Berkeley Center for Studies in Higher Education

Research & Occasional Paper Series: CSHE.14.2021

COLLEGE MAJOR RESTRICTIONS AND STUDENT STRATIFICATION

December 2021*

Zachary Bleemer and Aashish Mehta
Harvard University
University of California Santa Barbara

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 enforce GPA restriction policies that prohibit students with poor introductory grades from declaring popular majors. We investigate these GPA restrictions by constructing a novel 50-year dataset covering four public research universities' student transcripts and employing a dynamic difference-in-difference design around the implementation of 29 restrictions. Restricted majors' average URM enrollment share falls by 20 percent, which matches observational patterns and can be explained by URM students' poorer average pre-college academic preparation. Using first-term course enrollments to identify students who intend to earn restricted majors, we find that major restrictions disproportionately lead URM students from their intended major toward less-lucrative fields, driving within-institution ethnic stratification and likely exacerbating labor market disparities.

Keywords: Educational Equity, Higher Education Policy, Underrepresented Minorities, College Majors, Student Stratification

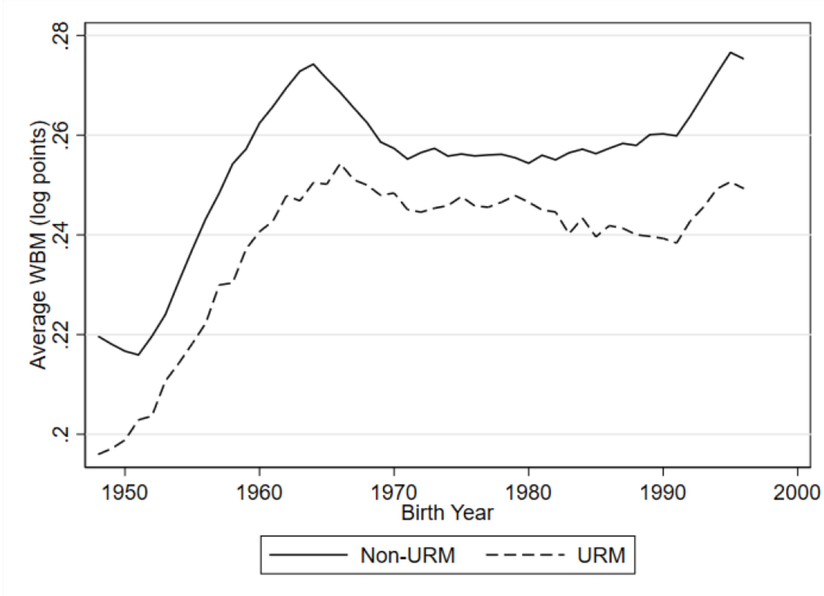
U.S. college graduates from underrepresented minority (URM) groups have persistently earned about 25 percent lower mid-career wages than similarly-educated non-URM workers. This ethnicity wage gap results from both labor market frictions – like hiring discrimination and less- extensive job networks – and differences in accumulated human capital driven by differential access to high-quality schools, within-school academic programs, and colleges.[1] Because Black- white wage convergence at the top of the wage distribution has occurred largely within educational group (Bayer and Charles, 2018), there is growing interest in ethnic stratification across higher- and lower-return college degrees (e.g. Bleemer, 2022). This study characterizes and decomposes long-run trends in an important dimension of collegiate human capital – college major attainment – and then carefully examines the role of an understudied class of university policies that appear to explain important dynamics in URM and non-URM graduates' degree attainment in lucrative fields of study.

Average wages vary widely by college major (Altonji, Arcidiacono, and Maurel, 2016), with some majors offering a relative wage premium that exceeds the average return to a college degree (Card, 1999). We construct an index of each major's economic value by estimating the mid-career wage value-added of each major within gender, ethnicity, age, and cohort bins among American Community Survey respondents, producing major-specific wage premium statistics that match widespread priors about lucrative majors and can be precisely validated in a quasi-experimental setting.[2] We begin by documenting the average wage premium of

---

* Thanks to Kelly Bedard, Dan Black, Chris Campos, David Card, Raj Chetty, David Deming, Claudia Goldin, Peter Kuhn, Todd Messer, Martha Olney, Jesse Rothstein, Cathy Weinberger, and Matt Wiswall; to seminar participants at Harvard University, UC Berkeley, and UC Santa Barbara; and to participants at several conferences for helpful comments. Thanks as well to San Singh, Alia Roca-Lezra, and Dan Ma for excellent research assistance. Bleemer was employed by the University of California in a research capacity throughout the period in which this study was conducted and acknowledges financial support from the National Academy of Education/Spencer Dissertation Fellowship and Opportunity Insights. Our work was supported by the UC Humanities Research Institute, and we are grateful to Chris Newfield and Heather Steffen for facilitating it. The conclusions of this research do not necessarily reflect the opinion or official position of the University of California or the State of California. Any errors that remainare our own. Bleemer: Department of Economics, Harvard University; zbleemer@g.harvard.edu. Mehta: Department of Global Studies, UC Santa Barbara; mehta@global.ucsb.edu.

Figure 1: Average College Major Premium 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 premia 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 A for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2018).

the majors earned by URM and non-URM college graduates since the 1950s. Figure 1 reveals that, while the gap between the average premiums associated with majors earned by URM and non-URM students had nearly disappeared by the late 1970s birth cohorts, it has since been steadily growing: in recent years, URM graduates have earned majors with about 3 percent lower average wages than those 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 policy and anti-prejudicial momentum toward closing ethnicity wage gaps in the United States (Lang and Lehmann, 2012).[3] We investigate the sources of this growing stratification by constructing a dataset covering annual degree attainment and average major premia by ethnicity at every U.S. college and university, permitting an observational decomposition of ethnic stratification into within- and between-institution components. While over a third of the rise in ethnic stratification across college majors can be explained by rising URM enrollment at universities where most students earn relatively lower-premium majors – which tend to be less-selective institutions – two-thirds of the rise can be explained by within-institutions dynamics over time, driven in particular by a sharp rise in ethnic stratification at public research universities. While public research universities enroll 33 percent of four-year 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 would particularly explain increasingly inequitable allocations of college majors at public research universities. While college major attainment is frequently studied as a choice that manifests student preferences (e.g. Wiswall and Zafar, 2018), a distinctive feature of public research universities in the U.S. is the increasing prevalence of major restriction policies that limit students' access to certain majors based on their grades in introductory courses. Table 1 demonstrates these policies' prevalence by documenting the restrictions imposed on five of the highest-premium majors at the 25 top-ranked public U.S. universities in 2019. These universities enroll about 750,000 undergraduates, or half of all students at top-100 universities (and 7 percent of all undergraduates). Three-quarters of the universities' most lucrative majors impose either high school or university course GPA restrictions, including every nursing major and nearly all mechanical engineering and finance majors. In contrast, fewer than ten percent of the same majors at top-ranked private universities have formal restrictions, though many limit access to high-premium majors with low grades and other discouragement mechanisms (Armstrong and Hamilton, 2013).

Table 1: Binding Major Restrictions at the Top 25 US&WR Ranked Public Universities, Fall 2019

Univ.	Undergrad. Students	Computer Science	Economics	Finance	Mechanical Engineering	Nursing
Cornell	14,907	2.5	2.7	3.3; A	2.5; 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	2.5	A
Michigan	29,821	-	-	A	A	A
UC Santa Barbara	22,186	3.2	2.85	2.85	A	*
UNC – Chapel Hill	18,862	-	-	3.0; A	*	A
UC Irvine	29,307	3.0	2.5	3.0; A	3	A
Georgia Tech	15,573	-	-	-	-	*
Florida	35,247	-	3.0	3.0	2.8	3.3
William and Mary	6,285	-	-	2.5; A	*	*
UC Davis	30,145	3	-	*	2.8	*
UC San Diego	28,587	3.3; A	2.5	*	A	*
Georgia	28,848	-	A	A	A	*
UI – Urbana-Champaign	33,955	3.75; A	-	A	3.75; A	*
UT – Austin	40,492	A	-	3.25; A	3.0; A	3.0; A
UW – Madison	32,196	-	-	2.75; A	A	2.75; A
Ohio State	45,946	3.2	-	3.0; A	3.4	A
Purdue	31,006	-	2.75	-	3.2; A	2.75
Rutgers	35,641	-	-	A	A	HS
Penn. State – Univ. Park	40,835	HS	-	3.2	HS	HS
Washington	31,331	A	A	2.5; A	A	2.8; A
Connecticut	19,241	3.0; A	-	A	3.0; A	3.0; A
UMD – College Park	29,868	-	-	A	2.7	3.0; A
Clemson	19,402	-	-	-	HS	A
Texas A&M	53,065	2.75; A	3.0	3.5; A	3.5; A	A

Note: The Fall 2019 minimum major admissions requirements 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. 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, August 2019

We quantify the role of major restriction policies in generating ethnic stratification by constructing a new detailed database covering the 900,000 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 dynamic difference-in-difference research design around the introduction of 29 major restrictions. Estimating models at the department level, we show that major restrictions lead students with below-average SAT scores and relatively poor first-year academic performance to earn degrees in alternative fields of study.[4] As a result, newly- imposed major restrictions caused the share of URM students who declared the restricted major to decline by an average of 20 percent, matching the observational difference in URM attainment between the restricted and unrestricted majors shown in Table 1.[5]

Finally, we trace the college majors declared by students who lose access to restricted majors by introducing interactions with students' major intentions to our dynamic difference-in-difference model and studying restrictions' impact on the students who intend to complete those majors. Students' intentions are estimated by training random forests on major declaration indicators using first-term course enrollment before restrictions' implementation. We find that restricting a major has divergent effects on the URM and non-URM students who intend to complete it: on average, URM students with a strong (0.2) predicted intention to earn a restricted major end up earning majors with over 1 percent lower average wages than similar-intention non-URM students after the restriction's implementation. A simulation exercise employing these estimates suggests that major restrictions can largely explain the rise of ethnic stratification across college majors since the mid-1990s at those UC campuses.

