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On the Meritocratic Allocation of Higher Education

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By

Zachary I. Bleemer

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Committee in charge:

Professor David Card, Chair
Professor Jesse Rothstein
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Abstract

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Access to higher education is a key determinant of lifetime earnings in the U.S. Since the 1960s, selective public universities have admitted students mostly on the basis of standardized test scores and other measures of academic preparation. In this dissertation, I employ quasi-experimental and structural research designs to investigate the efficiency and economic mobility ramifications of these 'meritocratic' admissions policies. I focus on the selective University of California (UC) system, with each chapter analyzing a newly-constructed longitudinal dataset that links all 1994-2018 UC applicants and most 1975-2018 UC enrollees to their national college enrollment, major choice, and degree attainment (whether at UC or elsewhere); their UC student transcripts (for UC enrollees); and their 2000-2019 California wages.

Chapter 2 studies race-based affirmative action, which broadened lower-'merit' underrepresented minority (URM) college applicants' access to UC campuses until the policy was banned by a ballot proposition in 1998. I employ a difference-in-difference research design to show that ending affirmative action caused underrepresented minority (URM) freshman applicants to cascade into lower-quality colleges. The well-known "Mismatch Hypothesis" implies that this cascade would provide net educational benefits to URM applicants, but URM applicants' degree attainment declined overall and in STEM fields, especially among less academically qualified applicants, and URM UC applicant's average wages fell in turn. These declines are not explained by URM students' performance or persistence in STEM course sequences, which were unchanged after Prop 209. Complementary regression discontinuity and institutional value-added analyses suggest that affirmative action's net wage benefits for URM applicants exceed its (potentially small) net costs for on-the-margin white and Asian applicants. These findings provide the first causal evidence that banning affirmative action exacerbates socioeconomic inequities and suggest that loosening meritocratic admissions policies may generate efficiency *and* economic mobility gains.

Chapter 3 further analyzes the efficacy of test-based meritocracy in college admissions by evaluating the impact of a grade-based "top percent" policy implemented by UC between 2001 and 2011. Eligibility in the Local Context (ELC) provided large admission advantages to the top four percent of graduates from each California high school. I first employ a regression discontinuity design to show that ELC led over 10 percent of barely-eligible applicants from low-opportunity high schools to enroll at selective UC campuses instead of less-selective public colleges and universities. Half of those participants were URM, and their average SAT scores

were at the 12^{th} percentile of their UC peers. Instrumental variable estimates show that ELC participants' more-selective university enrollment caused large increases in five-year degree attainment and annual early-career wages. I then analyze ELC's general equilibrium effects by estimating a structural model of university application, admission, and enrollment with an embedded top percent policy. I find that ELC and counterfactual expansions of ELC substantively increase disadvantaged students' net enrollment at selective public universities. Reduced-form and structural estimates show that ELC participants derived similar or greater value from more-selective university enrollment than their higher-testing peers, providing further evidence that access-oriented admission policies at selective universities can promote economic mobility without efficiency losses.

In Chapters 4 and 5, both coauthored with Aashish Mehta, I turn from meritocratic college admissions policies to the meritocratic allocation of lucrative fields of study. We study a popular class of policies – which we term 'major restriction' policies – that prohibit students with poor introductory course grades from earning their preferred college major. Chapter 4 employs a difference-in-difference event study design around the implementation of 28 major restrictions at four UC campuses since the 1970s to show that the policies are binding and differentially impact URM students and students with absolute (not comparative) academic disadvantage, closely paralleling the function of meritocratic college admissions policies in decreasing educational access for disadvantaged lower-'merit' students. A student-level extension of the event study design shows that major restriction policies tend to lead female and URM students to relatively lower-average-wage majors, generating cross-major stratification.

Chapter 5 focuses on one specific major restriction policy – which limited access to the UC Santa Cruz economics major between 2008 and 2012 – and uses a regression discontinuity design to show that lower-GPA students prohibited from declaring the economics major earned $22,000 (46%) lower annual early-career wages than they would have as economics majors. A decomposition of this wage effect shows that the return to majoring in economics would likely have been above-average for the near-threshold students rejected from the economics major, once again suggesting the potential for efficiency and economic mobility gains in implementing a less 'merit'-oriented allocation policy.

In sum, this dissertation presents a collage of evidence from three educational allocation policies suggesting that the reallocation of selective higher education to disadvantaged students with relatively poorer measured academic preparation can promote both economic mobility and allocative efficiency, with those students' net education and wage gains exceeding their crowded-out peers' net losses. These efficiency findings undermine the primary justification for the 1960s implementation of meritocratic admissions policies at public institutions.

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I used three key ingredients to produce this dissertation. The first was the massive University of California applicant and student database entrusted to me by the UC Office of the President and many UC campuses. This work could not have begun without Günter Waibel connecting me to UCOP's Institution Research and Academic Planning team, where Pamela Brown, Chris Furgiuele, Darin Jensen, Charles Masten, Brianna Moore-Trieu, Matt Reed, and especially Tongshan Chang were extremely helpful and supportive in constructing the applicant database. Collecting the student transcript database would have impossible without dozens of campus administrators' generous commitments; I am particularly grateful to UCSF's Doug Carlson and Jeff Harter for getting the project off the ground, and for the support of Leesa Beck, Elizabeth Bennett, Pamela Brown, Erin Crom, Bracken Dailey, Karen Denton, Tchad Sanger, Sergey Shevtchenko, and Walter Wong.

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Contents

Chapter 1

Introduction

> *"The more capable high school students should have the greater freedom of choice of collegiate institution, and selection procedures should give preference to the more able ... [to] predict success in the state colleges."*
>
> $\sim$ *Technical Committee for the California Master Plan for Higher Education, 1961*

American universities have long facilitated the transition to adulthood for America's most promising youths. As a result, the allocation of higher education – that is, the decentralized decision-making of colleges and universities that determines which institutions and (subsequently) fields of study are available to each young person – is a key determinant of lifetime earnings and economic mobility in the U.S. (Chetty et al., 2020*a*; Rothstein, 2019).

Consider American higher education as a marketplace where many highly-differentiated institutions sell their educational 'goods' to student-consumers. This market exhibits three market failures:

1. Students have imperfect information about the educational experiences offered by different institutions, and the quality of their information is correlated with their socioeconomic status, or SES (Bleemer and Zafar, 2018);

2. The societal value of students' educational choices depends on those choices' effects on many other people, with externalities arising from how much education students attain, match effects between institutions and students, and peer effects within institutions (Moretti, 2004*a*,*b*); and

3. There is a direct public interest in redistributing the highest-demand institutions to low-SES students.

This combination of market failures have contributed to American higher education's unusual market structure. Among private institutions, lower-demand institutions function in a manner consistent with a marketplace in the presence of externalities: prices are differentiated but competitive, and the government provides large price subsidies, especially to low-SES students. Higher-demand institutions, on the other hand, set highly-differentiated below-market prices (often spanning negative prices up to the millions of dollars, when *quid pro quo* donations are included) and carefully select their students on the basis of individual characteristics ranging from academic preparation and athletic ability to lineage and ethnicity (Arcidiacono, Kinsler and Ransom, 2019*b*).

America's public universities – which today enroll three-quarters of U.S. college students – were founded on the supposition that higher education was socially beneficial for anyone with sufficient preparation, and public universities provided 100 years of broad educational access to students from all backgrounds who satisfactorily completed the high school curriculum (Douglass, 2007). But when the mid-20th-century brought a surge of prospective undergraduates, many high-quality public universities were forced to adopt selective admissions policies to regulate this new demand. Since the 1960s, selective public universities have admitted students mostly on the basis of measured academic preparation for college-level coursework, with more measurably-prepared students generally being provided access to higher-demand (and higher-quality) institutions.

The term 'meritocracy' has been widely adopted to refer to these test- and grade-based selection mechanisms, and in this dissertation I adopt the term with that meaning. As a result, I use the term 'merit' (in quotation marks) as it is *de facto* employed by the admissions offices of most American public universities: as a characteristic of students largely determined by their standardized test scores, their high school grades and curriculum, and (to a lesser degree) other academic and socioeconomic information that help admissions officers determine how *academically* successful they believe a student would be at their university.[1] Many selective public universities also implement policies that aim to broaden access for lower-'merit' applicants – like race-based affirmative action, top percent policies, and holistic review (which emulates selective private universities' admissions mechanisms) – but meritocracy has remained the principal pillar of selective public university admissions for about 60 years.

Public universities' primary justification for their meritocratic admissions policies is the theory – enshrined in California's influential Master Plan for Higher Education – that highly-prepared students can best take advantage of rigorous universities' academic curricula. Public universities' goal in admitting students with maximal academic preparation is to identify students who perform well in introductory college courses and persist to graduation, presumably because those students would thus maximally benefit from their college education, generating public returns on their subsidized higher-education investment in the form of greater productivity, innovation, and/or entrepreneurship.[2] Meritocratic admissions may also generate positive spillovers by incentivizing K-12 learning, as students compete for scarce higher education opportunities by investing in their observable college preparation (Akhtari, Bau and Laliberte, 2020; Cotton, Hickman and Price, 2021).

The dominant criticism of public universities' meritocratic admissions policies, on the other hand, has been that they unfairly exclude low-SES students with poorer access to pre-college educational opportunities (Soares, 2020). The criticism generally takes one of two flavors:

- That the statistics used to measure applicants' academic preparation – particularly standardized test scores – are systematically biased in favor of high-SES students, who artificially raise their measured preparation through (e.g.) expensive test-prep courses and tutoring (Lemann, 1999), high school grade inflation (Bleemer, 2020*b*), and sometimes expensive malfeasance (Korn and Levitz, 2020).

[1] I directly estimate selective public universities preferences over applicants – and validate this notion of 'merit' as implemented by universities – in Chapter 3 below.

[2] See Westrick et al. (2019) and University of California (2020).

- That measures of academic preparation fail to capture the latent academic potential of low-SES students who had access to lower-quality secondary schools, fewer advanced placement courses, and fewer outside educational resources that hindered their ability to reveal that potential (Black, Cortes and Lincove, 2016; Tough, 2019).

In either formulation, this criticism is generally understood as a key trade-off prescribed by meritocratic admissions: meritocracy may identify the most-prepared students (which may have positive implications for long-run economic growth), but it exacerbates equity gaps between high- and low-SES families (with negative implications for economic mobility) (Arrow, Bowles and Durlauf, 2000).[3]

The chapters below illuminate both sides of this potential trade-off by carefully studying three university policies that vary the degree to which elite higher education is allocated by meritocracy. What happens when lower-'merit' students are provided with elite higher education, reallocating students across America's universities? Do they struggle academically, and does that lead to long-run negative consequences like drop-out or unemployment? And if not – if it turns out that elite higher education is valuable to them – then how does the educational value-added that they receive from elite higher education compare to the value-added received by their high-'merit' peers? Finally, how does the prior socioeconomic status of the targeted lower-'merit' students differ from that of the students who would have taken their places under strict meritocracy?

These questions get to the heart of the *efficiency* and *economic mobility* ramifications of meritocratic admissions policies. In this context, a policy's (allocative) efficiency refers to whether it results in an economically optimal sorting of students across colleges and universities. The chapters below measure economic outcomes in terms of degree attainment and wages: a policy that changes the allocation of higher education – by causing one group of students to switch from a less-selective university to an elite university, but then causing an equal number of students to attend the less-selective university instead of the elite university – are efficiency-improving if they increase average graduation rates and employment outcomes across all students.[4] If meritocracy successfully identifies the students who can best take advantage of elite universities, then it should be (reasonably) efficient: replacing high-'merit' students with lower-'merit' students at elite universities would lead to average declines in educational outcomes, because the newly-enrolled students would have a relatively harder time benefiting from the elite university's rigorous curriculum. A policy improves economic mobility, on the other hand, when it increases the net educational and labor market outcomes of students from low-SES households relative to those of high-SES students.

Chapter 2 provides an first set of answers to the research questions posed above by studying the efficiency and economic mobility ramifications of race-based affirmative action. Affirmative action is one of most popular "access-oriented" admissions policies – selective university policies that intentionally admit certain students with poorer measured academic preparation – in the United States, but it is also highly controversial: ten states have banned university affirmative

[3] Other recent criticisms of meritocratic admissions include that it is massively wasteful with regard to children's measured-preparation investments and dangerously demoralizing to the rejected (Markovits, 2019; Sandel, 2020).

[4] Proxying economic outcomes by wages faces two key limitations. First, to the degree that wages are biased measures of workers' marginal product (with the bias plausibly-correlated with university selectivity), wage differences may partially reflect signaling or other factors in addition to actual productivity gains. Second, wages may not capture the full public value of innovation, entrepreneurship, or other externalities. I intend to address this latter concern in future work.

action policies, and decades of Supreme Court cases have gradually constrained its operation. To study the effect of elite university enrollment on the lower-'merit' students admitted to those universities by affirmative action, I focus on a useful natural experiment induced by one of those state bans: Proposition 209, which ended California public universities' affirmative action policies in 1998. What happens to California's students when meritocracy intensifies after the end of the state's affirmative action regime?

I study the effects of Prop 209 using a difference-in-difference research design and a newly-constructed longitudinal database linking all 1994-2002 University of California applicants to their college enrollment, course performance, major choice, degree attainment, and wages into their mid-30s. As expected, ending affirmative action caused UC's 10,000 annual underrepresented minority (URM) freshman applicants to cascade into lower-quality public and private universities, with thousands of URM applicants going to a slightly less-selective school than they would have if affirmative action had continued. While the well-known "Mismatch Hypothesis" implies that this cascade would provide net educational benefits to URM applicants (Arcidiacono and Lovenheim, 2016), instead the opposite occurred: their undergraduate and graduate degree attainment declined overall and in STEM fields, especially among lower-testing applicants, and the average URM UC applicant's wages declined by 5 percent annually through at least their mid-30s. These costs added up: by the mid-2010s, Prop 209 had caused a cumulative decline in the number of early-career URM Californians earning over $100,000 by at least 3 percent. Prop 209 also deterred thousands of qualified URM students from applying to any UC campus.

Interestingly, there is no evidence that the lower-'merit' Black and Hispanic students who enrolled at more-selective universities under affirmative action were any less able to take advantage of those universities' academic curricula than their higher-'merit' peers: enrolling at less-selective UC campuses did not improve URM students' performance or persistence in STEM course sequences. More damning to meritocracy's claim to efficiency, a series of complementary statistical analyses – employing regression discontinuity and fixed-effect value-added designs – suggest that affirmative action's net wage benefits for URM applicants exceed its (potentially small) net costs for on-the-margin white and Asian applicants. In other words, not only did elite university enrollment benefit the lower-'merit' students targeted by affirmative action, but those benefits appear to exceed the benefits accrued by the higher-'merit' white and Asian students who gained access to those elite universities after the end of affirmative action. Altogether, the evidence from affirmative action suggests that loosening meritocratic admissions policies may not generate a efficiency-mobility trade-off at all; instead, access-oriented admission policies might accomplish both efficiency *and* economic mobility improvements.

Chapter 3 takes a second look at the efficiency and economic mobility ramifications of meritocracy by studying the effects of another access-oriented admissions policy: a top percent policy called Eligibility in the Local Context (ELC). ELC – which was implemented by the University of California between 2001 and 2011 – provided elite university admission advantages to the top four percent of graduates from each California high school, ranking students only by their high school grades. As in the case of Prop 209, I study ELC by constructing a longitudinal dataset linking the ELC era's 1.8 million UC applicants to a variety of educational and labor market outcomes. I first employ a regression discontinuity design to show that ELC functioned as an access-oriented admission policy: it caused over 10 percent of barely-eligible applicants from low-opportunity high schools to enroll at selective UC campuses instead of less-selective public colleges and universities. Half of those participants were URM, and their average SAT scores were at the 12^{th} percentile of their UC peers: ELC participants were from low-SES backgrounds and were sharply lower-‘merit’ than their peers at the universities where they ended up enrolling. Using a comprehensive structural model of university enrollment, I show that a broad range of top percent policies like ELC substantially increase low-SES students’ net enrollment at selective public universities. Next, I use an instrumental variables research design to show that ELC participants’ more-selective university enrollment was extremely beneficial to them, causing increases in five-year degree attainment by 30 percentage points and annual early-career wages by up to $25,000.

The chapter concludes with evidence from both the regression discontinuity design and the structural model suggesting that ELC participants derived similar or greater value from more-selective university enrollment than their higher-testing peers. These findings align closely with the Prop 209 analysis presented in the previous chapter, once again supporting the hypothesis that access-oriented admission policies at selective universities can promote economic mobility without efficiency losses, and likely with efficiency gains. In fact, the evidence from the structural model suggests that the return to elite universities is negatively correlated with both applicants’ SAT scores and high school GPAs: the less measurably-prepared the high school graduate (among the self-selected graduates who apply to UC), the greater their economic return to enrolling at an elite university. This represents a stark refutation of meritocracy’s central justifying theory.

The subsequent two chapters, which are coauthored with Aashish Mehta, turn to a closely related question: what happens when ‘elite’ college *majors* are restricted using meritocratic admissions policies? Many universities restrict access to their high-demand majors using ‘major restriction’ policies like mechanical GPA thresholds – requiring students to earn high grades in the department’s introductory courses if they want to declare the major – and full-blown competitive internal applications (even after the students have already been admitted to the university). We find that these policies mirror more-meritocratic university admissions policies in both function and outcome.

Once again, I construct a novel database to analyze these policies, this time linking four universities’ 1975-2016 student transcripts to education and wage outcomes over the subsequent years and decades. Chapter 4 employs a difference-in-difference event study design around those four universities’ implementation of 28 major restrictions to show that the policies tend to decrease enrollment in restricted majors, especially among URM students and students with absolute academic disadvantages. This closely parallels the function of meritocratic college admissions policies: by restricting access on educational preparedness, the policies decrease elite educational access for disadvantaged lower-‘merit’ students. We then extend the event study design to carefully assess the major choices of students who intend to earn restricted majors, showing that the restrictions drive female and URM students to earn relatively less-lucrative majors than their male and non-URM peers who had also intended to earn the restricted majors. Lastly, a case study of two universities’ economics majors suggests that these stratification effects are largely explained by URM and low-income students’ poorer pre-college academic opportunity and measured preparedness, which lead to poor performance in introductory classes. This evidence shows that meritocratic college major policies have important negative equity and mobility consequences.

Finally, Chapter 5 focuses on the longer-run ramifications of one specific major restriction policy: a 2008-2012 GPA restriction on UC Santa Cruz’s ‘elite’ economics major. Using a regression discontinuity design, we show that the economics major is extremely valuable even to students who perform relatively poorly in the field's introductory courses; lower-GPA students prohibited from declaring the economics major earned \$22,000 (46%) lower annual early-career wages than they would have as economics majors. A careful decomposition of this wage effect shows that the return to majoring in economics for near-threshold low-GPA students would likely have been above the average return received by UCSC economics majors, once again suggesting the potential for efficiency gains in implementing a less 'merit'-oriented allocation policy.

In sum, the presented evidence rejects the theory of an efficiency-equity trade-off when considering the consequences of feasible higher education policies that allocate elite education through non-meritocratic mechanisms. At best, there is no evidence that strict meritocracy efficiently allocates elite education relative to reasonable alternatives; at worst, the evidence suggest feasible changes that could very substantially improve higher education's allocative efficiency both across and within institutions, with simultaneous improvements in economic mobility and equity.

A number of questions remain unanswered. First, each of the analyzed policies is relatively small-scale (though Prop 209 reallocated thousands of students per year); perhaps larger-scale changes to meritocratic allocation policies would have unmeasured general equilibrium repercussions because of sizable changes in peer or sheepskin effects. Second, as discussed above, wages are an imperfect proxy for productivity; future study should analyze the relative returns to elite education for lower- and higher-'merit' students in terms of innovation, entrepreneurship, and potentially geography (since states have an interest in promoting local economic activity) as well as non-economic outcomes. Finally, the evidence presented in this study was all collected in the California public university context, where strong public support (despite recent disinvestment) has resulted in research universities with a wide variety of services aimed at supporting struggling students. The return to selective universities may have been far smaller absent those services. Given these caveats, the evidence presented in this dissertation provides a direct challenge to the presumed efficiency of the meritocratic allocation of higher education.

Chapter 2

Affirmative Action, Mismatch, and Economic Mobility after California's Proposition 209

> *"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, in introducing SP-1 and SP-2*[1]

2.1 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., 2020*a*). 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 impact 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

[1] Letter to the Regents of the University of California, July 5, 1995: Berkeley Bancroft Library CU-558, Cnt. 8.

selective universities' minimum admissions thresholds are benefited by admission.[2] 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.

This study combines longitudinal administrative data with a difference-in-difference research design to estimate the impact of affirmative action on students' college quality, course performance, choice of major, degree attainment, and wages over the subsequent 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 individually link each applicant to nationwide university records and annual California wages. I then 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 impact on performance and persistence in demanding courses. Finally, I employ 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.[3] 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 AIs 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-difference design estimated over the population of UC applicants. Each model estimates how URM applicant outcomes change after 1997 (the last year of affirmative action) relative to changes among non-URM applicants, with the second difference absorbing ethnicity-neutral enrollment trends in the 1990s.[4] High school fixed effects and AI covariates absorb spurious variation and observable selection bias into UC application.[5] I also

[2]See Hoekstra (2009); Zimmerman (2014); Anelli (2019); Kozakowski (2019); Sekhri (2020); Smith, Goodman and Hurwitz (2020). Few quasi-experimental studies examine selective universities' value to applicants with poorer measured academic qualifications, but Cohodes and Goodman (2014) and Bleemer (2018*a*) provide evidence of positive returns to selectivity for such students in other contexts.

[3]URM includes African-American (Black), Chicano and Latino (Hispanic), and Native American students.

[4]Non-URM applicants may not represent a traditional unimpacted 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 2.6, 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 percent of the observed differences are likely driven by changes in URM applicant outcomes.

[5]AI and ethnicity explained 40-70 percent of admissions variation at most UC campuses in the mid-1990s; see

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.[6] 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 percent lower average annual wages between ages 24 and 34, with larger proportional effects for lower-AI applicants.[7] 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.[8]

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-impacted 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 30s with 2014 wages over $100,000 by at least 3 percent. 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.[9] Estimating a difference-in-difference model of the proportion of California public high school students who applied to UC by ethnicity and AI bin, I find that UC annually received about 250 fewer Black and 900 fewer Hispanic applications after Prop 209, almost 80 percent of whom would likely have been admitted to at least one UC campus.[10] While 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.[11]

The baseline research design does not separately identify the impact of Prop 209 on non-URM applicants' outcomes. Instead, I exploit a large discontinuity non-URM admissions at UC Berkeley

Figure A.11. Cortes (2010) uses a similar design to compare student outcomes between Texas's affirmative action and Top Ten policies.

[6]I estimate institutional value-added by regressing degree attainment and wages on UC applicants' first enrollment institution, conditioning on observables following either Mountjoy and Hickman (2020) or Chetty et al. (2020*a*). See Appendix A.9.

[7]These changes cannot be explained by California labor market entry or exit: 69 percent of UC applicants had positive annual CA wages between ages 24-34, and URM applicants' employment remained unchanged after Prop 209 overall and in each AI quartile.

[8]This finding is in line with Chetty et al. (2020*b*)'s argument that educational differences cannot explain the U.S.'s Black-white wage gap, though that study does not discuss the role of university selectivity.

[9]Other potential threats – including non-reported applicant ethnicity, imperfect National Student Clearinghouse degree reporting, and some campuses' preemptive implementation of Prop 209 – are discussed below and in Appendix A.4. None meaningfully impacts the baseline findings.

[10]Card and Krueger (2005) reach a different conclusion when they proxy university applications with SAT 'score sends' from the College Board. My analysis uses actual university applications. See Appendix A.6.

[11]In particular, I perform a Monte Carlo exercise randomly selecting sets of detailed covariates like family income, parental occupation and education, and additional measures of academic preparation for model inclusion. While the baseline estimates are insensitive to additional covariates, bias on orthogonal unobserved characteristics could remain.

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. Employing 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.[12] This suggests that the value of selective public university access for on-the-margin non-URM students was small.[13]

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 absent affirmative action, which likely would have led to the opposite of Prop 209's effect on STEM degree completion.[14] 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.[15] 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, 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.[16] First, the estimated return to UC Berkeley and Davis admission for on-the-margin non-URM students appears small, while URM applicants' estimated wage return to more-selective enrollment before Prop 209 is large.[17] Second, that latter return exceeds the average observed change in institutional value-added experienced by URM UC applicants, suggesting that the URM applicants impacted by Prop 209 had received above-average returns to more-selective university enrollment (as in Dale and Krueger, 2014; Bleemer, 2018*a*).[18] These evidence suggest 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 impact of banning affirmative action, but out-of-state

[12]Appendix A.10 presents similar evidence among on-the-margin non-URM applicants to UC Davis before Prop 209.

[13]Appendix A.5 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.

[14]See Loury and Garman (1993); Holzer and Neumark (2000); Arcidiacono, Aucejo and Hotz (2016).

[15]This study's examination of STEM course performance contributes to a literature interested in the production and composition of STEM graduates (Ehrenberg, 2010; Griffith, 2010; Sjoquist and Winters, 2015*b*; Denning and Turley, 2017; Castleman, Long and Mabel, 2018). This is the first known study to estimate how student outcomes in specific STEM courses change under different policy regimes.

[16]Figure A.12 shows that relative enrollment at high- and low-value-add California universities was unchanged by Prop 209.

[17]Black, Denning and Rothstein (2020) also 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.

[18]Selection bias in the estimated value-added statistics will tend to exaggerate differences across institutions, implying that Prop 209's estimated effect on institutional value-added is likely biased upwards.

enrollment confounds identification of the policies' effects on impacted students.[19] Others estimate models of applicant and university behavior to predict how affirmative action *could* impact student enrollment and outcomes, but do not validate these predictions using actual policy variation.[20] 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.[21] 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 intermediate outcomes like university "value-added," STEM performance and persistence, and graduate degree completion.

Second, this is the first study to causally link changes in 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.[22] Much of the affirmative action literature has focused on whether it leads URM applicants to earn *lower* average wages (Sowell, 1972; Arcidiacono and Lovenheim, 2016), but my findings are inconsistent with this "Mismatch Hypothesis".[23] On the other hand, while most studies of heterogeneous university returns focus on a local margin (e.g. Hoekstra, 2009; Zimmerman, 2014), I estimate average returns to university quality across subsets of 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 the first 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. Jackson, Johnson and Persico, 2016; Chetty, Hendren and Katz, 2016). I find little evidence that affirmative action narrowed the Black-white mobility gap, which has received particular attention (Dobbie and Fryer Jr, 2011; Billings, Deming and Rockoff, 2014; Chetty et al., 2020*b*; Derenoncourt and Montialoux, 2021), but find that it improved Black students' educational attainment and relatively increased (mostly lower-income) Hispanic youths' wages.

[19]See Backes (2012); Hinrichs (2012, 2014); Blume and Long (2014); Hill (2017); Long and Bateman (2020).

[20]See Alon and Tienda (2005); Howell (2010); Arcidiacono, Aucejo and Hotz (2016). Kapor (2020) identifies a model of affirmative action's effect on enrollment and GPA using variation from the implementation of Texas's race-blind Top Ten policy.

[21]See Antonovics and Backes (2013, 2014); Arcidiacono et al. (2014); Arcidiacono, Aucejo and Hotz (2016). 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.

[22]For canonical examples, see Dale and Krueger (2002) and Arcidiacono (2004). Bowen and Bok (1998) and Arcidiacono (2005) use selection-on-observables and a structural model, respectively, to identify the effect of affirmative action on URM students' wages. Zimmerman (2019) shows that the largest returns to elite Chilean university enrollment accrue only to high-income students.

[23]Two recent 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*c*) discusses the limitations of that previous research 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 US undergraduate institutions and elite Chicago public high schools, respectively.

2.2 Background and Data

2.2.1 University of California Admissions in the 1990s

The University of California system is tasked by the 1960 Master Plan for Higher Education to educate roughly the top 12.5 percent of California public 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 percent of applicants with an average SAT score 1.5 sd above mean) to the less-selective Santa Cruz and Riverside campuses (with an 85 percent admission rate and SAT scores 0.5 sd 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.[24] The policy augmented UC's standard admissions protocol, which required that at least 50 percent of students be admitted solely based on their "Academic Index" (AI), a linear combination of high school GPA and SAT scores.[25] For example, archival documents from UC Berkeley (Figure A.13) show that it guaranteed admission to all applicants above an AI threshold (e.g. 7,150), but set a lower threshold (6,500) for African-American, American Indian, Chicano, and Latino "underrepresented minority" (URM) applicants. Applications with AIs below their respective threshold were "read" by admissions personnel, giving them a variable likelihood of admission, while those with AIs below a second threshold (7,000 for non-URM applicants, below 6,000 for URM applicants) were mostly mechanically rejected.

Figure 2.1 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.[26] At the most-selective Berkeley campus, for example, 1994-1995 URM applicants with AIs 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 zero 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 figure's superscripts show the empirical integrals under each curve by the

[24] "The Educational Opportunity Program was established on campus in 1964 and identified ethnicity, socio-economic status, and educational background as the three criteria it would use in targeting students. This was the first time that race emerged as a positive factor in university admissions." Unsigned memo, 1988; Berkeley Bancroft Library CU-558, Cnt. 5. Affirmative action is now practiced by public universities in at least half of states (see Appendix A.1).

[25] 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 employed variants of this formula.

[26] 'Normal' applicants exclude applicants without UC's minimum academic credentials and applicants to restricted programs like some engineering majors. Appendix A.2 presents annual admissions likelihoods by AI at each campus for 'normal' applicants.

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.[27] Figure 2.1 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.[28]

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 (Atkinson and Pelfrey, 2004; University of California, 2003). 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, 2018*a*).

2.2.2 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 University of California campus.[29] Each record contains an applicant's high school, gender, ethnicity, parental education, parental occupations, and family income.[30] 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-

[27]Prop 209 also prohibited racial preferences in university outreach and financial aid as well as affirmative action policies at the teaching-oriented California State Universities, though their lesser selectivity entailed those policies' smaller impact. Prop 209 also banned racial preferences in state hiring (Marion, 2009) and graduate school admissions (Yagan, 2016), though high school graduates shortly before and after 1998 were similarly-impacted, since both entered the labor market after 1998.

[28]Figure A.14 shows that some UC campuses saw declines in URM admissions and enrollment between 1995 and 1996 relative to academically-comparable non-URM applicants (particularly at UCLA and the less-selective UCs), but every UC campus saw sharp immediate declines in URM admission between 1997 and 1998, and the more selective UC campuses also saw sharp year-over-year declines in URM enrollment. Another approach to estimating the magnitude of each campus's racial preferences is to consider the annual difference between the R^2 of two linear regressions: admission on applicants' leave-one-out admissions probability by AI and ethnicity, and admission on that probability by just AI. Figure A.15 shows, for example, that the difference was about 0.25 at UCLA in '94-95, 0.15 in '96-97, and less than 0.05 after 1998. Most campuses saw small declines in 1996 and large declines in 1998.

[29]About one-third of UC students transfer from community colleges rather than enrolling as freshmen. Because affirmative action was likely less impactful 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.

[30]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 percent of applicants.

grade honors courses.[31] Application, admission, and enrollment indicators are available for each UC campus, as are degree attainment and major choice for UC enrollees.

The second dataset, 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 dataset, linked by full name and birth date.[32] Science, Technology, Engineering, and Mathematics (STEM) majors are categorized by CIP code following the U.S. Department of Homeland Security (2016).[33] NSC data are available starting with the 1995 applicant cohort.[34]

Third, I observe UC applicants' quarterly 2000-2017 wages from the California Employment Development Department, linked by SSN.[35] Wages are unavailable for workers not covered by 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 one percent. About 69 percent of UC applicants have positive covered wages in each of 6-16 years after UC application.

The fourth dataset 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*b*).

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.[36] 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.

[31] 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 percent of applicants submitted ACT instead of SAT scores.

[32] The NSC data include semesterly enrollment (by institution) and attainment (by institution, degrees, and majors) for all Title-IV postsecondary institutions that had commenced reporting to NSC, excluding students who opted against data disclosure.

[33] STEM includes the 278 "fields involving research, innovation, or development of new technologies using engineering, mathematics, computer science, or natural sciences (including physical, biological, and agricultural sciences)" identified by CIP code. Not all NSC majors have CIP codes; I assign each major to its modal CIP code (in the full observed NSC database) for categorization. See Tables A.14 and A.15 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), though a wider variety of majors are categorized, especially among STEM health fields.

[34] Some 1990s NSC records are incomplete, for which reason I augment them with administrative UC records in the undergraduate degree analysis below. Since UC enrollment declined after Prop 209, this could bias estimates of the impact of Prop 209 on degree attainment toward 0. See Appendix A.4.

[35] Social security numbers on UC applications are not verified unless the student enrolls at a UC campus. Among enrollees, the verified social security number differs from that reported on their application in fewer than 0.25 percent of cases. All statistics estimated using EDD data were originally published as institutional research (Bleemer, 2019*b*).

[36] 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/.

2.2.3 University of California Descriptive Statistics

Table 2.1 provides descriptive statistics of UC applications, admissions, and enrollment for California-resident freshman applicants in three two-year cohorts: '94-95, who applied before Prop 209's passage; '96-97, who applied after the ban was approved but before its mandatory implementation; and '98-99, 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 percent overall and 42 percent 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 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 percent 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.[37] Most campuses' URM admissions rates fell slightly in 1996 but then sharply declined in 1998, matched by a sharp rise in URM admits' test scores.[38] 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.

2.2.4 UC Applicants' University Enrollment

Figure 2.2 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 percent of median-AI URM applicants enrolled at the three more-selective UC campuses, while only about 3 percent of similar-AI non-URM applicants did so. After Prop 209, this gap largely closed, and URM applicants across the entire 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 their 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 2.3 broadly summarizes how Prop 209 reshaped UC applicants' enrollment across the

[37] About 20 percent of URM UC applicants were Black, with nearly all of the rest Hispanic. Only a small share of URM applicants are Native American. Among Hispanic applicants, about 75 percent were Chicano and the rest Latino. See Table A.17 for separate descriptive statistics for Black and Hispanic applicants.

[38] Appendix A.3 presents difference-in-difference analysis showing that URM UC applicants became 4-25 percentage points less likely (on average) to be admitted to each UC campus. While URM applicants were 9.3 percentage points more likely than academically-comparable non-URM applicants to be admitted to at least one campus before Prop 209, that gap declined by 7.9 percentage points after 1998. Prop 209 had generally-similar impacts on the admissions likelihood of Black and Hispanic UC applicants: though Black students received somewhat-larger admissions advantages under affirmative action relative to academically-comparable non-URM applicants, Prop 209 caused slightly larger admissions declines for Hispanic applicants to UC's more-selective campuses than for Black UC applicants (see Table A.19).

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.[39] 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 (70-95th percentiles) and decreased for those with somewhat-lower AIs (20-60th percentiles). The same pattern holds at lower AI bands for the selective and less-selective UC campuses: e.g. 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).[40] 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.[41]

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-selective universities where they replaced lower-AI URM students now rejected absent affirmative action.[42] This cascade explains why URM enrollment only declines at the more-selective UC campuses.[43]

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.

2.3 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

[39] Figure A.16 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 '96-97) to students in the top quartile.

[40] 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 Appendix A.4.

[41] Geiser and Caspary (2005) report similar findings for high-testing URM applicants. These out-of-state enrollment estimates are within the confidence intervals of Hinrichs (2020), who argues that affirmative action bans cause minimal cross-state migration.

[42] Figure A.17 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.

[43] See Table A.20.

those campuses could preserve their net enrollment despite the absence of affirmative action.[44] As a result, the estimates presented below identify the impact of Prop 209 on URM outcomes relative to its impact 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.[45] If universities' treatment effects for on-the-margin URM and non-URM students are similar, this implies that as much as 20 percent of the estimates described below could be explained by improved outcomes among non-URM students. I return to this argument in Section 2.6, 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-difference models of the form:

$$Y_{iy} = \alpha_{h_i} + \delta_y + \beta_0 URM_i + \sum_{t=1994}^{2002} \mathbb{1}_{\{t=y\}} \beta_y URM_i + \gamma X_{iy} + \epsilon_{iy} \tag{2.1}$$

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.[46] 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.[47] Standard errors are

[44]Figure 2.3 clearly shows that there is no 'control' group of URM UC applicants in the period; Prop 209 shifted URM UC applicants' college enrollment at every AI, even among the highest-AI URM applicants. Interestingly, it also shows that the non-URM students shifting into more-selective universities tend to have higher AI than the URM students exiting those universities, suggesting that if the baseline results below reflected non-URM student outcomes, they would be driven by high-AI applicants. In fact, most of the estimated effects are driven by low-AI applicants.

[45]Figure A.12 shows that annual growth in net California university enrollment appears unchanged by Prop 209, nor did Prop 209 observably impact the overall weighted-average institutional quality of that enrollment, with gains among non-URM students offsetting declines among URM students.

[46]All OLS estimation is conducted using the *felm* and *summary.felm(,robust)* functions in the *lfe* R package, version 2.8-5.

[47]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). About 15 percent of the sample is missing at least one test score (mostly the third SAT II); dummies are included for each missing value to preserve the full sample. I test models' sensitivity to covariate inclusion in Section 2.5. These detailed covariates (and fixed effects) importantly differentiate the presented enrollment effects of Prop 209 from previously-published results (Chang and Rose, 2010; Antonovics and Backes, 2014) by absorbing sample selection and omitted variable biases.

robust.[48]

I also estimate three variants on this model to better understand Prop 209's effects on student outcomes. First, I separately estimate the model by '96-97 URM AI quartile to observe heterogeneous treatment effects for students with different prior academic opportunities and preparation. Second, because some UC campuses began phasing out their affirmative action policies in 1996, I replace the model's 1996-1997 pre-period with 1994-1995 and re-estimate post-1998 outcomes relative to those earlier years.[49] Finally, I interact β_0 and β_y with indicators for whether the student is Black or Hispanic, identifying separate coefficients for each group to estimate heterogeneity in Prop 209's impact by URM ethnicity.[50]

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 non-random 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 2.5.

2.4 The Impact of Affirmative Action on Student Outcomes

Figure 2.4 visualizes the impact of Prop 209 on URM UC applicants with estimates of β_y from Equation 2.1 for a sequence of enrollment, educational attainment, and labor market outcomes, all estimated relative to 1997. The subsections below discuss each of the measured outcomes in turn. Given that many URM applicants' undergraduate enrollment remained unchanged by Prop 209, the presented reduced-form coefficients likely underestimate impacted students' treatment effect of enrolling at less-selective universities after the affirmative action ban.

2.4.1 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.[51] Prop 209 led to larger relative enrollment declines at the more-selective UC campuses for Black applicants, with the top AI quartile of Black applicants facing a 15 percentage point enrollment decline.[52]

I summarize these changes in university enrollment quality by characterizing each institution in two ways: (1) using traditional measures of university quality like selectivity and graduation

[48]Following Abadie et al. (2017), given that the data comprise the full population of UC applicants and that there is little reason to expect correlated random effects across any particular clusters of applicants, I do not cluster the reported standard errors.

[49]All models estimating National Student Clearinghouse outcomes omit 1994 applicants, for whom NSC records are unreliable.

[50]I omit Native American applicants from this final specification due to small sample size.

[51]See Table A.21. The empirical integral of URM students' relatively changed enrollment at each UC campus by AI between '95 and '98-99 – over the '98-99 distribution of URM UC applicants – provides a lower-bar estimate (assuming Prop 209's monotonicity across UC campuses) for the number of URM students who enter and exit each campus as a result of Prop 209. Table A.23 shows that at least 1,200 URM UC applicants exited UC campuses – with more than 800 exiting Berkeley and UCLA – and 800 entered UC campuses after Prop 209.

[52]See Table A.22.

rate, and (2) 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 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. (2020*a*); "CFSTY").[53] Appendix A.9 provides methodological details and the estimated value-added statistics.

Table 2.2 presents difference-in-difference estimates of how Prop 209 impacted URM UC applicants' quality of enrollment institution. The first row shows that prior to 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-30s wages, with smaller value-added declines among high-AI URM applicants.[54] The first panel of Figure 2.4 shows that the institutions where URM UC applicants enrolled remained relatively steady in terms of their "CFSTY" early-30s 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.

2.4.2 Degree Attainment

Next I turn to Prop 209's effects on URM UC applicants' educational outcomes: whether they earned a Bachelor's degree, an undergraduate STEM degree, and/or a graduate degree.[55] Given that Prop 209 caused the average URM UC applicant to enroll 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 2.4 presents estimates

[53] 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 over-estimation of the VA decline following Prop 209. Nevertheless, I show below that the wage value-added estimates underestimate the actual observed change in URM applicants' wages, suggesting that both value-added procedures understate selective universities' treatment effects among the URM students impacted by Prop 209.

[54] Table A.24 shows slightly-larger estimates when compared to the 1995 pre-209 baseline.

[55] I define Bachelor's and STEM degree attainment using the union of UC administrative records and the National Student Clearinghouse records, while graduate degree attainment is measured only in NSC (within 18 years of UC application). Bachelor's attainment and STEM major choice are measured using the union of UC and NSC records to augment imperfect NSC records from UC Santa Cruz; see Appendix A.4. This may upwardly bias the resulting estimates, since URM students are less likely to enroll at UC campuses following Prop 209 and thus less likely to have the opportunity that their degrees are measured in UC administrative data. Estimates for each separate data source (restricting UC data to UC enrollees) are presented in Table A.26; estimates are somewhat more-negative in NSC data and less-negative in UC data among UC enrollees.

from Equation 2.1 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 2.3 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 applicants before Prop 209, and if anything became even less likely to earn degrees after affirmative action was eliminated, with a 95-percent confidence interval of -1.69 to 0.27 percentage point change in average six-year degree attainment.[56] 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.[57]

The third and fourth columns of Table 2.3 show that URM applicants may have become less likely to earn STEM degrees conditional on earning a college degree (95-percent c.i. -1.65 to 0.35 percentage points).[58] 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 (s.e. 0.4). Table A.27 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 2.3 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, 2020) and may represent an important benefit to more-selective university enrollment. URM applicants became 1.3 percentage points (s.e. 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 (s.e. 0.21).[59] There is only weak evidence of a decline in law degree attainment, and no such evidence for medical degrees.[60]

[56]These estimates contrast with those presented by Arcidiacono et al. (2014), whose Table 3 suggests that Prop 209 increased URM UC graduation rates. Bleemer (2020*c*) shows that those findings reflect selection bias on applicant characteristics unobserved in those data: 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. The remaining difference between the two studies is explained by that study's sample restriction to UC enrollees.

[57]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 (2018*a*) 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), while applicants in the other quartiles do not appear to have faced declines in degree attainment despite enrolling at institutions with lower graduation rates.

[58]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, 2018*a*). I further analyze Prop 209's effect on UC enrollees' performance and persistence in STEM courses in Section 2.7.

[59]STEM graduate degrees are defined as masters- or doctoral-level degrees in any STEM field; see footnote 22.

[60]Table A.25 shows that URM UC applicants' educational declines after Prop 209 are generally somewhat larger when compared to a 1995 baseline, before some campuses began phasing out affirmative action.

2.4.3 Employment and Wages

Finally, I turn to the effect of Prop 209 on URM UC applicants' labor market outcomes. Figure 2.5 shows estimates of $\beta_{\cdot 98-99}$ annually estimated for each specified outcome six to sixteen 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 percent of applicants earned covered California wages annually before and after Prop 209.[61] Among wage-earning UC applicants, however, Prop 209 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 30s. As late as age 34, there is no evidence that the average wages of URM applicants impacted by Prop 209 recover to their earlier levels. Table A.28 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 2.4 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 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.[62] 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 one 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 2.4 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 difference-in-difference statistics in the presence of bounded group-specific trends.[63] Figure A.20 shows that the wage estimates presented in Panel (e) of Figure 2.4 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 percentiles per year.

Table 2.4 summarizes the changes in URM UC applicants' wages following Prop 209, showing that academically-comparable URM and non-URM workers earned similar wages before

[61] Figure A.19 shows that California labor market participation is unchanged after Prop 209 for all four AI quartiles of URM applicants. Prop 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.

[62] The wage distributions are observed among employed college-educated 2001-2017 ACS respondents (Ruggles et al., 2018).

[63] I estimate Rambachan and Roth (2020)'s fixed length confidence intervals using their *HonestDiD* package, version 0.1.0.

Prop 209 but diverged afterwards. 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).[64] 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 2.6 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.

While Prop 209 caused a small number of mostly-Black URM UC applicants to enroll at out-of-state Ivy+ institutions, the impact 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 effects of Prop 209 on Black and Hispanic applicants reported in Table 2.4.

Contextualizing Prop 209's Labor Market Impact

While 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 impact the composition of California's high-earning workers. About 56,000 URM students applied to UC between 1998 and 2002. Compared to a 1996-1997 baseline, the difference-in-difference 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 per 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.[65] 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 percent.[66] Given that 30-to-34 URM workers made up 46 percent of the 2010 California

[64]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.

[65]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 percent of UC applicants 14 years after application. For annual estimated URM wage threshold declines relative to each baseline, see Figure A.21.

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

workforce but only 14 percent of earners over \$100,000, this implies that affirmative action had been meaningfully mitigating inequality until Prop 209.

Figure A.22 shows that the fraction of early- and mid-30s 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.[67] For example, relative to a 2010 baseline, URM Californians between ages 33 and 37 became about 10 percent 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: (1) 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 (2) URM high school graduates deterred from UC application by Prop 209. The next section quantifies the magnitude of this latter group.

2.5 Application Deterrence and Model Robustness

The primary potential threat to the difference-in-difference 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*a*; Dickson, 2006; Yagan, 2016).[68] 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:

$$\frac{A_{syea}}{UC_{sye}} = \sum_{e' \in \{A,B,H\}} \sum_{y' \in \{96,98,00\}} \beta_{e'y'a} \mathbb{1}_{e=e', y \in \{y', y'+1\}} + \zeta_{sea} + \eta_{sya} + \epsilon_{syea} \tag{2.2}$$

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 '94-95 to '00-01; 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 2.2 by weighted least squares (weighting to the student level using UC_{sye}) separately for each a, and interpret β_{e98a}

[67] For this ACS analysis, I define Californians as those *born* in the state, to identify those likely impacted by Prop 209 and abstract away from post-education cross-state mobility.

[68] 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. Appendix A.6 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 the conclusion; in fact, after Prop 209 many highly-qualified URM public high school graduates sent SAT scores to a UC campus but nevertheless did not apply.

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.[69]

Figure 2.7 presents estimates of the Black and Hispanic $\beta_{e,\text{`}98-99,a}$ coefficients from Equation 2.2, scaled by the average total number of e UC-eligible California high school graduates in the '98-99 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.[70] 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.[71] The sum across the bars suggests that the number of Black and Hispanic UC applicants declined by 12-13 percent (about 1,200 per year), most of whom would have likely been admitted to some UC campus.[72]

Given this shift in the UC applicant pool, I test for the magnitude of sample selection bias in the baseline difference-in-difference estimates in the previous section by re-estimating 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).[73] 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.[74] In addition to the inverse mills ratios of these p statistics, I also 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.[75]

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[69]Table A.29 presents estimated coefficients for a specification of Equation 2.2 across all AI. It shows that URM application rates following Prop 209 declined by between 4 and 6 percent of all UC-eligible URM public high school graduates.

[70]That is, the blue 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 Figure A.11 for evidence that e and AI were highly predictive of applicants' admission at most UC campuses, even after 1998. Not every UC-eligible applicant was admitted to a UC campus; many were rejected from each campus to which they applied, and even the least-selective Riverside campus rejected low-AI applicants with certain intended majors. Admit estimates implicitly assume that each UC applicant's admission is small relative to the size and composition of the applicant pool.

[71]Table A.23 links these application declines to the AI- and campus-specific enrollment changes presented in Figure 2.3 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 '98-99, or 14 percent. This closely matches the differently-calculated estimates of Bleemer (2019*a*).

[72]Figure A.23 presents additional specifications of Equation 2.2. 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 percent relative to white applications.

[73]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. Its inclusion absorbs cross-group selection into UC application.

[74]As expected, including either of these p statistics as covariates in Equation 2.1 yields statistically-significant negative coefficients (implying negative selection out of UC application), while their inverse mills ratios yield significant positive coefficients.

[75]Rank is determined using UC GPA among UC applicants in that school-year. Parental education indicates the

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 re-estimate Equation 2.1 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 Figure 2.8 for six main outcomes.

While 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.[76] 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, though they may reflect sample selection bias on unobservables like orthogonal dimensions of their high school leadership activities.

2.6 Impact of Prop 209 on Non-URM UC Applicants

Prop 209 did not measurably impact the overall weighted-average institutional value-added of enrollment at public or all California universities (see Figure A.12); the decline in enrollment quality among URM students was offset by an improvement among non-URM students. As discussed in Section 2.3 above, I interpret the baseline difference-in-difference estimates as the impact of Prop 209 on URM UC applicants, despite the fact that – assuming constant treatment effects – as much as 20 percent of each estimate may reflect changes among non-URM applicants caused to enroll at more-selective universities. 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, 2018*a*), implying that non-URM outcomes explain less than 20 percent of each baseline estimate. First, single-difference estimates show that non-URM wage outcomes are smooth in the years before and after Prop 209, while URM wage outcomes sharply and persistently decline in 1998 (see Figure A.24). While this provides suggestive evidence of relatively small returns to more-selective UC enrollment for "crowding-in" non-URM students, the absence of an unimpacted comparison group prohibits separate identification of Prop 209's effect on non-URM students and secular trends.

Second, I employ 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

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applicants' parents' highest education level (with seven codes); parental occupation indicates the parents' occupation set (with 17^2 codes). Covariates with missing values are included with missing value indicators.

[76]For example, high school fixed effects explain 8.8 percent of variation in six-year degree attainment among bottom-AI-quartile UC applicants (Panel (a)); the addition of the AI covariates brings the R^2 to 12.9 percent; and adding the full suite of additional covariates raises the R^2 to 15.3 percent. Those same three R^2's for conditional log wages are 3.0, 5.8, and 6.9 percentage points. These increasing R^2 values suggest that adding sociodemographic covariates could have been expected to shift the estimated treatment effect of Prop 209 if the estimates exhibited sample selection bias on observables.

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.[77] Admissions officers then admitted some lower-AI applicants based on other application characteristics. Figure 2.10(a) shows the admissions likelihood of '96-97 non-URM Berkeley applicants at every AI, adding 70 points to 1996 AIs to align the two years' thresholds (7,360 and 7,430); admission was near-guaranteed above the threshold and provided to only half of slightly below-threshold applicants. Because applicants near Berkeley's admissions threshold are quasi-randomly distribution 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 '96-97 applicants using local linear regression discontinuity models following Calonico, Cattaneo and Titiunik (2014).[78] Figure 2.10(b) shows that the increased likelihood of Berkeley 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 unimpacted at the threshold.[79] Most of the students would likely have otherwise enrolled at UCLA or UCSD (6.1 percentage points, s.e. 3.5) or out-of-state universities (8.0 percentage points, s.e. 3.4).

Panels (d) to (f) of Figure 2.10 show that graduate school enrollment, early-30s wages, and the number of years spent by each applicant in their early 30s earning over \$150,000 per year are smooth across the Berkeley admissions threshold.[80] While 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

[77]See Figure A.13. Berkeley chose its annual threshold so that 50 percent of its admitted applicants had AI above the threshold. As a result, the threshold could not be chosen until after Berkeley observed all applicants' AIs, prohibiting applicants from manipulating their AI to exceed the threshold. Admissions around the threshold was noisier in '94-95; see Figure A.1.

[78]Estimates are produced using the *rdrobust* package, version 0.99.8 (Calonico, Cattaneo and Titiunik, 2015). Each plot visualizes the 6,086 '96-97 non-URM Berkeley applicants within 400 AI points of the threshold; regressions include a 1997 indicator covariate. The distribution of applicants is smooth across the threshold, with the McCrary (2008) test yielding a p-value of 0.58 at the threshold. Sociodemographic characteristics are also smooth across the threshold: I predict annual log early-30s wages by gender-ethnicity indicators, log parental income, and parental education among '96-97 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.

[79]See Appendix A.9 for value-added definition. The estimated change in institutional six-year graduation rate across the threshold is -0.2 percentage points, with a 2.1 standard error. About 9 percent of near-threshold students have no observed four-year enrollment, with only 1 percent enrolling at a community college but no four-year institution within six years.

[80]As above, "early 30s" is defined as 12-16 years after UC application, when most applicants are 30-34. There is no estimated change in likelihood of California employment across the Berkeley access threshold; despite their increased likelihood of out-of-state university enrollment, applicants' number of early-30s years employed in California increases by 0.14 years (s.e. 0.17). I use \$150,000 as a threshold instead of \$100,000 (as above) because of the strongly positively-selected sample, with one-third of in-sample applicants (within 400 AI of the threshold) earning over \$100,000 in their early 30s. \$150,000 is a better indicator of unusually high wages, achieved in an average 0.60 out of 5 years for in-sample applicants.

to UC Berkeley provides little measurable long-run economic return.

Appendix A.10 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 non-random 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.

2.7 STEM Course Performance and Persistence

Having documented Prop 209's high-level effects on impacted 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'd enrolled at a less-selective university with lower-testing peers.[81] However, no previous study has directly examined the impact of affirmative action on URM students' actual course performance and STEM course progression, instead focusing on overall grade point averages and major choice.[82] Complementing the 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 "Science Mismatch Hypothesis" by estimating the impact of Prop 209 on URM UC enrollees' performance and persistence along introductory STEM course sequences.[83]

Using five UC campuses' detailed course enrollment records, I match core introductory STEM course sequences across these campus (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-difference models estimated above:[84]

[81] For example, Loury and Garman (1993) argue that "with higher required levels of performance and smaller offsetting increases in actual performance, blacks at more selective schools will have poorer grades, be less likely to graduate, and choose less lucrative majors than if they had attended less selective institutions." Recent scholarship has frequently proxied "lucrative majors" with the STEM major designation; Arcidiacono, Aucejo and Hotz (2016), for example, notes that "STEM majors [earn] substantially more than other college degrees with the exception of perhaps business ... and the STEM premium has increased over time".

[82] Differences in overall GPAs are at least as likely to reflect differing grading standards across departments and between lower- and upper-division courses as they are to reflect student course performance (Arcidiacono, Aucejo and Spenner, 2012; Bleemer and Mehta, 2020*a*). 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.

[83] The main analysis below tests the Science Mismatch Hypothesis as stated by Griffith (2010) and Arcidiacono, Aucejo and Hotz (2016). Other studies have tested narrower versions of the Hypothesis, claiming only that URM students admitted under affirmative action are lower-performing in STEM courses than their non-URM peers, unconditional (Loury and Garman, 1993; Holzer and Neumark, 2000; Fischer and Massey, 2007) or conditional on prior academic preparation (Rose, 2005). I further analyze these alternative Hypotheses by examining the course performance and persistence of UC Berkeley students before and after Prop 209 in Appendix A.7, finding little evidence to support either.

[84] Introductory STEM courses include four courses in Chemistry (two introductory, two organic), two in Biology,

$$Y_{iysm} = \alpha_{h_i} + \delta_y + \beta_0 URM_i + \sum_{t=1994}^{2002} \mathbb{1}_{\{t=y\}} \beta_y URM_i + \gamma X_{iy} + \epsilon_{iysm} \tag{2.3}$$

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 4 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).[85] The model is stacked over s and estimated across courses, weighted evenly across students. Covariates X_{iy} include the components of AI as above. 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 weighted evenly across individuals, persistence can be heuristically understood as ranging from 0 to 100 percent. A student whose only completed STEM course is Chemistry 1, without ever completing Chemistry 2, would have persistence of 0 percent. A student who takes Chemistry 1, 2, and 3 but not 4 would have persistence 66.6 percent, since they persisted after two courses but not the third. A student who takes only all 3 Computer Science courses would have persistence of 100 percent. 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.[86] 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, '96-97 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 2.9 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 percent 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. Figure A.25 shows that Prop 209 similarly impacted Black and Hispanic UC enrollees' STEM persistence and performance outcomes.

I also estimate a difference-in-difference model of UC enrollees' likelihood of completing any STEM major (following Equation 2.1). URM UC enrollees' STEM major choice is precisely

two in Physics, and three in Computer Science. In nearly all cases, each of these courses 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. Specific course details are provided in Appendix A.8. Estimates are largely insensitive to omitting students in Colleges of Engineering, who may face different incentives around completing STEM course sequences.

[85] Persistence is not defined for the final course in each sequence. Repeated course grades are omitted.

