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COLLEGE MAJOR RESTRICTIONS AND STUDENT STRATIFICATION

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ABSTRACT

Underrepresented minority (URM) college students have been steadily earning degrees in relatively less lucrative fields of study since the mid-1990s. A decomposition reveals that this widening gap is principally explained by rising stratification at public research universities, many of which increasingly prevent students with poor introductory grades from declaring popular majors. We investigate these major restriction policies by constructing a novel 50-year dataset covering four public research universities' student transcripts and employing a staggered difference-in-difference design around the implementation of 25 GPA-based restrictions. Restrictions disproportionately filter out less-prepared students with fewer pre-college academic opportunities, decreasing average URM enrollment shares by 20 percent. They do not measurably improve allocative efficiency across majors, departments' wage value-added, or filtered students' educational attainment. Using first-term course enrollments to identify students who intend to earn restricted majors, we find that major restrictions disproportionately lead URM students toward less lucrative majors, explaining nearly all growth in within-institution ethnic stratification since the 1990s.

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1 Introduction

U.S. college graduates from underrepresented minority (URM) groups – Black, Hispanic, and Native American – have persistently earned about 25 percent lower wages than similarly-educated non-URM workers.[1] Substantial scholarly and public attention has been paid to the wage effects of race-based affirmative action and other admissions policies that reduce access gaps to high-quality universities (Chetty et al., 2020; Bleemer, 2022, 2023; Bhattacharya and Schvets, 2024), but there is also substantial wage heterogeneity within universities: wage premiums from high-value college majors can exceed even the average overall return to a college degree (Altonji, Arcidiacono, and Maurel, 2016; Andrews et al., 2022) and URM students are sharply underrepresented within universities in many lucrative fields of study (NCSES, 2023; Monarrez and Washington, 2020). This raises the possibility that student stratification across high- and low-wage majors contributes to the ethnicity wage gap. This study documents a surprising rise in college major stratification by ethnicity over the past twenty years, decomposes that rise to measure the possible role of within-institution explanations, and then carefully examines one such explanation – the proliferation of GPA-based college major restrictions, an understudied class of university policies – the unintended consequences of which largely account for URM graduates' recent trend toward less-lucrative fields of study.

We begin by constructing and causally validating a simple index of each major's economic value by estimating majors' wage value-added within gender, ethnicity, age, and cohort bins among mid-career 2009-2019 American Community Survey respondents. Figure 1 uses the resulting statistics to show the stratification of majors earned by URM and non-URM college graduates since the 1950s. While the wage premium gap between the majors completed by URM and non-URM graduates had nearly disappeared by the late 1970s birth cohorts, it has been steadily rising for the past 20 years, with recent URM graduates earning majors with 3 percent lower wage premiums than the majors earned by their non-URM peers.

College major stratification – the separation of non-URM and URM students into more and less lucrative college majors – thus provides a meaningful countervailing force against the antidiscrimination policy momentum toward closing ethnicity wage gaps in the United States (Lang and Lehmann, 2012). We investigate the sources of this growing stratification by constructing a dataset covering annual degree

[1]For seminal studies on ethnicity gaps in the collegiate workforce, see Darity and Mason (1998) and Bertrand and Mullainathan (2004) on hiring discrimination, Ioannides and Loury (2004) on job networks, Card and Krueger (1992) on school quality, Card and Giuliano (2016) on K-12 academic programs, Neal and Johnson (1996) on resulting human capital gaps, and Altonji and Blank (1999) for a review. Bayer and Charles (2018) show that overall Black-white wage convergence at the top of the wage distribution has largely occurred within education group.

attainment by ethnicity at every U.S. college and university, permitting an observational decomposition of ethnic stratification into within- and between-institution components. We find that while just over a third of the rise in ethnic stratification across college majors can be explained by rising URM enrollment at institutions that tend to award relatively lower-premium majors – mostly less-selective and for-profit institutions – about two-thirds of the rise can be explained by *within*-institution dynamics over time, driven in particular by a sharp rise in ethnic stratification at public research universities. While public research universities enroll a third of U.S. undergraduates, they account for almost half of both current within-institution stratification and of universities' recent trend toward greater stratification.

As a result, we turn our focus to potential mechanisms that could explain the increasingly inequitable distribution of college majors across ethnicities at public research universities. College major attainment in the US is often described as a choice that reflects student preferences (e.g. Wiswall and Zafar, 2021), but we find little evidence supporting demand-side explanations of the rise of ethnic stratification. Instead, we focus our attention on a previously-undocumented policy phenomenon at public research universities: the increasing prevalence of meritocratic major restriction policies, which explicitly limit students' access to certain majors based on their introductory course grades. Table 1 documents the restrictions imposed on five of the highest-premium majors at the 25 top-ranked public universities in the U.S. These universities enroll about 750,000 undergraduates, or half of all students at top-100 universities (and 7 percent of *all* undergraduates), and over 20 percent of their graduates earn degrees in these five high-premium majors. While about half of the majors imposed a restriction in 2002, three-quarters did so in 2019, including every nursing major and nearly all mechanical engineering and finance majors. We then construct a new dataset covering the major restriction policies at all public R1 universities (which enroll one-third of all US college students), showing in Figure 2 that half of all majors and three-quarters of high-premium majors impose some kind of access restriction. While fewer than twenty percent of high-premium majors at top-ranked private US universities have formal restrictions – though many limit access to high-premium majors using low grades and other 'soft' discouragement mechanisms – major restriction policies are ubiquitous at selective universities outside the United States.[2]

We investigate the causal effect of major restriction policies on ethnic stratification by constructing a new detailed database covering the 500,000 freshman students who enrolled at four public research universities – the University of California campuses at Berkeley, Davis, Santa Barbara, and Santa Cruz – between 1975 and 2018 and employing a staggered difference-in-difference research design around the introduction

[2]See Table A-1 for private university restrictions, almost all of which allow for discretion in admission. See Armstrong and Hamilton (2013) for a discussion of 'soft' discouragement mechanisms.

of 25 major restrictions. Estimating three-way fixed effect models at the department level, we show that major restrictions lead students with below-average academic preparation and performance to exit restricted majors. As a side-effect, newly-imposed major restrictions cause the share of URM students who declare the restricted major to decline by an average of 20 percent, mirroring the observational difference in URM attainment between the restricted and unrestricted majors at all R1 public universities.[3]

Next, we trace the college majors attained by students at these universities who exit restricted majors by estimating restrictions' impact on students who 'intend' those majors, measuring intentions with a machine learning algorithm trained on pre-restriction data to predict major declaration using first-term course enrollments. We find that restricting a major has divergent effects on the URM and non-URM students who intend to complete it, with URM students tending toward relatively less lucrative alternative fields of study. Notably, a simulation exercise employing these estimates suggests that major restrictions on their own can wholly explain the rise in college major stratification from the 1990s until the financial crisis and most of the rise thereafter.

We conclude with a discussion of the efficiency ramifications of major restriction policies. We find evidence against the hypotheses that major restrictions improve match quality by allocating majors toward students with comparative advantages in the field, improve the signal or human capital value of restricted majors for the students who remain in those degrees, or improve educational attainment among the students who are excluded from restricted majors due to their low grades. Instead, major restrictions are likely to generate new inefficiencies by disproportionately reducing aggregate science major attainment (Murphy, Schleifer, and Vishny, 1991), muting potential match effects between students' major choices and their comparative preferences (Kirkeboen, Leuven, and Mogstad, 2016), and allocating majors away from interested and academically-promising students with limited pre-college educational opportunity, a group who are likely to receive above-average returns from lucrative college major attainment (Bleemer and Mehta, 2022).

This study primarily contributes to three strands of prior literature. First, we provide a new measure of collegiate human capital and document a growing ethnicity gap with important ramifications for the relative wages of Black and Hispanic workers.[4] Our college major premiums generalize the 'STEM' categorization often used as a proxy for the economic value of college majors (e.g. Carrell, Page, and West, 2010;

[3]We observe similar declines in lower-income students' attainment of restricted majors, likely contributing to the growing regressivity of US higher education (Bleemer and Quincy, 2024).

[4]Black et al. (2006) find evidence that college majors explained 2.7 (1.4) percentage points of the Black-white (Hispanic-white) wage gap among 1993 workers, who were mostly members of the 1930-1970 birth cohorts, and Sloane, Hurst, and Black (2021) use a similar index of majors' economic value to study the gender wage gap.

Mourifie, Henry, and Meango, 2020), despite the existence of many high-premium non-STEM majors (e.g. nursing and business) and low-premium STEM majors like soil science and agronomy. Several studies have characterized and investigated dynamics in ethnic stratification across more- and less-selective universities (e.g. Dale and Krueger, 2014; Arcidiacono and Lovenheim, 2016; Black, Denning, and Rothstein, 2023), another potentially important dimension of collegiate human capital. The observed difference in average major premiums across URM and non-URM graduates could explain about three percentage points (over ten percent) of the ethnicity wage gap among young college-educated workers.[5]

Second, we provide evidence highlighting the role of an understudied class of university policies that appear to be driving this growing stratification. A number of studies have analyzed between-institution differences in STEM attainment by ethnicity (e.g. Arcidiacono, Aucejo, and Hotz, 2016), but we show that two-thirds of the growth in ethnic stratification across majors can be explained by within-university trends. Similarly, a large literature examines the demand side of major choice – students' subjective expectations and preferences (e.g. Wiswall and Zafar, 2015, 2018, 2021) and available information (e.g. Kizilcec et al., 2023) – but we find that supply-side policy variation plays a much larger role in the recent growth of college major stratification.[6] We provide causal evidence that universities' independent implementations of major restriction policies disproportionately lead URM students to earn less lucrative college majors, generating ethnic stratification with macro-level wage ramifications.

Finally, our study contributes two methodological innovations with broad applicability in applied microeconomics. Our primary contribution identifies treatment effects on individuals who *intend* a policy-impacted behavior – in our context, declaring a restricted major – by explicitly characterizing intentions (predicted using pre-implementation data) and then estimating a difference-in-difference model with predicted intention as the (fuzzy) second difference. Triple-difference interactions with demographic characteristics identify heterogeneous treatment effects among students who intend restricted majors. We also introduce a two-way fixed effect decomposition of grades into additive student and course-term effects (following Abowd, Kramarz, and Margolis, 1999) to characterize university students' academic performance, permitting cross-student comparisons over long time horizons and between disciplines despite cross-course

[5]The ethnicity wage gap has generally been closing across education groups since the 1940s, but in recent years the gap among college-educated workers has slightly grown in both absolute and relative terms; see Figure A-2. Gerard et al. (2021) show that race-neutral skill-based job sorting contributes to the racial wage gap in Brazil.

[6]Major-specific price discrimination (Andrews and Stange, 2019), incentive payments (Denning and Turley, 2017), and grading standards (Stinebrickner and Stinebrickner, 2014; Butcher, McEwan, and Weerapana, 2014) have also been shown to shape major attainment, as do pre-college academic preparation (Arcidiacono and Koedel, 2014) and peer composition (Brenoe and Zolitz, 2020). However, none of these explanations are both widespread and particularly prevalent at public research universities or have been shown to differentially discourage URM students from lucrative majors, suggesting their second-order role in the growth of ethnic stratification.

variation in grading standards.[7]

We begin in Section 2 by documenting the growth in ethnic stratification across U.S. college majors, decomposing its between- and within-institution components, and motivating the contributing role of major restriction policies. Section 3 describes our detailed UC student data. Section 4 presents difference-in-difference evidence that major restrictions decrease URM and lower-income enrollment and suggestive evidence that this is due to their relatively poorer academic preparation. Section 5 shows that restrictions disproportionately lead URM students to earn lower-premium degrees, and Section 6 uses those estimates to simulate major restrictions' stratification effects. Section 7 discusses the policies' efficiency ramifications, and Section 8 concludes. A series of online appendices consider alternative major premium statistics, analyze recent growth in *between*-institution stratification, present case-study evidence describing restrictions' likely causal mechanisms, consider restrictions' effects on gender stratification, and document growing high school opportunity gaps by ethnicity.

2 Motivation

2.1 Ethnic Stratification across U.S. College Majors

Stratification arises when some sub-populations are less likely to access desirable opportunities than others for (even partly) non-voluntary reasons (Darity, 2005). Let $r \in \{U, N\}$ denote the ethnicity of underrepresented minority (URM) and non-URM workers.[8] We index the average collegiate human capital obtained by r members of birth cohort t by $E_t(\omega_m|r)$, where ω_m is the average wage (residualized on demographics) earned by college graduates who earned major m compared to a baseline major, which we assign to be general agriculture.[9] We refer to our estimates of ω_m as major m's "major premium".[10]

Let Δ_r be an ethnic difference operator, so that *aggregate* college major stratification at t is:

$$S_t^{Agg} \equiv \Delta_r[E_t(\omega_m|r)] \equiv E_t(\omega_m|N) - E_t(\omega_m|U) \tag{1}$$

Figure 1 presents aggregate college major stratification by birth cohort for all college-educated and employed

[7] Caulkins, Larkey, and Wei (1995) and Wittman (2022) suggest similar two-way fixed effect specifications.

[8] URM designates Black, Hispanic, and Native American/Alaskan workers.

[9] See Appendix B for a formal definition of ω_m and Table A-2 for our estimates of ω_m. We abstract from all dimensions of collegiate human capital orthogonal to major attainment.

[10] Figure A-1 shows that quasi-experimental evidence from major attainment shocks due to major restrictions (Bleemer and Mehta, 2022) quantitatively validate ω_m as an index of majors' economic value in at least one local setting.

2009-2019 American Community Survey (ACS) respondents. It shows that URM students have long tended to complete lower-premium majors, but that this gap had fallen to less than one percentage point in the 1970s before widening to 2.6 percentage points by the mid-1990s.[11] Appendix B shows that the same stratification trends are observed when major premiums are estimated in different years, restricted to a single gender or ethnicity, conditioned on local geography, or replaced with median earnings by major (following Sloane, Hurst, and Black, 2021).

2.2 Decomposing Stratification Between and Within Institutions

We decompose the sources of the recent rise in ethnic stratification by college major using federal data on the annual number of college graduates by institution, major, and ethnicity since 1995 (IPEDS, 2022).[12] These data permit estimation of several cohort-specific probabilities for each four-year U.S. degree-granting institution i, including $P_t(i)$, $P_t(i|r)$, $P_t(m|r)$, and $P_t(m|i,r)$. Given that $E_t(\omega_m|i,r) = \sum_m P_t(m|i,r)\omega_m$ denotes each ethnic group's average major premium within institution i, aggregate stratification can be disaggregated across institutions:

$$S_t^{Agg} = \sum_i \left[P_t(i|N)E_t(\omega_m|i,N) - P_t(i|U)E_t(\omega_m|i,U)\right] \tag{2}$$

Institution i suffers *within-institution* (major) stratification when its URM graduates tend to complete lower-premium majors than their non-URM counterparts:

$$S_t(i) \equiv \Delta_r[E_t(\omega_m|i,r)] = \sum_m \omega_m \Delta_r[P_t(m|i,r)] \tag{3}$$

On the other hand, $\sum_i \{E_t(\omega_m|i,N)\Delta_r[P_t(i|r)]\}$ captures *between-institution* stratification, which is positive whenever URM students disproportionately attend institutions whose (non-URM) students specialize in low-wage majors. Aggregate stratification is the sum of between-institution stratification and a URM-weighted average of within-institution stratification:

$$S_t^{Agg} = \sum_i \{E_t(\omega_m|i,N)\Delta_r[P_t(i|r)]\} + \sum_i P_t(i|U)S_t(i) \tag{4}$$

[11] Ethnic stratification has followed similar trends among both male and female college graduates, though the gap has been persistently larger among male graduates (see Figure A-3).

[12] Monarrez and Washington (2020) use IPEDS data to present cross-sectional evidence of ethnic segregation across college majors, complementing the present study, but do not investigate the relationship between major attainment and wage stratification.

The within-institution component of Equation 4 can change for two reasons: (1) the reallocation of URM students into universities that were more stratified in 1995 ("static") and (2) increased stratification (relative to 1995) at the universities where URM students enroll ("dynamic"):

$$S_t \equiv \sum_i \{E_t(\omega_m|i,N)\Delta_r[P_t(i|r)]\} + \sum_i P_t(i|U)S_{95}(i) + \sum_i P_t(i|U)[S_t(i) - S_{95}(i)] \tag{5}$$

Figure 3 implements Equation 5 annually across all 3,600 four-year colleges and universities in the U.S., estimating ω_m from the ACS and all relevant probabilities from IPEDS.[13] It shows that dynamic within-institution stratification has played the largest role in driving the increase in ethnic stratification of college majors since the 1990s, explaining about 65 percent of the growth as URM students' universities increasingly stratify by major. There has also been substantial growth in between-institution stratification, which was negative in the late 1990s – indicating that institutions that disproportionately graduated URM students specialized in higher-premium majors – but had become positive by 2019. While URM students have always been more likely to graduate from institutions that were historically internally stratified, this tendency has slightly declined over time, making the static within-institution component the least impactful contributor to ethnic stratification's recent growth. In general, the figure shows that within-institution stratification has been a persistently large and swiftly-growing contributor to the college major ethnicity gap, explaining over 2.2 log points of the 2.8 point gap in 2019.[14]

Appendix C shows that the growth of between-institution stratification can be largely explained by the growing population of college-eligible URM students being accommodated at less-selective and for-profit institutions that specialize in low-premium majors. As these trends are well-studied (see Page and Scott-Clayton, 2016), the rest of our study focuses on the larger but relatively-understudied within-institution component of ethnic stratification.

Figure 4 further decomposes static and dynamic within-institution stratification into the contributions of six university sectors – the top 26 public universities discussed above, other R1 and R2 public research universities (following the Carnegie Classification), other public universities, and non-profit and for-profit private universities. Within-institution stratification increased in all six sectors, but increased the most at public research universities, especially at the top 26.[15] In 2019, public research universities educated about

[13] Assuming that students graduate at about age 22, the dynamics and magnitude of aggregate college major stratification are very similar whether tracked by birth year in the ACS (Figure 1) or by graduation year in IPEDS (Figure 3). Institutions outside the fifty states are omitted, and expected ω_m is assumed to be equal across ethnicities in institution $\times$ year cells in which no graduates of one ethnicity are observed.

[14] Black and Hispanic graduates experience similar stratification trends relative to non-URM students. See Figure JJ-2.

[15] For example, while overall within-institution stratification rose from 1.2 percent in 1995 to 2.3 percent in 2019, stratification

one-third of URM students but accounted for 46 percent of within-institution stratification and for 45 percent of the growth of dynamic within-institution stratification.[16] These findings motivate a closer analysis of the public research university sector to uncover the root causes of the upward trend in ethnic stratification by college major.

2.3 Potential Demand-Side Explanations for Stratification

Why are public research universities with high URM enrollment becoming increasingly stratified across majors? Two "demand-side" explanations find little support in available evidence. First, shifts in the labor market could have reduced URM students' wage return to high-premium majors, decreasing their incentive to earn degrees in those fields. For example, increasing racial discrimination in occupations associated with high-premium majors could reduce URM students' incentives to choose those majors. However, while the (uniformly-positive) wage return to high-premium majors does appear to be lower for URM students than for non-URM students, that gap has steadily shrunk over time, rejecting the possibility that declining economic incentives to earn high-premium majors explain the observed trend in ethnic stratification.[17]

Second, the steadily-expanding share of URM college enrollment in the U.S. may imply that URM college students are increasingly negatively-selected relative to non-URM students, which could shift their preferences towards less-challenging lower-premium majors. However, growth in college-going has actually been slower among URM than among non-URM high school graduates, suggesting that the rise in URM enrollment has more likely been driven by demographic shifts across the U.S. population than by increases in college-going among negatively-selected URM populations that previously had not enrolled in college.[18] The ethnicity gap in average SAT scores at public research universities also appears to have narrowed in recent years, suggesting that negative selection on pre-college academic preparation is unlikely to explain the observed widening of ethnic stratification within institutions.[19]

We thus find little evidence to suggest that student demand-side factors were first-order contributors to

at the top 26 publics rose from 2.1 to 4.6 percent. See Table A-3.

[16] Figure A-4 shows the distribution of stratification across universities by ownership, confirming much higher average stratification at R1 public universities than other public or private institutions.

[17] See Figure A-5.

[18] See Figure A-6. The inflow of URM college students tended to be absorbed by less selective institutions (Appendix C), which may have contributed to the increase in *between*-institution stratification documented above. However, dynamic within-institution stratification partials out this between-institution variation.

[19] See Figure A-7, which is restricted to average SAT scores at the four selective University of California campuses discussed in the next section. In other words, ethnic stratification grew even as the URM students at public research universities became (measurably) better equipped to complete restricted majors, suggesting that increased student filtering was unnecessary. Appendix G presents case-study evidence that average differences in academic preparation by ethnicity only stratify students across majors in the presence of major restriction policies.

the growth in within-institution stratification by college majors since the mid-1990s. The next subsection proposes a more promising institutional supply-side explanation.

2.4 Major Restriction Policies and Supply-Side Stratification

Recent growth in ethnic stratification across college majors has occurred disproportionately at public research universities, a sector in which many institutions have implemented major restriction policies that regulate access to designated fields of study (see Table 1). Departments generally justify major restrictions by arguing either that capacity constraints resulting from increases in student demand and limited educational resources require access limitations[20] or that lower-performing students cannot succeed in challenging fields of study. [21]

Major restriction policies take one of three forms: (1) an average grade requirement in introductory courses; (2) an internal application favoring academic performance, extracurricular activities, and professed interest; or (3) an external application submitted prior to enrollment at the institution. We refer to the first of these types as 'mechanical' restrictions and the latter two as 'discretionary', since they facilitate more nuanced decisions over who is permitted into restricted majors.[22]

If URM college students at R1 public universities have less pre-college preparation in STEM and other oft-restricted fields than their non-URM peers, then it would be unsurprising if they earned lower grades in their introductory college courses in those fields. Appendix D shows the annual Advanced Placement course availability at the high schools of URM and non-URM 1995-2016 matriculants at four R1 public universities. URM students typically graduated from high schools offering 1-2 fewer AP courses than their non-URM peers' high schools. In recent years, non-URM students' high schools were one-third more likely to offer AP BC calculus and twice as likely to offer AP Computer Science as those of URM students. These differences in pre-college educational opportunity suggest a possible role for major restriction policies based on introductory course grades in stratifying college major access by ethnicity.

We examine the observational relationship between the presence of major restrictions and ethnic strat-

[20]Thinly-stretched resources from 'over-enrollment' could reduce educational quality (Bound and Turner, 2007; Bound, Lovenheim, and Turner, 2010), in part through larger classes (Bettinger and Long, 2017). This may explain why restrictions are much more common at public than at (generally better-resourced) private universities (Table A-1).

[21]Bleemer and Mehta (2022) show that lower-performing students receive *above*-average wage returns from earning an economics major.

[22]Some universities also impose overall-GPA restrictions that require students to earn high overall average grades, but we generally ignore these restrictions because they can be 'gamed' by students choosing high-GPA courses. Restrictions are often complemented by 'soft' restrictions like low introductory course grades and verbal discouragement, but we focus on easier-to-observe mechanical and discretionary restrictions for empirical tractability.

ification across college majors by collecting a novel database of major restriction policies at all R1 public universities in 2021 and linking them to contemporaneous URM attainment shares in those majors (See Appendix A for details on our major restriction database). Table 2 reports estimated coefficients from linear regressions of each major's 2021 URM share on the presence of mechanical and discretionary major restrictions, with fixed effects absorbing differences in average URM shares across universities and fields. While about twenty percent of graduates from those university-majors were URM, restricted majors had lower URM enrollment by about 1.3 percentage points. The second and third columns show that this gap is largely explained by mechanical restrictions based on specified introductory courses, which are associated with ten percent lower URM enrollment compared to unrestricted departments, while discretionary and overall-GPA restrictions are weakly correlated with URM enrollment shares. These relationships are largely unchanged at the top 25 R1 public institutions, suggesting that restrictions play a similar role at more- and less-selective institutions.[23] The rightmost columns of Table 2 provide additional evidence tying GPA restrictions to ethnic stratification, showing that universities with great prevalence of mechanical restrictions are sharply more stratified in the major attainments of their URM and non-URM students.

While this descriptive evidence suggests that mechanical major restriction policies may play an important role in the recent growth in ethnic stratification across college majors, observational variation may differ from the causal effects of major restrictions. For example, if unrestricted high-demand majors are more likely to impose 'soft' restrictions (like low introductory course grades and verbal discouragement) designed to otherwise discourage enrollment, then observational differences would underestimate major restrictions' causal effects. The remainder of our study presents a series of quasi-experimental analyses designed to illuminate the causal relationship between college major restrictions and the major attainment of URM and non-URM students, and to shed light on the efficiency of major restrictions.

3 Data

We analyze the causal stratification ramifications of major restriction policies by studying restrictions implemented by four public research universities: the University of California campuses at Berkeley, Davis, Santa Barbara, and Santa Cruz. We conduct our baseline analysis using a novel student enrollment database collected as part of the UC ClioMetric History Project (Bleemer, 2018). The sample includes all undergraduate students who first enrolled as freshmen at each of four UC campuses in the observed sample period:

[23] Relatedly, Table A-4 shows that the low-GPA students admitted to a mechanically-restricted major by exception (which requires administrative discretion) are more likely to be URM than the average student in that major.

Berkeley (1975-2016), Davis (1980-2018), Santa Barbara (1986-2018), and Santa Cruz (1986-2018).[24] The data include students' cohort year, gender, ethnicity, high school, SAT score (since 1994), and home Zip code as well as their completed courses and letter grades. Students are linked by Zip code and enrollment year to average household income statistics from the IRS Statistics of Income. We link students to 2000-2020 annual wage records from the California Employment Development Department to observe labor market outcomes. See Appendix E for details on data construction and linkage.

Table 3 shows every formal major restriction policy that has been implemented by the four UC campuses. Each restriction's first (last) year is defined as the year prior to its first (last) appearance in the school's course catalog, since that entering cohort is typically the first that would face the new policy. Restrictions with GPA thresholds at or below 2.5 (a B-/C+ average in the requisite courses) are omitted, since their prevalence suggests pedagogical (rather than allocative) motivations for implementation.[25] Each campus has imposed about 12 restricted majors over the past 50 years, nearly all of which are mechanical restrictions. Major restrictions are seldom removed, though Davis's restrictions tend to be more numerous and shorter-lived than those at other campuses.

Table 4 presents major-aggregated descriptive statistics for each of the four UC campuses. Each campus graduated an annual average of 64 freshman-enrolled students (s.d. 81) per year in each of 58 majors. The average major was 55 percent female and 20 percent URM. There were 25 newly-imposed major restrictions during the period covered by the data. The total sample includes about 480,000 students who enrolled in almost 5,000 major-cohort pairs with at least 20 students.

Table 4's final column shows characteristics of majors soon to implement major restrictions. Those majors are twice the size of average majors, averaging 128 annual students. Only 13 percent of their students are URM, likely reflecting the fact that many of these majors are in STEM or other technical fields that tend to have below-average URM enrollment.

When measuring college students' academic performance, we abstract from differential grading standards across time and discipline in two ways.[26] First, we characterize students' overall academic performance by their individual GPA fixed effect ("GPA FE") from a two-way fixed effect model of GPA on student and course fixed effects (following Abowd, Kramarz, and Margolis, 1999).[27] Second, we measure

[24] About one-third of UC students are transfer students from community colleges. Transfer students are generally directly admitted by college major and are not bound by the presented GPA major restrictions, so they are omitted from all analysis except the aggregate simulation presented in Section 6. Students who drop out before declaring a major are included in the sample.

[25] Table A-5 shows that restrictions below 2.5 did not observationally have a substantive impact on URM enrollment.

[26] Figure A-8 presents average annual 1955-2016 grades by discipline at UC Berkeley, showing large and growing disciplinary gaps: e.g. the Humanities-STEM gap grew from 0.2 GPA points in 1970 to 0.4 points in the mid-2010s.

[27] Students' GPA fixed effect is a remarkably persistent characteristic; when separate individual effects are estimated for

students' academic performance with discipline-specific "normed GPAs" ($nGPA_d$), defined as the average number of within-course standard deviations by which their grade differed from the average grade in courses in discipline d. Figure 5 shows that URM students who attained soon-to-be-restricted majors earned lower introductory course grades in those fields than their non-URM peers by about 0.3 standard deviations, suggesting a likely mechanism by which major restrictions would stratify students by ethnicity.

We complement our analysis using annual survey responses from the CIRP Freshman Survey (HERI, 2022), which was fielded prior to students' first day of classes for most UC cohorts since 1966. The 31 percent of UC freshmen who respond to the survey cannot be linked to administrative records, but they report their intended major along with sociodemographic characteristics and beliefs about their ability to complete their intended major on time. See Appendix E for details.

4 Major Restrictions and Departmental Composition

4.1 Empirical Methodology

We investigate the effect of major restrictions on majors' student composition by using a staggered difference-in-difference design to estimate the effect of imposing new restrictions on the composition of freshman students who declare those restricted majors. Each newly-imposed major restriction in the sample period is considered an 'event,' omitting restrictions that were imposed within two years of the major's creation (prohibiting pre-period estimation), for fewer than four years (prohibiting estimation of longer-run effects), or with GPA thresholds below 2.5. We employ the resulting 25 events in a staggered three-way fixed effect model estimated over the unbalanced panel of all major-years with at least twenty enrollees at the four campuses:

$$Y_{cmy} = \alpha_{cm} + \gamma_{cy} + \zeta_{d_m y} + \sum_{t=-8}^{50} \beta_t \mathbb{1}\{y = R_{cm} + t\} + \epsilon_{cmy} \tag{6}$$

where Y_{cmy} is a characteristic of the students in incoming cohort y who declared campus c's major m (in discipline d_m); α_{cm}, γ_{cy}, and $\zeta_{d_m y}$ are fixed effects absorbing persistent average differences in departments' compositions and trends over time in the composition of campuses and disciplines (like the cyclicality of computer science enrollment); and R_{cm} is the first year that m's restriction appeared in the course catalog.[28]

students' first two years of courses and their later courses (among students with over 4 courses in each period), the resulting within-student correlation is 0.77. URM students arrive at UC with lower GPA FEs – by 0.38 points – and do not converge to their non-URM peers, remaining 0.36 below after their third year. See Figure A-9.

[28] The five disciplines are humanities, social sciences, natural sciences, engineering, and professional. Finer major categories yield highly similar estimates; see Figure A-10. β_{-8} is set to 1 when $y \leq R_{cm} - 8$, and an additional dummy indicates formerly

Standard errors are clustered by campus-major. We interpret the estimated $\hat{\beta}_t$ coefficients when $t > 0$ as the effect of implementing a major restriction policy on departmental composition, which assumes the absence of contemporaneous policy changes that differentially impacted newly-restricted majors.[29]

While a recent literature has highlighted the potential for treatment effect heterogeneity to bias estimation using fixed effect models like Equation 6, such bias is likely to be small in our setting because the number of treated units in our dataset is very small: out of 293 total departments, only 25 implement new major restrictions in our sample period, and only 46 ever implement a restriction. As a result, the vast majority (at least 84 percent) of the comparisons between departments that generate β_t are comparisons between treated and never-treated departments (Goodman-Bacon, 2021). The estimates presented below are qualitatively and largely quantitatively unchanged when the event study coefficients are estimated using Borusyak, Jaravel, and Spiess (2024)'s recent event study approach permitting triple-difference treatment effect heterogeneity across restrictions (see Figure A-11).[30]

Our $\hat{\beta}_t$ estimates could also be biased relative to the true effect of major restriction policies because of the mechanical relationship between the composition of students who earn restricted and unrestricted majors: students leaving Department A shift the composition of students in the Department B where they enroll instead, a violation of the stable unit treatment values assumption (SUTVA). Appendix F shows that the SUTVA violations in this context are likely to be very small due to the large number of alternative departments that students can choose between. It also shows that our tests of the null hypothesis are robust to using a Monte Carlo procedure to generate the correct distribution of the β_t coefficients under the null hypothesis that major restrictions do not systematically change departments' student composition; those p-values are presented in the last row of Table 5 below.

Because course catalogs may not record restrictions in their initial year of implementation – due to either administrative delays or grandfathering – major restrictions' first year of implementation is measured with noise. As a result, we estimate treatment effects relative to $t = -3$ and interpret β_{-2} through β_0 – from one year before to one year after the policy's stated implementation – as transitional years. The discussion below highlights changes between the pre-period before $t = -3$ and the period after $t = 0$. We present estimates of β_{-7} through β_{-4} to test for evidence that would reject parallel trends prior to

restricted majors (which turns off Equation 6's indicator).

[29]Our analysis implicitly assumes that newly-implemented restrictions did not motivate prospective students to enroll at other universities and earn the major there instead. Appendix I presents survey evidence that restriction implementation had no measurable effect on pre-matriculation intended majors' perceived likelihood of changing majors or graduating late as a result of major requirements, suggesting that students were unaware of the restrictions prior to arriving on campus, and that restrictions failed to arrest growing pre-matriculation interest in majors.

[30]Figures A-12 to A-14 plot restriction-specific estimates of Equation 6 for several outcomes.

restrictions' implementation. Figures A-12 to A-14 present department-by-department event studies and provide evidence that the transition years reflect event-year variation across departments rather than within-department non-parallel trends prior to each event.

4.2 Student Composition

Panel (a) of Figure 6 shows β estimates and 95-percent confidence intervals from Equation 6 for the log number of students who declare newly-restricted majors before and after the restrictions' implementation. The estimates suggest that major restrictions are put into place after several years of growth relative to other fields, reflecting a potential failure of parallel pre-trends that likely leads us to underestimate major restrictions' effects on the number of students enrolled. New restrictions cause an immediate cessation of this growth in the average department, with enrollment eventually stabilizing 10-20 percent below its peak and around the same enrollment level as several years prior to the restriction.

Panels (b) and (c) of Figure 6 show that the students who declare restricted majors have superior academic preparation and performance – by about 40 (out of 2400) SAT points and about 0.15 grade points per course – to those who had been declaring the major prior to restrictions' implementation. Assuming that these declines are explained by lower-preparation students exiting restricted majors, this implies that the exiting students had at least 200 fewer SAT points – two-thirds of a national standard deviation – than the average student in the major.[31] Table 5 shows that joint tests of these and all other outcomes' parallel pretrends (in years -7 to -4) fail to reject the null that majors with and without restrictions have similar trends in student composition prior to restrictions' implementation, despite restricted majors experiencing somewhat heightened growth prior to implementation.

GPA restrictions also changed majors' sociodemographic composition. Figure 7 shows that restricted majors saw their URM enrollment shares decline by about three percentage points, matching the cross-sectional relationship at 26 top-ranked public universities documented in Table 2 and representing a 20 percent relative URM enrollment decline from those departments' 13 percent base URM share.[32] This implies that URM students were at least three times more likely to exit restricted majors than their non-URM peers. The same pattern holds with regard to socioeconomic status: the average household income of majors' students (proxied by average incomes in their residential Zip code) rose by about four percent after

[31]This and other characterizations of the 'compliers' who exited restricted majors assume that major restrictions did not cause positively-selected students to select into the restricted major, an assumption for which we provide evidence in the following section, and that restrictions' aggregate enrollment effect was no larger than 20 percent.

[32]See Appendix J for disaggregated estimates by ethnicity, showing some evidence of disproportionate declines among Black students and disproportionate increases among white students.

restrictions were imposed.[33] Ruling out differential discouragement as an explanation of these findings, Appendix I presents survey evidence that URM and low-SES students became no less relatively likely to report intending to earn restricted majors in the years following restrictions' implementation. Consistent with filtering on prior preparation as the primary mechanism, Figure A-16 shows that restrictions are associated with declared majors coming from high schools that have about 0.3 more AP courses available (s.e. 0.12).[34] Appendix G uses a detailed case study to provide evidence that URM and lower-income students' poorer pre-college academic opportunity and preparedness can largely account for their lower enrollment following restrictions' implementation, an interpretation further bolstered by the evidence in Figure A-18 that URM enrollment declines in restricted majors largely occurred between (rather than within) coarse academic performance bins.[35]

These findings are summarized in Table 5.[36] As discussed above, bias due to the mechanical relationship between departments' composition proves to be insubstantial; e.g. the 3.0 percentage point decline in the URM share of restricted major attainment is larger than all but 2.5 percent of the Monte Carlo draws, suggesting that the observed decline is very unlikely to be explained by mechanical effects.

5 Major Restrictions and College Major Attainment

Characterizing the effects of major restriction policies on students' stratification across majors requires knowledge of the counterfactual majors students would attain as a result of the restrictions. We identify these alternative majors by observing the major attainment of students who *intend* to earn restricted majors before and after the restrictions are implemented.

[33]Figure A-15 provides additional evidence that higher-SES students were much less likely to exit restricted majors, especially among students from very high-income Zip codes.

[34]See Appendix D for evidence on the relatively low socioeconomic status of URM college students at the University of California. Figure A-17 shows that major restrictions had no measurable differential effect on the major declarations of California-resident and non-resident students.

[35]In particular, the figure shows that when the sample is split between students with above- and below-median GPA fixed effects, there is little observed relationship between major restriction implementation and within-bin URM shares, implying that the decline occurs between bins (which are themselves highly correlated with URM status; see Figure 5).

[36]Table A-6 shows that the presented estimates are insensitive to the small differences in outcome data availability between outcomes. Figure A-19 shows that the same patterns hold at each of the Berkeley, Santa Barbara, and Davis campuses. Interestingly, major restrictions have no observable immediate effects at Santa Cruz, suggesting that its restrictions were generally non-binding immediately following implementation. The presented difference-in-difference estimates average over all estimable major restrictions, suggesting that binding restrictions generate even greater stratification.

5.1 Empirical Methodology

We approximate students' revealed-preference major intentions by leveraging information from their first-term course enrollments, which they select in the first weeks after arriving on campus.[37] Because a wide variety of courses are available to students in their first term, their choices reveal substantial information about their major intentions.

Let M_{im} indicate whether student i declares campus-specific major m.[38] In order to isolate students' major intentions absent the access limitations of major restrictions, we begin by constructing a training sample of 50 percent of students between four and five years before major m's restriction implementation for each restricted major m. We then predict training-sample students' declaration of major m by indicators for enrollment in each available first-term course, gender, and URM status using a random forest estimator (Ho, 1995) at each campus.[39]

We employ the resulting prediction algorithm to estimate $\hat{M}_{im}$ for every student at that campus between six years before the restriction and four years after it (excluding the training sample). Students with higher $\hat{M}_{im}$ took courses that more strongly suggest their intention to major in m. Courses strongly predict major choice: the correlation between M_{im} and $\hat{M}_{im}$ is 0.37 in the out-of-sample students four to five years before the major restriction's implementation and remains 0.31 three to four years after implementation.[40]

Figure 8 plots the evolution of students' revealed-preference major intentions ($\hat{M}_{im}$) around the imposition of the 20 major restrictions with estimable intentions in our sample.[41] Intentions to declare restricted majors rose in the years leading up to those restrictions and then slightly and noisily declined after their

[37] We do not proxy UC students' major intentions using their self-reported 'intended majors' (e.g. Arcidiacono, Aucejo, and Hotz, 2016) because these self-reported intended majors are non-binding, can be strategically selected, and are not reported by about one-third of students (Bleemer, 2020).

[38] Students are associated with their final declared majors. Students who drop a major and declare another are no longer indicated as having declared the first major.

[39] We estimate each model using the default settings of the $randomForestSRC$ R package, version 2.12.0, which estimates 500 classification trees with no minimum node size. Courses with fewer than five enrollees in the training data are omitted. The sample is reweighted to give equal aggregate weight by gender and URM status. If fewer than 40 students in the training data declared the major, 50 percent of $t-3$ students are added to the training data. Abadie, Chingos, and West (2018) show that using a training sample minimizes bias from potential over-fitting in this context.

[40] Figure A-20 shows that the full distributions of M_{im} overall and for students in major m shift to the left over time as changes in introductory courses erode our capability to predict intended majors, but the small magnitudes of the shift among URM and non-URM students – and, as Figure A-21 shows, of differences in M_{im}'s predictiveness for URM and non-URM students – suggest little reason to expect these shifts to bias our baseline estimates.

[41] In particular, in a stacked student-major sample covering six years before to six years after each restriction – over 837,008 (144,737) observations of 372,490 (65,243) (URM) students – we estimate models of the form: $\hat{M}_{im} = \zeta_m + \sum_{t=-6}^{6} \beta_{it} \mathbb{1}\{y_i = R_m + t\} + \epsilon_{im}$ by weighted least squares, with weights equal to the inverse number of students at that campus so that each major is equally weighted in the analysis. Standard errors are clustered by major and by student and assume that $\hat{M}_{im}$ are observed without noise. Estimates of $\hat{M}_{im}$ are unavailable for restricted majors other than these 20 because either demographic data was unavailable or the majors were created too soon before the restriction.

imposition. However, the change in intentions does not differ by URM status, suggesting that the disproportionate decline in URM enrollment does not arise from differential discouragement from departments' introductory courses.[42]

Having characterized students' revealed intentions to declare restricted majors, we use $\hat{M}_{im}$ to identify changes in the major choices of students who intend restricted majors in the years before and after the restrictions are implemented. We estimate the following staggered difference-in-difference models over a stacked student-campus-major dataset by weighted least squares:

$$Y_{im} = \zeta_{my_i} + \gamma_t \hat{M}_{im} + \sum_{t=-6}^{6} \beta_{it} \mathbb{1}\{y_i = R_m + t\} \times \hat{M}_{im} + X_i + \epsilon_{im} \tag{7}$$

Major-cohort indicators ζ_{my_i} absorb within-campus major choice trends, leaving β_{it} to be identified by variation between students with greater and lesser intentions to declare the restricted major m relative to the baseline year. X_i includes the interactions between students' GPA fixed effect and gender to absorb spurious variation arising from academic capabilities.[43] We estimate either a single $\hat{\beta}_{it}$ for each t or separate coefficients by ethnicity, setting $\beta_{i,-3} = 0$, and cluster standard errors by major and by student as if $\hat{M}_{im}$ and X_i were observed without noise.[44]

5.2 Major Restrictions and Major Attainment

As above, we summarize stratification-relevant changes in students' major attainment by the average wage premium associated with that major (ω_m). Figure 9 Panel (a) shows that the decline in restricted major declaration does not translate into any overall change in the average premium of declared majors; on average, students who intend a restricted major but are pushed into other fields by the restriction appear to declare similar-premium majors instead.

However, the major choices of URM and non-URM students who intend restricted majors diverge after the restriction's implementation. Panel (b) presents estimates of $(\hat{\beta}_{URM,t} - \hat{\beta}_{NonURM,t})$ for the same outcome, characterizing the major choices of high-$\hat{M}_{im}$ URM students relative to non-URM students.

[42] Appendix H examines the stratifying effects of major restrictions by gender, showing that the decline in major intentions was wholly driven by female students, in line with other studies that have shown relatively larger discouragement effects of low grades (Ahn et al., 2019; Li and Zafar, 2021) and test scores (Azmat, Calsamiglia, and Iriberri, 2020) among female students.

[43] Figure A-22 shows little evidence of differential selection into major intention by academic capability, but conditioning on X_i partials out what appears to be spurious differential selection by URM students four years after restrictions' implementation. Excluding that year, all estimates are highly similar if X_i is omitted.