Our findings call attention to three inefficiencies of major restriction policies. First, restrictions' disproportionate presence in valuable high-premium and STEM majors reduces the U.S. human capital stock (National Academies, 2007). Second, restrictions mute potential match effects between students' major choices and their comparative skill advantages and preferences (Kirkeboen, Leuven, and Mogstad, 2016). Finally, restrictions are most binding for academically-promising students (for which reason they were admitted to selective universities) with limited pre-college educational opportunities, but such students would have likely earned above-average returns from the opportunity to earn lucrative majors (Bleemer and Mehta, 2021).

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.[6] Our college major index generalizes the Science, Technology, Engineering, and Mathematics (STEM) categorization often used as a proxy for the economic value of college majors (e.g. Carrell, Page, and West, 2010; 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. Chetty et al., 2020; Bleemer, 2022), another potentially important but more controversial dimension of collegiate human capital (Bleemer, 2021; Mountjoy and Hickman, 2020). The observed difference in average major premia across URM and non-URM graduates could explain three percentage points (about ten percent) of the ethnicity wage gap among young college-educated workers – though we only have a single quasi-experiment to evince the causal validity of our major wage premium statistics – while the contributions of labor market frictions and other human capital factors remain largely unquantified.[7]

Second, we provide evidence highlighting the role of an understudied class of university policies that contribute to 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 – particularly students' preferences and subjective expectations (e.g. Wiswall and Zafar, 2015, 2018) – but the disproportionate growth of ethnic stratification at public research universities suggests an important role for supply-side policy variation like those universities' burgeoning major restriction policies.[8] We provide causal evidence that the imposition of major restriction policies disproportionately decreases URM enrollment and leads URM students to earn less-lucrative college majors, generating ethnic stratification at public research universities with macro-level wage ramifications.

Finally, our study contributes two methodological innovations with broad applicability in applied microeconomics. First, we identify policy effects on individuals who intended a policy-impacted behavior – in our context, declaring a restricted major – by explicitly characterizing intentions (using predicted major attainment trained on pre-implementation records) and then estimating a difference-in-difference model with predicted intention as the second difference.[9] Triple-difference interactions with demographic characteristics identify heterogeneous treatment effects on restricted-major-intending students. Second, we characterize university students' collegiate academic performance by two-way fixed effect decomposition of grades into additive student and course-term effects (following Abowd, Kramarz, and Margolis, 1999), enabling comparisons of students' general academic performance over long time horizons despite variation in grading standards that challenges the interpretation of traditional grade point averages.[10]

We begin in Section 1 by documenting the growth in ethnic stratification across majors in U.S. higher education, decomposing its between- and within-institution components, and motivating the potential role of major restriction policies at public research universities. Section 2 describes our detailed student data. Section 3 presents difference-in-difference evidence that major restrictions decrease URM enrollment by pushing out academically-disadvantaged students. Section 4 shows that GPA restrictions disproportionately lead URM students to earn lower-premium degrees, and Section 5 uses those estimates to simulate the stratification effects of UC's major restrictions. Section 6 concludes. A series of online appendices consider alternative major premium statistics, analyze the growth in between-institution stratification, further investigate restrictions' causal mechanisms using a comparative case study, and consider major restrictions' role in recent trends in gender stratification by major and Asian, Black, and Hispanic students' major completion.

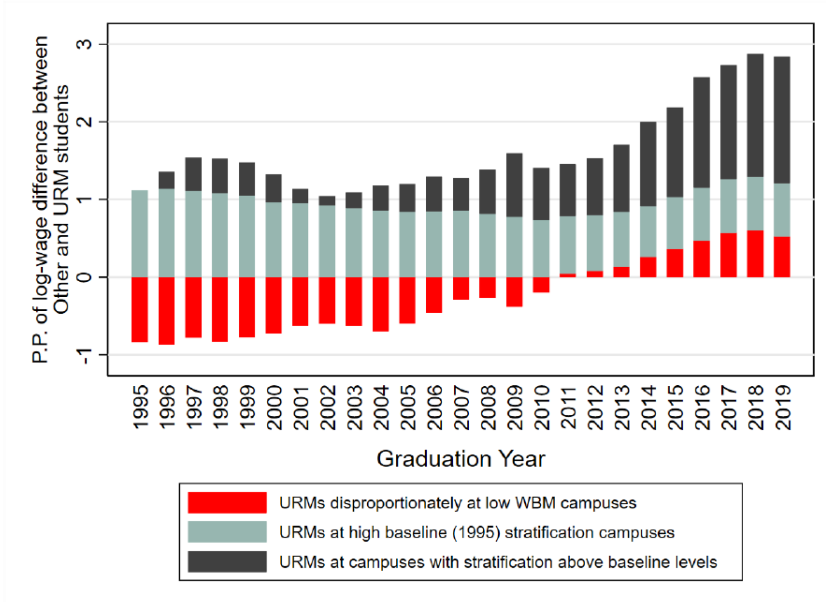
1. MOTIVATION

Stratification arises when some sub-populations are less likely to access desirable opportunities than others for (even partly) non-voluntary reasons (Darity, 2005). We begin by quantifying the growth in ethnic stratification by college majors in the U.S., decomposing its sources, and providing observational evidence suggesting an important role for major restriction policies.

1.1 Aggregate Trends in Ethnic Stratification of U.S. College Majors

Let $R \in U, N$ denote the ethnicity of underrepresented minority (URM) and non-URM workers.[11] We index the average collegiate human capital obtained by R members of birth cohort t by $E_t(W_m|R)$, where W_m is the average wage (conditional on demographics) earned by college graduates who earned major m compared to a baseline major, which we assign to be general agriculture.[12] We refer to our estimates of W_m as major m's "major premium". A comparison between the quasi-experimental wage effect of shifting students across UC Santa Cruz majors (Bleemer and Mehta, 2021) and the expected wage effect based on

Figure 2: 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 premia are assumed to be equal across ethnicities in institution × year cells in which no graduates of one ethnicity are observed. Major premia 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 A for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2018) and IPEDS.

average differences in premia across majors suggests that W_m effectively indexes majors' economic value, qualitatively but also perhaps quantitatively (see Figure A-1).

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

$$S_t^{Agg} \equiv \Delta R[E_t(W_m|R)\,] \equiv [E_t(W_m|N) \; - \; Et(W_m|U)] \tag{1}$$

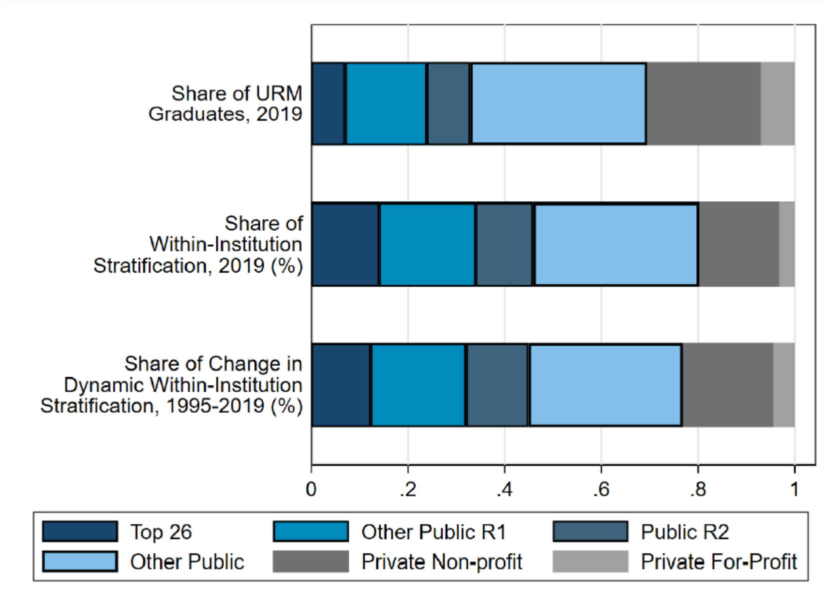
Figure 1 presents aggregate college major stratification by birth cohort for all college-educated and employed 2009-2019 American Community Survey respondents. It shows that URM students have tended to complete lower-premium majors, but that this gap had fallen to less than one percentage point in the 1970s before widening to 2.6 percentage points by the mid-1990s. 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). Appendix A shows that the same stratification trends are observed when major premia are estimated in different periods, restricted to a single gender or ethnicity, conditioned on local geography, or replaced with median earnings by major (following Sloane, Hurst, and Black, 2021).

1.2 Decomposing Ethnic Stratification Between and Within Institutions

We decompose the sources of the recent rise in ethnic stratification by college major using data on the annual number of college graduates by institution, major, and ethnicity since 1995 from the Integrated Postsecondary Education Data System (IPEDS). These data permit estimation of several time-varying 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 $E_t(W_m|i,R) = \sum_m P_t(m|i,R)W_m$ denotes each ethnic group's average major-wage within institution i, aggregate stratification can be disaggregated across institutions:

$$S_t^{Agg} = \sum_i [P(i|N)E_t(W_m|i,N) - P(i|U)E_t(W_m|i,U)] \tag{2}$$

Figure 3: 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 summation in Equation 4, and sector contributions to the change in dynamic within-institution stratification are sector subtotals of the third summation in Equation 5. The sample is limited to four-year degree-granting institutions in the 50 U.S. states. Average college major premia are assumed to be equal across ethnicities in institution × year cells in which no graduates of one ethnicity are observed. Major premia 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 A for details. Source: The 2009-2019 American Community Survey (Ruggles et al., 2018) and IPEDS.