[86] See Table A.30, and Table A.31 for course-specific estimates.

unchanged relative to academically-comparable non-URM enrollees after Prop 209, with a 95 percent confidence interval rejecting increases above 1.5 percentage points.[87] 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.

2.8 Discussion: Affirmative Action and Efficiency

The evidence presented above have 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 efficiency of affirmative action can be measured by the net impact 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.[88] Since net enrollment at more- and less-selective universities appears roughly unaffected by Prop 209, this net effect can instead be summarized by the average relative returns to more-selective university enrollment for these two groups of students.[89]

The single-difference and regression discontinuity estimates presented in Section 2.6 suggest that the non-URM students whose enrollment was impacted 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 Figure A.1).

That hypothesis is further supported by a comparison between the change in URM students' early-30s wages and the change in the wage value-added of their enrollment institutions. While Prop 209 led URM students to enroll at universities with lower early-30s wage value-added by as much as $1,000, those students' actual early-30s annual wages fell by more than $2,000 (see Tables 2.2 and 2.4). 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 impacted by affirmative action is significantly larger than universities' average treatment effect.[90] While

[87]The overall decline in STEM attainment thus appears driven by students who exit these UC campuses following Prop 209.

[88]Pareto efficiency is very unlikely in this context; even a single non-URM student benefiting from more-selective enrollment as a result of Prop 209 would prove the policy's inefficiency. This section instead focuses on Kaldor-Hicks allocative efficiency.

[89]Figure A.12 shows that enrollment growth at California universities may have slowed in 1997 and 1998, but that the decline in URM Californians' average institutional value-added was matched by an increase among non-URM Californians, resulting in no net change in enrollment quality after Prop 209.

[90]As discussed above, there is reason to believe that the presented value-added statistics remain somewhat biased by positive selection into more-selective universities, suggesting that they relatively overestimate differences between

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.[91] These evidence suggest that affirmative action improved the allocative efficiency of California higher education.

2.9 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 percent 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 percent. 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 that it causes to enroll. However, single-difference and regression discontinuity evidence suggest that those impacted 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 California higher education. White and Asian students were proportionally impacted by Prop 209, with no evidence of disparate impacts for one or the other.

These findings differ from several existing analyses of the impacts 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 to account for sample selection and omitted variable bias. They also contextualize the impact of university affirmative action policies

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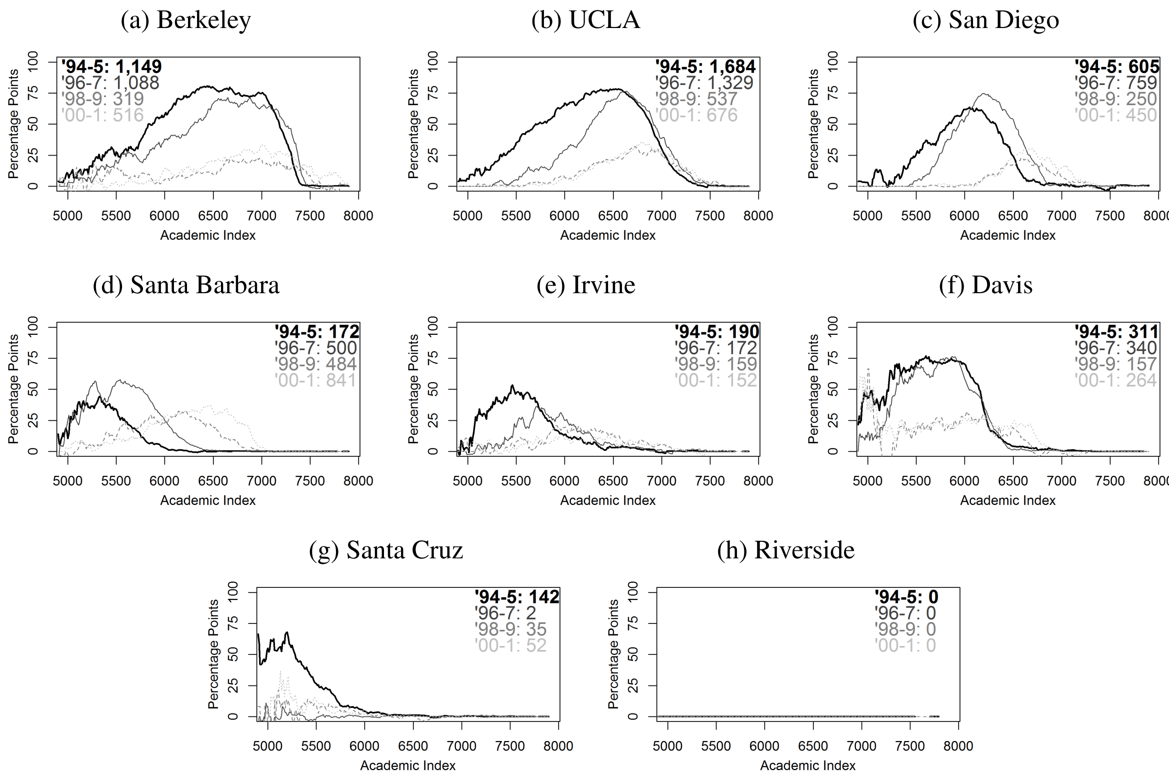
universities. Moreover, the VA estimates by quartile show that the VA wage estimates generally poorly match the observed effects of Prop 209, with the true impact more widely distributed across the AI distribution than the expected effects based on changes in VA. Figure A.26 visualizes these discrepancies, plotting smoothed (but not covariate-adjusted) difference-in-difference averages for both VA and actual degree attainment and early-30s wages. The two lines poorly mirror each other, suggesting both that VA poorly-explains and substantially underestimates the observed labor market effects of Prop 209.

[91] Table A.13 presents VA and observed degree attainment and early-30s wages for several VA specifications, aligning samples for missing data. In addition to confirming the discussion above, it shows that extending the "MH" approach to indicators for the set of *all* universities to which the applicant applied (as proxied by SAT score sends) somewhat improves the associated wage VA estimates, while allowing gender- and ethnicity-specific VA coefficients (using the "CFSTY" approach) yields precise 0's for the wage VA estimates across all AI quartiles, implying particularly poor performance.

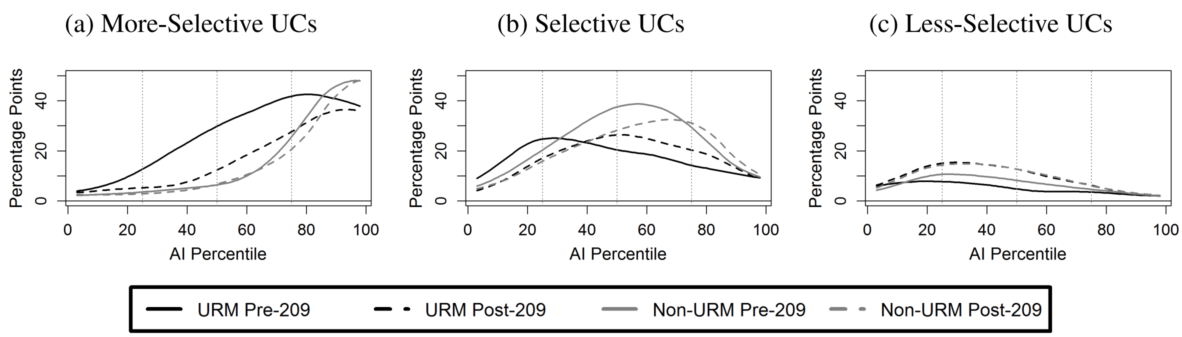
relative to other policies aiming to close opportunity gaps for low-income and Black and Hispanic youths. Some limitations remain. The presented estimates are reduced-form, averaging over many URM students who were likely unimpacted by the Prop 209 policy change, which means that they likely underestimate 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 U.S. – though perhaps not to close white-Black mobility gaps – and the equity and efficiency consequences of affirmative action's prohibition.

Figure 2.1: ‘Normal’ URM UC Applicants’ Greater Likelihood of Admission by Campus, Year, and AI



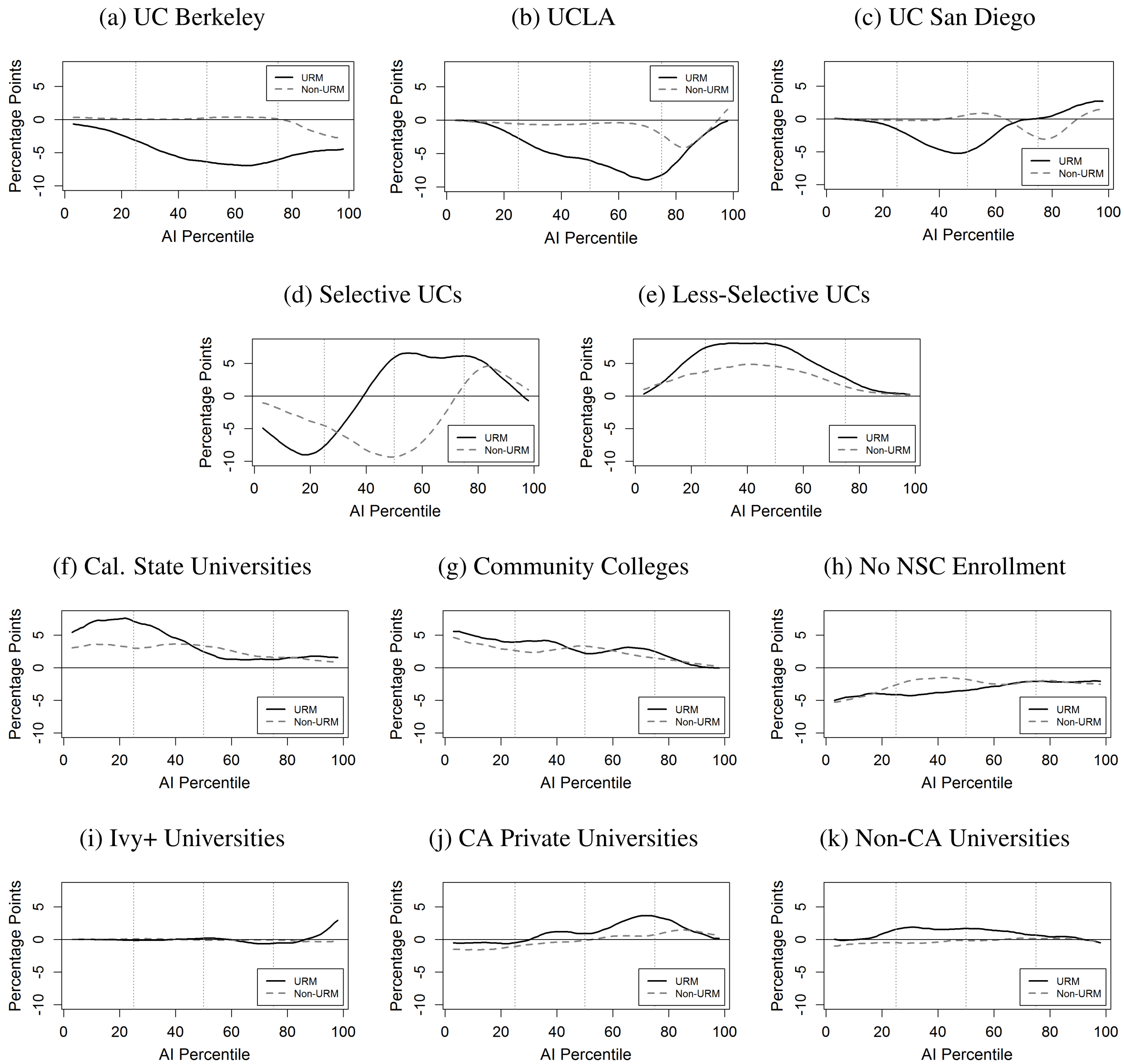
Note: The difference between the percent of URM applicants and the percent of non-URM applicants admitted to each campus by academic index (AI), in each of four two-year periods (1994-2001), with darker lines corresponding to earlier periods. The two later periods are after the implementation of Prop 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 (a) were UC-eligible, which means that they satisfactorily completing the required high school coursework, and (b) 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.

Figure 2.2: UC Enrollment before and after Prop 209 by Ethnicity and AI Percentile



Note: The percent of all UC applicants who first enroll at each set of UC campuses before ('96-97 cohorts) and after ('98-99 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.

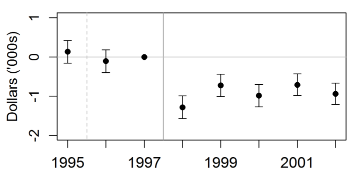
Figure 2.3: Changes in University Enrollment after Prop 209 by Ethnicity and AI Percentile



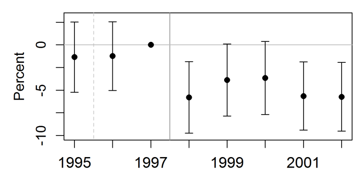
Note: Difference in percent of UC applicants who first enroll at each postsecondary institution(s) between 1998-1999 and 1996-1997, by URM status and by percentile of academic index (AI) measured among 1996-1999 URM UC applicants. First enrollment measured in NSC up to six years after UC application; university groups partition possible enrollments. Statistics are smoothed with a triangular kernel with bandwidth 15. "Ivy+" universities include the Ivy League, MIT, Stanford, and U. Chicago; private and non-CA universities exclude those institutions. Source: UC Corporate Student System and National Student Clearinghouse.

Figure 2.4: Annual Difference-in-Difference Estimates of URM UC Applicants' Outcomes after Prop 209

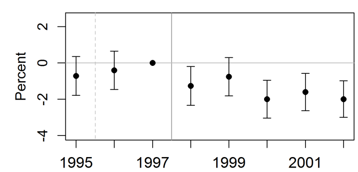
(a) Institutional Wage "Value-Added"



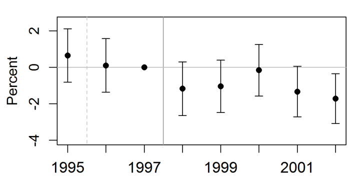
(b) BA Attain., Bottom AI Q.



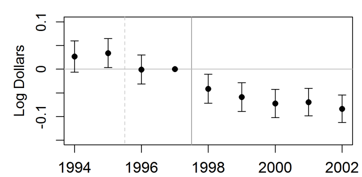
(c) STEM Degree Attainment



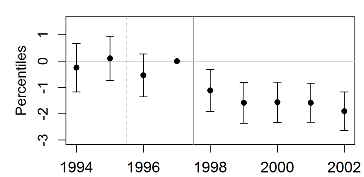
(d) Grad. Degree Attainment



(e) Avg. Annual Conditional Log Wages

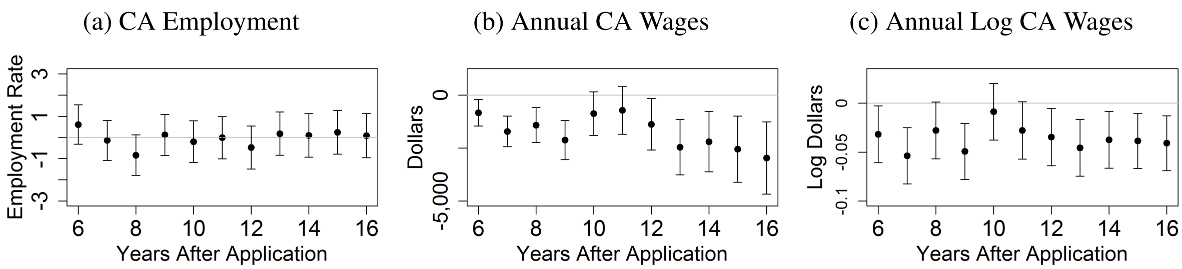


(f) Avg. Eth-Specific Wage Percentile



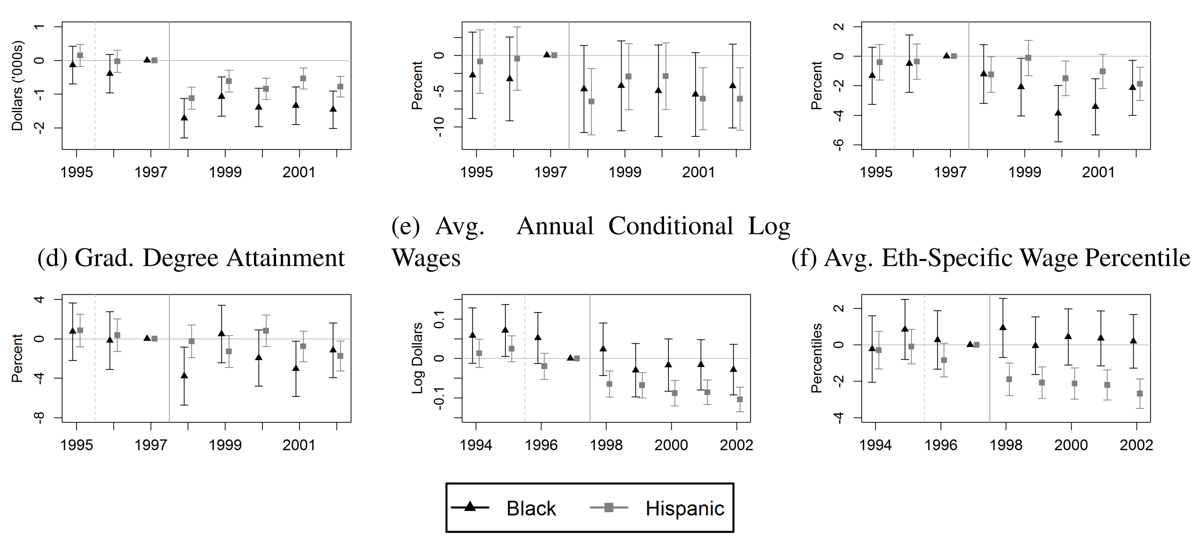
Note: OLS difference-in-difference coefficient estimates of Equation 2.1, the change in URM UC applicant outcomes relative to non-URM applicants, compared to the 1997 baseline. For details on outcomes (a) to (e), see notes to Tables 2.2 (with institutional value-added estimated following Chetty et al. (2020*a*)), 2.3, and 2.4. 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 footnote 47); 1994 NSC data are omitted. Panel (b) restricts the sample to the bottom AI quartile as measured among '96-97 URM UC applicants. Bars show robust 95-percent confidence intervals. Source: UC Corporate Student System, National Student Clearinghouse, California Employment Development Department, and the American Community Survey (Ruggles et al., 2018).

Figure 2.5: Annual Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Wage Outcomes



Note: Estimates of $\beta_{\cdot 98-99}$ from Equation 2.1, an OLS difference-in-difference model of 1996-1999 URM UC freshman California-resident applicants' employment outcomes compared to non-URM outcomes after Prop 209. Outcomes defined as non-zero California wages ("CA Employment") and California wages in dollars and log-dollars (omitting 0's) 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 footnote 47). Academic Index (AI) is defined in footnote 25. Annual wages CPI-adjusted to 2018 and winsorized at top and bottom 1 percent. Robust 95-percent confidence intervals shown. Figure A.18 presents separate estimates for Black and Hispanic applicants. Source: UC Corporate Student System and the California Employment Development Department.

Figure 2.6: Annual Difference-in-Difference Estimates of URM UC Applicant Outcomes by Ethnicity



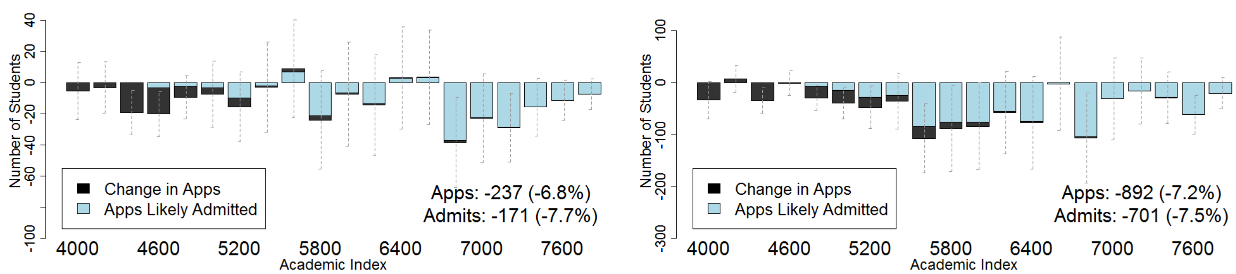
(a) Institutional Wage "Value-Added" (b) BA Attain., Bottom AI Q. (c) STEM Degree Attainment

(d) Grad. Degree Attainment (e) Avg. Annual Conditional Log Wages (f) Avg. Eth-Specific Wage Percentile

Note: OLS difference-in-difference coefficient estimates of an extension of Equation 2.1 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) to (e), see notes to Tables 2.2, 2.3, and 2.4; institutional value-added is estimated following Chetty et al. (2020*a*). 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 impacted in university enrollment by Prop 209. Models include high school fixed effects, ethnicity indicators, and the components of UC's Academic Index (see footnote 47); 1994 NSC data are omitted. Panel (b) restricts the sample to the bottom AI quartile as measured among '96-97 URM UC applicants. Native American applicants are omitted. Bars show robust 95-percent confidence intervals. Source: UC Corporate Student System, National Student Clearinghouse, California Employment Development Department, and the American Community Survey (Ruggles et al., 2018).

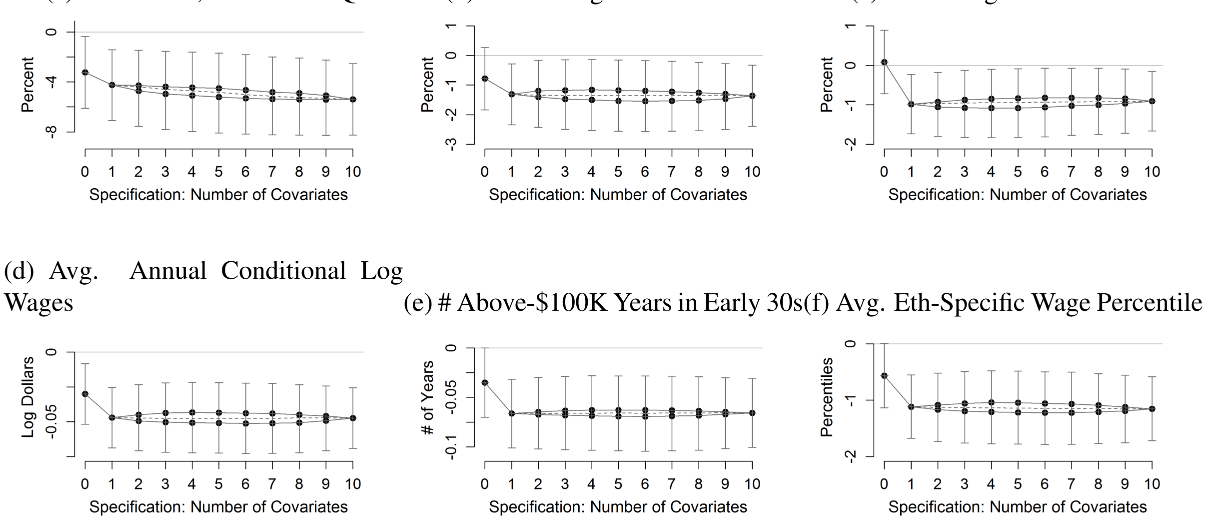
Figure 2.7: Estimated Declines in Annual 1998-99 Applications and Admissions by Ethnicity

(a) Black (b) Hispanic



Note: Estimates of the change in the annual number of UC applicants (and admits) in 1998-1999 by ethnicity (e) and 200-point AI bin, relative to 1994-1995. The height of each black bar is the product of $\beta_{e,98-99,a}$ (estimated in Equation 2.2) and $\sum_s UC_{s,98-99,e}$, the average number of UC-eligible California public high school graduates of ethnicity e in 1998-1999. The height of each overlaying blue bar is the product of the black bar and the percent of 1998-1999 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. 95-percent confidence intervals on the black bars from $\beta_{e,98-99,a}$ robust standard errors. Source: UC Corporate Student System and the California Department of Education.

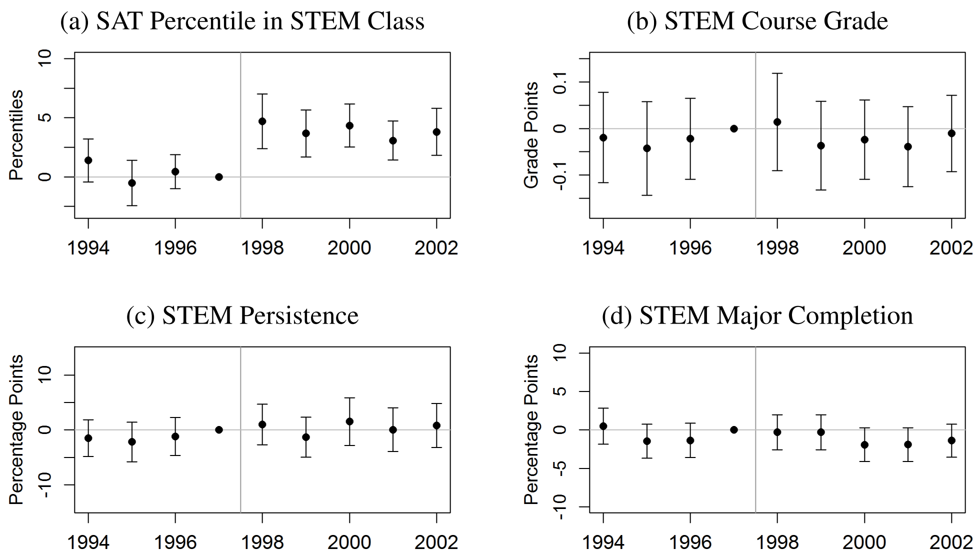
Figure 2.8: Alternative Covariate Specifications of URM UC Applicants' Post-1998 Estimated Outcomes



(a) BA Attain., Bottom AI Q.
(b) STEM Degree Attainment
(c) Grad. Degree Attainment
(d) Avg. Annual Conditional Log Wages
(e) # Above-$100K Years in Early 30s
(f) Avg. Eth-Specific Wage Percentile

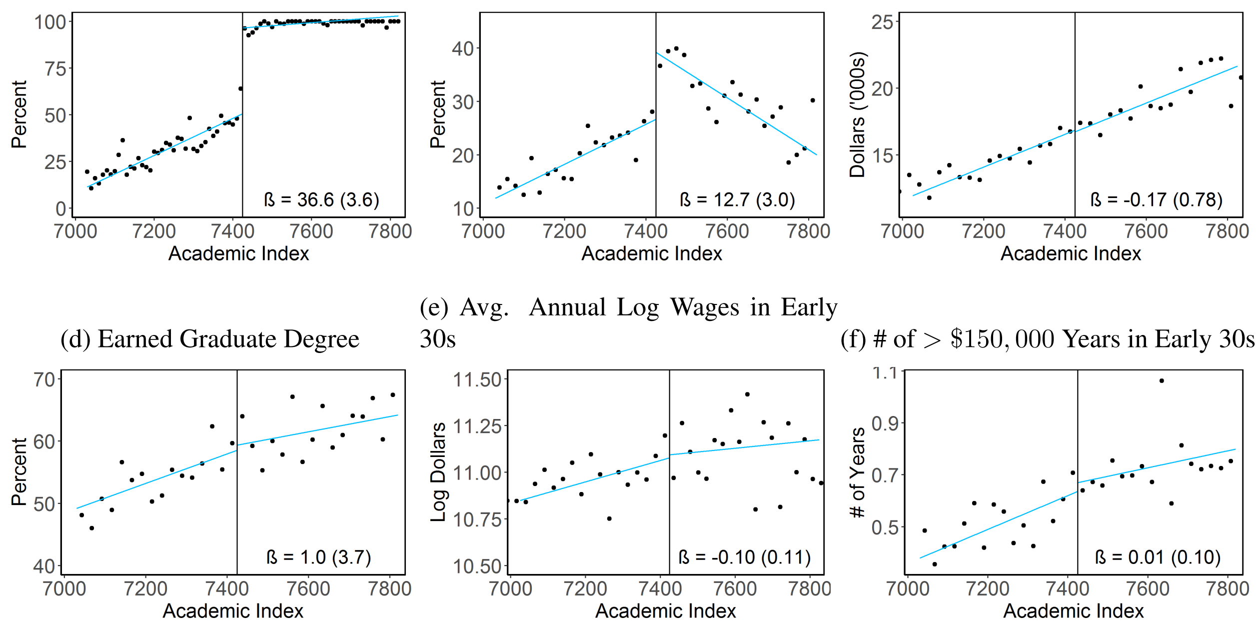
Note: Estimates of $\beta_{\cdot 98-99}$ from Equation 2.1, an OLS difference-in-difference model of the change in six '96-99 CA-resident freshman URM UC applicant outcomes after Prop 209 relative to non-URM applicants, with different specifications of the covariate matrix X_{iy}. Specification 0 sets X_{iy} to be null, while Specification 1 includes the components of UC's Academic Index (see footnote 47). Specifications 2-10 add additional sets of covariates progressively, presenting the highest and lowest $\beta_{\cdot 98-99}$ estimates from models including 1-9 additional sets of covariates, respectively: gender indicator, log family income, (7) highest parental education indicators, (289) parents' occupation indicators, high school GPA rank, number of 12th-grade honors courses, UC eligibility indicator, and Heckit control functions constructed using two estimates of p: $\frac{A_{s_i yea}}{UC_{s_i ye}}$ (see Equation 2.2) and the leave-one-out percent of UC-eligible graduates who applied to UC that year in i's school, gender, and ethnicity. For details on outcomes, see notes to Table 2.3 and 2.4. Panel (a) restricts the sample to the bottom AI quartile as measured among '96-97 URM UC applicants. Bars show the union of the robust 95 percent confidence intervals of the two presented estimates. Source: UC Corporate Student System, National Student Clearinghouse, and California Employment Development Department.

Figure 2.9: Difference-in-Difference Estimates of URM UC Enrollees' STEM Performance and Persistence



Note: Difference-in-difference WLS regression coefficient estimates of UCB, UCSB, UCD, UCSC, and UCR enrollees' introductory STEM course performance or persistence, differencing across URM status following Equation 2.3, 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 Appendix A.8) 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 footnote 47). UCSC is omitted from the GPA model because it did not mandate letter grades in the period. 95-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*b*).

Figure 2.10: Estimated Return to '96-97 UC Berkeley Enrollment for On-the-Margin Non-URM Applicants



(a) UC Berkeley Admission

(b) UC Berkeley Enrollment

(c) "CFSTY" Inst. VA

(d) Earned Graduate Degree

(e) Avg. Annual Log Wages in Early 30s

(f) # of $> \$150,000$ Years in Early 30s

Note: Regression discontinuity plots and estimates around the 1996-1997 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 2.2, 2.3, and 2.4 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.

Table 2.1: Descriptive Statistics of 1990s UC Admissions by Ethnicity

	Application			Admission			Enrollment		
	'94-5	'96-7	'98-9	'94-5	'96-7	'98-9	'94-5	'96-7	'98-9
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	1224	1227	1237	1320	1335	1339	1277	1294	1299
Selective UCs	1156	1160	1171	1193	1202	1222	1140	1156	1172
Less Selective UCs	1135	1134	1138	1157	1154	1158	1124	1121	1123
All UCs	1182	1187	1194	1207	1212	1216	1196	1208	1217
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	1054	1068	1083	1131	1158	1194	1102	1125	1149
Selective UCs	1017	1030	1045	1057	1074	1102	1018	1040	1068
Less Selective UCs	985	993	1006	1008	1019	1034	977	987	1004
All UCs	1025	1039	1048	1054	1071	1081	1052	1071	1077

Note: Count and mean average descriptive statistics of 1994-1999 California-resident freshman UC applicants who are or are not underrepresented minorities (URM). 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 1600 scale. Percent admitted and percent enrolled are conditional on applying to that campus. Campus-specific statistics are presented in Table A.16. Descriptive statistics by ethnicity available in Tables A.17 (Black and Hispanic) and A.18 (white and Asian). Source: UC Corporate Student System.

Table 2.2: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 University Quality

	First Four-Year Institution			First Institution			
				"MH" VA[1]		"CFSTY" VA[1]	
	Adm. Rate	Avg. SAT	6 Yr. BA Rate	BA 6	Earn 30s	BA 6	Earn 30s
Panel A: Difference-in-Difference 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 $\times$ Prop 209	3.6	-19.7	-1.7	-0.6	-384	-1.0	-922
($\beta_{'98-99}$)	(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 $\times$ Prop 209 ($\beta_{'98-99}$) by AI Quartile							
Bottom	1.8	-25.5	-3.3	-1.6	-638	-1.9	-796
Quartile	(0.6)	(3.7)	(0.6)	(0.4)	(214)	(0.5)	(246)
Second	5.2	-28.7	-3.0	-0.5	-618	-1.3	-1,547
Quartile	(0.5)	(3.0)	(0.5)	(0.4)	(197)	(0.4)	(237)
Third	5.6	-21.1	-1.0	0.1	-374	-0.4	-1,273
Quartile	(0.5)	(2.7)	(0.4)	(0.3)	(182)	(0.3)	(218)
Top	2.0	-7.4	-0.7	-0.8	-157	-1.0	-480
Quartile	(0.4)	(2.4)	(0.3)	(0.3)	(224)	(0.3)	(233)

Note: Estimates of β_0 and $\beta_{'98-99}$ from Equation 2.1, a difference-in-difference 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 footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 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 25^{th} and 75^{th} 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 in parentheses. [1]Value-added measures are estimated by regressing six-year BA attainment (in NSC) or average 12-to-16 year conditional wages (in EDD), when most applicants are in their early 30s, on college indicators, year FEs, and either indicators for each applicant's set of UC campus applications and admissions (following Mountjoy and Hickman (2020), "MH") or ethnicity indicators and quintics in SAT score and family income (following Chetty et al. (2020*a*), "CFSTY") using 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).

Table 2.3: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Educational Outcomes

	Earn Bach. Degree		Earn STEM Degree		Earn Graduate Degree		
	4 Years	6 Years	Uncondit.	Condit.	All	STEM	JD
Panel A: Difference-in-Difference 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 $\times$ 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 $\times$ Prop 209 ($\beta_{\text{'}98-99}$) 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 $\beta_{\text{'}98-99}$ from Equation 2.1, an OLS difference-in-difference 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 footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

Table 2.4: Difference-in-Difference 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	# > \$100 Wages
Panel A: Difference-in-Difference 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 $\times$ 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 $\times$ 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 $\times$ 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 $\beta_{\cdot 98-99}$ from Equation 2.1, an OLS difference-in-difference 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 number of years of non-zero California wages, average wages and log wages across years with non-zero wages, and number of years with wages above \$100,000, among the years 6-16 or 12-16 years after initial UC application. Outcomes 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 footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. The years 1996-1997 are omitted in Panel C because some universities preemptively curtailed their affirmative action programs in those years. Annual wages CPI-adjusted to 2018 and winsorized at top and bottom 1 percent. Robust standard errors in parentheses. Source: UC Corporate Student System and the California Employment Development Department.

Chapter 3

Top Percent Policies and the Return to Postsecondary Selectivity

In the space of several months I had made desperate attempts, with this and that professor, to enter as a degree student. Some, with twisted mouth, or even rudely, had responded that the racial laws prohibited it; others had had recourse to vague and flimsy pretexts. One night, having politely absorbed the fourth or fifth rejection, I was going home on my bicycle ... The passersby were few and hurried, and then one of them caught my attention ... He was the Assistant [Professor at the Institute for Experimental Physics]. ... I thought that I risked nothing but another rejection, and asked straight out if it would be possible to be accepted for experimental research work in his institute. The Assistant looked at me in surprise, and, in place of the long speech that I would have expected, he answered with two words of the Gospel: "Follow me." ~*Primo Levi, The Periodic Table*

3.1 Introduction

Since the 1960s, selective public universities in the U.S. have admitted students mostly using test scores and other measures of academic preparation.[1] Many universities provide admissions advantages to certain disadvantaged applicants in order to rectify unequal K-12 learning opportunities and promote socioeconomic mobility, but these 'access-oriented' admission policies are controversial on efficiency grounds: students with lower test scores are generally thought to derive smaller (or no) benefits from more-elite education when compared to the students admitted by test-based meritocracy (Arcidiacono and Lovenheim, 2016). This study investigates two open questions about the allocation of public higher education in the U.S. First, would lower-testing students benefit from selective university enrollment, and how would their return compare to that received by higher-testing students? Second, can available policies target lower-testing but high-value-add students, and how would implementing those policies shape universities' socioeconomic composition?

I answer these questions by studying an access-oriented admission policy implemented by the University of California (UC) between 2001 and 2011. Eligibility in the Local Context (ELC)

[1] Until surging demand for postsecondary education made open access impossible in the late 1950s, public universities provided low-cost education to any student who satisfactorily completed high school (Douglass, 2007; Goldin and Katz, 2008).

was a "top percent" policy that guaranteed selective university admission to applicants whose grades ranked in the top four percent of their high school class.[2] I construct a new UC applicant administrative dataset and use a regression discontinuity design to estimate ELC's effect on barely-eligible applicants' likelihood of admission and enrollment at each UC campus. I then link each applicant to national education records and annual California wages and employ an instrumental variable strategy to estimate the medium-run effects of more-selective university enrollment for ELC participants. Building on these reduced-form findings, I next estimate and validate a structural model of university application, admission, and enrollment with an embedded top percent policy in order to simulate the net effects of top percent policies on universities' enrollment composition. Finally, I extend both the quasi-experimental and structural research designs to investigate the relationship between students' meritocratic standing and their return to enrolling at a more-selective university.

I show that the admissions advantages conferred by ELC eligibility caused over 12 percent of barely-eligible applicants from less-competitive high schools to enroll at four selective UC campuses instead of enrolling at less-selective public colleges. Instrumental variable estimates show that these barely-eligible ELC 'participants' became 30 percentage points more likely to earn a college degree within five years — approximately matching the increase in graduation rates of the institutions they attended — and earned higher annual wages by as much as \$25,000 between ages 25 and 27. ELC's roughly 600 annual participants came from lower-income and more diverse families than the crowded-out students whom they replaced at UC, and model simulations show that a top percent policy providing equivalent admissions advantages to the top nine percent of each high school's graduates would meaningfully increase those UC campuses' lower-income and underrepresented minority (URM) enrollment (by about 4 and 8 percent, respectively).[3] Complementing reduced-form and institutional value-added evidence showing that even very low-testing ELC-eligible applicants receive large and above-average wage treatment effects from more-selective enrollment, the paper concludes with evidence that the model-based prediction of each student's meritocratic standing is weakly and *negatively* correlated with their estimated return to university selectivity.

I begin below by providing background on the ten-campus University of California and its 2001 Eligibility in the Local Context policy. I then describe the novel dataset used in this study, which includes far greater detail on 2001-2013 freshman UC applicants' socioeconomic, geographic, and academic characteristics than any previously studied records. Each applicant is linked to the internally-calculated 'ELC GPA' used to determine their ELC eligibility as well as National Student Clearinghouse enrollment and degree records and annual California Employment Development Department wage records through 2019.[4]

I next introduce the stacked regression discontinuity research design that I employ to study the reduced-form effects of ELC eligibility on applicant behavior and outcomes. I present evidence to

[2] Top percent policies have been implemented in Texas, Florida, and Georgia, and have been considered in several other states.

[3] As I discuss below, ELC was indeed "expanded" in 2012 to the top nine percent of applicants from each high school, but Appendix B.1 shows that every selective UC campus ceased providing admissions advantages to ELC-eligible students, *de facto* ending the policy's effects on the composition of UC enrollment.

[4] EDD employment records are maintained for state unemployment insurance provision and exclude out-of-state, federal, and self-employment. Appendix B.3 demonstrates the relative comprehensiveness of the relevant NSC records in this period.

support the design's key identification assumption that applicants' potential outcomes are smooth across their high schools' ELC GPA eligibility thresholds. I then show that ELC eligibility did not substantially affect admissions decisions at UC's most- and least-selective campuses, the former because they did not provide admissions advantages to eligible students and the latter because they were already admitting nearly all high-GPA applicants. However, the UC campuses at San Diego, Davis, Irvine, and Santa Barbara all provided large admissions advantages to ELC-eligible applicants: barely-eligible applicants from the bottom half of California high schools (ranked by SAT scores) became 10 to 35 percentage points more likely to be admitted to each campus as a result of their ELC eligibility. Over 12 percent of those applicants switched into enrolling at one of the four "Absorbing" UC campuses instead of enrolling at a teaching-oriented California State University, a less-selective UC campus, or a local community college.

Because top graduates from more-competitive high schools had little need for ELC eligibility to gain UC admission, almost 90 percent of those barely-eligible ELC participants were from the bottom half of California high schools by SAT. Two-thirds of participants came from families with below-median household incomes and about 45 percent were URM. Barely-eligible participants' average SAT scores were at the 12^{th} percentile of their Absorbing UC peers, altogether suggesting a negatively selected group of students.

Next, I turn to estimation of how ELC eligibility impacted near-threshold ELC participants' educational and labor market outcomes. I show that ELC eligibility caused substantial reduced-form increases in five-year degree attainment, seven-year graduate school enrollment, and early-career annual wages. ELC-eligible applicants became somewhat less likely to earn degrees in STEM fields, but they became more likely to earn any college degree while simultaneously spending fewer years enrolled in college (as a result of reductions in time-to-degree). To identify each of the four Absorbing UC campuses' treatment effects experienced by near-threshold ELC participants, I construct four instrumental variables by interacting the regression discontinuity design with applicants' distance to each campus. I find that enrolling at any of the Absorbing UC campuses increased five-year degree attainment by 30 to 34 percentage points and graduate school enrollment by 22 to 47 percentage points. The estimated effects on wages are noisier: enrolling at UC Davis increased near-threshold participants' annual early-career wages by about $25,000, but the positive wage effects at the other campuses are imprecisely estimated. Near-threshold ELC participants from the bottom quartile of high schools (who would have otherwise enrolled at institutions with 35 percent lower graduation rates on average) received benefits at least as large as those received by participants with better counterfactual enrollments, suggesting large returns to more-selective enrollment even for very disadvantaged applicants.

Having shown that more-selective university enrollment substantially benefits the low-testing students on the margin of ELC eligibility, I next turn to general equilibrium estimation of top percent policies' net effects on universities' student composition and average returns. I embed a top percent policy into a structural model of applicant and university decision-making adapted from Kapor (2020). The model flexibly characterizes students' preferences over universities and models university admissions as maximizing the observed and latent academic caliber of their student bodies. I estimate the model parameters by simulated maximum likelihood, separately identifying admission and enrollment preferences by exploiting the ELC policy, its post-2011 cessation, and distance-to-campus instruments. The resulting parameters align with prior research and successfully replicate the reduced-form effects of ELC eligibility.

I employ the model to conduct a series of counterfactual exercises. I first simulate how ELC shifts Absorbing UC campuses' enrollment composition by switching ELC's admission advantages off (on) in 2010-2011 (2012-2013), allowing each university's regular admissions threshold to adjust in order to maintain its level of enrollment. This allows me to identify the students who are crowded out by ELC, a group otherwise inaccessible in my regression discontinuity analysis. Both strategies provide highly similar results: the 600 annual ELC participants had lower average family incomes by $20,000 and were 15 percentage points more likely to be URM than their crowded-out peers. I also simulate the effect of providing ELC's admissions advantages to the top one, two, and up to the top nine percent of applicants from each California high school. The simulations show that top percent policies are indeed "access-oriented": the nine percent policy increases *net* lower-income and URM enrollment at Absorbing UC campuses each by about 350 students, despite the crowded-out students being negatively-selected relative to the average Absorbing UC student.

Finally, I further exploit the structural model to investigate the broader relationship between students' meritocratic standing and their estimated return to more-selective university enrollment. Abstracting from the ELC policy, I employ a selection-on-unobservables strategy (partially following Dale and Krueger (2002)) to show that the applicants' latent 'application merit' – or the preference index used by universities in admissions – is strongly correlated with applicants' future educational and employment success, but not with their estimated return to university selectivity; if anything, the average return to selectivity is *lower* for higher-'merit' applicants. These estimates complement the reduced-form evidence that the return to university selectivity scales similarly for ELC participants with stronger or weaker measured academic preparation. They also complement additional evidence showing that the wage return to near-threshold ELC participants' Absorbing UC campus enrollment equals or exceeds the *average* return to enrolling at those universities, estimating institutions' average 'value-added' following Chetty et al. (2020*a*). These findings suggest that the first-order net effect of top percent policies is to reallocate educational resources to high-GPA (and perhaps high non-cognitive skill) disadvantaged applicants without efficiency loss.

This study makes three primary contributions. First, it provides the first estimates of the medium-run impact of selective university admission under an access-oriented admission policy.[5] Expanding prior research that focused on the return to selective enrollment for students on the margin of universities' test-based admissions thresholds (Hoekstra, 2009; Anelli, 2019; Sekhri, 2020), I find that a broad array of students would earn large medium-run returns from selective university access, including many students who currently enroll at states' least-selective postsecondary institutions.[6] This evidence suggests that broadening selective research university access to many high school graduates with low socioeconomic status, as through low-cost

[5]One previous study, Bertrand, Hanna and Mullainathan (2010), estimates a positive wage return to caste-based affirmative action programs at engineering colleges in India, though that context is very different from the present study. Subsequent to this study, Bleemer (2020*a*) and Black, Denning and Rothstein (2020) find similar reduced-form returns to a race-based affirmative action policy in California and a top percent policy in Texas, but neither paper is amenable to an instrumental variable strategy that identifies effects for policy compliers. I discuss the latter paper in greater detail below.

[6]Zimmerman (2014) and Smith, Goodman and Hurwitz (2020) show substantial positive returns to less- or non-selective university enrollment for students at those institutions' admissions thresholds. Dale and Krueger (2002, 2014) show evidence of positive returns for disadvantaged students enrolling at highly-selective institutions instead of other selective institutions, and Cohodes and Goodman (2014) show that more-selective enrollment improves students' degree attainment.

access-oriented admission policies, is an impactful and potentially efficient economic mobility lever available to university administrators and state policymakers. While this has been suggested in observational and macroeconomic models (e.g. Chetty et al., 2020*a*; Capelle, 2019) and is assumed by studies focused on encouraging disadvantaged students' more-selective enrollment (e.g. Hoxby and Turner, 2013), it remains contentious in the literature on affirmative action (Arcidiacono, Aucejo and Hotz, 2016; Bleemer, 2020*a*).

Second, this study provides evidence on the impact of a college admission policy that admits students without regard to their standardized test scores (Black, Cortes and Lincove, 2016). Since at least 1960, when California enshrined standardized tests in its "Master Plan for Higher Education" to identify "applicants whose educational purposes are properly met by the college and whose abilities and training indicate probable success," public universities have used evidence of tests' "predictive validity" for college grades and retention to justify their rejection of lower-testing applicants (Westrick et al., 2019; Rothstein, 2004). I show that the benefits to more-selective enrollment are at least as large (and likely larger) for high-GPA students whose low SAT scores would typically have disqualified them from selective universities as they are for the higher-SAT students currently admitted to those universities. Indeed, despite being negatively-selected, near-threshold ELC participants' 75 percent average graduation rate was roughly equal to the institutional average (77 percent). As many public universities rethink how their meritocratic admissions policies rank applicants (Saboe and Terrizzi, 2019), these findings show that targeting high-GPA low-SAT applicants could simultaneously broaden university access and increase institutions' economic value-added.

Finally, this study contributes to a nascent structural literature modeling students' school application and enrollment decisions (Arcidiacono, 2005; Epple, Romano and Sieg, 2006; Howell, 2010; Chade, Lewis and Smith, 2014; Walters, 2018; Kapor, 2020), providing new detailed information about student and university preferences. The estimated model also provides novel estimates of the relative magnitude and compositional effects of top percent policies with different eligibility thresholds, facilitating straightforward comparison with other access-oriented university admissions policies (Long, 2004*b*).

3.2 Background and Literature

California has three public higher education systems: the University of California, the teaching-oriented California State University, and the two-year California Community Colleges. The University of California is tasked with educating the top 12.5 percent of California high school graduates at its nine undergraduate campuses: the most-selective Berkeley and Los Angeles (UCLA) campuses, the middle-selective Davis, San Diego, Santa Barbara, and Irvine campuses, and the least-selective Riverside, Santa Cruz, and Merced (founded in 2005) campuses. The system's California-resident freshman enrollment grows in proportion to the state's high school graduates, with about 30,000 such students earning degrees in 2011.

UC employed race-based affirmative action in undergraduate admissions until 1997, after which the practice was banned by ballot proposition. Eligibility in the Local Context was introduced in 2001 to expand access to UC campuses in a race-neutral manner (Atkinson and Pelfrey, 2004). Under ELC, graduates of participating California high schools — which by 2003 included 96 percent of public high schools and 80 percent of private high schools — were guaranteed admission to at least one UC campus if their grades were in the top four percent of their class.[7] Class rank was determined centrally by UC: high schools submitted students' transcripts to the UC Office of the President, which calculated UC-specific 'ELC grade point averages (GPAs)' on a four-point scale using certain eligibility-relevant second- and third-year courses.[8] ELC GPAs were weighted — adding one GPA point for each junior-year honors-level course — and rounded to the nearest hundredth. The 96th percentile of ELC GPAs at each high school was selected as the school's "ELC eligibility threshold" in that year, above which students were deemed 'ELC-eligible'.

ELC-eligible students received a letter in the fall of their senior year informing them of their eligibility, along with the guarantee of admission to at least one UC campus (but no guarantee to any specific campus). Below-threshold students with high GPAs were sent similar letters strongly suggesting that they would be guaranteed admission to at least one UC campus under another UC admissions policy.[9] In order to maintain eligibility, ELC-eligible students had to pass their high school's college-level senior curriculum and take the SAT. Administratively, each UC campus was informed of their applicants' ELC eligibility but retained independence in their admissions decisions.

There was widespread public concern that ELC participants might not be sufficiently prepared for selective university education: "top students in many high-poverty schools are woefully unprepared for college ... many of the new students will simply flunk out and the policy will be discredited" (Orfield, 1998). Nevertheless, though no comprehensive analysis was conducted following an inconclusive short-run program evaluation in 2002 (University of California, 2002), ELC was viewed as having succeeded in fulfilling its aims of increasing admitted students' ethnic and geographic diversity and was expanded in the 2012 admissions year to the top nine percent of each high school class. However, every campus ceased providing substantial admissions advantages to ELC-eligible applicants after this 'expansion,' forcing the system to coerce UC Merced to admit otherwise-rejected ELC-eligible students and rendering the program practically defunct (see Appendix B.1). As a result, this study focuses on the pre-2012 ELC policy.[10]

[7]Cullen, Long and Reback (2013) find that only a small number of students switched high schools in order to 'game' this kind of high-school-percentile admissions policy after Texas implemented a similar top percent policy.

[8]See Atkinson and Pelfrey (2004). The courses included two years of English and Mathematics, one year of History, Lab Science, a Non-English Language, and four other UC-approved courses. Students or their parents could opt out of their high school's providing their transcript to UC at their discretion. This centralized ELC administration importantly differs from Texas's program, where high schools were directly responsible for identifying the top ten percent of students; some high schools purposefully extended "Top Ten" eligibility to a greater proportion of students (Golden, 2000).

[9]UC's "Eligibility in the State-Wide Context" policy provided a *de jure* similar admissions guarantee for the top 12.5 percent of California seniors based on a publicly available linear combination of high school GPA and SAT scores. In practice, most UC campuses provided substantially larger admissions benefits to ELC-eligible students than to those eligible in the state-wide context. It is unknown whether UC applicants were aware of the difference.

[10]Appendix B.2 exploits this abrupt ELC cessation to replicate the main reduced-form results presented below using a difference-in-difference design after 2011.

3.2.1 Prior Literature

A large literature has examined how access to more-selective universities impacts students' educational and labor market outcomes.[11] Several studies have used quasi-experimental research designs exploiting minimum SAT and GPA admissions thresholds to show that university access increases on-the-margin enrollees' wages at less-selective universities (Zimmerman, 2014; Smith, Goodman and Hurwitz, 2020), for white men at a more-selective university (Hoekstra, 2009), and for all students at certain selective universities outside the U.S. (Anelli, 2019; Sekhri, 2020), though none of these studies explicitly observe applicants' counterfactual enrollment institutions.[12] Several other studies employ selection-on-observables research designs to control for sample selection bias arising from applicants' varying admission and taste; while Dale and Krueger (2002) find no wage return to university selectivity among a set of highly-selective universities, most studies find that more-selective enrollment conditionally correlates with higher post-graduate wages (Loury and Garman, 1995; Kane, 1998; Brewer, Eide and Ehrenberg, 1999; Andrews, Li and Lovenheim, 2016), at least among disadvantaged students (Dale and Krueger, 2014).[13] In the closest context to this study, Cohodes and Goodman (2014) examine a Massachusetts financial aid policy that incentivized students to enroll at less-selective universities, using a regression discontinuity design to find reduced-form declines in institutional graduation rate and students' own four-year degree attainment of 1.5 and 1.9 percentage points, respectively. The present study contributes by employing a rigorous quasi-experimental research design to estimate the medium-run return to more-selective university enrollment for notably disadvantaged applicants, and by explicitly analyzing heterogeneity in the return to more-selective enrollment for students with higher and lower traditional meritocratic rank.

A second literature has studied the effects of race-based affirmative action — another popular access-oriented admission policy — on admission, enrollment, and short-run educational outcomes. Affirmative action causes targeted disadvantaged students to enroll at more-selective institutions in the U.S. (Arcidiacono, 2005; Howell, 2010; Hinrichs, 2012, 2014; Backes, 2012; Antonovics and Backes, 2014; Blume and Long, 2014).[14] However, differences in setting, research design, and data availability have led researchers to conflicting conclusions about

[11] A related literature uses quasi-experimental research designs to examine heterogeneity in the return to higher education by field of study (e.g. Kirkeboen, Leuven and Mogstad, 2016; Hastings, Nielsen and Zimmerman, 2018; Bleemer and Mehta, 2020*c*).

[12] Hastings, Nielsen and Zimmerman (2018) exploit minimum score admissions thresholds in Chile to identify positive wage returns to more-selective university enrollment. Zimmerman (2019) shows that disadvantaged Chilean students are no more likely to become top earners if they are barely admitted to top business schools. Others use similar research designs to examine on-the-margin students choosing between community colleges and less-selective four-year universities, finding that enrolling at the four-year universities appears to increase students' likelihood of earning a college degree (Reynolds, 2012; Angrist et al., 2016; Goodman, Hurwitz and Smith, 2017) and medium-run wages (Mountjoy, 2019; Smith, Goodman and Hurwitz, 2020). Abdulkadiroglu, Angrist and Pathak (2014) show that on-the-margin access to selective high schools does not improve U.S. students' standardized test scores or university selectivity.

[13] Ge, Isaac and Miller (2018) follow the research design of Dale and Krueger (2002) but find that attending more-selective universities improves female students' postgraduate labor market outcomes. Griffith (2010) shows that observably similar students at more-selective universities are less likely to earn STEM degrees. An earlier generation of literature shows a positive correlation between university selectivity and wages (Wales, 1973; Morgan and Duncan, 1979; James et al., 1989; Behrman, Rosenzweig and Taubman, 1996).

[14] The same is true of affirmative action policies in India (Bertrand, Hanna and Mullainathan, 2010; Bagde, Epple and Taylor, 2016) and Brazil (Francis and Tannuri-Pianto, 2012).

affirmative action's impact on degree attainment (Cortes, 2010; Arcidiacono et al., 2014; Bleemer, 2020*a*) and major choice (Rose, 2005; Arcidiacono, Aucejo and Spenner, 2012; Arcidiacono, Aucejo and Hotz, 2016; Bleemer, 2020*a*). Closest to the present study, Bleemer (2020*a*) shows that ending race-based affirmative action in California led to decreases in selective university enrollment among targeted applicants, precipitating declines in undergraduate and graduate degree attainment and early-career wages.[15] This study uses a quasi-experimental and transparent identification strategy to clearly delineate the specific and heterogeneous effects of more-selective university enrollment for disadvantaged applicants.

As a result of political and judicial challenges to race-based affirmative action, top percent policies have become increasingly popular among public university systems: 31 percent of Americans live in states that have adopted top percent policies at their public universities. Nevertheless, surprisingly little research has examined their effect on impacted students' outcomes. In California, this likely results from the widespread belief — despite minimal evidence — that Eligibility in the Local Context had a negligible effect on eligible students' enrollment decisions, expressed in academic studies (Rothstein, 2000; Long, 2004*b*, 2007) and policy-oriented briefs and books (University of California, 2003; Kidder and Gandara, 2015; Zwick, 2017).

