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The Impact of the 2023 Students for Fair Admissions v Harvard Decision on Undergraduate Demographics

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The 2023 Supreme Court decision Students for Fair Admissions v. President and Fellows of Harvard College (SFFA) effectively ended the explicit consideration of race in college admissions. This paper examines the impact of SFFA on the racial, ethnic, and socioeconomic composition of undergraduate populations across institutional sectors. Using 2018–2024 data from the Integrated Postsecondary Education Data System (IPEDS), I analyze changes in the shares of White, Asian, Black, and Hispanic students, as well as the proportion receiving Pell Grants, state and local aid, and institutional grant aid. I employ a difference-in-differences framework that leverages variation in institutional selectivity and pre-existing state affirmative action bans, incorporating group-specific linear time trends to address differential pre-treatment trends. Results indicate that, among more selective institutions, Black student shares decline while White shares increase, with the largest effects in highly selective private institutions and more broadly across public institutions. Hispanic shares also decline and Asian shares rise, although less consistently. Pell and state aid receipt decrease, suggesting that reduced racial/ethnic diversity leads to less socioeconomic diversity as well.

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The Impact of the 2023 *Students for Fair Admissions v Harvard* Decision on Undergraduate Demographics

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Abstract

The 2023 Supreme Court decision *Students for Fair Admissions v. President and Fellows of Harvard College* (*SFFA*) effectively ended the explicit consideration of race in college admissions. This paper examines the impact of *SFFA* on the racial, ethnic, and socioeconomic composition of undergraduate populations across institutional sectors. Using 2018–2024 data from the Integrated Postsecondary Education Data System (IPEDS), I analyze changes in the shares of White, Asian, Black, and Hispanic students, as well as the proportion receiving Pell Grants, state and local aid, and institutional grant aid. I employ a difference-in-differences framework that leverages variation in institutional selectivity and pre-existing state affirmative action bans, incorporating group-specific linear time trends to address differential pre-treatment trends. Results indicate that, among more selective institutions, Black student shares decline while White shares increase, with the largest effects in highly selective private institutions and more broadly across public institutions. Hispanic shares also decline and Asian shares rise, although less consistently. Pell and state aid receipt decrease, suggesting that reduced racial/ethnic diversity leads to less socioeconomic diversity as well.

1 Introduction

Affirmative action in higher education refers to the consideration of race in admissions decisions to promote diversity and expand educational opportunity. Since its emergence in the 1960s, affirmative action has been shaped by a series of judicial decisions and state-level policy changes that have alternately expanded and restricted its use. Most recently, in 2023, the Supreme Court decided *Students for Fair Admissions v. President and Fellows of Harvard College (SFFA)*, holding that Harvard's race-conscious admissions policies violated the Equal Protection Clause of the Fourteenth Amendment by discriminating against Asian and White students *(Students for Fair Admissions v. President and Fellows of Harvard College* 2023). In doing so, the Court significantly decreased the use of race in college admissions, marking a substantial shift in the legal framework governing affirmative action.

The Court emphasized that racial classifications are subject to strict scrutiny, requiring that they serve a compelling governmental interest and be narrowly tailored to achieve that interest. Under the ruling, institutions may no longer consider race as an explicit factor in admissions decisions. However, the majority opinion clarified that applicants may discuss how race has shaped their experiences, provided such discussion is tied to individual qualities or contributions relevant to the university community. As Chief Justice Roberts wrote, race may be considered only if an applicant explains "how race may have affected his or her life, be it through discrimination, inspiration, or otherwise" *(Students for Fair Admissions v. President and Fellows of Harvard College* 2023).

The *SFFA* ruling therefore leaves open the possibility that colleges and universities may continue to consider race indirectly in admissions, particularly given the limited transparency of admissions processes at the most selective institutions. The lack of transparency in admission decisions also raises the possibility of imperfect compliance with the ruling. Empirical work examining how enrollment patterns changed post-*SFFA* is the most straightforward way to determine how the decision affected universities' abilities to shape the composition of their incoming classes. This study is among the first to provide such an empirical analysis.

My results also are important because they speak to how shifts in campus demographics may influence long-run underrepresented minority (URM) student outcomes. One prominent theory about how URM students may be affected by affirmative action is the mismatch hypothesis, which argues that admissions preferences for URM students can potentially lead to worse college outcomes by placing them in institutions where their prior academic preparation is lower than that of their peers (Sander 2004; Arcidiacono et al. 2014; Arcidiacono and Lovenheim 2016). In contrast, other research emphasizes the potential benefits of attending more selective colleges (e.g., Brewer et al. 1999; Hoekstra 2009; Bound et al. 2010; Andrews et al. 2016; Zimmerman 2019).[1] Some of the returns to college quality are driven by positive peer effects from interacting with higher-achieving classmates (Sacerdote 2001; Zimmerman 2003; Stinebrickner & Stinebrickner 2006). From this perspective, it is likely to be optimal for students to attend a more selective institution, suggesting that race-conscious admissions may enhance minority student outcomes. However, the returns to affirmative ac-

[1] Unique among these papers is Dale and Krueger (2002; 2014), who do not find much evidence of a return to college quality outside of those from disadvantaged backgrounds. See Lovenheim and Smith (2023) for a recent review of the literature on the returns to college quality.

tion for URM students are not necessarily uniform; they could depend on student-institution fit. Selective colleges often move at a faster pace and cover more advanced material, so the benefits of attending such institutions may vary based on a student's academic preparation relative to other students. As a result, race-conscious admissions either can improve or diminish outcomes depending on the alignment between student readiness and institutional rigor. In theory, with complete information about preparation and expected performance, racial preferences should not disadvantage URM students. In practice, however, information gaps may lead some minority students to experience unintended disadvantages (Arcidiacono et al. 2011a). While it is too soon to measure the effect of *SFFA* on these outcomes, this analysis is a first step in understanding the scope for such longer-run effects.

The effect of the *SFFA* ruling on URM student outcomes will be proportional to the change in the demographic composition of enrollment across colleges with different resources and selectivity. This paper examines the impact of the *SFFA* decision on racial and socioeconomic diversity across four-year undergraduate institutions and considers its broader implications for students and equity in higher education. My results speak to whether universities can continue to achieve diversity goals without explicitly considering race, and thus how the national ban on affirmative action in admissions affects the demographic composition of colleges and universities.

I use data from the Integrated Postsecondary Education Data System (IPEDS) to examine both racial enrollment and the distribution of grant aid patterns in U.S. higher education from 2018 to 2024. I focus on four racial/ethnic groups–White, Asian, Black, and Hispanic–as these groups have been most directly affected by race-based admissions policies. I limit the analysis to first-year enrollment data, as these students are the ones directly impacted by new admissions standards. To examine the effect of the ruling on socioeconomic diversity, I measure the share of students receiving financial aid grants: Pell, state/local, and institutional. I use Barron's 2009 Profiles of American Colleges to categorize institutions by selectivity, which places colleges into Tiers 1 through 4 as well as a large set of unranked, non-selective institutions. Using a difference-in-differences (DiD) framework, I analyze changes in racial and socioeconomic composition before and after the 2023 Supreme Court decision banning the explicit use of race in admissions. The policy being applied universally makes identifying causal effects challenging. To address this issue, I exploit two sources of variation in exposure to the ban: the selectivity of the institution and whether a public college was located in a state with a preexisting state affirmative action ban.

I first examine whether institutions with different levels of admissions selectivity exhibit differential changes in racial/ethnic enrollment, as more selective universities are generally better positioned to implement race-conscious admissions policies (Arcidiacono et al. 2011).

In this sense, the scope of affirmative action is closely related to a university's selectivity. When examining effects of *SFFA* by institutional selectivity, I estimate separate effects across public versus private institutions. One might be concerned that even low-selectivity universities are affected by the ruling because of a "cascade" effect whereby URM students who would have attended more selective institutions attend less-selective schools post-*SFFA*. This approach therefore identifies the net difference across institutions that may be differentially affected by the ruling. I then turn to a strategy that is less affected by this concern, comparing changes among public universities in states with preexisting affirmative action bans to those in states without such bans.

In addition to changes in racial/ethnic composition, I examine grant aid patterns to assess whether the decision changed income stratification across different types of institutions. It is unclear how changes to affirmative action will affect the socioeconomic (SES) distribution of students across colleges. Although 49.4 percent of Pell Grant recipients are Black or Latino (Levine and Reber 2023), the extent to which a ban alters socioeconomic diversity ultimately depends on how institutions adjust their admissions criteria. Some universities may substitute toward income-based measures as a proxy for race, as the University of California system did following California's affirmative action ban (Antonovics and Backes 2014a, 2014b). Given the correlation between race and socioeconomic status, eliminating racial preferences also may reduce socioeconomic diversity. There is little existing research directly examining the relationship between affirmative action bans and socioeconomic diversity in higher education.

My event-study analyses reveal the presence of approximately linear differential pre-treatment trends by university selectivity, which I address by including treatment group-specific linear time trends in all regression specifications. These pre-treatment trends are important in their own right, because they show that universities of differing selectivities exhibited different demographic trends prior to 2024. Hence, simple comparisons of demographic changes surrounding *SFFA* implementation will not produce unbiased estimates of the causal effect of banning affirmative action. I show results both with and without adjusting for linear pre-treatment trends in order to demonstrate the size of the resulting bias from ignoring this empirical issue.

In the first difference-in-differences analysis, I compare Barron's Tier 1 and Tier 2 institutions (treatment group) to Tier 3 and Tier 4 institutions (control group). I find that first-year White enrollment shares at more selective institutions increase by 1.4 percentage points (2.8% relative to the pre-treatment mean), while Black enrollment shares decline by 1.1 percentage points (20%); I find no statistically significant changes in the share of grant aid. Among highly selective public institutions, I find no statistically significant changes in enrollment shares by race, although the share of institutional grant aid increases by 3.9 percentage points (8.0%). This result, which is not statistically significant at conventional levels, partly may reflect that some of these schools already were subject to affirmative action bans.[2] In contrast, among highly selective private institutions, the White enrollment share increases by 2.0 percentage points (4.1%), while Black and Hispanic enrollment shares decline by 1.8 (31.6%) and 1.7 (14.6%) percentage points, respectively. The shares of Pell and state/local grant aid decrease at these institutions as well, by 1.0 (6.2%) and 1.3 (15.8%) percentage points, respectively, suggesting that they become less diverse with respect to both demographic composition and SES.

I then conduct the same analyses comparing institutions in Barron's selectivity Tiers 1 through 4 to unranked institutions. Unranked institutions are the least selective and typically are open access, which makes them unlikely to be directly impacted by the *SFFA* decision. I observe a similar pattern of results, with the White enrollment share increasing by 1.1 percentage points (2.1%) and the Black enrollment share decreasing by 0.8 of a percentage point (14.5%) among selective institutions relative to non-selective institutions. I also observe a decrease in Pell Grant aid of 2.2 percentage points (10.8%) and state/local grant aid of 1.7 percentage points (9.4%) alongside an increase in institutional grant aid of 2.0 percentage points (2.7%). These effects are more pronounced at public universities. Among private institutions, the only statistically significant enrollment change is a 1.0 percentage point (17%) decline in the Black first-year enrollment share, accompanied by declines in total, Pell, and state/local grant aid. These results indicate that public and private institutions respond differently to these changes across the selectivity spectrum. While demographic changes are widespread among public universities along the selectivity distribution, they are more concentrated at the most selective private institutions.

In the final difference-in-differences analysis, I compare public institutions in states without preexisting affirmative action bans (treatment group) to those in states with prior bans (control group). This comparison addresses a limitation of the first analysis, in which lower-selectivity schools in the control group still may have been partially affected by the policy. By using public schools in states where race-based admissions already were prohibited as the control group, I provide a robustness check that isolates the effect of the 2023 Supreme Court decision on institutions that were less likely to be directly affected by the decision.

These results align with those from the first analysis across different selectivity tiers, showing an increase in White enrollment and a decrease in Black enrollment at highly selective universities. When estimating this model for both all and non-selective public institutions, I

[2]When I restrict the regression to public schools in states without a preexisting ban, the White share increases by 2.5 percentage points, while the Black share decreases by 0.8 of a percentage point.

find that the Asian enrollment share increases, while the Hispanic enrollment share declines; the shares of any type of grant aid, Pell, and state/local grant aid also decline. Restricting the sample to Tier 1 and Tier 2 institutions, the White enrollment share increases and the Black enrollment share declines, alongside decreases in Pell, state/local, and institutional grant aid. I find no statistically significant changes in enrollment or grant aid shares among Tier 3 and Tier 4 institutions.

These findings show that the *Students for Fair Admissions v. Harvard* Supreme Court decision has had significant effects on the racial composition of full-time students at four-year institutions. While differences exist across schools depending on their level of selectivity and whether they are public or private, the overall patterns indicate that all four racial/ethnic groups, White, Asian, Black, and Hispanic, have been affected. Specifically, White enrollment at highly selective universities has increased the most, while Black enrollment at these institutions has decreased the most. Asian enrollment also has risen, and Hispanic enrollment has declined, although these changes are observed less consistently in the groups I analyze. These results are consistent with findings from previous research on changes in enrollment in states that banned affirmative action, which show that URM enrollment at selective public institutions decreased (Hinrichs 2012; Antonovics and Backes 2013).

Additionally, the overall distribution of grant aid trends shows a general decrease in total, Pell, and state/local aid, alongside a smaller increase in institutional aid at selective schools, particularly public ones. The simultaneous decline in overall grant aid and in Black and Hispanic student enrollment suggests that these students are among those most in need of financial assistance. My results imply that race-conscious admissions policies also decrease the enrollment of low-income students at selective universities, a group that is significantly underrepresented (Dynarski et al. 2021; Chetty et al. 2020). While the goal of the *SFFA* ruling was to eliminate explicit race-based admissions decisions, my analysis finds that in practice, this policy reduced both demographic and socioeconomic diversity.

My paper makes several contributions to the literature. I provide one of the first comprehensive analyses of the aggregate effects of the *SFFA* Supreme Court decision, and the first implementation of a DiD framework that uses lower-selectivity institutions as a relatively untreated comparison group. I also complement prior research on state-level affirmative action bans by examining a nationwide prohibition and similarly find that such policies reduce URM enrollment at selective universities.

Additionally, I provide new evidence on heterogeneity across sectors, showing that the largest enrollment effects occur at the most selective private institutions but are more evenly spread out across public institutions. Finally, my study is among the first to examine the relationship between racial and socioeconomic diversity following a national affirmative action ban. Because substituting toward income-based admissions criteria is a plausible strategy for maintaining racial/ethnic diversity, understanding how grant aid and socioeconomic composition shift after *SFFA* provides important new insight into institutional responses.

2 Race Based Admissions Legal Background

The first official measure to mandate affirmative action was in 1961 when President John F. Kennedy issued Executive Order 10925. This order required the U.S. government to ensure equal opportunity for all individuals, regardless of race, creed, color, or national origin, and directed agencies to "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" (Establishing the President's Committee on Equal Employment Opportunity 1961). The first major action following this Executive Order came in 1965, when President Lyndon B. Johnson issued Executive Order 11246, which reinforced Kennedy's directive by establishing more concrete anti-discrimination standards for federal agencies and contractors (Equal Employment Opportunity 1965). This order is generally viewed as the beginning of affirmative action implementation in the United States.

The federal government has never enacted explicit affirmative action policies for higher education. It has only done so in hiring, with the aim of increasing diversity across sectors. However, many selective universities have subsequently implemented such measures in admissions, guided by Supreme Court rulings and state-specific regulations. The first U.S. Supreme Court ruling on affirmative action in higher education was *Regents of the University of California v. Bakke* (1978). In this case, the Court ruled that racial quotas were unconstitutional, but using race as a factor in admissions to promote a diverse student body was permissible. About 20 years later, the U.S. Court of Appeals began limiting the use of affirmative action in public university admissions. In *Hopwood v. Texas* (1996), the Fifth Circuit effectively ended affirmative action at public colleges in Texas, ruling that race-based admissions cannot be used to "redress societal discrimination" and may only be justified for educational benefits, such as promoting diversity in the classroom. Similarly, in *Johnson v. Board of Regents of the University of Georgia* (2000), the Eleventh Circuit ruled that policies granting a "diversity" bonus to nonwhite applicants violated the Equal Protection Clause of the Fourteenth Amendment.

In *Gratz v. Bollinger* (2003), the Supreme Court held that the University of Michigan's policy of automatically awarding points to minority applicants was unconstitutional. In contrast, *Grutter v. Bollinger* (2003) upheld the University of Michigan Law School's race-conscious admissions policies, finding that they did not harm nonminority applicants and therefore did not violate the Fourteenth Amendment. *Fisher v. University of Texas at Austin* (2016) reinforced the Grutter decision, confirming that universities may consider race as part of a holistic admissions program.