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

$$S_t(i) = \Delta_R[E_t(W_m|i,R)] = \sum_m W_m \Delta_R[P_t(m|i,R)] \quad (3)$$

On the other hand, $\sum_i \{E(W_m|i,N)\Delta_R[P_t(i|R)]\}$ captures *between-institution* stratification, which is positive whenever URM students disproportionately attend institutions whose (non-URM) students specialize in low-wage majors. It follows that aggregate stratification is the sum of between-institution stratification and a URM-weighted average of within-institution stratification:

$$S_t^{Agg} = \sum_i \{E(W_m|i,N)\Delta_R[P_t(i|R)]\} + \sum_i P_t(i|U) S_t(i) \quad (4)$$

The within-institution summation in (4) can then be split to assess whether it changed due to (1) a reallocation of URM students into more stratified universities ("static") or (2) increased stratification (relative to 1995) within the universities that URMs attend ("dynamic"):

$$S_t = \sum_i \{E(W_m|i,N)\Delta_R[P_t(i|R)]\} + \sum_i P_t(i|U) S_{95}(i) + \sum_i P_t(i|U)[S_t(i) - S_{95}(i)] \quad (5)$$

Figure 2 implements Decomposition (5) annually across all 3,600 four-year colleges and universities in the U.S., estimating Wm 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 the universities where URM students are enrolled have increasingly stratified. There has also been substantial growth in between-institution stratification, which was negative in the late 1990s – indicating that the institutions that disproportionately graduate URM students specialized in higher-premium majors – but that this relationship had flipped by 2019. While URM students

Table 2: 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	2.0	1.0	1.1	1.2	1.0	0.2	1.2
2019	4.6	2.7	3.0	2.2	1.7	1.1	2.3

Note: The URM-weighted average of within-institution stratification $\left(S_t(i) = \sum_m W_m \Delta_R[P_t(m|i, R)]\right)$, 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., 2018) and IPEDS.

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.

Appendix B 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 institutions that specialize in low-premium majors (see Page and Scott-Clayton, 2016). However, the remainder of the present study primarily focuses on the larger but relatively-understudied within-institution component of ethnic stratification.

This component can be further decomposed into the contributions of each sector of American colleges and universities, permitting direct observation of which institutions appear most responsible for ethnic stratification's recent growth. We partition higher education into six sectors – the top 26 public universities discussed above, other R1 universities and R2 universities (following the Carnegie Classification), other public universities, and non-profit and for-profit private universities – and find that within-institution stratification increased in all six sectors. However, Table 2 shows that stratification within institutions increased to the greatest extent at public research universities, especially at the top 26 public universities. Figure 3 shows that in 2019, public research universities issued about a third of URM students' degrees but accounted for 46 percent of within-institution stratification and for 45 percent of the growth of dynamic within-institution stratification. These findings suggest that closer analysis of the inordinately-impacted public research university sector is most likely to reveal the root causes that are driving the recent national rise in ethnic stratification by college major.

1.3 Potential Demand-Side Explanations for Ethnic Stratification

Why are the public research universities with high URM enrollments becoming increasingly stratified across college 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 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 (see Figure A-4), rejecting the possibility that declining economic incentives to earn high-premium majors explain the observed trend in ethnic stratification.

Second, the steadily-expanding share of overall URM college enrollment may imply that URM college students are increasingly negatively-selected relative to non-URM students, which could increase URM students' relative effort costs of completing high-premium majors. However, the growth in college-going has been slower among URM than among non-URM high school graduates (Figure A-5), suggesting that the increase 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.[14] The ethnicity gap in average SAT scores at public research universities also appears to have narrowed in recent years (Figure A-6), suggesting that negative selection on pre-college academic preparation is unlikely to explain the observed widening of ethnic stratification within institutions.[15]

We thus find little evidence to suggest that demand-side factors were first-order contributors to the growth in within-institution stratification by college majors since the mid-1990s, though we do not present definitive evidence against demand-side explanations. The next subsection, however, provides a more promising supply-side explanation.

Table 3: Observational Relationship between Major Restrictions and URM Stratification

	URM Share in Major	
Any Restriction	-3.0 (1.3)	
Mechanical Restriction		-3.1 (1.1)
Discretionary Restriction		0.1 (1.6)
Institution FE	X	X
Field of Study FE	X	X
$\bar{Y}$	11.1	
Observations	98	

Note: Estimates from an OLS linear regression of a major's 2019 URM (Black or Hispanic) graduate share on whether the major is restricted, over the 26 institutions and four of the five majors presented in Table 1. Nursing is excluded because it is restricted on every campus at which is offered. Mechanical restrictions limit access to students with below-threshold introductory grades; discretionary restrictions limit access to students based on detailed applications, generally including both measured academic preparation along with essays and other materials. Each model includes institution and major fixed effects. Standard errors clustered by institution in parentheses.

Source: Integrated Postsecondary Education Data System.

1.4 Major Restriction Policies and Supply-Side Stratification

The growth in the college major ethnicity gap appears to have been particularly driven by in- creasing stratification at public research universities, a sector that has widely-implemented major restriction policies that regulate students' access to certain fields of study (see Table 1). Departments generally justify major restrictions by arguing either that capacity constraints resulting from sharp increases in student demand require access limitations or that lower-performing students cannot succeed in challenging fields of study.[16] They may also result from an increasing interest in 'prestige' departments that stratify students to improve their degrees' signal value (MacLeod and Urquiola, 2015).

Major restriction policies take one of three forms: (1) an average grade requirement in introductory courses; (2) an internal application favoring performance, extracurricular participation, and professed interest; or (3) an external application, such that students can only declare a major if they were admitted to that department prior to college enrollment. We refer to the first of these types as 'mechanical' restrictions and the second two as 'discretionary', since the latter facilitate more nuanced decisions over who is permitted into restricted majors.[17]

We examine the plausibility of an important relationship between major restriction policies and ethnic stratification by investigating the observational relationship between restriction policies and URM enrollment shares among the public research universities and lucrative majors whose restrictions are documented in Table 1. Table 3 reports estimated coefficients from linear regressions of the URM share of 2019 graduates in each major on the presence of mechanical and discretionary major restriction policies, with fixed effects absorbing differences in average URM shares across universities and fields. While about 11 percent of graduates from those universities' lucrative majors were URM, only 8 percent of graduates (over 25 percent fewer) from restricted majors were URM. The second column shows that this gap is wholly driven by mechanical restrictions, on which we will focus for the remainder of this study; there is no measurable relationship between the presence of discretionary restrictions and graduates' URM shares.

The set of descriptive and observational findings presented above suggest that mechanical major restriction policies may play an important role in the recent growth in colleges' ethnic stratification across college majors. The remainder of our study presents a series of quasi-experimental analyses designed to test the causal relationship between college major restrictions and the average premium of majors earned by impacted URM and non-URM students.

2. DATA

We analyze the causal stratification ramifications of major restriction policies by studying the restrictions implemented by four public research universities in California: the University of California campuses at Berkeley, Davis, Santa Barbara, and Santa Cruz.[18] For reference, these are among the country's most selective institutions, each admitting between 21 and 64 percent of freshman applicants in 2010.

Table 4: Fifty Years of Major Restrictions at Four Universities

Major	Years First	Years Last	Rule	Major	Years First	Years Last	Rule
UC Berkeley							
Business°	1970	-	A	Art	1993	-	A/3.3
Economics	1976	-	3.0	Psychology	2003	-	3.2
Computer Science	1979	2007	3.0	Public Health	2004	-	A/2.7
Political Economy	1980	2004	3.0-3.2	Oper. Research†	2005	-	3.2
Media Studies†	1980	-	A/3.2	Env. Econ. & Pol.	2009	-	2.7
Biochemistry*	1988	1989	2.7	Computer Science*	2013	-	3.0-3.3
UC Davis							
Statistics	1982	2004	3.0	Communication	2001	2013	2.5
Land. Architecture	1986	-	A	Human Dev.	2001	-	2.5
Psychology	1989	-	2.5	Managerial Econ.	2001	2011	2.8
Int. Relations	1992	2013	2.5	Biotechnology	2007	-	2.5
Computer Science	1997	2004	2.75	Design*	2011	2013	2.6
Exercise Science*	1997	2000	2.5	Mechanical Eng.*	2011	2014	2.8
Vit. and Enology	1998	-	2.5	Computer Science*	2016	-	3.0
Ferment. Science*	1998	2000	2.5				
UC Santa Barbara							
Computer Science°	<1983	2014	A/3.2	Political Science	1988	-	2.6
Communication°†	1983	-	2.5-3.0	Biology	1996	-	‡
Economics°	1984	-	2.7-2.85	Law and Society	1997	2006	2.5
Psychology°	1985	-	2.5-2.75	Biopsychology	2001	-	2.7-2.75
Mathematics°	1985	-	2.5	Computer Eng.	2003	2013	3
Electrical Eng.	1986	1996	3	Fin. Math. and Stat.	2005	-	2.5
UC Santa Cruz							
Economics	2002	-	2.8	Chemistry	2011	-	2.5
Physics	2008	-	2.7	Cognitive Science†	2011	-	2.5
Psychology	2011	-	2.7	Applied Linguistics*	2016	-	2.7

Note: Eligible major restrictions include GPA requirements for specified courses exceeding a C+ (2.3) or an internal competitive application. Does not include majors that are open to admits to a specific college but closed to admits to different colleges, like most Engineering majors; in any case, those policies have little changed 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 several 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.

We observe student outcomes at these campuses 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 at each of four University of California campuses in the observed sample period: UC Berkeley (1975 to 2016), UC Davis (1980 to 2018), UC Santa Barbara (1986 to 2018), and UC Santa Cruz (1975 to 2018).[19] The data include students' first year of enrollment, gender, ethnicity, and California residency; underrepresented minorities (URM) are defined to include Black, Hispanic, and Native American students. The data also cover each of the courses completed by each student and their grades in each course. For students who enrolled after 1993, we link the data to undergraduate application records that include SAT score, high school GPA, family income, and (for California-resident freshmen) high school.[20] Finally, we observe students' pre-college access to college-level coursework by linking public California high schools to 1997-2016 California Department of Education school records, which identify school-years in which each Advanced Placement or International Baccalaureate course was available.[21]

Table 5: Descriptive Statistics of UC Campus Majors

	All	Berkeley	Davis	Santa Barbara	Santa Cruz	3 Years Before Major Restriction
Number of Majors	216 [73]	77 [5]	91 [13]	54 [3]	40 [5]	
# Students	83 [110]	92 [111]	60 [91]	107 [138]	94 [101]	187 [161]
% Female	53 [22]	52 [21]	55 [23]	54 [23]	53 [22]	51 [21]
% URM	19 [17]	18 [17]	18 [17]	22 [20]	20 [15]	13 [7]
Sample Size, Overall						
Events	29	7	10	7	5	
Major-Years[1]	6,237	2,222	1,855	1,113	1,047	
Sample Size, Observe Demographics						
Events	25	7	7	6	5	
Major-Years[1]	5,763	2,222	1,455	1,039	1,047	

Note: Descriptive statistics of the average number of departments at each covered university, average number of students per department, and average percent of female and URM 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 4) and major-year observations, in the full sample and in the sample where student demographic characteristics (like ethnicity) are observed. [1] Only includes major-years with at least 20 observations; smaller departments are omitted from analysis.

