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WHAT HAPPENS TO CONTRACTORS AFTER STATES BAN AFFIRMATIVE ACTION?

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ABSTRACT

Using restricted Census business records, I explore how banning affirmative action in state contracting affects minority- and women-owned business enterprises (MWBEs). I find that ending affirmative action led MWBE contractors to gradually downsize, with the most pronounced reductions in force experienced by Black-owned businesses and larger MWBEs. Despite these workforce changes, existing MWBEs were no more likely to shut down than other businesses. New MWBEs were relatively less common after a state's ban, highlighting how bans can shift the demographic composition of new contractors. A calibrated model suggests bans are equivalent to considerable reductions in MWBE productivity and scrap values.

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1 Introduction

The scale and scope of government purchases—or procurement—is vast. In the U.S., public procurement accounts for approximately 10% of GDP. At the state and local level, governments purchase nearly $1.5 trillion in goods and services annually, fostering economic activity in sectors like construction, manufacturing, and professional services (Federal Contracting Alliance, 2025). Beyond securing public works, this purchasing power has historically been used to promote socioeconomic inclusion by granting preferences to business owners based on their race and sex, broadly termed affirmative action. These policies, which typically establish goals or mandates for awarding government contracts to minority- and women-owned business enterprises (MWBEs), were designed to mitigate racial and sex-based disparities in business ownership, wealth accumulation, and access to public-sector market opportunities (Council of Economic Advisers, 2024; Holzer and Neumark, 2006).

Despite their original intent, affirmative action policies have attracted intense legal and political scrutiny. Beginning in the mid-1990s and extending to the present, a host of statewide initiatives have sought to ban affirmative action outright in education, employment, and public contracting. Successful efforts include Proposal 2 in Michigan and Proposition 107 in Arizona, which prohibited each state from considering race and sex in matters involving state funds. Much of the current empirical literature considers how banning affirmative action affects educational and occupational outcomes (Antman et al., 2024; Bleemer, 2022; Zhang, 2022; Zitzewitz, 2025). A less understood yet equally salient component of these bans is their influence on public contracting, particularly on the MWBEs seeking to do business with the state.

To that end, this study contributes the first multi-state analysis of how banning affirmative action affects businesses operating in the public contracting marketplace. Confidential Census data facilitate the analysis. With them, I can identify the race and sex of business owners who contract with states through restricted-use survey responses. These data allow me to construct nationally representative samples of firms exposed to statewide affirmative action bans. Other Census records let me track these firms' outcomes longitudinally—enabling a panel analysis of firm-level employment, entry, and exit. The final sample is extensive, covering 114,000 unique firms operating throughout most of the United States.

My empirical approach to analyzing that data leverages the timing of statewide affirmative action bans, comparing disparities in MWBE and non-MWBE firm outcomes before and after a ban relative to states without a ban. To do so, I employ a stacked triple differences design. That is, for each state successfully passing a ban, I extract its contractors' outcomes over several years leading up to and directly following the ban. I combine that information with contractor outcomes from other states lacking a ban to create a state-specific dataset. After repeating that process for each state, I stack the resulting state-specific datasets and run a series of triple-differences regressions. The stacking procedure creates a "clean" control group with no affirmative action changes and allows me to compare bans that occur in otherwise disparate years across a consistent time window. Triple differences correct for confounding state-level changes unrelated to banning affirmative action.

Across the demographically diverse set of states in my sample, I find that banning affirmative action leads to a gradual reduction in MWBE size relative to non-MWBEs, as measured by employee count. Five years after a ban, MWBEs were 10.9 percent smaller than non-MWBEs, with no statistical difference beforehand. That result conceals substantial heterogeneity, particularly by race and size. When separated by racial ownership, I find that Black-owned firms encounter the most adverse effects—namely, an 18.8 percent decrease in relative size after a ban. Other racial categories are statistically unaffected. Likewise, larger enterprises were most negatively impacted: The largest MWBEs reduced employment by 13.2 percent relative to non-MWBEs, while smaller firms exhibited no statistically significant changes.

I find more modest heterogeneity by state and county type. On the one hand, states with substantial minority populations—such as Arizona—or small minority populations—like New Hampshire—saw statistically insignificant changes in relative MWBE employment levels. On the other hand, MWBEs in other states became roughly 7 percent smaller than non-MWBEs in the years following their ban on affirmative action. A breakdown by county classification reveals that more metropolitan counties endured the largest relative decrease in MWBE size. Altogether, these results showcase how affirmative action bans need not have uniform effects, even within states.

Turning to entry and exit, my results indicate that banning affirmative action led to relatively fewer new MWBE contractors. I find insufficient evidence to conclude that these new MWBEs enter at different sizes than their non-MWBE counterparts. In contrast to the entry results, banning affirmative action generally did not affect relative MWBE exit. Indeed, to the extent that there were marginally significant changes, MWBEs were *less* likely to exit than non-MWBEs after a ban. This result may seem puzzling at first glance: Given that MWBEs lost preferential treatment, one would expect a relatively higher post-ban exit rate. All the more so considering that MWBEs became relatively smaller. That I do not find such a change suggests that affirmative action bans may have affected MWBE incentives to exit.

To better understand those incentives and the broader implications for MWBE productivity, I build and calibrate a canonical model of firm employment and exit decisions. The model has two main features: (i) more productive firms hire more workers, and (ii) firms exit whenever the present discounted value of future operation falls short of a scrap (or sell) value. Upon calibrating it to pre-ban MWBE outcomes, I use the model to discern how much productivity and scrap values would need to change to replicate the estimated post-ban behavior. This exercise allows me to quantify the change in productivity and scrap values that are *outcome equivalent* to an affirmative action ban. I find that a statewide ban is outcome equivalent to an 8.6 percent decrease in pre-ban long-run MWBE productivity and a 10 percent decline in scrap values. When viewed in its entirety, this study suggests that, although MWBEs were no more likely to close, banning affirmative action led MWBEs to downsize relative to non-MWBEs, reduced the proportion of new MWBEs contracting with state governments, and were equivalent to substantial reductions in MWBE productivity and scrap values.

My research relates to and expands upon several strands of existing literature. One related area explores the impact of state affirmative action bans, focusing mainly on higher education and employment. Education studies find that banning affirmative action reduces admissions for underrepresented minorities, particularly at selective colleges, with various changes to application behavior (Antonovics and Backes, 2014; Backes, 2012; Card and Krueger, 2005; Hinrichs, 2012; Yagan, 2016). In the labor market, sharp declines in minority and women's employment followed California's ban on affirmative action (Myers, 2007), contributing to long-standing disparities in the U.S. economic outcomes (Blau and Kahn, 2017; Chetty et al., 2020). I add to this literature by providing estimates of how bans affect minorities and women who own and operate businesses in public contracting—a key segment of U.S. economic activity.

This business aspect relates to a robust literature on women and minority entrepreneurship. Much of the literature documents disparities faced by such entrepreneurs, often due to a lack of access to capital, experience, networking opportunities, or staffing capabilities (Bennett and

Robinson, 2024; Chiplunkar and Goldberg, 2024; Fairlie et al., 2022; Fairlie and Robb, 2007; Ubfal, 2024). More topically related work in this area includes studies by Fairlie and Marion (2012), who investigate how affirmative action bans in California and Washington affected minority and women self-employment rates, and Chatterji et al. (2014), who explore how set asides—an affirmative action policy that reserves competition on contracts to a specific group of businesses—affected the self-employment and employment rates of Black men. By linking confidential Census records, I build on these studies by identifying and tracking decisions of the firms most affected by affirmative action changes over time. These linkages allow me to analyze business dynamics at a depth beyond what could be obtained from self-employment information alone.

Separate from entrepreneurship, there is a growing literature assessing how affirmative action affects public contracting outcomes. Work in this area explores set asides (Brunjes and Rodriguez-Plesa, 2024; Cappelletti and Giuffrida, 2024; Carril and Guo, 2023; Myers and Chan, 1996), training programs (De Silva et al., 2020), outsourcing requirements (De Silva et al., 2012; Marion, 2009; Rosa, 2025, 2024), and preferences in evaluation criteria (Krasnokutskaya and Seim, 2011; Marion, 2007; Rosa, 2019)—often finding mixed results for contract execution and costs. I contribute to this literature by providing results on firm-level outcomes, allowing me to answer questions about how contractors adjust their workforce size and operational decisions in response to changes in state affirmative action policies.

Finally, the results tie into a broader macroeconomic literature on business dynamism—or firm entry, expansion, contraction, and exit. Much of this literature documents a secular decline in U.S. business dynamism and highlights implications for aggregate productivity (Akcigit and Ates, 2023; Decker et al., 2016, 2020). In contrast, this study explores *disparities* in business dynamism, finding that MWBE contractors become relatively less dynamic than non-MWBEs following a state's ban.

The remainder of the paper proceeds as follows. Section 2 provides institutional details on the history and application of affirmative action in the United States. Section 3 describes the data, and Section 4 lays out my empirical strategy for analyzing it. Section 5 contains the resulting analysis, while Section 6 outlines a modeling framework and discusses its implications. Section 7 presents results from the calibrated model; Section 8 concludes.

2 Institutional Details

In this section, I provide the institutional context that serves as a backdrop for much of the empirical analysis. In the U.S., affirmative action began mainly in employment with President John F. Kennedy's Executive Order 10925. This order mandated that federal contractors "take affirmative action to ensure that applicants are employed, and that employees are treated during employment, without regard to their race, creed, color, or national origin." Since then, affirmative action expanded to include minority- and women-owned firms, largely those doing business with the federal government. In 1971, President Richard M. Nixon directed federal agencies to develop comprehensive plans and specific program goals for minority business enterprises through Executive Order 11625. Later—in 1979—President Jimmy Carter created a women's business enterprise policy via Executive Order 12138, requiring agencies to take affirmative action in support of women's businesses.[1] Those efforts culminated in a host of initiatives geared towards supporting MWBEs in the federal marketplace. These initiatives include statutory goals—like the 2025 goal that 5% of all federal contract dollars go to women-owned businesses—business development programs, set-asides, and offices to ensure MWBEs are considered fairly in federal purchasing decisions.

