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The Persistent Effect of Temporary Affirmative Action†

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I estimate the dynamic effects of federal affirmative action regulation, exploiting variation in the timing of regulation and deregulation across work establishments. Affirmative action increases the black share of employees over time: in 5 years after an establishment is first regulated, the black share of employees increases by an average of 0.8 percentage points. Strikingly, the black share continues to grow at a similar pace even after an establishment is deregulated. I argue that this persistence is driven in part by affirmative action inducing employers to improve their methods for screening potential hires. (*JEL* J15, J23, J24, J83, K31)

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. 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. A temporary affirmative action regulation can correct those incentives and permanently reduce inequality by eliminating negative stereotypes, though it can also have the opposite effect (Coate

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and Loury 1993). While less emphasized in the literature, a transitory intervention can also have persistent effects through *employer*-level mechanisms that affect the racial composition of employee flows. For example, temporary affirmative action may induce persistent changes in an employer's recruitment and screening practices or the composition of its referral applicants.

In this paper, I study the dynamic effects of Executive Order 11246, the primary affirmative action regulation for employment in the United States. 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 US workforce (Office of Federal Contract Compliance Programs 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 typically the focus of affirmative action research (Holzer and Neumark 2000a).[1] My work builds on the influential analysis of Leonard (1984), and more recently Kurtulus (2016) and Kurtulus (2012), on the impacts of Executive Order 11246 on the employment and occupational advancement of women and minorities. 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 US workforce would eliminate the black-white jobless gap over this period.

Strikingly, I find that the black share of employees *continues to grow* even after an employer is deregulated. In the five years after an establishment is last observed as a contractor, its black share of employees increases by an *additional* 0.8 percentage points. This persistence is evident more than a decade following deregulation.

The interpretation of this persistence is potentially muddled by the fact that, for a given employer, I can only observe whether regulation is temporary ex post. Deregulated employers may continue to increase the black share of their workforce

[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.

because they anticipate becoming contractors in the future. There are two reasons employers may engage in this type of anticipatory behavior. I provide evidence that neither explain the observed persistence. First, employers may believe that increasing the black share of their workforce increases their chances of winning a subsequent contract. I describe how this belief would not be supported by the regulation itself. I also show that an establishment's black share of employees does not predict initial entry into federal contracting. Moreover, former contractors and establishments that have never held a contract face similar likelihoods for acquiring a future contract. Hence, the absence of initial selection into contracting suggests that former contractors are not increasing the black share of their workforce to increase their chances of winning a subsequent contract. Second, employers may derive *option value* from continuing AA-induced hiring practices if those practices involve significant adjustment costs.[2] Given that this option value would be increasing in the likelihood of future regulation, an option value-based explanation provides the following testable implication: if employers are sufficiently informed ex ante about their chances of becoming a contractor again, then future contractor status should predict the degree of persistence observed among former contractors. This prediction is not supported by the data: for previously regulated employers, the degree of persistence is *independent* of whether an employer wins a subsequent contract.

The observed persistence is difficult to reconcile with existing economic models of affirmative action, which focus on the aforementioned human capital channel (Fang and Moro 2011).[3] 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. 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.[4] Examples may include: employing and training personnel specialists, developing job tests, harnessing referral networks, building relationships with and utilizing intermediaries, such as employment agencies and schools, and learning by doing or experimentation.

Building on the seminal Phelps (1972) model of statistical discrimination, I show how the persistence found here may be driven in part 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

[2] While hiring or firing costs could potentially explain persistence in the black share of employees, they alone could not explain the continued *increase* in the black share of employees.

[3] One exception is Athey, Avery, and Zemsky (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 III.

[4] While 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.

their hiring standard for the protected group. I introduce an additional 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 suggestive evidence supporting two of the model's main predictions: (i) regulation increases the return to investments in screening and (ii) screening investments reduce between-group differences in hiring rates.

While the evidence suggests screening investments may play a role in compliance, I discuss several alternative mechanisms, including belief updating and worker preferences for or production complementarities with own-race coworkers, that I cannot rule out as operative due to data limitations. Regardless of the channel, the fact that a temporary intervention has long-term effects on a given employer's trajectory suggests that there exist multiple equilibria for the racial composition of its workforce.

There exists a substantial literature on labor market antidiscrimination 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 US 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 in the early years of the program, before the Reagan administration significantly defunded the agency charged with the regulation's enforcement in the early 1980s (Leonard 1984, 1990; Ashenfelter and Heckman 1976; Goldstein and Smith 1976; Heckman and Wolpin 1976; Smith and Welch 1984; Rodgers and Spriggs 1996; Kurtulus 2016). Prior work has found the regulation to have *little to no impact* after Reagan's inauguration (Leonard 1990; Kurtulus 2016).[5]

In an important prior contribution, Kurtulus (2016) exploits within employer variation in contractor status and one of the datasets 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 level increase of less than 0.1 percentage points 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.[6] Kurtulus (2016)

[5] Rodgers and Spriggs (1996) is an exception. Using data from 1979–1992, they find sizable cross-sectional differences between federal contractors and non-contractors in the minority share of employees.

[6] These results can be seen in table 5 and table 6 of Kurtulus (2016), respectively, combining point estimates for black men and women.

is the first to consider the potentially dynamic effects of the regulation on employment, an important innovation.[7]

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 for identification. I show that, in the presence of the type of persistence documented here, research designs applied in the existing literature will substantially understate the causal impact of regulation. Prior work, including Kurtulus (2016), relies on comparisons of contractors to non-contractors, either across or within employers. In the presence of persistence these comparisons will 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 case, a more appropriate comparison for assessing the regulation's impact is between employers that have *ever* been contractors to those that have *never* been contractors.

I apply a flexible event study research design that accommodates persistence and find that: (i) the regulation's causal effect on black employment is *substantially* larger than previously estimated, particularly after Reagan-era defunding; (ii) the impact accumulates over time; and (iii) 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, a critical step for interpretation.

This paper also 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 1970s. The authors find that these quotas significantly increased black employment relative to national trends.[8] 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.

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 II, I estimate the dynamic effects of regulation and deregulation in an event study framework. In Section III, I discuss potential causal mechanisms, outline the screening model, and present supporting evidence. Section IV concludes.

I. 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 outlaws discrimination on the basis of race, color, religion, sex, or national origin in all but the smallest private firms, Executive Order 11246 requires that firms

[7] Leonard (1984) also tabulates minority employment growth separately for establishments that enter federal contracting and establishments that exit federal contracting.

[8] Miller and Segal (2012) also find that hiring quotas had only a marginal impact on female employment.

with federal contracts[9] make active efforts to prevent discrimination.[10] For firms with 50 or more employees and holding a federal contract worth $50,000 or more, the requirements are more specific. 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" (Office of Federal Contract Compliance Programs 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 make a good faith effort to employ minorities at rates (at least) proportional to shares of the local and qualified workforce, though local and qualified are not specified explicitly or 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 the more specific requirements of Executive Order 11246 as affirmative action (AA) regulation. I will also refer to establishments as federal contractors if their parent firm meets the 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 US 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, I use only data from 1978 onward.

