The Persistent Effect of Temporary Affirmative Action

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Abstract

I estimate the dynamic effects of federal affirmative action regulation, exploiting variation in the timing of regulation and deregulation across work establishments. I find that affirmative action sharply increases the black share of employees, with the share continuing to increase over time: five years after an establishment is first regulated, its black share of employees increased by an average of 0.8 percentage points. This effect is proportionally larger for high and middle skill jobs. Strikingly, the black share continues to grow even after an establishment is deregulated. Building on the canonical Phelps (1972) model of statistical discrimination, I argue that this persistence is in part driven by affirmative action inducing employers to increase the precision with which they screen potential employees. I then provide supporting evidence.

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“...the [affirmative action] plan is a temporary measure, not intended to maintain racial balance, but simply to eliminate a manifest racial imbalance.”


1 Introduction

Affirmative action policies—those designed to increase diversity among employees, students, politicians, or businesses by advantaging candidates from underrepresented social groups—are practiced throughout the world (Fryer and Loury 2013). They are universally controversial. Even among their advocates, they are often introduced or supported as only temporary remedies for existing social inequities (Sowell 2004). The hope is that a temporary affirmative action program that enhances diversity and reduces inequality between groups can persistently alter those outcomes.

Whether a temporary policy will indeed have persistent effects remains an open question. Economic theory provides ambiguous predictions. The theoretical literature primarily focuses on the potential for affirmative action to reduce inequality by incentivizing human capital accumulation for disadvantaged groups (e.g. Lundberg and Startz 1983; for a review, see Fang and Moro 2011). If employers perceive that some group of workers is less productive or have more difficulty screening workers from that group, then the return to human capital investment for members may be inefficiently dampened. In this setting, an affirmative action regulation can correct those incentives, and even a temporary program can permanently reduce inequality by eliminating negative stereotypes. However, Coate and Loury (1993) demonstrate this need not be the case; indeed, affirmative action can reduce the return to investment for that group even further. In this case, an affirmative action policy must be maintained permanently for any protected group gains to persist. In general, the consequences of a temporary affirmative action policy may depend on the setting.

In this paper I study the dynamic effects of Executive Order 11246, the primary affirmative action regulation for employment in the U.S. The regulation applies to firms that have sizable contracts or subcontracts with the federal government. The Department of Labor estimates that such firms employ about a quarter of the U.S. workforce (OFCCP 2013). Regulated firms are mandated to make a “good faith” effort to employ minorities at rates (at least) proportional to their shares of the local and qualified workforce. I study the regulation’s effect on the employment of black workers, one of the regulation’s original targets, the largest minority group over my period of study, and a group that is often the focus of affirmative action research (Holzer and Neumark 2000a).\(^1\) My work builds on the influential analysis of Leonard (1984), and the recent, careful, and closely related studies of the impacts of Executive Order 11246 on the employment and occupational

\(^1\)Previous work finds that affirmative action regulation has had a negligible impact on female employment (Leonard 1989). Leonard posits that this may be due to the historical prioritization of minority employment in enforcement, or the secular growth of female employment over his period of study. In results not presented here, I also find that affirmative action regulation has minimal impact on female employment. By contrast, the results for Hispanic workers are qualitatively similar to those presented herein for black workers. These results for the Hispanic and female employee shares are available from the author upon request.
advancement of women and minorities by Kurtulus (2011, 2012). After describing my findings, I discuss how the present paper contributes to the existing body of work.

To estimate the dynamic effects of federal affirmative action regulation, I use an event study research design, exploiting variation in the timing of regulation and deregulation across work establishments. In particular, I utilize changes in employers’ status as a federal contractor using administrative data from 1978 to 2004. For many types of goods and services, the set of companies the government buys from at any given time is constantly changing. Turnover in these contractor relationships provides plausibly exogenous variation in which and when employers are subject to affirmative action regulation.

I find that affirmative action sharply increases an establishment’s black share of employees, with the share continuing to increase over time. Five years after an establishment is first subject to the regulation, its black share of employees increased by an average of 0.8 percentage points. To put this magnitude in perspective, note that a 0.8 to 1.3 percentage point increase in the black share of the U.S. workforce would eliminate the black-white jobless gap over this period. This effect is proportionally larger for middle and high skill occupations.

Strikingly, I find that the black share of employees continues to grow even after an employer is deregulated. This persistence is evident more than a decade following deregulation. By contrast, gaining and losing contractor status have symmetric associations with other employer characteristics. Establishment size increases when an establishment becomes a contractor, and decreases when it loses its contractor status. Moreover, following deregulation, an establishment’s likelihood of acquiring a new contract—and hence, becoming regulated again—quickly reverts to near the baseline rate.

This persistence is difficult to reconcile with existing economic models of affirmative action, which focus on the aforementioned human capital channel (Fang and Moro 2011). In particular, because the policy variation exploited here varies across individual employers, it should have minimal effects on the human capital investment incentives workers face in the broader labor market. Rather, any response is likely driven by changes at the employer level.

Given that employers continue to increase the black share of their workforce even when they are deregulated, a revealed preference argument would imply that it is profitable for them to do so. Consistent with this, I argue that the persistence found here is in part due to employers investing in what I term screening capital—investments that improve an employer’s ability to screen potential workers. Examples include: employing and training personnel specialists and departments, developing job tests, harnessing referral networks, developing relationships with and utilizing inter-

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2One possible exception is Athey et al. (2000) who study how the benefits of mentoring for lower-level employees can affect optimal promotion policies. Though their focus is on promotion rather than hiring and they do not explicitly model an affirmative action intervention, the persistence found here may be reconcilable with a modified version of their model. I discuss the mentoring channel in more detail when I discuss possible mechanisms in section 4.

3While the human resources literature typically divides the hiring process into recruitment and selection or screening activities, I do not make this distinction. Instead, I view screening broadly as choosing the ‘best’ candidates from a set of potential workers.
mediaries such as employment agencies and schools, and even learning by doing or experimentation (Arrow 1962; Fryer and Jackson 2008).

Building on the seminal Phelps (1972) model of statistical discrimination, I show how the persistence found here may be driven by affirmative action inducing employers to make (partially) irreversible investments to improve screening. In existing models, an employer can only comply with affirmative action by reducing their hiring standard for the protected group. I introduce a novel response margin, allowing employer investments in screening capital. I show that, under conditions often assumed in the statistical discrimination literature, screening investments will reduce between-group disparities in hiring rates; moreover, affirmative action will increase the return to such investments. If these investments are at least partially irreversible, temporary affirmative action regulation can generate persistent changes in screening capital, and hence produce a durable increase in the minority share of hires.

I then present evidence supporting the model’s predictions. First, the model predicts that regulation will increase the return to investments in screening. Using cross-sectional survey data, I show that regulated employers use more screening methods, including personnel specialists, job tests, credential checks, and intermediaries than otherwise similar unregulated employers. These results largely echo those in Holzer and Neumark (2000b).

Second, the model predicts that screening investments will reduce between-group differences in hiring rates. To test this, I exploit another source of variation in screening investment: employer size. It is well documented that larger employers use more resources in screening and use a wider variety of methods (Marsden 1994). Using administrative panel data, I show that employers’ black share is increasing in employer size. While previous work documents a positive cross-sectional correlation between employer size and black share (Holzer 1998; Carrington et al. 2000), it is not clear what drives this relationship. For example, the authors of these studies posit that the relationship may be driven by workplace discrimination law, which does not cover establishments with fewer than 15 employees, or the concentration of larger establishments in urban locations. To rule out these alternative explanations, I show that this relationship holds within-establishment and within-job (where jobs are defined as establishment by occupation cells) for a large sample of establishments that are all subject to workplace discrimination law.

Still, it is possible that larger establishments hire more black workers due primarily to increased legal or public pressure. I provide three pieces of evidence inconsistent with this claim. First, while it is arguably firm level visibility that would affect the public scrutiny faced by any business, I find that the relationship between employer size and black share is primarily establishment level.

Second, the cross-sectional relationship between employer size and black share is reversed for black-run businesses: for such employers, black share is decreasing (and white share increasing) in size. If black-run employers are better able to screen black workers at baseline, then screening improvements should tend to decrease hiring rate disparities. By contrast, private employers have

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For example, suppose an employer learns to screen from a particular group by hiring members and learning what characteristics predict productivity. Then one can view that initial set of hires as an investment to improve later screening.
historically faced little legal pressure to hire more white workers.

Third, using the universe of discrimination charges filed with the Equal Employment Opportunity Commission (EEOC) from 1990-2004, I find that, conditional on size, the expected number of racial discrimination charges against an establishment is increasing in black share at all but the highest black share levels. Hence, for nearly all establishments, hiring more black workers does not appear to reduce dealings with discrimination law enforcement. The same is true for claims that result in what the EEOC terms ‘merit resolutions’—charges with outcomes favorable to the claimant or where the EEOC finds that allegations have merit. These findings are perhaps unsurprising given that only 5% of charges claim discrimination in hiring and hence nearly all claims are filed by individuals who were employees at some point (Donahue and Siegelman 1991).

There exists a substantial literature on labor market anti-discrimination policies, including workplace discrimination law. Yet, while Title VII of the Civil Rights Act of 1964 and Executive Order 11246 were arguably two of the most controversial labor market interventions in U.S. history, we know little about their impact on the labor market (Donahue and Heckman 1991). This paper builds on an extensive literature documenting evidence that affirmative action regulation increased the black share of employees at federal contractors, at least prior to the early 1980’s, when the Reagan administration significantly defunded the agency charged with the regulation’s enforcement (Leonard 1984, 1990; Ashenfelter and Heckman, 1976; Goldstein and Smith, 1976; Heckman and Wolpin, 1976; Smith and Welch, 1984; Rodgers and Spriggs 1996; Kurtulus 2011).

In an important prior contribution, Kurtulus (2011) exploits within employer variation in contractor status and one of the data sets I also use here to estimate the impact of affirmative action regulation on the employment of women and minorities. Her combined estimates for black men and women imply that regulation generates an immediate 0.08 percentage point level increase in the black share of a firm’s employees, that this effect is roughly unchanged two years after a firm transitions to non-contractor, and that the initial impact of the regulation completely dissipates as early as four years after a firm gains contractor status. Kurtulus (2011) is the first to consider the potentially dynamic effects of the regulation on employment, an important innovation.

Building on this insight, a primary contribution of the present paper is to focus on the dynamic effects of regulation, particularly for temporarily regulated employers, and to consider the implications that persistence may have identification. I apply a flexible event study research design and find that: (1) the regulation’s causal effect on black employment is substantially larger than previously estimated, particularly after Reagan-era defunding; (2) the impact accumulates over time; and (3) an establishment’s black share of employees continues to increase many years following an employer’s deregulation. I also provide evidence that the persistence I find here is not driven by anticipatory behavior, selective attrition, or measurement error.

The persistent effect of temporary regulation I document here has important implications for interpreting existing research in this literature, including Kurtulus (2011, 2012), Leonard (1984, 1990), Rodgers and Spriggs (1996), Ashenfelter and Heckman (1976), Goldstein and Smith (1976), Smith and Welch (1984), and Heckman and Wolpin (1976). Previous papers apply research designs
based on comparisons of contractors to non-contractors, either across or within employers. In the presence of persistence these comparisons may substantially understate the causal impact of regulation because some employers that are currently non-contractors were previously contractors, and the minority share of those employers is still affected by the regulation. In particular, the more relevant comparison is between employers that have ever been contractors to those that have never been contractors. The flexible event study research design employed here accommodates such persistence. This may explain why in contrast to the existing literature I find that affirmative action regulation has a larger and more persistent effect on black employment.

Sociologists have argued that formal changes in personnel policies played a key role in defining both Title VII and affirmative action compliance (Dobbin 2009), though there is little evidence on whether those changes influence employee composition (Kalev et al. 2006). Using one of the data sets I use here, Holzer and Neumark (2000b) find that regulated firms search for, evaluate, and train workers more intensely. I highlight screening capital as one channel for the persistence I find here and provide novel evidence that this channel is empirically relevant.

This paper most closely relates to work by McCrary (2007) and Miller and Segal (2012), both in subject and research design. They study racial hiring quotas that federal courts imposed on municipal police departments and other law enforcement agencies in the 1970’s. The authors find that these quotas significantly increased black employment relative to national trends. In addition, Miller and Segal (2012) find that, following the termination of these court orders, black share gains do not erode, but the share does stagnate. By contrast, I find that the black share of employees continues to grow even after private employers are deregulated.

There are several potential reasons why the results presented here differ from those in Miller and Segal (2012). Police departments may differ from private employers in the objective they face when choosing whom to hire. Police departments are also relatively constrained in their screening methods, often relying solely on service exams in selecting among applicants. Indeed, existing evidence suggests that departments primarily complied by adjusting their hiring threshold for the protected groups (Gaines et al. 1989). Hence, the screening investments that I argue are an important driver of persistence for private employers may not be a relevant channel for police departments.

This paper builds on a literature that investigates how employers screen workers in hiring. Autor and Scarborough (2008) show that the introduction of job testing at a large retail firm did not reduce minority hiring despite minorities performing significantly worse on the test, and generated productivity gains for both minority and non-minority hires. They argue that job testing will not decrease (and may increase) minority hiring as long as the test is unbiased relative to the preexisting screen. Relatedly, Holzer et al. (2006) and Wozniak (2012) argue that the use of criminal background checks and drug tests increases black hiring by providing information that

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5 In the political domain, Beaman et al. (2009) find that gender quotas for leadership positions on Indian village councils lead to electoral gains for women in subsequent selections. The authors provide evidence that this persistence is in part driven by changes in voter attitudes toward female leaders.

6 Miller and Segal (2012) also find that hiring quotas had only a marginal impact on female employment.
is perceived to be more relevant for black candidates. Autor (2008) argues that temporary help firms serve as a screening device for employers, pre-screening candidates and allowing employers to audition workers without the legal risks associated with firing. I argue that affirmative action induces employers to adopt new screening devices, and that a temporary regulation may generate persistent changes in screening.

Recently, researchers have provided evidence that referral hiring is an important screening device for firms (e.g. Fernandez et al. 2000; Fernandez and Weinberg 1997; Brown et al. 2012). Given that referral networks tend to display homophily (McPherson et al. 2001), this screening method is likely ‘information-biased’ in the sense that it provides more information about one social group of workers than another. I find that for regulated and larger employers, the most recent hire is less likely to be a referral. Relatedly, Giuliano et al. (2009) and Aslund et al. (2009) show that, all else equal, hiring managers tend to hire workers drawn disproportionately from their own race or immigrant group, respectively. Both sets of authors provide evidence that suggests these results are in part driven by managers having a comparative advantage in screening own-group workers.

I argue that affirmative action causes employers to extract or more precisely interpret information about potential employees in a manner that reduces between-group employment disparities.

The remainder of the paper is organized as follows. The next section describes the relevant history and details of federal affirmative action regulation. In section 3 I estimate the dynamic effects of regulation and deregulation in an event study framework. In section 4 I discuss potential causal mechanisms and introduce the screening model. I then present supporting evidence in section 5. Section 6 concludes.

2 Institutional Background

Issued by President Lyndon B. Johnson in September 1965, Executive Order 11246 mandates that federal contractors take ‘affirmative action’ to ensure nondiscrimination in their hiring and employment. While Title VII of the Civil Rights Act of 1964 outlawed discrimination on the basis of race, color, religion, sex, or national origin in all but the smallest private firms, Executive Order 11246 required that firms with federal contracts make active efforts to prevent discrimination. For firms with 50 or more employees and holding $50,000 or more in federal contracts over a 12-month moving window, the requirements are more specific. In particular, such contractors are to identify underutilization of minorities and women in any occupation group relative to “availability.” In identifying availability firms must consider “the availability of minorities having requisite skills in an area in which the contractor can reasonably recruit” (OFCCP 2013). Moreover, contractors are required to make “good faith” efforts to rectify underutilization, including the use of numerical goals with timetables. Broadly speaking, affirmative action mandates that federal contractors

7 Using National Longitudinal Survey of Youth data, Finlay (2009) finds evidence that after state criminal history records become available on the Internet, labor market outcomes are worse for ex-offenders. Estimates for non-offenders from highly offending groups are positive, though statistically insignificant.