[44] When estimating β_{it} by gender or ethnicity, we also condition on the interaction between $\hat{M}_{im}$ and the characteristic as well as characteristic-by-t fixed effects.

High-$\hat{M}_{im}$ URM students' average major premium precipitously declined in the years following major restrictions: compared to the average non-URM student with $\hat{M}_{im} = 0.2$, major restrictions led similar-$\hat{M}_{im}$ URM students to declare majors with lower premiums by about 2 percentage points on average.[45] These findings suggest that major restrictions tend to lead URM students to declare relatively lower-premium majors, not because they are discouraged from attempting to declare restricted majors but because either (1) they are less likely to persist in declaring major m and select lower-ω majors instead or (2) their counterfactual alternative majors are lower-ω than those of non-URM students excluded from restricted majors.[46]

6 Major Restrictions and Aggregate Stratification

College major stratification by ethnicity has been rising since the late 1990s, and increasingly ubiquitous college major restriction policies tend to increase stratification. We estimate the potential contribution of new major restriction policies to college major stratification by comparing the observed growth in our four UC campuses' major premium gap with a simulated gap that our estimates suggest would be generated by the campuses' new major restrictions.

Let U_t and N_t be the sets of URM and non-URM UC students who matriculate at one of the four UC campuses in year t, and let ω_i be the wage premium of the major that *would be* earned by student i absent any major restrictions. Then aggregate stratification at those four campuses (following Equation 1) absent any restrictions can be written as

$$\frac{1}{|N_t|}\sum_{i\in N_t}\omega_i - \frac{1}{|U_t|}\sum_{i\in U_t}\omega_i. \tag{8}$$

Now let Γ be the set of majors that have been restricted since a base year, and let R_m be the set of years in which major $m \in \Gamma$ was restricted. Let $\beta^U_{R_m t}$ and $\beta^N_{R_m t}$ denote the effects of restrictions on the major premiums actually earned by URM and non-URM students who intended those majors in t. Given student i's predicted intention to major in m ($\hat{M}_{im}$), observed aggregate stratification is

$$\frac{1}{|N_t|}\sum_{i\in N_t}\Big(\omega_i + \sum_{m\in\Gamma}\beta^N_{R_m t}\hat{M}_{im}\Big) - \frac{1}{|U_t|}\sum_{i\in U_t}\Big(\omega_i + \sum_{m\in\Gamma}\beta^U_{R_m t}\hat{M}_{im}\Big). \tag{9}$$

[45] High-$\hat{M}_{im}$ URM students differentially exited STEM majors; see Figure A-23. Figure A-24 suggests that this may have decreased university costs by leading students toward lower-cost majors (Altonji and Zimmerman, 2019).

[46] Figure A-25 suggests that high-$\hat{M}_{im}$ URM students were not much more likely to exit m than high-$\hat{M}_{im}$ non-URM students, implying an important role for this second channel, though the confusing absence of a second difference in a regression of M_{im} on $\hat{M}_{im}$ – since the outcome M_{im} does not vary when $\hat{M}_{im} \approx 0$ – challenges the figure's straightforward interpretation.

We estimate the difference between Equations 8 and 9 – the contribution of new major restrictions to aggregate stratification – by imposing a series of simplifying assumptions. We abstract away from the small average differences in students' intentions to earn restricted majors by ethnicity (see Figure A-20) by replacing the non-URM average $\hat{M}_{im}$ with the URM average for each m. This permits us to estimate and employ a single causal coefficient between restrictions and ethnicity differences in major choice, $(\widehat{\beta^N_{R_m t} - \beta^U_{R_m t}})$, which we estimate to be about -0.09 – the average of the $\hat{\beta}_t$ coefficients 1-3 years following restriction implementation from a version of Figure 9 covering all students – when the restriction is in place.[47] Because we are unable to estimate major intentions many years after restrictions' implementation (due to changes in introductory curricula that decrease the reliability of our predicted major attainment), we also fix $\sum_{i \in U_t} \hat{M}_{im}$ at its average value 1-5 years following restrictions' implementation scaled by the contemporaneous URM population at that campus. We then simulate the contribution of newly-implemented major restrictions to the growth in UC college major stratification since 1995 as

$$SimGap_t \approx \widehat{(\beta^N - \beta^U)} \sum_{m \in \Gamma} \left(\mathbb{1}\{t \in R_m\} \frac{1}{|U_{\min(R_m)}|} \sum_{i \in U_{\min(R_m)}} \hat{M}_{im} \right), \tag{10}$$

an estimate of the contribution of post-1995 UC major restrictions to the ethnicity premium gap.[48]

Figure 10 shows that newly-implemented major restrictions alone effectively explain UC's growth in ethnic stratification between the 1995 and 2011 graduating classes, but that growth in stratification after 2011 – the first year in which graduating students largely chose majors after the '07-08 financial crisis – outstripped the effects of new restrictions. One important contributor to post-2011 stratification largely not captured by the simulation is the recent tightening of many high-premium UC departments' restrictions, likely in response to a post-crisis surge in student demand for lucrative majors (Blom, Cadena, and Keys, 2021). Some economics departments, for example, substantially sharpened the enforcement of their GPA restrictions immediately following the financial crisis (see Figures A-27 and A-28), as did computer science departments in the late 2010s.[49] Rising international student enrollment may have also played a role (Bound et al., 2020), with foreign students' enrollment share rising from 1 to 8 percent between 2011 and 2019 (see Figure A-29), though URM students could only be mechanically 'crowded out' from popular majors in

[47] Figure A-26 shows that the -0.09 statistic comes from the estimation of Equation 7 over all UC students.

[48] For restricted majors with no students either three years before or three years after the restriction's implementation – prohibiting estimation of the mean $\hat{M}_{im}$ in Equation 10 – we replace $\hat{M}_{im}$ with M_{mi}, observed major attainment, scaled by 1.26, the average ratio between predicted and actual URM majors 1-3 years following implementation.

[49] For example, Berkeley increased its computer science GPA threshold from 3.0 to 3.3 in 2015. Following Figures A-27 and A-28, we include Berkeley and Santa Cruz's economics departments' newly-binding 2007 and 2008 restrictions, respectively, as 'new' restrictions in the simulation.

the presence of major restrictions. We conclude that major restrictions alone can largely explain the recent growth in college major stratification at the observed University of California campuses.

7 Discussion: The Efficiency of Major Restrictions

The evidence presented above implies that major restrictions are a first-order contributor to growing ethnic stratification across college majors in the United States. Here we discuss the efficiency ramifications of major restriction policies. Major restrictions are likely to generate new inefficiencies relative to offering unrestricted majors by disproportionately reducing aggregate science major attainment (Murphy, Schleifer, and Vishny, 1991), muting potential match effects between students' major choices and their comparative preferences (Kirkeboen, Leuven, and Mogstad, 2016), and allocating majors away from interested and academically-promising students with limited pre-college educational opportunity, a group who are likely to receive above-average returns from lucrative college major attainment (Bleemer and Mehta, 2022). These efficiency costs could be counterbalanced by potential efficiency gains from GPA restrictions relative to the absence of meritocratic major allocation policies. In this section, we contextualize our analysis of restriction policies' equity ramifications with evidence that suggest the absence of such efficiency gains along three dimensions. We find no observable evidence that major restrictions improve match quality by allocating majors toward students with comparative advantages in the field, improve the signal or human capital value of restricted majors (for the students who remain in those degrees), or improve educational attainment or longer-run outcomes among the students who are excluded from restricted majors due to their low grades.

7.1 Do Major Restrictions Admit Students with Comparative Advantages?

Major restrictions may improve allocative efficiency by only permitting students with comparative advantage in the restricted majors to complete them, since restriction policies limit access on the basis of performance in departments' introductory courses. Indeed, Panel (a) of Figure 11 shows that major restrictions lead majors to enroll students with higher normed GPAs in their first-term courses in that discipline. This is partly by construction, since some of these courses would have been used to calculate the GPAs used to determine access to the restricted majors.

Panel (b), however, shows a near-identical effect on declared majors' average first-term normed GPAs in *other* disciplines.[50] The similarity between Panels (a) and (b) suggests that major restrictions do not *de*

[50] Mathematics and Statistics courses are considered in-discipline for all fields, since those courses are often required by (and

facto target students on the basis of their comparative advantages – that is, students with particular academic strengths in the restricted field – but instead target students whose academic performance is generally stronger across all fields (absolute advantage), a conclusion corroborated by the 0.001 Bayes factor on the change in the difference between in-discipline and out-of-discipline GPA.[51] The correlation between in- and out-of-discipline first-term normed GPAs is 0.84, suggesting that GPA restrictions offer little scope for revealing field-specific comparative advantages.[52]

7.2 Do Major Restrictions Increase Majors' Value-Added?

Excluding students with poor academic performance from restricted majors could improve the educational value of attaining the major for remaining students, either by increasing the major's signal value (as through 'prestige' departments, e.g. MacLeod and Urquiola, 2015) or by increasing the course's value through improved peer effects, more opportunities for active learning, or the introduction of more challenging material. We directly estimate variation in majors' wage value-added using models of the form $w_{iy} = \Omega_{m_i y} + \delta X_i + \epsilon_i$, where w_{iy} is freshman student i's California log annual wage in $y + 10$ (when the student is in her late 20s), X_i are student controls, and Ω_{my} are cohort-varying estimates of major m's wage value-added.[53]

Figure 12 presents difference-in-difference estimates following Equation 6 over two sets of Ω_{my} statistics in the years before and after new major restrictions. Panel (a) employs a version of Ω_{my} estimated absent any covariates, showing some noisy evidence that the average wages of restricted majors' students rise by about 5 percent following restrictions' implementation. This could occur for two reasons: (1) increased major value-added or (2) positive student selection into restricted majors, likely as a result of the restriction's leading negatively selected students to exit the major. Panel (b) includes both gender-interacted GPA fixed effects and ethnicity as covariates and bolsters the latter explanation: these observables alone absorb over three-quarters of the estimated wage increase (see the last two columns of Table 5), with a Bayes factor of 0.13 on the Δ coefficient estimated in Table 5. We conclude that major restrictions do not provide

included in the GPA calculations of) many restricted majors.

[51]The Bayes factor is estimated for the Δ term in Table 5 from a regression where the outcome is the difference between a major's average in-discipline and out-of-discipline GPA. Appendix K further shows that major restrictions do not differentially screen students with low student-major match quality in that major as estimated in a linear value-added model, nor do they lead exiting URM students toward majors where they have stronger student-major match quality.

[52]Major restrictions had little estimable effect on the 0.2 s.d. GPA gap between URM and non-URM students in both introductory and upper-division courses. See Figure A-31.

[53]If students have no wages ten years after initial enrollment, wages from 9 or 11 years after enrollment are included instead. If no such wages are available, the student is omitted. While prior work has found only small changes in California employment as a result of students' switching between majors due to major restrictions (Bleemer and Mehta, 2022), these findings could be biased by differential selection out of the labor market or cross-state migration.

measurable signal or human capital wage advantages to remaining students, though we cannot rule out the possibility the restrictions led to curricular changes that, while intellectually valuable, either did not improve students' wages or generated wage improvements that we are underpowered to measure.[54]

7.3 Do Major Restrictions Improve Exiting Students' Degree Attainment?

Major restrictions may improve the educational attainment or labor market outcomes of the low-GPA students pushed into alternative majors by restriction policies, perhaps because they are able to earn better grades elsewhere. One way to test this hypothesis would exploit the discontinuous access to restricted majors at the GPA threshold in a regression discontinuity model, which focuses exclusively on the outcomes of students on the margin of access to restricted majors. Bleemer and Mehta (2022) employ such a model to show that exclusion from one restricted major leads excluded students to sharply *lower* early-career wage earnings, generating additional efficiency *costs*. Unfortunately, Appendix L shows that the typical implementation of major restrictions does not generate the sharp discontinuities in major attainment that would be required to replicate this analysis for the majority of restricted majors.

As a result, we instead test this hypothesis by investigating ethnicity differences in the degree attainment of students who intended restricted majors in the years before and after restrictions' implementation following Equation 7, permitting estimation for all restricted students (including those away from the GPA margin). Figure 13 shows no evidence, overall or by ethnicity, that freshman students who intended restricted majors ended up becoming more likely to complete an undergraduate degree on time (in four years) or were able to complete their degree in fewer years, despite URM students flowing into less lucrative majors following restrictions' implementation.[55] We conclude that exclusion from restricted majors did not provide educational benefits to targeted students.

8 Conclusion

The gap in the economic value of college majors earned by underrepresented minority (URM) and non-URM graduates has increased more than three-fold since the mid-1990s, with Black and Hispanic graduates earning degrees that have 3 percent lower average earnings than those received by their white and Asian

[54]Grosz (2023) shows an even stronger result in the context of community college nursing programs, finding that replacing lotteries with selective admissions by academic preparation does not improve average student outcomes in the programs.

[55]The Bayes factors for comparisons between three years before and after the implementation period in panels (b) and (d), estimated from a 20% stratified random sample for efficiency, are 0.075 and 0.020.

peers. About two-thirds of this rise in ethnic stratification can be explained by the rise of within-institution stratification, which has in turn been largest at the large public research universities that enroll about a quarter of American college students. Those universities' increasingly prevalent major restriction policies have played an important role in stratifying their lucrative majors by ethnicity: evidence from four University of California campuses shows that major restrictions decrease URM enrollment by 20 percent and disproportionately push those URM students into less lucrative majors, largely explaining the rise in ethnic stratification since the 1990s. In the same manner that test-based meritocratic admissions policies inefficiently limit selective university access for disadvantaged applicants with poorer academic qualifications (Bleemer, 2022, 2024; Black, Denning, and Rothstein, 2023), major restrictions exacerbate equity gaps and hinder socioeconomic mobility without measurably enhancing efficiency. Future work should explore the political economy of major restriction implementation and cost-effective policy alternatives.

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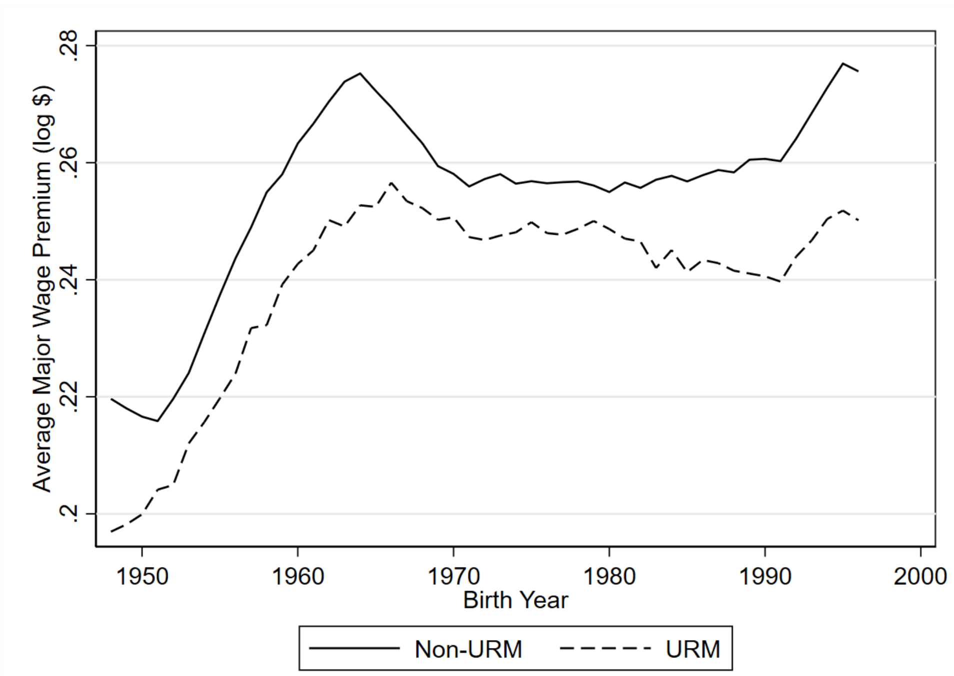
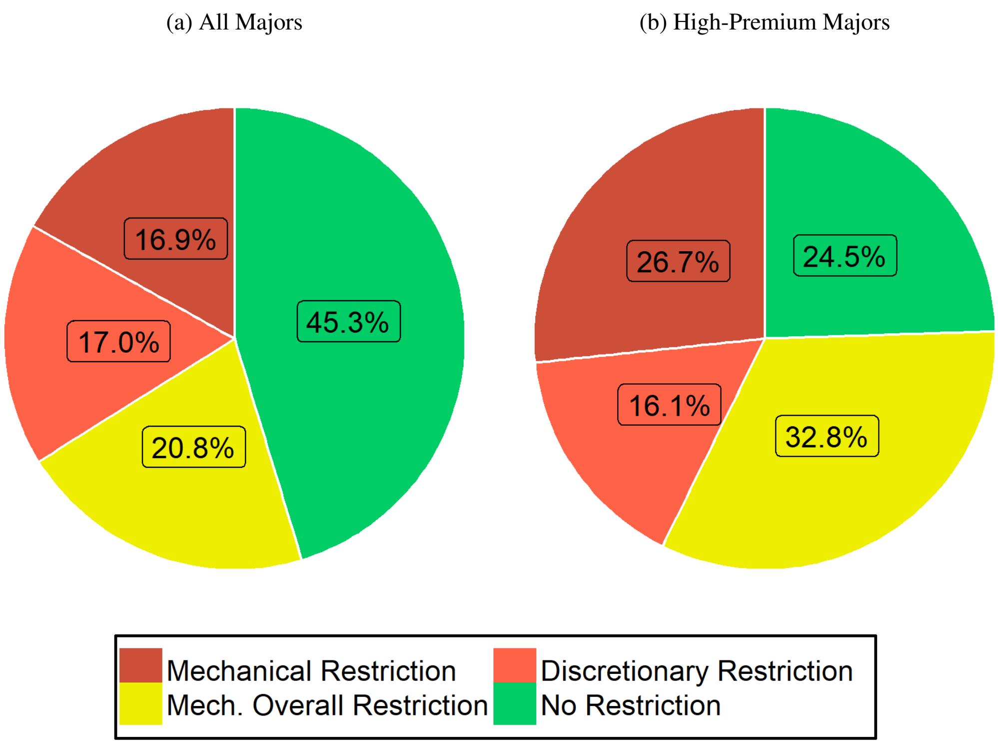
Figure 1: Average Premium of College Majors by Birth Cohort and Ethnicity



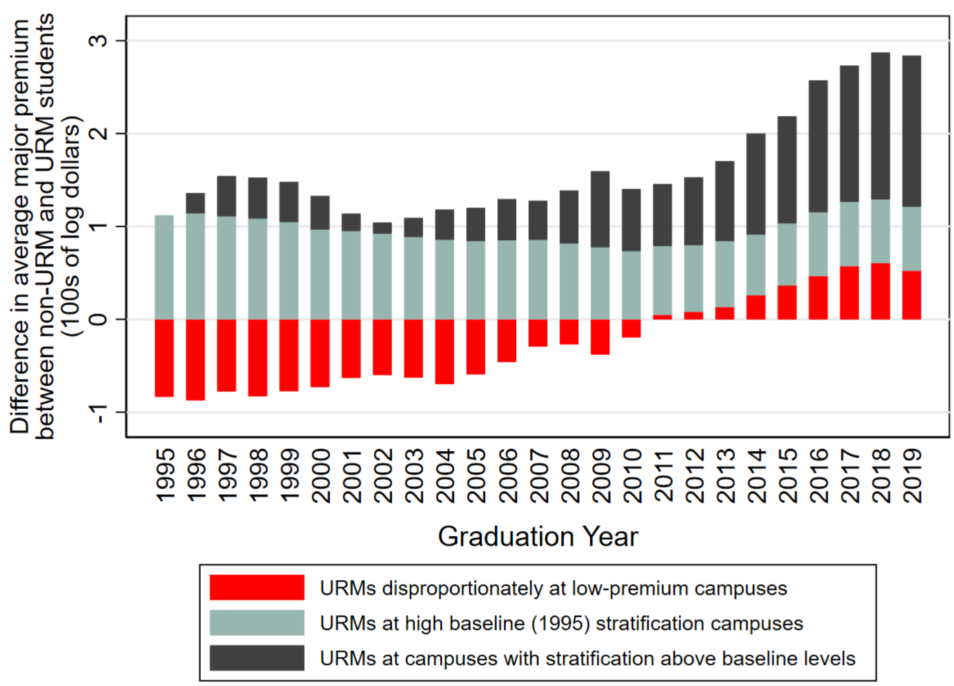
Note: Average college major wage premium of college graduates by birth cohort and ethnicity among 2009-2019 ACS respondents. Major premiums are estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, and year covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS; see Appendix B for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2020).

Figure 2: Prevalence of Major Restrictions at Public R1 Universities in the U.S.



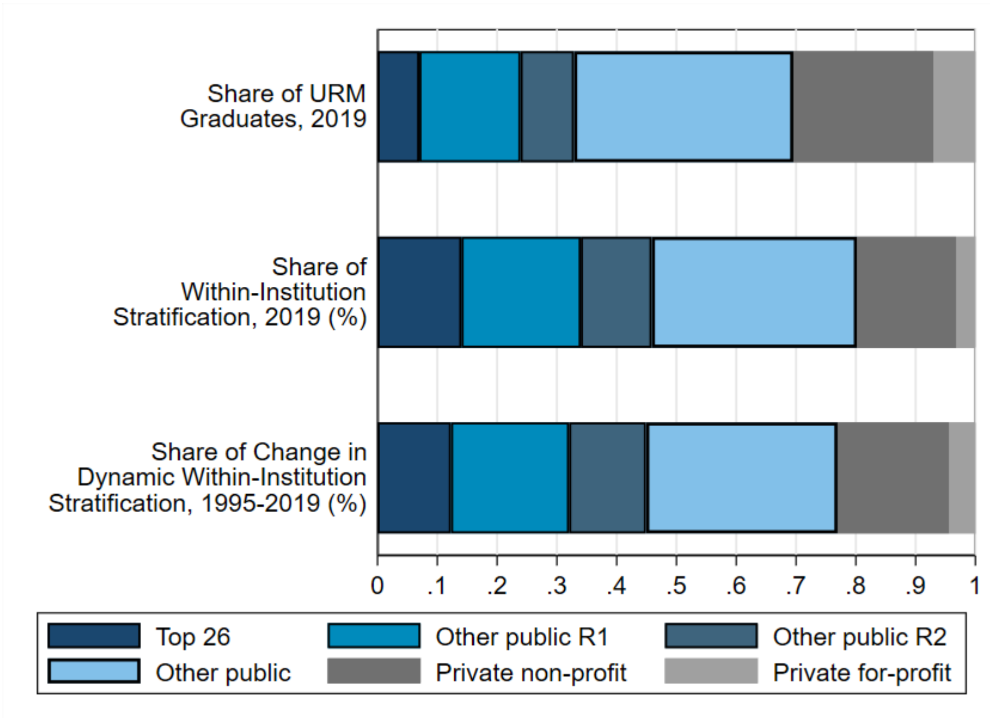
Note: Share of all graduate-weighted majors at the 106 public R1 universities with a mechanical major restriction (which require at least a 2.5 GPA in specified introductory courses), a discretionary major restriction (which require students to submit detailed applications and be selected for admission to the major), or an overall-GPA major restriction (which require students to earn at least a 2.5 GPA in all courses taken at the university and are omitted from our standard definition of restrictions). Majors with multiple types of restrictions are categorized into the first category appearing in that list for which they qualify. Shares are shown overall and in high-premium majors: engineering, computer science, business, economics, and nursing. Graduate counts from 2021; majors with fewer than 35 graduates (10 percent of graduate-weighted majors) are omitted. See Appendix A for details on restrictions. Source: IPEDS and university websites.

Figure 3: Annual Between- and Within-Institution Ethnic Stratification by Major



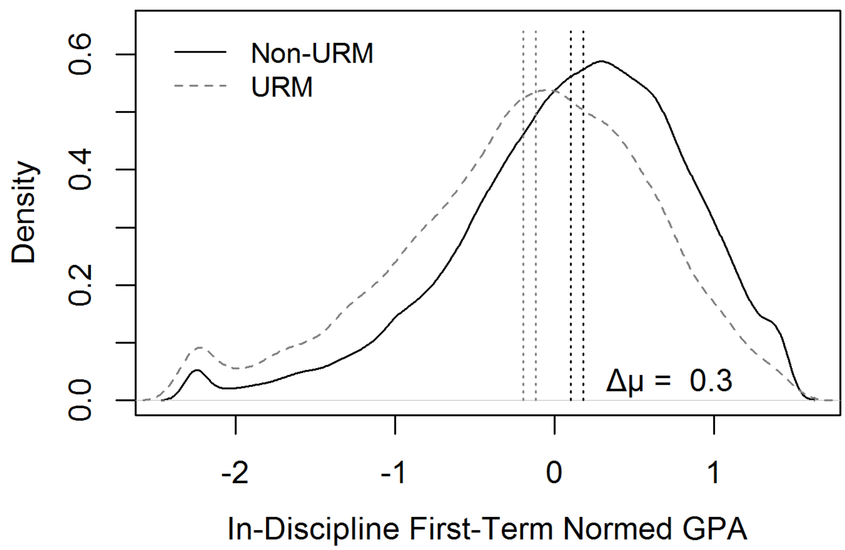
Note: Annual estimates of the three terms of Equation 5 for the 1995-2019 cohorts of college graduates, presenting average between-institution, static within-institution, and dynamic within-institution components of ethnic stratification across college majors in the U.S. higher education system. The static within-institution component fixes universities' level of stratification in 1995, while the dynamic component weights universities by their differential stratification (relative to 1995) in that year; otherwise the decomposition follows the traditional between-within pattern. The sample is limited to four-year degree-granting institutions in the 50 U.S. states. Average college major premiums are assumed to be equal across ethnicities in institution $\times$ year cells in which no graduates of one ethnicity are observed. Major premiums are estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, and year covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS; see Appendix B for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2020) and IPEDS.

Figure 4: Within-Institution Ethnic Stratification: Contributions of Sectors



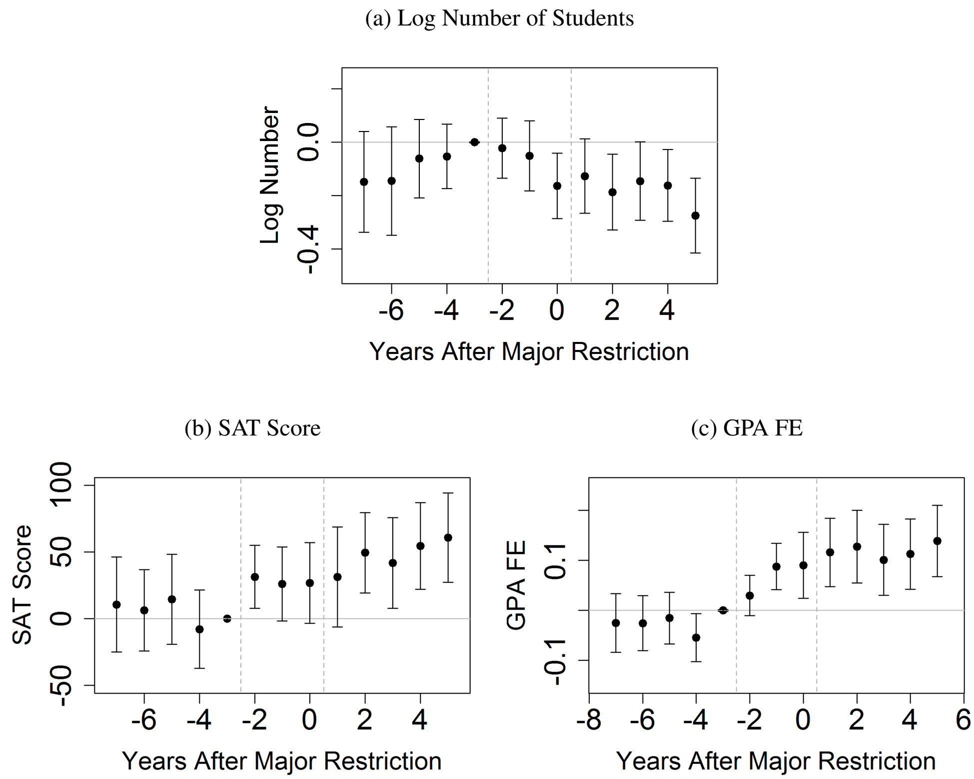
Note: The 2019 share of URM graduates, the 2019 contribution to within-institution stratification, and the contribution to the 1995-2019 change in dynamic within-institution stratification by higher education sector. For each sector T, its share of URM graduates is $P_t(T|U) = \Sigma_{i \in T} P_t(i|U)$. Sector contributions to within-institution stratification are sector subtotals of the second term in Equation 4, and sector contributions to the change in dynamic within-institution stratification are sector subtotals of the third term in Equation 5. The sample is limited to four-year degree-granting institutions in the 50 U.S. states. Average college major premiums are assumed to be equal across ethnicities in institution $\times$ year cells in which no graduates of one ethnicity are observed. Major premiums are estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, and year covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS; see Appendix B for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2020) and IPEDS.

Figure 5: Distribution of Introductory Course $nGPA$ by Ethnicity



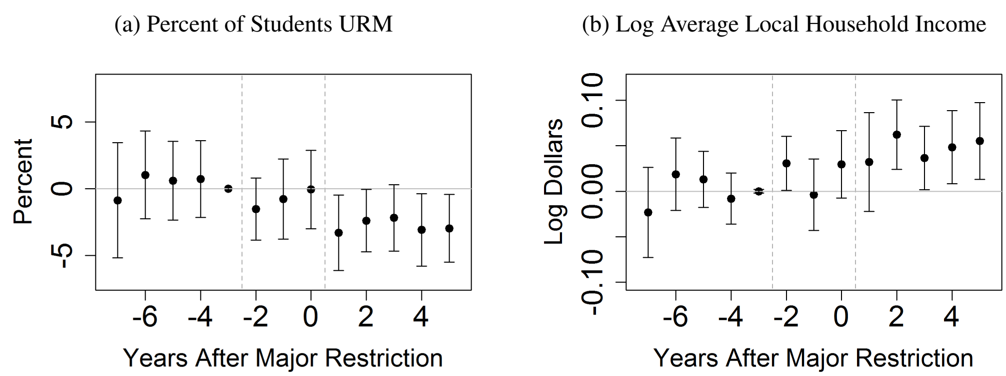
Note: Kernel density plots of winsorized normed first-term in-discipline grades (in standard deviations) among freshman students who declared restricted majors three cohorts before that major was restricted, by ethnicity. Dotted lines show the median (right) and mean (left) values by ethnicity. See the definition of $nGPA$ in Section 3; in-discipline courses include those in the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, or Professional) along with all math and statistics courses. Source: UC ClioMetric History Project Student Database.

Figure 6: Departments' Student Composition Before and After New Major Restrictions



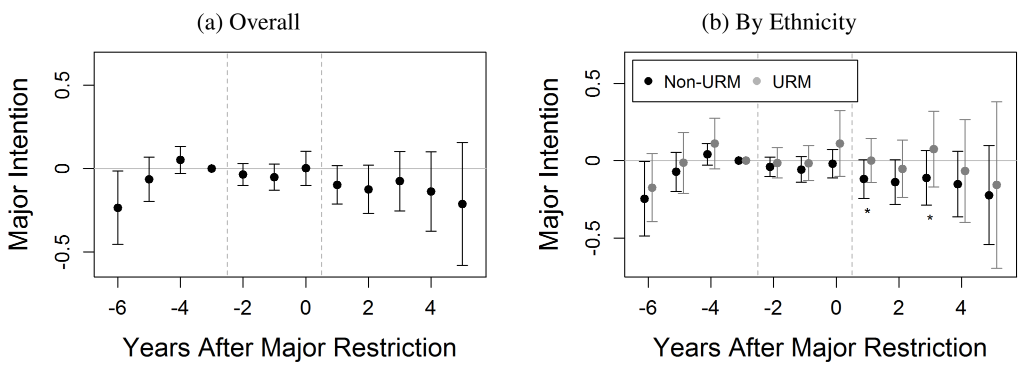
Note: Staggered difference-in-difference β estimates following Equation 6 of the characteristics of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted; standard errors are clustered by campus-major and assume that GPA fixed effects are observed without error; and 95 percent confidence intervals are shown. GPA fixed effect is the student effect from a two-way fixed effect model of grades on students and course-terms. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database.

Figure 7: Departments' Sociodemographic Composition Before and After New Major Restrictions



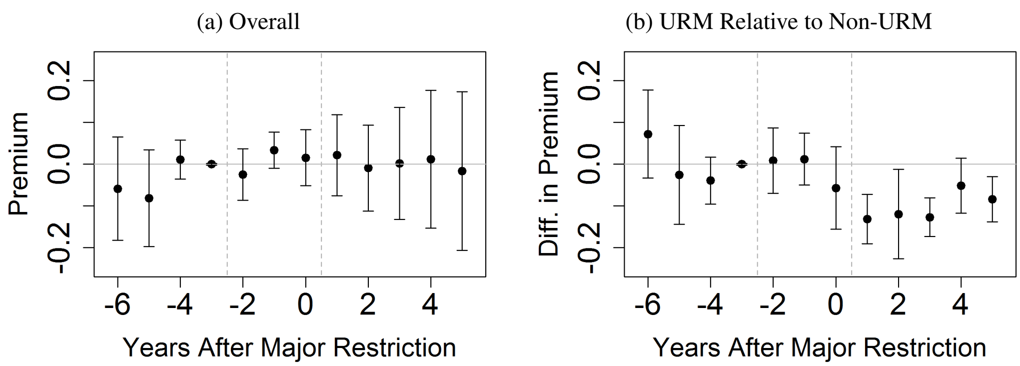
Note: Staggered difference-in-difference β estimates following Equation 6 of the URM share and the average local household income of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Average local household income is measured as the log of the CPI-adjusted mean adjusted gross income of tax-filing households in the student's Zip code in their first year of enrollment; see Appendix E. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted; standard errors are clustered by campus-major; and 95 percent confidence intervals are shown. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database and IRS SOI.

Figure 8: Estimated Changes in Students' Intentions for Restricted Majors



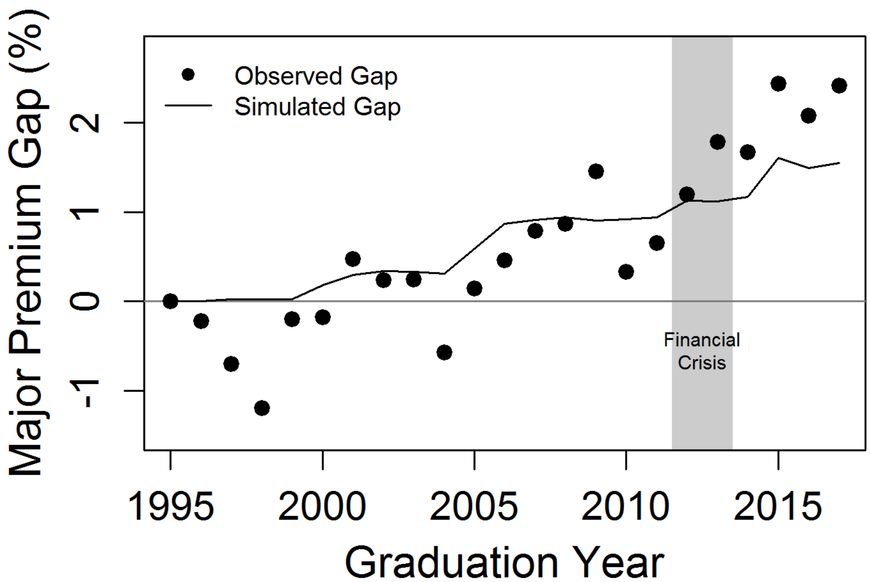
Note: Staggered difference-in-difference β_{it} estimates – overall and by URM ethnicity – of the average degree to which students exhibit their intention to earn newly-restricted majors ($\hat{M}_{im}$) before and after the implementation of the restriction, estimated over a stacked dataset of students i's major intentions in field m. See footnote 41 for the estimating equation. $\beta_{i,-3}$ is omitted; standard errors are two-way clustered by campus-majors m and by students i and assume that $\hat{M}_{im}$ is observed without error; and 95 percent confidence intervals are shown. Models include m fixed effects. Asterisks in (b) indicate within-period estimates by ethnicity that are statistically significantly different from each other at the 10 percent level using a two-sided test. Source: UC ClioMetric History Project Student Database.

Figure 9: Major Premiums (ω_i) of Students Who Intend a Major Before and After New Restrictions



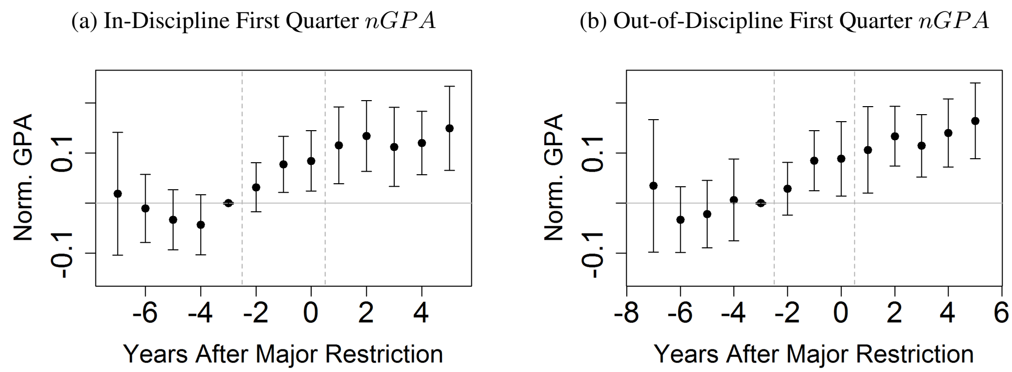
Note: Difference-in-difference β_{it} estimates following Equation 7 of the relationship between freshman students' intending the restricted major ($\hat{M}_{im}$) and the premium of the student's major (as defined in Appendix B) before and after the implementation of the restriction, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m. Panel (a) shows overall β estimates, while Panel (b) shows the differences between estimates changes for non-URM and URM students, both controlling for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection (See Figure A-22). Students who do not declare a major are omitted. β_{-3} is omitted; standard errors are two-way clustered by campus-majors m and by students i and assume that $\hat{M}_{im}$ is observed without error; and 95 percent confidence intervals are shown. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

Figure 10: Simulated Growth in UC Major Premium Gap from Only New Major Restrictions



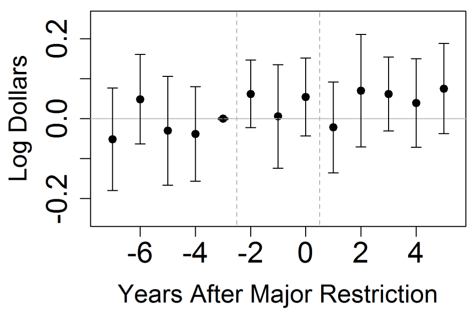
Note: The difference in the average major wage premium earned by URM and non-URM graduates of UC Berkeley, Davis, Santa Barbara, and Santa Cruz (relative to 1995) and the simulated difference that would be expected given the new major restrictions imposed by those campuses since 1995 following Equation 10. See text for details. Shaded region indicates the two cohorts of students who experienced the '07-08 financial crisis in their first year (assuming graduation after four years). Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

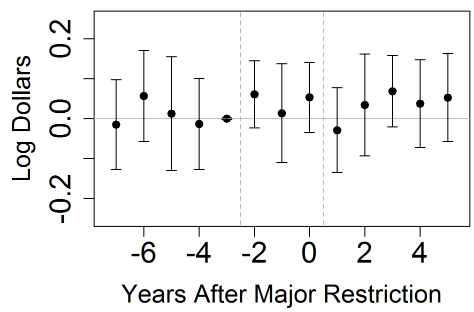
Figure 11: Major Restrictions Do Not Select Students with Measurable Comparative Advantages in the Field



Note: Staggered difference-in-difference β estimates following Equation 6 of department characteristics before and after the implementation of new major restriction policies, relative to other majors in that campus-year. The outcomes are defined as the average $nGPA$ (see Section 3) of freshman students with that declared major and cohort-year (defined by students' first year of enrollment) among courses taken in that discipline ('in-discipline') – expanding the discipline (Humanities, Social Sciences, Natural Sciences, Engineering, or Professional) to include math and statistics – or courses taken in other disciplines ('out-of-discipline'), allowing students to be included in more than one major's average if they have declared multiple majors. β_{-3} is omitted; standard errors are clustered by campus-major; and 95 percent confidence intervals are shown. Source: UC ClioMetric History Project Student Database.

Figure 12: Major Restrictions Do Not Measurably Increase Departments' Value-Added



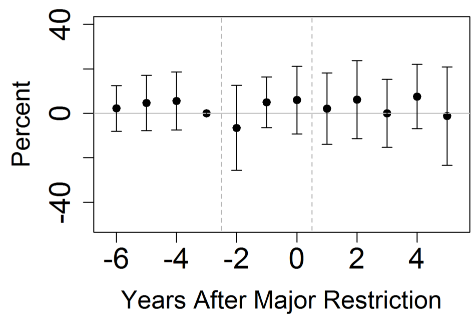


Note: Staggered difference-in-difference β estimates following Equation 6 of department characteristics before and after the implementation of new major restriction policies, relative to other majors in that campus-year. The outcomes are value-added fixed effects from linear regressions of wages on major-year fixed effects (stacking students with multiple majors) with either no controls (a) or controlling for students' GPA fixed effect interacted with gender and their ethnicity (b), where year is freshman students' first year of enrollment and wages are measured 10 years later. β_{-3} is omitted; standard errors are clustered by campus-major and assume that value-added and GPA fixed effects are observed without error; and 95 percent confidence intervals are shown. GPA fixed effect is the student effect from a two-way fixed effect model of grades on students and course-terms. Wage records exclude non-California, federal, and self-employment. Source: UC ClioMetric History Project Student Database and the California Employment Development Department (Bleemer and Mehta, 2020).