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 (Office of Federal Contract Compliance Programs 2013).

To enforce the regulation, the OFCCP conducts compliance evaluations, reviews of a small fraction of covered establishments each year (about 1 percent of covered establishments in 2004[11]) to determine whether their AAPs are sufficient and whether they have made good faith efforts to implement their plans. These reviews

[9] In particular, the basic requirements apply to firms with at least 25 employees and $10,000 or more in federal contracts over a 12-month moving window.

[10] 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.

[11] There 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.

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. 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 in Section IIA. 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 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.[12]

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.[13] 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 60 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.[14] The one potential exception is large contracts, for which firms may be subject to "pre-award" compliance evaluations—evaluations that occur before a firm can formally initiate the contract (Office of Federal Contract Compliance Programs 2013).[15] In practice, few contracts are sufficiently large to require pre-award compliance evaluations, and they are even less common for the firms I focus on in the analysis, which are not perennial contractors.

[12] 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 (General Accounting Office 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 a sample of contractors in the 1970s, goals for minority share gains are positively correlated with realized gains, but the goals were rarely met.

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

[14] 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.

[15] In 2013, this condition applied to contracts in excess of $10 million (Office of Federal Contract Compliance Programs 2013).

Moreover, there is no requirement that an establishment be in compliance when it is not holding a federal contract.

II. The Dynamic Effects of Affirmative Action

A. *Data*

To undertake this analysis, I use establishment-level EEO-1 form data collected by the US Equal Employment Opportunity Commission (EEOC) covering the years 1978–2004. Previous research studying affirmative action regulation use versions of the same data.[16] As part of the Civil Rights Act of 1964, private sector firms meeting certain size requirements are required to complete EEO-1 forms annually and submit them to the EEOC.[17] Firms are required to report their overall racial, ethnic, and gender composition and the racial, ethnic, and gender composition of each of their establishments meeting size requirements, disaggregated by 9 major occupation groups.[18] Employers are instructed to base demographic classifications on worker self-identification or visual inspection, where the former is the preferred method. There is no distinction between race and ethnicity in the data; in particular, Hispanic workers are classified as a distinct, non-overlapping group. 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 afterward.[19] 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.[20] 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 in Section I, while regulation status is assigned at the firm level, the regulation defines compliance and is enforced at the establishment level.[21] For the analysis, I limit the sample to establishments located in metropolitan statistical areas (MSAs) where the black share of the working age population is at least 5 percent at some point from 1978–2004.[22] This includes establishments from 204 MSAs, where more than 80 percent of MSA

[16] This includes Kurtulus (2016, 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).

[17] This requirement excludes institutions of higher education, Indian tribes, and tax-exempt private membership clubs. Institutions of higher education submit forms containing analogous information as part of the Integrated Postsecondary Education Data System. The EEOC collects distinct reports from referral unions (EEO-3), state and local governments (EEO-4), and primary and secondary public school districts (EEO-5).

[18] The nine occupation categories consist of: officials and managers, professionals, technicians, sales workers, administrative support workers, craft workers, operatives, laborers/helpers, and service workers.

[19] Results throughout are similar if I impose a uniform firm size cutoff of 100 employees for all establishment observations.

[20] Each of these is likely recorded with some error.

[21] This follows prior work in this literature with the exception of Kurtulus (2016, 2012), which conducts the analysis at the firm level.

[22] Population demographics are calculated using data from the National Cancer Institute Surveillance, Epidemiology, and End Results Program (SEER). Working age is defined as 15 to 64.

establishments are located.[23] 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 US establishments.[24] I estimate coverage rates for the EEO-1 data in 1990 in the online 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 percent of total employment.

B. *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 subject 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.

Previous research in this area suffers from an additional shortcoming: if regulation has an effect on employers that persists even when they are no longer contractors, previous estimates understate the regulation's full impact. 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 understate the causal impact of regulation because some employers that are currently non-contractors were previously contractors, and

[23] To define MSAs, I use 1980 census definitions.
[24] In addition, some firms fail to submit required EEO-1 forms.

the minority share of those employers is still affected by the regulation. In the online Appendix, I evaluate this bias empirically, and demonstrate that the magnitude of this bias may be severe.

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 preexisting 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 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 percent of the establishments that I observe becoming contractors in the first place[25] 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 IID.

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.[26] I first estimate regression models of the form

$$\text{black share}_{it} = \alpha_i + \lambda_{d(i),t} + X_{it}\gamma + \sum_{j=a}^{b} \theta_j D_{it}^j + \epsilon_{it}, \tag{1}$$

where black share$_{it}$ denotes the black share of establishment i in year t, α_i and $\lambda_{d(i),t}$ are establishment and census division-by-year fixed effects, X_{it} are controls

[25] That is, eventual contractors that do not enter the data as a contractor.

[26] 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. Fortunately, 73 percent of eventual contractors that do not enter the data as federal contractors first enter the data after 1978. Reassuringly, results throughout are similar if I restrict the analysis to these establishments.

for establishment size, and D_{it}^{j} are leads and lags for establishments first becoming contractors, defined as

$$D_{it}^{j} = D_i \mathbf{1}(t = \tau_i + j),$$

where D_i is an indicator for whether the establishment ever becomes a federal contractor, and τ_i is the year the establishment first becomes a contractor. I normalize the value of $\theta_{-1} = 0$. The sequence of θ_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 $b = 6$.[27]

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 θ_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 this so estimates of the event study endpoints, θ_{-6} and θ_6, are not driven by a mixture of various leads and lags. Relatedly, the sample of establishments driving identification of θ_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 (1) 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 θ_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 τ_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

[27] The results are similar for alternative windows.

Table 1—Event Study Samples, Summary Statistics

	All	Non-contractors sample	Regulation sample	Deregulation sample	Overlapping sample
Number of establishments	569,061	161,703	63,595	85,745	36,030
Number of firms	87,544	42,696	15,785	16,749	8,532
Establishment size[a]	227	184	174	165	170
	(580)	(392)	(337)	(319)	(307)
Industry percent share					
Agricultural services	0.3	0.4	0.2	0.2	0.2
Mining	0.9	0.4	0.4	0.3	0.2
Construction	2.2	1.6	1.4	1.5	0.9
Manufacturing	19.0	13.6	14.5	13.5	12.0
Transportation, comm., util.	10.2	6.1	6.1	4.4	4.3
Wholesale trade	5.7	4.6	4.3	3.6	3.4
Retail trade	28.3	39.2	40.8	49.4	51.0
Finance, insurance, real estate	10.1	8.2	8.0	6.5	7.5
Services	23.4	26.1	24.3	20.6	20.6
Black share quantile[a]	50.0	48.3	48.4	48.6	47.8
Standardized black share[a]					
Mean	0.000	−0.003	−0.002	0.001	−0.020
Median	−0.305	−0.341	−0.336	−0.330	−0.340
Black percent share of employees[a]	14.1	14.3	14.0	13.8	13.5
Black percent share of population, 15–64[a]	15.5	15.5	15.2	15.0	15.0

Note: Standard deviations are in parentheses.

