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Is Affirmative Action in Employment Still Effective in the 21st Century?

Noriko Amano-Patiño* Julian Aramburu† Zara Contractor‡

We are bounded by law to not disclose details of our findings until they are officially approved by the U.S. Census Bureau. This version of our draft (edited on October 31, 2022) does not reflect the current state of the project.

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Abstract

We study Executive Order 11246, an employment-based affirmative action policy targeted at firms holding contracts with the federal government. We find this policy to be ineffective in the 21st century, contrary to the positive effects found in the late 1900s (Miller, 2017). Our novel dataset combines data on federal contract acquisition and enforcement with US linked employer-employee Census data 2000–2014. We employ an event study around firms' acquiring a contract, based on Miller (2017), and find the policy had no effect on employment shares or on hiring, for any minority group. Next, we isolate the impact of the affirmative action plan, which is EO 11246's preeminent requirement that applies to firms with contracts over $50,000. Leveraging variation from this threshold in an event study and regression discontinuity design, we find similarly null effects. Last, we show that even randomized audits are not effective, suggesting weak enforcement. Our results highlight the importance of the recent budget increase for the enforcement agency, as well as recent policies enacted to improve compliance.

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DISCLARIMERS: 1) Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 1787. (CBDRB-FY22-P1787-R9346). **2)** Julian Aramburu's contribution to this paper was prior to joining Amazon. The views expressed in this paper are the author's own and do not represent the views, position, or opinions of Amazon nor any of its staff members.

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1 Introduction

Racial disparities in employment and earnings in the U.S. are sizable. The unemployment rate for Black workers is, and persistently has been, about twice that of whites,[1] and the average median weekly earnings for Blacks is 80% that of whites (U.S. Bureau of Labor Statistics, 2022). Between one-third and one-half of this earnings gap cannot be explained by worker or firm characteristics (Cajner et al., 2017). Similarly, Hispanics face disparities with respect to whites.[2] Gender labor market differences are also substantial and persistent: women's wages oscillate around 83% among full time workers (U.S. Bureau of Labor Statistics, 2022). To combat these disparities, employer-based affirmative action policies in employment are intended to level the playing field for groups that have been historically discriminated against or overlooked in the workplace because of race, ethnicity, gender, or sexual orientation.

Executive Order 11246 (henceforth EO 11246) is the oldest and most widespread of these regulations in the U.S., covering about 20% of the labor force (GAO, 2017). Established in 1965, EO 11246 requires any firm holding a contract with the federal government to instate a set of "affirmative action" measures to reduce disparities among the *protected groups* it employs; where the protected groups refer to those groups of individuals who have been discriminated against or overlooked due to race, ethnicity, gender, or sexual orientation. An extensive literature studies this policy and its effects in the 20th century. The main finding is that EO 11246 increased the shares of employees of protected groups throughout that period (Miller, 2017; Kurtulus, 2016).

In this paper, we ask what the effects of EO 11246 are in the 21st century. To answer the question, we first construct a novel matched employer-employee dataset that includes information on establishments' regulation status. Following Miller (2017), we rely on an event study design to identify the effects of the policy on the proportion of the stock and the flow of workers from protected groups and find small and insignificant effects. Next, leveraging our

[1] This ratio improves only slightly in good economic times, and persists no matter the level of educational attainment (Cajner et al., 2017).

[2] The Hispanic-white median weekly earnings gap is 78%; 76% among men and 79% among women. Though their employment rate is higher than that of whites, their unemployment is higher as well. Asian American and Pacific Islanders are the exception among minority groups with a higher average median weekly earnings and an unemployment rate that is typically below that of whites. See https://fredblog.stlouisfed.org/2022/05/comparing-unemployment-rates-by-race-the-great-recession-vs-covid-19/?utm_source=series_page&utm_medium=related_content&utm_term=related_resources&utm_campaign=fredblog. However, these averages paint an incomplete picture: Asian American and Pacific Islanders in the top 10% of income distribution earn almost 11 times as much as those at the bottom. See https://www.pewresearch.org/social-trends/2018/07/12/income-inequality-in-the-u-s-is-rising-most-rapidly-among-asians/).

rich data, we isolate the impact of the "affirmative action plan" requirement from the less stringent anti-discrimination requirements of EO 11246. We employ an event study and regression discontinuity design around the requirement's contract value threshold and find that this more stringent requirement also has small and insignificant effects. Finding no impact, we turn to investigate whether the audits carried out to enforce the policy have any effect on either the stock or the flow of workers from protected groups. Again, we find small and insignificant effects.

We make four contributions to the empirical literature on affirmative action in employment. First, to the best of our knowledge, ours is the first paper to study the effects of EO 11246 in the 21st century.[3] Second, the dataset we build, to the best of our knowledge, is the first one containing matched employer-employee data alongside establishment-level information on federal contract acquisition and enforcement. As such, we are the first to leverage this level of information to analyze the effects of EO 11246 on the proportion of new hires and log-wages. Third, we leverage exogenous variation from the affirmative action plan requirement's threshold to improve identification and isolate the impact of this additional requirement from the less stringent anti-discrimination requirements of EO 11246. Fourth, we turn to data on audits and local minority populations to investigate what drives the observed null effects. We find that the null effects are not driven by the regulation no longer binding,[4] but instead by lack of compliance.

We build our data linking together workers' histories in 19 states (from the Longitudinal Employer-Household Dynamics, LEHD)[5] to data on their employers EO 11246 regulation status (from the Federal Procurement Data System) and audit data from the enforcement agency, the Office of Federal Contract Compliance Programs (OFCCP). Because of the size of the LEHD, the lack of common identifiers across these datasets presents a computational challenge. We develop a two-step fuzzy string matching algorithm based on term frequency-inverse document frequency (tf-idf) scores of both names and addresses of each establishment in the LEHD. With our novel matched employer-employee data assembled, we turn to analyze the causal effects of EO 11246 on several different outcomes of the protected groups.

[3] Although both Miller (2017) and Kurtulus (2016) include a few years from the 2000's decade in their analysis (2000–03 and 2000–04 respectively), their data covers mostly the 80's and 90's and less than 16% of their data overlaps with the 2000's decade.

[4] (Currently qualitative) evidence that EEO 11246 also does not have effects for firms who under-employ minority groups

[5] Individual states approve project proposals upon Census approval of the proposal. Our project has close to the average number of states that other researchers using the LEHD have (23). We have access to this data for 19 states: Arizona, Arkansas, Colorado, District of Columbia, Delaware, Hawaii, Indiana, Iowa, Kansas, Maine, Maryland, Montana, Nevada, New Mexico, Oklahoma, Pennsylvania, Tennessee, Washington and Wisconsin. See Appendix Figure G.2.

Using a staggered event study design based on Miller (2017), we estimate the impact of becoming a federal contractor — and therefore becoming regulated by EO 11246. We use the Callaway and Sant'Anna's (2021) estimator,[6] and find quantitatively small effects that would translate into at most a 0.653% increase[7] in the share of minority workers — these effects are smaller when we look at each protected group separately. Moreover, the estimates are not significant even at the 90% level. It is possible that even if we do not see an effect in the stock of workers, there could be an effect in the flow of workers. To see if this is the case, we measure the effects of the regulation on the proportion of new hires who belong to these groups. We find similarly negligible effects on the proportion of new hires from protected groups (in this case, the effect would translate into an increase of at most 0.948% and it is insignificant even at the 90% confidence level). In spite of these null effects in establishment-level outcomes, at the worker-level, EO 11246 could operate by inducing firms to increase wages to retain workers from protected groups. Thus, we also measure the effects of EO 11246 on the log-wages of protected and non-protected workers.[8] We show that our estimates are qualitatively robust to using a naive TWFE estimator.

Next, we analyze the effect of a more stringent margin of EO 11246: the requirement to have an Affirmative Action Plan (AAP) for firms (with 50 or more employees) holding a federal contract with a value of $50,000 or more. An AAP is a report that identifies underrepresentation of workers in the protected groups in the different occupations there are in each establishment of the firm[9] relative to their availability in the *relevant labor market*, and detail strategies, goals, and timelines for eliminating such underrepresentation.

Studying the effects of the AAP requirement is important for two reasons. First, it strengthens our identification strategy. There is evidence that the single fact of becoming a contractor affects hiring decisions generated by the need to complete the project, service, or product awarded to the company (Kroft et al., 2020).[10] Moreover, Aizer et al. (2020) show that contracts with the government (in their case, war contracts) could affect the employment of protected groups even

[6] In contrast with the standard TWFE estimator of event study designs, Callaway and Sant'Anna's (2021) staggered Diff-in-Diff estimator remains consistent under multiple dimensions of treatment heterogeneity — including dynamic treatment effects, — and selection into treatment based on covariates.

[7] This is the upper bound of the 95% confidence interval of the highest estimate (see Table D.3).

[8] The magnitudes of these effects have not undergone Census Disclosure.

[9] The occupation groups considered are: 1. Officials and managers, 2. Professionals, 3. Technicians, 4. Sales workers, 5. Administrative support workers, 6. Craft workers, 7. Operatives, 8. Laborers/helpers, 9. Service workers.

[10] Kroft et al. (2020) study the construction industry, and find that winning a federal contract increases sales by 17%, expenditure on intermediate inputs by 15%, and the wage bill by 10%. The 10% increase in the wage bill is due to an 8% increase in the number of employees and a 2% increase in earnings per employee.

in the absence of EO 11246 (which was not in place in the time frame studied by Aizer et al. (2020)). Thus, the 'becoming a contractor' indicator may be conflating EO 11246 effects with the direct effects of acquiring a contract with the government. The causal effect of EO 11246 is typically recovered by measuring changes taking place upon acquiring a federal contract. We circumvent the threat of confounding the effect of the policy from the effect of becoming a contractor by exploiting the $50,000 value threshold of federal contracts that we observe in our data. Specifically, we compare federal contractors above and below this threshold using both an event study, and a regression discontinuity design,[11] improving on the identification strategy that was available in the EO 11246 literature.

Second, the AAP requirement of the regulation requires an *active effort* by federal contractors to comply with the policy. Specifically, coming up with an AAP and maintaining it, requires establishments to determine the racial composition of their personnel, distinguishing the availability of minorities in the labor market where it recruits, and defining goals and strategies to reduce underrepresentation in case the establishment is below the targets set by the policy. As such, the AAP requirement could induce a change on the hiring strategies of regulated establishments (McPherson et al., 2001; Giuliano et al., 2009). So there could be a positive effect of the AAP on the shares of workers of protected groups or the shares of new hires of protected groups among the set of establishments subject to this requirement, even after finding no results on the overall universe of contractors subject to the broader requirements of the policy.

Using both an event study and a regression discontinuity design, we do not find a causal effect of being subject to the AAP requirement on neither the share of workers nor new hires from protected groups. Our Callaway and Sant'Anna's (2021) estimates are quantitatively small and non-significant (even at the 90% confidence level) and would translate into at most a 0.324% increase[12] in the share of minority workers. The regression discontinuity estimates have not yet gone through Census disclosure. However, our disclosed qualitative results are in line with our findings from the event study design: we detect no significant effect of being subject to EO 11246 with an AAP requirement on the share of minority workers. The effects on the proportion of new hires from protected groups and the earnings of incumbent workers in these groups are similarly small[13] with both designs.

Finally, we turn to investigate whether EO 11246 is still binding and what the role of the

[11] We do not exploit the size requirement of 50 or more employees because several worker related regulations kick in at the 50 employee threshold, and this could contaminate the identification of the causal effect of affirmative action. Section 5.2 provides more details on this.

[12] This is the upper bound of the 95% confidence interval of the highest estimate (see Table E.3).

[13] The upper bound of the 95% confidence interval of the highest estimate for minorities in this case is 1.171% (see Table E.3).

enforcement measures is in explaining the lack of effects of EO 11246. EO 11246 requires regulated firms to identify underrepresentation of workers from protected groups. Thus, a firm without underrepresentation would not be expected to undergo any measurable changes. Using ACS data to reconstruct the benchmarks that firms use to compare their workforce composition and identify underrepresentation, which we use to separate the subsample of firms that are 80% below their benchmark. Reestimating the event study on this subsample of firms, we fail to identify any significant effects of the regulation on the share of workers and of new hires from protected groups.

Enforcement of EO 11246 is under the authority of the Office of Federal Contract Compliance Programs (OFCCP), which audits a sample of about 1% of all federal contractors each year. GAO (2017), a congressional report produced by the Government Accountability Office regarding compliance evaluations by the enforcement agency during 2000–2015 highlights the unpredictability of the compliance evaluations it performs from the firms' standpoint. Under the identifying assumption that, from the firms' perspective, these evaluations are random, we can identify the effect of audits on the proportion of workers in protected groups using an event study design similar to the ones described above. Our Callaway and Sant'Anna's (2021) estimates continue to be quantitatively small that would translate, in this case, into at most a 0.404% increase[14] in the share of minority workers. Again, these estimates are not significant even at the 90% level. We find similarly negligible effects on the proportion of new hires from protected groups and, qualitatively, we also find insignificant effects in the earnings of incumbent workers in these groups.[15]

We attribute part of the decreased success of the enforcement measures to the decrease in the budget allocated to the OFCCP. The OFCCP has seen their employees shrunk by 28% from 2001 to 2010. This decline has, in their own account, "resulted in a significant decrease of activity in some central areas related to compliance evaluations" (OFCCP (2010), p. 46). In conjunction with this, our results highlight the importance of the recent budget increase for the enforcement agency, as well as recent policies enacted to improve compliance.

1.1 Related Literature

We contribute to a long-standing body of literature that evaluates the effects of EO 11246. The first empirical evidence dates back to the 1970s. Ashenfelter and Heckman (1974) and Heckman and Wolpin (1976) are the first two studies to establish a positive effect of EO 11246 on the share

[14] This is the upper bound of the 95% confidence interval of the highest estimate (see Table F.3).

[15] All our estimates are qualitatively robust to using a naive TWFE estimator.

of Black workers hired by regulated establishments. The papers use EEO-1 records for the years between 1966 and 1973, and use a regression framework approach together with comparison in means of Black male employment growth[16] between establishments with and without federal contracts. Both papers find that Black male employment increased more relative to white male employment in contractor establishments during the years considered in the data. Smith and Welch (1984) and Leonard (1984) also document positive effects of the policy when analyzing data until 1980. Interestingly, Heckman and Wolpin (1976) and Leonard (1984) match EEO-1 records with audit records from the regulating agencies for the years 1974–1980, and both papers show that the share of Black workers is higher in audited contractors relative to non-audited ones. The authors argue that compliance reviews are an effective regulatory tool in increasing Black employment in regulated establishments.

Leonard (1990) stresses the importance of regulation compliance by extending the analysis to the 1980s, during the Reagan's Presidency (1981–1989). He shows that the results in his previous paper, Leonard (1984), which correspond to a period before 1980, completely disappear in the 1980s. A similar result is found by Kurtulus (2012), who uses EEO-1 data from 1973 to 2003. She finds that the share of Black workers grew more at contractor firms than at non-contractors, and also in line with Leonard (1990), finds that the positive effects she observes in the data took place primarily before and after the Reagan's Administration, with a marked decay in the 1980s. Both Kurtulus (2016) and Leonard (1990) suggest that the dismantling of the enforcement agency, the Office of Federal Contract Compliance Programs (OFCCP), during the Reagan Administration is responsible for the decrease in the effects of EO 11246 during the 1980s. Starting in 1980, the OFCCP reduced the audits conducted, and back-pay awards and the already rare penalty of debarment from federal contracts were phased out (Leonard, 1987).

The most recent paper in this literature is Miller (2017), which is an important contribution by being the first one to acknowledge the dynamic effects of the regulation, particularly for regulated employers who were subject to affirmative action in the past. Leveraging EEO-1 records from 1978 to 2004, the paper finds that EO 11246 has a positive and significant impact on the share of Black workers that an establishment employs. Moreover, it shows that the share of Black workers continues to grow even after an employer is deregulated. Last, and in line with Kurtulus (2016) and Leonard (1990), Miller (2017) finds that the positive effects of the policy are primarily driven by after Reagan-era years, when the enforcement strength of the policy was restored.

[16] The authors do not explicitly state why they focus on male workers only. As explained in Section 2, EO 11246 added the category of "sex" to the anti-discrimination provisions in an amendment in 1967.

This paper advances the previously described literature twofold. First, we extend the analysis of the effects of EO 11246 to the 2000s — we focus on 2001–2014. The previous literature focuses their analysis mostly on the 1980s and 1990s. To put our period of analysis into historical and political perspective, our data covers the Clinton (1993–2001), Bush (2001–2009) and most of the Obama administration (2009–2017). While the 1990s were characterized by a strong increase in enforcement activities from the OFCCP (Kurtulus, 2016; Holzer and Neumark, 2000), the 2000s were characterized by yet another decline and defund of enforcement activities (OFCCP, 2010; GAO, 2017).

Second, and importantly, a relevant contribution of this paper is, to the best of our knowledge, to construct and analyze the first large-scale, administrative dataset containing establishment- and worker-level information together with federal contract information of the employers. These data allow us to improve on the empirical analysis of the effects of the policy by considering different margins of effects as well as cleaner identification strategies.

The rest of the paper is organized as follows. We present the details and institutional background of EO11246 in Section 2. Section 3, presents details of our data. Sections 4, 5, and 6, present our empirical strategies and results. Section 7 concludes.

2 Institutional Setting

EO 11246, signed by President Lyndon B. Johnson in 1965, mandates that firms that hold contracts with the federal government (which we refer to as *federal contractors*) make active efforts to prevent discrimination in their hiring and employment on the basis of race, color, religion, sex, sexual orientation, gender identity or national origin.[17]

EO 11246 stipulates that firms with 50 or more employees holding a federal contract with a value of $50,000 or more must create a yearly Affirmative Action Plan (AAP) for each of their establishments. Such plans identify underrepresentation of minorities and women in any occupation group[18] relative to their availability in the *relevant labor market*.[19] As part of the plan, contractors are required to make "good faith" efforts to rectify underrepresentation of

[17] In its original version, EO 11246 required firms to make active efforts to prevent discrimination on the basis of race, color, religion, and national origin. In 1967, President Lyndon B. Johnson amended it by adding the category "sex" to the anti-discrimination provisions. Most recently, in 2014, President Barack Obama amended it, adding the category "sexual orientation and gender identity."

[18] The occupation groups considered are: 1. Officials and managers, 2. Professionals, 3. Technicians, 4. Sales workers, 5. Administrative support workers, 6. Craft workers, 7. Operatives, 8. Laborers/helpers, 9. Service workers.

[19] We define *relevant labor market* in the next paragraph.

minorities, including the use of numerical goals with timetables.

In order to monitor compliance, the Office of Federal Contract Compliance Programs audits the affirmative action plans of a small fraction of contractors each year. Underrepresentation is said to arise when the incumbency of workers in a protected group is below 80% of its availability in the relevant (and qualified) labor market. Where the "relevant labor market" of an establishment is defined as the geographic area where the establishment can reasonably recruit candidates for each of the nine job groups considered.

To create the goals and timetables required, firms compare their workforce racial, ethnic, and sex composition to the EEOC's benchmark, the *EEO-Tabulations*,[20] which are based on 5-year ACS data. These tabulations are available at the location-industry-occupation level,[21] however, the LEHD contains no information on the workers' occupations.

For example, if a firm's AAP shows that Black workers are underrepresented in managerial roles, the firm could choose to explore new recruitment channels to fill managerial vacancies or implement policies that ensure Black workers are not overlooked for promotions. The AAP must be submitted within 120 days of the start of the federal contract, and it must be updated annually for as long as the firm is a contractor.

EO 11246 also includes additional anti-discrimination policies, which apply to a broader set of firms. All federal contactors with at least 25 employees and a contract with a value of $10,000 or more over any 12 month must display EEOC posters in their websites and premises,[22] include equal opportunity taglines in every job advert postings,[23] and keep records of applications, hiring, promotions and terminations available for review.

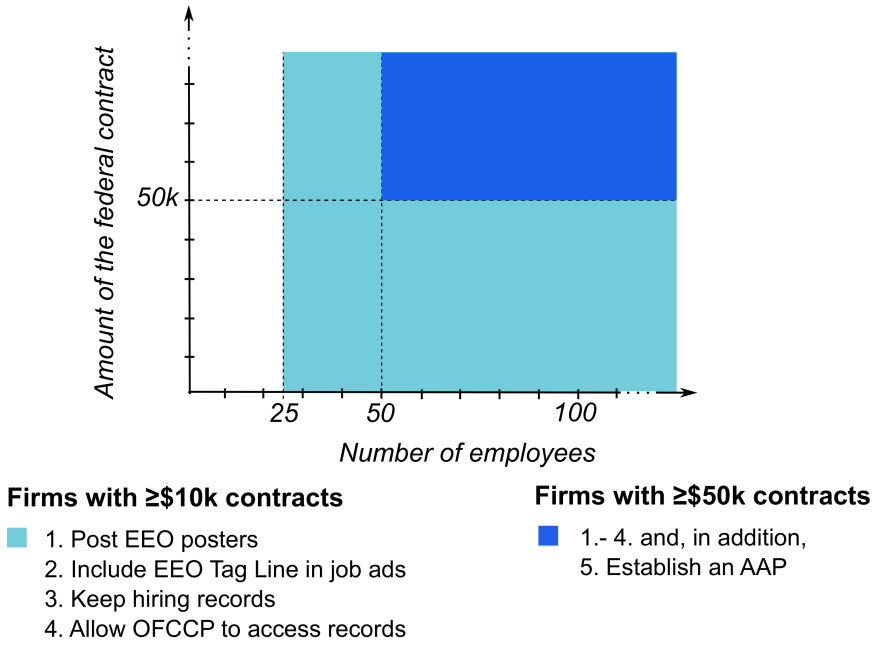
Enforcement of EO 11246 is under the authority of the Office of Federal Contract Compliance Programs (OFCCP). To monitor compliance, the agency evaluates a sample of about 1% out of all federal contractors each year. These evaluations consist of a desk audit and a possible visit. During an audit, the OFCCP checks whether all relevant affirmative action requirements have been implemented and also reviews the establishment's records to determine any signs of discrimination in recruiting, hiring, salaries, and promotions. For firms that are required to have an AAP, the OFCCP evaluates whether the plan is sufficient to address its needs, and whether the firm has made good faith efforts to implement it.