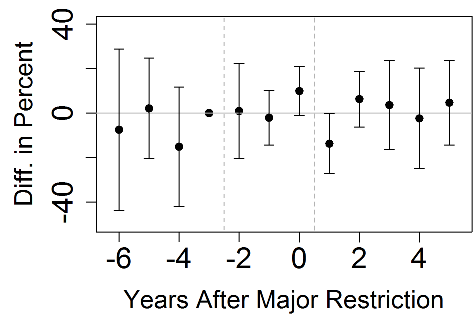
Figure 13: Major Restrictions Do Not Measurably Improve Excluded Students' Educational Outcomes

Panel A: On-Time Degree Attainment

(a) Overall

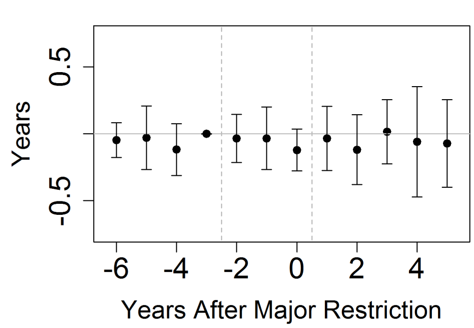


(b) URM Relative to Non-URM

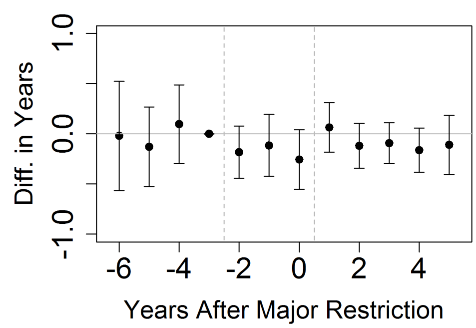


Panel B: Number of Years to Degree Attainment

(c) Overall



(d) URM Relative to Non-URM



Note: Difference-in-difference β_{it} estimates following Equation 7 of the relationship between freshman students' intending the restricted major ($\hat{M}_{im}$) and those students' on-time degree attainment and time-to-degree before and after the implementation of the restriction, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m. Panel A's outcome is defined as having earned a bachelor's degree within four academic years of initial enrollment; Panel B's is defined as the number of years between initial enrollment and degree attainment, conditional on earning a degree within eight years. Panels (a) and (c) shows overall β estimates, while Panels (b) and (d) shows the differences between estimates changes for non-URM and URM students, all controlling for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection (See Figure A-22). β_{-3} is omitted; standard errors are two-way clustered by campus-majors m and by students i; and 95 percent confidence intervals are shown. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database.

Table 1: 2002-2019 Major Restrictions at the Top 25 US&WR Ranked Public Universities

Univ.	# of Students	Comp. Science	Economics	Finance	Mech. Eng.	Nursing
Cornell†	14,907	*2.5*	<u>**2.7**</u>	<u>**3.3**</u>; A	<u>**2.5**</u>; A	*
UCLA	31,002	**3.5**; A	2.5	**3.3**	**3.5**; A	HS
UC Berkeley	30,853	**3.3**	3.0	A	*3.0; A*	*
Virginia	16,655	-	-	A	<u>**2.5**</u>	A
Michigan	29,821	-	-	A	*2.5*; A	A
UC Santa Barbara	22,186	**3.2**	**2.85**	**2.85**	<u>**A**</u>	*
UNC – Chapel Hill	18,862	-	-	<u>**3.0**</u>; A	*	A
UC Irvine	29,307	<u>**3.0**</u>	<u>**2.5**</u>	<u>**3.0**</u>; <u>**A**</u>	3.0	<u>**A**</u>
Georgia Tech	15,573	-	-	-	-	*
Florida	35,247	-	<u>**3.0**</u>	3.0	<u>**2.8**</u>	**3.3**
William and Mary	6,285	-	-	**2.5**; A	*	*
UC Davis	30,145	<u>**3.0**</u>	-	*	<u>**2.8**</u>	*
UC San Diego	28,587	*3.3; A*	<u>**2.5**</u>	*	**A**	*
Georgia	28,848	-	A	**A**	**A**	*
UI – Urbana-Champ.	33,955	**3.75**; A	-	<u>**A**</u>	**3.75**; A	*
UT – Austin	40,492	A	-	<u>**3.25**</u>; <u>**A**</u>	<u>**3.0**</u>; A	**3.0**; A
UW – Madison	32,196	-	-	<u>**2.75**</u>; A	A	**2.75**; A
Ohio State	45,946	**3.2**	-	<u>**3.0**</u>; A	**3.4**	A
Purdue	31,006	-	<u>**2.75**</u>	-	<u>**3.2**</u>; A	*2.75*
Rutgers	35,641	-	-	A	<u>**A**</u>	**HS**
Penn. State	40,835	HS	-	**3.2**	HS	HS
Washington	31,331	A	**A**	2.5; A	A	<u>**2.8**</u>; A
Connecticut	19,241	<u>**3.0**</u>; <u>**A**</u>	-	A	<u>**3.0**</u>; <u>**A**</u>	<u>**3.0**</u>; A
UMD – Coll. Park	29,868	-	-	A	<u>**2.7**</u>	<u>**3.0**</u>; A
Clemson	19,402	-	-	-	<u>**HS**</u>	A
Texas A&M	53,065	<u>**2.75**</u>; <u>**A**</u>	<u>**3.0**</u>	**3.5**; <u>**A**</u>	<u>**3.5**</u>; <u>**A**</u>	<u>**A**</u>

Note: The Fall 2019 undergraduate enrollment and major restriction policies for enrolled students at the top 25 public universities as ranked by US News and World Report in 2019, in addition to Cornell University (which is †part-public). A number indicates the minimum GPA required in department-specified courses for current students to declare the major, omitting restrictions of C+ or lower. Restrictions are <u>underlined</u> if they did not exist in 2002, **in bold** if they are (nominally) tighter in 2019 than in 2002, and *in italics* if they are looser in 2019 than in 2002, where 'HS' is tighter than 'A'. Chosen majors are the top-earning majors reported in Table 3 of Altonji, Blom, and Meghir (2012) averaged between male and female students, omitting Electrical Engineering due to its similarity with Computer Science. Finance is replaced with Business Administration, Business Economics, and Economics and Accounting majors when the institution does not offer a finance major.

#.##: The minimum GPA required in department-specified courses for students to declare the major. **A**: Students must submit a successful internal application after initial enrollment in order to earn the major. **HS**: Students must be directly admitted from high school to the major (with elevated admissions standards). *****: Major is not offered.

Source: University and department websites (in August 2019 and in 2002 using the Wayback Machine) and US News & World Report.

Table 2: Observational Relationship between Major Restrictions and URM Stratification

	URM Share in Major				Eth. Stratification		
	All Public R1 Inst.			Top 25	All Public R1 Inst.		
Any Restriction	-1.3				2.3		
	(0.4)				(1.0)		
Mechanical Restriction		-1.8	-2.0	-2.3		4.0	3.7
		(0.5)	(0.5)	(0.8)		(1.6)	(1.7)
Discretionary Restriction		-0.7	-0.6	-0.7		1.2	1.6
		(0.4)	(0.4)	(0.9)		(0.8)	(0.8)
Overall-GPA Mech. Restriction			-0.5	-0.7			-2.6
			(0.3)	(0.6)			(1.1)
Institution FE	X	X	X	X			
Field of Study FE	X	X	X	X			
$\bar{Y}$	20.9	20.9	20.9	19.0	3.1	3.1	3.1
Observations	4,350	4,350	4,350	1,272	106	106	106
Level of Observation	Institution-Major				Institution		

Note: The first four columns provide estimates from OLS regressions of a major's 2021 URM (Black, Hispanic, or Native American) graduate share on whether the major is restricted, over all majors with at least 35 2021 graduates at the 106 R1 public universities in the United States. Each of these models includes institution and four-digit CIP code fixed effects. The remaining columns provide estimates from institution-level OLS regressions of 2021 ethnic stratification by major at public R1 institutions ($S_{2021}(i)$ in Equation 3) on the shares of their graduates completing majors to which entry is restricted. Mechanical restrictions limit access to students with below-threshold grades in specified introductory courses; discretionary restrictions limit access to students based on detailed applications, generally including both measured academic preparation along with essays and other materials. Overall-GPA mechanical restrictions limit access to students with below-threshold average grades in all of their courses at the university (defined to exclude majors with specific mechanical restrictions). See Appendix A for details on restrictions. Standard errors in parentheses are two-way clustered by institution and four-digit CIP, omitting 49 singleton clusters, in columns 1-4 and robust in 5-7. The minimum considered GPA threshold is 2.5. The full sample includes 106 US public IPEDS-designated R1 universities (we deem Cornell non-public). The "Top 25" schools are as ranked by USN&WR after dropping Cornell and William & Mary (which is not R1).

Source: IPEDS and university websites.

Table 3: Major Restrictions Ever Imposed at Four UC Campuses

	Years				Years		
Major	First	Last	Rule	Major	First	Last	Rule
			UC Berkeley				
Business°	1970	-	A	Art	1993	-	A/3.3
Economics	1976	-	3.0	Psychology	2003	-	3.2
Comp. Sci.	1979	2007	3.0	Pub. Health	2004	-	A/2.7
Pol. Economy	1980	2004	3.0-3.2	Oper. Res.†	2005	-	3.2
Media Stud.†	1980	-	A/3.2	Env. Econ.	2009	-	2.7
Biochem.	1988	1989	2.7	Comp. Sci.*	2013	-	3.0-3.3
			UC Davis				
Statistics°	1982	2004	3.0	Comm.	2001	2013	2.5
Land. Arch.°	1986	-	A	Human Dev.	2001	-	2.5
Psychology°	1989	-	2.5	Manag. Econ.	2001	2011	2.8
Int. Relations	1992	2013	2.5	Biotech.	2007	-	2.5
Comp. Sci.	1997	2004	2.75	Design*	2011	2013	2.6
Exer. Sci.*	1997	2000	2.5	Mech. Eng.*	2011	2014	2.8
Vit. and Eno.	1998	-	2.5	Comp. Sci.*	2016	-	3.0
Ferment. Sci.*	1998	2000	2.5				
			UC Santa Barbara				
Comp. Sci.°	<1983	2014	A/3.2	Poli. Sci.	1988	-	2.6
Comm.°†	1983	-	2.5-3.0	Biology	1996	-	‡
Economics°	1984	-	2.7-2.85	Law and Soc.	1997	2006	2.5
Psychology°	1985	-	2.5-2.75	Biopsych.	2001	-	2.7-2.75
Mathematics°	1985	-	2.5	Comp. Eng.	2003	2013	3
Elec. Eng.°	1986	1996	3	Fin. Math.	2005	-	2.5
			UC Santa Cruz				
Economics	2002	-	2.8	Biochem.	2011	-	2.5
Physics	2008	-	2.7	Cog. Sci.†	2011	-	2.5
Psychology	2011	-	2.7	Appl. Ling.*	2016	-	2.7
Chemistry	2011	-	2.5				

Note: Characteristics of every mechanical and discretionary major restriction policy ever implemented by the four UC campuses, omitting GPA requirements below 2.5. Does not include majors that are open to students admitted to a specific college but closed to students admitted to different colleges, like most engineering majors; in any case, those policies changed little in this period. † indicates that the major has had restrictions since within two years of its creation; * indicates that the restriction only lasted (or has only lasted) for a small number of years, either of which lead the major to be omitted from analysis below; and ° indicates that the major was implemented prior to the beginning of our data. The reported years are one year before the first or last year in which the restriction is mentioned in the campus's course catalog. **A**: Students must submit a successful internal application after initial enrollment in order to earn the major. ‡ UCSB Biology implements a complex and highly-stratified major restriction that requires multiple course-catalog pages to explain (with dozens of alternative paths leading to different major specialties), though ultimately never requires GPA performance over 2.0 in any course.

Source: University of California course catalogs.

Table 4: Descriptive Statistics of UC Campus Majors

	All	Berkeley	Davis	Santa Barbara	Santa Cruz	3 Years Before Restriction
Number of Majors	58 [21]	64 [6]	76 [29]	50 [10]	39 [5]	
# Students	64 [81]	71 [83]	47 [67]	81 [100]	65 [73]	128 [125]
% Female	55 [23]	52 [22]	57 [24]	56 [24]	55 [23]	50 [23]
% URM	20 [18]	19 [16]	18 [18]	23 [21]	21 [16]	13 [9]
Sample Size						
Events	25	7	7	6	5	
Obs.	483,044	175,913	114,905	113,475	78,751	
Obs. in Sample*	451,664	166,171	102,107	108,086	75,300	

Note: Descriptive statistics of the average number of departments (with at least one freshman student) in each covered university-year, average number of freshman students per department, and average percent of female and URM freshman students across departments, for all departments and for departments three years before instituting major restrictions. Standard deviations in brackets. Events indicate number of new observable major restrictions (see Table 3) and major-year observations overall or in the estimation sample (* restricting to major-years with at least 20 freshman students), where year is defined as students' first year of enrollment.

Source: UC ClioMetric History Project Student Database.

Table 5: Summary of New Major Restrictions' Impact on Department Composition

	Log Num. of Students	SAT Score	GPA FE	Percent URM	Fam. Inc. (Log $)	Percent Female	First Term $nGPA$[1]		Average Wage[2]	
							In Disc.	Out of Disc.	No Cov.	GPA Cov.
4-7 Years Before Restriction	-0.10 (0.07)	5.3 (14.3)	-0.03 (0.02)	0.43 (1.14)	0.01 (0.01)	1.19 (1.51)	-0.02 (0.03)	-0.00 (0.03)	-0.02 (0.04)	0.01 (0.04)
Transition Years	-0.09 (0.05)	27.2 (13.0)	0.07 (0.02)	-0.63 (1.19)	0.02 (0.01)	2.18 (1.46)	0.07 (0.02)	0.07 (0.03)	0.04 (0.04)	0.04 (0.04)
1-5 Years After Restriction	-0.18 (0.06)	45.4 (15.8)	0.12 (0.03)	-2.56 (1.13)	0.04 (0.02)	1.47 (1.68)	0.13 (0.03)	0.13 (0.03)	0.04 (0.05)	0.02 (0.04)
Fixed Effects	X	X	X	X	X	X	X	X	X	X
Observations	4,963	3,648	4,962	4,905	4,848	4,905	4,811	4,726	3,635	3,595
$\bar{Y}$	4.2	1820		19.7	11.5	54.2	0.1	0.0		
Δ (Post-Pre)[3]	-0.08 (0.08)	40.1 (12.7)	0.15 (0.03)	-2.99 (0.86)	0.04 (0.02)	0.28 (1.38)	0.15 (0.03)	0.14 (0.03)	0.054 (0.039)	0.013 (0.038)
M.C. p-value[4]	[0.336]	[0.001]	[0.000]	[0.025]	[0.029]	[0.845]	[0.000]	[0.000]	[0.255]	[0.763]
Joint p-value of $\beta_{-4} - \beta_{-7} = 0$[5]	0.52	0.44	0.13	0.87	0.28	0.45	0.68	0.60	0.73	0.81

Note: Staggered difference-in-difference β estimates following Equation 6 of the measured characteristics of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Standard errors clustered by campus-major in parentheses. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. "Before" indicates 4-7 years before initial restriction implementation; "Transition" includes the year of implementation and two years earlier; and "After" includes 1-5 years following implementation. β_{-3} is omitted. Students can be included in more than one major's average if they have declared multiple majors. GPA fixed effect is the student effect from a two-way fixed effect model of grades on students and course-terms. Family income is measured by the CPI-adjusted mean adjusted gross income of tax-filing households in the student's Zip code in their first year of enrollment; see Appendix E. [1]See definition of first-term $nGPA$ in Section 3; in-discipline courses include those taken in the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, and Professional) plus Mathematics and Statistics courses, while out-of-discipline courses include all remaining courses. [2]Value-added fixed effects from linear regressions of wages on major-year fixed effects (stacking students with multiple majors) with either no covariates (left) or controlling for students' GPA fixed effect interacted with gender and their ethnicity (right), where year is freshman students' first year of enrollment and wages are measured 10 years later. [3]The difference between "After" and "Before" Major Restriction β coefficients, with standard error in parentheses. [4]An exact two-sided p-value on Δ (Post-Pre) from 1,000 Monte Carlo draws of placebo major restrictions, to account for mechanical correlations as students move between departments in general equilibrium. [5]A chi-squared test of $\beta_{-7} = \beta_{-6} = \beta_{-5} = \beta_{-4} = 0$ in the fully-disaggregated Equation 6 (as shown in, e.g., Figure 6), to test for parallel pre-trends.

Source: UC ClioMetric History Project Student Database, the UC Corporate Student System, and the California Employment Development Department.

For Online Publication:
Online Appendix

College Major Restrictions and Student Stratification

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Appendix A: Major Restrictions at Public R1 Universities

In order to measure the prevalence of major restriction policies in the United States, we compiled a comprehensive cross-sectional dataset of all major restrictions imposed at the 106 public R1 universities in Fall 2022. Public R1 universities awarded 31 percent of all four-year undergraduate degrees in the U.S. in 2021. We limited ourselves to the 4,399 six-digit CIP codes in which each respective university awarded at least 35 degrees in 2021 (as recorded in IPEDS), which covers 90 percent of graduate-weighted majors at public R1 universities.

A team of research assistants scoured the websites of each department at each public R1 university to identify the restrictions on declaring each covered major. The RAs were instructed to record restrictions from the perspective of an undeclared freshman student in the university's equivalent of a college of letters and sciences. The RAs also recorded restrictions that did not fit neatly into our restriction categories; for example, a small number of majors impose minimum SAT requirements. There were some cases where it was unclear which major matched to each CIP code; the RAs indicated those majors as possible mismatches, though there is no observable difference between the URM enrollments of matched and *potentially* mismatched majors.[56]

Figure 2 summarizes the resulting dataset. The dataset will become available when this study is published.

Appendix B: College Major Premium Statistics

College major choice has important causal consequences for subsequent labor market outcomes, generating wage differences which may exhibit even higher variance than the distribution of value-added across more- and less-selective American universities (Kirkeboen, Leuven, and Mogstad, 2016). This appendix details the observational estimates of college-major wage-premiums used in our study, shows that they are insensitive to choices regarding the estimation sample and specification, and provides two sets of evidence that compelling a student to complete an observationally-less-lucrative major causally reduces their subsequent earnings.

We index the economic quality of college majors by estimating the following model over 2009-2019 college-educated and employed ACS respondents between age 35 and 45 by OLS:

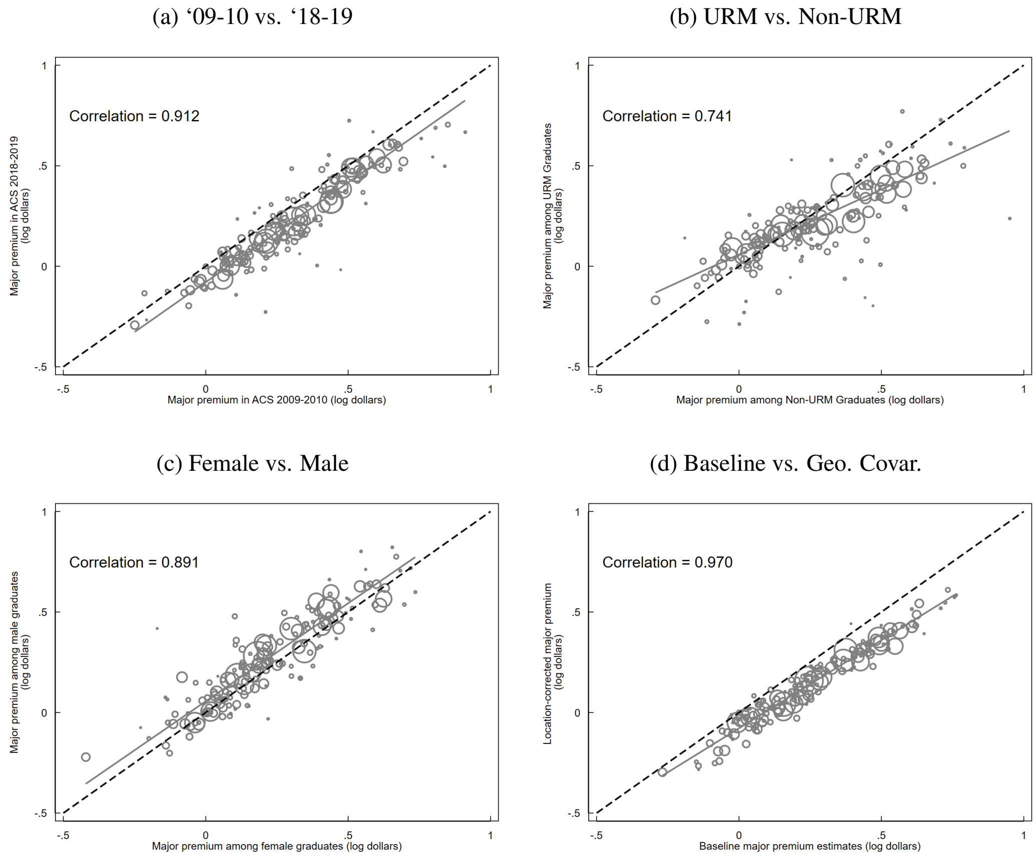
$$Wage_{it} = \omega_{m_i} + \alpha_{g_i e_i a_i d_i t} + \epsilon_{it} \tag{BB-1}$$

where log wage income $Wage_{it}$ is projected onto an additive function of the major earned by i and the full set of interactions between indicators for i's gender (g_i), six ethnicity categories (e_i), age (a_i), whether i earned more than one college major (d_i), and the survey year t. Respondents who report more than one major are randomly assigned to one of their majors. Our baseline estimates of ω_m are presented for each ACS major category in Table A-2.

We test the sensitivity of these ω_m coefficients and the resulting cohort trends in major choice by estimating a series of alternative specifications. First, we test for changes over time in estimated major premiums by separately estimating Equation BB-1 over the 2009-2010 and 2018-2019 ACS cohorts. Panel

[56]Dropping the potentially mismatched observations slightly increases the magnitude of the coefficient on restrictions.

Figure BB-1: Stability of Alternative College Major Premium Specifications



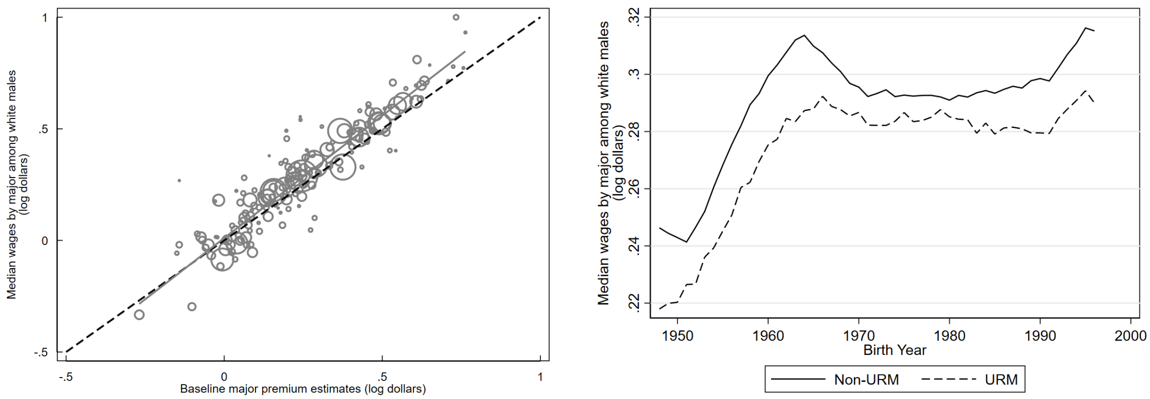
Note: This figure shows that alternative definitions of average college major premium – either using different samples of ACS students or absorbing local geographic wage variation – yield qualitatively-similar major premium coefficients. This figure correlates major premium coefficients estimated using a sequence of different subsamples and estimation strategies. In this study’s baseline specification (Equation BB-1), premiums are estimated by regressing log wages on major indicators and covariates over employees aged 35-45 in the 2009-2019 ACS. Panels (a) to (c) compare premium estimates across 2009-2010 and 2018-2019 ACS respondent subsamples, URM and non-URM subsamples, and female and male subsamples. Panels (d) and (e) respectively compare the baseline premium estimates to coefficients estimated in the presence of PUMA geographic fixed effects, and to coefficients from the baseline specification reestimated after dropping all workers whose majors were imputed from their occupation, age and gender in the ACS data. Source: The American Community Survey (Ruggles et al., 2020).

(a) of Figure BB-1 shows that the two sets of college major premium estimates are strongly correlated (0.91) parallel to the 45-degree line, confirming the understanding that major premiums have little changed in recent decades (Sloane, Hurst, and Black, 2021; Altonji, Kahn, and Speer, 2014) and consistent with evidence that ordinal major premiums have changed little over the past century (Bleemer and Quincy, 2024). Panel (b) shows a somewhat weaker and flatter relationship when Equation BB-1 is estimated separately among URM and non-URM workers, with a greater wage spread among non-URM workers, though the correlation (0.74) remains very strong. Panel (c) shows little evidence of differences in relative major-specific wage returns by gender (0.89), while Panel (d) shows that adding local region (PUMA) indicators to Equation BB-1 – to absorb, for example, cost-of-living differences across localities – yields near-identical estimates of ω_m (0.97). Correlations between our baseline estimates and each of these alternate premium statistics exceed 0.95 except for the correlation with the attenuated estimates from the URM subsample

Figure BB-2: Comparison between Major Premium Estimates and Median Wages by Major

(a) Median vs. Baseline

(b) Log Median Wages



Note: This figure shows that replacing ω_m with median wages by major yields qualitatively-similar major premium coefficients and stratification trends. Along the x-axis of Panel (a), the baseline ω_m coefficients are estimated by regressing log wages on major indicators and covariates over employees aged 35-45 in the 2009-2019 ACS. Along the y-axis of Panel (a) and in Panel (b), each major is characterized by within-major median wages estimated on a sample of males aged 45-55 who have worked at least 27 weeks in the last year, following (Sloane, Hurst, and Black, 2021). Source: The American Community Survey (Ruggles et al., 2020).

(roughly 1/6 of graduates), which is 0.80.

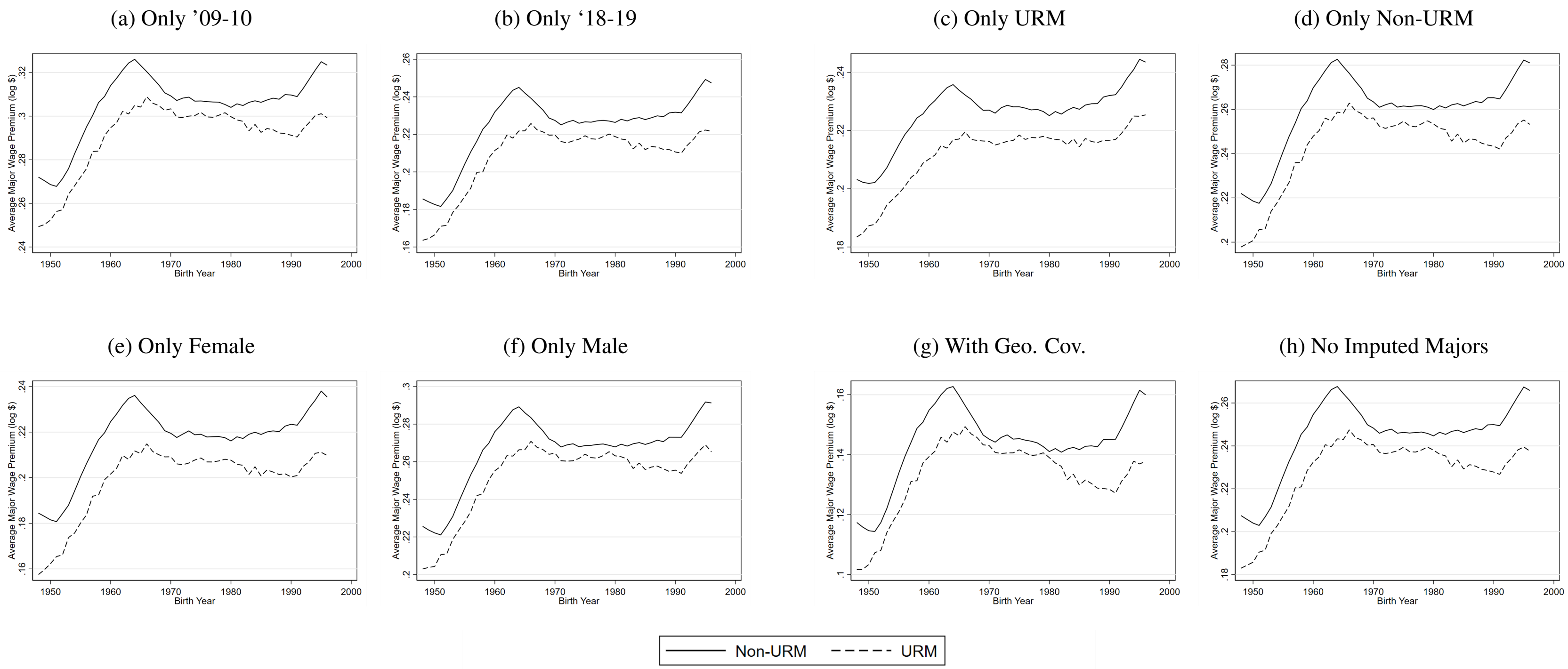
Figure BB-2 shows that replacing our baseline estimates of ω_m with the unconditional median wage of employed college graduates by major yields a highly-similar economic quality index across majors (0.92), suggesting that the wage differences across majors are generally unrelated to the fixed characteristics included in Equation BB-1 as covariates.[57] We restrict the median-wage sample to (1) native (2) white (3) male workers who (4) worked at least 27 weeks in the previous year for 30 hours per week and (5) excluding ACS respondents whose college majors are imputed, following Sloane, Hurst, and Black (2021) (who estimate gender-specific major premium statistics and focus on trends in the gender college major gap by cohort).[58]

Figure BB-3 replicates the average premium-by-cohort-by-ethnicity trends shown in Figure 1 using each of these alternative specifications. Though the relative levels of URM and non-URM college graduates' average major premiums over time vary by specification, all eight figures exhibit the same pattern described in the present study's introduction: the college major gap between URM and non-URM students had narrowed and was largely unchanging in the years leading up to the 1980 birth cohort, but has been steadily widening in the years since. This finding appears qualitatively robust to each alternative major premium specification.

[57] Median wage statistics are adjusted for inflation using the CPI for all urban wage and clerical workers.

[58] Excluding imputed respondents from our main specification results in unchanged coefficients (correlation 0.995).

Figure BB-3: Stratification trends using Alternative College Major Premium Estimates



Note: This figure shows that alternative definitions of average college major premiums – either using different samples of ACS students or different estimation strategies – yields qualitatively similar stratification patterns since the 1950s birth cohorts. This figure depicts average college major premiums by birth cohort and ethnicity among all college graduates (as in Figure 1) using a sequence of different subsamples and estimation strategies. Solid (dashed) lines estimate expected major premiums for non-URM (URM) workers. In the baseline specification, premiums are estimated by regressions of log wages on major indicators and control variables as explained in Appendix B over wage employees aged 35-45 in the 2009-2019 American Community Survey. Panels (a) to (f) restrict the premium estimation sample to 2009-2010 and 2018-2019 ACS respondents, URM and non-URM respondents, and female and male respondents. Premiums in Panel (g) are estimated in the presence of PUMA geographic fixed effects. Panel (h) estimates premiums after dropping the roughly 12% of the sample whose college majors were imputed from their occupation, age and gender. Source: The American Community Survey (Ruggles et al., 2020).

B.1 Causal Interpretation of ω_m

While a causal interpretation of observational wage differences between majors (ω_m) is potentially complicated by selection bias (Arcidiacono, 2004), two sets of evidence suggest that such bias is small. First, Figure A-1 presents quasi-experimental evidence from a GPA restriction case study (see Bleemer and Mehta, 2022) of a forecast coefficient for ω_m of 1.07 (s.e. 0.36): earning a major with higher ω_m by 0.1 causally increases students' wages by 0.107 log points in that context. When estimated separately by race, non-URM (URM) students are estimated to have forecast coefficients of 1.27 (0.77). Dahl, Rooth, and Stenberg (2023) uses a series of major access discontinuities in Sweden to estimate a forecast coefficient for ω_m of 0.954 (s.e. 0.132).[59] This evidence – the only available experimental or quasi-experimental estimates in the literature – suggest that differences in majors' ω_m are highly predictive of the causal effect of switching between those majors for on-the-margin students.

Second, Table BB-1 provides observational evidence that when major-wage-premiums that correct for additional measures of aptitude and opportunity are regressed on ω_m, the resulting forecast coefficients are insignificantly different from 1 (see Chetty et al. (2020) for methodology). This is so whether a less- or more-disaggregated major classification is used, and whether the sample excludes white or non-white students. The forecast coefficients only fall below one when using the smaller and likely attenuated non-white sample. If the added aptitude and opportunity measures capture selection, this indicates that between 86% and 100% of the variation in our baseline wage-premium estimates is causal.[60]

We emphasize that even if the true forecast bias of ω_m is more substantial (that is, the true forecast coefficient is < 1), this evidence suggests that an important share of cross-major wage variation is causal, implying that there are meaningful labor market consequences to rising ethnic stratification across majors.

Appendix C: Growth in Between-Institution College Major Stratification

While this study primarily focuses on the growth of within-institution ethnic stratification since the late 1990s, Figure 3 shows that over 30 percent of the increase in overall stratification has been driven by *between*-institution changes in where URM students enroll. One reason for these between-institution shifts in URM enrollment was the dramatic rise in URM college enrollment in the period (mirroring URM students' growing proportion of U.S. high school graduates); URM representation among recipients of 4-year degrees grew 80% between 1995 and 2019, but by differing proportions in each institution type (Figure CC-1). This appendix provides evidence showing that between-institution stratification increased because low-premium institutions – that is, institutions whose graduates tended to earn below-average-premium majors – absorbed a disproportionately large share of this influx of URM college students. It also confirms that institutions' average major premiums are strongly positively correlated with their selectivity: as additional URM students flowed into less-selective universities, those schools' low average major premiums drove part of the observed growth in ethnic stratification across majors over the past 25 years.

[59] Several other quasi-experimental studies have shown evidence of large wage returns to high-ω_m majors (e.g. Kirkeboen, Leuven, and Mogstad, 2016; Andrews, Imberman, and Lovenheim, 2017; Canaan and Mouganie, 2018), but these studies do not directly estimate the magnitude of ω_m's forecast bias.

[60] Table BB-1 uses data from NLSY 1997, which provides measures of aptitude and parental income not available to us in the ACS (Ruggles et al., 2020), but also allows premium estimation for a small number of coarse and detailed majors. Disciplines account for half of the variation in wage premiums across ACS majors.

Table BB-1: Selection-on-Observables Forecast Coefficients of Average Wages by Major

Major Type:	Disciplines				Detailed Majors			
Add'l Cov.:	None	Fam. Inc.	+AFQT	+Race	None	Fam. Inc.	+AFQT	+Race
***Panel A**: Full Sample*								
Forecast Coef.		1.02	1.03	1.03		1.00	1.01	1.00
β		(0.02)	(0.02)	(0.03)		(0.06)	(0.08)	(0.08)
Obs.			7				14	
1st Stg. Obs.			842				753	
***Panel B**: White Students*								
Forecast Coef.	1.02	1.02	1.05	1.05	1.01	1.01	1.03	1.03
	(0.02)	(0.01)	(0.04)	(0.04)	(0.04)	(0.04)	(0.07)	(0.07)
Obs.		7				14		
1st Stg. Obs.		589				524		
***Panel C**: Non-White Students*								
Full-Sample	0.91	0.99	0.92	0.88	0.94	0.92	0.88	0.86
β	(0.07)	(0.08)	(0.12)	(0.13)	(0.16)	(0.14)	(0.15)	(0.15)
Obs.		7				14		
1st Stg. Obs.		253				229		

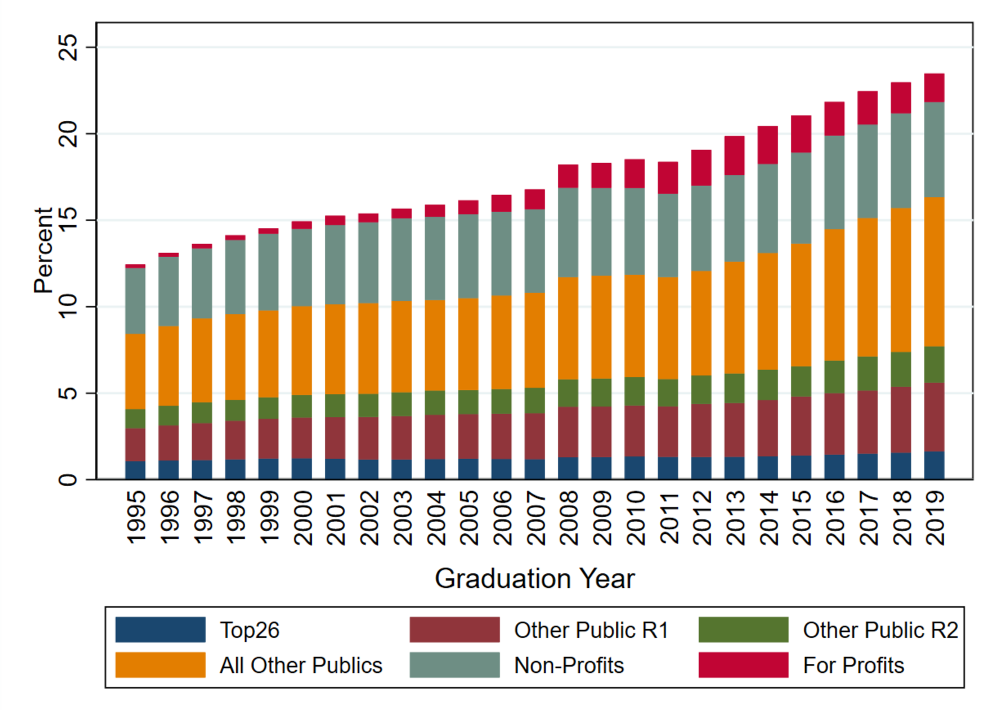
Note: Each cell in this table displays the result of an OLS regression of average earnings in each of 10 college disciplines (or 66 detailed majors), adjusted for successively more individual-level controls, on average earnings in the same college disciplines (or majors). The first-stage regressions in which we regress average earnings on college discipline fixed effects and the additional covariates are available from the authors; they include birth cohort fixed effects and use survey weights. The individual-level controls (in order of the column header) are: a third-order polynomial in family income rank, a third-order polynomial pre-college test score rank, and indicators for Black and other non-white races. Panel A uses the same baseline sample to estimate both sets of college major fixed effects; Panel B restricts the left-hand-side set of major fixed effects to be estimated in a first-stage regression of only white graduates; and Panel C restricts the left-hand-side set of major fixed effects to non-white graduates. '1st Stg. Obs.' reports the number of observations in the first-stage regression that produces the left-hand-side fixed effects. Regressions are weighted by the total number of within-sample respondents who select each major. The sample is restricted to male respondents with at least a college degree and to majors or disciplines reported by at least 20 such respondents. Standard errors are robust and do not correct for first-stage sampling error. The baseline first-stage fixed effect regression for disciplines (detailed majors) has an adjusted R^2 of 0.07 (0.09), while the fully-controlled regression has an adjusted R^2 of 0.12 (0.14). Panel A is replicated from Bleemer and Quincy (2024).

Source: NLSY97.

We measure the degree to which URM students' enrollment shifted across university sectors by recalculating the two-way decomposition presented in Equation 4 across six university sectors (T) instead of across the full set of 3,600 higher education institutions. Panel (a) of Figure CC-3 shows that URM students became much more likely to graduate from private for-profit colleges and much less likely to graduate from private non-profit institutions between the mid-1990s and 2019. This shift from more- to less-prestigious private institutions was much less pronounced among non-URM students. Panel (a) also shows that public institutions' combined share of URM graduates rose only slightly, while the top 26 public institutions' share declined.

Panel (b) of Figure CC-3 shows that in 2019, the least-prestigious (for-profit and other public) institutions that absorbed most of the influx of URM students tended to specialize in low-wage majors. The

Figure CC-1: Contributions to URM Representation, by Sector and Graduation Year



Note: This figure shows that the URM share of college students has been steadily increasing since the 1990s, with particular growth among non-research public universities and for-profit universities. This figure depicts the fraction of all four-year college degree completers who belong to underrepresented minority groups (Black, Hispanic or Native American), by university sector and year of graduation. Source: IPEDS.

average premium of college majors awarded by for-profit universities fell from among the highest among the university sectors in 1995 to the lowest by 2019. This wage drop is particularly pronounced among URM students, indicating that the influx of URM students into the for-profit sector was overwhelmingly accommodated by the expansion of low-premium majors. Panel B also confirms that the average premium of majors awarded at public universities are positively associated with their research prestige.

These trends – replicated in Panels A and B of Table CC-1 – suggest that the growing accommodation of URM students at less prestigious private and public institutions lifted between-institution stratification. However, Panel C of Table CC-1 shows that between-*sector* stratification increased by only about 0.29 percentage points, though Figure 3 shows that between-*institution* stratification increased by 1.36 percentage points. This indicates that most of the shift of URM students toward lower-premium institutions between 1995 and 2019 occurred within university sectors. Accordingly, Panel D shows large increases in within-sector stratification.

Figure CC-2 reconfirms that between-institution stratification was overwhelmingly driven by within-sector reallocation, especially among the most research intensive public institutions. Institutions with higher average major premiums graduated an increasing share of non-URM students (relative to URM students) over time. This relationship is particularly weak among non-profit institutions, and its strength tracks public institutions' research intensity. As in the main text, this again emphasizes the importance of public research universities as loci of ethnic stratification in higher education, here in the case of URM students switching

Table CC-1: Stratification Between and Within Sectors of Institutions

	Top 26 Publics	Other Public R1	Public R2	All Other Publics	Non-Profit Schools	For-Profit Schools	All Institutions
Panel A: Probability of graduating from each sector by URM status							
URM							
1995	0.086	0.154	0.088	0.350	0.306	0.017	1.000
2019	0.070	0.169	0.089	0.367	0.234	0.070	1.000
Other							
1995	0.105	0.204	0.084	0.275	0.322	0.009	1.000
2019	0.103	0.201	0.080	0.274	0.300	0.042	1.000
Panel B: Average college major premium by sector and URM status (log dollars)							
URM							
1995	0.259	0.227	0.223	0.212	0.234	0.284	0.227
2019	0.276	0.244	0.222	0.209	0.227	0.220	0.226
Other							
1995	0.274	0.237	0.225	0.202	0.235	0.264	0.230
2019	0.325	0.275	0.250	0.226	0.247	0.230	0.254
Panel C: Between-within sector decomposition of aggregate stratification (100s of log dollars)							
Between term							
1995	0.53	1.20	-0.09	-1.51	0.38	-0.20	0.31
2019	1.07	0.87	-0.23	-2.09	1.62	-0.65	0.60
Within term							
1995	0.12	0.16	0.02	-0.32	0.02	-0.03	-0.03
2019	0.34	0.52	0.25	0.61	0.47	0.07	2.25
Total							
1995	0.65	1.36	-0.07	-1.83	0.40	-0.23	0.28
2019	1.41	1.39	0.02	-1.49	2.09	-0.58	2.84
Panel D: Ethnic stratification within sector (100s of log dollars)							
1995	1.42	1.02	0.27	-0.90	0.06	-1.97	0.28
2019	4.89	3.05	2.80	1.62	2.02	0.97	2.84
Change	3.47	2.03	2.53	2.56	1.94	2.93	2.56

Note: This table shows several average probabilities used to calculate the decomposition presented in Figure 3 as well as a sectoral decomposition showing that between-institution stratification increased mostly within sector. Statistics in Panel A are $P_t(T|R) \equiv \Sigma_{i \in T} P_t(i|R)$ for sector T. Panels B and D are $E_t[\omega_m|R,T]$ and $S_T \equiv \Delta_R[E_t(\omega_m|T)]$ respectively, measured relative to General Agriculture. Panel C adapts Equation 4 to sum across sectors rather than institutions; the between term is $E_t(\omega_m|T,N)\Delta_r[P_t(T|r)]$ and the within term is $P_t(T|U)S_t(T)$, treating T as an aggregate unit.