Additionally, eight states have banned affirmative action in college admissions alongside federal rulings. California led the way with Proposition 209 (1996), followed by Washington with Initiative 200 (1998). Florida banned affirmative action through the One Florida Plan (1999). Michigan passed the Michigan Civil Rights Initiative (2006), Nebraska passed the Nebraska Civil Rights Initiative (2008), and Arizona passed Proposition 107 (2010). New Hampshire and Oklahoma implemented bans in 2012 via House Bill 623 and the Oklahoma Affirmative Action Ban Amendment, respectively. The Supreme Court upheld states' rights to ban race- and gender-based admissions in *Schuette v. Coalition to Defend Affirmative Action* (2014). In summary, prior to *SFFA* in 2023, universities were legally permitted to consider applicants' races as one factor among many in a holistic admissions process to promote student body diversity, except at public institutions located in states with explicit bans.

In *Students for Fair Admissions v. President and Fellows of Harvard College* (2023), the focus of this study, the Supreme Court effectively ruled that universities and colleges can no longer use race as a standalone factor in admissions. This ruling requires them to find different ways to diversify their campuses through a more colorblind approach.[3] Critically, the *SFFA* decision may produce different effects on admissions than state affirmative action bans because the latter only affected a small number of public institutions while the former affects every college and university in the country.

3 Literature Review

Several prior studies have analyzed affirmative action's effect on URM enrollment, focusing on state-level affirmative action bans. Eight states (California, Washington, Florida, Michigan, Nebraska, Arizona, New Hampshire, and Oklahoma) banned affirmative action in college admissions at public institutions prior to 2023.[4] These state bans provide a natural

[3]Following affirmative action bans in states such as California, Texas, and Florida, some policymakers adopted percentage plans that guaranteed automatic admission to top high school graduates. Although intended to promote diversity through race-neutral, merit-based criteria and broader geographic representation, evidence suggests these policies were less effective than race-conscious admissions in increasing URM enrollment, making them imperfect substitutes (Colin and Cook 2023).

[4]The legal justification for affirmative action rests on the idea that a diverse student body benefits not only underrepresented minority students but also those in the majority (Arcidiacono et al. 2014). Numerous studies have attempted to determine whether these policies genuinely benefit non-URM students, but the empirical evidence remains inconclusive. There is no concrete evidence indicating that non-URM students experience positive outcomes from affirmative action policies, particularly in quantifiable measures such as

experiment to examine how eliminating affirmative action in admissions affects segregation and diversity across colleges. Empirical analyses of these bans suggest that, for public colleges and universities, the elimination of race-based preferences reduced URM student enrollment at flagship and elite public schools but had little effect on the overall likelihood of attending a four-year college (Hinrichs 2012; Antonovics and Backes 2013). When affirmative action is banned, URM students do not re-sort in a way that maintains college quality (Hinrichs 2012). These enrollment responses to affirmative action bans primarily result from changes in admissions behavior rather than changes in application patterns among URM students (Card and Krueger 2005; Antonovics and Backes 2013).

A 2012 study by Hinrichs examined the effect of statewide affirmative action bans in California, Florida, Texas, and Washington (Hinrichs 2012). He found that the bans led to a reduction in Black enrollment shares at the top-50 ranked schools by 1.6 percentage points and at the top-50 ranked public schools by 1.7 percentage points. The Hispanic share declined by 1.8 and 2.0 percentage points, respectively, while the White enrollment share at these schools increased by about 3 percentage points. This study provides clear evidence that state affirmative action bans reduce minority representation at elite public universities and, consequently, lower school quality for URM students.[5] My research contributes to this literature by analyzing the effects of a nationwide affirmative action ban on the racial composition of both public and private universities, as well as the broader higher education landscape. Examining a national ban, rather than state-level bans, is important because a nationwide policy affects all institutions simultaneously, eliminating the option to avoid the policy by enrolling in an unaffected state or institution. State bans allow students to adjust their application and enrollment decisions across state lines, meaning observed effects may partly reflect strategic avoidance behavioral responses rather than the full impact of the policy itself. Research on state affirmative action bans focuses on racial/ethnic diversity, and I further extend this literature by examining the implications of a ban on socioeconomic diversity as well.

The only other study to examine the effect of the *SFFA* decision on student enrollment is an unpublished working paper, which analyzes the effects of *SFFA* on high school students' patterns of college entry (Bloem et al. 2026). They use PSAT, SAT, and AP test data from

future earnings (Hinrichs 2011; Daniel et al. 2000; Arcidiacono and Vigdor 2010).

[5]Hinrichs (2017) analyzed whether state affirmative action bans affect segregation and diversity across colleges. He found that state affirmative action bans can both increase and decrease segregation. The results for Black exposure to Whites and for Black-White dissimilarity suggest that the state affirmative action bans of 1995-2003 are associated with less segregation, whereas the state bans of 2004-2015 are associated with more segregation. States that banned affirmative action more recently have relatively smaller Black populations, meaning that even modest shifts in enrollment across institutions can generate large changes in measured segregation indices, potentially explaining the differing results across the years.

2021 to 2024 and link these records to National Student Clearinghouse college enrollment data to measure students' academic achievement and enrollment outcomes. They find that high-achieving URM students are approximately 14 percent less likely to enroll in highly selective colleges in 2024. Using a difference-in-differences approach that leverages preexisting state-specific bans on race-based admissions, they additionally show that the share of URM first-year domestic students at highly selective colleges declines in the first year after *SFFA*, with smaller declines observed at selective public institutions. In contrast, they show that enrollment patterns by income change little, with the highest-achieving non-URM students from lower-income neighborhoods enrolling in more highly selective universities. My analysis builds on this study by leveraging differences across colleges in their exposure to the affirmative action ban based on their selectivity, capturing changes across institutions beyond just public universities. I also use a different measure of income, individual grant aid receipt rather than median neighborhood income, which may more accurately capture the relationship between race and socioeconomic status. Financial aid reflects a student's individual economic circumstances more precisely than neighborhood-level averages, which rely on broader geographic generalizations.[6] The fact that the results from this study and those in Bloem et al. (2026) are similar to one another despite using different data sources and approaches helps to reinforce the conclusions from both studies.

There also is existing empirical research on how universities adjust their admissions practices following affirmative action bans to maintain a racially diverse student body. Early analyses (Chan and Eyster 2003) suggest that colleges may have incentives to overlook qualifications in order to achieve diversity after race-based admissions are prohibited. Subsequent studies build on this theory by calibrating a model of the U.S. higher education system in which schools maximize "quality" (Epple et al. 2008). In this model, quality is a combination of spending, student test scores, and racial and income diversity. Banning racial preferences leads universities to place more weight on characteristics positively correlated with URM status, such as income, and less weight on factors positively correlated with racial majority status, such as test scores. The results indicate a substantial decrease in racial diversity at the most selective colleges, despite universities' efforts to sustain diversity by adjusting admissions criteria. My results support this model, showing that when affirmative action is banned, highly selective universities admit both more high-income students and fewer URM students.

Similarly, Fryer et al. (2008) argue that URM students' incentives to invest in school

[6] Chetty et al. (2020) find that children who attend Ivy-Plus colleges tend to grow up in areas with a higher share of high-income peers than the average child, even after controlling for their parents' incomes. They also show that middle income students attend Ivy-Plus colleges at the lowest rates conditional on test scores.

decrease following an affirmative action ban. Universities place more emphasis on applicant characteristics positively correlated with being in a minority group and less emphasis on endogenous factors like test scores, which reduces the payoff for student effort. Hickman and Bodoh-Creed (2017) further suggest that bans affect student investment in two ways: high-ability minority students may increase their effort, as admission is no longer assured without investment, whereas lower-ability minority students may reduce their effort because the probability of admission is too low to justify investment. Hickman and Bodoh-Creed (2017) find that the latter effect dominates, leading to a substantial overall decline in student achievement following a ban. It is important to note that my analysis focuses on the short-run effects of an affirmative action ban, so endogenous responses by high school students are unlikely to have materialized. Future research is needed to assess the long-run effects of the federal ban and whether these responses emerge over time.

Finally, Antonovics and Backes (2014a, 2014b) examine how admissions weights shifted in the University of California system after California's affirmative action ban. They find that less weight was placed on SAT scores, while more weight was given to applicants from lower-income or less-educated families. This adjustment disproportionately hurt well-off nonminority students but effectively increased socioeconomic diversity in the student body. My results show declines in both URM enrollment and financial aid, suggesting that URM status and SES function as complements. This finding underscores the importance of how institutions adjust their admissions policies to maintain diversity.[7] I find no evidence that schools are placing greater weight on income as a substitute for race in their admissions decisions.

4 Data

a IPEDS

The main data used in this study are from The Integrated Postsecondary Education Data System (IPEDS), which is maintained by the National Center for Education Statistics and provides information on all U.S. colleges and universities that disburse Title IV aid. Beginning with the 2010–2011 school year, institutions were required to use newly defined racial categories when reporting fall enrollment (Sykes 2012). These categories include: non-resident alien, race and ethnicity unknown, Black or African American, American Indian

[7] Levine and Reber (2023) argue that class-based affirmative action is a poorly targeted mechanism for maintaining racial diversity. Although Black and Latino students would benefit, given that 49.4 percent of Pell Grant recipients are Black or Latino, more than half of Pell recipients are not Black or Latino. Therefore, a purely class-based policy would not effectively or precisely target URM students.

or Alaska Native, Asian, Native Hawaiian or Other Pacific Islander, Hispanic of any race, White, and two or more races. I also use the Student Financial Aid and Net Price data, which include the percentage of students receiving Pell Grant, state and local grant, and institutional grant aid.

b Data Frame Construction

I first create a master dataset containing information on every educational institution in IPEDS from 2018 to 2024. I use the IPEDS raw Institutional Characteristics complete datasets for all years, which provide directory and classification information for each institution. I construct a panel dataset consisting of the analysis variables: unitid (the institution's unique identifier), institution name, state, control type (public, private nonprofit, or private for-profit), and whether the school is a two-year or four-year institution. Because these characteristics are largely stable over time, I assume they remain constant at the initial value observed in the IPEDS data over this period. I therefore remove duplicate records so that only one observation per institution remains.

I also construct a selectivity variable using the criteria developed in *Barron's 2009 Profiles of American Colleges*. The categories include: *most competitive* (selectivity Tier 1), *highly competitive plus* (2), *highly competitive* (3), and *very competitive plus* (4). I treat the almost 2,500 institutions not ranked by Barron's as non-selective.

For enrollment outcomes, I use IPEDS' Fall Enrollment datasets disaggregated by race/ethnicity, gender, attendance status, and student level from 2018–2024. I merge each year's enrollment file to my master institution dataset, dropping all two-year and administrative institutions, since my analysis focuses on four-year colleges and universities. I restrict the sample to full-time, first-time, first-year, degree-seeking undergraduates, as this group is my primary population of interest and the students for whom enrollment effects are most salient.

I retain all race and ethnicity variables. The racial/ethnic categories include: American Indian or Alaska Native, Asian, Black or African American, Hispanic, Native Hawaiian or Other Pacific Islander, White, Two or More Races, and Race/Ethnicity Unknown. I also keep variables for the total number of students and the number of nonresident aliens. Additionally, I use each year's Student Financial Aid and Net Price data files and keep the percentage of full-time, first-time undergraduates awarded Pell, state and local, or institutional grant aid as my primary variables of interest.[8]

To complete my analysis dataset, I combine all individual year files into a single longitudinal panel and drop institutions located outside the 50 states and Washington, D.C.

[8]I drop the around thirty or fewer institutions that appear in this dataset but not the fall enrollment ones.

The excluded schools are located in American Samoa, the Federated States of Micronesia, Guam, the Northern Mariana Islands, Palau, Puerto Rico, the U.S. Virgin Islands, and the Marshall Islands. I also exclude the U.S. military academies, including the United States Military Academy, Naval Academy, Air Force Academy, Coast Guard Academy, and Merchant Marine Academy, as the Supreme Court exempted these institutions from the ruling.

Across all years in my sample, there are slightly more than 2,500 missing observations for first-year undergraduate enrollment, which accounts for 6.4% of the observations. For approximately 65 institutions with enrollment data missing for only a single year, I impute the missing value by averaging enrollment in the preceding and subsequent years. All remaining missing observations are left as missing. There is no correlation between treatment exposure and the missing observations, suggesting that the data are missing at random.[9]

I then construct share variables for each racial and ethnic category, as these proportions are more informative for my analysis than raw counts. Each share is calculated by dividing the number of students in the given racial/ethnic group by the total number of students at the institution. I also create weighted shares, weighting by total institutional enrollment averaged across all of the years of interest.

Table 1 shows mean enrollment percentages by race, selectivity tier, and control. The White enrollment share at Tier 1–2, Tier 1–4, and non-selective institutions was slightly above 50 percent prior to 2024 (columns 1, 4, and 7). In the 2024 school year, this share declined by approximately 3 percentage points across all groups (columns 3, 6, and 9). Asian students make up a larger share of enrollment at more selective institutions, accounting for roughly 11–13 percent of students compared to 3.8 percent at non-selective institutions. In 2024, the Asian enrollment share increased by 0.9 of a percentage point at Tier 1–2 schools, increased by 0.4 of a percentage point at Tier 1–4 schools, and remained unchanged at non-selective institutions. In contrast, Black students represent a larger share of enrollment at non-selective universities (about 16 percent) than at selective institutions (approximately 5 percent). In 2024, the Black enrollment share decreased by 0.5 of a percentage point at Tier 1–2 schools, remained unchanged at Tier 1–4 schools, and increased by 0.5 of a percentage point at non-selective institutions. Hispanic enrollment shares range from roughly 12 to 16 percent across the selectivity distribution. In 2024, the Hispanic share decreased by 0.6 of a percentage point at Tier 1–2 institutions, but increased by 1.2 percentage points at Tier 1–4 schools and by 1.6 percentage points at non-selective institutions. As I show below, these first differences reveal patterns that differ from those in the regression analysis. These

[9]For the Tier 1–2 versus 3–4 comparison, no observations are missing. For the Tier 1–4 versus non-selective comparison, I re-estimate the main regression using missing total enrollment as the outcome and obtain a coefficient of -0.001 with a standard error of 0.001. There is a near-zero relationship, indicating no statistically significant relationship between the treatment and missing data.

discrepancies underscore the need for empirical analysis, as first differences do not capture the causal impact of the policy. The empirical analysis is presented in Section 5. Table 1 also provides a breakdown of these patterns by public and private institutions.

Table 2 shows similar means for grant aid. The share of first-year students receiving any grant aid, Pell Grants, state and local grants, and institutional grants, increased from the 2018–2023 period to 2024 across Tier 1–2, Tier 1–4, and non-selective institutions. At Tier 1–2 schools, approximately 66 percent of first-year students receive grant aid overall, with about 17 percent receiving Pell Grants, 11 percent receiving state or local grants, and 65 percent receiving institutional grant aid. At Tier 1–4 institutions, roughly 77 percent of students receive any grant aid, including about 21 percent receiving Pell Grants, 19 percent receiving state or local grants, and 74 percent receiving institutional grants. Students at non-selective institutions generally receive grant aid at higher rates than those at more selective institutions, with the exception of institutional grants. All types of grant aid, across institutions of all selectivity levels, increased from 2018–2023 to 2024. These changes further underscore the need for empirical analysis, as first differences do not capture the causal impact of the *SFFA* decision.

I also estimate these first differences separately for institutions in states without a pre-existing affirmative action ban and those in states with a pre-ban. The results show broadly similar patterns across these groups, with comparable racial enrollment shares. For institutions in states without a pre-ban, the share of White students generally declines from the 2018–2023 period to 2024, while the shares of other racial groups tend to increase. In contrast, for institutions in states with a pre-ban, both White and Asian enrollment shares generally decline, while the shares of other groups increase. Appendix Tables A1 and A2 provide a more detailed breakdown of these patterns, including results by type of grant aid.

5 Empirical Methodology

a Difference-in-Difference Models

I estimate two difference-in-differences (DiD) models with panel fixed effects. My pre-treatment years are 2018-2023, and 2024 is my treated year. In the first analysis, I use less selective schools as the control group. The intuition behind this approach is that less selective institutions are not as well positioned to implement race-conscious admissions policies, and therefore serve as a plausible less treated comparison group (Arcidiacono et al. 2011). I first restrict the analysis to Barron's selectivity Tiers 1 and 2 versus Tiers 3 and 4 to isolate the policy's effect on the most selective institutions. I then expand the specification to include Tiers 1–4 compared to unranked (non-selective) schools in order to capture effects across the full selectivity distribution. I estimate all models separately for public and private institutions as well as for each racial group: White, Asian, Black, Hispanic, and Other. I also estimate these models for each grant aid category: any grant aid, Pell Grants, state/local grants, and institutional grants.

The selectivity-based difference-in-difference models are of the form:

$$Y_{it} = \beta_1(Selective_i \times I(2024)_t) + \alpha_i + \gamma_t + \epsilon_{it} \tag{1}$$

where Y_{it} is the specified racial share of students or share of students receiving aid at institution i in year t. The variable $Selective_i$ is an indicator equal to 1 for selective institutions (selectivity Tiers 1-2 or 1–4) and 0 for less selective institutions (Tiers 3-4 or non-selective). $I(2024)_t$ is an indicator variable equal to 1 for the year 2024 (the post-treatment year) and 0 otherwise. The model includes institution fixed effects (α_i), which absorb all constant institution-level characteristics (including selectivity level) and year fixed effects (γ_t), which control for common time shocks across institutions. ϵ is the error term. The coefficient of interest in equation (1) is β_1, which captures the differential change in student share at highly selective institutions relative to less selective institutions in 2024. Standard errors are clustered at the institution level to account for correlation within institutions over time. Observations are weighted by average enrollment to ensure that estimates are not too influenced by smaller institutions with noisier measurements and that they are representative of the student population.