[a] Quantiles and normalizations 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 regulation event or, for establishments that enter the data as contractors, the first year observed in the data.

contractors. Again, for contractors and former contractors, I only include years of data that are in the 6-year window around the event. For eventual contractors that do not enter the data as contractors, I further restrict the data window to years following the regulation 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 in 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 γ 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. This sample includes 36,030 establishments and 8,532 firms.

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

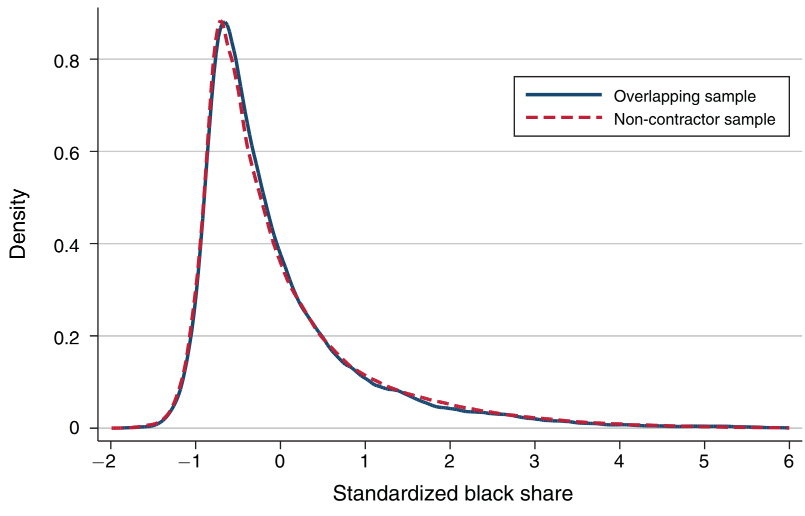

Figure 1. Selection into Federal Contracting

Notes: This figure is the estimated density for the normalized establishment black share in the overlapping and non-contractor samples of establishments. These samples are defined in Section IIB. Establishment black share is normalized to be mean zero and have standard deviation one within MSA by year cells. For the overlapping sample, this depicted density is that for the normalized black share at the last year observed before regulation event. Both densities use an Epanechnikov kernel with a bandwidth of 0.1.

be in retail trade.[28] Prior to regulation, establishments in the regulation and overlapping samples have employee black shares that are very similar to non-contractors. In Figure 1, I plot the estimated density for the normalized establishment black share in the overlapping and non-contractor samples, where establishment black share is normalized to have mean zero and standard deviation one within MSA by year cells. For the overlapping sample, I plot the density for the normalized black share in the last year observed before regulation event. The densities are nearly identical, indicating that there is little selection into contracting based on initial black share. The average black share of employees across establishments is 14.1 percent, while the average black share of the working age MSA population, weighted by the number of observations, is 15.5 percent.

The event study design requires variation in the timing of the event of interest for the contractor sample. Figure 2 displays this variation for the overlapping sample. Both regulation and deregulation events vary widely in their timing.

C. *Main Results*

I begin with the regulation event study. In Panel A of Figure 3, I plot the point estimates and 95 percent confidence intervals of the θ_j sequence for the overlapping sample, with standard errors clustered at the firm level. The estimated model

[28] I present additional summary statistics for the overlapping sample in the online Appendix, tabulating the years between regulation and deregulation events by establishment, the distribution of contractor spell durations by episode, and the number of contractor spells by establishment.

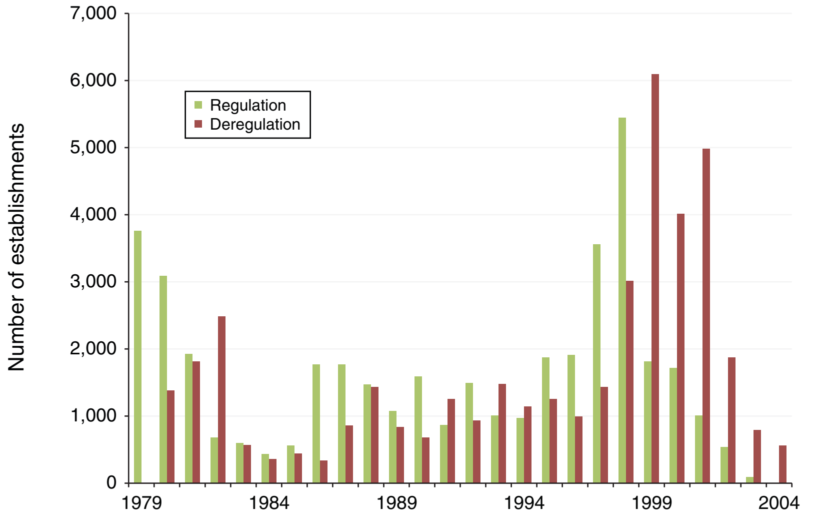

Figure 2. Variation in Regulation and Deregulation Events

Notes: This figure is a histogram of the establishment-level regulation and deregulation events as described in Section IIB. Regulation events, depicted in light grey, 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 dark grey, are the first year an establishment that was previously a contractor is (i) not a contractor, and (ii) never subsequently observed as a contractor in the data.

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 θ_j coefficients 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.

Moreover, the estimated θ_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

$$\text{(2)} \qquad \text{black share}_{it} = \alpha_i + \lambda_{d(i),t} + X_{it}\gamma + \beta t \times \mathbf{1}_{\exists \tau_i} + \Delta\beta(t - \tau_i + 1) \times \mathbf{1}_{(t \geq \tau_i)} + \epsilon_{it},$$

where $\mathbf{1}_{\exists \tau_i}$ is an indicator for whether an establishment is ever observed as a contractor. The coefficient $\Delta\beta$ is the change in slope associated with the regulation event. I present these parametric coefficient estimates in Table 2.

I assess the robustness of these estimates by altering the sample and set of controls used. The results from this exercise are presented in Table 2. Panel A uses the overlapping sample and panel B uses the full regulation sample. Column 4 restricts to a balanced sample of eventual contractors. To assess whether the results are affected by censoring or the spike in events beginning in 1998, column 5 restricts eventual