8 Executive Order 11246 did not cover discrimination on the basis of sex. The regulation was expanded to include women in 1967 under Executive Order 11375.
make a good faith effort to employ minorities at rates (at least) proportional to shares of local and qualified workforce, though local and qualified are not specified precisely. This regulation applies to all establishments under the firm, regardless of whether the particular facility is executing any portion of the contract. Hereafter, I will refer to Executive Order 11246 as affirmative action (AA) regulation. I will also refer to establishments as federal contractors if their parent firm meets the above size criteria.

Initially, 13 federal contracting agencies—for example, the Department of Defense and the General Services Administration—were responsible for enforcing AA regulation. Enforcement responsibilities were generally assigned on the basis of a contractor’s industry irrespective of the agency contracting with the firm (Anderson 1996). The Office of Federal Contract Compliance (OFCC) was also established in the U.S. Department of Labor to advise and coordinate enforcement activities across contracting agencies. Although all agencies received guidance from the OFCC, there was wide variation across agencies in the scope and quality of their enforcement activities. In 1978, the Carter Administration consolidated the AA regulation enforcement activities under the renamed Office of Federal Contract Compliance Programs (OFCCP). Enforcement personnel from contracting agencies were reassigned to the OFCCP. In the analysis below, I use only data from 1978 onwards.

While a firm is a contractor, it is required to write an Affirmative Action Plan (AAP) for each establishment. An AAP must describe the organizational structure of the firm and establishment, identify underutilization of minorities by job group, and detail strategies, goals, and timetables for eliminating underutilization in the next year and beyond. Each AAP must be updated annually while the firm is a contractor. Contractors must also “maintain and have available records for each job on all applicants, hires, promotions, terminations, and any other selection decisions” disaggregated by minority group (OFCCP 2013).

To enforce the regulation, the OFCCP conducts compliance evaluations, reviews of a small fraction of covered establishments each year (about 1% of covered establishments in 2004⁹) to determine whether their AAPS are sufficient and whether they have made good faith efforts to implement their plans. These reviews focus on a contractor’s performance in the last AAP year, where that calendar begins when the contractor updates their plan. The OFCCP also examines current year performance if a contractor is six or more months into its current AAP year. After the OFCCP notifies a contractor establishment that it has been selected for review, the establishment must submit its relevant AAP(s) and workforce flow data described above. Compliance evaluations consist of a desk audit and a possible site visit. As part of the desk audit, compliance officers determine whether an establishment’s AAP is adequate and whether the establishment made sufficient efforts to improve minority utilization, relying on the submitted personnel data and EEO-1 form I use here and describe below. If potential violations are identified during the desk audit, the OFCCP may conduct an on-site review at the establishment. During a site visit, compliance officers

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⁹There were 6,529 compliance evaluations conducted in 2004. This was actually a peak number over this period; from 2001-2010 the annual average was 4,500.
further investigate potential violations, verify the firm’s efforts to implement its AAP, and obtain information needed to work with the contractor to resolve any violations. Officers accomplish this in part by inspecting the contractor’s facilities and reviewing its personnel files.

If the OFCCP finds that a contractor is not in compliance, the OFCCP will seek a letter of commitment for minor violations or a conciliation agreement for major violations. Some of these agreements include financial settlements that involve back pay to alleged individual victims of discrimination. In 2004, the OFCCP collected $34.5 million from settlements on behalf of more than 9,000 workers. If the OFCCP and a contractor fail to resolve AA violations the OFCCP may take legal actions to penalize the contractor. The ultimate punishment for a contractor is to be debarred from doing business with the federal government, sometimes permanently. However, this outcome is quite rare. Only 43 companies were debarred up to 2001. About half refused to develop an affirmative action plan or submit personnel data, while the other half did not make sufficient efforts to implement plans or violated an existing conciliation agreement. About sixty percent of debarred firms were later reinstated, and for those contractors the median period of debarment was 9.5 months (Pincus 2003).

Critically, the allocation of federal contracts is administered separately from AA enforcement. Hence, the racial composition of a firm should have no direct effect on whether it acquires a federal contract. The one potential exception is very large contracts. For very large contracts, firms are formally required to ‘pre-award’ compliance evaluations—they must be in compliance before they can formally initiate the contract (OFCCP 2013). In practice, very few contracts are sufficiently large to require pre-award compliance evaluations, and they are even less common for the firms I focus on below, which are not perennial contractors. Moreover, there is no requirement that an establishment be in compliance when it is not holding a federal contract.

3 The Dynamic Effects of Affirmative Action

3.1 Data


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10 There are two additional points to note about compliance evaluations. First, the targeting of early compliance evaluations appears to have been limited. A 1975 GAO report states that early compliance evaluations were primarily targeted based on employer size (GAO 1975). Leonard (1985a) confirms this. Second, at least in the early years of the regulation, the goals that employers set for themselves do not appear to act as rigid quotas. Leonard (1985b) finds that for sample of contractors in the 1970’s, goals for minority share gains are positively correlated with realized gains, but the goals were rarely met.

11 Pincus (2003) estimates that more 500,000 companies were government contractors between 1972 and 2001.

12 Minority-owned businesses can sometimes qualify for set asides or other bid preferences for ‘disadvantaged’ businesses. Critically, eligibility depends on the background of the company’s ownership, not the racial composition of its employees.
Civil Rights Act of 1964, firms meeting certain size requirements are required to complete EEO-1 forms annually and submit them to the EEOC. Firms are required to report their overall racial and gender composition and the racial and gender composition of each of their establishments meeting size requirements, disaggregated by 9 major occupation groups. Before 1982, all firms with 50 or more employees were required to submit EEO-1 forms. In 1982, the firm size cutoff was adjusted up to 100. For federal contractors, the cutoff was 25 employees before 1982 and 50 afterwards. Firms are required to file a separate report for each establishment with at least 50 employees and the company headquarters. Establishments are consistently identified with firm and establishment identifiers. I observe each establishment’s location, contractor status, and industry. Moreover, over my period of study, the OFCCP primarily used the EEO-1 data to identify federal contractors.

I conduct my analysis at the establishment level. As discussed above, while regulation status is assigned at the firm level, the regulation defines compliance and is enforced at the establishment level. For the analysis, I limit the sample to establishments located in metropolitan areas where the black share of workers in the data is at least 5% at some point from 1978-2004. This includes establishments from 204 metropolitan areas, where more than 80% of metropolitan area establishments are located. Accordingly, this restriction does not substantively affect the results. I make the restriction to facilitate comparisons between local labor markets with significant black populations to those without, where compliance costs are presumably much smaller.

Due to the size requirements, establishments in the EEO-1 data are not representative of all U.S. establishments. I estimate coverage rates for the EEO-1 data in 1990 in Table A.1 in the Appendix. I calculate the proportion of employment accounted for in the EEO-1 data across industries by dividing EEO-1 reported employment by totals derived from County Business Patterns data for the 204 MSAs studied in this analysis. Unsurprisingly, industries that tend to have large establishments, e.g. manufacturing, are overrepresented, while industries that tend to have small establishments, e.g. services, are underrepresented. Overall, the EEO-1 data account for about 40% of total employment.

3.2 Research Design

I estimate the dynamic effects of AA regulation on the racial composition of regulated establishments. I focus on establishments that are temporarily regulated; those that are federal contractors for some initial period, but then never observed as a contractor again.

Estimating the causal effects of AA regulation is complicated by the fact that those firms sub-

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13 The 9 occupation categories consist of: officials and managers, professionals, technicians, sales workers, administrative support workers, craft workers, operatives, laborers/helpers, and service workers.
14 Results throughout are similar if I impose a uniform firm size cutoff of 100 employees for all establishment observations.
15 Each of these is likely recorded with some error.
16 This follows prior work in this literature with the exception of Kurtulus (2011, 2012), who conducts her analysis at the firm level.
17 To define metropolitan areas, I use 1980 Census definitions.
18 In addition, some firms fail to submit required EEO-1 forms.
ject to regulation, federal contractors, may differ from non-contractors on other dimensions that determine workforce composition, even in the absence of AA regulation. This makes simple comparisons of contractors to non-contractors difficult to interpret. Acknowledging this issue, previous researchers have estimated the causal effect of AA regulation by comparing black share growth across contractors and non-contractors (e.g., Leonard 1984). Comparing growth rates effectively nets out time-invariant level differences between establishments, potentially removing the influence of confounding factors from contractor to non-contractor comparisons. Still, a comparison of growth rates may be misleading if contractors and non-contractors are on different counterfactual trends. For example, firms that anticipate hiring more black workers may find it less costly to comply with AA regulation and hence may be more likely to seek federal contracts. Moreover, if the effect of AA on black share growth fades out over time—for example, if the operative channel is a constant level effect on the black share of new hires—then a comparison of growth rates may substantially understate the causal effect. In a further refinement of the literature, Kurtulus (2011) exploits within firm variation in contractor status, potentially alleviating concerns over selection. Previous research in this area suffers from an additional shortcoming: if regulation has an impact on employers that persists even when they are no longer contractors, previous estimates may be biased. This is because the research designs applied in existing work are based on simple comparisons of contractors to non-contractors, either within or across employers. In the presence of persistence these comparisons may substantially understate the causal impact of regulation because some employers that are currently non-contractors were previously contractors, and the minority share of those employers is still affected by the regulation.

To neutralize these concerns, I exploit variation in the timing of first and last federal contracts across establishments in an event study design. The idea is that the timing of when a firm is first or last a contractor will depend primarily on the availability and profitability of federal contracts rather than potential compliance costs, which seem unlikely to involve substantial idiosyncratic variation within an employer. I estimate two sets of event studies. First, I estimate models for establishment black share that include lead and lag indicators for the first year an establishment is reported as a contractor in the data. I refer to this model as the regulation event study. If lead indicators are not significantly different from zero (implying that establishments that become contractors are not on pre-existing trends), I take the lag indicator coefficients as estimates of the dynamic effects of initial contractor status on establishment black share. This approach follows McCrary (2007) and Miller and Segal (2012). Second, I estimate models where the event of

\footnote{As a test for selection bias, Kurtulus (2011) also estimates an event study for transitions from non-contractor to contractor. She finds that these transitions are associated with a slight increase in the black female share of employees on the order of 0.05 percentage points that completely dissipates in 4 years, and no detectable increase in the black male share. A key distinction between the event studies estimated in Kurtulus (2011) and those estimated here is that Kurtulus (2011) estimates a single model for all transitions from non-contractor to contractor, while I focus on the first such transition for employers that have never been contractors. This distinction is important because (a) many employers experience multiple such transitions from non-contractor to contractor and (b) regulation may have an effect that persists even when an employer is no longer a contractor. By the reasoning discussed above, in the presence of persistence, a model that treats all transitions from non-contractor to contractor the same may substantially underestimate the regulation’s impact. I discuss this bias in more detail later in section 3.3 and in the}
interest is an establishment losing its status as a federal contractor, and never acquiring another contract (in the sample window). This is the next year an establishment is observed after the last year it is reported as contractor. About 57% of the establishments that I observe becoming contractors in the first place eventually lose their contractor status. I refer to this model as the deregulation event study. I interpret changes in the pattern of coefficients following the event as reflecting the effects of becoming deregulated. For example, if the lag coefficients are negative or sloping downward, that would suggest fadeout of the effects of AA regulation on establishment composition.

One additional concern that applies to the research design here is that contractor status may affect establishment racial composition through channels other than AA regulation. I explore this issue further in section 3.4.

To identify the causal effect of AA regulation, I focus on the year an establishment is first identified as a contractor as the event of interest. I do this even though the establishment may lose and even regain contractor status later. I first estimate regression models of the form:

\[ Y_{it} = \alpha_i + \lambda_{d(i),t} + X_{it} \gamma + \sum_{j=a}^{b} \theta_j D_{it}^j + \epsilon_{it} \]  

(1)

where \( Y_{it} \) denotes the black share of establishment \( i \) in year \( t \), \( \alpha_i \) and \( \lambda_{d(i),t} \) are establishment and Census division by year fixed effects, \( X_{it} \) are controls for establishment size, and \( D_{it}^j \) are leads and lags for establishments first becoming contractors, defined as

\[ D_{it}^j = D_i 1(t = \tau_i + j) \]

where \( D_i \) is an indicator for whether the establishment ever becomes a federal contractor, and \( \tau_i \) is the year the establishment first becomes a contractor. I normalize the value of \( \theta_{-1} = 0 \). The sequence of \( \theta_j \) can be interpreted as the difference in establishment black share from the year prior to first contract and \( j \) periods thereafter. For estimation, I set \( a = -6 \) and \( b = 6 \).

In estimating (1), I exclude establishments that enter my sample as a federal contractor. I exclude these establishments from the main analysis for two reasons. First, by construction they cannot contribute directly to estimates of \( \theta_j \) for \( j < 0 \). In other words, I cannot assess whether these establishments exhibit pre-trends because they are not observed prior to becoming a contractor. Second, I do not know the year they were first regulated, and so for any given observation I do not know how many years it has been since their regulation event. For the establishments that become contractors, I only include years of data that are in the 6-year window around the event. I do

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20 That is, eventual contractors that do not enter the data as a contractor.

21 Note that I do not observe contractor status prior to an establishment’s appearance in the data. This could cause me to mismeasure the event of interest. For example, an establishment may have been a contractor in a year that I do not observe it in the data, so that the first year I observe the establishment as a contractor is in fact not the first year the establishment was a contractor.

22 The results are similar for alternative windows.
this so estimates of the event study endpoints, $\theta_{-6}$ and $\theta_6$, are not driven by a mixture of various leads and lags. Relatedly, the sample of establishments driving identification of $\theta_j$ may vary with $j$ because establishments are present in the data for a varying number of years. For this reason, as a robustness check I also estimate including only non-contractors and a balanced panel of contractors. For the balanced sample, because this restriction reduces the sample size significantly, I set $a = -5$ and $b = 5$.

If AA regulation has its intended effects and the event study design is valid, we should see that $\theta_j$ is approximately 0 for $j < 0$ and positive and increasing in $j$ for $j \geq 0$. The increase in establishment black share may be gradual rather than discontinuous because establishments are likely to adjust their racial composition by changing the composition of new hires.

I estimate an analogous event study model where the event of interest is an establishment losing its status as a federal contractor, and never acquiring another contract. I refer to this model as the deregulation event study. In that model, $D_i$ is an indicator for whether the establishment ever loses its contractor status without ever regaining it (over the sample window), and $\tau_i$ is the year the establishment is last observed as a contractor. For these establishments, AA regulation is temporary. The results from this exercise will inform us about persistence of the regulation effect when that regulation is lifted. Note that while selection out of contractor status might be endogenous—for example, if firms exit as contractors because they experience positive shocks to compliance costs—a natural selection story would bias the results against finding persistence. In estimating this model, I only include contractors that lose their status as contractors. Hence, the comparison is between establishments that lose their status as contractors and establishments that have never been contractors. Again, for contractors and former contractors, I only include years of data that are in the 6-year window around the event. I also estimate the model including only non-contractors and a balanced panel of one-time contractors. For the balanced sample, I set $a = -5$ and $b = 5$.

I present summary statistics for the sample of establishments used here in Table 1. There are four samples of interest. There is the non-contractor sample, establishments that are never observed as contractors in the data. These establishments serve as a control group in all the event studies, helping to identify the region by year fixed effects as well as the $\gamma$ coefficient. There are the full regulation and deregulation event samples. These include all eventual contractor establishments that do not enter or do not exit the data as a contractor, respectively. Finally, there is the ‘overlapping’ sample, which includes all eventual contractors that meet both criteria. To facilitate comparisons between the regulation and deregulation event studies, I focus on the overlapping sample for much of the analysis.

