience growth in women managers (Williams 2000).

Unionization

When union contracts specify seniority provisions, they tend to favor longstanding employees, who are usually white and male (Milkman 1985; Baron et al. 1991; Blau and Beller 1992; but see Leonard 1985). However, some unions have championed work-family programs that can help women pursue career goals (Osterman 1995; Kelly 2003). The effect of unionization on managerial diversity could therefore go either way.

Organizational Demography

Organizational Size

As organizations grow, they add more functions and vacancies, and thus should be able to expand the ranks of women and minorities in general and in management. Growth also renders organizations more visible and thus averse to appearing discriminatory. Conversely, employment growth may be an indication that an organization is successful, and this may mean that managerial jobs are more desirable and thus less likely to go to women and minorities (Reskin and Roos 1990). Research evidence is mixed (for example, Bielby and Baron 1986; Leonard 1990; Baron et al. 1991).

Diversity of Top Management Team

The gender and racial composition of management is likely to reproduce itself through homosocial reproduction (Kanter 1977), social closure (Tomaskovic-Devey 1993), or network effects (Burt 1998; Reskin and McBrier 2000). We thus expect that the diversity found in top management will affect future managerial composition.

Diversity of the Internal and External Labor Pools

Diverse labor pools provide diverse candidates for managerial jobs (Reskin and Roos 1990; Shenhav and Haberfeld 1992; Cohen, Broschak, and Haveman 1998) and may also enhance norms of inclusiveness among employers (Blum, Fields, and Goodman 1994, 245). We expect that diverse internal labor pools, as well as diverse state and industry labor markets, will lead to higher representation of women and minorities in management.

Availability of Managerial Jobs

Employers with small managerial cadres may find it difficult to hire from disadvantaged groups without harming opportunities for white men (Baron et al. 1991; Tomaskovic-Devey and Skaggs 1999), whereas growth in managerial ranks has been shown to increase diversity (Blum et al. 1994). Konrad and Linnehan (1995) and Leonard (1990, 52) find that an increased demand for managers positively affected white women more than it did African Americans. We expect an increase in the availability of managerial jobs to have a positive impact on managerial diversity.

Organizational Environment

Legal Environment

We include in our analysis measures for two additional elements of the legal environment that may affect managerial diversity. The first measure is legal awareness. Employers who employ legal counsel are likely to be more aware of regulations and to make greater efforts to avoid litigation, either by “bullet proofing” personnel policies (Bisom-Rapp 1999) or by appointing women and minorities to management jobs (Donohue and Heckman 1991; Leonard 1996; Skaggs 2001). The second measure pertains to the effect of the legal environment on the labor market. Employers who operate in industries with a high concentration of government contractors, covered by the Office of Federal Contract Compliance Program (OFCCP), may face stiffer competition over women and minorities, which means that they may find it more difficult to hire women and minorities. Hence, we include a measure of industry concentration of government contractors.

Economic Environment

Higher unemployment rates have been found to be correlated with increased litigation (Donohue and Siegelman 1991). When unemployment is high, vacancies in managerial jobs are rare and more often go to white men, who are better positioned in labor queues (Reskin and Roos 1990). Therefore, we expect lower managerial diversity when unemployment is high.

Industry Size

Growth in industry employment indicates market success, which means that managerial jobs are more attractive and therefore more likely to be desired by white men (Reskin and Roos 1990). Growth in industry employment may also indicate new opportunities for women and minorities. Because we include measures of growth in the proportion of each demographic group in the industry labor force, we expect that growth in industry employment will be associated with the increased presence of white men in management.

DATA AND METHODS

We use longitudinal data on establishment workforce composition, regulatory events, and employment practices. In fixed-effects models, we estimate the effects of compliance reviews and Title VII lawsuits on the log

odds of white men, white women, African American women, and African American men in management between 1971 and 2002. To explore how enforcement regimes shape the effectiveness of regulatory events, we compare the effects of having a first compliance review, or a first lawsuit, in three periods spanning multiple administrations: the interventionist enforcement era of the 1970s, the deregulation era that began in 1981, and the period that began with the next change of party in the White House. George H. W. Bush advocated a more interventionist regulatory approach than Reagan had advocated, but we cut off the period at the start of Bill Clinton's administration to allow for periods of roughly equal length. The results were substantially similar when we break the second period after Reagan and after G. H. W. Bush. Compliance reviews and lawsuits from the 1980s and 1990s show similar effects.

In these analyses, we explore the effect, for instance, of having a first compliance review in the 1970s on management composition during the interventionist years, during the deregulation era, and during the following period of increased regulation. Our prediction is that compliance reviews initiated under deregulation will have meager effects not only in the short term but also thereafter.

Data

EEOC Data

Federal law requires private employers¹ with more than one hundred employees, and government contractors and subcontractors with more than fifty employees and contracts worth \$50,000, to file annual EEO-1 reports. These reports detail the race, ethnicity, and gender of employees for nine broad occupational categories. We obtained these data for research purposes from the EEOC under an agreement provided by the Intergovernmental Personnel Act (IPA). Given that the occupational categories defined on the EEO-1 forms are broad, these data are not useful for studying intraoccupational segregation (Baron and Bielby 1985) and the upward movement of women and minorities within managerial ranks.² Our analyses therefore

1. Excluded employers, such as state and local governments, primary and secondary school systems, and institutions of higher educations, provide different reports (EEOC n.d.).

2. Some also argue that employers reclassified jobs, particularly in the 1970s, to improve their reports on women and minorities in management (Smith and Welch 1984). If this is the case, the changes we observe in managerial composition do not represent a real change in the careers of women and minorities but rather a mere shift in reporting. However, Jacobs (1992) finds evidence on reduced gender-earning gaps in management alongside the growth in the representation of women in management. And Leonard finds no evidence for reclassification in the EEO-1 data (Leonard 1990, 53). We examined the robustness of our findings to reclassification by eliminating cases of unusually large growth of managerial diversity. This did not alter the results reported in this article.

inform us best about the representation of women and minorities at the lower ranks of management. Yet, these are by far the best data available for our questions (for a discussion of the comparability of these data with other national labor market data, see Robinson et al. 2005).

Organizational Survey Data

We used the EEO-1 dataset for the year 1999 (the latest year of data available when we were sampling) to draw a random sample of establishments to survey. We stratified the sample by the number of years the establishment appeared in the EEO-1 data to guarantee that there would be a sufficient number of workplaces with data for multiple years. We eliminated establishments that did not contain EEO-1 data from 1992 forward, and chose 50 percent of cases from establishments with EEO-1 data at least that far back, and 50 percent from establishments with data dating at least back to 1980. We also stratified by size, selecting a sample in which 35 percent of establishments had fewer than five hundred employees in 1999, and by industry in order to ensure both representativeness and a comparison group for each workplace. We drew samples from food manufacturing, chemical manufacturing, computer equipment manufacturing, transportation equipment manufacturing, wholesale trade, retail trade, insurance, business services, and health services. We replicated the analyses reported here with weights based on the inverse probability that an establishment in each stratum of industry, size, and length in the EEO-1 dataset would complete the survey. The weights did not alter our findings. We report unweighted results here. The sampling unit was the establishment (i.e., a single location of a firm or a firm with a single location). Only one establishment per parent firm was sampled. We analyze establishments, regardless of their parent firms, as a means for controlling for the effects of acquisitions, mergers, and spinoffs on diversity.

The retrospective survey of employment practices was managed by the Princeton Survey Research Center and funded by the National Science Foundation and the Russell Sage Foundation. Before composing the survey instrument, we examined the wording and findings of other recent employment surveys (in particular Appelbaum, Bailey, and Berg 2000; Kelly 2000; Osterman 2000). We also conducted forty-one in-person interviews with human resources managers from a random sample of organizations in four different regions, as well as twenty pilot phone interviews. Data from these exploratory stages were not included in the analyses.