Many states operated similar affirmative action initiatives for purchases involving state funds, in part to align their practices with federal ones. For example, Michigan implemented a goal of awarding 5% of all state expenditures to women-owned businesses and 7% to minority-owned businesses in 1980. That tradition changed in 1996, when California voters approved Proposition 209, which barred the state from considering race, sex, or ethnicity in education, employment, or public contracting. Other states soon followed suit, leading to a total of nine states passing similar affirmative action bans. Because of data limitations discussed later in Section 3, this study focuses on five of them—chronologically, Michigan (2006), Nebraska (2008), Arizona (2010), New Hampshire (2012), and Oklahoma (2012).[2]

Although the exact language varies by state, the bans largely followed California by prohibiting the consideration of race, sex, or ethnicity in matters involving state education, state employment, or state contracting. For state contractors, this new legal environment brought

[1] For a more in-depth analysis of the history and controversies surrounding affirmative action, see Holzer and Neumark (2000), Fryer and Loury (2005), and Holzer and Neumark (2006).

[2] The other four states are California, Washington (repealed in 2022), Florida, and Idaho.

about many changes, and the examples are abundant. The Michigan ban effectively ended the aforementioned goals for MWBEs on state-funded contracts. The cities of Phoenix and Tucson operated state MWBE programs in the past, but the Arizona ban made such programs—which often seek to improve MWBE participation by notifying MWBEs of potential work opportunities, facilitating networking events with city officials, and running certification training seminars—illegal (Keen Independent Reserach, 2020). In Nebraska, the ban halted Omaha's Protected Business Enterprise Program. This program compelled contractors to outsource work to one of the city's MWBEs for jobs such as constructing libraries, renovating offices, or cleaning city buildings (Hansen, 2009). In this study, I leverage variation induced by these state-level changes to examine how banning affirmative action affects state contractors, focusing primarily on firm employment, entry, and exit decisions.

3 Data Sources

To conduct my analysis, I rely on confidential records from three Census data sources: the Survey of Business Owners (SBO), the Annual Business Survey (ABS), and the redesigned Longitudinal Business Database (LBD). This section describes these sources in detail and how I utilize them to construct a panel of state contractors.

3.1 The Survey of Business Owners (SBO)

Between 1972 and 2012, the U.S. Census Bureau collected information on the demographic characteristics of business owners through the SBO (U.S. Census Bureau, 2025). The Census conducted this survey every five years and included in it a representative sample of non-farm businesses filing Internal Revenue Service tax forms with receipts of at least $1,000. The Census administered the SBO at the firm level, which may include multiple domestic business locations—or establishments. Consequently, much of the empirical analysis will be at the firm level.

The SBO tracks business ownership by race, sex, and ethnicity.[3] Business ownership requires

[3] Races include White, Black or African American, American Indian or Alaska Native, Asian, Native Hawaiian or other Pacific Islander, and some other race. Ethnicity—used to denote whether a firm is Hispanic-owned—includes Mexican, Puerto Rican, Cuban, not Hispanic, and some other Hispanic origin.

at least 51 percent stock ownership in a company, and business owners can fall under more than one racial or ethnic group. I use these demographic characteristics to determine whether a firm is a minority- or women-owned enterprise. Specifically, I say a firm is minority-owned if its owner is not (non-Hispanic) White; a women-owned business has a woman as its owner. Later on, I will break down the minority category further into (non-Hispanic) Black, Hispanic, and other minority, where the "other minority" category contains the other racial groups and any multiracial ownership.

The SBO also asked respondents which types of customers accounted for 10% or more of their sales. In 2002 and thereafter, respondents could choose a state-and-local-government option (meaning that at least 10% of their sales came from a state buyer). I consider firms responding affirmatively to this question as state contractors; these firms constitute the set of businesses "exposed" to the contracting changes induced by state-level affirmative action bans. Earlier versions of the SBO either lacked this question or had small sample sizes, motivating my focus on bans implemented after 2002.

Respondents could choose other customer types, too—including the federal government, other businesses, and individuals. I use this information to construct a sample of placebo firms that were not directly affected by the changes in state contracting. To be in the placebo, a firm must make at least 10% of its earnings from individuals and have less than 10% of its sales coming from either the (state or federal) government or other businesses. The requirement on other business limits firms' ability to serve as subcontractors or suppliers to state contractors, and I perform all placebo tests in Appendix A.4.

3.2 The Annual Business Survey (ABS)

The Census discontinued the SBO in 2017, replacing it with the more frequent ABS. Like the SBO, the ABS contains a representative sample of non-farm businesses that submit Internal Revenue Service tax documents at the firm level, recording a similar set of demographic characteristics and whether the state accounts for at least 10% of a firm's customer base. In my empirical analysis, I use the 2017 ABS to identify state contractors that operated after a ban, enabling an analysis of entry behavior.

3.3 The Longitudinal Business Database (LBD)

The key source of my outcome variables is the LBD. This confidential Census dataset tracks year-to-year changes in *establishment* employment and payrolls for non-farm businesses, starting in 1976 (Chow et al., 2021). The LBD also contains a vintage-consistent NAICS code, establishment state and county location information, a firm identifier, and a taxpayer identification number—or EIN. These variables facilitate linkages between the establishment-level outcomes in the LBD and the SBO's and ABS's demographic information.

Specifically, I match survey respondents' EINs to the LBD's to obtain yearly employment, entry, and exit information. I then use the LBD's firm identifier to aggregate up to the firm level for consistency with the surveys' firm-level observations. This process creates a longitudinal database of state contractors for each survey year, and I perform this matching procedure for the 2002, 2007, and 2017 surveys.[4]

3.4 Final Samples and Disclosure Avoidance

Altogether, my final dataset covers survey respondents operating between 2003 and 2017 and includes affirmative action bans in five states: Michigan, Arizona, Nebraska, New Hampshire, and Oklahoma. To pinpoint the bans' impacts, I place three final restrictions on the firms in my sample. First, I remove all publicly owned firms, as I cannot attribute their ownership to any race, sex, or ethnicity. Second, I exclude firms with establishments in multiple states; this requirement prevents situations where some of a firm's establishments are in a state with a ban while others are not. Third, I drop firms operating establishments in states that had already banned affirmative action by the end of my sample period to create a set of "clean" control firms. These states with prevailing bans include California, Washington, and Florida. My final sample (rounded to comply with Census disclosure rules) includes approximately 114,000 state contractors, 7,000 of whom were in states that would eventually ban affirmative action.

The Census takes many steps to prevent disclosure of any confidential information. All research output must undergo a rigorous disclosure review process that includes requirements for coefficient reporting, rounding, and sample sizes (U.S. Census Bureau, 2021). These restrictions

[4] The Census changed its survey methodology in 2012, resulting in far fewer state contractor responses U.S. Census Bureau (2013). For this reason, I do not use the 2012 SBO cohort in my empirical analysis.

limit the granularity of certain heterogeneity analyses, such as investigations of smaller racial categories. In my empirical analysis, I ensure that every dimension of heterogeneity is large enough to comply with the Census disclosure requirements.

4 Empirical Strategy

Having documented the data sources, I now describe my empirical strategy for estimating the effect of affirmative action bans on public contractors.

4.1 Triple Differences

My main specification uses a triple-difference design, comparing the differences between MWBEs and non-MWBEs—which, in this instance, are firms owned by White men—in treated and control states. The treated states are Michigan, Arizona, Nebraska, New Hampshire, and Oklahoma. The control states are the remaining ones, excluding the aforementioned states that had already banned affirmative action. Therefore, one can interpret all results as changes in the gap between MWBE and non-MWBE outcomes relative to a national trend. Other studies using a triple difference design to evaluate affirmative action bans include Fairlie and Marion (2012) and Myers (2007).

A triple-difference specification is convenient in my context because it purges estimates of state-level changes that affect all firms. For example, if a state with a ban were to simultaneously enact a less business-friendly tax code, MWBEs in that state might have poorer business outcomes than MWBEs outside of the state, and a naïve difference-in-differences estimator using MWBEs only would attribute that change to the ban. A triple-differences specification largely avoids this issue, unless state-level changes have a differential impact on MWBEs. I explore this possibility further in Appendix A.4, where I fail to find a differential effect of bans on firms that are not state contractors.

4.2 A Stacked Design

I aim to understand how affirmative action bans changed business behavior in a consistent time window around the ban year. To facilitate this type of analysis, I use a stacked event-study design, similar to that of Cengiz et al. (2019). For each treated state, I extract its contractors' information and data from control-state firms over a nine-year window around the affirmative action ban: three years before the ban, the ban year, and five years after the ban. This process creates a set of "clean controls" for each treated state, consisting of contractors that experience no affirmative action changes. I then combine the treatment and control data for each treated state into a single stacked database. To avoid post-treatment selection into the sample, I use pre-ban survey responses for each treated state—specifically, the 2002 SBO respondents for Michigan and the 2007 SBO data for every other state.

Consider the Michigan ban as an illustration. For it, I draw upon the longitudinal database of state contractors constructed from the matched 2002 SBO and LBD, keeping firm outcomes occurring between 2003 and 2011 in Michigan and the control states.[5] I then repeat this process for the remaining treated states (using the 2007 SBO respondents and the appropriate time windows[6]) and "stack" the resulting data together. Going forward, I will refer to firms appearing around a specific state's ban year as a "cohort," meaning that each state with a ban will have its own cohort of firms.

4.3 Regression Specifications

My main specification is thus a panel stacked triple differences regression, given by

$$f(y_{jct}) = \alpha + \sum_{s=-4, s\neq -1}^{4} \big(\delta_s MWBE_j \times treat_{jc} \times year_{s=t}\big) + \gamma X_{jct} + \zeta_{ct} + \psi_{jc} + \varepsilon_{jct}, \quad (1)$$

where j is a firm index, c is a cohort index, and t indexes time. I denote event time by s, and I define $s = -1$ as the year a state bans affirmative action; that year serves as the reference period. Event time $s = 0$ is the first full year that a state implements its ban.

The right-hand-side variables in equation (1) include an indicator for whether a firm is a minority- or women-owned business, $MWBE_j$, an indicator for whether a firm resides in a treated state, $treat_{jc}$, and a year indicator, $year_{s=t}$, denoting whether year t is s years after a ban's implementation. The coefficients of interest are the δ_s values, which measure the gap

[5] Courts briefly overturned the Michigan ban in 2011, but it remained in place pending appeal for the rest of that year.

[6] These time windows are 2005–2013 for Nebraska, 2007–2015 for Arizona, and 2009–2017 for Oklahoma and New Hampshire.

between MWBE and non-MWBE outcomes in states with a ban relative to control states. The control variables, X_{jct}, include SBO survey effects, 2-digit NAICS code—or sector—indicators, and state indicators. The SBO effects control for potential survey differences, while the NAICS and state indicators account for possible level differences across various sectors and states, respectively. To further isolate the effect of interest, I interact each control variable with year, allowing for time trends. I also include year-cohort effects, ζ_{ct}, and firm-cohort effects, ψ_{jc}. As such, the δ_s coefficients will be unaffected by event-time-invariant firm-level differences, like initial managerial skill.