Non-contractors and the event study samples include establishments that are somewhat smaller than the average establishment in the data. They are more likely to be in Retail Trade. Prior to regulation, establishments in the regulation and overlapping samples have employee black shares that are very similar to non-contractors. Overall, non-contractors and eventual contracts look quite similar prior to regulation. The average black share of employees across establishments is 14.1%,
while the average black share of the working age MSA population, weighted by the number of observations, is 15.5%.

The event study design requires variation in the timing of the event of interest for the contractor sample. Figure 1 displays this variation for the overlapping sample. Both regulation and deregulation events vary widely in their timing.

3.3 Main Results

I begin with the regulation event study. In Panel A of Figure 2 I plot the point estimates and 95% confidence intervals of the $\theta_j$ sequence for the overlapping sample, with standard errors clustered at the firm level. The estimated model includes Census division by year fixed effects. The pattern is clear: while black share is flat for eventual contractors prior to their first contract, black share begins to increase as soon as those establishments become contractors. This suggests that the research design is valid and that the $\theta_j$'s have a causal interpretation. The effects are sizable and precisely estimated. Five years after first becoming a contractor, establishment black share grows by about 0.88 percentage points.

I assess the robustness of these estimates by altering the sample and set of controls used. The results from this exercise are presented in the top panel of Table 2. Columns (1) through (4) use the full regulation sample and columns (5)-(8) use the overlapping sample. Columns (4) and (8) restrict to a balanced sample of eventual contractors. Columns (1), (4), (5), and (8) include Census division by year fixed effects, columns (2) and (6) include MSA by year fixed effects, and columns (3) and (7) include Census division by 1-digit industry by year fixed effects. The coefficients from column (5) are plotted in Figure 2. All models include a quadratic in log establishment size. The results are similar across specifications.

Moreover, the estimated $\theta_j$ coefficients suggest a linear relationship between black share and years since first federal contract. Given this, I parameterize the event study to estimate the slope before and after the regulation event using the same sample. In particular, I estimate models of the form

$$Y_{it} = \alpha_i + \lambda_{d(i)t} + X_{it}\gamma + \beta t \times 1_{\exists \tau_i} + \Delta \beta (t - \tau_i + 1) \times 1_{(t \geq \tau_i)} + \epsilon_{it}$$

where $1_{\exists \tau_i}$ is an indicator for whether an establishment is ever observed as a contractor. I present these parametric coefficient estimates in Panel B of Table 2. Note that, except for the balanced sample, the slope estimates put more weight on years closer to the event simply because eventual contractors are more likely to be observed in those years. The coefficient on years since first contract ranges from 0.131 to 0.182. This implies that becoming a contractor increases an establishment’s black share by 0.131 to 0.182 percentage points for each year thereafter, on average. Across specifications, the pre-event slope bounces around in a small window centered at zero. The estimated slope for the balanced samples is somewhat lower, ranging from 0.132 to 0.146. Again, the pre-event slope is a relatively precise zero. Differences in the estimates may reflect differences.
across the samples, for example in establishment size, location, or industry.\footnote{For example, establishments in the balanced sample are larger and older than establishments in the full sample prior to regulation.}

Altogether, these results suggest that AA regulation has a sizable effect on establishment personnel composition. The slope estimates are comparable to those found in Leonard (1984), though Leonard (1990) finds that AA had no impact on black employment in the 1980’s using the same identification strategy.\footnote{Leonard (1984) finds that, from 1974 to 1980, affirmative action increased the relative growth rate of employment for black men and women by 0.84 and 2.13 percent annually.} I do not find this to be the case. This may be due to the differences in our research designs described above.\footnote{Note that our research strategies also require different samples. Specifically, I focus on establishments that do not enter the data as contractors. Differences in our estimates may be due in part to heterogeneous treatment effects.}

It is important to note that many establishments included in the regulation event study are no longer contractors in the years following their first year as a contractor. In Figure A.1 in the Appendix, I tabulate the number of eventual contractors in the overlapping sample that identify each lead and lag in Panel A, as well as the fraction of eventual contractors that are contractors in each year following the regulation event in Panel B. A year after their regulation event, only about 35\% of establishments are still contractors. The same statistics are displayed for the deregulation event study.

Next, I present results for the deregulation event study. In Panel B of Figure 2 I plot the point estimates and 95\% confidence interval of the $\theta_j$ sequence for the overlapping sample. The model includes Census division by year fixed effects. Prior to the deregulation event, an establishment’s black share of employees is increasing as it is following the regulation event. Strikingly, the black share continues to increase following deregulation. Before the event and while regulated, an establishment’s black share is increasing at a rate even larger than that found in the regulation event study. After the event, a positive slope remains. In this sense, temporary AA regulation generates ongoing increases in an establishment’s black share.\footnote{This could be driven by temporary AA generating a persistent level increase in the black share of new hires, for example.}

As for regulation event study, I assess the robustness of these estimates by varying the sample and set of controls used. I present the results in Table 3. Again, the estimates are comparable across specifications. I also estimate a parametric version of the model analogous to (2). The results are included in Panel B of Table 2. For the complete deregulation or overlapping samples, the pre-event slope estimates are nearly twice as large as the post-event slope estimates found for the regulation event study. After the deregulation event, this slope is half to two-thirds as large, so that the post-deregulation event and post-regulation event slopes are comparable. For the balanced sample, the pre-deregulation event slopes are about 50\% as high as the corresponding post-event slope estimates for the regulation event study. There is little to no change in slope after the deregulation event. Overall, establishment black share continues to grow after the deregulation event at a rate comparable to that which emerges when establishments are first regulated.

For the overlapping subsample of eventual contractors, the regulation and deregulation event...
studies can be effectively combined in one plot. I estimate a series of regulation event studies, with separate estimates for eventual contractors who experience their deregulation within 1 year, 2-3 years, 4-6 years, 7-9 years, and more than 9 years. Figure 3 displays the results. In all cases, the black share of employees continues to increase even after establishments are deregulated.

The event studies can also be easily combined into one parametric regression model. Using the overlapping sample, I estimate the following model:

\[ Y_{it} = \alpha_i + \lambda d(i)_t + X_{it} \gamma + \beta t \times 1_{t \geq \tau_i} + \beta^R(t - \tau_i^R + 1) \times 1_{(t \geq \tau_i^R)} + \beta^D(t - \tau_i^D + 1) \times 1_{(t \geq \tau_i^D)} + \epsilon_{it} \]  

(3)

where \( \tau_i^R \) and \( \tau_i^D \) denote regulation and deregulation event years, if applicable. I use all observation years, not restricting the data to any window around the event years. I estimate a pre-regulation slope, \( \beta \), of -0.037 (with standard error 0.031); a post-regulation slope change, \( \beta^R \), of 0.277 (0.059); and a post-deregulation slope change, \( \beta^D \), of -0.160 (0.037). The slope estimates are nearly identical if I include a quadratic post-regulation term. I discuss the slope fadeout associated with deregulation in more detail in section 3.8.

The persistent effect of temporary regulation I document here has important implications for interpreting existing research in this literature, including Kurtulus (2011, 2012), Leonard (1984, 1990), Rodgers and Spriggs (1996), Ashenfelter and Heckman (1976), Goldstein and Smith (1976), Smith and Welch (1984), and Heckman and Wolpin (1976). In particular, if regulation has an impact on employers that persists even when they are no longer contractors, previous estimates may be biased. This is because the research designs applied in existing work are based on comparisons of contractors to non-contractors, either within or across employers. In the presence of persistence these comparisons may substantially understate the causal impact of regulation because some employers that are currently non-contractors were previously contractors, and the minority share of those employers is still affected by the regulation. In the Appendix, I evaluate this bias in more empirical depth.

### 3.4 Coincident Changes in Employer Characteristics

One concern with interpreting the above results is that the regulation and deregulation events involve more than changes in the set of regulations to which an establishment is subject; contractor status may have direct implications for how an establishment is organized and who it employs. I explore two potential issues directly. First, the occupational composition of an establishment

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27With slight modification, I also estimate excluding non-contractors from estimation. In this case, the regulation and deregulation effects are identified using only variation in the timing of events among eventual contractors. This approach is appealing in that it does not rely on non-contractors to identify the counterfactual black share for eventual contractors. However, as McCrary (2007) points out, the trend break model is not identified using only eventual contractors. To circumvent this issue, I include observations more than 6 years prior to the regulation event, augment the model with an indicator for years more than 6 years preceding the regulation event, and limit the pre-regulation slope to apply to 6 years preceding regulation and thereafter. Reassuringly, the results are similar. I estimate a pre-regulation slope, \( \beta \), of -0.009 (with standard error 0.056); a post-regulation slope change, \( \beta^R \), of 0.282 (0.065); and a post-deregulation slope change, \( \beta^D \), of -0.153 (0.039). The coefficient on the indicator is -0.295 with standard error 0.355, statistically insignificant at the 10% level.
may change when it becomes a contractor or loses its status as contractor. If racial composition varies systematically by occupation, an establishment’s black share may change even if there are no changes in within-occupation black share. To assess the importance of occupational changes, I reestimate the event studies focusing on within-occupation changes. Second, the size or growth trajectory of establishments may change with either the regulation or deregulation events. Establishment size or growth may affect establishment composition independent of AA regulation, a question that will be explored further below. For this reason, I assess whether accounting for establishment size or growth has important implications for the above event study results.

To reestimate the event studies using within-occupation changes, I estimate (1) at the establishment by occupation level (rather than the establishment level.) In particular, I estimate

\[ Y_{i,o,t} = \alpha_{i,o} + \lambda_{d(i),t} + X_{i,t}\beta + \sum_{j=a}^{b} \theta_j D_{j,i,t} + \epsilon_{i,o,t} \]  

where \( o \) indexes occupation and \( Y_{n,o,t} \) is the black share of employees in an establishment by occupation cell. Note that (4) now includes occupation by establishment fixed effects. I weight observations by the current share of an establishment’s workers in that occupation, and cluster standard errors at the firm level.

I plot the coefficients in Figure 4, alongside the analogous coefficients displayed in Figure 2. Panel A displays the regulation event study and Panel B displays the deregulation events study. The results are very similar to those in Figure 2, implying that those results are not a feature of changes in occupational composition associated with gaining or losing contractor status.

Another employer characteristic that may vary with contractor status is establishment size. Government contracts may be sizable relative to employers’ total revenue, so that employers grow or shrink when they become contractors. To explore how establishment size evolves while an establishment is regulated, I estimate separate event studies for the overlapping sample and for a subsample restricting eventual contractors to those with more than 6 years between their regulation and deregulation events. For each sample, I estimate a variant of (1) replacing black share with log establishment size as the dependent variable.

The results are shown in Figure 5. For both samples, establishment size is growing preceding the regulation event. This may be due in part to establishments anticipating a future government contract. For the restricted subsample, establishment size continues to increase following the regulation event. Over 5 years after the regulation event, establishment size increases by about 5% for this subsample.

By contrast, establishment size begins to decrease two years before the deregulation event and continues to decline after the event for both samples. The size responses to regulation and deregulation appear roughly symmetric, in stark contrast to how the black share of employees evolves. This exercise also serves as a falsification test for the main regulation and deregulation event study results. The broadly symmetric results for establishment size suggest that the regulation
Given that establishment size decreases after the loss of contractor status, it is unclear whether the black share increases following deregulation actually reflect increases in the number of black employees. Instead, the number of black employees may stagnate or decline while the number of non-black employees declines at a faster rate. This alters the interpretation of the persistence somewhat. For example, this suggests the persistence may not come from changes in who is hired, but rather changes in relative turnover. As I will show below, as establishments grow, their black share tends to increase, while the opposite is true for declines. Hence, the size declines alone are unlikely to be responsible for the black share increases following deregulation.

To explore this question, I split the sample into establishments that decrease in size from their last year as contractor to the last year they are observed in a 6-year window following the deregulation event, and establishments that increase in size over that period. About 43% of establishments grow over this period. I then replicate the deregulation event studies separately for the two subsamples with the following modification.

To form more appropriate comparison groups for the two subsamples, it would seem natural to divide non-contractors into those that shrink and grow over comparable periods. This is complicated by the fact that there is no analogous ‘event’ to use as a point of reference for establishments that never become contractors. To deal with this issue, I assign pseudo ‘events’ to establishments that never acquire federal contracts. I do this by conditioning on two variables: the year I first observe the establishment in the data and the number of years between the first and last year. I then randomly assign an ‘age’ for each establishment’s pseudo event, taking draws from the conditional age distribution for former contractors that lost their contractor status. Then, using this pseudo event, I similarly split the sample into establishments that decrease and increase in size following ‘deregulation.’ Finally, I estimate the following model separately for the two subsamples:

$$Y_{it} = \alpha_i + \lambda d(i),t + X_{it} \gamma + \sum_{j=a}^{b} \theta_j D_{it}^j + \sum_{j=a}^{b} \tilde{\theta}_j \tilde{D}_{it}^j + \epsilon_{it}$$

where \( \tilde{D}^j \) are analogous leads and lags for each establishments event, real or synthetic. The \( \theta_j \) coefficients are the differential effects for establishments that are actually deregulated.

The results are shown in Panels A and B of Figure 6. In fact, the event studies for establishments that shrink and those that grow are quite similar.\(^{29}\) For establishments that grow, total black employees continue to grow after the loss of contractor status.

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\(^{28}\) These patterns also emerge in local labor markets with very small minority populations, suggesting the size responses are not due to regulation per se, but contractor status.

\(^{29}\) In results not shown here, I find that the results are not sensitive to whether establishment size is included as a control variable.
3.5 Anticipatory Behavior

An additional concern with interpreting the above results is that my definition of temporary is *ex-post*. I define an establishment as temporarily subject to AA regulation if it is a contractor for some period and then never observed as a contractor again in the future. But if employers anticipate that they will become contractors again, this may blur the distinction between regulated and unregulated. Relatedly, unregulated employers may perceive that increasing their black share will improve their chances of acquiring a future contract. In this section, I address these two concerns.

First note that it is not *a priori* clear how the regulation would incentivize anticipatory behavior in personnel decisions. In particular, the regulation does not require that firms be in compliance when they are not regulated. Moreover, as discussed in section 2, a firm’s racial composition should have no direct bearing on whether it is able to acquire a future contract. Nevertheless, it is possible that employers do not understand this, that contract allocation depends on racial composition for reasons outside of the law, or that adjustment costs make anticipatory behavior sensible. I assess the potential importance of anticipatory behavior in driving the previous results in two ways. First, I estimate how the likelihood of future regulation evolves after an employer loses its contractor status. Second, I investigate whether current black share predicts future contractor status.

To accomplish the former, I compute the conditional probability for a firm to acquire a future contract as a function of the number of years since the firm last held a contract. This enables two types of comparisons. First, I compare the acquisition likelihood for establishments that previously held a contract to those that have never held a contract. Second, I explore how the acquisition likelihood evolves with the number of years since a firm last held a contract. In the presence of important anticipatory behavior, we would expect to see a relationship between the likelihood of future contract acquisition and contemporaneous minority share gains. To make the findings applicable to the main results using the overlapping sample, I limit the analysis to establishments that do not enter the data as federal contractors.

These acquisition likelihoods are plotted in Figure 7. The vertical axis denotes the fraction of firms that acquire a federal contract in the future, either in the next year, as in Panel A, or in the next three years, as in Panel B. To avoid censoring, I exclude observations from 2004 and 2002-2004 in constructing Panel A and Panel B. The purple line depicts this likelihood for establishments that have previously held a federal contract, but have not have held a contract for a given number of years, as marked on the horizontal axis. For the 0 value on the horizontal axis, the purple line denotes the likelihood for current contractors. The blue line depicts the likelihood for establishments that have never held a federal contract, and the horizontal axis marks the number of years they’ve been observed in the data.

I estimate regression versions of these plots with controls that include a quadratic in establishment size, 1-digit industry fixed effects, and Census division by year effects to adjust for regional

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30The pattern of results is similar if I do not make this restriction.
and temporal variation. In the regression models, I also examine acquisition likelihoods for a five
year window and whether an establishment ever becomes a contractor as observed in the data.
For these two likelihoods I exclude observations from 2000-2004 and 2004, respectively. I also try
limiting the former contractors to those who have been previously observed as contractors for at
least three years. I present the results in Table 4.