In 2002, interviewers completed 833 interviews for a response rate of 67 percent, which compares favorably to similar surveys (Osterman 1994;

Kalleberg et al. 1996; Kelly 2000; Osterman 2000).³ The average interviewee was a human resources manager with eleven years of tenure with the establishment. The survey included questions on compliance reviews and lawsuits. We asked whether the organization had experienced either compliance reviews or lawsuits, when they had experienced the first, and how many reviews and lawsuits they had experienced in total. We also took histories of other personnel practices and organizational characteristics so that we could parcel out the net effects of compliance reviews and lawsuits. When a respondent could not answer a particular question, we sent her a copy of that question so that she could consult records and colleagues. We then followed up to obtain the missing answer. Despite this effort, we are missing information on the dates of adoption for 3 percent to 4 percent of the organizational practices variables, such as grievance procedures and diversity programs. A larger number of respondents could not obtain information about the dates of their organizations' first compliance review or lawsuit. Eighteen percent of those reporting compliance reviews could not tell us when the first review occurred. Thirty-two percent of those reporting lawsuits could not tell us the dates of the first suit. Missing values for dates of changes in organizational practices and of regulatory events were imputed by means of Ordinary Least Squares (OLS) regression, with industry, establishment age, and type of establishment as covariates. We did not impute the *presence* of a program or legal event; rather we imputed the year of adoption where that variable was missing. To test for robustness, we replicated the analyses reported below and omitted organizations with estimated data. The substance of the results did not change. For the review and lawsuit variables, standard errors generally decreased, and in some cases, coefficients became significant at the 0.05 level. This greater efficiency suggests that the estimated dates introduced some error, which was reduced by eliminating estimates. The models in which we omitted estimated dates provide marginally stronger support for our own hypotheses than do the models reported below. For instance, without estimated data, the effect of a compliance review from the 1970s on the presence of white men in the 1990s was significant and negative. We chose to present models with estimated data to avoid selectivity bias if the likelihood of a respondent not knowing the year of a compliance review or lawsuit was correlated with workforce diversity at her establishment.

Upon completion of the phone survey, we matched the survey data for each establishment with the corresponding annual EEO-1 records and removed all identifying information to guard confidentiality. The final dataset used in this analysis contains 814 cases and 18,474 establishment-years, with

3. We examined the possibility that respondents are different than nonrespondents by including in the models predicted values from a logistic regression of the likelihood of responding to the survey as a function of industry, establishment type (a headquarters, subunit, or stand-alone organization), size, contractor status, managerial diversity, and contact person's rank. This did not change our results (results are available upon request).

a median of twenty-three years of data for each establishment, a minimum of five years, and a maximum of thirty-two years.⁴ We supplemented this dataset with annual national, state, and industry labor market data from the Bureau of Labor Statistics.

Dependent Variable: Managerial Diversity

We examine four outcomes: the proportions of white men, white women, African American women, and African American men among managers in the establishment in a certain year. These variables are calculated from the EEO-1 data. Because there are large differences in the absolute magnitude of the change in the outcome variables across groups (e.g., white women varied between 15 percent and 26 percent in the average workplace, and African American women varied between 0.4 percent and 2 percent), we follow the convention of transforming these proportions into the log odds of each group in management.⁵ We chose to use log odds rather than log proportions because the distribution of log odds is closer to normal.

Independent Variables: Regulatory Events

We are interested in the effects of OFCCP compliance reviews and of Title VII lawsuits on managerial diversity. Overall, 66 percent of the employers we surveyed experienced lawsuits. Our survey question includes all Title VII lawsuits, including those that ended up in settlements. Thirty-two percent experienced compliance reviews. Overall, 48 percent of the organization-year spells covered federal contractors. Some organizations were consistently contractors, some were consistently noncontractors, and some moved in and out of the status. Because we expected compliance reviews to have lasting effects, even among firms that moved in and out of contractor status, we did not perform separate analyses for federal contractors but analyzed instead all of the establishment-years in the same models, while including a measure for current contractor status.

4. For ten cases, usable EEO-1 data exist for less than four years. These cases were excluded from the analysis. For an additional seven interviewed establishments, the survey data were unusable.

5. Log odds (i) = $\text{Log} (P_i / 1 - P_i)$, where P_i is the proportion of group i in management. Because the log odds, or logit, is undefined when $P_i = 0$ or $P_i = 1$, we substituted 0 with $1/2N_j$, and 1 with $1 - 1/2N_j$, where N_j is the number of managers in establishment j (Hanushek and Jackson 1977; Reskin and McBrier 2000). The results were robust to different strategies for substituting zeros. We chose the one that kept the distribution unimodal and closest to normal. Finally, the models include a binary variable that equals 1 when there were no managers from the focal group.

Employers who were reviewed or sued once were often subject to subsequent reviews or lawsuits. Among those that experienced lawsuits, the median number of lawsuits was six. Among those that experienced reviews, the median was two. In our pilot survey, we discovered that it was relatively easy for respondents to find out when their organizations first went through a compliance review or lawsuit but that organizational memory about the exact dates of subsequent events was not reliable. Thus, for subsequent events, we asked about the total number and estimated their dates by spacing them regularly between the reported date of the first review or suit and the survey date.

Independent Variables: Other Factors

Table 1 presents the means, standard deviations, definitions, and data sources for all variables. All variables vary annually. Binary variables are coded as 0 in all the organization-year cells before adoption or occurrence (and after revocation, if relevant) and 1 in the year of adoption and thereafter. We use fixed establishment effects to account for unobserved stable organizational characteristics. Variables that do not vary with time, such as industry or state, are therefore excluded from the analysis, but the variance stemming from these factors is accounted for by the establishment fixed effects. We use fixed-year effects to account for unmeasured environmental changes that may have affected all establishments alike.

Other Regulatory Activities

We use information from the EEO-1 data to measure whether and in what years the establishment was under the Federal Contractor Compliance Program. Adoption of an Affirmative Action Plan and experience with EEOC Charges are measured as binary variables based on the survey data.

Organizational Characteristics

The presence of nonunion (civil rights) Grievance Procedures is measured with a binary variable, based on survey data. Three count variables sum up a series of human resources practices. Each varies annually. Formal Human Resources Policies is a count variable consisting of written hiring, promotion, and discharge guidelines; job descriptions; promotion ladders; performance evaluations; pay grade system; and internal job postings. Diversity Programs is a count of six types of diversity programs: diversity staff or department; diversity committee; diversity included in managerial performance evaluations;

TABLE 1
Means and Standard Deviations of Variables Used in the Analysis of Enforcement Mechanisms and Managerial Workforce Composition

	Mean	Standard Deviation	Minimum	Maximum	Data	Type
Outcome Variables (proportion)						
Managers who are white men	0.702	0.236	0	1	EEO-1	Continuous
Managers who are white women	0.219	0.213	0	1	EEO-1	Continuous
Managers who are black women	0.013	0.040	0	0.667	EEO-1	Continuous
Managers who are black men	0.024	0.058	0	1	EEO-1	Continuous
Legal Interventions						
1st compliance review	0.149	0.356	0	1	Survey	Binary
1st compliance review was in the 70s	0.045	0.208	0	1	Survey	Binary
1st compliance review was in the 80s	0.085	0.278	0	1	Survey	Binary
1st compliance review was in the 90s	0.029	0.167	0	1	Survey	Binary
Number of Compliance Reviews	0.598	1.991	0	35	Survey	Continuous
1st discrimination lawsuit	0.341	0.474	0	1	Survey	Binary
1st lawsuit in the 70s	0.074	0.261	0	1	Survey	Binary
1st lawsuit in the 80s	0.218	0.413	0	1	Survey	Binary
1st lawsuit in the 90s	0.050	0.218	0	1	Survey	Binary
Number of lawsuits	4.669	10.215	0	50	Survey	Continuous
Other Regulatory Activities						
1st EEOC charge	0.315	0.464	0	1	Survey	Binary
Affirmative action status (government contractor)	0.482	0.500	0	1	EEO-1	Binary
Affirmative action planBinary	0.451	0.498	0	1	Survey	Binary

TABLE 1 Continued

	Mean	Standard Deviation	Minimum	Maximum	Data	Type
Organizational Structures						
Grievance procedure	0.358	0.479	0	1	Survey	Binary
Formal hiring/promotion practices	4.892	2.544	0	9	Survey	Count
Diversity programs	0.301	0.735	0	4	Survey	Count
Work-family programs	0.910	0.983	0	4	Survey	Count
Union agreement	0.251	0.433	0	1	Survey	Binary
Organizational Demography						
Establishment size	740	970	10	14,195	EEO-1	Continuous
Percent minorities in top management	3.308	9.768	0	100	Survey	Continuous
Percent women in top management	16.245	23.488	0	100	Survey	Continuous
No white men in management	0.007	0.082	0	1	EEO-1	Binary
No white women in management	0.125	0.330	0	1	EEO-1	Binary
No black women in management	0.711	0.453	0	1	EEO-1	Binary
No black men in management	0.551	0.497	0	1	EEO-1	Binary
Percent managers in establishment	0.124	0.089	0	0.789	EEO-1	Continuous
Internal labor pools (proportion)						
Non-managers who are white men	0.408	0.253	0	1	EEO-1	Continuous
Non-managers who are white women	0.383	0.252	0	1	EEO-1	Continuous
Non-managers who are black women	0.057	0.097	0	0.893	EEO-1	Continuous
Non-managers who are black men	0.052	0.090	0	0.940	EEO-1	Continuous
Core job workers who are white men	0.385	0.316	0	1	EEO-1	Continuous
Core job workers who are white women	0.388	0.320	0	1	EEO-1	Continuous
Core job workers who are black women	0.061	0.113	0	1	EEO-1	Continuous
Core job workers who are black men	0.056	0.107	0	0.963	EEO-1	Continuous