The key identification assumptions for a DiD model are the absence of both differential shocks between the treatment and control groups and differential pre-treatment trends. There is no reason to believe that any event in 2024 would have differentially affected schools by selectivity.[10] In addition, I define the treatment in three ways–Tiers 1–2, Tiers 1–4, and institutions in states without preexisting bans–and each specification yields similar results, suggesting that unobserved shocks are unlikely to be driving differences across groups.

To assess the validity of the parallel trends assumption, I estimate event study specifications (see Section 5, Part B), which indicate approximately linear pre-treatment trends (Figures 1–12). I therefore include linear time trends in my regression specifications to control for this variation. The model including the linear time trends takes the form:

$$Y_{it} = \beta_1(Treat_i \times I(2024)_i) + \beta_2(Treat_i \times t) + \alpha_i + \gamma_t + \epsilon_{it} \tag{2}$$

[10] It is worth noting that I use Fall 2024 enrollment data, which predates the federal funding cuts to higher education institutions and the rollback of student loan policies, both of which could have had differential effects across institutions.

$Treat_i \times t$ is the interaction of a linear time trend with treatment status. This specification allows treated and control institutions to follow different linear trends in the pre-period, so that β_1 isolates the effect of the ruling on 2024 demographics net of any preexisting differential linear trajectories between the two groups.

These pre-treatment trends are important as they indicate that institutional demographics were already shifting prior to 2024, which implies that simple before-and-after comparisons of demographic changes following the *SFFA* ruling would yield biased estimates of its causal effect. To make the magnitude of this bias transparent, I present results both with and without adjusting for linear pre-treatment trends in my regression tables.

To address concerns that even low-selectivity institutions may be indirectly affected through a "cascade" effect, where URM students re-sort across the selectivity distribution, I conduct an additional analysis to isolate the policy's impact. This specification also helps account for potential differential shocks between treatment and control groups, as the institutions in this analysis are similar in characteristics with the primary difference being the state policy environment in which they operate.

Specifically, I restrict the sample to public institutions and use those located in states with preexisting affirmative action bans as the control group, since these schools were effectively untreated by the *SFFA* ruling. This analysis is similar to the approach used by Bloem et al. (2026). While I recognize that public institutions in states with preexisting bans still may be indirectly affected for similar reasons as non-selective schools are, they represent the closest approximation to an untreated comparison group. I estimate this specification separately for four groups: all public institutions in ban states versus non-ban states, Tier 1 and 2 public institutions, Tier 3 and 4 public institutions, and non-selective public institutions. I disaggregate the analysis by selectivity tier to examine heterogeneous effects, as more selective institutions have greater capacity to practice race-conscious admissions and thus may be more affected by the ruling (Arcidiacono et al. 2011; Arcidiacono and Lovenheim 2016). I compare changes between ban and non-ban states using the following model:

$$Y_{it} = \beta_1(No\ Ban_i \times I(2024)_i) + \beta_2(Treat_i \times t) + \alpha_i + \gamma_t + \epsilon_{it} \tag{3}$$

where $No\ Ban$ is an indicator equal to 1 if an institution is under a pre-*SFFA* state affirmative action ban and is 0 otherwise. All other variables are as previously defined. As in equations (1) and (2), β_1 is the coefficient of interest that captures the differential change in student share in institutions in states without a ban already in place relative to those in a state with a ban in place in 2024. This equation is both with and without group-specific linear trends.

b Event Studies & Linear Time Trends

To assess the validity of the parallel trends assumption for my DiD model, I estimate an event study specification of the form:

$$Y_{it} = \sum_{k=2018,\, k\neq 2022}^{2024} \beta_k (Treat_i \times I(t=k)) + \alpha_i + \gamma_t + \epsilon_{it} \tag{4}$$

where *treat* is an indicator for whether the institution is either selective or untreated because of a pre-2024 state ban. The interactions between the treatment variable and year indicators are included for all years in the sample (2018–2024), with 2022 omitted as the base period. The β_k estimates for the pre period years (2018–2023) provide the test of parallel pre-trends: if treated and control institutions are on similar trajectories prior to the ruling, these coefficients will not be statistically significant or exhibit consistent trends over time. Similarly to my main regression analysis, this model is estimated with institution and year fixed effects, observations are weighted by average enrollment, and standard errors are clustered at the institution level.

6 Results

a Estimates by Institution Selectivity

Effects on Racial/Ethnic Composition

Figure 1 presents the event study graphs for changes in racial/ethnic composition at Tier 1–2 versus Tier 3–4 schools, using 2022 as the omitted base year. There is little evidence of pre-2024 trends for Black enrollment, while there is suggestive evidence of modest linear pre-trends for White, Asian, and Hispanic students. Figure 2 illustrates these patterns for public institutions, revealing apparent pre-trends for White students, while Figure 3 displays the patterns for private institutions, indicating pre-trends for White, Asian, and Hispanic shares.

Figure 4 presents the racial/ethnic event study for Tiers 1–4 versus non-selective schools. Linear pre-trends are again evident, particularly for White and Asian shares. These patterns also are shown for public institutions in Figure 5, which demonstrates pre-trends particularly for White and Asian shares, while Figure 6 shows that private institutions exhibit pre-treatment relative trends across all observed racial groups. These pre-treatment trends all appear linear, which motivates the inclusion of controls for linear trends in the difference-in-difference models to account for this variation.

Table 3 reports the results from estimating equations (1) and (2), comparing racial/ethnic changes in Tier 1–2 institutions to those in Tiers 3–4 (columns 1–2) as well as Tiers 1–4 to non-selective schools (columns 3–4). The table presents coefficients and standard errors as well as percent effects relative to the pre-treatment mean in brackets. Odd columns contain estimates without controls for linear pre-treatment trends and even columns include estimates with the controls. The estimates in column (2) show that first-year White enrollment shares at more selective institutions (Tiers 1–2) increase by 1.4 percentage points (2.8% of the pre-treatment mean), which is statistically significant at the 5 percent level. The point estimate for Asian enrollment share is 0.6 of a percentage point (4.6%), which is small and not statistically significant at even the 10 percent level, suggesting a modest increase. In contrast to the increases among White and Asian students, Black enrollment shares decline by 1.1 percentage points (20%), a statistically significant effect, while Hispanic shares decrease by a non-statistically significant 0.7 of a percentage point (5.7%).

Column 1 of Table 3 presents the results without adjusting for linear pre-trends. These estimates still indicate an increase in White enrollment, but it is smaller in magnitude and is not statistically significant, consistent with the downward pre-trend shown in Figure 1(a). The change in Asian enrollment remains not statistically significant, though it is slightly larger than in the trend-adjusted model, which aligns with the upward trend in Figure 1(b). Black share changes are identical across both specifications, reflecting the absence of pre-trends in Figure 1(c). For Hispanic enrollment, the no-trend results show a larger decrease than the trend-adjusted estimates, which is statistically significant at the 5 percent level and consistent with the downward pre-trend shown in Figure 1(d). These differences between the analyses with and without adjustment for pre-trends further underscore that a simple before-and-after comparison of the 2024 policy impact would not only be biased, but that the bias also would vary across groups due to these differential pre-trends.

Table 4 presents estimates disaggregated by public (Panel A) and private (Panel B) school status. Column (2) reports estimates for Tiers 1–2 versus Tiers 3–4. Among public institutions, none of the changes in racial/ethnic shares are statistically significant, and the estimates, particularly for White, Asian, and Hispanic enrollment, are imprecise. The White share decreases by 1.6 percentage points (0.8%), while Asian and Hispanic shares increase modestly by 0.1 of a percentage point; the Black share declines by 0.4 of a percentage point (6.9%). None of these estimates is statistically significantly different from zero.

For private institutions, the White share increases by 2 percentage points (4.1%), which is statistically significant at the 5 percent level. Asian shares also increase modestly by 0.6 of a percentage point (4.8%), although the estimate is not statistically significant. Black and Hispanic shares decline by 1.8 percentage points (31.6%) and 1.7 percentage points (14.6%), respectively, both significant at the 1 percent level. Overall, these results suggest that among Tier 1–2 schools, the White share increases and the Black share declines. While changes in Asian and Hispanic shares are less consistent, the estimates show a modest increase in Asian share and decline in Hispanic share. These patterns are more pronounced among private institutions.

The differences between public and private institutions may reflect that some public universities in the sample were effectively untreated, as their states had preexisting affirmative action bans. Appendix Table A3 shows that restricting the sample to public institutions in states without such bans yields estimates more similar to those for private institutions: the White share increases by 2.5 percentage points (4.6%), and the Black share decreases by 0.8 of a percentage point (13.5%). This difference suggests that the small effects in the full public sample are driven to some extent by institutions in states that had already implemented affirmative action bans and that therefore were less affected by the *SFFA* decision.

Column (4) of Table 3 reports the linear trend results for the comparison between Tiers 1–4 and non-selective schools. The results follow a similar pattern to those for Tiers 1–2 versus Tiers 3–4. Relative to non-selective institutions, White enrollment shares at more selective institutions increase by 1.1 percentage points (2.1%), while Black enrollment shares decrease by 0.8 of a percentage point (14.5%); both estimates are statistically significant at the 5 percent and 1 percent levels, respectively. The White increase and Black decrease in this sample are both 0.3 of a percentage point smaller than in the Tier 1–2 versus 3–4 comparison. The point estimates for Asian and Hispanic shares are small and are not statistically significant, suggesting modest relative declines in these groups' enrollments.

Column (4) of Table 4 presents these results separately for public and private institutions. In this specification, the effects are more pronounced among public institutions: the White enrollment share increases by 1.6 percentage points (3.1%), while Asian, Black, and Hispanic shares decline by 0.7 (4.1%), 0.6 (11.8%), and 0.6 (4.3%) of a percentage point, respectively. All of these estimates are statistically significant at the 10 percent level or higher. When the sample is restricted to public schools in states without a preexisting affirmative action ban, all point estimates increase in magnitude (Table A3, Panel A, Column 4). Among private institutions, the only statistically significant change is a 1.0 percentage point (17%) decline in Black first-year enrollment, while the point estimates for White, Asian, and Hispanic shares are close to zero. The aggregate results mirror those from the Tiers 1–2 versus Tiers 3–4 comparison: White shares increase and Black shares decrease, but the effects are more pronounced among public institutions in this specification. This difference between public and private institutions between the two specifications suggests that changes in the public sector occur broadly across the selectivity distribution, whereas in the private sector, effects are concentrated among the most selective schools.

Effects on Grant Aid

The racial and ethnic enrollment changes described above also correspond to shifts in the socioeconomic composition of students. Figures 7–9 present event study graphs for Tiers 1–2 versus Tiers 3–4, disaggregated by type of aid and institution sector. Figure 7 shows that there are pre-treatment trends for state/local and institutional grant aid shares. Figure 8 presents these trends for public schools, showing pre-trends in Pell, state/local, and institutional grant aid, while Figure 9 presents the corresponding trends for private schools, showing pre-trends in Pell and institutional grant aid shares. For these outcomes, controlling for linear time trends is particularly important.

Figures 10–12 present the event studies for Tiers 1–4 versus non-selective schools, showing similar pre-trends, particularly for total grant aid, Pell Grants, and institutional grants, further motivating the inclusion of linear trends in the regression specification and the focus on those results. It also is worth noting that the event studies for non-selective universities (Figures 10d and 11d) show particularly large fluctuations in institutional grant aid. While the institutional grant aid share trends downward, it stabilizes in 2022 and 2023. As a result, the shift in the share of institutional grant aid is not evident relative to these two years but it is apparent compared with earlier pre-treatment years, so these results should be interpreted cautiously; the estimates that include the pre-trend control may overstate the causal increase in the share of institutional grant aid.

Table 5 reports the results from estimating equations (1) and (2), comparing changes in grant aid between Tiers 1–2 institutions and those in Tiers 3–4 (column 2). I find no statistically significant changes across any grant aid category. Overall grant aid and institutional aid increase modestly, while Pell and state/local grant aid decline, although all point estimates are small.

Table 6, column (2) presents estimates for public schools (Panel A) and private schools (Panel B). Among public institutions, institutional aid increases by 3.9 percentage points (8.0%). However, this result should be interpreted cautiously, as it is driven by a sizable negative pre-trend that stabilizes in 2022 and 2023. The event study in Figure 8(d) provides little evidence of a clear effect. Among private institutions, Pell Grant aid declines by 1.0 percentage points (6.2%) and state/local aid declines by 1.3 percentage points (15.8%). The remaining estimates are small and are not statistically significant. The Pell Grant aid estimate that controls for linear trends is larger in magnitude than the estimate that does not, consistent with Figure 9(b), which shows an upward linear pre-trend. These results suggest modest decreases in the proportion of students receiving government aid alongside possible increases in institutional aid, with the latter more pronounced at public institutions.

Table 5 also reports results for changes in grant aid for Tier 1–4 institutions relative to non-selective schools (column 4). Pell and state/local grants decline by 2.2 percentage points (10.8%) and 1.7 percentage points (9.4%), respectively, both statistically significant at the 1 percent level. Institutional grant aid increases by 2.0 percentage points (2.7%), but this result should again be interpreted cautiously, as the event study in Figure 10(d) displays a sizable negative pre-trend that may lead to an overestimation of the effect. Compared to the results with linear pre-trend controls, the estimates without such controls (column 3) for total grant aid are larger in magnitude, reflecting the downward pre-trend shown in Figure 10(a). In contrast, the no-trend estimate for Pell Grants is smaller, consistent with the upward pre-trend in Figure 10(b).

Table 6, column (4), presents these results separately for public and private institutions. Among public institutions, the pattern mirrors the full sample: Pell and state/local grants decline by 2.1 percentage points (8.8%) and 1.5 percentage points (4.1%), respectively, while institutional grant aid increases by 3.1 percentage points (5.4%). Among private institutions, Pell and state/local grant aid also decline, by 2.4 percentage points (12.0%) and 1.6 percentage points (11.7%), respectively, while institutional grant aid decreases slightly, but the estimate is not statistically significant.

Overall, these results are consistent with those for Tier 1–2 vs. 3–4 institutions, indicating declines in the share of students qualifying for government aid alongside increases in those receiving institutional aid (relative to a large negative pre-treatment trend). The concurrent declines in Black and Hispanic enrollment shares, together with reductions in Pell and state/local grant aid at highly selective institutions, suggest a decrease in diversity along both racial and socioeconomic dimensions. This finding is particularly important and not obvious, as the effect on socioeconomic status depends on both the correlation between race and SES and how universities respond to the ban; in principle, the policy could shift SES in either direction. Following affirmative action bans, some states and universities have used income as a proxy for race in an effort to maintain campus diversity (Antonovics and Backes, 2014a, 2014b), but my results suggest that this phenomenon is not occurring in response to the *SFFA* ruling. Instead, the estimates indicate declines in the share of those receiving need-based aid, particularly Pell Grants, which is consistent with a shift away from lower-SES students rather than toward them.

b Estimates by Pre-Decision State Ban Status

Effects on Racial/Ethnic Composition

My second difference-in-difference analysis compares public institutions in states without a preexisting affirmative action ban to those in states with a prior ban. This analysis yields results that are similar to the selectivity-based analysis for highly selective universities. Appendix Tables A1–A4 present the corresponding event study graphs for each analysis, all of which exhibit linear pre-treatment trends.

Table 7 reports the estimates from equation (3), comparing racial/ethnic changes across these groups, dissegregated by selectivity tier. Column (2) presents the results for all schools across the selectivity distribution. Asian enrollment share increases by 0.5 of a percentage point (11.1%), and this estimate is statistically significant at the 1 percent level. White enrollment also increases, but only modestly and is not statistically significant. Black enrollment share declines slightly and is likewise not statistically significant. Hispanic enrollment decreases by 0.4 of a percentage point (2.8%), which is statistically significant at the 10 percent level. The observed changes in White and Black shares differ between the specifications with and without linear pre-trend controls: the no-trend estimate for White share is negative, while the corresponding estimate for Black share is positive. This pattern is consistent with the event study graphs (Figures A1a and A1b), which show a negative pre-trend for White share and a positive pre-trend for Black share.

Column (4) shows these results for Tier 1–2 schools. In this group, the White enrollment share increases by 2.9 percentage points (5.2%), while the Black enrollment share decreases by 1.2 percentage points (20.0%). Asian and Hispanic enrollment shares also decline, although the estimates are small and are not statistically significant. It is worth noting that the Asian share estimates are sensitive to the inclusion of pre-trends in the model, as they exhibit a large positive pre-trend (Figure A2b).

Column (6) displays that at Tier 3–4 schools, none of the estimates is statistically significant and each is small, indicating little change in racial/ethnic composition. At non-selective schools (column 8), the Asian share increases by 0.7 of a percentage point (18.5%), while the Hispanic share decreases by 0.5 of a percentage point (2.9%); changes in White and Black shares are small and are not statistically significant.