A larger literature has studied Texas Top Ten (TTT), a top percent policy that guarantees Texas public university admission to students in the top ten percent of their high school classes by GPA (as determined by the schools). That literature has largely focused on estimating whether TTT's admissions guarantee actually changes high school graduates' university enrollment (Long, Saenz and Tienda, 2010; Niu and Tienda, 2010*a*; Kapor, 2020); this study contributes by simulating how counterfactual top percent policies with different eligibility thresholds would affect universities' student compositions.[16] Difference-in-difference analysis of TTT's effects on student outcomes are confounded by the state's near-simultaneous cessation of race-based affirmative action, likely explaining Black, Denning and Rothstein (2020)'s findings that TTT appears to largely increase college-going on the *extensive* margin (switching non-college-goers into selective university enrollment) and that TTT participants do not appear more disadvantaged than the students they replace at selective universities. The present study complements Black, Denning and Rothstein (2020)'s findings on top percent policies' effects on degree attainment and wages by employing a more textured research design to show that top percent policies generate large returns for relatively disadvantaged participants by increasing the selectivity of their enrollment institutions, and by exploiting those selectivity changes to investigate students'

[15] Bertrand, Hanna and Mullainathan (2010) find that affirmative action increases impacted students' medium-run wages in the Indian contexts. Cestau et al. (2020) show that Black students at West Point have lower test scores but similar postgraduate achievement as their white peers. Arcidiacono (2005) estimates a structural model suggesting that the U.S. wage effect is small. The contentious affirmative action literature is reviewed by Arcidiacono and Lovenheim (2016) and Arcidiacono, Lovenheim and Zhu (2015), with an earlier literature reviewed by Holzer and Neumark (2006). A related literature examines whether attending a more-selective law school under an access-oriented admission policy has negative educational and labor market repercussions (Sander, 2004; Rothstein and Yoon, 2008), coming to contradictory conclusions, though there is general agreement that race-based affirmative action increases targeted students' likelihood of more-selective law school enrollment (Yagan, 2016).

[16] Daugherty, Martorell and McFarlin Jr. (2014) show that enrollees from one large urban school district would have otherwise enrolled at similarly-selective private universities. Cortes and Lincove (2019) show that TTT encourages public flagship university enrollment among high-performing low-income high school graduates.

relative returns to more-selective enrollment.[17]

Another literature has studied a wide variety of application-oriented policies like direct information provision (Hoxby and Turner, 2013; Gurantz et al., forthcoming), improved college counselors (Avery, 2013; Castleman and Goodman, 2017), and changes in testing policies (Pallais, 2015; Goodman, 2016) that could increase disadvantaged students' selective university enrollment by increasing disadvantaged students' likelihood of applying to selective universities. I show that low-cost changes in university admission policies provide an alternative policy mechanism that increases disadvantaged student enrollment.

Finally, this study's analysis of heterogeneity in the return to university selectivity contributes to a literature analyzing the role of 'mismatch' in university enrollment, or the theory that "those who attend the most selective colleges and perform less well because of mismatching would have had higher earnings if they had attended the somewhat less selective group of schools" (Loury and Garman, 1993). Recent studies have come to conflicting conclusions about the relative magnitude of 'mismatch' effects (Dillon and Smith, 2020; Mountjoy and Hickman, 2020; Bleemer, 2020*a*). The present study provides an unusually transparent research design with which to investigate the relevance of mismatch in the California context of the measurably 'mismatched' low-testing (but high-GPA) applicants targeted by top percent policies.

3.3 Data

I compile three primary data sources to conduct this study. The first, collected contemporaneously for administrative use by the UC Office of the President, covers all 1995-2013 California-resident freshman applicants to any of the nine undergraduate University of California campuses. Each record contains the applicant's home address at the time of application, high school attended, gender, 15-category ethnicity, parental education, SAT or ACT score, and family income, as well as whether they applied to, were admitted to, and/or enrolled at each campus and their intended majors.[18] The UC application data also include ELC eligibility status and ELC GPAs beginning in 2003. After 2011, an additional field denotes students' GPA percentile for each of the top nine percentiles.

I do not directly observe the high-school-specific ELC eligibility thresholds used to determine students' ELC eligibility. I estimate the threshold in each high school year in two ways: as the minimum GPA of an ELC-eligible applicant, or as the threshold that minimizes the number of applicants whose ELC eligibility is misclassified above or below the threshold.[19] In most cases these two are identical, but a small number of noisy ELC eligibility indicators (which could arise

[17]Furstenberg (2010) argues that TTT decreased targeted students' likelihood of degree attainment, but that study has substantial limitations: outcomes are only observed for enrollees at public universities, the only observed graduation rate is four-year (and it is only observed for a single cohort, the first that TTT was implemented), and transfers between universities are treated as non-graduation, all of which is compounded with technical limitations like a coarse discrete running variable.

[18]Seven percent of applicants' addresses cannot be geolocated. Parental education is observed as an index of maximum parental education for both parents. ACT scores or SAT scores on the 1600 scale are converted to the 2400 SAT scale using a standard cross-walk. Family income is not reported by 12 percent of applicants. Intended majors are non-binding, and about one-third of applicants select 'Undeclared'. I assign to each applicant the intended discipline(s) that they most frequently report across campuses.

[19]When multiple thresholds minimize eligibility in the latter case, I take their average.

from failure to complete the requisite high school courses, faulty data, or other sources) lead to differences at some schools. I use the latter calculation in the main results presented below, yielding minimized Type 1 and 2 errors of 1.3 and 2.8 percent respectively, but the presented results are robust to employing the former calculation instead (as shown in appendix tables).

The second dataset, from the National Student Clearinghouse's StudentTracker database, contains UC applicants' enrollment and graduation records across nearly all U.S. two- and four-year colleges and universities.[20] NSC records are censored by a small number of students and institutions, but their near-completeness throughout the study period means that it is highly unlikely that differential NSC reporting could be a substantial factor driving the results presented below.[21] Science, Technology, Engineering, and Mathematics (STEM) majors are categorized by CIP code following the U.S. Department of Homeland Security (2016).[22]

Third, I observe UC applicants' quarterly 2003-2019 wages from the California Employment Development Department, which maintains employment records for unemployment insurance administration.[23] The wage data were linked by reported social security numbers from UC applications, and are unavailable for workers outside California, self-employment, and federal employment.[24] Annual wages are measured as the sum of quarterly wages in that year, and are CPI-adjusted to 2019 and winsorized at five percent. About 55 percent of applicants in the sample have positive wages in each of seven to nine years after high school graduation.

Each institution in the NSC dataset is geolocated using IPEDS, and distances between applicants and institutions are calculated (as the crow flies) using the geodesic method. California high schools are geolocated using street addresses available from the California Department of Education (with 98 percent success across students) and categorized as rural, urban, or suburban using shapefiles from the National Center for Education Statistics.[25] Additional institutional characteristics are linked from the Integrated Postsecondary Education Data System (IPEDS) and Opportunity Insights's Mobility Report Cards (Chetty et al., 2020*a*).

[20] In particular, it contains semesterly enrollment records and graduation records (including degrees, majors earned, and year of graduation) for all degree-granting institutions that accept federal Title IV funding. Records are linked by first and last name, middle initial, and birth date, allowing for common nicknames and typos.

[21] NSC reports that about 4 percent of records are censored due to student- or institution-requested blocks for privacy concerns (National Student Clearinghouse Research Center, 2017). Enrollment is near-comprehensive for California public institutions (Dynarski, Hemelt and Hyman, 2015). Appendix B.3 shows that nearly all California colleges and universities were reporting to NSC by 2003 and that a comparison between UC and NSC records reveals very low degree attainment and major censorship rates.

[22] STEM includes the 278 "fields involving research, innovation, or development of new technologies using engineering, mathematics, computer science, or natural sciences (including physical, biological, and agricultural sciences)" identified by CIP code. Not all NSC majors have CIP codes; I assign each major to its modal CIP code (in the full observed NSC database) for categorization. Disciplines are also partitioned into arts, humanities, social sciences, natural sciences, engineering, professional, and business by hand-coding from NSC records; the discipline coding is available from the author.

[23] The most recent wages available are 2019, so every year more than eight years after graduation omits one class of ELC students from the observed sample. All wage statistics were originally estimated as institutional research (see Bleemer (2018*c*)).

[24] Social security numbers on UC applications are not verified unless the student enrolls at a UC campus. Among enrollees, the verified social security number differs from that reported on their application in fewer than 0.25 percent of cases.

[25] See the CDE Public Schools and Districts Data Files, the CDE's Private School Directory, and the NCES's School Locale Definitions. Rural schools are outside of any Census Urbanized Area; urban schools are inside a Census Principal City.

3.3.1 Summary Statistics

Table 3.1 reports summary statistics for 2003-2011 UC applicants.[26] The first column presents demographic characteristics, academic achievement measures, and enrollment decisions for all California-resident freshman applicants to any UC campus between 2003 and 2011, while the second summarizes applicants within 0.3 ELC GPA points of their high schools' ELC eligibility thresholds, the main sample used in the reduced-form analysis below. The latter applicants are academically above average, more likely to be female, and less likely to be Black or Hispanic.[27] The bottom half of the table shows that these applicants are relatively more likely to attend the more-selective "Unimpacted" and "Absorbing" UC campuses — these category names will be discussed below — but less likely to attend the less-selective "Dispersing" UC campuses.

The last four columns of Table 3.1 show summary statistics by high school quartile, ranking schools by the average SAT scores of near-threshold UC applicants.[28] Because the ELC program admitted four percent of every high school's applicants, there is reason to expect that its impact will be larger at lower-performing high schools where high-GPA students have fewer or lower-quality alternative enrollment options.[29] Indeed, applicants from the bottom quartile of high schools have lower SAT scores by 570 points and are far more likely to attend less-selective state colleges than applicants from the top quartile. Lower-quartile applicants are also much more likely to be Black and Hispanic (URM). Below, I refer to applicants from the bottom half and quartile of California high schools as the "B50" and "B25" samples, respectively.

3.4 ELC and College Enrollment

3.4.1 Empirical Methodology

I estimate the reduced-form effect of ELC eligibility on university enrollment using a regression discontinuity design (Hahn, Todd and van der Klaauw, 2001). Let $Y_i(1)$ and $Y_i(0)$ denote applicant i's potential outcomes if they are ELC-eligible or ineligible, respectively. The effect of ELC eligibility on near-threshold applicants is:

$$LATE_{RD}(Y) = \lim_{GPA \downarrow 0} E[Y_i(1)|GPA] - \lim_{GPA \uparrow 0} E[Y_i(0)|GPA] \tag{3.1}$$

[26]The main sample is restricted to 2003-2011 because ELC GPAs are not observed until 2003.

[27]Because the number of Black applicants near the ELC eligibility threshold is so low, most of the estimates below group Hispanic and Black applicants as "underrepresented minorities", or "URM" along with Native American applicants.

[28] For the purpose of calculating quartiles, high-school-years are ranked by the average SAT score of applicants within 0.3 ELC GPA points of their school's ELC eligibility threshold in the given year and then weighted by their number of applicants within the 0.3 GPA band, resulting in quartiles with approximately the same number of *students*, not high schools. All results below are robust to using leave-one-out average SAT scores to measure high school quartiles, but the aggregate high school averages are used so that each school-year is in a single quartile.

[29]Cortes and Lincove (2019) find greater takeup of Texas's top percent policy among students from less-competitive schools.

where GPA is the difference between an applicant's ELC GPA and their school's ELC eligibility threshold. I estimate $LATE_{RD}(Y)$ by $\hat{\beta}$ from a linear regression model:

$$Y_{it} = \beta ELC_i + f(GPA_i) + \delta X_i + \alpha_{h_i} + \gamma_t + \epsilon_{it} \tag{3.2}$$

where ELC_i indicates ELC eligibility, X_i includes gender-ethnicity indicators and a quadratic in SAT scores to absorb spurious variation in Y_{it}, and α_{h_i} and γ_t are high school and application year (t) fixed effects.[30] I estimate Equation 3.2 stacked across all participating high schools with the error terms ϵ_{it} clustered by $h_i \times t$, the level of treatment assignment.[31]

I estimate Equation 3.2 using two specifications of f. Because the running variable GPA_i is discrete — ELC GPAs are rounded to the nearest hundredth — my preferred specification is to include third-order polynomials of GPA_i on either side of the eligibility threshold and to estimate the model by OLS. I obtain highly statistically and substantially similar estimates by local linear regression with bias-corrected clustered standard errors following Calonico, Cattaneo and Titiunik (2014).[32] In both cases, I restrict the sample to freshman fall California-resident UC applicants within 0.3 GPA points of the eligibility threshold, resulting in the main sample of 171,411 applicants. Because the ELC eligibility threshold is slightly fuzzy, the baseline estimates instrument ELC_i with an indicator for having an above-threshold ELC GPA ($\mathbb{1}_{GPA_i \geq 0}$).

The key identifying assumption justifying the regression discontinuity design is that $E[Y_i(1)|GPA]$ and $E[Y_i(0)|GPA]$ are smooth at $GPA = 0$. I discuss and test the potential threats to this smoothness assumption in detail in Appendix B.2. The primary threat to the smoothness assumption is the possibility of applicants' selection into UC application as a result of being informed of ELC eligibility (which occurred before UC's application deadline). However, as noted above, nearly all students just *below* the eligibility threshold also received letters encouraging UC application, and high-GPA students were very likely to be admitted to many UC campuses even without the ELC policy. Tests of the smoothness assumption fail to reject several of its implications. First, Appendix Table B.14 shows that a detailed set of applicant characteristics — including gender, ethnicity, parental income and education, and SAT score — are smooth across the threshold among all, B50, and B25 UC applicants. Figure B.12 visualizes this smoothness for applicants' predicted five-year degree attainment based on all observed socioeconomic and academic characteristics.[33] Second, there is no evidence of an increase in applicant density above the eligibility threshold that would suggest that above-threshold students bunched into UC application. Third, I successfully replicate the baseline regression discontinuity estimates with a difference-in-difference design comparing above- and below-threshold students before and after 2011, when their admissions advantages ceased.

[30] Controls are omitted when they are collinear with the outcome variable, as when Y_{it} is the applicant's SAT score. Nearly all of the results presented below are quantitatively and statistically unchanged if these controls are selectively or completely omitted, or if high school fixed effects are omitted.

[31] Because the number of running variable values on each side of the threshold is relatively large, I cluster by treatment level instead of running variable bin following Kolesar and Rothe (2018).

[32] OLS estimation was conducted using the *felm* command in R's *lfe* package. Local linear regressions were estimated using the *rdrobust* package in R (Calonico, Cattaneo and Titiunik, 2015). The latter does not permit fixed effects; instead, I include indicator variables for all high schools with more than 50 applicants in the sample as controls.

[33] Five-year degree attainment is predicted by OLS using gender-ethnicity indicators, family income, max parental education indicators, year indicators, SAT score, and high school GPA using the full 1995-2013 sample of UC freshman California-resident applicants, excluding the estimation sample.

I also investigate another potential threat to the smoothness assumption: the possible presence of a student 'type discontinuity' at ELC eligibility thresholds. If ELC eligibility thresholds tended to occur at exactly 4.0 GPA, then above-threshold students could be positively selected as a result of grades being censored from above. Appendix B.2 provides evidence from Caetano (2015) tests suggesting that this threat is empirically small. I omit all schools with measured thresholds between 3.96 and 4.00 from the main specifications out of an abundance of caution, but the resulting estimates are substantively unchanged.

3.4.2 Admission and Enrollment

Figure 3.1 plots the likelihood of admission to each UC campus (conditional on applying to that campus) by the ELC GPA running variable, overall and applicants from the bottom half (B50) or quartile (B25) of high schools by SAT. Admission to UC's most-selective Berkeley and UCLA campuses appears unchanged on either side of the ELC eligibility threshold, implying that those two campuses provided no observable admissions advantage to ELC-eligible applicants. Four other campuses, however — San Diego, Irvine, Davis, and Santa Barbara — provided large admissions advantages to above-threshold students, with larger advantages for students from lower-testing high schools. Near-threshold B25 applicants became an average of 40 percentage points more likely to be admitted to UC Davis and UC Irvine as a result of ELC eligibility. The three least-selective UC campuses, on the other hand, were already granting admission to nearly all applicants just below the ELC eligibility threshold; ELC eligibility could hardly impact applicants' likelihood of admission at those schools.[34]

Table 3.2 presents estimates of ELC's effect on barely-eligible applicants' enrollment at UC and other postsecondary institutions.[35] Panel A shows near-threshold applicants' baseline likelihood of enrollment, while Panel B shows the $\hat{\beta}$ coefficients associated with ELC eligibility. At baseline, about 55 percent of near-threshold B50 students enrolled at a UC campus. Fourteen percent enrolled at Berkeley and UCLA, which are referred to as "Unimpacted" because admissions and net enrollment at those campuses were unchanged at the eligibility threshold. Another 33 percent enrolled at the four UC campuses that provided ELC-eligible applicants with large admissions advantages, termed "Absorbing" because net enrollment increased by 12.2 percentage points (40 percent) at the eligibility threshold. While nine percent of applicants enrolled at the three less-selective "Dispersing" UC campuses at baseline, their enrollment declined by 3.6 percentage points across the threshold as applicants switched into the

[34]Appendix B.5 shows that ELC eligibility had generally consistent effects on admissions at each UC campus in each year between 2003 and 2011. ELC eligibility also shifted UC applicants' relative likelihoods of applying to each campus, with barely-eligible applicants becoming slightly more likely to apply to campuses that provided ELC admissions advantages and slightly less likely to apply to the less-selective campuses. However, the application effects are an order of magnitude smaller than the changes in admissions likelihood, suggesting that the latter largely account for the resulting enrollment shifts (an interpretation confirmed by the structural model estimates below). See Figure B.13 and Table B.16.

[35]Coefficients are estimated using Equation 3.2 for enrollment in the fall semester following UC application. Baseline estimates are estimated following Abadie (2002), which requires the monotonicity assumption that no near-threshold ELC-eligible student became *less* likely to enroll at the Absorbing UC campuses. Non-UC institutions could not observe or infer applicants' ELC eligibility, implying that any enrollment changes at non-UC institutions resulted from changes in applicants' UC admission.

more-selective Absorbing campuses.[36]

The remaining columns of Table 3.2 show that barely ELC-eligible B50 applicants' enrollment declined by 6.0 percentage points at the CSU system and by 1.8 percentage points at community colleges. There is no observable change in private or out-of-state university enrollment.[37] These estimates show that near-threshold ELC-eligible applicants became less likely to enroll at less-selective public colleges and universities and more likely to enroll at the Absorbing campuses. This shift in enrollment is larger among B25 applicants, whose Absorbing UC enrollment increased by 16 percentage points, and smaller across all applicants; there is no evidence of net enrollment changes for applicants from the third or fourth high school quartiles.

3.4.3 Characteristics of Compliers

Who are the near-threshold applicants who enroll at Absorbing UC campuses as a result of their ELC eligibility? Following Abadie (2002), the average fixed characteristic W_i of ELC near-threshold "compliers" can be estimated by $\frac{LATE_{RD}(Absorb_i \times W_i)}{LATE_{RD}(Absorb_i)}$, where $Absorb_i$ indicates enrolling at an Absorbing UC campus, under two technical assumptions:

- Random assignment to ELC eligibility. This follows from the regression discontinuity setting.
- Monotonicity: $Absorb_i(1) - Absorb_i(0) \geq 0 \;\; \forall i \;\; s.t. \; |GPA_i| < \epsilon$, for some small bandwidth ϵ. This is justified by the admissions patterns shown in Figure 3.1.

I estimate ELC compliers' characteristics by replacing the endogenous variable in Equation 3.2 with $Absorb_i$. Table 3.3 presents $\hat{\beta}$ estimates for a series of characteristics, overall and by school subsample. The last line of each panel shows the mean characteristic of 2003-2011 California-resident freshman enrollees at the four Absorbing UC campuses, allowing comparison between ELC compliers and their eventual peers.

Panel B shows that 58 percent of compliers came from the bottom SAT quartile of high schools and almost 90 percent came from the bottom two SAT quartiles. This sharply contrasts with Absorbing UC campus student bodies, almost 60 percent of whom graduated from schools in the top two quartiles. Because so few near-threshold students from the top half of high schools participated in ELC, the analysis of student outcomes below exclusively focuses on students from the bottom two quartiles.

Panel A presents estimates of compliers' demographic and geographic characteristics. Compliers were more than twice as likely as their future peers to be underrepresented minorities (URM) and were 15 percentage points more likely to come from families with below-median incomes. ELC had less impact on the geographic diversity of UC's student body; about 8 percent of compliers were from rural California relative to 5.3 percent of Absorbing campus students. ELC compliers had far lower SAT scores than their eventual peers, by almost 300 SAT points

[36] Appendix Table B.15 presents estimated changes in admission and enrollment at each UC campus for barely above-threshold applicants, showing that these aggregated changes at the threshold are mirrored at each of the respective campuses.

[37] There is statistically insignificant evidence of a small above-threshold decline in non-enrollment. Students who take gap years following high school are categorized here as non-enrollees, as are students or institutions with masked records; see Appendix B.3.

overall and by 400 points among bottom-quartile applicants. Bottom-quartile ELC compliers had average SAT scores at the fifth percentile of Absorbing campus students. However, as a result of the structure of the ELC program, compliers' average high school GPA was comparable to that of their Absorbing campus peers. Near-threshold ELC compliers are thus best understood as relatively disadvantaged students with far lower standardized test scores than their average Absorbing UC peers, though they were top performers at their less-competitive high schools prior to enrollment.

3.5 Educational and Labor Market Outcomes

3.5.1 Reduced Form Estimates

ELC eligibility caused many barely-eligible UC applicants — from the bottom half (B50) or quartile (B25) of California high schools — to enroll at one of four Absorbing UC campuses instead of enrolling at less-selective public California colleges and universities. Panel (a) of Figure 3.2 visualizes the sharp increase in Absorbing UC campus enrollment for barely ELC-eligible B50 and B25 applicants.

Panel (b) of Figure 3.2 shows that above-threshold B50 (B25) students enrolled at institutions with higher graduation rates by 3.3 (5.4) percentage points, indexing institutions' selectivity using a novel five-year graduation rate defined over both two- and four-year institutions.[38] Appendix Table B.17 shows that these institutions are also more measurably selective across a host of alternative selectivity metrics. It also shows that the Absorbing UC campuses have higher sticker prices but similar estimated net prices for students with the family incomes of near-threshold applicants, though Absorbing UC campus enrollment may have increased those students' college costs by decreasing their likelihood of living at home through college.[39]

Panel (c) of Figure 3.2 shows a sharp increase in B50 and B25 applicants' own likelihood of undergraduate degree attainment within five years of graduating high school. The trends in Panels (b) and (c) appear to mirror each other fairly closely, with a similar flattening of applicants' institutional and own graduation rates just below the eligibility threshold — likely a feature of the college market unrelated to ELC — followed by sharp increases of 3-5 percentage points at the threshold. Panel (d) shows that applicants' likelihood of graduate school enrollment — defined as post-graduate university enrollment within seven years of high school graduation — also jumps at the eligibility threshold, which likely bodes well for applicants' long-run wages (Altonji and Zhong, 2020). Appendix Figure B.14 and Table B.23 show $\hat{\beta}$ estimates for additional reduced-form educational outcomes across the ELC eligibility threshold, presenting evidence that barely above-threshold students spend fewer years enrolled in undergraduate programs (despite their increased degree attainment) but may be less likely to earn a degree in a STEM field.

Panels (e) and (f) of Figure 3.2 show the average annual covered California wages and log wages earned by applicants between seven and nine years following high school graduation.[40] The

[38]Graduation rates are defined by linking all UC applicants to their first enrollment institution and measuring their five-year Bachelor's degree attainment from any institution, even if they transfer elsewhere. See Appendix B.4.

[39]Appendix Table B.18 shows similar conditional differences across the ELC eligibility threshold in the selectivity of the institutions where degree-attainers earn their undergraduate degrees.

[40]Average log wages omit years in which no California wages were earned.

plot shows reduced-form increases in annual wages of about $2,300 (or 0.10 log points), with some variation in the statistical significance of the various estimates in the polynomial and local linear specifications. Given that ELC only shifts students between California institutions and that there is no measurable change in applicants' number of years of California employment in either sample, it is unlikely that these estimates are explainable by the wage data's restriction to covered California employment.

3.5.2 Instrumental Variable Estimation

The admission and enrollment patterns discussed above imply that ELC eligibility could cause one of two changes in barely-eligible students' university enrollment: (1) it could lead students to enroll at an Absorbing UC campus instead of a less-selective public institution, or (2) it could lead students to enroll at an Absorbing UC campus instead of *another* Absorbing UC campus. As a result, the most natural instrumental variable strategy for measuring the effect of Absorbing UC campus enrollment – using ELC eligibility as an instrument for Absorbing UC enrollment following Equation 3.2 – could be biased by changes in student outcomes resulting from between-Absorbing-campus switches, which violate the strategy's monotonicity assumption. While I nevertheless report those estimates in Table 3.4, I also implement a more robust instrumental variable strategy that separately identifies ELC's treatment effect on the UC applicants who enrolled at each of the four Absorbing UC campuses because of ELC, constructing four instrumental variables by interacting the regression discontinuity design with distance-to-campus measures for each applicant (Card, 1993).[41] In particular, I estimate models of the form:

$$Y_{it} = \sum_{c \in Abs} \left(\beta_c E\hat{N}R_{ic} + f_c(GPA_i) \times Dist_{ic} \right) + \delta X_i + \gamma_t + \epsilon_{it} \tag{3.3}$$

where $Dist_{ic}$ is the as-the-crow-flies distance from i's home address to the four UC campuses $c \in Abs$ and the four $E\hat{N}R_{ic}$ enrollment indicators are instrumented by $(\mathbb{1}_{GPA_i \geq 0} \times Dist_{ic})$, the interaction between distance-to-campus and having an above-threshold ELC GPA.[42] I omit high

[41]This research design relies on the plausible exogeneity of which Absorbing campus each near-threshold UC applicant lives closest to. For example, it requires that the potential outcomes of near-threshold applicants who will attend Davis (iff they are ELC-eligible) because they live near to Davis must be equivalent to those of the near-threshold applicants who will attend Irvine (iff they are ELC-eligible) because they live near to Irvine. This assumption is testable on observables: the first row of Table 3.4 shows that there is no observable cross-campus difference in the observed academic preparedness of the students who enroll at one campus instead of another, measuring preparedness by their predicted likelihood of college graduation. The research design also assumes constant treatment effects in the relationship between students' outcomes and their Absorbing UC campus enrollment caused by their distances to each of the four UC campuses, though Table B.20 shows that enrollment at each campus is largely predicted by their log distance to *that* campus, not their distances to the other campuses.

[42]The last two rows of Table 3.4 show that the instrumental variables easily satisfy weak-instrument tests; the first-stage F-statistics range from 13 to 107 (Stock and Yogo, 2002), and the conditional first-stage F-statistics (Sanderson and Windmeijer, 2016) range from 33.6 to 104.1, all far above suggested minima. To improve the instrument's strength, I interact the Santa Barbara distance measure with an indicator for $t < 2011$, exploiting Santa Barbara's increasing popularity among applicants over time (it rose over the sample period from the lowest- to highest-ranked of the Absorbing UC campuses in the U.S. News & World Report rankings). Appendix Table B.19 shows the unadjusted estimates.

school fixed effects because they absorb key geographic variation across applicants, and continue to cluster ϵ_{it} by school-year.

The second row in Table 3.4 shows that the ELC participants who enroll at each of the four Absorbing UC campuses experienced similar increases in the five-year graduation rates of their enrollment institution, between 24 and 34 percentage points ($p = 0.24$ from a F-test of the coefficients' equality), with an overall average increase of 27 percentage points. Because the four campuses all have highly similar measured graduation rates — ranging from Davis's 74.3 percent to San Diego's 79.4 percent — this implies that each campus's enrollees' counterfactual enrollment would have been strikingly similar, with mean graduation rates around 50 percent. Between 46 and 54 percent of enrollees would have otherwise enrolled at CSU campuses and 21 to 28 percent would have enrolled at community colleges, depending on the Absorbing UC campus, with the remainder coming from the Dispersing UC campuses.

The same is true for applicants' own likelihood of graduation, which uniformly increases by between 30 and 34 percentage points (F-stat $p = 0.99$). Though the UCSB estimate is somewhat noisy, these coefficients' apparent equality suggests that the four campuses had highly similar attainment treatment effects for ELC participants, with the magnitude of the effect mirroring that of the change in institutional graduation rates. There is some evidence that UCSB caused a relatively greater decline in ELC participants' likelihood of earning a STEM degree than the other UC campuses, but their treatment effects on graduate school enrollment are also similar across institutions.[43]

The bottom half of Table 3.4 shows campus-specific instrumental variable estimates of the effect of ELC participation on early-career labor market outcomes. There is no evidence that enrollment at any of the campuses changed the number of years in which ELC participants are employed in California, and there is some heterogeneity in the wage effects across Absorbing campuses: there is clear evidence that UC Davis increased its students' annual early-career wages by about \$25,000, but the estimated coefficients are positive but imprecise for the other three Absorbing campuses, ranging from \$2,000 to \$16,000.

In total, this evidence suggests that ELC participants were very substantially benefited by

[43]There are at least two possible explanations for this decline in STEM major selection at the ELC eligibility threshold. The first, put forward by Sander and Taylor (2012), argues that less-prepared students likely earn lower grades in introductory science courses when their peers as a result of their peers' stronger academic preparation, discouraging them and leading them to less-challenging majors in other disciplines. However, Bleemer (2020*a*) shows that a natural experiment that led disadvantaged students to enroll in introductory STEM courses with less academically-prepared peers did not improve their performance or persistence in those courses. Alternatively, students who might have otherwise been pressured to earn STEM degrees (perhaps by parents or others advocating for higher-average-wage degrees) could face less (external or internal) pressure after enrolling in a more-selective university, leading them to earn non-STEM degrees. Indeed, Appendix Table B.21 shows noisy reduced-form evidence suggests that barely ELC-eligible students may have been less likely to report the intention of earning a Natural Science or STEM degree on their UC application. ELC-eligible applicants also because substantially more likely to earn a degree in their "intended" discipline (as reported on their UC applications), which increases in the reduced-form among B50 applicants by 2.6 percentage points (s.e. 1.2). Finally, additional speculative evidence can be found in Appendix Table B.22, which presents a 'transition table' showing reduced-form estimates of barely-eligible applicants' major choice changes by intended field of study (as reported on the UC application). The table shows that the largest observable cross-discipline switches among barely ELC-eligible applicants were of intended social science and STEM majors switching into social science degrees and undeclared majors switching from the natural sciences into business degrees, with clear evidence of intended STEM majors switching out of STEM degrees.

enrolling at Absorbing UC campuses instead of less-selective universities.[44] The next section further analyzes effect heterogeneity by comparing outcomes for students from more- or less-competitive California high schools.

3.5.3 Outcome Heterogeneity by Applicant Characteristics

The efficiency of the ELC policy requires that ELC not only provide substantial benefits to targeted participants, but also that those benefits be comparable in magnitude (or larger than) the benefits that would have been derived from Absorbing UC campus enrollment by the "crowded-out" applicants who would have enrolled at those campuses absent the ELC policy. The next section turns to a structural model of university application, admissions, and enrollment in order to characterize those students and their return to more-selective enrollment. Before doing so, this section presents reduced-form evidence on how the return to Absorbing UC campus enrollment differs for different subgroups of near-threshold ELC participants.

Panel A of Figure 3.3 graphs reduced-form estimates of the impact of ELC eligibility on near-threshold applicants' university selectivity (measured by institutional graduation rate) and on three measured outcomes for applicants from different quantiles of California high school. The figures show that students from lower high school quantiles tended to experience larger increases in university selectivity across the eligibility threshold and also tended to face larger increases in educational and labor market outcomes in the following years. These figures reiterate that the ELC policy's benefits almost exclusively obtained for applicants from California's least-competitive high schools.

This pattern of increasing returns may just reflect the higher number of near-threshold ELC participants at less-competitive California high schools. In order to isolate the relative effects of ELC eligibility for different ELC participants, I restrict the sample to the bottom half of California high schools and reestimate Equation 3.2 separately for each quartile, replacing the endogenous variable with an indicator for Absorbing UC campus enrollment ($Absorb_i$).[45] Panel B shows that second-quartile near-threshold ELC participants faced a smaller increase in university graduation rate (15 percentage points) than first-quartile participants (35 percentage points). Despite this tremendous institutional shift — the average bottom-quartile applicant switched from an average local comprehensive university (or above-average community college) into a top-ranked public research university — the return to Absorbing UC campus enrollment for those applicants was nearly as large or slightly larger than the return for the second-quartile students who switched, on average, from somewhat less-selective public universities. The standard errors on these estimates

[44] Appendix Table B.23 presents estimates from alternative specifications of these regression discontinuity and instrumental variable outcome models, including (1) showing reduced-form coefficients from local linear specifications following Calonico et al. (2019) and with an alternative definition of high school eligibility thresholds, and (2) exploiting the assumptions justifying treating Absorbing UC campus enrollment as the endogenous variable in order to estimate potential outcomes for barely below- and above-threshold ELC compliers. It shows, for example, that ELC eligibility increased B50 ELC participants' enrollment institution's graduation rate (likelihood of graduating within five years) from 50 (46) to 77 (75) percent.

[45] This instrumental variable strategy requires the exogeneity assumption that the only reason that applicant outcomes shift across the eligibility threshold is as a result of their Absorbing UC campus enrollment, which in turn requires that either applicants did not switch *between* Absorbing UC campuses across the threshold or that those applicants who did switch would have obtained similar outcomes at either of those campuses, with Table 3.4 providing some evidence for the latter claim.

are quite large, challenging clean parameterization of the relationship between counterfactual enrollment and the return to university selectivity, but this evidence strongly suggests that the value of more-selective university enrollment remains large (and perhaps growing in institutional selectivity) even for students who would have enrolled at non-selective institutions absent the ELC policy. I will return to this relationship below in the context of the structural model.

Appendix Table B.24 provides additional estimates of heterogeneity in the return to more-selective university enrollment under ELC, treating first enrollment institutions' graduation rates as an alternative endogenous variable in Equation 3.2 (a linear projection as in, e.g., Kling (2001)).[46] It shows that the returns to more-selective university enrollment appear statistically and substantively indistinguishable for URM and non-URM students and for male and female students, though many of the estimates have relatively large confidence intervals.

3.6 Structural Model of University Enrollment

More-selective university enrollment substantially benefits the low-testing high-GPA students targeted by ELC. However, while the reduced form analysis above showed that near-threshold ELC participants were lower-income and from less-competitive high schools than their Absorbing UC campus peers, its focus on partial equilibrium outcomes may ignore important general equilibrium effects like universities' dynamic admissions responses to ELC admissions advantages. As a result, the previous analysis cannot characterize compositional or outcome differences between the average "winners" or "losers" of the ELC policy; that is, the students who enrolled at Absorbing UC campuses as a result of ELC and those who were "crowded out" by ELC but otherwise would have enrolled at Absorbing UC campuses.[47] These characterizations — as well as characterizations of the "winners" and "losers" of counterfactual top percent policies with alternative eligibility thresholds — are central to the determination of top percent policies' efficiency, but require estimation of how the policies broadly shift applicants' and universities' decisions.

I analyze those decisions by constructing a three-period model of university applications, admissions, and enrollment adapted from Kapor (2020). First, California-resident high school seniors apply to a portfolio of universities (A_i), including at least one UC campus. Second, each university observes its applicant pool and determines which students to admit. Third, applicants observe which institutions have admitted them (B_i), as well as previously unobserved preference shocks, and choose where to enroll (C_i).

The model spans colleges $j \in 1, ..., J, CC, CSU$, where CC is the California community college system and CSU the California State University system. I assume that all students apply and are admitted to CC and CSU. Each college is characterized by average quality δ_j, with δ_{CC} normalized to 0. The following subsections explain the model by proceeding backward, from enrollment to admission to application.

[46] Appendix Table B.25 performs a series of linearity tests that provide suggestive evidence favoring this instrumental variable design, which imposes a linear relationship between university selectivity and applicant outcomes.

[47] I borrow this "winners" and "losers" terminology from Black, Denning and Rothstein (2020).

3.6.1 Student preferences

After receiving admissions offers, student $i \in I$ chooses to enroll at her most-preferred university j. Her utility of enrolling at j is given by

$$U_{ij} = \delta_j + x_{ij}\beta_j^x + \nu_{ij} + \epsilon_{ij} \tag{3.4}$$

where x_{ij} are student characteristics, $\nu_{ij} \sim N(0, \sigma_{\nu_j}^2)$ are $i.i.d.$ preference shocks always observed by students, and ϵ_{ij} is a previously unobserved idiosyncratic preference shock modeled by the Type I extreme value distribution (perhaps resulting from post-admission campus visits).[48] Student i enrolls at

$$C_i = \max_{j \in B_i} U_{ij}$$

after being admitted B_i. Following from the distribution of ϵ_{ij} (Train, 2003), i's expected utility from being admitted to B_i is given by

$$U_{iB} = \log\Big(\sum_{j \in B} \exp(\delta_j + x_{ij}\beta_j^x + \nu_{ij})\Big)$$

and her conditional likelihood of enrolling at C after being admitted to B is

$$P(C_i = C|B) = \frac{\exp(\delta_C + x_{iC}\beta_C^x + \nu_{iC})}{\sum_{j \in B} \exp(\delta_j + x_{ij}\beta_j^x + \nu_{ij})}.$$

3.6.2 University preferences

Selective universities prefer to enroll the highest-quality class of students, defining students' quality by

$$\pi_{ij} = z_i\beta_j^z + q_i + \mu_{ij}^{Admit} \tag{3.5}$$

where z_i is a vector of student characteristics, q_i is a caliber characteristic of student i unobserved by the student, and μ_{ij}^{Admit} is a normally-distributed error term capturing preference variation across application readers and other factors. Universities admit students $B(j)$ to maximize the quality of their enrollment class:

$$B(j) \;=\; \max_{B \subset A} \sum_{i \in B} E[\mathbf{1}_{\{C_i = j\}}\pi_{ij}] \;=\; \max_{B \subset A} \sum_{i \in B} P(C_i = j|B_i)\pi_{ij} \quad s.t. \quad \sum_{i \in B} P(C_i = j|B_i) \leq k_j$$

where universities' expected enrollment is capped at k_j.[49] Kapor (2020) shows that, under technical assumptions limiting universities' strategic behavior, this results in each university choosing an admissions threshold $\underline{\pi}_j$ such that it admits all applicants with $\pi_{ij} > \underline{\pi}_j$.

[48]While the relationship between U_{ij} and the financial return to i enrolling at j is not explicitly modeled, the β_j^x terms can be understood as potentially partially capturing student-university match effects on observable characteristics, with students of a particular type preferring enrollment at j because of their relatively large return to enrollment.

[49]This model excludes universities from 'balancing' their classes to maintain quotas of certain student types. Balancing classes by gender and/or ethnicity was legally prohibited at public California institutions throughout the study period.

Figure 3.4 presents an internal 2002 UC Davis admissions document explaining their admissions protocols. It shows how closely the presented model maps to the actual admissions practices of most UC campuses during the sample period: Davis assigned each applicant a score based on their characteristics, including a large boost for ELC eligibility, and then admitted all applicants with scores above a threshold determined on the basis of expected enrollment.

3.6.3 University applications

When students choose which universities to apply to, they do not observe ϵ_{ij}, the post-admissions preference shock; μ_{ij}^{Admit}, universities' preference shocks over students; or q_i, a measure of students' own 'caliber' only observed by universities. Instead of directly observing q_i, students observe a signal of their caliber denoted s_i, which is jointly normally distributed with q_i (independently across applicants) by

$$\begin{pmatrix} q_i \\ s_i \end{pmatrix} \sim N\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_q^2(w_i) & \\ \sigma_s^2(w_i) & \sigma_s^2(w_i) \end{pmatrix} \right)$$

where $\sigma_s^2(w_i)$ is the variance of students' signals, $\sigma_q^2(w_i)$ is the variance of students' actual q_i, and $w_i \subset z_i$ are i's sociodemographic characteristics. As in Kapor (2020), the covariance between s_i and q_i is normalized (without loss of generality) to equal $\sigma_s^2(w_i)$ in order to decompose q_i into two interpretable components, one known by students (s_i) and the other unobserved. This allows the marginal distribution of $q_i|s_i$, the information known by students at the time of application, to be written as

$$q_i|s_i \sim N\big(s_i, \sigma_{q|s}^2(w_i)\big)$$

where $\sigma_{q|s}^2(w_i) = \sigma_q^2(w_i) - \sigma_s^2(w_i)$. These variances are parameterized as

$$\sigma_s^2(w_i) = \log(1 + \exp(w_i\gamma^s))$$

$$\sigma_{q|s}^2(w_i) = \log(1 + \exp(w_i\gamma^{q|s}))$$

to constrain them to positive values.

Instead of interpreting q_i as a latent student 'ability' feature, it is best understood as an index of universities' preference for certain students that is unobserved by the econometrician and only partly observed by students. For example, students' applications might contain information — like athletics participation, extracurricular leadership positions, and essay-writing style — the value of which in university admissions is unknown to them. High-q_i students are those who submit unobserved application components that are valued in university admissions. Low-$\sigma_s^2(w_i)$ students are those with strong knowledge of the value of their unobserved application components.

Applicants expect benefits of applying to each university that are proportional to their likelihood of admission to the university and the utility of their being admitted to it, but face costs associated with applying to each additional university. As a result, their maximization problem can be stated as

$$\max_{A \subset J} V_i(A) = \Big(\sum_{B \subset A} P_i(B_i = B|A) U_{iB} \Big) - |A| w_i \gamma^c \tag{3.6}$$

where $P_i(B_i = B|A)$ is i's perceived likelihood of admission to university set B given application set A and γ^c parameterizes i's cost of applying to $|A|$ universities. Following from the distribution of μ_{ij}^{Admit}, i's perceived probability of admission to B is

$$P_i(B_i = B|A) = \int \prod_{j\in B} \Big(\Phi(z_i\beta_j^z + q_i - \underline{\pi}_j)\Big) \prod_{j\in A\setminus B} \Big(1 - \Phi(z_i\beta_j^z + q_i - \underline{\pi}_j)\Big)\phi(q_i|s_i; \sigma_{q|s}^2)dq_i.$$

3.6.4 Estimation

I define each of the covariate sets x_{ij}, z_i, and w_i to include a female gender indicator, three ethnicity indicators (Asian, URM, and other), and log family income.[50] Student preferences (x_{ij}) and university preferences (z_i) also include SAT score, high school GPA, and the estimated value-added of the closest community college as a measure of the quality of students' regional educational availability.[51] Universities' preferences over students also vary by a set of ELC covariates, including an ELC eligibility indicator, the ELC GPA running variable interacted with ELC eligibility (within a narrow bandwidth), and indicators for having a running variable above or below the bandwidth and for whether the ELC program is operative in that year. Finally, students' preferences also vary by a set of distance-to-university covariates — including the distance and squared distance between i's home and j as well as distance interacted with the covariates in w_i — which allow students to have heterogeneous preferences over enrolling at more-distant institutions. I assume that ELC covariates enter into university admissions decisions but not students' preferences over institutions, while (following a long literature) distance covariates enter into students' preferences but not university admissions decisions, each of which helps to separately identify student and university preferences. A Constant term is absorbed in the specifications of x_{ij} and z_i but is included in w_i.

I allow β_j^x to vary for each j for most covariates, but model the effects of distance and its interactions uniformly across universities. I allow separate β_j^z terms for each of the ELC covariates, but otherwise treat university preferences as uniform. All coefficients are deterministic. The socioeconomic covariates w_i enter into students' application costs (γ^c), the variance of their informational signal about their caliber (γ^s), and the variance of the gap between their signal and their true caliber ($\gamma^{q|s}$).

I estimate model parameters $\theta = \{\beta_j^x, \beta_j^z, \gamma^c, \gamma^s, \gamma^{q|s}, \delta_j, \sigma_{\nu_j}^2, \underline{\pi}_j\} \in \Theta \subset R^{99}$ by simulated maximum likelihood using the quasi-Newton method.[52] Following the reduced-form findings on the function of the ELC policy, I group UC campuses into four sets: two sets of Absorbing UC campuses (UCD/UCI and UCSD/UCSB, allowing their different ELC admissions advantage magnitudes), the Unimpacted campuses (UCB and UCLA), and the Dispersing campuses (UCSC, UCR, and UCM). Because enrollment at private and out-of-state universities is observably unchanged as a result of ELC, and because I do not observe application or enrollment to those institutions, I omit those institutions from the model and restrict the estimation sample to UC applicants who enroll at a public California institution. Students can apply to any combination of

[50]For applicants without observed family income, I predict income using high school and Zip code fixed effects, gender-ethnicity indicators, parental education and occupation indicators, and SAT and HS GPA.

[51]See section 3.8.1 for a discussion of these value-added statistics.

[52]Estimation is conducted using MATLAB's *fminunc* function with the BFGS algorithm and default parameterization.

the four combined UC universities (with 15 possible combinations), and all students are also able to enroll at either community college or CSU (each modeled as a single institution).

In order to compare admission and enrollment outcomes in the presence and absence of ELC, I restrict the sample to 2010-2013 UC applicants, the final two years of the ELC policy and the first two years of its absence (see Appendix B.1). It is useful to include non-ELC years both for parameter identification and because UC identified the within-school GPA centile (from first to ninth) of each applicant starting in 2012, permitting counterfactual analysis of alternative top percent thresholds. The resulting estimation sample includes 219,876 applicants.

3.6.5 Likelihood

For each student i, the likelihood of all observables in the data is:

$$l_i(\theta) = \int_s \int_{\nu_i} l_i^A(\theta, \nu_i, s) l_i^{B|A}(\theta, s) l_i^{C|B}(\theta, \nu_i) dF_i(s; \theta) dG_i(\nu_i; \theta) \tag{3.7}$$

where l_i^A is the likelihood of i applying to A_i, $l_i^{B|A}$ is her likelihood of being admitted to B_i if she applied to A_i, and $l_i^{C|B}$ is her likelihood of enrolling at C_i after being admitted to B_i. Following the structural assumptions described above, these terms take the following forms:

$$l_i^{C|B}(\theta, \nu_i) = \frac{\exp(\delta_C + x_{iC}\beta_C^x + \nu_{iC})}{\sum_{j \in B} \exp(\delta_j + x_{ij}\beta_j^x + \nu_{ij})}$$

$$l_i^{B|A}(\theta, s) = \int \prod_{j \in B} \left(\Phi(z_i \beta_j^z + q_i - \underline{\pi}_j) \right) \prod_{j \in A \setminus B} \left(1 - \Phi(z_i \beta_j^z + q_i - \underline{\pi}_j) \right) dF_i(q|s; \sigma_{q|s}^2)$$

$$l_i^A(\theta, \nu_i, s) = \frac{\exp \frac{1}{\lambda} V_i(A)}{\sum_{A' \subset J} \exp \frac{1}{\lambda} V_i(A')}$$

where the smoothing parameter λ is set to 0.1 (see Train (2003)).

3.6.6 Estimated Parameters

Tables 3.5, 3.6, and B.27 present the model's estimated equilibrium parameters, with standard errors from the inverse of the empirical Hessian matrix. The β_j^x and δ_j parameters shown in Table 3.5 are scaled relative to students' preferences for community college; all continuous variables are standardized, so the baseline applicant is a white male with mean attributes. Higher-income students prefer against community college enrollment. While high-SAT applicants have strong preferences for UC's most-selective campuses, high-GPA low-SAT students show a preference for CSU enrollment. Applicants' average preferences align with the UC campuses' selectivity — applicants generally prefer to enroll at more-selective schools — though the average applicant prefers CSU or CC enrollment to enrollment at the Dispersing UC campuses.

The final column in Table 3.5 shows that universities strongly prefer applicants with higher GPAs and SAT scores. With applicants' socioeconomic characteristics proxying other unobserved application components, the UC campuses appear to slightly prefer lower-income, female, Asian, and URM applicants. All of the applicant and university preference parameters are estimated with high precision.

Table 3.6 shows how ELC is embedded into the estimated UC admissions model.[53] As in the reduced-form analysis, the Davis and Irvine campuses provided the largest admissions advantage to ELC-eligible students, followed by the San Diego and Santa Barbara campuses. The Dispersing and Unimpacted campuses are precisely estimated to have only provided very small admissions advantages to ELC-eligible students.

The final row of Table 3.6 shows the model estimates of campuses' admissions thresholds ($\underline{\pi}_j$). The thresholds align with campuses' actual selectivity during the period; the Unimpacted campuses have the highest admissions threshold, followed by UCSD/UCSB, then UCD/UCI, and finally the Dispersing campuses.[54]

Appendix Table B.27 reports the remaining model parameters. Applicants faced positive costs for each additional application, and applicants preferred to enroll at less-distant institutions (with smaller distance costs for higher-income applicants). Lower-income and URM students had substantially more-negative signals of their unobserved caliber q, and applicants generally had strong knowledge of their caliber. Finally, it shows that the magnitudes of students' taste shocks are relatively large across institutions ($\sigma^2_{\nu_j}$ between 1.5 and 4, with standard errors around 0.75), especially for the Unimpacted UC campuses.

3.6.7 Model Validation

The previous subsection showed that the model parameters match widely-held beliefs about the direction and relative magnitude of relationships between observed applicant characteristics and their preferences and admissions outcomes. I further validate the model by testing the success with which it replicates the effects of near-threshold ELC eligibility on applicants' admissions and enrollment outcomes. I restrict the sample to 2010-2011 applicants in the model sample and use the model to estimate each applicant's unconditional likelihood of admission and enrollment at each set of UC campuses. I then compare the binned averages of those likelihoods with the binned averages of those applicants' actual admissions and enrollment outcomes among near-threshold applicants.

These comparisons are visualized in Figure 3.5. While the information provided to the model only includes the ELC GPA running variable within a narrow bandwidth on either side of the threshold, the figures show remarkable alignment between near-threshold applicants' simulated and actual admissions and enrollment outcomes, though applicants' admission to the San Diego and Santa Barbara campuses is underestimated for lower-GPA applicants. The estimated effects of ELC eligibility on UC admission at the eligibility threshold are closely matched by the model, while the effect of ELC eligibility on Absorbing UC campus enrollment is slightly under-predicted by the model. In general, the model effectively simulates the near-threshold effects of ELC relative to reduced-form estimates.

[53] Because Davis, Irvine, and the Dispersing UC campuses admit nearly all above-threshold applicants, the slope of their above-threshold running variable is only weakly identified. I assume those parameters to be 0.

[54] In 2011, the UC campuses' admissions rates were 21 and 26 (Berkeley and UCLA), 38 and 45 (San Diego and Santa Barbara), 46 and 45 (Davis and Irvine), and 64, 76, and 89 (Santa Cruz, Riverside, and Merced).

3.7 The Impact of Top Percent Policies on UC Enrollment Composition

3.7.1 "Winners" and "Losers" of ELC Implementation

In this section, I employ the previous section's model to quantify top percent policies' economic mobility potential by estimating the net effects of top percent policies on selective universities' enrollment composition, focusing on the net enrollment of socioeconomically-disadvantaged students. First, I estimate how the students who enrolled at Absorbing UC campuses because of ELC ("ELC participants") differed from the crowded-out students who were unable to enroll at those universities as a result of the ELC policy. I conduct this counterfactual enrollment exercise in two ways: by eliminating ELC from 2010-2011 admissions in the model (by setting $\beta^{ELC}_{Abs1} = \beta^{ELC}_{Abs2} = 0$) and by adding ELC to 2012-2013 admissions (by setting $ELC = 1$ for applicants in the top four percent of their high school class).[55] I then allow universities to adjust their admissions thresholds $\underline{\pi}_j$ so that their annual expected enrollment remains unchanged, assuming that each Absorbing campus would fill the same number of enrollment seats in one of two ways: through ELC or through their regular freshman admissions process.[56]

In both of these counterfactual exercises, the $\underline{\pi}_j$ parameters adjust as expected: the Unimpacted and Dispersing campuses' admissions thresholds hardly adjust, while $\underline{\pi}_{Abs1}$ and $\underline{\pi}_{Abs2}$ decrease in the former exercise (to expand enrollment absent ELC) and increases in the latter exercise (to shrink non-ELC enrollment); see Appendix Figure B.28. Moreover, the two counterfactual exercises provide very similar estimates for the impact of ELC. The first and third columns of Table 3.7 show that ELC shifted Absorbing UC campus enrollment by about 600 students per year: there are about 600 annual ELC participants and 600 annual crowded-out students.[57] ELC participants' counterfactual enrollments look very similar to the counterfactual enrollments of near-threshold participants estimated above: about half would have otherwise enrolled at CSU, with the remainder split between the Dispersing UC campuses and community colleges.[58] A comparison between the characteristics of simulated ELC participants and those of the estimated local compliers (replicated in column 5 from Table 3.3) shows near-identical URM shares (44-47 percent) and average family incomes (\$63,000-\$67,000). The average simulated ELC participants had somewhat higher SAT scores and high school GPAs than the barely above-threshold compliers.

The second and fourth columns of Table 3.7 show that the characteristics of the students crowded out by ELC appear more similar to the average Absorbing UC campus student, though they are also somewhat negatively-selected (as a result of their being the first students to be rejected in the presence of the ELC policy). Their household incomes were slightly lower than the Absorbing UC average, and about 30 percent were URM (compared to 20 percent overall). While

[55] I set $\beta^{ELC}_{Unimp} = \beta^{ELC}_{Disp} = 0$ in the latter exercise to isolate the admissions effects at the Absorbing UC campuses.

[56] In the counterfactual compositions presented below, I characterize ELC participants as anyone whose likelihood of Absorbing UC enrollment increases in the presence of ELC (and crowded-out applicants as anyone whose likelihood of Absorbing UC enrollment declines), weighted by their change in enrollment likelihood.

[57] That is, the sum of the differences in applicants' enrollment likelihoods in the presence or absence of ELC, conditional on those differences being positive, is 550 in the first simulation and 720 in the second. The sum of the negative differences is the same by construction (after $\underline{\pi}_{Abs1}$ and $\underline{\pi}_{Abs2}$ adjust).

[58] The reduced-form estimates report a somewhat higher relative share coming from the Dispersing UC campuses.

the crowded-out students had below-average SAT scores and high school GPAs, their average SAT scores remained substantially higher than ELC participants'.

Figure 3.6 compares the family income distributions of ELC winners and losers. It shows that the ELC policy increased annual Absorbing UC net enrollment among students with log family incomes between 9 and 11 and decreased annual net enrollment among students with log family incomes over 11.2. However, it also shows substantial overlap between the two distributions; by increasing selective university enrollment among top students from less-competitive California high schools, ELC increased lower-income enrollment at Absorbing UC campuses but also decreased many other lower-income applicants' likelihood of Absorbing UC enrollment through regular admissions channels.

3.7.2 Top Percent Policies and University Enrollment Composition

Next, I estimate how top percent policies with alternative percentile thresholds would impact the composition of the Absorbing UC campuses. As discussed above, 2012 and 2013 applicants were categorized by UC as being in the top one, two, and down to top nine percent of their high school classes, but UC campuses generally provided negligible admissions advantages to students using these class ranks. I simulate counterfactual enrollments as if the Absorbing UC campuses had provided the same admissions advantage to 2012-2013 applicants with GPAs above each rank-specific threshold that they had provided to ELC-eligible students prior to 2012. I estimate these simulations by setting $ELC = 1$ for applicants above each alternative rank-specific threshold and then allowing for $\underline{\pi}_j$ adjustments to equalize expected enrollment.

In 2012-2013, lower-income (URM) students made up about 9,500 (4,700) of the 17,200 freshman California-resident enrollees at the Absorbing UC campuses. Figure 3.7 shows that these net enrollments would increase by about 1 and 2.5 percent (respectively) if the campuses had continued providing similar-magnitude admissions advantages to the top four percent of each high school's graduates after 2011. However, those impacts would have been much larger — about 4 and 8 percent, respectively — if the Absorbing campuses had provided parallel admissions advantages to the top nine percent of each high school's graduates.[59] In sum, these simulations show that top percent policies can substantively increase universities' net enrollment of socioeconomically-disadvantaged students, with larger increases from lower thresholds.

[59]This equates to increases of lower-income and URM enrollment by about 2 percentage points each. Note that it is not obvious *a priori* whether top percent policies with lower percentile thresholds will have the same, larger, or smaller proportional effects on the proportion of lower-income or URM students at a selective university. On the one hand, as the policy provides admissions advantages to students with lower high school GPAs, those students are more likely to be disadvantaged, and as the number of policy losers increases the on-the-margin student is also less likely to be disadvantaged. On the other hand, the on-the-margin student will be coming from a more-advantaged high school (since broadening a top percent policy will increase the number of schools where students will want to take advantage of that policy), which may imply that they will be less likely to be lower-income or URM. However, Figure 3.7 shows that the former trends are dominant: the net effect is that the percentage point gap between the lower-income and/or URM share of ELC winners and losers grows as the policy's admissions threshold declines.