There are three points to note from Figure 7 and Table 4. First, the likelihood that a firm
that has never held a contract acquires one in the future is roughly constant in the age of the firm,
declining slightly with age.

Second, after a year without a contract, one-time contractors are only slightly more likely to
acquire a future contract as firms that have never held a contract. While current contractors are
about 45 percentage points more likely to hold a contract in 1 or 3 years than firms that have
never held a contract, this difference reduces to 5 and 13 percentage points after one year without
a contract. After 4 years, the difference reduces further to 0 and 5 percentage points. Yet, Figure
2 shows that such firms continue to increase their minority share after going four years without
a contract, while there is no evidence of anticipatory behavior for firms that have never held a
contract.

Third, for one-time contractors, there is a substantial decline in the likelihood of acquiring a
future contract with each passing year without a contract, especially after the first year. While
about 55% of firms who hold a contract in one year are contractors in the next, after one year
without a contract, only about 25% of former contractors hold one in the next. After 4 years,
this likelihood declines to about 8%. By contrast, robust black share increases continue following
deregulation.

These results suggest that persistence shown in Figure 2 is unlikely to be driven by firms
anticipating that they will become regulated again.[31]

Firms may perceive that increasing the black share of their employees will improve their chances
of acquiring a future contract. If so, AA regulation may alter the personnel decisions of unregulated
employers. This would complicate the interpretation of temporary AA regulation in this context.

Two pieces of evidence suggest a firm increasing its black share does not make the acquisition
of future contracts more likely. First, as discussed in section 2, the OFCCP has little to no explicit
influence on contract allocation. Second, before eventual contractors are first regulated, their black
share is very similar to those that never become contractors. This is evident in Table 1. Moreover,
initial black share is slightly negatively correlated with future contractor tenure.

3.6 Heterogeneity by Employer Size

In this section, I explore how the regulation’s effect varies with employer size. This exercise serves
two purposes. First, I assess whether estimates are sensitive to the selective attrition of establish-

[31]Still, if firms face substantial adjustment costs in reversing any changes in personnel practices made in response
to AA regulation, former contractors may set a lower threshold for anticipatory behavior than firms who have never
held a contract.
ments from the data. Second, I exploit the fact that compliance evaluations are targeted based on employer size (Leonard 1985a) to examine whether the regulation’s impact is more substantial where enforcement is stronger.

The absence of pre-existing trends in the regulation event study suggests that the black share gains following AA regulation indeed reflect the causal effect. However, the event study may produce biased estimates for the causal effect if establishments selectively exit from the data. The size thresholds for who must submit EEO-1 forms magnifies this concern. In particular, some firms that are near the threshold may alter their size to avoid reporting requirements.

To assess the potential role of selective attrition in producing the above results, I re-estimate both the regulation and deregulation event studies restricting estimation to establishments whose parent firms have at least 150 employees prior to their first federal contract. For eventual contractor establishments, I base this restriction on firm size in the latest year an establishment is observed prior to their regulation event. For non-contractor establishments, I use firm size in the latest year an establishment is observed prior to their pseudo regulation event, where pseudo event events are randomly assigned as described in section 3.4 based on the year I first observe the establishment in the data and the number of years between the first and last year. These establishments are not near the size threshold, and so any manipulation to avoid reporting seems unlikely. Note that over 90% of establishments in the overlapping sample satisfy this criteria.

The results are shown in column (2) of Table 5. Panel A presents regulation event slope estimates, while Panel B presents deregulation event slope estimates. The estimates for both event studies are very similar to those using the full overlapping sample. I conclude that selective attrition is unlikely to be an important concern here.

Leonard (1985a) studies the targeting of compliance evaluations conducted by the Department of Defense over the late 1970’s. He finds that contractor establishments that are part of multi-establishment firms are substantially more likely than singleton contractor establishments to be subject to a compliance evaluation. The likelihood of review is generally increasing in establishment size, though the relationship is concave. Motivated by these findings, I explore how the response to regulation depends of whether an establishment is part of a larger company, and establishment size. Note that I focus on a later period than Leonard (1985a), and the targeting of compliance evaluations has likely changed over time. Hence, these results should be interpreted with caution.

The results are presented in columns (3)-(6) in Table 5. Columns (3) and (4) report estimates based only on singleton establishments and establishments that part of multi-establishment firms, respectively. Column (5) reports estimates based only on establishments with fewer than 100 employees, while column (6) reports estimates based on establishments with 100 or more employees. The regulation appears to have little effect on singleton establishments. Larger establishments experience larger black share gains following the regulation event, and larger gains following deregulation, though similar patterns emerge for smaller establishments. Overall, it appears that establishments that are more likely to be evaluated by regulations respond more to regulation.

Note that while only about 13% of establishments in the overlapping sample are singletons,
they represent over 70% of sample firms. The significant heterogeneity found here implies that an analysis that weights firms equally, as in Kurtulus (2011; 2012), rather than establishments, as done in the present paper and previous work in the literature, will produce estimates of the regulation’s impact that are substantially smaller in magnitude.32

3.7 Heterogeneity by Skill Level

While AA regulation generates a sharp increase in minority share growth, and most of these gains are within-occupation, it is unclear what kinds of jobs are driving this growth. To clarify this, I repeat the within-occupation event study analyses separately by occupation skill level. Following Acemoglu and Autor (2011), I divide the occupation groups defined in the EEO-1 data into three skill groups: high, middle, and low. I label officials and managers, professionals, and technicians as “high skill”; sales workers, administrative support workers, craft workers, and operatives as “middle skill”; and laborers/helpers and service workers as “low skill.”

I present the results in Figure 8. Event study patterns are similar across occupation groups. In absolute terms, the effect on black share is largest in middle skill occupations. Five years after the regulation event, the black share of employees in middle skill occupations increased by 0.8 percentage points. Estimates for high and low skill occupations are similar in magnitude at about 0.6 percentage points, though they are relatively imprecise. In the EEO-1 data, 7.0%, 12.6% and 20.5% of high skill, middle skill, and low skill workers are black. Hence, in proportional terms, the effects of AA are similar for both high and middle skill occupations.

3.8 Slope Fadeout

The deregulation event study results suggest that while the black share of employees continues to increase following deregulation, this persistence is not complete. For example, in Panel B of Figure 2, the post-deregulation event slope is about 35% smaller than the pre-event slope. Moreover, Figure suggests the degree of persistence may depend on an establishment’s experience as a contractor. In this section, I explore possible fadeout in more detail.

Though deregulation is associated with a slope decrease in black share gains, it is not clear whether this decrease is due to deregulation per se or if this decrease would occur in the absence of deregulation. The slope must decrease at some point given that the black share is bounded above, where the ceiling depends on the availability of black workers. To assess whether the slope declines are caused by deregulation, I construct the following falsification test. First, I reassign ‘pseudo’ deregulation event years to one-time contractors. I do this by conditioning on the number of years between an establishment’s regulation event and its last year in the data, and then randomly assign an ‘age’ for each establishment’s pseudo event, taking draws from the conditional age distribution for the actual events. Then I reestimate the deregulation event study using these pseudo events. If the slope change in Panel B of Figure 2 is due to ‘age’ rather than deregulation per se, the same

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32 Weighting by establishment size yields results similar to those presented here.
slope change should be evident in the pseudo event study. If slope change is due to deregulation, the slope change should be significantly less pronounced.

I plot the coefficients for the pseudo deregulation event study in Panel A of Figure 9. There is no discernible slope change following the pseudo event, which suggests that the slope change in Panel B of Figure 2 is indeed due to the deregulation.

Next, I test whether the degree of persistence depends on an establishment's experience as a contractor. I will return to this qualitative finding after introducing a model of endogenous screening capital below. I split contractors into two groups, those with 6 or fewer years between their regulation and deregulation events and those with more than 6 years between events, and estimate deregulation event studies for each group. Establishments in the first group have had an average of 2 years as contractors prior to their deregulation event, while establishments in the second group have had an average 8 years. In these event study models, I extend the endpoint $b$ to 10 years following deregulation, and use the full deregulation sample for power. The results are plotted in Panel B of Figure 9. There are two things to note. First, the initial slope is higher for the experienced group (0.286 percentage points per year) than for the novice group (0.214 percentage points). Second, black share growth following deregulation is more persistent for the experienced group in both absolute and relative terms.

Finally, I assess the long run black share gains associated with AA regulation. I estimate a regulation event study with the endpoint $b$ extended to 20 years and using the full regulation sample. Figure 10 displays the results. The point estimates are increasing up to 16 years after the regulation event, and then bounce around 2.4 percentage points, though the confidence intervals are relatively wide in this range. The black share gains associated with regulation remain quite apparent in the long run.

### 4 Causal Channels

A temporary AA program may produce a persistent increase in black employment through several causal channels. As described earlier, a temporary policy may increase black human capital investment, thereby reducing skill gaps (Coate and Loury 1993). Given the variation in regulation exploited here, it is unlikely the results here are driven by changes in human capital accumulation. The regulation of a particular firm should have little influence on incentives faced by workers, who presumably search in a broader labor market.

Temporary AA may spur employers to update negative stereotypes or reduce taste-based discrimination simply by increasing their exposure to black workers. One may expect beliefs about worker quality to converge across employers. In that case, the impact of regulation on employer beliefs would dissipate over time as employers learned from other companies and updated their beliefs. Still, it is possible that these beliefs do not converge, or do so slowly, and that AA regulation

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33 For example, Boisjoly et al. (2006) show that white college students who are randomly assigned black roommates are significantly more likely to endorse affirmative action and have personal contact with members of other ethnic groups years after assignment.
continues to foster significant updating.

Temporary AA may also generate persistent black employment if employers face binding adjustment costs. Such costs may be relevant on a few margins. First, consider firing costs. If they are significant, a temporary policy may generate a long-lasting increase in the black share simply because workers hired while the policy was active are slow to leave. Given that I find that employee black share continues to grow after deregulation, firing costs alone cannot explain the results. However, firing costs in combination with some other mechanism could generate the persistence found here. For example, if the productivity of black potential workers is increasing in the black share of existing employees—say, due to mentoring (Athey et al. 2000) or role model effects (Chung 2000)—then temporary AA, in the presence of firing costs (or other adjustment costs), could produce the persistence found here.

Employers may also face adjustment costs when changing personnel practices. For example, a firm may formalize its hiring process as part of their AAP. They may maintain those formal practices following deregulation simply because it is costly to revert to an informal process. The lack of persistence found among police departments (Miller and Segal 2012) suggests that if adjustment costs are responsible for the persistence found here, they are somehow less significant or relevant for police departments.

For the remainder of the paper, I focus on the screening capital channel—AA may induce employers to make (partially) irreversible investments to improve screening. In this section, I formalize and discuss this channel in more detail.

### 4.1 A Screening Model with Endogenous Screening Capital

In this section I develop a simple screening model to explain the empirical results above. The model builds on the canonical Phelps (1972) model of statistical discrimination, and the setup is similar in spirit to Cornell and Welch (1996) and Morgan and Vardy (2009). In the model, an employer must hire a set of workers from a pool of candidates. The employer would prefer to hire the most productive candidates, but can only observe a noisy signal for each candidate’s productivity. To improve its screening precision, the employer can invest in what I term screening capital. This term is meant to encompass all methods by which employers can improve screening. I interpret screening broadly as choosing the ‘best’ candidates from a set of potential workers, including both recruitment and selection components of the hiring process, which are often difficult to disentangle conceptually. Examples include: employing and/or training personnel specialists and departments, developing job tests, developing relationships with and utilizing intermediaries such as employment agencies and schools, harnessing referral networks, or even learning by doing or experimentation. I show that if employers are initially less able to infer the quality of minority candidates—an assumption often made in the statistical discrimination literature—screening investments will decrease the hiring gap between groups. I discuss evidence justifying this assumption below.

I then introduce an AA regulation that constrains hiring rates to be equal across groups. I show that, under the same conditions, this regulation will increase the return to screening capital.
The intuition is that screening investments generate more substantial improvements in the expected quality of minority hires, and affirmative action compels the employer to hire more minority workers. Finally, if these screening capital investments are at least partially irreversible, then even a temporary AA regulation may generate persistent changes in hiring.

Suppose an employer must fill a mass \( n \) of vacancies from a mass \( a(n) > 2n \) of applicants. There are two groups of candidates: \( \gamma \in \{B, W\} \). Let \( \pi_\gamma \) denote the share of applicants from group \( \gamma \). Worker match productivity is distributed

\[
\theta \sim N \left( \mu_\theta(\gamma), \frac{1}{h_\theta} \right)
\]

where the mean, \( \mu_\theta(\gamma) \), is allowed to differ by group.

Neither the employer nor worker can observe a worker’s match productivity directly. Instead, for each candidate \( i \) from group \( \gamma \) they observe a noisy signal for match productivity

\[
s^\gamma_i = \theta_i + \epsilon^\gamma_i
\]

where \( \epsilon^\gamma_i \sim N \left( 0, \frac{1}{h_\gamma} \right) \). Note that the signal precision, \( h_\gamma \), is also allowed to vary by group.

Conditional on group \( \gamma \) and signal \( s \), the expected productivity for a given applicant is

\[
\mu(\gamma, s) = \frac{sh_\gamma}{h_\gamma + h_\theta} + \frac{\mu_\theta(\gamma)h_\theta}{h_\gamma + h_\theta}.
\]

That is, expected productivity is a weighted average of the group mean, \( \mu_\theta(\gamma) \), and the signal, \( s \), where the weights depend on the precision of the signal relative to productivity dispersion.

Let \( F(\hat{\mu}; \gamma) \) denote the cumulative distribution function for candidate’s expected productivity after the signal realization. Then,

\[
F(\hat{\mu}; \gamma) = \Phi \left( \frac{\hat{\mu} - \mu_\theta(\gamma)}{\sqrt{\frac{1}{h_\gamma} + \frac{1}{h_\theta} \frac{1}{h_\theta + h_\gamma}}} \right)
\]

and the expected productivity distribution for the entire applicant pool, \( \bar{F}(\hat{\mu}) \), is given by

\[
\bar{F}(\hat{\mu}) = \pi_B F(\hat{\mu}; B) + \pi_W F(\hat{\mu}; W).
\]

Given that the wage is fixed, the employer will simply hire the candidates with the highest expected productivity. Hence, it will set hiring threshold \( \mu^* \) such that \( 1 - \bar{F}(\mu^*) = \frac{n}{a(n)} \).

Now, I will compare hiring rates for the two groups. Let \( \lambda(\gamma) = 1 - F(\mu^*; \gamma) \) denote the hiring rate for group \( \gamma \). Under an assumption often made in the statistical discrimination literature, we

---

34 All that is required here is that there are sufficient applicants so that the employer only hires from the right tail of the expected productivity distribution for each group.

35 Suppose, for example, that the employer commits to a posted wage. This assumption follows both Cornell and Welch (1996) and Morgan and Vardy (2009).
can sign the difference in hiring rates.

**Proposition 4.1** Suppose

\[ \mu_\theta(B) \leq \mu_\theta(W) \]  
and

\[ h_B < h_W \]

Then \( \lambda(B) < \lambda(W) \).

In other words, if one group can be screened more precisely, that group will have an advantage at hiring.

The latter condition may hold if, for example, the primary screening method that the employer is endowed with is using referral networks, which tend to display group homophily (McPherson et al. 2001). Alternatively, workers may be better able to screen candidates from their own group (Giuliano et al 2009; Ashlund et al. 2012). In those cases, the employer may be endowed with a screening technology that favors the group better represented at the employer by some measure, for example, the group membership of the entrepreneur or owner.

### 4.1.1 Screening Capital

In the preceding section, the employer’s ability to screen candidates was fixed. Suppose now that the employer can invest in screening capital to improve screening. In particular, suppose the employer can now pay cost \( c(k) \) for an additional signal, \( s^k_i \), where

\[ s^k_i = \theta_i + \epsilon^k_i \]

where \( \epsilon^k_i \sim N (0, \frac{1}{k}) \). Moreover, let \( c'(k) > 0 \) and \( c''(k) > 0 \). That is, the employer can pay cost \( c(k) \) for signal with precision \( k \), where the cost is increasing and convex in \( k \). Note that the additional signal is equally informative about workers from both groups.

