AFFIRMATIVE ACTION, MISMATCH, AND ECONOMIC MOBILITY AFTER CALIFORNIA'S PROPOSITION 209*

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Proposition 209 banned race-based affirmative action at California public universities in 1998. Using a difference-in-differences research design and a newly constructed longitudinal database linking all 1994–2002 University of California applicants to their educational experiences and wages, I show that ending affirmative action caused underrepresented minority (URM) freshman applicants to cascade into lower-quality colleges. The "mismatch hypothesis" implies that this cascade would provide net educational benefits to URM applicants, but their degree attainment declined overall and in STEM fields, especially among less academically qualified applicants. URM applicants' average wages in their twenties and thirties subsequently declined, driven by declines among Hispanic applicants. These declines are not explained by URM students' performance or persistence in STEM course sequences, which were unchanged after Prop 209. Ending affirmative action also deterred thousands of qualified URM students from applying to any UC campus. Complementary regression discontinuity and institutional value-added analyses suggest that affirmative action's net educational and wage benefits

*Thanks especially to David Card, Rucker Johnson, and Jesse Rothstein, and to Peter Arcidiacono, Esteban Aucejo, Henry Brady, Christina Brown, Jonathan Holmes, V. Joseph Hotz, Enrico Moretti, Jack Mountjoy, Martha Olney, Marianne Page, Nina Roussille, Emmanuel Saez, Zoë Steier, and Christopher Walters, as well as conference participants at the NBER Economic Mobility Study Group and the 2019 WEAI Annual Conference for helpful comments. Frankie Lin, Sakthi Ponnuswamy, Sowgandhi Rayapudi, and Akcan Balkir provided excellent research assistance, and thanks to Tongshan Chang and Chris Furgiuele for help in obtaining and understanding the data. Thanks to John Douglass, Margaret Heisel, and especially Charles Masten for help in understanding the 1990s policy setting. The author acknowledges financial support from the National Academy of Education/Spencer Dissertation Fellowship, the Center for Studies in Higher Education, and the Stone Center on Wealth and Income Inequality. The author was employed in a research capacity by the University of California throughout the period in which this study was conducted. In June 2020, toward the end of the study period and after the study's release as a working paper, the UC Board of Regents endorsed a ballot proposition that would have effectively repealed Prop 209 if it had passed in November 2020. The release and findings of this study were not impacted by the Regents' stance. The conclusions of this research do not necessarily reflect the opinion or official position of the University of California, the California Employment Development Department, or the State of California. Any errors that remain are my own.

The Quarterly Journal of Economics (2022), 115–160. https://doi.org/10.1093/qje/qjab027.
Advance Access publication on September 1, 2021.

for URM applicants exceed its net costs for on-the-margin white and Asian applicants. *JEL Codes:* I24, J24, J31, H75.

Those who deny that preferences are not [sic] being given or that the granting of such preferences is without negative consequences do a great disservice to the need for finding reasonable solutions. Equally so, those who believe that social and economic equality of opportunity can be achieved merely by the passage of ballot initiatives, however justified the need might be, are misguided. The "heavy-lifting" to achieve a society of genuine inclusion and equality of opportunity merely begins with the removal of race-based decision-making.

—UC Regent Ward Connerly, 1995

I. Introduction

Educational attainment, income, wealth, and economic mobility exhibit racial disparities in the United States. Access to selective universities is a key determinant of economic success and intergenerational mobility (Chetty et al. 2020a). As a result, many selective universities provide admissions advantages to applicants from disadvantaged racial and ethnic groups. Proponents of affirmative action argue that it offsets applicant qualification gaps that result from systemically unequal educational opportunities (Johnson 2019). Detractors argue that affirmative action limits opportunity for Asian and white applicants and may have unintended consequences for targeted students. This study examines three questions at the basis of this disagreement. First, which students are targeted by affirmative action, and to what degree does affirmative action affect where those students go to college? Second, what are the short- and long-run effects of enrolling at a more selective university because of affirmative action? Finally, how are the net benefits and costs of affirmative action distributed across Asian, Black, Hispanic, and white university applicants?

Prior scholarship has arrived at conflicting conclusions about the value of enrolling at a more selective university because of access-oriented admissions policies like affirmative action. On the one hand, several recent studies have shown that applicants with test scores and grades at selective universities' minimum admissions thresholds are benefited by admission (e.g., Hoekstra 2009; Zimmerman 2014; Anelli 2019). Studies of affirmative action, however, have uncovered mixed evidence on student outcomes (Arcidiacono and Lovenheim 2016), with some finding support for the so-called mismatch hypothesis: that the lower-testing applicants targeted by affirmative action would benefit from enrolling at less selective universities, where they better "match" their peers' academic qualifications (Sowell 1972).

This study combines longitudinal administrative data with a difference-in-differences research design to estimate the impact of affirmative action on students' college quality, course performance, choice of major, degree attainment, and wages over 15 years. I construct a novel database of all 1994–2002 freshman applicants to the University of California (UC) system, which comprises all public research universities in the state, and link each applicant to nationwide university records and annual California wages. I compare the outcomes of Black and Hispanic UC applicants with those of academically comparable white and Asian applicants before and after California's Proposition 209, which ended affirmative action at UC in 1998. I also link the applicant data to institutional value-added statistics to measure Prop 209's effect on applicants' university quality, to California high school records to examine Prop 209's effect on UC application-sending, and to five UC campuses' student transcripts to estimate Prop 209's effects on performance and persistence in demanding courses. Finally, I use a regression discontinuity design to identify the value of being admitted to a selective public university for the on-the-margin white and Asian students likely to obtain greater university access after Prop 209.

I begin by documenting Prop 209's impact on admissions at UC's eight undergraduate campuses. Prop 209 curbed the large admissions advantages—some over 50 percentage points—provided by affirmative action to underrepresented minority (URM) UC applicants. As a result, UC's URM applicants cascaded into less selective colleges and universities: those with a high UC Academic Index (*AI*, a weighted average of high school grades and test scores) tended to flow from more selective UC campuses to less selective campuses and private universities, while those with lower *AI*s mostly flowed to less selective public colleges and universities. Overall, Prop 209 resulted in a net outflow of lower-income students from highly selective public universities.

How did less selective enrollment affect URM UC applicants? I estimate the average effect of Prop 209 using a difference-in-differences design estimated over the population of UC applicants. Each model estimates how URM applicant outcomes change after 1997 (the final year of affirmative action) relative to changes among non-URM applicants, with the second difference absorbing ethnicity-neutral enrollment trends in the 1990s.[1] High school fixed effects and *AI* covariates absorb spurious variation and observable selection bias into UC application. I also estimate effect heterogeneity by URM *AI* quartile and by URM ethnicity.

Implementing this model, I show that Prop 209 led URM UC applicants to enroll at relatively lower-quality colleges and universities on average, measured both by traditional metrics like graduation rate and by institutional value added. In contrast with the predictions of the mismatch hypothesis, URM UC applicants' average educational outcomes deteriorated after Prop 209: bachelor's degree attainment declined by 4.3 percentage points among URM applicants in the bottom *AI* quartile, and overall STEM and graduate degree attainment declined by 1.0 and 1.3 percentage points, respectively. Following these applicants into the labor market, I find that Prop 209 caused URM UC applicants to earn 5% lower average annual wages between ages 24 and 34, with larger proportional effects for lower *AI* applicants. The observed wage effects are driven by Hispanic applicants; despite parallel enrollment and degree attainment outcomes, I find no evidence of average wage deterioration among Black UC applicants after Prop 209.

These estimated effects are averaged across every URM UC applicant, many of whose enrollments were likely unchanged by the affirmative action ban. This implies that treatment effects for directly affected applicants were likely much larger. Given the magnitude of UC's applicant pool, these estimates imply that Prop 209 caused an aggregate decline in the number of URM Californians in their early thirties with 2014 wages over $100,000 by at least 3%. American Community Survey data confirm a 2010s pattern of relative wage deterioration among high-earning early career URM Californians.

The primary threat to this baseline research design is the possibility of sample selection bias arising from differential selection into UC application after Prop 209. Estimating a difference-in-differences model of the proportion of California public high school students who applied to UC by ethnicity and *AI* bin, I find

1. Non-URM applicants may not represent a traditional unaffected comparison group, since some likely "crowded into" more selective universities after Prop 209. I return to the question of non-URM applicant outcomes in Section VI, but the fact that non-URM applicants outnumber URM applicants by more than four to one in the applicant pool dilutes any "crowd-in" effects, implying that at least 80% of the observed differences are likely driven by changes in URM applicant outcomes.

that UC annually received about 250 fewer Black and 900 fewer Hispanic applications after Prop 209, almost 80% of whom would likely have been admitted to at least one UC campus. Although application deterrence could generate bias, I find that the baseline estimates are insensitive to a school-ethnicity-*AI* control function (following Card and Rothstein 2007) and other highly detailed socioeconomic and academic covariates.

The baseline research design does not separately identify the impact of Prop 209 on non-URM applicants' outcomes. Instead, I exploit a large discontinuity for non-URM admissions at UC Berkeley before Prop 209 to study the return to selective university access for on-the-margin non-URM applicants, many of whom may have been admitted if not for affirmative action. Using a regression discontinuity design, I find that students just below Berkeley's admissions threshold nevertheless ended up with similar educational and labor market outcomes after enrolling at other universities, though the confidence intervals cannot rule out positive treatment effects. This suggests that the value of selective public university access for on-the-margin non-URM students was small.

Next I turn to mechanisms explaining URM UC applicants' deteriorated educational outcomes after Prop 209. Several prior studies have suggested that URM students' STEM course performance and persistence would improve without affirmative action, which likely would have led to the opposite of Prop 209's effect on STEM degree completion (Loury and Garman 1993; Holzer and Neumark 2000; Arcidiacono, Aucejo, and Hotz 2016). However, while URM UC students earned lower grades and were less likely to persist along introductory STEM course sequences than their non-URM peers before Prop 209, these gaps are largely explained by students' prior academic opportunities and preparation, not their enrollment institution.[2] Prop 209 has no observable effect on students' STEM course performance and persistence, which do not appear to contribute to the effects of Prop 209 on students' educational and wage outcomes.

I conclude with a discussion of the efficiency of affirmative action. Two sets of evidence favor its allocative efficiency,

2. This study's examination of STEM course performance contributes to a literature interested in the production and composition of STEM graduates (e.g., Ehrenberg 2010; Sjoquist and Winters 2015; Denning and Turley 2017). This study estimates how student outcomes in specific STEM courses change under different policy regimes.

which in this case requires (to a first-order approximation) that the benefit of more selective university enrollment is greater for affirmative action's URM enrollees than for the non-URM students who would have enrolled in their place. First, the estimated return to UC Berkeley and UC Davis admission for on-the-margin non-URM students appears small, whereas URM applicants' estimated wage return to more selective enrollment before Prop 209 is large.[3] Second, that latter return exceeds the average observed change in institutional value added experienced by URM UC applicants, suggesting that the URM applicants affected by Prop 209 had received above-average returns to more selective university enrollment (as in Dale and Krueger 2014; Bleemer 2021b). This evidence suggests that affirmative action both promotes socioeconomic mobility among URM youths and improves higher education's allocative efficiency.

This study makes three main contributions. First, while previous studies have analyzed the intermediate effects of universities' affirmative action policies—sometimes coming to conflicting conclusions—they share common limitations. Several studies have exploited cross-state policy variation to estimate the educational effect of banning affirmative action, but out-of-state enrollment confounds identification of the policies' effects on affected students (Backes 2012; Hinrichs 2012). Others estimate models of applicant and university behavior to predict how affirmative action could affect student enrollment and outcomes but do not validate these predictions using actual policy variation (Arcidiacono 2005; Arcidiacono, Aucejo, and Hotz 2016; Kapor 2020). A third set of studies have analyzed administrative university data from before and after Prop 209, but limits on available covariates and outcomes have challenged attempts to separately identify the effect of affirmative action from compositional changes among UC's applicants and students (Arcidiacono et al. 2014).[4] This study augments previous research by implementing a quasi-experimental research design spanning all U.S. universities that identifies the individual-level effects of affirmative action and by analyzing new

3. Black, Denning, and Rothstein (2020) provide evidence against large returns to more selective university enrollment for the students who were "crowded out" of selective Texas universities by Texas Top Ten. However, Zimmerman (2019) shows that the largest returns to elite Chilean university enrollment accrue only to high-income students.

