Oxford Review of Economic Policy, 2024, **40**, 607–629
https://doi.org/10.1093/oxrep/grae034
Article

OXFORD

The long-run impacts of banning affirmative action in US higher education

Francisca M. Antman,* Brian Duncan, and Michael Lovenheim*****

*University of Colorado Boulder, USA, e-mail: francisca.antman@colorado.edu
**University of Colorado Denver, USA, e-mail: brian.duncan@ucdenver.edu
***Cornell University, NBER, and Hoover Institution, USA, e-mail: mfl55@cornell.edu

We thank the editors, William A. Darity, Jr and Isabel Ruiz, for their feedback as well as Matthew Kraft, Ken Mayhew, Ariella Spitzer, and seminar participants at the Ramón Areces Foundation and University of Carlos III in Madrid. This project was supported by the National Science Foundation, under NSF Award Number SES: 2121120.

Abstract

This paper estimates the long-run impacts of banning affirmative action on men and women from under-represented minority (URM) racial and ethnic groups in the United States. Using data from the US Census and American Community Survey, we use a difference-in-differences framework to compare the college degree completion, graduate degree completion, earnings, and employment of URM individuals to non-URM individuals before and after affirmative action bans went into effect across several US states. We also employ event study analyses and alternative estimators to confirm the validity of our approach and discuss the generalizability of the findings. Results suggest that banning affirmative action results in a decline in URM women's college degree completion, earnings, and employment relative to non-Hispanic White women, driven largely by impacts on Hispanic women. Thus, affirmative action bans resulted in an increase in racial/ethnic disparities in both college degree completion and earnings among women. Effects on URM men are more ambiguous and indicate significant heterogeneity across states, with some estimates pointing to a possible positive impact on labour market outcomes of Black men. These results suggest that the relative magnitude of college quality versus mismatch effects vary for URM men and women and highlight the importance of disaggregating results by gender, race, and ethnicity. We conclude by discussing how our results compare with others in the literature and directions for future research.

Keywords: affirmative action, higher education, racial disparities.

JEL codes: J15, J18, I23

I. Introduction

Affirmative action in undergraduate admissions is one of the most controversial sets of policies in higher education. These policies consist of preferences in the admission process given to students from under-represented minority (URM) groups: typically those from Black, Hispanic, and Native American backgrounds. Because college admissions in the United States are decentralized, there is no single affirmative action policy or practice. Rather, each post-secondary institution makes a separate decision about how it weights race/ethnicity in the admissions process. Affirmative action has a complex legal history at both the state and federal levels. The recent US Supreme Court decision in *Students for Fair Admissions, Inc. v. President and Fellows of Harvard College* ruled that explicit racial and ethnic preferences in college admissions are unconstitutional. This effectively renders existing affirmative action policies illegal; however it is unclear whether and how universities will comply with this ruling and how it will impact both majority and minority students in the long run.

Prior to the *Students for Fair Admissions* ruling, nine states had passed bans of some form on the use of affirmative action in public university admissions.[1] In this paper, we use the passage of affirmative action bans in the

[1] These states are: Texas (1997), California (1998), Washington (1999), Florida (2001), Georgia (2002), Michigan (2006), Arizona (2010), New Hampshire (2012), and Oklahoma (2013). The ban in Georgia only applied to the University of Georgia. The Texas ban was ruled

first four states to ban affirmative action—Texas, California, Washington, and Florida—to examine the long-run effects of banning affirmative action on educational attainment and labour market outcomes of exposed cohorts. We focus on these four states because they banned affirmative action in the late 1990s and early 2000s, allowing sufficient time to be able to assess long-run outcomes. Using data from the American Community Survey (ACS) on adults aged 25–51 based on their state of birth, we examine how outcomes evolve in cohorts who were over versus under age 17 at the time of the ban enactment (i.e. eligible to apply to college for the first time), separately by race/ethnicity and gender. Our analysis focuses on college degree completion,[2] graduate degree completion, log earnings conditional on employment, and employment for cohorts 10 years before and after the passage of an affirmative action ban.

Theoretical predictions of how these bans might affect our outcomes of interest are ambiguous. The ambiguity is driven by what Arcidiacono and Lovenheim (2016) term the 'quality-fit tradeoff'. Banning affirmative action leads to lower representation of URM students at selective universities (Kain *et al*., 2005; Cortes, 2010; Hinrichs, 2012; Bleemer, 2022). A substantial body of research shows that there are high educational attainment and labour market returns to attending a more selective college or university (e.g. Brewer *et al*., 1999; Black and Smith, 2004, 2006; Hoekstra, 2009; Long, 2010; Bound *et al*., 2010; Cohodes and Goodman, 2014; Andrews *et al*., 2016; Goodman *et al*., 2017).[3] Hence, we would expect banning affirmative action to negatively impact college degree completion and earnings of URM students on average.

The theoretical ambiguity and source of the trade-off noted above comes from the 'mismatch hypothesis' (Sander, 2004).[4] The mismatch hypothesis states that the match between the academic qualifications of students and the average academic qualifications of students at the institution is important for determining outcomes, with a closer match leading to better outcomes. Affirmative action (AA) policies induce URM students with lower academic achievement levels to enrol in selective universities. Indeed, the students receiving AA-based admission assistance at highly selective institutions can have achievement levels that are below those of any majority students (Arcidiacono and Lovenheim, 2016). These students thus are 'overmatched' relative to the institution, i.e. they have lower academic ability measures relative to the institutional average.

Affirmative action-induced overmatch can harm the subsequent labour market outcomes of students from under-represented backgrounds through two mechanisms. First, it can cause students to sort out of more technically demanding majors, such as STEM fields, which have been shown to lead to higher earnings on average (Arcidiacono *et al*., 2012; Andrews *et al*., 2022). These students have academic qualifications that more closely resemble (i.e. 'match') successful STEM majors at less selective colleges (Arcidiacono *et al*., 2016), which creates a potential trade-off between college quality and major choice. Second, their relative level of preparedness can negatively impact their degree completion. Simply put, the main question underlying the mismatch hypothesis is whether the returns to college quality are ubiquitous or whether they vary by the strength of the match between a student and their peers.

Evidence on mismatch is mixed, reflecting the highly challenging underlying identification problem:[5] by design, students receiving AA-based preferences have lower academic achievement than their majority peers. Thus, there is no natural control group within the same university, since there are no students who did not receive race-based admission preferences with the same pre-collegiate academic background. Moreover, comparing outcomes of students of the same race/ethnicity with similar pre-collegiate achievement levels across universities of differing quality or selectivity is confounded by unobservables related to the selection of students to schools. Arcidiacono *et al*. (2016) use data from California and show that minority students at the more selective schools (e.g. UC–Berkeley) are much less likely to graduate with a STEM degree than their majority peers, which is explained by lower incoming academic achievement levels of the minority students. However, students with these same achievement levels are successful at completing STEM degrees at less selective UC schools, which is evidence consistent with mismatch. These results suggest that mismatch may lead to reduced returns to college quality for URM students, insofar as it causes lower-achieving students to sort away from higher-return STEM majors.

unconstitutional in 2003, but only University of Texas at Austin reintroduced the use of race-based admissions after the 2003 ruling. As well, Texas implemented a 'top-10 percent rule' after its ban, which partially addressed the reduction in minority enrolment at the most selective public schools in Texas (Kain *et al*., 2005). See Arcidiacono and Lovenheim (2016) for further discussion of the history of state affirmative action bans.

[2] We use the terms 'attainment' and 'completion' interchangeably to refer to receipt of a college or graduate degree.

[3] See Lovenheim and Smith (2022) for a recent review of the returns to college quality literature.

[4] Affirmative action also could cause students to alter their pre-collegiate effort or change where they apply to college. Antonovics and Backes (2014) find little evidence that AA bans affect either margin of behaviour.

[5] Arcidiacono *et al*. (2015) and Arcidiacono and Lovenheim (2016) review the affirmative action literature on mismatch.

Dillon and Smith (2020) examine match effects more broadly in higher education, using data from the National Longitudinal Survey of Youth of 1979 and 1997. Their findings suggest that college quality effects are large and are evident for students across the achievement distribution. However, they do find some evidence that long-run earnings returns to college quality are lower for lower-achieving students, which is consistent with modest mismatch effects.

Some international evidence also can shed light on the role of mismatch in driving the returns to college quality. Machado *et al*. (forthcoming) use data from a selective Brazilian university that implemented an affirmative action quota of 45 per cent of seats in each major for Black and low-income students. Using a regression discontinuity design that leverages test-based cut-offs in admissions to specific majors, they show that marginally-admitted minority students experienced 14 per cent higher earnings after college. However, they also find evidence of similarly-sized adverse spillovers, whereby the returns of students in the same programme not admitted under the AA policy were reduced (including those of minority students). This paper thus suggests that mismatch effects may be small but also that there can be adverse spillovers to majority students from these policies. Moreover, affirmative action can harm majority students by displacing them to less selective colleges. Bertrand *et al*. (2010) study an affirmative action policy for lower-caste groups in India. They find a positive return for those admitted under the AA policy but a larger negative effect on upper-caste students who were displaced.

Depending on the relative magnitudes of the college quality and the college fit/match effects, affirmative action bans could have positive or negative effects on educational attainment and earnings of both minority and majority groups. A growing body of research has examined effects of state-level affirmative action bans, in part to attempt to disentangle the role of mismatch relative to college quality. Our paper contributes directly to this literature. Hinrichs (2012) and Backes (2012) estimate difference-in-differences models surrounding the banning of affirmative action in several states. They find that minority representation in selective public universities declined but that URM students were not less likely to attend college. Their evidence on graduation rates is inconclusive, however. Cortes (2010) examines the Texas affirmative action ban and also finds a large reduction in minority student enrolment at flagship public universities. She documents a sizeable post-ban decline in retention and 6-year graduation among URM students, which significantly widened the racial graduation gap between minority and majority students.

The paper most similar to ours in the literature is Bleemer (2022). He studies the elimination in 1998 of affirmative action in California through Proposition 209, using detailed administrative data on education and earnings among all applicants to the University of California system. Because he only has data from one state, he focuses on the change in the outcome gap between majority and URM students. His main findings indicate that the ban shifted URM students towards less selective public universities relative to White students, and the racial gap in collegiate attainment widened. He also documents a relative reduction in URM earnings up to 16 years post-application. This latter effect is concentrated among Hispanic students, even though both Black and Hispanic students experienced a decline in college quality relative to White students. The findings in Bleemer (2022) and Cortes (2010) are consistent with the college quality effect dominating any mismatch effects, but results are not disaggregated by gender.