Let \( F(\hat{\mu}; \gamma, k) \) denote the cumulative distribution function for candidates’ expected productivity after the signal realizations for a level of screening capital \( k \). Then,

\[ F(\hat{\mu}; \gamma, k) = \Phi \left( \frac{\hat{\mu} - \mu_\theta(\gamma)}{\sqrt{\frac{1}{h_\theta h_\gamma + h_\gamma + k}}} \right). \]

Under the conditions from Proposition 4.1, these investments will reduce hiring rate disparities.

**Proposition 4.2** Suppose (6) and (7) hold. Then

\[ \lambda(W, k) - \lambda(B, k) > 0 \]

---

36 This may be due in part to differences in nonverbal listening and speaking cues, as in Lang (1986).

37 A richer model could allow the employer to choose among capital that provides signals more informative for one group than the other.
is decreasing in $k$.

The intuition is that, with $h_B < h_W$, the additional signal will be more informative for $B$ workers.

Now, consider the employer’s problem when screening capital is available. Again, conditional on $k$, the employer will choose the candidates with highest expected productivity. Define $\mu^*(k)$ such that $1 - \bar{F}(\mu^*(k); k) = \frac{n}{a(n)}$. Then, in choosing $k$, the employer’s problem is

$$\max_k n \int_{\mu^*(k)}^{\infty} \mu \times f(\mu; k) d\mu - c(k).$$

Denote the employer’s solution by $k^*$.

### 4.1.2 AA Regulation

Suppose an AA regulation is introduced in this setting. I model the regulation as a mandate that the employer must equalize hiring rates across groups. Now the employer’s optimal strategy is to choose two hiring thresholds: $\mu_B^*$ and $\mu_W^*$. In particular, the employer will set $\mu_B^*(k)$ and $\mu_W^*(k)$ such that, for $\gamma \in \{B, W\},$

$$1 - F(\mu^*_\gamma(k); \gamma, k) = \frac{n}{a(n)}.$$

In choosing $k$, the employer’s problem is now

$$\max_k n \left[ \pi_B \int_{\mu_B^*(k)}^{\infty} \mu f(\mu; B, k) d\mu + \pi_W \int_{\mu_W^*(k)}^{\infty} \mu f(\mu; W, k) d\mu \right] - c(k).$$

**Proposition 4.3** Suppose (6) and (7) hold. Then $k^*$ is larger under AA.

The intuition is that screening investments generate more substantial improvements in the expected quality of minority hires, and affirmative action compels the employer to hire more minority workers.

Finally, suppose these screening capital investments have sunk costs. Then in a dynamic setting, even a temporary AA regulation may generate persistent increase in screening capital, and hence produce a durable reduction in between-group hiring rate disparities.

### 5 Screening Evidence

The screening model outlined above generates two main testable predictions. First, AA will increase the return to screening capital. Second, screening investments will reduce between-group disparities in hiring rates.

Combining the EEO-1 data with additional establishment-level survey data, I provide evidence supporting the model. First, I show that regulated employers use more screening methods than
otherwise comparable unregulated employers, particularly ‘formal’ methods such as personnel specialists, job tests, credential checks, and intermediaries. These results largely echo those in Holzer and Neumark (2000b).

Second, I show that an alternative source of variation in screening method use, *employer size*, also tends to equalize group representation among employees. Consistent with previous literature, I find that larger employers use more of the same screening methods. Moreover, I find that black share is increasing in employer size, while the *opposite* is true for black-run businesses.

Also note that a dynamic model would predict that employers expecting a longer spell of regulation would invest in more screening capital, all else equal. The intuition is that such an employer can spread (sunk) investment costs over more periods while the alternative means of compliance, lowering the hiring threshold for the covered group, entails an ongoing expense. This prediction further implies that employers with longer ex-ante regulation spells will display more persistent changes in hiring behavior following deregulation, consistent with findings discussed in 3.8.

In the next subsection, I introduce the data I use to study screening methods, the Multi-City Study of Urban Inequality (MCSUI).

### 5.1 Data

The Multi-City Study of Urban Inequality (MCSUI) is a cross-sectional survey of households and employers administered between June 1992 and May 1994 in four metropolitan areas: Atlanta, Boston, Detroit, and Los Angeles. I use the employer survey here, which includes over 3200 employers. The survey was conducted over the phone with individuals responsible for hiring at each establishment. The content focused on: establishment characteristics, including size and whether the establishment practices AA; employee characteristics, including the racial composition of employees and the last employee hired; and screening methods used, both generally and those used to select the most recent hire. The screening methods asked about include: whether the employer employs someone who specializes in hiring; whether the employer uses job tests, including skill tests, drug tests, and physicals; whether the employer verifies education credentials; and whether the employer checks criminal records; whether the employer uses intermediaries such as state, community, or private agencies, schools or unions to find job candidates; whether the employer uses employee referrals to find job candidates.

### 5.2 AA and Screening Capital

As a *prima facie* test of the model, I first assess whether AA employers indeed invest more in screening capital than non-AA employers. I do this by testing whether AA employers use more screening methods. This exercise is in large part a replication of Holzer and Neumark (2000b).

To test this, I estimate models of the form

\[
\text{method}_{i} = \alpha + \sum \lambda m(i) + \beta AA_{i} + \gamma \log(\text{size})_{i} + X_{i}\delta + \epsilon_{i}
\]  

(8)
where $i$ indexes employers, $\lambda_{m(i)}$ are MSA by central city fixed effects, and $AA_i$ is an indicator for whether the employer reports accounting for affirmative action in recruiting or hiring. I also include controls for other employer characteristic (namely, industry fixed effects), $X_i$, and the employer’s number of employees, size. I also report the coefficient on size because an existing literature establishes a robust link between employer size and screening intensity, particularly the use of formal screening methods (Marsden 1994; Barron et al. 1987). I will further discuss and exploit this relationship below.

I present results for various screening methods in Table 6. AA employers are more likely to adopt a number of screening methods, and the differences are substantial. Conditional on covariates, AA employers are 7.1 percentage points more likely to use a personnel specialist, 4.8 percentage points more likely to use skill tests, 13.5 percentage points more likely to check education credentials, 7.8 percentage points more likely to check criminal backgrounds, and 11.9 percentage points more likely to use intermediaries. These differences are all significant at the 0.01 level.

In the final two columns I present estimates of (8) where the outcome is an indicator for whether the most recent hire was a referral. In column (6) I restrict estimation to establishments where the most recent hire was white, and in column (7) I restrict estimation to all other establishments. When the last hire is white, that hire is 9.1 percentage points less likely to be referral at AA employers. When the last hire is non-white, that difference is less than 1 percentage point and statistically insignificant. Pooling all establishments, the last hire is 6.0 percentage points less likely to be a referral at AA employers. These estimates suggest that the use of these additional screening methods alters how hires are selected and therefore likely affects the composition of hires, even within white hires. The estimates also consistent with a particular interpretation of the screening model, where referrals are the endowed screening method; as employers invest in screening capital, they become less reliant on referrals.

5.3 Employer Size and Black Share

One additional point to note from Table 6 is that the coefficient on log size is the same sign as the coefficient on AA and statistically significant for each dependent screening variable. This is consistent with existing evidence that larger employers screen more intensely, use more screening methods, and rely less on referrals (Barron et al. 1987; Holzer 1987; Marsden 1994). In this next section, I exploit employer size as an additional source of variation in screening methods to test a prediction of the model.

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38 Holzer and Neumark (2000b), the former author a principal investigator of the MCSUI survey, argue that this indicator primarily reflects variation in federal contractor status. The indicator may also include some employers with voluntary affirmative action plans.

39 In contrast to the other results in this section, Holzer and Neumark (2000b) do not conduct an analysis similar to this.

40 This result does not imply that employers do not use referrals to comply with AA regulation. In particular, employers may change the composition of referrals they hire. Referrals may also play a key reinforcing role in compliance and persistence, even if they are not the employer’s initial focus. Any initial push to employ more minority workers may diversify the pool of potential referral hires, allowing the employer to better screen minority candidates in future hiring.
If regulated employers increase their black share of employees in part by investing in screening capital, then other exogenous sources of variation in screening capital across employers should also predict variation in black share in the same manner. I show above that, consistent with previous research, larger employers utilize more screening methods. As another test of the model, I examine whether employer size predicts an employer’s black share.

There are three types of explanations for the observed relationship between employer size and screening intensity. First, many screening methods may be cheaper for larger employers per-hire basis due to economics of scale. For example, employing a personnel specialist or developing a skill test may involve substantial fixed costs. Second, larger employers may face a higher return to worker quality and so put more emphasis on screening (Barron et al. 1987). Third, larger employers may face additional pressure from the EEOC; indeed, Title VII of the Civil Rights Act of 1964, which outlaws workplace discrimination, only applies to firms with at least 15 workers. Larger employers may adopt formal screening procedures to be in compliance, either by actually employing a more diverse workforce or by adopting personnel practices that are less susceptible to discrimination claims.

The first two explanations suggest that variation in screening capital deriving from employer size may be taken as exogenous. However, the third explanation is problematic because it suggests a confounding relationship between employer size and black share. In particular, larger firms may face pressure to both employ more minority workers and to use formal screening methods, even if the latter does not lead to the former. I will come back to this issue later and present additional evidence that suggests that EEO pressure is unlikely to be the primary driver of the relationship between employer size and black share that I observe.

To estimate the relationship between employer size and black share, I first use the EEO-1 data. I estimate models of the form

$$(\text{black share})_{it} = \alpha_i + \lambda_{d(i),t} + \beta_{est} \log(\text{establishment size})_{it} + \beta_{firm} \log(\text{firm size})_{it} + \epsilon_{it}. \quad (9)$$

I include firm size as an explanatory variable in some specifications because whatever causes larger establishments to do more worker screening may produce a similar relationship between firm size and screening. Hence, black share may also be increasing in firm size. I measure firm size as the total number of employees at establishments reported in the EEO-1 data under the same firm. Note that black share is still measured at the establishment level.

Results are presented in panel A of Table 7. All models include Census division by year fixed effects. Columns (2) and (3) include establishment fixed effects. Columns (5) and (6) use only within-job variation, where jobs are defined by establishment by occupation cells. In all models, establishment and firm size are significant predictors of establishment black share. Surprisingly, including establishment fixed effects increases the coefficient on log establishment size. Isolating

\[\text{Indeed, this may be why large employers are large in the first place. Alternatively, larger employers may face higher monitoring costs (Barron et al. 1987). This may also increase the return to worker quality, defined appropriately (e.g. work ethic).}\]
within-job variation reduces the coefficient on log establishment size by more than half, but the coefficient remains sizable. Larger employers do employ more workers in occupations that black workers tend to work in, but this alone cannot explain the relationship. The coefficients from column (5) imply that a 10% increase in establishment size predicts a 0.07% percentage point increase in the black share of employees within jobs, while a 10% increase in firm size predicts a 0.02% percentage point increase in the black share within jobs. Hence, the relationship is primarily establishment level.

Next, I explore the robustness of this result. I test several alternative explanations for the positive relationship between employer size and black share found here. First, I test whether this relationship is an artifact of the business cycle. For example, if establishments tend to grow during expansions, and black job seekers make up a larger fraction of the applicant pool during expansions, then employers will tend to increase their black share as they grow. Second, I test whether this relationship is due to AA. I find above that AA causes the black share of employees to increase. If this is primarily driven by regulated employers increasing their black share of new hires, for example, then the black share may increase more for growing establishments. Third, I test whether this relationship is generated by changes in the composition of occupations. For example, larger establishments may tend to require more workers in low skill occupations, while black workers make up a disproportionately large share of workers in those occupations.

I test these alternative hypotheses, focusing on within-job changes in black share. I estimate models of the form

$$
\Delta (\text{black share})_{i,t} = \alpha + \lambda d(i),t + \beta \Delta \log(\text{establishment size})_{i,t} + \epsilon_{i,t}
$$

where \(\Delta x_{it} = x_{i,t} - x_{i,t-1}\). I estimate this model using all the data, separately for recession and expansion years, and separately for contractors or one-time contractors and establishments with no contractor experience. Note that I measure size changes at the establishment level, not the job cell level.

The results are presented in panel B of Table 7. Using the full data, the first difference model produces a \(\beta\) coefficient of 0.781—that is, a 10% increase in establishment size predicts about a 0.08 percentage point increase in the black share of employees within jobs. Estimates are comparable during both economic recessions and expansions, contradicting the business cycle hypothesis. Finally, the relationship between employer size and black share is even larger for establishments with no contractor experience. This is consistent with AA regulation inducing employers to make screening investments they would otherwise make as larger establishments.

Though a single \(\beta\) coefficient can summarize the relationship between black share and size, this relationship may vary with size. To explore this relationship in more depth, I estimate models of the form

$$
(\text{black share})_{i,t} = \alpha_i + \lambda d(i),t + \sum_{s=1}^{S} \beta_s I_s^{i} + \epsilon_{i,t}
$$

where \(I^s\) are indicators for the following establishment size bins: 5-50, 51-100, 101-150, ..., 451-500,
and 501+. Critically, the EEO-1 data allow me to measure the relationship between employer size and black share using within-establishment variation in size. This is in contrast to a small existing literature documenting a positive cross-sectional relationship between establishment size and black share (Holzer 1998; Carrington et al. 2000). In this literature, it is unclear whether this cross-sectional relationship is driven by fixed differences between employers. For example, one explanation posited in Holzer (1998) is that larger firms tend to be located in more urban locations, where minorities tend to work. Carrington et al. (2000) use firm size to identify the causal effect of Title VII. By including establishment fixed effects, I can remove this type of variation. Moreover, all establishments in these data are covered by workplace discrimination laws. Hence EEO law coverage alone cannot explain any relationship found here.

I plot the $\beta_s$ coefficients in Figure 11. I plot two sets of coefficients, one set estimated using only establishments that have previously held a contract, and the other using only establishments that have not. I also include 95% confidence intervals for both sets of coefficients. For both sets, there is a clear positive relationship between establishment size and minority size. Relative to establishments with 5-50 employees, growing to 200-250 employees is associated with a black share increase of nearly 1.5 percentage points. Growing further to 500 or more employees is associated with an additional increase of 0.5 percentage points.

5.3.1 Black-Run Businesses

The model also predicts that screening investments will have different effects depending on the initial composition of the employer. For example, if an employer’s hiring manager is endowed with a higher ability to screen black job candidates than white job candidates, this would reverse the predictions of the model: an investment in screening capital would reduce the gap in screening precision between groups, increasing the rate at which white candidates are hired. In general, screening investments should reduce between-group differences in hiring rates. By contrast, EEO law is historically unlikely to pressure private employers to hire white workers. Hence, if the relationship between employer size and black share were entirely driven by EEO pressure, the relationship should not change sign depending on the employer’s endowed screening technology.

To test this prediction, I assess whether the size-minority share relationship is of opposite sign for minority-run businesses. The motivating intuition is that minority-run businesses should be better endowed to screen minority candidates than white-run businesses. However, as these employers grow they invest in screening capital and their endowed screening ability becomes less relevant. In particular, using the MCSUI data I estimate models of the form

$$(\text{black share})_i = \sum_{j \in \{B,W\}} \alpha^j + \lambda^j_{m(i)} + \beta^j \times \log\text{size}\cdot X_i \gamma^j + \epsilon_i$$

where $j$ indexes whether the establishment’s hiring manager is white or black.

The results are presented in Table 8. Column 1 does not include industry controls, column 2 includes 1-digit industry fixed effects, and column 3 includes 2-digit industry fixed effects. The
results are similar across columns. As predicted, while establishment black share is increasing in establishment size for white-run businesses, the opposite is true for black-run businesses. For white-run and black-run establishments, a 10 log point increase in size is associated with a 0.25-0.26 percentage point increase and a 0.40-0.54 percentage point decrease in black share.