Source: UC ClioMetric History Project Student Database.

Table 4 shows every formal major restriction policy that has been implemented by the four UC campuses since the 1970s, before which no restriction has been identified. Each restriction's first year is defined as the year prior to the major restriction first appearing in the school's course catalog, since that entering cohort is typically the first that would face the new binding major requirement. For major restrictions that are no longer implemented, a 'Last Year' is also recorded, again referring to the final cohort that likely faced the restriction. Restrictions with GPA caps at or below 2.3 (a C+ average in the requisite courses) are omitted, both because of their prevalence and because they are unlikely to bind in most cases. Each campus has imposed about 12 restricted majors over the past 50 years, though Davis's restrictions tend to be more numerous and shorter- lived than those at other campuses. Berkeley and Davis's Computer Science departments have implemented restrictions twice. Other than these cases, restrictions once imposed are seldom removed, indicating that their proliferation could explain growing stratification by major over time.

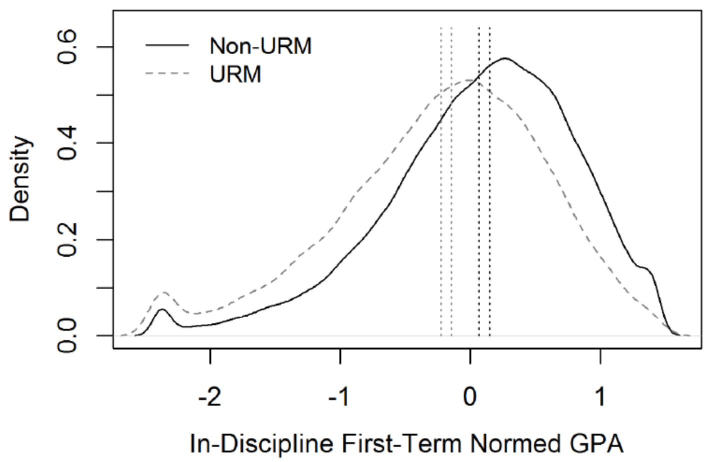
One possibly important effect of major restrictions is to stratify students by their university course performance, with higher-performing students permitted to enroll in restricted fields of study. Student grade point averages (GPAs) are often used to measure university course performance, but GPA is biased by differences in grading standards across academic disciplines. Figure A-7 displays average course GPAs by division at UC Berkeley throughout the sample period, showing large and growing gaps in average grades by discipline: Science and Engineering courses had average grades about 0.2 GPA points below the Humanities in 1970, but the gap had grown to almost 0.4 GPA points by the mid-2010s. The distributional shape of available grades may also differ by discipline. In order to abstract away from cross-field differences in grade availability, we measure "normed GPAs" as follows:

$$nGPA_i = \frac{1}{|C_i|}\sum_{c\in C_i}\frac{GPA_{ic}-\overline{GPA_c}}{sd(GPA)_c} \quad (6)$$

where student i's GPA is defined as the average number of standard deviations by which their grade was greater or less than the average grade in each course they completed (set C_i). Students with high normed GPAs are those who consistently out-perform their peers in their chosen courses. We also characterize students' average academic performance in college by their individual GPA fixed effect ("GPA FE"), estimated from a two-way fixed effect model that regresses GPA on individual and course fixed effects (following Abowd, Kramarz, and Margolis, 1999).

Table 5 presents descriptive statistics of the majors offered at each of the four UC campuses. Each campus offered an average of

Figure 4: Distribution of Introductory Course GPAs by Ethnicity



Note: Kernel density plots of winsorized normed first-term in-discipline grades among 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. Normed GPA is defined within-course following Equation 6; 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.

54 majors in each year of the sample period, with an annual average of 83 students per major (s.d. 110). The average major was 53 percent female and 19 percent URM. There were 29 newly-imposed major restrictions during the period covered by the data – with 5-10 at each of the four campuses – and 25 restrictions imposed in the period when ethnicity is observed. The total sample includes about 900,000 students who enrolled in 6,200 major-cohort pairs.

Table 5's final column shows characteristics of majors soon to implement major restrictions. Those majors are twice the size of average majors, averaging 187 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. Figure 4 shows that URM students who attained these soon-to-be-restricted majors had earned lower introductory course grades in those fields than non-URM students by about 0.3 standard deviations, further motivating the potential for restrictions to differentially impact URM students.

3. MAJOR RESTRICTIONS AND DIFFERENTIAL ACCESS

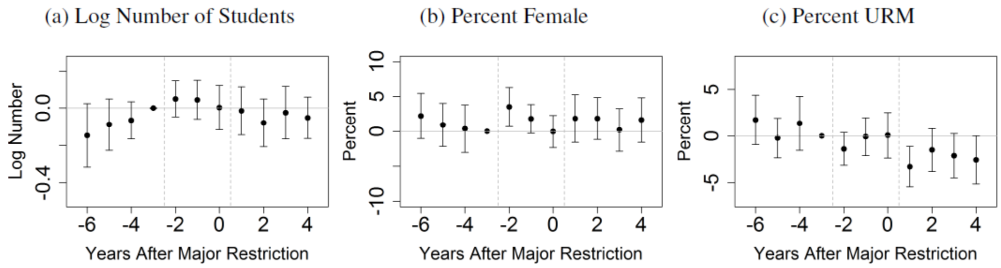
3.1 Empirical Methodology

We investigate the effect of major restrictions on the composition of students who declare each major by using a difference-in-difference event study design to estimate the effect of imposing a new restriction on the restricted major's student composition. Each newly-imposed major restriction in the sample period is considered an 'event'. Restrictions that were imposed within two years of the major's creation (prohibiting pre-period estimation) or for fewer than four years (prohibiting estimation of longer-run effects) are omitted, and mechanical restrictions are limited to those with GPA thresholds exceeding C+ (2.3). Using the resulting 29 events, models of the following form are estimated over the unbalanced panel of all majors in all available years at the four campuses:

$$Y_{cmy} = \alpha_{cm} + \gamma_{cy} + \sum_{t=-7}^{8} \beta_t \mathbf{1}\{y + t = P_{cm}\} + \epsilon_{cmy} \quad (7)$$

where Y_{cmy} is a feature of campus c's major m in cohort year y (like log number of students), α_{cm} and γ_{cy} are campus-major and campus-cohort fixed effects, and P_{cm} is the first cohort-year that faced major m's restriction at c. For example, $Y_{UCB,Econ,1990}$ could represent the log number of 1990-cohort students (that is, students whose first year of enrollment was 1990) who declared an economics major (whether or not they ultimately earned a degree) at UC Berkeley. Standard errors are clustered by campus-major.[22] We interpret the estimated $\hat{\beta}_t$ coefficients when $t > 0$ as the effect of implementing a major restriction policy on departmental enrollment, which assumes the absence of any other substantive policy changes that particularly impacted restricted majors at the time of the restrictions' implementation.

Figure 5: Department-Level Event Study: Student Demographics



Note: Event study β estimates of demographic characteristics of 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 estimate (e.g. as double-majors). Source: UC ClioMetric History Project Student Database.

The year of first implementation is noisily measured for major restrictions; course catalogs typically do not specify which cohort will be the first to face the major restriction, and timing of restrictions' catalog inclusion may differ by campus or department. As a result, $\beta - 3$ is set to 0 but care should be taken to not over-interpret β_0 through β_{-2}, which likely represent transitional years for the imposition of each restriction; the discussion below will highlight changes between the pre-period before $t = -3$ and the period after $t = 0$.

3.2 Findings

Panel (a) of Figure 5 shows β estimates and 95-percent confidence intervals from Equation 7 for the log number of students who declare newly-restricted majors before and after the imposition of the restrictions. The estimates suggest that major restrictions are put into place about five years after a major begins growing relative to other fields. Imposing the restriction causes an immediate cessation of this growth in the average department, with longer-run enrollment stabilizing around 10 percent below peak enrollment (similar to the pre-growth enrollment level), despite the observed increased student demand in that major.[23]

What were the characteristics of the students denied from the major as a result of newly- implemented major restrictions? The next two panels of Figure 5 shows that the proportion of female students in newly-restricted majors remained unchanged, but that the average proportion of URM students declined by 3.3 percentage points. Given the 10 percentage point decline in all major declarations and average pre-restriction URM share of 13 percent, this implies that URM students were over twice as likely to exit the major as a result of the restriction than non-URM students (about 25 vs. 12 percentage points).[24]

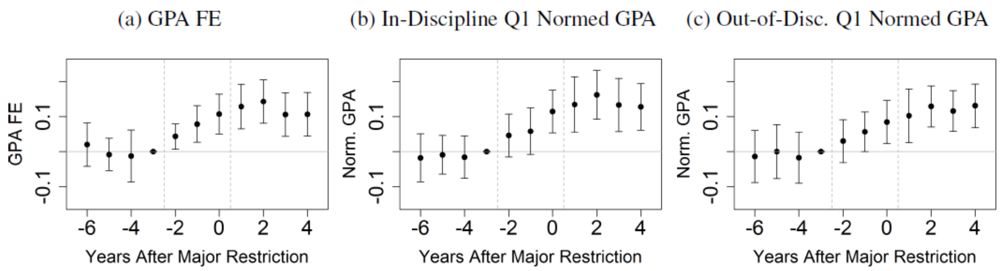
How did major restrictions differentially impact students with different levels of measured academic aptitude? The left panel of Figure 6 shows that newly-restricted majors' enrollees had higher average SAT scores by almost 40 points (on the 2400 scale), with the increase occurring over the three-year transitional period of the new restriction. This suggests that the students who exited the restricted major had average SAT scores as much as 300 points (or one nationwide standard deviation) lower than the average student declaring the major.