We contribute to this literature along a number of dimensions that enrich our understanding of the long-run effects of these bans on students. First, like Backes (2012) and Hinrichs (2012), we examine bans in multiple states. This provides a more comprehensive view of affirmative action bans, and we are able to examine heterogeneity across states in a consistent sample. Second, our use of nationally representative data allows us to employ non-ban states as a control group. We thus can examine effects separately on non-Hispanic White, non-Hispanic Black, and Hispanic students in order to assess impacts on each group rather than solely the difference in outcomes across groups.[6]

Third, we examine effects separately by gender, which we are the first to do in the literature. Men and women vary significantly in academic achievement prior to college, with girls outperforming boys in most cases (Reeves and Smith, 2022), producing a gender gap that favours women and is significantly larger for URM groups (Reeves and Kalkat, 2023). This suggests the possibility that URM women may be more likely than URM men to benefit from affirmative action policies in admission at selective institutions and thus be more likely to be harmed by affirmative action bans. While women also outperform men in college enrolment and college degree attainment (Goldin *et al*., 2006; Reeves and Smith, 2021), women select into different majors (Turner and Bowen, 1999) and sort into lower-wage occupations, which ultimately result in lower earnings (Blau and Kahn, 2017). This large array of differences across gender in both the educational and labour market contexts could drive heterogeneity in

[6] For brevity, throughout the paper we often refer to non-Hispanic Black and non-Hispanic White groups as Black and White groups, respectively, however these samples are always comprised of non-Hispanic Blacks and Whites. Hispanics can be of any race.

how men and women from minority and majority groups are affected by AA bans, which underscores the importance of estimating impacts separately by gender. Finally, we examine all residents according to their place of birth rather than college applicants or enrolees at specific universities. This permits a more general analysis of the impact of these bans on the entire state population and mitigates concerns about any out-migration that may be generated by the AA bans, which otherwise might affect our estimates.

We produce two sets of results based on slightly different empirical approaches. First, we estimate difference-in-differences models within each ban state that examine how outcomes change for non-Hispanic Black and Hispanic women (men) relative to non-Hispanic White women (men) across cohorts when a ban is implemented. We then include control group states and use the staggered timing of ban adoption to estimate how outcomes evolve in treated versus non-treated states when the bans are implemented, separately by race and gender.

Our findings indicate that AA bans induce at most small effects on the educational attainment, earnings, and employment of men of any race/ethnicity. There is a suggestive positive effect of AA bans on earnings and employment of Black men that aligns with the theoretical predictions of the mismatch hypothesis, however the estimates are in many cases imprecise and do not allow us to draw strong conclusions for this group. We find more robust evidence on adverse effects of affirmative action bans on women, which highlights the value of examining effects by gender. Hispanic women experience a decline in the likelihood of college completion of 4 percentage points, a (not statistically significant) reduction in graduate attainment of 1.7 percentage points, a decline in earnings of 8.1 per cent, and a reduction in employment of 3.6 percentage points. Black women also experience declines in earnings of 4.2 per cent, however this estimate is not significantly different from zero at conventional levels. There is little effect on educational attainment for White and Black women, though White women experience a statistically significant earnings increase of 3.3 per cent. Hence, for women, AA bans lead to large increases in racial/ethnic earnings disparities. This result extends the finding in Bleemer (2022) by expanding racial/ethnic earnings gaps from the AA ban in California to a broader group of states and shows it is localized to women.

We also find evidence of substantial heterogeneity across states. For example, the earnings point estimate for Hispanic women in Texas is positive, while it is negative in the other three ban states. It is not immediately clear why this heterogeneity exists, but we highlight it as an important consideration for future research.[7] On average, AA bans have adverse effects in particular on Black and Hispanic women, but this finding is not ubiquitous across all bans. Developing a better understanding of when AA bans may harm minority groups is critical if policy-makers are to address racial disparities in outcomes while maintaining race neutrality in college admissions.

II. Affirmative action in the United States

Affirmative action in US undergraduate admissions is closely tied to a similar federal employment programme announced by the Kennedy administration in 1961 through Executive Order 10925. The executive order requires Federal contractors to 'take affirmative action to ensure that applicants are employed, and employees are treated during employment, without regard to their race, creed, color, or national origin'. In 1965, President Johnson issued Executive Order 11246, which required Federal contractors to take similar affirmative action to ensure equal opportunity for both women and minorities. President Johnson explained the rationale for this policy in his 1965 commencement address to Howard University:

> You do not take a person who, for years, has been hobbled by chains and liberate him, bring him up to the starting line of a race and then say, 'You are free to compete with all the others', and still justly believe that you have been completely fair.

This quotation makes clear that the original intent of affirmative action was to counteract historically prevalent, racially discriminatory hiring practices to support racial equity in the labour market.[8] Note also the tension between the two executive orders—the first emphasizing equal treatment and the second emphasizing equal opportunity—which would resurface in the practical applications and legal arguments for and against affirmative action.

Although admissions policies at higher education institutions were not covered under these executive orders, American colleges and universities followed the federal government's lead in fighting against historical patterns of discrimination

[7] One source of heterogeneity is the Texas Top 10% Rule, which went into place after the ban on affirmative action and at the time gave automatic admission to any public university to students in the top 10 per cent of their high school class. While the Top 10% Rule led to an increase in the URM enrolment rate at more-selective Texas public universities, URM enrolment rates at these universities remained below the pre-AA ban levels (Kain *et al*., 2005).

[8] The history of affirmative action policies in other countries have distinct origins and impacts. See Deshpande and Ramashadran (2024, this issue) and Francis-Tan and Tannuri-Pianto (2024, this issue) for greater context on affirmative action in India and Brazil, respectively.

against racial and ethnic minorities by taking 'affirmative action' to admit more students from under-represented backgrounds. This necessarily meant admitting students with lower pre-collegiate academic achievement because of large average racial/ethnic differences in achievement levels in the US (Reardon *et al*., 2014). Colleges and universities did not announce these policies, *per se*, so it is not clear when exactly affirmative action policies began. As a set of college-specific admission preferences, they also likely varied across institutions and changed over time.

While not explicitly stated, affirmative action in higher education can be thought to have had three main goals in alignment with the federal government's rationale. The first was to correct for discrimination against URM groups in society and often at the specific institution that was adopting the affirmative action policy. The second was to adjust for the fact that URM students likely face more adversity prior to college, which raises the quality of URM applicants conditional on their measured pre-collegiate academic qualifications. Hence, a URM student with lower academic qualifications can have similar potential to a majority student with higher qualifications. The third goal was to increase diversity at the institution. While these were the factors that may have induced schools to adopt affirmative action in admissions to begin with, the legal landscape changed over time such that the third goal of supporting institutional diversity became the preeminent legal justification.

There are a number of aspects of affirmative action that often are overlooked but are important to understand in the broader context of higher education. First, because AA is fundamentally an admission preference, only schools that practise selective admissions have affirmative action policies. The vast majority of colleges in the US are non-selective (including the large 2-year sector), and even many public flagship institutions have high admission rates. Thus, AA is isolated to the small set of institutions that are selective or highly selective. Second, there is no single affirmative action policy; institutions exercise preferences for racial/ethnic diversity in different ways. The decentralized and opaque admissions practices of elite universities, in particular, make it challenging to know exactly how race is being used in the admissions process at any one institution.

Third, race-based affirmative action is but one of a myriad set of preferences that institutions exercise in the admissions process. Colleges have preferences for athletes, musicians, students from different countries and states, students with different academic interests, and children of alumni (legacies). Given the low admissions rates at highly selective schools and the fact that elite universities could more than fill their classes with students with top grades and test scores, all admissions at these universities reflect some aspect of the preferences of the institution. The desire for racial/ethnic diversity is but one dimension of those preferences, albeit a particularly important and controversial one.

The controversial nature of race-based affirmative action has led to a large set of legal cases that has shaped how institutions can weight race/ethnicity in the admissions process. Several universities had historically used a quota system or awarded admissions points to URM students. These practices were ruled unconstitutional in *Bakke v. California Board of Regents* (1978) and *Gratz v. Bollinger* (2003). Prior to the most recent Supreme Court decision, the most important decision that shaped AA practices was *Grutter v. Bollinger* (2003). The majority ruling in this decision allowed the use of race in admissions that was 'narrowly tailored' to achieve institutional goals surrounding diversity. This decision shifted the justification for affirmative action away from providing restitution for groups that historically faced discrimination in the higher education sector to providing the university community with the educational benefits of a diverse student body. In effect, universities could continue to use affirmative action to provide the benefits of diversity to majority students, so long as race was one of a constellation of factors in a holistic admissions process. However, institutions could not justify using AA to correct past discriminatory practices.

The decision in *Students for Fair Admissions v. Harvard* (2023) put an end to even a narrowly tailored use of race in admissions. Writing for the majority, Justice Roberts argues that universities have not explained what education benefits flow from a more diverse student body, what specific goals affirmative action is meant to achieve, and how universities measure whether they are achieving those goals. While the majority opinion eliminates the use of even 'narrowly tailored' racial preferences in admissions, Roberts leaves the door open for universities to continue to consider a student's racial/ethnic background within the context of their achievements on an individual level:

> A benefit to a student who overcame racial discrimination, for example, must be tied to *that student's* courage and determination. Or a benefit to a student whose heritage or culture motivated him or her to assume a leadership role or attain a particular goal must be tied to *that student's* unique ability to contribute to the university. In other words, the student must be treated based on his or her experiences as an individual—not on the basis of race.

The recency of this opinion and the ability to justify racial preferences in terms of an individual's achievements, as the Roberts opinion allows, makes it uncertain how colleges will respond. Nevertheless, it is clear that the US has entered a period of less legal support for affirmative action, which also may affect employers' willingness to support affirmative action efforts in this new legal climate. Thus, it is reasonable to ask how this ruling might affect racial and ethnic disparities in education, earnings, and employment, not only because of the close ties between education and labour market outcomes, but also because of potential 'chilling' effects.[9] Examining effects of state-level affirmative action bans provides some insight into this question and previews what we might expect to see going forward in US higher education and in the labour market. We now turn to our examination of what happened to education, earnings, and employment outcomes in Texas, Washington, California, and Florida when they enacted affirmative action bans. This provides new information on how the elimination of race-based admission preferences may affect majority and under-represented minority students in the future.

