The Admissions Illusion: SFFA v. Harvard Changed Who Goes to College, but Not How You Think

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Abstract

The Supreme Court's 2023 ban on race-conscious admissions was expected to dramatically reduce minority enrollment at selective universities. Using the universe of 1,835 U.S. institutions from IPEDS (2017–2024), I exploit continuous variation in pre-SFFA selectivity as a measure of treatment intensity. The headline finding obscures the real story: URM enrollment shares declined modestly at selective schools (-0.67 percentage points, $p = 0.044$), but the dominant compositional shift was a large increase in Asian enrollment (1.04 pp, $p < 0.001$) and decline in White enrollment (-1.63 pp, $p = 0.012$). A prior-ban-state placebo confirms the effect operates only where SFFA binds ($\beta = 0.01$, $p = 0.98$). The "admissions illusion" is that the policy debate centered on Black and Hispanic enrollment, while the largest reallocation occurred between Asian and White students.

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1. Introduction

On June 29, 2023, the Supreme Court struck down race-conscious college admissions in *Students for Fair Admissions v. Harvard*. The decision, which many called the end of affirmative action in American higher education, prompted predictions of catastrophic declines in Black and Hispanic enrollment at the nation's most selective universities. Amicus briefs from 60 leading research universities warned that race-neutral alternatives would be "wholly inadequate" (Brief of 60 Universities, 2023). The media followed suit: headlines tracked Black enrollment at individual elite campuses, parsing each percentage point as evidence of a new era of exclusion.

The predictions were wrong—or rather, they were looking at the wrong margin.

This paper uses the complete universe of U.S. degree-granting institutions from the Integrated Postsecondary Education Data System (IPEDS) to evaluate SFFA's impact on the racial composition of undergraduate enrollment. The empirical strategy exploits continuous variation in institutional selectivity—measured by the inverse of the pre-SFFA average admission rate—as a proxy for treatment intensity. Institutions that admitted fewer than 25 percent of applicants were overwhelmingly more likely to have practiced race-conscious admissions; open-access institutions that admitted most applicants had no reason to. The design compares within-institution changes in racial enrollment shares before and after the Fall 2024 enrollment cycle, the first to fully incorporate SFFA, weighting by this continuous intensity measure.

Three findings emerge. First, underrepresented minority (URM) enrollment shares at selective institutions declined, but the effect is modest: a 0.67 percentage point reduction for a one-unit increase in treatment intensity ($p = 0.044$). This translates to a standardized effect size of -0.029—small by conventional benchmarks. Neither the Black nor Hispanic share declines are individually statistically significant. Second—and this is the paper's central contribution—the dominant compositional shift was between Asian and White students. Asian enrollment shares rose by 1.04 percentage points at selective schools ($p < 0.001$, SDE $= 0.169$, "large positive"), while White shares fell by 1.63 points ($p = 0.012$, SDE $= -0.067$, "moderate negative"). Third, the entire effect is concentrated in states that had not previously banned affirmative action: a prior-ban-state placebo produces a near-perfect null ($\beta = 0.013$, $p = 0.98$), while the effect in non-ban states is larger and significant ($\beta = -0.843$, $p = 0.03$).

These results reframe the policy debate. The conventional narrative—that SFFA would decimate minority enrollment—treats admissions as a two-player game between universities and underrepresented applicants. The data reveal a three-player game. When race-conscious admissions was practiced, it constrained not only URM applicants' chances but also those

of Asian applicants, whose enrollment shares were held below what their academic credentials would predict. Removing the constraint reshuffled the deck, primarily between Asian and White students, while URM enrollment proved surprisingly resilient—likely because universities rapidly deployed race-neutral alternatives such as socioeconomic preferences, test-optional policies, and targeted recruitment (Kahlenberg, 2023).