[20] These tabulations are publicly available. See more information at https://www.census.gov/topics/employment/equal-employment-opportunity-tabulation/about/faq.html.

[21] See https://www2.census.gov/EEO_2006_2010/EEO_2006_2010_Tables_Nation(010)_CSV/.

[22] See Appendix Figure G.1 for an example of such posters.

[23] Until 2014, taglines read *"We are an equal opportunity employer. All qualified applicants will receive consideration for employment without regard to race, color, religion or sex."* From 2014 onwards, Obama's amendment modified the taglines to include sexual orientation, gender identity, and national origin considerations



If the audit finds that a contractor is not in compliance, then the OFCCP requests a letter of commitment, or seeks a conciliatory agreement. In rare cases, agreements may include financial settlements that involve back pay to alleged individual victims of discrimination. If the OFCCP and a contractor fail to resolve violations, the OFCCP may take legal actions to penalize the contractor. The most severe punishment that the OFCCP can impose is to debar the firm from doing business with he federal government, but this outcome is extremely rare and happens only in 0.009% of all audits (Pincus, 2003).

Importantly, the OFCCP does not influence which firms become federal contractors. The allocation of federal contracts depends on the contracting agency only. Therefore, the allocation of federal contracts is not affected by the racial composition of a firm. The one exception is contracts of $10 million or more, for which firms are subject to a pre-award compliance evaluation. We thus exclude contracts of $10 million or more.

3 Data

We create our dataset by merging four complementary data sources: federal contracts' data from the Federal Procurement Data System, audit data from OFCCP compliance evaluation and complaint investigation records, matched employer-employee data from the US Census Bureau, and the qualified labor shares of each protected group by county from the American Community

Survey. We first describe each data source, and then outline how we construct our dataset and discuss how our data presents advantages relative to the data used by previous literature.

3.1 Data Sources

3.1.1 Federal Procurement Data System

The Federal Procurement Data System (FPDS) data is publicly available data[24] containing the universe of federal contracts that exceed $2,500[25] available from 2001 onward.

An observation in this dataset is a "contract action." That is, either an initial award or a subsequent action — such as a modification, termination, renewal, or exercise of options. Each of these observations has detailed information about the contract, such as the dollar value of the funds obligated by the transaction; a four-digit code describing the product or service purchased; the codes for the agency, sub-agency, and contracting office making the purchase; the identity of the private vendor; the type of contract pricing (typically, fixed-price[26] or cost-plus[27]); the extent of competition for the award; characteristics of the solicitation procedure; the number of offers received; and the applicability of a variety of laws and statutes. To make the contract the unit of observation, we collapse all actions by contract ID.

The data also contain establishment-level information of the contractor firm such as its name, DUNS number, location, industry, and whether the establishment is Black- or minority-owned business.

3.1.2 OFCCP audit records

The OFCCP evaluates a random sample of only about 1% out of all contractors each year. Compliance evaluations consist of the agency evaluating whether all the affirmative action practices are in place among federal contractors. These include: record keeping, no evidence of discrimination in recruiting, hiring, salaries, and promotions. The evaluations are usually targeted towards larger contractors for which an AAP is applicable (Miller, 2017; GAO, 2017). In these cases, as mentioned in the introduction, the OFCCP evaluates the plan in detail with the aim of

[24] The data can be downloaded from https://usaspending.gov.

[25] Information on contracts below $2,500 (called micro-contracts) is not publicly available.

[26] I.e. a lump-sum contract in which the firms state the goods or services they will provide and establish the price that the government has to pay for them.

[27] Typical of construction contracts, these type of contracts specify both the cost of the goods or services to be provided together with a fee for the firm's overhead and profit.

determining whether the AAP is indeed in place, whether it is sufficient to address its needs, and whether the firm has made efforts to implement it.

Through a freedom of information request (number 2021-F-09671), we obtained data on closed compliance evaluations and complaint investigations, conducted by the OFCCP going as far back as possible.

The complaint investigations' data contain all complaints filed by the EO 11246, the Rehabilitation of 1973, the Vietnam Veterans Readjustment Act, or other investigative authorities to the OFCCP from 2004 to 2014. The data include the plaintiffs' base for their allegation[28] and the violation found by the OFCCP. Unfortunately, we don see the outcomes of the allegations; that is, we do not observe the type of agreement that is reached between the OFCCP and the firm in case the latter is found to be in violation of any of the criteria that the OFCCP investigates.[29]

The compliance evaluations' data contain all compliance evaluations performed by the OFCCP from 2000 to 2014. They also contain the number and type of violations of each of the evaluated establishments[30] and whether the OFCCP deems the establishment to be compliant or not with EO 11246.

We refer to these data as *OFCCP data*.

3.1.3 Matched Employer-Employee Data: LEHD-LBD-SSEL

The Longitudinal Employer-Household Dynamics (LEHD) is a quarterly matched employer-employee panel dataset constructed from unemployment insurance records and, at the time of this writing, available from 2001 to 2014. The dataset, which we assemble following Sorkin (2018),[31] is composed of three core files: the Employer Characteristics File (ECF), the Employment History File (EHF), and the Individual Characteristics File (ICF).

The ECF consolidates most employer and establishment-level information (size, age, location, and industry).

The EHF stores the complete in-state work history via an employer-employee combina-

[28] The bases of the complaints are categorized as discrimination on the basis of being: *1)* Black, *2)* women, *3)* other, *4)* of Hispanic national origin, *5)* of color, *6)* men, *7)* of foreign national origin, *8)* disabled, *9)* veteran, *10)* religion, *11)* Asian pacific, *12)* white, or *13)* American Indian or Alaskan.

[29] Namely, violations in terms of hiring, promotion, demotion, termination, layoff, recall, wages, seniority, harassment, job benefits, segregated facilities, retaliation, pregnancy leave policy, accommodation disability, religious day observance, or other.

[30] The possible violations are classified into the following categories: *1)* No record keeping, *2)* Recruitment violation, *3)* Past performance violation, *4)* Other violation, *5)* No AAP, *6)* Hiring violation, *7)* Salary violation, *8)* Accomodation violation, *9)* Selection or testing violation, *10)* Termination violation, *11)* Promotion violation, and *12)* Systemic discrimination.

[31] See description of the construction of the data in Appendix B.1.

tion for each individual that appears in the unemployment insurance records. Quarterly labor earnings[32] are present in this file.

Finally, the ICF includes the socio-demographic characteristics (age, gender, race,[33] ethnicity,[34] place of birth, and citizenship status) for each person who is ever employed in a state over the time period spanned by the state's unemployment insurance records. Although coverage by the LEHD varies by state, on average 96% of all private-sector jobs in the U.S. are covered. More information about the LEHD data is available in Abowd et al. (2009) and Vilhuber et al. (2018). We have access to this data for 19 states.[35]

Once we merge the LEHD into a single file, we link to it the Standard Statistical Establishment Listing (SSEL) and the Longitudinal Business Database (LBD) data. These data contain the names and addresses of the establishments in the LEHD, and we use these variables to match the LEHD to the rest of our data, as we explain in Appendix B.1.

The LEHD is a highly restricted database that can only be accessed at a Research Data Center (RDC) of the U.S. Census Bureau.[36] In order to protect the security and confidentiality of the data, any output coming from it is subject to strict disclosure reviews and procedures that are required before extracting and making results from the RDC public. All tables and figures in the remainder of the text were produced either with public data alone outside of the RDC, or based on disclosed data from the approved disclosure requests #8222, #9346, #9378, or #10092.

3.1.4 American Community Survey

Recall, from Section 2, that AAPs (short for *Affirmative Action Plans*) identify underrepresentation of the protected groups by comparing the diversity of their workforce with a benchmark. Such benchmark are the EEO-Tabulations, which are provided by the EEOC (see Section 2).

EEO-Tabulations are built using 5-year ACS data and contain racial[37] and gender composi-

[32] The unemployment insurance system measures earnings but not hours. The notion of earnings captured is as follows: "gross wages and salaries, bonuses, stock options, tips and other gratuities, and the value of meals and lodging." This omits the following components of compensation: "employer contributions to *Old-age, Survivors, and Disability Insurance* (OASDI); health insurance; unemployment insurance; workers' compensation; and private pension and welfare funds."

[33] The race variable in the ICF classifies individuals into the following racial groups: *1)* White Alone, *2)* Black or African American Alone, *3)* American Indian or Alaska Native Alone, *4)* Asian Alone, *5)* Native Hawaiian or Other Pacific Islander Alone, and *7)* Two or More Race Groups.

[34] The ICF includes a flag to indicate Hispanic or Latino individuals.

[35] Arizona, Arkansas, Colorado, District of Columbia, Delaware, Hawaii, Indiana, Iowa, Kansas, Maine, Maryland, Montana, Nevada, New Mexico, Oklahoma, Pennsylvania, Tennessee, Washington and Wisconsin. See Appendix Figure G.2.

[36] See https://lehd.ces.census.gov/data/.

[37] It also contains broad information on ethnicity. Specifically, it includes the proportion of Hispanic individu-

tion by occupation and industry of the local labor force.

However, since the LEHD do not include occupation information, we cannot link EEO-Tabulations to our data directly. One alternative to deal with this, would be to aggregate the EEO-Tabulations to the location-industry level. But this could lead to very coarse estimates of the numbers that firms use in practice to compare their labor force compositions and develop their AAP. Instead, we exploit the education information in the LEHD to generate our own tabulations of the available workforce in each location, defined by state and county, using ACS data (see Appendix B.1).

We find that our tabulations of the racial and gender composition of the local labor force by industry, and education approximate well those by industry and occupation.[38] We thus use tabulations that average the local racial and gender composition by industry of the local racial, ethnic, and gender composition across education levels as benchmarks, in order to classify firms into those above and below their corresponding targets.

3.2 Our novel matched employer-employee data

The core analysis of our data leverages a linkage between the LEHD-LBD-SSEL data described above and FPDS, and OFCCP data. We refer to the later linked files as the *OFCCP-FPDS data*. A key challenge in linking these data is that there are no common identifier between the LEHD-LBD-SSEL and the OFCCP-FPDS data. In addition, the size of the data does not allow to use simple matching based on strings of characters (such as the names and addresses of the establishments in our data).

We overcome this challenge by developing an efficient fuzzy matching algorithm that uses names and addresses to create a bridge between OFCCP-FPDS records and their corresponding counterpart in the LEHD-LBD-SSEL. We do this in two steps: first, conditional on the establishment's county, we find the best match in terms of its name. Second, we then find the best match in terms address and then keep only those instances in which the address satisfies a minimum threshold in the quality of the match. More details of this matching technique can be found in Appendix B.

Our linked data provide advantages relative to the EEO-1 Records: these are reports collected by the EEOC from firms meeting certain size requirements, as detailed below, which had been used by most of the literature studying the effects of EO 11246.[39] Federal contractor firms

als.

[38] See Appendix C.

[39] Among the few exceptions, are Holzer and Neumark (1999), Holzer and Neumark (2000a), Holzer and

with over 50 employees, and all firms with over 100 employees, are required to submit annual reports broadly describing the racial, ethnic and gender composition of its employees.[40,41] to the EEOC.

First, while EEO-1 Records data are not representative of all U.S. establishments due to size requirements for filling,[42] we have access to about 96% of the employment in the 19 states we have access to. Second, we observe worker-level data as opposed to the establishment-level information in the EEO-1 records. Third, by having access to the characteristics of the contracts as well as the size of the establishments, we are able to determine and impute whether a firm is subject to affirmative action regulation by observing the size of the company and the value of its contracts.[43] In contrast, EEO-1 Records rely on a self-reported measure of the regulation. In these data, establishments themselves report whether they are federal contractors, and whether they are subject to the regulation or not based on their size and amount of its contracts with the government. This is important in our case because there is anecdotal evidence and government reports highlighting the fact that firms are often unaware that they are regulated by EO 11246 (GAO, 2017).[44] Last, by observing characteristics of the federal contracts as well as the size of the establishments, we can exploit all the dimensions of the regulation in our empirical setting. While EEO-1 Records only allow to make comparisons across contractor status, our rich data allow us to exploit an interesting dimension of EO 11246: contractors above the $50,000 threshold in the value of the contract are subject to more stringent requirements than those with smaller contracts. We comment on these advantages further after we present our empirical strategies and results.

Neumark (2000), and Holzer and Neumark (2006).

[40] The personnel composition is disaggregated by 9 broad occupation groups: officials and managers, professionals, technicians, sales workers, administrative support workers, craft workers, operatives, laborers/helpers, and service workers.

[41] Appendix B.1 compares our linked matched employer-employee data to the EEO-1 records.

[42] Industries that tend to have larger establishments (e.g. mining, manufacturing) are overrepresented in the EEO-1 Records, and industries that tend to have small establishments (e.g. services) are underrepresented. Overall, EEO-1 Records account for about 34% of total employment.

[43] See Section 2 for details on how regulation changes according to the size of the firm and value of the contract, as well as for the general institutional background of the policy.

[44] According to the report by GAO (2017), out of 24 contractors they surveyed, 20 use third party support such as consulting firms and attorneys who are knowledgeable with the law in order to determine whether they are subject to specific regulations, including EO 11246 (p. 35). We held several discussions with one of these consulting firms, *Affirmity* (https://www.affirmity.com/), and they corroborated the lack of clarity that establishments holding contracts with the federal government have with respect to their obligations. See, for instance, https://ogletree.com/insights/i-think-my-company-is-a-federal-contractor-and-has-regulatory-obligations-but-where-can-i-look-to-search-for-that-information/

4 The Effect of EO 11246

Our empirical strategies leverage the different datasets presented in Section 3 to analyze the causal effects of EO 11246 on the share of Black workers employed at regulated establishments. We focus on Black workers because this is the group that most of the affirmative action research focuses on (Holzer and Neumark, 2000) but we also examine effects on Hispanic workers, women and on workers who belong to a minority group (including those who are Black or Hispanic).[45] The results for these groups are qualitatively similar to the results for Blacks, and we present them in Appendices D, E, and F. We refer to the groups of Blacks, Hispanics, minorities, and women as the *protected groups*.

In this section, we rely on the institutional setting to argue for the exogeneity of EO 11246 adoption. Based on that, we exploit the variation in the timing of adoption of a federal contract to estimate the impact of EO 11246 on the share of Black and other minority workers using the estimator proposed by Callaway and Sant'Anna (2021) which, in contrast with the standard TWFE estimator of event study designs, remains consistent under multiple dimensions of treatment heterogeneity — including dynamic treatment effects, — and selection into treatment based on covariates.[46] Then, in Appendix H, we show that our results are qualitatively robust to using a naive TWFE estimator.

Unlike the previous literature studying EO 11246 — that uses self-reported information on establishments' regulation status outlined in the EEO-1 records used by previous studies, — we use information coming from FPDS data to identify our sample of contractor establishments. We identify those establishments which, during 2001–2014 are *ever* contractors, and thus subject to EO 11246, in our data.

Our first research design exploits the event of first becoming subject to EO 11246. Therefore, we exclude establishments that enter our sample as contractors, and for the set of establishments that become contractors at some point in our sample, we include years in the data that are in the 5-year window around the event. We also exclude any federal contractor holding a contract worth $10 million or more during our sample period, given that these are subject to a pre-award compliance evaluation that includes an inspection of the racial composition of their personnel.

[45] We include the following racial categories in workers from *minority groups*: Black or African American, American Indian or Alaska Native, Asian, Native Hawaiian or Other Pacific Islander, Two or More Race Groups. Any individual who is Hispanic or Latino, regardless of race, is also consider among our group of "minorities."

[46] See Goodman-Bacon (2021).

In our matched employer-employee data, an establishment, j, is said to be a federal contractor when j is present both in the LEHD-LBD-SSEL and in the OFCCP-FPDS data (see Section 3). Because of this definition, under an imperfect matching procedure,[47] some establishments in our data may be incorrectly classified as non-federal contractors. Thus, if we used non-federal contractors as the control group, this group could be contaminated by the misclassification of some federal contractors as non-federal contractors. This misclassificaions could bias our results towards zero. To address this concern, we obtain our estimates using only the sample of federally-contracted establishments: we exploit variation in the timing of first becoming a federal contractor, where the controls are "not yet treated" establishments.

In what follows, we denote the treatment at time t, becoming subject to EO 11246 at time t, with D_t^{AA}.[48]

Following Callaway and Sant'Anna (2021), we estimate the group-time average treatment effects on the treated:

$$ATT^{AA}(g,t|X_j,\lambda_t) = \mathbb{E}[Y_t^g(D_t^{AA}) - Y_t^0(D_t^{AA})|X_j,\lambda_t,G^{AA}=g], \tag{1}$$

where g denotes the adoption time of EO 11246, G_j^{AA} denotes the cohort of establishment j (in relation to treatment), $Y_t^g(D_t^{AA})$ denotes the potential outcome at time t conditional on becoming subject to EO 11246 at time g, and $Y_t^0(D_t^{AA})$ denotes the untreated potential outcome at time t. $ATT^{AA}(g,t|X_j,\lambda_t)$ denotes the effect of becoming subject to EO 11246 at time g that is measured in time t, subject to establishment age, location, and size (at time g), X_j, and year fixed effects, λ_t. Note that we allow for heterogeneity across groups (i.e., we allow for $ATT^{AA}(g,t|X_j,\lambda_t) \neq ATT^{AA}(g',t|X_j,\lambda_t)$) and dynamic treatment effects (i.e. it is possible for $ATT^{AA}(g,t|X_j,\lambda_t) \neq ATT^{AA}(g,t'|X_j,\lambda_t)$). Moreover, by conditioning on becoming subject to EO 11246, we control for selection into treatment.

We have $T-g$ different ATT^{AA}'s for each group-time pair (g,t) — except for the last treated cohort in the panel, let us denote such cohort with $\bar{g}$: we cannot construct $ATT^{AA}(\bar{g},t|X_j,\lambda_t)$.[49] Let us denote with $\mathcal{G}$ the set of cohorts g for which we can define these $T-g$ different ATT^{AA}'s.[50]

After identifying the set of $\{ATT(g,t|X_j,\lambda_t)\}_{g\in\mathcal{G},t=g,\ldots,T}$, we aggregate these across t to get

[47] Appendix B explains this matching procedure in detail.
[48] AA stands for affirmative action. We use this rather than EO 11246 for ease of notation.
[49] Formally, $\bar{g} = \max_{i=1,\ldots,n} G_i$ denotes the latest treatment time G in the data.
[50] Formally, $\mathcal{G} = supp(G)\backslash\{\bar{g}\} \subseteq \{2,3,4,\ldots,T\}$. See Callaway and Sant'Anna (2021).

the average dynamic effects for event times $e = t - g$:

$$\theta_{AA}(e) = \sum_{g \in \mathcal{G}} \mathbb{1}_{AA}(g + e \leq T) ATT(g, g + e | X_j, \lambda_t) \mathbb{P}(G^{AA} = g | X_j, \lambda_t, G^{AA} + e \leq T) \qquad (2)$$

where $\theta_{AA}(e)$ are the counterparts of the event study estimates of the classical Diff-in-Diff under homogeneous treatment with time of exposure to treatment e. In particular, $\theta_{AA}(e)$ are the leads' counterparts for $e > 0$, the lags' counterparts for $e < 0$ and the baseline counterpart for $e = 0$.

We identify the set of $ATT^{AA}(g, t | X_j, \lambda_t)$ under the assumptions of conditional parallel trends based on "not-yet-treated" groups[51] and absorbing treatment. We follow the outcome regression approach (Heckman et al., 1997) to match establishments in the control group (the not-yet-treated establishments) to the adopters.

4.1 Results

We start by estimating (2), where the outcome variable $Y_{jt}^{g}(D_t^{AA})$ corresponds to the share of Black workers in establishment j.

Figure 1 plots the 'event study' estimates $\{\theta_{AA}^{share}(e)\}_{e=-5}^{5}$ which show no apparent pre-trends. The pre-treatment average share of Blacks is 5.62%. So the full effect is rather small: the biggest effect takes place 4 years after treatment, when $\theta_{AA}^{share}(4) = 0.001\,(0.0018)$. This effect would translate into a 0.006% increase in the share of Blacks. Moreover, the figure shows the 95% confidence bands but the estimates are not significant even at the 90% level.[52]

For comparison, at each year of exposure to treatment $e = 0, 1, \ldots, 5$, the upper bound of the effects we find are 100 times smaller than those found by Miller (2017) (See Panel A of Figure 3 in Miller (2017)).

[51] See Assumption 5 of Callaway and Sant'Anna (2021).

[52] See Table D.1. We tabulate the estimated $\{\theta_{AA}^{share}(e)\}_{e=-5}^{5}$ plotted in Figure 1, and the analogous coefficients corresponding to Hispanic, minorities in general, and women together with their corresponding figures in Appendix D.