III. Data

Data for this analysis are from the 2000 Census and the 2001 through 2021 American Community Survey microdata drawn from IPUMS USA (Ruggles *et al*., 2023). The sample is limited to US-born individuals aged 25 to 51 in the year they are surveyed. It is comprised of non-Hispanic Black, Hispanic, and non-Hispanic White respondents. For simplicity, we often refer to the non-Hispanic Black and non-Hispanic White samples as 'Black' and 'White' samples, respectively. Hispanics can be of any race. We exclude Native Americans from most of our analyses because they are too few in number to study separately, however we include them when we examine URM students as a single group. We also exclude Asians because the vast majority of the Asian population in the treated states lives in California. We thus cannot separate state-based heterogeneity from race-based heterogeneity for this group.[10]

The main treatment of interest is years of exposure to a state affirmative action ban among college-age students, r. This variable is calculated as the number of years the state's affirmative action ban had been in effect when the individual turned 17 years old (i.e. r = year turned 17 – year state affirmative action ban went into effect). For example, if $r = 3$, then the state affirmative action ban had been in place 3 years prior to the individual turning 17 years old, whereas if $r = -2$, then the ban went into effect 2 years after the individual turned 17. Constructed in this way, larger positive numbers suggest longer exposure to the state's affirmative action ban at the age most apply to college, and negative numbers indicate that an individual is a member of a cohort that applied to college before affirmative action was banned. Those with negative values of r were exposed to the affirmative action ban but are unlikely to be affected by it because they already have made college enrolment decisions. We consider these as pre-treatment groups, and any effects on these cohorts is a measure of how affirmative action bans in admissions may spill over to existing students or to older workers in the labour market.

As discussed above, nine states have passed bans on affirmative action in college admissions. Five of these states passed affirmative action bans more recently (NE, AZ, MI, NH, and OK), making it impossible to observe long-run outcomes, such as completed education and wages. We drop these late-adopting states from the analysis and focus on the ban states of Texas, California, Washington, and Florida, which were enacted in 1997, 1998, 1999, and 2001, respectively. This allows us to focus on longer-run outcomes of those who were exposed to AA bans prior to making college enrolment decisions.

IV. Empirical approach and results

We present a series of results below that examine how outcomes of URM and White individuals evolve after state affirmative action bans. First, we estimate how bans affect cross-cohort trends of URMs relative to non-Hispanic Whites within each state that passes a ban, separately by gender. These are essentially cross-race/ethnicity difference-in-differences models. Next, we use states that do not pass a ban as a control group to estimate cross-cohort difference-in-differences models of affirmative action bans by race/ethnicity and gender. We present a large number of estimates below, as we examine the effect of bans by race/ethnicity, gender, and, in some cases, by state. This leads to many hypothesis tests that could generate false positives. We therefore focus our attention on and draw our main conclusions from estimates where there is general agreement across specifications.

(i) Relative trends by race, gender, and state

We begin our empirical analysis with an examination of cross-cohort trends within each state that passes an affirmative action ban, separately by AA ban state, gender, and URM race/ethnicity, with non-Hispanic Whites

[9] These are only a handful of the wide-ranging outcomes that affirmative action policies might impact. Antman and Duncan (2015), for example, consider the impact of AA bans on the willingness to identify as a racial minority.

[10] While this is one limitation of our paper, we note that prior studies have shown that Asian students are likely to be significant beneficiaries of affirmative action bans (Espenshade and Chung, 2005). Note also that other papers in the literature group Asians and non-Hispanic whites as the comparison group when estimating the impacts of affirmative action policies on URM groups (Bleemer, 2022; Cortes, 2010).

serving as the reference category for each URM group. As discussed above, we create a treatment variable, r, that measures the number of years each cohort is potentially exposed to an affirmative action ban in their state of birth. We then estimate regressions of the following form:

$$Y_{iar} = \alpha + \delta URM_{iar} + \sum_{j=-10}^{10} 1_{\{j=r\neq -1\}}\gamma_r + \sum_{j=-10}^{10} 1_{\{j=r\neq -1\}}\beta_r URM_{iar} + \tau_{a,URM} + \epsilon_{iar}, \tag{1}$$

where Y is an outcome of interest for individual i, of age a who turned 17 years old r years before/after the state's affirmative action ban went into effect. The samples include either men or women who are Black or non-Hispanic White or are Hispanic or non-Hispanic White. *URM* is an indicator equal to one if the respondent is either non-Hispanic Black or Hispanic. We limit the sample to individuals in the ban states to those who turned 17 within 10 years of the state's affirmative action ban ($-10 \leq r \leq 10$). Given the samples we use and the age restrictions, the birth years of the individuals in the ban states range from 1970 to 1994.

Since individuals are observed in different survey years, we include indicators for each relative time value between –10 and 10, with –1 as the excluded category. All estimates thus are relative outcomes among cohorts that were 18 when the ban passed. The variables of interest in equation (1) are β_r, which show how outcomes evolve after ban passage separately for Black and Hispanic individuals relative to Whites. All models include age-by-URM fixed effects ($\tau_{a,URM}$). We estimate these models for each state, separately for a sample of non-Hispanic Blacks and non-Hispanic Whites and for a sample of Hispanics and non-Hispanic Whites. We also show URM estimates that combine the results for Black, Hispanic, and Native American respondents in order to make our results more consistent with prior research.

Appendix Figures A-1 through A-4 show estimates of β_r separately by AA ban state, URM race/ethnicity, and gender for college completion, graduate degree attainment, log earnings, and employment, respectively.[11] In Figure A-1, for men in the left panel, there are at most modest declines in collegiate attainment for Black or Hispanic respondents relative to White respondents after AA bans are passed in any state. The pre-treatment trends appear relatively flat, which suggests outcomes are not evolving differently across cohorts and racial/ethnic groups prior to ban passage. However, in some cases the pre-ban estimates are non-zero. As a result, there is a small post-ban decline in college completion in California, Washington, and Florida.

Table 1 presents difference-in-differences estimates for each state and outcome that effectively shows the difference between the average post-treatment estimates and pre-treatment estimates in Figures A-1 through A-4.[12] Black and Hispanic men are about 1.5–2 percentage points less likely to complete college relative to White men in Washington, Florida, and California after AA is banned. There is no statistically significant effect on collegiate attainment of men in Texas. These effects likely are biased away from zero due to some of the positive pre-treatment estimates in CA, WA, and FL. We confirm this below when we use non-ban states as a control group and find no effect of AA bans on college attainment for men. As a way to descriptively characterize the findings in Table 1, we report the simple average across the state-level estimates. These are not necessarily treatment effects on the treated, and ignore issues of statistical significance, but rather are a way to describe what the state-level estimates show on the whole. The simple average of estimates across states for Black male college completion is –1.1 percentage points.

In three of the four AA-ban states, Hispanic women experience larger declines in collegiate attainment relative to White women: –1.8 percentage points (Texas), –3.5 percentage points (California), and –3.7 percentage points (Florida). The point estimate in Washington is positive but it is not statistically different from zero at conventional levels. The average of the estimates across states is –1.7 percentage points. Black women experience a decline of 2.1 percentage points relative to White women in Florida, while the estimates in the other three states are small and are not statistically significant at even the 10 per cent level. These estimates align closely with the patterns shown later in the right panel of Figure 1 and together suggest little impact of banning AA on college completion among Black women.

Effects on graduate degree attainment, shown in Figure A-2 and in the second panel of Table 1, are more modest but present a similar pattern. There are small negative impacts on Black and Hispanic men relative to White men, but the effects are under 1 percentage point and are only statistically significant for Hispanic men in Texas and California. As well, these are likely biased away from zero by some positive pre-treatment estimates shown in

[11] Appendix Figures A-5 through A-8 show single difference estimates separately for White, Black, and Hispanic groups. The estimates in Figures A-1 through A-4 are the difference between the Black/Hispanic and White estimates in Figures A-5 through A-8.

[12] Specifically, we estimate $Y_{iar} = \alpha + \delta URM_{iar} + \gamma\, post_r + \beta\,(URM_{iar} \times post_r) + \tau_{a,URM} + \epsilon_{iar}$, where $post_r$ is an indicator equal to one if there is a ban in the state when the individual turned 17 years old (i.e. when r≥0) and all other variables are as previously defined.

Table 1: State-level estimates of exposure to affirmative action ban on outcomes of Blacks and Hispanics relative to non-Hispanic Whites

	Men			Women		
Outcome: college attainment	**Black**	**Hispanic**	**URM**	**Black**	**Hispanic**	**URM**
1. Texas	.0027	–.0036	–.0019	–.0083	–.0182***	–.0160***
	(.0080)	(.0049)	(.0047)	(.0078)	(.0052)	(.0049)
2. California	–.0148**	–.0195***	–.0190***	–.0019	–.0346***	–.0275***
	(.0070)	(.0040)	(.0038)	(.0075)	(.0042)	(.0040)
3. Washington	–.0186	–.0179	–.0156	.0046	.0219	.0172
	(.0290)	(.0188)	(.0141)	(.0257)	(.0183)	(.0136)
4. Florida	–.0134*	–.0216*	–.0021	–.0210**	–.0373***	–.0111
	(.0081)	(.0114)	(.0072)	(.0090)	(.0120)	(.0078)
Outcome: graduate degree						
1. Texas	–.0024	–.0072***	–.0059**	–.0027	–.0191***	–.0148***
	(.0042)	(.0027)	(.0025)	(.0045)	(.0028)	(.0028)
2. California	–.0037	–.0079***	–.0072***	–.0039	–.0197***	–.0170***
	(.0039)	(.0021)	(.0020)	(.0046)	(.0025)	(.0024)
3. Washington	–.0053	–.0085	–.0075	.0083	.0023	–.0000
	(.0160)	(.0107)	(.0077)	(.0162)	(.0103)	(.0081)
4. Florida	–.0068	–.0080	–.0023	–.0124**	–.0150**	–.0075
Outcome: ln(annual earnings)						
1. Texas	–.0016	.0240**	.0225*	–.0632***	.0227	–.0042
	(.0234)	(.0121)	(.0117)	(.0210)	(.0152)	(.0137)
2. California	–.0294	–.0069	–.0059	–.0820***	–.1260***	–.1097***
	(.0216)	(.0105)	(.0100)	(.0212)	(.0117)	(.0112)
3. Washington	.0522	–.0595	–.0068	–.1471**	–.0836	–.1053**
	(.0815)	(.0528)	(.0415)	(.0705)	(.0542)	(.0435)
4. Florida	.0303	.0083	.0554***	–.0318	–.0928***	–.0227
	(.0253)	(.0280)	(.0201)	(.0236)	(.0273)	(.0197)
Outcome: employed						
1. Texas	.0061	.0193***	.0183***	–.0099	.0047	–.0010
	(.0084)	(.0047)	(.0044)	(.0082)	(.0057)	(.0052)
2. California	.0159*	.0249***	.0258***	–.0074	–.0127***	–.0095**
	(.0082)	(.0040)	(.0038)	(.0081)	(.0045)	(.0042)
3. Washington	.0496*	.0024	.0230	–.0306	.0298	–.0016
	(.0286)	(.0192)	(.0151)	(.0258)	(.0235)	(.0157)
4. Florida	.0122	.0242**	.0334***	.0062	–.0086	.0066
	(.0099)	(.0105)	(.0077)	(.0096)	(.0114)	(.0080)

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.
Notes: Heteroskedasticity-robust standard errors are shown in parenthesis. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics with non-Hispanic Whites as the reference group and control for age fixed effects. Under-represented minorities (URM) include Blacks, Hispanics, and American Indians and estimates in URM columns report estimates for URM as one group relative to non-Hispanic Whites. Sampling weights were used in the calculations.