TABLE 1 Continued

	Mean	Standard Deviation	Minimum	Maximum	Data	Type
<i>External labor pools (proportion)</i>						
Industry labor force that is white male	0.445	0.153	0.145	0.742	CPS	Continuous
Industry labor force that is white female	0.326	0.146	0.103	0.624	CPS	Continuous
Industry labor force that is black female	0.042	0.025	0.004	0.119	CPS	Continuous
Industry labor force that is black male	0.041	0.019	0.009	0.106	CPS	Continuous
State labor force that is white male	0.388	0.061	0.116	0.595	CPS	Continuous
State labor force that is white female	0.353	0.063	0.093	0.496	CPS	Continuous
State labor force that is black female	0.048	0.034	0.000	0.201	CPS	Continuous
State labor force that is black male	0.042	0.030	0.000	0.186	CPS	Continuous
<i>Organizational Environment</i>						
In-house legal counsel	0.283	0.451	0	1	Survey	Binary
Attorney on retainer	0.337	0.473	0	1	Survey	Binary
Percent government contractors in industry	0.488	0.226	0.061	0.821	EEO-1	Continuous
State unemployment rate	6.144	2.031	2	18	BLS	Continuous
Industry employment (in '000)	3,748	2,780	996	11,458	CPS	Continuous
Period 71-80	0.211	0.408	0	1	EEO-1	Binary
Period 93-02	0.380	0.485	0	1	EEO-1	Binary
Year	1988	8.376	1971	2001	EEO-1	Binary

Note: N = 18,474.

diversity training; networking program for women or minorities; and a mentoring program for women and minorities. Work-Family Support counts four work-family factors: paid maternity leave; paid paternity leave; policy allowing flexible working hours; and top management support for work-family programs. Unionization is a binary variable, based on survey data.

Organizational Demography

Organization Size is measured using EEO-1 data on the total number of employees in the establishment. The Diversity of the Top Management Team, which is thought to predict growing diversity in the lower managerial ranks, is measured as the percent of women and African Americans in the top ten management positions, based on survey data. We obtained values at ten-year intervals and interpolated for intervening years. We include a binary variable coded 1 when there are no managers from the focal group. The diversity of the Establishment's Internal Labor Pool is measured with two EEO-1 variables: the proportion of the focal group in nonmanagerial jobs and the proportion in the core job. Core job is defined as the most common job category based on the EEO-1 data. Demographic diversity of the Establishment's External Labor Pool is measured using data on the percent of each group in the industry and state labor market from the Current Population Survey of the Bureau of Labor Statistics. The industry employment variables are logged. These annual measures also capture dynamics unique to each state and industry. The overall Availability of Managerial Jobs is measured using EEO-1 data on the number of employees who were classified as managers in the establishment.

Organizational Environment

Legal Awareness is measured with two binary variables for the presence of an in-house attorney and an employment attorney on retainer. We assume that the presence of legal counsel increases the likelihood that information about antidiscrimination compliance will be disseminated within the organization. The annual Proportion of Establishments in the Industry that Is Government Contractors is calculated from the EEO-1 data. Economic Conditions are measured with the state unemployment rate, and Industry Size is measured as the total employment in industry. Data for both variables were obtained from the Bureau of Labor Statistics. Two binary variables are coded 1 for the period 1971–1980 and 1993–2002.

Independent variables are measured in the year before dependent variables. The one exception is the size of the managerial workforce, for we suspect that change in the size of the managerial workforce will have instantaneous effects.

Method

We examine the effects of compliance reviews and lawsuits using three different modeling approaches. First, in the baseline model, we use binary variables representing the year of the first event, as well as variables that count each subsequent event. For an organization that experienced a compliance review in 1978, for example, the *First Review* variable is coded 1 from 1978 on. The *Number of Reviews* variable is coded 1 in 1978, 2 from the year of the second review, and so on. This model provides information on average intervention effects over a thirty-year period but does not explore over-time variation in the effects of legal enforcement.

Theory suggests that reviews initiated in the 1970s should be more effective than those initiated later. The assumption is that activist regulators introduce lasting changes to personnel routines and practices. To examine this idea, in the second model, we divide both first reviews and first suits into three categories. *First Review 1970s* is coded 1 from the year of the first review, if the organization had its first review before 1981. *First Review 1980s* is coded 1 from the year of the first review, for organizations that had their first review from 1981 through 1992. *First Review 1990s* is coded 1 from the year of the first review in organizations that had their first review from 1993 through 2002. We use the same coding procedure for lawsuits.

The second model is designed to indicate whether compliance reviews or lawsuits conducted in the 1970s were more effective, but it is not designed to tell us whether those reviews led to changes that had sustained effects. Perhaps reviews and lawsuits only produced short-term effects. To test whether their effects were sustained, in the third model we use interactions. We look at the effects of first review pre-Reagan on management diversity pre-Reagan, during Reagan-Bush I, and post-Bush I. For that purpose we interact *First Review 1970s* with a binary variable for 1971–1980 and with a binary variable for 1993–2002, omitting 1981–1992 as the comparison category. We also interact *First Review 1980s* with 1993–2002, omitting 1981–1992 as the comparison category. And we look at the effect of *First Review 1990s* in that decade. We use the same procedure for lawsuits. For these analyses, we do not present the coefficients of the interaction terms, but rather the linear combination of the coefficients for “first event” and for the interaction terms “first-event * period” (using the *Lincom* command in Stata). This way, the effects of each regulatory event in each period could be read directly from the table (Friedrich 1982). Finally, by including a measure for the *number* of legal interventions to date, we are able to explore whether compliance reviews in the 1970s continued to have effects in the 1980s because they changed corporate behavior permanently or because they were followed up by later reviews.

The four dependent variables we examine—the proportion of managers who are white men, white women, black men, and black women—are all

parts of the same whole, namely, the managerial jobs in an establishment in a given year. Thus, their error terms are likely to be correlated. Under these conditions, ordinary least square estimates are expected to be consistent and unbiased but not efficient. We therefore use seemingly unrelated regression,⁶ a generalized least squares estimation that takes into account the information on the covariance between the errors, hence producing more efficient estimators for data like ours (Zellner 1962; Felmlee and Hargens 1988). Seemingly unrelated regression also allows us to perform a formal test comparing the magnitude of the effects for each demographic group (Zellner 1962; Kalleberg and Mastekaasa 2001). We reproduced the analyses presented here, using OLS, and the substantive results remained the same.

As the data for this study were not generated in a random assignment process, our modeling strategy is designed to establish reliable estimates of the effects of reviews and lawsuits that are not biased by unobserved heterogeneity. We take several steps toward achieving this goal. First, we include in the analysis fixed establishment effects, which help us to rule out the possibility that organizations that face legal interventions differ from other organizations with respect to unmeasured stable organizational characteristics (Hsiao 1986; Hicks 1994; Western 2002). We achieve this model specification by subtracting the values of each observation from the establishment's mean (Hsiao 1986, 31). This transformation is equivalent to including in the model 814 dummy variables, one for each establishment in our data. Fixed-effects models estimate variation in the outcome variable that is associated with changes over time in the independent variables within an organization.⁷ Second, we include a dummy variable for each year between 1972 and 2001 (1971 is the omitted category, and 2002 is included only for measuring the outcome variable). This vector of coefficients captures unobserved changes that are associated with the mere passage of time and that are common to all organizations. The establishment and year fixed effects also offer an efficient means of handling the nonconstant variance (heteroskedasticity) of the errors, stemming from the cross-sectional and temporal aspects of the pooled data (Says 1989).⁸ Third, as discussed above, we also include in the models a series of time-varying variables that are likely to be associated both with legal interventions and with managerial diversity, such as EEOC charges, employer affirmative action status, formal human resources policies, diversity programs, and the gender and racial diversity of the top management team. The full list of control variables included in the analyses is presented in Table 1.