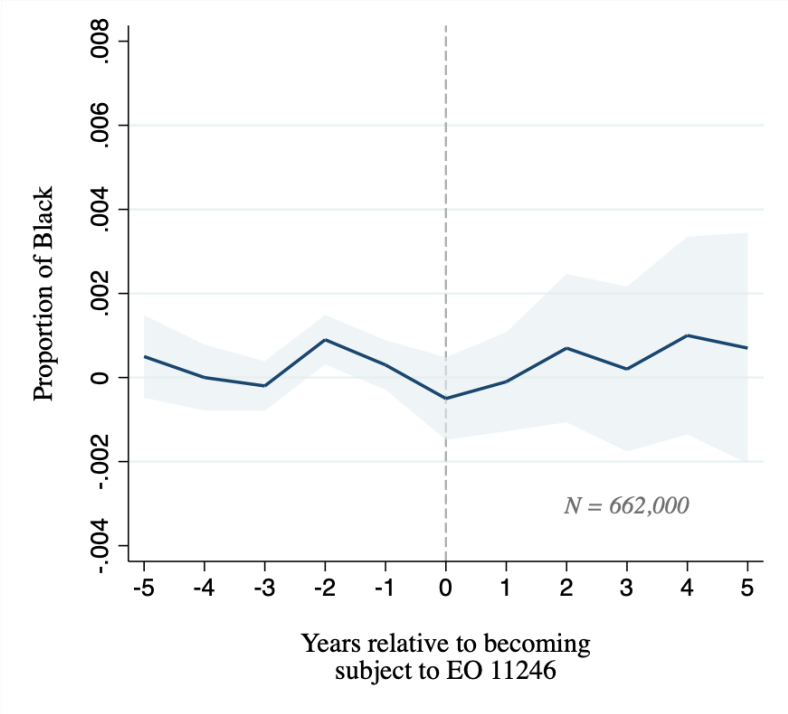


Figure 1: *Staggered event study estimates: effect of becoming subject to EO 11246 on the share of Black workers*

Notes: This figure plots the staggered event study coefficients $\{\theta_{AA}^{share}(e)\}_{e=-5}^{5}$ obtained following Callaway and Sant'Anna (2021) in the solid line, where the outcome variable is the percent Black of an establishment's employees. 95% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 1,000. The estimates include controls for year fixed effects, establishment's location, a quadratic in age and log establishment size. Standard errors are clustered at the firm level.

One explanation for the low effectiveness of the policy on the share of Black workers could be that turnover is low in federally contracted firms and, therefore, overall employment remains relatively constant. If that was the case, we could find positive effects on the share of Black newly hired workers even if there are null effects on the overall workforce.

To test this hypothesis, we re-estimate (2), but where the outcome variable $Y_{jt}^{g}(D_t^{AA})$ now corresponds to the share of Black new hires in establishment j. Figure 2 plots the corresponding 'event study' estimates $\{\theta_{AA}^{new}(e)\}_{e=-5}^{5}$ which again show no pre-trend. The pre-treatment average share of Black new hires is 7.02%. So the full effect is again quite small: perhaps coincidentally, the biggest effect takes place again 4 years after treatment, when $\theta_{AA}^{share}(4) = 0.0041\,(0.0021)$. This effect would translate into a 0.016% increase in the share of Black new hires. In addition, as in the case of $\{\theta_{AA}^{share}(e)\}_{e=-5}^{5}$ above, Figure 2 shows the 95% confidence

bands but the estimates are not significant even at the 90% level.[53]

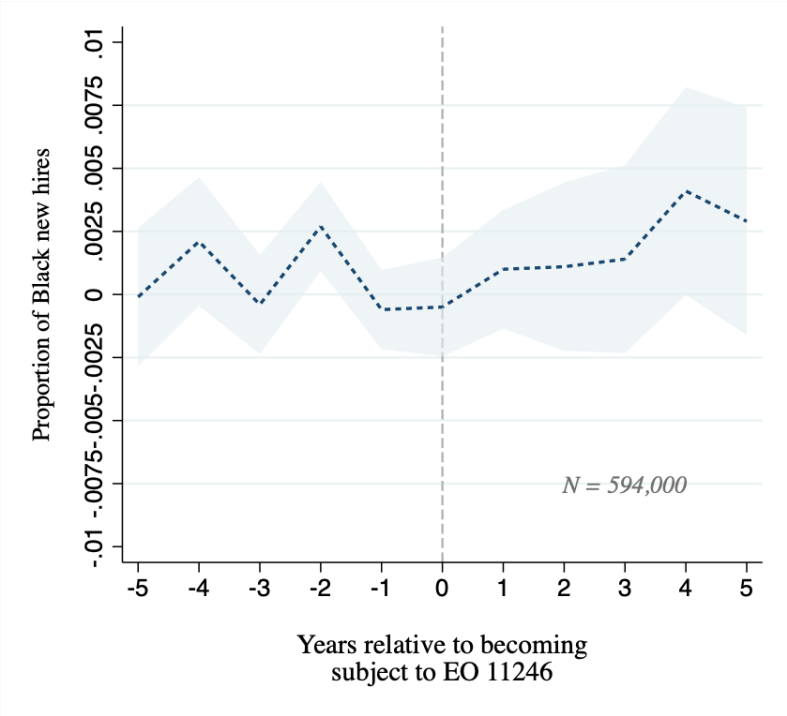


Figure 2: *Staggered event study estimates: effect of becoming subject to EO 11246 on the share of Black new hires*

Notes: This figure plots the staggered event study coefficients $\{\theta_{AA}^{new}(e)\}_{e=-5}^{5}$ obtained following Callaway and Sant'Anna (2021) in the short-dashed line, where the outcome variable is the proportion of Black new hires in an establishment's employees. 95% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 1,000. The estimates include controls for year fixed effects, establishment's location, a quadratic in age and log establishment size. Standard errors are clustered at the firm level.

Finally, the earnings gap could be affected by the policy. Note that even when we do observe that there are no changes in new hires at the firm level, it could still be the case that there was a shift in the type of workers that were hired. Indeed, even if the policy does not have a positive impact on the share of Black workers that an establishment hires, its effects could still operate through wages.

It is well documented that retention of Black workers is considerably lower than that of whites (McKay et al., 2007), so even if a regulated establishment does not increase its pool of Black workers, it could still increase their wages to retain the incumbent ones. But workers

[53] See Table D.2. We tabulate the estimated $\{\theta_{AA}^{new}(e)\}_{e=-5}^{5}$ plotted in Figure 2, and the analogous coefficients corresponding to Hispanic, minorities in general, and women together with their corresponding figures in Appendix D.

would be selectively leaving the sample due to turnover. So, in order not to confound the effects of the policy with the selective turnover, rather than considering new hires, we focus on incumbent workers. That is, we focus on workers who were already working at the establishment when it became regulated. Following the spirit of the comparisons we show above, we could consider measuring the earnings gap as the ratio of the average earnings of minority incumbent workers divided by the average earnings of white incumbent workers. However, the ratio of wages is not well defined unless there is at least someone at the firm in each of the groups being compared.

Thus, to measure the gap, we focus on incumbent workers, and we measure the gap at the individual level. This measure requires worker level information, and, to the best of our knowledge, we are the first ones to be able to look at this margin. These results have not undergone Census Disclosure.

Our empirical results from from estimating (2) are qualitatively robust to using a naive TWFE estimator similar to the one of Miller (2017).[54] See Appendix H.

5 The Effect of the Affirmative Action Plan

Next, we analyze the effect of the more stringent margin of EO 11246: the requirement to have an AAP (Affirmative Action Plan). As a reminder, this applies to firms with 50 employees or more that hold a federal contract with a value of $50,000 or more. To the best of our knowledge, we are the first ones to be able to isolate this stricter margin from the softer elements of the policy.[55] This is only possible to do with our newly constructed matched employer-employee data, that allows us to observe the contract value of federal contractors, in addition to detailed characteristics of the establishments.

To evaluate the effect of the AAP requirement, we use both the Callaway and Sant'Anna (2021) estimator, and a regression discontinuity design.[56]

Throughout the remainder of this section, we maintain our sample restriction to establishments in firms with 50 or more employees. This is to avoid confounding the effects of affir-

[54] This is to be expected as the bias created by dynamic treatment effects pushes the parameter estimates closer to zero and away from the true parameter. I.e. TWFE estimates are biased towards zero (see Goodman-Bacon (2021).

[55] Such as keeping records of applications, hiring, promotions and terminations available for review, placing posters on the walls and adding tag lines to job vacancy adverts.

[56] We do not exploit the size requirement of 50 or more employees because several worker related regulations kick in at the 50 employee threshold, and this could contaminate the identification of the causal effect of affirmative action. Section 5.2 provides more details on this.

mative action with several worker-related regulations that kick in at the 50-employee threshold. We continue excluding any contractor holding a contract worth $10 million or more.

An important advantage of our data compared to previously used data is that we observe the contractor status of an establishment together with the dollar value of its contracts. This allows us to exploit to isolate the effects of the AAP on the share of Black workers an establishment employs, net of the effect of becoming a contractor. We focus our analysis on comparisons across contractors above and below the contract value threshold in a staggered event study and a regression discontinuity designs.

5.1 Contract Value

Before jumping directly to our analysis, we provide some descriptive evidence regarding the value of the contracts. Given that with both our empirical approaches we perform comparisons of contractors above and below the $50,000 threshold, it is important to document three facts. First, that federal contracts below $50,000 are similar to federal contracts above this threshold. Second, that federally contracted establishments do not have contract values bunching just below $50,000 — which could suggest that they are trying to avoid the stricter dimension of EO 11246. Third, that the establishments that earn a federal contract below the $50,000 value threshold are comparable to those that earn a federal contract with a value above that.

Table 1 presents some descriptive statistics from contract level data (FPDS). Since part of our later analysis restricts the attention to a window around the $50,000 value threshold, we describe all contracts in our data in Column I, and then separately those in the $[25,000-50,000) value range in Column II, and $[50,000-75,000] in Column III. Reassuringly, contracts above the $50,000 value threshold are comparable to those below and, moreover, to the universe of contracts. They are comparable in contract characteristics such as duration, whether the contract is competed, whether it was fixed price, and in the number of offers received. The distribution of awarding agencies is also comparable across contract value ranges. Additionally, Appendix Figure G.3 shows that these contract characteristics evolve smoothly around the $50,000 contract value threshold.

[57] These competitions are open and carried out by the federal government to award the contract to the firm offering the lower costs for goods and services purchased.

[58] This is a type of contract in which the firms state the goods or services they will provide and establish the price that the government has to pay for them ex-ante. Ex-post adjustments to the original price can occur, although this imposes administrative burdens and are relatively rare (GSA, 2019). This is in contrast with "cost-plus" contracts, which are common among construction contracts. These type of contracts specify both the cost of the goods or services to be provided together with a fee for the firm's overhead and profit.

Table 1: *Summary statistics for contracts in the FPDS data*

	Column I	Column II	Column III
Contract Characteristics	**All contracts**	**Between $25-50K**	**Between $50-75K**
Duration (in years)	1.57	1.59	1.62
Num. offers received	3.68	3.79	3.94
% Competitively acquired[57]	0.68	0.69	0.67
% Fixed price contracts[58]	0.89	0.91	0.89
Share of contracts by Awarding Agency			
Department of Defense	0.57	0.58	0.59
General Services Administration	0.12	0.13	0.11
Veteran Affairs	0.09	0.08	0.07
Other government agencies	0.22	0.21	0.23
Total number of contracts	13,913,986	2,254,853	987,104

Source: FPDS Data.

Notes: The table contains descriptive statistics from contract level data coming from the FPDS. Since our regression discontinuity design restricts the analysis to a window around the $50,000 value threshold, we describe all contracts in our data in Column I, and then separately those in the $[25,000-50,000) value range in Column II, and $[50,000-75,000] in Column III. Reassuringly, a comparison of the three columns shows that the contract characteristics are comparable.

Next, we check that firms contract values do no bunch just below the threshold. Specifically, we are worried about firms bidding with contracts capped at $49,999.99 to avoid the regulation. As shown in Table 1, the vast majority of federal contracts are fixed-price contracts.[58] The contract value of $50,000 above which a firm becomes subject to the AAP requirement considers the final price of a contract, including any ex-post adjustments.

Figure 3 plots a histogram of the number of contractor establishments by maximum final, ex-post value of contracts held.[59] The figure presents visual evidence inconsistent with a bunching below the $50,000 value threshold. In fact, what we observe in the data is bunching *just at* the $50,000 threshold. Although milder, this bunching at $50,000 is also observed at other integer values, particularly those multiples of 5,000. This is reassuring, in the sense that this evidence goes against a strategic behavior to avoid being subject to affirmative action. Moreover, the fact that we observe a spike in the number of establishments that hold contracts with a value of $50,000 when they could bid a slightly smaller value is in line with two alternative explanations: either firms are not aware of the EO 11246 requirements varying by contract value, or they do not find it costly to comply with them. We discuss this further when we present our empirical results in the next subsection.

Last, we check that the characteristics of the establishments that earn a contract below the $50,000 value threshold are comparable to those earning a contract with a value above $50,000. Even though the information about federally contracted firms in the FPDS data is limited, we check two important characteristics that we observe in that data: the duration of these firms as federal contractors, and whether they are a Black- or minority-owned business.[60]

Table 2 shows that all observable characteristics are similar between all federal contractors and those that earn federal contracts in the bandwidth considered.[61] The duration as federal contractor is an important characteristic, given that it determines the duration under which the establishment is subject to EO 11246. Additionally, Appendix Figure G.5 shows that these durations, as well as the Black and minority owned status of the establishments vary smoothly around the $50,000 threshold.

[59] In this histogram, if an establishment has more than one contract in a given year, we take the maximum value.

[60] These comparisons rely solely on establishments' information observable in the FPDS data. Further checks using the richer establishment-level information in our matched have not been disclosed by Census Officers.

[61] Appendix Figure G.4 shows histograms for the duration as contractors of all establishments in the FPDS data, and of those in the bandwidth between $25,000 and $75,000.

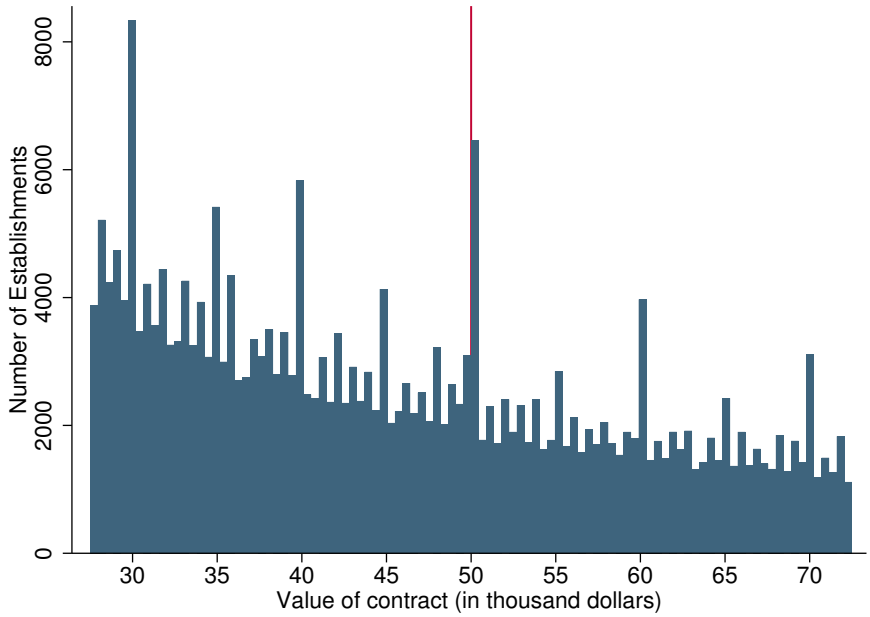


Figure 3: *No bunching below the $50,000 threshold*

Source: FPDS Data.

Notes: This figure shows number of establishments frequency distributions. Value of the contracts is discretized into left-inclusive bins of one-thousand dollars length. A bin labeled X includes all contract awards in the range $[\$1000X, \$1000(X+1))$. The vertical line indicates the location of the affirmative action plan regulation threshold.

Table 2: *Summary statistics of contractors from FPDS data*

	All contractors	**Between $25-50K**	**Between $50-75K**
Duration (in years)	3.02	3.34	3.69
% Black-owned	0.03	0.03	0.03
% Minority-owned	0.08	0.07	0.08
Number of contractors	1,558,648	231,405	120,375

Source: FPDS Data.

Notes: This table includes information from all contractors in the FPDS that we include in our sample of analysis. We exclude contractors which contracts are always below $10,000 or less, which are exempt from EO 11246, and those which ever get a contract worth $10 million or more, which are subject to a pre-award compliance evaluation that inspects the racial composition of the contractor. The first column shows information for all contractors, the second for those who ever earn a maximum value of a contract worth $[\$25,000, \$50,000)$, and the third for those who ever earn a maximum value of a contract worth $[\$50,000, \$75,000]$. All variables show averages.

5.2 Staggered Event Study

In what follows, the treatment at time t corresponds to becoming subject to EO 11246 with an AAP requirement at time t — i.e., having a contract of \$50,000 or more with the federal government at time t. We denote the treatment with D_t^{AAP}.[62]

We estimate a similar "event study" to the one in model (1), over the sample of contractor establishments with 50 or more employees, where the treatment is now D_t^{AAP}.

$$ATT^{AAP}(g,t|X_j,\lambda_t) = \mathbb{E}[Y_t^g(D_t^{AAP}) - Y_t^0(D_t^{AAP})|X_j,\lambda_t,G^{AAP}=g], \tag{3}$$

where the notation is analogous to that in (1). The control group consists of establishments which are federal contractors during the sample period with contracts that are always below that value threshold.[63]

Figure 4 plots the 'event study' estimates, $\{\theta_{AAP}^{share}(e)\}_{e=-5}^{5}$, which capture the dynamic effects for event times $e = t - g$:

$$\theta_D^{AAP}(e) = \sum_{g\in\mathcal{G}} \mathbb{1}_{AAP}(g+e\leq T)ATT^{AAP}(g,g+e|X_j,\lambda_t)\mathbb{P}(G^{AAP}=g|X_j,\lambda_t,G^{AAP}+e\leq T). \tag{4}$$

Similar to Section 4, the estimated coefficients show no apparent pre-trends. The pre-treatment average share of Blacks is 8.6%. The biggest coefficient is $\theta_{AAP}^{share}(0) = -0.0006\,(0.0006)$. This effect would translate into a slight decrease of 0.005% in the share of Blacks. As before, although the figure shows the 95% confidence bands, the estimates are not significant even at the 90% level.[64] This suggests no causal effect of becoming subject to EO 11246 with an AAP requirement on the share of Black workers that an establishment employs.

As in Section 4, to check that the null effects from Figure 4 hold also when we focus on the subsample of newly hired workers, we re-estimate (4), using as the outcome variable, $Y_{jt}^g(D_t^{AAP})$, the share of Black new hires in establishment j.

Figure 5 plots the corresponding 'event study' estimates $\{\theta_{AAP}^{new}(e)\}_{e=-5}^{5}$ which again show no pre-trend. The pre-treatment average share of Black new hires is 10.2%.

[62] *AAP* stands for Affirmative Action Plan.

[63] The identifying assumption in this case is that of conditional parallel trends based on a "never-treated" group. See Assumption 4 in Callaway and Sant'Anna (2021).

[64] See Table E.1. We tabulate the estimated $\{\theta_{AAP}^{share}(e)\}_{e=-5}^{5}$ plotted in Figure 4, and the analogous coefficients corresponding to Hispanic, minorities in general, and women together with their corresponding figures in Appendix E.

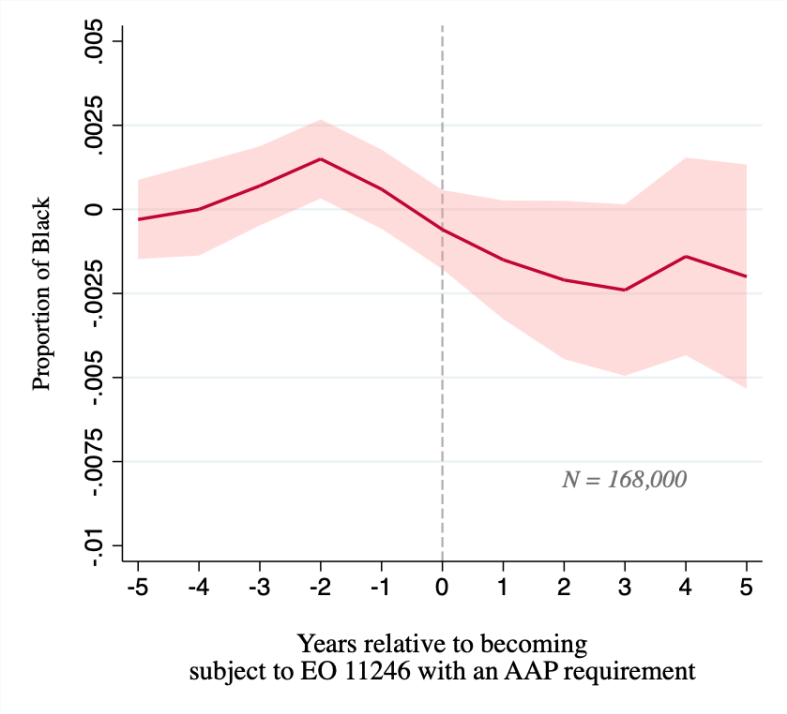


Figure 4: *Staggered event study estimates: effect of the AAP on the share of Black workers*
Source: AA-Contractor Data.
Notes: This figure plots the staggered event study coefficients $\{\theta_D^{AAP}(e)\}_{e=-5}^{5}$ from (4) obtained following Callaway and Sant'Anna (2021) in the solid line, where the outcome variable is the percent Black of an establishment's employees. 95% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 1,000. The estimates include controls for year fixed effects, establishment's location, a quadratic in age and log establishment size. Standard errors are clustered at the firm level.