5.3.2 Black Share and Discrimination Charges

If larger employers face more pressure to comply with workplace discrimination law (or if they are simply more aware of compliance issues), they may hire more minority workers to satisfy law enforcement officials and reduce costs associated with discrimination claims and litigation. In this case, workplace discrimination law may be responsible for the positive relationship between employer size and black share. However, it is not clear that hiring more minority workers will reduce discrimination claims and associated costs, particularly given that claims are typically filed by individuals. Only a small fraction of discrimination claims relate to hiring, especially after the mid-1970’s (Donahue and Siegelman 1991). Hence, nearly all claims are filed by individuals who were employees at some point.

I estimate the relationship between an establishment’s black share and the expected number of discrimination charges this establishment faces in a given year. To do this, I use data on the universe of discrimination claims filed with the Equal Employment Opportunity Commission from 1990 to 2004. These data include information for each charge filed, including the protected class of the complainant, the nature of the allegation, how that claim is resolved, and identifying information for the accused employer. I restrict analysis to claims alleging racial discrimination against black workers filed under Title VII of the Civil Rights Act of 1964. I match these claims to the EEO-1 data using the employer name and address. I am able to match 140,124 claims. I present summary statistics in Table A.2 in the Appendix.

I estimate Poisson models of the form

\[
\log(E[\text{charges}_{it}]) = \alpha_i + f(\log(\text{size}_{it}))\theta + g(\text{black share}_{it})\beta + \epsilon_{it} 
\]

where \( f(\cdot) \) is a 10-piece linear spline, and \( g \) is a 5th order fractional polynomial. I estimate models that do and do not include establishment fixed effects, \( \alpha_i \). In addition to discrimination charges, I also estimate models where the dependent variable is the number of claims that result in what the EEOC terms ‘merit resolutions’–charges with outcomes favorable to the claimant or where the EEOC finds that allegations have merit. These are charges that result in a withdrawal with benefits, settlement with benefits, successful conciliation, or unsuccessful conciliation. Of the claims I am able to match to the EEO-1 data, 14% are merit claims.

The estimated polynomials in black share are plotted in Figures 12. The black dashed lines mark the 10th and 90th percentiles of establishment black share in the data. Conditional on size, the expected number of racial discrimination charges against an establishment is increasing in black share at all but the highest black share levels. Hence, for nearly all establishments, hiring more
black workers does not appear to reduce dealings with discrimination law enforcement. Moreover, the expected number of claims that result in merit resolutions is also increasing in black share at all but the highest levels.

6 Conclusion

I estimate the dynamic effects of federal affirmative action regulation on the racial composition of regulated establishments, exploiting variation in the timing of federal contractor status across workplaces. I find that affirmative action sharply increases an establishment’s black share of employees, with the share continuing to increase over time. Five years after an establishment is first regulated, its black share of employees increased by an average of 0.8 percentage points. This response is strikingly asymmetric: even after establishments are deregulated, their black share continues to grow, in some cases for more than a decade. By contrast, establishment size moves roughly symmetrically with changes in contractor status. Moreover, following deregulation, an establishment’s likelihood of acquiring a new contract—and hence, becoming regulated again—quickly reverts to near the baseline rate.

I argue that this persistence is in part driven by affirmative action inducing employers to improve screening. In existing models (e.g., Coate and Loury 1993), an employer can only comply with affirmative action by reducing its hiring standard for the protected group. I introduce a novel response margin whereby the employer can make investments to increase the precision with which it screens potential employees. I show that if employers are initially less able to infer the quality of minority candidates—an assumption often made in the statistical discrimination literature—screening investments will reduce between-group disparities in hiring rates. Moreover, affirmative action will increase the return to such investments. If these investments are at least partially irreversible, temporary affirmative action regulation can generate persistent changes in screening capital, and hence produce a durable increase in the minority share of hires.

I then present evidence supporting predictions of the model. Using cross-sectional survey data, I show that regulated employers use more screening methods than otherwise similar unregulated employers, particularly formal methods like personnel specialists, job tests, credential checks, and intermediaries. Exploiting variation in screening capital deriving from employer size, I also show that screening capital predicts reductions in between-group employment disparities. In particular, I show that black share is increasing in employer size, and that this relationship holds within-establishment and within-job for a large sample of establishments that are all subject to workplace discrimination law. In this sense, affirmative action pushes employers to hire as though they were expanding. I provide several pieces of evidence that the relationship between employer size and black share is not the result of legal or public pressure to employ a diverse workforce.

Given that employers continue to increase the black share of their workforce even when they are no longer regulated, a revealed preference argument would imply that it is profitable for them to do so. This suggests that affirmative action leads firms to take actions that increase the profitability of
employing black workers. I provide evidence that these actions include screening investments. These investments might take several forms, and determining what types of capital are most significant for both compliance and persistence requires further study. Alternatively, affirmative action may prompt employers to change their personnel practices in a way that is prohibitively costly to reverse. To distinguish between these classes of explanations, one could measure the dynamic effects of affirmative action on productivity and profitability, ideally using worker level data. Relatedly, these two classes of explanations have different implications for the productive efficiency consequences of affirmative action.

Note that, even in the absence of adjustment costs, the results presented here do not necessarily imply the existence of any market inefficiency in personnel practices. Screening capital may be costly, and employers may not find additional capital to be a worthwhile investment in the absence of regulation. If regulation prompts an employer to invest, in the presence of sunk costs, the employer may find the screening capital profitable to use even following deregulation. Nevertheless, the results do suggest that the distortionary costs of affirmative action may be smaller than previously assumed. In particular, screening capital may involve upfront costs, but little ongoing distortion in who is employed. Given that the persistence following deregulation appears to be incomplete, compliance may involve a combination of screening investment and a lowered quality threshold for black hires. A lowered threshold is an ongoing distortion, hence only the capital response would be persistent absent adjustment costs.

Another remaining question is how the compliance activities of one employer affect the aggregate labor market. Consider screening investments as one response. If more precise screening improves the matching of workers to jobs, then screening investments may improve productive efficiency. But if improved screening primarily redistributes workers among competing employers without increasing match quality, improvements in productive efficiency will be negligible (Stiglitz 1975). Moreover, screening improvements can potentially amplify adverse selection in the labor market (Masters 2009). Relatedly, screening improvements at one employer may increase compliance costs for other employers. Other compliance activities may exhibit similar crowd out or reallocation across employers (Donahue and Heckman 1991). Newly available employee-employer matched data provide a unique opportunity to identify the extent of this reallocation, a potentially useful input for determining the aggregate impact of affirmative action regulation.

In contrast to persistence produced through endogenous human capital investment by workers as in Coate and Loury (1993), the persistent effect of temporary affirmative action found here appears to be employer level rather than market level. A potentially important implication is that employer churn may lead the effect of temporary affirmative action to fade out in the aggregate. If the persistence found here is in part driven by the human capital accumulation of employers—say, from hiring managers learning how to screen minority workers—then this fadeout may be partially muted by the transfer of knowledge across employers.

The results presented here also have implications outside of affirmative action policy. More generally, they suggest that minority workers face job search frictions that can be at least partially
surmounted by temporary intervention. Understanding the sources of these frictions and specific mechanisms that can mitigate these impediments remains an important area for future research.

References


Figure 1: Variation in Regulation and Deregulation Events

Notes: This figure is a histogram of the establishment-level regulation and deregulation events as described in section 3.2. Regulation events, depicted in green, are the first year an establishment is identified as a federal contractor. I exclude establishments that are contractors in the first year they are present in the data. Deregulation events, depicted in red, are the first year an establishment that was previously a contractor is: (1) not a contractor; and (2) never subsequently observed as a contractor in the data.
Figure 2: Regulation and Deregulation Event Studies

(A) Regulation Event

(B) Deregulation Event

Notes: These figures plot event study coefficients and 95% confidence intervals (dotted) estimated using model (1) and the overlapping sample, where the outcome variable is the percent black of an establishment’s employees. Panel A depicts the regulation event study; Panel B depicts the deregulation event study. The definitions of regulation and deregulation events are described in section 3.2. The coefficient for the year prior to the event ($\theta_{-1}$) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.
Figure 3: Regulation Event Study, by Duration

Notes: This figure plots event study coefficients and 95% confidence intervals (dotted) estimated using model 1 and the overlapping sample, where the outcome variable is the percent black of an establishment’s employees. Each line depicts the event study estimates for a distinct group of eventual contractors, grouped by the number of years between their regulation and deregulation events. The definitions of regulation and deregulation events are described in section 3.2. The coefficient for the year prior to the event ($\theta_{-1}$) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.
Figure 4: Regulation and Deregulation Event Studies, Within-Occupation

(A) Regulation Event

<table>
<thead>
<tr>
<th>Years Relative to Event</th>
<th>Establishment % Black</th>
</tr>
</thead>
<tbody>
<tr>
<td>-6</td>
<td>0</td>
</tr>
<tr>
<td>-5</td>
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<tr>
<td>-4</td>
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<td>-3</td>
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<tr>
<td>-2</td>
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<tr>
<td>-1</td>
<td>0</td>
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<tr>
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</tr>
<tr>
<td>5</td>
<td>0</td>
</tr>
<tr>
<td>6</td>
<td>0</td>
</tr>
</tbody>
</table>

Overall: Slope = -0.006 (0.029)  
Within-Occupation: Slope = -0.013 (0.032)

Post-Event ΔSlope = 0.182 (0.040)

Notes: These figures plot the event study coefficients and 95% confidence intervals (dotted) estimated using model (1) and the overlapping sample, where the outcome variable is the percent black of employees in an establishment by occupation cell. Panel A depicts the regulation event study; Panel B depicts the deregulation event study. The definitions of regulation and deregulation events are described in section 3.2. The coefficient for the year prior to the event (θ−1) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level. Observations are weighted by the establishment by occupation cell’s share of total establishment employment.
Figure 5: Establishment Size and Regulation and Deregulation Events

Notes: These figures plot the event study coefficients and 95% confidence intervals (dotted) estimated using model 1 and the overlapping sample, where the outcome variable is log establishment size. Panel A depicts the regulation event study; Panel B depicts the deregulation event study. The definitions of regulation and deregulation events are described in section 3.2. The ‘> 6 Years’ line restricts eventual contractors to those with more than 6 years between their regulation and deregulation events. The coefficient for the year prior to the event ($\theta_{-1}$) is normalized to zero. Estimated models include Census division by year fixed effects. Standard errors are clustered at the firm level.
Figure 6: Deregulation Event Study, by Subsequent Growth

(A) Shrinking Establishments

(B) Growing Establishments

Notes: These figures plot the deregulation event study coefficients and 95% confidence intervals for various outcome variables. The definition of deregulation events is described in section 3.2. Panels A and B plot estimates of model (5) using only establishments in the overlapping sample that shrink and grow following the deregulation event. See section 3.4 for details. Pseudo event years are assigned to non-contractors as described in section 3.4. The outcome variable for these two panels is the percent black of employees. In all models the coefficient for the year prior to the event ($\theta_{-1}$) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.
Notes: This figure plots the probability that an establishment acquires a future contract in some time period, conditional on the number of years since the establishment last held a contract or first appeared in the data. The vertical axis denotes the fraction of firms that acquire a federal contract in a given time period, either in the next year, as in Panel A, or in the next three years, as in Panel B. Panel A includes data from 1979 to 2003. Panel B includes data from 1979 to 2001. The purple line depicts this likelihood for firms that have previously held a federal contract, but have not have held a contract for a given number of years, as marked on the horizontal axis. For the 0 value on the horizontal axis, the purple line denotes the fraction of current contractors that will be contractors in the given time period. The blue line depicts this likelihood for establishments that have never held a federal contract, and for these establishments the horizontal axis marks the number of years they’ve been observed in the data.
Figure 8: Regulation and Deregulation Event Studies, by Skill Level

(A) Regulation: High Skill

(B) Deregulation: High Skill

(C) Regulation: Middle Skill

(D) Deregulation: Middle Skill

(E) Regulation: Low Skill

(F) Deregulation: Low Skill

Notes: These figures plot event study coefficients and 95% confidence intervals (dotted) estimated using model 4 and the overlapping sample, where the outcome variable is the percent black of employees in an establishment by occupation cell. The event studies are estimated separately for high skill (managers, professionals, technicians), medium skill (sales workers, administrative support, craft workers, operatives), and low skill (laborers and service workers) occupations. The left column of panels depicts regulation event studies; right column depicts deregulation event studies. The definitions of regulation and deregulation events are described in section 3.2. The coefficient for the year prior to the event ($\theta_{-1}$) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level. Observations are weighted by the establishment by occupation cell’s share of total establishment employment in the corresponding skill group.
Notes: This figure plots event study coefficients and 95% confidence intervals (dotted) estimated using model 1 and the deregulation sample, where the outcome variable is the percent black of an establishment’s employees. In Panel A, one-time contractors are assigned pseudo deregulation event years, as described in section 3.8. In Panel B, one-time contractors are grouped by the number of years between their regulation and deregulation events. The definitions of regulation and deregulation events are described in section 3.2. The coefficient for the year prior to the event ($\theta_{-1}$) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.
Figure 10: Long Run Regulation Event Study

Notes: This figure plots regulation event study coefficients and 95% confidence intervals (dotted) estimated using model (1) and the regulation sample, where the outcome variable is the percent black of an establishment’s employees and endpoint $b$ is extended to 20. The definition of the regulation event is described in section 3.2. The coefficient for the year prior to the event ($\theta_{-1}$) is normalized to zero. Estimated models include Census division by year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.
Figure 11: Establishment Size and Black Share

Notes: This figure plots coefficients and 95% confidence intervals from regression where the outcome variable is the percent black of an establishment’s employees and the explanatory variables of interest are a set of indicators for establishment size bins. All models include Census division by year fixed effects. The purple bars depict coefficient estimates from a model that includes only establishments that have previously held a federal contract. The blue bars depict estimates from a model that includes only establishments that have not previously held a contract. Standard errors are clustered at the establishment level.
Figure 12: Black Share and Discrimination Charges

(A) Discrimination Charges

(B) Merit Resolutions

Notes: These figures plot the expected number of racial discrimination charges received by an establishment as a function of its black share of employees, conditional on establishment size. This relationship is estimated using Poisson regression models. The regressions include a 10-piece linear spline in log establishment size and a 5th order fractional polynomial in establishment black share. In both panels, the red line depicts a model that includes establishment fixed effects. The black dashed lines mark the 10th and 90th percentile establishment black shares in the data. In Panel A the dependent variable is the number of charges that include an allegation of discrimination against black workers. In Panel B the dependent variable is the number of merit resolutions that include an allegation of discrimination against black workers. Merit resolutions are discrimination charges that result in a withdrawal with benefits, settlement with benefits, successful conciliation, or unsuccessful conciliation.
Table 1: Event Study Sample, Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Non-Contractors Sample</th>
<th>Regulation Sample</th>
<th>Deregulation Sample</th>
<th>Overlapping Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Establishments</td>
<td>569,061</td>
<td>161,703</td>
<td>63,595</td>
<td>85,745</td>
<td>36,030</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>87,544</td>
<td>36,623</td>
<td>12,141</td>
<td>12,888</td>
<td>6,212</td>
</tr>
<tr>
<td>Establishment Size*</td>
<td>227</td>
<td>184</td>
<td>174</td>
<td>173</td>
<td>170</td>
</tr>
<tr>
<td></td>
<td>(580)</td>
<td>(392)</td>
<td>(337)</td>
<td>(359)</td>
<td>(307)</td>
</tr>
<tr>
<td>Industry (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Agricultural Services</td>
<td>0.3</td>
<td>0.4</td>
<td>0.2</td>
<td>0.2</td>
<td>0.2</td>
</tr>
<tr>
<td>Mining</td>
<td>0.9</td>
<td>0.4</td>
<td>0.4</td>
<td>0.3</td>
<td>0.2</td>
</tr>
<tr>
<td>Construction</td>
<td>2.2</td>
<td>1.6</td>
<td>1.4</td>
<td>1.5</td>
<td>0.9</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>19.0</td>
<td>13.6</td>
<td>14.5</td>
<td>13.5</td>
<td>12.0</td>
</tr>
<tr>
<td>Transportation, Comm., Util.</td>
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<td>6.1</td>
<td>6.1</td>
<td>4.4</td>
<td>4.3</td>
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<td>Wholesale Trade</td>
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<td>4.3</td>
<td>3.6</td>
<td>3.4</td>
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<td>Retail Trade</td>
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<td>40.8</td>
<td>49.4</td>
<td>51.0</td>
</tr>
<tr>
<td>Finance, Insurance, Real Estate</td>
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<td>8.2</td>
<td>8.0</td>
<td>6.5</td>
<td>7.5</td>
</tr>
<tr>
<td>Services</td>
<td>23.4</td>
<td>26.1</td>
<td>24.3</td>
<td>20.6</td>
<td>20.6</td>
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<tr>
<td>Black Share Quantile*</td>
<td>50.0</td>
<td>48.3</td>
<td>48.4</td>
<td>50.3</td>
<td>47.8</td>
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<tr>
<td>Black Share of Employees* (%)</td>
<td>14.1</td>
<td>14.3</td>
<td>14.0</td>
<td>15.4</td>
<td>13.5</td>
</tr>
<tr>
<td>Black Share of Population, 15-64* (%)</td>
<td>15.5</td>
<td>15.5</td>
<td>15.2</td>
<td>15.6</td>
<td>15.0</td>
</tr>
</tbody>
</table>