Figure A-2. Hispanic women in Texas, California, and Florida are 1.5–2 percentage points less likely to obtain a graduate degree after AA is banned, with a positive but not statistically significant estimate in Washington. As with collegiate attainment, there is only a negative and statistically significant effect for Black women in Florida.

Figure A-3 presents estimates from equation (1) for log earnings, with difference-in-differences results shown in the third panel of Table 1. Among men, the estimates are inconsistent and imprecisely estimated. They range from

–2.9 per cent (CA) to 5.2 per cent (WA) for Black men relative to White men and –6.0 per cent (WA) to 2.4 per cent (TX) for Hispanic men relative to White men. Only one estimate is statistically significant (Hispanic men in Texas). The descriptive average across states is positive for Black men (1.3 per cent) while it is close to zero for Hispanic men (–0.9 per cent). Overall, the evidence of an impact of AA bans on earnings of Black or Hispanic men is relatively weak. Examining the trends in Figure A-3 indicates that these estimates are not being driven by differential pre-treatment trends. We confirm this finding below using the non-treated states as a control group.

The results point to more consistent reductions in earnings among URM women. Effects on earnings of Blacks relative to non-Hispanic Whites range from –3.2 per cent (FL) to –14.7 per cent (WA), with a descriptive average across states of –8.1 per cent. All but the Florida estimate is statistically significant at the 5 per cent level. Among Hispanic women, there is a relative decline in earnings ranging from –8.4 per cent (WA) to –12.6 per cent (CA) in three states, while the estimate in Texas is positive but not statistically significant. The simple average across states is –7.0 per cent. The adverse effect on outcomes among Hispanic women in California aligns with the results in Bleemer (2022). As shown in Figure A-3, these declines in post-ban relative earnings of minorities are not simply a reflection of pre-treatment trends. Earnings of Black, Hispanic, and White workers trend similarly across cohorts prior to the bans and then shift downward for URM women in all states but Texas.

Finally, Figure A-4 and the fourth panel of Table 1 show estimates of the impacts on employment. Among men, there is a general increase in employment of both URM groups following AA bans. The estimates all are positive and range from close to zero to as high as 5.0 percentage points. Most of the estimates are between 1 and 2.5 percentage points, and the descriptive average across states is 2.1 percentage points for Black men and 1.8 percentage points for Hispanic men. The event study results in Figure A-4 show little evidence of pre-treatment trends, which suggests that there are modest positive impacts of affirmative action bans on URM employment for men.

Among women, there is evidence of declines in employment, particularly among Black women. Outside of Florida, all of the estimates are negative in Table 1, however none is statistically significant. The average across states is –1 percentage point. For Hispanic women, there are declines in employment in California and Florida as well as positive estimates in Texas and Florida, with a cross-state descriptive average effect of 0.3 percentage points. The trends in Figure A-4 show little systematic evidence of changes in employment, which indicates there are at most modest impacts of AA bans on future employment using this method. Most of the labour market effects for women come through changes in earnings rather than the extensive margin of being employed.

There are several takeaways from the results in Table 1 and Figures A-1 through A-4. First, there is significant variation in effects across states. We are the first to show such significant variation in AA ban impacts because prior work examining these outcomes largely has focused only on one state. Understanding why this heterogeneity exists is beyond the scope of our analysis, but is an important direction for future work.

Second, there are significant gender differences in the effects of affirmative action bans. On average, URM women experience worse educational attainment and labour market outcomes due to affirmative action bans. This is particularly the case for log earnings, where we find sizeable negative impacts outside of Hispanic women in Texas. This gender heterogeneity could be caused by a number of factors. Women are an increasingly large fraction of higher education students (Goldin *et al*., 2006), and thus URM women may be larger beneficiaries of affirmative action. Women and men also major in different subjects (Turner and Bowen, 1999), which could lead to differential effects of affirmative action bans if there is an interaction between college selectivity and major. Finally, there are large gender differences in occupation selection (Blau and Kahn, 2017), which could drive gender-based differences in reactions to AA bans through changes to major choice and institutional selectivity. We highlight these potential mechanisms as important areas for future research.

The estimates thus far examine differences between URM and non-Hispanic White individuals in each AA ban state. This approach has two drawbacks. First, while equation (1) shows how much racial/ethnic differences in outcomes shift due to AA bans, both URM students and non-Hispanic White students are treated. The net effect is interesting, but we also care about the effect on each group. Second, equation (1) is identified off of the assumption that cross-cohort outcomes of URMs would trend similarly to such trends for non-Hispanic Whites in the absence of affirmative action bans. This is a strong assumption because of differences in educational sorting, occupational selection, and labour market outcomes by race/ethnicity. While there is little evidence of systematic differential pre-treatment trends in Figures A-1 through A-4, we can assess the robustness of our results to using an alternative control group. We now turn to another method of estimating the effect of AA bans that leverages variation from same-race groups in control states that do not pass AA bans during our sample period.

(ii) Race-specific difference-in-differences estimates using non-ban states as a control group

We estimate difference-in-differences models by racial/ethnic group (White, Black, Hispanic) using the variation in the timing of affirmative action bans in each state relative to those in the same cohorts in states that do not pass a ban.[13] Recent research on difference-in-differences models with staggered adoption timing shows that traditional OLS models will produce biased results if there are time-varying treatment effects (e.g. Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). We adopt the strategy from Callaway and Sant'Anna (2021) to address this problem, which effectively amounts to estimating difference-in-differences models for each treatment state relative to control states and then aggregating the state-specific estimates to an average treatment effect on the treated (ATT). To avoid over-weighting more populous states in the analysis, we implement this model after creating state-race-gender specific panels of birth cohorts by collapsing the data to the birth state, year age 17, race, and gender level using sample weights in the ACS. The models are estimated separately by race and gender using the same sample restrictions as in equation (1). The estimates thus identify how outcomes change across cohorts within each sex and

Table 2: Callaway and Sant'Anna ATT estimates of exposure to affirmative action ban on outcomes of non-Hispanic White, Black, and Hispanic men and women

	Men			Women		
Outcome: college attainment	**White**	**Black**	**Hispanic**	**White**	**Black**	**Hispanic**
Overall ATT	.0031	.0018	.0063	.0056	.0034	–.0395***
	(.0070)	(.0101)	(.0160)	(.0037)	(.0119)	(.0147)
		URM			URM	
		.0001			–.0079	
		(.0119)			(.0081)	
Outcome: graduate degree						
Overall ATT	.0046	.0051	.0013	.0027	–.0097	–.0165
	(.0037)	(.0046)	(.0093)	(.0034)	(.0138)	(.0144)
		URM			URM	
		.0009			–.0130	
		(.0047)			(.0120)	
Outcome: ln(annual earnings)						
Overall ATT	.0037	.0264	–.0065	.0332**	–.0424	–.0814**
	(.0180)	(.0270)	(.0355)	(.0147)	(.0596)	(.0347)
		URM			URM	
		.0124			–.0478*	
		(.0317)			(.0285)	
Outcome: employed						
Overall ATT	.0043	.0195	.0088	.0041	–.0035	–.0359**
	(.0046)	(.0190)	(.0080)	(.0053)	(.0243)	(.0175)
		URM			URM	
		.0118*			–.0006	
		(.0069)			(.0096)	

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.
Notes: Standard errors clustered at the birth state shown in parenthesis. Data collapsed to the birth state, year age 17, race, and gender level using sample weights. Treated states include TX, CA, WA, and FL. Control states exclude NE, MI, AZ, NH, and OK. Callaway and Sant'Anna (2021) regressions are estimated separately for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics, controlling for the average age of the state, year 17, race, and gender group. Under-represented minorities (URM) include Blacks, Hispanics, and American Indians and estimates in URM columns report results for URM as one group.

[13] The control states include all non-treated states outside of NE, MI, AZ, NH, and OK, since those states eventually pass an affirmative action ban. We use all of these non-treated states as controls to maximize statistical power; our event study estimates (Figures 1–4) indicate no pre-treatment trends, which suggests it is appropriate to include all of these non-treated states as a control group.

racial/ethnic group in states that ban affirmative action relative to states that do not. Standard errors are clustered at the birth state level.

For simplicity, we only show estimates that are aggregated across states. State-level estimates are available from the authors upon request. Table 2 presents the results, while Figures 1–4 show event study estimates for each of our four outcome variables of interest. Among men, there is little impact on collegiate attainment for any racial/ethnic group, and the differences between the White and Black/Hispanic point estimates is small. The 1–1.5 percentage point decline in collegiate attainment of Black and Hispanic men relative to White respondents shown in Table 1 thus is not evident here once we include control group states. Among women, there is no change in collegiate attainment among Blacks and Whites, but there is a statistically significant decline in college completion for Hispanics. The magnitude of this effect is larger than the descriptive average across states shown in Table 1, suggesting that the within-state estimates understate the decline in collegiate attainment for this group.[14]

Results for graduate degree attainment in the second panel align with the college completion results: small and insignificant estimates for men and a negative effect for Hispanic women (1.65 percentage points). However, this estimate is not statistically significant at even the 10 per cent level. Hence, for Hispanic women, there is a decline in collegiate and possibly graduate degree attainment, while there is no effect on degree attainment among other groups. Interestingly, our results do not point to higher degree attainment among Whites, even though they are the beneficiaries of these bans in terms of obtaining access to higher-quality post-secondary institutions on average