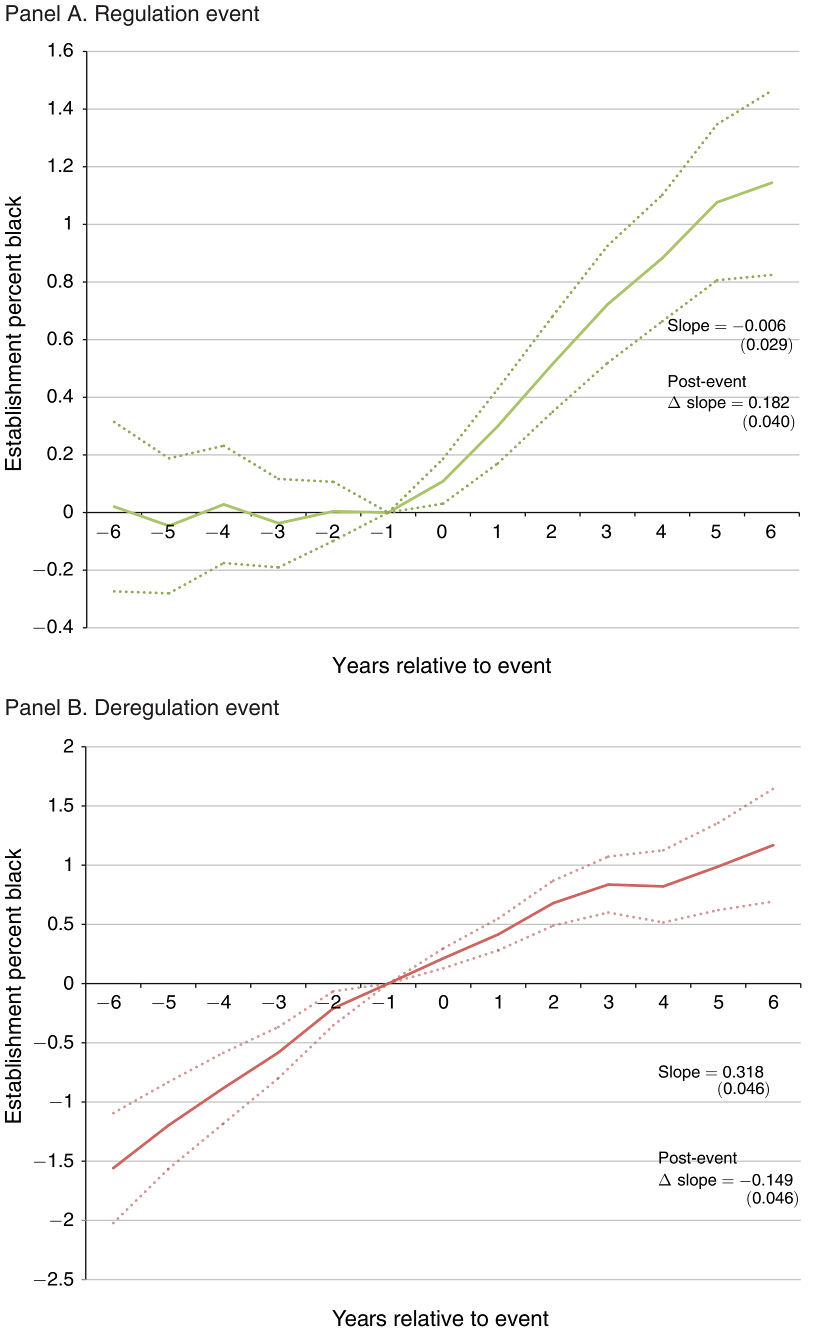

Figure 3. Regulation and Deregulation Event Studies

Notes: These figures plot event study coefficients and 95 percent 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 IIB. 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.

TABLE 2—REGULATION EVENT STUDY ESTIMATES

Outcome: Black share *Panel A: Overlapping sample*	(1)	(2)	(3)	Balanced (4)	Event < 1998 (5)	Within- occupation (6)
β	−0.006	0.018	0.022	−0.002	0.007	−0.013
	(0.029)	(0.027)	(0.027)	(0.032)	(0.036)	(0.032)
$\Delta\beta$	0.182	0.167	0.148	0.132	0.193	0.172
	(0.040)	(0.039)	(0.037)	(0.039)	(0.047)	(0.045)
Number of treated establishments		36,030		4,525	26,811	36,030
Panel B: Full regulation sample						
β	−0.005	0.014	0.037	−0.029	−0.002	−0.004
	(0.023)	(0.021)	(0.021)	(0.029)	(0.029)	(0.025)
$\Delta\beta$	0.166	0.160	0.131	0.146	0.184	0.148
	(0.034)	(0.033)	(0.031)	(0.038)	(0.041)	(0.038)
Number of treated establishments		63,595		6,066	45,694	63,585
Division × year FEs	✓			✓	✓	✓
MSA × year FEs		✓				
Industry × division × year FEs			✓			
Establishment FEs	✓	✓	✓	✓	✓	✓
Balanced				✓		

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 IIB. 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.

contractors to those with regulation event prior to 1998. Columns 1, 4, and 5 include census division-by-year fixed effects, column 2 includes MSA-by-year fixed effects, and column 3 includes census division-by-1 digit industry-by-year fixed effects. The coefficients from column 1 are plotted in Figure 3. All models include a quadratic in log establishment size.

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.[29]

[29] 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 in follow-up work Leonard (1990) finds that AA had no impact on black employment in the 1980s.[30] I do not find this to be the case. This may be due to the differences in our research designs described in Section IIB.[31]

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 the online Appendix, I tabulate the number of eventual contractors in the overlapping sample that identify each lead and lag as well as the fraction of eventual contractors that are contractors in each year following the regulation event. A year after their regulation event, only about 35 percent 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 3, I plot the point estimates and 95 percent confidence interval of the θ_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 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.[32]

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. 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 percent larger than 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.[33]

One concern with the persistence result is that it may be affected by data censoring. Establishments included in the overlapping or deregulation sample may become contractors after 2004, outside of the data window. About 42 percent and 40 percent of establishments in the overlapping and deregulation samples are present in 2004. To assess the importance of censoring for the results, I estimate the same event study model restricting the overlapping and deregulation samples to establishments with deregulation events prior to 1998. Only 28 percent and 27 percent of these

[30] 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.

[31] 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.

[32] This could be driven by temporary AA generating a persistent *level* increase in the black share of new hires, for example. Unfortunately, I do not have data on worker flows.

[33] In the online Appendix, I explore how the slope evolves over time in greater detail.

TABLE 3—DEREGULATION EVENT STUDY ESTIMATES

Outcome: Black share *Panel A: Overlapping sample*	(1)	(2)	(3)	Balanced (4)	Event < 1998 (5)	Within- occupation (6)
β	0.318	0.334	0.290	0.221	0.326	0.283
	(0.046)	(0.048)	(0.040)	(0.088)	(0.090)	(0.040)
$\Delta\beta$	−0.149	−0.161	−0.130	0.001	−0.141	−0.129
	(0.046)	(0.046)	(0.042)	(0.110)	(0.079)	(0.050)
Number of treated establishments		36,030		2,530	17,386	36,030
Panel B: Full deregulation sample						
β	0.274	0.288	0.268	0.203	0.321	0.233
	(0.038)	(0.040)	(0.034)	(0.059)	(0.051)	(0.040)
$\Delta\beta$	−0.101	−0.119	−0.115	0.021	−0.135	−0.080
	(0.045)	(0.045)	(0.041)	(0.085)	(0.048)	(0.047)
Number of treated establishments		85,745		5,682	41,573	85,745
Division × year FEs	✓			✓	✓	✓
MSA × year FEs		✓				
Industry × division × year FEs			✓			
Establishment FEs	✓	✓	✓	✓	✓	✓
Balanced				✓		

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 IIB. 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.

establishments are present in 2004. The estimates, presented in column 5, are virtually identical. In results not shown here, I also restrict the samples to establishments that exit the data prior to 2004. While selecting the sample on this outcome may be problematic, and so the coefficients should be interpreted with caution, the estimates are again virtually identical. Censoring is not a significant factor for the main results.