These findings are consistent with the results from equations (1) and (2): White shares increase and Black shares decrease at highly selective institutions, while changes in Asian and Hispanic shares are smaller and less consistent. They also align with findings from Bloem et al. (2026), who document reductions of comparable magnitudes in underrepresented minority enrollment at highly selective public universities using a similar empirical strategy.

Effects on Grant Aid

Figures A5–A8 show that linear pre-trends are present in the grant aid analyses comparing schools in states without a pre-ban to those with one. Table 8 reports the grant aid results, comparing public institutions in states without a preexisting affirmative action ban to those in states with a ban. Column (2) shows the results when comparing all schools across the whole selectivity distribution. The total grant aid share decreases by 2.5 percentage points (3.0%), Pell Grant aid decreases by 0.8 of a percentage point (1.8%), and state/local grant aid decreases by 5.0 percentage points (13.0%), while institutional grant aid share remains unchanged. For Tier 1–2 institutions in column (4), relative to schools in states with a preexisting ban, institutions in states without a ban experience declines in the share of students receiving all major forms of grant aid: Pell Grants decrease by 2.0 percentage points (10.5%), state/local grants decrease by 5.4 percentage points (19.2%), and institutional grants decrease by 7.4 percentage points (16.9%).

For Tier 3–4 institutions, there are no statistically significant changes in grant aid. Point estimates are small, with Pell and state/local aid declining and institutional aid increasing slightly, suggesting only modest shifts (column 6). Among non-selective institutions (column 8), Pell and state/local grant aid, as well as grant aid in total, all decline. The Pell share decreases by 0.7 of a percentage point (1.6%), state/local aid decreases by 5.6 percentage points (14.4%), and total grant aid decreases by 2.7 percentage points (3.3%). Overall, these results suggest that grant aid declined at public institutions following the *SFFA* decision. These results are consistent with those from the first DiD analysis: both racial and socioeconomic diversity have declined at selective universities.

7 Conclusion

The 2023 *Students for Fair Admissions v. Harvard* decision was a landmark Supreme Court case that effectively ended the use of race-based considerations in college admissions nationwide. Since President John F. Kennedy issued Executive Order 10925 in 1961, establishing the first formal mandate for affirmative action, race-conscious policies have become ubiquitous in higher education admissions, particularly at highly selective universities. Evidence clearly demonstrates that URM students attend higher ranked universities when affirmative action is present in admissions; however, the legal justification for affirmative action rests on the idea that a diverse student body benefits not only URM students but also those in the majority through exposure to diverse people and viewpoints (Arcidiacono et al. 2014). Therefore, affirmative action remains a highly debated issue, so empirical analysis is needed to examine the effects of this new nationwide ban.

This study shows new evidence that *SFFA* decreased URM enrollment at highly selective universities across the country. The most consistent patterns appear for Black and White students, with Black enrollment shares declining and White enrollment shares increasing. Hispanic enrollment also decreases and Asian enrollment increases, although with less consistency across specifications. These results are aligned with those of previous studies on the impact of individual state affirmative action bans, which find that URM enrollment at highly selective institutions decreased (Hinrichs 2012; Antonovics and Backes 2013). Similarly, the decrease in the share of students receiving grant aid following the *SFFA* Supreme Court case suggests that these institutions became less socioeconomically diverse in addition to becoming less racially diverse. Wealthier, White, and Asian students are now attending more elite schools in larger proportions.

Previous studies on the effects of state bans (Chan and Eyster 2003; Epple et al. 2008) show that banning racial preferences in admissions leads universities to place greater weight on characteristics positively correlated with URM status, such as income. Antonovics and Backes (2014a, 2014b) also find that the UC system responded to the state ban by using income as a proxy in admissions to help sustain diversity after the ban. However, even as these schools attempted to retain racial diversity, the most selective colleges still experienced large declines in URM shares. My results are consistent with these findings, as both racial and socioeconomic diversity have declined, which suggests that either admissions offices have not adjusted their practices to maintain a diverse student body or that their efforts have not been effective. I do not find evidence that schools are placing greater weight on income as a substitute for race in their admissions decisions. However, this study examines only very short-run effects, so future research is needed to determine whether admissions policies adjust more in the long run.

The findings in this study are important because changes in enrollment may affect minority student outcomes, aligned with the quality-fit tradeoff framework (Arcidiacono and Lovenheim 2016). Some research suggests that URM students may perform better without race-based admissions policies because they attend institutions for which they are better academically prepared (Sander 2004). In contrast, other research indicates that URM students may benefit from attending more selective colleges (e.g., Brewer et al. 1999; Hoekstra 2009; Bound et al. 2010; Andrews et al. 2016; Zimmerman 2019). While URM representation at highly selective universities has clearly declined following *SFFA*, future research should examine how these changes affect student outcomes.

Table 1: Pre and Post Treatment Means for Race/Ethnicity

	Tiers 1–2			Tiers 1–4			Non-Selective		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)	2018-2023 (7)	2024 (8)	Difference (9)
				All Schools					
White	0.502	0.472	-0.030	0.548	0.513	-0.035	0.504	0.470	-0.034
Asian	0.131	0.141	0.009	0.111	0.114	0.004	0.038	0.038	0.000
Black	0.056	0.051	-0.005	0.058	0.058	0.000	0.157	0.162	0.005
Hispanic	0.119	0.125	0.006	0.123	0.135	0.012	0.161	0.177	0.016
Other	0.192	0.212	0.020	0.160	0.179	0.019	0.140	0.153	0.013
				Public Schools					
White	0.505	0.475	-0.030	0.522	0.488	-0.034	0.507	0.464	-0.043
Asian	0.177	0.187	0.010	0.159	0.165	0.006	0.046	0.044	-0.001
Black	0.051	0.047	-0.004	0.054	0.055	0.001	0.142	0.145	0.003
Hispanic	0.127	0.140	0.012	0.143	0.158	0.015	0.180	0.205	0.025
Other	0.139	0.150	0.011	0.121	0.134	0.012	0.126	0.143	0.017
				Private Schools					
White	0.501	0.471	-0.030	0.555	0.520	-0.035	0.502	0.474	-0.029
Asian	0.125	0.135	0.009	0.098	0.102	0.003	0.034	0.034	0.000
Black	0.057	0.052	-0.005	0.059	0.059	0.000	0.165	0.173	0.008
Hispanic	0.118	0.123	0.005	0.118	0.130	0.012	0.150	0.161	0.010
Other	0.199	0.220	0.021	0.170	0.190	0.020	0.148	0.159	0.011

Notes: Author's tabulations of IPEDS data from 2018-2024 as described in the text. Tiers are defined using Barron's (2009) *Profiles of American Colleges.* Values represent enrollment shares for each racial group.

Table 2: Pre and Post Treatment Means for Grant Aid

	Tiers 1–2			Tiers 1–4			Non-Selective		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)	2018-2023 (7)	2024 (8)	Difference (9)
				All Schools					
Total	0.663	0.668	0.005	0.771	0.779	0.008	0.857	0.863	0.006
Pell	0.170	0.180	0.011	0.206	0.217	0.011	0.465	0.471	0.005
State/Local	0.107	0.114	0.007	0.183	0.192	0.009	0.315	0.338	0.023
Institutional	0.643	0.658	0.015	0.737	0.758	0.020	0.641	0.666	0.025
				Public Schools					
Total	0.580	0.581	0.001	0.706	0.718	0.011	0.806	0.821	0.014
Pell	0.203	0.221	0.017	0.235	0.248	0.013	0.446	0.460	0.014
State/Local	0.307	0.319	0.012	0.369	0.387	0.018	0.391	0.422	0.031
Institutional	0.488	0.524	0.036	0.576	0.627	0.051	0.521	0.572	0.051
				Private Schools					
Total	0.674	0.680	0.006	0.788	0.794	0.007	0.883	0.887	0.004
Pell	0.165	0.175	0.010	0.199	0.209	0.010	0.475	0.477	0.001
State/Local	0.082	0.088	0.006	0.137	0.143	0.007	0.275	0.289	0.015
Institutional	0.663	0.675	0.012	0.778	0.791	0.013	0.704	0.720	0.016

Notes: Author's tabulations of IPEDS data from 2018-2024 as described in the text. Tiers are defined using Barron's (2009) *Profiles of American Colleges*. Values represent grant aid shares for each type.

Table 3: Difference-in-Differences Estimates by Institution Selectivity on Race/Ethnicity: All Schools

	Tiers 1–2 vs. 3–4		Tiers 1–4 vs. Non-Selective	
	No Trend (1)	Trend (2)	No Trend (3)	Trend (4)
White	0.007	0.014**	0.000	0.011**
	(0.007)	(0.006)	(0.006)	(0.005)
	[1.4%]	[2.8%]	[0.1%]	[2.1%]
Asian	0.008	0.006	0.012***	-0.004
	(0.005)	(0.005)	(0.002)	(0.002)
	[6.2%]	[4.6%]	[8.8%]	[-2.7%]
Black	-0.011***	-0.011***	-0.008***	-0.008***
	(0.003)	(0.003)	(0.002)	(0.002)
	[-19.8%]	[-20.0%]	[-13.5%]	[-14.5%]
Hispanic	-0.011**	-0.007	-0.005*	-0.004
	(0.006)	(0.005)	(0.003)	(0.003)
	[-9.3%]	[-5.7%]	[-4.6%]	[-3.4%]
Other	0.007	-0.002	0.001	0.005
	(0.005)	(0.005)	(0.005)	(0.004)
	[3.6%]	[-0.9%]	[0.6%]	[2.7%]

Notes: Author's estimation of equations (1) and (2) using 2018-2024 IPEDS data. Each cell reports a separate regression estimated at the institution level and includes institution and year fixed effects. Standard errors, clustered at the institution level, are reported in parentheses. Percent changes are reported in brackets and are calculated relative to the pre-2024 treatment group mean. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 4: Difference-in-Differences Estimates by Institution Selectivity on Race/Ethnicity: Public vs. Private Schools

	Tiers 1–2 vs. 3–4		Tiers 1–4 vs. Non-Selective	
	No Trend (1)	Trend (2)	No Trend (3)	Trend (4)
		Panel A: Public Schools		
White	0.002	0.016	0.003	0.016***
	(0.009)	(0.011)	(0.005)	(0.005)
	[3.2%]	[-0.8%]	[0.5%]	[3.1%]
Asian	0.003	0.001	0.012***	-0.007**
	(0.006)	(0.006)	(0.003)	(0.003)
	[1.5%]	[0.6%]	[7.8%]	[-4.1%]
Black	-0.004	-0.004	-0.006**	-0.006***
	(0.005)	(0.004)	(0.003)	(0.002)
	[-6.9%]	[-6.9%]	[-11.5%]	[-11.8%]
Hispanic	0.006	0.001	-0.004	-0.006*
	(0.007)	(0.007)	(0.003)	(0.003)
	[4.5%]	[0.4%]	[-2.7%]	[-4.3%]
Other	-0.007	-0.014**	-0.005	0.003
	(0.006)	(0.006)	(0.003)	(0.004)
	[-5.1%]	[-10.1%]	[-4.2%]	[2.6%]
		Panel B: Private Schools		
White	0.015	0.020**	-0.005	0.000
	(0.009)	(0.008)	(0.018)	(0.015)
	[2.9%]	[4.1%]	[-1.0%]	[0.1%]
Asian	0.017**	0.006	0.010**	-0.001
	(0.007)	(0.005)	(0.004)	(0.003)
	[13.6%]	[4.8%]	[9.9%]	[-0.5%]
Black	-0.020***	-0.018***	-0.010**	-0.010*
	(0.004)	(0.004)	(0.005)	(0.005)
	[-35.6%]	[-31.6%]	[-16.9%]	[-17.0%]
Hispanic	-0.027***	-0.017***	-0.006	0.001
	(0.008)	(0.006)	(0.005)	(0.005)
	[-23.0%]	[-14.6%]	[-5.5%]	[0.8%]
Other	0.016**	0.009*	0.012	0.009
	(0.008)	(0.005)	(0.013)	(0.011)
	[8.0%]	[4.6%]	[7.0%]	[5.5%]

Notes: Author's estimation of equations (1) and (2) using 2018-2024 IPEDS data. Each cell reports a separate regression estimated at the institution level and includes institution and year fixed effects. Standard errors, clustered at the institution level, are reported in parentheses. Percent changes are reported in brackets and are calculated relative to the pre-2024 treatment group mean. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 5: Difference-in-Differences Estimates by Institution Selectivity on Grant Aid: All Schools

	Tiers 1–2 vs. 3–4		Tiers 1–4 vs. Non-Selective	
	No Trend (1)	Trend (2)	No Trend (3)	Trend (4)
Total	-0.010	0.017	-0.021***	-0.001
	(0.014)	(0.017)	(0.008)	(0.001)
	[-1.5%]	[2.5%]	[-2.8%]	[-0.2%]
Pell	0.005	-0.003	-0.009***	-0.022***
	(0.005)	(0.005)	(0.003)	(0.003)
	[2.8%]	[-2.0%]	[-4.4%]	[-10.8%]
State/Local	-0.001	-0.011	-0.020***	-0.017***
	(0.008)	(0.007)	(0.006)	(0.005)
	[-0.7%]	[-10.3%]	[-10.8%]	[-9.4%]
Institutional	-0.016	0.017	-0.030***	0.020**
	(0.014)	(0.013)	(0.009)	(0.008)
	[-2.5%]	[2.7%]	[-4.0%]	[2.7%]

Notes: Author's estimation of equations (1) and (2) using 2018-2024 IPEDS data. Each cell reports a separate regression estimated at the institution level and includes institution and year fixed effects. Standard errors, clustered at the institution level, are reported in parentheses. Percent changes are reported in brackets and are calculated relative to the pre-2024 treatment group mean. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 6: Difference-in-Differences Estimates by Institution Selectivity on Grant Aid: Public vs. Private Schools

	Tiers 1–2 vs. 3–4		Tiers 1–4 vs. Non-Selective	
	No Trend (1)	Trend (2)	No Trend (3)	Trend (4)
		Panel A: Public Schools		
Total	-0.021	0.008	-0.025**	0.001
	(0.024)	(0.028)	(0.012)	(0.016)
	[-3.6%]	[1.4%]	[-3.6%]	[0.1%]
Pell	0.010	0.006	-0.010**	-0.021***
	(0.009)	(0.007)	(0.005)	(0.004)
	[4.9%]	[2.7%]	[-4.3%]	[-8.8%]
State/Local	0.017	-0.002	-0.022**	-0.015**
	(0.016)	(0.015)	(0.009)	(0.008)
	[5.6%]	[-0.5%]	[-5.8%]	[-4.1%]
Institutional	-0.011	0.039*	-0.031**	0.031**
	(0.021)	(0.022)	(0.014)	(0.013)
	[-2.3%]	[8.0%]	[-5.4%]	[5.4%]
		Panel B: Private Schools		
Total	-0.007	0.009	-0.016*	-0.020***
	(0.013)	(0.012)	(0.009)	(0.007)
	[-1.0%]	[1.3%]	[-2.0%]	[-2.5%]
Pell	-0.002	-0.010*	-0.008	-0.024***
	(0.006)	(0.005)	(0.006)	(0.005)
	[-1.5%]	[-6.2%]	[-4.2%]	[-12.0%]
State/Local	-0.012**	-0.013***	-0.011**	-0.016***
	(0.005)	(0.004)	(0.004)	(0.004)
	[-14.4%]	[-15.8%]	[-8.0%]	[-11.7%]
Institutional	-0.006	0.010	-0.014	-0.004
	(0.012)	(0.011)	(0.011)	(0.008)
	[-0.8%]	[1.6%]	[-1.8%]	[-0.5%]

Notes: Author's estimation of equations (1) and (2) using 2018-2024 IPEDS data. Each cell reports a separate regression estimated at the institution level and includes institution and year fixed effects. Standard errors, clustered at the institution level, are reported in parentheses. Percent changes are reported in brackets and are calculated relative to the pre-2024 treatment group mean. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 7: Difference-in-Differences Estimates by Pre-Decision State Ban Status on Race/Ethnicity

	Total		Tiers 1–2	
	No Trend	Trend	No Trend	Trend
	(1)	(2)	(3)	(4)
White	-0.010**	0.004	0.016	0.029*
	(0.005)	(0.004)	(0.018)	(0.016)
	[-1.8%]	[0.8%]	[2.9%]	[5.2%]
Asian	0.008***	0.005***	0.020**	-0.011
	(0.002)	(0.002)	(0.007)	(0.013)
	[18.0%]	[11.1%]	[12.9%]	[-7.4%]
Black	0.006**	-0.001	-0.014*	-0.012**
	(0.003)	(0.002)	(0.007)	(0.004)
	[3.5%]	[-0.6%]	[-24.4%]	[-20.0%]
Hispanic	-0.009***	-0.004*	-0.033***	-0.001
	(0.003)	(0.002)	(0.007)	(0.013)
	[-5.9%]	[-2.8%]	[-31.2%]	[-1.2%]
Other	0.005	-0.004	0.011	-0.004
	(0.005)	(0.004)	(0.011)	(0.007)
	[4.3%]	[-3.7%]	[8.7%]	[-3.5%]
	Tiers 3–4		**Non-Selective**	
	No Trend	Trend	No Trend	Trend
	(5)	(6)	(7)	(8)
White	-0.024***	0.006	-0.009*	0.003
	(0.009)	(0.007)	(0.006)	(0.004)
	[-4.3%]	[1.0%]	[-1.7%]	[0.6%]
Asian	0.012*	-0.002	0.007***	0.007***
	(0.006)	(0.007)	(0.002)	(0.001)
	[8.3%]	[-1.8%]	[17.9%]	[18.5%]
Black	0.014***	0.000	0.006*	0.000
	(0.004)	(0.004)	(0.003)	(0.002)
	[23.4%]	[-0.6%]	[3.4%]	[-0.2%]
Hispanic	-0.010*	-0.003	-0.007*	-0.005*
	(0.006)	(0.009)	(0.004)	(0.003)
	[-7.8%]	[-2.0%]	[-4.8%]	[-2.9%]
Other	0.009	0.000	0.004	-0.005
	(0.007)	(0.010)	(0.005)	(0.004)
	[9.0%]	[-0.3%]	[3.4%]	[-4.4%]