The outcome variable, y_{jct}, is firm employment. To allow for zeros in the dependent variable, I use an inverse hyperbolic sine transformation, $f(\cdot)$. For positive outcome values, the coefficients will have a similar interpretation to those in a log-transformed regression. I note, however, that there are certain limitations to using inverse hyperbolic sine, such as the coefficient estimates being sensitive to transformations of the dependent variable (see Bellemare and Wichman (2020) and Bellemare and Norton (2023)). In Appendix A.3, I explore this issue further, finding little difference in the coefficients' interpretation when dropping zeros and using a log transformation instead.[7]

In analyzing heterogeneity, I use the following, more standard triple differences specification:

$$f(y_{jct}) = \alpha + \delta MWBE_j \times treat_{jc} \times post_{jct} + \gamma X_{jct} + \zeta_{ct} + \psi_{jc} + \varepsilon_{jct}. \tag{2}$$

Here, I maintain much of the structure from equation (1), except that I use a post indicator, $post_{jct}$, to indicate whether an outcome occurs after a cohort's ban year. This specification limits disclosure risk for these smaller populations, and the coefficient of interest, δ, compares gaps in MWBE and non-MWBE outcomes for all years after the ban relative to all years before the ban. That is to say, δ aggregates the ban's effect over all years for which a state institutes it.

To ensure my estimates reflect the broader economic environment, I use the Census-provided SBO weights in each of my firm-level regressions. I also winsorize all firm-reported variables above and below by 1%. In addition to being relatively common in firm-level analyses (see Rao and Risch (2025), Yagan (2015), and Kline et al. (2019)), this process reduces the influence of

[7]In practice, zero observations for employment are relatively rare, constituting only 6.3 percent of all observations.

outliers and markedly lowers disclosure risk. Throughout, I cluster all standard errors at the state-cohort level.

4.4 Discussion of Identifying Assumptions

The triple-differences specification presented here leverages as-good-as-random policy timing to draw inferences about the relative impact of affirmative action bans. As such, there are three primary threats to identification. First, states with bans could have been on different trajectories before the ban year, as would be the case if a state implemented a ban in response to a shrinking gap between MWBE and non-MWBE outcomes. In this instance, the triple differences coefficient would reflect a continuation of existing trends, rather than an impact of affirmative action bans more generally. This issue amounts to a violation of the conventional parallel-trends assumption, so the lack of pre-trends in the upcoming event study analysis suggests this concern is unlikely to be of first-order importance.

A second threat is the possibility that states enact concurrent policies or programs. This threat is common in studies of state-level policy changes, though I will present evidence suggesting that any such changes are unlikely to be driving my results. Specifically, I will show that the reaction to banning affirmative action is concentrated among state contractors and largely consistent with how firms respond to the potential loss of access to beneficial programs and business opportunities. I find no differential responses by non-contractors, as I show later in Appendix A.4.

The third threat to identification is survivorship bias, arising because firms must have been in business to be included in the data.[8] To explore this threat further, I complement my main results with a robustness exercise. There, I find statistically similar coefficients in a balanced and unbalanced panel, suggesting that differential behavior among surviving firms is limited in my context. Survivorship may also play a role in the SBO survey (Lafontaine et al., 2019), as time between SBO years and bans differs for most states. I investigate this potential issue with an additional robustness analysis—which shows that the main results are statistically unaffected by removing the control variables, including the SBO year controls (see Appendix A).

[8] See Brown et al. (1992) for an application to mutual funds. Lafontaine and Shaw (2016) discuss survivorship within the context of serial entrepreneurship.

Table 1: Unweighted Summary Statistics for State Contractors (Base Year)

	Treatment		Control	
	Mean	S.D.	Mean	S.D.
Employees	22.96	43.44	25.75	49.13
MWBE	0.26	0.44	0.29	0.45
Minority-owned	0.12	0.32	0.12	0.33
Women-owned	0.18	0.38	0.20	0.40

Note: Summary statistics for the full sample of 114,000 state contractors in treatment and control states. All statistics are measured in the base year, which is either the year a state banned affirmative or the firm's first year in existence, whichever comes first. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

5 Firm Adjustments to Affirmative Action Bans

This section applies my empirical strategy to the Census panel data to estimate the impact of state-level affirmative action bans. I begin with statistics summarizing the firms observed in the data. I then explore the bans' impact on employment before moving into a heterogeneity analysis. I end by considering entry and exit impacts, and the characteristics of new entrants.

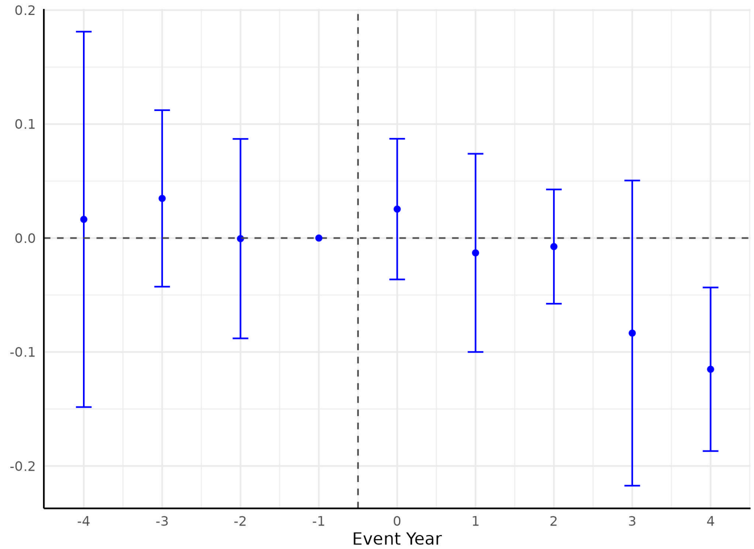
5.1 Summary Statistics

Table 1 reports unweighted summary statistics for the sample of firms observed across all data sources. The sample contains employment and activity information for 114,000 state contractors: 7,000 in treated states and 107,000 in control states. Contractors employ an average of 23–26 workers, although there is considerable dispersion. MWBE, minority-owned, and women-owned are indicator variables for a firm's ownership, so their averages correspond to sample shares. Treated and control states have roughly similar shares of minority-owned, women-owned, and MWBE firms—approximately 10 percent minority-owned, 20 percent women-owned, and 30 percent MWBEs.

5.2 Intensive Margin Response

To evaluate how contractors adjust operations in response to affirmative action bans, I follow the matched 2002 and 2007 SBO cohorts, stacked according to the procedure outlined in Section 4.2.

Figure 1: The Effect of Affirmative Action Bans on Employment



Note: Figure plots the three-way interaction coefficients, δ_s, and 95% confidence intervals attained from estimating equation (1). The dependent variable is the inverse hyperbolic sine of employment. Regressions use SBO weights, with data winsorized from above and below by 1%. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

I impose an additional requirement here that firms must have been operating before the state banned affirmative action. That requirement mitigates the impact of pre-ban exits, although I allow firms to leave the data afterward to account for post-ban exits. As a result, my sample is unbalanced and may be affected by changes in firm composition. Reassuringly, though, the results from a balanced panel are statistically similar, as shown in Appendix A.1.

Figure 1 presents the coefficients from the estimation of equation (1) on the matched SBO data. The coefficients indicate a gradual decline in the relative employment differential between MWBEs and non-MWBEs, likely driven by firms downsizing as old contracts expire.[9] The differential attains statistical significance five years after a ban, and that fifth-year estimate (of -0.1151) indicates that MWBEs are approximately 10.9 percent smaller than non-MWBEs five years following the ban.[10]

[9]Many contracts have five-year work limits, so firms would have completed most pre-ban contracts by the last event year.

[10]For the inverse hyperbolic sine transformation, one can interpret the coefficient δ_s as a percent change using the transformation $100 \times (\exp(\delta_s) - 1)$.

5.3 Heterogeneity Analysis

Although the event-study estimates point to a gradual reduction in MWBE size, that decrease need not be shared equally across all MWBEs. Indeed, it is possible that a state's ban had a distinct effect on, say, Black-owned MWBEs compared to Hispanic-owned ones, and that effect would be masked by grouping all MWBEs together. To explore this and other facets of heterogeneity, I estimate the triple-differences specification in equation (2) across various subsets of the data.

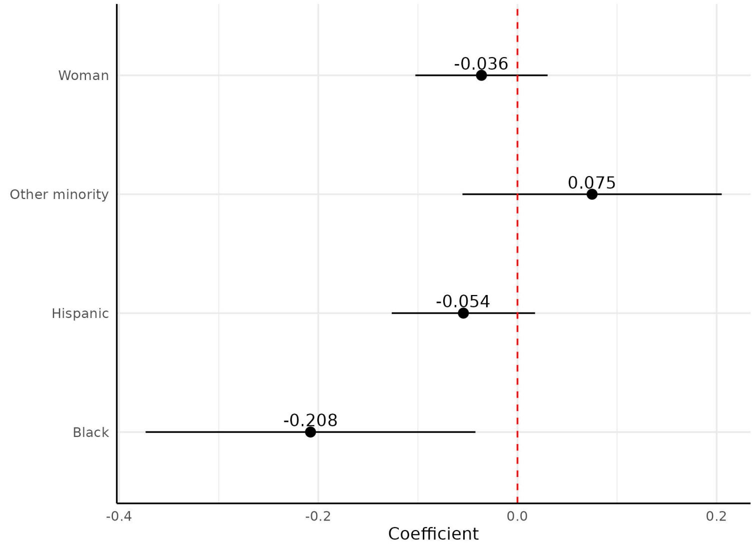
I begin with an analysis of heterogeneity by race. For each regression, I restrict my sample of MWBEs to a single race or sex category and include the entire set of non-MWBEs as a comparison group. Consequently, each coefficient compares the relative sizes of businesses owned by women or by the three racial categories with non-MWBEs in treated and control states before and after each affirmative action ban. Figure 2 plots the results.