3.8 Discussion: Who Benefits Most from More-Selective Enrollment?

Having shown that top percent policies can meaningfully increase selective universities' net enrollment of disadvantaged students, I conclude by discussing reduced-form and structural evidence on the relative return to more-selective university enrollment for applicants with higher or lower traditional meritocratic rank.

3.8.1 Reduced-Form Evidence

Section 3.5.3 presents reduced-form evidence showing that the benefits of selective university enrollment remain large even for students who would have otherwise enrolled at very low-selectivity institutions. While the reduced-form setting prohibits direct comparison of the return to university selectivity for students crowded out by ELC, I employ estimated "value-added" statistics for each college and university to conduct an alternative comparison: how does the effect of Absorbing UC enrollment for barely-eligible ELC participants compare to those institutions' *average* treatment effect for their enrolled students?

I estimate three measures of institutional value-added: the degree to which each institution tends to increase enrollees' five-year degree attainment, early-career wages, and early-career log wages. Value-added statistics are estimated using 2003-2011 UC applicants (holding out the main estimation sample) in a fixed effect specification following Chetty et al. (2020*a*), controlling for applicant ethnicity and fifth-order polynomials in SAT score and family income.[60]

Figure 3.8 shows how applicants' first enrollment institutions' estimated value-added varies near the ELC eligibility threshold. Panel (a) shows that the change in five-year degree attainment value-added at the eligibility threshold closely matches the change in applicants' actual five-year degree attainment (see Figure 3.2), suggesting that ELC applicants' educational value derived from the Absorbing UC campuses matched the value derived by average UC students. Panels (b) and (c), however, show that ELC participants' increase in institutional wage value-added is far smaller than the increases in early-career wages observed in Figure 3.2: Barely above-threshold B25 applicants enrolled at universities with \$730 (0.02) higher (log) wage value-added but actually earned higher annual wages by about \$2,200 (0.08) in their early careers. While the estimates on wages and wage value-added are not all statistically distinguishable, this suggests that the wage return to Absorbing UC campus enrollment for ELC participants may (substantially) exceed the average return to enrolling at those universities.

3.8.2 Structural Evidence

The structural model estimated above facilitates a more direct test of whether deviations from selective universities' regular meritocratic admissions procedures generate inefficiencies by admitting students who benefit relatively less from selective university enrollment, abstracting from the particulars of UC's ELC policy. Consider applicants' q_i caliber terms observed by UC

[60]For details on value-added estimation for each institution, see Appendix G.1 of Bleemer (2020*a*). Chetty et al. (2020*a*) argue that about 80 percent in the variation of these value-added statistics is 'causal,' implying that differences in the presented value-added statistics may *overstate* differences in institutions' average treatment effects.

campuses in the model. As described above, q_i indexes the latent characteristics of applicants that are valued by UC admissions offices but are unobserved by the econometrician; applicants with high q_i are those whose admissions outcomes are stronger than what would be expected given their test scores, grades, and other characteristics. Similarly, we can define

$$Q_i = z_i\beta^z + q_i$$

(omitting the ELC terms) as the application 'merit' of applicant i as observed by UC campuses. By selecting high-Q_i or high-q_i applicants, are universities admitting students who are generally better able to benefit from their admission? Using a similar selection-on-observables methodology to Dale and Krueger (2002) and Dillon and Smith (2020), I investigate this question by estimating a series of linear regression models relating applicant outcomes to the interaction between university selectivity and either $\hat{Q}_i$ or $\hat{q}_i$.

Among the model sample of applicants — that is, 2010-2013 freshman California-resident UC applicants who first enroll at a public California institution — I estimate each applicant's $\hat{q}_i$ from the posterior distribution implied by the estimated structural model parameters.[61] The estimated $\hat{q}_i$ statistics are normally distributed with mean 0 and standard deviation 0.15. I then estimate $\hat{Q}_i = z_i\hat{\beta}^z + \hat{q}_i$, excluding the ELC terms, and standardize $\hat{Q}_i$ for interpretability. I estimate linear regressions of the form

$$Y_i = \beta_1 GR_i + \beta_2\hat{Q}_i + \beta_3(GR_i \times \hat{Q}_i) + \gamma X_i + \epsilon_i \tag{3.8}$$

where GR_i is i's first enrollment institution's five-year graduation rate and X_i takes one of three forms: (1) null; (2) includes detailed covariates, including gender-ethnicity indicators, SAT score, HS GPA, log income, parental education and occupation indicators, ELC eligibility, and high school, Zip code, and year fixed effects; and (3) those same covariates in addition to fixed effects for every portfolio of UC applications and admissions across campuses (as in, e.g., Mountjoy and Hickman (2020)). These covariate sets are intended to absorb selection bias arising from applicants' non-random enrollment across more- or less-selective institutions. I estimate these models for two outcomes: five-year degree attainment and early-career wages (seven to eight years after high school graduation), with the latter models restricted to pre-2012 applicants (since wages for later applicants are not yet observed). I also estimate similar models replacing $\hat{Q}_i$ with either $\hat{q}_i$ or (standardized) SAT score and high school GPA, as well as models that allow GR_i to be a polynomial expansion of institutional graduation rate. The robust standard errors assume $\hat{Q}_i$ and $\hat{q}_i$ to be accurate.

Table 3.8 shows that enrolling at an institution with a higher graduation rate by 1 percentage point increases applicants' own five-year degree attainment by about 0.8 percentage points, matching the reduced-form relationship estimated for ELC participants. Applicants' measured $\hat{Q}_i$ is also strongly associated with positive outcomes: applicants with a 1 standard deviation higher $\hat{Q}_i$ tend to have higher five-year degree attainment by 16 percentage points and have higher early-career wages by \$10,000. However, there is no evidence that the return to more-selective university enrollment is larger for high-$\hat{Q}_i$ applicants; instead low-$\hat{Q}_i$ applicants benefit slightly

[61] In particular, I draw 1,000 sets of preference shocks, s_i's, and $q_i|s_i$ values, calculate each applicant's q_i and the likelihood of those values given the estimated parameters for each set, and then take the likelihood-weighted average of the resulting q_i's.

more from enrolling at more-selective institutions. In the most restrictive specifications — comparing applicants at different institutions with highly similar socioeconomic and academic backgrounds who had identical UC application and admission outcomes — enrolling at a more-selective institution provides broadly similar attainment and wage benefits to higher- or lower-$\hat{Q}_i$ applicants. Replacing $\hat{Q}_i$ with $\hat{q}_i$ results in smaller but still-negative $\hat{\beta}_3$ estimates, suggesting that the component of universities' applicant preferences orthogonal to socioeconomic and academic characteristics also does not identify higher-value-add students. Including interactions with both SAT score and HS GPA again results in negative interaction terms between university selectivity and each measure of college preparedness (with GPA correlating much more strongly with applicant outcomes than SAT).[62,63]

Taken together, these findings leverage the advantages of the structural model of public California university enrollment to provide evidence against the claim that traditional meritocratic admissions procedures identify the selective university applicants who would most benefit from that education. Instead, the kinds of students admitted under ELC or alternative access-oriented admission policies appear likely to obtain as high or higher benefits of selective university enrollment.

3.9 Conclusion

This study uses a novel comprehensive database of university applications linked to educational and wage outcomes to provide the first quasi-experimental estimates of the impact of more-selective university enrollment on the lives of the high-GPA low-SAT students targeted by top percent policies and other policies that curtail the influence of standardized test scores in university admissions. The University of California's 2001-2011 Eligibility in the Local Context program provided substantial UC admissions advantages to graduates in the top four percent of their high school class. Implementing a regression discontinuity design across high schools' eligibility thresholds, I find that ELC shifted university enrollment among barely-eligible applicants from much less-selective California public colleges and universities into four highly-selective UC campuses. As a result of this shift, barely ELC-eligible applicants became more than 30 percentage points more likely to earn a college degree within five years, graduate school enrollment increased by about 20 percentage points, and early-career annual wages (between seven to nine years following high school graduation) increased by as much as $25,000.

The study then turns to the general equilibrium effects of top percent policies like ELC, estimating a structural model of university application, admission, and enrollment for California public universities. The 600 ELC participants each year were well-characterized by the policy's near-threshold participants: about 65 percent came from families with below-median household incomes, almost half were Black or Hispanic, and their average SAT scores were at the 12^{th}

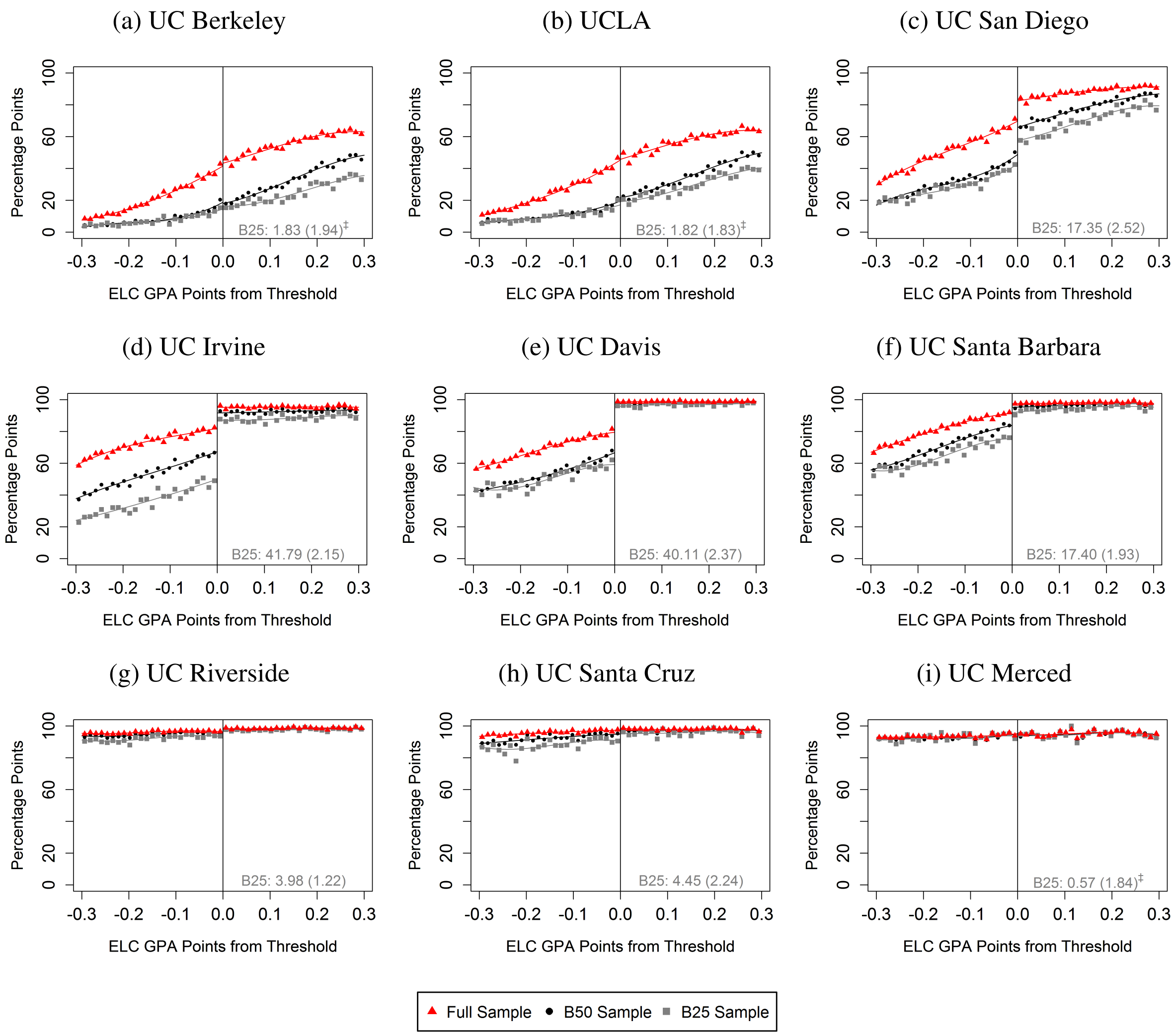
[62] All results are very similar in direction and magnitude when replacing (winsorized) income with log income. Estimates are presented in dollars for interpretability.

[63] Appendix Figure B.15 visualizes estimates from an alternative version of Equation 3.8, with fifth-order polynomials in GR_i interacted with in-sample tercile indicators for q_i, SAT, and HSGPA. Plots of the derivatives of the resulting polynomials (which represent the gains in degree attainment associated with the increase in GR_i at each GR_i) show substantial uniformity across most of the distribution of GR_i where each of the terciles has support in the data.

percentile of their Absorbing UC peers. Compared to the "crowded-out" students replaced by ELC participants, the participants were about 15 percentage points more likely to be underrepresented minorities (URM) and had lower average family incomes by 0.3 log points. A potential expansion of the ELC policy to the top nine percent of UC applicants from each California high school is estimated to increase lower-income and URM Absorbing UC enrollment by 4 and 8 percent, respectively (each about 350 students per year). Finally, both reduced-form and structural evidence are brought to bear on the efficiency of top percent policies, with both suggesting that the returns to more-selective enrollment experienced by the targeted disadvantaged applicants are no lower — and may be considerably higher — than they would have been for the regular-admissions students who would have otherwise enrolled in their place.

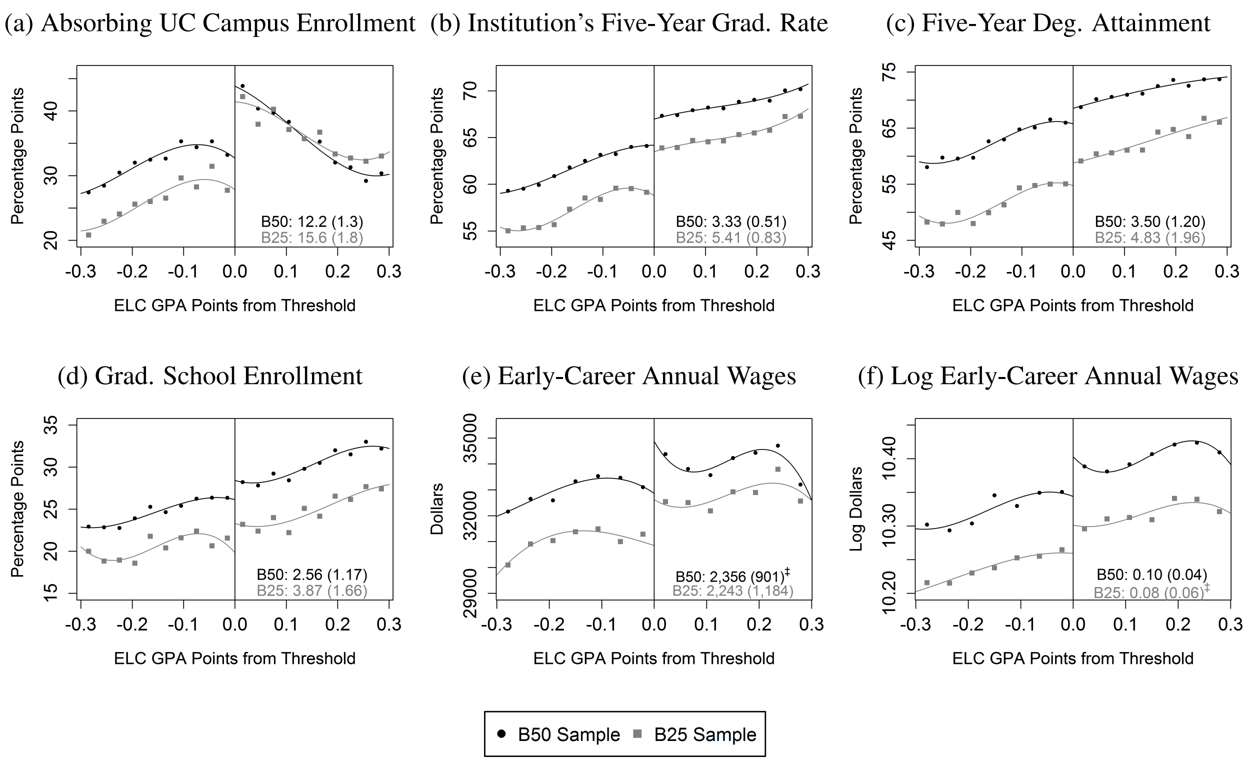
This study presents the first quasi-experimental analysis of the medium-run impact of selective university admission under an access-oriented admission policy, finding that broadening selective university access is an impactful and potentially-efficient economic mobility lever available to policymakers. It also provides unique analysis of how high-GPA low-SAT students perform at selective research universities that typically would have rejected them because of their poor standardized test scores, showing that the students likely to be advantaged by test-optional or no-test admissions policies would be substantially benefited (though selective universities' graduation rates may decline as they enroll more-disadvantaged students). Finally, this study challenges a central tenet supporting test-based meritocratic university admissions policies — that the policies efficiently allocate educational resources to students who will best be able to take advantage of them — by identifying a group of low-testing (perhaps high-noncognitive-skill) and low-opportunity applicants who appear to earn greater benefits from selective university enrollment than the higher-testing applicants who are typically admitted in their place.

Figure 3.1: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to each UC Campus



Note: Applicants' likelihood of admission to each UC campus by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Beta estimates are from cubic regression discontinuity models following Equation 3.2 for the B25 sample, with standard errors in parentheses clustered by high-school-year. Each panel conditions on applying to that UC campus. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ indicates reduced-form estimates with $p > 0.1$ for the null hypothesis ($\hat{\beta} = 0$) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). Source: UC Corporate Student System.

Figure 3.2: Local Effect of ELC Eligibility on UC Applicants' Education and Wage Outcomes

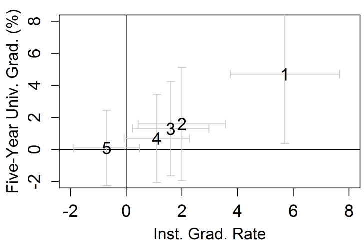


Note: Regression discontinuity plots of applicants' measured outcomes by ELC GPA distance from their high school's ELC eligibility threshold, among applicants from the bottom half (B50) or quartile (B25) of high schools by SAT. Points are binned averages; lines are cubic fits. Beta estimates are from cubic regression discontinuity models following Equation 3.2, with standard errors in parentheses clustered by high-school-year. Absorbing campus enrollment is measured in the fall semester following UC application. Institutions' graduation rates are defined for institution of first enrollment (within six years after graduating high school); see Appendix B.4 for details. Graduate school enrollment is defined as enrollment at a four-year institution following Bachelor's attainment within seven years of graduating high school. Early-career wages are averaged over California covered wages seven to nine years after high school graduation; log wages omit zeroes, and wages are winsorized at 5 percent. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ indicates reduced-form estimates with $p > 0.1$ for the null hypothesis ($\hat{\beta} = 0$) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

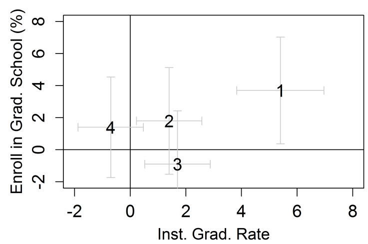
Figure 3.3: Local Effect of ELC Eligibility on UC Applicants' Outcomes by High School Quantile

Panel A: Reduced-Form Outcomes by HS Quantile

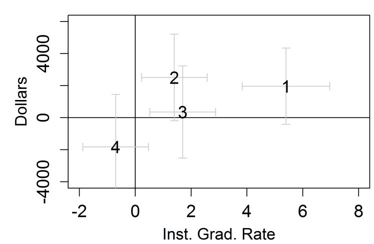
(a) Five-Year Deg. Attainment



(b) Enroll in Grad. School

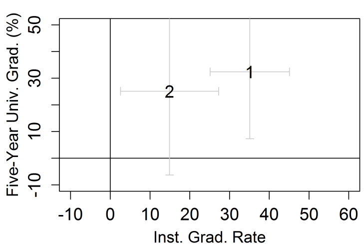


(c) Avg. Early-Career Wages

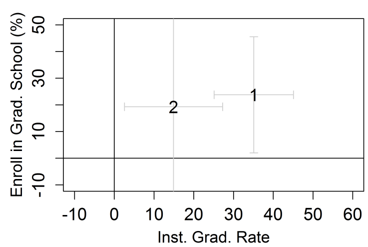


Panel B: Instrumental Variable Outcomes by Quartile, with Endogenous Variable $Absorb_i$

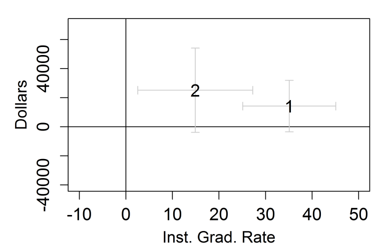
(d) Five-Year Deg. Attainment



(e) Enroll in Grad. School



(f) Avg. Early-Career Wages



Note: Estimates of $\hat{\beta}$ from Equation 3.2 (Panel A) and replacing the endogenous variable with Absorbing UC campus enrollment (Panel B) by high school SAT quantile (where 1 indexes the lowest quantile). The x-axis plots estimates for enrollment institution's graduation rate; the y-axis plots five-year degree attainment, enrollment in graduate school within seven years of UC application, and average California covered wages 7-9 years after high school graduation, winsorizing wages at 5 percent. Confidence intervals are clustered by school-year and are estimated independently by axis. Panel B restricts the sample to the bottom two quartiles. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

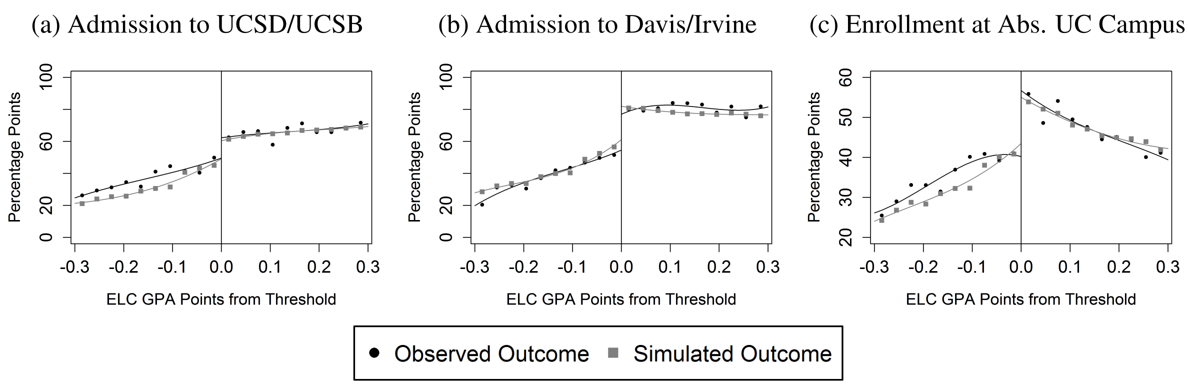
Figure 3.4: 2002 Admissions Protocol used by UC Davis

POINT RANGES & WEIGHTS FOR SELECTION CRITERIA

Criteria	Point range	Weight	Total possible score
HS GPA	2.8–4.0	1000	4000
5 Exams (SAT I/ACT & 3 SAT II)	200–800 each	1	4000
ELC (Eligibility in the Local Context)	0 or 1	1000	1000
Number of "a-f" courses beyond minimum	0–5	100	500
Individual Initiative	0 or 1	500	500
EOP (Educational Opportunity Program)	0 or 1	500	500
Pre-collegiate motivational program	0 or 1	500	500
First-generation university attendance	0 or 1	250	250
Non-traditional	0 or 1	250	250
Veteran/ROTC Scholarship	0 or 1	250	250
Significant Disability	0 or 1	250	250
Leadership	0 or 1	250	250
Special Talent	0 or 1	250	250
Perseverance	0 or 1	250	250
Marked improvement in 11th grade	0 or 1	250	250
TOTAL REVIEW			13,000

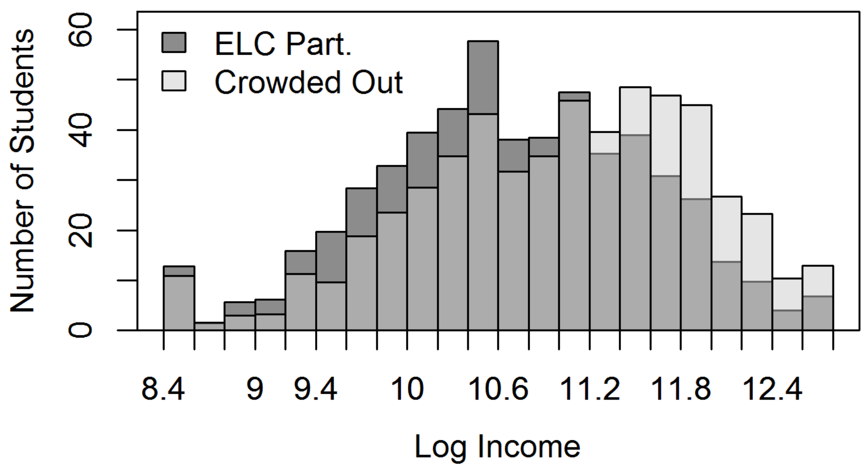
Note: This photograph shows an internal archival UC Davis admissions document visualizing Davis's 2002 freshman admissions protocol. Students were assigned points on the basis of applicant characteristics, and those with scores above a designated threshold were admitted to the campus. Source: Fall 2002 UC Davis Selection Criteria, Admissions Office Slide Collection, AR-123, Special Collections, UC Davis Library.

Figure 3.5: True and Simulated UC Admission and Enrollment for Near-Threshold B50 UC Applicants



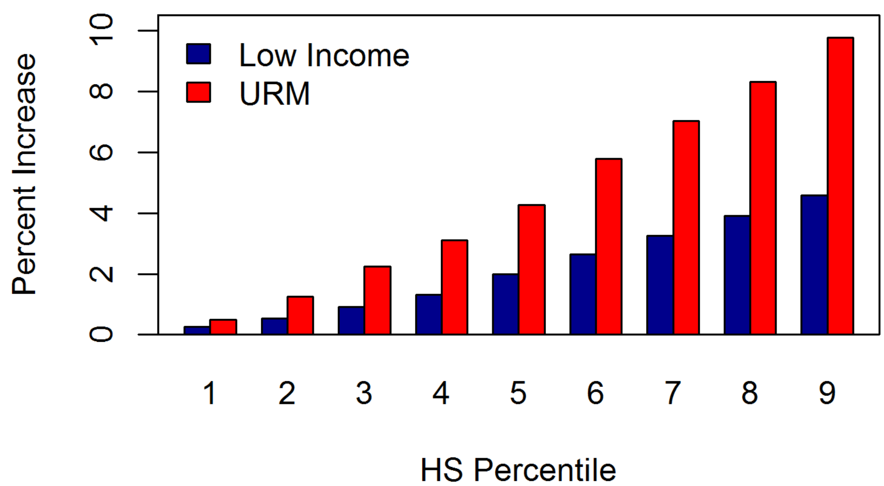
Note: Binned scatterplots and third-order polynomial best-fit lines of 2010-2011 UC applicants' (black) unconditional admission or enrollment at each set of UC campuses and (gray) simulated likelihoods of unconditional admission or enrollment at each set of UC campuses using the estimated parameters from Equation 3.7, by their ELC GPA distance from their high schools' ELC eligibility threshold. Sample restricted to 2010-2011 UC freshman California-resident applicants who (1) enroll at a public California institution in the fall after high school graduation, (2) who have ELC GPAs within 0.3 of their high school's eligibility threshold, and (3) graduated from the bottom half (B50) of high schools by SAT. Source: UC Corporate Student System and the National Student Clearinghouse.

Figure 3.6: Log Family Incomes of Simulated ELC Participants and Crowded Out Students



Note: Distribution of family incomes of annual ELC participants and crowded-out students under a simulation (employing the estimated parameters of the model described in Equation 3.7) in which 2010-2011 UC applicants were no longer provided an admissions advantage at the Absorbing UC campuses ($\beta^{ELC}_{Abs1} = \beta^{ELC}_{Abs2} = 0$). ELC participants are defined as applicants whose simulated likelihood of Absorbing UC campus enrollment increases, and crowded-out applicants those whose likelihood decreases; applicants are weighted by their net change in likelihood and halved to scale annually. Missing family incomes are imputed — see footnote 50 — and incomes are winsorized at 8.4 and 12.6. Sample restricted to 2010-2011 UC freshman California-resident applicants who enroll at a public California institution in the fall after high school graduation. Source: UC Corporate Student System and the National Student Clearinghouse.

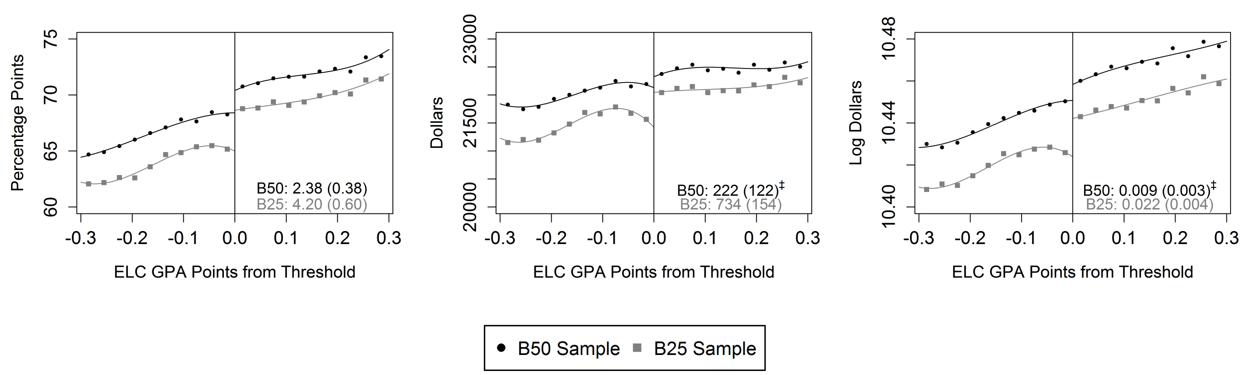
Figure 3.7: Simulated Changes in Absorbing UC Enrollment under Counterfactual Top Percent Policies



Note: Estimated percent changes in the number of low-income and URM Absorbing UC campus students under top percent policies in which those campuses provide their estimated ELC admissions advantage to the top x percent of graduates from each high school, with x ranging from 1 to 9, relative to no top percent policy. Estimates from simulations employing the estimated parameters of the model described in Equation 3.7. Each simulation assigns ELC eligibility to the top x percent of each high school's graduates; Absorbing campus enrollment characteristics are determined by weighting each applicant by their estimated likelihood of enrolling at those campuses. Missing family incomes are imputed — see footnote 50 — and low income is defined as applicants with family incomes below the California median. The sample is restricted to 2012-2013 UC freshman California-resident applicants who enroll at a public California institution in the fall after high school graduation. Source: UC Corporate Student System and the National Student Clearinghouse.

Figure 3.8: Local Effect of ELC Eligibility on UC Applicants' First Institutions' Estimated Value-Added

(a) Five-Year Deg. Attain. VA (b) Early-Career Wage VA (c) Log Early-Career Wage VA



Note: Regression discontinuity plots of the estimated value-added of applicants' initial enrollment institution (within 6 years of high school graduation) by ELC GPA distance from their high school's ELC eligibility threshold, among applicants from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Beta estimates are from cubic regression discontinuity models following Equation 3.2, with standard errors in parentheses clustered by high-school-year. Institutional value-added estimated for degree attainment and wages and log wages (averaged 7-9 years after graduating high school, omitting zeros in the log and winsorizing at 5 percent) using 2003-2011 UC applicants (holding out applicants in the main estimation sample) conditional on ethnicity and fifth-order polynomials in family income and SAT score following Chetty et al. (2020*a*). Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ indicates reduced-form estimates with $p > 0.1$ for the null hypothesis ($\hat{\beta} = 0$) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

Table 3.1: Descriptive Statistics of 2003-2011 UC Applicants

	CA Freshman Applicants	Near ELC Threshold	By SAT Quartile of High School[1] Bottom	Second	Third	Top
% Female	52.5	61.0	64.4	61.8	59.6	58.2
% White	31.9	35.7	13.0	38.1	48.8	43.0
% Asian	31.9	33.0	25.4	34.2	31.7	40.9
% Hispanic	24.2	21.0	50.5	18.2	9.9	5.6
% Black	5.2	3.2	6.9	3.1	1.6	1.0
SAT Score	1706	1843	1533	1787	1941	2104
HS GPA	3.67	4.03	3.81	4.01	4.11	4.19
Parent Income (Median)	60,000	68,700	34,000	70,000	95,000	118,300
% Missing Inc.	11.9	20.9	7.2	16.6	24.2	35.5
Enrollment Rates (%)						
Unimpacted UC	11.2	22.9	12.5	17.2	26.2	35.8
UCLA	5.6	11.0	7.2	8.3	12.8	15.6
Berkeley	5.6	11.9	5.3	8.9	13.4	20.1
Absorbing UC	21.4	29.1	31.7	37.3	30.6	16.8
San Diego	5.0	8.2	6.7	10.3	9.4	6.5
Santa Barbara	5.1	6.6	8.2	7.9	6.9	3.4
Irvine	5.5	6.9	8.2	9.1	7.0	3.1
Davis	5.8	7.4	8.6	10.0	7.2	3.7
Dispersing UC	9.6	5.2	10.9	6.0	3.1	0.8
Santa Cruz	4.0	2.0	2.5	2.7	2.1	0.7
Riverside	4.6	2.6	6.7	2.7	0.8	0.1
Merced	1.0	0.6	1.6	0.6	0.2	0.0
CSU	15.7	11.5	19.7	13.8	9.1	3.2
Community Coll.	7.9	3.9	7.5	5.0	2.5	0.8
CA Private Univ.	7.4	9.7	5.6	8.5	11.4	13.1
Non-CA Univ.	9.7	10.6	3.4	6.7	11.1	21.1
No NSC Enrollment	17.1	7.2	8.6	5.6	6.0	8.5
N	1,751,719	171,441	42,904	42,821	42,900	42,808

Note: Characteristics of 2003-2011 CA-resident freshman UC applicants overall and within 0.3 ELC GPA points of their high schools' ELC eligibility threshold ('Near'). SAT scores out of 2400; converted from ACT or 1600-point SAT if otherwise unavailable. Income is "Missing" when applicant does not report it on their UC application. Enrollment is measured in the fall semester following high school graduation; categories partition all applicants. [1]Applicant-weighted school-year quartiles by the SAT scores of applicants within 0.3 GPA points of their school's ELC eligibility threshold; statistics restricted to near-threshold applicants. Source: UC Corporate Student System and National Student Clearinghouse

Table 3.2: Local Effect of ELC Eligibility on First Enrollment Institution

	University of California Campuses Unimpacted	Absorbing	Dispersing	CSU	Comm. Coll.	CA Priv.	Non-CA	No Coll.
Panel A: Baseline Enrollment Likelihood (%)								
All	26.1	25.8	4.9	11.1	3.4	10.2	11.3	7.2
B50	14.0	32.9	9.0	18.8	6.4	7.1	5.1	6.6
B25	11.5	27.9	12.9	21.7	8.7	5.4	3.2	8.8
Panel B: Local Change in Enrollment Likelihood Caused by ELC Eligibility (p.p.)								
All	0.2	5.9	-1.7	-3.0	-0.8	-0.3	0.4	-0.7
	(0.7)	(0.8)	(0.4)	(0.5)	(0.3)	(0.5)	(0.5)	(0.4)
B50	1.0	12.2	-3.6	-6.0	-1.8	-0.4	-0.2	-1.1
	(0.9)	(1.3)	(0.7)	(1.0)	(0.6)	(0.7)	(0.6)	(0.7)‡
B25	1.2	15.6	-5.1	-7.3	-3.4	0.6	-0.3	-1.3
	(1.2)	(1.8)	(1.2)	(1.6)	(1.0)	(0.9)	(0.7)	(1.1)

Note: Reported coefficients are the estimated baseline (ELC-ineligible) proportion of near-threshold applicants who enroll at each group of institutions in the fall semester following UC application, and the estimated change in enrollment for barely above-threshold ELC-eligible applicants (β). Values in percentages; estimates overall and for students from the bottom half (B50) and quartile (B25) of high schools by SAT. Estimates from cubic regression discontinuity models following Equation 3.2; standard errors are clustered by school-year and omitted for baseline estimates (which are estimated following Abadie (2002)). ‡ Indicates estimates with $p < 0.1$ for the null hypothesis such that $p \not< 0.05$ (*insignificant* at conventional levels) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). Source: UC Corporate Student System and National Student Clearinghouse.

Table 3.3: Characteristics of Near-Threshold ELC Compliers

Panel A: Student Characteristics

	Female (%)	URM (%)	Rural High School (%)	SAT Score	HS GPA	Family Income[1] ($)	Below-Med. Fam. Inc.[1] (%)
All	68.3	43.9	8.1	1524	3.87	66,900	65.4
	(7.9)	(7.2)	(3.9)	(47.0)	(0.04)	(12,100)	(8.4)
Bottom Quartile	70.7	60.9	3.0	1396	3.76	45,900	78.9
	(7.0)	(7.0)	(3.5)	(31.0)	(0.03)	(5,700)	(6.0)
Second Quartile	59.0	10.3	20.76	1700	3.97	116,000	25.0
	(12.5)	(10.4)	(6.7)	(45.0)	(0.04)	(20,900)	(15.5)
Abs. Mean[2]	56.0	20.1	5.3	1796	3.80	87,300	49.8

Panel B: High School SAT Quartiles

	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
All	57.5	31.0	2.1	9.3
	(7.6)	(7.0)	(7.4)	(5.1)
Abs. Mean[2]	20.0	22.2	24.6	33.2

Note: Reported coefficients are estimated characteristics of near-threshold ELC compliers, or the barely above-threshold students who enroll at Absorbing UC campuses as a result of their ELC eligibility. Estimates for characteristic W_i follow Equation 3.2, replacing the endogenous variable with an indicator for Absorbing UC enrollment ($Absorb_i$) and defining the outcome as $Absorb_i \times W_i$. Standard errors in parentheses are clustered by school-year. See the text for definition of high school quartiles. ACT scores and 1600-point SAT scores are converted to 2400-point SAT scores using contemporaneous standard formulas. Rural high schools defined following designation from the National Center for Education Statistics. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. [1]Family income is missing if not reported on the UC application (12 percent of applicants). Median California household income defined at $76,000 in 2019 dollars; missing-income families are assumed to have above-median income. [2]The true average for freshman CA-resident students who first enrolled at an Absorbing UC campus between 2003 and 2011. Source: UC Corporate Student System and National Center for Education Statistics.

Table 3.4: Instrumental Variable Estimates of the Effect of Absorbing UC Enrollment for ELC Participants

	Absorbing Campus IV	Campus-Specific IVs Davis	UCSD	UCSB	Irvine	F^1
Predicted Grad.[2]		-0.37	-0.38	0.65	-0.46	0.561
		(0.52)	(0.79)	(0.91)	(0.66)	
Institution's 5-Year Grad. Rate	26.78	24.1	34.3	33.8	24.0	0.235
	(3.83)	(4.78)	(7.32)	(8.76)	(6.36)	
Grad. Within 5 Years (%)	28.59	32.80	30.47	33.59	30.19	0.987
	(9.92)	(11.42)	(17.56)	(22.19)	(15.13)	
Earn STEM Degree (%)	-14.28	-7.64	-6.69	-45.63	-20.00	0.060
	(8.81)	(10.94)	(17.27)	(20.25)	(14.31)	
Enr. At Grad School within 7 Yrs. (%)	20.94	31.08	21.86	46.68	39.42	0.653
	(9.78)	(11.84)	(18.11)	(22.23)	(15.67)	
Num. Yrs. Pos. CA Wages[3]	0.47	0.33	0.19	-0.14	0.43	0.898
	(0.30)	(0.35)	(0.48)	(1.01)	(0.45)	
Avg. Early-Career Wages[3]	20,341	24,819	16,095	1,555	7,788	0.049
	(8,199)	(10,581)	(13,836)	(28,973)	(12,635)	
Avg. Early-Career Log Wages[3]	0.76	0.82	0.20	0.13	0.24	0.011
	(0.33)	(0.30)	(0.48)	(0.64)	(0.32)	
First Stage F	91.6	106.5	12.8	21.2	62.7	
Conditional F		67.9	53.5	48.2	62.8	

Note: Estimates of the effect of Absorbing UC campus enrollment on educational and labor market outcomes for near-threshold ELC-eligible students, following Equation 3.2 replacing the instrumented ELC_i variable with an indicator for Absorbing UC campus enrollment in the first column and following Equation 3.3 for campus-specific effects. Institutions' graduation rates are defined for institution of first enrollment (within six years after graduating high school); see Appendix B.4. Graduate school enrollment is defined as enrollment at a four-year institution following Bachelor's attainment within seven years of graduating high school. Log distance to Santa Barbara is set to 0 after 2010 to increase instrument strength; see Appendix Table B.19 for unadjusted estimates. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Conditional F statistic estimated following Sanderson and Windmeijer (2016). [1]F-test of the null hypothesis of equality among the four campus enrollment coefficients. [2]The predicted values from an OLS regression (across the full sample of 1995-2013 UC freshman California-resident applicants, excluding the study's primary sample) of five-year NSC graduation on gender by ethnicity indicators, maximum parental education indicators (7 categories), family income, missing income indicator, SAT score, HS GPA, and year indicators. [3]The number of years between 7 and 9 years after high school graduation in which the applicant has positive covered California wages, and the applicants' unconditional average annual wages and conditional average log wages in the period, winsorizing wages at 5 percent. Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

Table 3.5: Main Applicant and University Preference Model Parameters

	Applicant Preferences (β_j^x), Relative to CC					Univ. Pref. (β_j^z)
	Unimp. UC	UCSD/UCSB	UCD/UCI	Disp. UC	CSU	All UC
Log Inc.	0.15 (0.03)	0.26 (0.02)	0.19 (0.02)	0.06 (0.02)	0.20 (0.01)	-0.12 (0.004)
Female	-0.42 (0.06)	-0.03 (0.03)	0.02 (0.03)	0.00 (0.03)	0.11 (0.03)	0.10 (0.01)
Asian	0.93 (0.09)	-0.18 (0.05)	-0.31 (0.05)	0.02 (0.04)	-0.02 (0.03)	0.23 (0.01)
URM	2.55 (0.08)	1.00 (0.04)	1.82 (0.04)	0.28 (0.04)	-0.23 (0.03)	0.04 (0.01)
SAT	1.07 (0.05)	0.50 (0.02)	-0.11 (0.02)	-0.19 (0.02)	-0.18 (0.02)	0.53 (0.005)
HS GPA	-0.87 (0.07)	-0.81 (0.03)	-0.79 (0.03)	-1.02 (0.02)	0.27 (0.01)	1.15 (0.01)
CC VA	-0.01 (0.03)	-0.04 (0.02)	-0.02 (0.02)	-0.32 (0.02)	-0.13 (0.01)	-0.04 (0.003)
δ_j	4.97 (0.10)	2.18 (0.04)	2.13 (0.04)	-0.47 (0.04)	0.76 (0.03)	

Note: Parameter estimates from maximum simulated likelihood (maximized by the BFGS Quasi-Newton algorithm) of Equation 3.7. Parameters measure applicant preferences for each set of universities (see Equation 3.4) and universities' preferences for applicants (see Equation 3.5). Continuous variables are standardized in-sample. 'CC VA' is the estimated value-added of the nearest community college to applicants' home address, estimated following Chetty et al. (2020*a*); see Appendix G.1 of Bleemer (2020*a*). Reported standard errors from the inverse of the empirical Hessian matrix. Missing family incomes are imputed; see footnote 50. Sample restricted to 2010-2013 UC freshman California-resident applicants who enroll at a public California institution in the fall after high school graduation. Source: UC Corporate Student System and the National Student Clearinghouse.

Table 3.6: ELC and Admissions Model Parameters

	Unimpacted	UCSD/UCSB	UCD/UCI	Dispersing
ELC Eligibility	0.15	0.80	1.69	0.40
	(0.06)	(0.08)	(0.08)	(0.17)
ELC GPA $\times$ Above	0.60	-0.53	0	0
	(0.87)	(1.47)		
ELC GPA $\times$ Below	1.39	0.79	1.09	-0.69
	(0.92)	(1.14)	(1.16)	(2.83)
Above Bandwidth	0.23	-0.08	-0.47	-0.19
	(0.04)	(0.07)	(0.08)	(0.17)
Below Bandwidth	-0.14	-0.28	-0.31	0.10
	(0.04)	(0.05)	(0.05)	(0.13)
No ELC	-0.18	-0.28	-0.33	-0.51
	(0.04)	(0.05)	(0.05)	(0.13)
$\underline{\pi}_j$	1.95	0.46	0.15	-1.63
	(0.04)	(0.05)	(0.05)	(0.13)

Note: Parameter estimates from maximum simulated likelihood (maximized by the BFGS Quasi-Newton algorithm) of Equation 3.7. Parameters measure universities' preferences for applicants (see Equation 3.5) with regard to their ELC GPAs and eligibility. 'ELC GPA' running variable is set to zero outside a 0.08 GPA bandwidth from the eligibility threshold; 'above' and 'below' bandwidth indicates applicants with ELC GPAs outside that bandwidth above or below the threshold, with 'below bandwidth' including all applicant without ELC GPAs. 'No ELC' indicates students who applied to UC after 2011; all other ELC variables are 0 after 2011. The 'ELC GPA$\times$Above' coefficients for UCD/UCI and Dispersing campuses are set to 0 since those schools admit nearly all above-threshold applicants. Reported standard errors from the inverse of the empirical Hessian matrix. Sample restricted to 2010-2013 UC freshman California-resident applicants who enroll at a public California institution in the fall after high school graduation. Source: UC Corporate Student System and the National Student Clearinghouse.

Table 3.7: Simulated Counterfactual UC Enrollment with and without ELC Admissions

	Remove ELC, '10-11		Add ELC, '12-13			
	ELC Part.	Crowded Out	ELC Part.	Crowded Out		
Annual Enr.						
Absorbing UC	-549	549	717	-717		
Unimpacted UC	30	-30	77	-77		
Dispersing UC	98	-98	-124	124		
CSU	277	-254	-443	405		
CC	143	-166	-227	265	LATE Compliers	Absorbing UC Average
URM	44.1	27.2	46.9	32.8	43.9	20.1
Family Income	62,900	85,200	63,100	83,200	66,900	87,300
SAT	1625	1729	1627	1693	1524	1796
HS GPA	3.98	3.66	4.01	3.66	3.87	3.80

Note: Characteristics of applicants who become more likely (ELC participants) or less likely (crowded out) to enroll at the Absorbing UC campuses as a result of those campuses' implementation of ELC, on the basis of two counterfactual simulations employing the estimated parameters of the model described in Equation 3.7. The first simulation restricts the sample to pre-2012 and sets $\beta^{ELC}_{Abs1} = \beta^{ELC}_{Abs2} = 0$, eliminating the Absorbing UC campuses' ELC admissions advantage; the second simulation restricts the sample to post-2011 and assigns ELC eligibility to the top four percent of applicants from each high school. Applicants are weighted by half of their net change in Absorbing UC enrollment likelihood to scale annually. Complier characteristics and Absorbing UC student averages from Table 3.3 are presented for comparison. Missing family incomes are imputed; see footnote 50. Sample restricted to 2010-2013 UC freshman California-resident applicants who enroll at a public California institution in the fall after high school graduation. Source: UC Corporate Student System and the National Student Clearinghouse.

Table 3.8: Estimated Relationship between Student 'Merit' and Return to University Selectivity

Var:	$\hat{Q}$			$\hat{Q}$			$\hat{q}$		SAT	
Y_i:	Five-Year Deg. Attain.			Early Wages (7-8 Yr.)			Deg.	Wages	Deg.	Wages
Inst. Grad. Rate	0.77 (0.01)	0.77 (0.01)	0.81 (0.01)	220 (15)	199 (17)	207 (20)	0.81 (0.01)	207 (20)	0.80 (0.01)	206 (20)
Var	15.68 (0.40)	-3.80 (1.63)	-0.39 (2.33)	9851 (789)	3060 (3337)	1788 (4936)	2.23 (0.47)	334 (985)	2.66 (0.49)	1423 (1033)
Var × Inst. Grad. Rate	-0.11 (0.01)	-0.10 (0.01)	-0.05 (0.01)	-116 (13)	-98 (14)	-63 (18)	-0.04 (0.01)	-6 (15)	-0.05 (0.01)	-29 (16)
HS GPA									9.73 (0.48)	6537 (1036)
HS GPA × Inst. Grad. Rate									-0.01 (0.01)	-40 (19)
Det. Covariates		X	X		X	X	X	X	X	X
Adm. Portfol.			X			X	X	X	X	X
Observations	110,114	107,300	107,300	51,144	49,339	49,339	107,300	49,339	107,300	49,339

Note: Estimates of Equation 3.8 for 2010-2013 freshman California-resident UC applicants who first enroll at a public California institution. Institutions' graduation rates are defined for each applicant's institution of first enrollment (within six years after graduating high school); see Appendix B.4 for details. Applicants' university-observed caliber $\hat{q}_i$ — a latent index of universities' preferences for certain applicants on the basis of unobservables — is estimated using the posterior distribution of q_i's resulting from the structural model parameters described above, and applicant summed admissions merit $\hat{Q}_i$ is estimated by $z_i\hat{\beta}^z + \hat{q}_i$, excluding the ELC covariates. $\hat{q}_i$, $\hat{Q}_i$, SAT, and HSGPA are standardized. Detailed covariates include gender-ethnicity indicators, SAT score, HS GPA, log income, parental education and occupation indicators, ELC eligibility, and high school, zip code, and year fixed effects; admissions portfolios include indicators for every combination of UC campuses to which the applicant applies and UC campuses to which they are admitted. Five-year degree attainment indicates earning a college degree within five years of high school graduation. Early-career wages are measured as average observed wages 7-8 years after high school graduation; wages are winsorized at 5 percent and are unobserved for post-2011 applicants. Robust standard errors in parentheses assume that $\hat{q}_i$ and $\hat{Q}_i$ are accurately measured. Source: UC Corporate Student System, the National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

Chapter 4

Major Choice Restrictions and Student Stratification

4.1 Introduction

Undergraduate major selection has substantial long-run labor market implications: students earn higher post-graduate wages if they earn degrees in 'high-return' professional degrees (Deming and Noray, 2020; Bleemer and Mehta, 2020*c*) or degrees in their preferred field of study (Kirkeboen, Leuven and Mogstad, 2016; Daly and Le Maire, 2019). Underrepresented minority (URM) and lower-income university students are underrepresented in many high-earning fields like computer science and economics, which likely exacerbates income inequality (Monarrez and Washington, 2020). Meanwhile, many universities impose restrictions – like minimum GPA requirements and competitive internal applications – on which fields of study are available to enrolled students, with restrictions particularly prevalent in those same high-demand fields. This study analyzes whether and how major restrictions contribute to the socioeconomic stratification of university students across fields of study.

Prior studies on major selection has largely focused on student preferences; a recent survey does not mention major restrictions in its discussion of the 'supply side' of choosing a college major (Altonji, Arcidiacono and Maurel, 2016).[1] However, major restrictions are widely implemented at selective public universities in the United States. Table 4.1 shows the restrictions imposed on five of the highest-wage college majors at the 25 top-ranked US public universities (according to US News & World Report). These universities enroll about 750,000 undergraduates, or half of all students at top-100 American universities (and 7 percent of *all* American undergraduates).[2] Half of these schools restrict their computer science majors –

[1]Stange (2015), Andrews and Stange (2019), and Denning and Turley (2017) discuss major-specific price discrimination and incentive payments, which are important – though presently less-common – supply-side contributors to major choice. Stinebrickner and Stinebrickner (2014) show that even students at a small private university without major restrictions are over-optimistic about their likelihood of earning STEM majors, though they attribute major switching to demand-side factors. Rask (2010) argues that low grades in STEM courses explain a small portion of lower persistence in STEM courses (see also Butcher, McEwan and Weerapana (2014)). Altonji, Arcidiacono and Maurel (2016) do mention major restrictions as a potential source of identifying variation to estimate major-specific returns, an approach implemented by Bleemer and Mehta (2020*c*).

[2]Wage statistics as reported by Altonji, Blom and Meghir (2012).

typically to students who earn high grades (minimum 2.5-3.75 GPAs) in introductory computer science courses – while 10 have restricted economics majors. Only two schools do not restrict their finance majors, and only Georgia Tech does not restrict Mechanical Engineering. Every university with a Nursing school restricts entry to that major.[3]

This study analyzes the impact of major restrictions using a new dataset of demographic and course records for the over 900,000 students who enrolled between 1975 and 2018 at four selective public universities: UC Berkeley, UC Davis, UC Santa Barbara, and UC Santa Cruz. It employs difference-in-difference event study designs at the departmental and student level to estimate the effect of the 29 new major restrictions imposed during the period. It then examines Economics as a case study, comparing students' persistence in required courses by socioeconomic characteristics at two universities, one of which had a minimum grade restriction.

We find that major restrictions lead to an 10-20 percent decline in the number of students declaring that major on average. URM students and students with poorer academic preparation are much more likely to exit restricted majors than their peers. Major restrictions impede major choice for students with *absolute* academic disadvantage, not comparative disadvantage in the field; the students who exit restricted majors earned similarly-low first-quarter grades across all disciplines, not just in the restricted field. On average, restrictions cause female and URM students intending restricted majors to instead enroll in relatively lower-return fields of study. The case study shows that URM and lower-income students become less likely to earn degrees in a restricted field because of their lower average grades in introductory courses, which is explained in part by their lower SAT scores and more-limited prior access to related AP and IB high school courses. This evidence implies that major restrictions inefficiently limit student choice on the basis of students' pre-enrollment educational opportunity and demographically stratify students across majors by average wages.

We begin in section 2 with a discussion of the setting and data analyzed in this study. The four universities in the analysis–UC Berkeley, UC Santa Barbara, UC Davis, and UC Santa Cruz–are highly-ranked public research universities in California. Comprehensive student records since the 1970s–including students' complete transcripts as well as demographic and geographic information–were collected by separate agreements with each university's Office of the Registrar, collated into a single database, and merged with students' post-1993 application records maintained by the University of California Office of the President. We identified major restrictions using paper and digitized versions of the four universities' historical course catalogs, which provide annual records of the conditions under which each major can be declared.

Section 3 presents difference-in-difference analysis at the university-major level of the impact of newly-implemented major restrictions on which students declare the major. We combine majors offered by the same department with comparable restrictions and omit short-lived restrictions, restrictions imposed on new majors, and restrictions imposed too early or too recently to estimate their impact with the observed data, ending up with 24 major restriction 'events' in the sample period. The resulting panel is defined at the cohort-major level over time, where cohort is defined by students' first year of enrollment. We find that restrictions tend to be imposed after a period of enrollment growth, resulting in an immediate and persistent enrollment decline of about 12

[3]Grade restrictions of C+ (2.3) or below are excluded, as they are generally put in place to prevent students who cannot pass upper-division courses from beginning technical majors, not to manage demand among students capable of passing introductory courses.

percent. There is no observed impact on the proportion of declared majors who are female, but the proportion of URM majors falls by about 2.7 percentage points, or a 20 percent decline from the 14 percentage point baseline. Majors' average SAT score increases by over 30 points. Restricting to the freshman fall grades received by declared majors, normalized across courses to correct for differing standards in different disciplines, we find that majors' grades in related and unrelated courses improve by 0.15 and 0.13 standard deviations, respectively, suggesting the absence of selection on comparative advantage.

Section 4 turns to a student-level event study design to assess which majors are chosen by students who lose access to restricted majors. We characterize students who 'intend' restricted majors using a projection of their first-quarter courses estimated by LASSO. Female students become less likely to intend majors after they are restricted, but there is no evidence of an effect on the average academic aptitude of intended majors, overall or by subgroup. However, we find that female and URM students who intend restricted majors tend to earn relatively lower-return majors than their male and non-URM peers after the restriction is implemented, generating stratification across fields likely to exacerbate existing inequities.

We investigate the mechanisms by which major restrictions stratify students using a case study of the Department of Economics at UC Santa Barbara (UCSB), which has a 2.85 minimum GPA requirement in introductory courses between 2010 and 2016. Section 5 shows that female, URM, and lower-income students are less likely than their peers to enroll in Economics 1, the department's mandatory first course, and earn substantially lower grades in the course. Holding other socioeconomic characteristics equal, students who attended high schools that did not offer AP Micro- or Macroeconomics earned about 0.16 fewer grade points on average. Students' socioeconomic characteristics and measured preparedness (SAT score and high school GPA) explain 11-18 percent of grade variation in economics students' introductory courses; even holding SAT score and ethnicity fixed, students who completed Economics 2 were less likely to declare the economics major if they were Female (5.5 p.p.) or Hispanic (5.83 p.p.), with a notable positive correlation between major declaration and having attended a high school that taught college-level economics.