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 4 displays the results. In all cases, the black share of employees continues to increase even after establishments are deregulated.[34]

In the online Appendix I investigate heterogeneous treatment effects across establishments and occupations. First, I assess whether estimates are sensitive to the selective attrition of establishments from the data via changes in size. I find that they are not. Second, I exploit the fact that compliance evaluations are targeted based on

[34] In the online Appendix I combine both event studies into one parametric regression model. I also estimate a variant of this model that excludes non-contractors from estimation. This approach is appealing in that it does not rely on non-contractors to identify the counterfactual black share for eventual contractors. The results are similar.

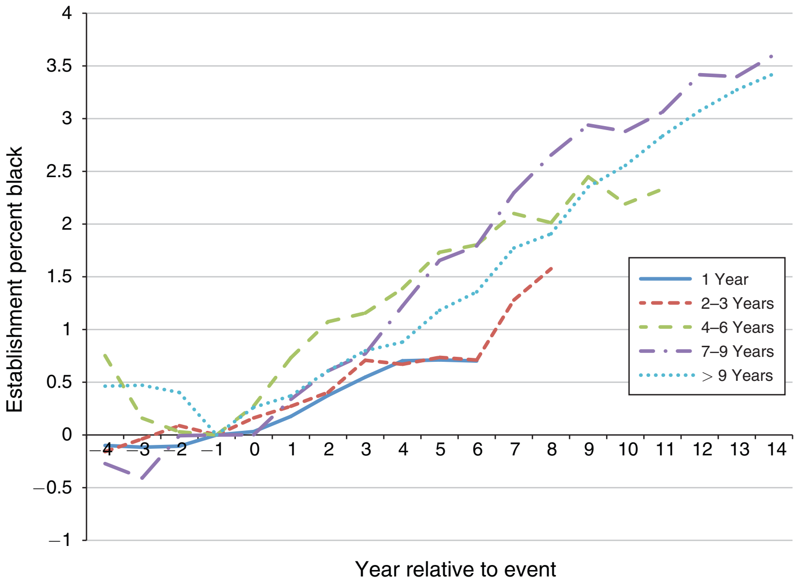

Figure 4. Regulation Event Study, by Duration

Notes: This figure plots event study coefficients 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 IIB. 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.

employer size (Leonard 1985a) to examine whether the regulation's impact is more substantial where enforcement is stronger. I find that establishments that are part of multi-establishment firms, which are historically more likely than singleton establishments to be audited as contractors, experience substantially larger black share gains following regulation. Third, I test whether the degree of persistence depends on an establishment's experience as a contractor. I find that the slope persistence is higher for establishments with more contracting experience. Fourth, 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. Event study patterns are similar across occupation groups.

D. *Coincident Changes in Employer Characteristics*

One concern with interpreting the results in Section IIC 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 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. For this reason, I assess whether accounting for establishment size or growth has important implications for the event study results in Section IIC.

To reestimate the event studies using within-occupation changes, I estimate (2) at the establishment by occupation level (rather than the establishment level.) In particular, I estimate

$$\text{(3)} \quad \text{black share}_{iot} = \alpha_{io} + \lambda_{d(i),t} + X_{it}\gamma + \beta t \times \mathbf{1}_{\exists \tau_i} + \Delta\beta(t - \tau_i + 1) \times \mathbf{1}_{(t \geq \tau_i)} + \epsilon_{iot},$$

where o indexes occupation and black share$_{iot}$ is the black share of employees in an establishment by occupation cell. Note that (3) 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 present the coefficient estimates in column 8 of Tables 2 and 3. Panel A displays the regulation event study and panel B displays the deregulation events study. The estimates are similar to those in column 1, 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, in the online Appendix, I estimate separate event studies for the overlapping sample and for a subsample restricting eventual contractors to those with more than six 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. 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 five years after the regulation event, establishment size increases by about 5 percent 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 and deregulation events as constructed indeed reflect a meaningful event and its reversal. These patterns also emerge in local labor markets with population black shares less than 5 percent, suggesting the size responses are not due to regulation per se, but contractor status.

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. In Section IIIB, I find that 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 percent 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, I 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:

$$\text{(4)} \qquad \text{black share}_{it} = \alpha_i + \lambda_{d(i),t} + X_{it}\gamma + \sum_{j=a}^{b} \theta_j D_{it}^j + \sum_{j=a}^{b} \tilde{\theta}_j \tilde{D}_{it}^j + \epsilon_{it},$$

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

The results are shown in the online Appendix. In fact, the event studies for establishments that shrink and those that grow are quite similar. For establishments that grow, total black employees continue to grow after the loss of contractor status. In results not shown here, I also find that the results are not sensitive to whether establishment size is included as a control variable.

E. *Anticipatory Behavior*

An additional concern with interpreting the results in Section IIC 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. Deregulated employers may continue to increase the black share of their workforce because they anticipate becoming contractors in the future. There are two reasons employers may engage in this type of anticipatory behavior. First, employers may perceive that increasing their black share will improve their chances of acquiring a future contract. Second, currently unregulated employers may derive option value from continuing AA-induced hiring practices if those practices involve significant adjustment costs. In this section, I address these two concerns.

First note that it is a priori unclear 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 I, 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 or that contract allocation depends on racial composition for reasons outside of the law.

Even if firms do not perceive that increasing their black share of employees improves their chances of acquiring a contract, similar anticipatory behavior can be rationalized in the presence of adjustment costs. In particular, firms may derive *option value* from continuing AA-induced personnel practices if those practices involve significant adjustment costs. If pausing or restarting those personnel practices is costly, then profit-maximizing, deregulated firms may continue those practices if there is a chance they will be regulated again in the future. The option value associated with continuing AA-induced personnel practices will be increasing in the perceived likelihood of future regulation; at the extreme, there is no such option value for a firm that is certain it will not acquire a future contract. Hence, an option value-based explanation provides the following testable implication: if employers are sufficiently informed ex ante about their chances of becoming a contractor again, then future contractor status should predict the degree of persistence observed among former contractors.

I provide three pieces of evidence that the persistence I document is unlikely to be driven by anticipatory behavior. First, I show that there is no evidence of initial selection into federal contracting. Second, I show that former contractors and establishments that have never held a contract face similar likelihoods for acquiring a future contract. Hence, the absence of initial selection into contracting suggests that former contractors are not increasing the black share of their workforce to increase their chances of winning a subsequent contract. Third, I show that the data do not support the hypothesis that option value drives persistence: for previously regulated employers, the degree of persistence is *independent* of whether an employer wins a subsequent contract.

If employers perceived that a larger black share of employees improved an employer's chances of winning a federal contract, we would expect first time contractors to have larger black shares than similar employers that have never held a contract. This is not the case—before eventual contractors are first regulated, their black share is very similar to those that never become contractors. This is evident in

Figure 1. Moreover, initial black share is slightly *negatively* correlated with future contractor tenure.

For the second exercise, 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 black 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.[35]

These acquisition likelihoods are plotted in Figure 5. The vertical axis denotes the fraction of firms that acquire a federal contract in the future in the next year. To avoid censoring, I exclude observations from 2004 in constructing the plot. The purple line depicts this likelihood for establishments that have previously held a federal contract, but have not 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.