The paper contributes to a growing literature evaluating SFFA's effects. Three contemporaneous NBER papers use proprietary data from individual institutions or the Common Application to study early post-SFFA admissions (Bleemer, 2024; Arcidiacono and Lovenheim, 2024; Dynarski et al., 2024). This paper complements them by providing the first universe-level analysis using IPEDS, which covers all 1,835 institutions with admission data rather than a selected subset. The universe panel enables the continuous treatment intensity design, the selectivity-tier cascade analysis, and the prior-ban-state placebo—tests that require broad institutional coverage and are infeasible with data from individual campuses.

The results also speak to the broader economics of discrimination in competitive markets. The finding that the largest enrollment reallocation occurred between Asian and White students—not between White and URM students—is consistent with models of constrained optimization where removing a binding constraint produces adjustments at unexpected margins (Becker, 1957; Fryer and Loury, 2005). The "admissions illusion" is a specific instance of a more general phenomenon: policy debates focus on the intended beneficiaries of an intervention while neglecting the margins where the largest adjustments actually occur.

2. Institutional Background

Race-conscious admissions before SFFA. Since *Grutter v. Bollinger* (2003), universities could consider race as one factor among many in holistic admissions review. The practice was widespread among selective institutions: surveys estimated that 60–70 percent of four-year institutions with admission rates below 50 percent used some form of race-conscious admissions (Espinosa et al., 2019). Open-access institutions—community colleges and less selective four-year schools—had little reason to consider race, as they admitted the vast majority of applicants regardless.

Prior state-level bans. Nine states prohibited race-conscious admissions before SFFA through ballot initiatives, executive orders, or legislation: California (1996), Washington (1998), Florida (1999), Michigan (2006), Nebraska (2008), Arizona (2010), New Hampshire (2012), Oklahoma (2012), and Idaho (2020). Research on these bans, particularly California's Proposition 209, documented declines in URM enrollment at flagship universities but found

that system-wide effects were smaller as students redistributed across campuses (Bleemer, 2022; Hinrichs, 2012; Backes, 2012).

The SFFA decision. The Court's ruling in June 2023 applied nationally and immediately. Universities had roughly one admissions cycle to adjust: applications for Fall 2024 were reviewed under the new regime. Many institutions responded by eliminating race questions from applications, expanding socioeconomic diversity initiatives, adopting test-optional policies, and increasing targeted recruitment (Kahlenberg, 2023). The speed of these adjustments is notable—they were implemented in months, not years—and may explain why the enrollment effects appear more muted than the state-level precedents would predict.

3. Data

The analysis draws on three IPEDS survey components. The *12-Month Enrollment* survey (EFFY) provides unduplicated headcount enrollment by race and ethnicity for all Title IV degree-granting institutions, covering 2017–2024. This is the primary outcome source, offering eight years of panel data with the Fall 2024 cycle as the first full post-SFFA observation. The *Admissions* survey provides application, admission, and enrollment counts for 2019–2023, from which I compute the pre-SFFA average admission rate that serves as the treatment intensity measure. The *Institutional Characteristics* survey provides time-invariant attributes including HBCU status, sector (public/private), and state.

Sample construction. I restrict to institutions with admission rate data for at least two of the four pre-SFFA years (2019–2022) and undergraduate enrollment of at least 100. This yields a panel of 1,835 institutions observed over eight years (14,239 institution-year observations). The sample includes 78 highly selective institutions (admit rate below 25 percent), 163 selective institutions (25–50 percent), 647 moderately selective (50–75 percent), and 937 open-access (above 75 percent). Sixty-five HBCUs and 319 institutions in prior-ban states are retained for placebo analysis.

The restriction to institutions with admission data excludes approximately 4,200 schools—primarily community colleges and for-profit institutions that practice open admissions and do not report to the IPEDS Admissions survey. These excluded institutions have near-universal acceptance and would receive treatment intensity near zero, contributing little variation to the identifying interaction. Pre-SFFA racial composition at excluded schools (mean URM share: 35.2 percent) is higher than the analysis sample (27.9 percent), reflecting the concentration of minority enrollment at open-access institutions. Importantly, the continuous intensity design relies on within-sample variation across the selectivity spectrum, not extrapolation to

institutions outside the sample.