So the full effect is again quite small: fluctuates around zero and is not significant at the 95% level.[65]

The biggest coefficient is $\theta_{AAP}^{share}(4) = 0.0025\,(0.0026)$, which would imply a 0.026% increase in the share of Black new hires. The figure shows the 95% confidence bands but, as can be seen in Table E.2, the estimates are not significant even at the 90% level.[66]

[65] The estimated $\{\theta_{AAP}^{new}(e)\}_{e=-5}^{5}$ plotted in Figure 5, and those corresponding Hispanic, minorities in general and women together with analogous figures in Appendix E.

[66] We tabulate the estimated $\{\theta_{AAP}^{share}(e)\}_{e=-5}^{5}$ plotted in Figure 4, and the analogous coefficients corresponding to Hispanic, minorities in general, and women together with their corresponding figures in Appendix E.

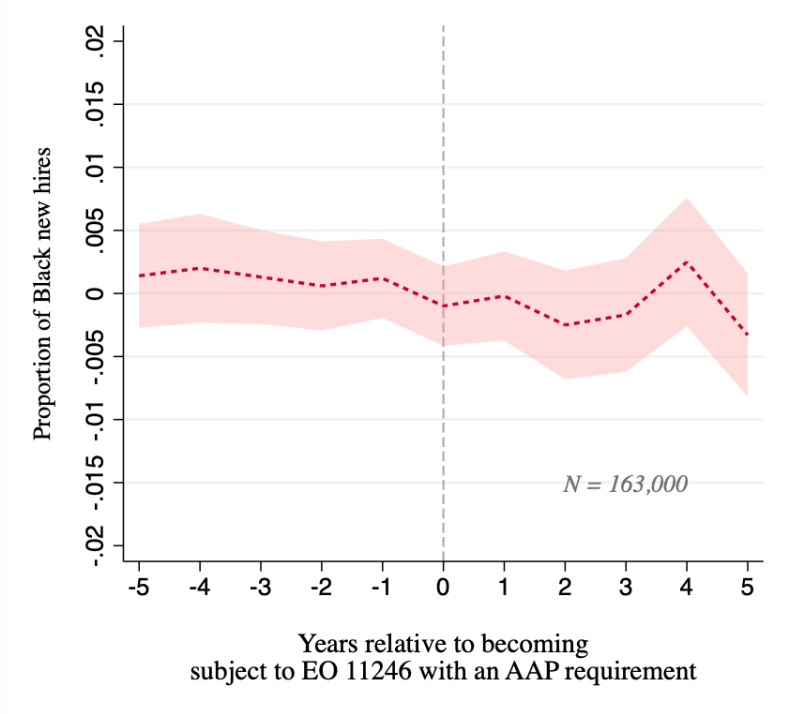


Figure 5: *Staggered event study estimates: effect of the AAP requirement on the share of Black new hires*

Notes: This figure plots the staggered event study coefficients $\{\theta_D(e)\}_{e=-5}^{5}$ obtained following Callaway and Sant'Anna (2021) in the short-dashed line, where the outcome variable is the proportion of Black new hires in an establishment's employees. 95% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 1,000. The estimates include controls for year fixed effects, establishment's location, a quadratic in age and log establishment size. Standard errors are clustered at the firm level.

5.3 Regression Discontinuity

To corroborate our results, we turn to a more local analysis, in which we exploit the $50,000 value threshold in a regression discontinuity setting. We impose further sample restrictions with respect to the event study. Within contractor firms that have 50 or more employees, we keep in our sample those establishments that enter our matched employer-employee data with a maximum value of a contract in the $25,000–$75,000 value bandwidth. Beyond dropping establishments who enter our data as contractors, as we do in the staggered event study designs, we now impose the requirement that the contractor must be first observed in the $25,000–$75,000 bandwidth from 2005 onward. Since our data starts in 2001, we impose this further restriction to consider only establishments that we can observe for 4 years without any federal contract with aim to identify, as best as we can, first federal contracts. This is a costly and very conservative sample cut, but our results do not change when we relax this to 2002, the second

year we have in our sample.

Now, we estimate the following equation:

$$y_j^k = \alpha^k + \beta^k \mathbb{1}_{c_j \geqslant 50} + f(c_j) + \mathbb{1}_{c_j \geqslant 50} \times f(c_j) + \gamma^k X_j + \lambda_{t(j)} + \varepsilon_j \tag{5}$$

where c_j is the value of the contract that the establishment j has the first time it is observed as contractor (in thousand dollars), $\mathbb{1}_{c_j \geqslant 50}$ is an indicator that takes the value of 1 if $c_j \in [\$50,000, \$75,000]$,[67] and 0 if $c_j \in [\$25,000, \$50,000)$, $f(c_j)$ is a local linear polynomial on the contract value (Gelman and Imbens, 2019), X_j is a vector of controls measured at the year before the regulation (payroll and employment at the establishment, establishment age, an indicator for establishments in multi-unit firms, and number of contracts the establishment holds), and $\lambda_{t(j)}$ a year fixed effect. For establishments that have only one contract, c_j is the value of that contract. For establishments that hold multiple contracts in a given year, c_j is the value of the contract worth the highest amount. For instance, if in a given year establishment j holds a contract of $c_j^1 < \$50,000$ and a second contract of $c_j^2 \geq \$50,000$, that establishment is classified as treated in this specification.

Since c_j is the value of the contract the first time establishment j is observed as federal contractor in the data, treatment status can switch for a given establishment over time. That is, if establishment j is first observed with a value of $c_j^1 < \$50,000$ and hence is classified as control can, in subsequent years, get another contract $c_j^2 \geq \$50,000$ and become treated. Appendix Figure G.8 shows how frequent these cases are in our data. Of the sample of establishments that are classified as treated the first time they are observed in the data, only 13% of them would be classified as controls 5 years after, 71% are not observed as federal contractors afterwards, and 16% continue in the sample as a treated establishment. For the control group the percentages are relatively similar: only 10% would be classified as treated 5 years after, 74% are not observed as contractor in subsequent periods, and 16% continue in the sample as a control establishment. Since the sample cuts we impose in this design are quite restrictive, and given that these treatment status switching patterns are dealt with by our staggered event study design,[68] in this setting we keep our definition of regulated (treated) and not regulated (control) establishment based on the maximum value of the contract the first year an establishment is observed.

Each establishment is one observation in our data, and we condition them on the first time

[67] Note that $\mathbb{1}_{c_j \geqslant 50} = 1$ implies that the indicator $\mathbb{1}_{j,AAP}$ from Section 5.1, also takes the value of 1. However, $\mathbb{1}_{j,AAP} = 1$ does not require the value of the contract c_j to belong to the interval $[\$50,000, \$75,000]$.

[68] In that design, contrary to the regression discontinuity, an establishment that is first observed with a value of $c_j < 50,000$ and then gets another contract of $c_j \geq 50,000$ in a subsequent year is classified as treated.

they become a federal contractor — regardless of whether they are below or above the $50,000 threshold. The outcomes we analyze are percent of Black workers, percent of Black *new hires*, and the relative earnings gap between Black and white workers. The first of the outcomes looks at the stock of workers while the second one measures the effects on the flow of workers. All three outcomes are measured either in the treatment year ($k = 0$) or k years after treatment, where $k \in \{1,5\}$) and the latter two require worker level information, and, to the best of our knowledge, we are the first ones to be able to look at these margins. Table 3 presents the qualitative results from our estimation of β^k from equation (5) for these three outcomes. We cluster standard errors at the firm level. The first column shows β^{-1} — i.e. the effects on the outcome in the year before the establishment is first treated — as a check that the pre-treatment outcome varies smoothly at the threshold. The evidence indicates that this is the case: $\hat{\beta}^{-1}$ is not statistically significant for any of the outcomes considered.

Table 3: *Main results - Regression discontinuity*

	$t-1$ (baseline)	$t+0$	$t+1$	$t+5$
Proportion Black workers	$-$	$-$	$+$	$+$
Proportion new hires Black	$-$	$-$	$-$	$-$
Relative Black-white Earnings	$-$	$-$	$-$	$+$
N	*2,600*	*2,600*	*2,200*	*1,800*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Notes: This table presents the qualitative results for the estimates of β^k in model (5), for $k \in \{-1,0,1,5\}$. $-$ indicates a negative point estimate, $+$ a positive one. *N* is the number of establishments, rounded to the nearest 100.

Subsequent columns in Table 3 present the estimates of β^k for $k = 0$, $k = 1$, and $k = 5$. In line with the results of the event study design in Section 5.2, we fail to detect an impact of the affirmative action plan on any of the outcomes considered: the estimates $\{\hat{\beta}^k\}_{k\in\{0,1,5\}}$ are not statistically significant even at the 10% level, and they jump above and below zero.

Taken together, the results from the RD design indicate that even after isolating the effect of the more stringent element of the policy, the AAP, we detect no causal effect of affirmative action on the share of Black workers, the share of Black new hires nor the Black-white earnings gap.

Table H.2 presents the qualitative features of our estimates of $\{\beta^k\}_{k\in\{0,1,5\}}$ from equation

(5) for all protected groups for these three outcomes. The results do not indicate that the affirmative action plan operates in any of the groups.

6 Why is EO 11246 Not Effective?

Section 4 and Section 5 show that neither the general regulation nor the more stringent element of EO 11246, the AAP, have an effect on the shares of protected groups nor on the shares of new hires of these groups during the 2000s.

To investigate whether enforcement from the OFCCP, the enforcement agency, could explain the lack of effects of EO 11246, we leverage on audit data from the OFCCP. (See Section 3.1.2 for a description of these data).[69]

6.1 The effect of enforcement by the OFCCP

Using our novel data, we estimate the impact of being audited in a staggered event study design similar to that in Section 4.

The auditing information in our matched employer-employee data coming from the OFCCP, allows us to identify the sample of establishments that were *ever* audited during the period 2001–2014. We use this information to estimate a similar event study to the one in model (3), over the sample of contractor establishments with 50 or more employees.

$$ATT^{audit}(g,t|X_j,\lambda_t) = \mathbb{E}[Y_t^g(D_t^{audit}) - Y_t^0(D_t^{audit})|G^{audit} = g], \tag{6}$$

where now $D_{j,t}^{audit}$ is a dummy indicating that establishment j has been audited at time t. The control group consists of establishments which are federal contractors during the sample period that have not been audited at time t.

We identify the set of $ATT^{audit}(g,t|X_j,\lambda_t)$ under the assumptions of conditional parallel trends based on a "never-treated" group [70] and absorbing treatment.

Figure 6 plots the 'event study' estimates $\{\theta_D^{audit}(e)\}_{e=-5}^{5}$, , which capture the dynamic

[69] Note that the enforcement agency, the OFCCP, does not have a fixed budget and, hence, it has periods with fewer staff members — from 2001 to 2010, for example, OFCCP employees shrank by 28%. According to the agency's report, "this massive reduction resulted in a significant decrease of activity in some central areas related to compliance evaluations" (OFCCP (2010), p. 46).

[70] See Assumption 4 of Callaway and Sant'Anna (2021).

effects for event times $e = t - g$:

$$\theta_D^{audit}(e) = \sum_{g \in \mathcal{G}} \mathbb{1}_{audit}(g + e \leq T) ATT(g, g+e | X_j, \lambda_t) \mathbb{P}(G^{audit} = g | X_j, \lambda_t, G^{audit} + e \leq T). \quad (7)$$

As in Section 5, the estimated $\{\theta_D^{audit}(e)\}_{e=-5}^{5}$ show no apparent pre-trends. The biggest coefficient is $\theta_{audit}^{share}(0) = 0.001\,(0.0014)$. This effect would translate into a 0.010% increase in the share of Blacks. As can be seen from Table F.1, the estimates are not significant even at the 90% level.[71] This suggests no causal effect of the OFCCP's enforcement activities on the share of Black workers that an establishment employs.

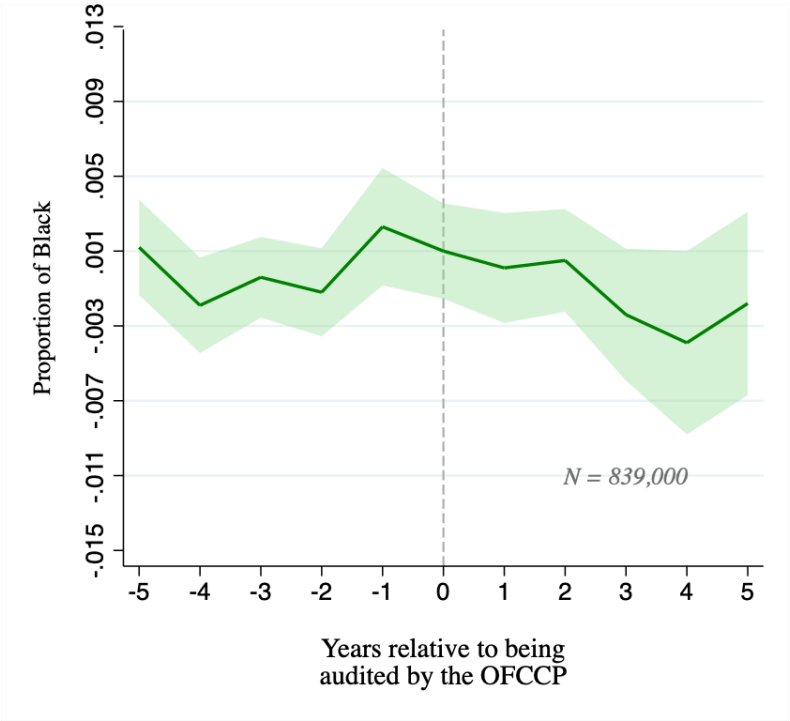


Figure 6: *Staggered event study: effect of being audited on the share of Black workers*

Notes: This figure plots the staggered event study coefficients $\hat{\theta}_D^{audit}(e)$ obtained following Callaway and Sant'Anna (2021) in the solid line, where the outcome variable is the percent Black of an establishment's employees and the event corresponds to the establishment being audited. 95% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 1,000. The estimates include controls for year fixed effects, establishment's location, a quadratic in age and log establishment size. Standard errors are clustered at the firm level.

Once again, we check that the null effects from Figure 6 hold also when we focus on the subsample of newly hired workers by re-estimating (7), using as the outcome variable, $Y_{jt}^{g}(D_t^{audit})$,

[71] We tabulate the estimated $\{\theta_{audit}^{share}(e)\}_{e=-5}^{5}$ plotted in Figure 6, and the analogous coefficients corresponding to Hispanic, minorities in general, and women together with their corresponding figures in Appendix F.

the share of Black new hires in establishment j.

Figure 7 plots the corresponding 'event study' estimates $\{\theta^{new}_{audit}(e)\}^{5}_{e=-5}$ which again show no pre-trend. The pre-treatment average share of Black new hires is 11.59% and the largest coefficient, $\theta^{new}_{audit}(0) = 0.0005\,(0.0035)$. This effect would translate into a 0.006% increase in the share of Blacks. As can be seen from Table F.2, the estimates are not significant even at the 90% level.[72]

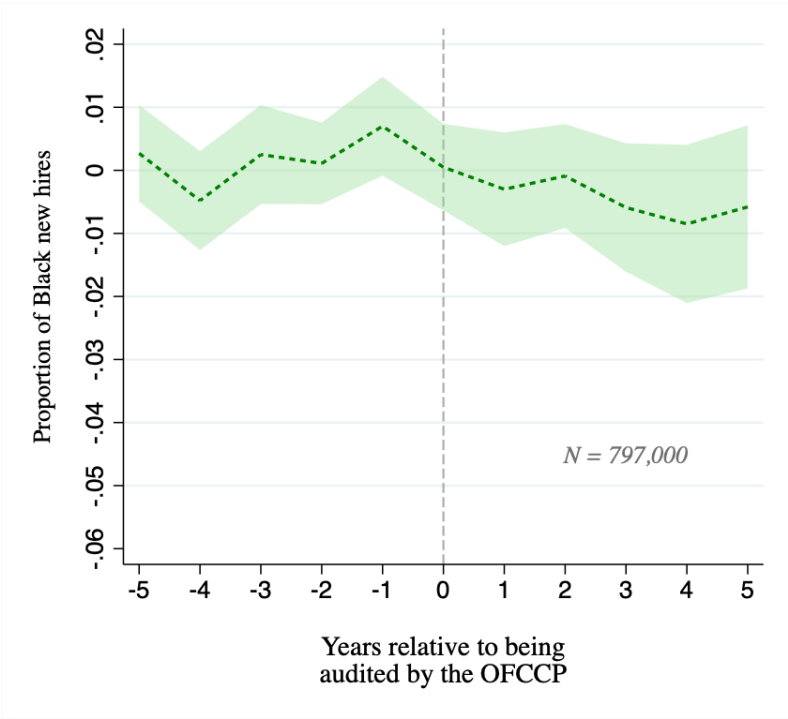


Figure 7: *Staggered event study: effect of being audited on the share of Black new hires*

Notes: This figure plots the staggered event study coefficients $\hat{\theta}^{audit}_{D}(e)$ obtained following Callaway and Sant'Anna (2021) in the solid line, where the outcome variable is the percent Black of an establishment's employees and the event corresponds to the establishment being audited. 90% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 1,000. The estimates include controls for year fixed effects, establishment's location, a quadratic in age and log establishment size. Standard errors are clustered at the firm level.

6.2 Is EO 11246 binding?

Since EO 11246 has been in place since 1965, It could also be the case that the policy is no longer binding for most firms. Recall that federal contractors are required to identify underrepresentation of protected groups by occupation. If they find underrepresentation of any of

[72] We tabulate the estimated $\{\theta^{share}_{audit}(e)\}^{5}_{e=-5}$ plotted in Figure 7, and the analogous coefficients corresponding to Hispanic, minorities in general, and women together with their corresponding figures in Appendix F.

the protected groups, they must lay out strategies and a time line to eliminate it. Thus, if they were already meeting these numbers by the time they acquired a federal contract, we should not expect effects of the regulation.

We use ACS data to determine the benchmarks that firms use to compare their workforce racial composition to identify underrepresentation. These are the data that the EEOC use to provide tabulations to contractor firms, and that firms use as a benchmark to define underrepresentation.

To investigate whether EO 11246 is no longer binding, we first identify the subsample of firms that are underutilizing[73] workers from the protected groups relative to the ACS benchmark. Next, we reestimate coefficients in (2) on the sub-sample of firms that are below that 80% target.[74] We again fail to identify any causal effect of the regulation on the share of Black, minorities or female workers that these non-compliant establishments employ. We interpret this preliminary evidence against the hypothesis that regulation is not binding, and in line with the first hypothesis of weak enforcement presented above.

7 Concluding Remarks

In this paper, we construct a novel dataset to study the effects of EO 11246. Our data allow us to advance the previous literature by considering additional dimensions of the policy as well as outcomes that were not possible to study before.

We document four main findings. First, using our newly constructed data, we find that the policy is no longer effective in increasing the share of protected employees in regulated establishments, and the effect does not grow over time like it used to in the 20th century.

Second, we are able to identify the effects of being subject to the more stringent dimension of the regulation: the affirmative action plan. Using alternative identification strategies, we show that even this more stringent element of the policy does not increase the share of protected employees in regulated establishments either.

Third, we consider alternative margins which were previously unexplored by the literature,

[73] As a reminder, underutilization of a group arises when the incumbency within that group is below 80% of its availability in the relevant labor market.

[74] We estimate the models in the sub-sample of firms that are below the target one year before regulation. For each of the protected groups $z \in \{Blacks, minorities, females\}$, we consider a firm to be below a target if that firm is below the 80% target for group z in at least one age-education category. The EEOC defines availability by occupation, which we do not observe in the LEHD. We use age-education bins to define availability in the ACS. We use 4 categories of education (less than high school, high school, some college or Associate degree, Bachelor's degree or more), and 5 bins of age (18–28; 29–38; 39–48; 49–58; 58–65).

mainly the share of new hires and the relative earnings gap of protected workers, and we document the absence of effects of the policy in these outcomes too.

Last, using data on audits and local minority populations, we show that this lack of effects is not driven by the policy no longer binding but by lack of compliance.

Our findings suggest that issues in enforcement of the policy could be one of the drivers explaining its lack of effects during the 21st century. These issues with enforcement capabilities and their potential adverse impact on the effectiveness of the policy are also discussed in recent congressional reports and acknowledged by policymakers (GAO, 2017). In line with this, there are two recent measures that were taken in order to revert those issues and strengthen oversight activities to improve contractor compliance. First, during 2021, the Biden Administration requested a 33% increase in the budget for the OFCCP, effective for fiscal year 2022 (OFCCP, 2021). Second, the OFCCP has developed a *"contractor portal"* where regulated federal contractors must submit and update online their AAPs on an annual basis, and must certify that they are meeting their requirements. The portal has been officially launched on February 2022, and all contractors will need to certify their AAPs by June 30, 2022.[75] In light of the results in this paper, we embrace these two measures with enthusiasm.

[75] Details on the Contractor Portal can be accessed at this website by the OFCCP: https://www.dol.gov/agencies/ofccp/contractorportal?utm_medium=email&utm_source=govdelivery.

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Appendix A. Details of the LEHD

The LEHD data consist of quarterly worker-level earnings submitted by employers for the administration of state unemployment insurance (UI) benefit programs, linked to establishment-level data collected for the Quarterly Census of Employment and Wages (QCEW) program. The result is incredibly rich administrative dataset which, nevertheless, has some limitations that require us to impose assumptions in order to define unemployment spells and job-to-job transitions. In this section, we describe in detail the assumptions we imposed to deal with these shortcomings.