In the online Appendix, 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 and temporal variation. In the regression models, I also examine acquisition likelihoods for three year and five year windows and whether an establishment ever becomes a contractor as observed in the data.[36] I also try limiting the former contractors to those who have been previously observed as contractors for at least three years. The results are similar across approaches.

There are three points to note from Figure 5. 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 than firms that have never held a contract. While current contractors are about 45 percentage points more likely to hold a contract in one or three years than firms that have never held a contract, this difference reduces to 5 and 13 percentage points after 1 year without a contract. After four years, the difference reduces further to 0 and 5 percentage points. Yet, Figure 3 shows that such firms continue to increase their black 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

[35] The pattern of results is similar if I do not make this restriction.

[36] For these two likelihoods I exclude observations from 2000–2004 and 2004 only, respectively.

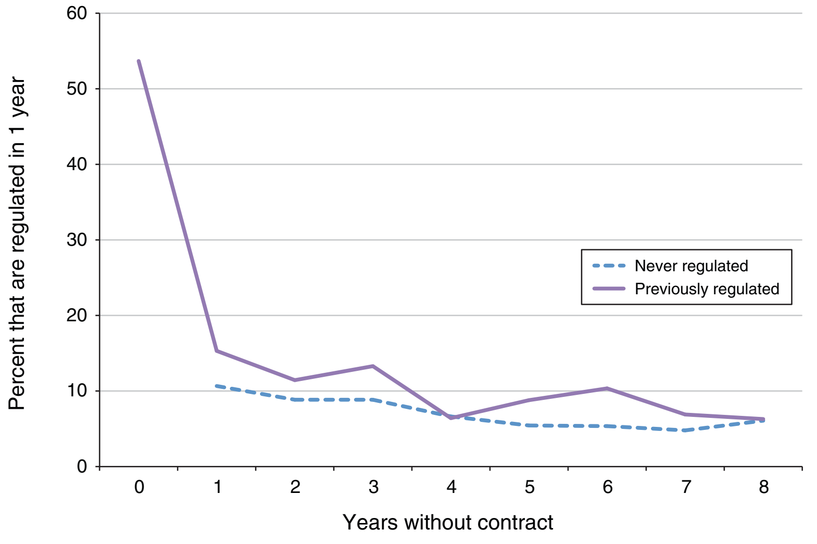

Figure 5. Likelihood of Regulation Next Year

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 the next year. The likelihoods are computed using data from 1979 to 2003. The solid line depicts this likelihood for firms that have previously held a federal contract, but have not held a contract for a given number of years, as marked on the horizontal axis. For the 0 value on the horizontal axis, the solid line denotes the fraction of current contractors that will be contractors in the given time period. The dashed 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.

the first year. While about 55 percent of firms who hold a contract in one year are contractors in the next, after one year without a contract, only about 25 percent of former contractors hold one in the next. After four years, this likelihood declines to about 8 percent. By contrast, robust black share increases continue following deregulation.

These results, combined with the absence of initial selection into contracting, suggest that the persistence documented in Figure 3 is unlikely to be driven by firms increasing the black share of their workforce to increase their chances of winning a subsequent contract.

Finally, I assess the potential for option value to explain the documented persistence. The option value associated with continuing AA-induced personnel practices will be increasing in the perceived likelihood of future regulation. To assess whether this option value is a significant component of an employer's decision making, I test whether the degree of persistence following an establishment's exit from contracting is increasing in the perceived likelihood of the firm acquiring a future contract. Unfortunately, establishment expectations over future contractor status are not observable. Instead, I compare the behavior of establishments based on their *ex post realizations* of contractor status. The motivating assumption is that, in cases when establishments *do* win a subsequent contract ex post, they believed the likelihood of winning a contractor was higher ex ante than in cases when establishments *do not* win a subsequent contract. In particular, some establishments know ex ante that they are unlikely to become contractors in the future (for example, they know their parent firm will no longer pursue a contract). For these firms, persistence in their personnel practices cannot be attributed to option value. Hence, if the degree of persistence is independent of ex post contractor status, this would undermine the notion that observed persistence is driven by option value.

In practice, I compare the behavior of establishments following a deregulation event—transitions from contractor to non-contractor, where the establishment never becomes a contractor again as observed in the data—to behavior following an analogously defined "temporary deregulation" event—transitions from contractor to non-contractor, where an establishment *does* win a subsequent contract. In particular, I focus on an establishment's *last* temporary deregulation event observed in the data.

I estimate and compare event study models for the deregulation event and the temporary deregulation event. To estimate the former event study, I use non-contractors and the overlapping sample as in Figure 3. I estimate the temporary deregulation event study model using non-contractors and eventual contractors that do not enter the data as contractors and experience a temporary deregulation event. As in Section IIB, for the establishments that are at some point contractors, I only include years of data that are both (i) in a six-year window around the event of interest and (ii) after the establishment's regulation event. For the temporary deregulation event study, I further restrict the data to years prior to the establishment's subsequent contract. Note that the same eventual contractor can serve in the estimation of both event study models.[37] I present summary statistics for both samples in the online Appendix.

I present the results for this exercise in Figure 6. The light grey line depicts the same deregulation event study estimates for the overlapping sample that are shown in 3. The dark grey line depicts analogous event study estimates where the event of interest is *temporary deregulation*, described above. The patterns are strikingly similar. In either case, the black share of employees continues to increase following deregulation at a similar slope. This suggests that the observed persistence is not driven by option value.

III. Potential 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 the skill gaps (Coate and Loury 1993). Given the variation in regulation exploited here, it is unlikely the results 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.[38]

[37] That is, an establishment can transition from contractor to non-contractor more than once.

[38] 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.

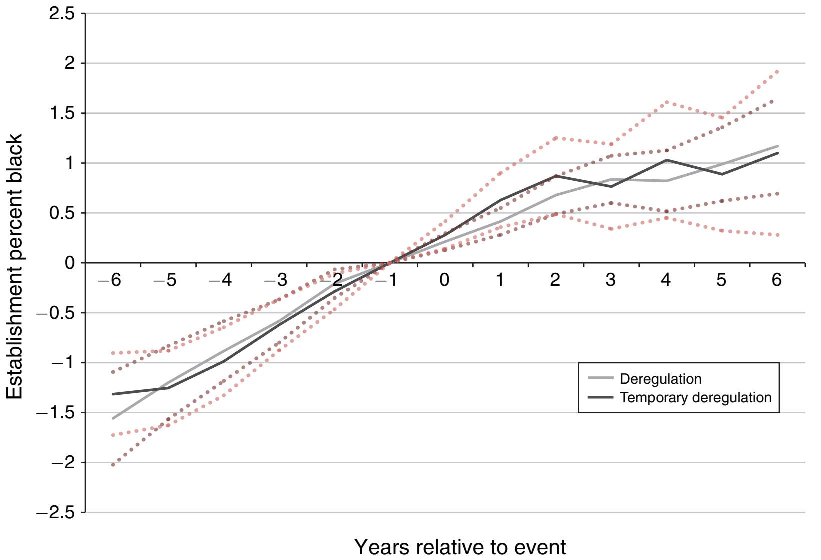

Figure 6. Subsequent Contracts and Anticipation

Notes: This figure plots event study coefficients and 95 percent confidence intervals (dotted) estimated using model (1), where the outcome variable is the percent black of an establishment's employees. The light grey line depicts the deregulation event study estimates using the overlapping sample and the standard definition of the deregulation event, as described in Section IIB. The dark grey line depicts event study estimates where the event of interest is "temporary deregulation": a transition from contractor to non-contractor, where an establishment eventually regains its contractor status. The estimation sample is limited to non-contractors and eventual contractors that do not enter the data as contractors and experience a "temporary deregulation" event. 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.