Treatment intensity. I define treatment intensity as

$$\text{Intensity}_i = 1 - \overline{\text{AdmitRate}}_{i,\,2019\text{–}2022}/100.$$

A school admitting 15 percent of applicants receives intensity 0.85; one admitting 85 percent receives 0.15. This continuous measure avoids arbitrary selectivity bins and exploits the full institutional spectrum. The identifying assumption is that more selective institutions were more likely to have practiced race-conscious admissions, an assumption supported by survey evidence and the prior-ban-state literature.

4. Empirical Strategy

The main specification is a two-way fixed effects model with continuous treatment intensity:

$$Y_{it} = \alpha_i + \gamma_t + \beta\,(\text{Intensity}_i \times \text{Post}_t) + \varepsilon_{it} \tag{1}$$

where Y_{it} is the enrollment share of a racial group (URM, Black, Hispanic, Asian, or White) at institution i in year t, α_i are institution fixed effects, γ_t are year fixed effects, and $\text{Post}_t = \mathbf{1}\{t \geq 2024\}$. The coefficient β captures the differential change in enrollment share at more-selective institutions after SFFA. Standard errors are clustered at the state level to account for correlation in admissions practices and policy environments within states.

Identification. Because SFFA applies nationally at a single point in time, this is not a staggered adoption design. The identification comes from cross-sectional variation in treatment intensity interacted with the common temporal shock. The parallel trends assumption requires that, absent SFFA, enrollment shares at more- and less-selective institutions would have followed parallel trajectories. I assess this with an event study interacting intensity with year indicators (reference: 2022) and report the full coefficient vector in Table 3.

What this comparison identifies and what it does not. The continuous intensity design identifies the *differential* effect of SFFA on more-selective relative to less-selective institutions. It does not identify the *level* effect on any single institution. A finding of $\beta = 0$ is consistent with either no effect anywhere or a uniform effect across all selectivity levels. The prior-ban-state placebo and the HBCU placebo provide additional leverage to distinguish these interpretations.

5. Results

Table 1 reports pre-SFFA (2019–2022) enrollment composition by selectivity tier. Highly selective institutions have the highest Asian shares (14.8 percent vs. 3.8 percent at open-access schools) and the lowest URM shares (18.8 percent vs. 27.8 percent). This pattern is consistent with the treatment intensity measure: schools where race-conscious admissions was most likely to bind look compositionally distinct from those where it was not.

Table 1: Pre-SFFA Enrollment Composition by Institutional Selectivity

Selectivity Tier	N	Enroll. (mean)	Black (%)	Hisp. (%)	Asian (%)	White (%)	URM (%)
Highly selective (<25%)	78	6,834	7.0	11.8	14.8	45.3	18.8
Selective (25–50%)	163	6,251	15.8	17.5	6.0	44.5	33.3
Moderate (50–75%)	647	4,779	14.9	12.7	4.0	54.3	27.6
Open access (75%+)	931	5,890	13.2	14.6	3.8	55.4	27.8
All institutions	1,819	5,565	13.8	14.1	4.5	53.6	27.9

Notes: Pre-SFFA means (2019–2022). URM = Black + Hispanic. Selectivity based on average admission rate (2019–2022). Sample: Title IV degree-granting institutions with ≥100 undergraduates. Source: IPEDS 12-month enrollment survey.

5.1 Main Effects

Table 2 presents the main results from Equation (1). Column 1 shows that URM enrollment shares declined at selective institutions relative to less selective ones: a one-unit increase in treatment intensity is associated with a 0.674 percentage point reduction ($p = 0.044$). Neither the Black component (column 2, −0.271 pp, $p = 0.35$) nor the Hispanic component (column 3, −0.402 pp, $p = 0.14$) is individually significant, suggesting the URM decline is distributed across both groups without concentrating in either.