Relative to non-MWBEs, I find that Hispanic-owned firms and firms owned by the other minority category experience no statistically significant change in size after a state bans affirmative action. Women-owned firms as a whole have a similar outcome. Conversely, Black-owned businesses downsize considerably. Relative to non-MWBEs, Black-owned businesses reduce their workforce by 18.8 percent, nearly double the effect estimated in the final event study year. These results indicate that Black-owned firms were most adversely affected by the changes to affirmative action.[11]

Next, I consider an analysis of heterogeneity by state, as illustrated by Figure 3. To construct this image, I estimate equation (2) separately for each state and for the non-ban states, and report the coefficients for the three-way interaction term. The estimates, therefore, compare how the difference between a state's MWBE and non-MWBE outcomes changes relative to a national trend. I find that out of the five states with bans in the sample, three of them caused MWBEs to shrink relative to non-MWBEs: Oklahoma, Nebraska, and Michigan. In those states, affirmative action bans caused MWBEs to reduce employment by roughly 7 percent. The other two states—New Hampshire and Arizona—saw no statistical change.

[11]Observe that one could draw many different interpretations from these heterogeneity findings. On the one hand, affirmative action might be combating discrimination, so removing it exposes more adversely affected firms to discriminatory practices. On the other hand, affirmative action could be supporting less effective firms. Given that I lack sufficient data to credibly disentangle these explanations, I will remain largely agnostic about the cause of the estimated differences.

Figure 2: The Effect of Affirmative Action Bans on Employment by Race



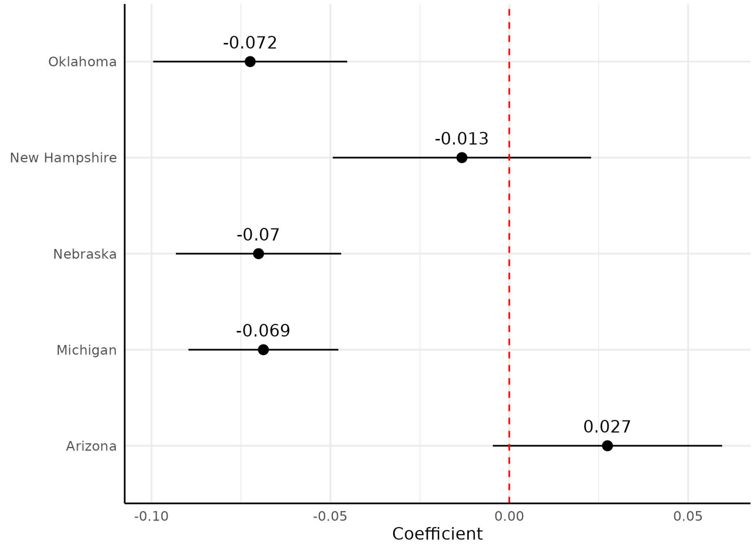
Note: Figure plots the three-way interaction coefficient, δ, and 95% confidence intervals attained from estimating equation (2) separately by MWBE race and sex. The dependent variable is the inverse hyperbolic sine of employment. Regressions use SBO weights, with data winsorized from above and below by 1%. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

While a null effect may seem surprising at first blush, it aligns with the earlier results by race. Demographically, New Hampshire is predominantly White,[12] so many of its MWBEs are women-owned; In Arizona, a large segment of MWBEs are Hispanic-owned. Figure 2 suggests that these owners did not make significant employment adjustments, consistent with the null effects estimated here.

Beyond an owner's race and state, affirmative action bans may have had heterogeneous effects on firms of different sizes. On the one hand, if affirmative action programs primarily benefit smaller MWBEs, one would expect that banning such programs would mainly affect relative employment at those smaller firms. On the other hand, most of the gains from affirmative action might accrue to larger MWBEs, in which case the bans would primarily affect larger businesses. I explore this aspect of heterogeneity in Figure 4. There, I plot the three-way interaction coefficient in equation (2), but I restrict the sample of firms to those in various quartiles of the base-year employment distribution—with the base year being the year affirmative action was banned. Thus, the results presented in Figure 4 compare relative employment between similarly sized MWBE and non-MWBE contractors following a state's change in affirmative action policy.

[12] In the 2010 Census, New Hampshire's population was 93.9 percent White.

Figure 3: The Effect of Affirmative Action Bans on Employment by State



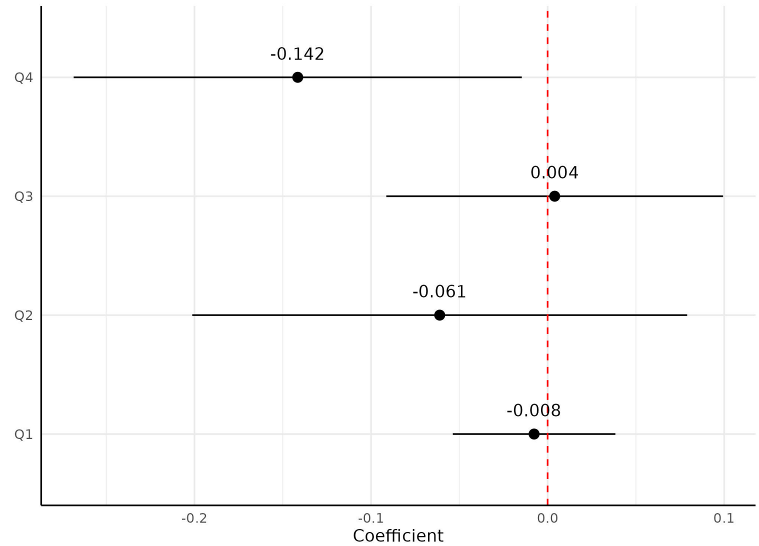
Note: Figure plots the three-way interaction coefficient, δ, and 95% confidence intervals attained from estimating equation (2) separately by state. The dependent variable is the inverse hyperbolic sine of employment. Regressions use SBO weights, with data winsorized from above and below by 1%. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

I estimate that the decline in relative employment is concentrated in the biggest firms. For contractors in the upper (or fourth) quartile of the base year's distribution, MWBEs reduced employment by 13.2 percent relative to non-MWBEs. The other quartiles exhibit no statistical difference. These results suggest that larger MWBEs may have borne most of the brunt of losing race and sex-based preferences in contracting, as indicated by their downsizing behavior post-ban.

In my final heterogeneity analysis, I explore whether the bans affected firm employment decisions more in the most economically distressed counties. I base this analysis on the Small Business Administration's HUBZone designations—which qualify a non-metropolitan county as a HUBZone if its median household income is less than 80 percent of the state's median household income, or if its unemployment rate is 40 percent higher than the state's. In metropolitan counties—which tend to be more economically diverse—smaller, census tracts can qualify if they have a poverty rate of at least 25 percent or if at least half of their households have incomes below 60 percent of the area's median gross income.[13] In what follows, I estimate separate regressions

[13]There are other ways for areas to qualify as HUBZones, too, like being within the boundaries of an Indian reservation or in an area with a recent military base closure. However, most HUBZones qualify as counties or census tracts under the listed criteria.

Figure 4: The Effect of Affirmative Action Bans on Employment by Firm Size at Baseline



Note: Figure plots the three-way interaction coefficient, δ, and 95% confidence intervals attained from estimating equation (2) separately by baseline (or ban year) employment quartile. The dependent variable is the inverse hyperbolic sine of employment. Regressions use SBO weights, with data winsorized from above and below by 1%. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

for firms located in counties that are either all HUBZone, partially HUBZone (by having some HUBZone-qualified census tracts), or no HUBZone. The resulting coefficients thus compare MWBE and non-MWBE contractors operating in counties with more similar socioeconomic positioning.

Table 2 presents the results. The estimates suggest that relative employment responses were mostly muted for MWBEs located in either all- or no-HUBZone counties. In contrast, the more metropolitan, partially HUBZone counties were the only ones to see a statistical decline, albeit a marginal one. In those counties, MWBEs reduced employment by 5 percent relative to non-MWBEs. Thus, there is little evidence to suggest that banning affirmative action affected MWBEs more in the most economically distressed counties. Instead, most of the relative employment changes arise in the more metropolitan counties with higher income dispersion.

5.4 Extensive Margin Responses

Thus far, my analysis points to an overall downsizing of MWBE firms in response to a state's ban, with notable heterogeneity. A separate, but related, issue is that such bans may also affect business formation and whether firms cease operations entirely. To explore these extensive-

Table 2: Impacts of Affirmative Action Bans on Different Counties

	$\sinh^{-1}$(Employment)
All HUBZone	-0.04843
	(0.03247)
Part HUBZone	-0.05146*
	(0.02962)
No HUBZone	0.06129
	(0.07739)

Note: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Table reports the effects of banning affirmative action in counties with different HUBZone compositions. All HUBZone means the entire county is a HUBZone, part HUBZone means some of the county is a HUBZone, and No HUBZone means that there are no HUBZones in the county. Coefficients are from the triple differences regression presented in equation (2). Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

margin concerns, I construct collapsed datasets from the full unbalanced firm panels (which include pre-ban activity) and use them to track entry and exit behavior.

More precisely, I collapse the firm-level data by minority status, women ownership, sector, state, and year to obtain a count of firms. I also track the number of new entrants and exits by counting firms that come into existence in year t (for entrants) and firms that disappear that year (for exits). Thus, an example observation would be the number of new minority-owned firms by men in the construction sector in New Hampshire in 2013. Recall that my underlying survey data include snapshots of state contractors at various points in time, provided by the 2002 and 2007 SBOs, as well as the 2017 ABS. Because a firm in the SBO could not have entered after the survey, I use the linked SBO data to study exit.[14] Likewise, I use the linked ABS data to study entry, as those firms could not have exited before 2017. In sum, the exit dataset follows the 2002 (for Michigan) and 2007 (for the remaining states) SBO firms, while the entry dataset tracks the 2017 ABS respondents.

From there, I run regressions very similar to the one presented in equation (1), with three main changes. First, I do not transform the outcome variables, which are now the number of new entrants and exits. Second, I index the collapsed "cells" by j instead of firms. This specification

[14] For example, a firm in the 2002 SBO could not have entered in 2007.

allows me to explore whether there are breaks in the entry and exit behavior of MWBEs relative to non-MWBEs in response to a state's affirmative action ban. Third, I weigh each observation by the sum of existing firms' SBO weights in the ban year.

Figure 5a presents the exit results relative to the ban year. Although MWBEs eventually reduce their size following a state's ban, I fail to find a similar effect on firm exit: all coefficients are statistically indistinguishable from zero at the 95 percent confidence level. At lower confidence levels, MWBEs are eventually *less* likely to exit. When viewed in tandem with the employment results, these coefficients suggest that affirmative action bans ultimately decrease MWBE business output enough to reduce employment but not enough to drive them out of business entirely. These results relate to Bates and Williams (1996), who find that minority owned businesses with any local-government sales were no more likely to go out of business than other small firms following a tightening of federal affirmative action laws.