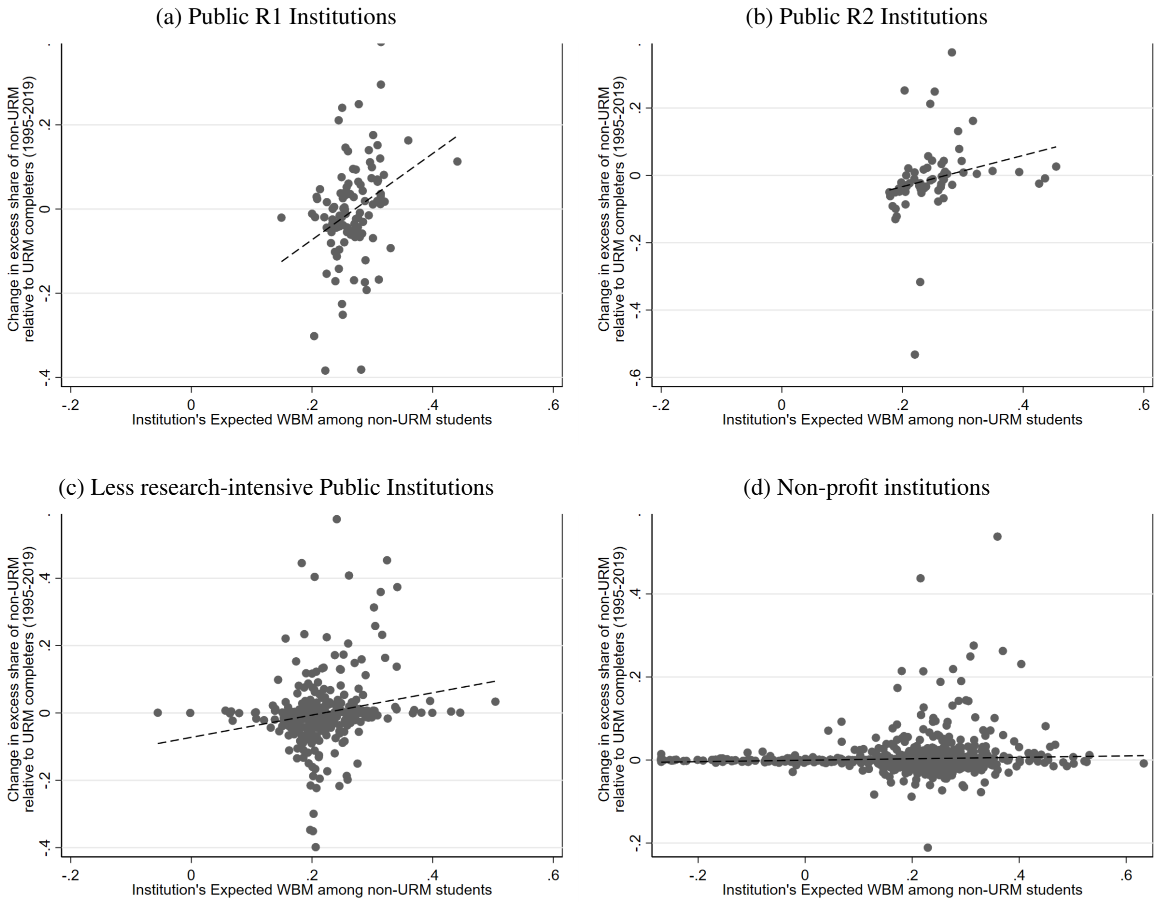
Source: 2009-2019 American Community Survey (Ruggles et al., 2020) and IPEDS.

from higher- to lower-premium public research universities in the 2000s and 2010s.

Finally, Table CC-2 confirms that the average premium of majors awarded to an institution's graduates is strongly positively correlated with measures of its selectivity, including 6-year graduation rates (among URM, non-URM and all students), SAT and ACT scores (at the 75th percentile), and freshman applicant rejection rates (one minus the admission rate).

In summary: between-institution college major stratification increased because of two patterns in the absorption of a dramatic influx of URM students, each of which has been studied elsewhere. First, the public sector took on more URM students but tended to absorb them in less prestigious institutions – in

Figure CC-2: Stratification between institutions, by Sector



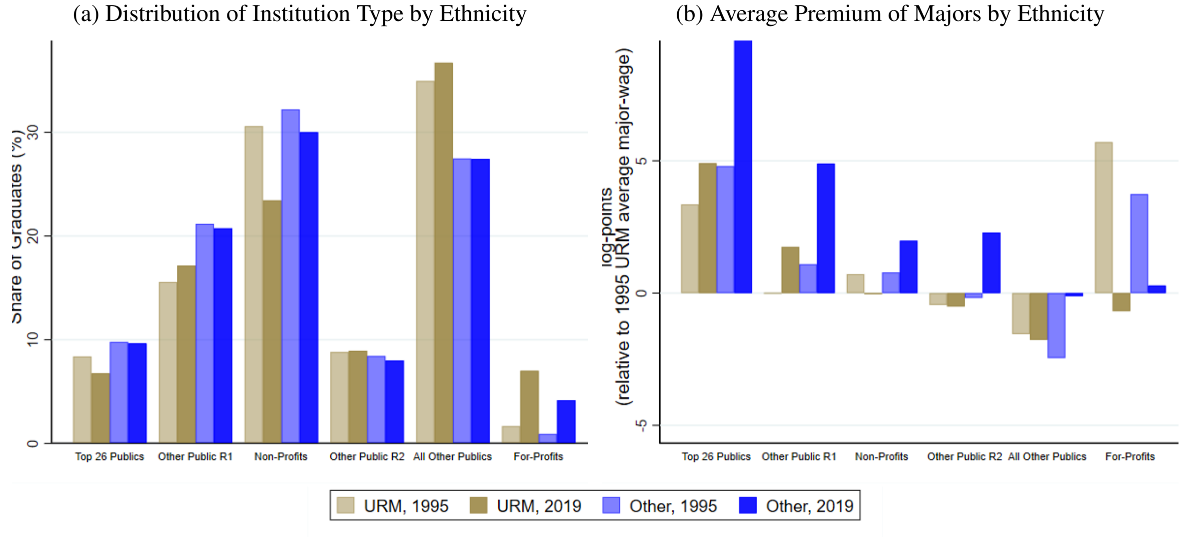
Note: This figure shows that that share of non-URM students graduating from more prestigious public institutions grew faster than the share of URM students graduating from them. The vertical axis is the difference between non-URM and URM students, in the 1995-2019 change in an institution's share of graduates of each ethnic group (i.e., $\Delta_R[P_{2019}(i|R) - P_{1995}(i|R)]$). The horizontal axis depicts the average college wage premium awarded by those institutions, averaged between 1995 and 2019. Source: IPEDS and ACS (Ruggles et al., 2020).

part as a result of the declining prevalence of affirmative action (Bleemer, 2022), the persistent use of legacy admissions policies (Arcidiacono, Kinsler, and Ransom, 2022), and other university admissions policies that disadvantage URM applicants on average – that focused on lower-premium majors. Second, private non-profit universities did not expand their URM populations proportionally, so many of them earned degrees from for-profits that in turn expanded their offerings of lower-premium majors, especially for URM students (Deming, Goldin, and Katz, 2012).

Appendix D: Pre-College Academic Opportunity of URM UC Students

The most likely mechanism explaining the disproportionate impact of major restriction policies on URM and lower-income UC students is those students' relatively poorer pre-college academic opportunity, which could lead them to lower introductory course grades in restricted fields. Figure A-18 and Appendix G

Figure CC-3: US Graduates' Institutions and Average Major Premiums by Ethnicity, 1995-2019



Note: This figure shows that over the last 25 years, URM students increasingly graduate from less-selective universities that award lower-value college majors, and that non-URM students' average major premiums have risen more (or fallen less) than those of their URM peers across all institution types. Panel (a) shows the share of URM and non-URM (other) students who earn degrees at each institution type in 1995 or 2019. Panel (b) shows the average premium of majors (as defined in Appendix B) earned by URM and non-URM students who complete degrees in each institution type in 1995 or 2019, relative to the average major premium earned by URM students in 1995. Institution types partition US higher education and have been ordered left-to-right by selectivity. Source: IPEDS and ACS (Ruggles et al., 2020).

Table CC-2: Correlation between University Selectivity and Average Major Premium

Selectivity Measure	Correlation	N
6-year Graduation rate	0.310	1,924
6-year Non-URM Graduation rate	0.310	1,905
Rejection Rate	0.198	1,710
SAT Verbal Score	0.440	1,197
SAT Math Score	0.505	1,197
ACT Combined Score	0.466	1,236
ACT English Score	0.416	1,154
ACT Math Score	0.490	1,154

Note: This table shows that the ranking of universities by their average major premium is strongly positively correlated with their ranking by several traditional measures of university selectivity. Spearman correlations between 2019 measures of institutional selectivity and the average premium of their 2019 graduates' college majors. Test score statistics are institutions' reported 75th percentile of scores.

Source: The 2009-2019 American Community Survey (Ruggles et al., 2020) and IPEDS.

present evidence favoring that explanation. In this appendix, we directly measure academic opportunity gaps by investigating the characteristics and available courses at URM and non-URM UC students' high schools.

As discussed in Appendix E, we match freshman California-resident UC students to their high schools' course availability and composition starting in 1997, including whether the school was public or private and, at public schools, (a) which Advanced Placement (AP) courses were offered, (b) what share of students at

Table DD-1: Relative Characteristics of Disadvantaged UC Students

		All HS's		Public HS's	
	Zip Log Mean AGI	Private HS	# Unique AP Courses	% Grads UC-Eligible	% Students FRPM-Eligible
URM	-0.37 (0.00)	-0.42 (0.16)	-1.47 (0.02)	-9.35 (0.11)	21.63 (0.15)
Black	-0.40 (0.00)	-0.10 (0.37)	-1.08 (0.06)	-5.70 (0.27)	21.76 (0.40)
Hispanic	-0.44 (0.00)	-3.97 (0.19)	-1.42 (0.03)	-8.77 (0.13)	25.87 (0.19)
Asian	-0.12 (0.00)	-6.88 (0.16)	0.18 (0.02)	2.04 (0.11)	6.35 (0.16)
Lower-Income	-0.76 (0.00)	-4.75 (0.12)	-2.57 (0.02)	-15.33 (0.08)	29.85 (0.11)
Non-URM $\bar{Y}$	11.5	12.7	11.2	52.3	28.1
Observations	276,956	249,942	210,509	182,658	118,824

Note: This table shows that URM UC students come from relatively lower-income and lower-opportunity high schools than non-URM students, though not to the same degree as the gap between students from below-median- and above-median-income Zip codes. This table presents regression coefficients from three separate regressions of an outcome on either a URM indicator, three disaggregated race indicators, or an indicator for lower-income students. The regressions are estimated across 1997-2016 UC freshman California-resident students. The outcomes reflect either the average income in the student's home Zip code or characteristics of their high school measured in the year of their high school graduation. Freshman UC students are identified by age at matriculation (see Appendix E); nine percent fail to match to observed high schools and are omitted, mostly because they actually transferred from community college. AP course data for students who graduated in 2002, 2003, and 2010 are unavailable. Private high schools are assumed to offer the same number of AP courses as the 90th percentile of public high schools in that year, weighted by UC enrollment. The final two columns restrict to students coming from public high school graduates, and report on the shares of students at those schools eligible for automatic admission under California's top-x% program and free or reduced-price meals. 'Lower-income' refers to all students coming from a Zip code in the bottom half of average household incomes among students in their campus-cohort. Zip code household income is defined as CPI-adjusted mean adjusted gross income of tax-filing households in the student's Zip code in their first year of enrollment; see Appendix E. Source: UC ClioMetric History Project Student Database, IRS SOI, and the California Department of Education.

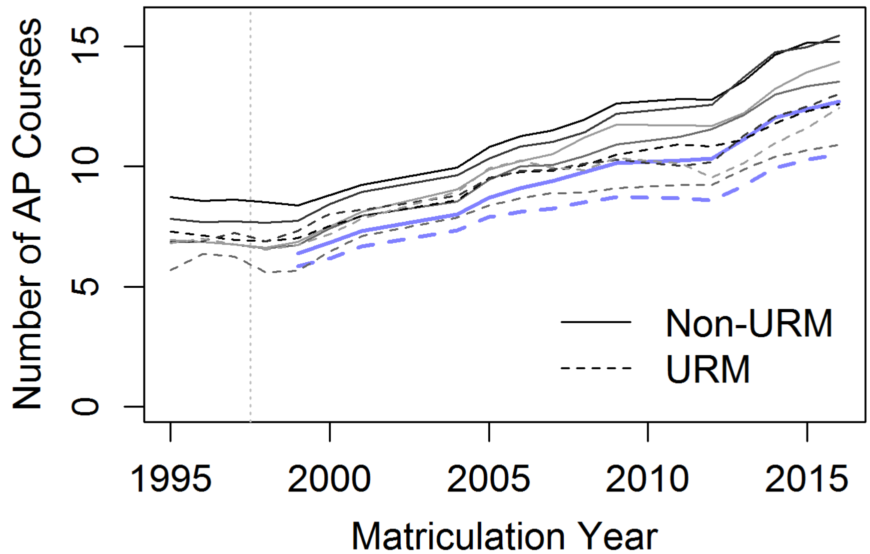
the school were eligible for free- or reduced-price meals (a standard measure of school-level socioeconomic status), and (c) what share of graduates satisfied the University of California's eligibility requirements (a measure of academic preparedness).[61]

Table DD-1 shows that URM students attended high school in Zip codes with almost 40 percent lower household incomes than those of their non-URM peers. While they were about equally likely to attend private high schools, their schools offered about 15 percent fewer AP courses. Among the 90 percent who attended public high school, their schools had far lower UC-eligible graduation rates and much higher shares of low-income students than their non-URM peers. As in other contexts, Black and Hispanic UC students share similar backgrounds in terms of educational opportunity, while Asian UC students' backgrounds share more in common with their white peers. These gaps are uniformly larger when comparing students from Zip codes with below- and above-median household incomes; the URM/non-URM gap tends to be about two-thirds the magnitude of the below/above-median household income gap.

AP course availability differs more between URM and non-URM students than between UC campuses. Figure DD-1 plots the average number of AP courses available by URM status at the high schools attended by all Californians (blue) and at each of the four UC campuses, which are included with the darkest lines

[61] We also observe college-level International Baccalaureate (IB) course, but we omit these from our analysis because of IB's rarity in California; 94 percent of college-level courses in California are AP.

Figure DD-1: Number of AP Courses Available to UC Students by Ethnicity

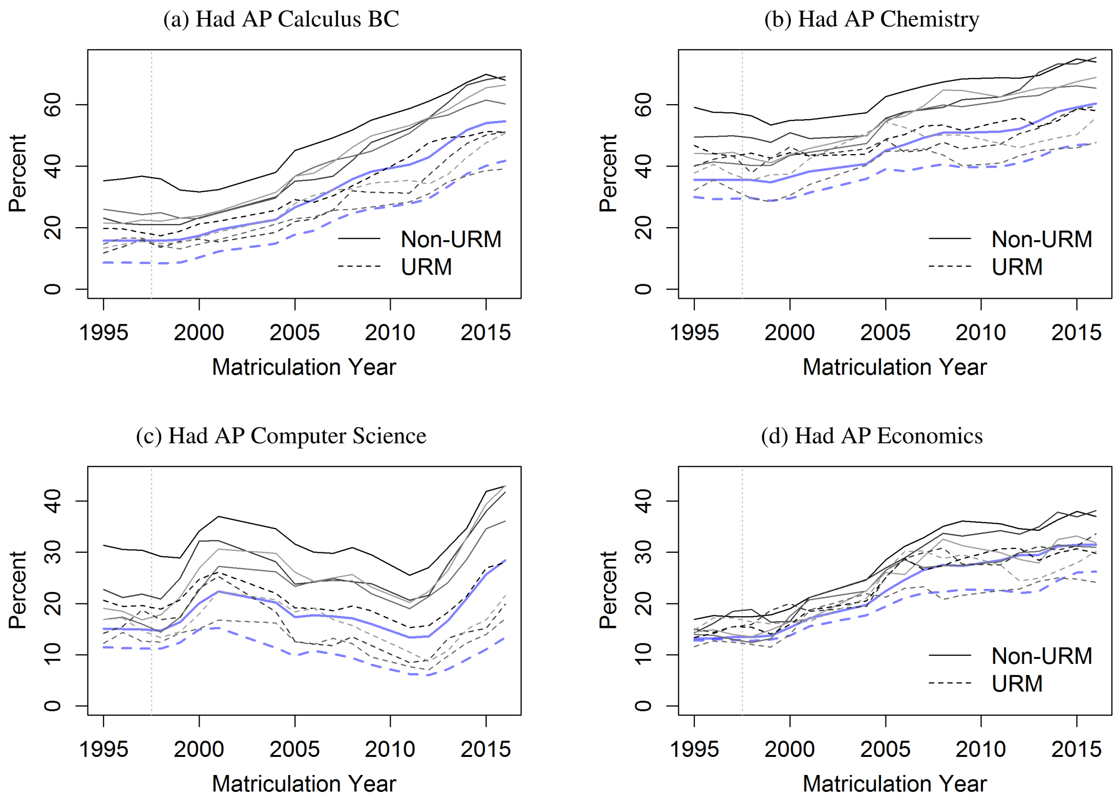


Note: This figure shows that URM students at all four UC campuses have persistently had less access to advanced high school courses prior to UC enrollment, with a growing gap over time mirroring a statewide shift in high school resources by student ethnicity. The average number of unique Advanced Placement classes at the high schools from which all Californians (blue) and freshman students at the University campuses at Berkeley, Santa Barbara, Davis, and Santa Cruz (black to lighter gray, respectively) graduated by graduation year and ethnicity, restricting to public California high school graduates. Classes are measured in students' final year of high school. Statistics are two-year moving averages. Freshman UC students are identified by age at matriculation (see Appendix E); nine percent fail to match to observed high schools and are omitted, mostly because they actually transferred from community college. Data for students who graduated in 2002, 2003, and 2010 are unavailable, as are state-wide degree attainment data by ethnicity prior to 1998. Source: UC ClioMetric History Project Student Database and the California Department of Education.

as the most selective of the four campuses (Berkeley) and the progressively lighter lines representing the other three campuses by selectivity (Santa Barbara, Davis, and Santa Cruz). It shows that students at more selective campuses and non-URM students have persistently attended high schools with greater AP course access, and that the gaps have grown over the past 25 years. One striking feature of this chart is that the difference in AP course availability between Berkeley and Santa Cruz has been generally smaller than the difference in AP course availability between URM and non-URM students at each of the four campuses, highlighting the sharp ethnicity divide in course availability. Notice as well that there is suggestive evidence that the URM AP gap seems to has widened more across the state of California than it has at the four UC campuses, providing further evidence that the evolution of UC admission policy throughout this period led to slight relatively positive selection of URM students over time (as in Figure A-7).

Figure DD-2 narrows in on four particularly popular quantitative AP courses – in integral calculus, chemistry, computer science, and economics, each of which is closely associated with frequently-restricted majors at UC and other public universities – and visualizes their availability by ethnicity and campus (or across California) since the mid-1990s. It shows rising access gaps in all four of these courses, particularly in computer science and economics; indeed, in recent years non-URM students were about twice as likely to graduate from a high school offering an AP computer science course than URM students, both across all California graduates and among students at most UC campuses. Figure DD-3 isolates the racial differences in college-level course availability across the state of California.

Figure DD-2: UC Students' High School Course Availability by Ethnicity

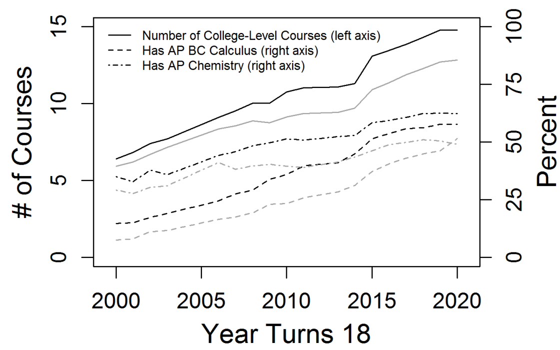


Note: This figure shows that URM UC students have long had particularly poor access to technical AP courses like BC (integral) calculus, chemistry, and computer science (but not economics until recently) at their high schools. The percent of all Californians (blue) and freshman students at the University campuses at Berkeley, Santa Barbara, Davis, and Santa Cruz (black to lighter gray, respectively) who graduated from high schools where each respective Advanced Placement course was available by graduation year and ethnicity, restricting to public California high school graduates. Classes are measured in students' final year of high school. Statistics are two-year moving averages. Freshman UC students are identified by age at matriculation (see Appendix E); nine percent fail to match to observed high schools and are omitted, mostly because they actually transferred from community college. Data for students who graduated in 2002, 2003, and 2010 are unavailable, as are state-wide degree attainment data by ethnicity prior to 1998. AP computer science and economics are defined as the union of all respective AP courses (e.g. either micro- or macroeconomics). Source: UC ClioMetric History Project Student Database and the California Department of Education.

While the previous three figures restrict analysis to *public* California high schools, the fact that URM and non-URM California-resident UC students are similarly-likely to attend private high schools suggests that the inclusion of private high school students in these figures would be unlikely to meaningfully shift the presented trends. Indeed, Figure A-35 shows that URM UC students' private high school attendance has relatively declined over the past 25 years, potentially exacerbating the widening AP course gaps.[62] Figures A-36 and A-37 show that even if you assume that private high school graduates had access to the same number and variety of AP courses as students in the 90th percentile of course availability among UC students in their cohort, it is nevertheless the case that URM UC students have long tended to graduate from relatively lower-opportunity high schools, and to a growing degree over the past 30 years.

[62]Interestingly, prior to Prop 209 (when UC was still implementing race-based affirmative action in admissions) URM UC students tended to be *more* likely than non-URM students to have graduated from private high schools at most campuses.

Figure DD-3: California High School Students' Course Availability by Ethnicity



Note: This figure shows that URM Californians have long had relatively poorer access to college-level coursework in high school. The average number of college-level (AP or IB) courses available at non-URM (black) and URM (gray) tenth-graders' California public high schools, and the share of such students who attend a high school where AP BC Calculus or AP Chemistry was available, by expected graduation year. AP course records are unavailable in 2001-2002 and 2009. Source: California Department of Education.

Appendix E: Data Appendix for University of California Records

The UC ClioMetric History Project student database was constructed by a partnership between UC Berkeley's Center for Studies in Higher Education and the UC Office of the President's Institutional Research and Academic Planning group. The database contains comprehensive student transcript records from seven UC campuses' Offices of the Registrar. Student records are available from the first year in which campuses recorded maintained comprehensive digital student records instead of only paper records – 1975 at UC Berkeley, 1980 at UC Davis, and 1986 at UC Santa Barbara and UC Santa Cruz – until 2016 at UC Berkeley (when the campus switched to a new digital record platform) and 2018 at the other three campuses (when the UC-CHP data were collected).

Students' first year of enrollment is defined as the year that their undergraduate application stated that they wished to enroll at that UC campus, or at UC Santa Barbara is defined as the earliest year in which they enrolled in a course at the university, even if they do not ultimately receive a grade in the course. Students are defined as underrepresented minorities if they report their ethnicity to be African, African American, Alaska Native, Black, Caribbean, Chicano, Guamanian, Hispanic, Latino, Native American, or Puerto Rican.[63] Asian students are not included in our definition of underrepresented minorities because they are not underrepresented at US universities or at UC campuses (even among observable Asian subgroups for most of our sample period); Asian college graduates also earn substantially higher-premium majors than any other ethnic group (Figure JJ-1) and do not become less likely to earn majors following restriction (Figure JJ-3). Courses are observed even if students do not earn a grade in the course (e.g. if they withdraw from the course after the beginning-of-semester add/drop period or take the course pass/fail). Letter grades were optional at UC Santa Cruz until 1999, leading some department-years to have no freshman students who earned grades in first-term courses and one department-year – the 1992 UC Santa Cruz Community Studies cohort – to have no estimable average GPA fixed effect.

[63]Ethnicity is self-reported to UC campuses; students were only permitted to select a single race or ethnicity. Gender and ethnicity (parental addresses) are not observed at UC Davis in 1988-1990 (1988-1992).

About one-third of University of California students enter as transfer students from community college. While we do not directly observe students' transfer status in some campus-years, we identify transfer students by their precise age at first enrollment: transfer students are those who turn 20 years old no later than September of their first enrollment year. Figure A-34 shows that among students for whom we directly observe transfer status – namely, students who enroll at UC in 1994 or later – our definition accurately categorizes 98 percent of freshman students and 94 percent of transfer students.

We link these student records at the individual level to 1994-2016 UC undergraduate application records using internal student identification numbers, and to 2000-2020 annual wage records from the California Employment Development Department using social security numbers, which are provided to the campus upon enrollment and verified for accuracy.[64] Only wages covered by California unemployment insurance are observed, which excludes federal employment, self-employment, and out-of-state employment; all wage analysis excludes individuals without positive wages. Student identifiers were used solely for the matching procedure; once the match was completed, the data were de-identified (in that individual identifiers like name and identification numbers were omitted) and all statistical analysis was conducted using the resulting de-identified dataset.

We also link the student records to supplementary datasets at the aggregate level. Using public statistics from the US Census and the Internal Revenue Service's Statistics of Income dataset, students are linked by 1980 Census tract (1975-1984), 1990 Census block group (1985-1994), or Zip code (1995-2016) and enrollment year to either the average reported Census income of their region in 1980 or 1990 (1975-1994) or the average adjusted gross income reported on household tax filings from their Zip code in their first year of enrollment (1995-2020), winsorized at the top and bottom 2% by year and CPI-adjusted to 2018. Due to IRS data availability, 1988-2000 students are linked to 1998 averages, 2001-2003 to 2001 averages, and 2008 to 2007 averages.

Students who attended public California high schools are also linked to their pre-college access to college-level coursework using 1997-2016 California Department of Education school records, which provide school-year indicators for which each Advanced Placement or International Baccalaureate courses were available and (after 2003) the share of enrolled students eligible for free and reduced price meals. AP and IB course indicators are unavailable in 2001-2002 and 2009. High schools are linked by NCES and CEEB identifiers following a crosswalk available at https://ire.uncg.edu/research/NCES_CEEB_Table/.

Finally, we compile a dataset covering 1966-2010 responses to the CIRP Freshman Survey – administered prior to students' first day of college classes – among freshman students from the four UC campuses (HERI, 2022). Responses are not individually linked to administrative student records, but include students' campus, first year of enrollment, intended major, gender, ethnicity, parental education, and separate responses of "no chance", "very little chance", "some chance", and "very good chance" to student's "best guess as to the chances that" they will (a) "change major field" or (b) "Need extra time to complete your degree requirements".[65] The survey was not conducted every year at all four campuses; data are available

[64] All statistics produced using admissions and wage data are replicated from Bleemer and Mehta (2020).

[65] HERI (2022) reports the survey data with anonymized university codes to not reveal respondents' origin campus. However, the (non-anonymized) years in which each university has participated in the CIRP survey is available at https://ucla.app.box.com/v/TFS-Participation-Hist-Excel. All four UC campuses have participated in the survey in a unique set of years, which in combination with respondent counts and popular reported majors permits us to identify which anonymized code referred to each university: 2770 for Berkeley (second-closest match on years, since the closer match had too few respondents and very different majors than Berkeley students), 3047 for Santa Barbara (closest and perfect match), 2805 for Davis (closest and perfect match), and 3054 for Santa Cruz

Table EE-1: Descriptive Statistics of UC Campus Majors, Among CIRP Respondents

	All	Berkeley	Davis	Santa Barbara	Santa Cruz	3 Years Before Major Restriction
Number of Majors	52 [6]	54 [5]	53 [6]	50 [6]	52 [8]	
# Students	19 [26]	24 [27]	18 [24]	18 [26]	18 [25]	26 [18]
% Female	54 [30]	50 [29]	56 [31]	57 [32]	53 [29]	54 [29]
% URM	15 [20]	16 [21]	18 [22]	13 [19]	17 [20]	14 [10]
% First Gen.	16 [19]	18 [18]	19 [20]	14 [18]	16 [20]	16 [11]
Sample Size						
Events	14	3	2	7	2	
Observations	97,984	28,387	9,379	30,183	30,035	
Observations in Est.*	93,494	27,515	8,873	28,760	28,346	
Response Rate	0.31	0.24	0.31	0.27	0.55	

Note: Descriptive statistics of the average number of reported majors (with at least one freshman student) in each covered university-year, average number of freshman students per department, and the average percent of female, URM, and first generation freshman students across departments, for all departments and for departments three years before instituting major restrictions. First generation students are defined by neither of their parents having enrolled in college. Standard deviations in brackets. Events indicate number of new observable major restrictions (see Table 3) with surveys observed at that campus in $t = -3$ and major-year observations overall or in the estimation sample (* restricting to major-years with more than 5 freshman students), where year is defined as students' first year of enrollment. Response rate reports the number of freshman who completed the survey divided by the total freshman enrollment in years in which the survey was fielded.

Source: CIRP Freshman Survey (HERI, 2022) and the UC ClioMetric History Project Student Database.

for the following cohorts:

- UC Berkeley: 1966-1991 omitting 1970.
- UC Santa Barbara: 1966-1994 omitting 1977, 1998, 2000, 2001, 2004, 2006, and 2008.
- UC Davis: 1966-1968, 1976, 1986, 1988, 1991, 1994, 1997, 2001, 2004.
- UC Santa Cruz: 1966-1992 omitting 1968, 1976, and 1979; and the even years in 1994-2010.

Majors are reported in 86 categories and are hand-matched to identify event years; about 9 percent of respondents report "undecided" intended majors and are omitted.[66] The "chance" variables are recoded as binary responses, where both "some" and "very good chance" are recorded as "yes". Survey weights are unavailable for most cohorts and are not employed.

Table EE-1 summarizes students' responses to the CIRP freshman survey aggregated at the intended major by cohort level, as in Table 4 in the main paper. It shows that about 100,000 freshman students

(closest match by 6 years).

[66] Implementation years for three majors – UCSC Economics, UCSC Physics, and UCB Economics – are adjusted earlier by one year to permit baseline estimation (three years prior to implementation) in a regular CIRP survey year.

responded to the survey in the years it was distributed, a response rate of about 31 percent. URM students are slightly underrepresented among survey respondents – the average intended major has 15 percent URM enrollment, relative to 20 percent in the administrative data among declared major. Only 14 restrictions were implemented in years that permit estimation of their effects on students pre-matriculation preferences, with half of those restrictions implemented at UC Santa Barbara.

Appendix F: Spillovers between Majors and SUTVA Violations

University students choose at least one college major from a fixed set of available majors. When a student is prohibited from choosing their preferred major, they are generally obligated to choose another major instead.[67] As a result, comparisons between 'treatment' and 'control' majors following the implementation of major restriction policies may overstate the effect of those policies, since the 'control' majors are also mechanically impacted by the major restriction (in that they absorb students excluded from the restricted major). This appendix investigates the magnitude of the potential bias resulting from this SUTVA violation in Equation 6 and presents an unbiased Monte Carlo statistical test for the null hypothesis that major restrictions do not affect departmental composition.

To illustrate the potential SUTVA violation, consider a university with only two college majors, R and NR, neither of which is restricted in period 1. Say that 50 students per year earn each major, including 10 students in each major who are URM. Now let's assume that R implements a restriction in period 2, and assume that the restriction has no effect on non-URM students but leads 5 URM students to earn NR instead of R. In this setting, implementing the major restriction reduces the URM share in R from $\frac{10}{50} = 20\%$ to $\frac{5}{45} = 11.1\%$, so the effect of the major restriction is -8.9%. However, if we were to run our regression analysis (omitting the fixed effects for the sake of the example, though the same logic holds in their presence), we would estimate the effect of implementing the restriction to be $11.1 + 7.2 = 18.3\%$, almost double the true effect, because the URM share in the 'control' major would rise (to $\frac{15}{55} = 27.2\%$) as it absorbs the URM students who exit R.

However, now consider a case where there are six total majors, R and NR1 to NR5, each of which has 50 students (with 10 URM) in period 1. When R implements a major restriction, the 5 URM students are going to have to choose which of the other 5 majors they enroll in. Since they are all equal-size, it hardly matters which they choose; the degree of bias in our regression estimate is now going to be much smaller. For example, if they all choose NR1, then the average unrestricted major's URM share would rise to $\frac{\frac{15}{55}+(\frac{10}{50}\times 4)}{5} = 21.5\%$ and the regression will estimate the effect of a major restriction to be $8.9+(21.5-20) = 10.4\%$ instead of the true 8.9%. Alternatively, if each student chose a different unrestricted major, then the regression would estimate $0.089 + (\frac{11}{51} - 0.2) = 10.5\%$.

As these examples show, when it comes to estimated changes in the average characteristics of students who earn one or another college major as estimated by Equation 6, the magnitude of the SUTVA violation swiftly falls in the number of alternative majors available at the institution where the restriction is implemented. Table 4 shows that the average institution-year in our sample has 58 majors available to its students. On average (ignoring changes in the denominator), this implies that a 1 percent decrease in URM shares in a randomly-selected major that imposes a new major restriction would lead to a $\frac{1}{58} = 0.02\%$ increase in

[67] Note that this is not required for double-major students and students who drop out prior to their third year.

URM shares in other departments, yielding a beta estimate of 1.02 percent.

There are two potential complicating factors. The first is that Table 4 shows that the majors that impose restrictions tend to be about twice as large as the typical major at their institutions, which would approximately double the SUTVA violations described above (since twice as many students are impacted). The second is that SUTVA violations are magnified if the majors where pushed-out students land tend to be relatively smaller majors, where the new students' effect on the average characteristic of students in those majors would be magnified. However, Figure A-23(b) shows that they tend to flow into social science majors, which tend to be relatively large. As a result, we conclude that the presented estimates of β_t likely include only negligible bias resulting from mechanical correlation across college majors.

We examine a further complication generated by the violation of SUTVA in our setting in the last row of Table 5. Student enrollments are mechanically correlated across department even without major restrictions because students leaving one department end up in another. As a result, the estimated 95-percent confidence intervals of our β_t coefficients will be biased, potentially leading us to reject the null that $\beta_t = 0$ even if restriction implementation did not affect departmental compositions. We obviate this problem by developing a Monte Carlo procedure to approximate the distribution of β_t under the null hypothesis and estimate the likelihood that the data could have arisen under the null.

In particular, we randomly pull 1,000 draws of 25 campus-year pairs as placebo restrictions and fit Equation 6 over each set for each outcome. We then estimate empirical p-values for two-sided tests of the statistical significance of the $\Delta(Post - Pre)$ estimates presented in Table 5, shown in the last row of Table 5. These p-values confirm the statistical significance of all of the relevant findings presented in the main text: for example, the evidence that the implementation of major restrictions leads departments to enroll fewer URM and lower-income students is statistically significant, even accounting for mechanical correlations resulting from student movements between departments.

Appendix G: The Mechanisms of Major Restrictions – A Case Study of Economics

To further illuminate how major restrictions influence the majors that students enter, we compare entry into the high-return economics majors at UC Santa Barbara (UCSB) and UC Davis between 2010 and 2016.[68] These majors provide a useful case study for several reasons:[69]

1. UC Davis and UC Santa Barbara were similarly-selective institutions; both were ranked between 38 and 42 in every annual US News & World Report national university ranking in the period.

2. Each campus had a similarly-structured progression of introductory courses that students were required to take prior to major declaration: two quarters of calculus, introductory micro- and macroe-

[68] Economics is among the highest-premium majors offered by UC campuses; see Table A-2. UC Berkeley's economics major is omitted because Berkeley's semester schedule (as opposed to UCSB and Davis's quarter schedules) yields a different lower-division economics curriculum, with introductory micro- and macroeconomics combined into a single course. This prohibits direct comparison with the other campuses. UCSC economics also provides a limited test case, since its restriction was non-binding in its early years of implementation (Bleemer and Mehta, 2022).

[69] While a surge in international student enrollments during this period could have crowded students out of the economics majors at both schools, the surge was larger at UC Davis.

conomics (Economics 1 and 2), and one or two additional courses depending upon students' chosen track.

3. All economics tracks at Santa Barbara had a 2.85 grade point average restriction (over 3-5 introductory economics courses), while the Davis economics major was unrestricted.[70]

4. The Santa Barbara restrictions (and Davis's non-restriction) did not change in the sample period.

5. Despite UCSB's restriction, the economics majors at each school graduated more students than any other major in the period, suggesting substantial demand.

As a result, we investigate the mechanisms driving major restrictions' effect on campus stratification by examining differences in students' economics course grades, course enrollment, and major declaration at each campus $u \in \{D, SB\}$ using a series of linear regression models:

$$Y_{iyct} = \alpha_{ct} + \gamma_y + \beta_c X_i + \epsilon_{iyct} \tag{GG-1}$$

$$Y_{iyct} = \alpha_{ct} + \gamma_{y,SB_i} + \beta_c X_i + \beta'_c X_i \times SB_i + \epsilon_{iyct} \tag{GG-2}$$

where each outcome Y_{iyct} for student i in cohort y who completed course c in term t is modeled as a function of students' demographic, socioeconomic, high school opportunity, and academic preparedness characteristics.[71] Cohort and course-term fixed effects are included for each campus, and standard errors are clustered by high school. Propensity weights ensure that the Davis and Santa Barbara student samples are balanced on observed covariates, including the full set of covariates described above as well as county fixed effects for Californians.[72] Our preferred interpretation of these models is that between-campus differences in students' propensity to declare the major mainly reflect the effect of UCSB's economics major restriction.

The first two regression models presented in Table GG-1 examine which of the students who enrolled in ECON 1 eventually declared economics majors, where ECON 1 enrollment is a signal of students' potential interest in majoring in economics.[73] The first model includes only demographic and socioeconomic characteristics as covariates, directly testing whether UCSB's major is more stratified. We take the baseline Davis estimates, where any student is permitted to declare an economics major after passing the introductory courses, to reveal how "preferences" for the major differ by ethnicity and income.[74] They reveal a significant

[70] UC Davis's Managerial Economics track, like many business-oriented economics majors, had a 2.8 GPA major restriction before 2013. That track catered to almost half of the students in economics-based majors at UC Davis. Similarly, UCSB offered an alternative means of qualifying for its Business Economics major until Summer 2011. While Davis's 'partial' major restriction and the early exception to Santa Barbara's restriction could attenuate the comparative results discussed below, the coefficient estimates are similar (but less-precise) if the sample is split before 2014 and models are re-estimated separately in both periods (available from the authors).

[71] These characteristics include gender, ethnicity, log parental income, SAT score, high school GPA, California residency, California public school enrollment, and the presence of AP and IB economics for students from public CA high schools. An indicator for missing income marks students who omitted their family income on their college application, usually connoting above-average income or wealth (Bleemer, 2022).

[72] In particular, each observation is weighted by the student's inverse likelihood of enrolling at that campus (from a first-stage regression on the full X_i as well as high school county fixed effects), recovering the average treatment effect for students at both campuses.

[73] Economics major declaration includes both Economics and Economics & Accounting at UCSB and both Economics and Managerial Economics at UC Davis.

[74] By "preference" here, we mean simply students' relative desire to complete different majors given their aptitudes, inclinations, and personal circumstances.

Table GG-1: 2010-2016 Economics Major Enrollment Propensities at UC Davis and UCSB

Dep. Var:	Earn Economics Major, Conditional on ECON 1						Enroll in ECON 1	
	Davis	UCSB	Diff.	Davis	UCSB	Diff.	Davis	Diff.
Female	-8.68	-5.84	2.85	-8.57	-5.94	2.63	-9.09	-4.49
	(1.25)	(1.30)	(1.55)	(1.24)	(1.27)	(1.54)	(0.56)	(0.88)
Asian	6.06	3.07	-2.99	5.69	4.11	-1.58	6.90	-0.18
	(1.22)	(1.47)	(1.92)	(1.21)	(1.37)	(1.80)	(0.79)	(1.02)
URM	0.60	-10.07	-10.68	-0.84	-3.92	-3.08	-7.00	3.56
	(1.40)	(1.40)	(1.93)	(1.45)	(1.41)	(1.96)	(0.72)	(0.97)
Log Fam. Inc.	0.64	1.96	1.32	0.86	0.28	-0.58	0.83	-0.29
	(0.45)	(0.43)	(0.61)	(0.49)	(0.40)	(0.62)	(0.24)	(0.34)
Miss. Income	4.40	6.55	2.15	4.76	2.26	-2.50	3.06	-1.21
	(1.83)	(1.92)	(2.62)	(1.87)	(1.90)	(2.64)	(1.07)	(1.47)
Out-of-State	-4.50	-4.30	0.20	-4.74	0.69	5.43	4.34	-2.45
	(2.30)	(2.58)	(3.41)	(2.43)	(2.63)	(3.52)	(1.52)	(2.06)
International	0.96	-0.23	-1.19	0.26	5.64	5.38	17.02	14.09
	(1.79)	(2.22)	(2.62)	(2.06)	(2.22)	(2.78)	(5.45)	(3.15)
CA Private HS				4.07	-0.59	-4.66	1.35	1.66
				(1.85)	(1.83)	(2.44)	(1.13)	(1.42)
High School Offered[1]:								
AP Macro				0.34	4.76	4.42	-1.23	-0.27
				(1.96)	(2.04)	(2.82)	(1.18)	(1.51)
AP Micro				1.49	4.25	2.76	-5.25	4.18
				(2.81)	(2.95)	(4.16)	(1.26)	(2.06)
IB Economics				-4.37	2.96	7.34	0.27	-0.75
				(3.07)	(4.04)	(5.24)	(2.07)	(3.74)
SAT Score[2]				-1.78	6.96	9.55	-1.12	1.45
				(0.55)	(0.56)	(0.83)	(0.37)	(0.49)
HS GPA[2]				-1.44	5.47	7.42	-2.59	0.85
				(0.66)	(0.53)	(0.86)	(0.41)	(0.50)
Course-Term FEs		X			X		X	
Campus-Cohort FEs		X			X		X	
R^2		0.02			0.04		0.06	
Observations		16,974			16,974		62,512	
Mean of Y	32.2	26.4	-	32.2	26.4	-	29.0	

Note: This table shows that URM and otherwise-disadvantaged students who took Economics 1 at UC Santa Barbara – which implemented a major restriction – were much less likely to ultimately declare the major than such students at UC Davis, which had no such restriction, though these differences are fully absorbed by those students' poorer measured pre-college academic opportunity and preparation. Propensity-score-weighted WLS regression models among 2010-2016 freshman-applicant Santa Barbara and Davis students of economics major declaration and ECON 1 enrollment on student characteristics. Major declaration models conditional on having earned a grade in ECON 1. Main effects estimated for Davis and Santa Barbara; 'Diff' is estimated as the difference between Santa Barbara and Davis. Standard errors clustered by high school in parentheses. Inverse propensity score weights estimated using the full set of listed covariates as well as California county indicators. Family income is missing for the $\sim$ 13 percent of students who did not report family income on their application; estimates relative to the mean observed log income. [1]High school course offerings are only observed for public CA high schools. [2]Normalized to mean 0, s.d. 1.