6. Available in Stata using the SUREG procedure.

7. The intercept in these models is not an explanation of the "between unit" or over-time variance. Rather, it is a characterization of the variance that attempts to minimize the "true" explanation, or a measure of the "specific ignorance," as opposed to the "general ignorance" captured by the error term (Maddala 1977; Says 1989).

8. Using the Huber-White robust standard errors did not change the results of the analysis.

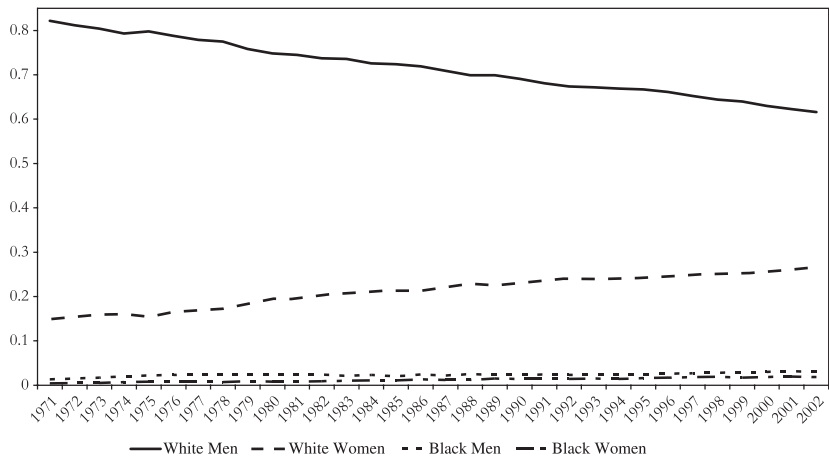
We supplemented the analysis with three robustness tests that examine the possible effect of unmeasured variance. Unobserved heterogeneity can exist if an establishment is not in the dataset for part of the time series (not all establishments were in the dataset at the outset) for a reason that is correlated with the outcome variable (examples would be organization size and age). To verify that the results were not driven by the selection of establishments into the dataset, we replicated the analysis using a subsample of establishments available for the whole period. The results were substantially similar to those reported below. It is also plausible that unobserved heterogeneity is responsible both for the occurrence of a lawsuit and for changes in managerial diversity. For example, layoffs may have reduced managerial diversity and caused the establishment to face lawsuits. Another scenario may have been that a newly arrived CEO hired many new women and minorities and this caused both increases in management diversity and in lawsuits. To ensure that the effects that we observe in our analyses are not spurious in this way, we added a dummy variable coded 1 prior to the first lawsuit as a proxy for an unspecified event. We added such variables two and three years before the occurrence of the first event. If the results were spurious, the proxy variables would have shown significant effects, and the estimates for “lawsuit” would have declined (Snyder 2003). Our results remained robust to this check. The proxy coefficients were not significant, and their inclusion did not alter our main coefficients. Finally, we examined the robustness of the results to the possibility that errors of one year are correlated with errors of the next year, using the Cochrane-Orcutt method⁹ in which we multiplied the equation for time $t - 1$ by the autocorrelation coefficient, ρ , and subtracted it from the equation for time t . The substantive results were robust to this correction. Results for these robustness checks are available from the authors upon request.

FINDINGS

The demographic composition of management in the establishments in our sample changed significantly over time. While in 1971, white men held 82 percent of management jobs in the average establishment, by 2002 they held only 61 percent. White women in management grew from 15 percent to 26 percent in the average establishment. Black women grew from 0.4 percent to 2.0 percent, and African American men from 1.3 percent to 3.1 percent (see Figures 1 and 2).

Similar trends exist in the population of organizations submitting EEO-1 reports. The gains for women and minorities in the entire EEO-1 dataset are slightly higher than in our sample, which consists of more stable and older

9. Available in Stata using the `xtregar` procedure.



Based on EEO-1 reports 1971–2002 sampled for Princeton University Human Resources Survey 2002. Varying N. Maximum N = 814

Figure 1. Proportion of White Men and Women and Black Men and Women Among Managers, 1971–2002.



Based on EEO-1 reports 1971–2002 sampled for Princeton University Human Resources Survey 2002. Varying N. Maximum N = 814

Figure 2. Proportion of Black Men and Women Among Managers, 1971–2002.

establishments. Women, both black and white, gained more also according to the Current Population Survey, which includes government and nonprofit jobs.

These percentages do not sum up to one 100 percent, particularly in later years. This gap represents a significant rise in the representation of other groups, in particular Latinos, during this period. It is also noticeable that black women saw large gains in management in this period. This is consistent with evidence on the decline in racial segregation among white and black

women in the post-1964 era, which was larger than the parallel decline in segregation among men (Bell and Nkomo 1994).

What role did compliance reviews and lawsuits play in bringing about these changes? Both lawsuits and compliance reviews had significant positive effects on the subsequent share of white women, black women, and black men in management. How effective were compliance reviews and lawsuits initiated under different regulatory regimes? We find that compliance reviews initiated in the 1970s were significantly more effective than those initiated in the 1980s or 1990s, and their effects persisted into the two latter periods. Employers who faced compliance reviews in the 1970s appear to have altered their behavior in ways that have had lasting effects for white women, black women, and black men. Compliance reviews initiated in the 1980s had more limited effects.

Lawsuits, by contrast, had more stable effects over time. Their effects were not altered by regime change. This finding is in keeping with the argument that presidents can have instantaneous effects on administrative law by appointing new officials and determining regulatory intensity, but only delayed effects on the judiciary by replacing justices who leave office with justices with different political leanings. Below we discuss the findings in greater detail.

Regulatory Events and Managerial Diversity

Table 2 presents the results of the baseline model estimating the effects of the first compliance review or lawsuit, and of the number of reviews and suits. This model includes all the control variables listed in Table 1. Coefficients for the control variables are reported in Appendix Table 1.

The average percent change in the odds of a group in management that is associated with a change in a variable is calculated by exponentiating its coefficient β as follows: $[\exp(\beta) - 1] \times 100$. If β is smaller than 0.1, it can be multiplied by 100 and interpreted directly as a relative effect. The r-square values in these tables represent the percent variance explained by the predictors when excluding the unique effects of each establishment (for the fixed establishment effects are achieved by differencing from the establishment mean, rather than by including dummy variables).

In Table 2 we see that the number of compliance reviews reduced the share of white men in management and increased the share of black women and black men. Repeated lawsuits reduced white men's share, whereas the other three groups saw increases. Initial events had only meager effects according to this model. The initial lawsuit had a net negative effect on white men and a net positive effect on white women. It also shows a negative effect on black men; however, summing the coefficients for first lawsuit and the number of lawsuits (using the `Lincom` command in Stata) indicates that first lawsuit did not have an effect on black men ($B = -0.012$ and $SE = 0.014$). The initial compliance review coefficients show a weak positive effect on

TABLE 2

Fixed Effects Estimates of the Log Odds of White Men and Women and Black Women and Men in Management after Compliance Reviews and Title VII Lawsuits, 1971–2002

	White Men	White Women	Black Women	Black Men
<i>Compliance Reviews</i>				
First compliance review	–0.030 0.023	0.044 0.023	0.028 0.020	0.033 0.021
Number of compliance reviews	–0.029** 0.010	0.013 0.010	0.039** 0.009	0.041** 0.009
<i>Title VII Lawsuits</i>				
First lawsuit	–0.048** 0.016	0.068** 0.016	–0.013 0.014	–0.031* 0.015
Number of lawsuits	–0.007** 0.002	0.016** 0.002	0.020** 0.002	0.019** 0.002
R-sq	.3350	.3148	.3548	.2674
Chi-sq	9,636	8,624	10,406	7,099

Coefficients from seemingly unrelated regression, unstandardized coefficients, standard errors below the coefficients.

Note: The analysis includes all variables appearing in Table 1 as well as fixed establishment and year effects. Coefficients for the control variables are in Appendix Table 1. *N* (organization year, organizations) = 18,474, 814. Number of parameters = 64.

** $p < 0.01$; * $p < 0.05$; (two tailed test).

white women (significant only at a 90 percent level of confidence) and no effects on black men and women. Later models suggest that the apparent null effect is a consequence of the fact that the effect varies over time.