While some of these trends likely reflect the impact of UCSB's major restriction, they may also reflect student preferences or other 'soft' restrictions imposed by the department (like low introductory course grades and high workloads). In order to better isolate the effect of UCSB's major restriction, we compare UCSB's covariate estimates with those of UC Davis, which requires a similar introductory course curriculum but does not impose a minimum GPA restriction. In this single-difference framework, we interestingly find that URM students actually appear slightly *more* likely to enroll in Economics 1 at UC Santa Barbara and earn higher grades than non-URM students on average. Nevertheless, URM students who take Economics 1 are 11 percentage points less likely to declare the economics major under the major restriction, and lower-income students (proxied by their not reporting family income) are *26 percent less likely* to declare the major at UC Santa Barbara than their higher-income peers. College-level economics course availability at students' high schools is also a much stronger predicter of major declaration at UC Santa Barbara, reflecting those students' performance advantages in introductory economics courses, as are SAT scores and high school GPAs (reflecting the major restriction's focus on absolute-advantage students). In general, this case study shows that even after these students were screened and admitted to a selective public research university, GPA minima are more likely to discourage students with fewer previous educational opportunities, and appear to have differentially-large negative effects on URM and lower-income students' likelihood of declaring the major.

Course catalogs provide imprecise information about which student cohort was the first to experience new major restrictions. While we assume that restriction first binds the cohort that begins one year prior to the restriction's first appearing in the catalog, we treat the years immediately to each side of that year as 'transition' years and focus our analysis on average changes before and after that period. As a result, we conduct pre-trend analysis relative to three years prior to each major restriction 'event', and find no notable evidence of pre-trends in the four preceding years on demographics or preparedness. We do observe steady growth in majors' log total enrollment leading up to the restriction's imposition.

This study makes three main contributions. First, it contributes to an equity-oriented literature interested in socioeconomic stratification across (MacLeod and Urquiola, 2015; Chetty et al., 2020*a*; Arcidiacono, Kinsler and Ransom, 2019*a*,*b*) and within (Schultz et al., 2011; Arcidiacono, Aucejo and Hotz, 2016; Mourifie, Henry and Meango, 2020; Brenoe and Zolitz, 2020; Card and Payne, 2021) universities, providing the first known evidence that a popular university policy magnifies stratification. Major restrictions likely have substantive implications for impacted students' postgraduate outcomes: Kirkeboen, Leuven and Mogstad (2016) show evidence of large postgraduate wage declines among students prohibited from earning degrees in their preferred discipline, and Bleemer and Mehta (2020*c*) show that falling just below an economics department's GPA major restriction substantially decreases rejected students' early-career wages.[4]

Second, this study documents an important determinant of student major selection that has been largely omitted from the large academic literature on major choice.[5] While that literature has largely focused on the demand-side of major choice – particularly students' preferences and subjective expectations (Arcidiacono, Aucejo and Spenner, 2012; Zafar, 2013; Kinsler and Pavan, 2015; Wiswall and Zafar, 2015, 2018) – this brief describes a widely-implemented supply-side policy that substantially limits many students' access to high-average-wage majors. This study also documents an important source of selection bias in the estimation of major-specific returns; majors (like engineering and nursing) that many universities restrict are likely to yield substantially upwardly-biased estimates of major-specific returns absent sample selection corrections, especially for relatively-disadvantaged students.[6]

Finally, this study contributes to a literature immediately interested in the aggregate number of STEM degrees awarded by American universities (Ehrenberg, 2010; National Academies, 2007; Wang, 2013; Sjoquist and Winters, 2015*a*,*b*; Castleman, Long and Mabel, 2018). Half of the major restrictions imposed by the four universities discussed below were imposed in STEM fields, and major restrictions generally impose a previously-unreported ceiling on STEM major growth in many fields at many universities, particularly discouraging URM and less-relatively-prepared students from earning high-demand STEM majors.

[4]Griffith (2010) shows that students with lower measured preparedness are less likely to earn STEM majors, while Arcidiacono, Aucejo and Hotz (2016) and Bleemer (2020*a*) come to different conclusions about whether enrollment at more-selective universities under affirmative action decreases URM students' STEM degree attainment.

[5]See Altonji, Blom and Meghir (2012) and Altonji, Arcidiacono and Maurel (2016) for surveys.

[6]E.g. see Carnevale, Cheah and Hanson (2015). In his study of the contribution of sample selection bias to cross-major differences in mean wages, Arcidiacono (2004) argues in favor of "large differences in preferences that high ability individuals have for the more lucrative fields". These could be demand-side preferences, but also appear to reflect supply-side access to lucrative restricted majors. Interestingly, Bleemer and Mehta (2020*c*) do not find evidence of this upward bias in the context of UC Santa Cruz's economic major.

4.2 Background and Data

As shown in Table 4.1, major restriction policies are widely-implemented at selective public universities in the U.S.[7] Major restrictions at U.S. universities take one of three forms: (1) an average grade requirement in introductory courses; (2) an internal application favoring performance, extracurricular participation, and professed interest; or (3) an external application, such that students can only earn a degree in the field if they had directly applied to the major prior to enrollment. We refer to the first of these types as 'mechanical' restrictions and the second two as 'discretionary', since the latter restrictions facilitate more-nuanced decisions over who is permitted into restricted majors.footnoteThese restrictions are often complemented by 'soft' restrictions like low introductory course grades and verbal discouragement, but empirical tractability leads us to focus exclusively on easier-to-observe mechanical and discretionary restrictions.

This paper focuses on the connection between major restriction policies and student stratification across majors. Table 4.2 presents observational evidence suggesting the potential of a relationship between the two. Focusing on the selective public universities and lucrative majors listed in Table 4.1, the table presents estimates from fixed-effect linear regressions of the share of 2019 graduates from each major who were underrepresented minorities (defined as Black or Hispanic) on the presence of mechanical and discretionary major restriction policies. It shows that about 11 percent of graduates from those universities' lucrative majors were URM, but among restricted majors only 8 percent (over 25 percent fewer) were URM. The second column shows that this gap is wholly driven by mechanical restrictions; there is no measurable relationship between the presence of discretionary restrictions and majors' URM shares.

As a result, we present a series of analyses below focusing on the stratification ramifications of mechanical restriction policies. As a result of data availability, we examine the restrictions implemented by the four observed University of California campuses: at Berkeley, Davis, Santa Barbara, and Santa Cruz.[8] This study focuses on the restrictions imposed by four selective public universities in California: the University of California campuses at Berkeley, Davis, Santa Barbara, and Santa Cruz. For reference, these are among the country's most selective institutions; their 2008 U.S. News & World Report rankings across all U.S. universities were 21, 42, 44, and 79, respectively.

We observe student outcomes at these campuses using a novel student enrollment database collected as part of the UC ClioMetric History Project (Bleemer, 2018*b*). The sample includes all undergraduate students who first enrolled at each of four University of California campuses in the observed sample period: UC Berkeley (1975 to 2016), UC Davis (1980 to 2018), UC Santa Barbara (1986 to 2018), and UC Santa Cruz (1975 to 2018).[9] The data include first year of enrollment,

[7]Major restrictions are generally justified by either capacity constraints resulting from temporary over-demand – though many remain in place for decades – or on the pedagogical grounds that lower-performing (but passing) students cannot succeed in challenging fields of study. Thinly-stretched resources from 'over-enrollment' could negatively-impact educational quality (Bound and Turner, 2007; Bound, Lovenheim and Turner, 2010), in part by through increased class sizes (Bettinger and Long, 2017). They may also result from an increasing interest in 'prestige' departments.

[8]All but one of the UC restrictions implemented in our study period were mechanical restrictions, so below we estimate the overall average effects of major restriction policies.

[9]Ethnicity is observed after 1975 (Berkeley and Santa Cruz), 1987 (Santa Barbara), or 1990 (Davis).

gender, ethnicity, and California residency; underrepresented minorities (URM) are defined to include Black, Hispanic, and Native American students. The data also cover each of the courses completed by each student and their grades in each course. For students who enrolled after 1993, we link the data to UC Office of the President undergraduate application records that include SAT score, high school GPA, family income, and (for California-resident freshmen) high school.[10] Finally, we observe students' pre-college access to college-level coursework by linking public California high schools to 1997-2016 California Department of Education school records, which identify school-years in which each Advanced Placement or International Baccalaureate course was available.[11]

Table 4.3 shows every formal major restriction policy that has been implemented by the four UC campuses since the 1970s, before which no restriction has been identified. Each restriction's first year is defined as the year prior to the major restriction first appearing in the school's course catalog, since that entering cohort is typically the first that would face the new binding major requirement. For major restrictions that are no longer implemented, a 'Last Year' is also recorded, again referring to the final cohort that likely faced the restriction. Restrictions with GPA caps at or below 2.3 (a C+ average in the requisite courses) are omitted, both because of their prevalence and because they are unlikely to bind in most cases. Each campus has imposed about 12 restricted majors over the past 50 years, though Davis's restrictions tend to be more-numerous and shorter-lived than those at other campuses. Berkeley and Davis's Computer Science departments have implemented restrictions twice.

One possibly-important effect of major restrictions is to stratify students by their university course performance, with higher-performing students permitted to enroll in restricted fields of study. Student grade point averages (GPAs) are often used to measure university course performance, but GPA is biased by differences in grading standards across academic disciplines. Figure 4.1 displays average course GPAs by division at UC Berkeley throughout the sample period, showing large and growing gaps in average grades by discipline: Science and Engineering courses had average grades about 0.2 GPA points below the Humanities in 1970, but the gap had grown to almost 0.4 GPA points by the mid-2010s. The distributional shape of available grades may also differ by discipline.

In order to abstract away from cross-field differences in grade availability, a new "Normed GPA" measure is calculated as follows:

$$nGPA_i = \frac{1}{|C_i|} \sum_{c \in C_i} \frac{GPA_{ic} - \overline{GPA}_c}{sd(GPA)_c} \tag{4.1}$$

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

Table 4.4 presents descriptive statistics of the majors offered at each of the four UC campuses.

[10] All statistics produced using UCOP data are replicated from Bleemer and Mehta (2020*b*).

[11] California Department of Education course-level school information available at http://www.cde.ca.gov/ds/sd/df/filesassign.asp.

Each campus offered an average of 54 majors in each year of the sample period, with an annual average of 86 students per major (s.d. 115). The average major was 53 percent female and 20 percent URM. There were 29 newly-imposed major restrictions during the period covered by the data – with 5-10 at each of the four campuses – and 25 restrictions imposed in the period when ethnicity is observed. The total sample includes about 900,000 students who enrolled in 6,300 major-cohort pairs.

Table 4.4's last column shows characteristics of majors soon to implement major restrictions. Those majors are twice the size of average majors, averaging 203 annual students, and only 14 percent of their students are URM.

4.3 Department-Level Event Study

4.3.1 Empirical Methodology

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

$$Y_{cmy} = \alpha_{cm} + \gamma_{cy} + \sum_{t=-7}^{8} \beta_t \mathbb{1}_{\{y+t=P_{cm}\}} + \epsilon_{cmy} \tag{4.2}$$

where Y_{cmy} is a feature of campus c's major m in cohort year y (like log number of students), α_{cm} and γ_{cy} are campus-major and campus-cohort fixed effects, and P_{cm} is the first cohort-year that faced major m's restriction at c. For example, $Y_{UCB,Econ,1990}$ could represent the log number of 1990-cohort students (that is, students whose first year of enrollment was 1990) who declared an economics major (whether or not they ultimately earned a degree) at UC Berkeley. Standard errors are clustered by campus-major.

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

4.3.2 Results

Panel (a) of Figure 4.2 shows β estimates and 95-percent confidence intervals from Equation 4.2 for the log number of students who declare newly-restricted majors before and after the imposition of the restrictions. The estimates suggest that major restrictions are put into place about five years after a major begins growing relative to other fields. Imposing the restriction causes an immediate cessation of this growth in the average department, with longer-run enrollment stabilizing around 20 percent below peak enrollment (similar to the pre-growth enrollment level), despite the observed increased student demand in that major.

What were the characteristics of the students denied from the major as a result of newly-implemented major restrictions? The next two panels of Figure 4.2 shows that the proportion of female students in newly-restricted majors remained unchanged, but that the average proportion of URM students declined by 3 percentage points. Given the 20 percentage point decline in all major declarations, this implies that URM students were over twice as likely to exit the major as a result of the restriction than non-URM students (about 17 vs. 37 percentage points).[12]

How did major restrictions differentially impact students with different levels of measured academic aptitude? The left panel of Figure 2 shows that newly-restricted majors' enrollees had higher average SAT scores by almost 40 points (on the 2400 scale), with the increase occurring over the three-year transitional period of the new restriction. This suggests that the students who exited the restricted major had average SAT scores of about 200 points (over half of a nationwide standard deviation) lower than the average student declaring the major.

Panel (b) of Figure 4.3 shows that major restrictions yield students whose normed GPAs averaged across their first-quarter courses in the same discipline as the restricted major were higher. This is partly by construction, since some of these courses may have been used to calculate the introductory course GPA used to determine access to restricted majors. Panel (c), however, shows a near-identical effect on the average first-quarter Normed GPA earned by students in the major when calculated only over courses in other disciplines.[13] These results imply that students who exit restricted majors had average normed first-quarter GPAs about 0.75 standard deviations lower than the major's average, even when their GPA is calculated using only courses outside the major's discipline. The similarity between Panels (b) and (c) suggests that major restrictions do not target students based on their comparative advantages – that is, students with particular academic strengths in the restricted field – but instead target students whose academic performance is generally stronger across *all* fields (absolute advantage).

These results, summarized in Table 4.5, indicate that major restrictions reduce the number of students who declare the restricted major, with URM students far more likely to exit the major than non-URM students. The restrictions appear to select students with general academic advantages as opposed to students with advantages specific to the field of study. The next section devolves this analysis to the student level in order to understand where students flow when they exit restricted majors.

[12]This and similar estimates below of the characteristics of major restriction 'compliers' – that is, students who would have declared the major if not for the restriction – require assuming that the major restriction did not impact the likelihood of major declaration of students who would otherwise have *not* declared the major. If the major restriction immediately encouraged positively-selected students to declare that major (perhaps believing that the restriction would increase the major's educational quality or postgraduate return), then these estimates could be overestimates of the true effect.

[13]Mathematics and Statistics courses are omitted from all majors' "Outside Normed GPA", since those courses are often required by (and included in the restriction GPA calculations of) majors in nearly all disciplines.

4.4 Student-Level Event Study

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

4.4.1 Empirical Methodology

While a small number of previous studies have proxied University of California students' major intentions using the 'intended majors' reported on their undergraduate applications (e.g. Arcidiacono, Aucejo and Hotz, 2016), these self-reported intended majors are generally non-binding, can be strategically selected, and are not reported by about one-third of students (who report an 'undeclared' intended major). As a result, students' self-reported intended majors likely poorly characterize students actual major choice intentions. Instead, we develop a revealed-preference proxy of students' major intentions using students' freshman Fall courses, which students select in the first weeks after they arrive on campus. Because a large number of courses are available to students in their first quarter, their choices reveal substantial information about their intended majors. Let M_{im} indicate if student i majors in field m, with m reflecting a campus-major pair. We predict students' intention to major in m by separately estimating the following model for each m by penalized (LASSO) regression:

$$M_{im} = \alpha_m + \sum_{c \in C_i} \beta^c_{mi} FF_{ic} + \gamma X_i + \epsilon_{im} \tag{4.3}$$

where FF_{ic} indicates whether i took course c (out of all available courses C_i) in their freshman fall quarter. We allow β^c_{mi} to differ either by gender or by URM ethnicity and include those two indicators as X_i. To avoid biased $\hat{M}_{im}$'s resulting from by changes in student behavior after the imposition of major restriction policies, equation 4.3 is estimated for each major over a training sample of students who arrived on campus three or four years prior to the policies' implementation in that major.[14] Students with higher $\hat{M}_{im}$ took courses that more strongly suggest their intention to major in m. Since universities have a large number of majors, the distribution of $\hat{M}_{im}$ is strongly left-skewed.

We use these individual-level $\hat{M}_{im}$ estimates to answer two questions. First, we measure the degree to which students' revealed major intentions shift as a result of major restriction policies by estimating the following single-difference model over a stacked sample across majors:

$$\hat{M}_{im} = \zeta_m + \sum_{t=-6}^{6} \beta_{it} \mathbb{1}_{\{y_i + t = R_m\}} + \epsilon_{im} \tag{4.4}$$

with the number of times each student appears in the sample equal to the number of restricted majors imposed at their university within six years of their matriculation (and thus the number of

[14] The training sample consists of half of eligible students, and is omitted from the second-stage regression analysis below.

available $\hat{M}_{im}$'s).[15] We estimate specifications in which the coefficients of interest β_{it} are fixed across all students and specifications where fixed β_{it} varies by either the student's gender or ethnicity. We set $\beta_{-3} = 0$ for all i and estimate Equation 4.4 by weighted least squares, with weights equal to the inverse number of students at that campus so that each major is equally weighted in the analysis (matching the previous section). The standard errors ϵ_{im} are clustered by major and by student.[16]

Second, we use these predicted intentions of earning restricted majors to identify the major choices of students who intend majors in the years before and after their restrictions. Our estimation strategy extends this event study design in a difference-in-difference framework, estimated by WLS over the same stacked dataset:

$$Y_{im} = \zeta_{my_i} + \gamma \hat{M}_{im} + \sum_{t=-6}^{6} \beta_{it} \mathbb{1}_{\{y_i + t = R_m\}} \times \hat{M}_{im} + \epsilon_{im} \tag{4.5}$$

These regressions include major-cohort indicators ζ_{my_i} to flexibly absorb within-campus major choice trends, exploiting variation between students with stronger and weaker intentions of earning the restricted major m relative to the baseline year. As above, β_{it} is estimated either overall or by gender or ethnicity, with $\beta_{-1} = 0$ for all i.

We summarize the 'quality' of students' resulting major choices by the mean wage earned by college graduates with that major (conditional on gender, ethnicity, and age) as observed in the American Community Survey (Ruggles et al., 2020), relative to the lower-wage "General Agriculture" major (see Table C.1).[17] If major restrictions tend to stratify campuses by leading URM students to enroll in lower-average-wage majors, Equation 4.5 would reveal ethnicity differences in the average wage-by-major of the majors received by students who intend restricted majors.

4.4.2 Results

Figure 4.4 shows that in the years leading up to new major restrictions, university course enrollment shifts in such a way as to indicate a 0.3 percentage point increase in students' intention to earn those restricted majors. Once the restrictions are in place, overall major intentions stabilize around 0.2 percentage points below their peak, wholly drive by a larger decline among female students. There is no observable heterogeneity in average major intentions by ethnicity. These estimates indicate that female students are discouraged from ever trying to earn restricted majors, though there is no parallel effect for male students. Given the department-level estimates presented above, this suggests that major restrictions have dissimilar dynamics by gender and ethnicity: female students are discouraged from intending restricted majors but are not ultimately less likely to complete them, whereas URM students continue to take the requisite introductory courses but are nevertheless less likely to complete restricted majors.

Panel (a) of Figure 4.5 shows that students who intend restricted majors become much less likely to earn those majors after the restrictions' implementation. A student with a predicted 50

[15] The data includes 21 restricted majors in this section, since we do not observe Berkeley course records three years prior to that campus's economics major restriction (prohibiting estimation of $\hat{M}_{im}$).

[16] The presented 95-percent confidence intervals treat $\hat{M}_{im}$ as if it were observed without noise.

[17] A crosswalk between ACS majors and the data's 525 UC majors is available from the authors.

percent chance of earning the restricted major – on the basis of their demographics and freshman fall courses – becomes about 20 percentage points less likely to earn the major as a result of the restriction, on average. Panel (b) shows that students who intend restricted majors have similar GPA FE's in the years before and after the restrictions' implementation, suggesting no sharp swings in the academic aptitude of the students who intend restricted majors as a result of the restriction. Figure C.4 shows that both of these estimates are broadly similar by gender and ethnicity.

Finally, Figure 4.6 presents estimates of how the implementation of major restrictions impacts the 'quality' of majors earned by students who intended those majors by gender and ethnicity, following Equation 4.5 and measuring major quality by average wages by major (WBM). Interestingly, it shows that on average, the major restrictions implemented by the four analyzed University of California campuses led intended students to earn *higher*-WBM majors on average, in part a reflection of the many low-WBM majors restricted at UC in the period. However, the restrictions appear to generate substantial WBM gaps by both gender and ethnicity, with female and URM intended majors earning relatively lower-WBM majors than their male and non-URM intended-major peers. An interaction of the terms in Equation 4.5 with the restricted major's WBM suggests that hgiher-WBM restricted majors tend to lead students to lower-WBM majors on average, but that both low-WBM and high-WBM major restrictions generate gender and ethnic stratification across majors by WBM.

The evidence presented thusfar suggests that major restrictions differentially impact disadvantaged students and lead them to select less-lucrative college majors. The next section provides greater detail about one specific major restriction – imposed by the Department of Economics at UC Santa Barbara – in order to provide some insight into why restrictions function in this manner.

4.5 Analysis of Mechanisms: A Case Study of Economics

To shed light on how major restrictions influence the majors that students enter, we compare entry into the high-return economics majors at UC Santa Barbara (UCSB) and UC Davis between 2010 and 2016.[18] These majors provide a useful case study for several reasons:

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

2. Each campus had a similarly-structured progression of introductory courses that students were required to take prior to major declaration: two quarters of calculus, introductory micro- and macroeconomics (Economics 1 and 2), and one or two additional courses depending upon students' chosen track.

[18]Economics is among the highest-WBM majors ovvered by UC campuses; see Table C.1. UC Berkeley's economics major is omitted because Berkeley's semester schedule (instead of UCSB and Davis's quarter schedules) yields a different lower-division economics curriculum, with introductory micro- and macroeconomics combined into a single course. This prohibits direct comparison with the other campuses. UCSC economics also provides a limited test case, since its restriction was non-binding in its early years of implementation Bleemer and Mehta (2020*c*).

3. All economics tracks at Santa Barbara had a 2.85 grade point average restriction (over 3-5 introductory economics courses), while the Davis economics major was unrestricted.[19]

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

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

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

$$Y_{iyct} = \alpha_{ct} + \gamma_y + \beta_c X_i + \epsilon_{iyct} \tag{4.6}$$

$$Y_{iyct} = \alpha_{ct} + \gamma_y + \beta_c X_i + \beta_c' X_i \times UCSB_i + \epsilon_{iyct} \tag{4.7}$$

where each outcome Y_{iyctu} for student i in cohort y who completed course c in term t is modeled as a function of students' demographic, socioeconomic, high school opportunity, and academic preparedness characteristics.[20] Cohort and course-term fixed effects are included for each campus, and standard errors are clustered by high school. Propensity weights ensure that the Davis and Santa Barbara student samples are balanced on observed covariates, including the full set of covariates described above as well as county fixed effects for Californians.[21] Our preferred interpretation of these models is that between-campus differences students' propensity to declare the major mainly reflect the effect of UCSB's economics major restriction.

The first two regression models presented in Table 4.6 examine which of the students who enrolled in ECON 1 eventually declared economics majors, where ECON 1 enrollment is a signal of students' potential interest in majoring in economics.[22] The first model includes only demographic and socioeconomic characteristics as covariates, directly testing whether UCSB's major restriction induces social stratification. The baseline Davis estimates, where any student is permitted to declare an economics major after passing the introductory courses, reveal how "preferences" for the major differ by race and income.[23] They reveal a significant relative preference for the subject among Asian students, but not among URM students. There is some

[19]UC Davis's Managerial Economics track, like many business-oriented economics majors, had a 2.8 GPA major restriction prior to 2013. That track catered to almost half of the students in economics-based majors at UC Davis. While Davis's 'partial' major restriction could attenuate the results discussed below, the coefficient estimates are similar (but less-precise) if the sample is split prior to 2014 and models are re-estimated separately in both periods (available from the authors).

[20]These characteristics include gender, ethnicity, log parental income, SAT score, high school GPA, California residency, California public school enrollment, and the presence of AP and IB economics for students from public CA high schools. An indicator for missing income marks students who omitted their family income on their college application, usually connoting above-average income or wealth (Bleemer, 2020*a*).

[21]In particular, each observation is weighed by the student's inverse likelihood of enrolling at that campus, recovering the average treatment effect for students at both campuses.

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

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

evidence that preference for economics increases with income; the high-income students who do not report family income statistics are much more likely than average to declare the major.[24]

At Santa Barbara, by comparison, Asian students who took ECON 1 are not significantly more likely to declare an Economics major, while URM students are 10 percentage points less likely to declare an economics major. The magnitude of this URM difference is appreciable relative to an average declaration propensity of 26.4 percent at UCSB.[25] The difference between the campuses in URM students' propensity to declare an economics major is similarly large and statistically significant. Income also appears to have stronger effects on enrollment at Santa Barbara. This is consistent with the major restriction muting student preferences, and doing so in a way that stratifies students on race and income, as students who exit the economics major are very likely to instead earn lower-return majors (Bleemer and Mehta, 2020*c*).

The second regression model in Table 4.6 includes academic opportunity and preparation covariates. In contrast to the previous results, racial differences between similarly-prepared students are much smaller than the unconditional gaps, though URM students remain somewhat less likely to declare an economics major at UCSB than at Davis, by 3.1 (s.e. 2.0) percentage points.[26] This suggests that the primary stratifying effect of the major restriction is to induce selection on the basis of prior preparation.

The other coefficients in this regression confirm that impression. At Davis, ECON 1 students with higher SAT scores and high school GPAs are less likely to select an economics major, while the opposite is true at UCSB. This suggests that economics tends not to be the top choice of the most-prepared (ECON 1) students, but that the major restriction systematically prevents less-prepared students from declaring the major at UCSB.[27] Second, while exposure to economics in high school does not predict major declaration at Davis, it does at UCSB. This suggests that the restriction induces selection on prior general preparation and on prior exposure to economics.

The final model in Table 4.6 examines selection (conditional on prior opportunity and preparation) along a different margin: enrollment in a student's first economics course. The UCSB outcomes differ significantly from those at Davis in three respects. First, female students are less likely to take ECON 1 at UCSB, in line with the student-level event study estimates from Section 4.4 and again suggesting that the major restriction mutes preferences. Second, students with *lower* SAT scores and high-school GPAs are more likely take ECON 1 at Davis, while those who attended private school are not. In contrast, high SATs and high school GPAs are not associated with taking ECON 1 at UCSB, and private high-school attendance is. Each of these results are consistent with the major restriction inducing significant positive self-selection into the first course in the major based on prior preparation, perhaps because students who feel they are

[24]The coefficient on missing income has been adjusted to reflect the difference in outcome propensity between missing-income students and a student with average log family income.

[25]Major declaration propensity among plausibly-interested students is significantly lower at UCSB (26.4%) than it is at Davis (32.2%). This difference is similar in magnitude to the effects of major restrictions on major size reported in Section 4.3.

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

[27]The major restriction may also make the economics major more appealing to highly-prepared students for other reasons, e.g. by shrinking class sizes (and increasing peer academic aptitude) or improving the major's signal quality.

less likely to qualify for the major do not attempt it. Finally, students who have taken AP Micro and are therefore eligible to opt out of ECON 1 tend to do so at Davis, but not at UCSB, where the major restriction only considers ECON 1 grades from courses taken at UCSB.

The results presented in Table 4.6 reveal more positive selection and self-selection into the economics majors at UCSB than at Davis, that selection is on prior academic preparation and exposure to economics in high school, and that this selection results in stratification on race and income. Our preferred interpretation is that the greater observed positive selection at UCSB arises from that campus's major restriction. The following subsection investigates alternative interpretations of the presented statistics.

4.5.1 Robustness

One alternative explanation for the patterns described above is that quantitative aptitude covaries with prior preparation to a greater degree among UCSB students. If this were the case, and students' course and major choices responded to it, this could explain the higher degree of selection on prior preparation and economics experience at UCSB. However, the first two models presented in Table 4.7 – which model ECON 1 students' performance in the first two calculus courses – show that this is not the case for quantitative skills. The baseline (Davis) coefficients confirm significant variation in math-preparation with observables, including prior preparation: higher SAT scores, high school GPAs and family incomes predict better mathematical performance, as do being Asian and female, while URM students had worse math grades. However, there is almost no evidence of a stronger relationship between student characteristics and math performance at UCSB than at Davis in either of the first two calculus courses.

Another alternative explanation for the observed patterns is that UCSB might provide lower grades to less-prepared students in its introductory courses, discouraging those students using 'soft' restrictions rather than relying on its mechanical restriction policy. The next two columns in Table 4.7 show that in fact, the opposite is the case: higher SAT scores are more *weakly* associated with ECON 1 grade gains at UCSB than at Davis, and the URM grade penalty is smaller at UCSB than at Davis. This implies that UCSB provides somewhat more-lenient grades in its introductory courses, but its major restriction nevertheless deters disadvantaged and lower-preparation students from earning the major.

The final three columns of Table 4.7 illuminate how UCSB's major restriction – which selects on socioeconomic status, prior academic opportunity, and measured academic preparation – generates larger racial and income gaps in major declaration. While racial grade gaps are less pronounced at UCSB than at Davis, the restriction makes every grade gap more consequential at UCSB. UCSB students with higher high school GPAs and SAT scores obtain much higher grades in ECON 1, 2 and 10A, and those who had access to IB or AP economics perform much better in ECON 1 and 2. URM students also obtain lower grades in these threshold courses than their equally prepared counterparts, clarifying why prior preparation does not fully explain URM students' lower likelihood of economics major declaration.

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

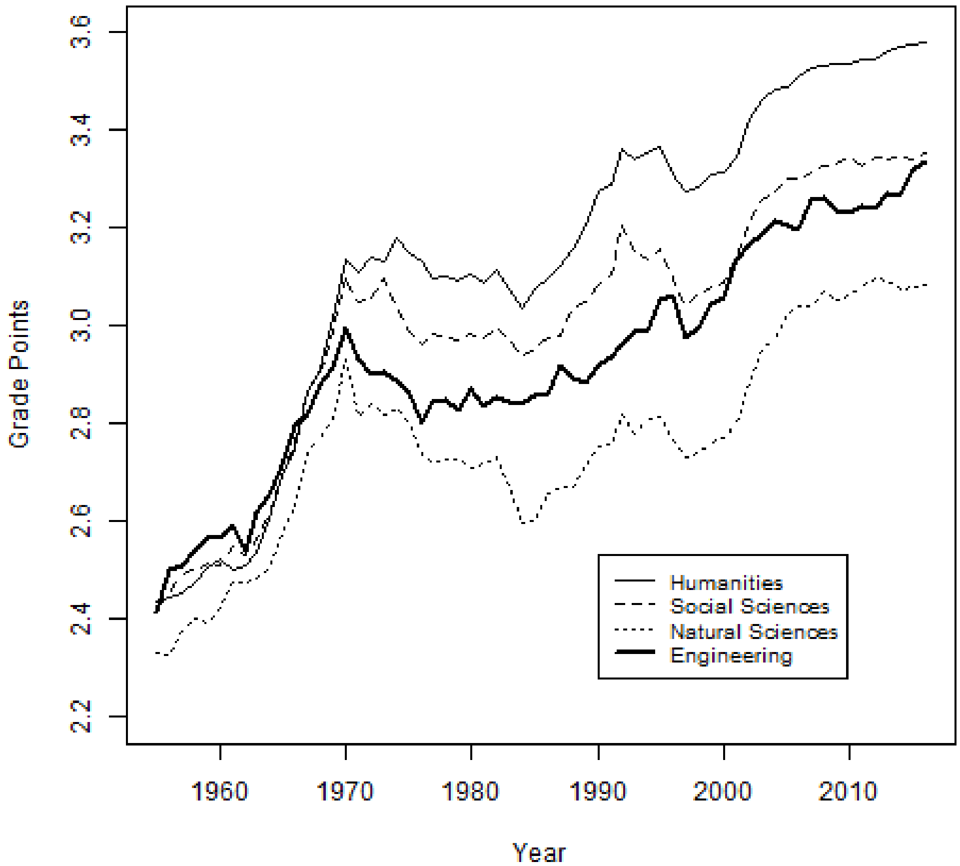
4.6 Conclusion

UC Berkeley, UC Davis, UC Santa Barbara, and UC Santa Cruz have imposed 45 significant policies restricting students' major choice in the past 50 years, in line with similar behavior at selective public universities across the country. These restrictions, most of which require students to earn high grades in specific introductory courses before being permitted to declare a major, tend to decrease the number of students in the major by 10-20 percent, with URM students about twice as likely to exit the major than non-URM students. Despite only targeting relevant coursework, the restrictions push out students with *absolutely* poorer early university performance, not students who perform poorly in the targeted courses, and tend to discourage female students from attempting the restricted major in the first place. As a result, major restrictions have the net effect of leading URM (and female) students to enroll in less-lucrative majors, even when implemented by relatively low-return majors.

Major restrictions' systematic stratification of students by pre-enrollment characteristics can be explained by the close correlation between introductory course performance and prior student opportunity and preparation. Underrepresented minority students, lower-income students, and students whose high schools did not offer related advanced courses earn substantially lower grades in introductory courses and become less likely to persist in restricted majors.

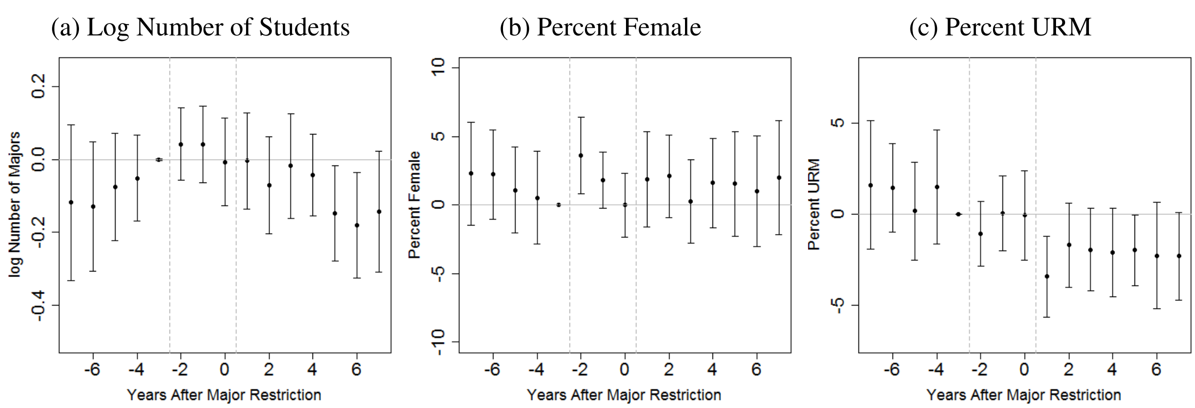
Like most public universities, each UC campus has explicit undergraduate admissions policies in place targeting disadvantaged applicants and encouraging their enrollment. Mechanical major restrictions systematically restrict those applicants' access to many fields of study – including many of the campuses' most-lucrative majors and many STEM fields. In the same way that meritocratic admissions policies limit selective universities' access to applicants with poorer academic qualifications, the stratification generated by major restriction policies exacerbates equity gaps between high- and low-SES families, with negative implications for economic mobility.

Figure 4.1: Average UC Berkeley Grades by Discipline over Time



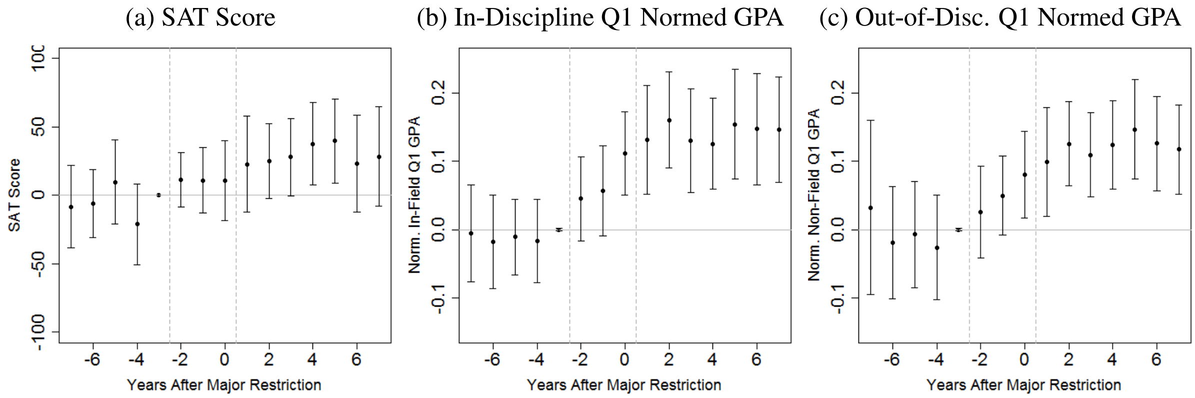
Note: Average grade points earned by undergraduate students in Humanities, Social Science, Natural Science, and Engineering courses at UC Berkeley annually from 1955 to 2016. Departments categorized by the authors. Source: UC ClioMetric History Project Student Database.

Figure 4.2: Department-Level Event Study: Student Demographics



Note: Event study β estimates of demographic characteristics of students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major estimate (e.g. as double-majors). Source: UC ClioMetric History Project Student Database.

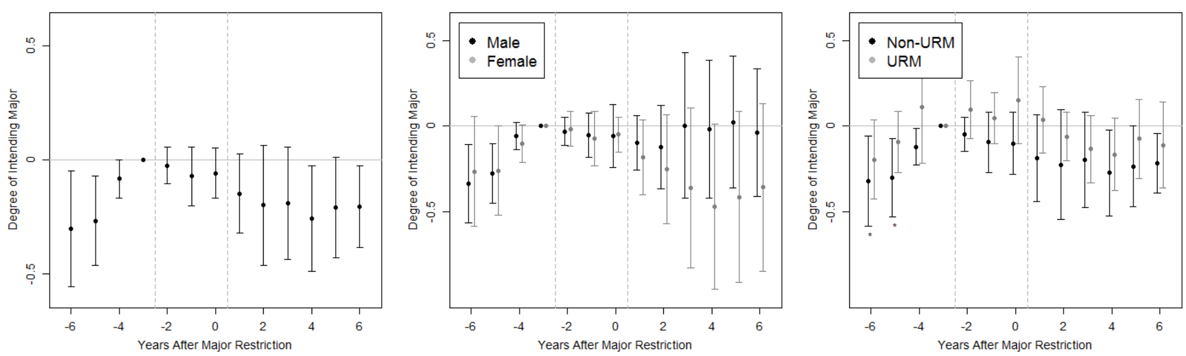
Figure 4.3: Department-Level Event Study: Student Academic Characteristics



Note: Event study β estimates of the measured aptitude of students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. β_{-3} is omitted, and standard errors are clustered by campus-major. Students can be included in more than one major estimate (e.g. as double-majors). Normed GPA is defined above in Equation 4.1; out-of-discipline courses include those taken outside the major's discipline (Humanities, Social Sciences, Natural Sciences, Engineering, and Professional) and excluding Mathematics and Statistics courses, while in-discipline courses include those in the major's discipline. Source: UC ClioMetric History Project Student Database and UC Corporate Student System.

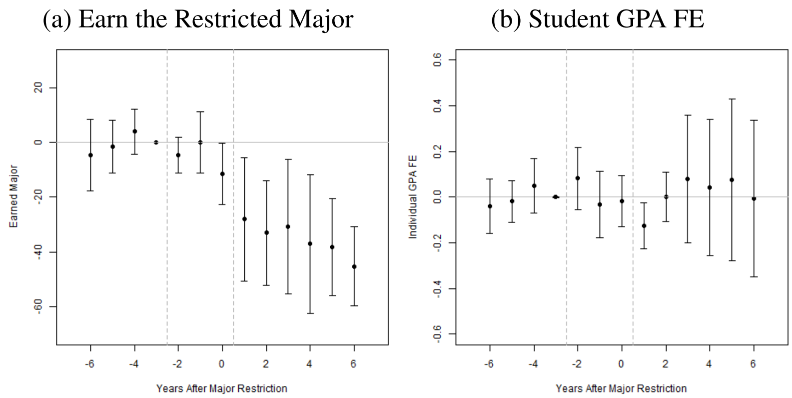
Figure 4.4: Estimated Changes in Students' Intentions for Restricted Majors

(a) Overall (b) By Gender (c) By Ethnicity



Note: Event study β_{it} estimates – overall and by gender and URM ethnicity – of the average degree to which students exhibit their intention to earn newly-restricted majors ($\hat{M}_{im}$) before and after the implementation of the restriction, following Equation 4.4 and estimated over a stacked dataset of students i's major intentions in field m. $\beta_{i,-3}$ is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include m fixed effects. Asterisks reflect p-values from hypothesis tests of equality in each period by gender or ethnicity: $*$ fifteen percent, $**$ five percent, and $***$ one percent. Source: UC ClioMetric History Project Student Database.

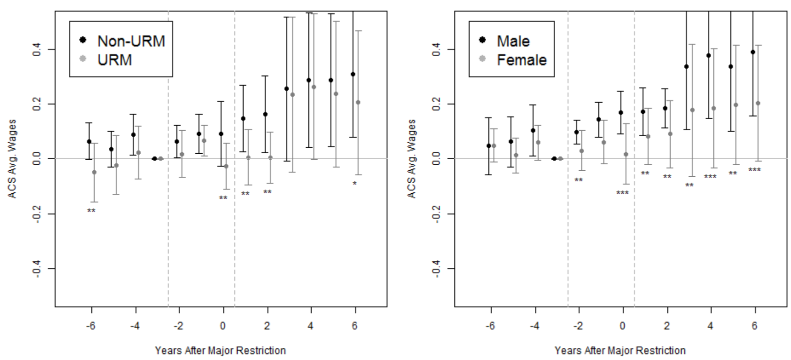
Figure 4.5: Estimated Changes in Major Choice and Composition of Students Who Intend Restricted Majors



Note: Difference-in-difference event study β_{it} estimates of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and their major choice or student characteristic before and after the implementation of the restriction, following Equation 4.5 and estimated over a stacked dataset of students i's major intentions in field m. Outcomes are defined as whether the student earns the restricted major and the student's GPA FE, their individual fixed effect from a two-way fixed effect model of GPA on student and course effects. β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database.

Figure 4.6: Estimated Changes in the WBM of Students who Intend Restricted Majors

(a) By Ethnicity (b) By Gender



Note: Event study β_{it} estimates of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and their major choice or student characteristic before and after the implementation of the restriction, following Equation 4.5 and estimated over a stacked dataset of students i's major intentions in field m. β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Asterisks reflect p-values from hypothesis tests of equality in each period by gender or ethnicity: $*$ fifteen percent, $**$ five percent, and $* * *$ one percent. Source: UC ClioMetric History Project Student Database and the American Community Survey.

Table 4.1: Binding Major Restrictions at the Top 25 US&WR Ranked Public Universities, Fall 2019

Univ.	Undergrad. Students	Computer Science	Economics	Finance	Mechanical Engineering	Nursing
Cornell	14,907	2.5	2.7	3.3; A	2.5; A	*
UCLA	31,002	3.5; A	2.5	3.3	3.5; A	HS
UC Berkeley	30,853	3.3	3.0	A	3.0; A	*
Virginia	16,655	-	-	A	2.5	A
Michigan	29,821	-	-	A	A	A
UC Santa Barbara	22,186	3.2	2.85	2.85	A	*
UNC – Chapel Hill	18,862	-	-	3.0; A	*	A
UC Irvine	29,307	3.0	2.5	3.0; A	3	A
Georgia Tech	15,573	-	-	-	-	*
Florida	35,247	-	3.0	3.0	2.8	3.3
William and Mary	6,285	-	-	2.5; A	*	*
UC Davis	30,145	3	-	*	2.8	*
UC San Diego	28,587	3.3; A	2.5	*	A	*
Georgia	28,848	-	A	A	A	*
UI – Urbana-Champaign	33,955	3.75; A	-	A	3.75; A	*
UT – Austin	40,492	A	-	3.25; A	3.0; A	3.0; A
UW – Madison	32,196	-	-	2.75; A	A	2.75; A
Ohio State	45,946	3.2	-	3.0; A	3.4	A
Purdue	31,006	-	2.75	-	3.2; A	2.75
Rutgers	35,641	-	-	A	A	HS
Penn. State – Univ. Park	40,835	HS	-	3.2	HS	HS
Washington	31,331	A	A	2.5; A	A	2.8; A
Connecticut	19,241	3.0; A	-	A	3.0; A	3.0; A
UMD – College Park	29,868	-	-	A	2.7	3.0; A
Clemson	19,402	-	-	-	HS	A
Texas A&M	53,065	2.75; A	3.0	3.5; A	3.5; A	A

Note: The Fall 2019 minimum major admissions requirements for enrolled students at the top 25 public universities as ranked by US News and World Report in 2019, in addition to Cornell University (which is part-public). A number indicates the minimum GPA required in department-specified courses for current students to declare the major, omitting restrictions of C+ or lower. Chosen majors are the top-earning majors reported in Altonji, Blom and Meghir (2012) averaged between male and female students, Table 3, omitting Electrical Engineering due to its similarity with Computer Science. Finance includes Business Administration, Business Economics, and Economics and Accounting majors when otherwise unavailable.

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

Source: University and department websites and US News & World Report, August 2019

Table 4.2: Observational Relationship between Major Restrictions and URM Stratification

	URM Share in Major	
Any Restriction	-2.8	
	(1.3)	
Mechanical Restriction		-3.1
		(1.1)
Discretionary Restriction		1.0
		(1.6)
Institution FE	X	X
Field of Study FE	X	X
$\bar{Y}$	11.1	
N	98	

Note: Estimates from an OLS linear regression of a major's 2019 URM (Black or Hispanic) graduate share on whether the major is restricted, over the 26 institutions and five majors presented in Table 4.1. Mechanical restrictions limit access to students with below-threshold introductory grades; discretionary restrictions limit access to students on the basis of detailed applications, generally including both measured academic preparation along with essays and other materials. Each model includes institution and major fixed effects. Standard errors clustered by institution in parentheses. Source: The Integrated Postsecondary Education Data System.

Table 4.3: Fifty Years of Major Restrictions at Four Universities

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

Note: Eligible major restrictions include GPA requirements for specified courses exceeding a C+ (2.3) or an internal competitive application. Does not include majors that are open to admits to a specific college but closed to admits to different colleges, like most Engineering majors; in any case, those policies have little changed in this period. † indicates that the major has had restrictions since within two years of its creation; * indicates that the restriction only lasted (or has only lasted) for a small number of years, either of which lead the major to be omitted from analysis below; and ° indicates that the major was implemented prior to the beginning of our data. The reported years are one year prior to the first or last year in which the restriction is mentioned in the campus's course catalog. **A**: Students must submit a successful internal application after initial enrollment in order to earn the major. ‡ UCSB Biology implements a complex and highly-stratified major restriction that requires several course-catalog pages to explain (with dozens of alternative paths leading to different major specialties), though ultimately never requires GPA performance over 2.0 in any course. Source: University Course Catalogs.

Table 4.4: Descriptive Statistics of UC Campus Majors

	All	Berkeley	Davis	Santa Barbara	Santa Cruz	3 Years Prior to Major Restrict.
Number of Majors	216 [73]	77 [5]	91 [13]	54 [3]	40 [5]	
# Students	86 [115]	92 [111]	60 [91]	107 [138]	113 [132]	203 [196]
% Female	53 [22]	52 [21]	55 [23]	54 [23]	53 [22]	51 [21]
% URM	20 [17]	18 [17]	20 [17]	23 [20]	22 [15]	14 [7]
Sample Size, Overall						
Events	29	7	10	7	5	
Major-Years[1]	6,263	2,222	1,855	1,113	1,073	
Sample Size, Observe Demographics						
Events	25	7	7	6	5	
Major-Years[1]	5,648	2,222	1,455	1,039	932	

Note: Descriptive statistics of the average number of departments at each covered university, average number of students per department, and average percent of female and URM students across departments, for all departments and for departments three years prior to instituting major restrictions. Standard deviations in brackets. Events indicate number of new observable major restrictions (see Table 4.3) and major-year observations, in the full sample and in the sample where student demographic characteristics (like ethnicity) are observed. [1] Only includes major-years with at least 20 observations; smaller departments are omitted from analysis. Source: UC ClioMetric History Project Student Database.

Table 4.5: Summary of Department Difference-in-Difference Estimates around Major Restriction Implementation

	Log Num. of Students	Percent Female	Percent URM	SAT Score	GPA FE	Q1 GPA[1] In Disc.	Q1 GPA[1] Out of Disc.
4-7 Yrs. Before Restriction	-0.09	1.46	1.17	-6.89	-0.00	-0.01	-0.01
	(0.07)	(1.36)	(1.01)	(12.10)	(0.03)	(0.02)	(0.03)
Transition Years	0.02	1.72	-0.33	10.91	0.08	0.07	0.05
	(0.05)	(0.96)	(0.94)	(11.38)	(0.02)	(0.03)	(0.03)
1-5 Yrs. After Restriction	-0.08	1.43	-2.20	29.44	0.12	0.14	0.12
	(0.06)	(1.54)	(1.01)	(14.20)	(0.03)	(0.04)	(0.03)
Campus-Major FE	X	X	X	X	X	X	X
Campus-Year FE	X	X	X	X	X	X	X
Observations	6379	5749	5749	4224	6199	4803	5775
R^2	0.88	0.90	0.84	0.90	0.98	0.71	0.59
Δ (Post-Pre)[2]	0.02	-0.02	-3.37	36.33	0.13	0.15	0.13
	(0.08)	(1.37)	(0.87)	(13.77)	(0.03)	(0.03)	(0.03)

Note: Event study β estimates of the measured characteristics of students who declare restricted majors before and after the implementation of the restriction, relative to other majors in that campus-year. Standard errors clustered by campus-major in parentheses. Outcomes are averages by declared major and cohort-year, defined by students' first year of enrollment. "Before" indicates 3-7 years prior to initial restriction implementation; "Transition" includes the year of implementation and two years earlier; and "After" includes 1-5 years following implementation. β_{-3} is omitted. Students can be included in more than one major estimate (e.g. as double-majors). [1]First-quarter normed GPA is defined above in Equation 4.1; "Outside Area" normed GPA is calculated only using first-quarter courses taken outside the major's division (Humanities, Social Sciences, Natural Sciences, Engineering, and Professional) and excluding Mathematics and Statistics courses. [2] The difference between "After" and "Before" Major Restriction β coefficients, with standard error in parentheses.

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

Table 4.6: 2010-2016 Economics Major Enrollment Propensities at UC Davis and UCSB

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

Note: Propensity-score-weighted WLS regression models among 2010-2016 freshman-applicant Santa Barbara and Davis students of economics major declaration and ECON 1 enrollment on student characteristics. Major declaration models conditional on having earned a grade in ECON 1. Main effects estimated for Davis and Santa Barbara; 'Diff' is estimated as the difference between Santa Barbara and Davis. Standard errors clustered by high school in parentheses. Inverse propensity score weights estimated using the full set of listed covariates as well as California county indicators. Family income is missing for the $\sim$ 13 percent of students who did not report family income on their application; estimates relative to the mean observed log income. [1]High school course offerings are only observed for public CA high schools. [2]Normalized to mean 0, s.d. 1.

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

Table 4.7: Robustness Table: Other Aspects of Economics Major Qualification at Davis and Santa Barbara

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

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

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

Chapter 5

Will Studying Economics Make You Rich? A Regression Discontinuity Analysis of the Returns to College Major

5.1 Introduction

Forty-year-old U.S. workers with undergraduate degrees in economics earned median wages of \$90,000 in 2018. By comparison, those who had majored in other social sciences earned median wages of \$65,000, and college graduates with any major other than economics earned \$66,000. Relative to workers with lower-wage majors, the observational premiums earned by workers with high-wage majors like engineering, nursing, and economics are similar in size to the wage gap between college graduates and non-graduates (Altonji, Blom and Meghir, 2012). These gaps have motivated a large literature examining the determinants of students' major choices (Zafar, 2013; Stange, 2015; Arcidiacono, Aucejo and Hotz, 2016; Wiswall and Zafar, 2018; Patnaik et al., 2020). However, average wage differences between majors do not necessarily reflect the causal effect of choosing one major over another. This study directly analyzes the treatment effects of earning an undergraduate degree in the popular high-earning field of economics.[1]

Estimating the causal effects of earning specific college majors is challenged by students' non-random assortment across majors: most students self-select their college major, and many universities and departments use admissions and grade requirements to restrict entry into certain majors. As a result, observational wage differences across majors may reflect selection bias. We overcome this challenge by using a regression discontinuity design that exploits a fuzzy discontinuity in economics major access at a large moderately-selective public university (Angrist and Lavy, 1999).[2] We implement this design to estimate the effect of studying economics on students' early-career earnings and industries, as well as how the major's effect on earnings is mediated by changes in students' other educational outcomes, career preferences, and early-career industries. We then characterize and estimate the biases that arise when using

[1] Economics is a particularly popular major at highly-selective universities. The 2020 federal College Scorecard shows that economics was the most-earned major at 11 of the top 20 highest-ranked American universities (as ranked by U.S. News & World Report), and was among the top five majors at 34 of the 50 highest-ranked universities.

[2] This design was recommended (but not implemented) by both Altonji, Blom and Meghir (2012) and Altonji, Arcidiacono and Maurel (2016).

observational average wage difference between economics and other majors as a proxy for the treatment effect of majoring in economics.

The specific case we analyze is the Department of Economics at the University of California, Santa Cruz. UCSC Economics imposed a GPA restriction policy in 2008: students with a grade point average below 2.8 in Economics 1 and 2 were generally prevented from declaring an economics major.[3] Students who just met the GPA threshold were 36 percentage points more likely to declare the economics major than those who just failed to meet it. Most of these students would have otherwise earned degrees in other social sciences. Students just above the threshold who majored in economics were surprisingly representative of *all* UCSC economics majors on observables; for example, their average SAT scores was at the 41^{th} percentile of economics majors.

Comparing the major choices and average wages of above- and below-threshold students shows that majoring in economics caused a $22,000 (46 percent) increase in the annual early-career wages of barely above-threshold students. It did so without otherwise impacting their educational investment – as measured by course-adjusted average grades and weekly hours spent studying – or outcomes like degree attainment and graduate school enrollment. The effect is nearly identical for male and female students, may be larger for underrepresented minority students, and appears to grow as workers age (between ages 23 and 28). About half of the wage effect can be explained by the effect of majoring in economics on students' industry of employment: relative to students who did not qualify for the major, economics majors became more interested in business and finance careers and were more likely to find employment in higher-wage economics-related industries like finance, insurance, and real estate (FIRE) and accounting. Most of the barely above-threshold economics majors would have otherwise earned degrees in lower-earning fields like psychology and sociology, and differences in either OLS-estimated average wages by major (with or without controls) or median wages by major (estimated at the university, state, or national level) slightly *underestimate* the estimated local average treatment effect. This suggests that the net magnitude of selection bias and treatment effect heterogeneity is small in this context.[4]

Our data include comprehensive 2000-2014 UCSC student and course records linked to biannual administrative student surveys, National Student Clearinghouse educational outcomes, and annual California UI employment records. These highly-detailed records allow us to test several alternative explanations for above-threshold students' higher postgraduate earnings. We show that detailed student characteristics are smooth across the GPA threshold and that grade distributions in economics courses remained unchanged in the period. There is no evidence of students bunching above the threshold, as might be expected if threshold-crossing was somehow manipulated. We also show that wages were smooth across the grade threshold prior to the policy's implementation but slightly discontinuous during an interstitial period with a less-binding major restriction policy, generating similar (but noisier) instrumental variable estimates to the main specification. While our main empirical strategy estimates linear regression

[3]Like many universities, UCSC has multiple "tracks" for its economics major. Students just above the GPA threshold mostly chose its "Business Management Economics" track, in which about one-third of required courses are taken in business- and finance-related subdisciplines.

[4]Our results mirror the well-known finding that causal estimates of the return to schooling slightly exceed the mean differences recovered from OLS (Angrist and Keueger, 1991; Card, 1999), with our study focusing on heterogeneity in the return to schooling.

discontinuity models with standard errors clustered by GPA (Lee and Card, 2008), we confirm the estimates using a number of other specifications, including "Honest RD" estimates following Kolesar and Rothe (2018).[5]

This is one of the first studies to employ a quasi-experimental research design to identify labor market returns to college major choice in the U.S.[6] A small number of previous studies have analyzed major-specific returns in other countries by exploiting centralized field-specific enrollment assignment rules (Kirkeboen, Leuven and Mogstad, 2016; Hastings, Nielsen and Zimmerman, 2018; Daly and Le Maire, 2019). However, the external validity of those estimates in the U.S. may be limited: American universities offer a broader core liberal arts curriculum, permit students to choose their majors years after their initial enrollment, and provide students with more discretion over their courses, all of which could narrow field-specific returns. A large literature has employed selection-on-observables methods and structural estimation to identify major-specific returns (James et al., 1989; Rumberger and Thomas, 1993; Black, Sanders and Taylor, 2003; Arcidiacono, 2004; Hamermesh and Donald, 2008), generally arguing that selection bias explains a substantial portion of U.S. wage variation across majors.