Turning to entry, Figure 5b presents the results from the collapsed firm-level data using the 2017 ABS. The estimates suggest that banning affirmative action results in relatively fewer new MWBE contractors compared to non-MWBEs. The year-five estimate (of -1.019) indicates that roughly one fewer minority- or women-owned contractor enters a given state by sector cell. With 20 NAICS sectors, a back-of-the-envelope calculation suggests that treated states lose approximately 35 new MWBE contractors per year, five years after banning affirmative action.[15]

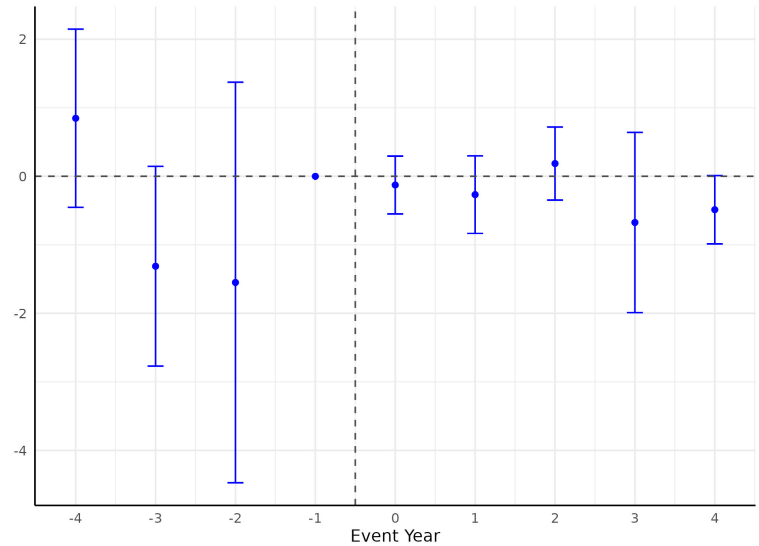
5.5 An Analysis of New Entrants

Although the number of new entrants declined, there is a distinct possibility that banning affirmative action changed the composition of new entrants, as has been observed after other economic events—such as recessions (Foster et al., 2016). To explore this possibility further, I estimate a regression specification comparing the likelihoods of being at the extremes of the size distribution for firms entering on or after a ban year. That is, I use the linked 2017 ABS data to ascertain the distribution of active firms' employment levels during the ban year. I then

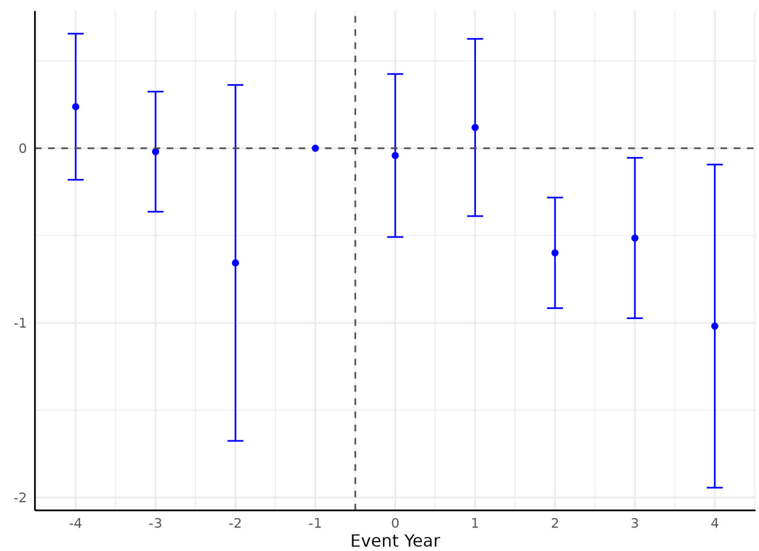
[15] I calculate that number by first noting there are 20 two-digit sectors and that if MWBEs were either women or minority owned (and not both), there would be approximately 20 (sectors) $\times$ 2 (minority-owned cell and women-owned cell) = 40 fewer MWBEs. However, the summary statistics presented in Table 1 suggest that roughly 4 percent of firms are women- and minority-owned, meaning that $(1 - 4/30) \times 100 \approx 86.6$ percent of minority- or women-owned firms are not double-counted as MWBEs. Multiplying 40 by 0.866 and rounding yields the result.

Figure 5: The Effect of Affirmative Action Bans on Entry and Exit

(a) Exit



(b) Entry



Note: Figure plots the three-way interaction coefficients, δ_s, and 95% confidence intervals attained from estimating equation (1) on the collapsed firm data. The dependent variable is the count of firms in each minority-owned-by-women-owned-by-sector-by-state-by-year cell, with the top panel showing exit results and the bottom panel showing entry results. All regressions use SBO weights at baseline. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

Table 3: Size Impacts of Affirmative Action Bans

	Top Employment ($Q4$)	Bottom Employment ($Q1$)
Entrants	0.03916	-0.1657
	(0.1205)	(0.2591)

Note: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Table reports the probability of being in the top or bottom quartile of the employment distribution the year affirmative actions was banned for new entrants. Coefficients are from the final event year ($s = 4$) of the triple differences event study presented in equation (1) and measure outcomes relative to the year affirmative action was banned ($s = -1$). Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

create an indicator variable for whether a firm is in the top or bottom quartile of the ban year's employment distribution. From there, I estimate the relative probability that MWBEs are at the top or bottom of the ban year's distribution four years after states remove affirmative action by running the regression presented in equation (1) and reporting the last event-time coefficient. I restrict the sample to firms appearing on or after a ban, which I refer to as entrants.

Table 3 presents the results. I find that whether MWBE entrants are more likely than non-MWBEs to be in the top or bottom quartiles of the employment distribution lacks precision. Consequently, although the point estimates suggest that MWBE entrants are relatively more likely to be large and less likely to be small than non-MWBE entrants, the results are statistically insignificant. I conclude that there is insufficient evidence to suggest that banning affirmative action alters the relative positioning of new entrants at the upper and lower extremes of the size distribution.

5.6 Robustness of the Results

Before moving further, I pause here to discuss the robustness of my main findings. Appendix A.1 explores the sensitivity of my employment analysis to using a balanced sample. There, I find that the coefficients are similar whether the sample is balanced or unbalanced, suggesting that my results are not sensitive to the composition of surviving firms. In Appendix A.2, I consider robustness to including control variables, again finding similarity in coefficients with and without controls. Appendix A.3 considers whether the employment coefficients remain robust upon changing the inverse hyperbolic sine transformation, which can be sensitive to units of measurement. With zero observations excluded, I do not find substantial differences relative to a log transformation, which is independent of the dependent variable's units.

I also conduct a placebo analysis in Appendix A.4 using firms that should not have been exposed to their state's affirmative action ban. These firms sell directly to consumers and not to the government or other businesses. Reassuringly, I find no differential reaction to state bans, making concurrent changes an unlikely explanation for the relative employment changes measured here.

6 A Conceptual Framework

My empirical results show that, following a statewide affirmative action ban, MWBEs eventually become relatively smaller and less likely to enter than non-MWBEs. There is insufficient evidence for differential exit. To further contextualize these findings, this section presents a stylized, partial equilibrium model of a firm's employment, entry, and exit decisions.[16] The model itself is fairly standard, but its predictions facilitate a discussion of how the estimated coefficients relate to economic fundamentals and a quantification of those fundamentals.

6.1 Employment

For simplicity, I assume a firm with productivity φ uses labor l as an input in production, with φ measuring how well firms convert labor into output. In a contracting setting, one can view more productive firms as being generally more capable of finding, winning, and executing contracts. Firm profits are

$$\pi(l) = R(\varphi, l) - l,$$

where $R(\varphi, l)$ is the firm's revenue function.[17]

Under conventional assumptions on revenue—namely, that revenue is concave in labor and has a marginal product of labor that increases in productivity—one can show that optimal labor, $l(\varphi)$, increases in productivity. This theoretical implication connects to the empirical

[16] I opt for a partial equilibrium analysis because the linked data follow waves of state contractors, rather than all operating firms.

[17] Here, I normalize profits by the wage so that revenues are relative to the going wage rate. I also do not model a fixed cost. To the extent that firms still choose to operate, this choice will not affect optimal labor.

results: Empirically, I find a comparative decrease in employment, suggesting a gradual decline in MWBE productivity relative to non-MWBEs. Given that the bans eliminated programs and goals allowing MWBEs to more readily find contracting opportunities, a decrease in productivity is consistent with the institutional changes at the time.

Observe that one could also attribute declining employment to a decrease in demand, as would occur if MWBEs were to lose market power after a state's ban. Although the gradual relative decline seems to preclude immediate demand changes, formally disentangling these competing channels is challenging without firm-level prices and quantities (Foster et al., 2008), which are unavailable for firms in the data at hand. As such, any demand changes will be subsumed by the model's productivity parameter.

6.2 Exit

Now consider an incumbent firm's exit decision. This choice is inherently dynamic, as firms weigh exiting against their discounted stream of future profits. In characterizing this decision, I assume firms discount at rate $\beta \in (0,1)$ and that productivity evolves according to a first-order Markov process with conditional distribution function $H(\varphi_{t+1} \mid \varphi_t)$. Let $V_I(\varphi)$ denote an incumbent firm's value function. A firm can choose to operate and receive continuation value $\pi(\varphi)+\beta E_H[V_I(\varphi') \mid \varphi]$, with $\pi(\varphi) = R(\varphi, l(\varphi))-l(\varphi)$ being optimal period profits. Alternatively, a firm can exit. I assume that exit is permanent and that upon exit, firms receive a scrap value, $\phi > 0$. This value reflects the business's expected worth if the owner were to sell it.

The present discounted value of an incumbent firm is then

$$V_I(\varphi) = \max \left\{\phi, \pi(\varphi) + \beta E_H[V_I(\varphi') \mid \varphi]\right\}. \tag{3}$$

Firms exit whenever

$$\phi > \pi(\varphi) + \beta E_H[V_I(\varphi') \mid \varphi]. \tag{4}$$

Under this setup, one can show that the continuation value (on the right-hand side of equation (4)) increases in productivity; therefore, there is a unique productivity level, φ^*, under which firms will not operate. Higher scrap values and lower continuation values increase exit by increasing φ^*.