The effects of the control variables are consistent across all the stages of our analysis (reported in Appendix Tables 1–3). Most have the expected effects. Of particular interest here are the variables that represent other sorts of federal efforts. The coefficients for federal contractor do not show the expected effect when compliance reviews are controlled. For white men, white women, and black men, contractor status had no effect. For black women, it shows a negative effect, which may indicate that employers make efforts to recruit black women to management prior to applying for a government contract and relax their efforts once the contract is approved (Leonard 1990, 65; Baron et al. 1991, 1389).¹⁰ Affirmative action plans,

10. In an alternative specification of the model reported in Table 2, we measured contractor status, using two binary variables, one for joining and one for leaving contractor status. About 20 percent of government contractors in our sample ceased to be contractors at some point. The estimated effect of leaving the contractor status was not significantly different from zero. We thus surmised that the negative coefficient of the original variable was attributed to joining the contractor-compliance program rather than leaving it.

TABLE 3

Fixed Effects Estimates of the Log Odds of White Men and Women and Black Women and Men in Management after Compliance Reviews and Title VII Lawsuits in Three Periods, 1971–2002

	White Men	White Women	Black Women	Black Men
<i>Compliance Reviews</i>				
1st compliance review was in the 70s	−0.145*	0.296**	0.162**	0.249**
	0.062	0.064	0.054	0.058
1st compliance review was in the 80s	−0.081**	0.075**	−0.039	0.045
	0.027	0.028	0.024	0.026
1st compliance review was in the 90s	0.003	0.048	0.031	0.020
	0.031	0.032	0.027	0.029
Number of reviews	−0.017	−0.002	0.031**	0.033**
	0.010	0.010	0.009	0.010
<i>Title VII Lawsuits</i>				
1st lawsuit was in the 70s	−0.029	−0.010	0.002	0.042
	0.045	0.047	0.039	0.043
1st lawsuit was in the 80s	−0.067**	0.093**	−0.026	−0.046**
	0.019	0.019	0.016	0.018
1st lawsuit was in the 90s	−0.009	0.029	0.008	−0.024
	0.023	0.024	0.020	0.022
Number of lawsuits	−0.006*	0.015**	0.020**	0.019**
	0.002	0.002	0.002	0.002
R-sq	.3356	.3159	.3555	.2684
Chi-sq	9,663	8,668	10,428	7,127
Log Likelihood Ratio Test	Chi-sq(16) = 74.68 p < .001			

Coefficients from seemingly unrelated regression, unstandardized coefficients, standard errors below the coefficients.

Note: The analysis includes all variables appearing in Table 1 as well as fixed establishment and year effects. Coefficients for the control variables are in Appendix Table 2. *N* (organization year, organizations) = 18,474, 814. Number of parameters = 68.

** $p < 0.01$; * $p < 0.05$; (two tailed test).

which are required of federal contractors, had positive effects on white women and black men. This leads us to surmise that presidential affirmative action edicts operate through the organizational practices and regulatory interventions they elicit. EEOC charges did improve the prospects of black men in management, and of course, these charges are often the first step on the road to a lawsuit (and lawsuits show effects across the board). The models also include period effects (with the omitted period being 1981–1992). These period effects are net of fixed-year effects (which we do not report here). Net of the time trend, the negative coefficient for black women indicates a slower growth in their share in management in

the 1990s as compared to the 1980s. This is evident also from looking at Figure 2.

Regulatory Approach and Managerial Diversity

Table 3 is identical to Table 2, but the models here include separate measures for initial compliance reviews and initial lawsuits according to their period of origin, 1971–1980, 1981–1992, or 1993–2002. In these models, the number of compliance reviews and lawsuits continues to matter. But the *timing* of the first compliance review also matters. Compliance reviews initiated in the 1970s had significant effects on white women, black women, and black men in management. Compliance reviews initiated in the 1980s had significant effects only on white women, and the magnitude of the coefficient is significantly lower than the magnitude of the coefficient for the 1970s ($\text{Chi-sq}(1) = 12.47, p < .001$). The coefficients for first reviews in the 1970s represent the average effect of the review for the entire period so that the managerial diversity of an establishment that faced a compliance review in 1975 would be significantly higher, due to this compliance review, for more than a quarter of a century. An employer who faced a first compliance review in 1985 would see a much smaller average difference in the proportion of white women in management and over a shorter period.

In these models, first lawsuit had a time-varying effect only for white women and black men; lawsuits initiated in the 1980s had a significant positive effect on white women and a significant negative effect on black men. The positive coefficient for lawsuits in the 1980s in the model for white women is significantly smaller than the one for compliance reviews in the 1970s ($\text{Chi-sq}(1) = 9.16, p < 0.001$). In Table 3, the number of lawsuits shows effects on all four outcomes. The number of reviews shows effects only on black women and black men. Repeated lawsuits were more effective than repeated reviews.

Thus far, we have described the effects of compliance reviews and lawsuits in terms of change in the *odds* of each group in management. The effect on the *proportion* of each group in management varies for different starting points. For example, according to Table 3, the effect of first compliance review in the 1970s is estimated to be an average increase of about 35 percent in the odds of white women in management [$B = 0.296$; $(\exp(0.296) - 1) \times 100 = 34.4$]. In an organization where 6 percent of management was comprised of white women, as is the case in the first quartile in our sample, this would translate to an increase of 32 percent in their proportion in management, from 6 percent to 7.9 percent, controlling for all other factors included in the model. If the baseline proportion of women in management was 15 percent, which is the sample median, the estimated B coefficient (0.296) would translate to a 19 percent increase in the proportion of white women in

management, from 15 percent to 18 percent.¹¹ First compliance reviews in the 1980s had significantly smaller effects: in the median organization a first compliance review in the 1980s is estimated to raise the proportion of white women in management by 6.5 percent, from 15 percent to 16 percent.

What do these effects mean in relation to the change in the proportion of women and minorities in management between 1971 and 2002? Continuing with the example of white women, to get a sense of the magnitude of these effects, one should consider that the overall change in the average proportion of white women in management between 1971 and 2002, in our sample, was from 15 percent to 26 percent, an 11 percentage-point increase (as Figure 1 shows). By comparison, a first compliance review in the 1970s is estimated to be responsible for an increase of 4 percentage points in the proportion of white women in management in the median organization. First compliance review in the 1980s is responsible for an increase of 1 percentage point in the proportion of white women in management in the median organization.

In Table 4, we explore whether first reviews and suits had lasting effects. If vigorous compliance reviews have the effect of changing organizational routines, reviews conducted in the 1970s should continue to affect the proportion of women and African Americans in management in the 1980s and 1990s. The weak-willed compliance reviews of the 1980s should have smaller, and less lasting, effects. In Table 4, we interact the period of the first review, or suit, with the period under observation. So that, for instance, we interact First Review 1970s with the periods 1970s and 1990s (we cut off the decades at the point of change in administration, as noted above), leaving the 1980s as the omitted category. To get estimates for the effect of reviews and suits in each period, we added the coefficients for first review (or suit) to the interaction term for the relevant period, using the Lincom procedure in Stata. For example, to calculate the effects of First Review 1970s on managerial composition in the 1970s, we added the interaction coefficient First Review in the 1970s * Period 1971–1980 to the coefficient for First Review 1970s. The coefficients in Table 4 thus represent the effects of reviews and lawsuits in each of the examined periods. When relevant, we report below whether the coefficients for different periods are significantly different (that is, whether the interaction coefficient is significant).

11. To evaluate the magnitude of the effect as a percent change in the proportion of a focal group in management, we used the following calculation: $\Delta P_{ij}/P_{ij} = [\exp(L_{1ji})/(1 + \exp(L_{1ji})) - (\exp(L_{0ji})/(1 + \exp(L_{0ji}))]/(\exp(L_{0ji})/(1 + \exp(L_{0ji})))$, where j denotes the focal demographic group and i is the legal intervention (review or lawsuit). L_{0ji} is the log-odds of group j in management before the unit change in D_i (that is, before first review or lawsuit) and $L_{1ji} = L_{0ji} + B_{ij}$, and is the log odds of group j in management after the unit change in D_i (after first review or lawsuit), with B_{ij} being the regression coefficient associated with first legal intervention in the model for the j group, estimating the change in the log odds of group j in management resulting from first review or suit (Petersen 1985).