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 continues to foster significant updating.

There are also reasons why, independent of an employer's conscious decisions, the composition its employees may exhibit state dependence, whereby temporary AA may persistently alter a firm's trajectory. If employers rely on referrals to find new workers, and referral networks tend to display group homophily (McPherson, Smith-Lovin, and Cook 2001), then a shock to an employer's workforce composition can persist by affecting the composition of future hires. The dynamics may be similar if potential applicants prefer to work with or are more productive working with others who share their background. Alternatively, managers may be more likely to hire candidates that share their background, either because they are better able to screen those candidates, or managers or candidates prefer those matches (Giuliano, Levine, and Leonard 2009; Åslund, Hensvik, and Skans 2014).

Temporary AA may also generate a persistent increase in 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, then temporary AA, in the presence of firing costs (or other adjustment costs), could produce the persistence found here.

Data limitations make distinguishing between all potential mechanisms difficult. Instead, my approach is to focus on one potential channel, and provide evidence that the channel is empirically relevant. For the remainder of the paper, I focus on what I call the *screening capital* channel—AA may induce employers to make (partially) irreversible investments to improve screening. In the next section, I formalize and discuss this channel in more detail.

A. *A Screening Model with Endogenous Screening Capital*

In this section, I outline a simple screening model consistent the empirical results above, building on the canonical Phelps (1972) model of statistical discrimination. A more detailed exposition is given in Appendix A. 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 may 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.[39]

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. This 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, Smith-Lovin, and Cook 2001). Alternatively, workers may be better able to screen candidates from their own group (Giuliano, Levine, and Leonard 2009; Åslund, Hensvik, and Skans 2014).[40] In those cases, the employer may be

[39] 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, Raphael, and Stoll (2006) and Wozniak (2015) argue that the use of criminal background checks and drug tests increases black hiring by providing information that is perceived to be more relevant for black candidates. Autor (2001) 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.

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

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.

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. Given the employer-specific nature of many screening investments, this condition seems plausible. Alternatively, changes in screening practices may be sticky if it is costly to revert.

B. *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. Consistent with the first prediction, Holzer and Neumark (2000b) find 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. Here, I provide support for the second prediction using EEO-1 data and additional establishment-level survey data.

In particular, I show that an alternative source of variation in screening method use, *employer size*, also predicts more equalized group representation among employees. Prior research has documented that larger employers spend more time screening and use more screening methods (Barron, Black, and Loewenstein 1987; Holzer 1987; Marsden 1994; Holzer and Neumark 2000b). Using EEO-1 data, I find that black share is increasing in employer size, including within employers and jobs. By contrast, and consistent with screening investments reducing between-group disparities, I find that black share is *decreasing* for black-run businesses using another dataset.

These results should not be interpreted as causal estimates for the effects screening methods. Nonetheless, they are consistent with the screening model, and suggest that screening capital may be an empirically relevant channel.

Employer Size and Black Share.—If regulated employers increase their black share of employees in part by investing in screening capital, then other employers that invest in screening capital for other reasons should also see reductions in between-group differences in hiring rates. In particular, if most of these firms are less able to infer the quality of black candidates prior to their investment, as assumed in the model, then screening investments should lead to increases in black share. While I do not observe screening methods in the EEO-1 data, prior work has documented that larger employers also spend more time screening and use more screening methods, particularly formal methods. Hence, as another test of the model, I examine whether employer size predicts an employer's black share.

Prior work identifies two possible reasons for the relationship between employer size and screening. First, many screening methods may be cheaper for larger employers on a 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, Black, and Loewenstein 1987).[41] Critically, under either explanation larger employers invest more in screening for reasons unrelated to preferences for workforce diversity.

Holzer (1998) documents a positive cross-sectional relationship between establishment size and black share using data from the Multi-City Study of Urban Inequality, a sample of about 3,200 employers in 4 metropolitan areas.[42] I build on this work by examining the same relationship using EEO-1 data, panel data that follows a substantially larger set of establishments. The panel feature of the data is important, as some (relatively) fixed employer characteristics—like location or product market—may be correlated with employer size and black share for reasons unrelated to screening practices (Holzer 1998). By including establishment fixed effects, I can remove variation deriving from fixed differences across employers. Moreover, all establishments in these data are covered by workplace discrimination laws. Hence EEO law coverage alone cannot explain any relationship found here.

I estimate models of the form

$$\text{(5)} \quad \text{black share}_{it} = \alpha_i + \lambda_{d(i),t} + \beta^e \log(\text{est. size})_{it} + \beta^f \log(\text{firm size})_{it} + \epsilon_{it}.$$

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 Table 4. All models include census division-by-year fixed effects. Columns 2 and 3 include establishment fixed effects. Columns 4 and 5 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 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 percent increase in establishment size predicts a 0.06 percent percentage point increase in the black share of employees within jobs,

[41] Indeed, this may be why large employers are large in the first place. Alternatively, larger employers may face higher monitoring costs (Barron, Black, and Loewenstein 1987). This may also increase the return to worker quality, defined appropriately (e.g., work ethic).

[42] I describe these data in further detail below.

Table 4—Employer Size and Black Share

Outcome: Black share				Within-job	
	(1)	(2)	(3)	(4)	(5)
log establishment size	1.434	1.659	1.617	0.666	0.568
	(0.027)	(0.035)	(0.037)	(0.025)	(0.027)
log firm size			0.104		0.227
			(0.028)		(0.025)
Establishment FEs		✓	✓	✓	✓
MSA by year FEs	✓	✓	✓	✓	✓
R^2	0.230	0.927	0.927	0.872	0.872

Notes: Each column reports the coefficient estimates for a regression, with standard errors in parentheses clustered at the establishment level. The outcome variable is the percent black of an establishment's employees (or, as in columns 4 and 5, the percentage 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.

while a 10 percent increase in *firm* size predicts a 0.02 percent percentage point increase in the black share within jobs. Hence, the relationship is primarily *establishment* level. In the online Appendix, I show that this relationship is present across the business cycle and among both contractors and non-contractors.

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.

Unfortunately, EEO-1 data does not identify the race of business owners or hiring managers. Instead, I use data from the Multi-City Study of Urban Inequality (MCSUI), the same data used in Holzer and Neumark (2000b).

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 about 3,200 employers. The survey was conducted over the phone with individuals responsible for hiring at each establishment. The content focused on establishment and employee characteristics, including establishment size and whether the establishment practices AA,[43] the race of the hiring manager and the racial composition of employees. Information on the hiring manager and racial composition of employees refers only to positions that do not require a college degree.