Panel (b) of Figure 6 shows that major restrictions yield students whose normed GPAs averaged across their first-term courses in the same discipline as the restricted major were higher. This is partly by construction, since some of these courses may have been used to calculate the introductory course GPA used to determine access to restricted majors. Panel (c), however, shows a near-identical effect on the average first-term Normed GPA earned by students in the major when calculated only over courses in other disciplines.[25] These results imply that students who exit restricted majors had average normed first-term GPAs about 0.75 standard deviations lower than the major's average, even when their GPA is calculated using only courses outside the major's discipline.

The similarity between Panels (b) and (c) suggests that major restrictions do not target students based on 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).[26]

These results, summarized in Table 6, indicate that major restrictions reduce the number of students who declare the restricted

Figure 6: Department-Level Event Study: Student Academic Characteristics



Note: Event study β estimates of the measured academic performance of 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 estimate (e.g. as double-majors). GPA fixed effect is the student effect from a two-way fixed effect model of grades on students and course-terms. Normed GPA is defined within-course following Equation 6; 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, while in-discipline courses include those in the major's discipline. Source: UC ClioMetric History Project Student Database.

major, with URM students far more likely to exit the major than non-URM students.[27] However, these estimates may be upwardly biased by the mechanical outflow of students from the restricted major into the 'control' majors, which would bias the estimated betas away from 0 by moving the control majors in the opposite direction of the restricted majors. We test the importance of this mechanical channel by conducting a placebo bootstrap exercise, pulling 1,000 draws of 29 campus-year pairs as placebo restrictions and re-estimating the difference-in-difference models over each set.

These bootstraps permit estimation of empirical p-values for one-sided tests of the statistical significance of the presented beta estimates, shown in the last row of Table 6. For example, the 3.31 percentage point decline in the URM share of restricted major attainment is a larger decline than all but 3.7 percent of the placebo estimates, suggesting that the observed decline is very unlikely to be explained by the observed cross-major mechanical correlations.

The restrictions appear to select students with general academic advantages as opposed to students with advantages specific to the field of study. Appendix C uses a case study of two UC campuses' economics majors, only one of which is restricted, to further investigate the mechanisms by which major restrictions limit URM students' access to lucrative majors, showing that URM students' poorer pre-college academic opportunity and preparedness fully explains the observed URM enrollment decline. The next section devolves this section's department-level analysis to the student level in order to understand where students flow when they exit restricted majors.

4. MAJOR RESTRICTIONS AND ETHNIC STRATIFICATION

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

4.1 Empirical Methodology

We approximate revealed-preference major intentions using students' first-term courses, which they select in the first weeks after arriving on campus.[28] Because a wide variety of courses are available to students in their first term, their choices reveal substantial information about their intended majors.

Let M_{im} indicate whether student i declares a major in field m, with m reflecting a campus-major pair.[29] In order to isolate students' major intentions absent the presence of major restriction policies, we begin by constructing a within-campus training sample of 50 percent of students between four and five years before major m's restriction's implementation for each restricted major m. We then predict training-sample students' intention to major in m by indicators for enrollment in each first-term course, gender, and URM status using a random forest estimator (Ho, 1995).[30]

Table 6: Summary of Department Difference-in-Difference Estimates around Major Restriction Implementation

	Log Num. of Students	Percent Female	Percent URM	SAT Score	GPA FE	First Term GPA[1] In Disc.	Out of Disc.
4-7 Yrs. Before Restriction	-0.10 (0.06)	1.12 (1.34)	0.96 (0.94)	-3.61 (11.74)	-0.00 (0.03)	-0.01 (0.02)	-0.01 (0.03)
Transition Years	0.03 (0.05)	1.66 (0.94)	-0.41 (0.94)	11.90 (11.35)	0.08 (0.02)	0.07 (0.03)	0.06 (0.03)
1-5 Yrs. After Restriction	-0.04 (0.06)	1.39 (1.40)	-2.35 (1.09)	30.24 (13.82)	0.12 (0.03)	0.14 (0.04)	0.12 (0.03)
Campus-Major FE	X	X	X	X	X	X	X
Campus-Year FE	X	X	X	X	X	X	X
Observations	6,354	5,867	5,867	4,200	6,174	5,835	5,753
$\bar{Y}$	4.3	52.8	18.7	1819		0	0
Δ (Post-Pre)[2]	0.06 (0.08)	0.28 (1.22)	-3.31 (0.84)	33.85 (13.27)	0.12 (0.03)	0.13 (0.03)	0.13 (0.03)
Placebo p-value[3]	[0.626]	[0.872]	[0.037]	[0.058]	[0.000]	[0.000]	[0.004]

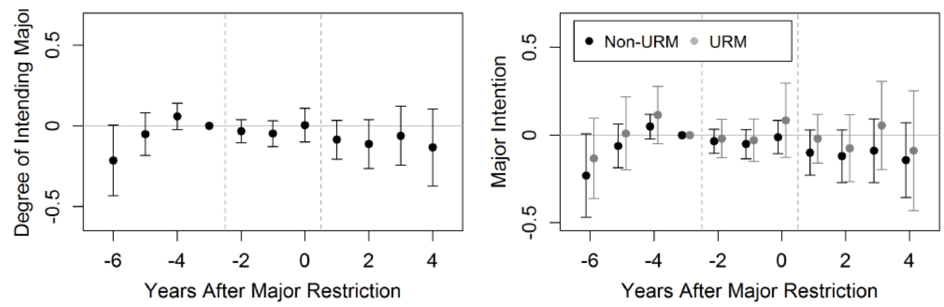
Note: Event study β estimates of the measured characteristics of students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Standard errors clustered by campus-major in parentheses. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. "Before" indicates 3-7 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 estimate (e.g. as double-majors). [1]First-term normed GPA is defined within-course following Equation 6; 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, while in-discipline courses include those in the major's discipline. [2] The difference between "After" and "Before" Major Restriction β coefficients, with standard error in parentheses. [3]An exact p-value on Δ (Post-Pre) from 1,000 draws of placebo major restrictions, to account for mechanical correlations as students move between departments.

Source: UC ClioMetric History Project Student Database and UC Corporate Student System.

We employ the resulting prediction algorithm to estimate $\widehat{M}_{im}$ for every student at that campus between six years before the restriction and four years after it (excluding the training sample).[31] Students with higher $\widehat{M}_{im}$ took courses that more strongly suggest their intention to major in m. Courses strongly predict major choice: the correlation between M_{im} and $\widehat{M}_{im}$ 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. Students who declare major m three years before the major's implementation have a mean (s.d.) $\widehat{M}_{im}$ of 0.13 (0.18).[32]

Figure 7 plots the evolution of students' revealed-preference major intentions ($\widehat{M}_{im}$) around the imposition of the 20 major restrictions with estimable intentions in our sample.[33] Intentions to declare restricted majors rose in the years leading up to those restrictions and then slightly declined after their imposition, by a noisily-estimated 10 percentage points. However, the change in intentions does not exhibit a URM gap, suggesting that the decline in URM enrollment does not arise from differential discouragement from departments' introductory courses.[34]

Figure 7: Estimated Changes in Students' Intentions for Restricted Majors



Note: Event study 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, following Equation 33 and estimated over a stacked dataset of students i's major intentions in field m. $\beta_{i,-3}$ is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include m fixed effects. Asterisks reflect p-values from hypothesis tests of equality in each period by gender or ethnicity: $*$ ten percent, $**$ five percent, and $***$ one percent. Source: UC ClioMetric History Project Student Database.

Having characterized students' revealed intentions to declare restricted majors, we use $\widehat{M}_{im}$ 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 dynamic difference- in-difference models over a stacked student-campus-major dataset by weighted least squares:

$$Y_{im} = \zeta_{my_i} + \gamma\widehat{M}_{im} + \sum_{t=-6}^{6}\beta_{it}\mathbf{1}\{y_i + t = R_m\} \times \widehat{M}_{im} + \epsilon_{im} \quad (8)$$

These regressions include major-cohort indicators ζ_{my_i} to flexibly absorb within-campus major choice trends, leaving β_{it} to be identified by variation between students with stronger and weaker intentions of declaring the restricted major m relative to the baseline year. We estimate either a single $\hat{\beta}_{it}$ for each t (over all i) or two coefficients by ethnicity, setting $\beta - 3 = 0$ for all i, and cluster standard errors by major and by student as if $\widehat{M}_{im}$ were observed without noise.[35]

4.2 Results

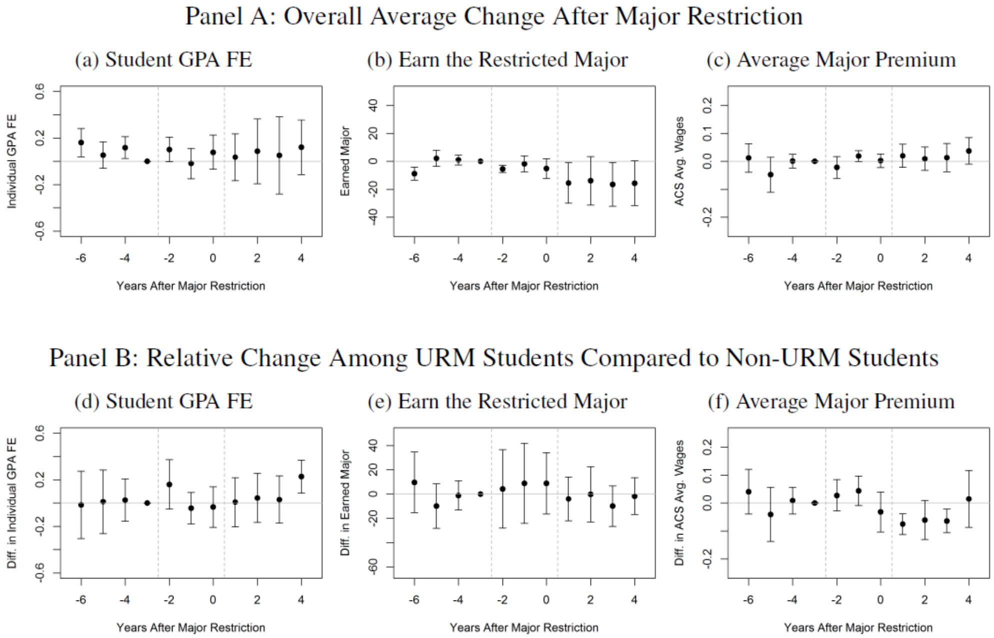
Panel A of Figure 8 begins by showing that overall, the imposition of major restrictions does not cause a measurable average shift in the average academic performance of the students who intend the restricted majors, as measured by students' two-way GPA fixed effect. However, major restrictions decrease the likelihood with which students who intended the restricted major are able to successfully declare it by about 15 percentage points, though the average effect between one and four years after the policy's implementation is only marginally statistically significant.