TABLE 4

Fixed Effects Estimates of the Log Odds of White Men and Women and Black Women and Men in Management after Compliance Reviews and Title VII Lawsuits in Three Periods, 1971–2002. Interaction Model

	White Men	White Women	Black Women	Black Men
1st compliance review was in the 70s				
Effects in the 1970s	–0.192**	0.254**	0.103	0.223**
	0.071	0.073	0.062	0.066
Effects in the 1980s	–0.078	0.288**	0.173**	0.230**
	0.066	0.068	0.058	0.062
Effects in the 1990s	–0.111	0.313**	0.262**	0.217**
	0.073	0.076	0.064	0.069
1st compliance review was in the 80s				
Effects in the 1980s	–0.104**	0.122**	–0.022	0.061*
	0.032	0.033	0.028	0.030
Effects in the 1990s	–0.042	0.053	–0.004	0.023
	0.034	0.035	0.030	0.032
1st compliance review was in the 90s				
	0.017	0.045	0.054	0.011
	0.032	0.033	0.028	0.030
Number of reviews	–0.028**	–0.001	0.019	0.039**
	0.012	0.012	0.010	0.011
1st lawsuit was in the 70s				
Effects in the 1970s	0.052	–0.028	0.000	–0.016
	0.056	0.059	0.049	0.053
Effects in the 1980s	0.001	–0.072	–0.025	0.017
	0.048	0.050	0.042	0.045
Effects in the 1990s	–0.098	0.053	–0.003	0.013
	0.053	0.055	0.046	0.050
1st lawsuit was in the 80s				
Effects in the 1980s	–0.102**	0.127**	0.005	0.005
	0.021	0.022	0.018	0.020
Effects in the 1990s	–0.056*	0.062*	–0.074**	–0.122**
	0.026	0.026	0.023	0.024
1st lawsuit was in the 90s				
	–0.007	0.022	–0.008	–0.054*
	0.025	0.025	0.021	0.023
Number of lawsuits	–0.006*	0.017**	0.023**	0.025**
	0.003	0.003	0.002	0.003
R-sq	.3367	.3172	.3564	.2699
Chi-sq	9,713	8,719	10,472	7,177
Log Likelihood Ratio Test	Chi-sq(24) = 111.26 p < .001			

Coefficients from seemingly unrelated regression, unstandardized coefficients, standard errors below the coefficients.

Note: The analysis includes all variables appearing in Table 1 as well as fixed establishment and year effects. Coefficients for the control variables are in Appendix Table 3. The omitted period is 1981–1992. Coefficients for the effects of reviews and lawsuits in 1971–1980 and in 1993–2001 are calculated as the linear combination of main and interaction coefficients, using Stata's Lincom command. *N* (organization-year, organizations) = 18,474, 814. Number of parameters = 74.

** p < 0.01; * p < 0.05; (two tailed test).

The results are striking. Establishments that experienced their first compliance review in the 1970s saw significant effects in the 1970s, 1980s, and 1990s on white women and black men. These 1970s reviews also had significant effects on black women in the 1980s and 1990s (and at the 0.10 significance level in the 1970s). Early compliance reviews had lasting effects on employers, and those effects were not washed out by the variable that counts subsequent compliance reviews. Employers that experienced their first compliance reviews in the 1980s saw much weaker effects, and these effects did not last into the 1990s. First review in the 1980s had the expected effects on white men, white women, and black men only for the 1980s, and the coefficients are substantially smaller than those for first review in the 1970s for that decade (for white women the difference between the periods is significant at the 90 percent level of confidence, and for black men, at the 95 percent level of confidence). Compliance reviews that were initiated in the 1990s did not show effects. The count, Number of Reviews (to date), had the expected effects on white men and on black men. It may be that the effects of reviews conducted in the 1980s and 1990s appear here in the effect of the count variable.

It seems that black women were affected most by the deregulation of the 1980s, as they were the only group that did not gain from compliance reviews initiated in this period. This result is consistent with Leonard's (1990) findings that black women saw the largest decline in employment growth in contractor establishments in the early 1980s (Leonard 1990, 58). It also seems that, overall, black men benefited most from compliance reviews. Black men and white women benefited similarly from initial compliance reviews, but black men benefited more from repeated reviews. This is consistent with the pattern Leonard reported for the 1970s, where he found that compliance reviews had a positive effect on the move of black men into management occupations, and less of an effect on women (Leonard 1984c, Tables 3, 5, and 6).

The pattern for lawsuits is quite different. Lawsuits are thought to create incentives for employers to hire women and blacks for management jobs. The count variable for lawsuits shows that they did have effects on all four groups. Repeated lawsuits reduced the subsequent proportion of white men and increased the subsequent proportions of white women, black women, and black men in management. We know from Table 2 that, overall, throughout the three decades, first lawsuit had a negative effect on white men and a positive effect on white women. However, we learn from Table 4 that the *timing* of the first lawsuit was not as consequential as the timing of the first compliance review. First lawsuits in the 1970s had no significant effects, net of the count of lawsuits. First lawsuits in the 1980s had significant positive effects on white women in both the 1980s and the 1990s, net of the count variable. However, first lawsuits had no effects on black women and men in the 1980s, and negative effects on both in the 1990s. The

findings about negative effects were arguably anticipated by Skaggs's (2001) findings on declines in managerial diversity following a settlement or award associated with litigation. Unlike the results of compliance reviews, white women gained most from first lawsuits, whereas black men and women benefited less.

The analysis supports our main hypotheses. Our most striking finding is that compliance reviews from the 1970s had effects net of the simple number of reviews, and those effects survived past the period of aggressive enforcement. It appears that the reviews changed organizational behavior patterns, and that these changes had lasting effects. It is also the case that lawsuits mattered; the number of suits had a significant effect on all groups, and the suits from the 1980s (which were increasingly about harassment) show a net positive effect on white women, both in the 1980s and the 1990s.

CONCLUSION

We know relatively little about how effective federal legal interventions have been in bringing equality to the workplace. Early studies show that employers subject to affirmative action edicts, and those employers that underwent compliance reviews, added more blacks and moved more blacks up the ranks (Donohue and Heckman 1991). A recent study shows that lawsuits in the 1980s and 1990s increased diversity in supermarket management (Skaggs 2001). Using a unique dataset on organizational practices and experiences with federal enforcement, coupled with the detailed workforce reports employers send to Washington every year, we have explored the effects of affirmative action compliance reviews and discrimination lawsuits on the entrance of women and minorities into management, at least into its lower levels, between 1971 and 2002.

Our findings reveal two important insights. First, policy interventions that stimulate change in organizational routines appear to have significant and lasting effects on workforce diversity (Edelman and Petterson 1999). The interventionist compliance reviews of the 1970s increased the share of white and black women, and black men, in management not only in the 1970s but also in the 1980s and 1990s. Lawsuits, which punish organizations and thereby create an incentive not to discriminate, also increase managerial diversity, but initial lawsuits do not have the lasting effects that initial compliance reviews have. This is likely because lawsuits do not elicit the kinds of permanent organizational changes that compliance reviews elicit; instead, lawsuits act as disincentives to discriminate, which erode over time. Repeated lawsuits are more effective than repeated compliance reviews, and this is likely because lawsuits create short-term incentives that must be renewed to be effective. Given that compliance reviews are consciously geared toward changing organizational routines, our finding about their sustained

effect, as compared to the short-term effect of lawsuits, is consistent with the sociological insight that organizational structures and routines, as much as incentives to managers, influence workforce diversity.

As it happens, judges have increasingly supported the kinds of organizational change mechanisms that would appear to have been catalyzed by early compliance reviews. In the last decade, Title VII rulings have encouraged institutional innovation in workplaces, "prescribing an approach that enables employers to avoid liability by preventing or redressing harassment or bias problems" (Sturm 2001, 489). Some courts have even required employers to adopt certain antidiscrimination programs, such as diversity training (Krawiec 2003, 53). Legal scholars debate the utility of this judicial approach, which Krawiec (2003) calls "negotiated governance," and defines as "models that seek to improve government regulation and/or the litigation process through more cooperative governance methods that provide a governance role to the regulated group and other interested parties" (Krawiec 2003, 487). Sturm used data from three case studies, of Deloitte & Touche, Intel Corporation, and Home Depot, to support what she terms a problem-solving approach where "compliance is achieved through and evaluated in relation to improving institutional capacity to identify, prevent, and redress exclusion, bias, and abuse" (Sturm 2001, 463). This model emerged from criticism of a rule-based approach to antidiscrimination enforcement and the legal ambiguity it entails. The idea is that "any rule broad enough to cover the variety of contexts and conduct that might arise will inevitably be quite general and ambiguous, and it will produce considerable uncertainty about the boundaries of lawful conduct" (461). Successful solutions to discrimination should therefore be designed at the organizational level. Skeptics argue that there is virtually no evidence that employer compliance structures are effective (Edelman 1992; Bisom-Rapp 1999; Edelman, Fuller, and Mara-Drita 2001; Krawiec 2003).