4. Bagde, Epple, and Taylor (2016) and Bertrand, Hanna, and Mullainathan (2010) show that Indian universities' caste-based affirmative action improves targeted students' grades and wage outcomes, respectively.

intermediate outcomes like university "value added," STEM performance and persistence, and graduate degree completion.

Second, this study causally links university quality to wage outcomes in the context of affirmative action, bridging the affirmative action literature with a literature identifying heterogeneity in the return to higher education (Dale and Krueger 2002; Arcidiacono 2004). Much of the affirmative action literature has focused on measuring mismatch (Arcidiacono and Lovenheim 2016), but my findings are inconsistent with the mismatch hypothesis at the mean.[5] On the other hand, while most studies of heterogeneous university returns focus on a relatively high-testing local margin (e.g., Hoekstra 2009; Anelli 2019), I estimate average returns to university quality across all URM UC applicants after an affirmative action ban. I also present regression discontinuity evidence highlighting the importance of applicants' counterfactual enrollments and heterogeneity in estimating the return to selective university enrollment.

Finally, I provide direct evidence that affirmative action has first-order implications for intergenerational mobility and socioeconomic gaps by ethnicity. A growing literature examines the mechanisms explaining opportunity gaps for lower-income and URM youths and the efficacy of available policies to narrow those gaps (e.g., Chetty, Hendren, and Katz 2016; Jackson, Johnson, and Persico 2016). I find little evidence that affirmative action narrows the Black-white mobility gap, which has received particular attention (Dobbie and Fryer 2011; Billings, Deming, and Rockoff 2014; Chetty et al. 2020b; Derenoncourt and Montialoux 2021) but find that it improved Black students' educational attainment and relatively increased (mostly lower-income) Hispanic youths' wages.

II. Background and Data

II.A. UC Admissions in the 1990s

The UC system is tasked by the 1960 Master Plan for Higher Education to educate roughly the top 12.5% of California public

5. Two studies of affirmative action mismatch also analyze the University of California in the 1990s (Arcidiacono et al. 2014; Arcidiacono, Aucejo, and Hotz 2016). Bleemer (2020) discusses the limitations of that previous research and the arguments of Sander and Taylor (2012) in the specific context of Prop 209 and reconciles their analysis with my baseline findings. Dillon and Smith (2020) and Barrow, Sartain, and de la Torre (2020) find evidence of test- and income-based mismatch at U.S. undergraduate institutions and elite high schools, respectively.

high school graduates. The system enrolled 137,000 undergraduates at eight campuses in 1999, with the campuses ranging in selectivity from the highly selective Berkeley and Los Angeles (UCLA) campuses (which admitted 35% of applicants with an average SAT score 1.5 std. dev. above mean) to the less selective Santa Cruz and Riverside campuses (with an 85% admission rate and SAT scores 0.5 std. dev. above mean). Ranking campuses by their admissions rates in the period, I refer to the Berkeley, UCLA, and San Diego campuses as more selective; the Santa Barbara, Irvine, and Davis campuses as selective; and the Santa Cruz and Riverside campuses as less selective. In 1999, California also had a 22-campus system of teaching-oriented universities—the California State University (CSU) system—and 114 two-year community colleges.

Affirmative action began at UC in 1964, the first year that the number of eligible applicants to UC Berkeley exceeded the number of available seats, and is now practiced by public universities in at least half of states (see Online Appendix A). The policy augmented UC's standard admissions protocol, which required that at least 50% of students be admitted solely based on their Academic Index (*AI*), a linear combination of high school GPA and SAT scores.[6] For example, archival documents from UC Berkeley (Online Appendix Figure A.3) show that it guaranteed admission to all applicants above an *AI* threshold (7,150), but set a lower threshold (6,500) for African American, American Indian, Chicano, and Latino URM applicants. Applications with *AI*s below their respective threshold were "read" by admissions personnel, giving them a variable likelihood of admission, while those with *AI*s below a second threshold (7,000 for non-URM applicants, below 6,000 for URM applicants) were mostly mechanically rejected.

Figure I summarizes the relative admissions likelihood of normal URM and non-URM applicants to each campus by *AI* in two-year increments from 1994 to 2001.[7] At the most selective

6. In particular, $AI = min(HSGPA, 4) \times 1{,}000 + SATI + SATIIs$. The index included both SAT I components (math and verbal) and three SAT II scores: writing, math, and a third of the student's choosing. All SAT components were scored out of 800, so the maximum *AI* was 8,000. Some campuses used variants of this formula.

7. Normal applicants exclude those without UC's minimum academic credentials and applicants to restricted programs like some engineering majors. Online Appendix B presents annual admissions likelihoods by *AI* at each campus for normal applicants.

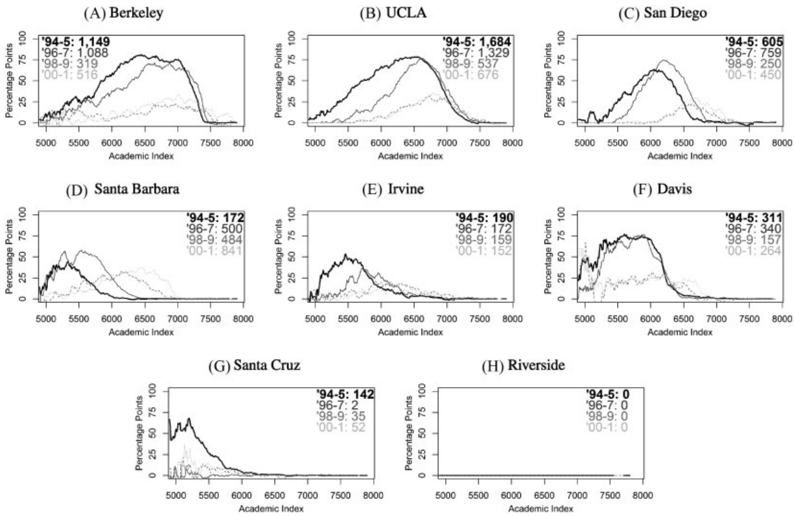

FIGURE I

Normal URM UC Applicants' Greater Likelihood of Admission by Campus, Year, and *AI*

The difference between the percent of URM applicants and the percent of non-URM applicants admitted to each campus by academic index (*AI*), in each of four two-year periods (1994–2001), with darker lines corresponding to earlier periods. The two later periods are after the implementation of Prop 209 ended UC's affirmative action policies. The displayed statistics show the total annual number of additional URM students admitted to each campus in each period based on their higher likelihood of admission, calculated as the sum of the products between the increased admissions likelihood and the number of URM applicants by year and *AI*. The sample is restricted to freshman fall California resident applicants who were "normal," in that they (i) were UC-eligible, which means that they satisfactorily completed the required high school coursework, and (ii) selected intended majors that did not have special admissions restrictions (e.g., engineering at some campuses). UC Riverside admitted all such applicants. URM includes Black, Chicano, Latino, and Native American applicants. *Source*: UC Corporate Student System.

Berkeley campus, for example, 1994–1995 URM applicants with *AI*s between 6,000 to 7,100 were 80 percentage points more likely to be admitted than same-*AI* non-URM applicants. The admissions advantage declines to 0 above $AI = 7{,}400$ because all such applicants were admitted. Seven of the eight UC campuses provided admissions advantages to URM applicants under affirmative action, with the advantage shifting to higher *AI* applicants over time as the campuses became more selective. UC Riverside admitted all normal UC applicants. The statistics presented in each panel show the empirical integrals under each curve by the contemporaneous *AI* distribution of each campus's URM applicants, estimating the excess number of annual URM admissions relative to simulated URM admissions under the non-URM *AI* admissions rule. Many campuses admitted hundreds of URM applicants annually by affirmative action.

Increasing political controversy around affirmative action culminated in the mid-1990s, when the policy was prohibited first by the UC Regents in July 1995 and then by a voter referendum in November 1996. While the original Regents policy (SP-1) was rescinded in 2001, Prop 209 has prohibited UC and other public California institutions from "discriminat[ing] against, or grant[ing] preferential treatment to, any individual or group on the basis of race, sex, color, ethnicity, or national origin" since the fall 1998 admission cohort.[8] Figure I shows that most campuses continued providing large admissions advantages to URM applicants in 1996 and 1997 (though some programs were curtailed), but those advantages shrank considerably in 1998.[9]

Starting in 1998, UC implemented outreach programs to increase enrollment from majority-URM high schools, but those programs wound down after 2001 with little evidence of success (UCOP 2003; Atkinson and Pelfrey 2004). Instead, UC's primary policy response to the end of affirmative action was its Eligibility in the Local Context top percent policy, which did not begin until 2001 (Bleemer 2021b).

II.B. Data

This study analyzes the effects of Prop 209 using four primary data sources. The first, collected contemporaneously for administrative use by the UC Office of the President, covers all 1994–2002 California-resident freshman applicants to any UC

8. Prop 209 also prohibited racial preferences in UC outreach and financial aid as well as affirmative action at the less selective CSU system. Prop 209 banned racial preferences in state hiring (Marion 2009) and graduate school admissions, though college-bound high school graduates shortly before and after 1998 all entered the labor market after 1998.

9. Online Appendix Figures A.4 and A.5 show that some UC campuses saw relative declines in URM admissions and enrollment in 1996, particularly at UCLA and the less selective UCs, but every UC campus saw sharp immediate declines in URM admission in 1998. The more selective UC campuses also saw sharp 1998 declines in URM enrollment.

campus.[10] Each record contains an applicant's high school, gender, ethnicity, parental education, parental occupations, and family income.[11] Academic preparation measures include SAT and ACT standardized test scores by component, SAT II scores, high school grade point averages, and the number of 12th-grade honors courses.[12] Application, admission, and enrollment indicators are available for each UC campus, as are degree attainment and major choice for UC enrollees.

The second data set, an extract from the National Student Clearinghouse's (NSC) StudentTracker database, contains enrollment and graduation records—covering nearly all U.S. two- and four-year colleges and universities—for all students in the UC application data set, linked by full name and birth date. Science, technology, engineering, and mathematics (STEM) majors are categorized by CIP code following the U. S. Department of Homeland Security (2016).[13] I define bachelor's (graduate) attainment as being within 6 (18) years of UC application. NSC data are available starting with the 1995 applicant cohort. Online Appendix D provides additional details.

Third, I observe UC applicants' quarterly 2000–2017 wages from the California Employment Development Department, linked by SSN.[14] Wages are unavailable for workers not covered by

10. About one-third of UC students transfer from community colleges rather than enrolling as freshmen. Because affirmative action was likely less effective for those applicants and because of limited data availability about those students' academic background (prohibiting selection correction on observables), transfer applicants are not directly analyzed in the present study, though freshman applicants may enroll at a community college and transfer to UC later.

11. Parental education is observed as an index of maximum parental education for up to two parents, from 1 (no high school) to 7 (graduate degree). Parental occupations are observed as one of 17 occupation codes each for two parents (or 289 total codes), including codes like Clerical, Laborer, and Professional as well as Homemaker, Retired, Other, or Deceased. Family income is not reported by about 15% of applicants.

12. Throughout the study period, each UC applicant was required to submit an SAT score and SAT II scores in writing, mathematics (1 or 2), and a third field of their choosing. Only 0.9% of applicants submitted ACT instead of SAT scores.

13. See Online Appendix Tables A.1 and A.2 for the most common STEM and non-STEM majors in the data. This definition generally aligns with that used by Arcidiacono, Aucejo, and Hotz (2016). Not all NSC majors have CIP codes; I assign each major to its modal CIP code (in the full observed NSC database) for categorization.

14. Social Security numbers on UC applications are not verified unless the student enrolls at a UC campus. Among enrollees, the verified Social Security

California unemployment insurance, including out-of-state, federal, and self-employed workers. Annual wages are measured as the sum of quarterly wages, CPI-adjusted to 2018, and winsorized at the top and bottom 1%. About 69% of UC applicants have positive covered wages in each of 6–16 years after UC application.

The fourth data set includes comprehensive student transcripts—including course enrollments and grades—for five UC campuses: Berkeley, Davis, Santa Barbara, Santa Cruz, and Riverside. The transcripts were obtained from campus Offices of the Registrar and are linked by name and birth date (Bleemer 2018).

Additional educational administrative data come from several sources. Universities' admissions rate, average SAT scores, and six-year graduation rates from IPEDS are linked to NSC institutions.[15] Aggregated data from the California Department of Education provide the annual number of graduates from each public high school by gender and ethnicity. Finally, a comprehensive College Board SAT-taker database covering public California high school students is linked by name and birth date to the UC applicant pool.