A.1 Job-to-job Transitions

The data does not contain beginning and end dates of these employment spells. Because of this, we follow Hyatt et al. (2014) to define dominant jobs at the quarterly level.

We say that an individual i is employed at employer j in time t if the worker received positive wages w from that employer in quarter t. We say that an individual i is *beginning-of-quarter employed* at employer j in time t if the worker received positive wages from that employer in both t and $t-1$.

The *dominant beginning-of-quarter* employer in quarter t is the employer from which the worker had the highest earnings summing over quarter t and quarter $t-1$. This job is chosen from among the employers where the worker had positive earnings in both quarter t and quarter $t-1$.

We then consider transitions between employer j and employer k when we see an individual moving from the dominant beginning-of-quarter employer j in t, to the dominant beginning-of-quarter employer k in either $t+1$ or a subsequent quarter. This approach allows us to uniquely link the main job held on the first day of the quarter to the main job held at the start of the subsequent quarter. It does, however, have the obvious disadvantage of dropping job transitions between short duration jobs during the quarter. Thus this categorization restricts each worker to have only one job flow per quarter.

A.2 Unemployment

The LEHD does not contain unemployment indicators. Many papers using the LEHD rely on earnings tests to identify an individual as employed (Sorkin, 2018). However, since unemployment is one of the primary outcomes of interest in this paper, we depart from this approach.

A drawback of the Employment History File (EHF) is that it contains information for the 19 states for which our project was approved.[76] If a worker in one of our approved states was employed in a state we do not have access to, she will appear to have no earnings — and thus would be typically classified as unemployed.

We circumvent this issue by using the EHF National Indicator File from the LEHD, which provides information about the presence of wage records for workers in any state in the U.S.

We say an individual is employed if she has wage records in any state in the U.S. regardless of the level of earnings — the EHF National Indicator File does not contain information about the level of earnings.

Appendix B. Details of the Data Construction

B.1 The data construction

Our data consists of the linkage of six different datasets.

1. Enforcement data from OFCCP audit records
2. Federal Procurement Data System (FPDS)
3. Standard Statistical Establishment Listing (SSEL)
4. Longitudinal Business Database (LBD)
5. Longitudinal Employer-Household Dynamics (LEHD)
6. American Community Survey (ACS)

As described in Section 3, the LEHD consists of the linkage of three core files: the Employer Characteristics File (ECF), the Employment History File (EHF), and the Individual Characteristics File (ICF).[77] This linkage is done following Vilhuber et al. (2018) through internal identifiers defined by the US Census Bureau.

To match the three core files in the LEHD, we start by identifying the workers' main jobs in the EHF, for which we follow Sorkin (2018). We then bring the ICF and ECF files using workers and establishments identifiers, respectively.

[76] Arizona, Arkansas, Colorado, District of Columbia, Delaware, Hawaii, Indiana, Iowa, Kansas, Maine, Maryland, Montana, Nevada, New Mexico, Oklahoma, Pennsylvania, Tennessee, Washington and Wisconsin.

[77] See description of these datasets in Section 3.

There are no names or exact addresses of the establishments in the LEHD. So, in order to prepare the LEHD to be merged with the publicly available FPDS data and our FOIA requested OFCCP data (both identified with establishments' names and addresses), we merge the LEHD to the Standard Statistical Establishment Listing (SSEL) and the Longitudinal Business Database (LBD) data that contain the names and addresses of the establishments in the LEHD, together with the internal identifiers defined by the US Census Bureau.

Once we have constructed the LEHD-LBD-SSEL data, we use the matching algorithm described below (Section B.2) to merge these data to both the FPDS and OFCCP data. We start by using our matching algorithm to match FPDS with OFCCP data. Then we proceed to merge the resulting FPDS-OFCCP with the LEHD-LBD-SSEL data.

Last, as we describe in Section 3, we construct our own EEO-Tabulations averaging each county's racial and gender composition by industry and education in the ACS using the ACS 5-year extract from 2006-2010. That is, than aggregating the number of workers across occupations within an industry, we aggregate the number of workers by location-industry-education bin, where education is defined in ranges as in the LEHD. Then, we match the numbers we obtain using this grouping to the individual level ACS file. We re-do the counts in the EEO tabulations (i.e. we count numbers of workers by location-industry-occupation) and match these numbers to the individual ACS data as well to compare the two tabulations. See Appendix C to look at the correlation between the two tabulations.

B.2 The matching algorithm

Given the size of our data, the key challenge in building the novel data for this project is merging our data sources together as there is no reliable common variable between the LEHD-LBD-SSEL and the OFCCP and FPDS data except for establishments' name and address.

In order to overcome this challenge, we develop a two-step fuzzy matching method based on the natural language processing algorithm, term frequency–inverse document frequency (tf-idf) from Scikit-learn.

The tf-idf score of a word is a numerical statistic that reflects its importance relative to a document within a corpus (a collection of documents). In short, the tf–idf value increases proportionally to the number of times a word appears in the document and is offset by the number of documents in the corpus that contain the word, which helps to adjust for the fact that some words appear more frequently in general.[78]

[78] An alternative to using tf-idf scores would be to measure the distance between strings of words with measures such as the Levenshtein distance or the Jaro-Winkler distance. These methods however, are inefficient when dealing

We split the U.S. into 3-digit zipcode regions and within each of these regions we proceed as follows. First, we consider the names of establishments and split these into n-grams of size

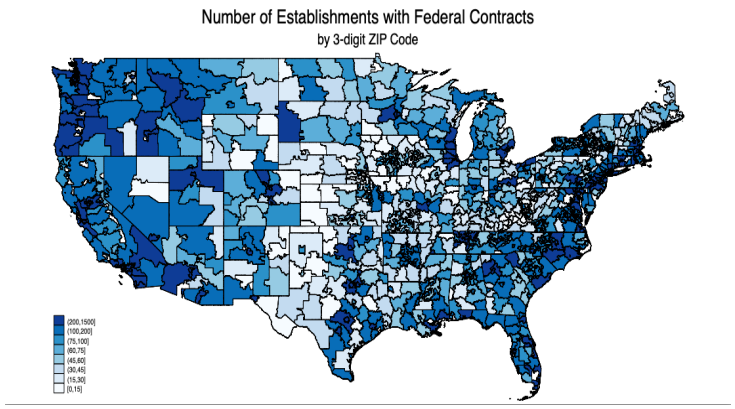


Figure B.1: *Three digit zipcode areas.*

three.[79]

In our first step, as a reference, we use the establishments' names in the FPDS and refer to these data as the *names' training data* in the rest of this section. As a counterpart, we refer to the establishments' names in the OFCCP Data as our *names' test or query data*. We map each of the documents (establishments' names) in the names' training data into a vector of the tf-idf scores of the document's n-grams. The tf-idf scores are normalized so that they have norm 1 and they belong to $\mathbb{R}^K$ where K denotes the total number of distinct n-grams in the training corpus.

We then proceed to map the documents in the names' query data into vectors of tf-idf scores using as our reference the set of distinct n-grams in the training corpus. All documents are mapped into a vector in the unit sphere in $\mathbb{R}^K$. If any of the documents in the names' query data have no n-grams in common with the documents in the training data, the document is mapped into a vector of zeros, $0^K \in \mathbb{R}^K$. Otherwise, vectors have norm 1.

In order to match the names' query data with the names' training data, we compute the *cosine similarity scores*[80] between each possible pair of vectors. If the score between a pair is "high enough" — meaning the angle between them is "small enough," — we proceed to analyze the establishments' addresses. Otherwise we assume the pair is a bad match.

with large data as the time required to implement them grows quadratically with the number of observations.

[79] That is, we split the name of each establishment into 3-character pieces. As an example, the n-grams of the word 'Department', would be ' De', 'Dep', 'epa', 'par', 'art', 'rtm', 'tme', 'men', 'ent', 'nt '.

[80] The cosine similarity score between vectors $x, y \in \mathbb{R}^K$ is given by $x \cdot y / \|x\| \|y\| = \cos(x, y)$. That is, it measures the angle between x and y, where the angle (with range $(0, \pi)$) is mapped into the $(0, 1)$ interval. If the angle between x and y exceeds π, then we measure its negative which will be in the $(0, \pi)$ interval.

For the subset of names that are a potential match (i.e. those that have a cosine similarity score above the threshold), we analyze the similarity of establishments' addresses between the FPDS and the OFCCP data in a similar fashion.

In our second step, we repeat the algorithm just described to merge the resulting FPDS-OFCCP with the LEHD-LBD-SSEL data.

Table B.1: *Advantages of our novel data with respect to the EEO-1 Records*

	LEHD + FPDS	EEO-1 records
Firm characteristics		
Name, Address & industry	✓	✓
Presence of federal contract	✓	✓
Value of federal contract	✓	✗
Workforce by occupation	✗	✓
Workforce by race/ethnicity	✓	✓
Workforce by age/educ	✓	✗
Age of the firm	✓	✗
Worker characteristics		
age, educ, race, gender, ethnicity	✓	✗
earnings, employment, employer	✓	✗

Appendix C. Our Reconstructed EEO Tabulations

Federal contractors with over 50 employees and a federal contract of at least 50,000 usd are also subject to affirmative action regulation. Affirmative action mandates regulated federal contractors and subcontractors to create an affirmative action plan. Such plan identifies under-utilization of minorities and women in any occupation group relative to their availability in the local labor market.

As part of the plan, contractors are required to make "good faith" efforts to rectify under-utilization of minorities, including the use of numerical goals with timetables. The affirmative action plan must be submitted within 120 days of the start of the federal contract, and it must be updated annually for as long as the contract is in place. In order to monitor compliance, the Office of Federal Contract Compliance Programs audits the affirmative action plans of a small fraction of contractors each year.

To create the goals and timetables required, firms compare their workforce racial, ethnic, and sex composition to the EEOC's benchmark, the *EEO Tabulations*,[81] which are based on

[81] See more information at https://www.census.gov/topics/employment/equal-employment-opportunity-tabulation/about/faq.html

5-year ACS data. These tabulations are available at the location-industry-occupation level,[82] however, the LEHD contains no information on the workers' occupations.

We could aggregate the EEO Tabulations to the location-industry level but this could lead to very coarse estimates of the numbers that firms use in practice to compare their labor force compositions and develop their AAP. Hence, we exploit the education variable available in the LEHD to generate our own tabulations of the available workforce in each location — defined by state and county.

C.1 Tabulations' comparison using education to proxy for occupation

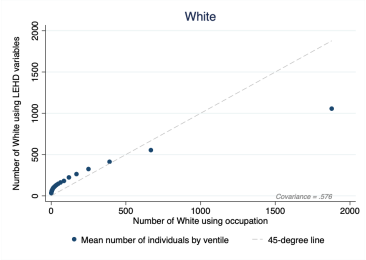
We use the ACS 5-year extract from 2006-2010.

Rather than aggregating the number of workers across occupations within an industry, we first consider aggregating the number of workers by location-industry-education, where education is defined in ranges as in the LEHD, shown below.

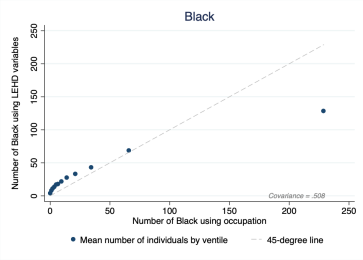
	Education
Group	Level
1	Less than high school (1-8)
2	High school or equivalent, no college (9)
3	Some college or Associate degree (10-12)
4	Bachelor's degree or advanced degree (13-16)

The following binned scatterplots illustrate the correlation between the two measures. We refer to the numbers computed using by location-industry-occupation as the *occupation counts* and those obtained by location-industry-education-age as the *education-age counts*. The plots are constructed by grouping the occupation counts into equally sized bins, specifically into ventiles. Then we compute the mean of each of our counts within each of these bins and the figures show the scatterplot of these data points. For reference, we also plot the identity line (which would underlie the dot in that bin if the means of the counts coincided).

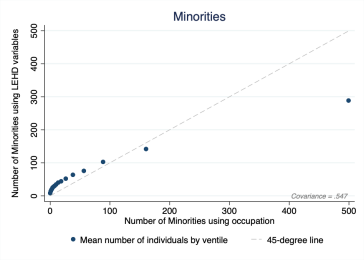
[82] See https://www2.census.gov/EEO_2006_2010/EEO_2006_2010_Tables_Nation(010)_CSV/.



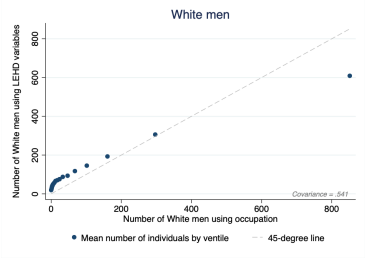
(a) *white workers*



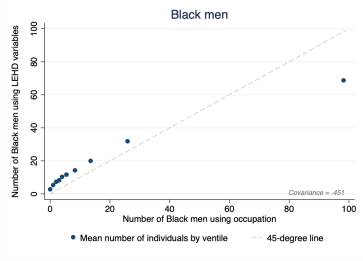
(b) *Black workers*



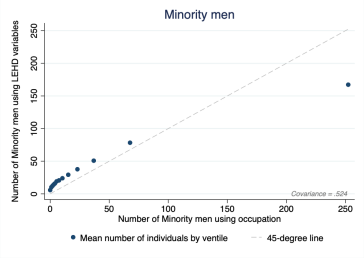
(c) *minority workers*



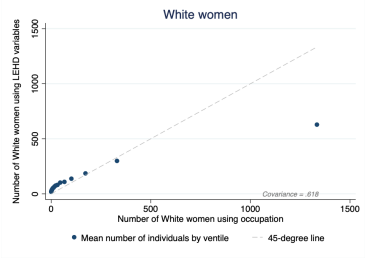
(d) *white male workers*



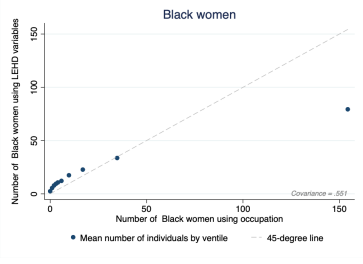
(e) *Black male workers*



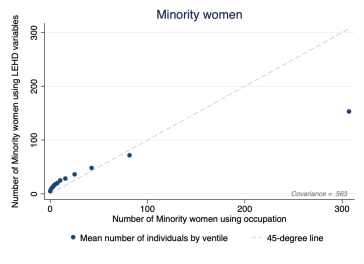
(f) *minority male workers*



(g) *white female workers*



(h) *Black female workers*



(i) *minority female workers*

Notes: The plots are constructed by grouping the occupation counts into equally sized bins, specifically into ventiles. Then we compute the mean of each of our counts within each of these bins and the figures show the scatterplot of these data points. For reference, we also plot the identity line (which would underlie the dot in that bin if the means of the counts coincided).

Appendix D. Details of the results in Section 4

D.1 Estimated coefficients underlying the figures in Section 4

Table D.1: *Estimated effects of becoming subject to EO 11246 on the share of the different protected groups.*

Being subject to EO 11246		Proportion of workers who are Black	Hispanic	minorities	women
Lags					
	$e = -5$	0.0005	−0.0002	0.0000	−0.0007
		(0.0005)	(0.0005)	(0.0007)	(0.0006)
	$e = -4$	0	0	0.0006	−0.0006
		(0.0004)	(0.0005)	(0.0007)	(0.0006)
	$e = -3$	−0.0002	0.0004	0.0000	0.0000
		(0.0003)	(0.0004)	(0.0006)	(0.0006)
	$e = -2$	0.0009	**−0.0003	0.0006	0.0002
		(0.0003)	(0.0003)	(0.0006)	(0.0005)
	$e = -1$	0.0003	0.0001	0.0003	−0.0001
		(0.0003)	(0.0003)	(0.0005)	(0.0005)
Baseline					
	$e = 0$	−0.0005	0.0007	−0.0002	−0.0004
		(0.0005)	(0.0005)	(0.0005)	(0.0004)
Leads					
	$e = 1$	−0.0001	−0.0004	−0.0007	−0.0006
		(0.0006)	(0.0006)	(0.0007)	(0.0007)
	$e = 2$	0.0007	0.0004	0.0008	−0.0008
		(0.0009)	(0.0008)	(0.0010)	(0.0010)
	$e = 3$	0.0002	0.0013	0.0019	−0.0012
		(0.0010)	(0.0011)	(0.0016)	(0.0014)
	$e = 4$	0.001	0.0017	0.003	−0.0002
		(0.0012)	(0.0013)	(0.0018)	(0.0017)
	$e = 5$	0.0007	0.0024	0.0029	−0.0007
		(0.0014)	(0.0016)	(0.0025)	(0.0021)
Pre-treatment mean of dependent variable		0.0562	0.0723	0.183	0.352
Number of observations		662000	662000	662000	662000

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: The table presents the estimates of $\theta_{AA}^{share}(e)$ in equation (2) where the outcome variable corresponds to the proportion of Black new hires, for $e \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$.

Table D.2: *Estimated effects of becoming subject to EO 11246 on the share of new hires in the different protected groups.*

Being subject to EO 11246		Proportion of new hires who are Black	Hispanic	minorities	women
Lags					
	$e = -5$	−0.0001	−0.001	−0.0016	0.0011
		(0.0014)	(0.0016)	(0.0022)	(0.0026)
	$e = -4$	0.0021	0.001	0.0047 *	−0.0016
		(0.0013)	(0.0014)	(0.0019)	(0.0021)
	$e = -3$	−0.0004	0.0018	−0.0003	0.0018
		(0.0010)	(0.0011)	(0.0016)	(0.0018)
	$e = -2$	0.0027 **	−0.0022 *	0	0.0006
		(0.0009)	(0.0010)	(0.0016)	(0.0017)
	$e = -1$	−0.0006	0.0014	−0.0003	−0.0005
		(0.0008)	(0.0010)	(0.0014)	(0.0016)
Baseline					
	$e = 0$	−0.0005	0.0002	−0.0001	−0.0004
		(0.0010)	(0.0010)	(0.0012)	(0.0015)
Leads					
	$e = 1$	0.001	−0.0012	−0.0003	0.0001
		(0.0012)	(0.0013)	(0.0015)	(0.0018)
	$e = 2$	0.0011	−0.0001	0.0002	−0.0025
		(0.0017)	(0.0014)	(0.0019)	(0.0021)
	$e = 3$	0.0014	0.0018	0.0029	−0.001
		(0.0019)	(0.0020)	(0.0026)	(0.0025)
	$e = 4$	0.0041 *	0.0007	0.0036	−0.0009
		(0.0021)	(0.0020)	(0.0030)	(0.0031)
	$e = 5$	0.0029	0.0011	0	−0.0014
		(0.0023)	(0.0024)	(0.0036)	(0.0039)
Pre-treatment mean of dependent variable		0.0702	0.0913	0.22	0.341
Number of observations		594000	594000	594000	594000

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: The table presents the estimates of $\theta_{AA}^{new}(e)$ in equation (2) where the outcome variable corresponds to the proportion of Black new hires, for $e \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$.