Source: UC ClioMetric History Project Student Database, UC Corporate Student System, and California Department of Education.

relative preference for the subject among Asian students, but not among URM students. There is some evidence that preference for economics increases with income; the (presumably) higher-income students who do not report family income statistics are much more likely than average to declare the major.[75]

At Santa Barbara, by comparison, Asian students who took ECON 1 are not significantly more likely to declare an Economics major, while URM students are 10 percentage points less likely to declare an economics major than white students. The magnitude of this URM difference is appreciable relative to an average declaration propensity of 26.4 percent at UCSB.[76] The difference between the campuses in URM students' relative propensity to declare an economics major is similarly large and statistically significant. Income also appears to have stronger effects on enrollment at Santa Barbara. This is consistent with the major restriction muting student preferences, and doing so in a way that stratifies students on ethnicity and income, as students who exit the economics major are very likely to instead earn lower-return majors (Bleemer and Mehta, 2022).

The second regression model in Table GG-1 includes academic opportunity and preparation covariates. Ethnic differences between similarly-prepared students are much smaller than the unconditional gaps estimated in the previous model, though URM students remain somewhat less likely to declare an economics major at UCSB than at Davis, by 3.1 (s.e. 2.0) percentage points.[77] This suggests that the primary stratifying effect of the major restriction is to induce selection based on prior preparation.

The other coefficients in this regression confirm that impression. At Davis, ECON 1 students with higher SAT scores and high school GPAs are less likely to select an economics major, while the opposite is true at UCSB. This suggests that economics tends not to be the top choice of the most-prepared (ECON 1) students, but that the major restriction systematically prevents less-prepared students from declaring the major at UCSB. Second, while exposure to economics in high school does not predict major declaration at Davis, it does at UCSB. This suggests that the restriction induces selection on prior general preparation and on prior exposure to economics.

The final model in Table GG-1 examines selection (conditional on prior opportunity and preparation) along a different margin: enrollment in a student's first economics course. The UCSB outcomes differ significantly from those at Davis in three respects. First, female students are less likely to take ECON 1 at UCSB, in line with the student-level difference-in-difference estimates in Appendix H, and again suggesting that the major restriction mutes preferences. Second, students with *lower* SAT scores and high-school GPAs are more likely take ECON 1 at Davis, while those who attended private school are not. In contrast, high SATs and high school GPAs are not associated with taking ECON 1 at UCSB, and private high-school attendance is. Each of these results is consistent with the major restriction inducing positive self-selection into the first course in the major based on prior preparation, perhaps because students who feel they are less likely to qualify for the major do not attempt it. Finally, students who have taken AP Micro and are

[75]The coefficient on missing income has been adjusted to reflect the difference in outcome propensity between missing-income students and a student with average log family income. See Bleemer (2022) for the predicted family income distribution of income non-reporters.

[76]Major declaration propensity among plausibly-interested students is significantly lower at UCSB (26.4%) than it is at Davis (32.2%). This difference is similar in magnitude to the effects of major restrictions on major size reported in Section 4.

[77]In fact, only SAT score (not HS GPA or courses) partially absorbs URM students' lower likelihood of major declaration at UCSB. If SAT scores are poorer predictors of URM students' academic performance than they are for non-URM students (Vars and Bowen, 1998), then the URM student effect would be over-absorbed in this context. Indeed, interacting SAT score with URM status estimates a sharply negative coefficient for URM students at UCSB and yields a baseline URM coefficient (at mean SAT) of -4.5 (s.e. 2.2) percentage points.

therefore eligible to opt out of ECON 1 tend to do so at Davis, but not at UCSB, where the major restriction only considers ECON 1 grades from courses taken at UCSB.

The results presented in Table GG-1 reveal that there is more positive selection and self-selection into the economics majors at UCSB than at Davis. Observationally, that selection is on prior academic preparation and exposure to economics in high school, and this selection appears to result in stratification on ethnicity and income. Our preferred interpretation is that the greater observed positive selection at UCSB arises from that campus's major restriction. The following subsection investigates alternative interpretations of the presented statistics.

G.1 Explaining Stratification by Pre-College Preparation

One alternative explanation for the patterns described above is that quantitative aptitude covaries with prior preparation to a greater degree among UCSB students. If this were the case, and students' course and major choices responded to it, this could explain the higher degree of selection on prior preparation and economics experience at UCSB. However, the first two models presented in Table GG-2 – which model ECON 1 students' performance in the first two calculus courses – show that this is not the case for quantitative skills. The baseline (Davis) coefficients confirm significant variation in math preparation by observables, including prior preparation: higher SAT scores, high school GPAs, and family incomes predict better mathematical performance, as do being Asian and female, while URM students had worse math grades. However, there is almost no evidence of a stronger relationship between student characteristics and math performance at UCSB than at Davis in either of the first two calculus courses.

Another alternative explanation for the observed patterns is that UCSB might provide lower grades to less-prepared students in its introductory courses, discouraging those students using 'soft' restrictions rather than relying on its mechanical restriction policy. The next two columns in Table GG-2 show that in fact, the opposite is the case: higher SAT scores are more *weakly* associated with ECON 1 grade gains at UCSB than at Davis, and the URM grade penalty is smaller at UCSB than at Davis. This implies that UCSB provides somewhat more lenient grades in its introductory courses, but its major restriction nevertheless deters disadvantaged and lower-preparation students from earning the major.

The final three columns of Table GG-2 reveal how UCSB's major restriction generates larger ethnic and income gaps in major declaration by selecting on socioeconomic status, prior academic opportunity, and measured academic preparation. UCSB students with higher high school GPAs and SAT scores obtain much higher grades in ECON 1, 2, and 10A, and those who had access to IB or AP economics perform much better in ECON 1 and 2. At UCSB, URM students also obtain lower grades in these threshold courses than their equally prepared non-URM counterparts, clarifying why prior preparation does not fully explain URM students' lower likelihood of economics major declaration. Although these ethnicity grade gaps are less pronounced at UCSB than at Davis, the restriction makes grade gaps more consequential at UCSB.

These results confirm major restriction filtering as the most likely interpretation for differences in the role of ethnicity, exposure to economics, and prior preparation in major completion between Davis and Santa Barbara.

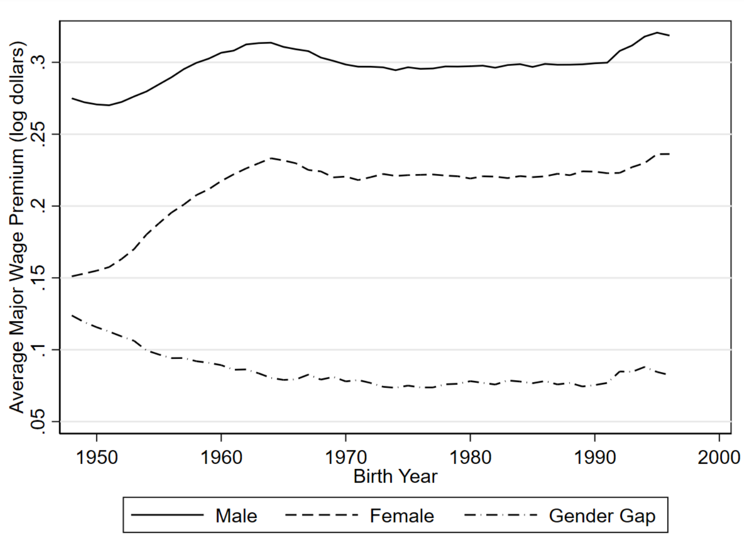
Table GG-2: Economics Students' Course Performance at Davis and Santa Barbara

	Grade in Calc. I		Grade in Calc. II		Difference in:		UCSB-only determinants of:		
					ECON 1	ECON 2	ECON 1	ECON 2	ECON 10A
	UCD	Diff.	UCD	Diff.	Grade	Grade	Grade	Grade	Grade
Female	0.06	-0.05	0.12	-0.03	0.09	-0.01	-0.14	-0.13	-0.03
	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Asian	0.17	-0.07	0.21	-0.14	-0.06	-0.15	0.02	-0.04	0.01
	(0.03)	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)	(0.02)	(0.02)	(0.04)
URM	-0.11	-0.05	-0.17	-0.05	0.09	0.06	-0.11	-0.12	-0.12
	(0.04)	(0.06)	(0.04)	(0.06)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)
Log Fam. Inc.	0.02	-0.01	0.00	0.02	-0.02	0.00	0.01	0.02	0.01
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Miss. Income	-0.09	0.08	-0.07	0.09	-0.01	0.04	-0.02	0.01	-0.01
	(0.05)	(0.07)	(0.06)	(0.07)	(0.05)	(0.05)	(0.02)	(0.03)	(0.05)
Out-of-State	-0.08	0.33	0.02	0.17	-0.00	-0.10	0.10	0.11	0.25
	(0.07)	(0.09)	(0.07)	(0.09)	(0.07)	(0.07)	(0.04)	(0.05)	(0.07)
International	0.42	0.32	0.46	0.07	0.02	-0.12	0.48	0.40	0.41
	(0.05)	(0.06)	(0.07)	(0.08)	(0.06)	(0.06)	(0.06)	(0.04)	(0.08)
CA Private HS	-0.07	0.13	-0.02	0.02	-0.01	-0.08	0.02	0.01	0.01
	(0.04)	(0.06)	(0.06)	(0.06)	(0.04)	(0.05)	(0.03)	(0.03)	(0.05)
High School Offered[1]:									
AP Macro	0.02	0.04	0.03	0.06	0.06	0.13	0.07	0.13	0.06
	(0.05)	(0.07)	(0.05)	(0.07)	(0.05)	(0.05)	(0.03)	(0.04)	(0.05)
AP Micro	-0.00	0.06	-0.08	0.12	0.19	0.08	0.06	0.04	0.02
	(0.07)	(0.10)	(0.08)	(0.09)	(0.07)	(0.07)	(0.04)	(0.05)	(0.07)
IB Economics	-0.08	-0.07	0.03	0.09	0.03	0.09	0.09	0.15	0.13
	(0.13)	(0.18)	(0.14)	(0.13)	(0.08)	(0.12)	(0.05)	(0.08)	(0.12)
SAT Score[2]	0.24	0.03	0.21	-0.04	-0.08	-0.01	0.23	0.27	0.19
	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
HS GPA[2]	0.16	0.01	0.17	0.04	-0.03	-0.03	0.14	0.15	0.16
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Course-Term	X	X	X	X	X	X	X	X	X
Campus-Cohort	X	X	X	X	X	X	X	X	X
R^2	0.16		0.11		0.21	0.18	0.18	0.18	0.08
Observations	10,168		11,554		16,974	13,884	7,829	6,216	3,565
Mean of Y	2.89		2.75		2.61	2.58	2.56	2.55	2.76

Note: This table shows that disadvantaged UCSB students' exiting the economics major appears likely to be explained by the binding GPA restriction, despite those students earning slightly higher relative grades at UCSB (where grades' stakes are much higher). Propensity-score-weighted WLS regression models among 2010-2016 freshman-applicant Santa Barbara and Davis students of grades earned in first and second quarters of calculus, ECON 1 and 2, and the subsequent ECON 10A course at Santa Barbara on student characteristics. Mathematics grades are conditional on ECON 1 enrollment. Main effects estimated for Davis and Santa Barbara; 'Diff' estimated as the difference between Santa Barbara and Davis. Standard errors clustered by high school in parentheses. Inverse propensity score weights estimated using the full set of listed covariates as well as California county indicators. Family income is missing for the ~ 13 percent of students who did not report family income on their application; estimates relative to the mean observed log income. Calculus I and II courses are MATH 2A/B, 3A/B, or 34A/B at UCSB and 16A/B and 21A/B at Davis. [1]High school course offerings are only observed for public CA high schools. [2]Normalized to mean 0, s.d. 1.

Source: UC ClioMetric History Project Student Database, UC Corporate Student System, and California Department of Education.

Figure HH-1: Average College Major Premium by Birth Cohort and Gender



Note: This figure shows that the college major premium gap between male and female college graduates closed in the 1950s birth cohorts but has stagnated around 0.08 log dollars since. Average college major premium of college graduates by birth cohort and gender among 2009-2019 ACS respondents, and the difference between those averages. Major premiums are estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, year, and double-major covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS; see Appendix B for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2020).

Appendix H: Major Restrictions and Gender Stratification

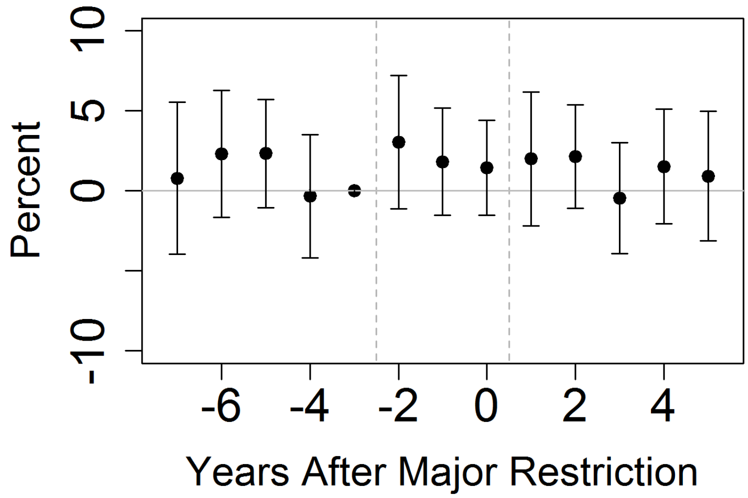
Prior studies of college major stratification have more commonly analyzed major attainment gaps by gender than by ethnicity or parental income. In addition to Sloane, Hurst, and Black (2021)'s recent review of trends in college major choice by gender, many studies have documented differences in student preferences and preparation that contribute to the gap (Wiswall and Zafar, 2018; Mourifie, Henry, and Meango, 2020; Card and Payne, 2021), and others have evaluated a series of policies intended to narrow the gender gap in STEM major attainment (e.g. Canaan and Mouganie, 2021). This section extends our analyses above by comparing trends in the economic value of students' college major choices by *gender* over time and then characterizes the differential effects of major restrictions on male and female students' major choice.

Figure HH-1 is analogous to Figure 1 with differences by gender instead of ethnicity. It shows that the gap between the average premium of majors earned by men and women closed between the 1950 and 1965 birth cohorts from about 0.12 log dollars to 0.08 log dollars, but that the gap has remained largely unchanged in the subsequent 30 years, with some slight growth in recent years.[78] As a result, there is no trend in aggregate gender stratification to decompose; despite substantial policy innovation in recent years (e.g Colwell, Bear, and Helman, 2020), female college graduates consistently earn similarly less-lucrative degrees than their male peers.[79]

Figure HH-2 shows that the implementation of major restriction policies has no average effect on the

[78] Evidence presented by Black et al. (2008) (and evidence presented by Brown and Corcoran, 1997) suggest that college majors explained 9-13 (8) percentage points of the male-female wage gap among 1993 (1984) workers, who were mostly members of the 1930-1970 (1920-1960) birth cohorts. Turner and Bowen (1999) note that gender convergence of major choice had ceased by the

Figure HH-2: Departments' Female Share Before and After New Major Restrictions



Note: This figure shows that new major restrictions have no observable average net effect on departments' gender composition. Staggered difference-in-difference β estimates following Equation 6 of the female share of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database.

share of female students earning the restricted major, a finding confirmed in Table 5. However, this average effect appears to be the result of two offsetting effects observable using the student-intention estimates discussed in Section 5. Figure HH-3 plots estimates from a version of the staggered difference-in-difference model described in Footnote 41 that replaces URM status with gender. It shows some evidence of a decline in female students' revealed *intention* to earn restricted majors after the imposition of the restriction, perhaps as a result of competition aversion (Neiderle and Vesterlund, 2007; Buser, Niederle, and Oosterbeek, 2014): when a major restriction is implemented, female students are discouraged (on average) from the first-term courses most commonly selected by students who earn that major. This evidence is in line with other studies that have shown relatively larger discouragement effects of low grades (Ahn et al., 2019; Li and Zafar, 2021) and test scores (Azmat, Calsamiglia, and Iriberri, 2020) among female students.

However, this decline in major intention appears to be offset by an increased likelihood of major completion conditional on intention, plausibly because women have higher grades in the relevant introductory courses (see Figure A-33). Panel (b) of Figure HH-4 – which presents estimates of Equation 7 replacing URM status with gender – provides some evidence of this; if anything, female students who intend the restricted major become slightly *more* likely on average to earn that major. This evidence is clarified by the case study presented in Appendix G: female students are less likely to enroll in a major's introductory courses when the major is restricted, but female enrollees earn higher grades in the course, resulting in a

early 1960s birth cohorts.

[79] Sloane, Hurst, and Black (2021) implement an alternative measure of the economic value of majors – indexing majors by the median hourly wage of native white men between ages 43 and 57 – but arrive at similar conclusions, though they emphasize the gap's narrowing in the 1950s birth cohorts rather than the stagnation over the subsequent decades. See Figure A-32.

Figure HH-3: Estimated Changes in Students' Intentions for Restricted Majors by Gender

Note: This figure shows that major restrictions have no observable differential effect on the composition or major choices of student who intend restricted majors by gender. Staggered difference-in-difference β_{it} estimates by gender of the average degree to which students exhibit their intention to earn newly-restricted majors ($\hat{M}_{im}$) before and after the implementation of the restriction, estimated over a stacked dataset of students i's major intentions in field m. See footnote 41 for the estimating equation. $\beta_{i,-3}$ is omitted; standard errors are two-way clustered by campus-majors m and by students i and assume that $\hat{M}_{im}$ is observed without error; and 95 percent confidence intervals are shown. Models include m fixed effects. Asterisks indicate within-period estimates by ethnicity that are statistically significantly different from each other at the 10 percent (*) or 5 percent (**) level using a two-sided test. Source: UC ClioMetric History Project Student Database.

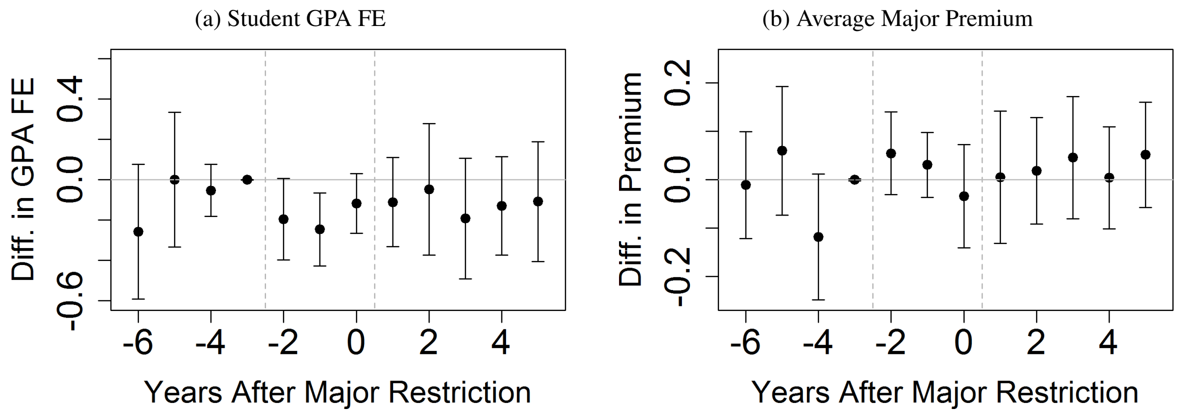
higher likelihood of attaining the major conditional on taking the introductory course among female students. These dynamics may explain why the growing popularity of major restriction policies has not meaningfully shifted the relative value of college majors earned by women in the United States.

Appendix I: Survey Evidence on Pre-Matriculation Knowledge of Major Restrictions

Could pre-matriculation knowledge about newly-implemented major restriction policies have shaped the enrollment choices or major intentions of UC students? If restrictions diverted high school students to enroll at other institutions, then the observed enrollment reductions at impacted campuses could reflect students' reallocation to other universities (where they might have earned that major) instead of diversion to other fields within the same university. Restrictions could alternatively generate sorting patterns among matriculants, with some students drawn to restricted majors for perceived prestige or peer effect benefits.

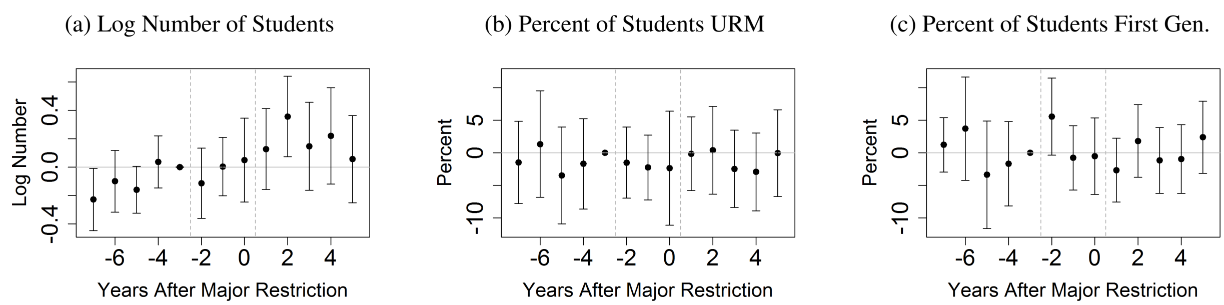
Figure 8 provides one set of evidence rejecting URM students' differential discouragement from intending restricted majors, revealing that students' first-quarter courses suggest little aggregate discouragement from newly-restricted majors and no evidence of disproportionately weaker intentions among URM students. We further investigate students' pre-matriculation knowledge of major restrictions by analyzing students' responses to the CIRP Freshman Survey, which were elicited from most UC cohorts immediately prior to students' first week of classes. Appendix E describes details on data construction and descriptive

Figure HH-4: Estimated Changes Among Intended Majors by Gender



Note: This figure shows that newly-implemented major restrictions did not systematically impact selection into intending the restricted major or the premiums of majors earned by intended majors by gender. Triple-difference β_{it} estimates of the difference between male and female students' relationships between students' intending the restricted major ($\hat{M}_{im}$) and their GPA fixed effect or major premium before and after the implementation of the restriction, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m by gender. Outcomes are defined as the student's GPA fixed effect (their individual fixed effect from a two-way fixed effect model of GPA on student and course effects) and the premium of the student's major (as defined in Appendix B), with the latter controlling for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection. β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

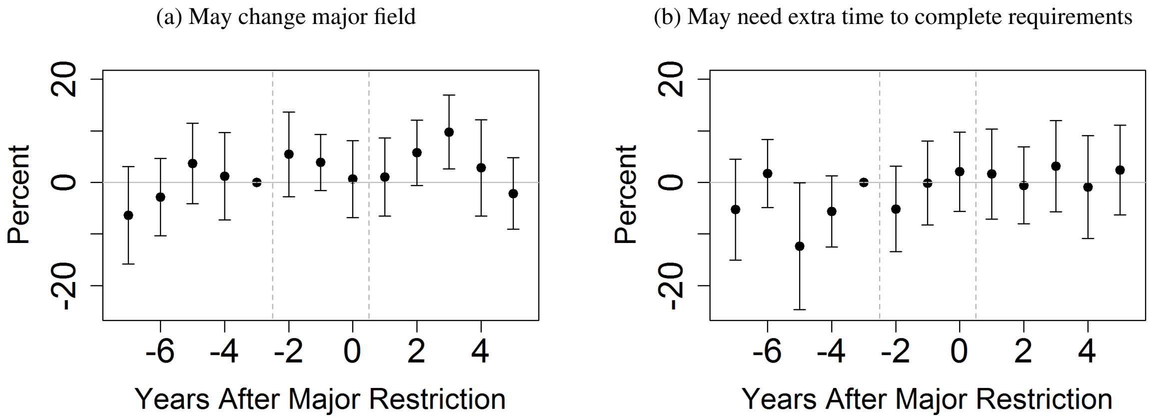
Figure II-1: Characteristics of Pre-Matriculation Students Who Report Intending the Major



Note: Staggered difference-in-difference β estimates following Equation 6 of the characteristics of freshman students who report the intention to earn restricted majors on the CIRP Freshman Survey (reported prior to the first day of courses) before and after the implementation of the restriction, relative to other intended majors in that campus-year. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. First generation students are defined by neither of their parents having enrolled in college. β_{-3} is omitted, and standard errors are clustered by campus-major. Source: CIRP Freshman Survey (HERI, 2022).

statistics. We aggregate responses by students' reported intended major and estimate a series of staggered difference-in-difference models following Equation 6. Because the survey was not fielded at every campus in every year, we are limited to estimating the effects of implementing 14 major restrictions, about half the number from the main analysis (and including several restrictions from years prior to the beginning of our UC administrative data). As above, we interpret the estimated $\hat{\beta}_t$ coefficients when $t > 0$ as the effect of

Figure II-2: Characteristics of Pre-Matriculation Students Who Report Intending the Major



Note: Staggered difference-in-difference β estimates following Equation 6 of the characteristics of freshman students who report the intention to earn restricted majors on the CIRP Freshman Survey (reported prior to the first day of courses) before and after the implementation of the restriction, relative to other intended majors in that campus-year. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. The outcomes reflect the share of student who responded "some" or "very good chance" to the question (a) "What is your best guess as to the chances that you will change major field?" or (b) "What is your best guess as to the chances that you will need extra time to complete your degree requirements?". β_{-3} is omitted, and standard errors are clustered by campus-major. Source: CIRP Freshman Survey (HERI, 2022).

implementing a major restriction policy on departmental composition.

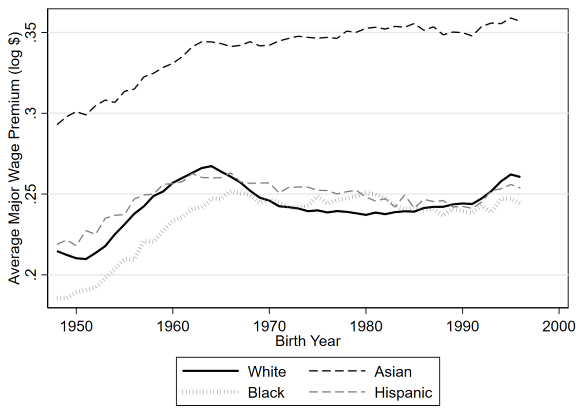
Panel (a) of Figure II-1 shows that reported intent of earning restricted majors was rising in the years prior to restrictions' implementation, mirroring rising enrollment in those majors (see Figure 6), but there is no evidence of a decline in reported intentions following restrictions' implementation; if anything, intentions may have become somewhat more widespread before leveling off. Panels (b) and (c) show no evidence of ethnic or socioeconomic compositional changes among the students who intended restricted majors, suggesting that disadvantaged students were not differentially discouraged from enrollment by restrictions' implementation.

Figure II-2 turns to students' beliefs about their impending educational experiences. Panel (a) shows slight evidence that students who intended restricted majors 2-3 cohorts following restrictions' implementation became about 10 percentage points (from a 66 percent base) more likely to report concern about switching majors. However, the absence of meaningful or systematic positive effects in other years – and the absence of any measurable impact on students' beliefs that they will need additional time to complete their degree requirements, shown in panel (b) – provides further evidence that students appear to have been largely unaware of restriction policies in the weeks prior to matriculation.

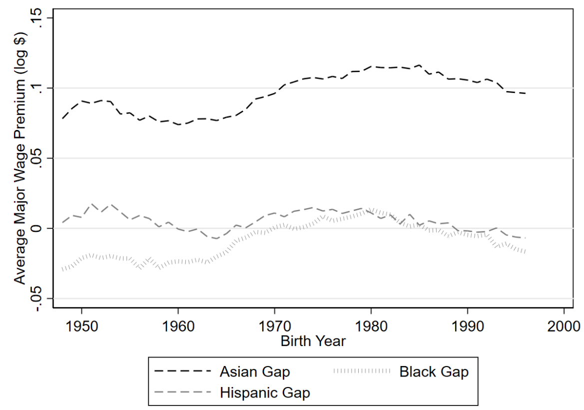
We conclude that pre-matriculation survey responses provide additional evidence that students had minimal knowledge of major restriction policies prior to matriculation, making diversion to other universities or any other enrollment sorting effects unlikely and second-order at best.

Figure JJ-1: Average College Major Premium by Birth Cohort and Specific Ethnicity

(a) Average Major Premium



(b) Average Major Premium Gap



Note: This figure shows that Black and Hispanic college graduates have followed similar college-major-premium trends since the mid-1960s – with major-premium gaps (relative to white graduates) that declined steadily by over 0.02 log dollars since 1980 – but that Asian graduates earn far higher-premium majors than any other ethnic group on average. Average college major premium by birth cohort and specific ethnicity – white, Asian, Black, and Hispanic – among 2009-2019 ACS respondents, and the difference between each ethnicity-cohort's average major premium and white graduates' average major premium in that cohort. College major premiums are estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, and year covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS; see Appendix B for details. Source: The 2009-2019 ACS (Ruggles et al., 2020).

Appendix J: Asian, Black, and Hispanic Major Attainment

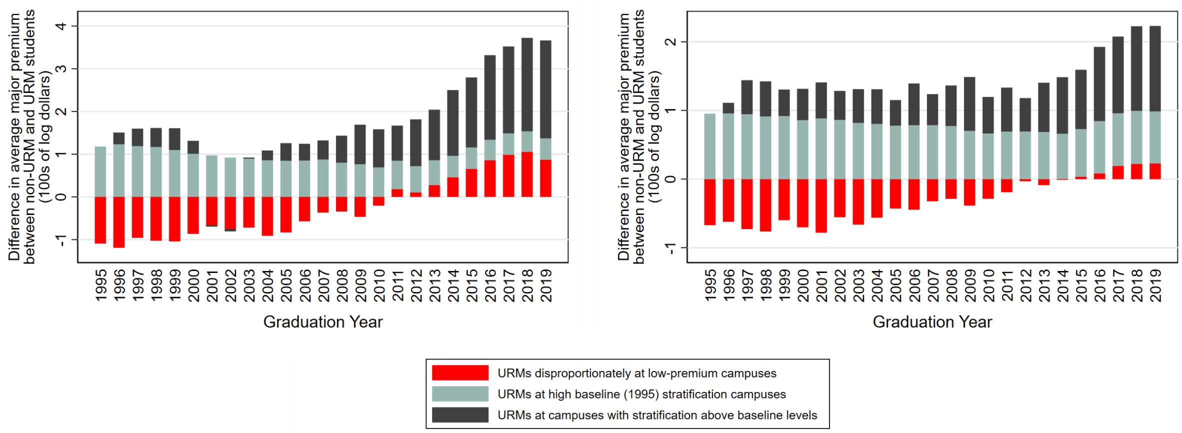
This study's baseline specifications compare the college major choices of underrepresented minority (URM) and non-URM students nationally and in the University of California system. In this appendix, we further disaggregate our presented findings into four ethnic groups – Black and Hispanic (URM) and white and Asian (non-URM) – to discuss ethnicity-specific trends and provide justification for our presented baseline aggregates. We omit the small population of students who are not part of any of these four categories (e.g. Native American students) due to insufficient power.

Figure JJ-1 replicates Figure 1 by ethnicity, showing the average college major premium earned by graduates in each birth cohort since the 1950s. While Black college graduates earned lower-premium majors than Hispanic graduates in the '50s and '60s birth cohorts, since the mid-1970s the two groups have earned similar-premium majors. Between the 1979 and 1996 birth cohorts, the college major premium gap between white and Black (Hispanic) college graduates grew by 0.021 (0.027) log dollars, similar trends that could plausibly be explained by similar mechanisms. Asian graduates, on the other hand, make up only a small share of college graduates but tend to earn much higher-premium degrees than white graduates, though the trend in premiums has been roughly similar to that of white graduates in recent years. The white-Asian gap declined during the relevant period by 0.016 log dollars (to about 0.1 log dollars). See Black et al. (2006) for further discussion of differences in college major choice between white and Asian college graduates.

Figure JJ-2 decomposes stratification over time separately for Black and Hispanic college graduates, relative to all non-URM graduates. Overall, between- and within-institution stratification trended up for both Black and Hispanic graduates, helping to justify our combining them in our analysis. All three trends

Figure JJ-2: Annual Between- and Within-Institution Stratification by Specific Ethnicity

(a) Black vs. non-URM graduates (b) Hispanic vs. non-URM graduates



Note: This figure shows qualitatively similar trends in between-institution, within-institution and total stratification for Black and Hispanic college graduates, with larger increases for Black graduates. Annual estimates of the three terms of Equation 5 for the 1995-2019 cohorts of college graduates, presenting average between-institution, static within-institution, and dynamic within-institution components of ethnic stratification across college majors in the U.S. higher education system. The static within-institution component fixes universities' level of stratification in 1995, while the dynamic component weights universities by their differential stratification (relative to 1995) in that year; otherwise the decomposition follows the traditional between-within pattern. The sample is limited to four-year degree-granting institutions in the 50 U.S. states. The universe of graduates excludes Hispanic graduates in (a), Black graduates in (b) and Native American or Alaskan Native graduates in both. Average college major premiums are assumed to be equal across ethnicities in institution $\times$ year cells in which no graduates of one ethnicity are observed. Major premiums are estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, and year covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS; see Appendix B for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2020) and IPEDS.

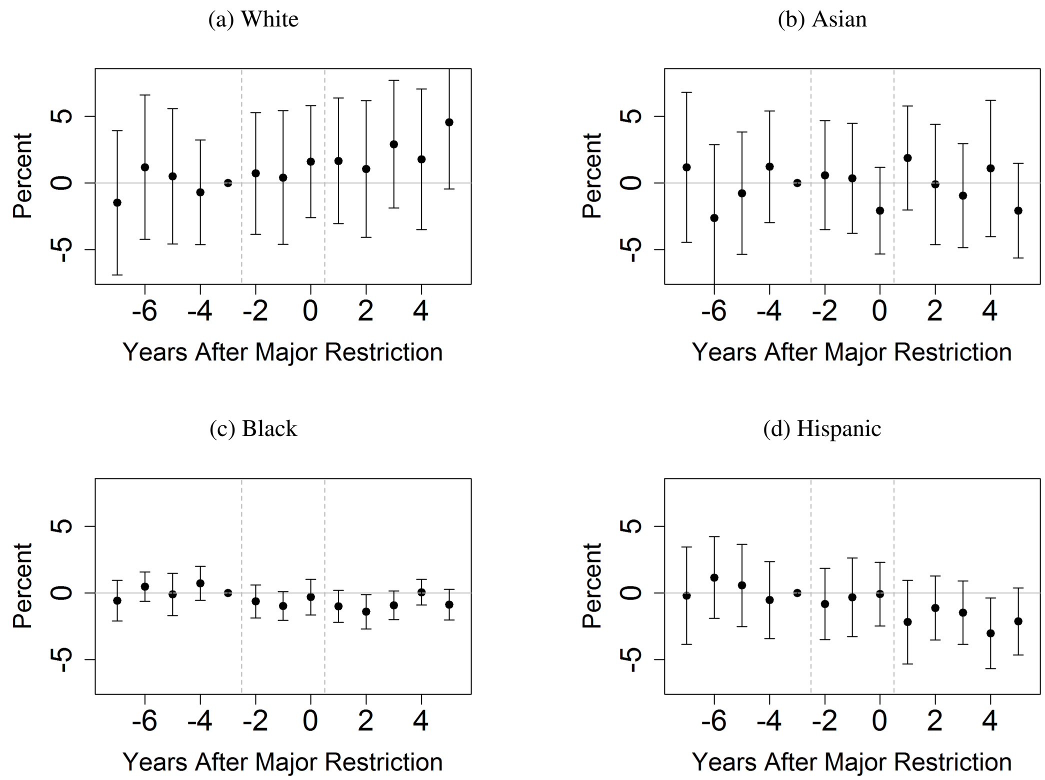
are more pronounced for Black graduates, who also increasingly earned degrees at historically less stratified campuses, consistent with reductions over time in their access to selective institutions.

How are these trends differentially explained by the impact of college major restrictions on students' major attainment? Figure JJ-3 presents difference-in-difference estimates of the impact of implementing a new major restriction on the share of students in each major by ethnic group. Of the 3.3 percentage point decline in restricted majors' URM enrollment between the pre-and post periods (Table 5), 1.2 percentage points of the decline was among Black students and 2.1 percentage points among Hispanic students. This suggests that Black students were the more impacted by the policy; their decline was twice what would have been expected if the effects were evenly proportional (using the full sample's enrollment shares). However, the declines faced by both groups (somewhat less clearly estimated for Black students) suggest that major restrictions tend to lead both groups to exit restricted majors.[80]

Both white and Asian enrollment increased in restricted majors following the restrictions' implementation, though the dynamics presented in Figure JJ-3 are noisily estimated. Enrollment increased by 0.9 percentage points among Asian students – whereas 1.1 percentage points would have been expected given Asian students' enrollment share – and 1.8 percentage points among white students (as expected). The remaining increase was among students of other or unreported ethnicities. This suggests that each group of

[80]UC majors in the sample were 15 percent Hispanic and 3 percent Black on average.

Figure JJ-3: Department-Level Difference-in-Difference Estimates by Specific Ethnicity



Note: This figure disaggregates the effects of major restrictions by ethnicity and shows clear evidence of declines in Black and Hispanic attainment in restricted majors and noisier evidence of an increases among white students. Staggered difference-in-difference β estimates following Equation 6 of the ethnicity shares of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database.

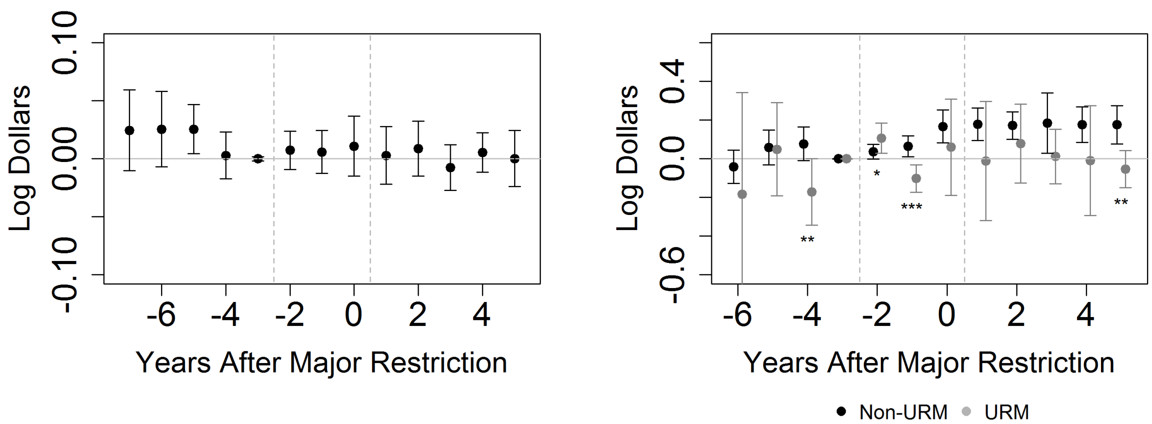
non-URM students was similarly impacted by major restriction policies, which justifies grouping them in the baseline analysis in the main text.

Appendix K: Major Restrictions and Match Effects

Section 7.1 presents evidence that major restriction policies do not provide educational access on the basis of comparative advantage. This appendix leverages a linear wage value-added model framework with heterogeneous treatment effects to directly estimate the effect of major restriction policies on overall and ethnicity-specific student 'match effects' in their attained college major.

We estimate two sets of major-specific value-added models (VAMs) across students separately by UC campus. First we estimate a VAM without treatment effect heterogeneity to measure overall average treatment effects:

Figure KK-1: Effect of Major Restriction Policies on Student Match Quality



Note: This figure shows that major restrictions neither (a) screened for students with above-average value-added in the restricted major nor (b) differentially led URM students toward majors where they achieved stronger match effects in wage terms, though they may have led some non-URM students toward fields where they had comparative advantages. Staggered difference-in-difference β estimates following Equation 6 of the student-major match quality (Ω_i^*) of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year (a), and difference-in-difference β_{it} estimates following Equation 7 of the relationship between freshman students' intending the restricted major ($\hat{M}_{im}$) and students' student-major match quality before and after the implementation of the restriction, estimated over a stacked dataset of students i's major intentions in field m. Student-major match quality is the second-order match effect between a student and their chosen major in linear value-added terms, as estimated following Equation KK-3. Outcomes are averages by declared major and cohort-year defined by students' first year of enrollment in Panel (a) – and students can be included in more than one major's average if they have declared multiple majors – and are at the student level in (b). Panel (b) controls for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection (See Figure A-22). β_{-3} is omitted, and standard errors are clustered by campus-major (a) or two-way clustered by campus-majors m and by students i (b). Source: UC ClioMetric History Project Student Database and the California Employment Development Department (Bleemer and Mehta, 2020).

$$w_{iy} = \Omega_{m_i}^{nh} + X_i + \epsilon_i \tag{KK-1}$$

where w_{iy} is freshman student i's California log annual wage in year $y + 10$ (when the student is in their late 20s) and Ω_m^{nh} are estimates of major m's overall average value-added.[81] The covariates X_i include students' individual GPA fixed effect interacted with gender (see Section 3) along with ethnicity and cohort fixed effects. Second, we estimate a VAM allowing for treatment effect heterogeneity:

$$w_{iy} = \Omega_{m_i i}^{h} + X_i + \epsilon_i \tag{KK-2}$$

where $\Omega_{m_i i}^{h}$ allows each institution's value-added to differ additively by students' gender, ethnicity, and GPA fixed effect quartile. We define a student's match quality in their chosen major by:

$$\Omega_i^* = \Omega_{m_i i}^{h} - \Omega_{m_i}^{nh}. \tag{KK-3}$$

[81] If students have no wages ten years after initial enrollment, wages from 9 or 11 years after enrollment are included instead. If no such wages are available, the student is omitted. Prior work has found that switching between majors – even between much more- and less-lucrative majors – has little effect on the California labor force participation (Bleemer and Mehta, 2022), but the presented results could be biased if restrictions led to changes in labor force participation or cross-state migration.

For example, a student with $\Omega_i^* = 0.01$ earned a major in which students like them tended to receive a wage 'bump' from earning that major (in a value-added sense) that was about one percentage point greater than the average 'bump' received by students who earned that major.

We estimate Equation KK-3 in two different settings to answer two questions. First, we investigate whether the implementation of major restriction policies changed the composition of students who earned the restricted majors in such a way as to increase the estimated match effects of declared students, which would suggest a potential efficiency improvement generated by restrictions relative to alternative allocation mechanisms. We split the student population of each campus into 'training' and 'testing' sets of equal magnitude and expand the student dataset to the student-major level, so that students with multiple majors appear multiple times in the data. We estimate Equations KK-1 and KK-2 over the training dataset and then generate estimates of $\hat{\Omega}_i^*$ for each member of the testing dataset. We then characterize each department-cohort by the average $\hat{\Omega}_i^*$ of its (training-dataset) students in that cohort and estimate the effect of implementing a major restriction on those majors' students' match effects following the staggered difference-in-difference design presented in Section 4. Standard errors are underestimated, since they do not account for noise in the estimates of $\hat{\Omega}_i^*$.

The resulting estimates are shown in Panel (a) of Figure KK-1. They show no evidence of a change in match quality in majors that implement major restriction policies. This implies that the GPA thresholds used by most major restriction policies to screen potential students appear to do so in a manner that is observably orthogonal to the value that those students would have derived from declaring the restricted major.