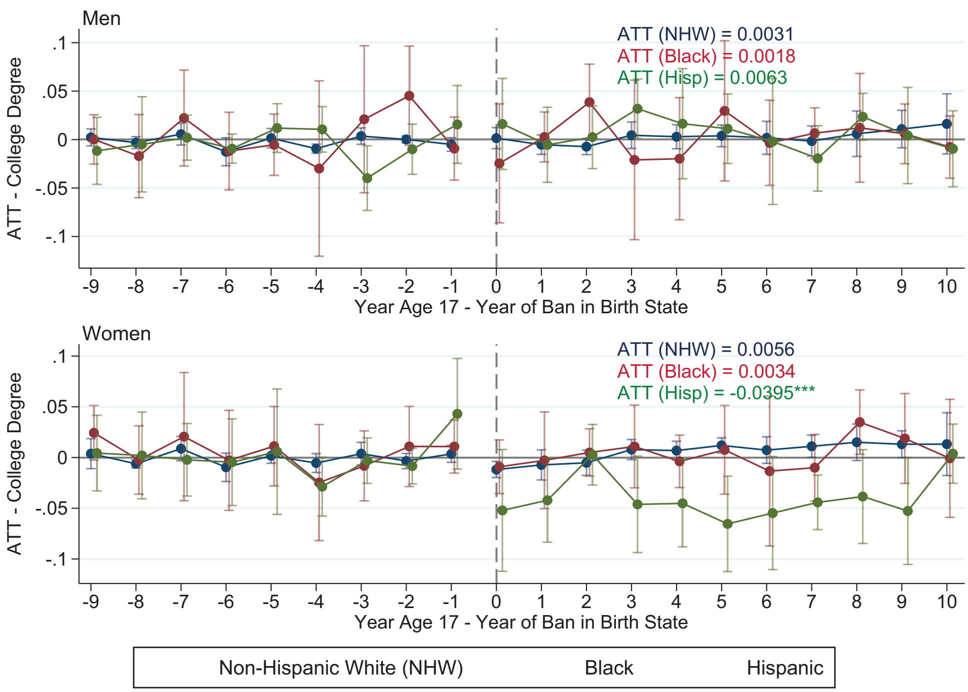

Figure 1: Callaway and Sant'Anna estimates of the effect of exposure to affirmative action bans on college degree attainment of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Data collapsed to the birth state, year age 17, race, and gender level using sample weights. Treated states include TX, CA, WA, and FL. Control states exclude NE, MI, AZ, NH, and OK because they enact bans later in the sample period. Callaway and Sant'Anna (2021) regressions are estimated separately for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics, controlling for the average age of the state, year 17, race, and gender group.

[14] This difference is driven by increases in collegiate attainment among Hispanic women in control states over this period. Hence, relative to the increase in control states, college completion for Hispanic women declines more substantially post-AA ban.

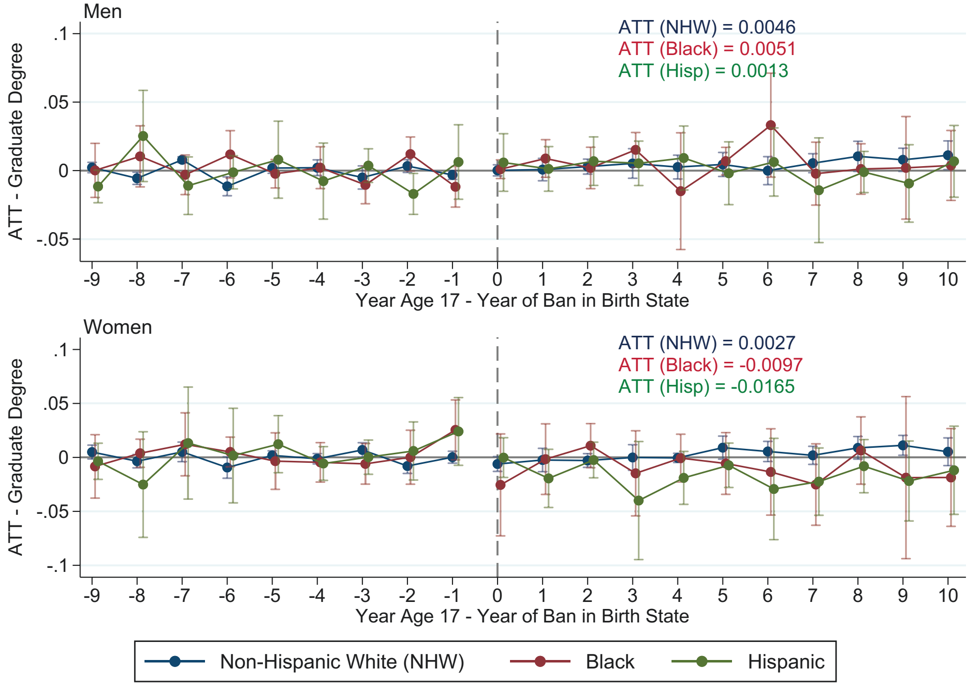

Figure 2: Callaway and Sant'Anna estimates of the effect of exposure to affirmative action bans on graduate degree attainment of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Data collapsed to the birth state, year age 17, race, and gender level using sample weights. Treated states include TX, CA, WA, and FL. Control states exclude NE, MI, AZ, NH, and OK because they enact bans later in the sample period. Callaway and Sant'Anna (2021) regressions are estimated separately for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics, controlling for the average age of the state, year 17, race, and gender group.

(Bleemer, 2022; Hinrichs, 2012). Figures 1 and 2 present corresponding event studies for these estimates. The pre-treatment cross-cohort trends are flat, and the decline in outcomes among Hispanic women is clear. This decline also appears to reflect a level shift: the effect is stable over time across cohorts.

The third panel of Table 2 shows Callaway and Sant'Anna (2021)'s difference-in-differences estimates of log earnings. Effects for White and Hispanic men are small, while there is a sizeable positive coefficient for Black men of 2.6 per cent that is not statistically significant at conventional levels. While this is suggestive of a modest positive impact of AA bans on earnings of Black men, the estimate is imprecise: the 95 per cent confidence interval includes effects between 7.9 per cent and –2.7 per cent. The event study estimates in Figure 3 also are suggestive of a modest positive effect. There is no evidence of differential pre-treatment trends, and all but two of the post-treatment estimates are positive. A positive impact of AA bans on the earnings of Black men is consistent with the mismatch hypothesis, however we caution against drawing too strong a conclusion from this evidence because of the wide confidence interval.

The earnings estimates are clearer among women: White women experience a 3.3 per cent increase in earnings, earnings of Black women decline by 4.2 per cent, and earnings of Hispanic women are reduced by 8.1 per cent. The estimate for Black women is not statistically significant, while the effects for the other two groups are statistically significant at the 5 per cent level. Relative to White women, earnings of Black women decline by 7.6 per cent and earnings of Hispanic women decline by 11.5 per cent. These estimates are similar to the average across states in Table 1, if somewhat larger for Hispanic women.[15] Across methods, we find a robust pattern of declining earnings for URM women. Event study estimates in Figure 3 show that these results are not a reflection of differential

[15] Our results for Hispanic women align with those in Bleemer (2022), but he does not find evidence of reduced earnings among Blacks relative to Whites. He also does not examine effects separately by gender.

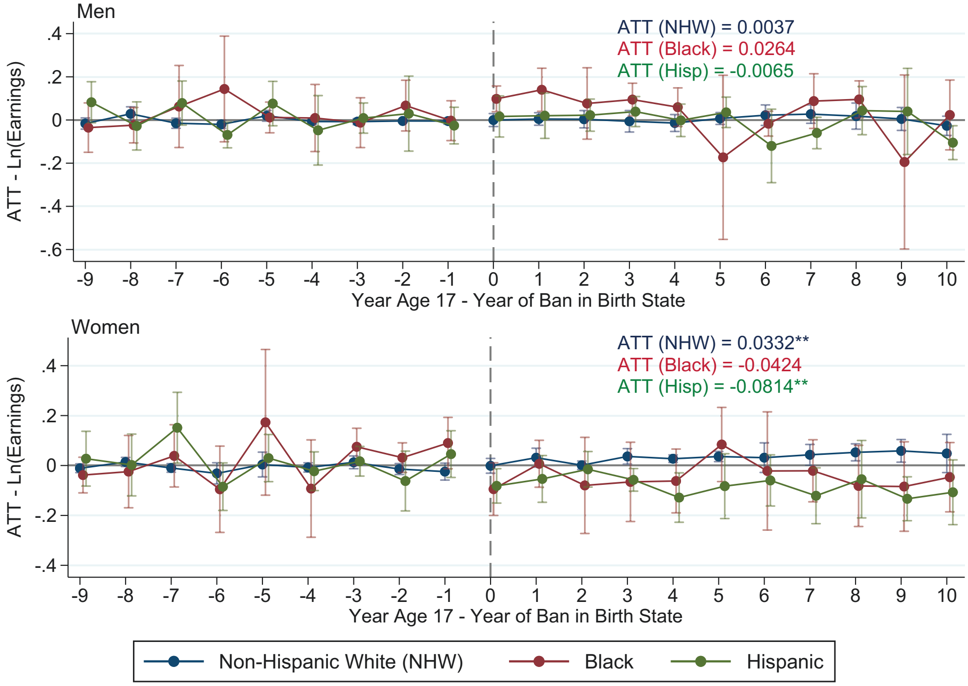

Figure 3: Callaway and Sant'Anna estimates of the effect of exposure to affirmative action bans on the log earnings of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Data collapsed to the birth state, year age 17, race, and gender level using sample weights. Treated states include TX, CA, WA, and FL. Control states exclude NE, MI, AZ, NH, and OK because they enact bans later in the sample period. Callaway and Sant'Anna (2021) regressions are estimated separately for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics, controlling for the average age of the state, year 17, race, and gender group.

pre-treatment trends, and the post-ban effects are relatively stable. That Table 2 shows an increase in earnings among White women and a decline in URM earnings is consistent with the adverse effects of college quality changes outweighing any mismatch effects.

These estimates are quite large, especially given the small percentage of URM women who attend selective universities in these states. Prior research provides estimates of the effect of going to the flagship university on earnings of 20–25 per cent (Brewer *et al*., 1999; Hoekstra, 2009; Andrews *et al*., 2016). While it is not possible with our data to estimate the effect of the ban on the number of URM students who are displaced to less-selective institutions, the magnitude of our earnings effects suggests it is unlikely that these displaced students alone are driving our results. The large earnings effects could be driven by economy-wide changes in the labour market, however they would have to differentially impact cohorts that were entering college when the ban took place or after as we see little evidence that these bans affected older cohorts (i.e. pre-ban relative trends). Our results are consistent with a broader set of students being affected by these bans, perhaps through changes in college-going expectations or labour market participation. These broader effects are an important area for future study, as they are beyond the scope of our analysis.