As above, we summarize stratification-relevant changes in students' major choice by the mean wage earned by college graduates with that major (conditional on gender, ethnicity, and age) as observed in the American Community Survey (Ruggles et al., 2020).[36] Interestingly, the last panel shows that the decline in restricted major declaration does not translate into any aggregate 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 three outcomes, characterizing the major choices of high-$\widehat{M}_{im}$ URM students relative to non-URM students.[37] URM students did not exhibit differential selection into major intention by average academic performance, and there is only slight evidence that high-$\widehat{M}_{im}$ URM students were less likely to declare the major than high-$\widehat{M}_{im}$ non-URM students. However, high-$\widehat{M}_{im}$ URM students' average major premium precipitously declined in the years following major restrictions; compared to the average non-URM student with $\widehat{M}_{im} = 0.2$, major restrictions led similar-$\widehat{M}_{im}$ URM students to declare majors with lower premia by about 1.2 percentage points on average.[38]

These findings suggest that major restrictions tend to have the net effect of leading URM students to declare lower-premium majors, not because they are discouraged from attempting to declare restricted majors but because they are unable to declare the

Figure 8: Changes in Major Choice and Composition of Students Who Intend Restricted Majors



Note: Difference-in-difference event study β_{it} estimates of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and their major choice or student characteristic before and after the implementation of the restriction, following Equation 8 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. 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), whether the student declares the restricted major, and the premium of the student's major (as defined in Appendix A). 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 климатических ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

major despite their intentions. Appendix C uses the economics case study to provide additional evidence that the major restrictions disproportionately prohibit URM students from declaring restricted majors (as opposed to URM students being discouraged from attempting restricted majors), showing that this can be explained by URM students' poorer average pre-college academic opportunity and preparation.

The presented difference-in-difference estimates average over all estimable major restriction policies, even though some of them impose restrictions on lower-premium majors. While we do not have sufficient power to restrict our analysis to the high-premium majors that are increasingly restricted at many U.S. colleges and universities, it would not be surprising to observe sharper increases in stratification across majors in those cases.[39]

5. DISCUSSION: MAJOR RESTRICTIONS AND ETHNIC 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 college major restriction policies to college major stratification by comparing the observed growth in UC's major premium gap with the 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 covered UC campuses in year t, and let W_i be the (unobserved) wage premium of the major that would be earned by student i absent any major restrictions. Then aggregate stratification at those four campuses (as defined in Equation 1) absent any restrictions can be written as

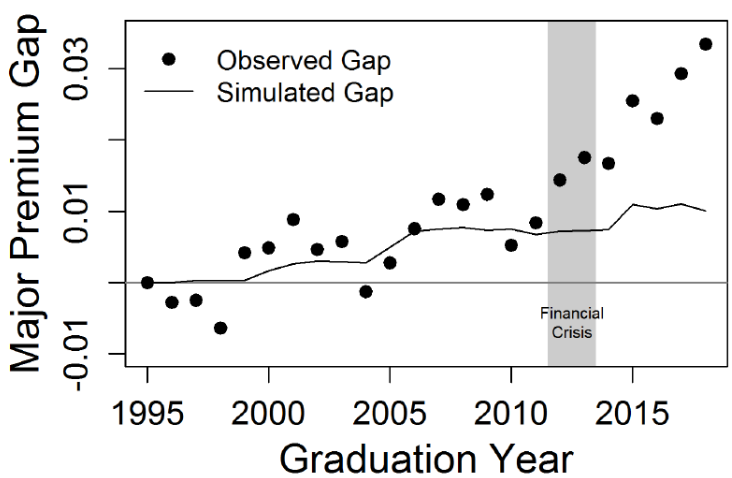
$$\frac{1}{|N_t|}\sum_{i\in N_t} W_i - \frac{1}{|U_t|}\sum_{i\in U_t} W_i \quad (9)$$

Now let Γ be the set of campus-major pairs ever restricted since some base year, and let P_r be the set of years in which major $r \in \Gamma$ was restricted. Let $\beta^U_{P_r t}$ and $\beta^N_{P_r t}$ denote the effects of major restriction policies on the major premia earned by URM and non-URM students who intended those majors. Given student i's predicted degree of intention to major in r ($\widehat{M}_{ir}$), observed aggregate stratification is

$$\frac{1}{|N_t|}\sum_{i\in N_t}\left(W_i + \sum_{r\in\Gamma}\beta^N_{P_r t}\widehat{M}_{ir}\right) - \frac{1}{|U_t|}\sum_{i\in U_t}(W_i + \sum_{r\in\Gamma}\beta^U_{P_r t}\widehat{M}_{ir} \quad (10)$$

We estimate the difference between Equations 9 and 10 – the contribution of new major restrictions to aggregate stratification – by imposing a series of simplifying assumptions. We abstract away from major restrictions' small differential impact on students' major intentions by replacing the non-URM average $\widehat{M}_{ir}$ with the URM average. This permits us to estimate and employ a single

Figure 9: Growth in UC Major Premium Gap, Actual and Simulated from New 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 major restrictions imposed by those campuses since 1995 following Equation 11. 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). URM includes Black, Hispanic, and Native American students. Source: UC ClioMetric History Project Student Database and the American Community Survey (Ruggles et al., 2020).

causal coefficient between restrictions and ethnicity differences in major choice, $(\widehat{\beta^N_{P_r t} - \beta^U_{P_r t}})$, which we estimate to be about −0.06 – the average of the $\hat{\beta}_t$ coefficients 1-3 years following restriction implementation (from Figure 8) – when the restriction is in place. 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 (trained on pre-restriction data), we also fix $\sum_{i\in U_t}\widehat{M}_{ir}$ at its average value 1-3 years following restrictions' implementation scaled by the URM population at that campus. This means we can simulate the contribution of newly-implemented major restrictions to the growth in UC college major stratification since 1995 as

$$SimGap_t \approx \left(\widehat{\beta^N - \beta^U}\right)\sum_{r\in\Gamma}\left(\mathbf{1}\{t \in P_r\}\frac{1}{U_{\min(P_r)}}\sum_{i\in U_{\min(P_r)}}\widehat{M}_{ir}\right) \quad (11)$$

This sums the estimated contribution of each post-1995 major restriction to the ethnicity premium gap.[40]

Figure 9 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 whose graduating class largely chose majors after the '07-08 financial crisis – outstripped the imposition of new restrictions. One important contributor to this recent stratification 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.[41] Some economics departments, for example, substantially sharpened the enforcement of their GPA restrictions immediately following the financial crisis (see Figures A- 15 and A-16), and computer science departments similarly tightened their restrictions in the late 2010s; for example, Berkeley increased its computer science GPA threshold from 3.0 to 3.3 in 2015. Our findings suggest that the compounding restrictions in these fields – which are also among the five largest majors at all four UC campuses – likely explain an appreciable share of UC's post-2011 stratification growth. We conclude that major restriction policies alone can largely explain the recent growth in college major stratification at the observed University of California campuses, providing further evidence that major restrictions are a first-order contributor to the recent growth in ethnic stratification across college majors in the United States.

6. 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 peers. About two-thirds of this rise in ethnic stratification can be explained by the rise of within-institution stratification, which in turn has been particularly increasing at the large public research universities that enroll about a quarter of American college students.

Those universities' increasingly popular major restriction policies appear to have played an important role in stratifying their lucrative majors by ethnicity: implementing a new major restriction policy tends to decrease the restricted department's URM enrollment by a quarter and disproportionately push URM students who intended the major into less-lucrative fields instead. In the same way that test-based meritocratic admissions policies inefficiently limit selective universities' access to applicants with poorer academic qualifications, the stratification generated by major restriction policies exacerbates equity gaps between high- and low-SES families, with negative implications for efficiency, economic mobility, and the ethnicity wage gap.[42]

REFERENCES

Abowd, John M., Francis Kramarz, and Davis N. Margolis. 1999. "High Wage Workers and High Wage Firms." Econometrica 67 (2):251–333.

Ahn, Thomas, Peter Arcidiacono, Amy Hopson, and James R. Thomas. 2019. "Equilibrium Grade Inflation with Implications for Female Interest in STEM Majors." NBER Working Paper 26556. Altonji, Joseph, Peter Arcidiacono, and Arnaud Maurel. 2016. "The Analysis of Field Choice in College and Graduate School: Determinants and Wage Effects." Handbook of the Economics of Education 5:305–396.

Altonji, Joseph, Erica Blom, and Costas Meghir. 2012. "Heterogeneity in Human Capital Investments: High School Curriculum, College Major, and Careers." Annual Review of Economics 4 (1):185–223.