Second, we investigate whether imposing major restrictions changes the student-major match quality of students who intended to earn restricted majors. For each campus-major that implemented a restriction, we estimate Equations KK-1 and KK-2 over the training-sample half of students in the cohorts between four and five years prior to the restriction's implementation and then estimate $\hat{\Omega}_i^*$ for all other students in the years before and after the restriction's implementation. We then estimate the effect of implementing a major restriction on intended majors' student-major match quality separately by URM status using the difference-in-difference design presented in Section 5. As above, standard errors are underestimated.

Panel (b) of Figure KK-1 shows slight evidence of *improved* match quality for high-intention non-URM students and null or weakly *decreased* match quality for high-intention URM students. Combined with the estimates from Panel (a), this suggests that the non-URM students who exited restricted majors may have tended to flow toward majors where they had somewhat better match quality (though not higher overall value-added, as shown in Figure A-26, and there is some evidence of a non-parallel pre-trend), whereas URM students flowed toward majors where they had similar match quality. This evidence suggests that the URM students who disproportionately exited restricted majors not only flowed toward lower-paying majors (Figure A-26) but did so despite accruing no additional match value from those alternative majors.

We conclude that while the available evidence cannot reject that major restriction policies may have led some non-URM students to declare majors where they held comparative advantages, we find no evidence that restrictions either improved allocative efficiency by admitting differentially high-value-add students or led disproportionately-impacted URM students toward majors where they could achieve particularly-high value-added, measuring majors' value in terms of early-career wages.

Appendix L: Regression Discontinuity Designs and Major Restrictions

Mechanical major restriction policies are designed to exclude students with low grades in departments' introductory courses from declaring college majors in that department. For example, from 1997 to 2004 the UC Davis Department of Computer Science had a stated policy that students who wished to declare the major would have to take eight specified introductory courses: Computer Science 20, 30, 40, and 50 (or ECE 70) and Mathematics 21A, 21B, 21C, and 22A. Students were required to earn a 2.75 GPA in these courses in order to declare the major. The continuous nature of the determinative GPA variable and the sharp major declaration rule suggest that mechanical major restriction policies are a promising source of quasi-random variation for the estimation of the educational or labor market return to accessing the restricted major for on-the-margin students using a regression discontinuity design (van der Klaauw, 2002).

Bleemer and Mehta (2022) use exactly this research design to show large labor market costs of exclusion from the UC Santa Cruz economics major during the 2008-2012 period in which that restriction was sharply binding. Unfortunately, a variety of compromises in the on-the-ground implementation of major restriction policies make the majority of restrictions poor candidates for such a research design. Figure LL-1 shows the 'first stages' of four such designs. Panel (a) shows one typical pattern: the UC Berkeley Department of Psychology appears to have provided some leniency for students below its GPA threshold, so that while below-threshold students are much less likely to declare the major than above-threshold students, there is no sharp change in declaring psychology at the eligibility threshold itself. Panels (b) and (c) show a second typical pattern, including at the UC Davis Department of Computer Science: departments provide leniency to barely below-threshold students, resulting in no sharp change in major attainment at the threshold itself.[82] Panel (d) shows one example of an actual fuzzy discontinuity at the published threshold, at UC Santa Barbara's Department of Economics, but the change in major attainment is relatively small (below than 20 percentage points and half the size of the discontinuity studied in Bleemer and Mehta (2022)) because so many below-threshold students were nevertheless permitted to earn the major, challenging use of the discontinuity to identify changes in longer-run outcomes.[83]

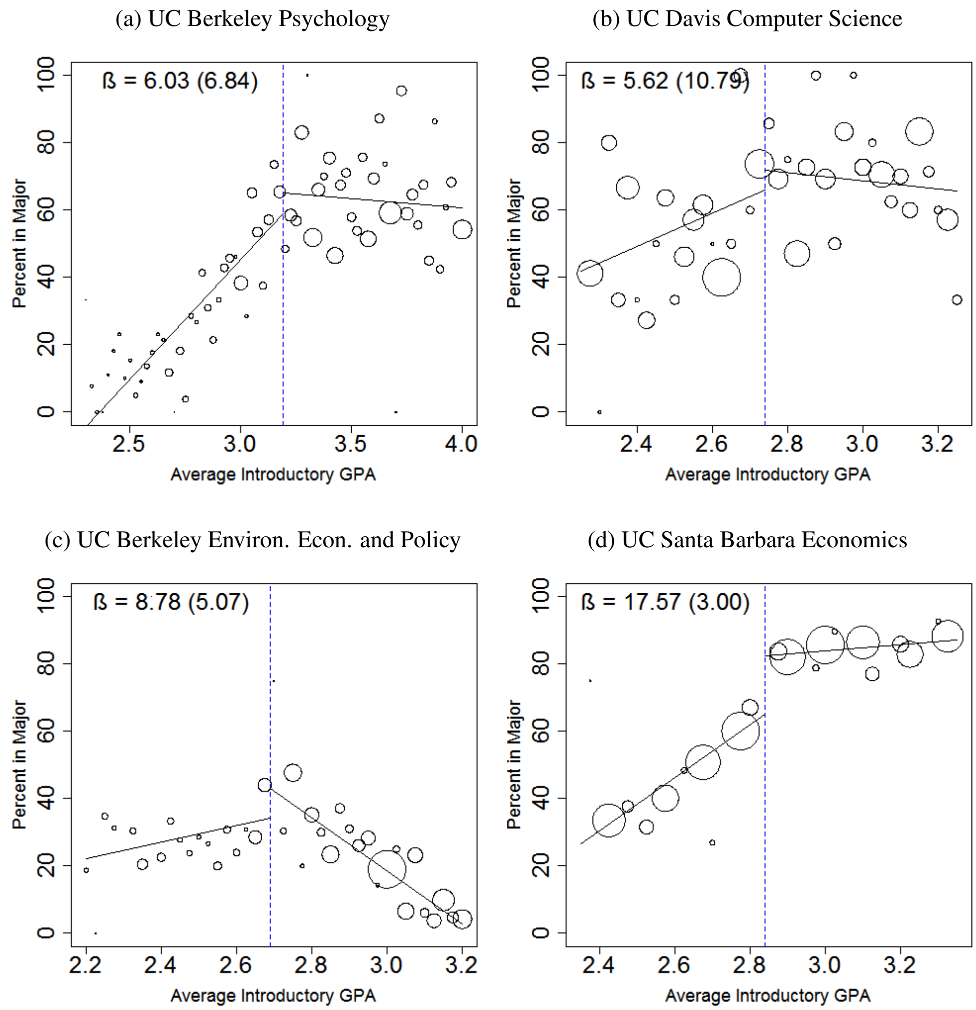
Additional problems limit the implementation of regression discontinuity research designs in other major restrictions. First, some departments provided students with some discretion in choosing which introductory courses to include in the department-specific GPA calculation used to determine eligibility, challenging researchers' definition of the relevant running variable (see, e.g., UCSB Computer Engineering in 2003-2013). Second, many departments enroll far too few students annually to precisely estimate student outcomes no matter the size of the first stage (e.g. between 1997 and 2000, UC Davis Exercise Science graduated only 250 students). Third, students may be able to exert additional effort in the final course in the restriction sequence to earn the necessary grade required to achieve an above-threshold GPA, producing a classic bunching pattern that suggests the violation of continuous potential outcomes at the eligibility threshold that invalidates regression discontinuity designs.

As a result of these empirical and structural limitations, we do not pursue regression discontinuity analysis in our estimation of the general effects of major restriction policies on below-threshold students. Instead, we implement the alternative research design discussed in Section 8.3 to estimate the average effect of being excluded from restricted majors.

[82] Future work could employ a learning strategy to identify thresholds from the observed data.

[83] URM students are underrepresented among below-threshold students admitted to restricted majors by discretion (Table A-4).

Figure LL-1: Major Declaration Above and Below the Published GPA Threshold

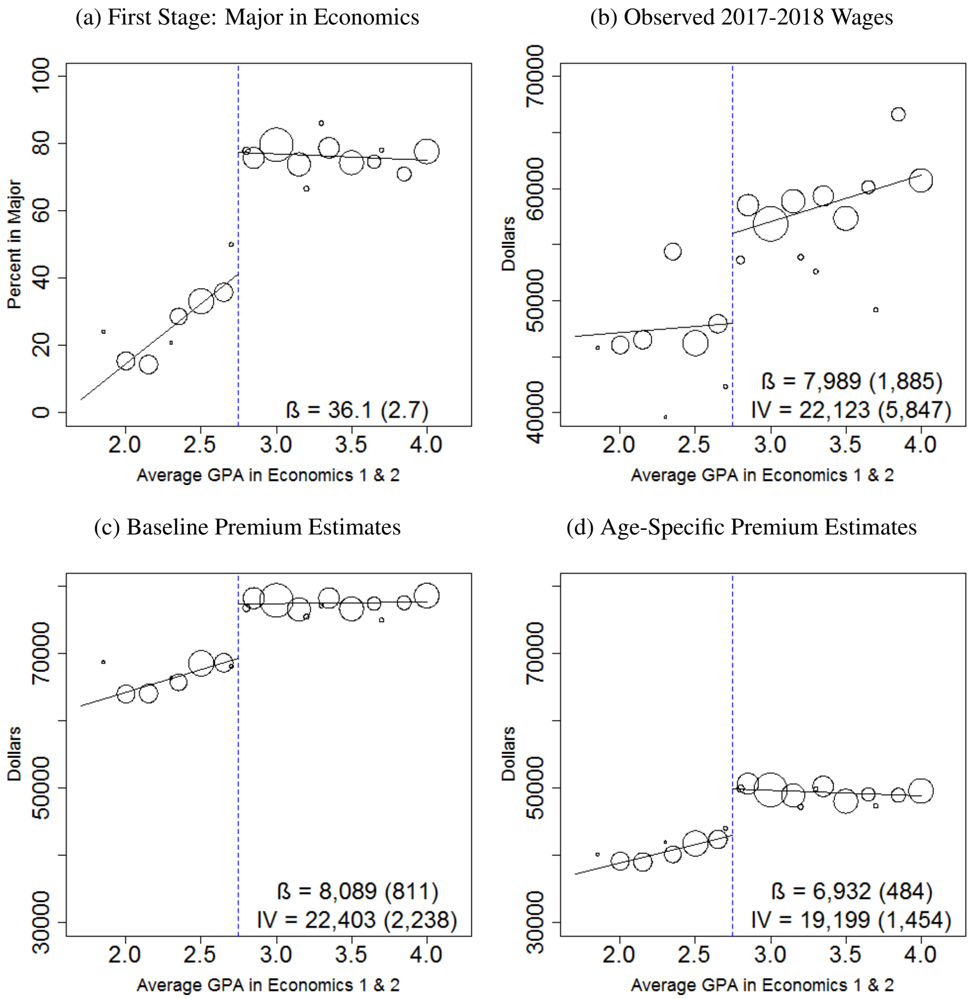


Note: This figure shows that major restrictions generally do not produce sharp discontinuities in major attainment at the stated GPA threshold, challenging implementation of regression discontinuity designs in estimating their effects on near-threshold students. The share of students at each major-specific GPA (in 0.025-width bins) who declare the reported major, where major-specific GPA is calculated from specified courses defined by the major's restriction policy. Students who take multiple qualifying courses for a single course category are assigned the grade from the earlier course to prohibit strategic manipulation of the running variable. Santa Barbara Economics includes the 2009-2018 cohorts; Berkeley Psychology includes the 2003-2014 cohorts; Davis Computer Science includes the 1997-2002 cohorts; and Berkeley Environmental Economics and Policy includes the 2009-2014 cohorts. The size of each circle corresponds to the proportion of students who earned that GPA. Cohort years are defined by year of entry. Source: UC ClioMetric History Project Student Database.

The fact that major restrictions do not generally admit analysis via regression discontinuity designs does not imply that they are not impactful with regard to student enrollment in restricted majors. To the contrary, the evidence provided in the main text clearly shows that major restrictions have substantial effects on overall enrollment as well as enrollment composition. However, Figure LL-1 shows that the disenrollment effects of major restriction policies are often spread continuously across students below the GPA threshold (or discontinuously in a manner discordant with the published policy), limiting the use of regression discontinuity analyses in documenting the effects of restrictions on below-threshold students.

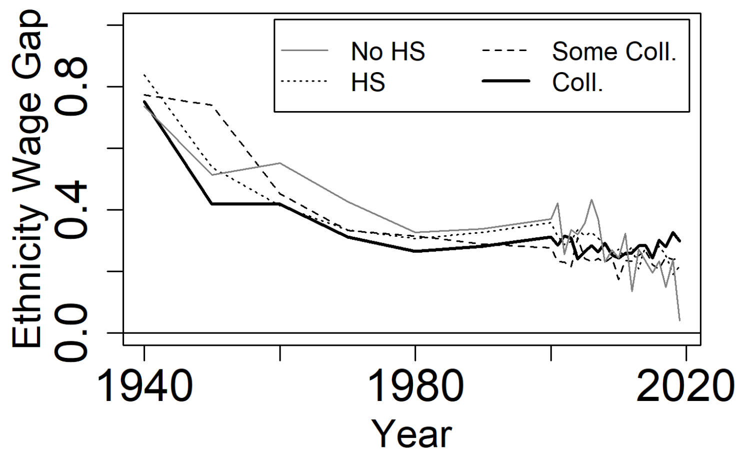
Other Appendix Figures and Tables

Figure A-1: Quasi-Experimental Validation of College Major Premium Estimates



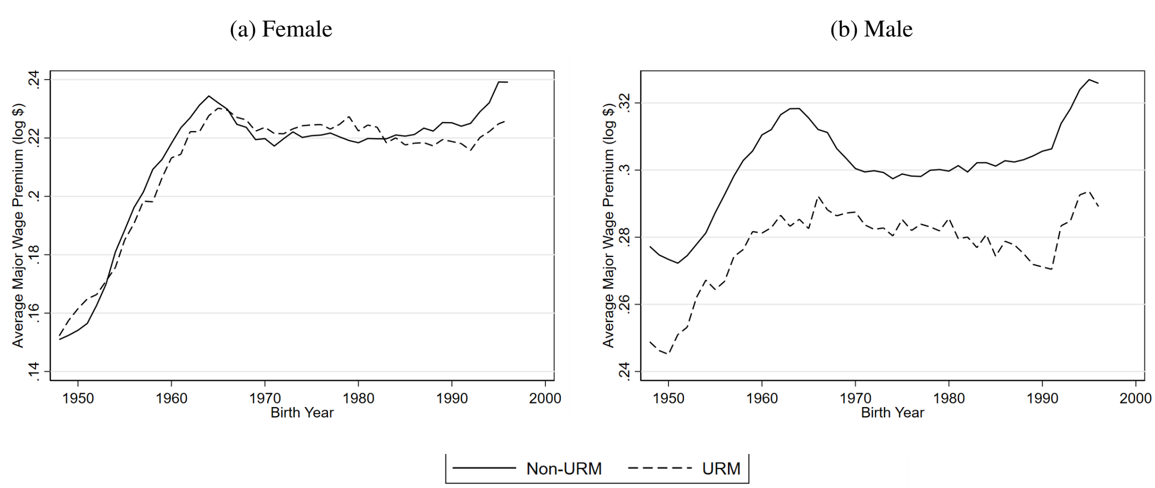
Note: This figure shows that the college major premium (ω_m) estimates accurately predict the change in students' observed annual earnings resulting from a quasi-experimental shift in the college majors of 2008-2012 UC Santa Cruz compliers (who preferred to major in economics), whether using the baseline major premium estimates or replacing the estimation sample with same-age ACS respondents. The similarity of the regressions in Panels (b) and (c) implies a forecast coefficient for ω_m of 1.07 (s.e. 0.36). Panels (a) and (b) replicate Figures 1 and 2 from Bleemer and Mehta (2022), showing a sharp discontinuity in access to the economics major at UC Santa Cruz between 2008 and 2012 as a result of the department's 2.8 GPA major restriction policy, visualizing both the (first-stage) decline in economics major attainment and the change in average annual California wages earned by students. Panels (c) and (d) show the economic value of majors earned by the UCSC students, measuring economic value by the baseline major premium estimates (Table A-2) and using a comparable set of statistics (also following Equation BB-1) estimated over a population of workers of similar age as those in the UCSC wage estimation sample (ages 24-29). College major premium estimates are additively inflated by the average log wage of the hold-out major (general agriculture) in each sample and exponentiated for comparability with the UCSC wage estimates. The size of each circle corresponds to the proportion of students who earned that GPA. Cohort years are defined by year of entry. The forecast coefficient of 1.07 is the 2SLS-estimated coefficient on the observational major wage premiums we use in this study, using panel (c) as the first-stage regression. Source: UC ClioMetric History Project Student Database, the American Community Survey (Ruggles et al., 2020), and (Bleemer and Mehta, 2022).

Figure A-2: Average Mid-Career Ethnicity Wage Gap Over Time by Education Level



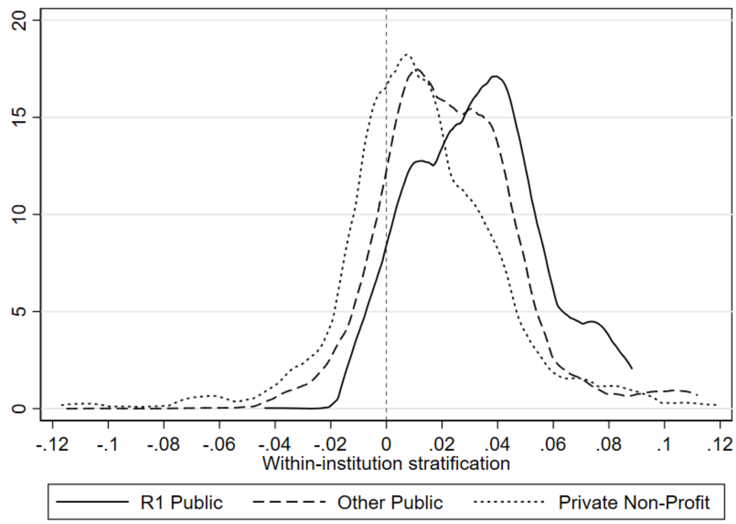
Note: This figure shows that URM workers have historically earned substantially lower wages than similarly-educated non-URM workers, but that while that gap has closed across education groups since the 1940s, that convergence has recently slowed – and even reversed – among college-educated (but not less-educated) workers. This figure shows the difference in mean log wages earned by male native-born non-URM and URM workers between ages 38 and 42 by year and education level: no high school degree, high school degree but no college, some college but no four-year college degree, and (at least) a four-year college degree. The sample is restricted to individuals with positive observed wages. URM includes Black, Hispanic, and Native American workers; non-URM includes all other workers. Samples include 1% samples of the 1940, 1950, and 1970 U.S. censuses, 5% samples of the 1960, 1980, 1990, and 2000 censuses, and all subsequent ACS respondents; averages are weighted by sample weights. Source: The 1940-2000 U.S. Decennial Census and the 2001-2019 American Community Survey (Ruggles et al., 2020).

Figure A-3: Aggregate College Major Ethnic Stratification Separately by Gender



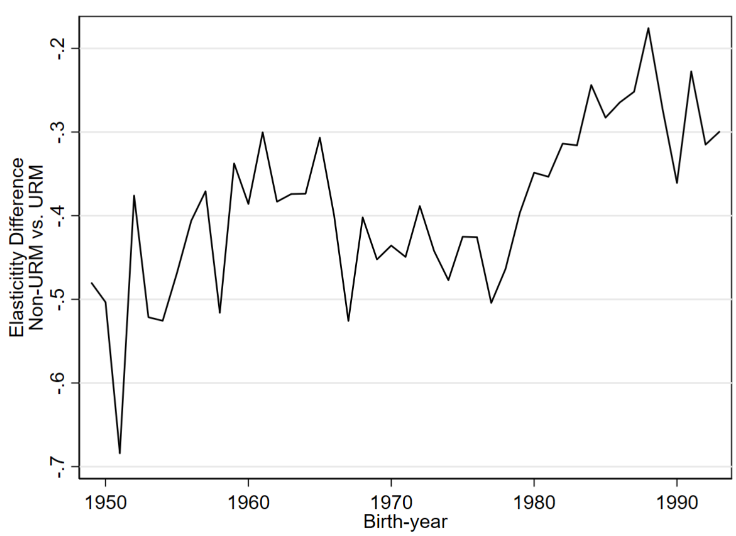
Note: This figure shows that while college major stratification has long been smaller among women than among men, the qualitative trend has been the same among both groups, with a narrowed gap in the 1970-1980 birth cohorts that has slowly widened since that time. This figure depicts the average college major premium attained by birth cohort and ethnicity among all female (a) and male (b) college graduates, as in Figure 1. College major premiums are estimated by regressions of log wages on major indicators and covariates listed in Appendix B over wage employees aged 35-45 in the 2009-2019 American Community Survey. Source: The American Community Survey (Ruggles et al., 2020).

Figure A-4: URM-Student-Weighted Distribution of Within-Institution Stratification in 2021, by Institution Type



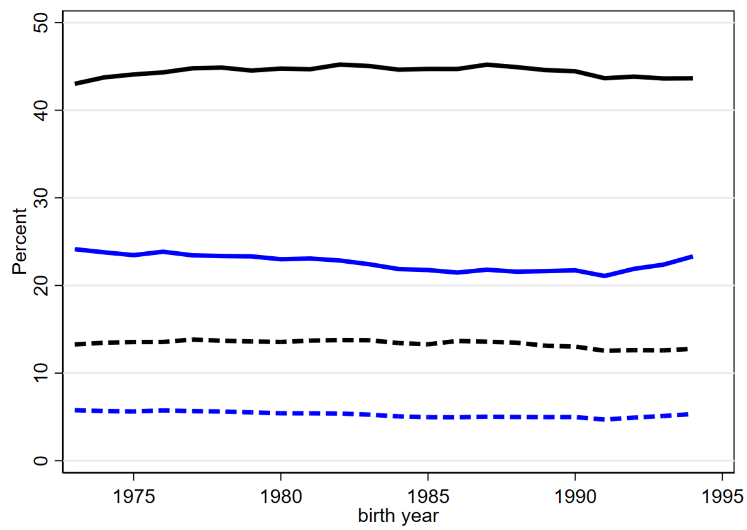
Note: This figure shows that within-institution ethnic stratification by major is more severe at public than at private non-profit institutions, and more severe still at research-intensive public universities. The distribution of 2021 within-institution stratification (Equation 3) by institutional type. Each institution is weighted by the number of URM students it graduated that year. The sample is limited to four-year public or non-profit degree-granting institutions in the US. Average college major premiums are assumed to be equal across ethnicities in institution $\times$ year cells in which no graduates of one ethnicity are observed. Major premiums are estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, and year covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS; see Appendix B for details. Source: 2009-2019 American Community Survey (Ruggles et al., 2020) and IPEDS.

Figure A-5: Racial Difference in Elasticity of Wages to College Major Premiums



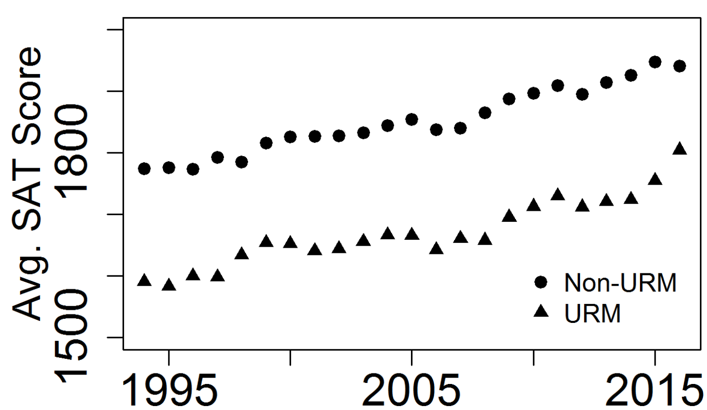
Note: This figure shows that URM students' wage return to completing higher-premium majors has *grown* relative to those of non-URM students. Ethnic differences in elasticities are birth-year-specific coefficients on the interaction between a students' major premium and a URM dummy in a log-wage regression that includes birth-year x URM and birth-year x ACS-year x major x gender indicators. Sample is restricted to employees with a 4-year college degree between ages 25 and 60. Source: American Community Survey (Ruggles et al., 2020).

Figure A-6: Probability of High School Graduates' College and R1 Completion by Ethnicity



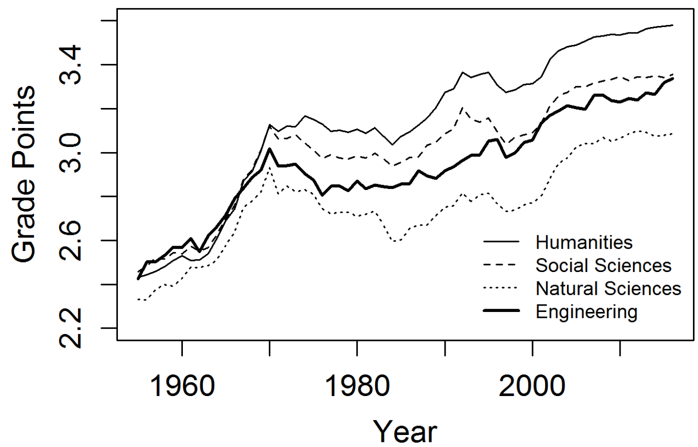
Note: This figure shows that the chances of URM high school graduates earning a college degree, or a college degree from an R1 university, have not grown significantly faster than those of non-URM graduates. Solid lines are the fraction of high-school graduates in each ethnic group and birth cohort who have completed at least a four-year degree. Dashed lines are the fraction who have completed a 4 year degree from an R1 institution. Blue (black) lines reflect probabilities for URM (non-URM) students. Source: The American Community Survey (Ruggles et al., 2020) and IPEDS.

Figure A-7: Average SAT Score of UC Students by URM Status and Cohort



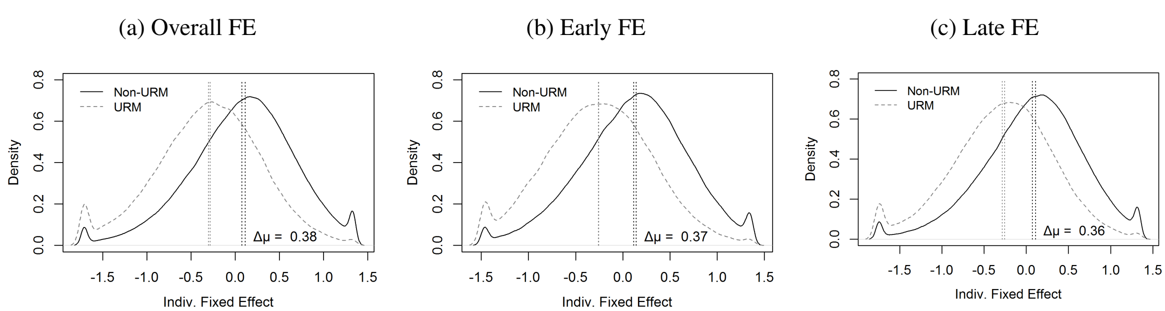
Note: This figure shows that R1 URM students' SAT scores have not declined over the past 30 years, suggesting that the increasing stratification within these institutions is not driven by negative selection among URM students. Average SAT score at UC Berkeley, UC Davis, UC Santa Cruz, and UC Santa Barbara by freshman cohort and URM status. Each campus is equally weighted in each series. Source: UC ClioMetric History Project Student Database.

Figure A-8: Average UC Berkeley Grades by Discipline over Time



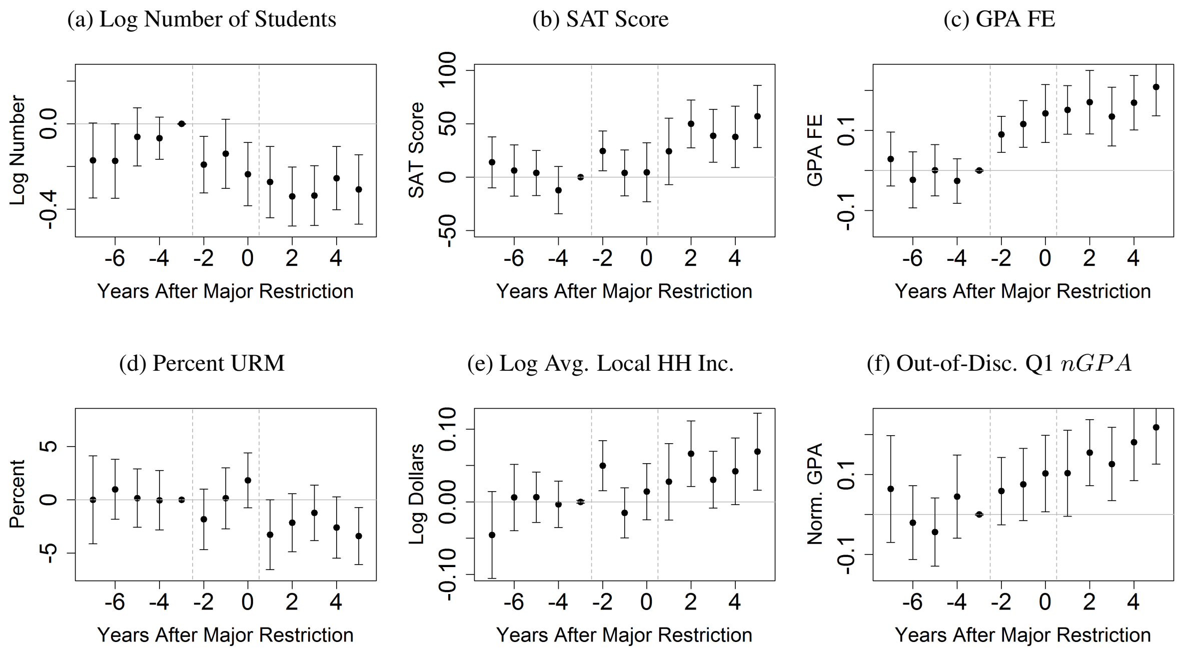
Note: This figure shows stark evidence of differential grade inflation by discipline at one public university, justifying the need for normalized GPAs over time and course. Average grade points earned by undergraduate students in Humanities, Social Science, Natural Science, and Engineering courses at UC Berkeley annually from 1955 to 2016. Departments categorized by the authors. Source: UC ClioMetric History Project Student Database.

Figure A-9: Distribution and Non-Convergence of GPA Fixed Effects



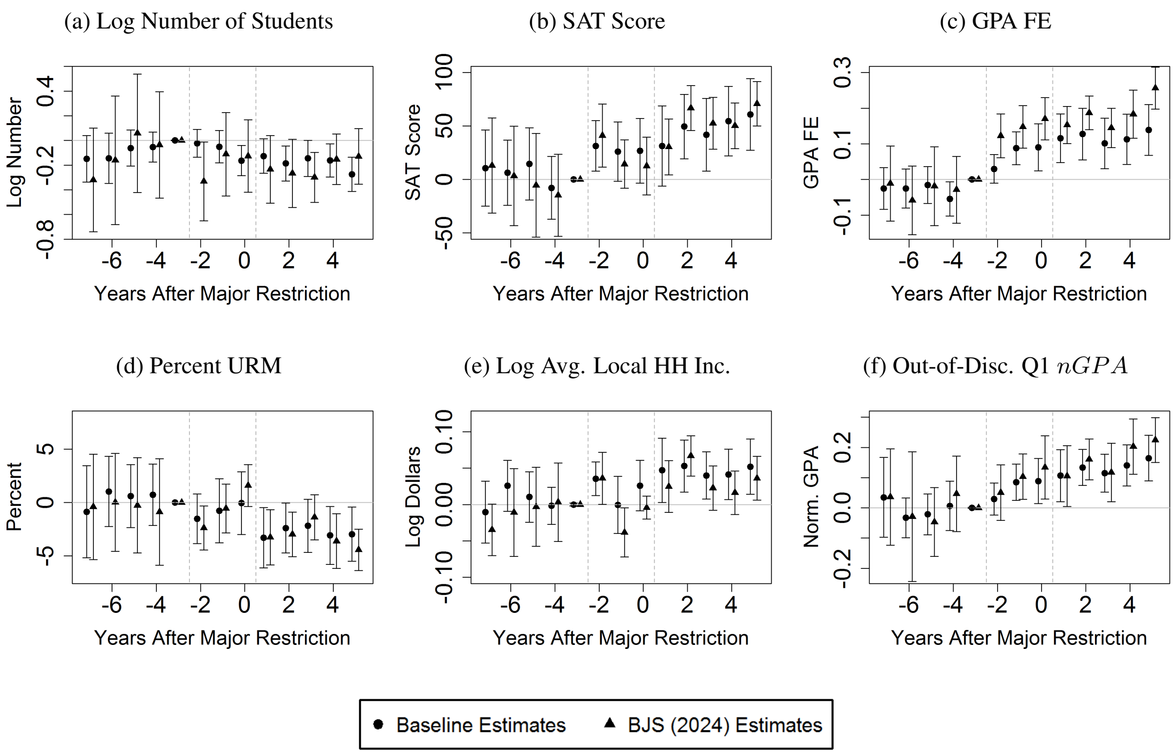
Note: This figure shows that URM UC students have consistently poorer average academic performance (as measured by grades) that does not converge over time, suggesting that educational allocation on the basis of even later-year academic performance would likely generate stratification. Distribution of observed students' GPA fixed effects by ethnicity overall and when estimated with separate individual effects for courses taken in the first two academic years ("Early") and courses taken in subsequent years ("Late"). Coefficients from two-way fixed effect regressions of GPA on student and course-term fixed effects estimated separately by UC campus; fixed effects are de-meaned by campus and are presented without shrinkage, though students with fewer than five courses in either relevant period are omitted. Dotted lines show the median and mean by URM status, and the reported coefficient is the difference between the non-URM and URM means. Source: UC ClioMetric History Project Student Database.

Figure A-10: Robustness of Department-Level Event Studies to Major-Year Fixed Effects



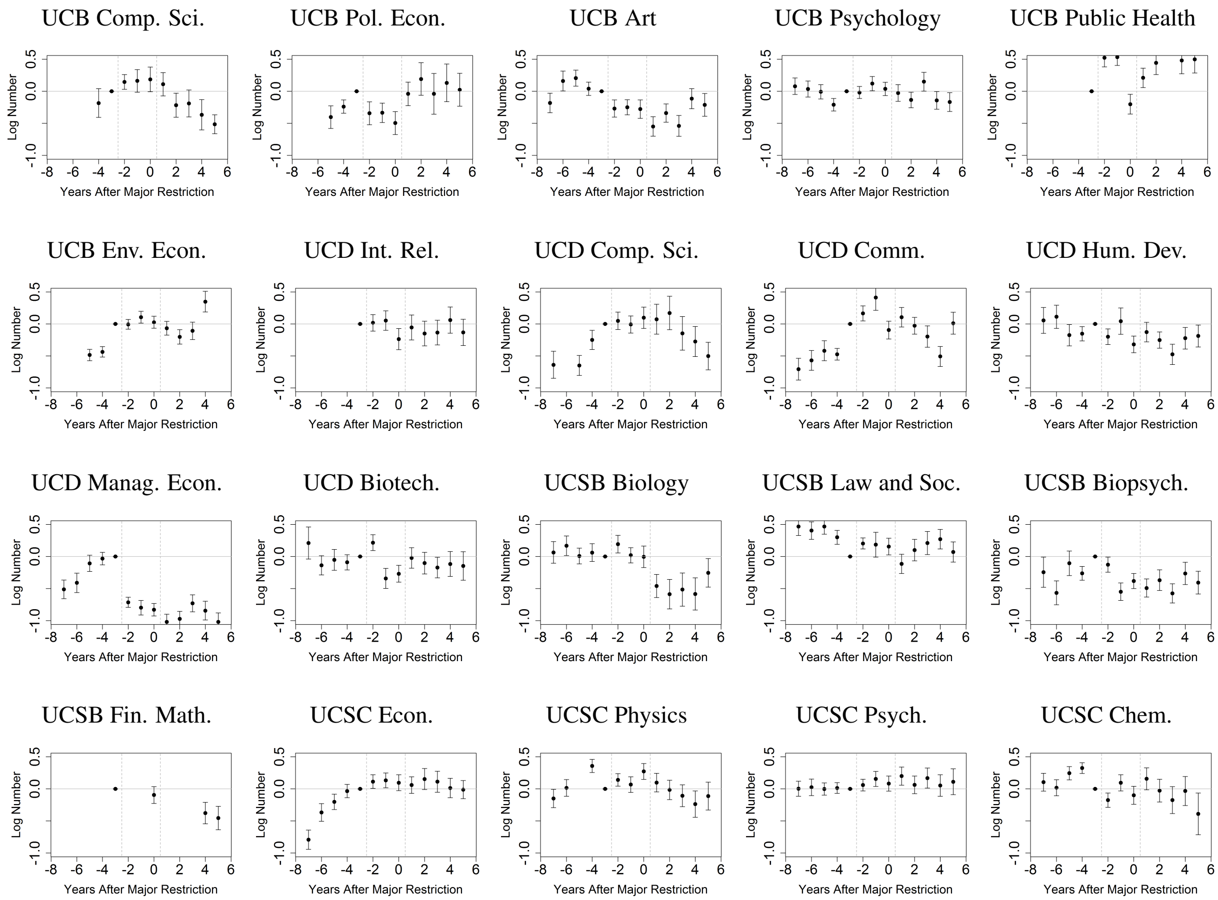
Note: This figure shows that the difference-in-difference coefficients shown in Figures 6, 7, and 11 are qualitatively robust to including major-specific cohort fixed effects instead of discipline-cohort effects, despite the empirical limitations generated by imperfect major matches across campuses. Staggered difference-in-difference β estimates following Equation 6 of demographic and academic characteristics of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year, estimated replacing discipline-year fixed effects with major-year fixed effects (grouping similar majors under headers like biology, ethnic studies, and agriculture). Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major's average if they have declared multiple majors. See Section 3 for the definition of $nGPA$. Out-of-discipline courses include those taken outside the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, and Professional) and excluding Mathematics and Statistics courses. Source: UC ClioMetric History Project Student Database and IRS SOI.

Figure A-11: Robustness of Department-Level Event Studies to Heterogeneous Treatment Effects



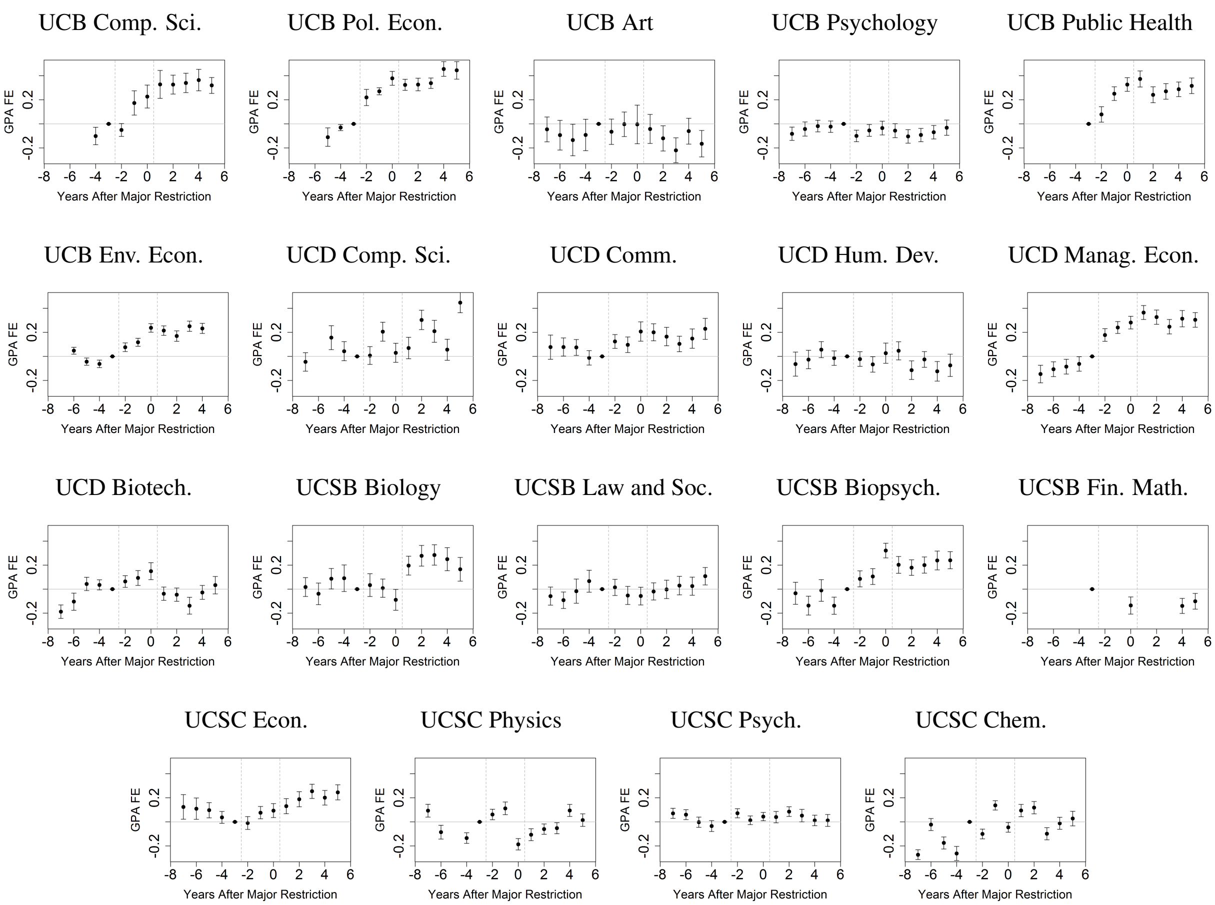
Note: This figure shows that the difference-in-difference coefficients shown in Figures 6, 7, and 11 are robust to being alternatively estimated following Borusyak, Jaravel, and Spiess (2024), who propose an estimator that permits unrestricted treatment effect heterogeneity. Staggered difference-in-difference β estimates following Equation 6 of demographic and academic characteristics of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year, estimated using the baseline specification and using the estimator (and package) proposed by Borusyak, Jaravel, and Spiess (2024), with the latter absorbing campus- and major-year fixed effects. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted – or for Borusyak, Jaravel, and Spiess (2024), coefficients are shown relative to the first pre-period – and standard errors are clustered by campus-major. Students can be included in more than one major's average if they have declared multiple majors. See Section 3 for the definition of $nGPA$. Out-of-discipline courses include those taken outside the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, and Professional) and excluding Mathematics and Statistics courses. Source: UC ClioMetric History Project Student Database.