II.C. UC Descriptive Statistics

Table I provides descriptive statistics of UC applications, admissions, and enrollment for California-resident freshman applicants in three two-year cohorts: 1994–95, who applied before Prop 209's passage; 1996–97, who applied after the ban was approved but before its mandatory implementation; and 1998–99, following the ban. The presented statistics indicate a university system steadily increasing in reputation and selectivity throughout the 1990s, with increases in non-URM applications of 25% overall and 42% at the more selective campuses. Admissions rates consistently fell at all but the least selective Riverside campus, but increasing yield rates—the percent of admitted students who enrolled—stemmed the decline in the proportion of applicants who

number differs from that reported on their application in fewer than 0.25% of cases. All statistics estimated using EDD data were originally published as institutional research (Bleemer 2019b).

15. Average SAT scores are measured as the sum of the mean of universities' 25th and 75th math and verbal SAT percentiles. Admissions rates (and SAT scores) are fixed at 2006 (2000); graduation rates are contemporaneous. See https://nces.ed.gov/ipeds/.

TABLE I

DESCRIPTIVE STATISTICS OF 1990S UC ADMISSIONS BY ETHNICITY

	Application			Admission			Enrollment		
	1994–95	1996–97	1998–99	1994–95	1996–97	1998–99	1994–95	1996–97	1998–99
Panel A: Non-URM applicants									
Average number or percent of applicants									
More selective UCs	15,659	18,941	22,262	48.2	43.2	37.7	15.2	13.4	13.2
Selective UCs	12,705	14,383	17,358	77.3	72.7	63.2	19.0	19.2	16.7
Less selective UCs	7,251	7,827	10,098	83.7	85.5	84.5	15.7	18.4	17.5
All UCs	33,602	37,972	42,268	84.8	83.5	83.9	49.6	49.4	49.6
Average SAT score									
More selective UCs	1,224	1,227	1,237	1,320	1,335	1,339	1,277	1,294	1,299
Selective UCs	1,156	1,160	1,171	1,193	1,202	1,222	1,140	1,156	1,172
Less selective UCs	1,135	1,134	1,138	1,157	1,154	1,158	1,124	1,121	1,123
All UCs	1,182	1,187	1,194	1,207	1,212	1,216	1,196	1,208	1,217
Panel B: URM applicants									
Average number or percent of applicants									
More selective UCs	3,843	4,113	4,438	56.7	49.8	27.1	17.8	15.9	10.0
Selective UCs	2,889	2,970	3,356	78.2	74.5	57.2	18.0	16.5	15.6
Less selective UCs	2,229	2,200	2,757	81.6	79.2	76.2	17.9	16.4	17.9
All UCs	9,478	9,498	9,922	81.3	79.4	73.4	47	44.3	39.6
Average SAT score									
More selective UCs	1,054	1,068	1,083	1,131	1,158	1,194	1,102	1,125	1,149
Selective UCs	1,017	1,030	1,045	1,057	1,074	1,102	1,018	1,040	1,068
Less selective UCs	985	993	1,006	1,008	1,019	1,034	977	987	1,004
All UCs	1,025	1,039	1,048	1,054	1,071	1,081	1,052	1,071	1,077

Note. Count and mean average descriptive statistics of 1994–1999 California-resident freshman UC applicants who are or are not underrepresented minorities (URMs). Statistics are averaged across campuses: Berkeley, UCLA, and San Diego are more selective; Santa Barbara, Irvine, and Davis are selective; and Santa Cruz and Riverside are less selective. URM includes Black, Hispanic, and Native American applicants. SAT score was on the 1,600 scale. Percent admitted and percent enrolled are conditional on applying to that campus. Campus-specific statistics are presented in Online Appendix Table A.3. Descriptive statistics by ethnicity are available in Online Appendix Tables A.4 (Black and Hispanic) and A.5 (white and Asian). *Source*: UC Corporate Student System.

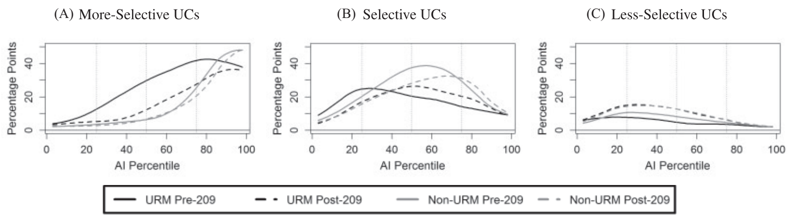

FIGURE II

UC Enrollment before and after Prop 209 by Ethnicity and *AI* Percentile

The percent of all UC applicants who first enroll at each set of UC campuses before (1996–97 cohorts) and after (1998–99 cohorts) the end of affirmative action, by URM status and by percentile of academic index (*AI*) measured among 1996–1999 URM UC applicants. First enrollment measured in NSC up to six years after UC application. Statistics are smoothed with a triangular kernel with bandwidth 15. *Source*: UC Corporate Student System and National Student Clearinghouse.

enrolled at each campus. The average SAT scores of most campuses' applicants also rose steadily, as did the average scores of students admitted to each campus.

Almost 20% of UC applicants were URM in 1997, and URM applicants' average SAT scores rose through the period, potentially reflecting deterrence among lower-testing URM students. Online Appendix Table A.4 presents separate descriptive statistics by URM ethnicity, showing that about 20% of URM UC applicants were Black and nearly all of the rest were Hispanic. Most campuses' URM admissions rates fell slightly in 1996, then sharply declined in 1998, matched by a sharp rise in URM admits' test scores. See Online Appendix C for additional details on URM UC admissions after Prop 209.

URM enrollment rates fell precipitously at UC's more selective campuses, slightly declined at the selective campuses, and slightly increased at the less selective campuses. The next section examines the URM cascade from more to less selective universities after Prop 209 in greater detail.

II.D. UC Applicants' University Enrollment

Figure II shows how URM UC applicants' decreased likelihood of UC admission after Prop 209 affected their UC enrollment. Enrollment shares are shown for the full *AI* distribution of URM UC applicants for the two cohorts before and after Prop 209 and are smoothed across percentiles. Before Prop 209, about 30% of median-*AI* URM applicants enrolled at the three more selective

UC campuses, while only about 3% of similar-*AI* non-URM applicants did so. After Prop 209, this gap largely closed, and URM applicants across the whole *AI* distribution became less likely to enroll at more selective UC campuses. Higher-*AI* URM applicants became more likely to enroll at the selective and less selective campuses—likely as a result of being rejected from the more selective campuses—while lower-*AI* URM applicants' selective UC enrollment declined. Meanwhile, the increasing selectivity of UC campuses also led to decreased enrollment likelihoods of all but the highest-*AI* non-URM applicants.

Figure III broadly summarizes how Prop 209 reshaped UC applicants' enrollment across the public and private sectors of U.S. higher education. Each panel plots the percentage point difference in enrollment likelihood before and after Prop 209 for URM and non-URM UC applicants at each URM *AI* percentile. URM applicants' relative likelihood of enrollment at Berkeley and UCLA declined across the *AI* spectrum.[16] UC San Diego exhibits a pattern common to California's other public universities: URM enrollment increased relative to non-URM enrollment for higher-*AI* applicants (70th–95th percentiles) and decreased for those with somewhat-lower *AI*s (20th–60th percentiles). The same pattern holds at lower *AI* bands for the selective and less selective UC campuses: for example, URM applicants at the 25th *AI* percentile became relatively less likely to enroll at the selective UC campuses but more likely to enroll at the less selective campuses. The teaching-oriented CSU system and California community colleges also absorbed some low-*AI* URM applicants (relative to changes among non-URM applicants).[17] Some high-*AI* URM applicants were absorbed by the highly selective Ivy+ universities, and middle-*AI* URM applicants became more likely to enroll at other private and out-of-state universities.

Overall, these patterns are consistent with a cascade of URM students from more to less selective institutions after Prop 209, with URM students from more selective schools enrolling at less

16. Online Appendix Figure A.6 shows that the URM students who exited Berkeley and UCLA following Prop 209 also came from much lower-income households than those who replaced them, generating a net enrollment shift at UC's more selective campuses from students in the bottom three income quartiles (fixed in 1996–97) to students in the top quartile.

17. The increase in community college enrollment and decrease in the number of students with no observed enrollment in NSC likely reflects community colleges' entry into NSC reporting; see Online Appendix D.

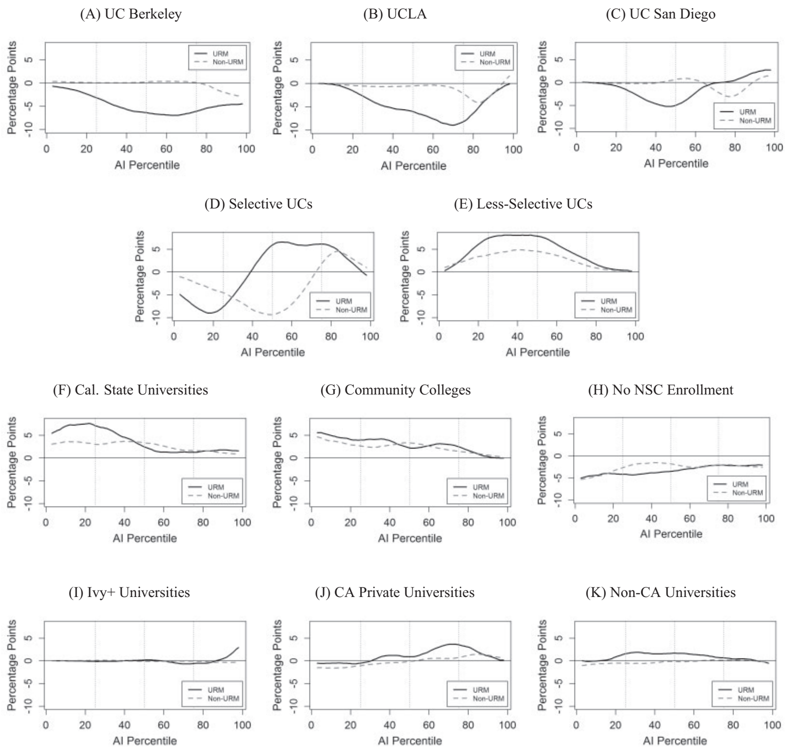

FIGURE III

Changes in University Enrollment after Prop 209 by Ethnicity and *AI* Percentile

Difference in percent of UC applicants who first enroll at each postsecondary institution(s) between 1998–99 and 1996–97, by URM status and by percentile of academic index (*AI*) measured among 1996–99 URM UC applicants. First enrollment measured in NSC up to six years after UC application; university groups partition possible enrollments. Statistics are smoothed with a triangular kernel with bandwidth 15. Ivy+ universities include the Ivy League, MIT, Stanford, and the University of Chicago; private and non-CA universities exclude those institutions. *Source:* UC Corporate Student System and National Student Clearinghouse.

selective universities where they replaced lower-*AI* URM students now rejected absent affirmative action.[18] This cascade explains why URM enrollment only declines at the more selective UC campuses (see Online Appendix Table A.6).

18. Online Appendix Figure A.7 shows that this cascade pattern is not reflected in applicants' UC application portfolios, implying that the observed patterns result from admissions rather than application decisions.

Prop 209's broad impact on where URM UC applicants go to college highlights the importance of analyzing California student outcomes across all U.S. institutions, since restricting to students at a smaller set of universities (like the UC system) will generate sample selection bias. The following section describes this study's baseline research design, which exploits longitudinal records for all California-resident UC applicants—following students wherever they enroll—to credibly estimate the effects of affirmative action on student outcomes.