D.2 The effects on all protected groups

Figure D.1: *The effect of becoming regulated by EO 11246 on the proportion of protected groups*

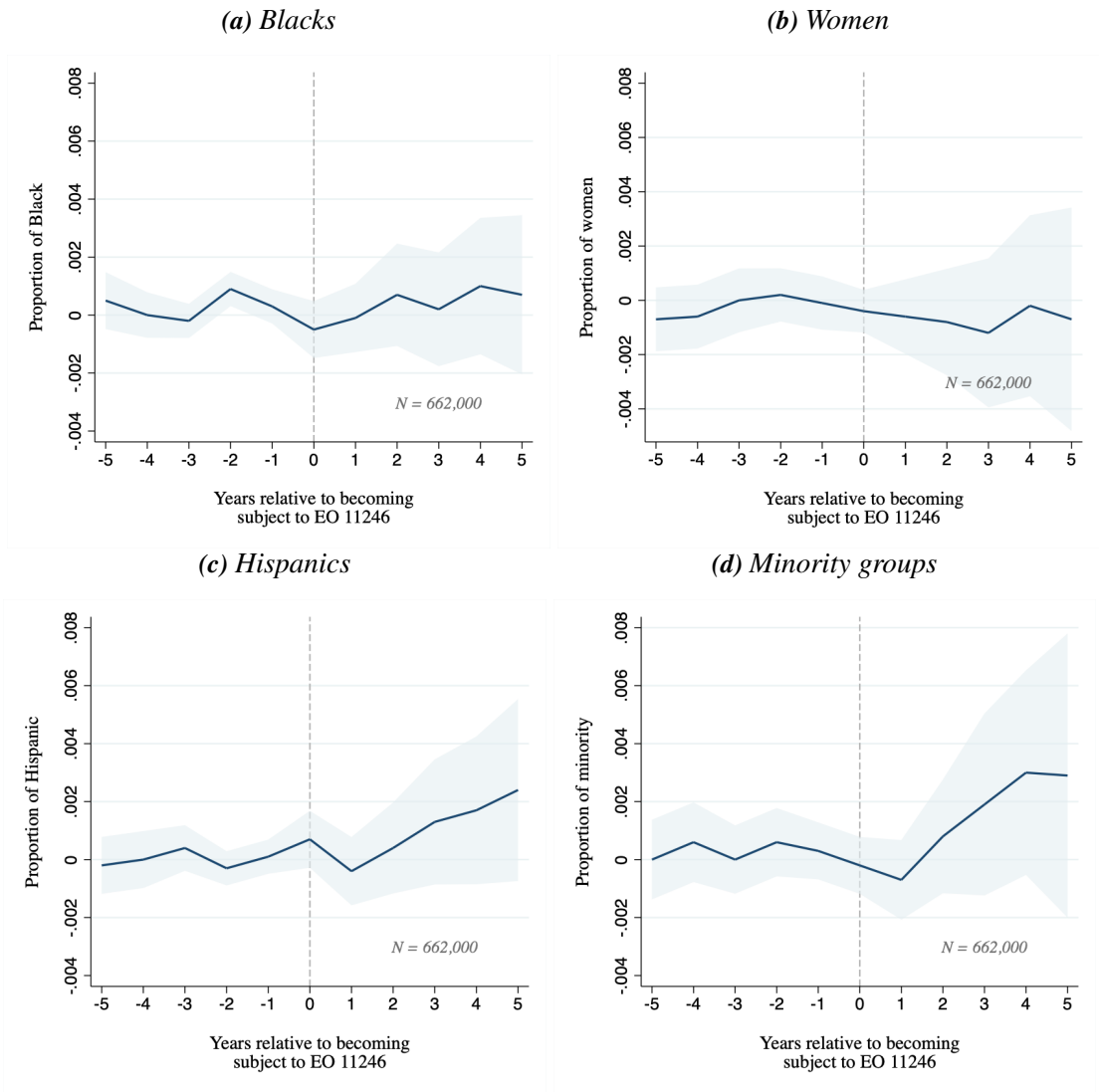


Table D.3: *The effect of becoming subject to EO 11246*

Pre-treatment average proportion of	Black	Hispanic	Minority	Women
	0.05620	0.07230	0.18300	0.35200
		%		
Estimated average treatment effect, $\overline{\hat{\theta}_{AA}^{share}(e)}$	0.0019	0.0074	0.0235	−0.0229
Estimated maximum effect, $\max_e \hat{\theta}_{AA}^{share}(e)$	0.0056	0.0174	0.0549	−0.0070
Upper bound of the 95% CI of maximum effect	0.3352	0.5536	0.6528	0.3132
Pre-treatment average proportion of newly hired	**Black**	**Hispanic**	**Minority**	**Women**
	0.0702	0.0913	0.2200	0.3410
		%		
Estimated average treatment effect, $\overline{\hat{\theta}_{AA}^{new}(e)}$	0.0117	0.0038	0.0231	−0.0347
Estimated maximum effect, $\max_e \hat{\theta}_{AA}^{new}(e)$	0.0288	0.0164	0.0792	0.0034
Upper bound of the 95% CI of maximum effect	0.8216	0.5720	0.9480	0.3628
Results on log-earnings have not undergone disclosure				

Appendix E. Details of the results in Section 5

E.1 Estimated coefficients underlying the figures in Section 5.2

Table E.1: *Estimated effects of becoming subject to EO 11246 with an AAP requirement on the share of workers of the different protected groups.*

Being subject to EO 11246 with an AAP requirement	Proportion of workers who are Black	Hispanic	minorities	women
Lags				
$e = -5$	−0.0003	0.0004	−0.0007	−0.001
	(0.0006)	(0.0007)	(0.0009)	(0.0011)
$e = -4$	0	0.0008	0.0006	−0.0011
	(0.0007)	(0.0012)	(0.0015)	(0.0014)
$e = -3$	0.0007	0	0.0005	0.0017
	(0.0006)	(0.0006)	(0.0009)	(0.0009)
$e = -2$	0.0015 *	−0.0009	0.0014	−0.0002
	(0.0006)	(0.0005)	(0.0008)	(0.0008)
$e = -1$	0.0006	0.0004	0.0008	−0.0006
	(0.0006)	(0.0006)	(0.0008)	(0.0009)
Baseline				
$e = 0$	−0.0006	0.0006	0.0005	0.0001
	(0.0006)	(0.0005)	(0.0006)	(0.0008)
Leads				
$e = 1$	−0.0015	0.0004	−0.0002	−0.001
	(0.0009)	(0.0008)	(0.0011)	(0.0013)
$e = 2$	−0.0021	0.0016	0.0005	−0.0017
	(0.0012)	(0.0010)	(0.0014)	(0.0014)
$e = 3$	−0.0024	0.0014	0.0002	0
	(0.0013)	(0.0014)	(0.0020)	(0.0018)
$e = 4$	−0.0014	−0.0002	−0.0002	−0.001
	(0.0015)	(0.0016)	(0.0021)	(0.0019)
$e = 5$	−0.002	−0.0017	−0.0024	−0.0004
	(0.0017)	(0.0021)	(0.0026)	(0.0023)
Pre-treatment mean of dependent variable	0.086	0.076	0.212	0.391
Number of observations	168000	168000	168000	168000

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: The table presents the estimates of $\theta_{AAP}^{share}(e)$ in equation (4) where the outcome variable corresponds to the proportion of Black new hires, for $e \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$.

Table E.2: *Estimated effects of becoming subject to EO 11246 with an AAP requirement on the share of new hires in the different protected groups.*

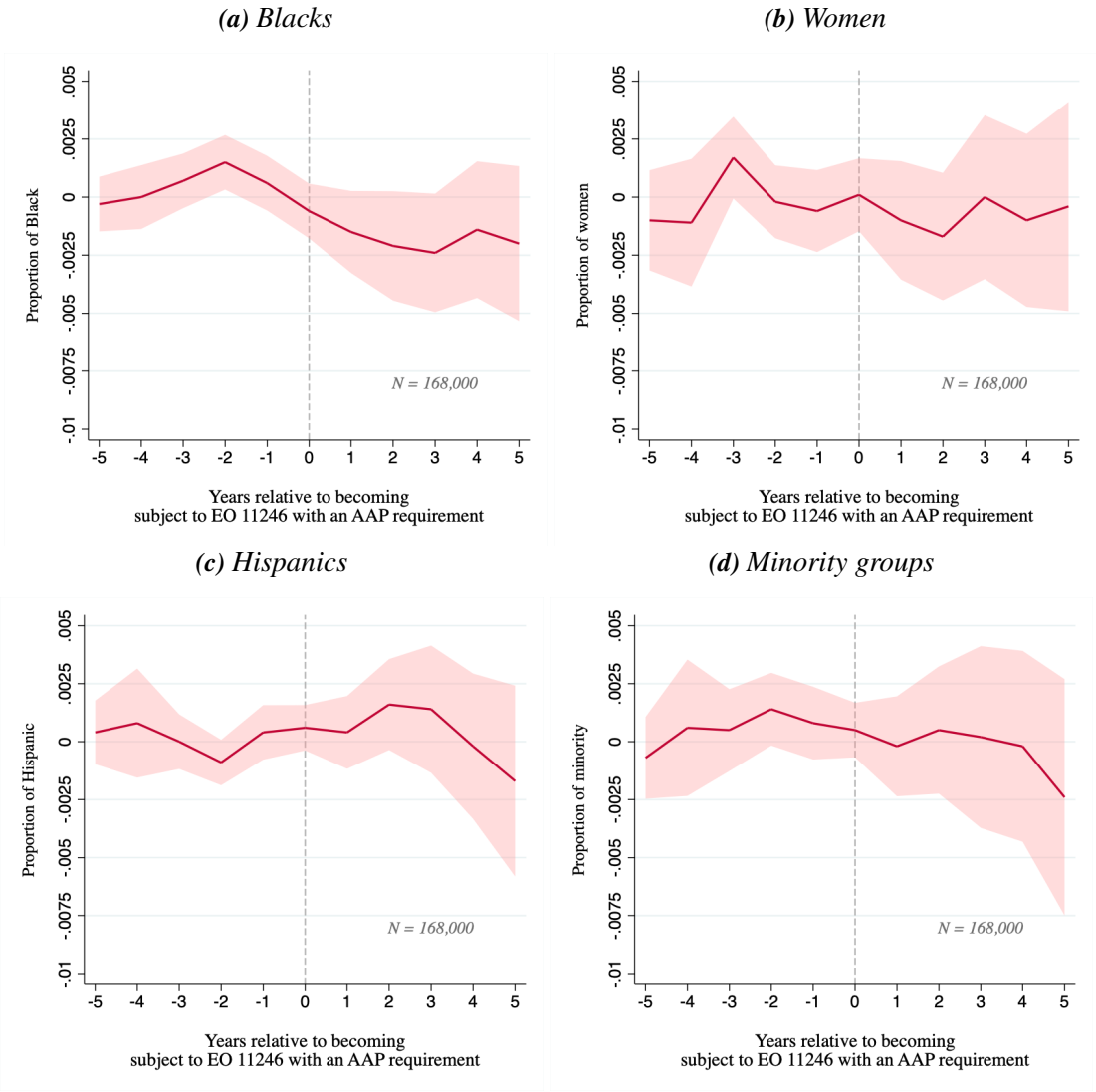
Being subject to EO 11246 with an AAP requirement	Proportion of new hires who are Black	Hispanic	minorities	women
Lags				
$e = -5$	0.0014	0.0008	−0.0005	0.0059
	(0.0021)	(0.0023)	(0.0032)	(0.0041)
$e = -4$	0.002	0.0038	0.0097	**−0.0066
	(0.0022)	(0.0024)	(0.0036)	(0.0036)
$e = -3$	0.0013	−0.0007	−0.0014	0.0046
	(0.0019)	(0.0021)	(0.0029)	(0.0030)
$e = -2$	0.0006	−0.0034 *	0.001	−0.0043
	(0.0018)	(0.0016)	(0.0025)	(0.0029)
$e = -1$	0.0012	0.0027	0.0007	0.0018
	(0.0016)	(0.0015)	(0.0022)	(0.0026)
Baseline				
$e = 0$	−0.001	−0.001	0.0002	−0.0018
	(0.0016)	(0.0016)	(0.0021)	(0.0025)
Leads				
$e = 1$	−0.0002	−0.0014	0.0007	−0.002
	(0.0018)	(0.0020)	(0.0027)	(0.0031)
$e = 2$	−0.0025	0.0027	0.0003	−0.0041
	(0.0022)	(0.0021)	(0.0029)	(0.0033)
$e = 3$	−0.0017	0.0031	0.0025	−0.0009
	(0.0023)	(0.0043)	(0.0047)	(0.0043)
$e = 4$	0.0025	0.0008	0.005	−0.0025
	(0.0026)	(0.0036)	(0.0044)	(0.0038)
$e = 5$	−0.0033	0.001	−0.0006	−0.0034
	(0.0025)	(0.0042)	(0.0049)	(0.0045)
Pre-treatment mean of dependent variable	0.102	0.089	0.245	0.4
Number of observations	163000	163000	163000	163000

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: The table presents the estimates of $\theta^{new}_{AAP}(e)$ in equation (4) where the outcome variable corresponds to the proportion of Black new hires, for $e \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$.

E.2 The effects on all protected groups

Figure E.1: *The effect of becoming regulated by EO 11246 with an AAP requirement on the proportion of protected groups*



E.3 Results for all protected groups from the RD specification in Section 5.3

Table H.2 presents the estimates of β^k from equation (5) for these two outcomes. The results do not indicate that the affirmative action plan operates through these alternative channels, although a future version of this paper will explore these additional margins further.

Table E.3: *The effect of becoming subject to EO 11246 with an AAP requirement*

Pre-treatment average proportion of	Black	Hispanic	Minority	Women
	0.0860	0.0760	0.2120	0.3910
		%		
Estimated average treatment effect, $\overline{\hat{\theta}_{AA}^{share}(e)}$	−0.0143	0.0027	−0.0057	−0.0261
Estimated maximum effect, $\max_e \hat{\theta}_{AA}^{share}(e)$	−0.0052	0.0122	0.0106	0.0039
Upper bound of the 95% CI of maximum effect	0.0576	0.3560	0.3244	0.1668
Pre-treatment average proportion of newly hired	**Black**	**Hispanic**	**Minority**	**Women**
	0.1020	0.0890	0.2450	0.4000
		%		
Estimated average treatment effect, $\overline{\hat{\theta}_{AA}^{new}(e)}$	−0.0105	0.0077	0.0331	−0.0980
Estimated maximum effect, $\max_e \hat{\theta}_{AA}^{new}(e)$	0.0255	0.0276	0.1225	−0.0360
Upper bound of the 95% CI of maximum effect	0.7596	1.1528	1.1712	0.7528
Results on log-earnings have not undergone disclosure				

Table E.4: *Regression discontinuity: Proportion of workers from other protected groups*

	$t-1$ (baseline)	$t+0$	$t+1$	$t+5$
Proportion minority workers	−	−	+	−
Proportion female workers	−	+	+	−
N	*2,600*	*2,600*	*2,200*	*1,800*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: FPDS-LEHD Data.
Notes: This table presents the qualitative results for the estimates of β^k in model (5), for $k \in \{-1, 0, 1, 5\}$. − indicates a negative point estimate, + a positive one. *N* is the number of establishments, rounded to the nearest 100.

Appendix F. Details of the results in Section 6

F.1 Estimated coefficients underlying the figures in Section 6

Table F.1: *Estimated effects of being audited by the OFCCP on the share of workers from the different protected groups.*

	Being subject to an audit	Proportion of workers who are Black	Hispanic	minorities	women
Lags					
	$e = -5$	0.0012	−0.0015	0.0009	0.0007
		(0.0013)	(0.0013)	(0.0021)	(0.0017)
	$e = -4$	−0.0019	−0.0003	−0.0015	0.0015
		(0.0013)	(0.0012)	(0.0016)	(0.002)
	$e = -3$	−0.0004	−0.0003	−0.0024	−0.0024
		(0.0011)	(0.0008)	(0.0014)	(0.0013)
	$e = -2$	−0.0012	0	−0.0011	0.002
		(0.0012)	(0.0011)	(0.0016)	(0.0019)
	$e = -1$	0.0023	−0.0007	0.0031	0.0037
		(0.0016)	(0.0009)	(0.0017)	(0.002)
Baseline					
	$e = 0$	0.001	−0.0008	0.0013	−0.0008
		(0.0013)	(0.0013)	(0.0014)	(0.0015)
Leads					
	$e = 1$	0.0001	0.0003	0.0005	−0.0009
		(0.0015)	(0.0012)	(0.0024)	(0.002)
	$e = 2$	0.0005	−0.0008	−0.0008	0.0013
		(0.0014)	(0.0014)	(0.0024)	(0.0027)
	$e = 3$	−0.0024	0.0006	−0.0025	−0.0004
		(0.0018)	(0.0015)	(0.0029)	(0.0027)
	$e = 4$	−0.0039	−0.0006	−0.0045	0.0001
		(0.0025)	(0.0016)	(0.0036)	(0.0033)
	$e = 5$	−0.0018	−0.001	−0.0035	0.0005
		(0.0025)	(0.0019)	(0.0044)	(0.0041)
Pre-treatment mean of dependent variable		0.1048	0.638	0.2708	0.4007
Number of observations		839000	839000	839000	839000

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: The table presents the estimates of $\theta^{share}_{audit}(e)$ in equation (7) where the outcome variable corresponds to the proportion of Black new hires, for $e \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$.

Table F.2: *Estimated effects of being audited by the OFCCP on the share of new hires in the different protected groups.*

	Proportion of new hires who are			
Being subject to an audit	Black	Hispanic	minorities	women
Lags				
$e = -5$	0.0027	−0.0012	0.0036	0.0051
	(0.0039)	(0.0037)	(0.0077)	(0.0071)
$e = -4$	−0.0048	−0.0016	−0.0077	0.0047
	(0.0040)	(0.0035)	(0.0058)	(0.0063)
$e = -3$	0.0025	0.0009	−0.0002	−0.0087
	(0.0040)	(0.0031)	(0.0050)	(0.0053)
$e = -2$	0.0011	−0.0004	−0.0002	0.0055
	(0.0033)	(0.0034)	(0.0060)	(0.0068)
$e = -1$	0.007	−0.0039	0.0081	0.0095
	(0.0040)	(0.0031)	(0.0060)	(0.0071)
Baseline				
$e = 0$	0.0005	0.0051	−0.0015	−0.0091
	(0.0035)	(0.0035)	(0.0064)	(0.0066)
Leads				
$e = 1$	−0.003	0.0032	−0.0084	−0.0127
	(0.0046)	(0.0040)	(0.0079)	(0.0074)
$e = 2$	−0.0009	0.0036	−0.0091	−0.012
	(0.0042)	(0.0046)	(0.0072)	(0.0072)
$e = 3$	−0.0059	0.0028	−0.0142	−0.0042
	(0.0052)	(0.0054)	(0.0083)	(0.0088)
$e = 4$	−0.0085	0.0012	−0.0290 **	0.0009
	(0.0064)	(0.0053)	(0.0109)	(0.0108)
$e = 5$	−0.0058	0.0005	−0.0244 *	−0.0018
	(0.0066)	(0.0059)	(0.0106)	(0.0132)
Pre-treatment mean of dependent variable	0.1159	0.07485	0.2938	0.3993
Number of observations	797000	797000	797000	797000

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: The table presents the estimates of $\theta^{new}_{audit}(e)$ in equation (7) where the outcome variable corresponds to the proportion of Black new hires, for $e \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$.

F.2 The effects on all protected groups

Figure F.1: *The effect of being audited on the proportion of protected groups*

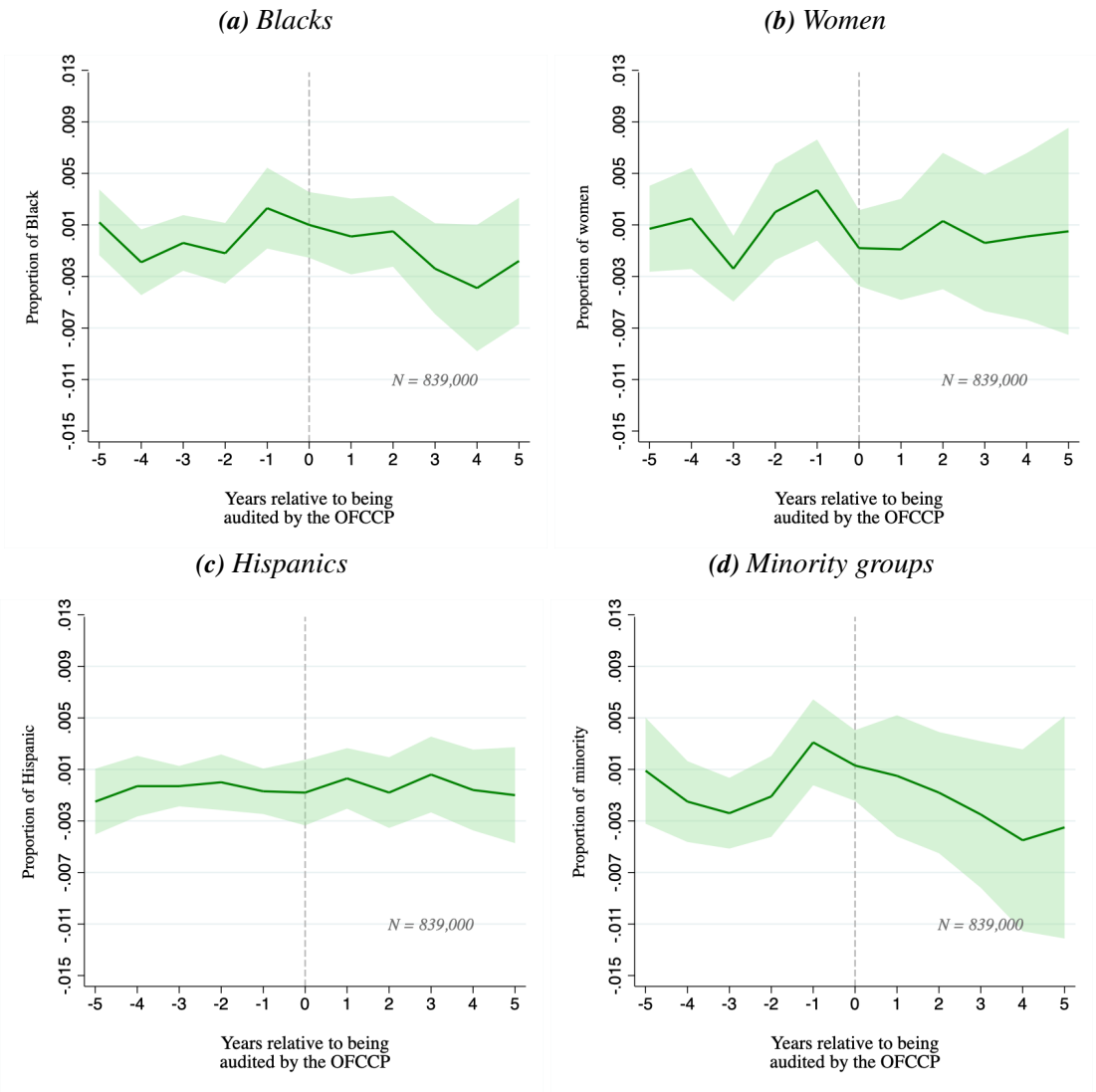


Table F.3: *The effect of being audited*

Pre-treatment average proportion of	Black	Hispanic	Minority	Women
	0.1048	0.6380	0.2708	0.4007
		%		
Estimated average treatment effect, $\overline{\hat{\theta}_{AA}^{share}(e)}$	−0.0114	−0.0245	−0.0429	−0.0013
Estimated maximum effect, $\max_e \hat{\theta}_{AA}^{share}(e)$	0.0105	0.0383	0.0352	0.0521
Upper bound of the 95% CI of maximum effect	0.3548	0.3540	0.4044	0.6592
Pre-treatment average proportion of newly hired	**Black**	**Hispanic**	**Minority**	**Women**
	0.1159	0.07485	0.2938	0.3993
		%		
Estimated average treatment effect, $\overline{\hat{\theta}_{AA}^{new}(e)}$	−0.0456	0.0205	−0.4241	−0.2589
Estimated maximum effect, $\max_e \hat{\theta}_{AA}^{new}(e)$	0.0058	0.0382	−0.0441	0.0359
Upper bound of the 95% CI of maximum effect	0.7360	1.1960	1.1044	2.2068
Results on log-earnings have not undergone disclosure				