Finally, we examine employment effects of AA bans. Aligned with the results in Table 1, there are small positive employment estimates among Black and Hispanic men relative to White men. However, none of the estimates is statistically significant, and the relative effects are smaller than in Table 1. Combined with the positive but not significant earnings estimate, our results provide suggestive evidence of a modest improvement in labour market outcomes for African American men. These estimates align with the theoretical predictions of the mismatch hypothesis, though we emphasize that the estimates are imprecise and thus it is difficult to draw too strong a conclusion from these results.

For Black women, there is a modest negative relative effect that matches the average across states in Table 1. The effect for Hispanic women is –3.6 per cent, which is statistically significant at the 5 per cent level and is larger in

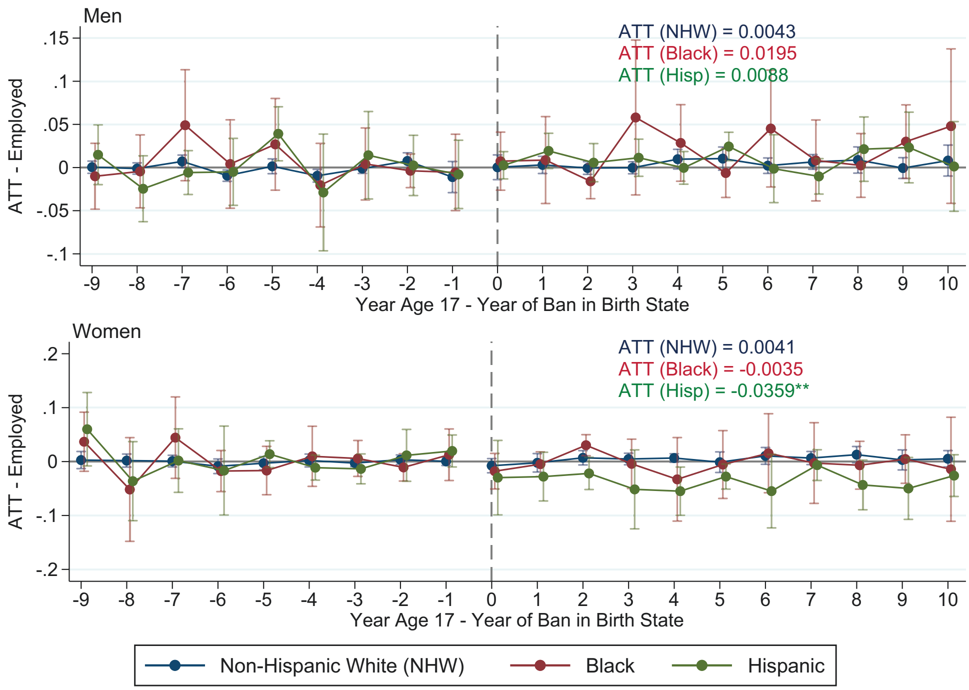

Figure 4: Callaway and Sant'Anna estimates of the effect of exposure to affirmative action bans on the employment of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Data collapsed to the birth state, year age 17, race, and gender level using sample weights. Treated states include TX, CA, WA, and FL. Control states exclude NE, MI, AZ, NH, and OK because they enact bans later in the sample period. Callaway and Sant'Anna (2021) regressions are estimated separately for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics, controlling for the average age of the state, year 17, race, and gender group.

magnitude than the estimates in Table 1. The difference-in-differences estimates that include control states clearly show a reduction in employment for Hispanic women, which along with the adverse impact on earnings indicates that this group fares much worse in the labour market due to affirmative action bans. Figure 4 again shows no evidence of differential pre-ban trends while also showing a stable negative effect on Hispanic female employment for the cohorts who were impacted by the ban.

A key question that arises from these results is why Hispanic (and to some extent Black) women are adversely affected by AA bans while we find some evidence of positive effects among Black men. One potential explanation is that Black men receive more admission help than Black or Hispanic women, potentially stemming from gender differences in academic achievement at the time of college application. This would lead to larger mismatch effects among Black men, while for URM women less intensive admission assistance would induce less mismatch, so that the college quality effect dominates. Another possibility is that differences in college major choice and college dropout rates for affected URM men and women are such that mismatch effects dominate college quality effects for URM men while the pattern is reversed for women. Further exploration of these possibilities is an important topic for future research.

V. Conclusion and policy implications

We present new evidence on the effect of affirmative action bans on the long-run outcomes of those exposed to these bans in four states: Florida, Texas, Washington, and California. Using multiple difference-in-differences designs, we highlight a number of findings. First, Hispanic women experience the largest adverse effects of AA bans, with evidence of substantial reductions in post-secondary attainment, earnings, and employment. There is suggestive evidence of reduced earnings among Black women as well, and earnings among White women increase; neither group experiences substantial changes in degree attainment, however. Effects among men are more modest.

Educational attainment and labour market outcomes change little for Hispanic and White men, while we find suggestive evidence of improved earnings and employment for Black men.

This study is important, especially in light of the recent US Supreme Court decision in *Students for Fair Admissions v. Harvard*, which significantly curtailed if not outright eliminated racial preferences in higher education admissions. Our results suggest that this ruling could lead to worse educational attainment outcomes for Hispanic women and to worse labour market outcomes for both Hispanic and Black women. White women may experience an increase in labour market earnings, thus leading to an expanded racial/ethnic disparity in earnings among women. Our results indicate that men are unlikely to be strongly affected, with some suggestive evidence of improved labour market outcomes for Black men.

There are a number of caveats in using our results to predict the effects of the *Students for Fair Admissions* ruling. First, a national ban could have different impacts from a state-specific ban, since students can avoid a state ban by attending college in another state. Second, the Supreme Court ruling left open the possibility of using race in a personalized context, and thus it is unclear as of this writing how admissions policies will change. Third, we find evidence of substantial heterogeneity in effects across states. Understanding the sources of this heterogeneity is important for assessing the situations in which changes to affirmative action will help or harm students from different racial/ethnic groups. Such an analysis is beyond the scope of our study, but we view it as an important direction for future research.

Appendix

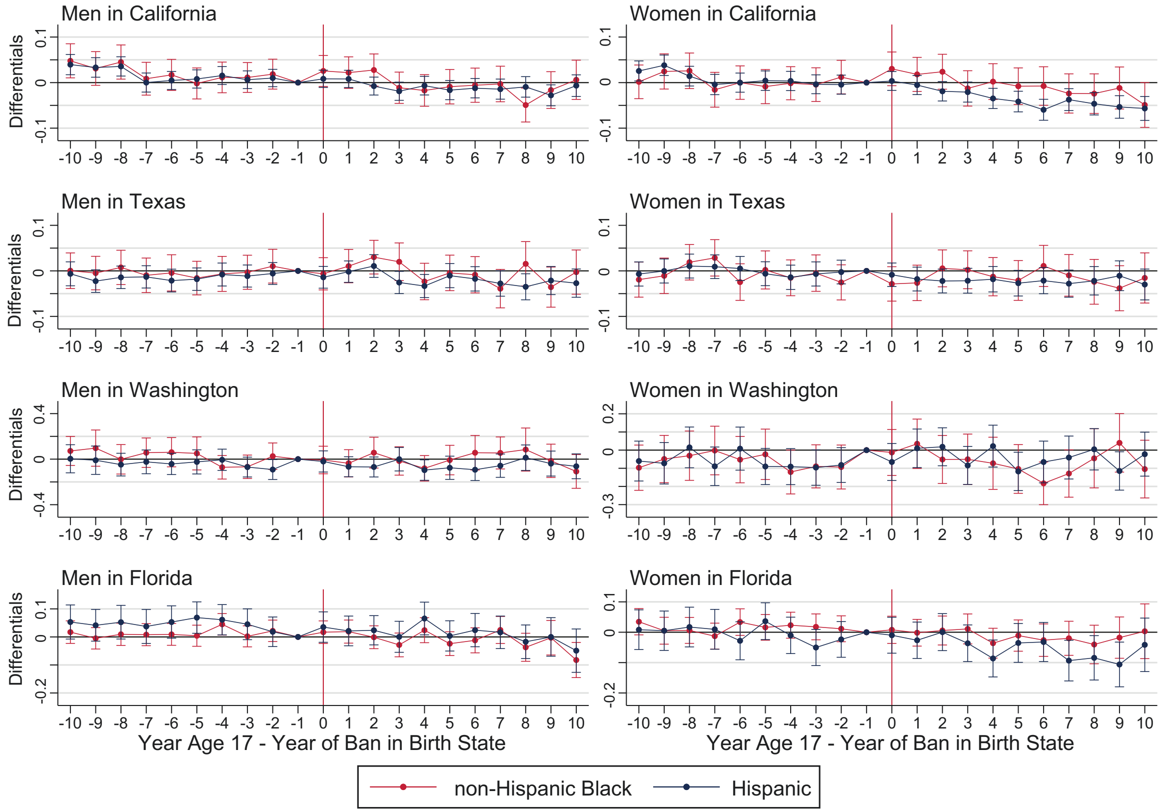

Figure A-1: Effect of exposure to affirmative action bans on college degree attainment of Blacks and Hispanics relative to non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