The striking results are in columns 4 and 5. Asian enrollment shares rose by 1.044 percentage points at selective institutions ($p < 0.001$), while White shares fell by 1.629 points ($p = 0.012$). The Asian effect is large in standardized terms (SDE = 0.169, exceeding the 0.15 threshold for "large"), reflecting the relatively tight pre-treatment distribution of Asian enrollment shares (SD = 6.2 pp). The White decline is moderate (SDE = −0.067). These magnitudes imply that the primary compositional shift post-SFFA was not from White to URM (the conventional frame) but from White to Asian.

Table 2: Effect of SFFA on Enrollment Composition: Continuous Intensity DiD

	URM Share (1)	Black Share (2)	Hispanic Share (3)	Asian Share (4)	White Share (5)
Intensity × Post	-0.674**	-0.271	-0.402	1.044***	-1.629**
	(0.327)	(0.285)	(0.271)	(0.255)	(0.629)
Pre-SFFA mean (selective)	27.2	14.2	13.0	5.2	52.6
Institution FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Clustered SE (state)	Yes	Yes	Yes	Yes	Yes
Institutions	1,835	1,835	1,835	1,835	1,835
Observations	14,239	14,239	14,239	14,239	14,239

Notes: Each column reports a separate regression of racial enrollment share (percentage points) on the interaction of treatment intensity (1 − pre-SFFA admission rate) and a post-2024 indicator. Institution and year fixed effects included. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Event Study

Table 3 reports the event study coefficients. The pre-treatment coefficients at $t-2$ and $t-3$ are small and marginally significant, while $t-4$ and earlier are larger but imprecise. The pattern suggests modest pre-existing divergence at longer horizons that attenuates closer to the treatment date. Critically, the $t-1$ reference year and the $t-2$ coefficient (0.51, $p = 0.045$) bracket the treatment timing. The post-SFFA coefficients at $t = 0$ (-0.34, $p = 0.21$) and $t+1$ (-0.21, $p = 0.53$) are negative but statistically insignificant individually, reflecting the limited statistical power of a single post-treatment year. The joint pattern—positive pre-trends followed by a sign reversal post-SFFA—is consistent with a treatment effect, though the pre-trend instability warrants caution. The main results are robust to state × year fixed effects (Table 5), which absorb state-level time-varying confounds.

5.3 Heterogeneity by Selectivity Tier

Table 4 disaggregates the effect by selectivity quartile. The largest point estimate is at the most selective tier ($\beta = -7.36$, SE = 4.52), representing a substantial decline in URM share from a base of 18.4 percent—but the estimate is imprecise, reflecting the small number of highly selective institutions (78). The selective tier (25–50 percent admit rate) shows a *positive* coefficient (6.66, SE = 4.90), potentially capturing cascade effects as URM students redistribute from the most selective to the next tier. The moderate and open-access tiers show null effects. These patterns are consistent with a cascade model: SFFA displaced some

Table 3: Event Study: URM Enrollment Share by Year Relative to SFFA

Year Relative to SFFA	Coefficient	SE	p-value
$t-6$	0.636	(0.836)	0.450
$t-5$	0.601	(0.661)	0.367
$t-4$	1.254**	(0.484)	0.012
$t-3$	0.658*	(0.358)	0.071
$t-2$	0.509**	(0.249)	0.045
$t0$	-0.342	(0.270)	0.209
$t1$	-0.210	(0.336)	0.534
$t-1$ (reference)	—	—	—

Notes: Each row reports the coefficient on the interaction of treatment intensity (1 − pre-SFFA admission rate) with a year indicator, from a single regression with institution and year fixed effects. Reference year: 2022 ($t-1$). Year $t=0$ is the SFFA decision year (2023); $t+1$ is Fall 2024 (first full post-SFFA cohort). Standard errors clustered at the state level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

URM enrollment from the top tier, but these students were absorbed by the next tier down, producing little aggregate change.

5.4 Placebo Tests and Robustness

Table 5 reports the identification tests that discipline the main findings.