Notes: Author's estimation of equation (3) using 2018-2024 IPEDS data. Each cell reports a separate regression estimated at the institution level and includes institution and year fixed effects. Standard errors, clustered at the institution level, are reported in parentheses. Percent changes are reported in brackets and are calculated relative to the pre-2024 treatment group mean. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table 8: Difference-in-Differences Estimates by Pre-Decision State Ban Status on Grant Aid

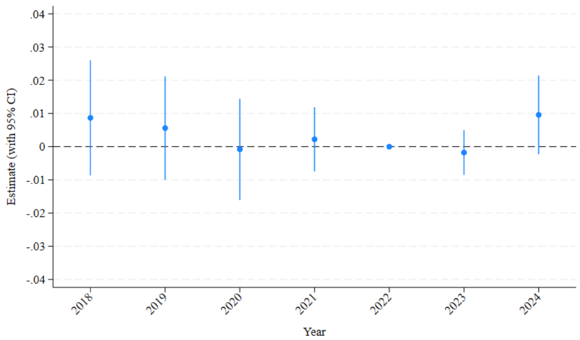
	Total		Tiers 1–2	
	No Trend (1)	Trend (2)	No Trend (3)	Trend (4)
Total	0.004	-0.025**	-0.039	-0.055
	(0.008)	(0.010)	(0.032)	(0.034)
	[0.5%]	[-3.0%]	[-7.2%]	[-10.0%]
Pell	0.009**	-0.008**	-0.026**	-0.020*
	(0.004)	(0.004)	(0.010)	(0.009)
	[2.0%]	[-1.8%]	[-13.7%]	[-10.5%]
State/Local	-0.062***	-0.050***	-0.068***	-0.054**
	(0.012)	(0.013)	(0.014)	(0.023)
	[-15.9%]	[-13.0%]	[-24.0%]	[-19.2%]
Institutional	0.048***	0.000	-0.035*	-0.074***
	(0.011)	(0.012)	(0.019)	(0.021)
	[8.7%]	[-0.1%]	[-8.0%]	[-16.9%]

	Tiers 3–4		Non-Selective	
	No Trend (5)	Trend (6)	No Trend (7)	Trend (8)
Total	0.034	0.006	0.002	-0.027***
	(0.028)	(0.042)	(0.008)	(0.010)
	[4.5%]	[0.8%]	[0.3%]	[-3.3%]
Pell	0.028*	-0.005	0.008*	-0.007*
	(0.014)	(0.014)	(0.004)	(0.004)
	[11.9%]	[-2.2%]	[1.9%]	[-1.6%]
State/Local	-0.001	-0.003	-0.069***	-0.056***
	(0.022)	(0.019)	(0.014)	(0.015)
	[-0.3%]	[-1.0%]	[-17.7%]	[-14.4%]
Institutional	0.075**	0.042	0.050***	-0.001
	(0.034)	(0.035)	(0.012)	(0.013)
	[11.8%]	[6.6%]	[9.0%]	[-0.2%]

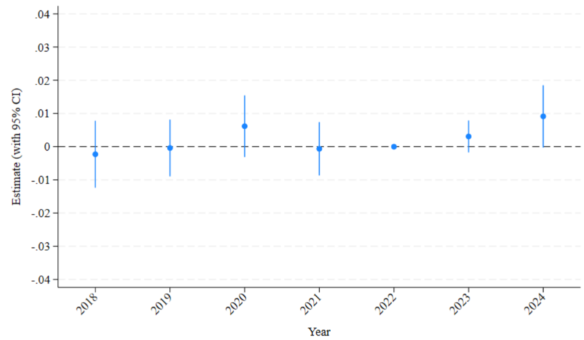
Notes: Author's estimation of equation (3) using 2018-2024 IPEDS data. Each cell reports a separate regression estimated at the institution level and includes institution and year fixed effects. Standard errors, clustered at the institution level, are reported in parentheses. Percent changes are reported in brackets and are calculated relative to the pre-2024 treatment group mean. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Figure 1: Selectivity Tiers 1–2 vs 3–4 Race/Ethnicity Event Studies

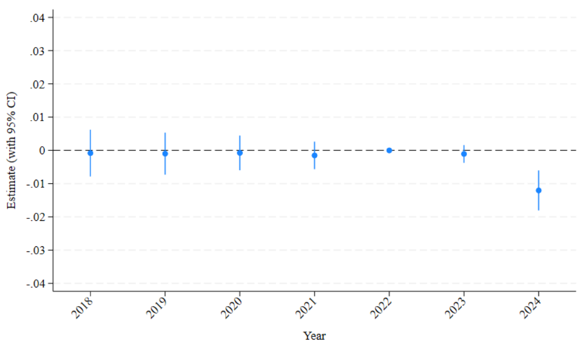
(a) Share White



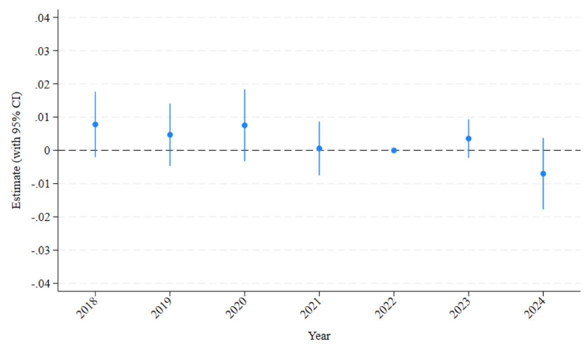
(b) Share Asian



(c) Share Black



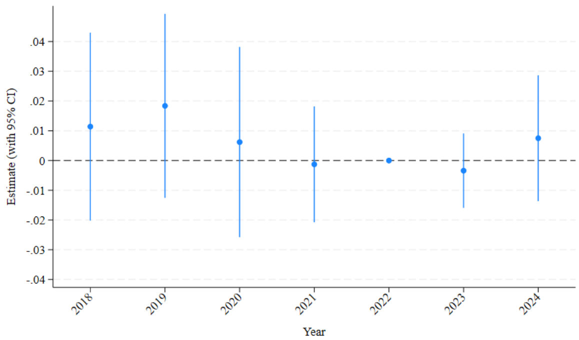
(d) Share Hispanic



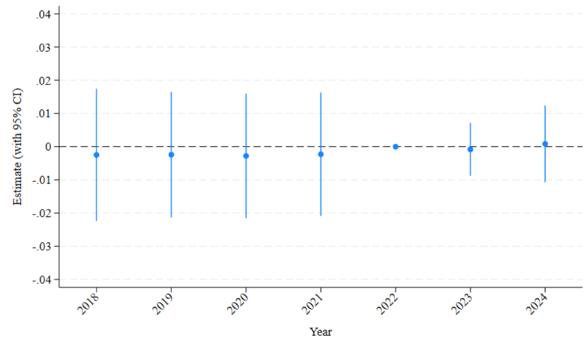
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 2: Selectivity Tiers 1–2 vs 3–4 Race/Ethnicity Event Studies (Public)

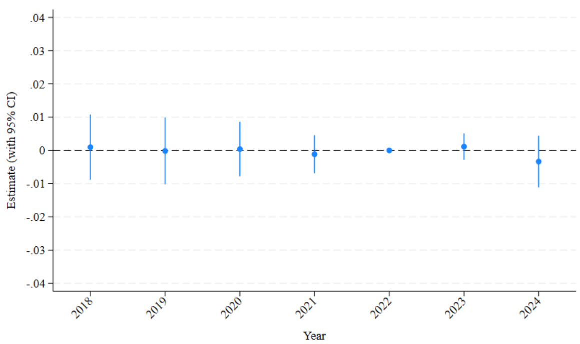
(a) Share White



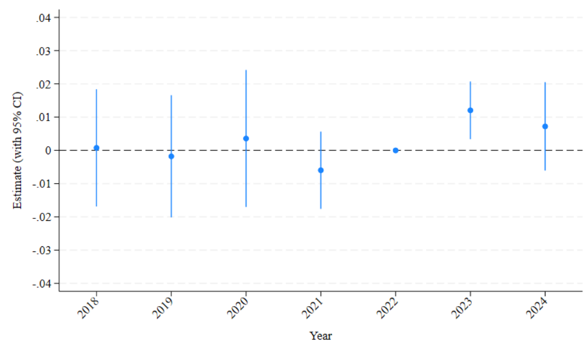
(b) Share Asian



(c) Share Black



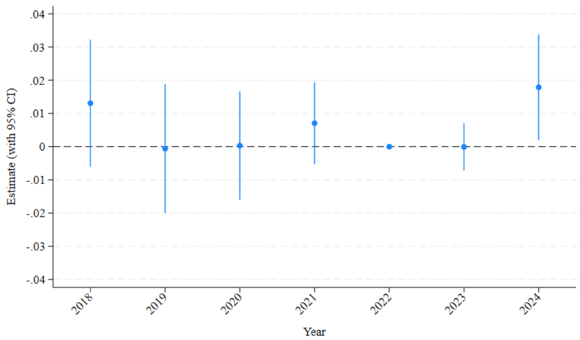
(d) Share Hispanic



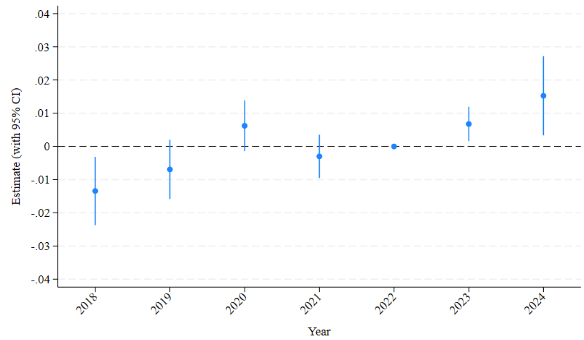
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 3: Selectivity Tiers 1– 2 vs 3–4 Race/Ethnicity Event Studies (Private)

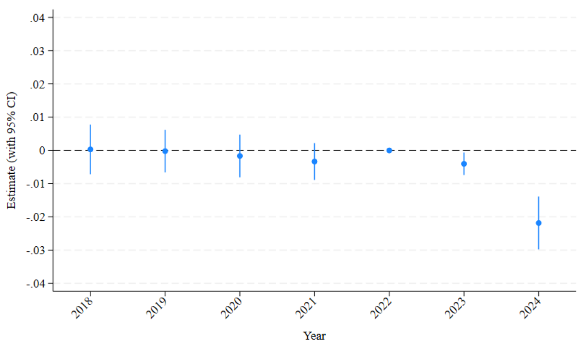
(a) Share White



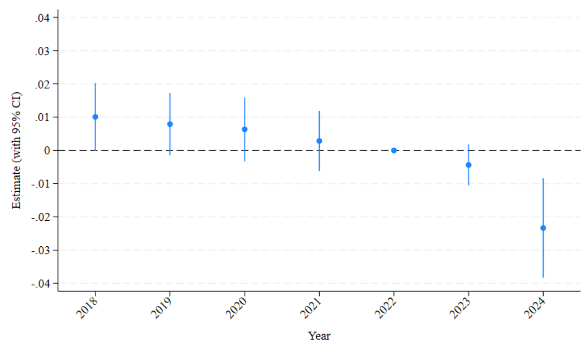
(b) Share Asian



(c) Share Black



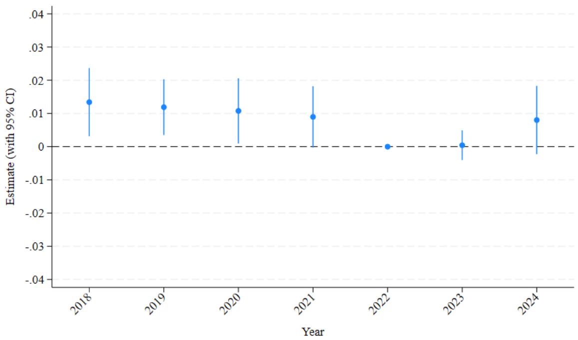
(d) Share Hispanic



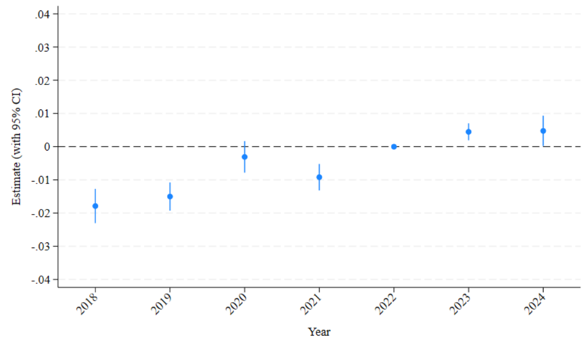
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 4: Selectivity Tiers 1-4 vs Non-Selective Race/Ethnicity Event Studies

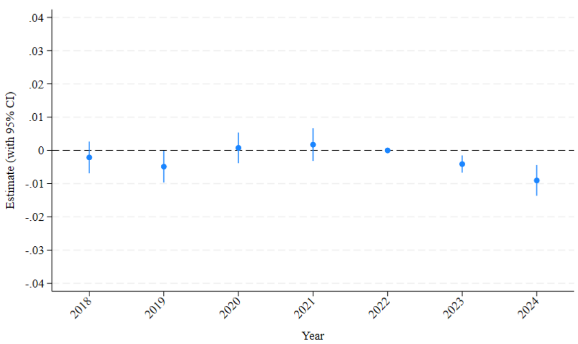
(a) Share White



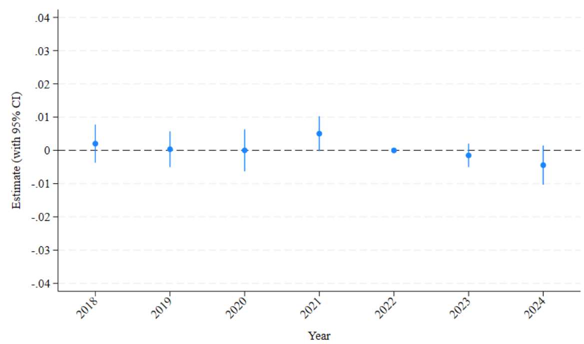
(b) Share Asian



(c) Share Black



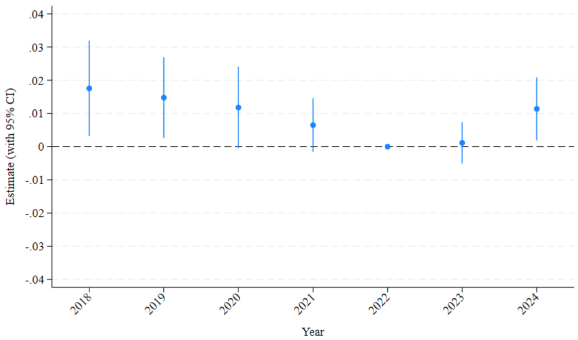
(d) Share Hispanic



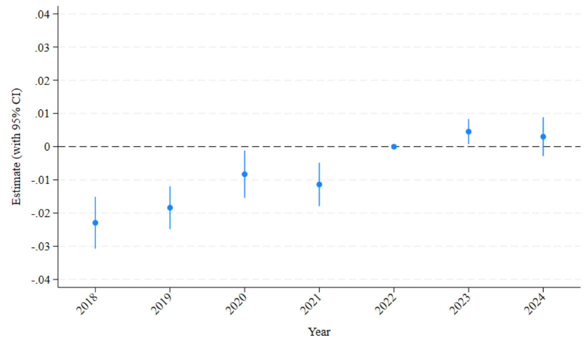
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 5: Selectivity Tiers 1-4 vs Non-Selective Race/Ethnicity Event Studies (Public)