Appendix G. Additional Figures

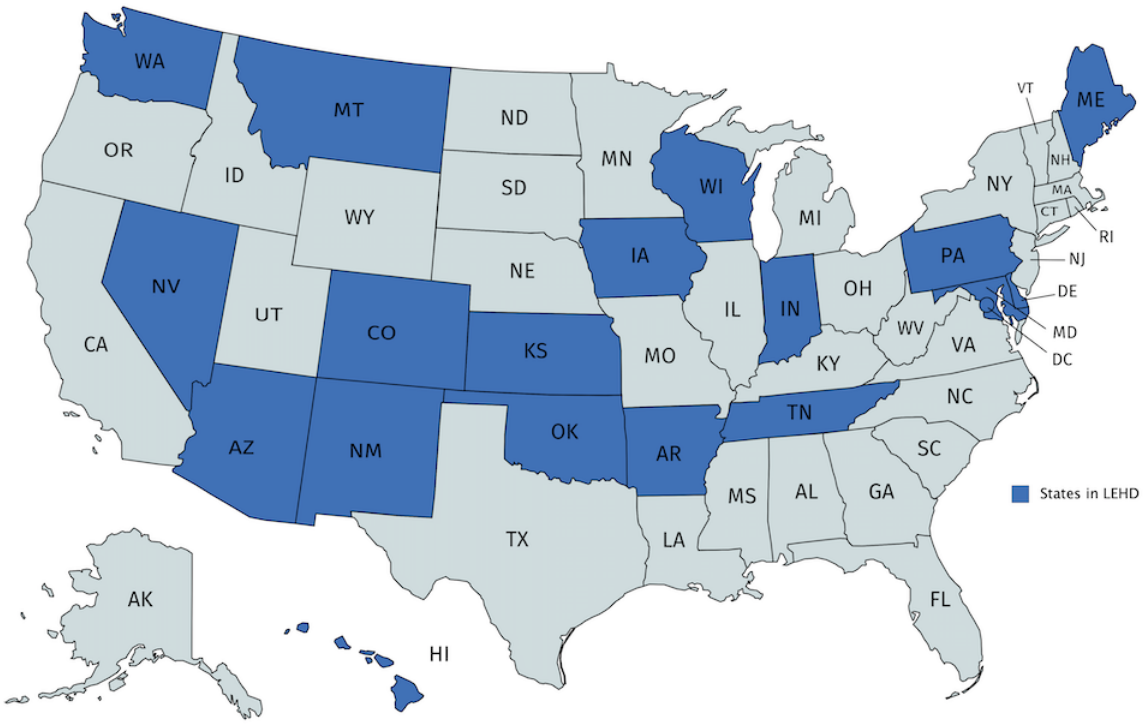
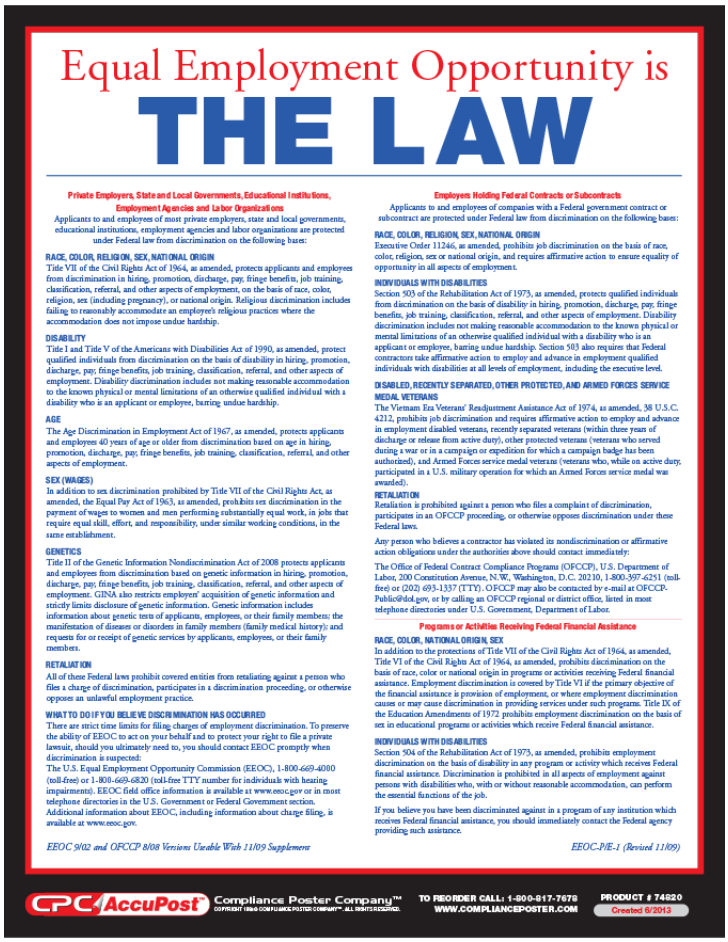


Figure G.2: *States for which we have access to LEHD data*



Equal Employment Opportunity is

THE LAW

Private Employers, State and Local Governments, Educational Institutions, Employment Agencies and Labor Organizations

Applicants to and employees of most private employers, state and local governments, educational institutions, employment agencies and labor organizations are protected under Federal law from discrimination on the following bases:

RACE, COLOR, RELIGION, SEX, NATIONAL ORIGIN

Title VII of the Civil Rights Act of 1964, as amended, protects applicants and employees from discrimination in hiring, promotion, discharge, pay, fringe benefits, job training, classification, referral, and other aspects of employment, on the basis of race, color, religion, sex (including pregnancy), or national origin. Religious discrimination includes failing to reasonably accommodate an employee's religious practices where the accommodation does not impose undue hardship.

DISABILITY

Title I and Title V of the Americans with Disabilities Act of 1990, as amended, protect qualified individuals from discrimination on the basis of disability in hiring, promotion, discharge, pay, fringe benefits, job training, classification, referral, and other aspects of employment. Disability discrimination includes not making reasonable accommodation to the known physical or mental limitations of an otherwise qualified individual with a disability who is an applicant or employee, barring undue hardship.

AGE

The Age Discrimination in Employment Act of 1967, as amended, protects applicants and employees 40 years of age or older from discrimination based on age in hiring, promotion, discharge, pay, fringe benefits, job training, classification, referral, and other aspects of employment.

SEX (WAGES)

In addition to sex discrimination prohibited by Title VII of the Civil Rights Act, as amended, the Equal Pay Act of 1963, as amended, prohibits sex discrimination in the payment of wages to women and men performing substantially equal work, in jobs that require equal skill, effort, and responsibility, under similar working conditions, in the same establishment.

GENETICS

Title II of the Genetic Information Nondiscrimination Act of 2008 protects applicants and employees from discrimination based on genetic information in hiring, promotion, discharge, pay, fringe benefits, job training, classification, referral, and other aspects of employment. GINA also restricts employers' acquisition of genetic information and strictly limits disclosure of genetic information. Genetic information includes information about genetic tests of applicants, employees, or their family members; the manifestation of diseases or disorders in family members (family medical history); and requests for or receipt of genetic services by applicants, employees, or their family members.

RETALIATION

All of these Federal laws prohibit covered entities from retaliating against a person who files a charge of discrimination, participates in a discrimination proceeding, or otherwise opposes an unlawful employment practice.

WHAT TO DO IF YOU BELIEVE DISCRIMINATION HAS OCCURRED

There are strict time limits for filing charges of employment discrimination. To preserve the ability of EEOC to act on your behalf and to protect your right to file a private lawsuit, should you ultimately need to, you should contact EEOC promptly when discrimination is suspected:

The U.S. Equal Employment Opportunity Commission (EEOC), 1-800-669-4000 (toll-free) or 1-800-669-6820 (toll-free TTY number for individuals with hearing impairments). EEOC field office information is available at www.eeoc.gov or in most telephone directories in the U.S. Government or Federal Government section.

Additional information about EEOC, including information about charge filing, is available at www.eeoc.gov.

EEOC 9/02 and OFCCP 8/08 Versions Usable With 11/09 Supplement

Employers Holding Federal Contracts or Subcontracts

Applicants to and employees of companies with a Federal government contract or subcontract are protected under Federal law from discrimination on the following bases:

RACE, COLOR, RELIGION, SEX, NATIONAL ORIGIN

Executive Order 11246, as amended, prohibits job discrimination on the basis of race, color, religion, sex or national origin, and requires affirmative action to ensure equality of opportunity in all aspects of employment.

INDIVIDUALS WITH DISABILITIES

Section 503 of the Rehabilitation Act of 1973, as amended, protects qualified individuals from discrimination on the basis of disability in hiring, promotion, discharge, pay, fringe benefits, job training, classification, referral, and other aspects of employment. Disability discrimination includes not making reasonable accommodation to the known physical or mental limitations of an otherwise qualified individual with a disability who is an applicant or employee, barring undue hardship. Section 503 also requires that Federal contractors take affirmative action to employ and advance in employment qualified individuals with disabilities at all levels of employment, including the executive level.

DISABLED, RECENTLY SEPARATED, OTHER PROTECTED, AND ARMED FORCES SERVICE MEDAL VETERANS

The Vietnam Era Veterans' Readjustment Assistance Act of 1974, as amended, 38 U.S.C. 4212, prohibits job discrimination and requires affirmative action to employ and advance in employment disabled veterans, recently separated veterans (within three years of discharge or release from active duty), other protected veterans (veterans who served during a war or in a campaign or expedition for which a campaign badge has been authorized), and Armed Forces service medal veterans (veterans who, while on active duty, participated in a U.S. military operation for which an Armed Forces service medal was awarded).

RETALIATION

Retaliation is prohibited against a person who files a complaint of discrimination, participates in an OFCCP proceeding, or otherwise opposes discrimination under these Federal laws.

Any person who believes a contractor has violated its nondiscrimination or affirmative action obligations under the authorities above should contact immediately:

The Office of Federal Contract Compliance Programs (OFCCP), U.S. Department of Labor, 200 Constitution Avenue, N.W., Washington, D.C. 20210, 1-800-397-6251 (toll-free) or (202) 693-1337 (TTY). OFCCP may also be contacted by e-mail at OFCCP-Public@dol.gov, or by calling an OFCCP regional or district office, listed in most telephone directories under U.S. Government, Department of Labor.

Programs or Activities Receiving Federal Financial Assistance

RACE, COLOR, NATIONAL ORIGIN, SEX

In addition to the protections of Title VII of the Civil Rights Act of 1964, as amended, Title VI of the Civil Rights Act of 1964, as amended, prohibits discrimination on the basis of race, color or national origin in programs or activities receiving Federal financial assistance. Employment discrimination is covered by Title VI if the primary objective of the financial assistance is provision of employment, or where employment discrimination causes or may cause discrimination in providing services under such programs. Title IX of the Education Amendments of 1972 prohibits employment discrimination on the basis of sex in educational programs or activities which receive Federal financial assistance.

INDIVIDUALS WITH DISABILITIES

Section 504 of the Rehabilitation Act of 1973, as amended, prohibits employment discrimination on the basis of disability in any program or activity which receives Federal financial assistance. Discrimination is prohibited in all aspects of employment against persons with disabilities who, with or without reasonable accommodation, can perform the essential functions of the job.

If you believe you have been discriminated against in a program of any institution which receives Federal financial assistance, you should immediately contact the Federal agency providing such assistance.

EEOC-P/E-1 (Revised 11/09)

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Figure G.1: *Sample poster*

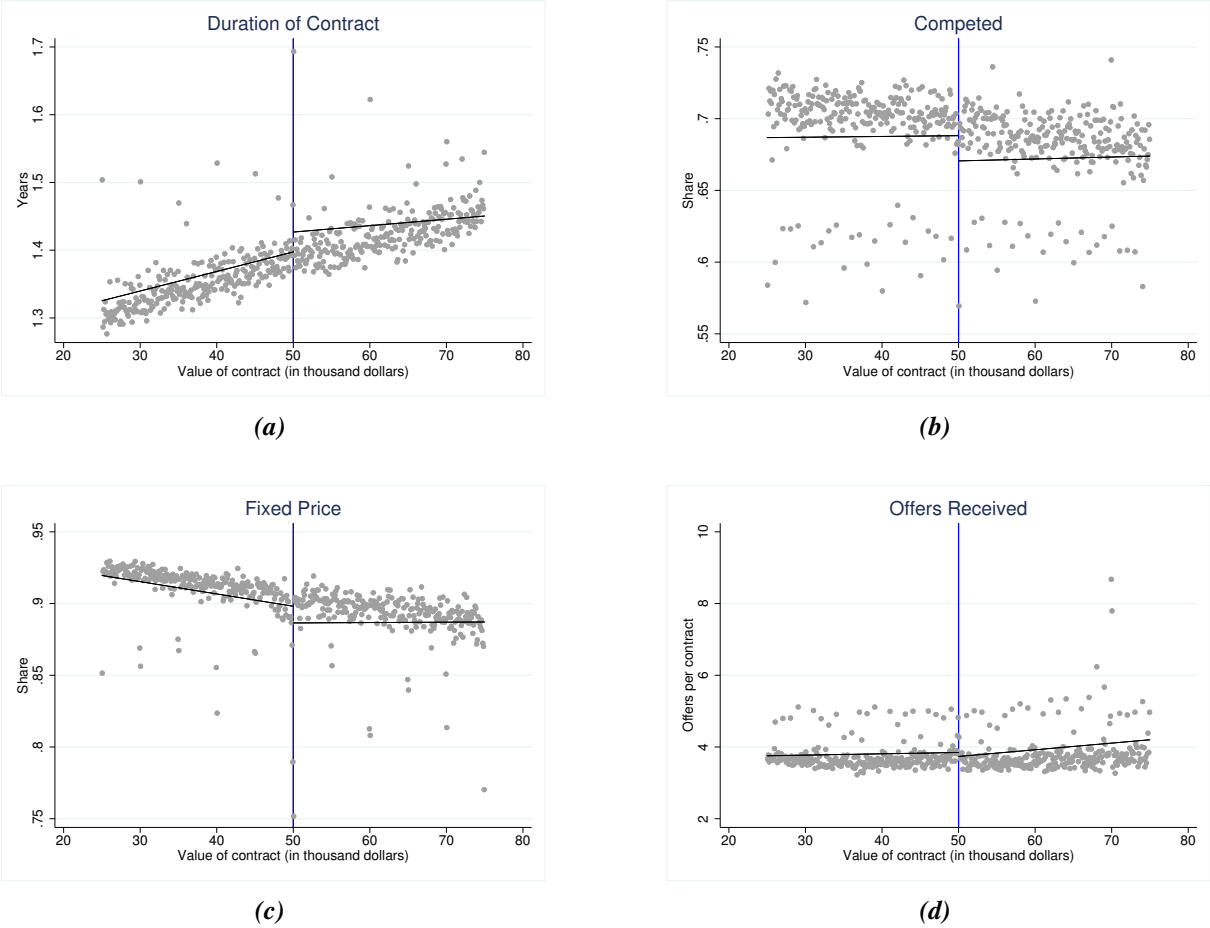
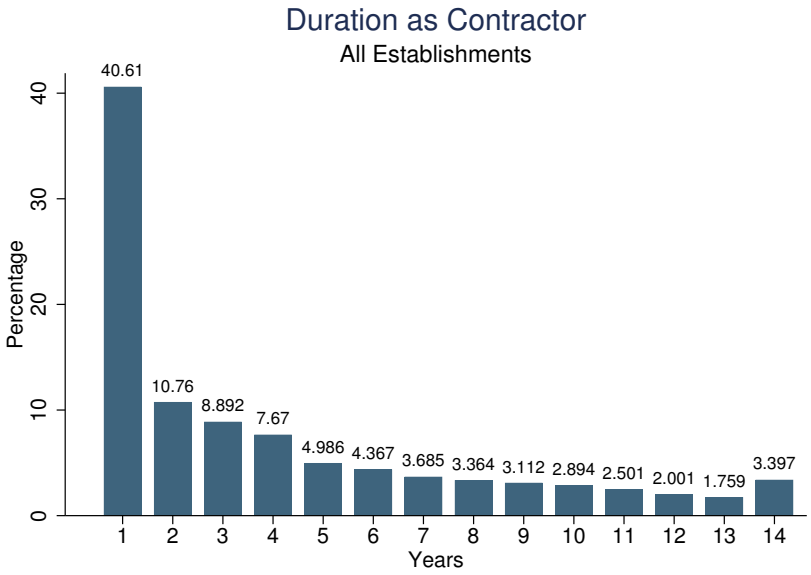
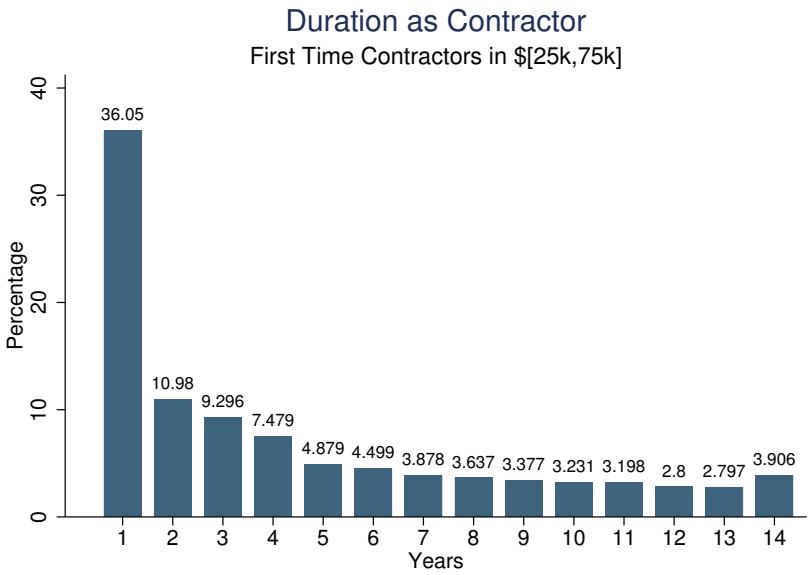


Figure G.3: *Contracts characteristics around $50,000 threshold*

Notes: These figures show the mean of each variable by bins of the value of the contract. Panel (a) shows duration of the contracts, in years. Panel (b) and (c) the share of contracts who are competed and fixed price, respectively, and panel (d) the number of offers received. Bins are equally spaced and of size $250. Vertical line represents the threshold of $50,000. We fit a first-order polynomial to approximate the population conditional mean functions at each side of the threshold.



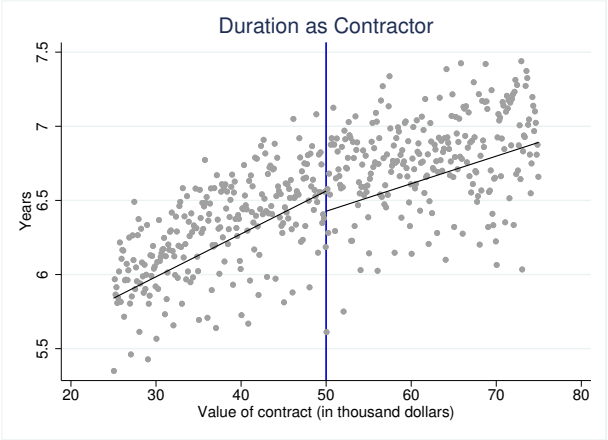
(a)



(b)

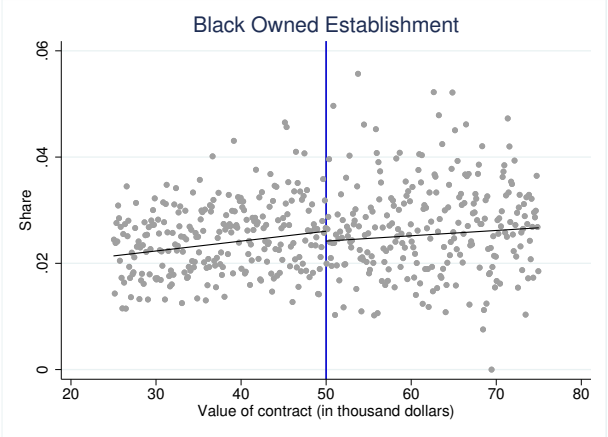
Figure G.4: *Duration as contractors*

Notes: Panel (a) in this figure shows the distribution of the duration (in years) of all contractors observed in the FPDS data, and panel (b) that of the establishments which are first observed as contractors with a contract of a value between $25,000 and $75,000. Duration as contractor is measured as the years in between an establishment is first and last observed in the FPDS data.