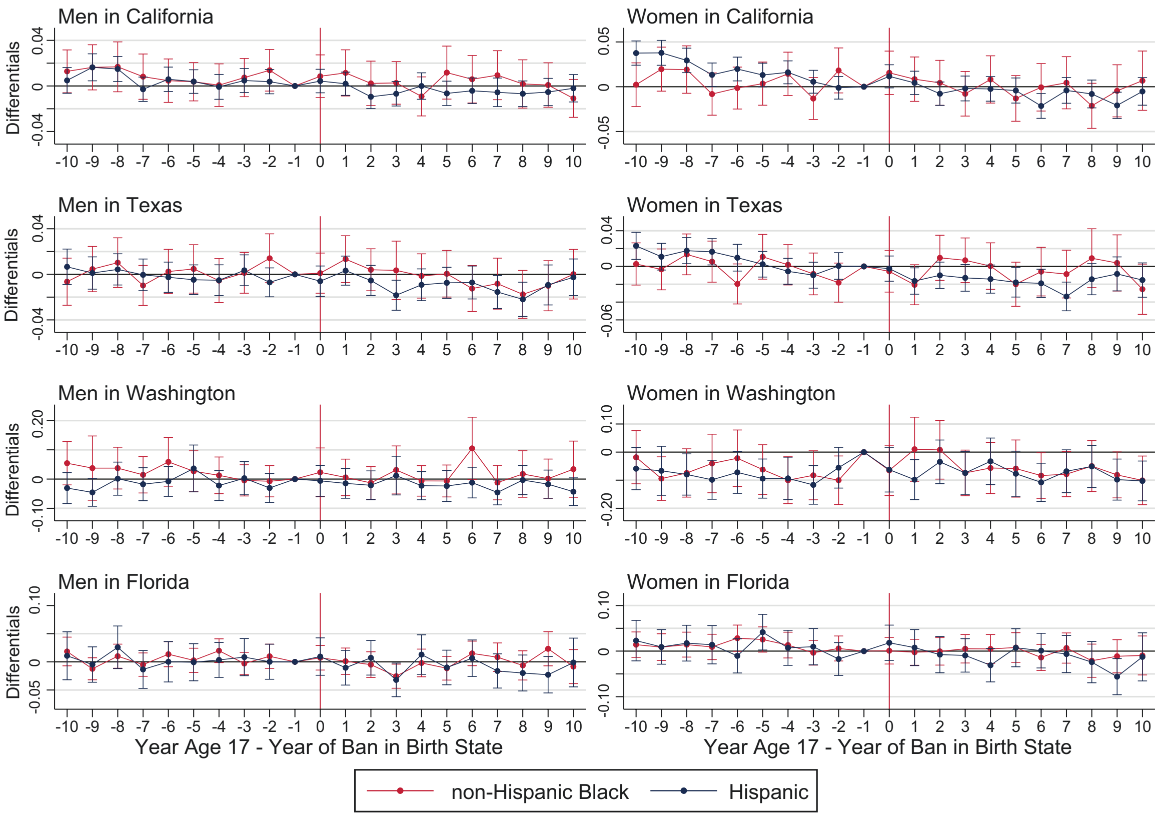

Figure A-2: Effect of exposure to affirmative action bans on graduate degree attainment of Blacks and Hispanics relative to non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

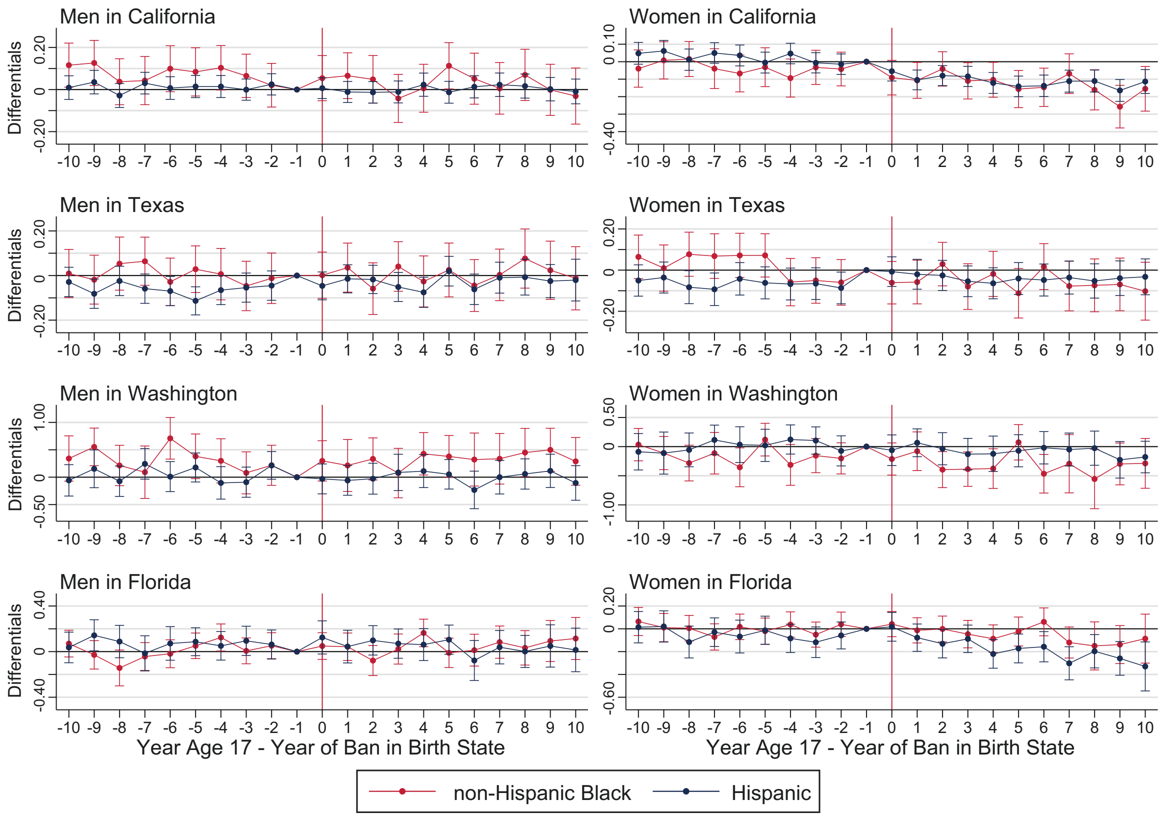

Figure A-3: Effect of exposure to affirmative action bans on the log earnings of Blacks and Hispanics relative to non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

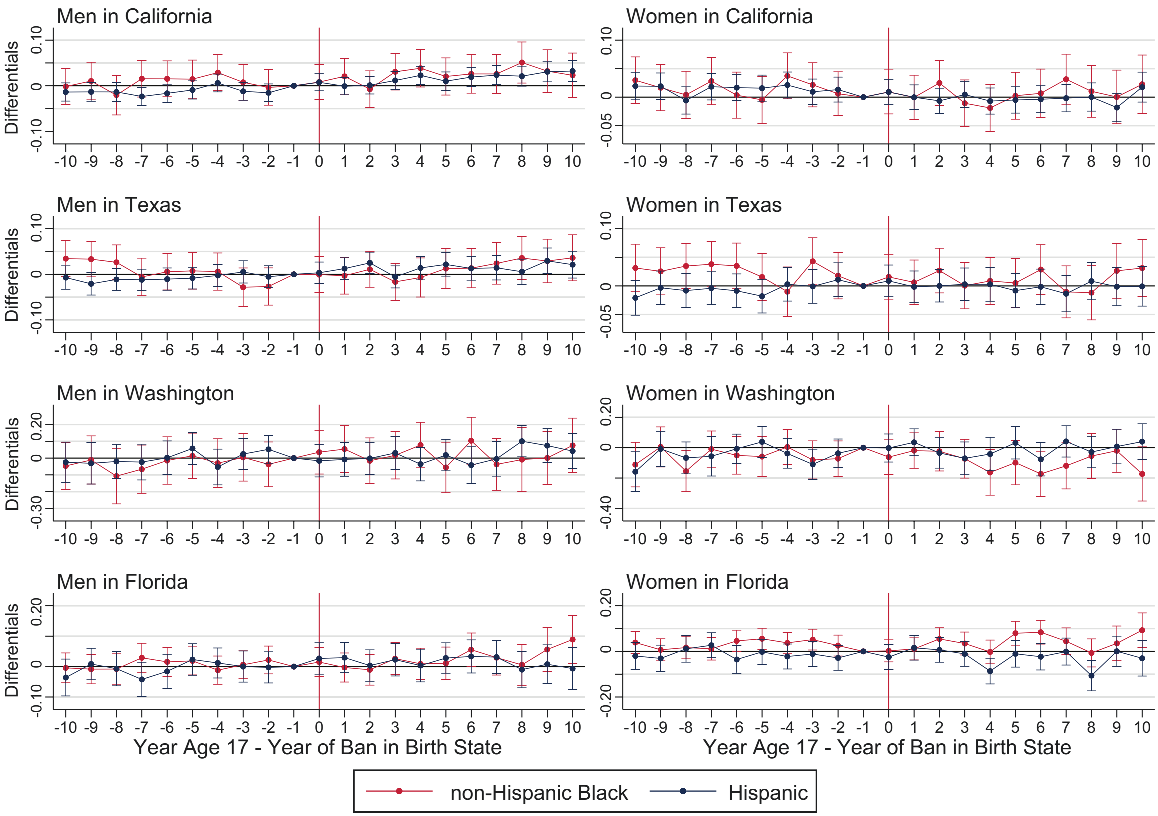

Figure A-4: Effect of exposure to affirmative action bans on the employment of Blacks and Hispanics relative to non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

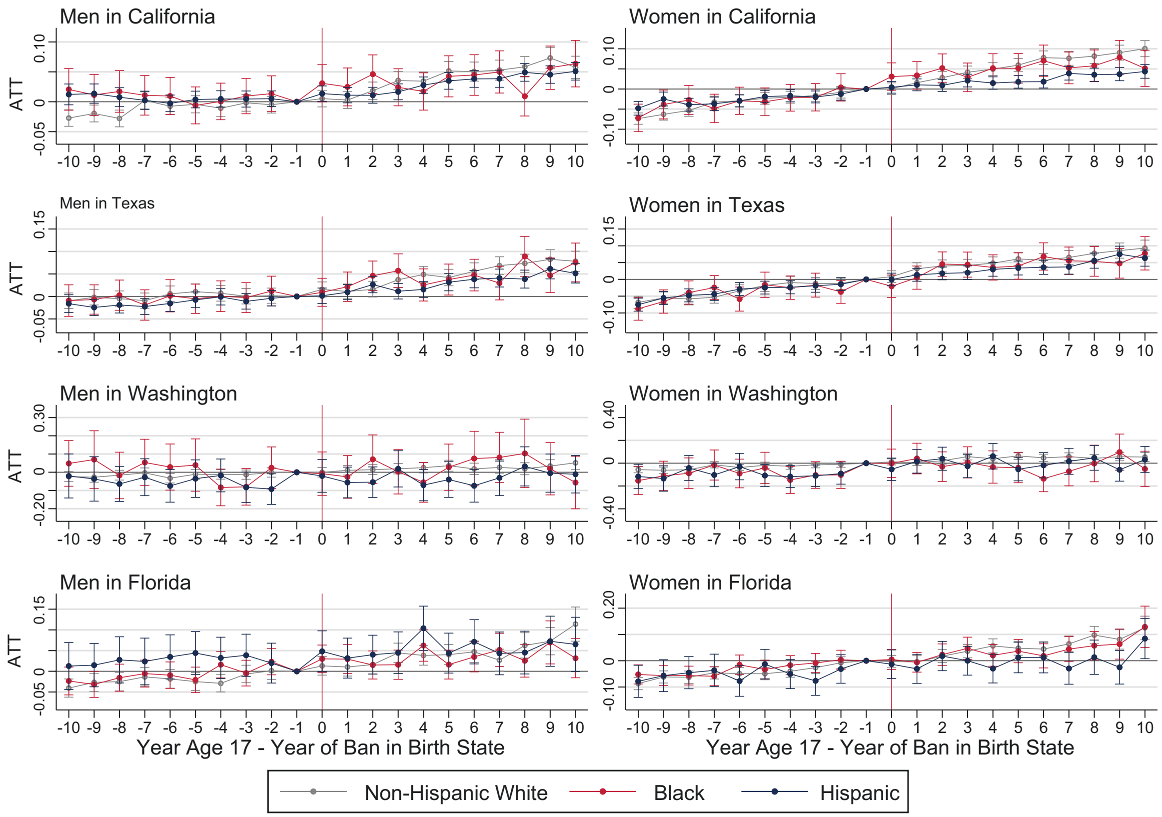

Figure A-5: Effect of exposure to affirmative action bans on the college degree attainment of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