Empirically, the event-study coefficients plotted in Figure 1 trend downward, indicating a comparative employment decline over time. Because less productive firms hire fewer workers, this trend suggests MWBEs have a relatively poorer productivity evolution post-ban, in the sense that the post-ban Markov process is more likely to produce lower productivity levels. Observe that MWBE contractors expecting lower future productivity will have lower continuation values, which should make exit relatively more likely. My finding that the exit difference between MWBEs and non-MWBEs remains stable suggests that lower scrap values offset the lower MWBE continuation values. That is to say, MWBE sell values decrease post ban.

6.3 Entry

Moving to entry, I assume potential entrants draw entry costs, c_e, from the distribution function F_{c_e}. Should a potential entrant incur its entry cost, it will become an entrant and draw an initial productivity from the distribution function G. The value function for a potential entrant after observing c_e is then

$$V_E(c_e) = \max\left\{0, -c_e + E_G[V_I(\varphi)]\right\},$$

with potential entrants deciding to enter if

$$c_e < E_G[V_I(\varphi)]. \tag{5}$$

Thus, fewer potential entrants, higher entry costs, lower values of incumbency, and decreased chances of drawing a high initial productivity all contribute to reduced entry.

The absence of information on potential entrants limits a complete analysis of entry behavior, as fewer MWBE (or more non-MWBE) potential entrants could drive the shift in entry observed in the data. That said, the investigation of new entrants presented in Section 5.5 indicates a lack of a relative size change post ban, suggesting that initial productivity draws may have remained comparatively stable after states changed their affirmative action laws. As mentioned earlier, the relatively smaller size of MWBEs suggests proportionally lower productivity draws, implying a lower value of incumbency relative to non-MWBEs. Thus, even without a change in entry costs, the reduced prospects of obtaining good future productivity draws would lower entry for MWBEs relative to non-MWBEs.

6.4 Summary

In sum, the model suggests that the relatively lower MWBE employment post ban corresponds to a decline in relative productivity, likely driven by reduced MWBE contracting opportunities once provided through affirmative action. The model also suggests that the relatively stable post-ban exit levels mean that MWBE sell values declined, indicating that MWBEs became relatively less valuable to the market than non-MWBEs. Although the lack of information on potential entrants limits the model's implications for entry, the relatively lower value of owning an MWBE—given by the value of incumbency—likely contributed to the MWBE entry differential observed post-ban.

7 Quantitative Analysis

Using my conceptual framework as a basis, I now seek to quantify how much banning affirmative action would have needed to change MWBE primitives to generate the observed post-ban behavior. In doing so, I will parameterize various features of the model and select parameter values producing outcomes that most closely resemble the MWBE data. Before moving into that analysis, though, I briefly discuss my modeling choices here to provide context for my subsequent measurements.

In what follows, I will consider a ban on affirmative action as being "outcome equivalent" to certain unanticipated changes in MWBE productivity and scrap values. I am not attempting to model an affirmative action ban directly, as various institutional changes could influence post-ban decision-making in unmodeled ways. Instead, I am measuring the extent to which productivity and scrap values must change from the pre-ban status quo to recreate the employment and exit outcomes observed in MWBEs post-ban, referring to those changes as outcome equivalent to banning affirmative action. Additionally, to economize on the number of parameters, I will model all treated MWBEs as a homogenous group. This choice abstracts away from some of the heterogeneity presented earlier, yielding an average effect across all MWBEs.[18] Finally, I do not calibrate the entry cost parameters, as it requires unavailable data on prospective entrants.

[18] One could extend my framework to multiple groups by separately calibrating each group's parameters.

7.1 Parameterization

With those caveats in mind, I assume each MWBE contractor produces a differentiated product with downward-sloping, iso-elastic demand $q_{jt} = p_{jt}^{-\sigma}$, where $\sigma > 0$ is the constant price elasticity of demand.[19] I assume a production technology linear in labor: $q_{jt} = \varphi_{jt} l_{jt}$. Thus, a firm's revenue function is

$$R(\varphi_{jt}, l_{jt}) = (\varphi_{jt} l_{jt})^{\frac{\sigma-1}{\sigma}},$$

and its profit function is

$$\pi(\varphi_{jt}, l_{jt}) = R(\varphi_{jt}, l_{jt}) - l_{jt}.$$

Observe that I have normalized profits by the wage rate and do not have a fixed cost of labor. Consequently, those fixed costs will load onto scrap values, ϕ, and the results will be relative to prevailing wages.

I assume pre-ban MWBE productivity evolves according to the following AR(1) process:

$$\log(\varphi_{jt+1}) = (1-\rho)\mu + \rho \log(\varphi_{jt}) + \varepsilon_{jt}, \quad \varepsilon_{jt} \sim \mathcal{N}(0, \sigma_\varepsilon^2).$$

Here, μ is the long-run productivity log-mean, ρ is a persistence parameter, and σ_ε^2 denotes the log-variance of the productivity shocks. I endeavor to determine how much of a long-run MWBE productivity and scrap-value shock banning affirmative action equates to. Thus, post-ban, I will allow the long-run log-mean to change additively by $\tilde{\mu}$, making the post-ban log-mean $\mu + \tilde{\mu}$. Similarly, I will allow scrap values to change multiplicatively by $\tilde{\phi}$ so that post-ban scrap values are $\phi \times \tilde{\phi}$. These change parameters are the focus of my quantitative analysis.

Lastly, because I do not observe most MWBE firms in their first year of production, I must make an assumption about their initial productivity. Consequently, I assume MWBEs start the first period of pre-ban production with an initial productivity drawn from the distribution function G_0, where G_0 is log-normal with log-mean μ_0 and log-variance σ_0^2.

[19] Traditional iso-elastic demand functions also have a multiplicative constant capturing the scale of demand. Because a productivity change would not be identified separately from a scale demand change, I have normalized this constant to 1.

7.2 Calibration Strategy

In total, I must determine ten parameters: σ, β, μ, ϕ, ρ, σ_ε, μ_0, σ_0, $\tilde{\mu}$, and $\tilde{\phi}$. I set the model period to one year and calibrate the first two parameters (σ and β) externally, using standard values from the literature (see Foster et al. (2008) for a range of price elasticity estimates obtained from Census microdata). Specifically, I set the demand elasticity to $\sigma = 5$, implying a markup of 25 percent. The discount factor is fixed at $\beta = 0.96$. I internally calibrate the remaining parameters by matching various SBO-weighted data moments to moments obtained through model simulations.

In the model, current-period productivity determines an MWBE's employment, so I calibrate the initial and pre-ban productivity means (μ_0 and μ) to match the average initial and pre-ban MWBE employment levels, respectively. Productivity variance shapes tail employment behavior. Thus, I choose the initial variance parameter (σ_0) to match the initial share of "large" MWBEs, where I define a large firm as having 100 or more employees. I use the pre-ban large share to set the variance of the AR(1) process (σ_ε). Because scrap values determine exit behavior, I set its parameter (ϕ) to match the percent of MWBEs who exit pre-ban. The persistence parameter (ρ) governs the convergence of the AR(1) process to its long-run mean, so I select it to match the average pre-ban MWBE growth rate.

I use the policy variation induced by statewide affirmative action bans to calibrate the change parameters ($\tilde{\mu}$ and $\tilde{\phi}$). To do so, I estimate the triple-differences specification in equation (2), using two different dependent variables: the inverse hyperbolic sine of employment and an indicator for firm exit.[20] The coefficients allow me to predict the average change in employment and exit for MWBEs following a ban, and I match those predictions to the adjustments firms make in the simulated data when long-run productivity changes by $\tilde{\mu}$ and scrap values change by $\tilde{\phi}$. Appendix B contains further details.

Table 4: Calibrated MWBE Parameter Values

Externally Calibrated Parameters			
	Value	Definition	Basis
σ	5.00	Elasticity coefficient	Standard
β	0.96	Discount factor	Standard

Internally Calibrated Parameters					
	Value	Definition	Moment	Data	Model
μ	0.06	AR(1) long-run mean	Avg. pre-ban employees	12.51	12.92
ϕ	30.30	Scrap value	Perc. pre-ban exit	4.57	4.51
ρ	0.96	Persistence of AR(1)	Perc. growth rate pre-ban	26.97	26.94
σ_ε	0.16	Std. dev. AR(1) shocks	Perc. large pre-ban	1.86	1.63
μ_0	0.71	Initial mean prod.	Avg. initial employees	12.49	12.27
σ_0	0.31	Initial std. dev. prod.	Perc. initial large	1.69	1.12
$\tilde{\mu}$	-0.09	Change in long-run mean post-ban	Perc. change employment pre-to-post-ban	-1.03	-1.29
$\tilde{\phi}$	0.90	Change in scrap value post-ban	Perc. point change exit pre-to-post-ban	0.42	0.16

Note: Externally and internally calibrated parameter values and their targeted moments. All moments use SBO weights. Growth rate describes a firm's growth in employment. Large firms have more than 100 employees. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12995.

7.3 Calibration Results

Table 4 presents the calibrated parameter values, the SBO-weighted moments, and the moments obtained through model simulation.[21] Overall, the model fits the targeted moments well. The initial productivity specification and parameters can recreate initial employment levels. In a similar vein, the log-mean and standard deviation parameters reproduce the observed pre-ban size distribution. The persistence parameter (of 0.96) implies that, on average, 96 percent of an MWBE's log-productivity carries over into the next period. This estimate is on par with Hopenhayn et al. (2022), who use similar moments to estimate a model of firm dynamics. The scrap value parameter (of 30.30) means that firms sell for roughly 30 wage units. While this estimate may seem high at face value, recall that it includes the present value of fixed-cost payments, which I have normalized to zero.[22]

The parameter values of interest are the change parameters. I find that a $\tilde{\mu}$ of -0.09 and a

[20]Both regressions use the linked SBO-LBD contractor data from the earlier intensive-margin analysis. The employment regression requires that firms existed before the ban, while the exit regression uses the full, unbalanced panel.

[21]Observe that the employment prediction here is somewhat conservative. The triple-differences regression suggests that MWBEs reduced employment by approximately 4 percent relative to non-MWBEs in treated states after the ban, but a statistically insignificant 3 percent employment increase for all treated, post-ban firms offsets that change.

[22]For example, if fixed costs are 1 wage unit per period, the present value of the fixed-cost stream is $\sum_{t=0}^{\infty} \beta^t = \frac{1}{1-0.96} = 25$. A scrap value of 30.30 equates to a fixed cost of 1.21 wage units.