(a)

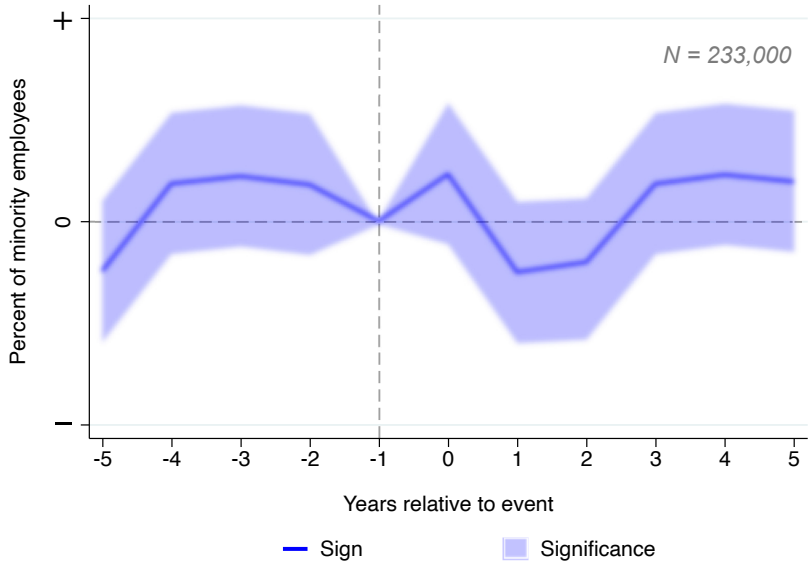
(b)



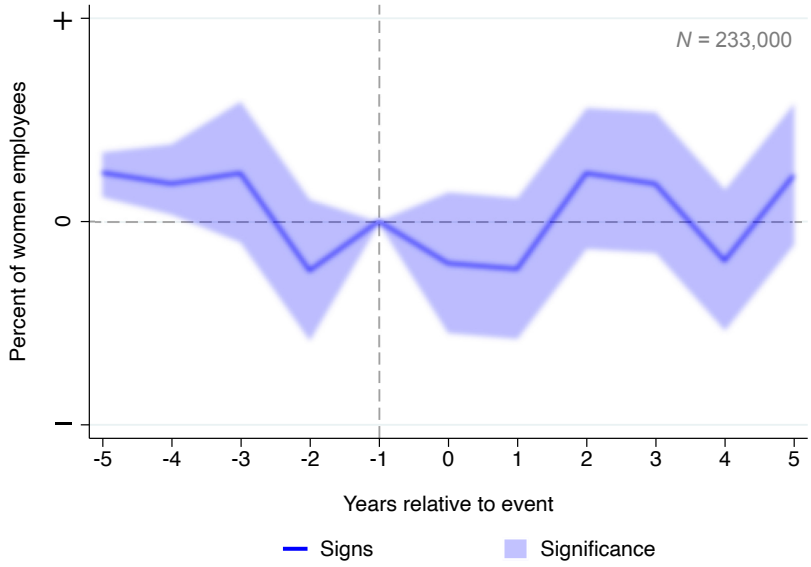
(c)

Figure G.5: *Characteristics of contractors*

Notes: These figures show the averages of each variable by bins of the value of the contract. Pael (a) shows the duration of establishments as contractors, in years. Panel (b) shows the share of establishments that are minority-owned. Panel (c) the share of Black-owned establishments. Bins are equally spaced and of size $250. Vertical line represents the threshold of $50,000. We fit a first-order polynomial to approximate the population conditional mean functions at each side of the threshold.

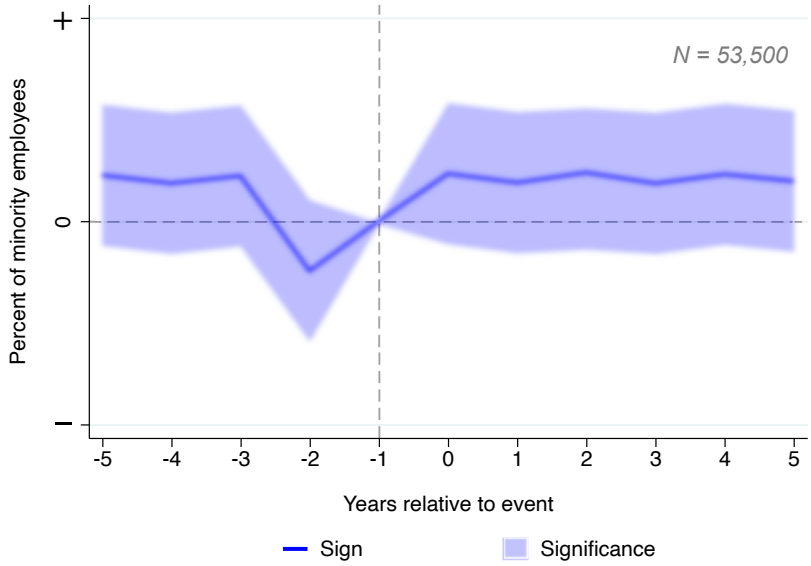


(a) *Minority workers*

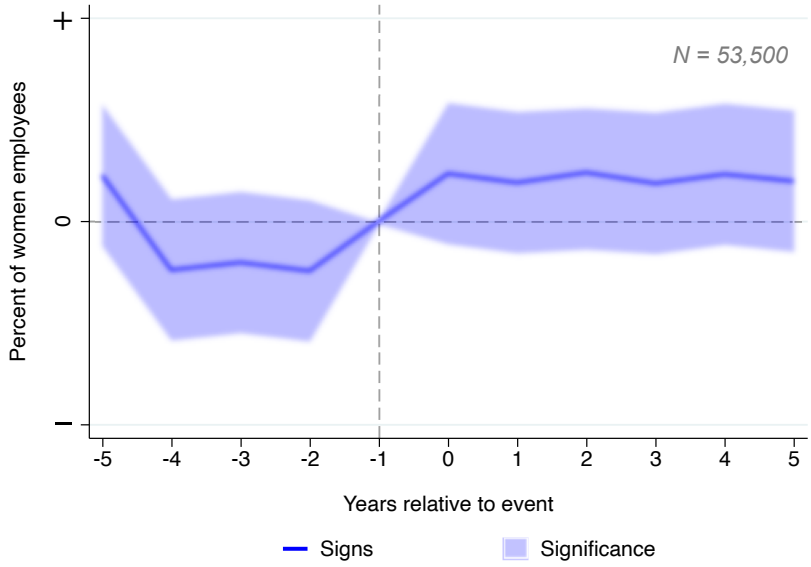


(b) *Female workers*

Figure G.6: *Event study - Effect of becoming a contractor on minority and female workers*
Notes: This figure plots event study coefficients qualitatively. Solid line represents the sign of a point estimate. Solid lines above (below) zero represent a positive (negative) point estimate. Within positive or negative point estimates, the differences in levels and slopes of the solid line is for illustration purposes only, and does not indicate a larger or lower magnitude. 90% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 50,000. These are estimates using model (8). The outcome variable in panel (a) is the percent of minority workers in an establishment, panel (b) the percent of female workers. The coefficient for the year prior to the event (θ_{-1}) is normalized to zero. The estimated model includes county-by-year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.



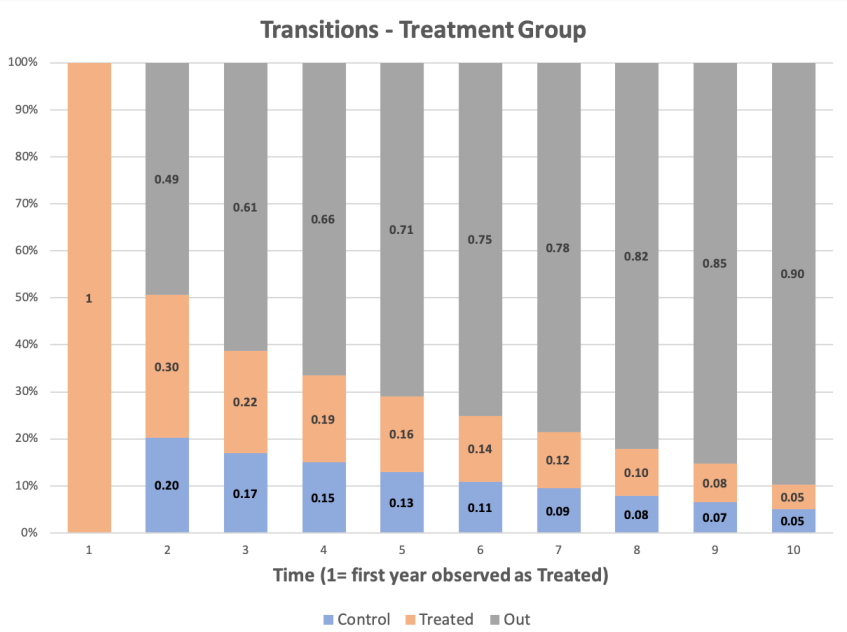
(a) *Minorities*



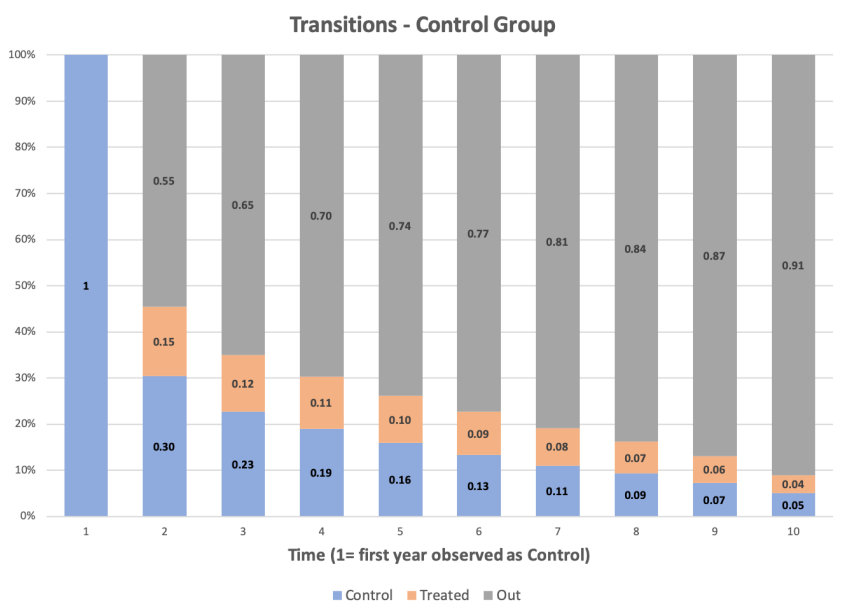
(b) *Females*

Figure G.7: *Event study - Effect of AAP on minority and female workers*

Notes: This figure plots event study coefficients qualitatively. The event is the first time that an establishment obtains a contract worth $50,000 or more. The control group are contractor establishments that never cross that threshold. Solid line represents the sign of a point estimate. Solid lines above (below) zero represent a positive (negative) point estimate. Within positive or negative point estimates, the differences in levels and slopes of the solid line is for illustration purposes only, and does not indicate a larger or lower magnitude. 90% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 500. The outcome variable in panel (a) is the percent of minority workers in an establishment, panel (b) the percent of female workers. The coefficient for the year prior to the event (θ_{-1}) is normalized to zero. The estimated model includes county-by-year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.



(a) *Treated establishments*



(b) *Control establishments*

Figure G.8: *Treatment status transitions*

Notes: Panel (a) in this figure shows the treatment transitions for the treatment group, and panel (b) for the control group. The x axis represents the years after being first observed as contractor, with 1 being the first year. "Out" means that the establishment is out of the FPDS data, and hence it is not a contractor any more.

Appendix H. Robustness Checks

H.1 The effect of becoming regulated by EO 11246

One concern is that, in a context of null results, we would wonder whether they are an artifact of point estimates that are imprecisely estimated rather than a true zero effect. The fact that the estimates fluctuate around zero in positive and negative signs should alleviate this concern, but nevertheless, we did a second robustness exercise that aims to increase the precision of our estimates by pooling the before- and after-regulation years in the event study in order to gain statistical precision. These results have not undergone Census disclosure.

H.1.1 TWFE Estimates

Our empirical results from from estimating (2) are qualitatively robust to using a naive TWFE estimator similar to the one of Miller (2017),

$$y_{jt} = \sum_{k=-5}^{k=5} \theta_k D_{jt}^k + \gamma X_{jt} + \alpha_j + \lambda_t + \varepsilon_{jt} \tag{8}$$

where y_{jt} denotes the share of Black workers in establishment j at time t. α_j denotes establishment j's fixed effect, λ_t is a year fixed effect, X_{jt} is a quadratic polynomial on the log of the establishment size. D_{jt}^k are leads and lags of establishment j first becoming a contractor, defined as

$$D_{jt}^k = D_j \mathbb{1}(t = \tau_j + k)$$

with D_j an indicator for whether the establishment first becomes a contractor, and τ_j is the year the establishment first becomes a contractor. We normalize $\theta_{-1} = 0$, and cluster standard errors at the firm level.

Figure H.1 plots qualitatively the point estimates and confidence intervals of the estimated θ_j's in the equation above using the linked LEHD-FPDS data. These qualitative findings confirm our results using CSDiD: we do not find a causal effect of becoming regulated with affirmative action on the percent of Black employees that an establishment employs. Both before and after the regulation, the point estimates fluctuate around zero, and they are not significant even at the 90% level. Appendix G shows the estimates of θ_j for the share of minorities and women, and we still fail to detect an effect of the policy on those outcomes.

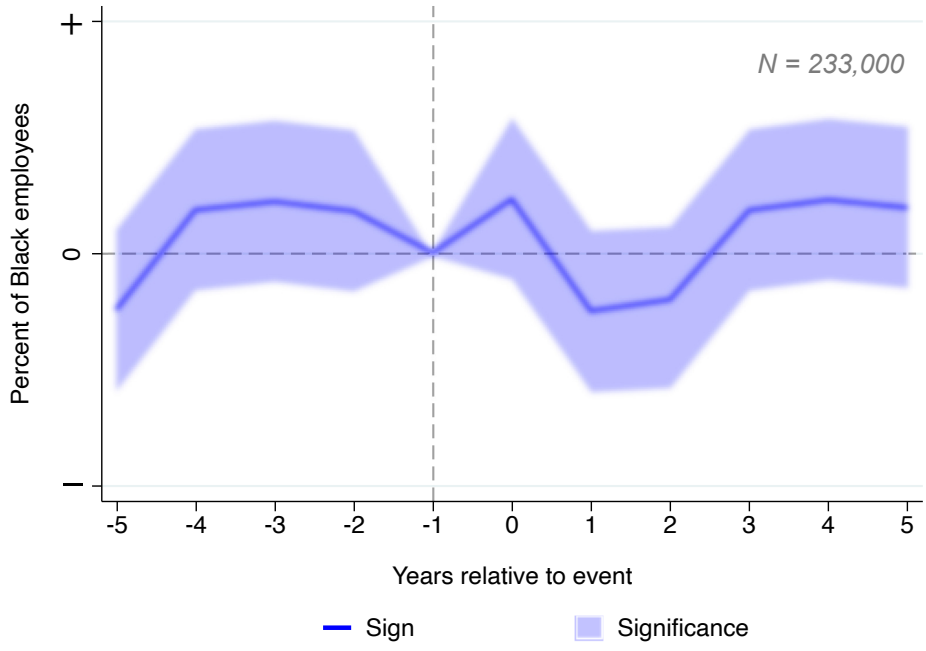


Figure H.1: *Event study - Effect of becoming a contractor on Black workers*
Source: FPDS-LEHD Data.
Notes: This figure plots event study coefficients **qualitatively**. The solid line represents the sign of a point estimate. Solid lines above (below) zero represent a positive (negative) point estimate. Within positive or negative point estimates, the differences in levels and slopes of the solid line is for illustration purposes only, and does not indicate a larger or lower magnitude. 90% confidence intervals are shaded. N represents the number of establishments in the estimation sample, rounded to the nearest 50,000. These are estimates using model (8), where the outcome variable is the percent Black of an establishment's employees. The coefficient for the year prior to the event (θ_{-1}) is normalized to zero. The estimated model includes county-by-year fixed effects and a quadratic in log establishment size. Standard errors are clustered at the firm level.

The TWFE estimates are biased downwards as part of the control group are recent adopters.[83]

Note that these results are qualitatively similar to those we found using our data: we find that the point estimate for the impact of the regulation five years after first becoming a contractor is not significant at the 90% level. Furthermore, we show that there is no evidence of positive effects over the years immediately following regulation.

H.1.2 Further outcomes

We conclude the robustness checks for the effect of first becoming regulated by EO 11246 by briefly considering two additional outcome variables y_j for the model in (8): percentage of

[83] The effect magnitudes can only be compared if we apply for a quantitative disclosure of the TWFE estimates.

Black *new hires*, and relative earnings gap between Black and white workers. These outcomes require worker level information and, to the best of our knowledge, we are the first ones to be able to look at these margins. The former outcome looks at the flow of workers rather its stock. One explanation for the low effectiveness of the policy on the share of Black workers could be that turnover is low in contractor firms, and therefore the overall employment remains relatively constant. In that case, we could still find positive effects on the share of Black workers among newly hired employees. For the relative earnings gap, we measure this as the ratio of the average earnings of Black incumbent workers divided by the average earnings of White incumbent workers. We only consider earnings for incumbent workers, those who were already working at the establishment when it became regulated, to avoid indirect earnings effects due to any compositional change in hiring. This measure requires worker level information and, to the best of our knowledge, we are the first ones to be able to look at this margin. Even if the policy does not have a positive impact on the share of Black workers that an establishment hires, its effects could still operate through wages. It is well documented that retention of Black workers is considerably lower than that of whites (McKay et al., 2007), so even if a regulated establishment does not increase its pool of Black workers, it could still increase their wages in order to retain the incumbent ones. Table H.2 presents the estimates of β^k from equation (5) for these two outcomes. The results do not indicate that the affirmative action plan operates through these alternative channels, although a future version of this paper will explore these additional margins further.

Table H.1: *Additional results - Regression discontinuity*

	$t-1$ (baseline)	$t+0$	$t+1$	$t+5$
Proportion new hires Black	$-$	$-$	$-$	$-$
Relative Black-white Earnings	$-$	$-$	$-$	$+$
N	*2,600*	*2,600*	*2,200*	*1,800*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: FPDS-LEHD Data.
Notes: This table presents the qualitative results for the estimates of β^k in model (5), for $k \in \{-1, 0, 1, 5\}$. $-$ indicates a negative point estimate, $+$ a positive one. *N* is the number of establishments, rounded to the nearest 100.

H.2 The effect of the Affirmative Action Plan

H.2.1 TWFE Estimates

Our empirical results from from estimating (2) are qualitatively robust to using a naive TWFE estimator similar to the one of Miller (2017),

$$y_{jt} = \sum_{k=-5}^{k=5} \theta_k D_{jt}^k + \gamma X_{jt} + \alpha_j + \lambda_t + \varepsilon_{jt}$$

where y_{jt} denotes the share of Black workers in establishment j at time t. α_j denotes establishment j's fixed effect, λ_t is a year fixed effect, X_{jt} is a quadratic polynomial on the log of the establishment size. D_{jt}^k are leads and lags of establishment j first becoming a contractor, defined as

$$D_{jt}^k = D_j \mathbb{1}(t = \tau_j + k)$$

with D_j an indicator for whether the establishment first becomes a contractor, and τ_j is the year the establishment first becomes a contractor. We normalize $\theta_{-1} = 0$, and cluster standard errors at the firm level.

Figure H.1 plots qualitatively the point estimates and confidence intervals of the estimated θ_j's in the equation above using the linked LEHD-FPDS data. These qualitative findings confirm our results using the Callaway and Sant'Anna (2021) estimator: we do not find a causal effect of becoming regulated with affirmative action on the percent of Black employees that an establishment employs. Both before and after the regulation, the point estimates fluctuate around zero, and they are not significant even at the 90% level. Appendix G shows the estimates of θ_j for the share of minorities and women, and we still fail to detect an effect of the policy on those outcomes.

The TWFE estimates are biased downwards as part of the control group are recent adopters.[84]

Note that these results are qualitatively similar to those we found using our data: we find that the point estimate for the impact of the regulation five years after first becoming a contractor is not significant at the 90% level. Furthermore, we show that there is no evidence of positive effects over the years immediately following regulation.

[84] The effect magnitudes can only be compared if we apply for a quantitative disclosure of the TWFE estimates.

H.2.2 Further outcomes

We conclude the robustness checks for the effect of first becoming regulated by EO 11246 by briefly considering two additional outcome variables y_j for the model in (8): percentage of Black *new hires*, and relative earnings gap between Black and white workers. These outcomes require worker level information and, to the best of our knowledge, we are the first ones to be able to look at these margins. The former outcome looks at the flow of workers rather its stock. One explanation for the low effectiveness of the policy on the share of Black workers could be that turnover is low in contractor firms, and therefore the overall employment remains relatively constant. In that case, we could still find positive effects on the share of Black workers among newly hired employees. For the relative earnings gap, we measure this as the ratio of the average earnings of Black incumbent workers divided by the average earnings of White incumbent workers. We only consider earnings for incumbent workers, those who were already working at the establishment when it became regulated, to avoid indirect earnings effects due to any compositional change in hiring. This measure requires worker level information and, to the best of our knowledge, we are the first ones to be able to look at this margin. Even if the policy does not have a positive impact on the share of Black workers that an establishment hires, its effects could still operate through wages. It is well documented that retention of Black workers is considerably lower than that of whites (McKay et al., 2007), so even if a regulated establishment does not increase its pool of Black workers, it could still increase their wages in order to retain the incumbent ones. Table H.2 presents the estimates of β^k from equation (5) for these two outcomes. The results do not indicate that the affirmative action plan operates through these alternative channels, although a future version of this paper will explore these additional margins further.

H.3 Understanding the lack of effects of EO 11246

Table H.2: *Additional results - Regression discontinuity*

	$t-1$ (baseline)	$t+0$	$t+1$	$t+5$
Proportion new hires Black	$-$	$-$	$-$	$-$
Relative Black-white Earnings	$-$	$-$	$-$	$+$
N	*2,600*	*2,600*	*2,200*	*1,800*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: FPDS-LEHD Data.
Notes: This table presents the qualitative results for the estimates of β^k in model (5), for $k \in \{-1, 0, 1, 5\}$. $-$ indicates a negative point estimate, $+$ a positive one. *N* is the number of establishments, rounded to the nearest 100.