III. Empirical Methodology

I estimate the impact of Prop 209 on URM UC applicants by comparing the change in URM applicant outcomes after Prop 209 to the change in outcomes of non-URM students with similar prior academic opportunity and preparation. Treating non-URM applicants as a comparison group differences out shifts in UC campuses' reputation and selectivity that shaped all UC applicant outcomes. However, non-URM UC applicants are not a traditional control group; Prop 209 likely increased some non-URM students' admissions likelihoods at some UC campuses so that those campuses could preserve their net enrollment despite the absence of affirmative action.[19] As a result, the estimates presented below identify the effect of Prop 209 on URM outcomes relative to its effect on non-URM outcomes. There are about four times as many non-URM UC applicants as URM applicants, so if UC campuses' net enrollment did not respond to Prop 209, every 1 percentage point average decrease in URM applicants' enrollment likelihood corresponds to almost a 0.25 percentage point average increase in non-URM applicants' enrollment likelihood.[20] If universities' treatment effects for on-the-margin URM and non-URM students

19. Figure III shows that there is no control group of URM UC applicants; Prop 209 shifted URM UC applicants' college enrollment at every *AI*, even among the highest-*AI* URM applicants. It also shows that the affected non-URM students tend to have higher *AI* than the URM students exiting those universities, suggesting that if the baseline results below reflected non-URM outcomes they would be driven by high-*AI* applicants. In fact, most of the estimated effects are driven by low-*AI* applicants.

20. Online Appendix Figure A.2 shows that annual growth in net California university enrollment appears unchanged by Prop 209, nor did Prop 209 observably affect the overall weighted-average institutional quality of that enrollment, with gains among non-URM students offsetting declines among URM students.

are similar, this implies that as much as 20% of the estimates described below could be explained by improved outcomes among non-URM students. I return to this argument in Section VI, presenting evidence that the benefits of Prop 209 to non-URM students likely explain a smaller share of the presented estimates.

To implement the proposed research design, I estimate difference-in-differences models of the form:

$$(1)\quad Y_{iy} = \alpha_{h_i} + \delta_y + \beta_0 URM_i + \sum_{t=1994}^{2002} \mathbb{1}_{\{t=y\}} \beta_y URM_i + \gamma X_{iy} + \epsilon_{iy},$$

where Y_{iy} is an outcome for California-resident freshman applicant i after they applied to UC in year y. I present results from two model specifications, both estimated by OLS. First, I restrict the sample to 1994–2002 applicants and set β_{1997} to 0, estimating the difference between URM and non-URM applicants' outcomes in the years before and after Prop 209. The β_{1996} coefficient can be interpreted as a placebo test that observed post-1998 effects are driven by Prop 209, while the β_{1994} and β_{1995} coefficients could possibly reflect changes in applicant outcomes as a result of SP-1 and Prop 209's passage (which led some UC campuses to begin phasing out affirmative action in 1996). To estimate the effect of Prop 209 more concisely, I also estimate a specification further restricting the sample to 1996–1999 applicants and estimating a single $\beta_{'98-99}$ term, averaging outcomes two years after 1998 relative to the two years prior. No UC campus implemented any other known changes in their admissions processes in this period.

Each model includes high school fixed effects α_{h_i}, which absorb spurious cross-school application and outcome variation, and the components used to construct UC's Academic Index (X_{iy}), which absorb variation in applicants' observed academic preparation.[21] Standard errors are robust.

I also estimate three model variants to better understand Prop 209's effects on student outcomes. First, I separately estimate the model by 1996–97 URM *AI* quartile to observe

21. That is, X_{iy} includes verbal and math SAT scores, high school GPA, SAT II writing score, SAT II math score (and an indicator for submitting a math 2 SAT II score), and a third SAT II score (along with indicators for which score was submitted). Fifteen percent of the sample is missing at least one test score (mostly the third SAT II); dummies are included for each missing value. I test models' sensitivity to covariate inclusion in Section V.

heterogeneous treatment effects for students with different prior academic opportunities and preparation. Second, because some UC campuses began phasing out affirmative action in 1996, I replace the model's 1996–97 preperiod with 1994–95 and reestimate post-1998 outcomes relative to those earlier years. Finally, I interact β_0 and β_y with indicators for whether the student is Black or Hispanic (omitting Native American students because of sample size), identifying separate coefficients to estimate heterogeneity in Prop 209's effect by URM ethnicity.

It remains possible that the β_y estimates reflect sample selection bias resulting from the impact of Prop 209 on the composition of UC applicants, since a nonrandom selection of URM applicants may have been discouraged from UC application by their decreased likelihood of admission. I quantify the degree of Prop 209's URM application deterrence and test the model's sensitivity to alternative specifications in Section V.

IV. The Effect of Affirmative Action on Student Outcomes

Figure IV visualizes the effect of Prop 209 on URM UC applicants with estimates of β_y from equation (1) for a sequence of enrollment, educational attainment, and labor market outcomes, all estimated relative to 1997. The subsections discuss the measured outcomes in turn. Given that many URM applicants' undergraduate enrollment remained unchanged by Prop 209, the presented reduced-form coefficients likely underestimate affected students' treatment effect of enrolling at less selective universities after the affirmative action ban.

IV.A. Institutional Quality

Prop 209 caused URM UC applicants to be 7.6 percentage points less likely to enroll at the more selective UC campuses—particularly driven by the second and third URM *AI* quartiles—and led to small corresponding enrollment increases across the spectrum of other public and private higher education institutions.[22] Prop 209 led to larger relative enrollment declines at the

22. See Online Appendix Table A.7. The empirical integral of URM students' changed enrollment at each UC campus by *AI* between 1995 and 1998–99—over the 1998–99 distribution of URM UC applicants—provides a lower-bar estimate for the number of URM students who enter and exit each campus as a result of Prop 209. Online Appendix Table A.9 shows that at least 1,200 URM applicants

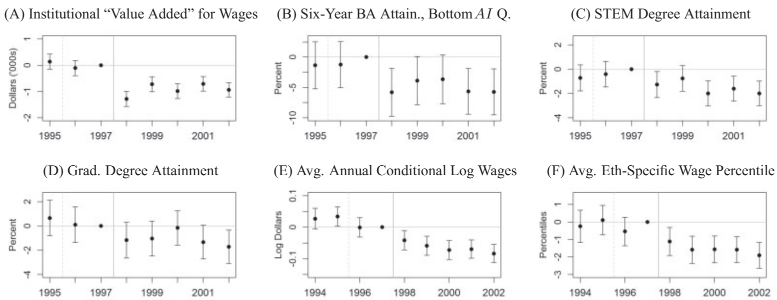

FIGURE IV

Annual Difference-in-Differences Estimates of URM UC Applicants' Outcomes after Prop 209

OLS difference-in-differences coefficient estimates of equation (1), the change in URM UC applicant outcomes relative to non-URM applicants, compared with the 1997 baseline. For details on outcomes A–E, see notes to Tables II (with institutional value added estimated following Chetty et al. 2020a), III, and IV. Panel F's outcome is defined as the average annual ethnicity-specific wage percentile between 6 and 16 years after UC application, omitting zero-wage years; percentiles are defined relative to the empirical distribution of wages earned in that year by same-ethnicity (URM, Asian, or white/other) college-educated California ACS respondents born between 1974 and 1978, few of whom were directly impacted in university enrollment by Prop 209. Models include high school fixed effects, ethnicity indicators, and the components of UC's Academic Index (see note 21); 1994 NSC data are omitted. Panel B restricts the sample to the bottom *AI* quartile as measured among 1996–97 URM UC applicants. Annual wages are CPI-adjusted to 2018 and winsorized at the top and bottom 1%. Bars show robust 95% confidence intervals. *Source*: UC Corporate Student System, National Student Clearinghouse, California Employment Development Department, and the American Community Survey (Ruggles et al. 2018).

more selective UC campuses for Black applicants, with the top *AI* quartile of Black applicants facing a 15 percentage point enrollment decline (see Online Appendix Table A.8).

I summarize these changes in university enrollment quality by characterizing each institution in two ways: (i) using traditional measures of university quality like selectivity and graduation rate, and (ii) using a set of novel value-added (VA) statistics, which estimate each institution's average treatment effects on their students' degree attainment and average wages between ages 30 and 34. I estimate the value-added statistics using fixed effect OLS regression over the 1995–1997 sample of UC applicants

exited UC campuses—with more than 800 exiting Berkeley and UCLA—and 800 entered UC campuses after Prop 209.

matched to their first enrollment institution, absorbing observable selection across institutions using either students' UC application and admission portfolios (following Mountjoy and Hickman 2020; MH) or ethnicity indicators and fifth-order polynomials in SAT score and family income (following Chetty et al. 2020a; CFSTY).[23] Online Appendix I provides methodological details and the estimated value-added statistics.

Table II presents difference-in-differences estimates of how Prop 209 affected URM UC applicants' quality of enrollment institution. The first row shows that before Prop 209, URM students tended to enroll at higher-quality institutions—as measured by lower admissions rates, higher average SAT scores and graduation rates, and higher estimated value added—than academically comparable non-URM UC applicants. The second row shows that Prop 209 caused URM UC applicants to enroll at less selective universities with lower average SAT scores and graduation rates after 1998, with larger observed institutional declines among lower-*AI* applicants. Those institutions are also estimated to have lower average value added: Prop 209 caused URM UC applicants to enroll at institutions that (on average) lead their students to lower likelihoods of bachelor's degree attainment by 0.5–0.9 percentage points and whose graduates earn \$400–\$900 lower annual early thirties wages, with smaller value added declines among high-*AI* URM applicants. The first panel of Figure IV shows that the institutions where URM UC applicants enrolled remained relatively steady in terms of their CFSTY early thirties annual wage value added between 1995 and 1997, but sharply and persistently declined by almost \$1,000 after 1998. In summary, Prop 209 caused URM UC applicants to enroll at lower-quality colleges and universities.

IV.B. Degree Attainment

Next I turn to Prop 209's effects on URM UC applicants' educational outcomes: whether they earned a bachelor's degree, an

23. I do not shrink the value-added statistics, and both sets of covariates likely fail to fully absorb selection bias across universities. Given students' positive selection across institutional value added and that most URM students enroll at lower-VA institutions following Prop 209, both of these factors likely lead toward overestimation of the VA decline following Prop 209.

TABLE II

DIFFERENCE-IN-DIFFERENCES ESTIMATES OF URM UC APPLICANTS' POST-1998 UNIVERSITY QUALITY

	First four-year institution			First institution value added			
				MH VA		CFSTY VA	
	Adm. rate (%)	Avg. SAT	6-yr. BA rate (%)	BA (%)	Earn ($)	BA (%)	Earn ($)
Panel A: Difference-in-differences coefficients							
URM (β_0)	−7.3	37.1	3.5	2.0	1,896	2.8	2,862
	(0.2)	(1.0)	(0.1)	(0.1)	(75)	(0.1)	(84)
URM × Prop 209 ($\beta_{'98-99}$)	3.6	−19.7	−1.7	−0.6	−384	−1.0	−922
	(0.2)	(1.3)	(0.2)	(0.2)	(93)	(0.2)	(105)
$\bar{Y}$	51.1	1,187.6	68.2				
Obs.	173,958	171,565	169,945	177,365	173,878	176,092	173,591
Panel B: Estimates of URM × Prop 209 ($\beta_{'98-99}$) by *AI* quartile							
Bottom quartile	1.8	−25.5	−3.3	−1.6	−638	−1.9	−796
	(0.6)	(3.7)	(0.6)	(0.4)	(214)	(0.5)	(246)
Second quartile	5.2	−28.7	−3.0	−0.5	−618	−1.3	−1,547
	(0.5)	(3.0)	(0.5)	(0.4)	(197)	(0.4)	(237)
Third quartile	5.6	−21.1	−1.0	0.1	−374	−0.4	−1,273
	(0.5)	(2.7)	(0.4)	(0.3)	(182)	(0.3)	(218)
Top quartile	2.0	−7.4	−0.7	−0.8	−157	−1.0	−480
	(0.4)	(2.4)	(0.3)	(0.3)	(224)	(0.3)	(233)

Note. Estimates of β_0 and $\beta_{'98-99}$ from equation (1), a difference-in-differences model of 1996–1999 URM UC freshman California-resident applicants' outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. Outcomes defined as characteristics of the first four-year university or the first two- or four-year institution at which the applicant enrolled within six years of high school graduation as measured in the NSC. Models include high school fixed effects and the components of UC's Academic Index (see note 21). Academic Index (*AI*) is defined in note 6; models by *AI* quartile are estimated independently, with quartiles defined by the *AI* distribution of 1996–97 URM UC applicants. IPEDS data (first three columns) are linked to NSC by OPE ID; admission rate and average SAT score (which is averaged across the available 25th and 75th math and verbal score percentiles) are fixed by institution in 2001, the earliest observed year, while six-year graduation rate is contemporaneous. Robust standard errors are in parentheses. Value-added (VA) measures are estimated by regressing six-year BA attainment or average conditional wages 12 to 16 years after UC application, when most applicants are in their early thirties, on college indicators, year fixed effects, and either MH indicators for each applicant's set of UC campus applications and admissions (following Mountjoy and Hickman 2020) or CFSTY ethnicity indicators and quintics in SAT score and family income (following Chetty et al. 2020a), estimated over the 1995–1997 UC applicant pool. *Source:* UC Corporate Student System, National Student Clearinghouse, the California Employment Development Department, and the Integrated Postsecondary Education Data System (IPEDS).