This study's reduced-form regression discontinuity design provides unusually transparent evidence of postsecondary education's heterogeneous and persistent role in shaping students' labor market outcomes. Our estimated early-career wage return to economics rivals the baseline return to a college degree, implying that major choice is a first-order heterogeneity component in the return to higher education.[7] A related literature has used quasi-experimental research designs to highlight university selectivity as another important dimension of heterogeneous university treatment effects (Hoekstra, 2009; Zimmerman, 2014; Cohodes and Goodman, 2014; Bleemer, 2018*a*, 2020*a*). However, even students who are quasi-randomly switched to enrolling at universities with 25 percentage points higher graduation rates – a large increase in selectivity – receive an early-career wage return 30 percent lower than the return to majoring in economics at UCSC (Bleemer, 2018*a*).[8] These findings imply that widespread but understudied university policies that shape student major choice – like GPA restrictions, variable tuition, and grade inflation – have important long-run efficiency and social mobility ramifications.[9]

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[5]Because of the small number (20) of discrete GPAs available to students, these latter estimates are likely conservative.

[6]The only known quasi-experimental study to previously identify heterogeneous returns by college major in the U.S. is Andrews, Imberman and Lovenheim (2017), who analyze the return to majoring in business by exploiting a GPA threshold policy at several University of Texas campuses. Their suggestive finding of a large wage return to business majors closely parallels our own estimates with regard to economics.

[7]One reason for the economics major's large return is the relatively-low return to economics majors' second-choice social science fields, highlighting the importance of counterfactual student choices in measuring educational returns (Kirkeboen, Leuven and Mogstad, 2016).

[8]As in nearly all previous studies on the return to education and university selectivity, we are unable to distinguish whether the observed returns result from changes in human capital or signaling. We discuss this further in Section 5. Other recent papers on heterogeneous university returns by university quality include Sekhri (2020) and Canaan and Mouganie (2018).

[9]The close correspondence between observational and causal estimates of major-specific returns also suggests the potential for private pecuniary gains resulting from providing students with locally-relevant information about average wages by majors, which has been shown to increase students' enrollment in high-wage majors (Berger, 1988; Beffy, Fougère and Maurel, 2012; Hastings, Neilson and Zimmerman, 2015; Wiswall and Zafar, 2015). See Bleemer and Mehta (2020*a*) on GPA restrictions, Andrews and Stange (2019) on variable tuition, and Ahn et al. (2019) on grade inflation. Policies encouraging economics major choice (e.g. Porter and Serra (2020)) are particularly likely to provide

This is also the first study to present quasi-experimental evidence that major choice causally affects students' career preferences or industry of employment, though prior studies have documented that students select majors partly on the basis of career preferences (Wiswall and Zafar, 2018). The correlation between college graduates' majors and their occupations and industries of employment is notably weak: fewer than 60 percent of most majors' students work in the top *ten* highest-employment (five-digit) occupations for that major (Altonji, Blom and Meghir, 2012).[10] Nevertheless, majoring in economics causes students to report a stronger preference for business and finance careers prior to labor market entry – likely in part as a result of perceived job availability – and to be more likely to ultimately work in related industries like FIRE and accounting. These changed industry preferences could reflect the fact that knowledge and skills acquired in the economics major may be particularly useful in these industries, providing students with industry-specific human capital (Altonji, Kahn and Speer, 2014; Kinsler and Pavan, 2015).

5.2 Background

The University of California, Santa Cruz is a moderately-selective public research university in northern California. In 2010 UCSC admitted 64 percent of freshman applicants, resulting in a 3,290-student class largely split between white (38%), Asian (27%), and Hispanic (24%) students. Nearly all (98%) of its students were California residents. In many ways, UCSC is relatively representative of the average U.S. university; among four-year U.S. universities in the 2010 IPEDS database (weighted by enrollment), UCSC is at the 42nd percentile in admissions rate, the 59th percentile in average student SAT scores, the 42nd percentile in middle-income students' average net price of attendance, and the 53rd percentile in student-to-faculty ratio.[11] The UCSC Department of Economics had 25 ladder-rank faculty and 7 lecturers in 2010 and taught 8,800 student enrollments that academic year, implying that each faculty-member taught an average of 91 students per quarter, among the highest loads at the university.[12]

The UCSC Department of Economics's 2003 GPA restriction was the university's first policy limiting enrolled students' access to a particular college major (Bleemer and Mehta, 2020*a*). The restriction was first recorded in UCSC's 2003 Course Catalog, which stated that students with a GPA in Economics 1 and 2 ($EGPA$) below 2.8 would only be allowed to declare the major "at the discretion of the department". If students re-took one of the courses, only the initial grade was used to calculate $EGPA$. This policy hardly changed *de jure* over the following ten years, though the 2012 course catalog is the first to note that for students with below-2.8 $EGPA$s, "appeals are rarely granted". Starting in 2013, calculus grades were added to the $EGPA$ calculation.

However, the Department's "discretion" left substantial room for year-over-year *de facto*

students with substantial pecuniary returns.

[10]A substantial academic literature studies how university policies shift students toward science and engineering majors (Sjoquist and Winters, 2015*b*; Denning and Turley, 2017; Castleman, Long and Mabel, 2018), though none directly investigate whether this actually bolsters the STEM labor force.

[11]Calculations from the Integrated Postsecondary Education Data System. Average SAT calculated as the summed averages of the 25th and 75th percentiles of each SAT test component. Average net price defined over federal financial aid recipients with family incomes between $48,000 and $75,000.

[12]Altonji and Zimmerman (2019) show that economics and business degrees have below-average educational costs.

differences in below-2.8 students' access to the major.[13] The difference in the probability of majoring in economics above and below the $EGPA$ threshold remained small (below 15 percentage points) until the 2008 entering cohort, and then ranged from 25 to 60 percentage points until 2012.[14] As a result, this study focuses on these latter five cohorts of freshman UCSC students.

5.3 Data

The student database analyzed in this study (UC-CHP, 2020) was collected from the UCSC Office of the Registrar as part of the UC ClioMetric History Project (Bleemer, 2018*b*). The sample covers all freshman-admit students who first enrolled at UCSC between 1999 and 2014.[15] For each student, we observe gender, ethnicity, cohort year, (pre-enrollment) home address, California residency status, high school, and SAT score as well as UCSC course enrollments and grades.[16] The $EGPA$ running variable is calculated by averaging students' grade point averages in Economics 1 and 2, using their earliest letter grades if they retook either course.

These student records are linked by name and birth date to the National Student Clearinghouse StudentTracker database (NSC, 2019), which contains undergraduate and graduate enrollment and degree attainment records for nearly all American colleges and universities, and by social security number to UI employment records from the CA Employment Development Department (EDD, 2019), which include annual wages and six-digit NAICS industry code.[17] We proxy family income by the mean adjusted gross income in the student's home ZIP Code in their first year of enrollment (IRS, 2018).[18]

UCSC students are also linked to survey responses from the biannual UC Undergraduate Experience Survey (UCUES), conducted online in the spring of even-numbered years (SERU, 2019). The 2nd/3rd and 3rd/4th year response rates among the 2008-2012 students in the main sample were 29 and 28 percent, with the response rates and respondent characteristics smooth across the GPA threshold.[19] Among the survey's many questions are responses about number of hours per week spent studying and students' intended careers.[20]

Non-economics majors are categorized into four disciplines: humanities, social sciences, natural sciences, and engineering. Combining the three tracks of the economics major —

[13]Figure D.1 shows 2000-2014 UCSC students' likelihood of majoring in economics by $EGPA$ for each cohort.

[14]This change was likely driven by increased demand after the 2007-2008 financial crisis; see Figure D.2.

[15]Community college transfer students are omitted from our analysis because they followed a different admission rule into the economics major.

[16]ACT test scores (submitted by 4% of applicants instead of SAT scores) and SAT scores on a 1600 point basis are converted to 2400-point SAT scores using standard concordance tables.

[17]NSC match quality is near-complete but missing for some students who opt out of coverage. For example, 97 percent of UCSC undergraduate degrees awarded to the 2008-2012 cohorts appear in NSC (see Appendix 1 of Bleemer (2018*a*)). EDD NAICS code reflects the industry of employment from the year's latest non-missing quarter (Census, 2019). UI employment records exclude out-of-state, federal, and self-employment. All EDD-related analysis was originally conducted for the purpose of institutional research (see Bleemer and Mehta (2020*b*)).

[18]Income statistics are from the IRS Statistics of Income (SOI). Wage and income statistics are winsorized at the top and bottom 2% and CPI inflation-adjusted to 2019 (BLS, 2019).

[19]See Figure D.3. UCUES data were provided by the Survey Experience in the Research University (SERU) Consortium at UC Berkeley's Center for Studies in Higher Education and linked by student ID.

[20]Full questions and responses are provided in Appendix A.

economics, business management economics, and global economics — it was the second-most-popular major at UCSC for the 2008-2012 cohorts (11.7 percent of students), below psychology (12.9 percent) but ahead of environmental studies (6.1 percent) and sociology (6.0 percent).

Table 1 presents descriptive statistics for 2008-2012 UCSC freshman-admit students. Relative to the full sample of 15,400 UCSC students, the 3,053 students who complete Economics 1 and 2 are more likely to be male and Asian and come from slightly higher-income neighborhoods. Of those students, the 55 percent who actually declare the Economics major are 41 percent female (compared to 56 percent across UCSC), 44 percent Asian (compared to 27 percent), and have similar average SAT scores to the average UCSC student (1716 out of 2400).

5.4 Empirical Design

We identify the relationship between economics major choice (the treatment) and resulting outcomes (Y) by exploiting a discrete fuzzy grade discontinuity in economics major access (Hahn, Todd and van der Klaauw, 2001). Figure 5.1 shows the first stage estimate of the impact of meeting the 2.8 GPA threshold on economics major choice for the 2008-2012 cohorts. Above-threshold students were about 36 percentage points more likely to declare the economics major. Some below-threshold students were nevertheless able to declare the major — "at the discretion of the department" — and about 20 percent of above-threshold students chose not to declare the major. Each bubble is scaled by the proportion of students who earned that $EGPA$; because the $EGPA$ is calculated over only two letter grades, students could earn one of only 14 common or 6 uncommon $EGPA$s.

Let $Y_i(1)$ denote the outcome that UCSC student i would experience if they majored in economics, and $Y_i(0)$ denote the outcome they would experience if they did not. Outcomes of interest include (for example) post-graduation earnings, industry of employment, study time, and graduate school attendance. Let C be the group of policy compliers: the subset of students who major in economics if they are above the GPA threshold but not if they are below it. The effect of the major on policy compliers whose $EGPA$ was near the threshold (the local average treatment effect) is given as:

$$LATE_{RD}(Y) \equiv \lim_{EGPA \downarrow 2.8} E[Y_i(1)|EGPA, i \in C] - \lim_{EGPA \uparrow 2.8} E[Y_i(0)|EGPA, i \in C] \quad (5.1)$$

so long as $E[Y_i(1)|EGPA, i \in C]$ and $E[Y_i(0)|EGPA, i \in C]$ are smooth at $EGPA = 2.8$.

We test several implications of this smoothness assumption. First, we find that the empirical grade distribution does not spike at or near the 2.8 $EGPA$ threshold, and the 2008-2012 distribution is highly similar to the 2003-2007 grade distribution, years when the $EGPA$ threshold was loosely enforced.[21] This pattern implies that students did not manipulate their course grades to meet the GPA threshold. Second, we find that detailed student socioeconomic characteristics are smooth across the GPA threshold, as is a one-dimensional summary of student characteristics generated by flexibly predicting each student's 2017-2018 average wages by

[21] See Figure D.4. Both distributions share the same shape as the 2000-2002 grade distribution (prior to the $EGPA$ restriction's implementation), though average $EGPA$s trended downward over time. Students' Economics 2 grades are smooth across the threshold.

socioeconomic observables. This indicates that effects estimated across the threshold are unlikely to be driven by anything other than qualification for the major. [22] Finally, as a placebo test, we find that economics major selection and early-career wages are smooth across the 2.8 $EGPA$ threshold in 2000-2002, before the GPA restriction was introduced.[23]

Our baseline specification for estimating Equation 5.1 is linear in the running variable ($EGPA$) on either side of the threshold and clusters standard errors by the 20 observed $EGPA$s above 1.8 (Lee and Card, 2008). We also check that our results are robust to using a number of alternative specifications. These include (1) allowing quadratic running variable terms, (2) adding demographic controls and high school fixed effects, (3) narrowing the bandwidth to 0.5 $EGPA$ points on either side of the threshold, and (4) estimating "honest" local linear RD coefficients with optimal bandwidth and triangular kernel following Kolesar and Rothe (2018).[24] We note below the rare occasions in which any of the alternative specifications result in coefficients that differ substantially or statistically from those presented in the figures.[25]

The last columns of Table 1 present estimated characteristics of the students who majored in economics as a result of their barely above-threshold $EGPA$s (estimated following Abadie (2002)). These students' observable characteristics are surprisingly similar to those of the average UCSC economics student: 36 percent are female, 41 percent are Asian, and essentially all of them are California residents. Despite their low introductory course grades, there is no indication that they were much less prepared for success than other economics majors: their mean SAT score is at the 41^{th} percentile of all economics majors, while the mean income of their ZIP Codes of residence is at the 48^{th} percentile of their economics peers.[26] The representativeness on observables of our above-threshold policy compliers suggests that our estimated local average treatment effects may be similar to the average treatment effect of majoring in economics at UCSC.

5.5 Baseline Return to the Economics Major

Figure 5.2 shows that 2008-2012 UCSC students with above-threshold $EGPA$s had far higher early-career wages than their below-threshold peers.[27] Measuring average California wages in 2017 and 2018 – when students in the sample were 23 to 28 years old – above-threshold students

[22]See Figure D.5. Predicted wages are estimated by OLS on the 2017-2018 wages of 2008-2012 UCSC students who did not complete Economics 1 and 2. Predicted wages are imputed only for students with observed 2017-2018 wages to match our main labor market estimation sample.

[23]See Figure D.6. We also exploit the small increase in economics major choice across the less-binding 2003-2007 GPA threshold to noisily replicate the instrumental variable wage results in the main specification below (first-stage 6.2 percentage points (2.9 s.e.), IV $32,500 ($19,600)).

[24]The small number of running variable values suggests that these last estimates will be conservative. Tables D.1 to D.4 present regression coefficients from these alternative specifications for all main results.

[25]All OLS and IV regressions are estimated using the *felm* function in the *lfe* R package, version 2.8-5. Honest local linear regressions are estimated by the *RDHonest* R package, version 0.3.2.

[26]This absence of significant positive selection may result from the substantial noise in introductory course grades, which reflect a host of professor, TA, and extracurricular determinants (e.g. Sacerdote (2001); Fairlie, Hoffmann and Oreopoulos (2014)). A linear regression of $EGPA$ on high school fixed effects and gender-ethnicity indicators interacted with SAT score, mean ZIP Code GPA, and cohort provides an adjusted R^2 of only 0.15.

[27]Impacted students mostly graduated between 2012 and 2016, implying that their early-career earnings and industries were not shaped by a postgraduate recession (Altonji, Kahn and Speer, 2016).

earned about \$8,000 higher wages than below-threshold students, with a standard error of \$1,900.[28] Given that they were also 36 percentage points more likely to major in economics, the IV estimator suggests that students who just met the GPA threshold earned higher early-career wages by about \$22,000 if they declared the economics major, rising from \$37,000 to over \$59,000. Measuring wages in log dollars provides a similar 0.58 log dollar estimated treatment effect, though that estimate is statistically noisy in the Kolesar and Rothe (2018) specification.

The estimated returns to majoring in economics are nearly identical when estimated separately by student gender: \$21,700 (s.e. \$8,800) for men, \$22,600 (\$5,700) for women. The unexpectedly high observed earnings of students with $EGPA = 2.35$ visible in Figure 5.2 obtains only for male students, driving those estimates' higher standard errors. The return is also similar in magnitude among underrepresented minority (Black, Hispanic, and Native American) students: \$27,600 (\$13,500).[29]

These estimates do not appear to be solely driven by college graduates' first employment after graduation. Figure 5.3 presents estimates of the annual wage return to majoring in economics 4-9 years after graduating high school for three partitions of our baseline sample: the 2008-2009 cohorts, 2010 cohort, and 2011-2012 cohorts. It shows suggestive evidence that the wage return grows larger as workers age from 23 to 28, though the small number of cohorts challenges separate identification of age and cohort effects. Figure D.8 contextualizes this finding by using American Community Survey wage data (Ruggles et al., 2020) to visualize the median wages of U.S. economics majors annually from ages 22 to 62 along with the weighted median wages of U.S. college graduates who earned the second-choice majors that UCSC's policy-complying economics majors would have earned if economics had been unavailable (discussed further below). The relative observational return to economics increases with age in workers' 20s and 30s and remains large throughout workers' careers, resulting in a \$536,000 observational net present value of majoring in economics.[30]

5.6 Why do Economics Majors Earn Higher Salaries?

5.6.1 Educational Performance, Resources, and Attainment

Figure 5.4 shows how the characteristics of UCSC students' postsecondary education differed as a result of being provided access to the economics major. Panels (a) and (b) show that access to the economics major does not change students' likelihood of earning a college degree or enrolling in a graduate degree program (within seven years of matriculating).[31] Above-threshold students

[28] Students with earnings in only one of the two averaged years are assigned their observed year's wages; students with no observed wages in either year are dropped. Some RD specifications provide somewhat larger wage return estimates.

[29] See Figure D.7. California wages are observed for 80-90 percent of the sample, likely the result of nearly all UCSC freshman students being California residents. There is some evidence that students' likelihood of 2017-2018 California employment rises at the GPA threshold, though the estimates are not robust across different specifications; see Figure D.9.

[30] The observational wage return to economics shrinks (though remains large) after age 50, possibly reflecting informational obsolescence (Deming and Noray, 2020).

[31] Near-threshold students had a 96 percent Bachelor's attainment rate – including degrees earned at other institutions by 2018 – compared to 94 percent across the 2008-2012 UCSC freshman cohorts.

also have similar time-to-degree as below-threshold students. Economics major access does not provide students with smaller class sizes; if anything, average class sizes grow larger.[32] It does not lead students to earn higher or lower grades when adjusted for course difficulty (c), nor does it change the weekly amount of time students report studying outside of class.[33]

Instead, the primary estimable difference in students' postsecondary education is the content of that education. Barely above-threshold economics majors completed 13 more economics courses than non-majors, for a total of 17 economics courses on average. This caused the economics majors to take 9 fewer courses in other social sciences and about 4 fewer courses across other disciplines. About 7 of the additional economics courses were in traditional economics sub-disciplines, while almost 6 were in sub-disciplines related to business, finance, and accounting also offered by UCSC's economics department. Access to the economics major did not change the number of mathematics and statistics courses that students completed, but they did complete an average of two additional courses in quantitative methodology.[34]

If there was no signal value of economics degree attainment, then these estimates would imply a wage elasticity of economics course-taking of about 0.3.[35] However, this estimate is likely upwardly-biased by the potentially high signal value of economics degrees relative to students' second-choice majors. We are unable to distinguish between the degree's signal value and the value of additional human capital accumulation in this setting.[36]

5.6.2 Industrial Composition

Majoring in economics causally impacts the industries in which students are employed in their early careers. This could reflect either industry-specific human capital formation or changes in students' preferences across industries. Panel (a) of Figure 5.5 suggests that part of the effect arises from student preferences; survey responses from students' sophomore and junior spring quarters (prior to labor market entry) show that barely above-threshold economics majors became more than 50 percentage points more likely to report an interest in a business or finance career than non-majors, though this could in part reflect increased employment opportunity in those

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[32] For plots showing estimates for additional educational outcomes like time to degree and class size, see Figure D.10.

[33] Above-threshold students earn slightly lower unadjusted grade point averages than below-threshold students as a result of relatively lower grading standards in UCSC's economics department; see Figure D.10.

[34] Quantitative methodology courses include any course that mentions 'statistics', 'econometrics', 'psychometrics' or 'quantitative/math/research/information methods' in its title. See Figures D.11 and D.12.

[35] Arteaga (2018) finds that, in the setting of a Colombian university, a policy change that resulted in a 15 percent reduction in course-taking among economics majors caused a 16 percent decline in students' early-career wages, implying a unit wage elasticity of economics course-taking. It is unsurprising that we estimate a lower elasticity, given that: (1) below-threshold UCSC students excluded from the economics major took other courses instead of economics courses, whereas the Colombian students graduated having completed fewer aggregate courses; and (2) below-threshold UCSC students earned a different college major instead, which could change the signal value of their degree.

[36] One potential strategy to directly estimate the signal value of UCSC's economics degree would be to compare the wages of economics majors and non-majors who took comparable numbers of economics courses. Unfortunately, as at many U.S. public universities, many UCSC economics courses were *de facto* or *de jure* restricted to economics majors. Figure D.13 shows that there is essentially no overlap between the distribution of economics courses completed by 2008-2012 UCSC economics majors and non-majors, thwarting that design.

industries.[37] Panel (b) shows that economics major access increases students' early-career likelihood of working in the most-impacted finance, insurance, and real estate (FIRE) and accounting industries by 25 percentage points, split two-thirds/one-third between the two. Economics majors became 17 percentage points less likely to work in the education, healthcare, and social assistance industries in 2017-2018.[38]

Panel (c) of Figure 5.5 shows the effect of majoring in economics on the average wages earned in students' industries of employment. Industries are defined by six-digit NAICS codes, and industry mean wages are measured using the 2017-2018 wages of all 2008-2012 UCSC students. Barely above-threshold economics majors work in industries with higher mean wages by about \$10,000, implying that just under half of the \$22,000 wage return to majoring in economics can be explained by economics majors working in higher-paying industries.[39]

5.7 Average Wage-by-Major Statistics

Differences in the average wages earned by college graduates with different majors are often presented as useful for students' major selection (Carnevale, Cheah and Hanson, 2015; U.S. Department of Education, 2019), but they could be misleading as a result of self-selection into majors. To examine this concern empirically, this section compares the causal return to majoring in economics at UCSC to observational differences in wages by major estimated using data from various reference populations (e.g., all UCSC graduates or college graduates in California).

Denote the average wage of college graduates in reference population R who completed major m by $\widetilde{w}_m^R$. Among students at UCSC who have taken Econ 1 and 2, let m_i be student i's chosen major, $w_i(m)$ be the latent wages they would have earned if they had selected major m, and $w_i = w_i(m_i)$ be their observed wage given that they chose m_i. T is the treatment major (economics). Let P_m^0 be the probability of choosing non-economics major m for the barely below-threshold students who would have earned economics majors if their $EGPA$s had been slightly higher (that is, below-threshold policy compliers); P_m^R be the probability of a student in R selecting m conditional on not selecting economics; and $\overline{w}_m^0$ and $\overline{w}_m^1$ be the expected latent wages in major m of UCSC policy compliers just below and above the GPA threshold. We can then estimate Equation 5.1 in our sample of UCSC Econ 1 and 2 takers either using each student's observed wage as the dependent variable, or replacing it with the $\widetilde{w}_m^R$ of their chosen major. These regressions yield estimates, respectively, of:

[37] First-year career-intention survey responses (prior to majoring in economics) are smooth across the threshold. We examine sophomore and junior responses because those students have (likely) already declared the economics major but have not yet been hired into postgraduate employment. Six 2012 sophomore respondents – economics majors with 2.7 $EGPA$s – are omitted from estimation as outliers; see Figure D.14.

[38] See Table D.5, which shows estimated changes for each two-digit NAICS code. Accounting – in which UCSC Economics offers several courses – is the most-impacted six-digit NAICS code outside of FIRE industries.

[39] This conclusion is supported by a \$15,400 estimated IV wage coefficient in the presence of 6-digit-NAICS fixed effects, though that estimate is statistically noisy (s.e. \$8,000). If industries are partitioned into just three groups – FIRE, accounting, and all other industries combined – the two can explain only a \$2,300 (IV) wage increase at the threshold. Mean industry wages calculated using earlier UCSC cohorts and 2009-2010 wages provide nearly identical estimates, suggesting this information *could* have been partly known by students. NAICS codes with fewer than 10 observed workers are omitted.

$$LATE_{RD}(w) = \overline{w}_T^1 - \sum_{m \neq T} P_m^0 \overline{w}_m^0 \tag{5.2}$$

$$LATE_{RD}(\widetilde{w}_m^R) = \widetilde{w}_T^R - \sum_{m \neq T} P_m^0 \widetilde{w}_m^R \tag{5.3}$$

These equations show that wage-by-major statistics from R can be used to predict the treatment effect of earning an economics major for barely above-threshold UCSC students if they are similar to policy compliers' latent wages by major near the GPA threshold.

Figure 5.6 shows the average early-career wages by major for barely above-threshold economics majors' ten most common second-choice majors — led by psychology (20%), environmental studies (14%), and "technology and information management" (TIM, 12%) — and for UCSC's three economics tracks.[40] Average wages by major ($\widetilde{w}_m^R$) are calculated in three ways: by linear regression of UCSC students' early-career wages on major dummies with and without detailed student controls, and by the median wages of all early-career college graduates in California.[41] The figure also shows estimates of $LATE_{RD}(\widetilde{w}_m^R)$ for each set of average wage statistics as the difference between two dashed horizontal lines. These are estimates of Equation 5.3, which implicitly weights the average wage in each counterfactual major by the likelihood that a below-threshold policy complier would select it. They are juxtaposed, at the far right, with the causally-identified return to majoring in economics — our estimate of Equation 5.2.[42]

At UCSC and across the state, economics majors have substantially higher average wages than college graduates who earned the observed counterfactual majors.[43] Using either OLS estimates or median wages, the difference between the average wages of economics majors and the weighted-average wage among the counterfactual majors *underestimates* the causally-estimated return to majoring in economics by up to 21 percent.

Why might wage-by-major estimates differ from the treatment effect of majoring in economics? To see the possible sources of bias, note that linear regression of observed wages on treatment in population R estimates $\beta_{OLS}^R(w) \equiv \widetilde{w}_T^R - \sum_{m \neq T} P_m^R \widetilde{w}_m^R$, and that it is generically true in a Rubin Causal Model that

$$\begin{aligned}\beta_{OLS}^R(w) = &\overbrace{E(w_i(T)|m_i = T) - E(w_i(\sim T)|m_i = T)]}^{\text{Average Treatment Effect on Treated in } R\ (ToT^R)} \\ &+ \underbrace{[E(w_i(\sim T)|m_i = T) - E(w_i(\sim T)|m_i \neq T)]}_{\text{Selection Bias}}\end{aligned} \tag{5.4}$$

[40]Above-threshold policy compliers are more likely to choose the business management economics track than the average economics major. The fraction of economics majors on the BME track only increases slightly and statistically-insignificantly across the GPA threshold (10.5 percentage points, s.e. 6.1), suggesting that the large share of policy compliers on that track largely results from local student demand, not department policy. See Figure D.15.

[41]National wage-by-major medians display a similar pattern; see Table D.6. CA and U.S. statistics from the American Community Survey (Ruggles et al., 2020). See Table D.7 for a UCSC-ACS major crosswalk.

[42]The imputed wage estimates partition students by their *set* of majors to calculate averages, whereas the major-specific estimates assign multi-major students to their higher-earning major; see Figure D.16. Estimates of below- and above-threshold UCSC policy compliers' imputed and actual wages follow Abadie (2002).

[43]Business management economics (BME) majors have somewhat higher average wages than other economics majors at UCSC, but not elsewhere. UCSC's high-wage TIM major includes the economics department's core course sequence as required courses.

Equation 5.4 shows that OLS overestimates economics majors' true wage gains if those selecting economics would have earned more in non-economics majors than those who did not select economics — due to, e.g., stronger prior quantitative training or stronger preferences for high wages. Combining Equations 5.2, 5.3, and 5.4 yields

$$\begin{aligned} LATE_{RD}(\widetilde{w}_m^R) - & LATE_{RD}(w) \\ & = \underbrace{[LATE_{RD}(\widetilde{w}_m^R) - \beta_{OLS}^R(w)]}_{\text{Counterfactual Major Correction}} \\ & \quad + \underbrace{[ToT^R - LATE_{RD}(w)]}_{\text{Treatment Effect Heterogeneity}} + [\text{Selection Bias}] \end{aligned} \tag{5.5}$$

Equation 5.5 decomposes the difference between the observational difference in average wages by major in population R and our estimated treatment effect of majoring in economics at UCSC. The counterfactual major correction is positive whenever the majors selected by below-threshold UCSC policy compliers are systematically higher-earning than those selected by non-economics majors in R - as is clear from comparing the definition of $\beta_{OLS}^R(w)$ to Equation 5.3. The treatment effect heterogeneity term is positive whenever economics majors in R have larger latent treatment effects than those of policy compliers near the GPA threshold. Selection bias is positive when economics majors in R would have earned higher wages in non-economics majors than non-majors in R.

The left-hand side of Equation 5.5 is negative and small when R consists of all UCSC graduates, and the counterfactual major correction is very small. This implies that the treatment effect heterogeneity and selection bias terms must roughly cancel each other out.[44] Figure 5.6 shows this clearly: above-threshold policy compliers have lower average earnings than the average UCSC students on their economics tracks, but their wages would have been even lower — to an even greater degree than the difference in average wages by major — if they'd earned their second-choice majors instead.[45,46] Combined with the fact that selection bias resulting from observable characteristics is positive ($\$19,247 - \$17,461 > 0$), this suggests that $ToT^{UCSC} < \beta_{OLS}^{UCSC} < LATE_{RD}(w)$: the average economics major earned a return smaller than the observational wage difference, while students who were barely unable to declare the economics major may have earned a return larger than the observational wage difference.

Together, these results suggest that OLS and wage-by-major medians well-approximate, and in fact slightly underestimate, the causal effect of majoring in economics identified by our instrumental variable design.

[44] With all UCSC graduates as R, we estimate $LATE_{RD}(\widetilde{w}_m^R) = \$19,427$ (Figure 6), $LATE_{RD}(w) = \$22,123$ (Figure 6), and $\beta_{OLS}^R(w) = \$20,039$ (Table D.6). The LHS is then -\$2,876, the counterfactual major correction is -\$792, and the heterogeneity and selection terms sum to -\$2,084 — less than 10% of the estimated treatment effect by magnitude.

[45] This is consistent with students having comparative advantage in their preferred major (Kirkeboen, Leuven and Mogstad, 2016), one dimension of treatment effect heterogeneity.

[46] Using the CPI-adjusted 2009-2010 wage-by-major medians of earlier UCSC cohorts to impute the 2008-12 cohorts' wages yields $LATE_{RD}(\widetilde{w}_m^R)$ estimates strikingly similar to the true local average treatment effect (Figure D.17), suggesting that those effects are relatively stable over time.

5.8 Conclusion

The UC Santa Cruz Department of Economics's 2008-2012 binding major restriction policy provides an unusual opportunity to transparently identify the personal early-career wage return to earning an economics major in college. We show that the wage return to economic education is very high relative to education in students' second-choice social science disciplines, causing a 46 percent increase in mid-20s earnings despite no change in educational investment or degree attainment. About half of the observed effect can be attributed to economics majors' specialization in particular high-wage industries, in part reflecting changes in students' reported preferences across professions. Mirroring a similar finding from studies of the return to additional years of education (Card, 1999), we show that major-specific OLS estimates and differences in median wages by major both slightly underestimate the observed wage return to economics. For reference, a comparison between the national median wages of college graduates with economics degrees and those of graduates with degrees in UCSC economics students' second-choice majors suggests that majoring in economics raises the net present value of a student's college education by $536,000, with the early-career annual wage difference widening over time.

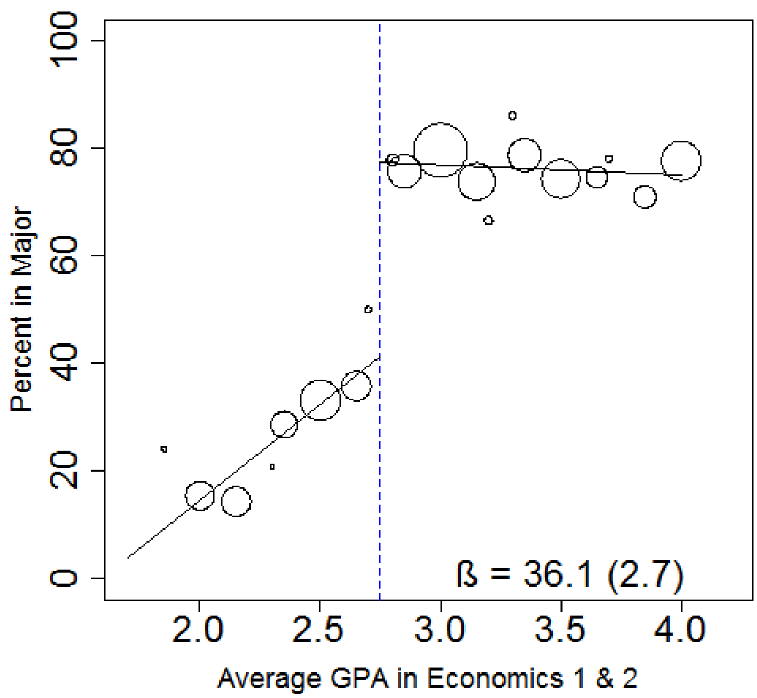
These findings imply that students' major choices could have financial implications roughly as large as their decision to enroll in college (Autor, 2014), highlighting the centrality of heterogeneity in the private returns to higher education. They also point to students' college major choice as a key decision point where policy-makers can intervene to substantially impact youths' long-run labor market outcomes.[47] Finally, these findings highlight the relationship between major-specific returns and industrial composition, suggesting an important role for preferences and industry-specific human capital acquisition in postsecondary education.

These findings come with four caveats. First, our results are estimated for students at a moderately-selective public university — at the 60th percentile of the university average SAT distribution — where nearly all students eventually earn a Bachelor's degree (at UCSC or elsewhere); the findings may not be representative of the average university student. Second, our analysis is restricted to students who already choose to take introductory economics courses, and may not extend to other students. Third, there are many U.S. states (unlike California) where economics majors do not earn above-average early-career wages, suggesting an important role for local labor demand in shaping major-specific returns.[48] Finally, higher education's broad public and non-pecuniary returns imply that wage returns are insufficient in themselves for drawing conclusions about the efficiency of educational policies (e.g. see McMahon (2009)).

[47] Indeed, Bleemer and Mehta (2020*a*) show that GPA-based major restrictions regressively shape students' major choices, tending to decrease disadvantaged students' access to universities' high-demand majors.

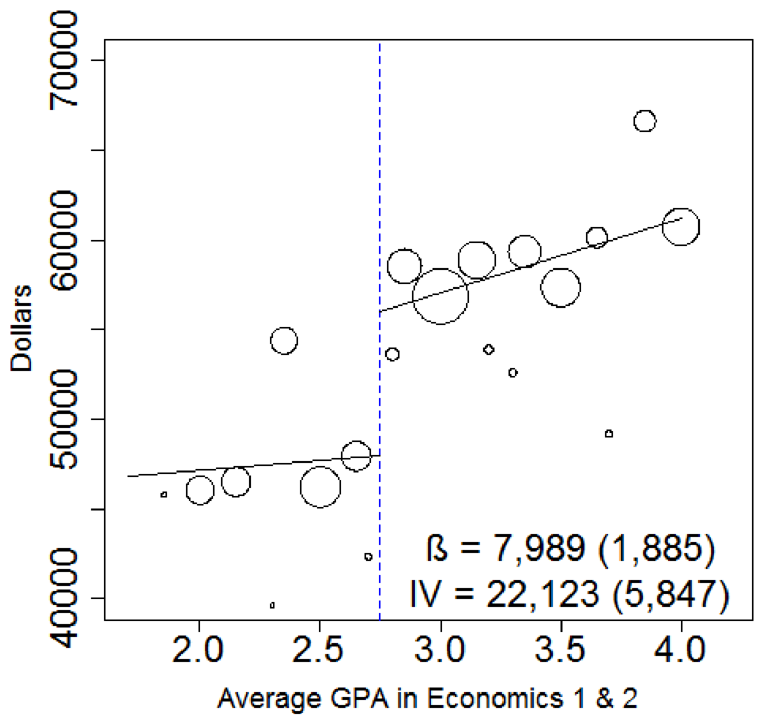
[48] For example, in the 15 states where industries' employment shares among college graduates are least similar to California's, 2017-2018 ACS statistics show that economics majors do not have higher median wages than other college graduates, and earn lower wages than non-majors in most two-digit industries. See Figure D.18.

Figure 5.1: The Effect of the UCSC Economics GPA Threshold on Majoring in Economics



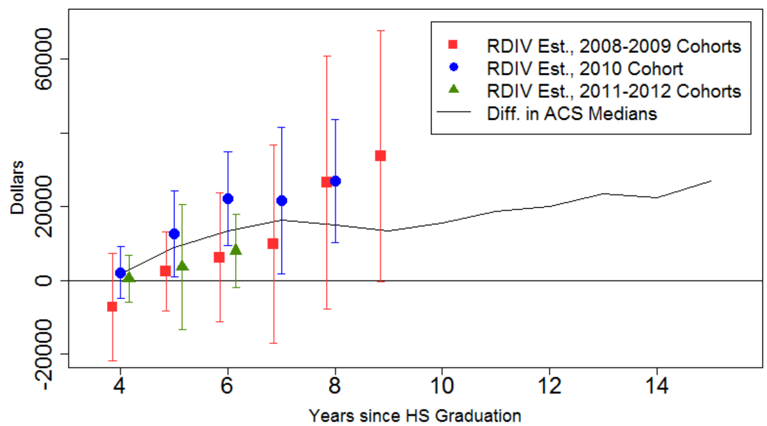
Note: Each circle represents the percent of economics majors (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. $EGPA$s below 1.8 are omitted, leaving 2,839 students in the sample. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification; standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database.

Figure 5.2: The Effect of the UCSC Economics GPA Threshold on Annual Wages



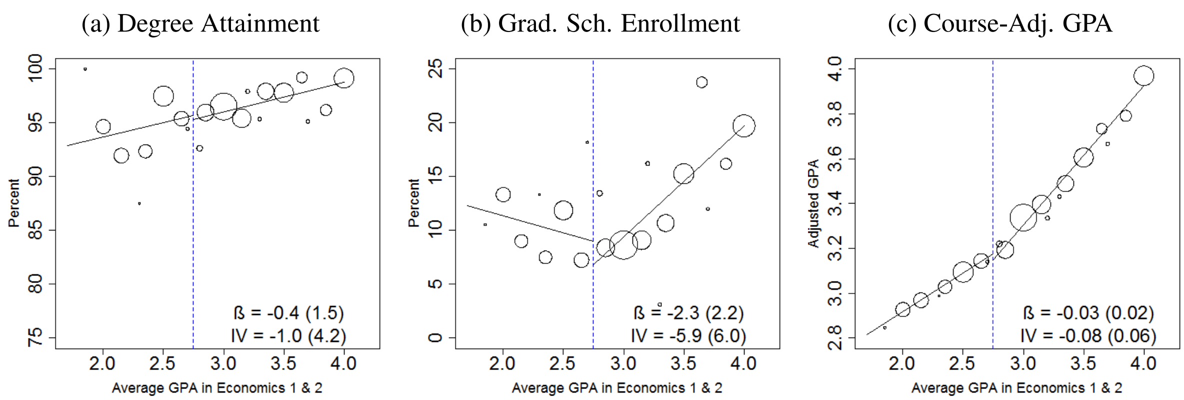
Note: Each circle represents the mean 2017-2018 wages (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. 2017-2018 wages are the mean EDD-covered California wages in those years, omitting zeroes. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. $EGPA$s below 1.8 are omitted, leaving 2,446 students with observed wages. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard errors (clustered by $EGPA$) in parentheses. Sources: The UC-CHP Student Database and the CA Employment Development Department.

Figure 5.3: Estimated Wage Return to Economics Major by Age



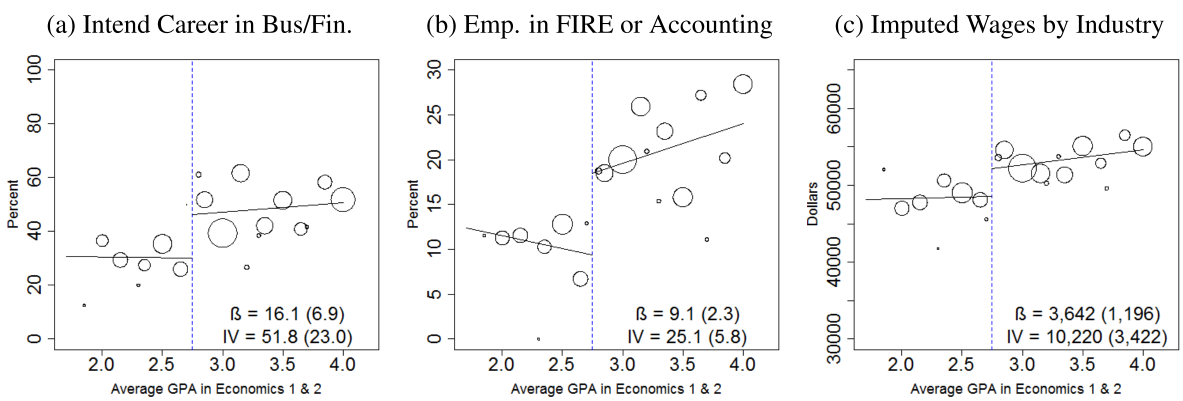
Note: This figure shows regression discontinuity instrumental variable β estimates at the 2.8 GPA threshold of the effect of majoring in economics on earnings in each of 4-9 years after high school graduation, splitting the sample into the 2008-2009, 2010, and 2011-2012 UCSC incoming-class cohorts. The bars show 95% confidence intervals from standard errors clustered by $EGPA$. The black line shows the difference between the national median wages of economics majors and those of college graduates with majors in barely above-threshold UCSC students' second-choice majors, as measured in the ACS; see Figure D.8. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Sources: The UC-CHP Student Database, the CA Employment Development Department, and the American Community Survey (Ruggles et al., 2020).

Figure 5.4: The Effect of Economics Major Access on Education and Attainment



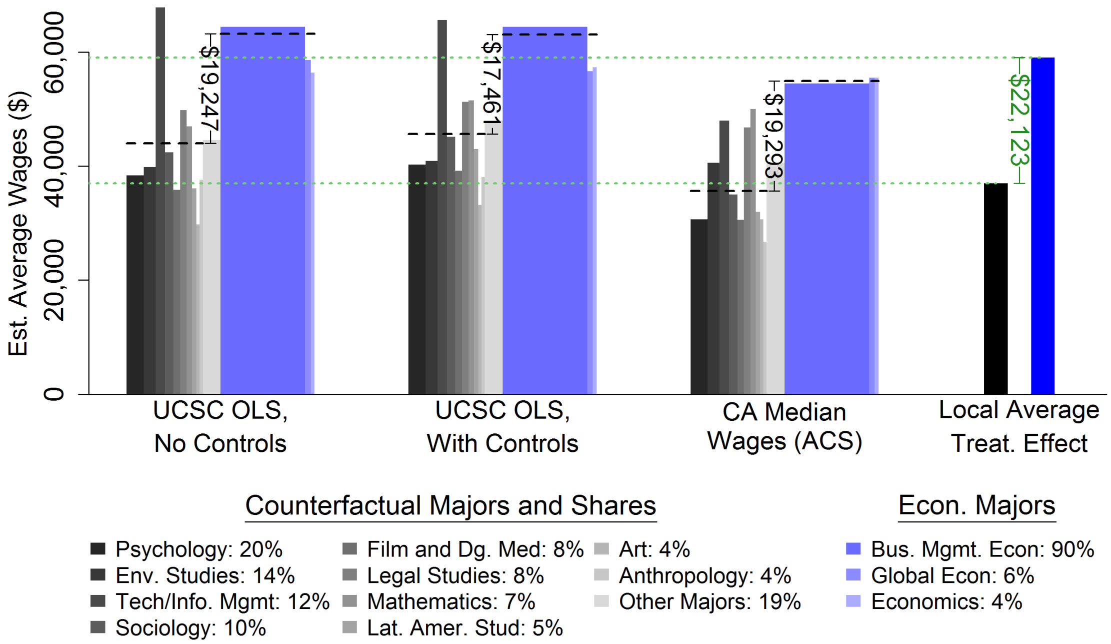
Note: Each circle represents the mean educational characteristic (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Undergraduate degree attainment is measured in 2018. Graduate school enrollment indicates enrollment at a four-year university after undergraduate degree attainment within seven years of UCSC matriculation. Course-Adjusted College GPA is calculated as the mean of the differences between students' grades and each course's fixed effect from a two-way student-course fixed effect model (see Figure D.10). $EGPA$s below 1.8 are omitted, leaving 2,839 students in the sample. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Sources: The UC-CHP Student Database and the National Student Clearinghouse.

Figure 5.5: Effect of Economics Major Access on Industry Preferences and Employment



Note: Each circle represents the mean outcome measure (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Intended career in business/finance indicates selecting "Business, finance-related professions" on a survey asking "Career hope to eventually have after education complete" (see Appendix A) among the 834 in-sample second- and third-year UCUES respondents. Employment in FIRE and accounting indicates 2017 or 2018 employment in the finance, insurance, and real estate (NAICS codes 52 and 531) or accounting (541211) industries; see Figure D.5. Imputed wages by industry (6-digit NAICS) are calculated as the mean 2017-2018 wages of all 2008-2012 freshman-admit UCSC students. Imputed wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specifications and instrumental variable specifications (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Six 2012 sophomore respondents were omitted from estimation; see Figure D.14. Sources: The UC-CHP Student Database, the SERU database, and the CA Employment Development Department.

Figure 5.6: Average Wage Differences between Economics and Counterfactual Majors



Note: This figure shows average early-career 2017-2018 wages by major of UCSC students (estimated by OLS, with and without control variables) and all California college graduates (ACS medians) for UCSC's three economics tracks and for the ten most common counterfactual majors earned by below-threshold UCSC policy compliers, juxtaposed with the causally-identified local average treatment effect on early-career wages for below- and above-threshold UCSC policy compliers (following Abadie (2002)). The black dotted lines show the average wages of the majors chosen by below- and above-threshold policy compliers, calculated by assigning each 2008-2012 UCSC student to their corresponding majors' average wage – leave-one-out in the UCSC no-controls sample – and using the linear RD IV model on the resulting imputed wages. Counterfactual major shares are estimated by the linear RD IV model predicting an indicator for earning that major; the shares sum to over 100% because below-threshold policy compliers earn more multiple majors. Bar widths are proportional to the major shares. UCSC statistics from 2008-2012 UCSC students matched to 2017-2018 wages; California statistics calculated from age 23-28 2017-2018 ACS respondents. OLS coefficients from regressions of wages on major indicators with or without covariates (gender-ethnicity, SAT score, ZIP Code average AGI, cohort year, and high school fixed effects), partitioning students by their highest-earning major. See Figure D.7 for UCSC-ACS major mapping. Wages and wage-by-major averages are CPI-adjusted to 2018 and winsorized at 2% above and below. Sources: The UC-CHP Student Database, the CA Employment Development Department, and the American Community Survey (Ruggles et al., 2020).

Table 5.1: Descriptive Statistics of 2008-2012 UCSC Enrollment Cohorts

	Freshman Students	Econ 1 & 2 Enrollees	Economics Majors	Near-Threshold Econ. Majors	(s.e.)
Female (%)	55.7	41.3	40.9	35.6	(7.3)
White (%)	40.8	32.4	32.8	27.9	(6.5)
Asian (%)	26.5	41.4	43.7	41.1	(8.1)
Hispanic (%)	24.3	19.2	16.7	18.3	(7.1)
Black (%)	2.9	1.9	1.7	6.2	(1.8)
CA Resident (%)	97.1	97.4	97.2	99.7	(2.5)
SAT Score (2400 scale)	1720	1697	1716	1667	(14)
Mean ZIP Code Inc. ($)	92,060	95,819	99,477	86,770	(7,309)
Number of Students	15,423	3,053	1,689		

Note: This table presents mean demographic and socioeconomic statistics for 2008-2012 UCSC freshman-admit students, those who take Economics 1 and Economics 2, and those who then declare the economics major. The final columns present the average characteristics of the students who majored in economics because of their barely above-threshold $EGPA$s, estimated following Equation 5.1 by treating the interaction between each characteristic and economics major indicator as the outcome (Abadie, 2002). Mean ZIP Code Income measures the mean adjusted gross income of tax-filers in the student's home ZIP Code in the year they graduated high school. Sources: The UC-CHP Student Database and IRS Statistics of Income (SOI).

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Appendix A

Appendix to Chapter 2

A.1 Appendix A: Public Universities Practicing Affirmative Action in 2020

Many public and private universities are non-transparent about their undergraduate admissions policies. However, most universities publish annual "Common Data Set" reports that provide a response to the question: What is the "relative importance of each of the following academic and nonacademic factors in first-time, first-year, degree-seeking (freshman) admission decisions: ... Racial/ethnic status: Very Important, Important, Considered, and Not Considered".

The following is a list of states with public universities where race/ethnic status is at least considered in undergraduate admissions – according to their most recent common data set available in July 2020 – naming the university in parentheses if it differs from the state's flagship public university: CO, CT, DE, GA (Georgia Tech), IL, IN, LA (Grambling State), ME (University of Southern Maine), MD, MA, MI, NJ, NY, NC, OH, OR, PA, RI, SC, TN, TX, UT, VT, VI, and WI. The University of New Hampshire reports considering race in admissions, but is prohibited by law from providing preference to applicants based on their race. The University of New Mexico does not report whether or not it considers race in admissions.

A.2 URM and Non-URM Admissions by UC Campus and AI, 1994-2001

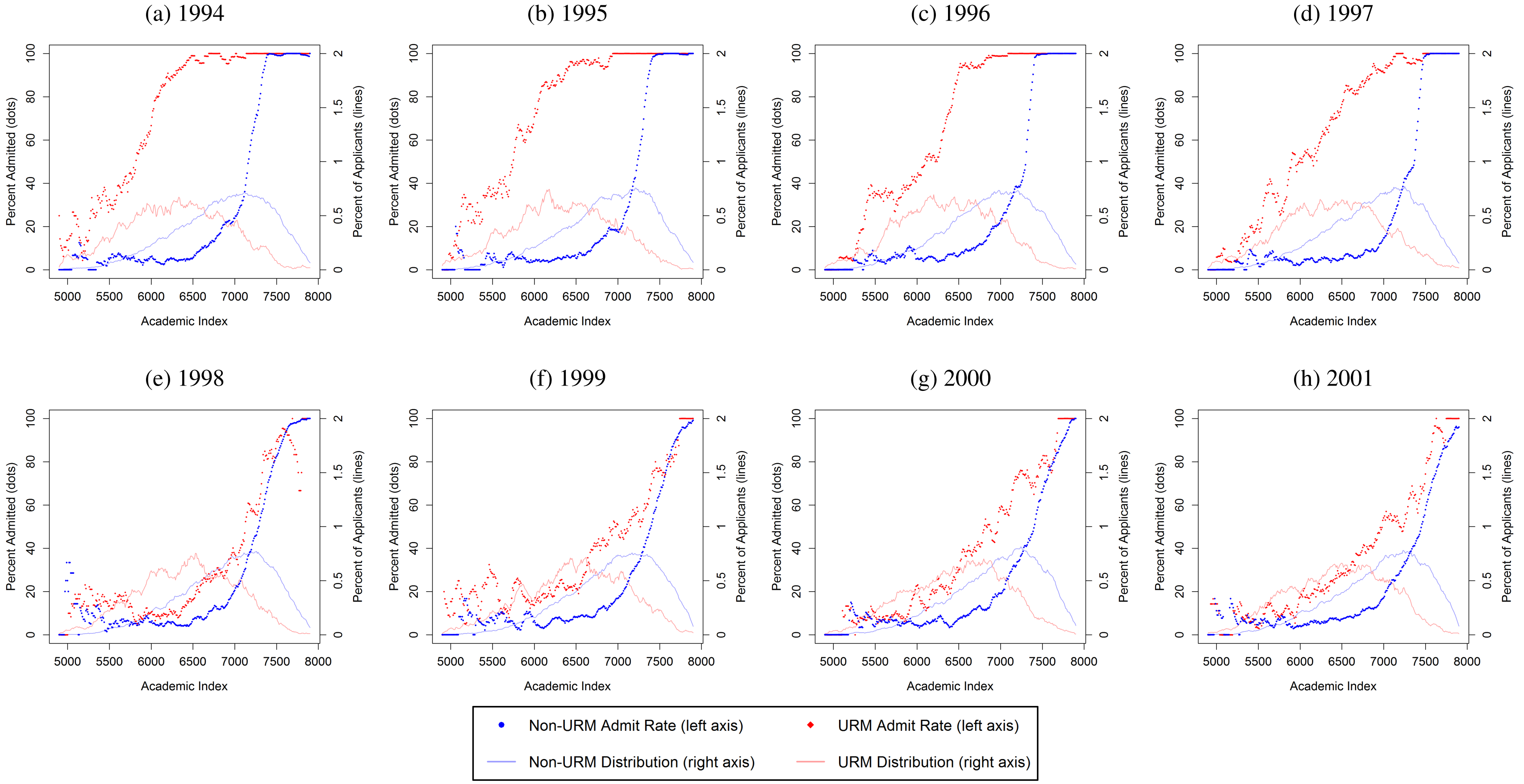
The figures below show the raw admissions likelihood and application distribution of URM and non-URM applicants to each UC campus by Academic Index from 1994 to 2001. The figures clarify how affirmative action was practiced by different UC campuses before 1998, and how Prop 209 changed the admissions likelihood of URM applicants (and, to some degree, non-URM applicants).[1] For example, UC Davis and UC Santa Cruz guaranteed admission to nearly all UC-eligible URM applicants before 1996, while UC Berkeley extended their admissions guarantee to URM students with AI more than 1,000 points lower than the guarantee extended to non-URM

[1]Latino UC applicants – who made up about one in five URM UC applicants in the period – received somewhat smaller admissions advantages than American Indian, Black, and Chicano UC applicants in some years at some campuses (e.g. see Figure A.13). They are omitted from the figures in this Appendix.

students. The URM and non-URM admissions rates sharply converged after Prop 209, though at most campuses URM applicants at nearly every AI remained more likely to be admitted than non-URM applicants. The differences between the admissions likelihoods of URM and non-URM UC applicants in different years are summarized in Figure 2.1.