[43] Holzer and Neumark (2000b), the former author and 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.

TABLE 5—HIRING MANAGER RACE, SIZE, AND BLACK SHARE

Outcome: Black share	(1)	(2)	(3)
log size × white-run	2.504	2.541	2.626
	(0.388)	(0.383)	(0.386)
log size × black-run	−5.385	−5.419	−4.114
	(1.444)	(1.561)	(1.444)
1-digit industry by race FEs		✓	
2-digit industry by race FEs			✓
MSA by CC by race FEs	✓	✓	✓
Number of white-run establishments	2,166	2,124	2,124
Number of black-run establishments	198	190	190
R^2	0.195	0.199	0.253

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). The labels “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.

I assess whether the size-black share relationship is of opposite sign for black-run businesses. I estimate models of the form

$$\text{black share}_i = \sum_{j\in\{B,W\}} \alpha^j_{s(i)} + \lambda^j_{m(i)} + \beta^j \times \log(\text{size})_i + \epsilon_i, \tag{6}$$

where i indexes establishments, j indexes whether the establishment’s hiring manager is white or black, $\alpha_{s(i)}$ are industry fixed effects, $\lambda_{m(i)}$ are MSA by central city fixed effects, a *size* is the number of employees at the establishment.

The results are presented in Table 5. 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.

IV. Discussion

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 significantly increases an establishment’s black share of employees, with the share continuing to increase over time. This response is strikingly asymmetric: even after establishments are deregulated, their black shares *continue to grow*. I show that this persistence is unlikely to be an artifact of anticipatory behavior.

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.

I argue that this persistence is in part due to employer changes in screening practices. Regardless of the mechanism, the fact that a temporary intervention has long term effects on a given employer's trajectory suggests that there exist multiple equilibria for the racial composition of its workforce. The results do not necessarily imply the existence of any market inefficiency. However, if there exist multiple equilibria, then basic market forces may not ensure that the most efficient worker allocation is reached.

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.

Mathematical Appendix

A. *A Screening Model with Endogenous Screening Capital*

Suppose an employer must fill a mass n of vacancies from a mass $a(n) > 2n$ of applicants.[44] The wage is fixed.[45] There are two groups of candidates: $\gamma \in \{B, W\}$. Let π_γ denote the share of applicants from group γ. 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 γ they observe a noisy signal for match productivity

$$s_i^\gamma = \theta_i + \epsilon_i^\gamma,$$

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

Conditional on group γ and signal s, the expected productivity for a given applicant is

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

[44] 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.

[45] Suppose, for example, that the employer commits to a posted wage. This assumption follows both Cornell and Welch (1996) and Morgan and Vardy (2009).

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,

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

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 the hiring threshold μ^* 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 γ. Under an assumption often made in the statistical discrimination literature, we can sign the difference in hiring rates.

PROPOSITION A1: *Suppose*

$$\mu_\theta(B) \;\leq\; \mu_\theta(W) \tag{A1}$$

and

$$h_B \;<\; h_W. \tag{A2}$$

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

In other words, if one group can be screened more precisely (and members are not less productive on average), that group will have an advantage at hiring. Justifications for this assumption are discussed in Section IIIA.

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_i^k, where

$$s_i^k \;=\; \theta_i + \epsilon_i^k,$$

where $\epsilon_i^k \;\sim\; N\left(0,\frac{1}{k}\right)$. 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.[46]

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,

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

Under the conditions from Proposition A1 these investments will reduce hiring rate disparities.

PROPOSITION A2: *Suppose* (A1) *and* (A2) *hold. Then,*

$$\lambda(W,k) - \lambda(B,k) > 0$$

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

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

Denote the employer's solution by k^*.

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: μ_B^* and μ_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

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

[46] A richer model could allow the employer to choose among capital that provides signals more informative for one group than the other.

PROPOSITION A3: *Suppose* (A1) *and* (A2) *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.

B. *Proofs*

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

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

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

$$\begin{aligned} \lambda(\gamma) &= 1 - \Phi(z^*(\gamma)) \\ &= 1 - \Phi\left(\frac{\mu^* - \mu_\theta(\gamma)}{\sqrt{\frac{1}{h_\theta}\frac{h_\gamma}{h_\theta + h_\gamma}}}\right). \end{aligned}$$

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:[47] *Suppose* (A1) *and* (A2) *hold. Then,*

$$\lambda(W, k) - \lambda(B, k) > 0$$

[47] The proof is similar to claim III.D1 in Autor and Scarborough (2008).

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,

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

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

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

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* (A1) *and* (A2) *hold. Then* k^* *is larger under affirmative action.*

PROOF:

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,

$$\pi_\gamma \int_{\mu_\gamma^*(k)}^{\infty} \mu \, dF(\mu \,|\, \gamma, k) = \pi_\gamma \int_{F(\mu_\gamma^* | \gamma, k)}^{1} Q(p \,|\, \gamma, k)\, dp$$

$$= \pi_\gamma \int_{\alpha}^{1} Q(p \,|\, \gamma, k)\, dp,$$

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

$$\begin{aligned}\Delta &= \pi_B \int_{\alpha}^{1} Q(p \mid B, k)\, dp + \pi_W \int_{\alpha}^{1} Q(p \mid W, k)\, dp - \pi_B \int_{\alpha_B}^{1} Q(p \mid B, k)\, dp \\ &\quad - \pi_W \int_{\alpha_W}^{1} Q(p \mid W, k)\, dp \\ &= \pi_B \int_{\alpha}^{\alpha_B} Q(p \mid B, k)\, dp - \pi_W \int_{\alpha_W}^{\alpha} Q(p \mid W, k)\, dp.\end{aligned}$$

Note that

$$Q(p \mid \gamma, k) = \mu_\theta(\gamma) + \sqrt{\frac{1}{h_\theta} \frac{h_\gamma + k}{h_\theta + h_\gamma + k}}\, \Phi^{-1}(p)$$

and so

$$\frac{\partial Q(p \mid \gamma, k)}{\partial k} = \sqrt{\frac{1}{h_\theta}}\, \Phi^{-1}(p) \frac{\partial}{\partial k}\left(\frac{h_\gamma + k}{h_\theta + h_\gamma + k}\right) > 0,$$

which is decreasing in h_γ.

Hence,

$$\begin{aligned}\frac{\partial}{\partial k}\Delta &= \frac{\partial}{\partial k}\left[\pi_B \int_{\alpha}^{\alpha_B} Q(p \mid B, k)\, dp - \pi_W \int_{\alpha_W}^{\alpha} Q(p \mid W, k)\, dp\right] \\ &> \frac{\partial}{\partial k}\left[\pi_B \int_{\alpha}^{\alpha_B} Q(p \mid B, k)\, dp - \pi_W \int_{\alpha_W}^{\alpha} Q(p \mid B, k)\, dp\right] \\ &> 0,\end{aligned}$$

where the first inequality holds because $\frac{\partial Q(p \mid \gamma, k)}{\partial k}$ is decreasing in h_γ and the second inequality holds because $\frac{\partial Q(p \mid \gamma, k)}{\partial k}$ is increasing in p. ∎

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