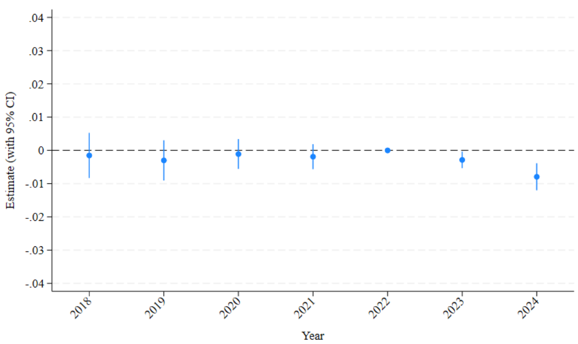
(a) Share White



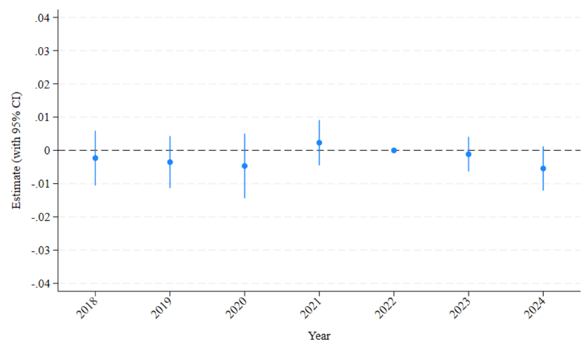
(b) Share Asian



(c) Share Black



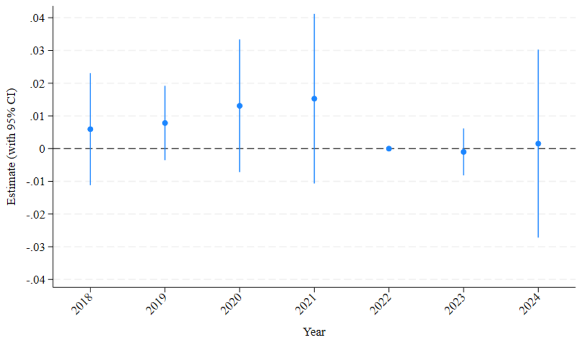
(d) Share Hispanic



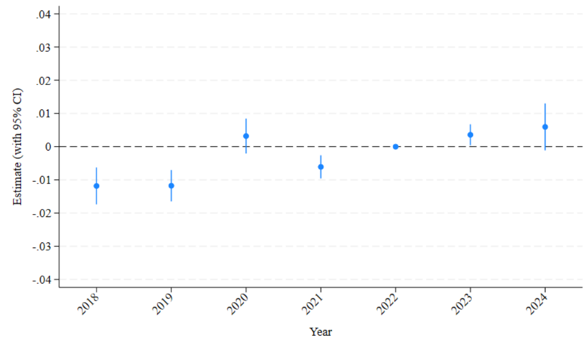
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 6: Selectivity Tiers 1-4 vs All Race/Ethnicity Event Studies (Private)

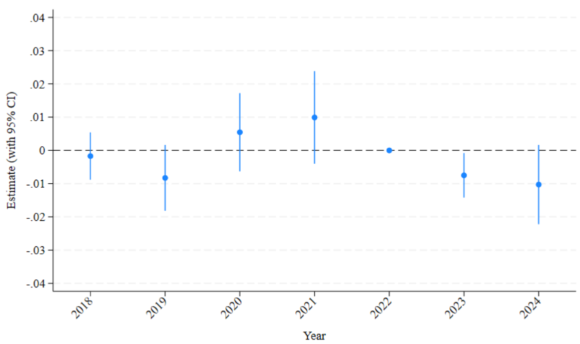
(a) Share White



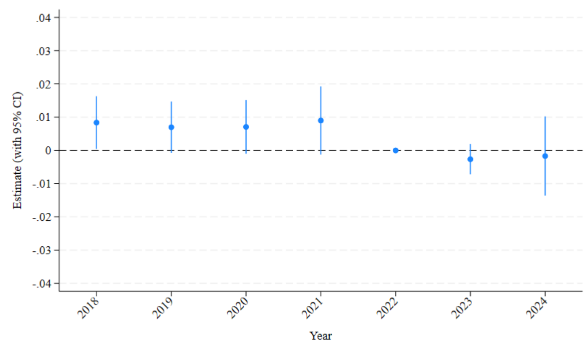
(b) Share Asian



(c) Share Black



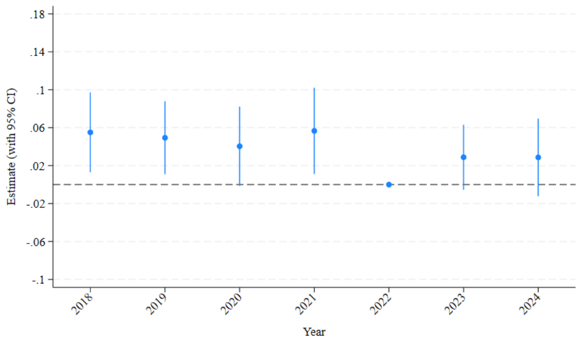
(d) Share Hispanic



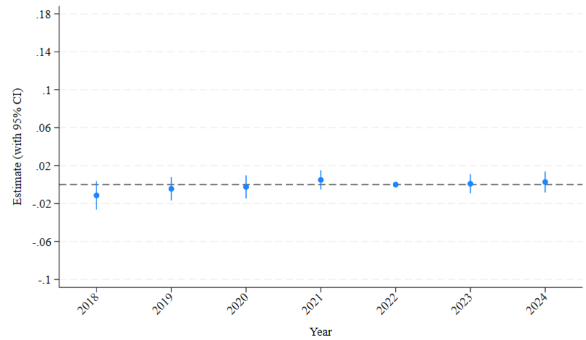
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 7: Selectivity Tiers 1–2 vs 3–4 Grant Aid Event Studies

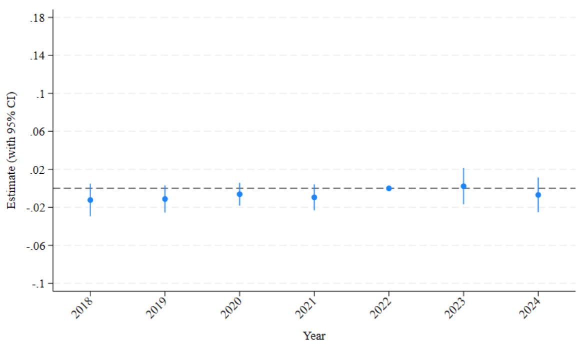
(a) Share Total Grants



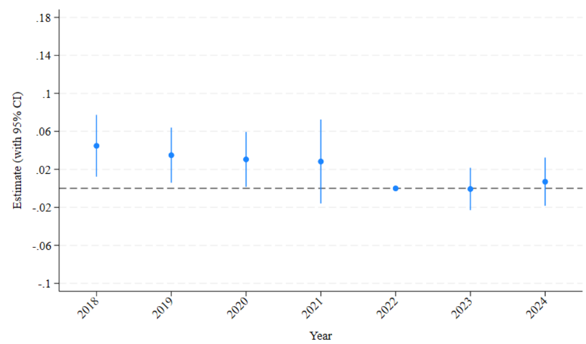
(b) Share Pell Grants



(c) Share State/Local Grants



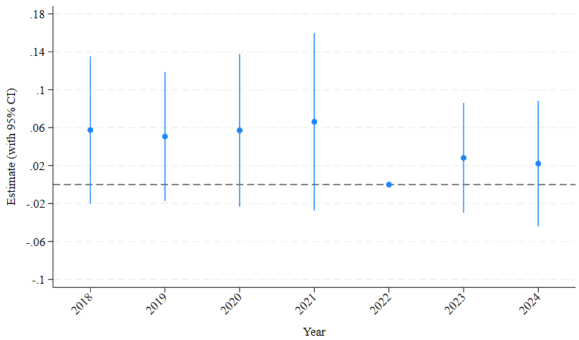
(d) Share Institutional Grants



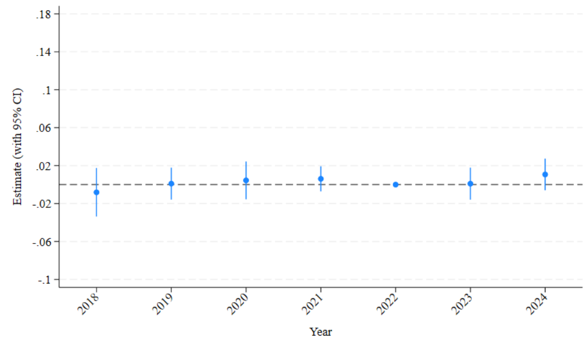
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 8: Selectivity Tiers 1–2 vs 3–4 Grant Aid Event Studies (Public)

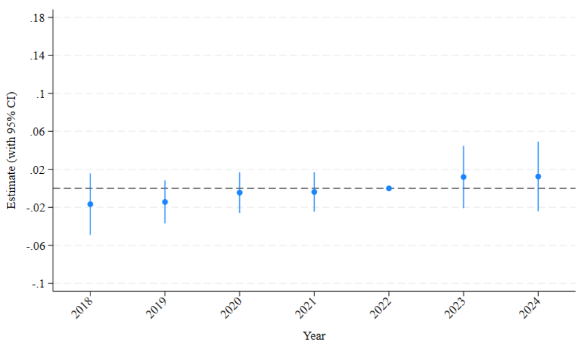
(a) Share Total Grants



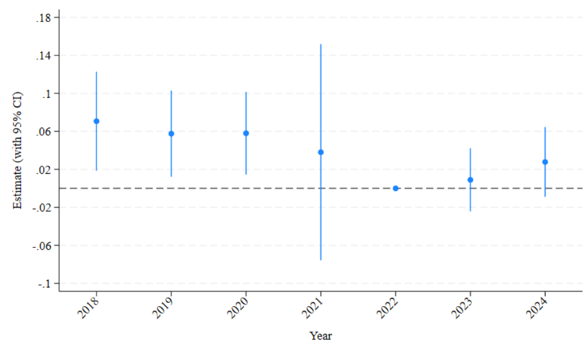
(b) Share Pell Grants



(c) Share State/Local Grants



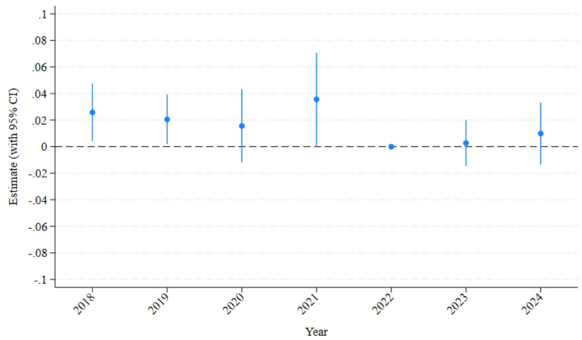
(d) Share Institutional Grants



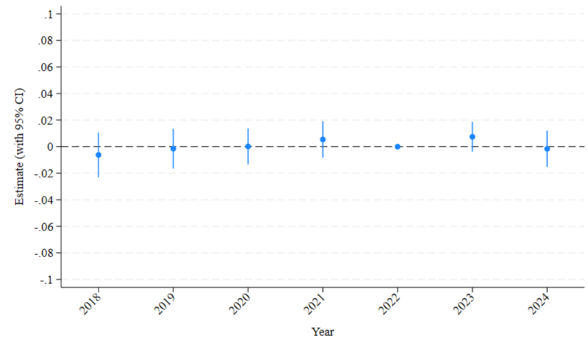
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 9: Selectivity Tiers 1– 2 vs 3–4 Grant Aid Event Studies (Private)

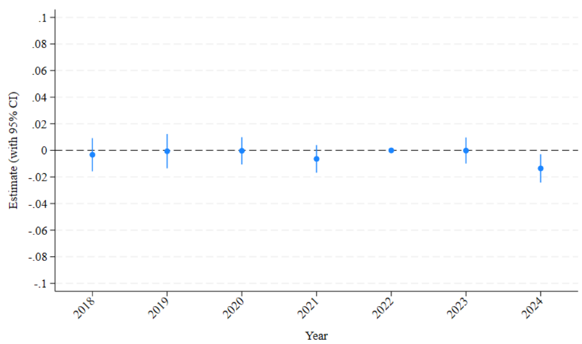
(a) Share Total Grants



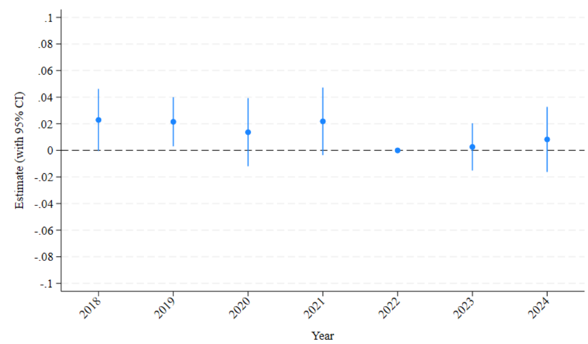
(b) Share Pell Grants



(c) Share State/Local Grants



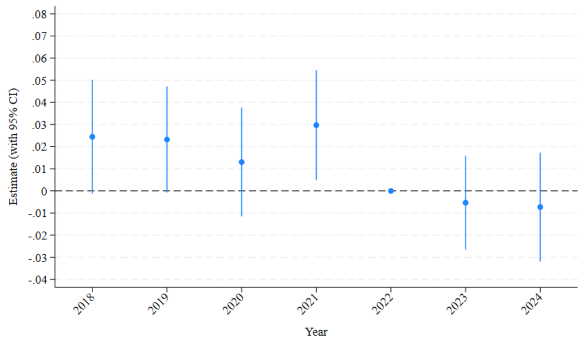
(d) Share Institutional Grants



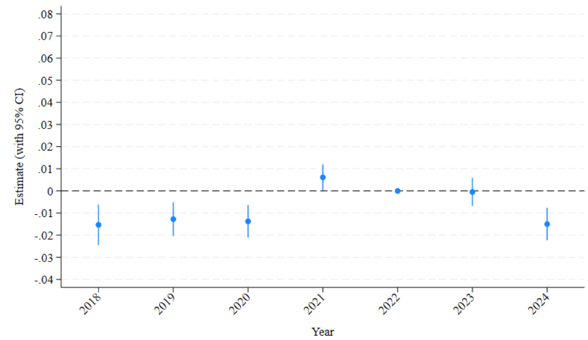
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 10: Selectivity Tiers 1-4 vs Non-Selective Grant Aid Event Studies

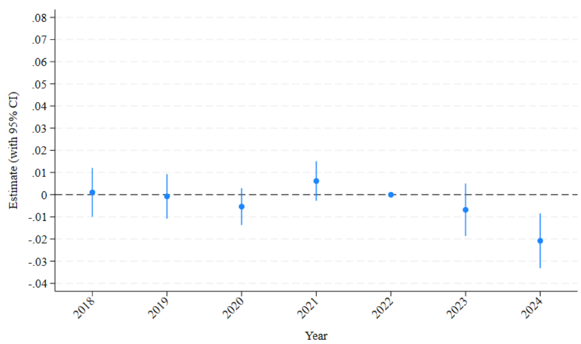
(a) Share Total Grants



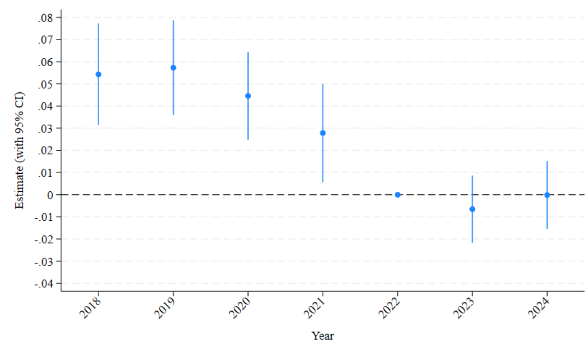
(b) Share Pell Grants



(c) Share State/Local Grants



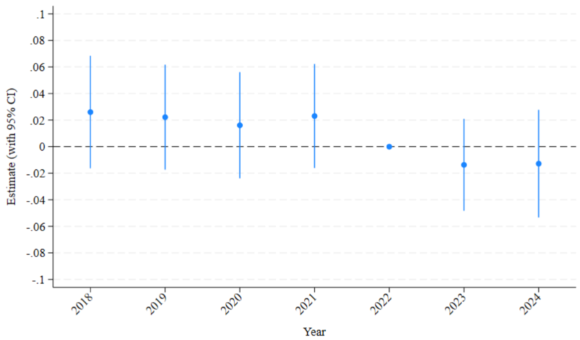
(d) Share Institutional Grants



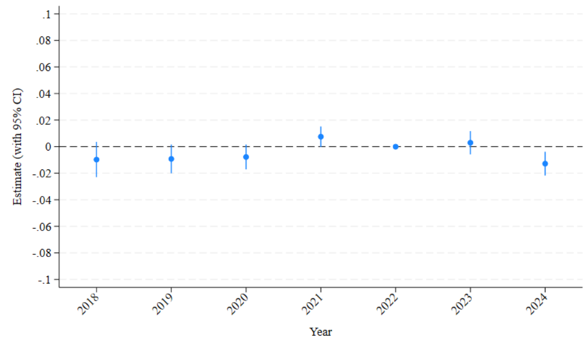
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 11: Selectivity Tiers 1-4 vs Non-Selective Grant Aid Event Studies (Public)