Notes: Standard deviation in parentheses.
* Quantiles defined at level of MSA by year cell. For regulation and overlapping sample, this is quantity at last year observed before regulation event. For deregulation sample, this is quantity at last year observed before deregulation event.
Table 2: Regulation Event Study Estimates

<table>
<thead>
<tr>
<th></th>
<th>Full Regulation Sample</th>
<th>Overlapping Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
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<tr>
<td>$\theta_{-6}$</td>
<td>-0.011</td>
<td>-0.126</td>
</tr>
<tr>
<td></td>
<td>(0.115)</td>
<td>(0.108)</td>
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<tr>
<td>$\theta_{-5}$</td>
<td>-0.010</td>
<td>-0.108</td>
</tr>
<tr>
<td></td>
<td>(0.094)</td>
<td>(0.089)</td>
</tr>
<tr>
<td>$\theta_{-4}$</td>
<td>0.001</td>
<td>-0.051</td>
</tr>
<tr>
<td></td>
<td>(0.080)</td>
<td>(0.079)</td>
</tr>
<tr>
<td>$\theta_{-3}$</td>
<td>-0.085</td>
<td>-0.124</td>
</tr>
<tr>
<td></td>
<td>(0.060)</td>
<td>(0.060)</td>
</tr>
<tr>
<td>$\theta_{-2}$</td>
<td>-0.016</td>
<td>-0.051</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>$\theta_0$</td>
<td>0.095</td>
<td>0.098</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.033)</td>
</tr>
<tr>
<td>$\theta_1$</td>
<td>0.250</td>
<td>0.261</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.053)</td>
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<tr>
<td>$\theta_2$</td>
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<td>0.458</td>
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<tr>
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<td>(0.071)</td>
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<tr>
<td>$\theta_3$</td>
<td>0.623</td>
<td>0.634</td>
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<tr>
<td></td>
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<td>(0.089)</td>
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<td>0.814</td>
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<tr>
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<td>(0.104)</td>
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<td>$\theta_5$</td>
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<td>1.044</td>
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<td>(0.125)</td>
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<td>$\theta_6$</td>
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<td>1.172</td>
</tr>
<tr>
<td></td>
<td>(0.151)</td>
<td>(0.147)</td>
</tr>
</tbody>
</table>

| Parametric       |           |           |           |           |           |           |           |           |
| $\beta$          | -0.005    | 0.014     | 0.037     | -0.029    | -0.006    | 0.018     | 0.022     | -0.002    |
|                  | (0.023)   | (0.021)   | (0.021)   | (0.029)   | (0.029)   | (0.027)   | (0.027)   | (0.032)   |
| $\Delta \beta$   | 0.166     | 0.160     | 0.131     | 0.146     | 0.182     | 0.167     | 0.148     | 0.132     |
|                  | (0.034)   | (0.033)   | (0.031)   | (0.038)   | (0.040)   | (0.039)   | (0.037)   | (0.039)   |

| Div. × Year FEs  | ✓         | ✓         | ✓         | ✓         |           |           |           |           |
| MSA × Year FEs   | ✓         |           |           |           | ✓         |           |           |           |
| Ind. × Div. × Year FEs | ✓    | ✓         |           |           |           |           |           |           |
| Est. FEs         | ✓         | ✓         | ✓         | ✓         | ✓         | ✓         |           |           |
| Balanced         | ✓         |           |           |           |           |           |           | ✓         |
| # of Treated Est.| 63,595    | 6,066     | 36,030    | 4,525     |          |           |           |           |

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the firm level. The estimated models are regulation event studies, variants of the model (1) in Panel A and a parametric analog in Panel B. The definition of regulation events is described in section 3.2. The estimation sample includes non-contractor establishments in all columns, the regulation sample in columns (1)-(4), and the overlapping sample in columns (5)-(8). Columns (4) and (8) include only non-contractors and balanced panels of eventual contractors in the regulation and overlapping samples. All models include establishment fixed effects and a quadratic in log establishment size. Columns (1), (4), (5), and (8) include Census division by year fixed effects, columns (2) and (6) include MSA by year fixed effects, and columns (3) and (7) include Census division by 1-digit industry by year fixed effects.
### Table 3: Deregulation Event Study Estimates

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Full Deregulation Sample</th>
<th>Overlapping Sample</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>$\theta_{-6}$</td>
<td>-1.312</td>
<td>-1.429</td>
</tr>
<tr>
<td></td>
<td>(0.192)</td>
<td>(0.207)</td>
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<tr>
<td>$\theta_{-5}$</td>
<td>-1.030</td>
<td>-1.120</td>
</tr>
<tr>
<td></td>
<td>(0.155)</td>
<td>(0.166)</td>
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<tr>
<td>$\theta_{-4}$</td>
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<td>-0.805</td>
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<tr>
<td></td>
<td>(0.114)</td>
<td>(0.113)</td>
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<tr>
<td>$\theta_{-3}$</td>
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<td>-0.520</td>
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<tr>
<td></td>
<td>(0.086)</td>
<td>(0.090)</td>
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<tr>
<td>$\theta_{-2}$</td>
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<td>-0.229</td>
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<tr>
<td></td>
<td>(0.058)</td>
<td>(0.061)</td>
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<tr>
<td>$\theta_0$</td>
<td>0.231</td>
<td>0.208</td>
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<tr>
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<td>(0.036)</td>
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<tr>
<td>$\theta_1$</td>
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<td>$\theta_2$</td>
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<td>$\theta_3$</td>
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<td>(0.096)</td>
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<td>$\theta_4$</td>
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<td>0.834</td>
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<tr>
<td></td>
<td>(0.143)</td>
<td>(0.128)</td>
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<td>$\theta_5$</td>
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<td>0.947</td>
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<td>(0.172)</td>
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<tr>
<td>$\theta_6$</td>
<td>1.179</td>
<td>1.241</td>
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<tr>
<td></td>
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<td>(0.221)</td>
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<tr>
<td>$\beta$</td>
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<tr>
<td></td>
<td>(0.038)</td>
<td>(0.040)</td>
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<tr>
<td>$\Delta \beta$</td>
<td>-0.101</td>
<td>-0.119</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.045)</td>
</tr>
</tbody>
</table>

|                  | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          |
| Div. × Year FEs  | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          |
| MSA × Year FEs   | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          |
| Ind. × Div. × Year FEs | ✓    | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          |
| Est. FEs         | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          | ✓          |
| Balanced         | ✓          | ✓          |            |            |            |            |            | ✓          |
| # of Treated Est.| 85,745     | 5,682      | 36,030     | 2,530      |            |            |            |            |

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the firm level. The estimated models are deregulation event studies, variants of the model (1) in Panel A and a parametric analog in Panel B. The definition of deregulation events is described in section 3.2. The estimation sample includes non-contractor establishments in all columns, the deregulation sample in columns (1)-(4), and the overlapping sample in columns (5)-(8). Columns (4) and (8) include only non-contractors and balanced panels of eventual contractors in the deregulation and overlapping samples. All models include establishment fixed effects and a quadratic in log establishment size. Columns (1), (4), (5), and (8) include Census division by year fixed effects, columns (2) and (6) include MSA by year fixed effects, and columns (3) and (7) include Census division by 1-digit industry by year fixed effects.
Table 4: Likelihood of Future Regulation

<table>
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<th>Division by Year FEs</th>
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<th>✓</th>
<th>✓</th>
<th>✓</th>
<th>✓</th>
<th>✓</th>
<th>✓</th>
<th>✓</th>
</tr>
</thead>
<tbody>
<tr>
<td>N Observations</td>
<td>836,625</td>
<td>736,595</td>
<td>646,244</td>
<td>836,625</td>
<td>698,967</td>
<td>616,620</td>
<td>546,132</td>
<td>698,967</td>
</tr>
</tbody>
</table>

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the firm level. Columns (1), (4), (5), and (8) include data from 1979 to 2003. Columns (2) and (7) include data from 1979 to 2001. Columns (3) and (7) include data from 1979 to 1999.
<table>
<thead>
<tr>
<th></th>
<th>All (1)</th>
<th>Firm Size ≥ 150 (2)</th>
<th>Single Establishment (3)</th>
<th>Multi Establishment (4)</th>
<th>Small Establishment (5)</th>
<th>Large Establishment (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Regulation Event</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\beta$</td>
<td>-0.006</td>
<td>-0.008</td>
<td>-0.027</td>
<td>-0.022</td>
<td>0.002</td>
<td>-0.017</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.031)</td>
<td>(0.032)</td>
<td>(0.030)</td>
<td>(0.042)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>$\Delta\beta$</td>
<td>0.182</td>
<td>0.190</td>
<td>0.017</td>
<td>0.202</td>
<td>0.170</td>
<td>0.192</td>
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<tr>
<td></td>
<td>(0.040)</td>
<td>(0.042)</td>
<td>(0.037)</td>
<td>(0.039)</td>
<td>(0.054)</td>
<td>(0.050)</td>
</tr>
<tr>
<td><strong>Panel B: Deregulation Event</strong></td>
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<td></td>
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<tr>
<td>$\beta$</td>
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<td>0.314</td>
<td>-0.030</td>
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<td>(0.046)</td>
<td>(0.041)</td>
<td>(0.047)</td>
<td>(0.058)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>$\Delta\beta$</td>
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<td>-0.141</td>
<td>0.014</td>
<td>-0.135</td>
<td>-0.227</td>
<td>-0.074</td>
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<td></td>
<td>(0.046)</td>
<td>(0.048)</td>
<td>(0.046)</td>
<td>(0.050)</td>
<td>(0.054)</td>
<td>(0.061)</td>
</tr>
<tr>
<td>Div. × Year FEs</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Est. FEs</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td># of Treated Est.</td>
<td>36,030</td>
<td>33,314</td>
<td>4,703</td>
<td>31,327</td>
<td>18,519</td>
<td>17,511</td>
</tr>
</tbody>
</table>

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the firm level. The estimated models are parametric regulation and deregulation event studies in Panel A and Panel B. The definition of regulation and deregulation events is described in section 3.2. The estimation sample in column (1) includes non-contractor establishments and the overlapping sample. Column (2) restricts the analysis to establishments that are part of firms with at least 150 employees. Column (3) restricts the analysis to singleton establishments, and column (4) restricts the analysis to establishments that are part of multi-establishment firms. Column (5) restricts the analysis to establishments with less than 100 employees, and column (6) restricts the analysis to establishments with at least 100 employees. All columns include Census division by year fixed effects. For eventual contractor establishments, these restrictions are based on the latest year an establishment is observed prior to their regulation event. For non-contractor establishments, these restrictions are based on the latest year an establishment is observed prior to their pseudo regulation event, where pseudo event events are assigned based on the year I first observe the establishment in the data and the number of years between the first and last year. More details on how pseudo events are assigned are described in section 3.4.
Table 6: Affirmative Action and Screening Methods

<table>
<thead>
<tr>
<th>Personnel Specialist†</th>
<th>Skill Test</th>
<th>Check Education</th>
<th>Check Crim. Record</th>
<th>Use Intermediary</th>
<th>Last Hire Referral</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Affirmative Action</td>
<td>0.071**</td>
<td>0.048**</td>
<td>0.135**</td>
<td>0.078**</td>
<td>0.119**</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.017)</td>
<td>(0.020)</td>
<td>(0.019)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>log(Size)</td>
<td>0.121**</td>
<td>0.011*</td>
<td>0.035**</td>
<td>0.038**</td>
<td>0.043**</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>1-Digit Industry FEs</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>MSA by CC FEs</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>N Firms</td>
<td>2766</td>
<td>2757</td>
<td>2546</td>
<td>2514</td>
<td>2766</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.300</td>
<td>0.038</td>
<td>0.113</td>
<td>0.142</td>
<td>0.080</td>
</tr>
<tr>
<td>Mean of Dep. Var.</td>
<td>0.325</td>
<td>0.239</td>
<td>0.377</td>
<td>0.323</td>
<td>0.634</td>
</tr>
</tbody>
</table>

Notes: Each column reports the coefficient estimates for a regression of the form (6), with robust standard errors in parentheses. Data are from the Multi-City Study of Urban Inequality (MCSUI). Affirmative Action status is defined by whether the establishment reports that “Affirmative Action or Equal Opportunity Law plays any role in recruiting or hiring. For each column, the outcome variable is a particular screening method. ‘Personnel Specialist’ is an indicator for whether the establishments most recent search was carried out by a personnel official. The outcome variables for columns (2)-(5) are indicators for whether the indicated method is typically used in screening. ‘Last Hire Referral’ is an indicator for whether the last employee hired was an employee referral. Column (6) restricts estimation to establishments where the last hire was white, while column (7) restricts estimation to all other establishments. All models include 1-digit industry fixed effects and MSA by central city status fixed effects. Establishments are weighted using survey sample weights.