Figure A-12: Individual Department Event Studies: Log Number of Students



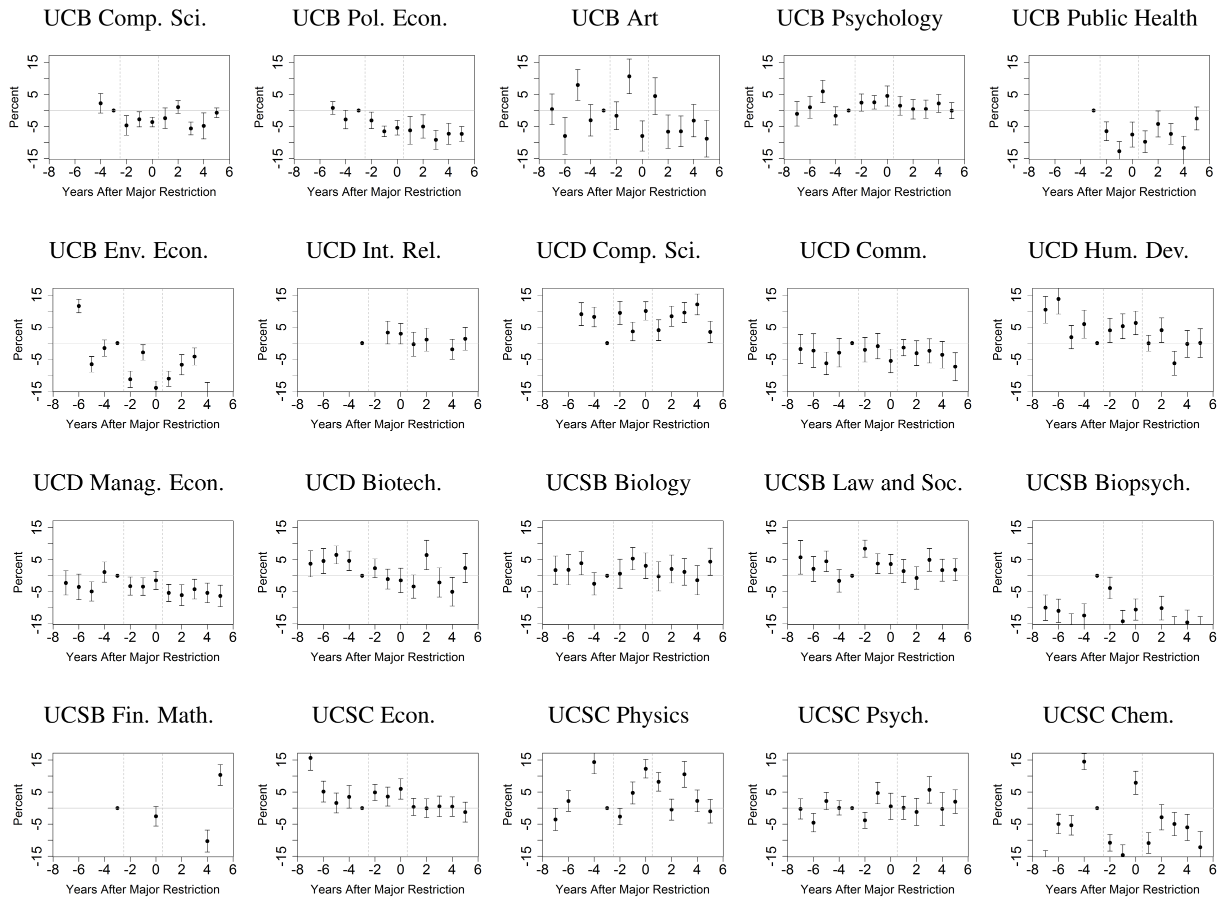
Note: This figure visualizes substantial heterogeneity in the enrollment effects of major restriction implementation, with some policies apparently failing to immediately bind with regard to net enrollment, though implementation tends to arrest growth and lead to small enrollment declines. Staggered difference-in-difference β estimates following Equation 6 of the log number of freshman students in each respective major before and after the implementation of its restriction, relative to other majors in that campus-year. Estimated over the full sample of campus-major-cohorts, but only including one 'event' per figure. β_{-1} is omitted and standard errors are clustered by campus-major. Coefficients are missing in the earliest years during which the major did not exist and in the latest years when the restriction was lifted. Source: UC ClioMetric History Project Student Database.

Figure A-13: Individual Department Event Studies: GPA Fixed Effect



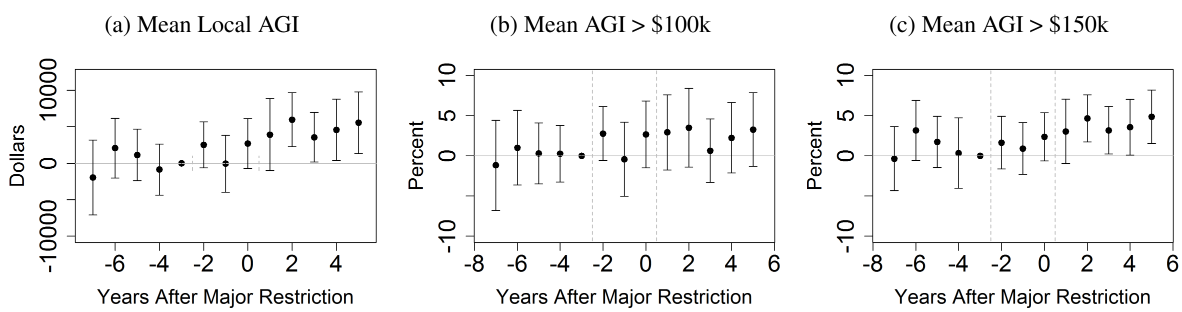
Note: This figure shows heterogeneity in the academic composition effects of major restriction policies, though restriction implementation tends to lead majors to enroll academically-stronger students. Staggered difference-in-difference β estimates following Equation 6 of each major's declared freshman students' GPA fixed effect before and after the implementation of its restriction, relative to other majors in that campus-year. GPA fixed effects are defined as students' individual fixed effect from a two-way fixed effect model of GPA on student and course effects. Estimated over the full sample of campus-major-cohorts, but only including one 'event' per figure. β_{-1} is omitted and standard errors are clustered by campus-major. Coefficients are missing in the earliest years during which the major did not exist and in the latest years when the restriction was lifted. Source: UC ClioMetric History Project Student Database.

Figure A-14: Individual Department Event Studies: Percent of Majors URM



Note: This figure shows that major restriction implementation tends to decrease majors' URM enrollment, particularly in majors where Figures A-12 and A-13 suggest that the implemented restriction was immediately binding. Staggered difference-in-difference β estimates following Equation 6 of the percent of declared freshman students in each respective major who are underrepresented minorities (URM) before and after the implementation of the major's restriction, relative to other majors in that campus-year. Estimated over the full sample of campus-major-cohorts, but only including one 'event' per figure. β_{-1} is omitted and standard errors are clustered by campus-major. Coefficients are missing in the earliest years during which the major did not exist and in the latest years when the restriction was lifted. Source: UC ClioMetric History Project Student Database.

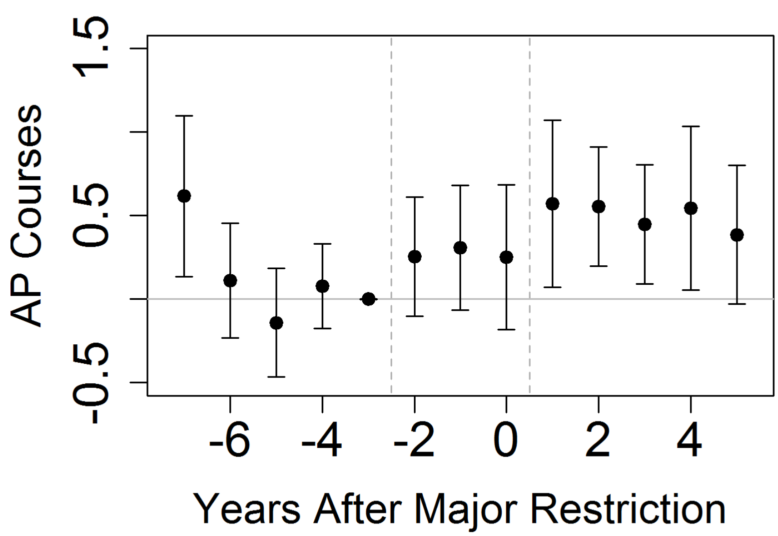
Figure A-15: Departments' Economic Composition Before and After New Major Restrictions



Note: This figure shows that implementing a new major restriction increased the affluence of the students who successfully declared the restricted major, with particularly sharp relative enrollment increases among high-income students (from Zip codes above the 80th household income percentile across UC students). Staggered difference-in-difference β estimates following Equation 6 of average local household incomes of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Average local household income is measured as the CPI-adjusted mean adjusted gross income of tax-filing households in the student's Zip code in their first year of enrollment; see Appendix E. Panels (b) and (c) estimate differences in the percent of students with average local household incomes above $100,000 and $150,000, respectively. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database and (Bleemer and Mehta, 2020).

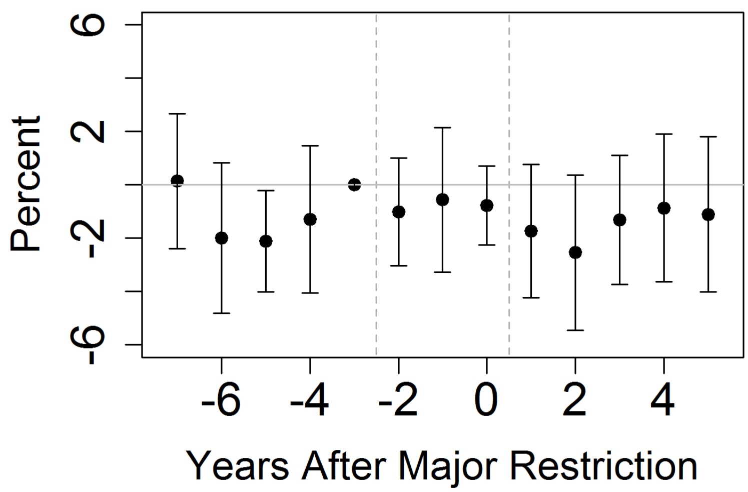
Figure A-16: Departments' Pre-College Academic Opportunity Composition Before and After New Major Restrictions

(a) Number of AP Courses at HS



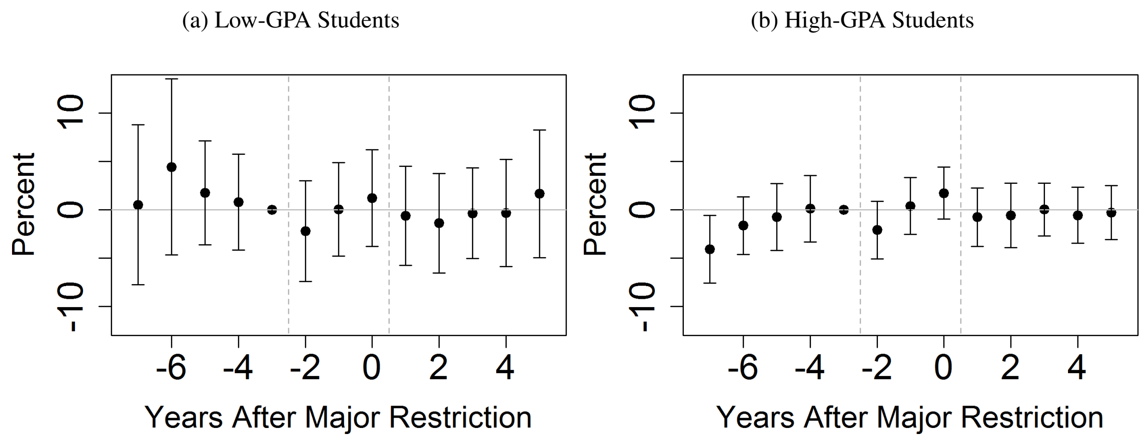
Note: This figure shows that major restrictions disproportionately exclude students who attended low-opportunity high schools as measured by AP course availability, though the estimates are somewhat noisy because the sample is restricted to the 1994-2018 California residents with observed high schools. Staggered difference-in-difference β estimates following Equation 6 of the average number of AP courses available at declared majors' high schools, relative to other majors in that campus-year. High school characteristics are observed for 1994-2018 enrollees from California high schools; private high schools (attended by about 10 percent of such students) are assigned to the 90th percentile of AP course availability among public schools in that year. Δ (Post-Pre) with standard errors is estimated to be 0.31 (0.12). Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted; standard errors are clustered by campus-major; and 95 percent confidence intervals are shown. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database and the California Department of Education.

Figure A-17: Departments' Share of In-State Students Before and After New Major Restrictions



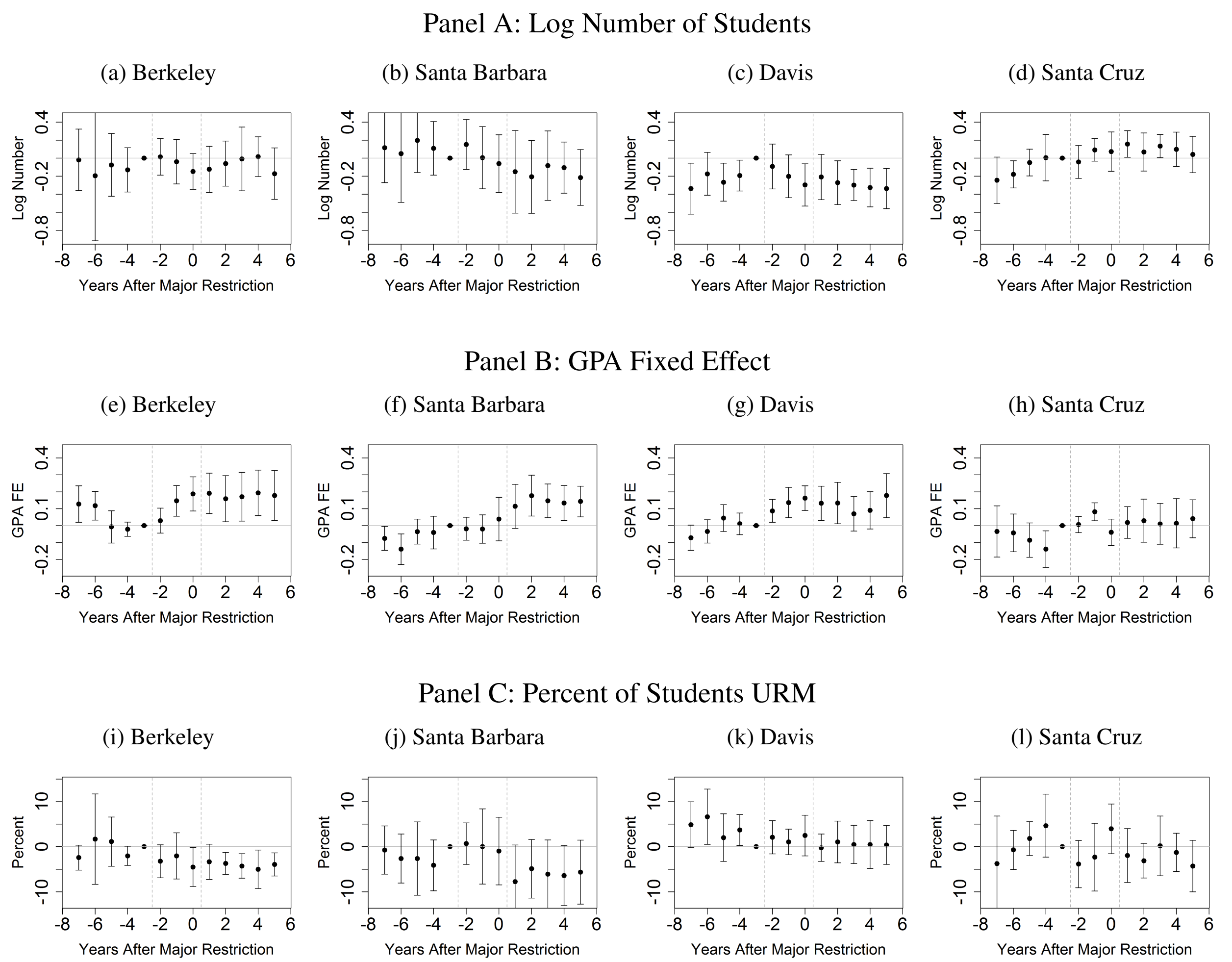
Note: This figure shows that implementing a new major restriction had no measurable effect on the share of in-state students in that major, suggesting that major restrictions were not differentially binding for in-state students relative to their out-of-state and international peers. Event study β estimates following Equation 6 of the share of freshman students who declare restricted majors before and after the implementation of the restriction who were California residents, relative to other majors in that campus-year. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database and (Bleemer and Mehta, 2020).

Figure A-18: Departments' Ethnicity Composition Before and After New Major Restrictions for Students with Above- and Below-Median Academic Performance



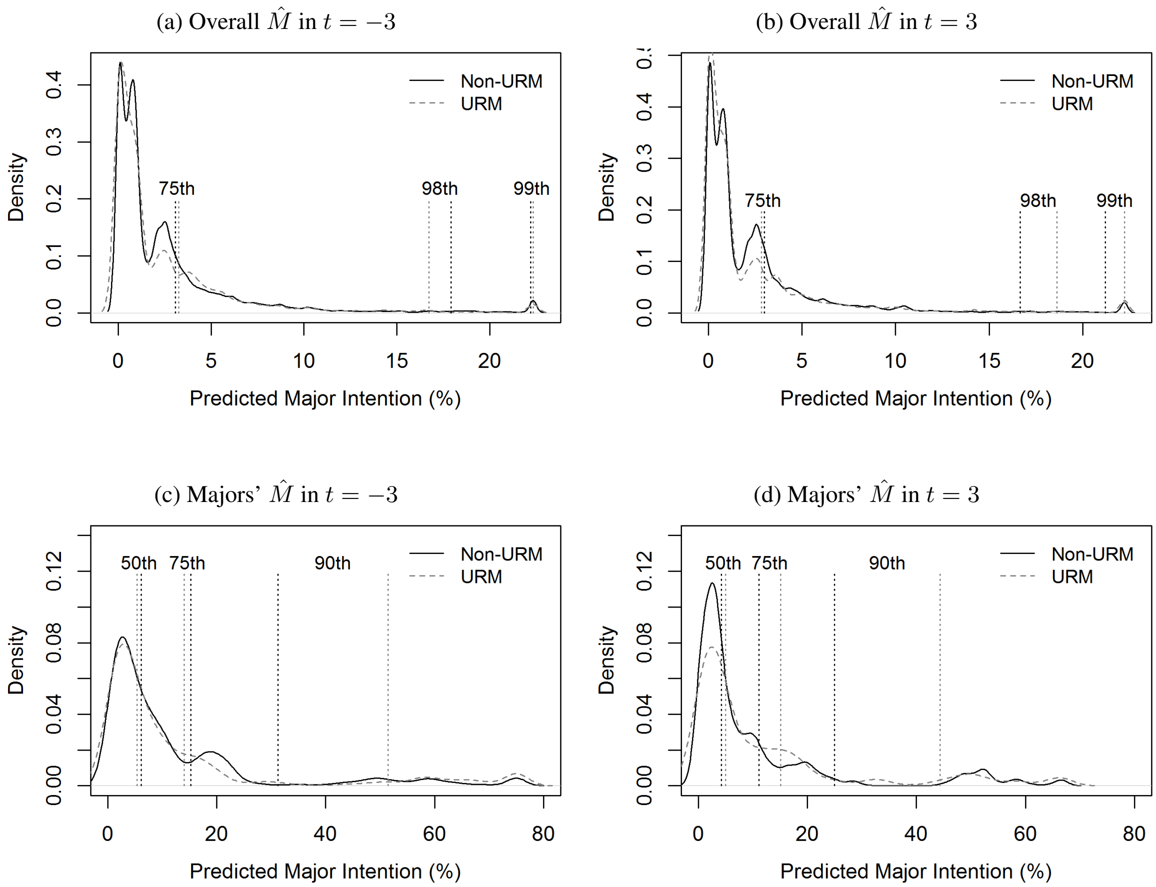
Note: This figure shows that splitting the sample at the median GPA fixed effect largely absorbs the relationship between major restriction policies and departmental URM shares, implying that the decline in URM shares occurs largely between, not within, academic performance buckets. Staggered difference-in-difference β estimates following Equation 6 of the URM share of freshman students who declare restricted majors before and after the implementation of the restriction with the sample split between students with below- and above-median GPA fixed effects, relative to other majors in that campus-year. GPA fixed effects are estimated by two-way fixed effect regressions of GPA on student and course-term fixed effects estimated separately by UC campus; fixed effects are de-meaned by campus and are presented without shrinkage, though students with fewer than five courses are omitted. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted; standard errors are clustered by campus-major; and 95 percent confidence intervals are shown. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database.

Figure A-19: Campus-Specific Department-Level Difference-in-Difference Estimates



Note: This figure shows that the main effect of implementing new major restriction policies is replicable at Berkeley, Davis, and Santa Barbara, but that major restrictions have no immediate estimable effect at the Santa Cruz campus, which apparently did not enforce its restrictions. Staggered difference-in-difference β estimates following Equation 6 of demographic and academic characteristics of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year and estimated separately by campus. GPA fixed effect is the student effect from a two-way fixed effect model of grades on students and course-terms. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major's average if they have declared multiple majors. Source: UC ClioMetric History Project Student Database.

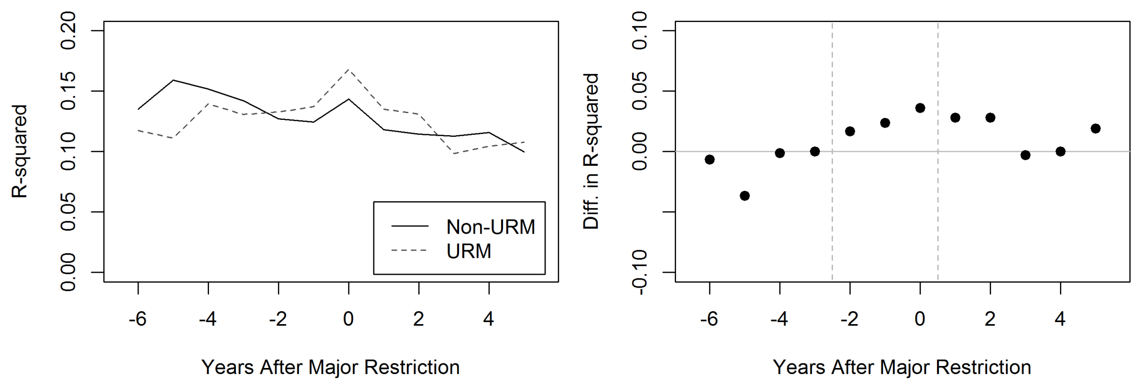
Figure A-20: Distribution of Estimated Major Intentions by Ethnicity



Note: This figure shows that over one percent of both URM and non-URM students in the non-training sample were predicted to earn restricted majors using their first-term Fall courses with a probability of at least 20 percent, though the distribution is skewed toward 0, even among students who end up declaring the major. Kernel density plots of winsorized $\hat{M}$, students' predicted likelihood of earning each restricted major (as estimated by random forest as described in Section 5), overall and among students who earned the restricted major, by ethnicity and number of years before or after the major's restriction was imposed. Measured in a stacked dataset of students for each restricted major. Percentiles are indicated by ethnicity. Source: UC ClioMetric History Project Student Database.

Figure A-21: The Predictive Power of $\hat{M}$ Before and After New Major Restrictions

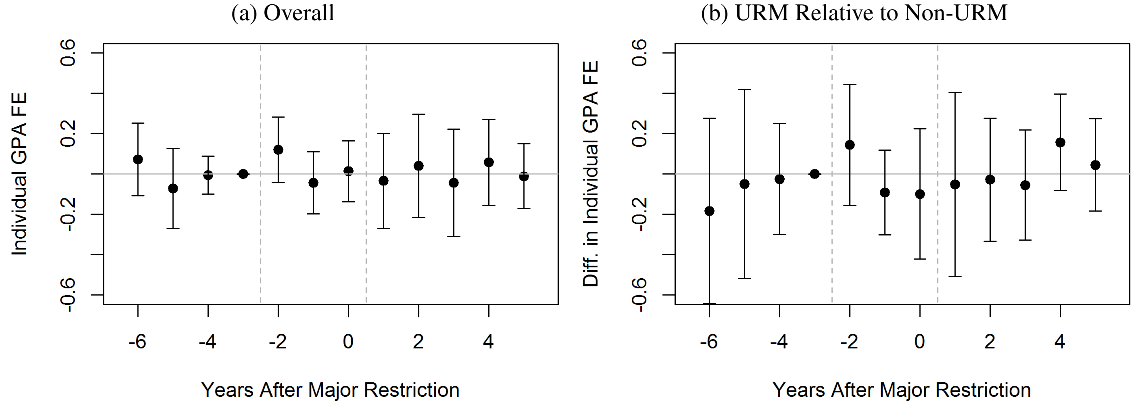
(a) Overall (b) Diff. By Ethnicity



Note: This figure shows that while the predictiveness of estimated major intentions ($\hat{M}_{im}$) deteriorates over time (as majors' introductory courses shift), there is no clear different pattern in that deterioration by ethnicity, suggesting that changes in $\hat{M}_{im}$'s predictiveness cannot explain high-$\hat{M}_{im}$ URM students' relative flow into lower-premium majors following restrictions' implementation. The R^2's from linear regressions of major attainment (M_{im}) on estimated major intentions ($\hat{M}_{im}$) estimated by ethnicity and number of years before or after the implementation of a major restriction policy on major m, and the difference between the non-URM and URM R^2's relative to the $t = -3$ baseline. Regressions are estimated over a stacked dataset of students i's major intentions in field m, as in Equation 7. Source: UC ClioMetric History Project Student Database.

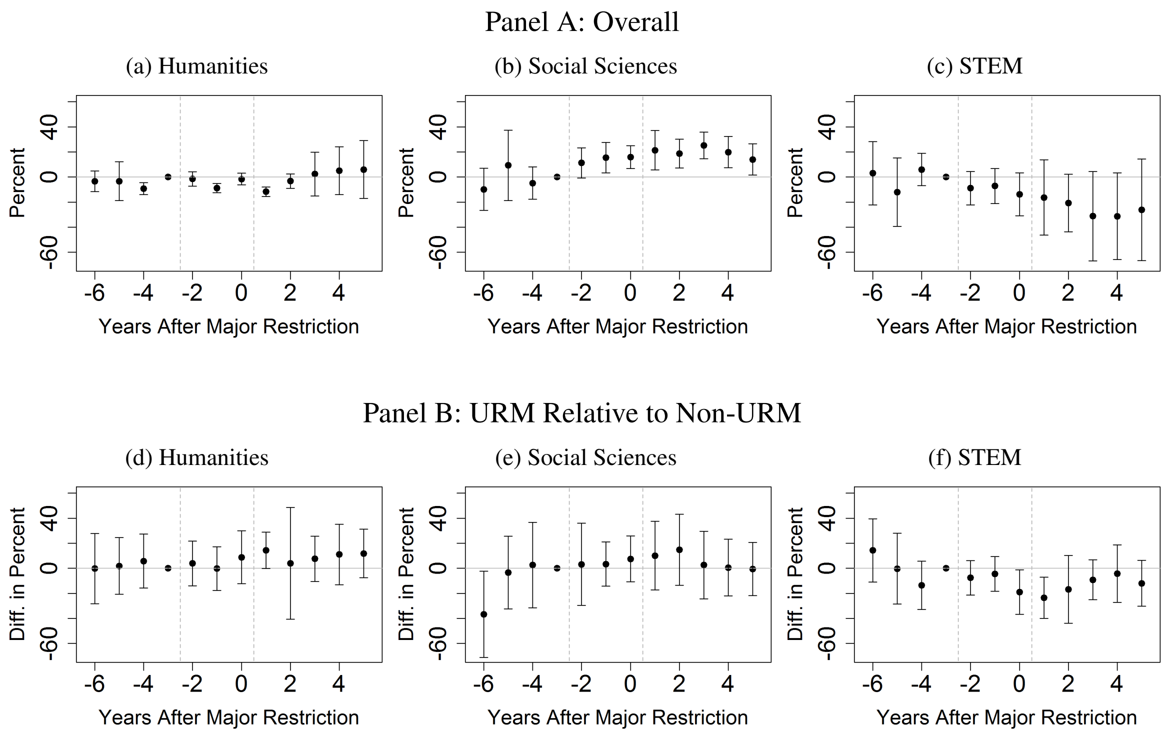
Figure A-22: Changes in Academic Composition of Students Who Intend Restricted Majors

(a) Overall (b) URM Relative to Non-URM



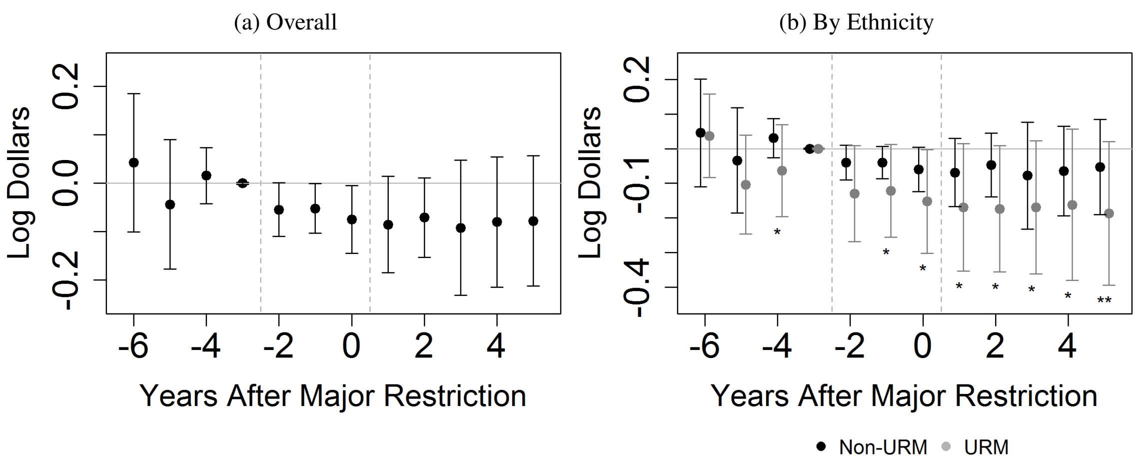
Note: This figure shows that the implementation of new major restrictions did not systematically alter the academic composition of students who took the major's common introductory courses. Difference-in-difference β_{it} estimates of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and their GPA fixed effect before and after the implementation of the restriction, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m. Panel (b) shows the differences between estimates changes for non-URM and URM students. Students' GPA fixed effect is their individual fixed effect from a two-way fixed effect model of GPA on student and course effects. β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

Figure A-23: Changes in Major Discipline for Students Who Intend Restricted Majors



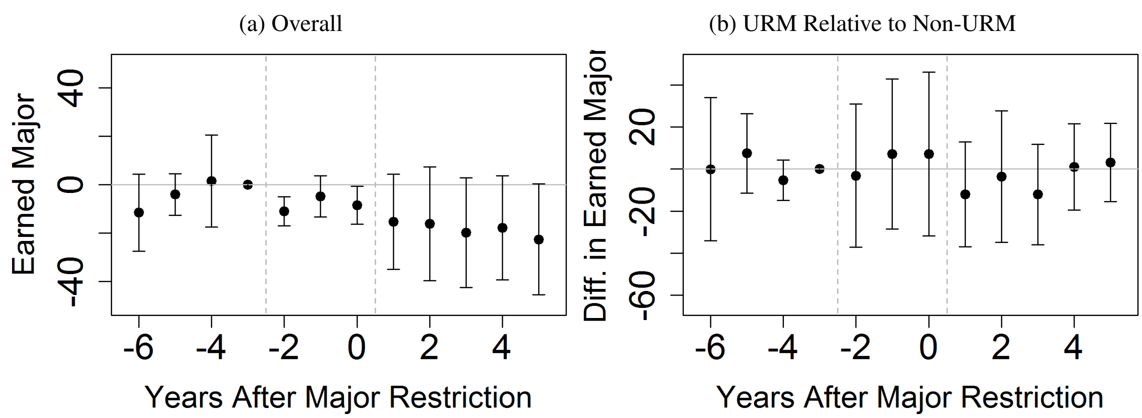
Note: This figure shows that the implementation of new major restrictions had the net overall effect of shifting students from STEM into the social sciences, while URM students differentially exited STEM fields and entered humanities fields instead. Difference-in-difference β_{it} estimates of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and whether they earned degrees in the humanities, social sciences, or STEM disciplines, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m. Panel B shows the differences between estimates changes for non-URM and URM students. Models control for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection (See Figure A-22). β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

Figure A-24: Changes in Majors' Educational Costs for Students Who Intend Restricted Majors



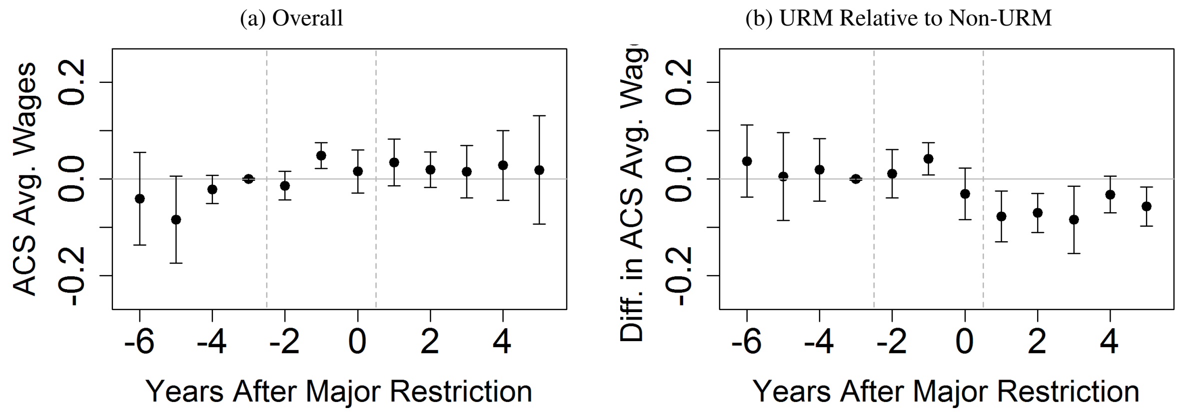
Note: This figure shows that the implementation of new major restrictions tended to lead students toward majors with relatively lower total *average* (not necessarily marginal) educational costs per graduate (driven by declines among URM students), a potential motivation for implementing the restrictions. Difference-in-difference β_{it} estimates of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and the average total educational log cost per graduate of their college major, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m. Average total educational costs per graduate – which include personnel costs for faculty, advising and administration along with financial aid, plant maintenance, library costs, and student services as measured in the Florida State University system and reported by (Altonji and Zimmerman, 2019), Table 5.3; costs for students with multiple majors are averaged across majors. A college major crosswalk available from the authors. Panel (b) shows separate (interacted) estimates for non-URM and URM students; asterisks show the statistical significance of the hypothesis of inequality between ethnicities at the ten (*) and five ($**$) percent level. Models control for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection (See Figure A-22). β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

Figure A-25: Changes in Major Choice of Students Who Intend Restricted Majors



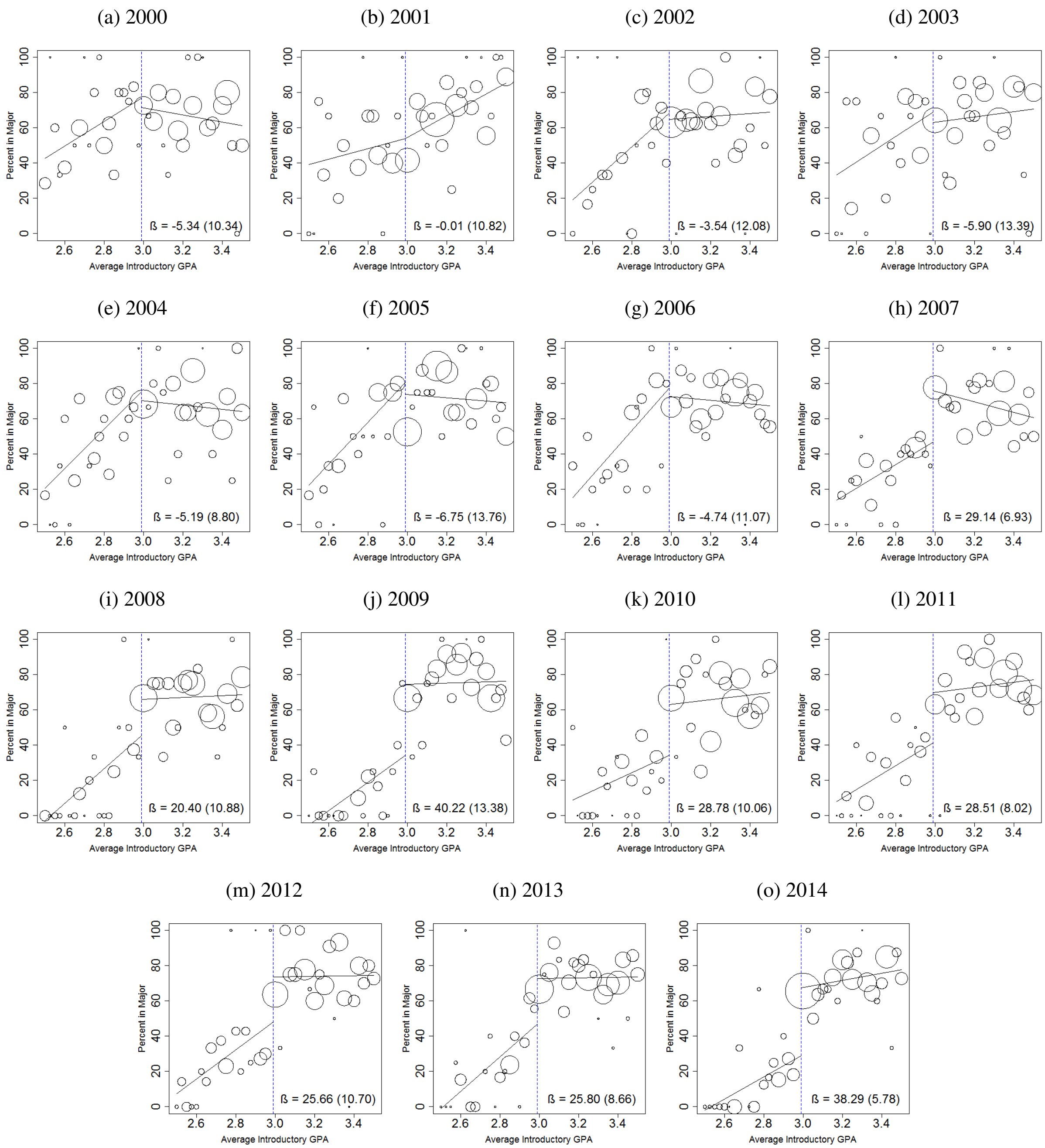
Note: This figure would appear to suggest that students who intended restricted majors became less likely to successfully declare the major after restrictions' implementation, with some evidence that the decline was driven by URM students, though the regression's unusual form (regressing M_{im} on $\hat{M}_{im}$) challenges straightforward interpretation (since the second difference effectively drops out of the regression, with control-group low-$\hat{M}_{im}$ students essentially never selecting major m). Difference-in-difference β_{it} estimates of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and their declaring major m before and after the implementation of the restriction, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m. Panel (b) shows the differences between estimates changes for non-URM and URM students. Models control for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection (See Figure A-22). Students who do not declare a major are omitted. β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

Figure A-26: The Major Premiums (ω_i) of **All** Students Who Intend a Major Before and After New Restrictions



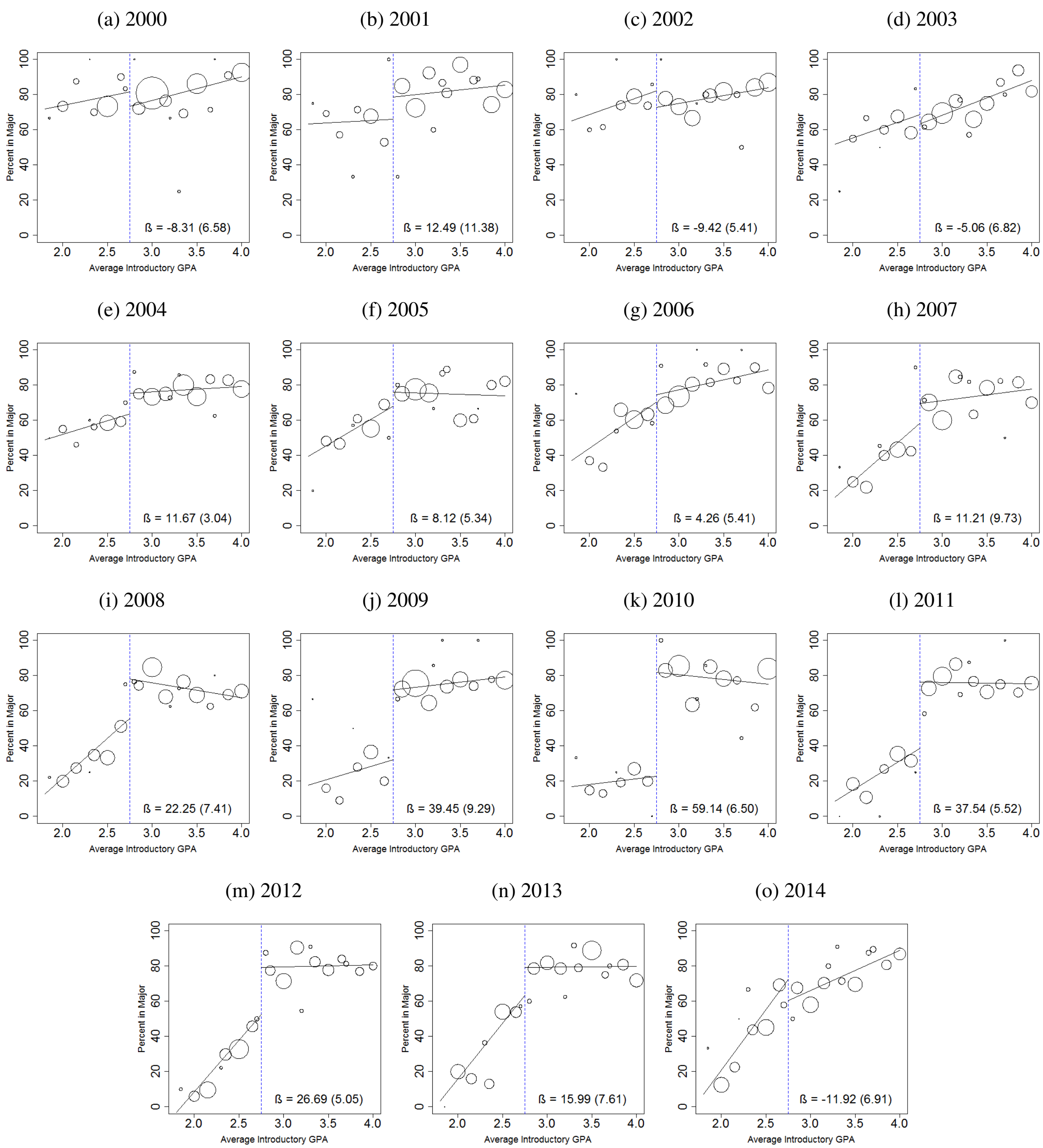
Note: This figure replicates Figure 9 among all UC students (both freshmen and transfers), showing that restrictions' implementation differentially led high-intention URM students toward lower-premium majors, though to a lesser degree than among only freshman students. Difference-in-difference β_{it} estimates following Equation 7 of the relationship between all freshman and transfer students' intending the restricted major ($\hat{M}_{im}$) and the premium of the student's major (as defined in Appendix B) before and after the implementation of the restriction, following Equation 7 and estimated over a stacked dataset of students i's major intentions in field m. Panel (a) shows overall β estimates, while Panel (b) shows the differences between estimates changes for non-URM and URM students, controlling for the interaction between students' GPA fixed effects and gender to absorb spurious variation generated by differential selection (See Figure A-22). A college major crosswalk available from the authors. β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

Figure A-27: Berkeley Economics Major Declaration at the Admission Threshold by Year



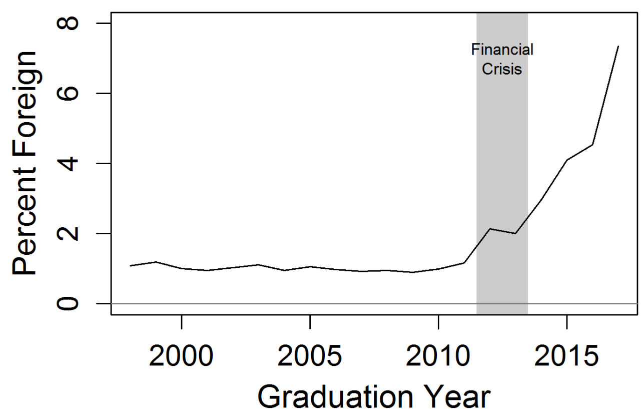
Note: This figure shows that UC Berkeley's economics major restriction policy was hardly binding until the 2007 cohort but was somewhat binding thereafter. Each circle represents the percent of economics majors (y axis) among each start cohort of UC Berkeley students who earned a given introductory economics GPA (x axis). The size of each circle corresponds to the proportion of students who earned that GPA. Cohort years are defined by year of entry. Majoring in economics indicates declaring (and never rescinding) the economics major. Fit lines and beta estimate (at the 3.0 GPA threshold) from linear regression discontinuity specification; standard error (clustered by GPA) in parentheses. The economics GPA is the mean of intro economics, two semesters of calculus, the first-taken of intermediate micro- or macroeconomics, and intro statistics; calculus could be omitted if "Advanced Placement" credit is observed. Source: UC-CHP Student Database.