The second insight to emerge from our study is that the presidential administration's regulatory approach shapes the efficacy of administrative legal interventions but less so the efficacy of judicial interventions. When Washington deregulated, the efficacy of compliance reviews declined significantly. Compliance reviews from the 1980s had more meager effects than did reviews from the 1970s, net of the simple number of reviews. The administration of George H. W. Bush moved back toward more interventionist policies. Then the Clinton administration made efforts to reinvigorate the OFCCP, going after the worst offenders, targeting the most visible areas of noncompliance, strengthening sanctions, resolving disputes more quickly, and debarring more contractors (seven in three years, as compared to four in the first four years of the Reagan administration). Still, these regulatory changes did not bring back the administrative efficacy of the 1970s, and this may be because OFCCP staffing under Clinton in 1996 was still down by more than half from the high of 1,700 in the late 1970s (Anderson 1996).

For those who argue that civil rights enforcement has been merely ceremonial, we offer clear evidence that compliance reviews and Title VII lawsuits have had a significant impact on the careers of women and minorities—an impact that has withstood changes in regulatory intensity. Yet the effects of lawsuits are not always as expected. Blacks benefited significantly less than white women from lawsuits. Black women and men gained from lawsuits only after repeated events. The first lawsuit sometimes caused the proportion of black men and women to decline. We can only speculate about why black men and women did not see the same gains from these interventions as did white women. OFCCP compliance reviews, which are more sensitive to regime change, have had long-lasting effects on managerial diversity.

White women may have gained more from Title VII enforcement due to their increasing propensity to bring charges. Title VII enforcement is dependent on complaints, and if blacks are less likely to complain, enforcement will be less effective for remedying racial discrimination. Nielsen and Nelson (2005) studied EEOC charge statistics, finding that “less than 1 percent (0.85 percent) of African Americans who felt they were discriminated against filed an EEOC complaint” (Nielsen and Nelson 2005, 704). We do not have comparable information for sex discrimination charges, but Donohue and Siegleman (2005) found that during the 1990s, the share of racial discrimination suits declined significantly, while sex discrimination suits increased.

Lawsuits create a disincentive to discriminate in the near term, but they may not lead to permanent organizational change for a variety of reasons. Wooten and James (2004) examine employer response to Title VII lawsuits. Many of the employers they surveyed had faced repeated suits, because these employers did not change their employment practices, due to a failure of organizational learning. Organizational leaders often responded by defending the organization’s flawed routines, by justifying those routines, by defining the problem as an isolated anomaly, and by targeting the lawsuit rather than the discrimination as the problem to be addressed. Organizational members failed to learn because the complexity of discrimination made it difficult for them to see solutions because the rarity of lawsuits prevented reinforcement of the lesson, and because they looked for solutions in existing organizational routines. Future research is needed to determine more definitively just how lawsuits and compliance reviews do, or do not, lead to organizational change. Researchers need to explore the mechanisms that make changes endure and examine what makes changes effective for different demographic groups.

APPENDIX TABLE 1

Fixed Effects Estimates of the Log Odds of White Men and Women and Black Women and Men in Management after Compliance Reviews and Title VII Lawsuits, 1971–2002. Controls for models presented in Table 2.

	White Men	White Women	Black Women	Black Men
<i>Other Regulatory Activities</i>				
First EEOC charge	0.005	0.007	0.021	0.047**
	0.015	0.016	0.013	0.014
Government contractor	0.013	0.024	−0.034*	−0.003
	0.017	0.017	0.015	0.016
Affirmative action plan	−0.060**	0.049**	0.004	0.035*
	0.015	0.016	0.013	0.014
<i>Organizational Structures</i>				
Grievance procedure	0.001	−0.026	−0.032*	−0.067**
	0.016	0.017	0.014	0.015
Formal personnel policies	0.005	−0.007	−0.006*	−0.007*
	0.004	0.004	0.003	0.003
Diversity programs	−0.036**	0.060**	0.052**	0.004
	0.009	0.009	0.007	0.008
Work-family programs	−0.057**	0.043**	0.026**	0.006
	0.008	0.008	0.007	0.007
Union agreement	−0.031	−0.049	0.000	−0.030
	0.032	0.032	0.027	0.030
<i>Organizational Demography</i>				
Establishment size (log)	−0.035**	−0.027*	−0.650**	−0.482**
	0.011	0.011	0.009	0.010
Proportion minorities in top management	−0.001	−0.002	0.007**	0.011**
	0.001	0.001	0.001	0.001
Proportion women in top management	0.000	0.001	0.002**	−0.002**
	0.001	0.001	0.001	0.001
No focal group in management	−0.340**	−0.216**	−0.640**	−0.568**
	0.044	0.011	0.011	0.011
Percent managers in establishment	−1.047**	0.423**	−4.446**	−3.929**
	0.101	0.104	0.088	0.095
Proportion of focal group in non-managerial jobs	1.289**	1.386**	1.522**	1.620**
	0.060	0.065	0.161	0.185
Proportion of focal group in core-job	−0.119**	−0.150**	−0.647**	0.252
	0.036	0.038	0.105	0.129
<i>External Labor Pools (proportions)</i>				
White men	0.156	−0.039	−0.094	0.221**
in industry labor force (log)	0.082	0.084	0.071	0.077
White women	−0.016	0.118	−0.005	0.051
in industry labor force (log)	0.059	0.061	0.051	0.055
Black women	−0.104**	0.065**	0.002	0.083**
in industry labor force (log)	0.020	0.020	0.017	0.019

APPENDIX TABLE 1 (Continued)

	White Men	White Women	Black Women	Black Men
Black men	-0.119**	0.101**	0.033	0.015
in industry labor force (log)	0.024	0.025	0.021	0.023
White men	-0.033	0.220	-0.297	0.281
in state labor force	0.307	0.316	0.268	0.288
White women	-0.836**	1.010**	-1.228**	-0.484*
in state labor force	0.246	0.255	0.215	0.231
Black men	0.552	0.732	-1.880**	-3.405**
in state labor force	0.703	0.723	0.613	0.660
Black women	-0.154	0.749	2.306**	2.187**
in state labor force	0.565	0.582	0.493	0.531
Organizational environment				
In-house legal counsel	-0.095**	0.153**	0.016	0.053*
	0.022	0.023	0.019	0.021
Attorney on retainer	0.020	-0.050**	-0.044**	-0.004
	0.017	0.017	0.015	0.016
Percent government contractors	0.619**	-0.679**	-0.418*	0.114
in industry	0.120	0.123	0.104	0.112
State unemployment rate	0.010**	-0.019**	-0.012**	0.001
	0.004	0.004	0.003	0.003
Industry employment (in '000,000)	0.035**	-0.055**	-0.007**	-0.016**
	0.005	0.005	0.004	0.004
Period 71-80	0.498**	-0.590**	-0.045	0.018
	0.058	0.059	0.050	0.054
Period 93-02	-0.229**	0.197**	-0.084	-0.003
	0.049	0.050	0.043	0.046
R-sq	.3350	.3148	.3548	.2674
Chi-sq	9,636	8,624	10,406	7,099

Coefficients from seemingly unrelated regression, unstandardized coefficients, standard errors below the coefficients.

Note: The analysis includes fixed establishment effects and thirty binary variables for the years 1972–2001 (1971 is the omitted year and 2002 is included in the analysis only for measuring the outcome variable). *N* (organization-year, organizations) = 18,474, 814. Number of parameters = 64.

** $p < 0.01$; * $p < 0.05$; (two tailed test).

APPENDIX TABLE 2

Fixed Effects Estimates of the Log Odds of White Men and Women and Black Women and Men in Management after Compliance Reviews and Title VII Lawsuits, 1971–2002. Controls for models presented in Table 3.