* Denotes statistical significance at $p < 0.05$ level. ** Denotes statistical significance at $p < 0.01$ level.
Table 7: Employer Size and Black Share

<table>
<thead>
<tr>
<th>Outcome: Percentage Black</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>Within-Job</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>log Establishment Size</td>
<td>1.434</td>
<td>1.659</td>
<td>1.617</td>
<td>0.666</td>
<td>0.568</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.035)</td>
<td>(0.037)</td>
<td>(0.025)</td>
<td>(0.027)</td>
<td></td>
</tr>
<tr>
<td>log Firm Size</td>
<td>0.104</td>
<td>0.227</td>
<td>0.104</td>
<td>0.227</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.025)</td>
<td>(0.028)</td>
<td>(0.025)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Establishment FEs</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>MSA by Year FEs</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.230</td>
<td>0.927</td>
<td>0.927</td>
<td>0.872</td>
<td>0.872</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Outcome: Δ Percentage Black</th>
<th>All</th>
<th>Recession</th>
<th>Expansion</th>
<th>Non-Contractors</th>
<th>Contractors</th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcome: Δ Percentage Black</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Δ log Establishment Size</td>
<td>0.781</td>
<td>0.746</td>
<td>0.801</td>
<td>0.873</td>
<td>0.757</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.037)</td>
<td>(0.030)</td>
<td>(0.053)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>MSA by Year FEs</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.002</td>
<td>0.001</td>
<td>0.002</td>
<td>0.002</td>
<td>0.002</td>
</tr>
</tbody>
</table>

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the establishment level. In Panel A the outcome variable is the percent black of an establishments employees (or, as in columns (5) and (6), the percent black of an establishment by occupation cell). Firm size is the total number of employees at establishments reported in the EEO-1 data under the same firm. Columns (2) and (3) include establishment fixed effects. Columns (4) and (5) isolate within-job variation in black share. In Panel B the outcome variable is the change in percent black of employees in a establishment by occupation cell over the previous year. All columns include MSA by year fixed effects. Column (2) includes only data from the years 1980-1982, 1990-1992, and 2001-2003. Column (3) includes only the remaining years. Column (4) includes only observations for establishments that have not previously held a federal contract. Column (5) includes only observations for establishments that have previously held a federal contract.
### Table 8: Manager Race, Size, and Black Share

<table>
<thead>
<tr>
<th></th>
<th>Percent Black</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>log Size × White-Run</td>
<td>2.50**</td>
</tr>
<tr>
<td></td>
<td>(0.485)</td>
</tr>
<tr>
<td>log Size × Black-Run</td>
<td>-5.39**</td>
</tr>
<tr>
<td></td>
<td>(1.371)</td>
</tr>
</tbody>
</table>

1-Digit Industry by Race FEs ✓
2-Digit Industry by Race FEs ✓ ✓ ✓
MSA by CC by Race FEs ✓ ✓ ✓

<p>| | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>N White-Run Establishments</td>
<td>2166</td>
<td>2124</td>
<td>2124</td>
</tr>
<tr>
<td>N Black-Run Establishments</td>
<td>198</td>
<td>190</td>
<td>190</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.195</td>
<td>0.199</td>
<td>0.253</td>
</tr>
</tbody>
</table>

Notes: Each column reports the coefficient estimates for a regression, with robust standard errors in parentheses. Data are from the Multi-City Study of Urban Inequality (MCSUI). ‘White-Run’ and ‘Black-Run’ are indicators for whether the employee that oversaw the most recent search identifies as white or black. All models include MSA by central city status by race fixed effects. Column (2) includes 1-digit industry by race fixed effects. Column (3) includes 2-digit industry by race fixed effects.

* Denotes statistical significance at $p < 0.05$ level. ** Denotes statistical significance at $p < 0.01$ level.
A Appendix: Persistence Can Introduce Bias

The persistent effect of temporary regulation I document here has important implications for interpreting existing research in this literature, including Kurtulus (2011, 2012), Leonard (1984, 1990), Rodgers and Spriggs (1996), Ashenfelter and Heckman (1976), Goldstein and Smith (1976), Smith and Welch (1984), and Heckman and Wolpin (1976). In particular, if regulation has an impact on employers that persists even when they are no longer contractors, previous estimates may be biased. This is because the research designs applied in existing work are based on simple comparison of contractors to non-contractors, either within or across employers. In the presence of persistence these comparisons may substantially understate the causal impact of regulation because some employers that are currently non-contractors were previously contractors, and the minority share of those employers is still affected by the regulation. In this section, I explore the extent of this bias empirically, using the baseline model of Kurtulus (2011) as a motivating example.

The core specification estimated in Kurtulus (2011) is of the form

$$Y_{it} = \alpha_i + \tau_{t(i),t} + \beta I_{it}^{current} + X_{it} \gamma + \epsilon_{it}$$  \hspace{1cm} (A.1)

where $I^{current}$ is an indicator for whether an establishment is currently a contractor. This specification models the effect of regulation as a level effect that depends only on the current period contractor status. An assumption implicit in this model is that whatever effect regulation has dissipates completely when an employer is no longer a contractor.

For the sake of comparison, I also estimate a modified version of (A.1),

$$Y_{it} = \alpha_i + \tau_{t(i),t} + \tilde{\beta} I_{it}^{previous} + X_{it} \gamma + \epsilon_{it}$$  \hspace{1cm} (A.2)

where $I^{previous}$ is an indicator for whether an establishment has ever previously been a contractor. This specification models the effect of regulation as a level effect that depends only on whether the establishment was ever previously a contractor. While this specification does not allow the effect of regulation to accumulate over time, a pattern I document in the main analysis, it does allow for a particular form of persistence. If the effect of regulation takes the form assumed in (A.1), this model will underestimate the effect of regulation.

As in the main analysis, I exclude establishments that enter the sample as a federal contractor. In addition, for the establishments that become contractors, I only include years of data that are at most 6 years prior to their regulation event. To demonstrate the influence of persistence on the results, I estimate both (A.1) and (A.2) for a series a estimation samples, moving the data window from the year of the regulation event to 6 years following the regulation event. For each sample, I also restrict the set of eventual contractors included in the estimation to those that are present for the full set of years following the regulation event. This way, the $\tilde{\beta}$ estimates reflect, in principle, the impact of regulation averaged across 0-6 years following the event, and not a more complicated weighted average that depends on the frequency with which establishments are observed at each year following regulation. Each model includes Census division by year fixed effects, and a quadratic
in log establishment size.

The $\beta$ and $\tilde{\beta}$ estimates for each window are displayed in Figure A.3. For the smallest window, which excludes all years following the regulation event, the estimates coincide at 0.141. As the window widens, these estimated coefficients diverge sharply. The $\beta$ coefficient declines to 0.068 when the window expands to three years after the event, and is statistically indistinguishable from zero. Using the full size year window, the $\beta$ coefficient declines further to 0.051. This pattern emerges despite the fact that the effect of initial regulation increases over time, as demonstrated in the main analysis. This discrepancy reflects the fact that many establishments are not contractors in some years following their regulation event, but their black share continues to increase. By contrast, the $\tilde{\beta}$ coefficient increases substantially as the window expands. With a three year post-event window, the coefficient has more than doubled to 0.340. Using the full size year window, the $\tilde{\beta}$ coefficient increases further to 0.388. A simple adjustment allowing for some form of persistence increases the estimated effect of regulation by an order of magnitude.

B Appendix: Proofs of Propositions

In this section I prove the propositions stated in section 4.1.

I begin by introducing some notation that will be helpful throughout. Define $z^*(\gamma)$ and $z^*(\gamma, k)$ as

$$z^*(\gamma) \equiv \frac{\mu^* - \mu_\theta(\gamma)}{\sqrt{\frac{1}{h_\theta h_\theta + h_\gamma}}} \quad \text{and} \quad z^*(\gamma, k) \equiv \frac{\mu^* - \mu_\theta(\gamma)}{\sqrt{\frac{1}{h_\theta h_\theta + h_\gamma + h_\gamma + k}}}.
$$

**Proposition 4.1** Suppose

$$\mu_\theta(B) \leq \mu_\theta(W)$$

and

$$h_B < h_W$$

Then $\lambda(B) < \lambda(W)$.

**Proof** We have that

$$\lambda(\gamma) = 1 - \Phi(z^*(\gamma))$$

$$= 1 - \Phi\left(\frac{\mu^* - \mu_\theta(\gamma)}{\sqrt{\frac{1}{h_\theta h_\theta + h_\gamma}}}\right)$$

By assumption, we have that $\mu^* \geq \mu_\theta(W) \geq \mu_\theta(B)$ and $0 < h_B < h_W$. Hence, $z^*(B) > z^*(W)$ and $\lambda(B) < \lambda(W)$.

**Proposition 4.2** Suppose (6) and (7) hold. Then

$$\lambda(W, k) - \lambda(B, k) > 0$$
is decreasing in \( k \).

**Proof** Given that the total hiring rate is constant, a change in \( k \) must either leave group hiring rates unchanged or move them in opposite directions. Further,

\[
\frac{\partial \lambda(\gamma)}{\partial k} = \frac{\partial}{\partial k} [1 - \Phi(z(\gamma, k))]
\]

\[
= \phi(z(\gamma, k)) \left[ \frac{h_\theta}{2 (k + h_\gamma)(k + h_\gamma + h_\theta)} z(\gamma, k) - \frac{\partial \mu^*/\partial k}{\sqrt{\frac{1}{h_\theta h_\gamma + h_\gamma + k}}} \right]
\]

\[
= \phi(z(\gamma, k)) \left[ \frac{1}{2 (k + h_\gamma)(k + h_\gamma + h_\theta)} \left( \mu^* - \mu_\theta(\gamma) \right) - \frac{\partial \mu^*}{\partial k} \right].
\]

Given that \( f(B) > f(W) \), it can’t be that \( \frac{\partial \lambda(B)}{\partial k} = \frac{\partial \lambda(W)}{\partial k} = 0 \). Hence, \( \frac{\partial \lambda(B)}{\partial k} \) and \( \frac{\partial \lambda(W)}{\partial k} \) must be opposite signed, and so \( f(B) > f(W) \) implies \( \frac{\partial \lambda(B)}{\partial k} > 0 \) and \( \frac{\partial \lambda(W)}{\partial k} < 0 \).

**Proposition 4.3** Suppose (6) and (7) hold. Then \( k^* \) is larger under affirmative action.

This claim can be restated as

\[
\frac{\partial}{\partial k} \Delta > 0.
\]

where

\[
\Delta = \pi_B \int_{\mu^*_B(k)}^{\infty} \mu dF(\mu|B, k) + \pi_W \int_{\mu^*_W(k)}^{\infty} \mu dF(\mu|W, k) - \pi_B \int_{\mu^*(k)}^{\infty} \mu dF(\mu|B, k) - \pi_W \int_{\mu^*(k)}^{\infty} \mu dF(\mu|W, k).
\]

To show this, I first change the variable of integration, expressing the integrals in terms of quantile functions rather than cumulative distribution functions. In particular, let \( Q(p|\gamma, k) = \{ \mu : p = F(\mu|\gamma, k) \} \). Then

\[
\pi_\gamma \int_{\mu^*_\gamma(k)}^{\infty} \mu dF(\mu|\gamma, k) = \pi_\gamma \int_{0}^{1} \frac{1}{F(\mu^*_\gamma|\gamma, k)} Q(p|\gamma, k) dp
\]

\[
= \pi_\gamma \int_{0}^{1} Q(p|\gamma, k) dp
\]

where \( \alpha = \frac{n}{a(n)} \). In these terms, we can express \( \Delta \) as

\[
\Delta = \pi_B \int_{\alpha}^{1} Q(p|B, k) dp + \pi_W \int_{\alpha}^{1} Q(p|W, k) dp - \pi_B \int_{\alpha}^{1} Q(p|B, k) dp - \pi_W \int_{\alpha}^{1} Q(p|W, k) dp
\]

\[
= \pi_B \int_{\alpha}^{1} Q(p|B, k) dp - \pi_W \int_{\alpha}^{1} Q(p|W, k) dp.
\]

\textsuperscript{42}The proof is similar to Claim III.D.1 in Autor and Scarborough (2008).
Note that
\[ Q(p|\gamma, k) = \mu_\theta(\gamma) + \sqrt{\frac{1}{h_\theta h_\gamma} \Phi^{-1}(p)} \]
and so
\[ \frac{\partial Q(p|\gamma, k)}{\partial k} = \sqrt{\frac{1}{h_\theta} \Phi^{-1}(p)} \frac{\partial}{\partial k} \left( \frac{h_\gamma + k}{h_\theta + h_\gamma + k} \right) > 0 \]
which is decreasing in \( h_\gamma \).

Hence,
\[
\frac{\partial}{\partial k} \Delta = \frac{\partial}{\partial k} \left[ \pi_B \int_\alpha^{\alpha_B} Q(p|B, k) dp - \pi_W \int_{\alpha_W}^{\alpha} Q(p|W, k) dp \right] \\
> \frac{\partial}{\partial k} \left[ \pi_B \int_\alpha^{\alpha_B} Q(p|B, k) dp - \pi_W \int_{\alpha_W}^{\alpha} Q(p|B, k) dp \right] \\
> 0
\]
where the first inequality holds because \( \frac{\partial Q(p|\gamma, k)}{\partial k} \) is decreasing in \( h_\gamma \) and the second inequality holds because \( \frac{\partial Q(p|\gamma, k)}{\partial k} \) is increasing in \( p \).
Notes: These figures graph summary statistics for sample used to construct the main event study plots presented in Figure 2. In Panel A I tabulate the number of establishments used to identify each lead and lag in the regulation and deregulation event studies. In Panel B I show the fraction of eventual contractors that are contractors at each node of the event studies.
Notes: This figure plots the frequencies for the number of contractor episodes experienced by establishments in the overlapping sample. The ‘> 2 Years’ and ‘> 4 Years’ bars refer to eventual contractors with more than 2 or 4 years between their regulation and deregulation events.
Figure A.3: Persistence and Bias

Notes: Each bar represents the coefficient estimates for a regression, along with a 95% confidence interval, with standard errors clustered at the firm level. The purple bars depict $\beta$ coefficient estimates for (A.1). The blue bars depict $\tilde{\beta}$ coefficient estimates for (A.2). The estimation samples exclude establishments that enter the sample as a federal contractor. For the establishments that become contractors, I only include years of data that are at most 6 years prior to their regulation event. The ‘Post-Event Window’ corresponds to different estimation samples. I estimate both (A.1) and (A.2) for a series a estimation samples, moving the data window from the year of the regulation event to 6 years following the regulation event. For each sample, I also restrict the set of eventual contractors included in the estimation to those that are present for the full set of years following the regulation event. Each model includes Census division by year fixed effects, and a quadratic in log establishment size.
Table A.1: EEO-1 Reporting Rates by Industry, 1990

<table>
<thead>
<tr>
<th>Industry</th>
<th>EEO-1 Coverage Rate (%)</th>
<th>Industry Size (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Agricultural Services</td>
<td>22.7</td>
<td>0.5</td>
</tr>
<tr>
<td>Mining</td>
<td>90.7</td>
<td>0.4</td>
</tr>
<tr>
<td>Construction</td>
<td>11.0</td>
<td>5.7</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>63.3</td>
<td>18.6</td>
</tr>
<tr>
<td>Transportation, Communications, Utilities</td>
<td>62.3</td>
<td>6.3</td>
</tr>
<tr>
<td>Wholesale Trade</td>
<td>19.4</td>
<td>7.2</td>
</tr>
<tr>
<td>Retail Trade</td>
<td>28.8</td>
<td>20.5</td>
</tr>
<tr>
<td>Finance, Insurance, and Real Estate</td>
<td>43.9</td>
<td>8.3</td>
</tr>
<tr>
<td>Services</td>
<td>30.9</td>
<td>32.6</td>
</tr>
<tr>
<td><strong>Overall</strong></td>
<td><strong>37.8</strong></td>
<td><strong>100.0</strong></td>
</tr>
</tbody>
</table>

Notes: Coverage rates are calculated by dividing EEO-1 reported employment by County Business Patterns employment totals for the 204 MSAs used in the analysis. Industry size is the fraction of total County Business Patterns reported employment in that industry.

Table A.2: Discrimination Charge Data, Summary Statistics

<table>
<thead>
<tr>
<th>Issue (%)</th>
<th>Full Data</th>
<th>Matched Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Charges</td>
<td>Merit Resolutions</td>
</tr>
<tr>
<td>Total</td>
<td>639,058</td>
<td>113,782</td>
</tr>
<tr>
<td>Discharge</td>
<td>55</td>
<td>50</td>
</tr>
<tr>
<td>Discipline</td>
<td>9</td>
<td>9</td>
</tr>
<tr>
<td>Harassment</td>
<td>16</td>
<td>18</td>
</tr>
<tr>
<td>Hiring</td>
<td>6</td>
<td>5</td>
</tr>
<tr>
<td>Promotion</td>
<td>11</td>
<td>12</td>
</tr>
<tr>
<td>Suspension</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Terms and Conditions</td>
<td>25</td>
<td>27</td>
</tr>
<tr>
<td>Wages</td>
<td>8</td>
<td>10</td>
</tr>
<tr>
<td>Other</td>
<td>24</td>
<td>28</td>
</tr>
</tbody>
</table>

Notes: Data include all Title VII discrimination charges filed with EEOC from 1990 to 2004 alleging discrimination against black workers. Matched sample includes all charges that I was able to match in the EEO-1 data, restricting to the 204 MSAs used in the analysis. Each charge can include multiple allegations, so issue percentages do not sum to 100. Merit resolutions are discrimination charges that result in a withdrawal with benefits, settlement with benefits, successful conciliation, or unsuccessful conciliation.