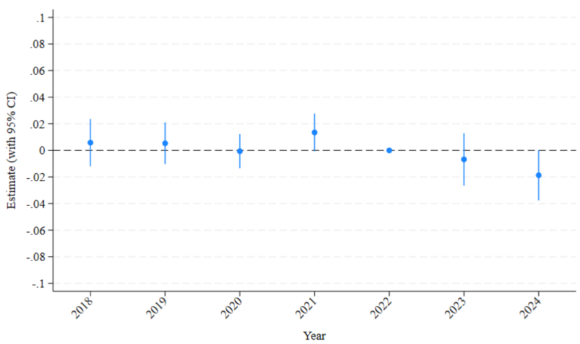
(a) Share Total Grants



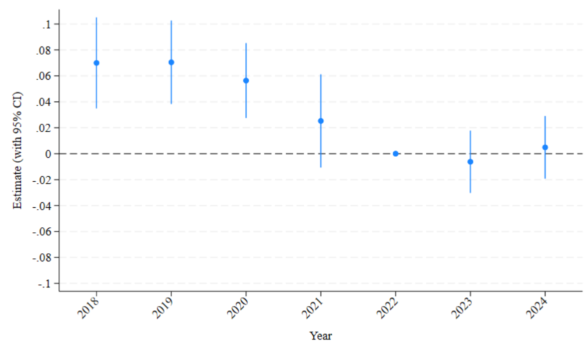
(b) Share Pell Grants



(c) Share State/Local Grants



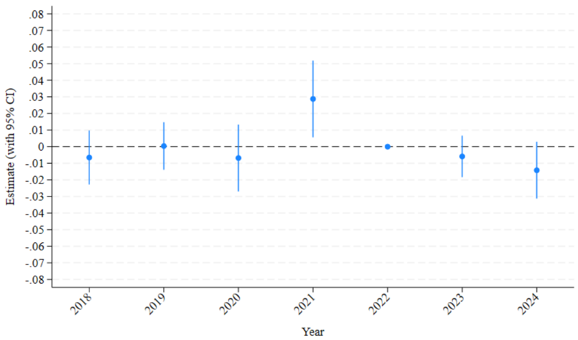
(d) Share Institutional Grants



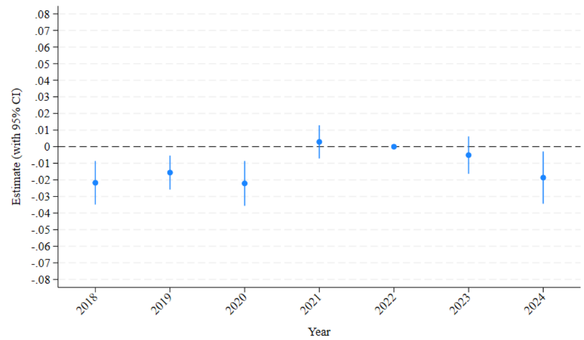
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure 12: Selectivity Tiers 1-4 vs Non-Selective Grant Aid Event Studies (Private)

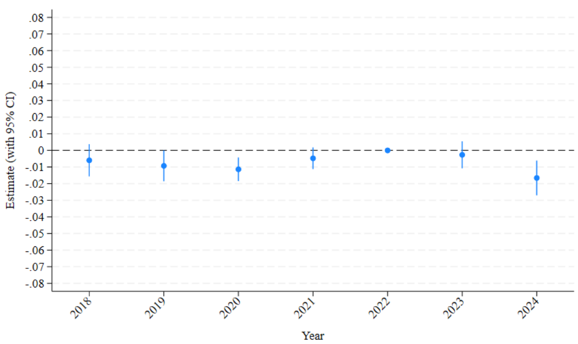
(a) Share Total Grants



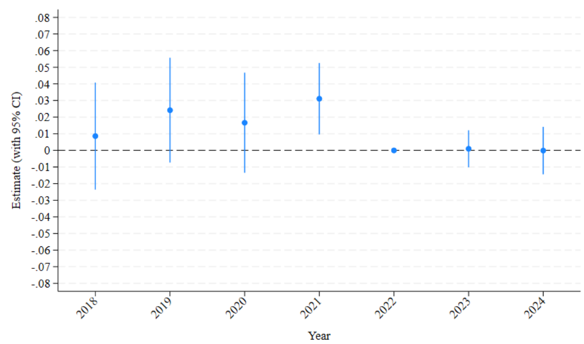
(b) Share Pell Grants



(c) Share State/Local Grants



(d) Share Institutional Grants



Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

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Appendix A: Supplementary Tables and Figures

Table A1: Pre and Post Treatment Means for Race/Ethnicity by Pre-Ban Status

States without Pre-Ban

	Total			Tiers 1–2		
	2018-2023	2024	Difference	2018-2023	2024	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
White	0.531	0.491	-0.039	0.554	0.534	-0.020
Asian	0.045	0.046	0.001	0.155	0.172	0.017
Black	0.156	0.161	0.005	0.058	0.051	-0.007
Hispanic	0.151	0.171	0.019	0.105	0.110	0.005
Other	0.117	0.131	0.014	0.128	0.133	0.006

	Tiers 3–4			Non-Selective		
	2018-2023	2024	Difference	2018-2023	2024	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
White	0.571	0.531	-0.040	0.528	0.489	-0.040
Asian	0.141	0.146	0.005	0.038	0.039	0.000
Black	0.058	0.063	0.005	0.163	0.168	0.005
Hispanic	0.129	0.143	0.014	0.153	0.173	0.020
Other	0.102	0.117	0.015	0.117	0.132	0.015

Table A1: Pre and Post Treatment Means for Race/Ethnicity by Pre-Ban Status *(Continued)*

States with Pre-Ban

	Total			Tiers 1–2		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)
White	0.438	0.391	-0.047	0.395	0.343	-0.052
Asian	0.075	0.067	-0.008	0.227	0.222	-0.005
Black	0.079	0.078	0.000	0.035	0.039	0.004
Hispanic	0.257	0.291	0.035	0.177	0.207	0.029
Other	0.151	0.172	0.021	0.165	0.189	0.024

	Tiers 3–4			Non-Selective		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)
White	0.338	0.321	-0.017	0.442	0.394	-0.048
Asian	0.204	0.204	-0.001	0.068	0.060	-0.008
Black	0.042	0.034	-0.008	0.081	0.080	0.000
Hispanic	0.245	0.267	0.022	0.259	0.294	0.035
Other	0.170	0.174	0.004	0.150	0.172	0.022

Notes: Author's tabulations of IPEDS data from 2018-2024 as described in the text. Tiers are defined using Barron's (2009) *Profiles of American Colleges*. Values represent enrollment shares for each racial group.

Table A2: Pre and Post Treatment Means in States without Pre Ban for Grant Aid

States without Pre-Ban

	Total			Tiers 1–2		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)
Total	0.818	0.835	0.017	0.549	0.549	0.000
Pell	0.435	0.451	0.016	0.188	0.197	0.008
State/Local	0.387	0.401	0.014	0.283	0.290	0.007
Institutional	0.554	0.623	0.069	0.439	0.476	0.037

	Tiers 3–4			Non-Selective		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)
Total	0.759	0.785	0.025	0.826	0.842	0.016
Pell	0.236	0.254	0.018	0.449	0.465	0.016
State/Local	0.346	0.368	0.022	0.391	0.405	0.014
Institutional	0.636	0.711	0.075	0.552	0.621	0.069

Table A2: Pre and Post Treatment Means in States without Pre Ban for Grant Aid *(Continued)*

	States with Pre-Ban					
	Total			Tiers 1–2		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)
Total	0.746	0.756	0.010	0.649	0.653	0.003
Pell	0.427	0.438	0.011	0.237	0.275	0.038
State/Local	0.397	0.474	0.077	0.360	0.385	0.025
Institutional	0.435	0.439	0.004	0.598	0.633	0.035
	Tiers 3–4			Non-Selective		
	2018-2023 (1)	2024 (2)	Difference (3)	2018-2023 (4)	2024 (5)	Difference (6)
Total	0.741	0.712	-0.029	0.748	0.759	0.011
Pell	0.299	0.283	-0.015	0.436	0.446	0.010
State/Local	0.608	0.622	0.014	0.390	0.471	0.081
Institutional	0.497	0.473	-0.024	0.429	0.434	0.005

Notes: Author's tabulations of IPEDS data from 2018-2024 as described in the text. Tiers are defined using Barron's (2009) *Profiles of American Colleges*. Values represent grant aid shares for each type.

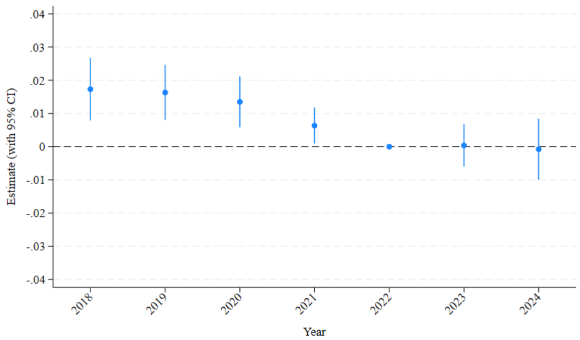
Table A3: Difference-in-Differences Estimates by Institution Selectivity on Race/Ethnicity & Grant Aid: Public Schools in States without a Pre-Ban

	Tiers 1–2 vs. 3–4		Tiers 1–4 vs. Non-Selective	
	No Trend	Trend	No Trend	Trend
	(1)	(2)	(3)	(4)
		Panel A: Race/Ethnicity		
White	0.014*	0.025*	0.002	0.018***
	(0.008)	(0.013)	(0.005)	(0.006)
	[2.6%]	[4.6%]	[0.4%]	[3.2%]
Asian	0.007	-0.003	0.014***	-0.010***
	(0.007)	(0.006)	(0.003)	(0.003)
	[4.7%]	[-1.7%]	[9.8%]	[-7.0%]
Black	-0.012**	-0.008**	-0.006***	-0.007***
	(0.005)	(0.004)	(0.003)	(0.003)
	[-21.1%]	[-13.5%]	[-10.8%]	[-12.3%]
Hispanic	-0.004	0.001	-0.007***	-0.006
	(0.004)	(0.009)	(0.002)	(0.004)
	[-4.0%]	[0.7%]	[-5.3%]	[-4.5%]
Other	-0.005	-0.016**	-0.003	0.005
	(0.005)	(0.006)	(0.004)	(0.005)
	[-4.1%]	[-12.2%]	[-3.1%]	[4.4%]
		Panel B: Grant Aid		
Total	-0.044	-0.014	-0.022	0.005
	(0.031)	(0.034)	(0.015)	(0.020)
	[-8.1%]	[-2.6%]	[-3.1%]	[0.7%]
Pell	-0.007	-0.001	-0.010**	-0.021***
	(0.009)	(0.006)	(0.005)	(0.004)
	[-3.5%]	[-0.3%]	[4.3%]	[-9.6%]
State/Local	-0.008	-0.021**	-0.008	-0.004
	(0.012)	(0.010)	(0.008)	(0.006)
	[-3.0%]	[-7.6%]	[-2.3%]	[-1.1%]
Institutional	-0.043*	0.001	-0.034**	0.032**
	(0.024)	(0.021)	(0.016)	(0.013)
	[-9.7%]	[-0.2%]	[-5.7%]	[5.5%]

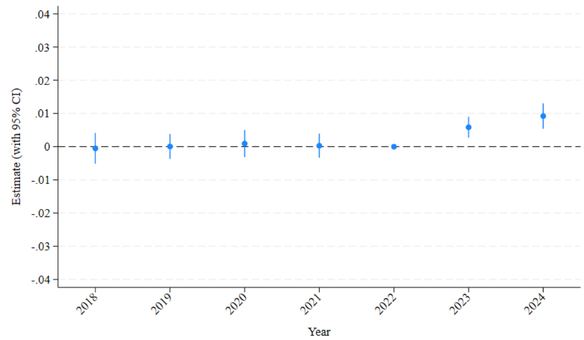
Notes: Author's estimation of equation (3) using 2018-2024 IPEDS data. Each cell reports a separate regression estimated at the institution level and includes institution and year fixed effects. Standard errors, clustered at the institution level, are reported in parentheses. Percent changes are reported in brackets and are calculated relative to the pre-2024 treatment group mean. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Figure A1: Pre-Ban States Event Studies by Race/Ethnicity

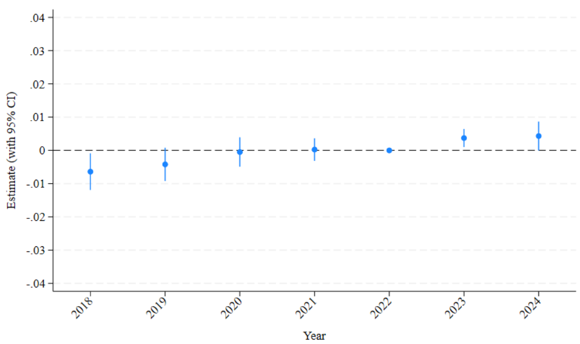
(a) Share White



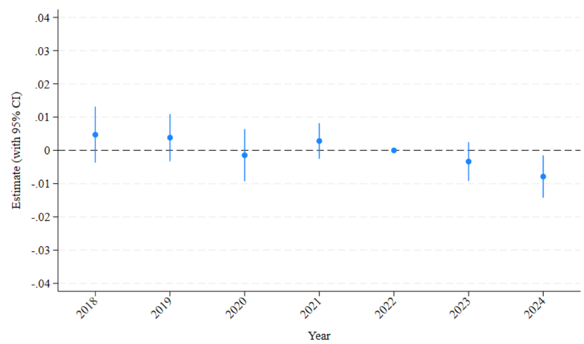
(b) Share Asian



(c) Share Black



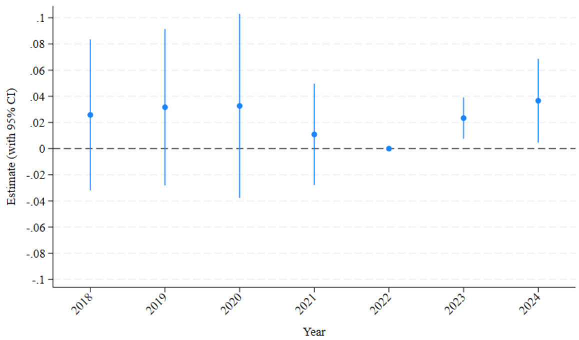
(d) Share Hispanic



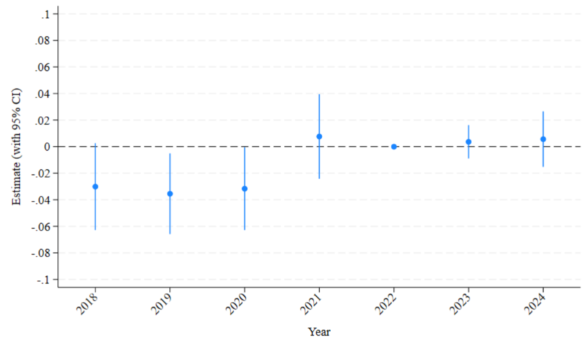
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure A2: Pre-Ban States Event Studies Tiers 1–2 by Race/Ethnicity

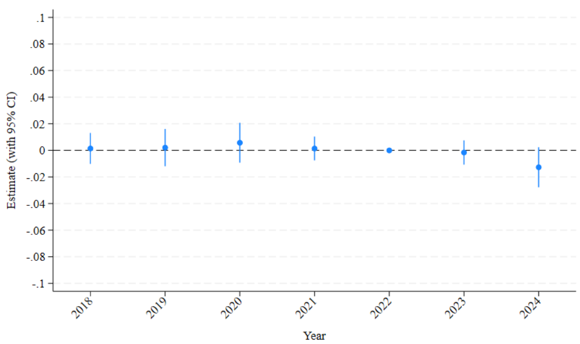
(a) Share White



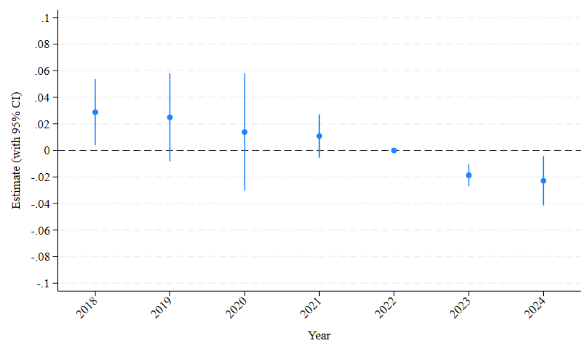
(b) Share Asian



(c) Share Black



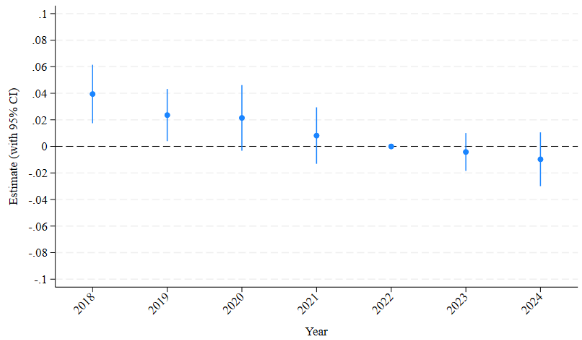
(d) Share Hispanic



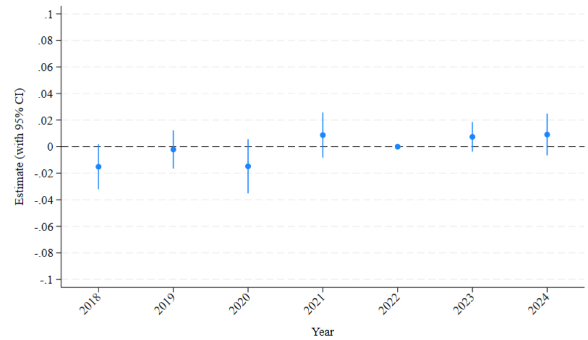
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure A3: Pre-Ban States Event Studies Tiers 3–4 by Race/Ethnicity