Altonji, Joseph G. and Rebecca M. Blank. 1999. "Race and Gender in the Labor Market." Handbook of Labor Economics 3:3143–3259.

Andrews, Rodney and Kevin Stange. 2019. "Price Regulation, Price Discrimination, and Equality of Opportunity in Higher Education: Evidence from Texas." American Economic Journal: Economic Policy 11 (4):31–65.

Arcidiacono, Peter. 2004. "Ability Sorting and the Returns to College Major." Journal of Econometrics 121 (1-2):343–375.

Arcidiacono, Peter, Esteban Aucejo, and V. Joseph Hotz. 2016. "University Differences in the Graduation of Minorities in STEM Fields: Evidence from California." American Economic Review 106 (3):525–562.

Arcidiacono, Peter, Esteban Aucejo, and Ken Spenner. 2012. "What happens after enrollment? An analysis of the time path of racial differences in GPA and major choice." IZA Journal of Labor Economics 1 (5).

Arcidiacono, Peter, Josh Kinsler, and Tyler Ransom. 2019. "Legacy and Athlete Preferences at Harvard." Journal of Labor Economics .

Armstrong, Elizabeth A and Laura T Hamilton. 2013. Paying for the Party. Harvard University Press.

Azmat, Ghazala, Caterina Calsamiglia, and Nagore Iriberri. 2020. "Gender Differences in Response to Big Stakes." Journal of the European Economic Association 14 (6):1372–1400.

Bayer, Patrick and Kerwin Kofi Charles. 2018. "Divergent Paths: A New Perspective on Earnings Differences Between Black and White Men Since 1940." Quarterly Journal of Economics 133 (3):1459–1501.

Bertrand, Marianne and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor market Discrimination." American Economic Review 94 (4):991–1013.

Bettinger, Eric P. and Bridget Terry Long. 2017. “Mass Instruction or Higher Learning? The Impact of College Class Size on Student Retention and Graduation.” Education Finance and Policy 13 (1):97–118.

Black, Dan, Amelia Haviland, Seth Sanders, and Lowell Taylor. 2006. “Why Do Minority Men Earn Less? A Study of Wage Differentials Among the Highly Educated.” Review of Economics and Statistics 88 (1):300–313.

Black, Dan A., Amelia M. Haviland, Seth G. Sanders, and Lowell J. Taylor. 2008. “Gender Wage Disparities among the Highly Educated.” Journal of Human Resources 43 (3):630–659.

Bleemer, Zachary. 2018. “The UC ClioMetric History Project and Formatted Optical Character Recognition.” Center for Studies in Higher Education Research Paper Series 18 (3).

———. 2020. “Mismatch at the University of California before Proposition 209.” UC-CHP Policy Brief 2020 (5).

———. 2021. “Top Percent Policies and the Return to Postsecondary Selectivity.” Center for Studies in Higher Education Research Paper Series 21 (1).

———. 2022. “Affirmative Action, Mismatch, and Economic Mobility after California’s Proposition 209.” Quarterly Journal of Economics.

Bleemer, Zachary and Aashish Mehta. 2020. Major Restrictions, Socioeconomic Stratification, and Student Outcomes. Oakland, CA: UCOP Institutional Research and Academic Planning.

———. 2021. “Will Economics Make You Rich? A Regression Discontinuity Analysis of the Return to College Major.” American Economic Journal: Applied Economics.

Blom, Erica, Brian C Cadena, and Benjamin J Keys. 2021. “Investment over the business cycle: Insights from college major choice.” Journal of Labor Economics 39 (4):1043–1082.

Bound, John, Michael Lovenheim, and Sarah Turner. 2010. “Why Have American College Completion Rates Declined? An Analysis of Changing Student Preparation and Collegiate Resources.” American Economic Journal: Applied Economics 2:129–157.

Bound, John and Sarah Turner. 2007. “Cohort Crowding: How Resources Affect Collegiate Attainment.” Journal of Public Economics 91:877–899.

Brenoe, Anne Ardila and Ulf Zolitz. 2020. “Exposure to More Female Peers Widens the Gender Gap in STEM Participation.” Journal of Labor Economics 38 (4):1009–1054.

Brown, Charles and Mary Corcoran. 1997. “Sex-Based Differences in School Content and the Male-Female Wage Gap.” Journal of Labor Economics 15 (3):431–465.

Butcher, Kristin F., Patrick J. McEwan, and Akila Weerapana. 2014. “The Effects of an Anti- Grade-Inflation Policy at Wellesley College.” Journal of Economic Perspectives 28 (3):189–204. Canaan, Serena and Pierre Mouganie. 2021. “The Impact of Advisor Gender on Female Students’ STEM Enrollment and Persistence.” The Journal of Human Resources .

Card, David. 1999. “The Causal Effect of Education on Earnings.” Handbook of Labor Economics 3:1801–1863.

Card, David and Laura Giuliano. 2016. “Universal Screening Increases the Representation of Low- Income and Minority Students in Gifted Education.” Proceedings of the National Academy of Sciences 113 (48):13678–13683.

Card, David and Alan B. Krueger. 1992. “School Quality and Black-White Relative Earnings: A Direct Assessment.” Quarterly Journal of Economics 107 (1):151–200.

Card, David and A. Abigail Payne. 2021. “High School Choices and the Gender Gap in STEM.” Economic Inquiry 59 (1):9–28.

Carrell, Scott E., Marianne E. Page, and James E. West. 2010. “Sex and Science: How Professor Gender Perpetuates the Gender Gap.” Quarterly Journal of Economics 125 (3):1101–1144.

Caulkins, Johnathan P., Patrick D. Larkey, and Jifa Wei. 1995. “Adjusting GPA to Reflect Course Difficulty.” Manuscript .

Chetty, Raj, John Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan. 2020. “Income Segregation and Intergenerational Mobility Across Colleges in the United States.” Quarterly Journal of Economics 135 (3):1567–1633.

Colwell, Rita, Ashley Bear, and Alex Helman. 2020. Promising Practices for Addressing the Underrepresentation of Women in Science, Engineering, and Medicine. Washington, D.C.: The National Academies Press.

Darity, William. 2005. “Stratification economics: The role of intergroup inequality.” Journal of Economics and Finance 29 (2):144–153.

Darity, William A. and Patrick L. Mason. 1998. “Evidence on Discrimination in Employment: Codes of Color, Codes of Gender.” Journal of Economic Perspectives 12 (2):63–90.

Deming, David J., Claudia Goldin, and Lawrence F. Katz. 2012. “The For-Profit Postsecondary School Sector: Nimble Critters or Agile Predators?” Journal of Economic Perspectives 26 (1):139–164.

Denning, Jeffrey and Patrick Turley. 2017. “Was that SMART? Institutional Financial Incentives and Field of Study.” Journal of Human Resources 52 (1):152–186.

Gerard, Francois, Lorenzo Lagos, Edson Severnini, and David Card. 2021. “Assortative Matching or Exclusionary Hiring? The Impact of Employment and Pay Policies on Racial Wage Differences in Brazil.” American Economic Review 111 (10):3418–3457.

Ho, Tin Kam. 1995. “Random decision forests.” In Proceedings of 3rd international conference on document analysis and recognition, vol. 1. IEEE, 278–282.

Ioannides, Yannis M. and Linda Datcher Loury. 2004. “Job Information Networks, Neighborhood Effects, and Inequality.” Journal of Economic Literature 42 (4):1056–1093.

Kirkeboen, Lars, Edwin Leuven, and Magne Mogstad. 2016. “Field of Study, Earnings, and Self- Selection.” Quarterly Journal of Economics 131 (3):1057–1111.

Lang, Kevin and Jee-Yeon K. Lehmann. 2012. “Racial Discrimination in the Labor Market: Theory and Empirics.” Journal of Economic Literature 50 (4):959–1006.

MacLeod, W. Bentley and Miguel Urquiola. 2015. “Reputation and School Competition.” American Economic Review 105 (11):3471–3488.

Mountjoy, Jack and Brent R. Hickman. 2020. “The Returns to College(s): Estimating Value-Added and Match Effects in Higher Education.” Manuscript .

Mourifie, Ismael, Marc Henry, and Romuald Meango. 2020. “Sharp Bounds and Testability of a Roy Model of STEM Major Choices.” Journal of Political Economy 128 (8):3220–3283.

National Academies. 2007. Rising Above the Gathering Storm. Washington, D.C.: The National Academies Press.

Neal, Derek A. and William R. Johnson. 1996. “The Role of Premarket Factors in Black-White Wage Differences.” Journal of Political Economy 104 (5):869–895.

Novgorodsky, David and Bradley Setzler. 2019. “Practical Guide to Event Studies.” Manuscript . Page, Lindsay C. and Judith Scott-Clayton. 2016. “Improving College Access in the United States:

Barriers and Policy Responses.” Economics of Education Review 51:4–22.

Ruggles, Steven, Sarah Flood, Ronald Goeken, Josiah Grover, Erin Meyer, Jose Pacas, and Matthew Sobek. 2018. IPUMS USA: Version 8.0. Minneapolis, MN: IPUMS.

———. 2020. Integrated Public Use Microdata Series USA: Version 10.0 [dataset]. Minneapolis, MN: Minnesota Population Center, IPUMS. Https://doi.org/10.18128/D010.V10.0. Accessed March 2020.

Schmidt, Stephen J. 2021. “Minimum Grade Requirements for Economics Majors: Effects on Enrollments and Student Learning.” AEA Papers and Proceedings 111:107–111.

Sloane, Carolyn M., Erik G. Hurst, and Dan A. Black. 2021. “College Majors, Occupations, and the Gender Wage Gap.” Journal of Economic Perspectives 35 (4):223–248.

Stange, Kevin. 2015. “Differential Pricing in Undergraduate Education: Effects on Degree Production by Field.” Journal of Policy Analysis and Management 34 (1):107–135.

Stinebrickner, Ralph and Todd R. Stinebrickner. 2014. “A Major in Science? Initial Beliefs and Final Outcomes for College Major and Dropout.” The Review of Economic Studies 81 (1):426– 472.