	White Men	White Women	Black Women	Black Men
<i>Other Regulatory Activities</i>				
First EEOC charge	0.006	0.008	0.024	0.051**
	0.015	0.016	0.013	0.014
Government contractor	0.011	0.023	−0.038*	−0.006
	0.017	0.017	0.015	0.016
Affirmative action plan	−0.060**	0.050**	0.004	0.038**
	0.015	0.016	0.013	0.014
<i>Organizational Structures</i>				
Grievance procedure	−0.001	−0.025	−0.032*	−0.065**
	0.016	0.017	0.014	0.015
Formal personnel policies	0.006	−0.007	−0.007	−0.007*
	0.004	0.004	0.003	0.003
Diversity programs	−0.036**	0.061**	0.055**	0.005
	0.009	0.009	0.007	0.008
Work-family programs	−0.055**	0.041**	0.024**	0.004
	0.008	0.008	0.007	0.007
Union agreement	−0.033	−0.047	−0.002	−0.031
	0.032	0.033	0.027	0.030
<i>Organizational Demography</i>				
Establishment size (log)	−0.035**	−0.028*	−0.651**	−0.482**
	0.011	0.011	0.009	0.010
Proportion minorities in top management	−0.001	−0.002	0.007**	0.011**
	0.001	0.001	0.001	0.001
Proportion women in top management	0.000	0.001	0.002**	−0.002*
	0.001	0.001	0.001	0.001
No focal group in management	−0.341**	−0.217**	−0.641**	−0.569**
	0.044	0.011	0.011	0.011
Percent managers in establishment	−1.050**	0.428**	−4.437**	−3.918**
	0.101	0.105	0.088	0.095
Proportion of focal group in non-managerial jobs	1.297**	1.409**	1.517**	1.626**
	0.060	0.064	0.161	0.185
Proportion of focal group in core-job	−0.123**	−0.161**	−0.647**	0.250
	0.035	0.038	0.105	0.129
<i>External Labor Pools (proportions)</i>				
White men	0.170*	−0.043	−0.092	0.205**
in industry labor force (log)	0.082	0.084	0.071	0.077
White women	−0.007	0.116	−0.008	0.042
in industry labor force (log)	0.059	0.061	0.051	0.055
Black women	−0.101**	0.063**	0.000	0.081*
in industry labor force (log)	0.020	0.020	0.017	0.019

APPENDIX TABLE 2 (Continued)

	White Men	White Women	Black Women	Black Men
Black men	-0.120**	0.101**	0.032	0.015
in industry labor force (log)	0.024	0.025	0.021	0.023
White men	-0.039	0.242	-0.292	0.286
in state labor force	0.307	0.316	0.268	0.288
White women	-0.833**	1.000**	-1.222**	-0.490*
in state labor force	0.246	0.255	0.215	0.231
Black men	0.601	0.613	-1.995**	-3.445**
in state labor force	0.703	0.723	0.612	0.660
Black women	-0.148	0.787	2.358**	2.199**
in state labor force	0.566	0.582	0.493	0.531
Organizational environment				
In-house legal counsel	-0.093**	0.151**	0.015	0.053**
	0.022	0.023	0.019	0.021
Attorney on retainer	0.023	-0.051**	-0.045**	-0.003
	0.017	0.017	0.015	0.016
Percent government contractors	0.628**	-0.693**	-0.442**	0.089
in industry	0.119	0.123	0.104	0.112
State unemployment rate	0.010**	-0.020**	-0.012**	0.001
	0.004	0.004	0.003	0.003
Industry employment (in '000,000)	0.035**	-0.056**	-0.009*	-0.017**
	0.004	0.005	0.004	0.004
Period 71-80	0.505**	-0.600**	-0.055	0.003
	0.058	0.059	0.050	0.054
Period 93-02	-0.231**	0.197**	-0.087*	-0.027
	0.049	0.050	0.042	0.046
R-sq	.3356	.3159	.3555	.2684
Chi-sq	9,663	8,668	10,428	7,127
Log Likelihood Ratio Test	Chi-sq(16) = 74.68 p < 0.001			

Coefficients from seemingly unrelated regression, unstandardized coefficients, standard errors below the coefficients.

Note: The analysis includes fixed establishment effects and thirty binary variables for the years 1972–2001 (1971 is the omitted year and 2002 is included in the analysis only for measuring the outcome variable). *N* (organization-year, organizations) = 18,474, 814. Number of parameters = 68.

** p < 0.01; * p < 0.05; (two tailed test).

APPENDIX TABLE 3

Fixed Effects Estimates of the Log Odds of White Men and Women and Black Women and Men in Management after Compliance Reviews and Title VII Lawsuits, 1971–2002. Controls for models presented in Table 4.

	White Men	White Women	Black Women	Black Men
<i>Other Regulatory Activities</i>				
First EEOC charge	0.003	0.010	0.023	0.051**
	0.015	0.016	0.013	0.014
Government contractor	0.014	0.020	−0.035*	−0.012
	0.017	0.018	0.015	0.016
Affirmative action plan	−0.062**	0.051**	0.007	0.043*
	0.015	0.016	0.013	0.014
<i>Organizational Structures</i>				
Grievance procedure	−0.003	−0.023	−0.032*	−0.064**
	0.016	0.017	0.014	0.015
Formal personnel policies	0.006	−0.007	−0.007*	−0.007*
	0.004	0.004	0.003	0.003
Diversity programs	−0.038**	0.064**	0.055**	0.007
	0.008	0.009	0.007	0.008
Work-family programs	−0.055**	0.042**	0.025**	0.004
	0.008	0.008	0.007	0.007
Union agreement	−0.032	−0.050	−0.004	−0.032
	0.032	0.033	0.027	0.030
<i>Organizational Demography</i>				
Establishment size (log)	−0.034**	−0.027*	−0.650**	−0.482**
	0.011	0.011	0.009	0.010
Proportion minorities in top management	0.000	−0.002	0.007**	0.012**
	0.001	0.001	0.001	0.001
Proportion women in top management	0.000	0.001	0.002**	−0.002**
	0.001	0.001	0.001	0.001
No focal group in management	−0.341**	−0.217**	−0.641**	−0.568**
	0.044	0.011	0.011	0.011
Percent managers in establishment	−1.049**	0.434**	−4.420**	−3.898**
	0.101	0.104	0.088	0.095
Proportion of focal group in non-managerial jobs	1.295**	1.405**	1.531**	1.636**
	0.060	0.064	0.161	0.185
Proportion of focal group in core-job	−0.122**	−0.163**	−0.655**	0.246
	0.035	0.038	0.105	0.129
<i>External Labor Pools (Proportions)</i>				
White men	0.157	−0.029	−0.090	0.219**
in industry labor force (log)	0.081	0.084	0.071	0.076
White women	−0.009	0.121*	−0.002	0.050
in industry labor force (log)	0.059	0.061	0.051	0.055
Black women	−0.102**	0.065**	0.002	0.083**
in industry labor force (log)	0.020	0.020	0.017	0.019

APPENDIX TABLE 3 (Continued)

	White Men	White Women	Black Women	Black Men
Black men	-0.123**	0.103**	0.031	0.016
in industry labor force (log)	0.024	0.025	0.021	0.023
White men	-0.041	0.244	-0.289	0.288
in state labor force	0.307	0.316	0.268	0.289
White women	-0.847**	1.027**	-1.231**	-0.497*
in state labor force	0.247	0.255	0.215	0.231
Black men	0.575	0.649	-1.978**	-3.412**
in state labor force	0.703	0.724	0.613	0.660
Black women	-0.151	0.846	2.392**	2.204**
in state labor force	0.565	0.582	0.493	0.531
Organizational environment				
In-house legal counsel	-0.094**	0.152**	0.015	0.052*
	0.022	0.023	0.019	0.021
Attorney on retainer	0.024	-0.054**	-0.047**	-0.006
	0.017	0.017	0.015	0.016
Percent government contractors	0.618**	-0.676**	-0.427**	0.107
in industry	0.119	0.123	0.104	0.112
State unemployment rate	0.010**	-0.020**	-0.012**	0.001
	0.004	0.004	0.003	0.003
Industry employment (in '000,000)	0.035**	-0.055**	-0.009*	-0.017*
	0.004	0.005	0.004	0.004
Period 71-80	0.514**	-0.607**	-0.056	-0.008
	0.057	0.059	0.050	0.054
Period 93-02	-0.227**	0.191**	-0.090*	-0.035
	0.048	0.050	0.042	0.045
R-sq	.3367	.3172	.3564	.2699
Chi-sq	9,713	8,719	10,472	7,177
Log Likelihood Ratio Test	Chi-sq(24) = 111.26 p < 0.001			

Coefficients from seemingly unrelated regression, unstandardized coefficients, standard errors below the coefficients.

Note: The analysis includes fixed establishment effects and thirty binary variables for the years 1972–2001 (1971 is the omitted year and 2002 is included in the analysis only for measuring the outcome variable). *N* (organization-year, organizations) = 18,474, 814. Number of parameters = 74.

** $p < 0.01$; * $p < 0.05$; (two tailed test).

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