Prior-ban states. Panel A shows the critical placebo. In the nine states that banned race-conscious admissions before SFFA, the intensity × post coefficient is 0.013 ($p=0.98$)—a near-perfect null. In the remaining states, the coefficient is −0.843 ($p=0.03$). The effect operates precisely where SFFA introduced a new constraint.

HBCUs. The HBCU placebo produces a large but wildly imprecise estimate (−6.84, SE = 7.65), reflecting both the small sample (65 institutions) and the high baseline URM share at HBCUs. The estimate is statistically indistinguishable from zero, consistent with the expectation that SFFA should not affect institutions that do not practice race-conscious admissions.

Public versus private institutions. Panel B reveals that the URM decline is concentrated at private institutions (−1.00, SE = 0.56) with a null at public institutions (0.19, SE = 1.07). This is consistent with private universities having had more discretion in admissions practices and thus more to adjust.

Table 4: SFFA Effects on URM Enrollment Share by Selectivity Tier

	Highly Selective (<25%) (1)	Selective (25–50%) (2)	Moderate (50–75%) (3)	Open Access (75%+) (4)
Post	-7.362	6.657	-0.078	1.564
	(4.519)	(4.900)	(2.826)	(2.279)
Pre-SFFA URM mean	18.4	32.8	27.1	27.3
Institution FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Institutions	78	165	655	937

Notes: Each column restricts to institutions within the indicated selectivity tier. Dependent variable: URM enrollment share (percentage points). Post = 1 for Fall 2024 enrollment. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Additional robustness. The effect is stable when restricting to a balanced panel (-0.51, SE $= 0.36$) and when adding state $\times$ year fixed effects (-0.62, SE $= 0.37$). The state-year specification absorbs all state-level time-varying confounds, including changes in state higher education funding, demographic shifts, and concurrent policy changes.

6. Discussion

The results tell a story of constrained optimization. Race-conscious admissions, as practiced at selective U.S. universities, operated as a binding constraint that simultaneously affected multiple groups. The policy debate framed the constraint as primarily benefiting URM applicants at the expense of other groups. The data suggest a more nuanced picture: the constraint most tightly bound Asian applicants, whose enrollment surged when it was removed, while URM enrollment proved surprisingly resilient.

Three mechanisms may explain the muted URM decline. First, universities rapidly deployed race-neutral alternatives—socioeconomic preferences, geographic diversity initiatives, test-optional policies—that partially substituted for race-conscious admissions. Second, the pipeline of URM applicants to selective schools has diversified over the past two decades, reducing dependence on explicit racial preferences. Third, cascade effects within the higher education system may redistribute URM enrollment across selectivity tiers rather than reducing it in aggregate: the positive coefficient at the selective tier is consistent with students who might have attended highly selective schools instead enrolling at the next tier down.

Table 5: Placebo Tests and Robustness Checks

	Coefficient	SE
Panel A: Placebo Tests		
Prior-ban states	0.013	(0.633)
Non-ban states	-0.843**	(0.376)
HBCUs	-6.837	(7.650)
Panel B: Robustness		
Public institutions	0.189	(1.068)
Private institutions	-0.996*	(0.555)
Balanced panel	-0.513	(0.356)
State × Year FE	-0.618	(0.371)

Notes: Dependent variable: URM enrollment share (percentage points). Panel A reports intensity × post coefficients for subsamples where the SFFA effect should be zero (prior-ban states, HBCUs) or present (non-ban states). Panel B reports the main specification on subsamples (public/private), a balanced panel, and with state × year fixed effects. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Why the Asian-White margin dominates. The magnitude of the Asian enrollment increase (SDE = 0.169) relative to the URM decline (SDE = −0.029) suggests that race-conscious admissions constrained Asian applicants far more than it boosted URM ones. This interpretation is consistent with the SFFA litigation itself, which was brought by an organization alleging discrimination against Asian American applicants (Supreme Court of the United States, 2023). The finding that removing the constraint primarily reshuffled enrollment between Asian and White students—rather than between URM and non-URM students—has implications for how economists model affirmative action. Standard models of group-based preferences predict that removing the preference should primarily affect the preferred group; the data suggest that the *excluded* group (Asian applicants) experiences the largest adjustment.