undergraduate STEM degree, and/or a graduate degree.[24] Given that Prop 209 caused the average URM UC applicant to enroll

24. I define undergraduate degree attainment using the union of UC and NSC data to augment imperfect NSC records from UC Santa Cruz; see Online Appendix D. This may upwardly bias the resulting estimates, since URM students are less likely to enroll at UC campuses following Prop 209. Estimates for each separate data source are presented in Online Appendix Table A.12; estimates are

TABLE III
DIFFERENCE-IN-DIFFERENCES ESTIMATES OF URM UC APPLICANTS' POST-1998 EDUCATIONAL OUTCOMES

	Earn bach. deg. (%)		Earn STEM deg. (%)		Earn graduate deg. (%)		
	4 years	6 years	Uncondit.	Condit.	All	STEM	JD
Panel A: Difference-in-differences coefficients							
URM	− 1.90	− 2.61	0.46	0.44	4.83	0.60	0.92
	(0.41)	(0.40)	(0.31)	(0.41)	(0.42)	(0.17)	(0.19)
URM × Prop 209	− 0.85	− 0.71	− 0.98	− 0.65	− 1.31	− 0.58	− 0.21
	(0.51)	(0.50)	(0.38)	(0.51)	(0.53)	(0.21)	(0.22)
$\bar{Y}$	47.8	74.6	22.2	27.1	36.0	5.4	4.9
Obs.	199,321	199,321	199,321	148,771	199,321	199,321	199,321
Panel B: Estimates of URM × Prop 209 ($\beta_{\cdot 98-99}$) by *AI* quartile							
Bottom quartile	− 2.09	− 4.25	− 1.23	− 1.42	− 2.77	− 0.86	− 0.08
	(1.21)	(1.44)	(0.65)	(1.08)	(1.25)	(0.33)	(0.32)
Second quartile	0.55	− 0.52	− 1.05	− 0.44	− 1.11	0.34	− 0.65
	(1.23)	(1.22)	(0.80)	(1.03)	(1.21)	(0.37)	(0.42)
Third quartile	0.98	1.22	− 0.76	− 0.82	− 1.26	− 0.53	− 0.68
	(1.19)	(1.05)	(0.89)	(1.07)	(1.16)	(0.42)	(0.48)
Top quartile	− 0.71	− 0.03	0.81	0.14	− 0.14	− 0.32	− 0.24
	(1.10)	(0.88)	(0.96)	(1.09)	(1.13)	(0.56)	(0.61)

Note. Estimates of β_0 and $\beta_{\cdot 98-99}$ from equation (1), an OLS difference-in-differences model of 1996–1999 URM UC freshman California-resident applicants' educational outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. Outcomes defined as having earned a bachelor's degree in five or six years, having earned a bachelor's degree in a STEM field (unconditional or conditional on six-year degree attainment), or having ever earned a graduate degree (any, JD, or MD), all as measured in the union of UC administrative records and the NSC. Models include high school fixed effects and the components of UC's Academic Index (see note 21). Academic Index (*AI*) is defined in note 6; models by *AI* quartile are estimated independently, with quartiles defined by the *AI* distribution of 1996–97 URM UC applicants. Robust standard errors are in parentheses. *Source*: UC Corporate Student System and National Student Clearinghouse.

at a lower-quality university more similar to their academically comparable non-URM peers' institutions, the mismatch hypothesis entails that URM applicants' outcomes will improve after Prop 209. Figure IV presents estimates from equation (1) for six-year BA attainment among bottom-*AI*-quartile applicants, unconditional STEM degree attainment, and graduate degree attainment, instead showing that all three abruptly and persistently decline in 1998 following Prop 209.

Table III provides additional details on the impact of Prop 209 on URM UC applicants' degree attainment. The first two columns show that URM UC applicants were already less likely to earn bachelor's degrees than academically comparable non-URM

somewhat more negative in NSC data and less negative in UC data among UC enrollees.

applicants before Prop 209, and if anything became even less likely to earn degrees after affirmative action was eliminated, with a 95% confidence interval of −1.69 to 0.27 percentage point change in average six-year degree attainment.[25] This effect is wholly driven by the bottom *AI* quartile of URM applicants, whose enrollment was shown above to largely flow from the more selective and selective UC campuses to less selective public and private California universities.[26]

The third and fourth columns of Table III show that URM applicants may have become less likely to earn STEM degrees conditional on earning a college degree (95% confidence interval −1.65 to 0.35 percentage points).[27] In combination with the decline in overall degree attainment, this provides strong evidence for Prop 209 causing a decline in unconditional STEM degree attainment by 1.0 percentage point (std. err. 0.4). Online Appendix Table A.13 presents major-specific estimates of changes in URM UC applicants' fields of study; the fields with largest increases after 1998 are biology (0.62 percentage points) and miscellaneous humanities fields (0.30), while those with the largest decreases are economics (−0.39), history (−0.32), and mathematics (−0.29), suggesting substantial heterogeneity between and within disciplines.

The last three columns of Table III show the relative impact of Prop 209 on URM students' likelihood of earning a graduate degree. Graduate degrees tend to offer large labor market returns (Altonji, Arcidiacono, and Maurel 2016; Altonji and Zhong 2021) and may represent an important benefit to more selective university enrollment. URM applicants became 1.3 percentage points

25. These estimates contrast with Arcidiacono et al. (2014), whose Table 3 suggests that Prop 209 increased URM UC graduation rates. Bleemer (2020) shows that those findings reflect selection bias on unobserved applicant characteristics: replacing the highly censored SAT score and high school GPA covariates available in their data with continuous measures of the same metrics fully attenuates the observed effect.

26. Applicants' changed degree attainment is less than half of the change in the six-year graduation rates of the institutions where they enroll, a lower ratio than those estimated by Cohodes and Goodman (2014) and Bleemer (2021b) in other contexts. This suggests that the degree attainment of students targeted by affirmative action was relatively less sensitive to enrollment change. The bottom *AI* quartile had an estimated ratio closer to 1 (as in those other studies).

27. This finding contrasts with a number of previous studies that show that increased university selectivity tends to decrease students' likelihood of earning STEM degrees along different margins (Arcidiacono, Aucejo, and Hotz 2016; Mountjoy and Hickman 2020; Bleemer 2021b). I further analyze Prop 209's effect on UC enrollees' performance and persistence in STEM courses in Section VII.

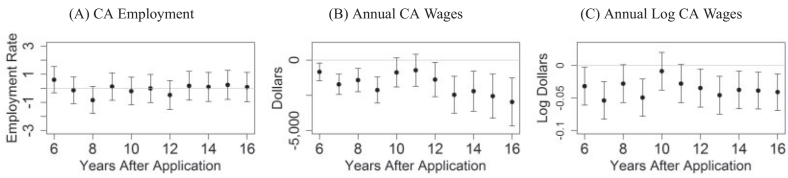

FIGURE V

Annual Difference-in-Differences Estimates of URM UC Applicants' Post-1998 Wage Outcomes

Estimates of $\beta_{'98-99}$ from equation (1), an OLS difference-in-differences model of 1996–1999 URM UC freshman California-resident applicants' employment outcomes compared to non-URM outcomes after Prop 209. Outcomes are defined as nonzero California wages (CA Employment) and California wages in dollars and log-dollars (omitting zeros) as measured in the California Employment Development Department database, which includes employment covered by California unemployment insurance. Coefficients in each year after UC application are estimated independently. Models include high school fixed effects and the components of UC's Academic Index (see note 21). Academic Index (*AI*) is defined in note 6. Annual wages are CPI-adjusted to 2018 and winsorized at the top and bottom 1%. Robust 95% confidence intervals are shown. Online Appendix Figure A.8 presents separate estimates for Black and Hispanic applicants. *Source*: UC Corporate Student System and the California Employment Development Department.

(std. err. 0.5) less likely to earn graduate degrees after Prop 209 relative to academically comparable non-URM applicants, with particularly large declines among lower-*AI* applicants. Almost half of this decline can be explained by a decline in STEM-oriented masters and doctoral degrees, for which attainment declines 0.58 percentage points (std. err. 0.21). There is only weak evidence of a decline in law degree attainment, and no such evidence for medical degrees.

IV.C. Employment and Wages

Finally, I turn to the effect of Prop 209 on URM UC applicants' labor market outcomes. Figure V shows estimates of $\beta_{'98-99}$ annually estimated for each specified outcome 6–16 years after UC application (when most applicants were age 34). The first panel shows that Prop 209 had no net effect on URM UC applicants' California labor market participation; 69% of applicants earned covered California wages annually before and after Prop 209.[28] Among wage-earning UC applicants, however, Prop 209

28. Online Appendix Figure A.9 shows that California labor market participation is unchanged after Prop 209 for all four *AI* quartiles of URM applicants. Prop

caused URM workers' wages to persistently decline by an average of $1,800 (0.05 log points) or $2,400 (0.04 log points) in their early thirties. As late as age 34, there is no evidence that the average wages of URM applicants affected by Prop 209 recover to their earlier levels. Online Appendix Table A.14 shows that these wage declines are proportionally larger for lower-*AI* URM applicants, who also faced the greatest educational deterioration.

The last two panels of Figure IV present the dynamics of URM UC applicants' wages in the years before and after Prop 209. Panel E shows estimated β_y coefficients for the average of observed log wages 6–16 years after UC application. URM applicants' wages sharply decline between 1997 and 1998, reflecting the impact of Prop 209, but there is also evidence of a persistent relative trend of declining URM UC applicants' wages throughout the period. This trend is likely the result of ethnicity-specific wage dynamics in the California labor market, with URM workers' wage distribution potentially declining relative to the non-URM distribution as a result of rising inequality in the state (Juhn, Murphy, and Pierce 1991).

Following the insight of that study, I account for these wage dynamics by replacing applicants' wages with their percentile in the contemporaneous ACS wage distribution of same-ethnicity college-educated California workers born between 1974 and 1978, most of whom were already in college prior to Prop 209's 1998 implementation. Panel F shows that the resulting percentiles are unchanging in the periods either before or after Prop 209, successfully removing the time trend, with an approximately 1 percentage point decline observed between 1997 and 1998 caused by Prop 209. On average, a 1 percentile change in the 2001–2017 URM wage distributions corresponds to $1,940, closely matching the estimated decline in URM UC applicants' wages after Prop 209 shown in Table IV and suggesting that the baseline wage estimates reliably capture the effect of Prop 209.

I examine the wage estimates' sensitivity to alternative parallel trends assumptions using the method of Rambachan and Roth (2020), who provide robust confidence intervals for

209 could have either increased or decreased URM applicants' likelihood of covered California employment: less selective university enrollment likely decreases applicants' likelihood of seeking employment outside the state (since the credential is more geographically specific), but increased out-of-state enrollment might have led to out-of-state employment.