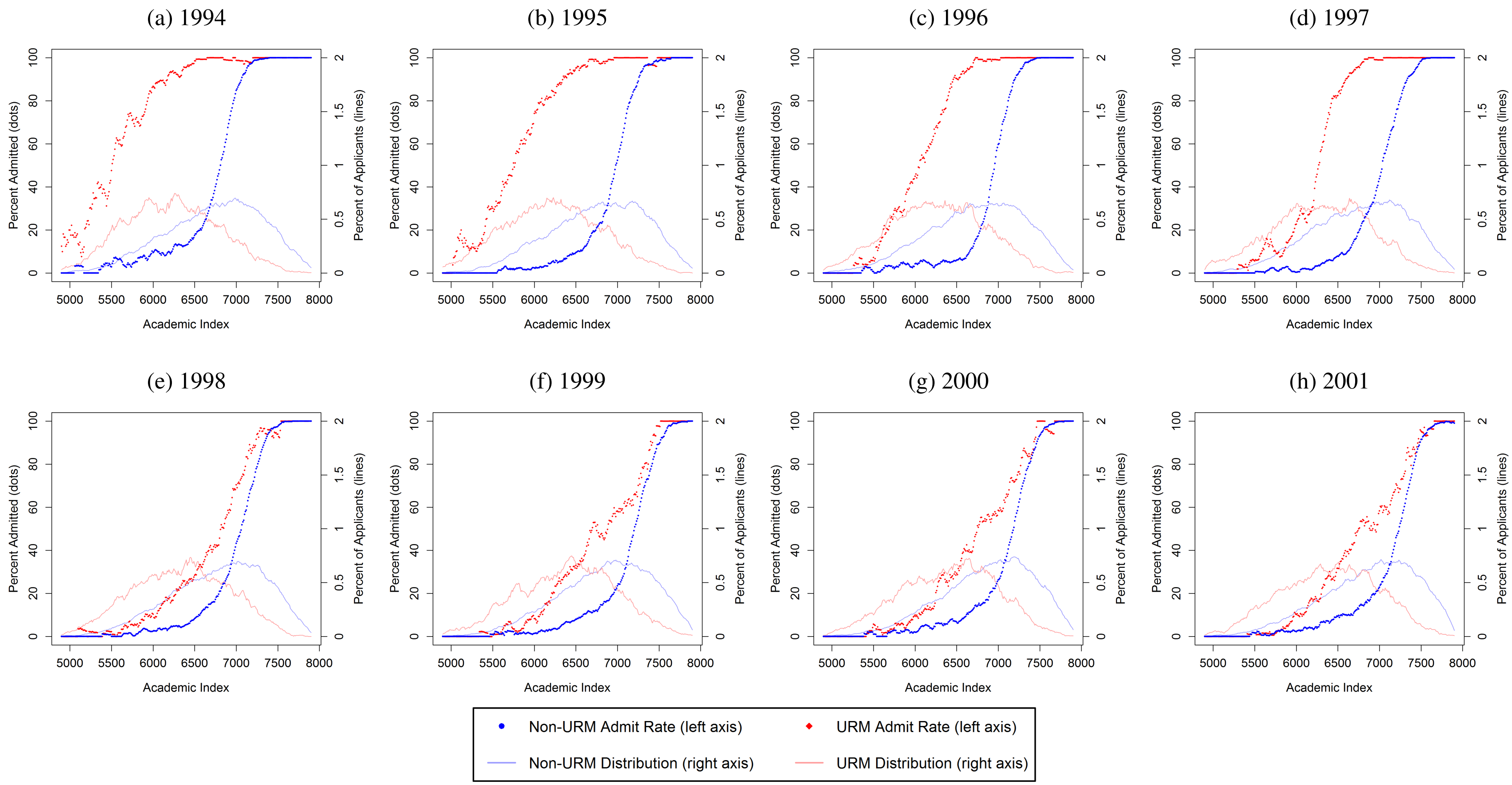
The AI distribution of applicants was most-dissimilar by ethnicity at the Berkeley and UCLA campuses, which had far higher shares of low-AI URM applicants than low-AI non-URM applicants, reflecting the large admissions advantages provided by those campuses to even lower-AI URM applicants under affirmative action. The distribution of applicant AI rose over time at most campuses, likely driven both by grade inflation and growing cross-campus interest in UC enrollment among high-AI California high school graduates.

Figure A.1: Annual “Normal” Admissions at UC Berkeley



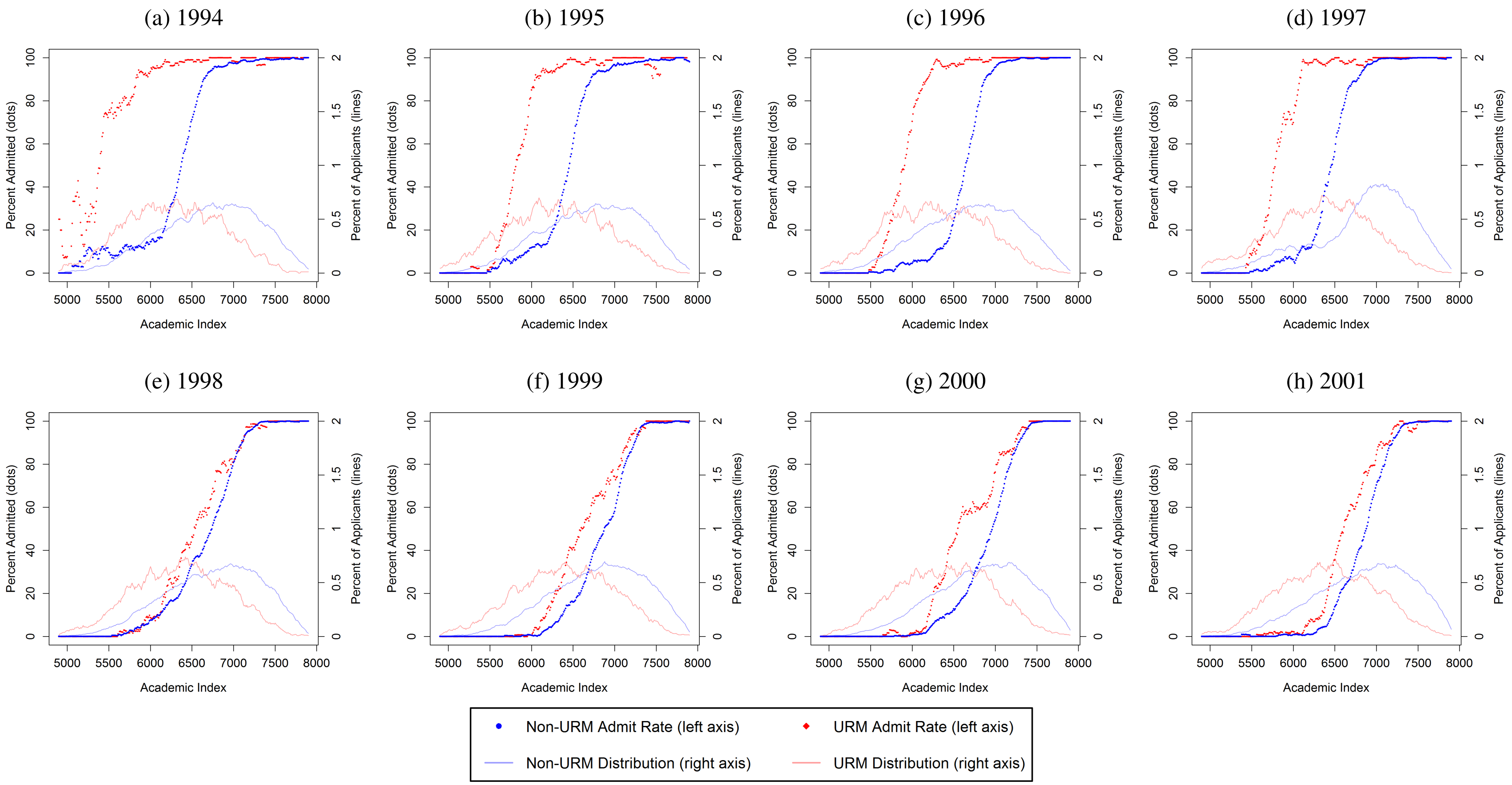
Note: This figure shows the 1994-2001 annual UC Berkeley admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UC Berkeley applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UC Berkeley by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC's minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Figure A.2: Annual "Normal" Admissions at UCLA



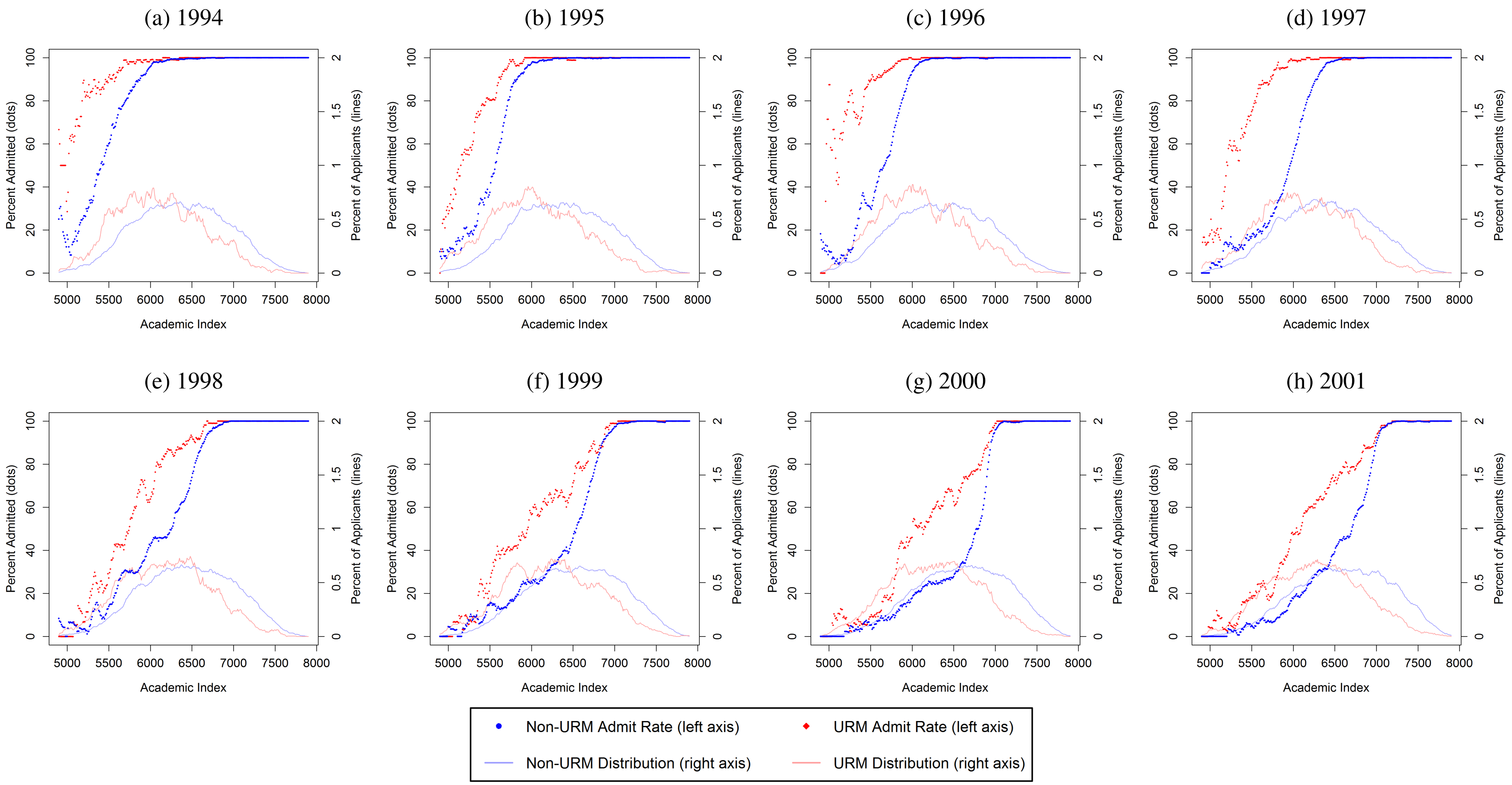
Note: This figure shows the 1994-2001 annual UCLA admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UCLA applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UCLA by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC's minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Figure A.3: Annual "Normal" Admissions at UC San Diego



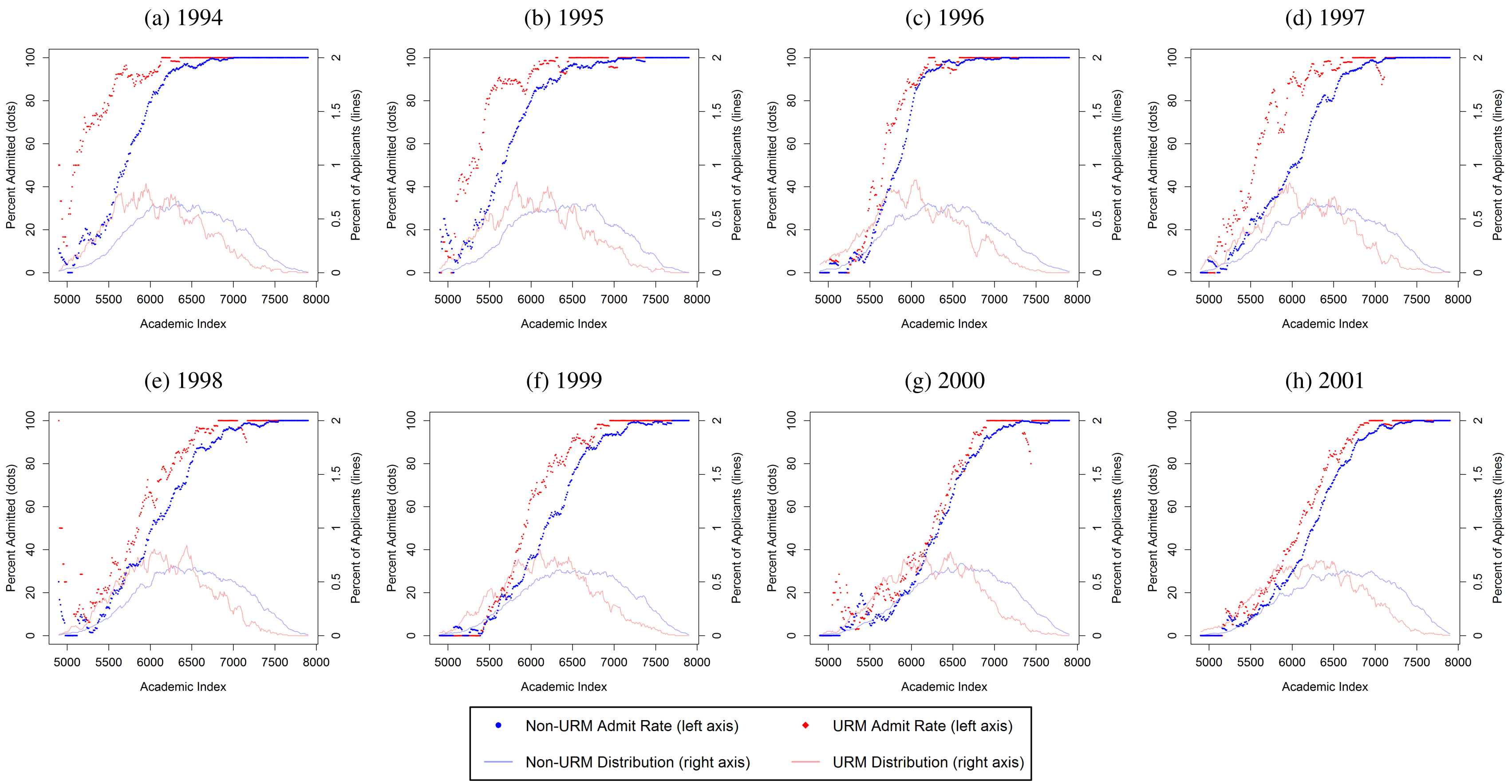
Note: This figure shows the 1994-2001 annual UC San Diego admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UC San Diego applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UC San Diego by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC's minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Figure A.4: Annual “Normal” Admissions at UC Santa Barbara



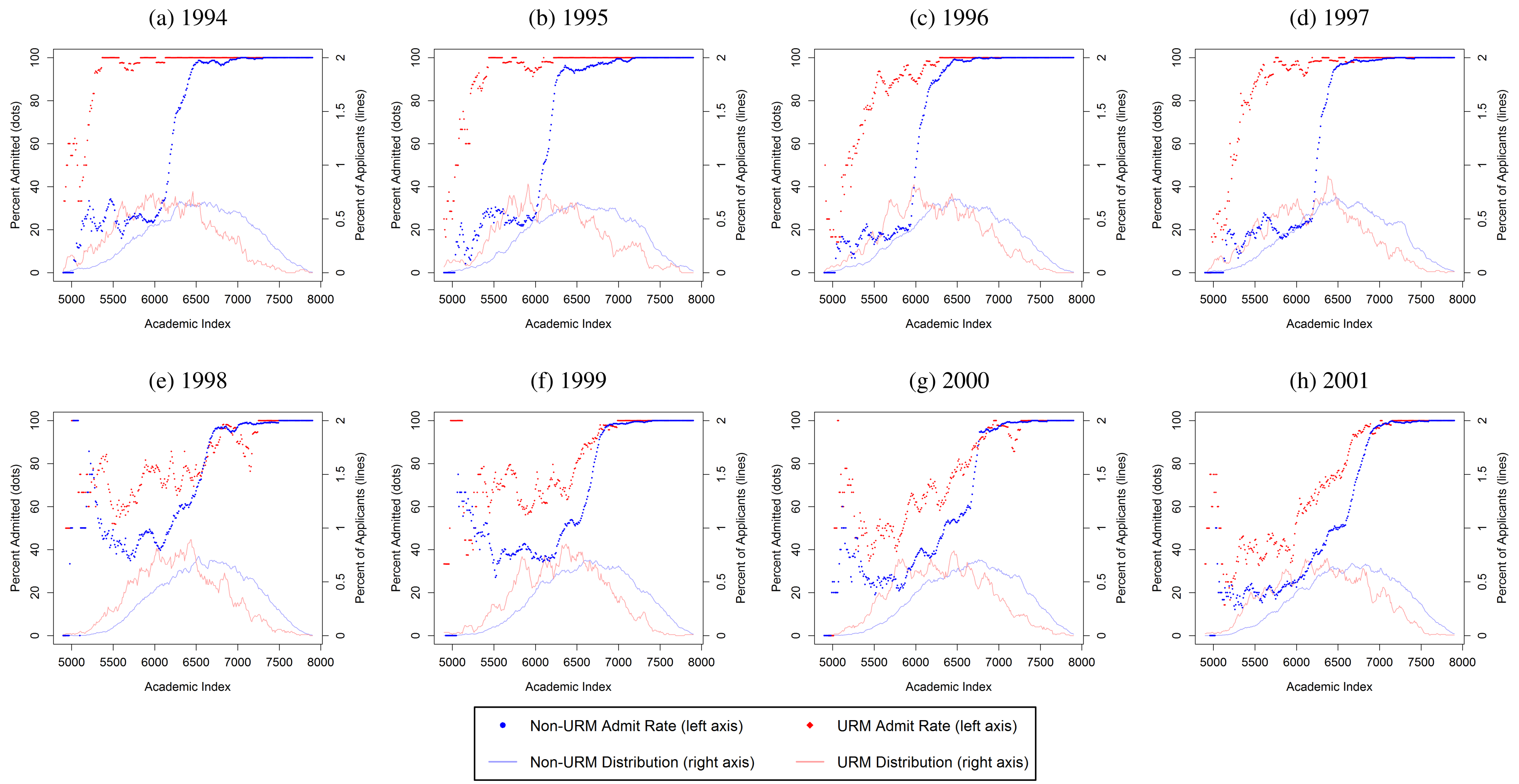
Note: This figure shows the 1994-2001 annual UC Santa Barbara admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UC Santa Barbara applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UC Santa Barbara by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC's minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Figure A.5: Annual "Normal" Admissions at UC Irvine



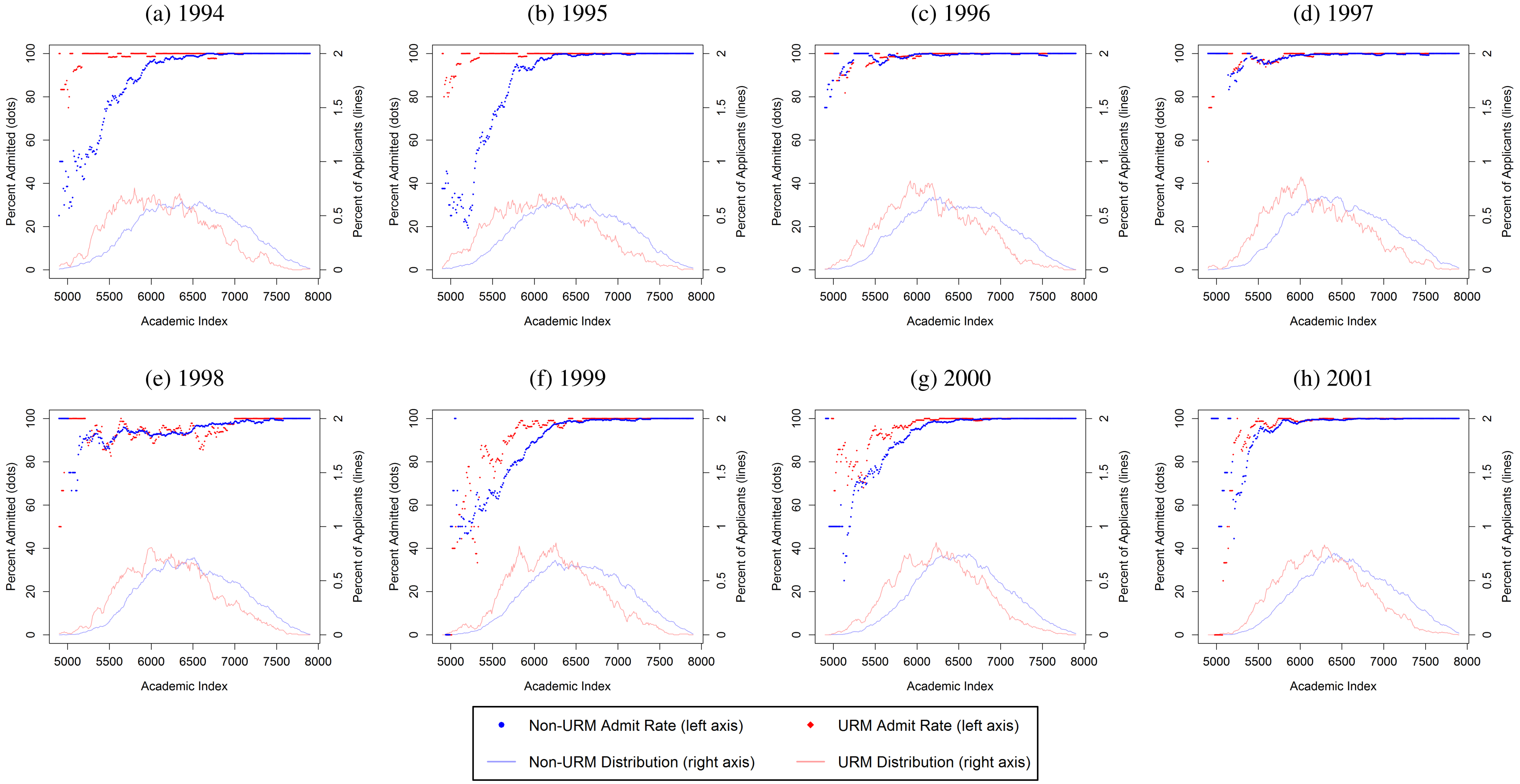
Note: This figure shows the 1994-2001 annual UC Irvine admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UC Irvine applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UC Irvine by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC's minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Figure A.6: Annual “Normal” Admissions at UC Davis



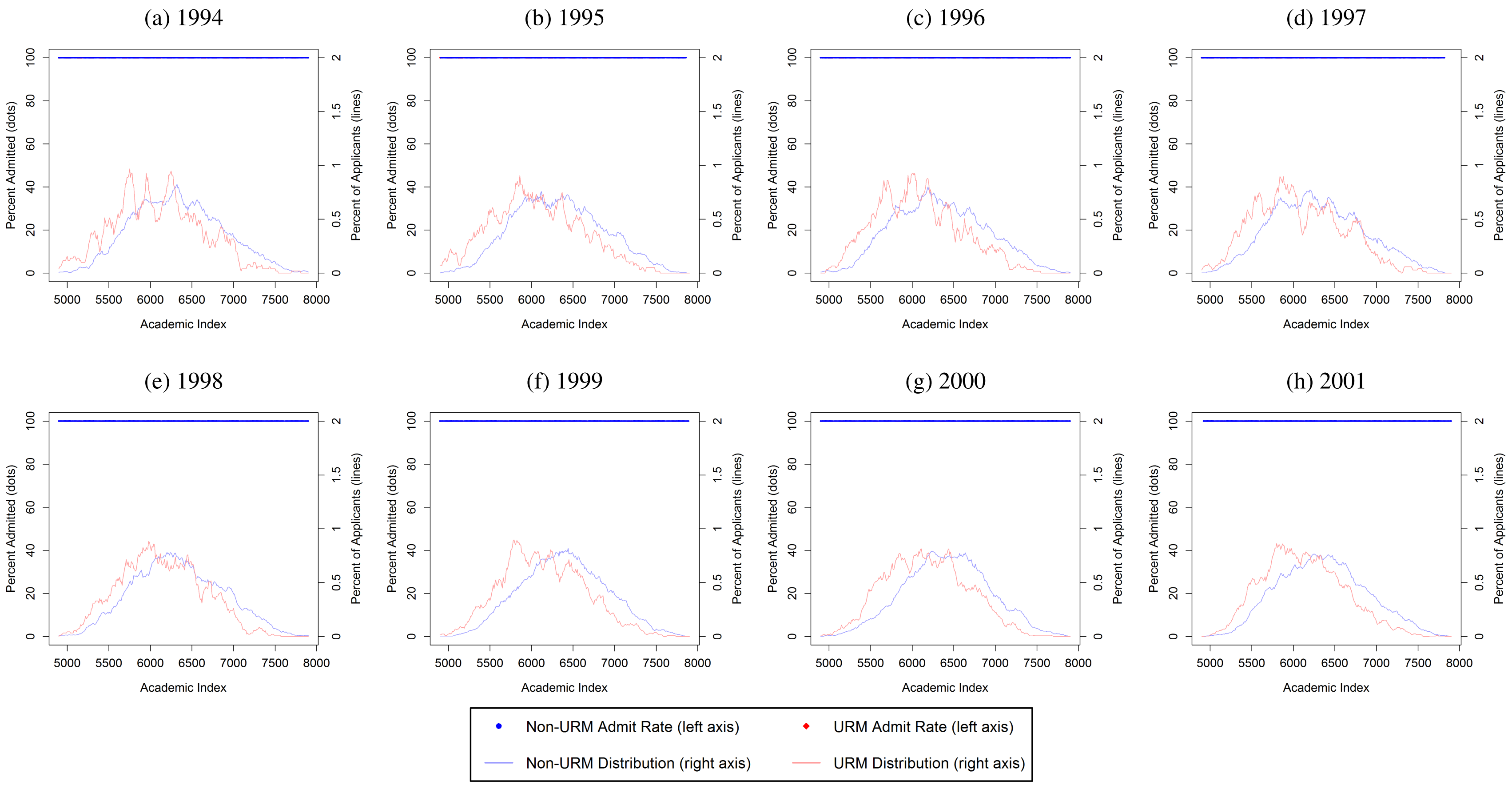
Note: This figure shows the 1994-2001 annual UC Davis admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UC Davis applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UC Davis by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC’s minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Figure A.7: Annual "Normal" Admissions at UC Santa Cruz



Note: This figure shows the 1994-2001 annual UC Santa Cruz admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UC Santa Cruz applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UC Santa Cruz by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC's minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Figure A.8: Annual "Normal" Admissions at UC Riverside



Note: This figure shows the 1994-2001 annual UC Riverside admissions rate for URM and non-URM applicants by Academic Index, as well as the annual distribution of UC Riverside applicants by Academic Index and ethnicity. Raw percent of URM and non-URM students admitted to UC Riverside by Academic Index (AI) – the sum of (top-censored) high school GPA, SAT I score, and three SAT II scores – each year from 1994 to 2001 (left axis). The lines show the probability density function of URM and non-URM UC applicants by AI (right axis). Admission rates and distributions are smoothed with a uniform kernel of bandwidth 50; AI below 4900 and above 7900 are omitted. The sample is restricted to freshman fall California-resident applicants who (a) were UC-eligible, meaning that they satisfactorily completed UC's minimum high school coursework requirement, and (b) reported an intended major that did not have special admissions restrictions, like engineering at some campuses. Latino (but not Chicano) applicants received slightly smaller admissions advantages (see Figure A.13) and are omitted from these figures; URM includes American Indian, African American (Black), and Chicano applicants. Source: UC Corporate Student System.

Table A.1: Difference-in-Difference Estimates of Post-1998 URM Admissions by UC Campus

Campus:	UCB	UCLA	UCSD	UCSB	UCI	UCD	UCSC	UCR	Total
URM	37.3	26.8	23.8	17.0	10.1	27.5	7.0	4.2	9.3
	(0.6)	(0.5)	(0.5)	(0.5)	(0.6)	(0.5)	(0.6)	(0.6)	(0.3)
URM ×	-24.5	-16.0	-18.7	-6.3	-3.1	-18.6	-5.8	-3.7	-7.9
Prop 209	(0.7)	(0.6)	(0.6)	(0.6)	(0.7)	(0.7)	(0.8)	(0.7)	(0.4)
$\bar{Y}$	32.3	35.1	51.8	65.2	65.8	70.1	81.8	85.0	82.3
Obs.	88,905	108,327	93,238	82,061	70,343	73,834	45,053	45,396	199,321

Note: OLS coefficient estimates of β_0 and $\beta_{`98-99}$ from Equation 2.1, a difference-in-difference model of 1996-1999 URM UC freshman California-resident applicants' UC admission compared to non-URM applicants after Prop 209. Models are conditioned on applying to that UC campus. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47), and are estimated independently by campus or "Total" (all applicants to any UC campus). Robust standard errors in parentheses. Source: UC Corporate Student System.

A.3 UC Admissions and Yield after Prop 209

Table A.1 presents estimates of Equation 2.1's β_0 and $\beta_{`98-99}$ for admission to each UC campus, estimated on the 1996-1999 sample of applicants to that campus. While URM applicants were 37 and 27 percentage points more likely than comparable non-URM applicants to be admitted to Berkeley and UCLA under affirmative action, these advantages fell to 13 and 11 percentage points after Prop 209.[2] URM applicants faced similar-magnitude declines in their admissions likelihood at San Diego and Davis, and their admissions advantage fell at every campus. Among all applicants to any UC campus, URM applicants' admissions advantage over non-URM applicants (to be admitted to at least one campus) fell from 9.3 to 1.4 percentage points.

Table A.20 shows that admitted URM applicants became more likely to enroll at every UC campus after Prop 209, though URM applicants who were admitted to some UC campus became **less** likely to enroll at UC, a case of Simpson's Paradox reflecting the decline in the number of UC campuses to which URM applicants were admitted. Antonovics and Sander (2013) argue that this "warming effect" across UC campuses resulted from an increase in the signaling value of attending UC for URM applicants. As in that study, conditioning on the set of UC campuses to which applicants were admitted flips the sign of the UC-wide coefficient (to 2.8 percentage points); compared to academically-similar students admitted to the same UC campuses, post-1998 URM students are more likely to enroll at some UC campus. Admissions and enrollment statistics are slightly larger when estimated relative to the '94-95 baseline; see Table A.2.

[2]Note that these models do not control for family income or other measures of pre-college opportunity likely correlated with URM status. Since those factors remained part of UC admissions, it is unsurprising that the presented models still identify advantages for URM applicants despite Prop 209.

Table A.2: Difference-in-Difference Estimates of Post-1998 URM Admissions by UC Campus, Compared to ‘94-5 Baseline

Campus:	UCB	UCLA	UCSD	UCSB	UCI	UCD	UCSC	UCR	Total
Application conditional on UC application (%)									
URM	11.8	9.9	-1.8	-8.6	-8.9	-4.8	-3.2	-8.2	
	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.3)	
URM × Prop 209	-2.9	-5.7	-1.3	3.1	-0.8	1.5	0.9	5.9	
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	
$\bar{Y}$	43.9	53.5	48.1	40.8	35.7	37.8	23.1	23.8	
Obs.	190,540	190,540	190,540	190,540	190,540	190,540	190,540	190,540	
Admission conditional on application (%)									
URM	43.5	37.8	23.5	10.8	20.3	32.6	13.2	15.2	13.4
	(0.6)	(0.5)	(0.6)	(0.5)	(0.6)	(0.6)	(0.6)	(0.6)	(0.3)
URM × Prop 209	-29.6	-26.8	-19.7	-1.4	-14.0	-24.0	-12.9	-15.2	-12.4
	(0.7)	(0.6)	(0.7)	(0.7)	(0.7)	(0.8)	(0.8)	(0.7)	(0.4)
$\bar{Y}$	34.5	38.5	52.8	67.8	68.2	69.7	81.9	84.1	82.9
Obs.	82,637	100,991	91,227	77,640	67,320	70,424	43,987	44,165	190,540
Enrollment conditional on **application** (%)									
URM	14.6	12.9	0.3	-1.5	-1.6	4.4	-1.6	2.0	8.3
	(0.6)	(0.5)	(0.5)	(0.6)	(0.6)	(0.7)	(0.7)	(0.8)	(0.4)
URM × Prop 209	-10.6	-10.6	-2.2	2.8	-1.5	-4.4	-1.3	-4.5	-11.6
	(0.7)	(0.6)	(0.6)	(0.7)	(0.7)	(0.8)	(0.9)	(0.9)	(0.5)
$\bar{Y}$	16.4	14.8	13.0	16.4	18.0	18.7	17.1	17.2	49.6
Obs.	83,559	101,940	91,720	77,804	67,980	72,062	44,031	45,302	190,540
Enrollment conditional on **admission** (%)									
URM	-20.8	-17.9	-17.3	-7.8	-14.2	-12.0	-6.6	-3.5	1.6
	(1.1)	(0.9)	(0.8)	(0.7)	(0.8)	(0.8)	(0.8)	(0.9)	(0.5)
URM × Prop 209	10.9	9.2	10.7	5.2	5.1	6.2	3.2	0.8	-6.3
	(1.5)	(1.3)	(1.2)	(1.0)	(1.1)	(1.1)	(1.1)	(1.1)	(0.6)
$\bar{Y}$	42.7	38.5	24.7	24.1	26.6	27.3	20.8	21.0	59.7
Obs.	28,497	38,849	48,126	52,669	45,891	49,074	36,025	37,155	157,881

Note: This table shows that URM declines in UC admissions and enrollment were larger after Prop 209 when compared to ‘94-95 as a baseline. OLS coefficient estimates of β_0 and $\beta_{'98-99}$ from Equation 2.1, a difference-in-difference model of 1994-1995 and 1998-1999 URM UC freshman California-resident applicants’ UC applications, admissions, and enrollment compared to non-URM applicants after the 1998 end of UC’s affirmative action program. The years 1996-1997 are omitted because some universities preemptively curtailed their affirmative action programs in those years. Models include high school fixed effects and the components of UC’s Academic Index (see footnote 47), and are estimated independently by campus or “Total” (all applicants to any UC campus). Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

A.4 Data Quality

A.4.1 Applicants who Decline to Report Ethnicity

The percent of UC applicants who declined to report ethnicity on their application increased from 4.1 percent in ‘96-97 to 10.5 percent in ‘98-99, potentially challenging the identification of URM applicants.[3] To identify the ethnicity of missing-ethnicity applicants, I estimate a multinomial logistic regression of ethnicity (Asian, Black, Hispanic, and white) on the leave-one-out ethnicity shares of each known-ethnicity applicant for applicants’ first name, middle name, last name, high school, zip code, and Census block, holding out a randomly-selected 10 percent of applicants. I then predict each missing-ethnicity applicant’s likelihood of being each ethnicity, classifying them if their estimated likelihood of being that ethnicity exceeds 75 percent.[4]

In ‘96-97, I find that among the 88 percent of missing-ethnicity applicants whose ethnicity can be classified, 68 percent are white, 29 percent are Asian, 2.5 percent are Hispanic, and 0.6 percent are Black. The URM shares are hardly higher in ‘98-99; of the 87 percent classified, whites and Asians make up 65 and 29 percent, while Hispanics and Blacks make up 4.2 and 1.3 percent. Thus, while the decline in URM reporting incentives may have disproportionately increased non-reporting among URM university applicants (Antman and Duncan, 2015), the very large majority of non-reporters remains non-URM. These results justify the assumption in the baseline analysis that missing-ethnicity applicants are non-URM. No presented result changes statistically or qualitatively if predicted-URM applicants are re-assigned as URM.

A.4.2 National Student Clearinghouse Coverage

Dynarski, Hemelt and Hyman (2015) show that national NSC enrollment coverage at four-year institutions was below 50 percent in 1996, rising to over 80 percent by 2000.[5] Coverage at the somewhat-selective institutions at which UC applicants tended to enroll was much higher. Appendix A in Bleemer (2018*a*) shows that while some California community colleges were not reporting enrollment statistics to NSC by the mid-1990s, only a small number of universities may not have been reporting graduation statistics by 1999 (the earliest year that 1996 applicants could plausibly earn a four-year degree), the largest of which was 2,100-student adult-education-oriented Brandman University. The same trend likely holds for other states; Table A.21 shows that only 6.2 percent of the baseline sample did not have observed enrollment in NSC, some of whom likely enrolled at community colleges before the colleges’ NSC participation (and others who actually choose against postsecondary enrollment).

[3]Throughout this study, applicants are categorized as “Black” if they self-report their ethnicity as “Black/African American”; as “Hispanic” if they self-report as “Chicano/Mexican-American” or “Latino/Other Spanish-American”; and as “Asian” if they self-report their ethnicity as “Chinese/Chinese-American,” “East Indian/Pakistani,” “Japanese/Japanese-American,” “Korean,” “Pilipino/Filipino,” “Thai/Other Asian,” or “Vietnamese”.

[4]Types 1 and 2 error by ethnicity, measured using the 10 percent of hold-outs, are: 13.2% and 15.2% (white), 3.9% and 12.4% (Asian), 0.3% and 55.5% (Black), and 1.2% and 27% (Hispanic). I replace non-reported ethnicity with predicted ethnicities in Figures 2.4(f) and 2.7 to avoid dropping data.

[5]NSC reports that about 4 percent of records are censored due to student- or institution-requested blocks for privacy concerns, and that the only public university in California with censorship greater than 10 percent is UC Berkeley (National Student Clearinghouse Research Center, 2017).

Table A.3: Difference-in-Difference Estimates of **Asian** UC Applicants' Post-1998 Enrollment

	UC Campuses by Selectivity				Comm.		CA	Non-CA	Not in
	Most	Middle	Least	CSU	Coll.	Ivy+	Priv.	Univ.	NSC
Asian	6.5 (0.3)	-1.7 (0.3)	-1.3 (0.2)	-2.1 (0.3)	2.2 (0.3)	0.8 (0.1)	-1.6 (0.2)	-3.6 (0.2)	0.5 (0.2)
Asian × Prop 209	-0.2 (0.4)	0.1 (0.4)	1.5 (0.2)	-0.1 (0.3)	-1.1 (0.3)	0.0 (0.2)	-0.6 (0.3)	0.8 (0.3)	-0.5 (0.2)
$\bar{Y}$	22.6	20.6	6.4	12.7	11.7	2.8	8.8	9.1	5.8
Obs.	150,968	150,968	150,968	150,968	150,968	150,968	150,968	150,968	150,968

Note: Estimates of β_0 and $\beta_{\cdot 98-99}$ from Equation 2.1, an OLS difference-in-difference model of 1996-1999 Asian UC freshman California-resident applicants' enrollment outcomes compared to non-Asian outcomes after the 1998 end of UC's affirmative action program (restricting the sample to non-URM applicants). Outcomes defined as the first institution of enrollment by college or university type within six years of graduating high school, as measured in the NSC. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. "Ivy+" universities include the Ivy League, MIT, Stanford, and the University of Chicago; private and non-CA universities exclude those institutions. Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

A comparison between UC and NSC graduation records suggests that only UC Santa Cruz failed to report a substantial number of earned degrees among the late 1990s graduation cohorts, while a comparison between NSC and UC major reporting (measured by which students earned STEM degrees) shows that NSC routinely captures more than 90 percent of STEM degree attainment at all campuses throughout the period (conditional on degree reporting in both data sets). The six-year graduation and STEM major choice estimates presented in Panel A of Table 2.3 are robust when restricted to NSC records only or to NSC records augmented by only UCSC degrees (see Table A.26). As a result, differential NSC non-reporting by URM applicants is unlikely to explain the observed degree attainment patterns. Moreover, this concern does not extend to the graduate degree estimates; most such degrees are not earned at the same institutions where applicants earned their undergraduate degrees, and NSC coverage was very wide by the time applicants in the sample were earning graduate degrees.

A.5 Differential Impact of Prop 209 on Asian UC Applicants

The baseline difference-in-difference analysis in the main text does not differentiate between groups of non-URM UC applicants, but there is some speculation that affirmative action policies differentially impact Asian applicants relative to white applicants (Arcidiacono, Kinsler and Ransom, 2020). I test for heterogeneity in Prop 209's effect on non-URM students by restricting the UC applicant sample to non-URM students and re-estimating versions of Equation 2.1 with Asian students as the treated group (replacing URM).[6] Table A.3 presents estimates of Prop 209's effect on Asian students' enrollment institutions. The coefficients on Asian students' enrollment at more-selective and selective UC campuses are precisely-estimated zeroes: ending UC's

[6]Table A.18 presents descriptive statistics for white and Asian UC applicants before and after Prop 209, with both showing similar admissions trends after 1998.

affirmative action program did not lead to a relative increase in Asian UC applicants' enrollment at those campuses. There is a small measurable enrollment shift from community and private California colleges into non-California universities and the less-selective UC campuses, though the effects' magnitudes are a small fraction of those observed for URM students. Figure A.9 shows that Prop 209 also caused no estimable change in Asian applicants' longer-run wage outcomes relative to other non-URM applicants. I conclude that there is little reason to treat white and Asian applicants as having been differently-treated by Prop 209, conditional on prior academic opportunities and preparation as measured by the components of AI.

A.6 Selection into Application: Reanalyzing Card and Krueger (2005)

Figure 2.8 shows that the annual proportion of URM California high school graduates who applied to some UC campus declined (relative to non-URM applications) after 1998 among both low- and high-AI students. This contrasts with the evidence presented by Card and Krueger (2005) (hereafter CK), who use a difference-in-difference design to show that the annual proportion of URM California SAT-takers who send their scores to UC campuses – an oft-used proxy for university application, since score-sending is a mandatory component of many universities' applications – declined overall, but remained steady (or perhaps increased) among the high-SAT and/or high-GPA URM test-takers who were competitive candidates for selective university admission.

I reconcile these findings by matching the College Board SAT-takers database – only available for California public high school students, whereas CK includes private high schools – to the UC application database by name, birthdate, and high school.[7] While the College Board data show that more than 90 percent of UC Berkeley or UCLA applicants sent their SAT scores to those campuses, fewer than 60 percent of students who send their SAT scores to each of those campuses actually apply to them. This suggests that SAT-sending may be a poor proxy for university application in some contexts.

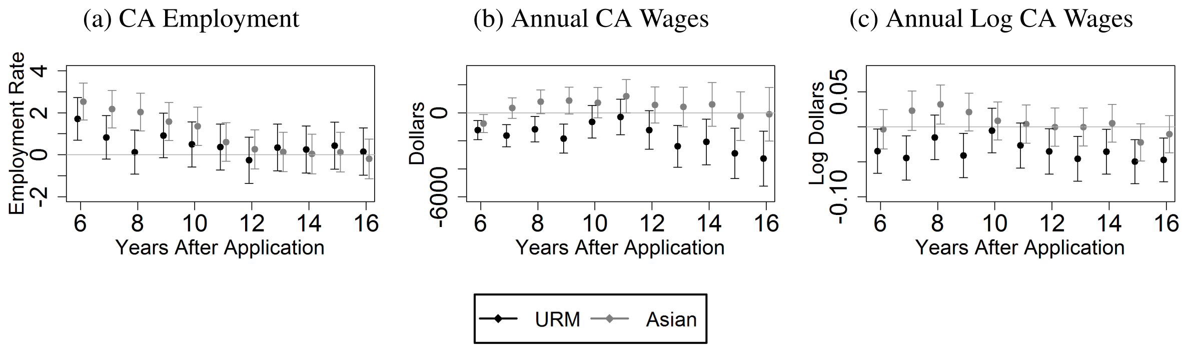
Table A.4 shows that among students at all California high schools (reported by CK) or at public California high schools, California URM SAT-takers who reported A and A+ average high school grades were no less likely to send their scores to any UC campus or to the more-selective Berkeley and UCLA campuses after 1998 relative to non-URM SAT-takers; indeed, URM send rates increased in 1995 and 1996 and only slightly declined in 1998. However, the pattern in actual university applications appears quite different: high-GPA URM students' relative likelihood of UC and Berkeley/UCLA application declined sharply in 1996 – when the application deadline was only a few months after the passage of Prop 209 – recovered in 1997, and then sharply (and somewhat-persistently) declined again in 1998 when the proposition went into effect. Models restricted to high-SAT test-takers reveal a similar pattern.[8]

[7]The match rate of public-HS SAT-submitting freshman UC applicants to the College Board – matching any six of the seven pieces of available information (three names, three birthdate components, and high school) and dropping a small number of possible duplicate matches – is 93 percent among 1994-2001 applicants.

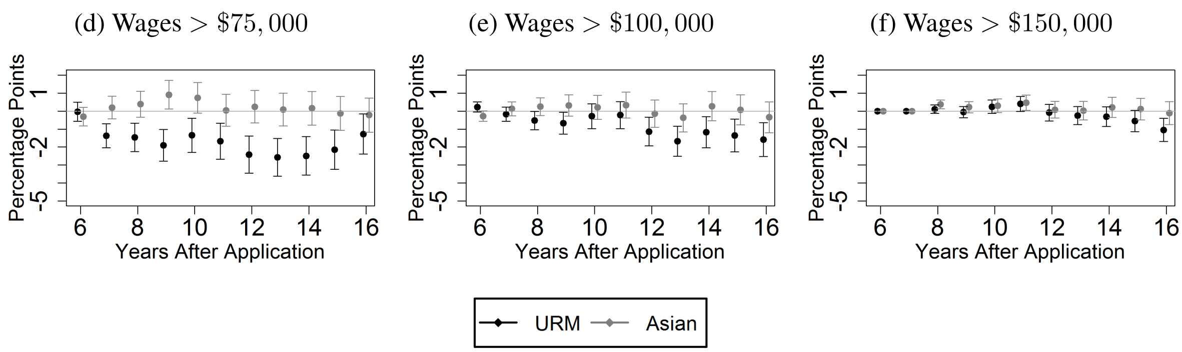
[8]See Tables A.5 and A.6. Table A.7 shows that score-sending to Berkeley and UCLA became a poor proxy for URM students' applications to those schools in 1996 (and worse still in 1999), when URM score-senders across the SAT distribution became less likely to apply to either, though after 1998 it became a particularly poor proxy for

Figure A.9: Difference-in-Difference Estimates of Asian and URM UC Applicants' Post-1998 Wage Outcomes

Panel A: Employment and Wages



Panel B: Minimum Wage Thresholds



Note: This figure shows simultaneous difference-in-difference estimates for URM and Asian labor market outcomes relative to white students, showing that Asian students' long-run labor market outcomes closely-tracked white students' outcomes while URM students' outcomes deteriorated. Estimates of $\beta_{\cdot 98-99}$ from an extension Equation 2.1 adding indicators for Asian students and Asian interacted with post-209 ($\beta'_{1998-1999}$), an OLS difference-in-difference model of 1996-1999 URM and Asian UC freshman California-resident applicants' educational outcomes compared to other non-URM students' outcomes after the 1998 end of UC's affirmative action program. Outcomes defined as non-zero California wages ("CA Employment"), California wages in dollars and log-dollars (omitting 0's), and unconditional indicators for having wages above specified wage thresholds ($75,00, $100,000, and $150,000) 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 footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. Annual wages CPI-adjusted to 2018 and winsorized at top and bottom 1 percent. Robust 95-percent confidence intervals shown. Source: UC Corporate Student System and the California Employment Development Department.

In total, URM UC relative application rates declined by 1.9 percentage points between 1998 and 2000 (relative to 1994-1995), and relative application rates to the Berkeley and UCLA campuses declined by 1.8 percentage points. These patterns are consistent with Figure 2.8, which shows a decline in high-AI URM application rates, and suggests that academically-strong URM

low-SAT students.

Table A.4: Replication of Table 4 in Card and Krueger (2005) with New Specifications: *"Changes in the Relative Probability that Minority Students Send SAT Scores to Selective and Most Selective State Universities"*

	All UC Campuses			Berkeley and UCLA Only		
Dep. Var.:	Send	Send	Apply	Send	Send	Apply
URM × 1995	0.021	0.009	-0.002	0.023	0.011	-0.008
	(0.010)	(0.012)	(0.014)	(0.012)	(0.014)	(0.013)
URM × 1996	0.027	0.016	-0.029	0.030	0.015	-0.035
	(0.010)	(0.012)	(0.013)	(0.011)	(0.014)	(0.013)
URM × 1997	0.028	0.015	-0.006	0.037	0.029	-0.007
	(0.009)	(0.011)	(0.013)	(0.011)	(0.013)	(0.013)
URM × 1998	0.025	0.009	-0.028	0.029	0.011	-0.032
	(0.009)	(0.011)	(0.013)	(0.011)	(0.013)	(0.013)
URM × 1999	0.032	0.015	-0.019	0.026	0.013	-0.032
	(0.009)	(0.011)	(0.013)	(0.011)	(0.013)	(0.013)
URM × 2000	0.033	0.013	-0.038	0.039	0.017	-0.037
	(0.009)	(0.011)	(0.013)	(0.011)	(0.013)	(0.013)
URM × 2001	0.036	0.006	-0.002	0.045	0.025	-0.001
	(0.009)	(0.011)	(0.012)	(0.011)	(0.013)	(0.012)
CK Controls[1]	X	X	X	X	X	X
A/A+ GPA Only	X	X	X	X	X	X
Public HS Only		X	X		X	X
Source	CK	Replication		CK	Replication	
Average(1999-2001) - Average(1994-1995)[2]						
Estimate	0.018	0.006	-0.019	0.019	0.013	-0.018
(Std. Err.)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
Obs.	-	179,682	179,682	-	179,682	179,682

Note: Difference-in-difference OLS regression coefficient estimates across all California 1994-2001 SAT-takers (or restricted to those from public high schools) of URM students' likelihood of either sending SAT scores or applying to any UC campus or the Berkeley and UCLA campuses, relative to 1994 and non-URM students. Models correspond to columns (3) and (6) in Card and Krueger (2005), with the sample restricted to SAT-takers who report A or A+ high school average grades. Test-taking and applicant records merged by name, birthdate, and high school. **1** "CK Controls" include indicators by year, ethnicity, SAT score category (< 1150, $1150 - 1300$, and > 1300), father's and mother's education, reported high school GPA (A or A+), and 8 class rank indicators (including missing). **2** Estimates from CK include 1994-1996 instead of 1994-1995, but the results suggest that URM application rates began falling in 1996 (following the passage of SP-1 and Prop 209). Standard errors (in parentheses) are robust. Source: College Board and UC Corporate Student System.

students were dissuaded from UC application by Prop 209 despite sending their SAT scores to UC campuses (which they may have done many months earlier, on the day they took the test).

Table A.5: Replication of Card/Krueger (2005), Table 4, for All UC Campuses

	Any UC Campus						
	Send	Apply	Send	Apply	Send	Apply	Apply
URM × 1995	0.005	-0.012	0.002	-0.007	0.009	-0.002	-0.004
	(0.004)	(0.004)	(0.013)	(0.015)	(0.012)	(0.014)	(0.013)
URM × 1996	-0.002	-0.033	0.016	-0.012	0.016	-0.029	-0.032
	(0.004)	(0.004)	(0.013)	(0.015)	(0.012)	(0.013)	(0.013)
URM × 1997	-0.010	-0.040	0.011	-0.026	0.015	-0.006	-0.008
	(0.004)	(0.004)	(0.013)	(0.015)	(0.011)	(0.013)	(0.013)
URM × 1998	-0.019	-0.044	-0.010	-0.054	0.009	-0.028	-0.029
	(0.004)	(0.004)	(0.013)	(0.015)	(0.011)	(0.013)	(0.013)
URM × 1999	-0.020	-0.049	0.001	-0.027	0.015	-0.019	-0.022
	(0.004)	(0.004)	(0.013)	(0.015)	(0.011)	(0.013)	(0.013)
URM × 2000	-0.022	-0.047	0.012	-0.030	0.013	-0.038	-0.040
	(0.004)	(0.004)	(0.012)	(0.015)	(0.011)	(0.013)	(0.013)
URM × 2001	-0.028	-0.038	0.004	-0.014	0.006	-0.002	-0.006
	(0.004)	(0.004)	(0.012)	(0.014)	(0.011)	(0.012)	(0.012)
CK Controls[1]	X	X	X	X	X	X	X
Pred. Eth.							X
Sample	Full		High SAT		High GPA		
R^2	0.20	0.31	0.12	0.18	0.09	0.17	0.17
N	891,254	891,254	208,765	208,765	179,682	179,682	179,682

Note: This table shows that while the proportion of competitive URM applicants sending their SAT scores to UC only slightly declined after Prop 209, there is a larger decline in actual URM applications to those schools, suggesting that score-sending is a poor proxy in this context. Difference-in-difference OLS regression coefficient estimates across all California 1994-2001 public-HS SAT-takers of URM students' likelihood of either sending SAT scores or applying to any UC campus, relative to 1994 and non-URM students. Models are either unrestricted, restricted to SAT-takers with scores above 1150, or restricted to SAT-takers who report A or A+ GPAs, following the first three columns of Table 4 of Card and Krueger (2005). Test-taking and applicant records merged by name, birthdate, and high school. The final column augments reported ethnicity by predicting the ethnicities of non-reporters using name and high school; see Appendix A.4 for details. Standard errors (in parentheses) are robust. [1] "CK Controls" include indicators by year, ethnicity, SAT score category (< 1150, $1150 - 1300$, and > 1300), father's and mother's education, reported high school GPA (A or A+), and 8 class rank indicators (including missing). Source: College Board and UC Corporate Student System.

A.7 Course Performance and Persistence at Berkeley after Prop 209

Section 2.7 shows that the STEM performance and persistence of URM students across five UC campuses does not improve following Prop 209, despite those students' enrollment at less-selective campuses. Following previous literature, I also test whether the persistence and performance of URM students at UC Berkeley – the campus where Prop 209 most impacted URM students' likelihood of admission – improved after 1998, when Prop 209 caused a decline in the URM share of the student body by more than half. I restrict the sample to 1996-1999 Berkeley students and estimate Equation 2.3 with and without academic covariates (α_{h_i} and X_{iy}). The last column of Table A.8 shows that before Prop 209, Berkeley's URM students earned lower average grades by 0.84 grade points and were 19 percentage points less likely to persist along

Table A.6: Replication of Card/Krueger (2005), Table 4, for UC's Most-Selective Campuses

	Berkeley and UCLA								
	Send	Apply	Send	Apply	Send	Apply	Send	Apply	Apply
URM × 1995	0.002	-0.004	0.000	-0.013	0.011	-0.008	-0.006	-0.018	-0.019
	(0.004)	(0.003)	(0.016)	(0.015)	(0.014)	(0.013)	(0.011)	(0.012)	(0.012)
URM × 1996	-0.005	-0.026	0.024	-0.006	0.015	-0.035	0.002	-0.021	-0.022
	(0.004)	(0.003)	(0.015)	(0.015)	(0.014)	(0.013)	(0.011)	(0.012)	(0.011)
URM × 1997	-0.007	-0.030	0.012	-0.021	0.029	-0.007	-0.004	-0.035	-0.038
	(0.004)	(0.003)	(0.015)	(0.015)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)
URM × 1998	-0.016	-0.032	-0.007	-0.047	0.011	-0.032	-0.007	-0.035	-0.037
	(0.004)	(0.003)	(0.015)	(0.015)	(0.013)	(0.013)	(0.010)	(0.011)	(0.011)
URM × 1999	-0.018	-0.041	-0.005	-0.027	0.013	-0.032	-0.008	-0.075	-0.076
	(0.004)	(0.003)	(0.015)	(0.015)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)
URM × 2000	-0.020	-0.033	0.016	-0.011	0.017	-0.037	-0.006	-0.028	-0.031
	(0.004)	(0.003)	(0.015)	(0.015)	(0.013)	(0.013)	(0.010)	(0.011)	(0.011)
URM × 2001	-0.020	-0.027	0.021	-0.003	0.025	-0.001	0.014	-0.007	-0.007
	(0.004)	(0.003)	(0.015)	(0.015)	(0.013)	(0.012)	(0.010)	(0.011)	(0.011)
CK Controls[1]	X	X	X	X	X	X	X	X	X
Pred. Eth.									X
	Full		High SAT		High GPA		AI 5500-7000		
R^2	0.24	0.30	0.21	0.23	0.17	0.21	0.12	0.11	0.11
N	891,254	891,254	208,765	208,765	179,682	179,682	212,133	212,133	212,133

Note: This table shows that while the proportion of competitive URM applicants sending their SAT scores to Berkeley and UCLA only slightly declined after Prop 209, there is a larger decline in actual URM applications to those schools, suggesting that score-sending is a poor proxy in this context. Difference-in-difference OLS regression coefficient estimates across all California 1994-2001 public-HS SAT-takers of URM students' likelihood of either sending SAT scores or applying to either UC Berkeley or UCLA, relative to 1994 and non-URM students. Models are either unrestricted, restricted to SAT-takers with scores above 1150, restricted to SAT-takers who report A or A+ GPAs, restricted to SAT-takers with academic indices between 5500 and 7000 (who faced the most-dramatic decline in admissions likelihood at Berkeley and UCLA), following the last three columns of Table 4 of Card and Krueger (2005). Test-taking and applicant records merged by name, birthdate, and high school. The final column augments reported ethnicity by predicting the ethnicities of non-reporters using name and high school; see Appendix A.4 for details. Standard errors (in parentheses) are robust. [1] "CK Controls" include indicators by year, ethnicity, SAT score category (< 1150, $1150 - 1300$, and > 1300), father's and mother's education, reported high school GPA (A or A+), and 8 class rank indicators (including missing). Source: College Board and UC Corporate Student System.