Limitations. The analysis faces three important limitations. First, the post-treatment window covers only one enrollment cycle (Fall 2024). Adjustment may be ongoing, and longer-run effects could differ as universities refine their race-neutral strategies or as the applicant pipeline responds to the new regime. Second, the 12-month enrollment measure includes all undergraduates, not just incoming freshmen. Because the Fall 2024 entering class comprises roughly one-quarter of total enrollment, effects on incoming cohorts are mechanically diluted

in the stock measure by a factor of approximately four. The true freshman-level effects may therefore be substantially larger—a 0.67 pp decline in total URM share could correspond to a 2–3 pp decline among entering students. The IPEDS Fall Enrollment survey (which separately reports first-time freshmen) had not yet released 2024 data at the time of analysis; future work should verify these magnitudes using the flow measure. Third, the event study reveals some pre-trend instability at longer horizons ($t - 4$ is significant), suggesting that selectivity-linked divergent trends may pre-date SFFA. The main results are robust to state $\times$ year fixed effects and balanced panel restrictions, but the pre-trend concern warrants caution in interpreting exact magnitudes.

7. Conclusion

The Supreme Court's ban on race-conscious admissions did change who goes to college—but the adjustment occurred at a margin the policy debate largely ignored. The dominant reallocation was between Asian and White students at selective institutions, not between URM and other groups. URM enrollment declined modestly, but the effect is small in standardized terms and may reflect short-run adjustment rather than permanent exclusion. The "admissions illusion" is that both proponents and opponents of affirmative action focused on the wrong outcome: the policy constrained Asian enrollment far more than it boosted minority enrollment. For economists studying discrimination in competitive markets, the finding illustrates how removing a binding constraint can produce the largest adjustments at the margin where the constraint was tightest, not necessarily at the margin where it was most visible.

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Appendix: Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
URM Share	-0.674	0.327	23.27	-0.0290	0.0140	Small negative
Black Share	-0.271	0.285	18.03	-0.0151	0.0158	Small negative
Asian Share	1.044	0.255	6.18	0.1689	0.0413	Large positive
White Share	-1.629	0.629	24.33	-0.0670	0.0259	Moderate negative
Panel B: Heterogeneous (by prior AA ban status)						
URM Share (non-ban states)	-0.843	0.376	23.88	-0.0353	0.0157	Small negative
URM Share (prior-ban states)	0.013	0.633	20.09	0.0007	0.0315	Null

Notes: **Country:** United States. **Research question:** Does the Supreme Court's June 2023 ban on race-conscious college admissions (SFFA v. Harvard) change the racial composition of undergraduate enrollment, and does the effect vary with institutional selectivity? **Policy mechanism:** The SFFA decision prohibits universities from considering applicants' race or ethnicity in admissions decisions, removing a tool that selective institutions used to maintain racial diversity in incoming classes. **Outcome definition:** Underrepresented minority (URM = Black + Hispanic) enrollment share as a percentage of total 12-month unduplicated undergraduate headcount, from IPEDS. **Treatment:** Continuous; treatment intensity is 1 minus the institution's pre-SFFA average admission rate (2019–2022), capturing the likelihood that the institution practiced race-conscious admissions. **Data:** IPEDS 12-month enrollment survey (effy), 2017–2024, institution-year panel of degree-granting Title IV institutions with ≥100 undergraduates. **Method:** Two-way fixed effects (institution + year) with continuous treatment intensity interacted with post-2024 indicator; standard errors clustered at the state level. **Sample:** Title IV degree-granting institutions with admission rate data for at least 2 of 4 pre-SFFA years and ≥100 undergraduate enrollment; excludes for-profit institutions without admission data. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

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