Figure A-28: UCSC Economics Major Declaration at the Admission Threshold by Year



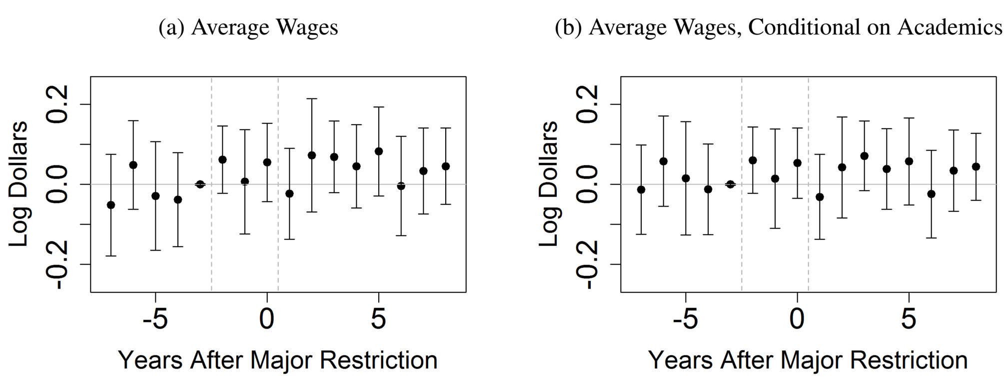
Note: This figure shows that UCSC's economics major restriction policy was hardly binding until the 2008 cohort, most binding in 2010, and became less binding after 2013 (in part because the introductory GPA rule changed). Each circle represents the percent of economics majors (y axis) among each cohort year of UCSC students who earned a given GPA in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that introductory GPA. Cohort years are defined by year of entry. Majoring in economics indicates declaring any of UCSC's three economics major tracks: economics, global economics, or business management economics. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification; standard error (clustered by GPA) in parentheses. This figure replicates Figure A-1 of (Bleemer and Mehta, 2022). Source: UC ClioMetric History Project Student Database.

Figure A-29: Percent of UC Students from Outside the United States



Note: This figure shows that the University of California saw a large increase in international student enrollment starting in 2012, which likely interacted with active major restriction policies to further decrease URM students' enrollment in high-premium majors as shown in Figure 10. The annual share of freshman fall students at UC Berkeley, UC Davis, UC Santa Barbara, and UC Santa Cruz who graduated from a high school outside the United States. "Graduation Year" is four years after students' initial enrollment. Source: UC ClioMetric History Project Student Database.

Figure A-30: Extension of Figure 12 to 6-8 Years Following Restrictions' Implementation

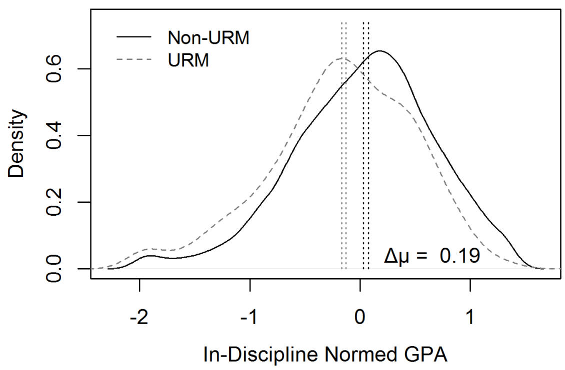


Note: This figure replicates Figure 12 but also shows estimates for $\beta_6 - \beta_8$, revealing no observable longer-run improvement in restricted majors' estimated wage value-added. Staggered difference-in-difference β estimates of department characteristics before and after the implementation of new major restriction policies, relative to other majors in that campus-year. The outcomes are value-added fixed effects from linear regressions of wages on major-year fixed effects (stacking students with multiple majors) with no controls or controlling for students' GPA fixed effect interacted with gender and their ethnicity, where year is freshman students' first year of enrollment and wages are measured 10 years later. β_{-3} is omitted; standard errors are clustered by campus-major and assume that value-added and GPA fixed effects are observed without error; and 95 percent confidence intervals are shown. GPA fixed effect is the student effect from a two-way fixed effect model of grades on students and course-terms. Wage records exclude non-California, federal, and self-employment. Estimates of Δ (Post-Pre) extending the methodology shown at the bottom of Table 5 to a comparison of the coefficients 6-8 years following restrictions to the pre-period – the difference between "6-8 Years After" and "Before" Major Restriction β coefficients (with standard error in parentheses) – are 0.026 (0.034) without correction for selection on observables and -0.009 (0.034) with this correction. This suggests that student observables fully absorb noisily-estimated observed wage gains in these cohorts. Source: UC ClioMetric History Project Student Database and the California Employment Development Department.

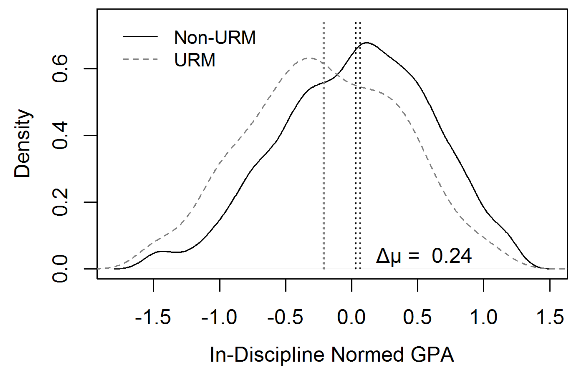
Figure A-31: Distribution of Restricted-Major Academic Performance by Ethnicity

Panel A: Grade Distribution in $t = -3$, Before Restrictions' Implementation

(a) Introductory GPA

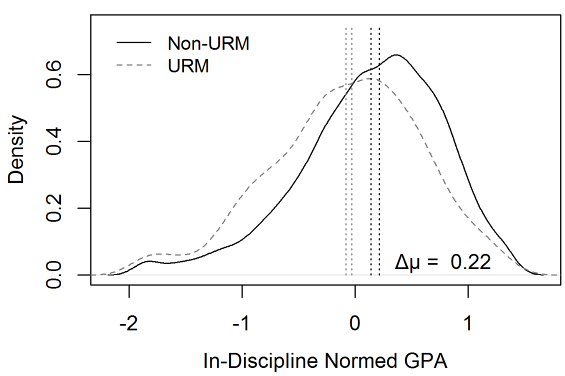


(b) Upper-Division GPA

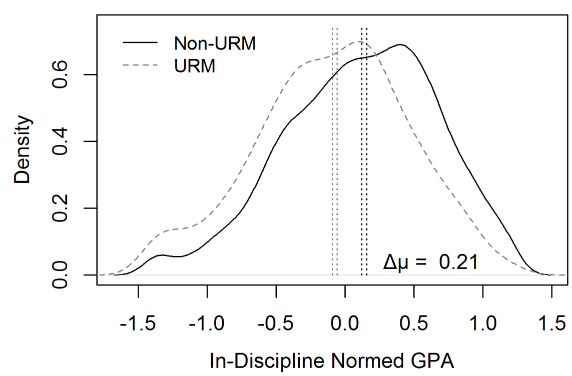


Panel B: Grade Distribution in $t = 3$, After Restrictions' Implementation

(c) Introductory GPA

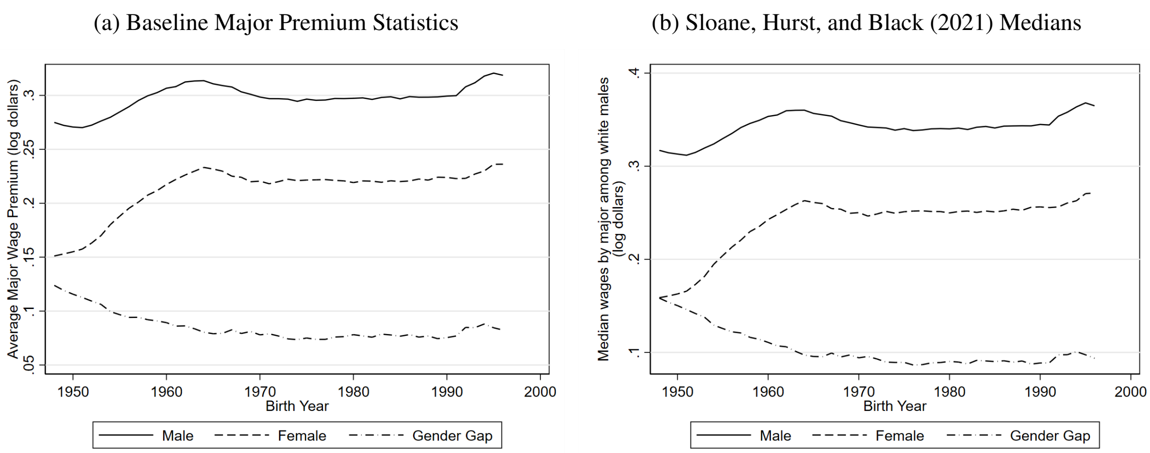


(d) Upper-Division GPA



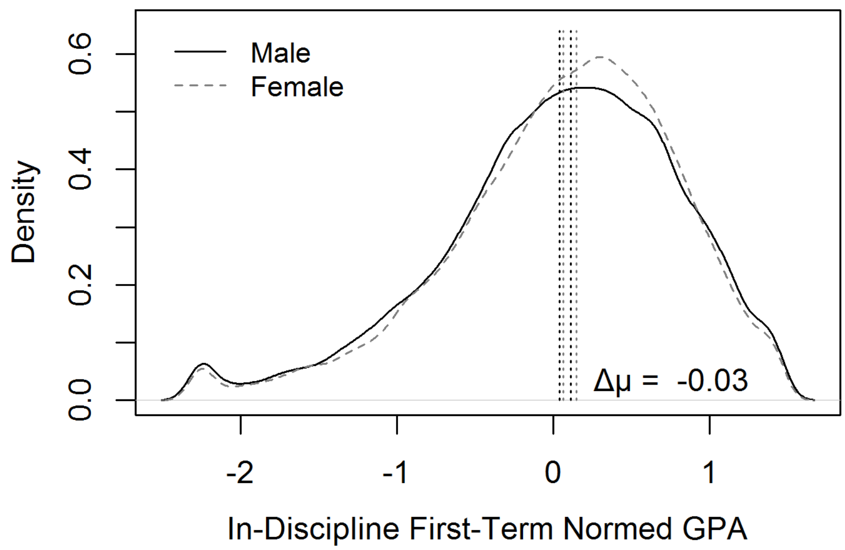
Note: This figure shows that URM students consistently earned lower grades in both introductory and upper-division courses in soon-to-be-restricted majors, and major restrictions did not lead to any measurable ethnic convergence in students' academic performance. Distribution of observed students' in-discipline normed GPA for courses taken in the first two academic years ("Introductory") and courses taken in subsequent years ("Upper-Division") by ethnicity, among students who earned either soon-to-be-restricted majors (three years before implementation) or recently-restricted majors (three years after implementation). See Section 3 for the definition of $nGPA$; in-discipline courses include those taken in the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, and Professional) as well as Mathematics and Statistics courses. Dotted lines show the median and mean by URM status, and the reported coefficient is the difference between the non-URM and URM means. Source: UC ClioMetric History Project Student Database.

Figure A-32: Average College Major Premium by Birth Cohort and Gender



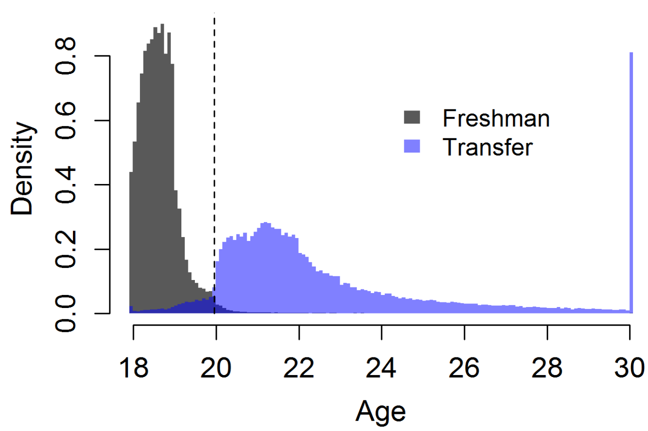
Note: This figure shows that the trends in average economic value of college majors earned by male and female college-graduate cohorts in the U.S. are highly similar when economic value is alternatively estimated using this study's baseline college major premium statistics or using the median wage statistics preferred by Sloane, Hurst, and Black (2021). College graduates' average major premium by birth cohort and gender among ACS respondents and the difference between those averages. The left panel presents estimating using the baseline major premium statistics estimated by OLS regression of log wages on major indicators and gender, ethnicity, age, year, and double-major covariates over wage employees aged 35-45 appearing in the 2009-2019 ACS (replicating Figure HH-1); see Appendix B for details. The right panel follows Sloane, Hurst, and Black (2021) by assigning each major to the median CPI-adjusted hourly wage earned by native white men with "strong labor market attachment" (that is, who worked at least 30 hours per week for at least 27 weeks in the prior year) between ages 43 and 57 appearing in the 2014-2017 ACS. The specification remains slightly different from that of Sloane, Hurst, and Black (2021): we do not drop ACS respondents with missing (imputed) responses. Source: The 2009-2019 American Community Survey (Ruggles et al., 2020).

Figure A-33: Distribution of Introductory Course $nGPA$ by Ethnicity



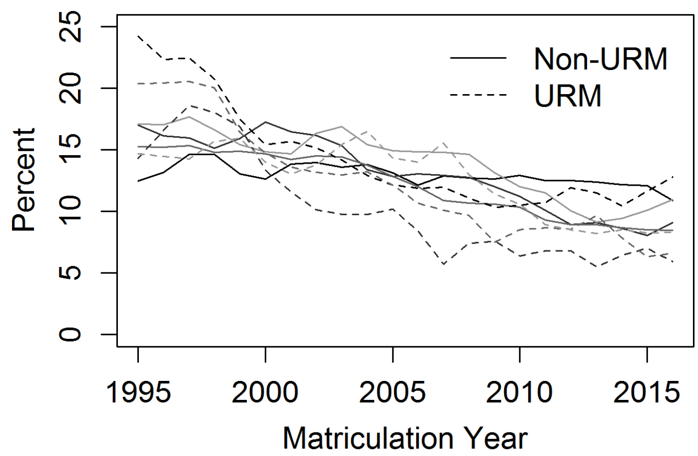
Note: This figure shows that women are less likely to earn low grades in restricted majors' introductory courses than men, explaining their over-performance (in terms of the major restriction) conditional on enrolling in the major's introductory courses. Kernel density plots of winsorized normed first-term in-discipline grades (in standard deviations) among freshman students who declared restricted majors three cohorts before that major was restricted, by gender. Dotted lines show the median (right) and mean (left) values by gender. See the definition of $nGPA$ in Section 3; in-discipline courses include those in the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, or Professional) along with all math and statistics courses. Source: UC ClioMetric History Project Student Database.

Figure A-34: The Quality of an Age Proxy for Identifying Freshman (Non-Transfer) Students



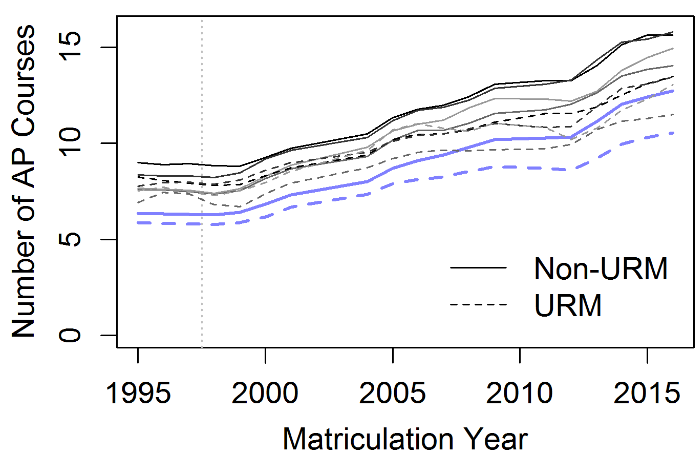
Note: This figure shows that identifying freshman students by whether they turn 20 before October of their first enrollment year effectively minimizes misclassification error. The age distribution (by month) of 1993-2018 students as of September of their first enrollment year in the UC data, by whether their undergraduate application indicated that they are applying as freshman or transfer students. The dotted line indicates the break that minimizes misclassification error, with students who turn 20 before October of their first enrollment year being classified as freshmen. 1.9 percent of freshman students are above the threshold; 5.6 percent of transfers are below the threshold. Source: UC ClioMetric History Project Student Database.

Figure A-35: Percent of UC Students from Private High Schools by Ethnicity



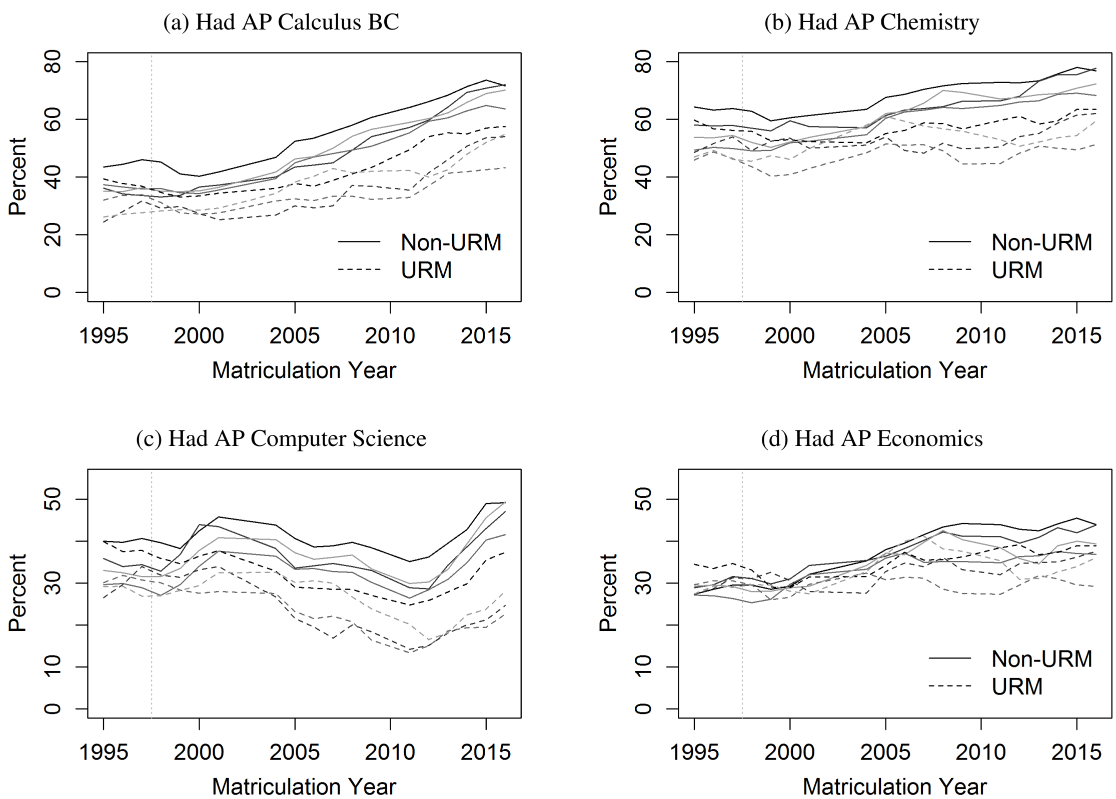
Note: This figure shows that URM UC students were somewhat *more* likely than non-URM students to have enrolled at private high schools prior to Proposition 209 in 1998, but have generally been as or less likely since. The percent of freshman California-resident students at the University campuses at Berkeley, Santa Barbara, Davis, and Santa Cruz (black to lighter gray, respectively) who graduated from private high schools by graduation year and ethnicity. Statistics are two-year moving averages. Freshman UC students are identified by age at matriculation (see Appendix E); nine percent fail to match to observed high schools and are omitted, mostly because they actually transferred from community college. Data for students who graduated in 2002, 2003, and 2010 are unavailable. Source: UC ClioMetric History Project Student Database and the California Department of Education.

Figure A-36: **Private-Adjusted** Number of AP Courses Available to UC Students by Ethnicity



Note: This figure shows that URM students at all four UC campuses have persistently had less access to advanced high school courses prior to UC enrollment, with a growing gap over time mirroring a statewide shift in high school resources by student ethnicity, even when students from private high schools are assumed to have high AP course opportunity. The average number of unique Advanced Placement classes at the high schools from which all Californians (blue) and freshman students at the University campuses at Berkeley, Santa Barbara, Davis, and Santa Cruz (black to lighter gray, respectively) graduated by graduation year and ethnicity, assuming that all private high school graduates came from high schools with the 90th student-weighted percentile number of AP courses measured from public high schools. Classes are measured in students' final year of high school. Statistics are two-year moving averages. Freshman UC students are identified by age at matriculation (see Appendix E); nine percent fail to match to observed high schools and are omitted, mostly because they actually transferred from community college. Data for students who graduated in 2002, 2003, and 2010 are unavailable, as are state-wide degree attainment data by ethnicity prior to 1998. Source: UC ClioMetric History Project Student Database and the California Department of Education.

Figure A-37: **Private-Adjusted** UC Students' High School Course Availability by Ethnicity



Note: This figure shows that URM UC students have long had particularly poor access to technical AP courses like BC (integral) calculus, chemistry, and computer science (but not economics until recently) at their high schools, even when all private high school students are assumed to have had access to these courses. The percent of freshman students at the University campuses at Berkeley, Santa Barbara, Davis, and Santa Cruz (black to lighter gray, respectively) who graduated from high schools where each respective Advanced Placement course was available by graduation year and ethnicity, assuming that each course was offered at every private California high school in every year. Classes are measured in students' final year of high school. Statistics are two-year moving averages. Freshman UC students are identified by age at matriculation (see Appendix E); nine percent fail to match to observed high schools and are omitted, mostly because they actually transferred from community college. Data for students who graduated in 2002, 2003, and 2010 are unavailable, as are state-wide degree attainment data by ethnicity prior to 1998. AP computer science and economics are defined as the union of all respective AP courses (e.g. either micro- or macroeconomics). Source: UC ClioMetric History Project Student Database and the California Department of Education.

Table A-1: Major Restrictions at the Top 25 US&WR Ranked **Private** Universities, Spring 2022

Univ.	Undergrad. Students	Computer Science	Economics	Finance	Mechanical Engineering	Nursing
Princeton	4,773	-	-	-	-	*
Columbia	6,170	-	-	-	A	*
Harvard	5,222	-	-	*	-	*
MIT	4,361	-	-	-	-	*
Yale	4,703	-	-	*	-	*
Stanford	6,366	-	-	-	-	*
Chicago	6,989	-	-	-	*	*
UPenn	9,872	A	-	A	A	A
CalTech	901	-	-	-	-	*
Duke	6,717	-	-	-	-	*
Johns Hopkins	6,331	-	-	*	-	*
Northwestern	8,194	-	-	*	-	*
Dartmouth	4,170	-	-	*	-	*
Brown	6,792	-	-	-	-	*
Vanderbilt	7,057	-	-	*	A	*
Washington in St. Louis	7,653	-	-	-	-	*
Rice	4,076	-	-	-	-	*
Notre Dame	8,874	-	-	A	-	*
Emory	7,010	-	-	A	3.3	-
Georgetown	7,357	-	-	*	*	*
Carnegie Mellon	7,073	3.6; A	-	3.0; A	-	*
USC	19,606	3.0; A	-	A	3.0; A	*
NYU	27,444	-	-	A	A	A
Tufts	6,114	-	-	*	-	*
Wake Forest	5,441	-	-	A	-	*

Note: This table shows that major restrictions (and mechanical GPA restrictions in particular) are much less popular at top private universities than they are at top public universities (Table 1). The Spring 2022 minimum major admissions requirements for enrolled students at the top 25 private universities as ranked by US News and World Report in 2022. A number indicates the minimum GPA required in department-specified courses for current students to declare the major, omitting restrictions of C+ or lower. Chosen majors are the top-earning majors reported in Altonji, Blom, and Meghir (2012) averaged between male and female students, Table 3, omitting Electrical Engineering due to its similarity with Computer Science. Finance includes Business Administration, Business Economics, and Economics and Accounting majors when otherwise unavailable.

HS: Students must be directly admitted from high school to the major (with elevated admissions standards). **A**: Students must submit a successful internal application after initial enrollment in order to earn the major. ***:** Major is unavailable.

Source: University and department websites and US News & World Report, March 2022.

Table A-2: Estimated College Major Premiums (ω_m)

Major Code and Name		β	s.e.	Major Code and Name		β	s.e.
6202	Actuarial Science	0.763	0.049	3202	Pre-Law and Legal Studies	0.230	0.028
2419	Petroleum Engineering	0.756	0.082	6100	General Medical and Health Services	0.226	0.028
6106	Health and Medical Preparatory Programs	0.733	0.038	2503	Industrial Production Technologies	0.225	0.029
2404	Biomedical Engineering	0.724	0.039	2602	Common Foreign Language Studies	0.225	0.024
3611	Neuroscience	0.708	0.049	6402	History	0.221	0.022
4006	Cognitive Science and Biopsychology	0.650	0.051	5401	Public Administration	0.213	0.031
6108	Pharmacy, Pharm Sciences, and Admin.	0.633	0.024	5004	Geology and Earth Science	0.213	0.026
2405	Chemical Engineering	0.623	0.023	6103	Health and Medical Administrative Services	0.203	0.025
3603	Molecular Biology	0.620	0.030	2107	Computer Networking and Telecommunications	0.203	0.030
3601	Biochemical Sciences	0.610	0.025	6006	Art History and Criticism	0.202	0.029
2407	Computer Engineering	0.607	0.022	2106	Computer Information Management and Security	0.202	0.028
2418	Nuclear Engineering	0.606	0.056	2500	Engineering Technologies	0.198	0.035
4005	Mathematics and Computer Science	0.593	0.061	6104	Medical Assisting Services	0.198	0.031
5008	Materials Science	0.574	0.038	1902	Journalism	0.197	0.023
2408	Electrical Engineering	0.565	0.021	5006	Oceanography	0.196	0.043
5501	Economics	0.548	0.022	5299	Miscellaneous Psychology	0.194	0.036
2415	Metallurgical Engineering	0.545	0.048	1901	Communications	0.192	0.021
3607	Pharmacology	0.542	0.063	6110	Community and Public Health	0.184	0.031
2401	Aerospace Engineering	0.533	0.027	1103	Animal Sciences	0.184	0.027
2414	Mechanical Engineering	0.531	0.021	2101	Computer Programming	0.179	0.038
5402	Public Policy	0.524	0.048	5206	Social Psychology	0.178	0.059
3701	Applied Mathematics	0.523	0.035	3201	Court Reporting	0.170	0.073
2412	Industrial and Manufacturing Engineering	0.511	0.024	2303	School Student Counseling	0.161	0.038
3605	Genetics	0.508	0.043	5200	Psychology	0.156	0.021
6207	Finance	0.500	0.021	3301	English Language and Literature	0.156	0.021
2416	Mining and Mineral Engineering	0.499	0.065	1301	Environmental Science	0.155	0.024
2102	Computer Science	0.492	0.021	4002	Nutrition Sciences	0.151	0.032
3600	Biology	0.489	0.021	5000	Physical Sciences	0.142	0.052
5003	Chemistry	0.480	0.022	4801	Philosophy and Religious Studies	0.139	0.024
5505	International Relations	0.470	0.027	5507	Sociology	0.139	0.022
6205	Business Economics	0.468	0.031	5502	Anthropology and Archeology	0.135	0.025
5007	Physics	0.460	0.023	5301	Criminal Justice and Fire Protection	0.135	0.021
3702	Statistics	0.456	0.034	5503	Criminology	0.133	0.029
3606	Microbiology	0.455	0.028	4007	Interdisciplinary Social Sciences	0.115	0.030
2417	Naval Architecture and Marine Engineering	0.454	0.051	1101	Agriculture Production and Management	0.111	0.028
2410	Environmental Engineering	0.452	0.033	2601	Linguistics and Comparative Language and Lit.	0.110	0.031
6212	Management Information Systems and Statistics	0.444	0.023	5201	Educational Psychology	0.109	0.041
2501	Engineering and Industrial Management	0.435	0.041	3604	Ecology	0.095	0.030
2403	Architectural Engineering	0.431	0.044	5504	Geography	0.094	0.025
2409	Engineering Mechanics, Physics, and Science	0.429	0.042	2310	Special Needs Education	0.089	0.024
2406	Civil Engineering	0.428	0.022	5202	Clinical Psychology	0.087	0.057
2105	Information Sciences	0.426	0.025	2308	Science Teacher Education	0.085	0.027
5506	Political Science and Government	0.426	0.021	1903	Mass Media	0.082	0.024
2400	General Engineering	0.419	0.022	3401	Liberal Arts	0.081	0.022
2413	Materials Engineering and Materials Science	0.418	0.034	4000	Interdisciplinary & Multidisciplinary Studies	0.081	0.029
3608	Physiology	0.404	0.030	2399	Miscellaneous Education	0.076	0.025
2499	Miscellaneous Engineering	0.403	0.030	2305	Mathematics Teacher Education	0.071	0.027
5599	Miscellaneous Social Sciences	0.403	0.048	4101	Physical Fitness, Parks, Recreation, and Leisure	0.069	0.022
3609	Zoology	0.398	0.030	1303	Natural Resources Management	0.067	0.026
5001	Astronomy and Astrophysics	0.395	0.062	6004	Commercial Art and Graphic Design	0.063	0.023
5801	Precision Production	0.393	0.158	2603	Other Foreign Languages	0.063	0.035
3700	Mathematics	0.379	0.022	3402	Humanities	0.060	0.033
6107	Nursing	0.375	0.021	3302	Composition and Speech	0.059	0.030
6204	Operations, Logistics and E-Commerce	0.367	0.027	1302	Forestry	0.052	0.031
6201	Accounting	0.367	0.021	2001	Communication Technologies	0.052	0.031
6210	International Business	0.362	0.027	5500	General Social Sciences	0.051	0.029
2402	Biological Engineering	0.343	0.038	2313	Language and Drama Education	0.050	0.024
5005	Geosciences	0.339	0.046	2300	General Education	0.040	0.021
3801	Military Technologies	0.335	0.087	1106	Soil science	0.038	0.055
5901	Transportation Sciences and Technologies	0.333	0.025	6211	Hospitality Management	0.035	0.025
5601	Construction Services	0.332	0.026	6199	Miscellaneous Health Medical Professions	0.034	0.030
2100	Computer and Information Systems-General	0.325	0.022	1105	Plant Science and Agronomy	0.025	0.030
1104	Food Science	0.309	0.040	6005	Film, Video and Photographic Arts	0.025	0.028
2502	Electrical Engineering Technology	0.297	0.027	2311	Social Science or History Teacher Education	0.024	0.025
6200	General Business	0.290	0.021	2309	Secondary Teacher Education	0.020	0.023
2301	Educational Administration and Supervision	0.286	0.030	2306	Physical and Health Education Teaching	0.009	0.023
5098	Multidisciplinary or general science	0.284	0.023	5404	Social Work	0.004	0.022
6206	Marketing	0.284	0.021	1100	General Agriculture	0.000	0.000
4001	Intercultural and International Studies	0.278	0.030	5203	Counseling Psychology	-0.003	0.034
6105	Medical Technologies Technicians	0.277	0.025	2304	Elementary Education	-0.006	0.021
5205	Industrial and Organizational Psychology	0.273	0.047	2312	Teacher Education: Multiple Levels	-0.012	0.026
3699	Miscellaneous Biology	0.273	0.031	2901	Family and Consumer Sciences	-0.018	0.024
5102	Nuclear and Industrial Radiology Technologies	0.261	0.050	5701	Electrical and Mechanic Repairs and Technologies	-0.022	0.046
6299	Miscellaneous Business	0.257	0.028	3501	Library Science	-0.022	0.050
1102	Agricultural Economics	0.252	0.037	1199	Miscellaneous Agriculture	-0.027	0.075
6109	Treatment Therapy Professions	0.252	0.023	3602	Botany	-0.029	0.048
1501	Area, Ethnic, and Civilization Studies	0.252	0.026	2314	Art and Music Education	-0.041	0.024
1904	Advertising and Public Relations	0.250	0.025	6000	Fine Arts	-0.050	0.023
2599	Miscellaneous Engineering Technologies	0.249	0.027	5403	Human Services and Community Organization	-0.059	0.027
6203	Business Management and Administration	0.246	0.021	6001	Drama and Theater Arts	-0.069	0.026
6209	Human Resources and Personnel Management	0.245	0.024	6002	Music	-0.074	0.024
1401	Architecture	0.243	0.023	6003	Visual and Performing Arts	-0.085	0.035
5002	Atmospheric Sciences and Meteorology	0.241	0.038	2307	Early Childhood Education	-0.102	0.025
2411	Geological and Geophysical Engineering	0.240	0.142	6099	Miscellaneous Fine Arts	-0.142	0.085
6403	United States History	0.236	0.054	6007	Studio Arts	-0.143	0.030
6102	Communication Disorders Sciences and Services	0.235	0.024	2201	Cosmetology Services and Culinary Arts	-0.150	0.038
2504	Mechanical Engineering Related Technologies	0.232	0.036	4901	Theology and Religious Vocations	-0.269	0.024

Note: This table presents the ω_m statistics used in the study to index college majors' economic value. Estimates from an OLS regression of annual log income on major indicators across all employed college-educated respondents to the 2009-2019 ACS between ages 35 and 45, conditioning on an indicator for earning more than one college major and the interactions between gender, ethnicity (six categories), age, and survey year. Individuals with at least two majors are randomly assigned to one of their reported majors. Standard errors are robust.

Source: The 2009-2019 American Community Survey (Ruggles et al., 2020)

Table A-3: Annual Within-Institution Stratification by Sector

Year	Top 26 Publics	Other Public R1	Public R2	All Other Publics	Non-Profit Schools	For-Profit Schools	All Institutions
1995	2.1	0.9	1.2	1.2	1.0	0.2	1.2
2019	4.7	2.7	3.1	2.2	1.7	1.1	2.3

Note: This table shows that within-institution stratification has long been particularly high at selective public universities, but has become moreso in the past 25 years. The URM-weighted average of within-institution stratification $\left(S_t(i) = \sum_m \omega_m \Delta_R[P_t(m|i,R)]\right)$, measured in log dollars, overall and by university sector. Years indicate college graduation cohort years. The higher education sectors partition four-year U.S. institutions; R1 and R2 research universities follow the Carnegie Classification.

Source: 2009-2019 American Community Survey (Ruggles et al., 2020) and IPEDS.

Table A-4: Compositional Effects of a Strict Major Restriction at UCSC Economics

	All Econ. Majors	Below-GPA Stud. All	Below-GPA Stud. Majors	Above-GPA Majors Only
Female (%)	40.9	44.6	44.1	40.3
URM (%)	18.8	27.7	23.1	17.9
Avg. Zip AGI ($)	104,334	93,747	100,453	105,126
Log Avg. Zip AGI (log $)	11.45	11.33	11.39	11.46
California Resident (%)	97.2	97.4	96.5	97.3
International (%)	1.1	0.5	1.0	1.1
Observations	1,691	1,213	286	1,405

Note: This table shows that the UCSC economics majors with below-threshold GPAs (and who were thus admitted by exception) are lower-income and more likely to be URM than the other majors, implying that stricter enforcement of the restriction would amplify the restriction's stratification effects. Average descriptive characteristics of 2008-2012 freshman-entry economics majors at the University of California, Santa Cruz overall (column 1) and by whether the students earned grades in their introductory economics courses that placed them below (3) or above (4) the major's 2.8 GPA restriction policy, along with the characteristics of *all* students who completed those introductory courses but earned below-2.8 GPAs (2). Average local household income (AGI) is measured as the CPI-adjusted mean adjusted gross income of tax-filing households in the student's Zip code in their first year of enrollment; see Appendix E.

Source: UC ClioMetric History Project Student Database and IRS SOI.

Table A-5: Observational Relationship between Major Restrictions and URM Stratification at Different GPA Levels

	URM Share in Major	
Mechanical Restriction		
GPA 2.01–2.49	-0.9	-1.0
	(1.5)	(1.5)
GPA 2.50–2.99	-1.4	-1.4
	(0.6)	(0.6)
GPA $\geq$3.00	-2.3	-2.5
	(0.8)	(0.8)
Discretionary Restriction		
On-Campus Application	-0.8	-0.7
	(0.4)	(0.4)
Direct-from-HS Application	-3.3	-3.5
	(2.3)	(2.3)
Overall-GPA Mechanical Restriction		
GPA 2.01–2.49		2.4
		(1.5)
GPA 2.50–2.99		-0.1
		(0.4)
GPA $\geq$3.00		-1.0
		(0.5)
$\bar{Y}$	21.1	
Observations	4,399	

Note: Estimates from an OLS linear regression of a major's 2021 URM (Black, Hispanic, or Native American) graduate share on whether the major is restricted. The sample includes the 4,399 majors with at least 35 2021 graduates at the 106 R1 public universities in the United States. Mechanical restrictions limit access to students with below-threshold grades in specified introductory courses; discretionary restrictions limit access to students based on detailed applications, generally including both measured academic preparation along with essays and other materials. Overall-GPA mechanical restrictions limit access to students with below-threshold average grades in all of their courses at the university (defined to exclude majors with specific mechanical restrictions). See Appendix A for details on restrictions. Restrictions at GPA 2.0 or lower are common and omitted. Each model includes institution and four-digit CIP code fixed effects. Standard errors two-way clustered by institution and four-digit CIP in parentheses.

Source: IPEDS and university websites.

Table A-6: Summary of New Major Restrictions' Impact on Department Composition, **Aligning Samples Across Outcomes**

	Log Num. of Students	SAT Score	GPA FE	Percent URM	Fam. Inc. (Log $)	Percent Female	First Term $nGPA^1$ In Disc.	First Term $nGPA^1$ Out of Disc.	Average Wage[2] No Cov.	Average Wage[2] GPA Cov.
4-7 Years Before Restriction	-0.08 (0.07)	5.7 (14.3)	-0.03 (0.02)	0.55 (1.15)	0.006 (0.012)	1.12 (1.53)	-0.02 (0.03)	-0.00 (0.03)	-0.01 (0.04)	0.01 (0.04)
Transition Years	-0.07 (0.06)	27.4 (13.0)	0.07 (0.02)	-0.72 (1.18)	0.018 (0.013)	2.24 (1.45)	0.07 (0.02)	0.07 (0.03)	0.04 (0.04)	0.04 (0.04)
1-5 Years After Restriction	-0.19 (0.06)	45.4 (15.8)	0.12 (0.03)	-2.55 (1.12)	0.041 (0.015)	1.55 (1.69)	0.13 (0.03)	0.13 (0.03)	0.04 (0.04)	0.03 (0.04)
Fixed Effects	X	X	X	X	X	X	X	X	X	X
Observations	4,722	3,614	4,722	4,722	4,722	4,722	4,722	4,722	3,595	3,595
$\bar{Y}$	4.2	1821		19.9	11.6	54.1	0.1	0.0		
Δ (Post-Pre)[3]	-0.10 (0.08)	39.6 (12.7)	0.15 (0.03)	-3.09 (0.85)	0.035 (0.017)	0.43 (1.38)	0.15 (0.03)	0.14 (0.03)	0.058 (0.038)	0.014 (0.037)
M.C. p-value[4]	[0.243]	[0.001]	[0.000]	[0.022]	[0.038]	[0.766]	[0.000]	[0.000]	[0.203]	[0.752]
Joint p-value of $\beta_{-4} - \beta_{-7} = 0$[5]	0.42	0.42	0.23	0.80	0.31	0.54	0.69	0.59	0.68	0.80

Note: Staggered difference-in-difference β estimates following Equation 6 of the measured characteristics of freshman students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Standard errors clustered by campus-major in parentheses. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. "Before" indicates 4-7 years before initial restriction implementation; "Transition" includes the year of implementation and two years earlier; and "After" includes 1-5 years following implementation. β_{-3} is omitted. Students can be included in more than one major's average if they have declared multiple majors. GPA fixed effect is the student effect from a two-way fixed effect model of grades on students and course-terms. Family income is measured by the CPI-adjusted mean adjusted gross income of tax-filing households in the student's Zip code in their first year of enrollment; see Appendix E. [1]See definition of first-term $nGPA$ in Section 3; in-discipline courses include those taken in the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, and Professional) plus Mathematics and Statistics courses, while out-of-discipline courses include all remaining courses. [2]Value-added fixed effects from linear regressions of wages on major-year fixed effects (stacking students with multiple majors) with either no covariates (left) or controlling for students' GPA fixed effect interacted with gender and their ethnicity (right), where year is freshman students' first year of enrollment and wages are measured 10 years later. [3]The difference between "After" and "Before" Major Restriction β coefficients, with standard error in parentheses. [4]An exact two-sided p-value on Δ (Post-Pre) from 1,000 Monte Carlo draws of placebo major restrictions, to account for mechanical correlations as students move between departments in general equilibrium. [5]A chi-squared test of $\beta_{-7} = \beta_{-6} = \beta_{-5} = \beta_{-4} = 0$ in the fully-disaggregated Equation 6 (as shown in, e.g., Figure 6), to test for parallel pre-trends.

Source: UC ClioMetric History Project Student Database, the UC Corporate Student System, and the California Employment Development Department.