TABLE IV
DIFFERENCE-IN-DIFFERENCES ESTIMATES OF URM UC APPLICANTS' POST-1998 CA WAGE OUTCOMES

	Average 6–16 years after UC app.				Average 12–16 years after UC app.			
	# Years emp.	Total wages ($)	Log wages	# > $100K wages	# Years emp.	Total wages	Log wages ($)	# > $100K wages
Panel A: Difference-in-differences coefficients								
URM	0.09	−159	0.01	− 0.06	0.05	−807	− 0.00	− 0.03
	(0.04)	(359)	(0.01)	(0.02)	(0.02)	(531)	(0.01)	(0.01)
URM × Prop 209	− 0.00	−1,822	− 0.05	− 0.08	0.00	−2,382	− 0.04	− 0.07
	(0.04)	(438)	(0.01)	(0.03)	(0.02)	(639)	(0.01)	(0.02)
$\bar{Y}$	7.55	60,888	10.69	1.48	3.30	79,064	10.89	1.01
Obs.	199,321	178,156	178,156	199,321	199,321	152,977	152,977	199,321
Panel B: Estimates with separate coefficients for Black and Hispanic applicants								
Black	− 0.60	−2,004	− 0.08	− 0.16	− 0.27	−1,903	− 0.09	− 0.09
	(0.07)	(645)	(0.02)	(0.03)	(0.04)	(948)	(0.02)	(0.02)
Hispanic	0.38	596	0.05	− 0.02	0.19	−300	0.03	− 0.01
	(0.04)	(403)	(0.01)	(0.02)	(0.02)	(595)	(0.01)	(0.02)
Black × Prop 209	0.03	−479	− 0.03	− 0.01	0.02	−581	− 0.03	− 0.02
	(0.09)	(856)	(0.02)	(0.05)	(0.05)	(1,259)	(0.03)	(0.03)
Hispanic × Prop 209	− 0.04	−2,300	− 0.05	− 0.12	− 0.01	−3,000	− 0.05	− 0.09
	(0.05)	(482)	(0.01)	(0.03)	(0.03)	(699)	(0.02)	(0.02)
$\bar{Y}$	7.56	60,939	10.69	1.48	3.30	79,136	10.89	1.01
Obs.	197,804	176,825	176,825	197,804	197,804	151,854	151,854	197,804

Note. Estimates of β_0 and $\beta_{'98-99}$ from equation (1), an OLS difference-in-differences model of 1996–1999 URM UC freshman California-resident applicants' wage outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. Panel B interacts the coefficients with Black and Hispanic indicators to separately estimate outcomes for each group; Native American applicants are omitted. Outcomes are defined as the number of years of nonzero California wages, average wages, and log wages across years with nonzero wages, and number of years with wages above $100,000, among the years 6–16 or 12–16 years after initial UC application. Outcomes are measured in the California Employment Development Department database, which includes employment covered by California unemployment insurance. Models include high school fixed effects and the components of UC's Academic Index (see note 21). Academic Index (*AI*) is defined in note 6; models by *AI* quartile are estimated independently, with quartiles defined by the *AI* distribution of 1996–97 URM UC applicants. Annual wages are CPI-adjusted to 2018 and winsorized at the top and bottom 1%. Robust standard errors are in parentheses. *Source*: UC Corporate Student System and the California Employment Development Department.

difference-in-differences statistics in the presence of bounded group-specific trends. Online Appendix Figure A.10 shows that the wage estimates presented in Figure IV, Panel E are sensitive to alternative assumptions, but that the wage percentile estimates in Panel F are robust to the assumption of annual differential trends of up to almost 0.15 percentile per year. I also find that the pretrend persists if the ACS wage distribution is fixed in a given year, implying that ethnicity-specific wage dynamics, not the form of the percentile transformation, explain the resulting parallel trends (Online Appendix Figure A.11).

Table IV summarizes the changes in URM UC applicants' wages following Prop 209, showing that academically comparable URM and non-URM workers earned similar wages before Prop 209 but diverged afterward. The second panel shows striking evidence of heterogeneity across URM students: while the wages of Hispanic students sharply declined following Prop 209 relative to academically comparable non-URM applicants, there is little such evidence for Black applicants (though their smaller sample size results in larger standard errors).[29] This widens a previously existing gap between the two groups, with Black applicants already earning lower average wages than academically comparable Hispanic students (who also earn somewhat higher wages than academically comparable non-URM applicants). Figure VI contextualizes this finding: while Black UC applicants faced similar or larger declines in university quality and educational outcomes than Hispanic UC applicants after Prop 209, and Hispanic UC applicants' wage outcomes deteriorated after 1998, there was no observable parallel decline among Black UC applicants. This suggests that while UC's affirmative action provided long-run wage returns to Hispanic students, its average labor market benefits to Black Californians may have been small, although this finding is tempered by Black applicants' wider confidence intervals and the unavailability of a Black-specific ACS wage distribution (due to small sample size).

While Prop 209 caused a small number of mostly Black URM UC applicants to enroll at out-of-state Ivy+ institutions, the effect of their exit from California on the presented wage statistics can be narrowly bounded. Consider, for example, the number of years in which URM applicants earn at least \$100,000 in the 6–16 years after UC application. Observationally, URM Ivy+ enrollees are about 15 percentage points less likely than other top-*AI*-quartile applicants to work in California annually, and almost one-third of URM Ivy+ enrollees who work in California earn over \$100,000 between 6 and 16 years after UC application. Given the 0.5 (1.0) percentage point increase in Ivy+ enrollment among URM (Black) UC applicants after Prop 209, this implies an expected decline in the number of years earning over \$100,000 of about 0.003 (0.005), small changes relative to the 0.08 fewer high-earning years among URM applicants and the 0.11-year gap between the estimated

29. Estimating independent effects of Prop 209 on Black and Hispanic outcomes (e.g., dropping non-Black URM applicants to estimate the effect on Black applicants) does not change the presented results.

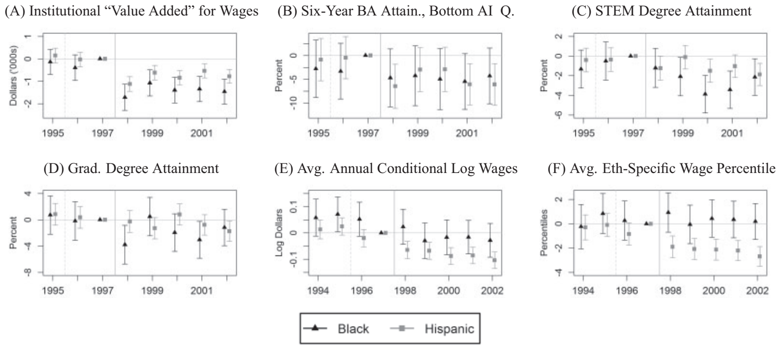

FIGURE VI

Annual Difference-in-Differences Estimates of URM UC Applicant Outcomes by Ethnicity

OLS difference-in-differences coefficient estimates of an extension of equation (1) interacting β_t with Black and Hispanic indicators, estimating the change in Black and Hispanic UC applicant outcomes relative to non-URM applicants compared to the 1997 baseline. For details on outcomes A–E, see notes to Tables II, III, and IV; institutional value added is estimated following Chetty et al. (2020a). Panel F's outcome is defined as applicants' average annual ethnicity-specific wage percentile between 6 and 16 years after UC application, omitting zero-wage years; percentiles are defined relative to the empirical distribution of wages earned in that year by same-ethnicity (URM, Asian, or white/other) college-educated California ACS respondents born between 1974 and 1978, few of whom were directly affected in university enrollment by Prop 209. Models include high school fixed effects, ethnicity indicators, and the components of UC's Academic Index (see note 21); 1994 NSC data are omitted. Panel B restricts the sample to the bottom *AI* quartile as measured among 1996–97 URM UC applicants. Native American applicants are omitted. Bars show robust 95% confidence intervals. *Source*: UC Corporate Student System, National Student Clearinghouse, California Employment Development Department, and the American Community Survey (Ruggles et al. 2018).

effects of Prop 209 on Black and Hispanic applicants reported in Table IV.

1. *Contextualizing Prop 209's Labor Market Impact.* Although UC does not educate enough of the California workforce for its admissions policies to shift most moments of the state's aggregate wage distribution, the high wages earned by its graduates imply that its policies may meaningfully affect the composition of California's high-earning workers. About 56,000 URM students applied to UC between 1998 and 2002. Compared to a 1996–97 baseline, the difference-in-differences estimates imply that Prop 209 caused each of those applicants to become about

1.3 percentage points less likely to earn at least $100,000 a year in California in 2014, 12 to 16 years after college application, though some of that decline may reflect secular ethnicity-specific wage dynamics in California.[30] This implies a decline in the number of high-earning URM Californians by over 700. American Community Survey estimates show that there were 27,000 URM Californians earning over $100,000 in 2014, implying that Prop 209 caused a decline in the number of such workers among UC applicants by about 3%.[31] Given that 30-to-34 URM workers made up 46% of the 2010 California workforce but only 14% of earners over $100,000, this implies that affirmative action had been meaningfully mitigating inequality until Prop 209.

Online Appendix Figure A.13 shows that the fraction of early and mid-thirties URM Californians earning wages above $100,000 indeed disproportionately declined in the years that those cohorts would have lost selective university access as a result of Prop 209.[32] For example, relative to a 2010 baseline, URM Californians between ages 33 and 37 became about 10% less likely to earn over $100,000 between 2012 (when they all would have enrolled at university before Prop 209) and 2017 (when they all would have enrolled after Prop 209). Members of several comparison groups—including slightly older URM Californians, similar-aged URM non-Californians, and similar-aged non-URM Californians—all became slightly more likely to earn over $100,000 over the period. This suggests that the baseline estimates' focus on UC applicants may yield an underestimate of the aggregate labor market effect of Prop 209 for high earners, with further declines likely coming from two groups: (i) URM non-UC applicants who could have become less likely to earn admission to the more selective public CSU universities, which were also bound by Prop 209; and (ii) URM high school graduates deterred from UC application by Prop 209. The next section quantifies the magnitude of this latter group.

30. In 2014, $100,000 was approximately the 90th (95th) percentile of wages among California (U.S.) workers aged 30 to 34, though it was earned by more than 20% of UC applicants 14 years after application. For annual estimated URM wage threshold declines relative to each baseline, see Online Appendix Figure A.12.

31. The estimated $130–$150 million decline in 2014 wages earned by URM Californians between ages 30 and 34 represents a 0.4–0.5% aggregate decline for that group. All ACS statistics are calculated using data from IPUMS (Ruggles et al. 2018).

32. For this ACS analysis, I define Californians as those born in the state, to identify those likely affected by Prop 209 and abstract away from posteducation cross-state mobility.

V. Application Deterrence and Model Robustness

The primary potential threat to the difference-in-differences design is that Prop 209 may have deterred some URM students from sending an application to UC, which could have further contributed to income inequality but may also generate sample selection bias in the baseline estimates (Long 2004; Dickson 2006; Yagan 2016).[33] I quantify the magnitude of this potential bias by first estimating the number and character of missing URM UC applications. I match the applicant data to the annual number of 1994–2001 UC-eligible graduates from each public California high school by gender and ethnicity—with UC eligibility indicating that they had satisfactorily completed accredited college-level coursework—and estimate models of the form:

$$\frac{A_{syea}}{UC_{sye}} = \sum_{e' \in \{A,B,H\}} \sum_{y' \in \{96,98,00\}} \beta_{e'y'a} \mathbb{1}_{e=e', y \in \{y', y'+1\}} + \zeta_{sea} + \eta_{sya} + \epsilon_{syea}, \tag{2}$$

where A_{syea} is the number of UC-eligible UC applicants from school s in years $\{y, y+1\}$ of ethnicity e in AI range a, and UC_{sye} is the number of UC-eligible high school graduates in those years. ζ_{sea} and η_{sya} are school-ethnicity and school-year fixed effects. Years are grouped into four pairs, from 1994–95 to 2000–01; ethnicities are grouped into Asian, Black, Hispanic, and white; and AI bins are defined as 200-point bins from 4,000 to 8,000. I estimate equation (2) by weighted least squares (WLS) (weighting to the student level using UC_{sye}) separately for each a, and interpret β_{e98a} as the average change in the proportion of UC-eligible e high school graduates who applied to UC following Prop 209, implicitly assuming that the true distribution of AI across school-year-ethnicity cohorts remains unchanged over time.[34]

Figure VII presents estimates of the Black and Hispanic $\beta_{e,\text{'98–99},a}$ coefficients from equation (2), scaled by the average

33. Card and Krueger (2005) use SAT sends (measured by College Board) as a proxy for university applications and present evidence that the decline in UC applications after 1998 was wholly driven by low-testing students unlikely to be qualified for UC admission. Online Appendix F replicates their finding using College Board data and shows that replacing SAT sends with actual applications (observed by linking College Board and UC applicant records) reverses that conclusion.

34. Online Appendix Table A.15 presents estimated coefficients for a specification of equation (2) across all AI. It shows that URM application rates following Prop 209 declined by 4%–6% of all UC-eligible URM public high school graduates.