Sun, Liyang and Sarah Abraham. 2021. “Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects.” Journal of Econometrics .

Turner, Sarah E. and William G. Bowen. 1999. “Choice of Major: The Changing (Unchanging) Gender Gap.” Industrial and Labor Relations Review 52 (2):289–313.

Vars, Frederick and William Bowen. 1998. “Scholastic Aptitude Test scores, race, and academic performance in selective colleges and universities.” In The Black–White test score gap, edited by C. Jencks and M. Phillips. Washington, D.C.: Brookings Institution Press, 457–479.

Wiswall, Matthew and Basit Zafar. 2015. “Determinants of College Major Choice: Identification Using an Information Experiment.” Review of Economic Studies 82:791–824.

———. 2018. “Preference for the Workplace, Investment in Human Capital, and Gender.” Quarterly Journal of Economics 133 (1):457–507.

Wittman, Donald. 2020. “GPA is a Terrible Measure of College Success: There are Much Better Grade-Based Alternatives.” Manuscript .

ENDNOTES

[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.

[2] See Figure A-1 for evidence that major premium statistics effectively capture major-specific returns for interested on-the-margin students, the relevant group for our analysis below.

[3] Bayer and Charles (2018) show that the Black-white 90th percentile wage gap experienced positional convergence between 1970 and 2014 (partialing out changes in the wage structure), but even in 2014 most workers were from pre-1980 birth cohorts; major choice trends may have slowed or reversed convergence among young workers. 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.

[4] We find no evidence that major restrictions decrease enrollment among students with a comparative disadvantage in the restricted field; the students who exit restricted majors have similarly low grades across all disciplines.
[5] Because course catalogs provide imprecise information which cohorts were 'grandfathered' or otherwise unimpacted by new major restriction policies, the estimated difference-in-difference models allow 1-year 'transition' windows around restriction implementations. We do not find evidence of differential pre-trends in students' demographics or preparedness nor evidence of sensitivity to alternative estimation strategies (e.g. Sun and Abraham, 2021).
[6] Sloane, Hurst, and Black (2021) use a similar index of majors' economic value to study the gender wage gap.
[7] 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.
[8] Altonji, Arcidiacono, and Maurel (2016) do not mention major restrictions in their handbook chapter's discussion of the 'supply side' of college major choice, but several other university policies have also been shown to shape students' major choices, including major-specific price discrimination (Stange, 2015; Andrews and Stange, 2019), major-specific incentive payments (Denning and Turley, 2017), and differential grading standards (Arcidiacono, Aucejo, and Spenner, 2012; Stinebrickner and Stinebrickner, 2014; Butcher, McEwan, and Weerapana, 2014). Differences in pre-college academic preparation (Card and Payne, 2021) and peer composition (Brenoe and Zolitz, 2020) can also influence within-institution stratification. 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.
[9] Students also report intended majors on their applications, but those are generally non-binding and may be strategically and selectively reported; our estimates reflect an incentivized measure of students' revealed preferences.
[10] Caulkins, Larkey, and Wei (1995) and Wittman (2020) suggest similar two-way fixed effect specifications.
[11] URM designates Black, Hispanic, and Native American/Alaskan workers.
[12] See Appendix A for a formal definition of Wm and Table A-1 for our Wm estimates.
[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 2). Institutions outside the fifty states are omitted, and expected Wm is assumed to be equal across ethnicities in institution × year cells in which no graduates of one ethnicity are observed.
[14] The inflow of URM college students tended to be absorbed by less selective institutions (Appendix B), which may have contributed to the increase in between-institution stratification documented above. However, the within- institution stratification on which we focus in this study partials out this between-institution variation.
[15] We provide evidence in Appendix C that average differences in academic preparation by ethnicity only stratify students across majors in the presence of major restriction policies. In this context, it is noteworthy that ethnic stratification grew even as the URM students at public research universities became better equipped to complete restricted majors (as measured by SAT scores) than their predecessors, suggesting that increased filtering of students was unnecessary.
[16] Thinly-stretched resources from 'over-enrollment' could negatively impact educational quality (Bound and Turner, 2007; Bound, Lovenheim, and Turner, 2010), in part through increased class sizes (Bettinger and Long, 2017). Bleemer and Mehta (2021) show that lower-performing students receive above-average wage returns from earning an economics major.
[17] These restrictions are often complemented by 'soft' restrictions like low introductory course grades and verbal discouragement, but we focus exclusively on easier-to-observe mechanical and discretionary restrictions for empirical tractability.
[18] All but one of the UC restrictions implemented in our study period were mechanical restrictions, so below we estimate the overall average effects of major restriction policies. These particular four campuses were selected because of data availability.
[19] Ethnicity is observed after 1975 (Berkeley and Santa Cruz), 1987 (Santa Barbara), or 1990 (Davis).
[20] All statistics produced using UC admissions data are replicated from Bleemer and Mehta (2020).
[21] California Department of Education course-level school information available at http://www.cde.ca.gov/ds/sd/df/filesassign.asp.
[22] The estimates presented below are qualitatively and largely quantitatively unchanged when the event study coefficients are estimated using a "stacked" event study approach with common treatment effects across cohorts (Sun and Abraham, 2021) as implemented by Novgorodsky and Setzler (2019). See Figure A-8. Figures A-9 to A-11 visualize the full observed treatment effect heterogeneity by plotting major-restriction-specific estimates of Equation 7 for several outcomes.
[23] This average decline is substantially larger than the estimated enrollment decline at Union College's economics department after they implemented a GPA restriction (Schmidt, 2021).
[24] The share of majors URM fell by 2.0-3.3 percentage points, depending on whether pre-transition or transition years are selected as a baseline (Table 6, and $0.25 \approx (-0.02/0.13) + (0.10)$). This and similar estimates below of the characteristics of major restriction 'compliers' – that is, students who would have declared the major if not for the restriction – require assuming that the major restriction did not impact the likelihood of major declaration of students who would otherwise have *not* declared the major. If the major restriction immediately encouraged positively-selected students to declare that major (perhaps believing that the restriction would increase the major's educational quality or postgraduate return), then these estimates could be overestimates of the true effect.
[25] Mathematics and Statistics courses are omitted from all majors' out-of-discipline, since those courses are often required by (and included in the GPA calculations of) nearly all restricted majors.
[26] This strong relationship can partly be explained by the correlation between in-discipline and out-of-discipline first-term normed GPA of 0.84, suggesting that GPA restrictions offer little scope for revealing field-specific comparative advantage.
[27] Figure A-12 presents estimates for the main outcomes by campus. The patterns described above – with newly-implemented major restrictions leading to arrested growth, increased average academic preparation among students, and decreased URM share in those majors – largely hold at the Berkeley, Santa Barbara, and Davis campuses. Interestingly, major restrictions have no observable

immediate effects at UC Santa Cruz, suggesting that that campus's restrictions were generally non-binding. Indeed, Figure A-16 shows that UCSC's restriction in economics was non- binding for its first seven years before tightening in 2008.

28 Some previous studies have proxied UC students' major intentions using the 'intended majors' reported on their undergraduate applications (e.g. Arcidiacono, Aucejo, and Hotz, 2016), but these self-reported intended majors are non-binding, can be strategically selected, and are not reported by about one-third of students (Bleemer, 2020). We focus instead on students' revealed preferences.

29 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.

30 We estimate the random forest 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, half of t 3 students are added to the training data.

31 About one-third of University of California students are transfer students from community colleges. Major restrictions are binding for both freshman and transfer students, and both are maintained in the estimation sample.

32 Figure A-13 shows that the full distributions of Mˆ overall and for students who declare the major slightly shift to the left over time, as departmental changes in introductory course sequences erode our capability of predicting students' intended majors, but the small magnitudes of the shift among both URM and non-URM students suggest little reason to expect these shifts to bias our baseline estimation.

33 In particular, we estimate

$$\widehat{M}_{im} = \zeta_m + \sum_{t=-6}^{6} \beta_{it} \mathbf{1}\{y_t + t = R_m\} + \epsilon_{im}$$

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 (matching the previous section). The standard errors are clustered by major and by student and assume that $\widehat{M}_{im}$ are observed without noise. Estimates of $\widehat{M}_{im}$ are unavailable for other majors either because gender and ethnicity data was unavailable or the majors were created too soon before the restriction.

34 Figure DD-2 shows 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 competition among female students (Ahn et al., 2019; Azmat, Calsamiglia, and Iriberri, 2020).

35 When estimating βit by gender or ethnicity, we also condition on the interaction between $\widehat{M}_{im}$ and the characteristic as well as characteristic-by-t fixed effects.

36 See Appendix A. Crosswalks between ACS majors and UC majors are available from the authors.

37 See Figure A-14 for separate β estimates by URM status.

38 The major premium gap appears to abruptly close again 4 years after the restriction is imposed, though panel (d) shows a sharp rise in URM students' relative academic performance that year as well; we interpret this as the result of growing mismeasurement of $\widehat{M}_{im}$, which at that point is estimating major intentions using the course-taking behavior of students at least eight years prior, and not evidence that the major restrictions' differential impact suddenly ceases after four years.

39 Figure DD-3 presents suggestive evidence of a similar relative flow of high-$\widehat{M}_{im}$ female students into lower- premium majors, though interpretation of those findings is challenged by the observed differential selection into having a high $\widehat{M}_{im}$ by gender following restrictions' implementation (Figure DD-2).

40 For restricted majors that had no observable students either three years before or three years after the restriction's implementation – prohibiting estimation of the mean $\widehat{M}_{ir}$ in Equation 11 – we replace $\widehat{M}_{ir}$ with M_{ir}, observed major attainment, scaled by 1.26, the average ratio between predicted and actual URM majors 1-3 years following restrictions' implementation.

41 While the presence of major restrictions precludes clear observation of major demand on our study campuses, recessions increase national demand for lucrative fields of study (Blom, Cadena, and Keys, 2021).

42 See Bleemer (2021) and Bleemer and Mehta (2021) for evidence on the efficiency of test-based admissions policies and major restriction policies, respectively.