$\tilde{\phi}$ of 0.90 replicate the post-ban changes in MWBE employment and exit. The $\tilde{\mu}$ value translates into a 8.6 percent decrease in median long-run MWBE productivity.[23] Likewise, the $\tilde{\phi}$ value means that scrap values must decrease by 10 percent to reconstruct post-ban exit. As such, I conclude that banning affirmative action is *outcome equivalent* to an 8.6 percent decrease in median long-run productivity and a 10 percent reduction in scrap values from the pre-ban status quo. One can also contrast the present discounted value of incumbency—given by $V_I(\varphi)$ in equation (3)—under the pre-ban and ban-equivalent parameters. Doing so implies a 12 percent decrease in MWBE value for a median-productivity firm under the ban-equivalent parameters.

8 Concluding Remarks

Whether to eliminate affirmative action—or preferences based on an individual's race or sex—continues to be a hotly contested issue. This study uses confidential Census data on 114,000 firms throughout much of the United States to examine how the elimination of affirmative action affected minority- and women-owned business enterprises, in the context of state and local contracting.

I find that MWBEs gradually downsized relative to non-MWBEs after a ban. Size reductions were most severe for Black-owned businesses and larger MWBEs, with no statistical difference for the other race, sex, and size categories. Results by county type suggest that more metropolitan counties experienced the largest relative declines in MWBE employment, while state-level results indicate that MWBEs in New Hampshire and Arizona were relatively less affected than those in other states. As for entry and exit, I find that states with bans attracted fewer new MWBEs, but existing MWBEs were generally no more likely to exit than non-MWBEs. To understand the productivity and value implications, I calibrate a model of firm employment and exit to the pre-ban MWBE data. I find that banning affirmative action is equivalent to notable reductions in long-run MWBE productivity and scrap values.

All in all, my study quantifies the effects of banning affirmative action in state contracting, highlighting the types of businesses most affected and the implications for business dynamism, productivity, and value. My focus on state contractors contributes to an understanding of the

[23] The long-run productivity median of the AR(1) process is $\exp(\mu)$ pre-ban and $\exp(\mu)\exp(\tilde{\mu})$ to replicate post-ban behavior. Thus, the percent decrease is $100 \times (\exp(\tilde{\mu}) - 1)$.

business implications of banning affirmative action—a salient feature of the statewide bans, with limited firm-level empirical studies. Consequently, this research adds further information for policymakers considering the broader impacts of removing affirmative action.

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A Robustness

This appendix investigates the robustness of the results to various specification changes. Subsections A.1–A.3 consider different regression specifications—including ones with a balanced panel, no control variables, and different transformations of the dependent variable. Subsection A.4 contains a placebo analysis, where I explore whether unexposed firms react to state affirmative action bans.

A.1 Balanced Versus Unbalanced Panel

The results presented in Figure 1 use an unbalanced sample. In such a sample, the composition of firms can change due to businesses exiting after a ban. If these compositional changes drive the main coefficient estimates, survivorship bias becomes a notable issue, as it would imply that differences in firm outcomes are primarily explained by differences in the *types* of surviving firms.

The lack of differential exit in Figure 5a suggests that compositional changes are unlikely to explain most of the employment differential. Here, I go a step further by re-running the employment regression on a balanced sample. By doing so, I eliminate any differences in firm composition because firms must operate each year to remain in the sample. Statistically similar coefficient estimates would then suggest that survivorship bias is not a first-order concern.

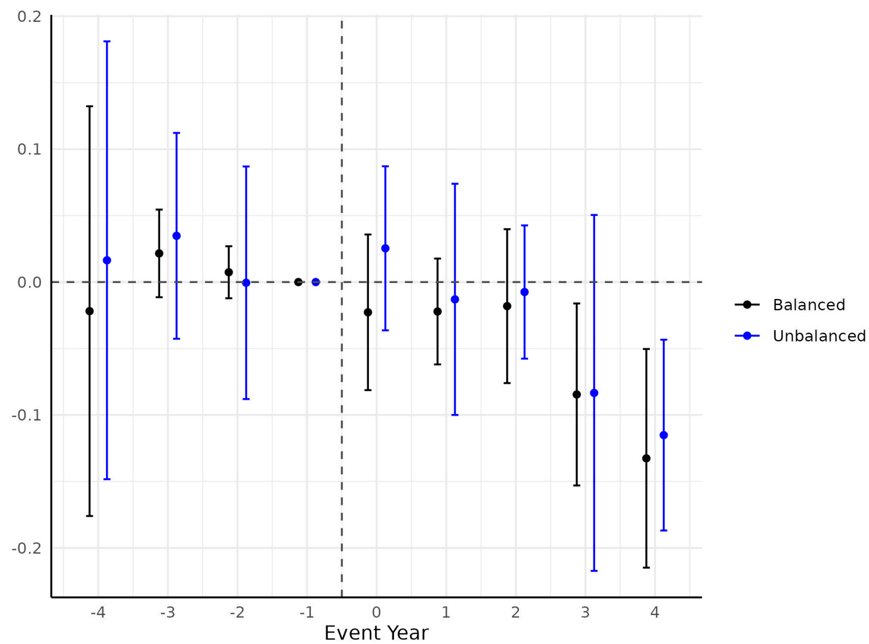
Table 5 presents counts of the number of firms in the balanced and unbalanced samples. Observe that these counts are for the linked SBO sample only, as it is the one used in the main analysis. Figure 6 presents the results. Reassuringly, the estimates are similar across the sample types.

Table 5: Robustness—Number of Firms in the SBO Sample, Balanced vs Unbalanced

	Treatment	Control	All
Unbalanced sample	4600	82000	86600
Balanced sample	3400	62500	65900

Note: Table reports counts of firms in the SBO data used to analyze robustness to having a balanced panel. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

Figure 6: Robustness—Effect of Affirmative Action Bans on Employment, Balanced vs Unbalanced

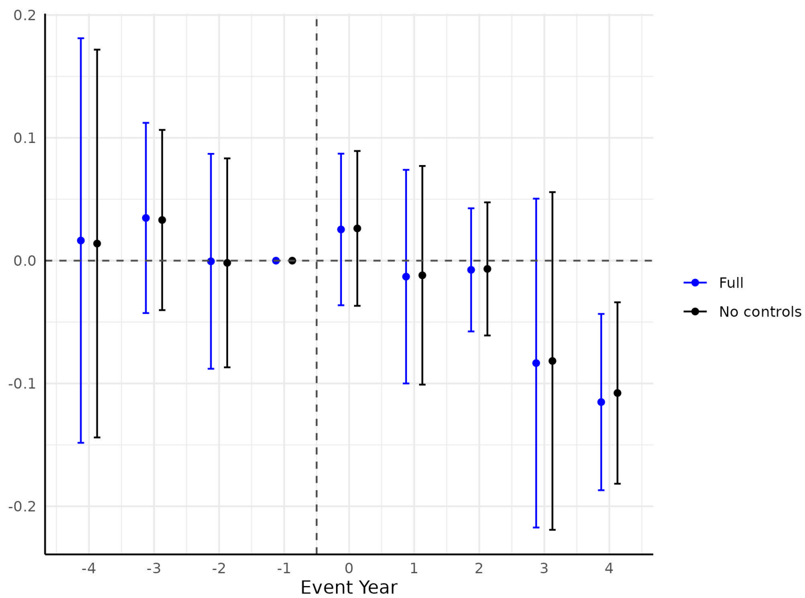


Note: Figure plots the three-way interaction coefficients, δ_s, and 95% confidence intervals attained from estimating equation (1) on a balanced and unbalanced panel of firms. The dependent variable is the inverse hyperbolic sine of employment. Regressions use SBO weights, with data winsorized from above and below by 1%. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

A.2 Control Variables

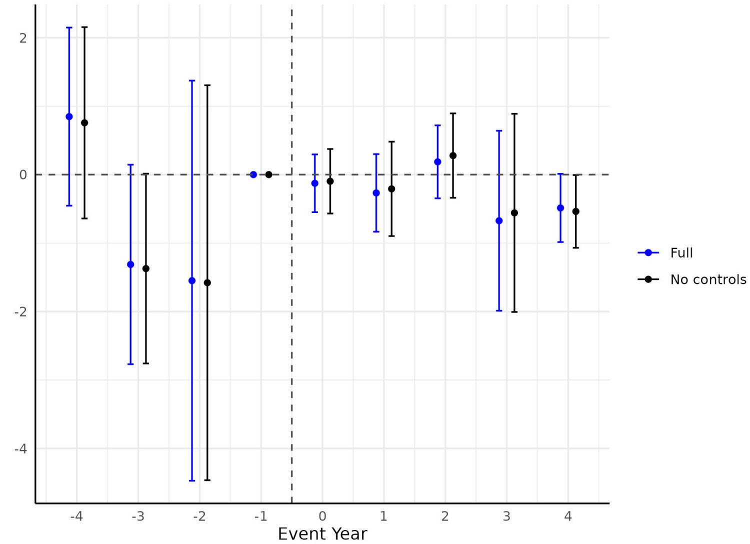
This appendix reports the sensitivity of the coefficient estimates to the inclusion of the control variables.