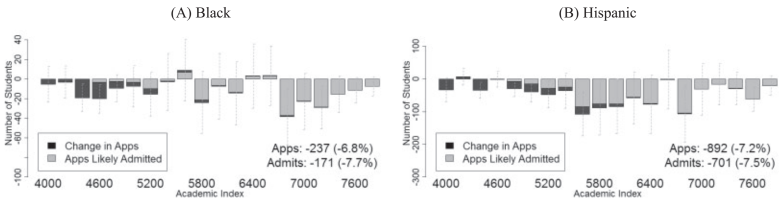

FIGURE VII

Estimated Declines in Annual 1998–99 Applications and Admissions by Ethnicity

Estimates of the change in the annual number of UC applicants (and admits) in 1998–99 by ethnicity (e) and 200-point AI bin, relative to 1994–95. The height of each black bar is the product of $\beta_{e,\text{'}98-99,a}$ (estimated in equation (2)) and $\sum_s UC_{s,\text{'}98-99,e}$, the average number of UC-eligible California public high school graduates of ethnicity e in 1998–99. The height of each overlaying light gray bar is the product of the black bar and the percent of 1998–99 UC-eligible e UC applicants in that AI range admitted to at least one UC campus. The statistics in the bottom right sum the bars across all AI and report the sums as a share of all e UC applicants. Ninety-five percent confidence intervals on the black bars are from $\beta_{e,98-99,a}$ robust standard errors. *Source*: UC Corporate Student System and the California Department of Education.

total number of e UC-eligible California high school graduates in the 1998–99 cohorts. The figure also shows the proportion of those applicants who would have likely been admitted to some UC campus had they applied, where admission is predicted solely by e and AI.[35] The figure shows that while some deterred Black and Hispanic high school graduates were unlikely to be admitted to any UC campus, there were also a large number of applicants certain to be admitted to some campus—indeed, very likely to be admitted to UC's more selective campuses—who were deterred from UC application after Prop 209.[36] The sum across the bars

35. That is, the light gray bar is the product of the black bar and the proportion of 1998–1999 URM UC applicants in bin a who were admitted to at least one campus. See Online Appendix Figure A.1 for evidence that e and AI were highly predictive of applicants' admission at most UC campuses, even after 1998. Admit estimates implicitly assume that each UC applicant's admission is small relative to the size and composition of the applicant pool.

36. Online Appendix Table A.9 links these application declines to the AI- and campus-specific enrollment changes presented in Figure III to show that application deterrence caused a decline in URM UC enrollment by about 450 students, half from Berkeley and UCLA. Combined with the estimated enrollment decline among UC applicants, this implies that Prop 209 caused an annual decline in URM UC enrollment of about 800 students in 1998–99, or 14%. This closely matches the differently calculated estimates of Bleemer (2019a).

suggests that the number of Black and Hispanic UC applicants declined by 12–13% (about 1,200 a year), most of whom would have likely been admitted to some UC campus.[37]

Given this shift in the UC applicant pool, I test for the magnitude of sample selection bias in the baseline difference-in-differences estimates in the previous section by reestimating the models with a series of additional covariates that could partially absorb remaining bias. First, I follow Card and Rothstein (2007) and construct a cross-school Heckit control function treating $p = \frac{A_{s_i yea}}{UC_{s_i ye}}$ as applicant i's likelihood of applying to UC (Heckman 1979). This control function formally requires the exclusion restriction that the within-school-ethnicity-cohort choice to apply to any UC campus is (conditionally) uncorrelated with student outcomes, and it absorbs cross-group selection into UC application. I also construct an alternative Heckit function defining p by the leave-one-out percentage of UC-eligible high school graduates who applied to UC by an applicant's school, gender, and ethnicity. In addition to the inverse Mills ratios of these p statistics, I collect a detailed set of applicant covariates excluded from the main specifications: gender, parental education, log family income, parental occupations, UC eligibility, high school GPA rank, and the number of enrolled 12th-grade honors courses.[38]

I conduct a Monte Carlo exercise, randomly selecting sets of these additional covariates for model inclusion (following Card, Fenizia, and Silver 2018) to test the presented estimates' sensitivity to alternative covariate specifications. In particular, I reestimate equation (1) specifying X_{iy} in the following ways: null (no covariates); including only the components of AI (as in the baseline specification); and then adding between 1 and 9 additional sets of covariates, selecting those that lead to the largest and smallest estimates of $\beta_{'98-99}$. The resulting estimates are shown in Online Appendix Figure A.15 for six main outcomes.

37. Online Appendix Figure A.14 presents additional specifications of equation (2). It shows that URM students were particularly discouraged from applying to the Berkeley and UCLA campuses, and that UC-ineligible applicants were only slightly deterred by Prop 209. As a placebo test, it also shows that application rates among Asian students increased by less than 2% relative to white applications.

38. Rank is determined using UC GPA among UC applicants in that school-year. Parental education indicates the applicants' parents' highest education level (with 7 codes); parental occupation indicates the parents' occupation set (with 17^2 codes). Covariates with missing values are included with missing value indicators.

Although the *AI* components are important covariates for several outcome measures, likely absorbing substantive changes in the composition of UC applicants around 1998, there is no further combination of these highly detailed control functions and covariates that meaningfully changes any of the $\beta_{'98-99}$ estimates, with the exception of six-year degree attainment growing slightly more negative.[39] These results show that the baseline estimates are highly insensitive to alternative model specifications conditioning on applicants' academic, demographic, and socioeconomic status and cross-school application behavior, although they may reflect sample selection bias on unobservables like orthogonal dimensions of their high school leadership activities.

VI. Impact of Prop 209 on Non-URM UC Applicants

Prop 209 did not measurably affect the overall weighted-average institutional value-added of enrollment at public or all California universities (see Online Appendix Figure A.2) or among UC applicants; the decline in enrollment quality among URM students was offset by an improvement among non-URM students. As discussed in Section III, I interpret the baseline difference-in-differences estimates as the effect of Prop 209 on URM UC applicants, despite the fact that—assuming constant treatment effects—as much as 20% of each estimate may reflect changes among non-URM applicants caused to enroll at more selective universities.[40] Two sets of additional evidence suggest that the per applicant impact of Prop 209 is smaller for non-URM than URM applicants (as in Dale and Krueger 2014; Bleemer 2021b), implying that non-URM outcomes explain less than 20% of each baseline estimate. First, single-difference estimates show that non-URM outcomes are generally smooth in the years before and after Prop 209, while URM educational and wage outcomes sharply and persistently decline in 1998 (see Online Appendix

39. For example, high school fixed effects explain 8.8% (3.0%) of variation in six-year degree attainment (conditional log wages); the addition of the AI covariates brings the R^2 to 12.9% (5.8%) and adding the full suite of additional covariates raises the R^2 to 15.3% (6.9%). These increasing R^2 values suggest that the covariates could have been expected to shift the estimated effect of Prop 209 if the estimates exhibited sample selection bias.

40. Online Appendix E shows that relative to academically comparable white applicants, Asian applicants enrolled at similar universities and had indistinguishable wage outcomes after Prop 209, suggesting proportional effects of affirmative action for both groups.

Figure A.16). Although this provides suggestive evidence of relatively small returns to more selective UC enrollment for "crowding-in" non-URM students, the absence of an unaffected comparison group prohibits separate identification of Prop 209's effect on non-URM students and secular trends.

Second, I use an alternative research design to directly estimate the admissions return to one UC campus—UC Berkeley, the most-selective campus and the campus where URM applicants' relative admissions advantages were largest until Prop 209—for the non-URM applicants who were on the Berkeley admissions margin in the years before Prop 209. These non-URM students were likely among those who would have most benefited from Prop 209, since many of them could have been admitted in the absence of Berkeley's affirmative action policy.

In 1996 and 1997, Berkeley guaranteed admission to applicants above an annually determined *AI* threshold.[41] Admissions officers then admitted some lower-*AI* applicants based on other application characteristics. Figure VIII, Panel A shows the admissions likelihood of 1996–97 non-URM Berkeley applicants at every *AI*, adding 70 points to 1996 *AI*s to align the two years' thresholds (7,360 and 7,430); admission was nearly guaranteed above the threshold and provided to only half of slightly below-threshold applicants. Because applicants near Berkeley's admissions threshold are quasi-randomly distributed on one or the other side of the threshold, differentiated by small test score or grade differences, I interpret outcome differences on either side of the threshold as resulting from the above-threshold non-URM applicants' greater access to UC Berkeley.

I estimate the effects of UC Berkeley admission for on-the-margin non-URM 1996–97 applicants using local linear regression discontinuity models following Calonico, Cattaneo, and Titiunik (2014). Each plot visualizes the 6,086 1996–97 non-URM Berkeley applicants within 400 *AI* points of the threshold; regressions include a 1997 indicator covariate.[42] Figure VIII, Panel B shows that the increased likelihood of Berkeley

41. See Online Appendix Figure A.3. Berkeley chose its annual threshold so that 50% of its admitted applicants had *AI* above the threshold. As a result, the threshold could not be chosen until after Berkeley observed all applicants' *AI*s, prohibiting applicants from manipulating their *AI* to exceed the threshold. Admissions around the threshold were noisier in 1994–95; see Online Appendix Figure B.1.

42. The distribution of applicants is smooth across the threshold, with the McCrary (2008) test yielding a *p*-value of .58. Sociodemographics are also smooth across the threshold: I predict annual log early thirties wages by gender-ethnicity

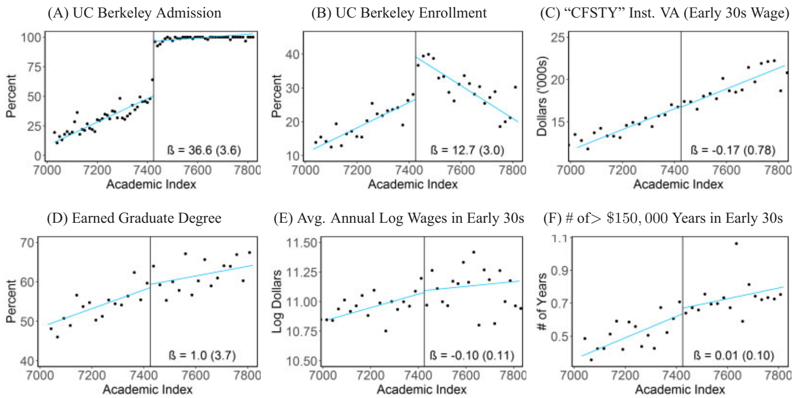

FIGURE VIII

Estimated Return to 1996–97 UC Berkeley Enrollment for On-the-Margin Non-URM Applicants

Regression discontinuity plots and estimates around the 1996–97 UC Berkeley guaranteed admission *AI* threshold among non-URM applicants, estimated by local linear regression following Calonico, Cattaneo, and Titiunik (2014). See the notes to Tables II, III, and IV for a description of the outcome variables; CFSTY institutional value-added measured relative to CSU Long Beach. Reduced-form coefficients from local linear regressions (conditional on year), with bias-corrected robust standard errors in parentheses. Running variable defined as $AI + (70 \times \mathbb{1}_{1997})$ to align thresholds over years. *Source*: UC Corporate Student System, National Student Clearinghouse, and the CA Employment Development Department.

admission causes about one-third of newly admitted on-the-margin non-URM students to enroll. However, those students would have otherwise enrolled at similar-quality institutions on average; Panel C shows that the CFSTY wage value added of applicants' enrollment institutions is unaffected at the threshold. Most of the students would likely have otherwise enrolled at UCLA or UCSD (6.1 percentage points, std. err. 3.5) or out-of-state universities (8.0 percentage points, std. err. 3.4).

Figure VIII, Panels D–F show that graduate school enrollment, early thirties wages, and the number of years spent by each applicant in their early thirties earning over \$150,000 a year

indicators, log parental income, and parental education among 1996–97 freshman UC-eligible UC applicants—omitting in-sample applicants within 400 *AI* of the threshold—and find that crossing the threshold yields lower predicted income by 0.00027 log points, with standard error 0.018.

are smooth across the Berkeley admissions threshold.[43] Although the estimated standard errors cannot reject moderate returns to UC Berkeley admission, the observed effects suggest that on-the-margin non-URM students have access to alternative similar-value universities, and switching enrollment to UC Berkeley provides little measurable long-run economic return.

Online Appendix J presents comparable estimates for UC Davis, the only other UC campus to set a binding *AI* admissions threshold before Prop 209. It shows that on-the-margin non-URM applicants rejected from UC Davis enroll at lower-value-added universities but similarly face no observable change in their educational or wage outcomes, though there is some evidence of nonrandom selection into applying to Davis above its admissions threshold. Nevertheless, if these estimated returns to UC Berkeley and Davis are externally valid for the non-URM students who crowded into more selective UC campuses following Prop 209, this suggests that Prop 209 provided minimal benefits to non-URM students.