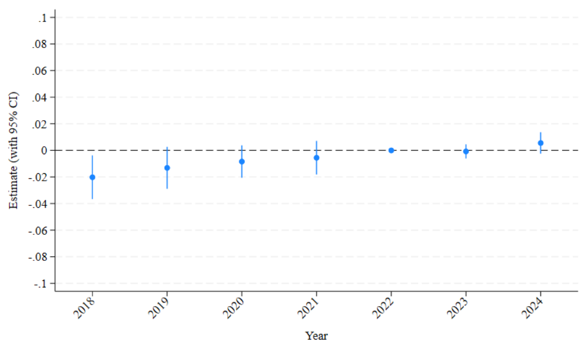
(a) Share White



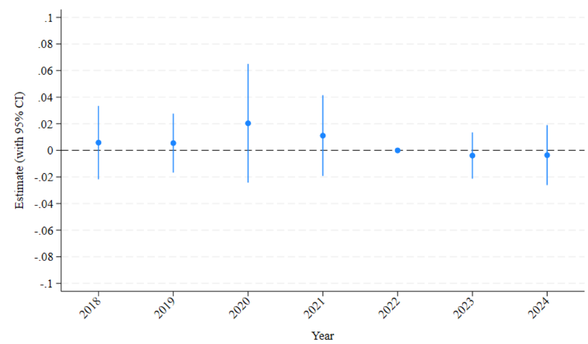
(b) Share Asian



(c) Share Black



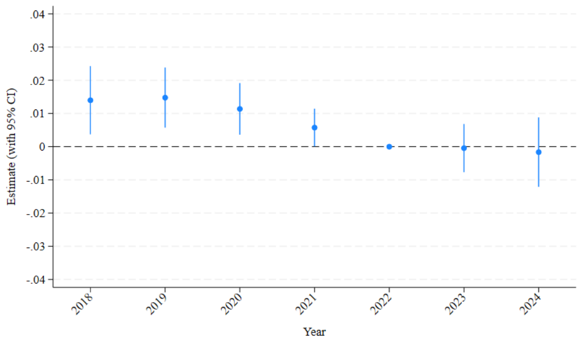
(d) Share Hispanic



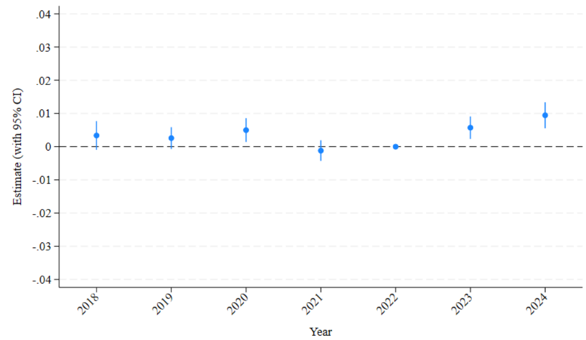
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure A4: Pre-Ban States Event Studies Non-Selective by Race/Ethnicity

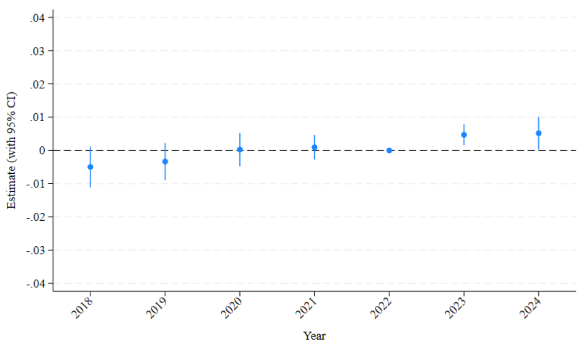
(a) Share White



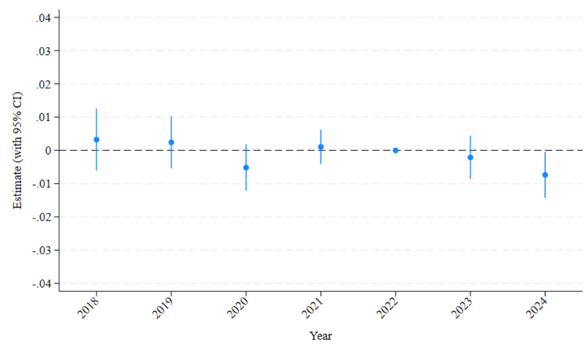
(b) Share Asian



(c) Share Black



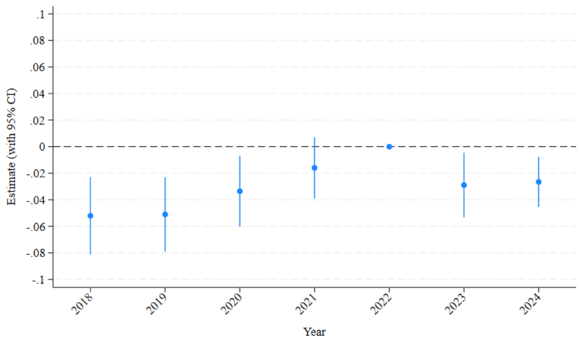
(d) Share Hispanic



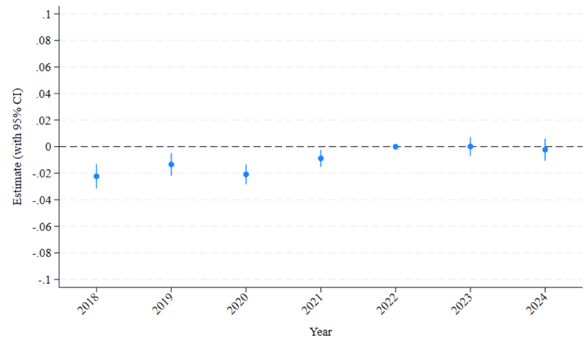
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of racial/ethnic shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure A5: Pre-Ban States Event Studies by Grant Aid

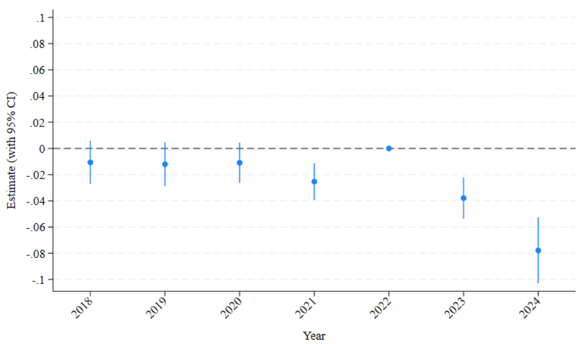
(a) Share Total Grants



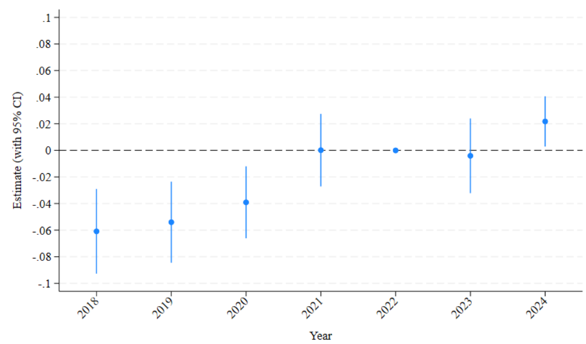
(b) Share Pell Grants



(c) Share State/Local Grants



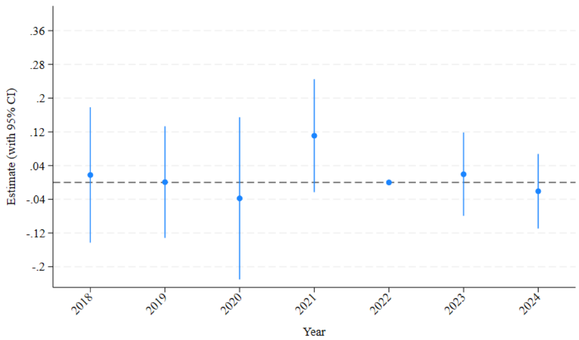
(d) Share Institutional Grants



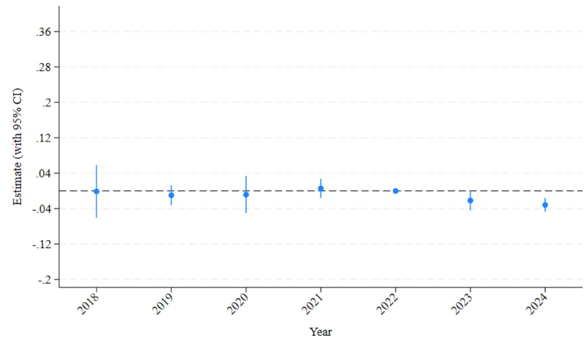
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure A6: Pre-Ban States Event Studies Tiers 1–2 by Grant Aid

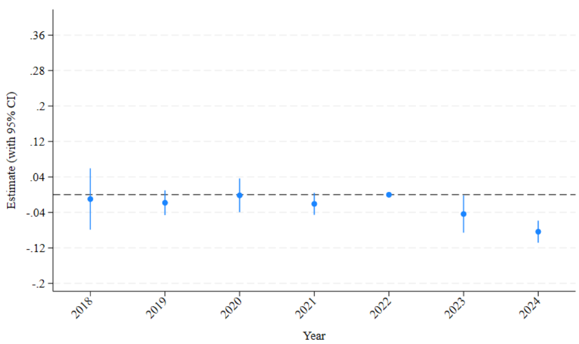
(a) Share Total Grants



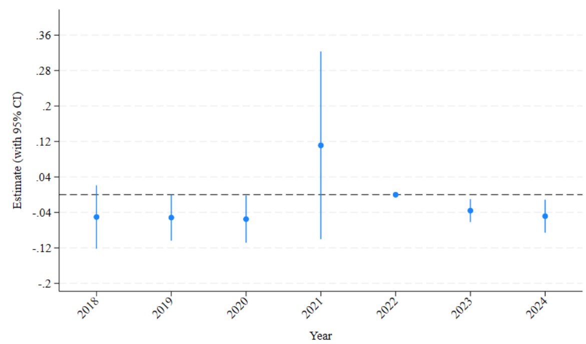
(b) Share Pell Grants



(c) Share State/Local Grants



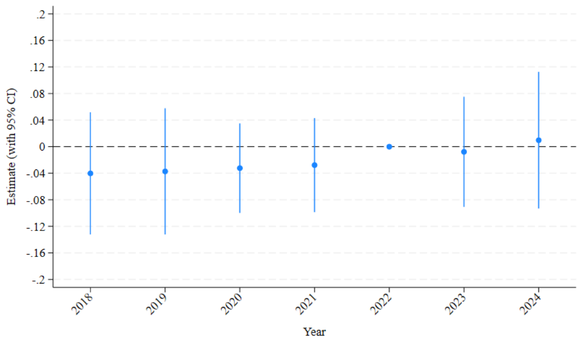
(d) Share Institutional Grants



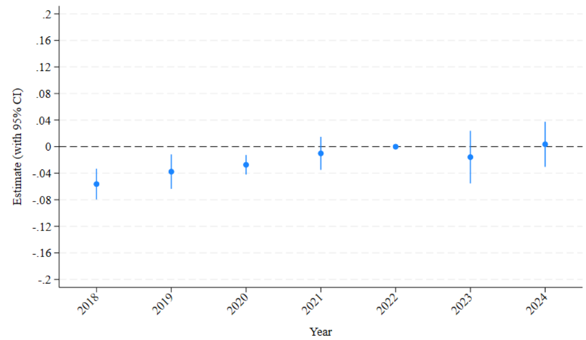
Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure A7: Pre-Ban States Event Studies Tiers 3–4 by Grant Aid

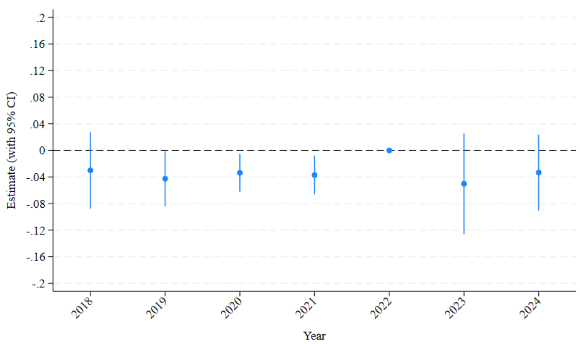
(a) Share Total Grants



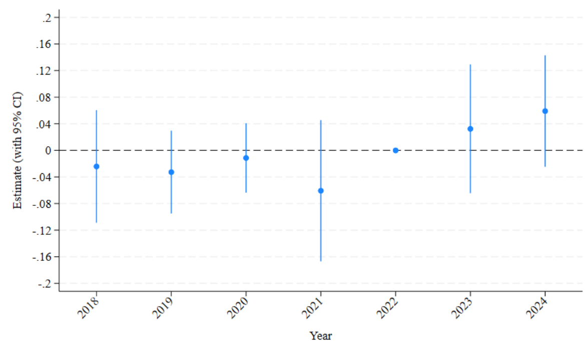
(b) Share Pell Grants



(c) Share State/Local Grants

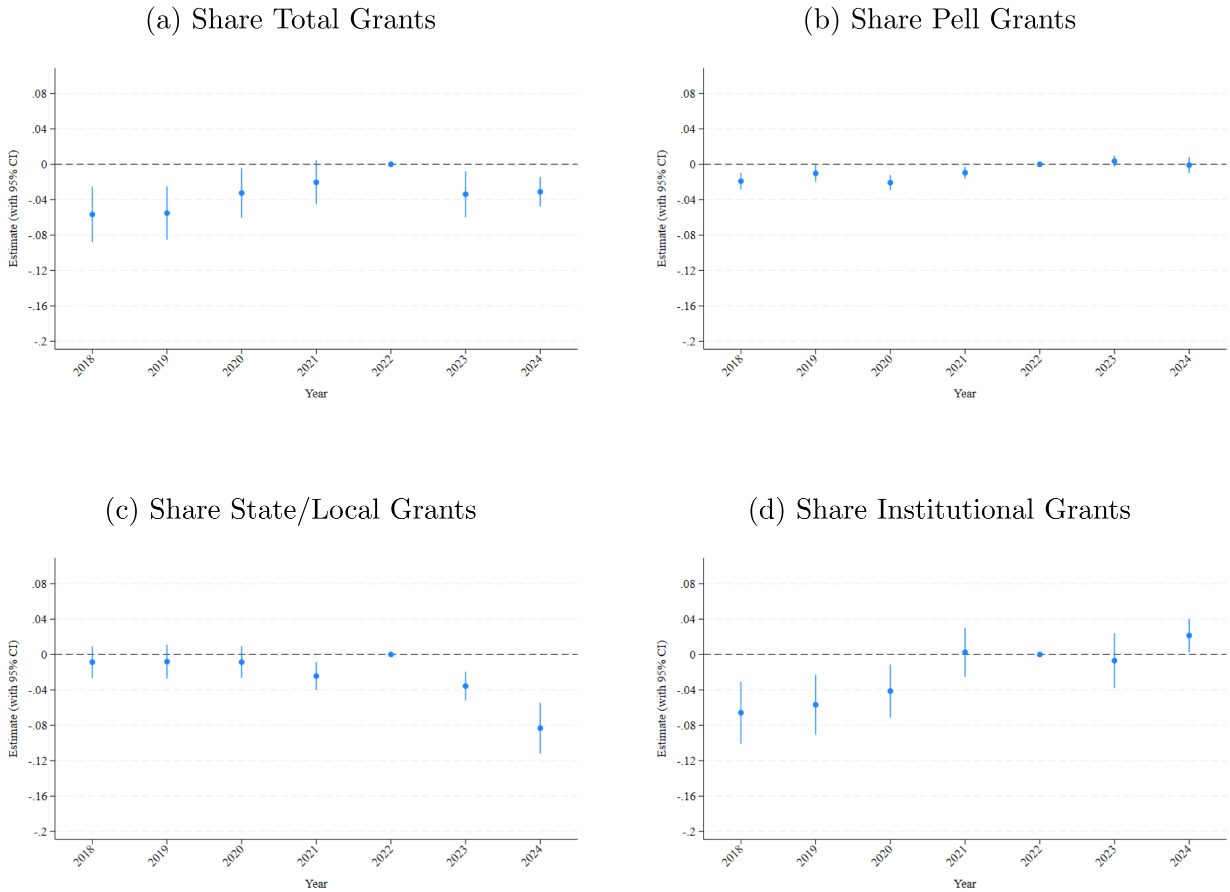


(d) Share Institutional Grants



Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.

Figure A8: Pre-Ban States Event Studies Non-Selective by Grant Aid



Notes: Estimation of equation (4) using 2018-2024 IPEDS data. This figure plots event-study estimates of grant aid distribution shares relative to the omitted year (2022). The y-axis shows estimated changes in group shares. Error bars show 95% confidence intervals based on standard errors that are clustered at the institution level.