Figure 7: Robustness to Controls—Effect of Affirmative Action Bans on Employment



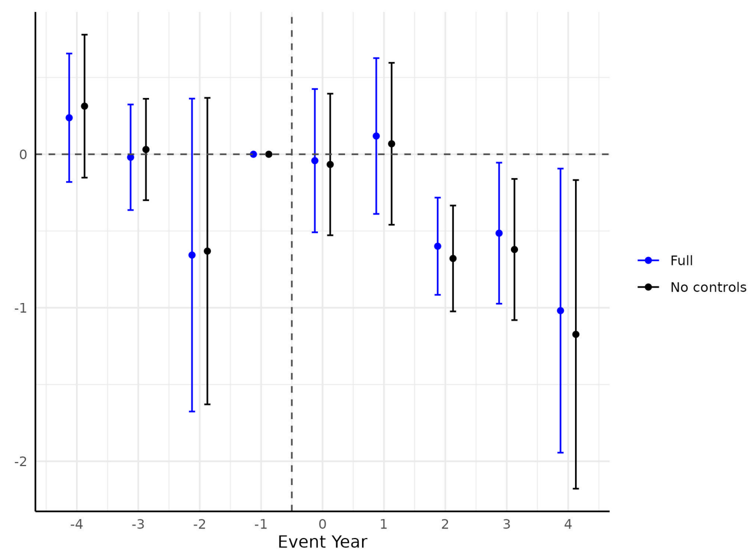
Note: Figure plots the three-way interaction coefficients, δ_s, and 95% confidence intervals attained from estimating equation (1) with and without the control variables. The dependent variable is the inverse hyperbolic sine of employment. Regressions use SBO weights, with data winsorized from above and below by 1%. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

Figure 8: Robustness to Controls—Effect of Affirmative Action Bans on Exit



Note: Figure plots the three-way interaction coefficients, δ_s, and 95% confidence intervals attained from estimating equation (1) on the collapsed firm data with and without control variables. The dependent variable is the count of firms in each minority-owned-by-women-owned-by-sector-by-state-by-year cell. All regressions use SBO weights at baseline. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

Figure 9: Robustness to Controls—Effect of Affirmative Action Bans on Entry



Note: Figure plots the three-way interaction coefficients, δ_s, and 95% confidence intervals attained from estimating equation (1) on the collapsed firm data with and without control variables. The dependent variable is the count of firms in each minority-owned-by-women-owned-by-sector-by-state-by-year cell. All regressions use SBO weights at baseline. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

A.3 Dependent Variable Transformation

Many regression specifications use inverse hyperbolic sine-transformed dependent variables. While convenient for handling zeros, this transformation can be sensitive to units. For example, an inverse-hyperbolic-sine-transformed regression may yield considerably different coefficients if employment were to be measured in tens or hundreds, but banning affirmative action in percentage terms should be invariant to outcome units.

To explore whether such sensitivity is an issue here, I conduct a robustness exercise by comparing results from an inverse-hyperbolic sine regression and a unit-invariant log-transformed model, each with zero observations dropped. Because inverse hyperbolic sine should approximate logs for positive values, the coefficients for both regressions should be statistically similar. Dropping zeros allows me to run the log model, but also means that the coefficients may differ from the ones presented in Figure 1. One should expect such differences given the different samples, and those differences are not the focus here. Instead, this analysis investigates whether units cause the inverse hyperbolic sine transformation to differ substantially from the log transformation.

Table 6 contains the results. I find that the coefficients are indeed similar across specifications, suggesting that the units of the dependent variable do not drive the main results.

Table 6: Robustness—Dependent Variable Transformations

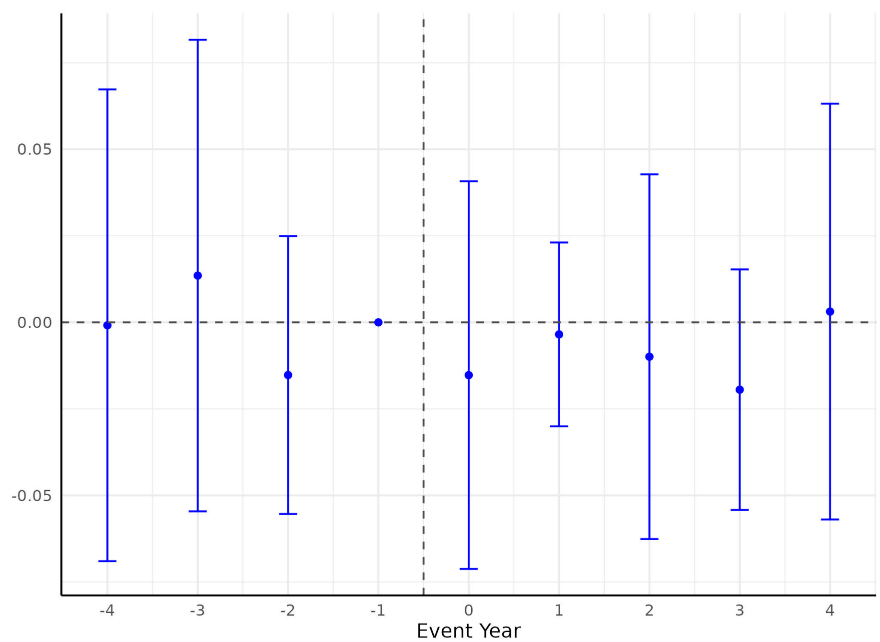
	$\sinh^{-1}$(Employment)	log(Employment)
$MWBE \times treat \times year_{s=-4}$	-0.00492	-0.00598
	(0.02889)	(0.0301)
$MWBE \times treat \times year_{s=-3}$	-0.01047	-0.01356
	(0.02946)	(0.03052)
$MWBE \times treat \times year_{s=-2}$	-0.02043	-0.02285
	(0.01502)	(0.0151)
$MWBE \times treat \times year_{s=0}$	-0.009444	-0.01038
	(0.01721)	(0.01836)
$MWBE \times treat \times year_{s=1}$	-0.04655	-0.05398*
	(0.029)	(0.03055)
$MWBE \times treat \times year_{s=2}$	-0.04625	-0.05317
	(0.03376)	(0.03611)
$MWBE \times treat \times year_{s=3}$	-0.04383**	-0.0482**
	(0.02066)	(0.02229)
$MWBE \times treat \times year_{s=4}$	-0.05848	-0.06435
	(0.04537)	(0.04852)

Note: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Table reports employment regression output under different transformations of the dependent variable. Coefficients are from the triple differences event study presented in equation (1) and measure outcomes relative to the year affirmative action was banned ($s = -1$). Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

A.4 Placebo Analysis

The main results focus on firms exposed to changes in statewide affirmative action bans, specifically those that performed work for state governments. However, there is a possibility that the observed results are a consequence of other state-level changes unrelated to the ban, such as the passage of more business-friendly state laws that differentially affect MWBEs and non-MWBEs. This subsection explores that possibility further by running the same specification used to create Figure 1, except using a "placebo" group of firms that should not have been exposed to state contracting changes. As mentioned in Section 3.1, these placebo firms are the ones in the SBO that report having 10% of their earnings coming from individuals and less than 10% coming from either the (state or federal) government or other businesses. There are 432,500 placebo firms: 22,500 in treated states, and 410,000 in control states. Figure 10 presents the results, where I find that these unexposed firms do not change their workforce in response to the ban.

Figure 10: Placebo Analysis—Effect of Affirmative Action Bans on Employment, Unexposed Firms



Note: Figure plots the three-way interaction coefficients, δ_s, and 95% confidence intervals attained from estimating equation (1) on placebo firms. Placebo firms report having 10% of their earnings coming from individuals and less than 10% coming from other sources. The dependent variable is the inverse hyperbolic sine of employment. Regressions use SBO weights, with data winsorized from above and below by 1%. Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12853.

B Calibration Details

This section presents more details on my calibration routine. In calibrating the model, I use a procedure similar in spirit to simulated method of moments, matching moments from simulated data to those from actual data. In what follows, I consider the data simulation and moment generation processes separately.

B.1 Data Simulation

Starting with a vector of parameter values, I simulate data by solving an incumbent firm's value function twice: once with the pre-ban parameters and once with the post-ban ones (see equation (3)). This process uses standard value-function-iteration methods and yields cutoff productivity levels below which a firm will choose not to operate.

With those cutoffs in hand, I then draw a large number[24] of initial productivity levels from the distribution function G_0 and simulate their evolution forward for nine total periods. For the first four periods, I forward-simulate using the pre-ban Markov process and mark firms with productivity levels below the pre-ban cutoff as having exited. Those firms can no longer produce, so I stop simulating future productivity values for them. In the last five periods, I repeat that procedure for the remaining firms using the post-ban Markov process and the post-ban cutoff, ultimately yielding data on firm productivity and exit decisions. From there, I map productivity levels into employment using the first-order conditions for optimal labor, creating the simulated data.

B.2 Moments

What remains is to construct the moments for the simulated and actual data. Many of the moments are straightforward: average employment levels match the pre-ban or initial employment levels in the simulated and actual data, exit percentages match the exit rates, and the percentage large matches the proportion of large (or greater than 100-employee) firms. The pre-ban growth rates match the average actual and simulated growth rates of firms, as measured by the average percent change in the number of employees in the pre-ban period.

[24] In practice, I simulate $20,000$ initial firms.

Table 7: Calibration—Regression Coefficients

	$\sinh^{-1}$(Employment)	Exit
$MWBE \times post$	0.00484	0.009436***
	(0.005807)	(0.001342)
$post \times treat$	0.0279	-0.004045
	(0.02589)	(0.007937)
$MWBE \times post \times treat$	-0.04312*	-0.001194
	(0.0231)	(0.009069)

Note: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Table reports the effects of banning affirmative action on employment and exit, where exit is an indicator equal to one if the firm exits the sample in period s. Coefficients are from the triple differences regression presented in equation (2). Results were approved for release by the U.S. Census Bureau under FSRDC Project Number 2075, with approval number CBDRB-FY26-P2075-R12995.

The moments measuring employment and exit changes post-ban are more involved. For those moments, I use regression coefficients, with the dependent variables being the inverse hyperbolic sine of employment and an indicator for whether a firm exits in a given year.[25]

More precisely, the data moments correspond to the predicted change in MWBE outcomes in a treated state post-ban. This change equates to the sum of coefficients presented in Table 7, which uses the triple-differences specification in equation (2). The sum of these terms is -0.0103 for employment and 0.0042 for exit, respectively—matching the moments presented in Table 4 when put into percentage terms. The firm, year, and state fixed effects absorb the other interactions, so I exclude them.

To obtain a similar measurement in the simulated data (which has all treated firms and no control group), I run the following, more simplistic regression:

$$f(y_{jt}) = \alpha + \delta post_{jt} + P(t) + \psi_t + \varepsilon_{jt},$$

where $post_{jt}$ is a post-ban indicator, ψ_t are firm effects, and P is a flexible (cubic) polynomial in time, t.[26] This polynomial captures time trends in the model, which would be controlled for by the year effects in equation (2). Note that, conditional on initial productivity, firms are homogeneous in the model. As such, the sector, survey, and state controls appearing in X_{jct} become unnecessary when one includes firm fixed effects. The coefficient of interest is then δ,

[25] The employment regression uses the unbalanced panel for consistency with my earlier results. The exit regression uses the full, unbalanced panel (including pre-ban exits), enabling comparisons across pre- and post-ban exits.

[26] Observe that there are no cohorts in the simulated data, which is why there are no cohort subscripts here.

which measures the change in outcomes for treated MWBEs after a ban. In my calibration, I match this coefficient to the sum of interactions presented in Table 7.