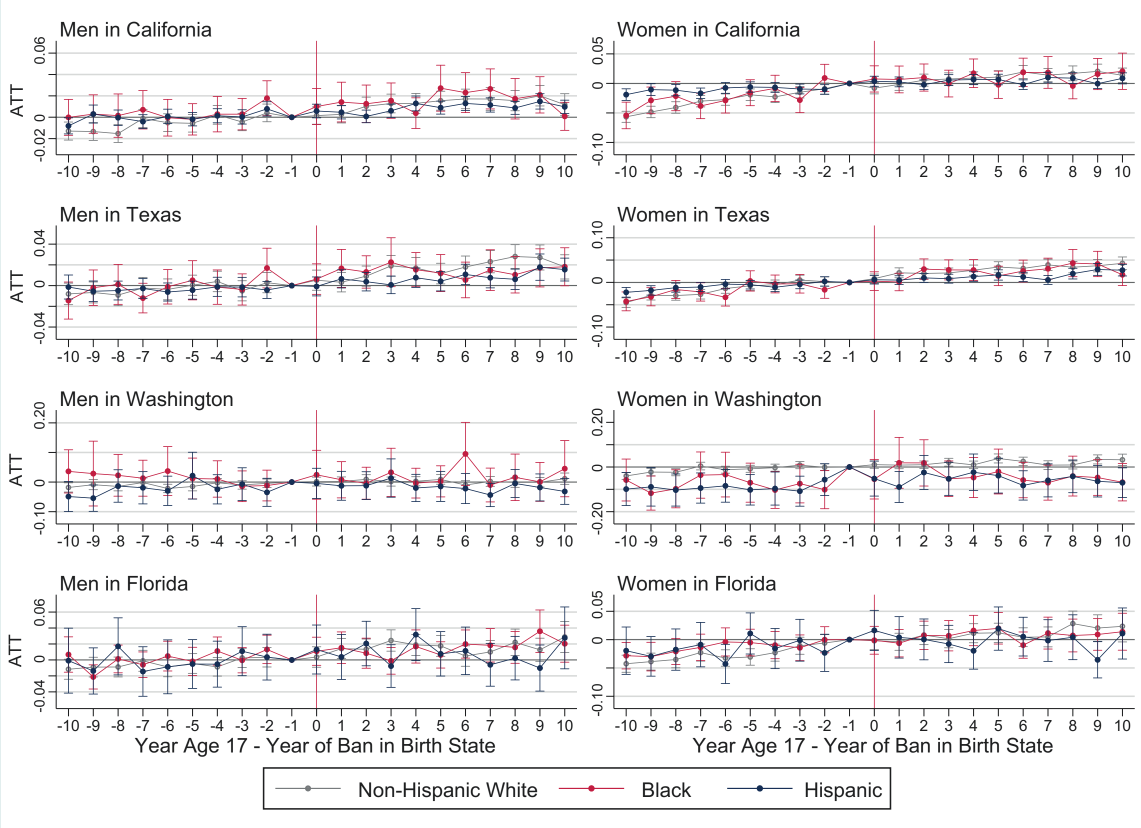

Figure A-6: Effect of exposure to affirmative action bans on the graduate degree attainment of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

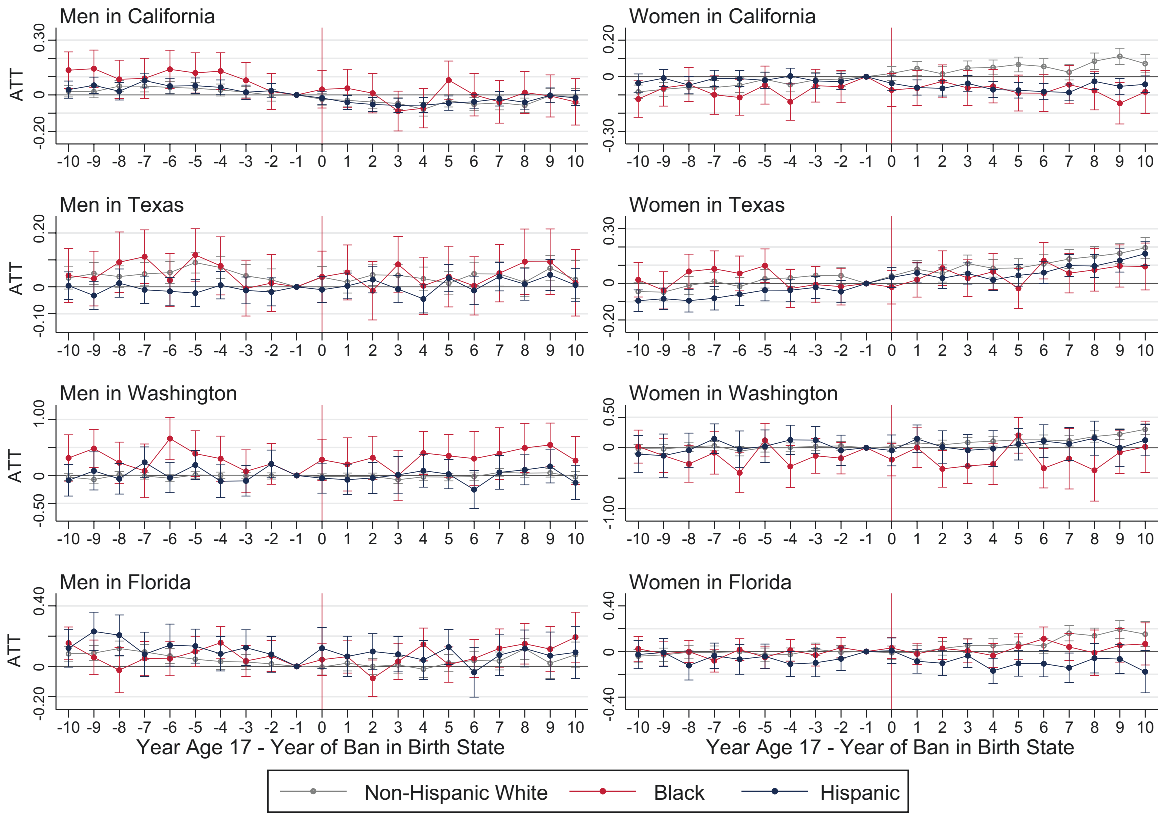

Figure A-7: Effect of exposure to affirmative action bans on the log earnings of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

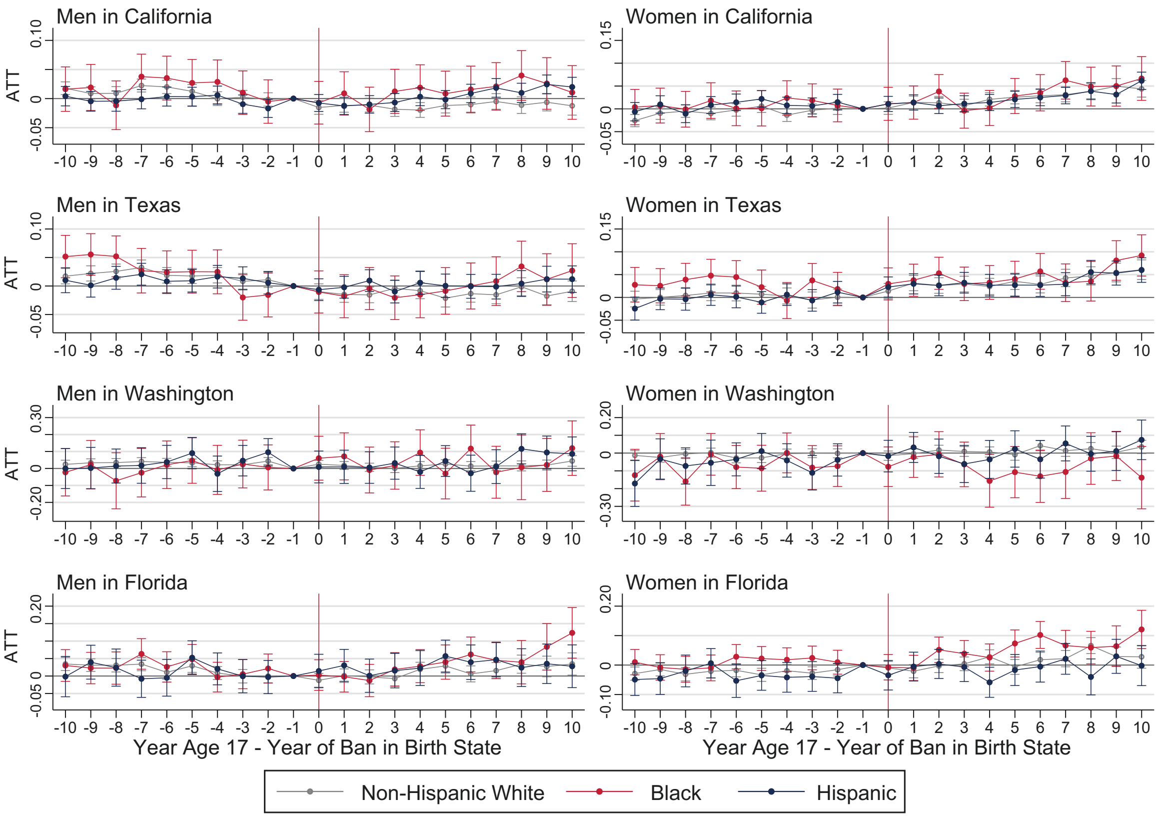

Figure A-8: Effect of exposure to affirmative action bans on the employment of Blacks, Hispanics, and non-Hispanic Whites

Sample: Census 2000 and ACS 2001–2021. Ages 25–51 in survey year and aged 17 within 20 years of State affirmative action ban.

Notes: Heteroskedasticity-robust 95 per cent confidence intervals shown around the point estimates. Regressions are estimated separately by state and for men and women and for non-Hispanic Whites, non-Hispanic Blacks, and Hispanics and control for age fixed effects. Sampling weights were used in the calculations.

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