VII. STEM Course Performance and Persistence

Having documented Prop 209's high-level effects on young URM Californians, I next turn to an investigation of educational mechanisms that could explain these effects. Several previous studies have hypothesized that students who attend more selective universities as a result of affirmative action will earn lower grades and become less likely to persist in demanding courses, especially in STEM fields, than if they had enrolled at a less selective university with lower-testing peers (Loury and Garman 1993; Arcidiacono, Aucejo, and Hotz 2016). However, previous studies have focused on the effect of affirmative action on overall grade point averages and major choice instead of URM students' actual course performance and STEM course progression.[44] Complementing the

43. There is no estimated change in the likelihood of California employment across the Berkeley access threshold; despite their increased likelihood of out-of-state university enrollment, applicants' number of early thirties years employed in California increases by 0.14 years (std. err. 0.17). A better indicator of unusually high wages for this strongly positively selected sample is \$150,000, rather than \$100,000; in their early thirties, they earned the former (latter) in 0.6 (1.7) out of 5 years.

44. Differences in overall GPAs are at least as likely to reflect differing grading standards across departments and between lower- and upper-division courses as

finding that Prop 209 failed to increase URM UC applicants' likelihood of earning a STEM degree—indeed, it led to the opposite effect—I further test this STEM mismatch hypothesis by estimating the impact of Prop 209 on URM UC enrollees' performance and persistence along introductory STEM course sequences. I test alternative formulations of this hypothesis in Online Appendix G and arrive at similar conclusions.

Using five UC campuses' detailed course enrollment records, I match core introductory STEM course sequences across these campuses (e.g., each campus's two-course introductory physics sequence) and estimate models of students' performance and persistence along these sequences using an extension of the baseline difference-in-differences models estimated above:[45]

(3)

$$Y_{iysm} = \alpha_{h_i} + \delta_y + \beta_0 URM_i + \sum_{t=1994}^{2002} \mathbb{1}_{\{t=y\}} \beta_y URM_i + \gamma X_{iy} + \epsilon_{iysm}$$

for student i from high school h_i in cohort y who takes course s in term m. I define three outcomes of interest for each completed course: the student's SAT percentile relative to their peers; the student's grade (out of four grade points); and the student's persistence, defined as an indicator for whether they completed the subsequent course in the sequence (e.g., whether the student completed Chemistry 2 after completing Chemistry 1). Persistence is not defined for the final course in each sequence, and repeated course grades are omitted. The model is stacked over s and estimated across courses, weighted evenly across students. Covariates X_{iy} include the components of AI as before. Standard errors are two-way clustered by student and course.

This definition of persistence mirrors the concept employed in the STEM mismatch hypothesis. Because the regression is

they are to reflect student course performance (Arcidiacono, Aucejo, and Spenner 2012; Bleemer and Mehta 2020). Differences in major choice may reflect that students have different preferences across majors at more or less selective institutions in a manner unrelated to course performance.

45. Introductory STEM courses include four courses in chemistry (two introductory, two organic), two in biology, two in physics, and three in computer science. Each course generally requires the previous course as a prerequisite. When universities on the quarter system include three courses along a sequence, I include the first and third course. Course details are provided in Online Appendix H. Estimates are largely insensitive to omitting engineering students, who may face different STEM course incentives.

weighted evenly across individuals, persistence can be heuristically understood as ranging from 0 to 100%. A student whose only completed STEM course is Chemistry 1, without ever completing Chemistry 2, would have persistence of 0%. A student who takes Chemistry 1, 2, and 3 but not 4 would have persistence 66.6%, since they persisted after two courses but not the third. A student who takes only all three computer science courses would have persistence of 100%. The STEM mismatch hypothesis holds that URM students admitted by affirmative action have lower STEM persistence than they would have had at less selective universities.

In the two years before Prop 209, URM UC enrollees earned lower average grades in introductory STEM courses by 0.42 GPA points and were less likely to persist along STEM course sequences by 11.2 percentage points (see Online Appendix Tables A.16 and A.17). These gaps are fully explained by URM enrollees' poorer prior academic opportunity and preparation; their performance and persistence was indistinguishable from those of academically comparable non-URM students across the five UC campuses. Relative to academically comparable non-URM UC students, however, 1996–97 URM students were 7.3 percentiles lower in their classes' SAT distribution, largely reflecting their enrollment at relatively more selective UC campuses. The first panel of Figure IX shows that Prop 209 caused URM students to enroll in STEM courses in which their average SAT percentile was about 4 percentage points higher, closing the gap by more than half. However, this increase in class rank did not translate into any observable improvement in those students' likelihood of STEM persistence or course grades. URM enrollees STEM performance and persistence were unchanged when their class rank improved; the 95% confidence interval around the estimated change in STEM persistence narrowly bounds 0, from −2.3 to 3.5 percentage points, small effects relative to the raw STEM persistence ethnicity gap of 11.2 percentage points before Prop 209. Online Appendix Figure A.17 shows that Prop 209 similarly affected Black and Hispanic UC enrollees' STEM persistence and performance outcomes.

I also estimate a difference-in-differences model of UC enrollees' likelihood of completing any STEM major (following equation (1)). URM UC enrollees' STEM major choice is precisely unchanged relative to academically comparable non-URM enrollees after Prop 209, with a 95% confidence interval rejecting increases above 1.5 percentage points; the overall decline

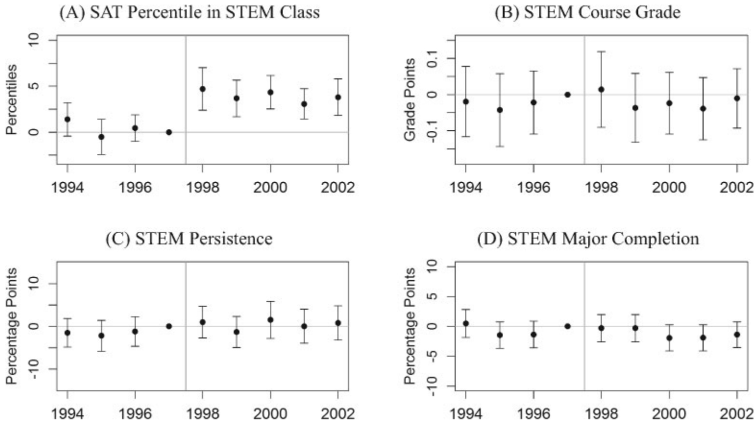

FIGURE IX

Difference-in-Differences Estimates of URM UC Enrollees' STEM Performance and Persistence

Difference-in-differences WLS regression coefficient estimates of UCB, UCSB, UCD, UCSC, and UCR enrollees' introductory STEM course performance or persistence, differencing across URM status following equation (3), relative to 1997. In Panels A–C, each observation is a CA resident freshman student-course pair in an introductory biology, chemistry, physics, or computer science course (see Online Appendix H) taken within 2.5 years of matriculation, stacking over courses and weighted evenly across observed students. SAT percentile is the fraction of other 1994–2002 freshman CA resident peers who have lower SAT scores than the student; persistence indicates completing the subsequent course in the introductory STEM course sequence; and course grade is the grade points received in completed courses. In Panel D each observation is a student; the outcome indicates completing any UC STEM degree. Models include high school fixed effects, ethnicity indicators, and the components of UC's Academic Index (see note 21). UCSC is omitted from the GPA model because it did not mandate letter grades in the period. Ninety-five percent confidence intervals are two-way clustered by student and course sequence level (e.g., second chemistry course). *Source*: UC Corporate Student System and UC-CHP Database (Bleemer 2018).

in STEM attainment thus appears driven by students who exit these UC campuses after Prop 209. These findings suggest that selectivity differences between public research universities are at best a second-order determinant of URM students' relative persistence and performance in STEM courses; instead, they appear largely explained by compositional differences in prior academic opportunity and preparation. In turn, the absence of changed STEM performance and persistence after Prop 209 suggests that course performance or persistence are not primary explanations for the effect of Prop 209 on students' educational and wage outcomes.

VIII. Discussion: Affirmative Action and Efficiency

The evidence presented so far has implications for both the equity and efficiency of affirmative action. While affirmative action may have second-order effects on students whose admission was unrelated to the policy, such as through peer effects (Sacerdote 2011) and the effect of campus diversity (Carrell, Rullerton, and West 2009), to a first approximation the (Kaldor-Hicks allocative) efficiency of affirmative action can be measured by the net effect of Prop 209 on two groups of students: the URM students targeted by affirmative action and the non-URM students who would have been admitted otherwise. Because net enrollment at more and less selective universities appears roughly unaffected by Prop 209 (see Online Appendix Figure A.2), this net effect can instead be summarized by the average relative returns to more selective university enrollment for these two groups of students.

The single-difference and regression discontinuity estimates presented in Section VI suggest that the non-URM students whose enrollment was affected by Prop 209 received minimal returns from those changes, in line with the hypothesis that the return to more selective university enrollment was relatively larger for the URM students targeted by affirmative action than it was for the non-URM students who replaced them after Prop 209. Unfortunately, Berkeley's URM admissions policies did not generate a sharp change in admissions likelihood at any *AI*, prohibiting parallel analysis for that group of students (see Online Appendix Figure B.1).

That hypothesis is further supported by a comparison between the change in URM students' early thirties wages and the change in the wage value added of their enrollment institutions. While Prop 209 led URM students to enroll at universities with early thirties wage value added, which was lower by as much as $1,000, those students' actual early thirties annual wages fell by more than $2,000 (see Tables II and IV). Assuming that the presented value-added statistics either approximate or relatively overestimate the average difference in treatment effects of enrolling at those universities, this suggests that the wage effect of more selective university enrollment for the students affected by affirmative action is significantly larger than universities' average treatment effect. While the local average wage treatment effect for "crowding-in" non-URM students remains unobserved, that effect is very likely to be lower than the above-average effects for the URM students who benefited from affirmative action. This evidence suggests that affirmative action improved the allocative efficiency of California higher education.

IX. Conclusion

Proposition 209 banned race-based affirmative action at public California universities starting in 1998. In the years immediately after the ban, URM UC applicants' university enrollment sharply shifted away from UC's most selective Berkeley and UCLA campuses, causing a cascade of students to enroll at lower-quality public institutions and some private universities. Contrary to the mismatch hypothesis, less selective university enrollment did not lead UC's remaining URM students to earn higher grades in challenging courses, but it did cause URM applicants to become less likely to earn STEM degrees and any graduate degrees, and undergraduate degree attainment declined among lower-testing URM applicants. These poorer educational outcomes in turn contributed to a 5% average annual decline in Hispanic—but not Black—applicants' early career wages, exacerbating inequality by decreasing the number of early-career URM Californians earning over $100,000 by at least 3%. Prop 209 also discouraged thousands of additional academically competitive URM students from sending applications to public research universities, likely leading to additional reductions in California's high-earning URM workforce.

Affirmative action decreases non-URM student enrollment for each net additional URM student it causes to enroll. However, single-difference and regression discontinuity evidence suggest that those affected non-URM students—whose more selective university enrollment increased following Prop 209—experienced relatively small long-run educational or wage effects after Prop 209. URM students, on the other hand, had received above-average wage returns to more selective university enrollment under affirmative action, and thus faced disproportionate declines after Prop 209, suggesting that Prop 209 reduced both the equity and efficiency of Californian higher education. White and Asian students were proportionally affected by Prop 209, with no evidence of disparate effects for one or the other.

These findings differ from several existing analyses of the effects of affirmative action, even those focusing on Prop 209, and highlight the importance of high-quality and detailed administrative data and a transparent research design to help account for sample selection and omitted variable bias. They also contextualize the effect of university affirmative action policies relative to other policies aiming to close opportunity gaps for low-income and Black and Hispanic youths. Some limitations remain. The presented estimates are reduced-form, averaging over many URM students who were likely unaffected by the Prop 209 policy change, which means that they likely underestimate the effect of Prop 209 on students whose enrollment was shifted by UC's policy change. They omit the impacts of Prop 209 on URM Californians dissuaded from UC application by Prop 209, who may have benefited from affirmative action at UC. The estimates also omit labor market outcomes for (endogenously selected) non-Californian and self-employed workers. Nevertheless, this study documents the meaningful potential of affirmative action policies to promote economic mobility in the United States—though perhaps not to close white-Black mobility gaps—and the equity and efficiency consequences of affirmative action's prohibition.

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

Code replicating the tables and figures in this article can be found in Bleemer (2021a) in the Harvard Dataverse, https://doi.org/10.7910/DVN/X0NJLX.

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