STEM course sequences. These gaps are broadly present across most introductory STEM courses. If admissions mismatch is a primary cause of these large ethnicity gaps, then Prop 209 would be expected to sharply narrow them. In fact, Prop 209 does lead Berkeley's (higher-testing) URM students to earn slightly higher STEM grades (by 0.18 grade points), but if anything their STEM persistence slightly declined.

Panel B of Table A.8 adds academic covariates and shows that, as was the case across the five UC campuses, cross-high-school and *AI* differences wholly explain URM students' low persistence and performance before Prop 209; in the period when Berkeley was implementing affirmative action, URM students earned similar grades and were (if anything) **more** likely to persist in some of Berkeley's STEM fields than their academically-comparable non-URM peers. Unlike at those other campuses, however, ending affirmative action led to relative **declines** in URM students' persistence and (perhaps) performance across most STEM courses. Why would

Table A.7: The Relationship between SAT Send Rates and Most-Selective UC Application

		Coef.	St. Err.	p			Coef.	St. Err.	p
Send		0.371	(0.003)	0.000	Send×SAT		0.189	(0.003)	0.000
URM		0.020	(0.002)	0.000	URM×SAT		0.007	(0.002)	0.000
Norm. SAT		-0.001	(0.001)	0.371	Send×URM×SAT		-0.035	(0.006)	0.000
Send×URM		0.023	(0.006)	0.000					
Indicator	1995	-0.001	(0.001)	0.479	Send×URM×	1995	-0.005	(0.009)	0.572
	1996	0.002	(0.001)	0.155		1996	-0.032	(0.009)	0.000
	1997	0.003	(0.001)	0.015		1997	-0.041	(0.009)	0.000
	1998	0.002	(0.001)	0.027		1998	-0.042	(0.009)	0.000
	1999	0.008	(0.001)	0.000		1999	-0.058	(0.009)	0.000
	2000	0.007	(0.001)	0.000		2000	-0.052	(0.009)	0.000
	2001	-0.003	(0.001)	0.011		2001	-0.045	(0.009)	0.000
Send×	1995	0.032	(0.005)	0.000	Send×SAT×	1995	0.001	(0.004)	0.886
	1996	0.042	(0.004)	0.000		1996	0.009	(0.004)	0.021
	1997	0.026	(0.004)	0.000		1997	0.016	(0.004)	0.000
	1998	0.030	(0.004)	0.000		1998	0.012	(0.004)	0.001
	1999	0.042	(0.005)	0.000		1999	-0.002	(0.004)	0.619
	2000	0.046	(0.005)	0.000		2000	-0.001	(0.004)	0.773
	2001	0.080	(0.005)	0.000		2001	0.003	(0.004)	0.482
URM×	1995	0.001	(0.003)	0.875	URM×SAT×	1995	0.001	(0.003)	0.682
	1996	-0.004	(0.003)	0.253		1996	-0.001	(0.003)	0.615
	1997	-0.001	(0.003)	0.706		1997	0.001	(0.003)	0.775
	1998	0.000	(0.003)	0.942		1998	-0.003	(0.003)	0.362
	1999	-0.007	(0.003)	0.026		1999	-0.006	(0.003)	0.032
	2000	-0.001	(0.003)	0.849		2000	-0.002	(0.003)	0.484
	2001	0.002	(0.003)	0.434		2001	-0.000	(0.003)	0.965
SAT×	1995	-0.001	(0.001)	0.337	Send×URM×SAT×	1995	0.008	(0.008)	0.320
	1996	0.002	(0.001)	0.139		1996	0.015	(0.008)	0.061
	1997	0.003	(0.002)	0.053		1997	0.004	(0.008)	0.572
	1998	0.007	(0.002)	0.000		1998	0.000	(0.008)	0.959
	1999	0.012	(0.002)	0.000		1999	0.021	(0.008)	0.007
	2000	0.009	(0.002)	0.000		2000	0.021	(0.008)	0.007
	2001	-0.000	(0.001)	0.865		2001	0.029	(0.008)	0.000
CK Controls[1]					X				
R^2					0.51				
N					841,358				

Note: This regression shows that score-sending to Berkeley and UCLA became a poor proxy for URM students' applications to those schools in 1996, when URM score-senders across the SAT distribution became less likely to apply to either, though after 1998 it became a particularly poor proxy for low-SAT students. Quadruple-difference OLS regression of an indicator of applying to either UC Berkeley or UCLA on interactions between score-sending to one of those schools, URM status, normalized SAT score, and year (holding out 1994), restricting the sample to 1994-2001 SAT-takers from California public high schools. All coefficients are from the same regression. Standard errors are robust; p-values report statistical tests from the null hypothesis. [1] "CK Controls" include indicators by year, ethnicity, SAT score category (< 1150, $1150 - 1300$, and > 1300), father's and mother's education, reported high school GPA (A or A+), and 8 class rank indicators (including missing). Source: College Board and UC Corporate Student System.

URM Berkeley students' relative STEM performance and persistence decline after Prop 209, instead of remaining steady as it did across the UC system? Table A.9 shows that the effects of

Prop 209 on URM persistence were tightly-estimated 0's at the other four other observed UC campuses. One hypothesis is that Berkeley's post-209 'holistic review' admissions policy inefficiently targeted under-performing students as a result of its inability to provide direct race-based admissions advantages (Chan and Eyster, 2003; Fryer, Loury and Yuret, 2008). Under that hypothesis, the decline would likely be (partly) absorbed by family background covariates like parental income, education, and occupation; however, adding those covariates does not change the estimated coefficient. An alternative hypothesis is that SAT scores are relatively negatively-biased measures of low-testing URM students' academic preparation, such that Berkeley's selection away from those students causes a decline in URM enrollees' relative overperformance (Vars and Bowen, 1998; Niu and Tienda, 2010*b*). This hypothesis is supported by the finding that the relative decline in URM performance is driven by URM students in the bottom two terciles of SAT scores, with no observed declines among high- or low-GPA high-SAT students (see Table A.9). However, the question remains open for future research.

A.8 Introductory STEM Courses at UC Campuses

Section 2.7 estimates changes in URM UC students' persistence and performance in introductory STEM courses after Prop 209. I identify those introductory courses – four courses in Chemistry (two introductory, two organic), two in Biology, two in Physics, and three in Computer Science – using contemporaneous course catalogs and the student transcript data.[9] I chose these fields because they are uniformly available across campuses, offer similarly-structured introductory course sequences, and are not generally required for non-STEM majors (like Mathematics and Statistics, in which many non-STEM fields often require partial course sequence completion). Some schools had multiple versions of a given introductory course, all of which are included in the analysis. Where schools on quarter systems required three courses in a sequence instead of two, I define the sequence by its first and third courses. Here is the full list:

- Intro. Chem.: UCB CHEM 1A/B, UCD CHEM 2A/C, UCR CHEM 1A/B, UCSC CHEM 1B/C, UCSB CHEM 1A/B
- Organic Chem.: UCB CHEM 3A/B or 112A/B, UCD CHEM 8A/B or 118A/B, UCR CHEM 112A/B, UCSC CHEM 108A/B or 112A/B, UCSB CHEM 6A/B or 107A/B
- Biology: UCB BIO 1B/A, UCD BIO 1A/C, UCR BIO 5A/C, UCSC BIOL 10-12 or 20A/C, UCSB MCDB/EECB/BIOL 1A/4A/5A and 1C/4C/5C/2
- Physics: UCB PHYSICS 8A/B, UCD PHYSICS 1A/B or 5A/C or 7A/C or 9A/C, PHYSICS PHYS 2A/C, UCSC PHYS 5A/C or 6A/C or 7A/B, UCSB PHYS 6A/C
- Computer Science: UCB COMPSCI 61A/B/C, UCD ECOMPSCI 20-or-30/40/50, UCR EEC 10/12/14, UCSC CMPS 12A/B/C-or-101, UCSB CMPSC 10/20/30

Berkeley allowed students to take BIO 1A before BIO 1B, but only 25% of students did so. Berkeley also allowed many students to skip CHEM 1B; persistence to CHEM 1B is defined to include students who complete CHEM 3A or 12A.

[9]Catalogs for UC Berkeley available from the Berkeley Library, and for other campuses from CollegeSource Online.

A.9 Value-Added Statistics

In order to characterize the change in institutional quality faced by URM UC applicants after Prop 209, I estimate university and college value-added statistics for two student outcomes – six-year degree attainment (as measured in the union of NSC and UC records) and average wages 12-16 years after UC application, when most applicants are in their early 30s – using the 1995-1997 sample of UC California-resident freshman fall applicants who enroll at a postsecondary institution. Applicants' early-30s wages are averaged over years in which they have observed EDD-covered wages, and the wages are CPI-adjusted to 2018 and winsorized at the top and bottom one percent. The value-added statistics are estimated using a fixed effect specification:

$$Y_{iy} = \zeta_y + \alpha_{U_i} + X_i + \epsilon_{iy} \tag{A.1}$$

where U_i is the first institution where applicant i enrolled (in NSC) after applying to enroll in y, within six years of y. Value-added coefficients α_U are estimated using year fixed effects ζ_y and three sets of X_i covariates, which are intended to absorb the sample selection bias that arises from applicants' non-random enrollment across postsecondary institutions. First, following Mountjoy and Hickman (2020) ("MH"), I define X_i to include indicators for every combination of UC campuses to which the applicant applied and UC campuses to which they were admitted.[10] Second, I augment this approach by estimating a much higher-dimension version of this model including indicators for every combination of postsecondary institutions to which the applicant applies, proxying application by SAT sends (as in Card and Krueger (2005)) by matching the applicant pool to College Board's SAT database by name and birthdate ("MH+"). This approach limits the sample size to public high school graduates matched in the available College Board data and as a result of the high-dimensionality of applicants' score-send set, with unique sets dropped from the sample. Third, following Chetty et al. (2020*a*) ("CFSTY"), I define X_i to include (15) ethnicity indicators and quintics in both SAT score and family income.[11] I also estimate a version of "CFSTY" value-added statistics for the interaction between institution indicators α_{U_i} and applicant ethnicity: white, Asian, Black, or Hispanic. For interpretative simplicity (and because they already prove too conservative), I do not shrink the value-added coefficients or otherwise account for noise in their estimation.

Value-added coefficients are not calculated for institutions with fewer than 50 in-sample enrollees. Effective sample sizes differ across specification – for example, students who apply and are admitted to a unique set of UC campuses are omitted from "MH" value-added estimation – and wage VA measures omit the 26 percent of applicants with no observable wages 12-16 years after UC application. The total samples for the "CFSTY" value-added measures after omissions are 112,707 for six-year graduation and 82,807 for early-30s wages. More than half of in-sample applicants (66,400) enroll at a UC campus, with the remainder enrolling at CSU campuses (14,800), California community colleges (10,800), and private and out-of-state universities (20,700, with 3,900 at USC and 1,500 at Stanford). The sample size statistics in the tables below

[10] This strategy was first proposed by Dale and Krueger (2002), and is implemented by Mountjoy and Hickman (2020) using applications and admissions to schools in the University of Texas system.

[11] Chetty et al. (2020*a*) measure incomes in age-specific rank instead of dollars. I include a dummy for applicants without observed family income – winsorizing family income at the top and bottom 1 percent – but omit the few applicants without observed SAT scores.

show the number of students who enroll at each school and have observable early-30s wages.

In order to evaluate the quality of these estimated value-added statistics, I also estimate a version of Equation A.1 replacing the outcome with applicants' high school GPAs (on a weighted 5 point scale). GPAs are not included as a covariate in any value-added specification, and thus provide a useful placebo to test whether the covariate sets are fully absorbing the sample selection bias that arises from both universities' admissions decisions and applicants' subsequent enrollment choice. Effective value-added statistics should likely largely absorb cross-institution differences in applicants' high school GPAs.

Tables A.10, A.11, and A.12 present "MH" and "CFSTY" value-added coefficients for the full set of available institutions, omitting coefficients with insufficient sample sizes. "CFSTY" coefficients are presented overall and for Hispanic applicants (as well as Black applicants at UC and CSU campuses, where their sample size is sufficiently high). For UC and CSU campuses, I also present an additional series of statistics: "Raw" estimates of α_{U_i} from a version of Equation A.1 with null X_i and estimates of high school GPA "value-added". All value-added coefficients are estimated relative to CSU Long Beach (LB), a high-enrollment teaching-oriented California public university.

Panel A of Table A.10 shows that the students who enroll at UC campuses are 20-40 percentage points more likely to earn a college degree within 6 years than those who enroll at LB. Some of this gap – around 10-15 percentage points in most cases – is absorbed by both sets of covariates, with the "MH" covariates tending to absorb more of the gap. Similarly, the students who enroll at the most-selective UC campuses have higher average early-30s wages than LB enrollees by 25 to 30 thousand dollars, though about half of the gap is absorbed by covariates. UC campuses' wage VA statistics are uniformly lower for Hispanic students, especially at the more-selective campuses, but highly varying for Black students, whose wage VA is above-average at half of UC campuses.

The final columns of Table A.10 show that there is substantial high school GPA variation across UC campuses, with UC Berkeley enrollees having higher average GPAs than UC Santa Cruz enrollees by almost a half of a letter grade. The "MH covariates" fully absorb this variation, while the "CFSTY" covariates absorb only absorb about half of the variation on average, with poorer performance at the more-selective UC campuses. This suggests that "CFSTY" value-added statistics likely still incorporate a degree of sample selection bias, with the coefficients strongly suggesting that the bias is positively correlated with university selectivity. As discussed in the text, this likely implies that the baseline difference-in-difference in URM UC applicants' "CFSTY" institutional value-added measures are somewhat upwardly-biased relative to the actual average difference in average treatment effects across those institutions.

The highest wage VA coefficients among public universities were estimated for the California Polytechnic Institute (Cal Poly), a teaching-oriented university in the CSU system. Panel B of Table A.10 shows that most CSU campuses had degree and wage VA estimates similar to CSU Long Beach, lower than most UC campuses, but that three CSU campuses – Cal Poly, CSU Sacramento, and San José State – appear comparable to UC. Those three also have notably-high ethnicity-specific VA coefficients for Hispanic students. Sample sizes are generally too small to estimate ethnicity-specific VA coefficients for Black students outside of the UC system. Even though the "MH" application and admission partition does not include outcomes at the CSU campuses, the "MH" procedure nevertheless largely eliminates cross-campus average differences in enrollees' high school GPAs, while the "CFSTY" estimates continue to identify some cross-campus GPA variation.

Table A.8: Difference-in-Difference Estimates of URM Berkeley Students' Post-1998 STEM Outcomes

	Chemistry				Biology		Physics		Comp. Science			
	1	2	3	4	1	2	1	2	1	2	3	Combined
					Panel A: Unconditional Difference-in-Difference							
Grade in Course (if earned grade)												
URM	-0.75	-0.96	-0.98	-0.64	-0.93	-0.73	-0.86	-0.63	-0.64	-0.57	-0.00	-0.84
	(0.05)	(0.08)	(0.09)	(0.10)	(0.09)	(0.11)	(0.09)	(0.17)	(0.19)	(0.27)	(0.16)	(0.08)
URM × Prop 209	0.18	0.34	0.26	0.21	0.31	0.09	0.01	-0.02	-0.12	0.03	-0.76	0.18
	(0.08)	(0.14)	(0.15)	(0.17)	(0.14)	(0.21)	(0.15)	(0.27)	(0.31)	(0.41)	(0.45)	(0.08)
$\bar{Y}$	2.85	2.64	2.53	2.74	2.71	2.63	2.69	2.90	2.90	3.05	3.19	2.76
Obs.	4,837	3,339	3,270	2,348	2,392	2,263	2,504	1,307	1,757	1,238	1,139	26,394
Indicator for Persistence to Next Course (%)												
URM	-11.6	-11.4	-23.4		-30.4		-27.1		-25.9	-13.7		-18.6
	(2.6)	(2.6)	(3.3)		(3.9)		(3.8)		(7.4)	(9.2)		(2.8)
URM × Prop 209	-6.1	-5.0	0.1		-5.2		9.6		6.1	1.3		-3.1
	(4.2)	(4.8)	(5.8)		(6.5)		(6.4)		(12.2)	(15.9)		(2.6)
$\bar{Y}$	60.2	87.8	68.5		70.2		48.0		67.9	81.2		68.0
Obs.	4,949	3,393	3,321		2,418		2,542		1,777	1,256		19,656
					Panel B: Conditional on Academic Preparation							
Grade in Course (if earned grade)												
URM	0.15	0.01	0.04	0.14	-0.00	0.23	0.04	-0.05	-0.12	-0.05	0.09	0.05
	(0.05)	(0.10)	(0.10)	(0.13)	(0.09)	(0.12)	(0.10)	(0.20)	(0.22)	(0.28)	(0.22)	(0.05)
URM × Prop 209	-0.13	-0.09	-0.06	-0.04	-0.02	-0.09	-0.14	-0.08	-0.14	-0.19	0.46	-0.04
	(0.07)	(0.15)	(0.16)	(0.21)	(0.13)	(0.21)	(0.15)	(0.35)	(0.32)	(0.61)	(0.52)	(0.04)
Acad. Prep.	X	X	X	X	X	X	X	X	X	X	X	X
$\bar{Y}$	2.85	2.64	2.53	2.74	2.71	2.63	2.69	2.90	2.90	3.05	3.19	2.76
Obs.	4,837	3,339	3,270	2,348	2,392	2,263	2,504	1,307	1,757	1,238	1,139	26,394
Indicator for Persistence to Next Course (%)												
URM	5.8	-4.4	0.1		-0.1		2.2		-8.0	0.4		3.1
	(3.2)	(2.9)	(4.4)		(5.0)		(5.3)		(10.3)	(12.0)		(2.2)
URM × Prop 209	-9.9	-9.4	-12.9		-16.5		1.7		-4.3	-15.3		-10.1
	(4.6)	(5.4)	(6.6)		(7.9)		(8.0)		(15.3)	(20.0)		(2.2)
Acad. Prep.	X	X	X		X		X		X	X		X
$\bar{Y}$	60.2	87.8	68.5		70.2		48.0		67.9	81.2		68.0
Obs.	4,949	3,393	3,321		2,418		2,542		1,777	1,256		19,656

Note: This table shows course-specific and stacked regression coefficients showing evidence of deteriorated unconditional URM course persistence in Chemistry and Biology courses at Berkeley after Prop 209, and widespread deterioration in performance and persistence relative to academically-similar non-URM students. Difference-in-difference OLS regression coefficient estimates across 1996-1999 UC Berkeley CA-resident freshman enrollees' introductory STEM courses, differencing across URM status and post-1998 using Equation 2.3. The final column stacks across courses, weights equally across students, and clusters standard errors by student and course; clustered standard errors may be downward-biased as a result of few clusters (15). Persistence indicates completing the subsequent course in the introductory STEM course sequence; course grade is the grade points received in completed courses. Academic covariates include high school fixed effects and the components of UC's Academic Index (see footnote 47). Standard errors (in parentheses) are robust. The specific courses comprising each sequence can be seen in Appendix A.8; courses taken after the first 2.5 years of matriculation are omitted. Source: UC Corporate Student System and UC-CHP Database (Bleemer, 2018*b*).

Table A.9: Additional Specifications of Difference-in-Difference Models of Science Persistence

	Other Campuses					Restricted Samples, UC Berkeley			
	Santa Barbara	Davis	Santa Cruz	Riverside	Berkeley Add'l Cov.	High SAT Scores High GPA	High SAT Scores Low GPA	Low SAT Scores High GPA	Low SAT Scores Low GPA
URM	1.4	1.0	-3.6	0.6	6.1	-5.3	-4.9	7.3	12.4
	(4.4)	(2.7)	(1.4)	(2.2)	(2.0)	(4.2)	(4.3)	(7.8)	(2.9)
URM × Prop 209	-0.3	-0.3	2.9	-1.0	-10.0	-5.4	12.6	-9.4	-9.0
	(4.6)	(1.8)	(2.0)	(3.7)	(2.7)	(5.5)	(5.4)	(10.1)	(6.1)
Acad. Prep.	X	X	X	X	X	X	X	X	X
Parental Cov.					X				
$\bar{Y}$	50.1	56.8	60.5	55.7	68.0	76.0	65.0	62.2	49.7
# of Obs.	6,857	29,470	15,149	14,072	19,656	9,808	5,441	1,647	2,712

Note: This table helps to arbitrate between competing explanations for the relative decline in URM Berkeley students' STEM persistence after Prop 209. The table provides evidence against the hypothesis that holistic review negatively-selected URM students, and evidence favoring the hypothesis that the enrollment decline among lower-SAT URM students caused selection away from students whose academic capabilities are underestimated by standardized tests. Difference-in-difference OLS regression coefficient estimates across 1995-2000 UC Berkeley or other UC campus enrollees' introductory STEM courses (excluding out-of-state, transfer, and engineering students), differencing across URM status and post-1998 using Equation 2.3. The outcomes indicates whether the student completes the following course in the specified course sequence; see Appendix A.8. Academic covariates include high school fixed effects and the components of UC's Academic Index (see footnote 47). Parental covariates include parental income (with an indicator for missing income), (289) parental occupation fixed effects, and (7) max parental education fixed effects. The last four columns partition students by whether their high school GPAs and SAT scores are in the top tercile of 1996-1999 URM Berkeley students' grades and scores. Standard errors (in parentheses) are robust. Source: UC Corporate Student System and UC-CHP Database (Bleemer, 2018*b*)

Table A.10: 1995-1997 Value-Added Estimates for Public California Universities

	6-Yr. Grad.			Wages in Early 30s					High School GPA			
	Raw	MH	CFSTY	Raw	MH	CFSTY	CFSTY		Raw	MH	CFSTY	Sample
Inst.	All	All	All	All	All	All	Black	Hisp.	All	All	All	Size
Panel A: University of California System												
Berkeley	34.5	19.8	24.0	30,100	12,900	16,800	3,900	4,400	0.66	0.04	0.37	9,078
Davis	31.7	18.7	22.2	20,800	10,100	12,400	18,100	9,500	0.45	0.02	0.28	5,927
Irvine	29.1	18.0	20.6	14,900	7,200	7,000	16,400	1,300	0.37	0.01	0.21	5,730
UCLA	35.7	20.1	25.8	24,900	8,900	15,000	5,200	4,200	0.61	0.01	0.39	8,271
Riverside	33.2	25.1	28.1	9,000	6,400	4,700	11,700	1,000	0.21	0.01	0.12	1,204
San Diego	36.3	20.4	25.4	21,800	8,400	11,100	15,200	4,800	0.62	0.03	0.38	5,648
Santa Barbara	29.1	19.2	19.6	12,800	7,600	6,900	1,300	-1,400	0.24	-0.00	0.11	8,104
Santa Cruz	21.7	14.6	12.9	-2,600	-1,900	-9,000	-1,100	-10,500	0.19	-0.02	0.04	3,976
Panel B: California State University System												
Cal Poly.	21.8	12.8	12.3	25,600	19,100	19,500	21,800	10,600	0.34	0.06	0.20	2,626
Cal Poly. Pom.	0.5	0.3	-2.8	7,100	6,500	3,800		-1,200	0.02	0.00	-0.03	1,031
Chico	21.3	17.8	12.9	7,800	7,200	2,900		200	0.01	0.03	-0.04	372
Dom. Hills	-8.1	-8.6	0.2	-5,400	-6,400	3,800	-1,400	-1,300	-0.10	-0.15	0.03	137
East Bay	5.6	2.9	4.8	5,700	1,100	5,200	-7,600		0.07	-0.06	0.07	216
Fresno	9.5	4.8	9.3	6,700	2,600	5,000		2,500	0.19	0.03	0.22	311
Fullerton	4.2	5.2	3.7	1,400	1,800	900	2,800	-1,100	-0.05	-0.02	-0.06	835
Long Beach	0.0	0.0	0.0	0	0	0	0	0	0.00	0.00	0.00	1,286
Monteray Bay	10.1	10.8	8.6	-6,700	-2,800	-6,100			-0.10	-0.04	-0.09	60
Northridge	-3.8	-4.1	-2.3	-900	-700	-700	-5,600	-3,400	-0.09	-0.05	-0.05	995
Sacramento	5.3	2.1	2.4	13,000	8,800	10,200		9,100	0.11	-0.00	0.06	453
San Bern.	-0.8	-1.0	1.8	100	1,900	3,900		0	-0.01	0.00	0.03	270
San Marcos	2.4	0.4	-0.3	-3,800	-4,100	-6,400		-3,800	0.08	0.00	0.07	112
Stanislaus	8.1	2.9	2.9	7,800	3,500	5,900			0.20	0.01	0.13	69
Humboldt St.	2.3	-1.2	-5.0	-11,300	-10,900	-15,300			0.10	0.02	-0.02	204
San Diego St.	3.4	2.2	1.4	400	-300	500	1,000	-3,800	-0.02	-0.01	-0.04	1,677
San Fran. St.	-0.1	-0.3	-3.9	3,000	1,300	300	-4,100	-2,200	-0.03	-0.05	-0.07	918
San Jose St.	-0.5	-1.0	-3.1	16,800	14,700	13,800	-6,300	14,700	-0.03	-0.04	-0.05	728
Sonoma St.	11.4	7.8	0.4	-5,100	-7,400	-8,600			0.06	-0.01	-0.03	88

Note: This table shows value-added estimates for the University of California and California State University public university systems. Value-added estimates from Equation A.1 using 1995-1997 UC CA-resident freshman fall applications. See text for outcome definitions and covariate definitions "MH" (following Mountjoy and Hickman (2020)) and "CFSTY" (following Chetty et al. (2020*a*)). "Raw" coefficients estimated with null X_i. Ethnicity-specific coefficients estimated by interacting U_i with five ethnicity buckets: white, Black, Hispanic, Asian, and other. Sample size for "CFSTY" wage value-added coefficients. Estimates are not shrunk or otherwise adjusted for noise. Source: UC Corporate Student System, National Student Clearinghouse, and the CA Employment Development Department.

Table A.11: 1995-1997 Value-Added Estimates for California Community Colleges

	6-Yr. Grad.		Wages in Early 30s					6-Yr. Grad.		Wages in Early 30s			
	MH	CFSTY	MH	CFSTY		Samp.		MH	CFSTY	MH	CFSTY		Samp.
Inst.	All	All	All	All	Hisp.	Size	Inst.	All	All	All	All	Hisp.	Size
Allan H.	-17.6	-13.5	-6,100	-3,300		61	LA Valley	-20.0	-17.0	-300	-1,400		51
Am. River	-17.1	-16.9	-7,300	-5,000		85	MiraCosta	-2.7	-1.8	5,100	500		86
Cabrillo	-25.6	-29.0	7,700	9,200		63	Moorpark	-5.7	-8.3	6,300	4,800		168
Canada	5.9	0.0					Mt. SA	-14.5	-13.9	-2,000	-3,900	-7,500	451
Cerritos	-21.1	-15.6	-4,200	-2,300	-10,100	185	Mt. SJ	-15.6	-13.4	1,600	2,600		69
Chabot	-1.8	-1.1	7,900	8,800	2,600	174	Ohlone	-9.0	-12.3	16,600	13,400		94
Chaffey	-20.3	-17.3	-12,100	-9,000	-4,800	81	Or. Coast	-31.2	-34.1	-12,200	-16,900		65
SF	2.8	-0.5	6,900	4,300	-9,200	405	Palomar	-11.1	-13.9	-4,100	-7,700		105
San Mateo	1.7	-2.6	17,300	15,200		259	Pasadena	-14.6	-15.0	-3,100	-6,100	-13,200	369
C. of Des.	-18.5	-9.4	-1,100	6,400	6,400	67	Riverside	-11.6	-5.1	1,500	3,100	-800	583
Cuesta	-14.4	-18.2	400	-1,400		129	Sac.	-15.4	-10.0	-200	2,800		174
Cypress	-14.5	-14.5	-2,700	-7,200		112	Saddleback	-7.0	-11.6	5,500	2,600		213
De Anza	-0.6	-2.4	15,000	12,600	13,700	651	SB Valley	-2.8	6.7	2,300	6,000	700	77
Diab. Vall.	0.5	-3.3	9,300	8,700	1,400	478	SD	-26.0	-26.3	-18,400	-17,100		56
East LA	-32.5	-23.3	-9,700	-6,300	-12,500	50	SD Mesa	-13.0	-12.4	-1,100	-2,400	-8,000	295
El Camino	-18.1	-16.4	-6,000	-5,400	-7,700	308	SD Mir.	-11.2	-10.8	3,000	1,700		75
Foothill	-3.6	-5.1	10,000	9,500		258	SJ Delta	-20.3	-22.0	-3,500			
Fresno	-23.4	-23.3	-13,500	-14,800		87	Santa Ana	-18.8	-17.9	-5,200	-3,100	-7,700	156
Fullerson	-12.0	-11.7	-5,800	-7,800	-11,200	154	S. Barb.	-28.9	-33.9	-8,100	-10,700		72
Hartnell	-14.4	-7.5	4,400	5,700	6,600	56	S. Monica	-12.7	-12.9	-1,000	600	-9,200	671
Irv. Vall.	-11.6	-17.3	1,200	-1,900		213	S. Rosa	-6.5	-8.9	-5,000	-4,200		91
Laney	-4.2	-3.8	4,500	4,100		86	Sierra	-14.8	-15.7	-2,900	-2,600		108
Las Positas	-10.8	-14.3	6,600	7,800		55	Skyline	4.0	2.0	17,900	18,000		141
L. Beach	-20.4	-18.9	-2,900	-1,900	-7,600	184	Solano	-4.4	0.2	28,100	31,400		52
LA Pierce	-15.2	-17.1	-4,600	-8,400		75	Ventura	-15.0	-9.6	-3,500	-2,500	-2,100	101

Note: This table shows value-added estimates for estimable California Community Colleges. Value-added estimates from Equation A.1 using 1995-1997 UC CA-resident freshman fall applications, excluding colleges with fewer than 50 in-sample enrollees (or 30 enrollees for ethnicity-specific estimates). See text for outcome definitions and covariate definitions "MH" (following Mountjoy and Hickman (2020)) and "CFSTY" (following Chetty et al. (2020*a*)). Ethnicity-specific coefficients estimated by interacting U_i with five ethnicity buckets: white, Black, Hispanic, Asian, and other. Sample size for "CFSTY" wage value-added coefficients. Estimates are not shrunk or otherwise adjusted for noise. Source: UC Corporate Student System, National Student Clearinghouse, and the CA Employment Development Department.

Table A.12: 1995-1997 Value-Added Estimates for Private and Out-of-State Universities

	6-Yr. Grad.		Wages in Early 30s					6-Yr. Grad.		Wages in Early 30s			
	MH	CFSTY	MH	CFSTY		Samp.		MH	CFSTY	MH	CFSTY		Samp.
Inst.	All	All	All	All	Hisp.	Size	Inst.	All	All	All	All	Hisp.	Size
American	32.4	27.5	27,500	22,500		52	Pitzer	30.6	31.3	-800	-2,100	-3,400	113
Arizona	6.7	-0.2	7,900	3,600		101	P. L. Naz.	20.9	16.7	-6,900	-9,300		87
AZ State	22.3	21.0					Pomona	28.9	32.9	13,400	14,200	6,200	299
Asuza Pac.	25.6	25.8	-2,300	-600		84	Port. State	1.2	-0.6				
Biola	24.2	23.3	-14,500	-15,300		101	Princeton	32.3	35.9	36,700	35,800		166
Boston C.	-20.8	-20.0	12,500	13,100		127	Rice	10.3	12.6				
Boston U.	23.2	20.9	3,200	300		245	St. Mary's	26.4	25.3	11,700	12,700	4,300	333
Brandeis	26.8	28.3	8,500	7,800		59	Santa Clara	32.2	31.7	31,000	31,400	27,700	545
BYU	-10.3	-11.2	400	2,200		159	Scripps	28.4	28.3	3,700	-2,300		92
Bryn Mawr	27.8	30.4					S. Meth.	26.3	23.3				
CA Luth.	24.3	23.0	12,400	7,400		87	Spelman	34.2	46.0			-7,300†	32
Carleton	28.4	29.1					Stanford	28.2	32.0	37,100	36,800	23,300	1,116
CMU	19.7	18.8					Swarthmore	33.1	35.7				
Clar. Mc.	28.3	30.4	27,700	25,900	11,800	239	Syracuse	30.5	30.0	19,300	20,600		113
CO State	24.8	21.3	6,700	4,400		50	Tufts	28.9	29.8	4,900	500		80
Columbia	23.9	27.6	12,000	12,700		189	Tulane	28.9	27.6	20,000	17,500		80
Cornell	26.3	28.8	18,300	19,200		320	Colorado	24.9	20.2	17,700	14,900		472
Creighton	26.7	24.0	26,800	22,400		59	Michigan	30.2	30.9	29,500	31,800		99
Dartmouth	-57.8	-55.5	26,500	24,600		119	Nevada	10.8	8.5				
Duke	-21.2	-18.7	40,300	42,900		167	Oregon	26.2	18.6	2,100	-6,400		253
Georgetown	29.3	33.3	37,400	40,300	18,100	169	U. Penn.	28.0	30.7	38,200	39,700		271
Gonzaga	26.5	25.7					Puget Sound	24.6	21.9	700	-5,600		90
Harvard	-37.2	-32.9	20,100	19,000		89	Redlands	28.6	29.2	-700	-2,700	1,900	157
H. Mudd	24.5	26.7	27,500	27,200		109	USF	27.2	24.3	12,100	12,600	9,500	460
J. Hopkins	22.1	25.3	25,500	26,100		121	USC	20.8	21.7	17,400	18,100	5,900	3,192
La Sierra	4.9	8.0	-100	-4,600		75	U. Pacific	24.2	25.5	26,100	26,300	7,000	421
Lew. & Clk.	30.7	25.6	-2,400	-12,100		62	Virginia	32.6	33.2				
Loyola M.	22.0	21.6	11,700	12,700	9,800	853	Washington	24.9	25.7				
Mills	29.3	27.6	-9,200	-10,400		72	Wisconsin	24.0	23.3	5,800	3,500		106
Mt. Holyoke	-48.8	-48.8					Vanderbilt	28.4	29.7	16,800	19,200		101
Mt. St. M.	23.8	28.2	4,300	6,800	1,900	129	Wash. In SL	21.8	24.8				
NYU	23.2	21.8	-7,700	-10,500		242	Wellesley	30.0	33.9	9,100	12,000		88
N. Arizona	24.7	17.0	4,500				Wesleyan	34.7	34.2				
Northwest.	24.4	27.5	20,100	20,900		210	Westmont	-42.6	-44.4	-8,300	-12,000		123
Oberlin	0.9	-0.1					Whitman	32.7	33.1				
Occidental	33.6	34.5	1,800	3,900	-4,100	194	Whittier	26.2	29.3	6,900	9,600	5,600	147
Penn. State	21.8	17.5					Williams	33.0	35.1				
Pepperdine	29.3	27.3	4,700	6,000	3,200	316	Yale	29.0	33.8	39,100	39,300	13,400	260

Note: This table shows value-added estimates for all estimable private and non-California colleges and universities. Value-added estimates from Equation A.1 using 1995-1997 UC CA-resident freshman fall applications, excluding colleges with fewer than 50 in-sample enrollees (or 30 enrollees for ethnicity-specific estimates). See text for outcome definitions and covariate definitions "MH" (following Mountjoy and Hickman (2020)) and "CFSTY" (following Chetty et al. (2020*a*)). Ethnicity-specific coefficients estimated by interacting U_i with five ethnicity buckets: white, Black, Hispanic, Asian, and other. Sample size for "CFSTY" wage value-added coefficients. Estimates are not shrunk or otherwise adjusted for noise. † Spelman is a historically Black college; this estimate is for Black students. Source: UC Corporate Student System, National Student Clearinghouse, and the CA Employment Development Department.

Table A.13: Comparison Between Various Value-Added Estimates and Student Outcomes for Matched Samples

	"MH" VA[1]				"MH+" VA[1]				"CFSTY" VA[1]				Eth.-Specific "CFSTY" VA[1]			
	Six-Year Deg.		Early-30s Wage		Six-Year Deg.		Early-30s Wage		Six-Year Deg.		Early-30s Wage		Six-Year Deg.		Early-30s Wage	
	VA	Obs.	VA	Obs.	VA	Obs.	VA	Obs.	VA	Obs.	VA	Obs.	VA	Obs.	VA	Obs.
Panel A: Difference-in-Difference Coefficients																
URM	2.0	-2.8	1,860	-786	3.0	-3.2	2,378	-1,010	2.8	-2.9	2,818	-805	1.7	-2.2	1,359	-808
	(0.1)	(0.4)	(83)	(573)	(0.1)	(0.5)	(84)	(633)	(0.1)	(0.4)	(94)	(574)	(0.1)	(0.4)	(91)	(601)
URM × Prop 209	-0.6	-0.5	-447	-2,239	-1.2	0.0	-1,032	-2,039	-1.0	-0.5	-952	-2,243	0.1	-0.1	57	-2,115
	(0.2)	(0.5)	(102)	(691)	(0.2)	(0.6)	(104)	(765)	(0.2)	(0.5)	(115)	(692)	(0.2)	(0.5)	(110)	(723)
Obs.	177,365	177,365	136,237	136,237	145,690	145,690	112,205	112,205	176,092	176,092	136,032	136,032	169,534	169,534	129,477	129,477
Panel B: Estimates of URM × Prop 209 ($\beta_{\cdot 98-99}$) by AI Quartile																
Bottom Quartile	-1.6	-3.6	-591	-2,152	-2.3	-3.7	-883	-1,169	-1.9	-3.6	-734	-2,152	-1.1	-3.1	97	-1,485
	(0.4)	(1.6)	(235)	(1,579)	(0.5)	(1.8)	(262)	(1,797)	(0.5)	(1.6)	(270)	(1,582)	(0.5)	(1.7)	(288)	(1,685)
Second Quartile	-0.5	-0.7	-448	-1,384	-1.4	-0.1	-1,493	-316	-1.3	-0.6	-1,269	-1,382	0.2	0.0	454	-1,512
	(0.4)	(1.3)	(219)	(1,450)	(0.4)	(1.4)	(232)	(1,585)	(0.4)	(1.3)	(264)	(1,451)	(0.4)	(1.3)	(253)	(1,500)
Third Quartile	0.1	1.8	-468	-2,160	-0.7	2.1	-1,291	-2,648	-0.4	1.9	-1,372	-2,117	0.9	1.9	85	-1,899
	(0.3)	(1.1)	(202)	(1,451)	(0.3)	(1.2)	(206)	(1,598)	(0.3)	(1.1)	(242)	(1,452)	(0.3)	(1.1)	(219)	(1,515)
Top Quartile	-0.8	-0.1	-387	-2,637	-0.5	0.4	-726	-2,624	-1.0	-0.3	-708	-2,641	0.1	-0.3	284	-2,517
	(0.3)	(0.9)	(248)	(1,648)	(0.2)	(1.0)	(231)	(1,788)	(0.3)	(0.9)	(257)	(1,648)	(0.3)	(0.9)	(223)	(1,707)

Note: This figure tests the performance of several institution and institution-gender-ethnicity value-added estimates against actual changes in student outcomes after Prop 209, with some measures performing relatively-well in measuring degree attainment but all measures generally underestimating (and poorly explaining the patterns in) declines in early-30s wages. Estimates of β_0 and $\beta_{\cdot 98-99}$ from Equation 2.1, a difference-in-difference 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 estimated value-added of the first two- or four-year institution at which the applicant enrolled within six years of UC application as measured in the NSC, or actual student outcomes matching the value-added measures: six-year Bachelor's degree attainment or average conditional California wages between 12 and 16 years after UC application. Outcome samples are restricted to observations with observed VA (implying that the student first enrolled at an institution with sufficient sample size to estimate VA), and wage VA samples restricted to observations with observed early-30s wages (omitting observations with no California employment in that period, 12-16 years after UC application). Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Robust standard errors in parentheses. [1] Value-added measures are estimated by regressing six-year BA attainment (in NSC) or 15-year conditional wages (in EDD) on college indicators, year FEs, and either indicators for each applicant's set of UC campus applications and admissions (following Mountjoy and Hickman (2020), "MH"), indicators for each applicant's complete set of institutions to which they sent their SAT scores (using matched College Board testing data; an extension of Mountjoy and Hickman (2020), "MH+") or ethnicity indicators and quintics in SAT score and family income (following Chetty et al. (2020*a*), "CFSTY") using the 1995-1997 UC applicant pool. Ethnicity-specific coefficients estimated by interacting U_i with five ethnicity buckets: white, Black, Hispanic, Asian, and other. Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department.

Table A.11 shows that California's community colleges have estimated degree VA below most of the institutions in the UC or CSU systems, but there is substantial variation in community colleges' wage VA estimates, with many colleges having wage VA estimates comparable to CSU or UC campuses. The high-wage-VA community colleges are clustered in the high-wage and high-cost-of-living "South Bay" of northern California, like Ohlone College in Fremont, Skyline College in San Bruno, De Anza in Cupertino, and Foothill College in Los Altos. Though the table does not show it, the estimates show that there is relatively little variation across community colleges in their UC-applicant enrollees' average high school GPAs: the standard deviation of raw average high school GPA coefficients is 0.09 across community colleges, whereas the standard deviation across "MH" estimates of high school GPA is 0.04 (and 0.09 for "CFSTY").

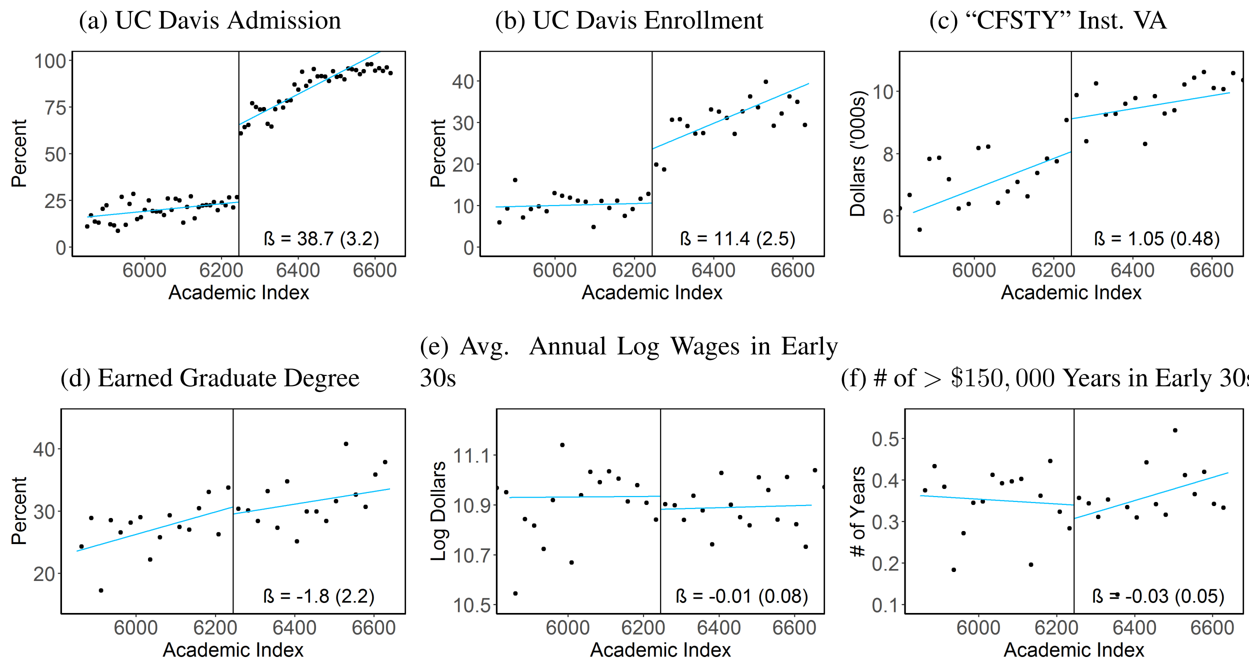
Table A.12 shows that the private and out-of-state universities where UC applicants tend to enroll have degree VA estimates as larger or larger than the UC system, and many have wage VA estimates higher than UC, though there is a great deal of variation.[12] With many of these institutions among the nation's more-selective, Wage VA estimates are highest at many of the nation's more-selective universities, including Ivy League institutions like Princeton, the University of Pennsylvania, and Yale as well as Duke and Stanford. Out-of-state flagship public universities tend to have similar VA estimates to the UC system, while California's less-selective private institutions vary widely, from the high-VA Santa Clara University to lower-VA Mills College (though even the lower-VA California institutions have high degree VA estimates relative to less-selective public institutions). As in the case of the UC campuses, there is substantial variation in average high school GPAs across these institutions (s.d. 0.25), but most is absorbed by "MH" value-added estimates (s.d. 0.08; 0.15 using "CFSTY").

Figure 2.3 shows that Prop 209 tended to shift URM UC students' enrollment from the more-selective UC campuses into the less-selective campuses, CSU campuses, and some private and out-of-state institutions. Students also cascaded out of the moderately and less-selective UC campuses into other institutions, yielding unchanged URM enrollment at all but the more-selective UCs. The estimates presented in these tables specify the way in which these switches led students to enroll at institutions with lower estimated value-added in terms of degree attainment and early-career wages, as summarized in Table 2.2.

There has been minimal quasi-experimental validation of university value-added statistics. I conclude by testing the degree to which value-added measures explain the observed changes in URM applicant outcomes after Prop 209. Table A.13 presents VA and observed degree attainment and early-30s wages for several VA specifications, aligning samples for missing data. It shows that changes in URM applicants' university enrollment's estimated value-added statistics yield relatively-accurate predictions of the decline in degree attainment by AI quartile, but underestimates of the actual changes in observed early-30s wages. The "MH" value-added statistics yield the most compressed distribution of value-added statistics across universities, as would be expected given their near-complete absorption of cross-school variation in high school GPAs, but this yields poorer performance in explaining outcome variation after Prop 209. Allowing gender- and ethnicity-specific VA coefficients (using the "CFSTY" approach) yields precise 0's for the wage VA estimates across all AI quartiles, implying particularly poor performance.

[12] A small number of institutions, like Duke University and Dartmouth College, may have low degree VA estimates as a result of incomplete NSC degree reporting in the sample period.

Figure A.10: Estimated Return to ‘96-97 UC Davis Enrollment for On-the-Margin Non-URM Applicants



Note: This figure shows that on-the-margin 1996-1997 non-URM applicants to UC Davis would have otherwise enrolled at lower-value-added institutions but experienced similar educational and wage outcomes, though interpretation is challenged by the increase in above-threshold students likelihood of applying to Davis. Regression discontinuity plots and estimates around the 1996-1997 UC Davis guaranteed admission AI threshold among non-URM applicants, estimated by local linear regression following Calonico, Cattaneo and Titiunik (2014). See the notes to Tables 2.2, 2.3, and 2.4 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 $AI+(250\times\mathbb{1}_{1997})$ to align thresholds over years. Source: UC Corporate Student System, National Student Clearinghouse, and the CA Employment Development Department.

A.10 Return to UC Davis Enrollment for On-the-Margin Non-URM Applicants

Figures A.1 to A.8 show that only two UC campuses exhibited discontinuities in their applicants’ likelihood of admission before Prop 209 when ordered by AI: the campuses at Berkeley and Davis. As a result, UC Davis’s admissions policies admit a regression discontinuity design that could provide additional evidence, along with Section 2.6, on the return to UC admission for the on-the-margin non-URM students who may gain access to the campus following Prop 209.

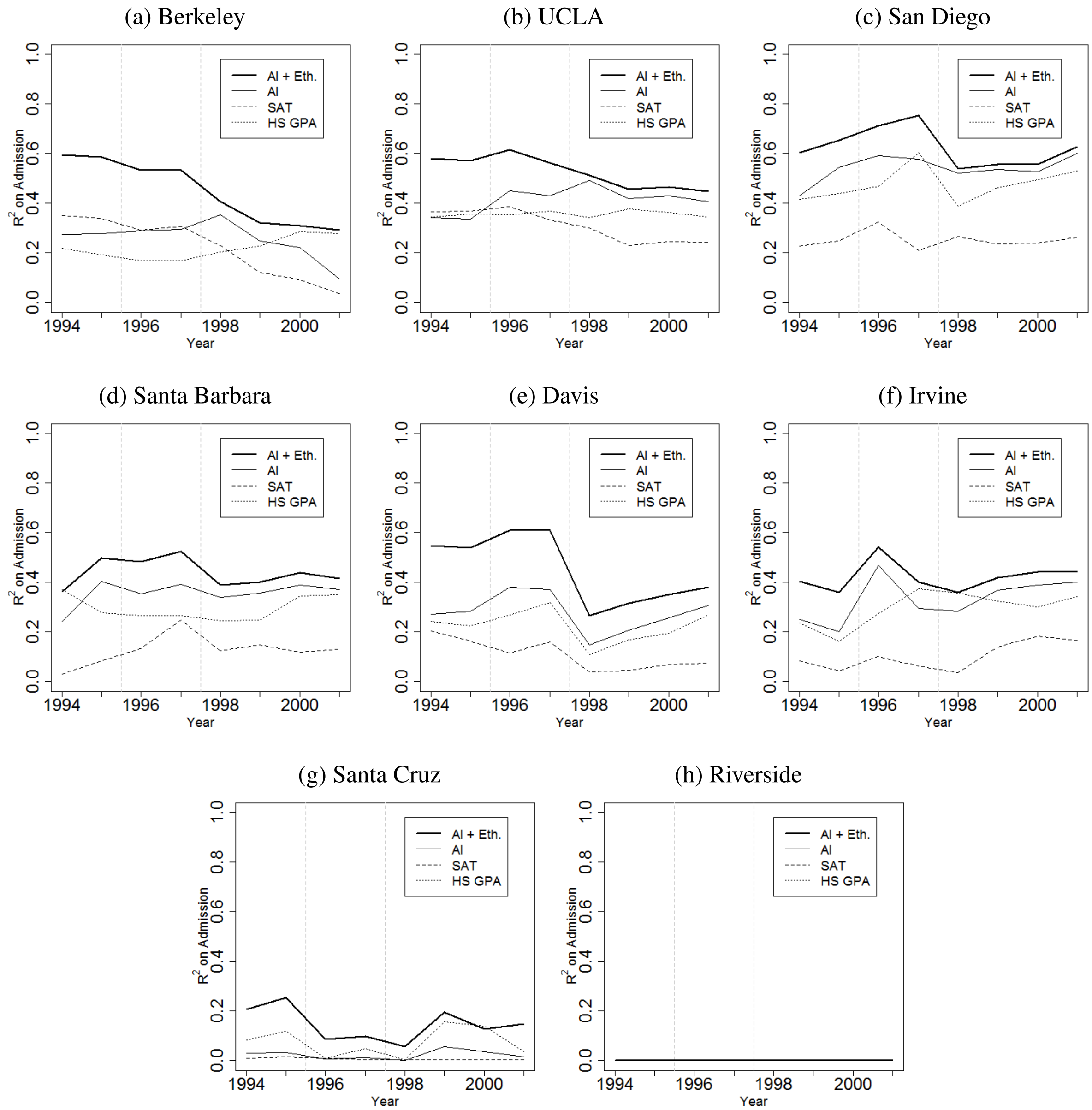
The challenge in interpreting the return to enrollment at UC Davis for on-the-margin non-URM 1996-1997 applicants is that the discontinuities themselves – at exactly 6,000 in 1996 and 6,250 in 1997 – appear to have been known by some applicants. McCrary (2008) tests fail at both thresholds (p=0.016 and p=0.025) as a result of a 13 percent increase in students’ likelihood of applying to UC Davis at the campus’s AI admissions threshold. As in Section 2.6, I test for selection on observables at the UC Davis AI admissions threshold by characterizing each applicant by their expected log wages on the basis of demographic and socioeconomic features and find weak evidence of negative selection above the threshold, with lower predicted wages by 0.025 log points (s.e. 0.020 log points) immediately above the threshold.

Despite these limitations to the research design, Figure A.10 shows how UC Davis's applicants above and below that school's AI admissions threshold differ in terms of educational and employment outcomes. Above-threshold students are 40 percentage points more likely to attend Davis, and excluding a small group of applicants immediately above the threshold, take-up appears to be close to half, with enrollment increases around 20 percentage points. Unlike in the Berkeley context, UC Davis is a higher-value-added institution than on-the-margin applicants' counterfactual enrollments, leading to an estimated \$1,000 increase in wage value-added at the threshold, about four times the average increase in value-added for non-URM enrollees at California public universities after Prop 209 (see Figure A.12). But as in the case of UC Berkeley, enrolling at UC Davis does not generate returns for on-the-margin non-URM students, who are no more likely to earn a graduate degree or earn higher wages if they have access to UC Davis; indeed, all three point estimates are negative (and statistically indistinguishable from 0).

The smoothness of the resulting wage trends suggests that these findings are not just limited to the differentially-selected students close to the eligibility threshold, but also reflect broader negligible treatment effects of access to UC Davis on non-URM student outcomes prior to Prop 209. This evidence further supports the main text's claim that non-URM students on the margin of admission to UC campuses prior to Prop 209 appear to derive small benefits from enrolling at those campuses, particularly in comparison with the estimated costs faced by URM students who lost access to selective universities following Prop 209.

A.11 Other Appendix Figures and Tables

Figure A.11: Annual Explanatory Power of Academic Index and Ethnicity for UC Admission

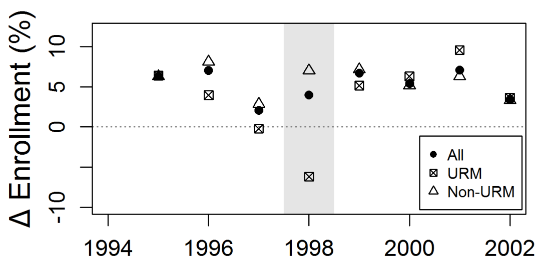


Note: This figure shows that a large share of UC campuses' admissions, especially before 1998 but also after, can be explained strictly by students' Academic Index, with a large additional share explained by ethnicity before 1998. The R^2 coefficients of annual OLS regressions of admission on the leave-one-out likelihood of admission for students with the same Academic Index (AI), SAT score, high school GPA (rounded to the nearest hundredth), or AI and ethnicity, among 'normal' UC freshman fall applicants to each campus. 'Normal' applicants are freshman fall California-resident applicants who (a) were UC-eligible, which means that they satisfactorily completing the required high school coursework, and (b) who selected intended majors that did not have special admissions restrictions (e.g. engineering at some campuses). Figure A.15 shows the differences between the first and second line for each campus. Source: UC Corporate Student System.

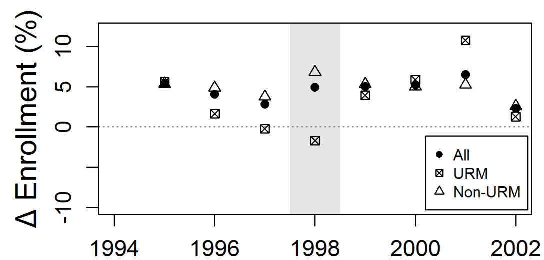
Figure A.12: Annual Changes in Undergraduate Enrollment at California Institutions

Panel A: Annual Change in Freshman Fall Undergraduate Enrollment

(a) Public California Universities

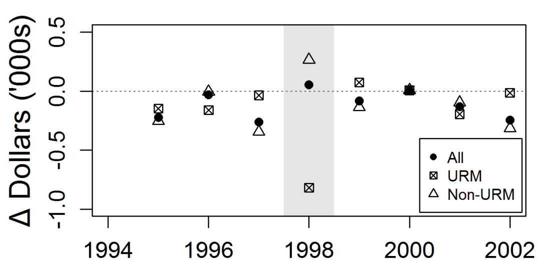


(b) California Universities

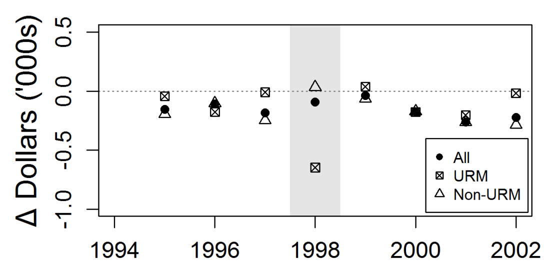


Panel B: Annual Change in Undergraduate Enrollment "CFSTY" Value-Added

(c) Public California Universities

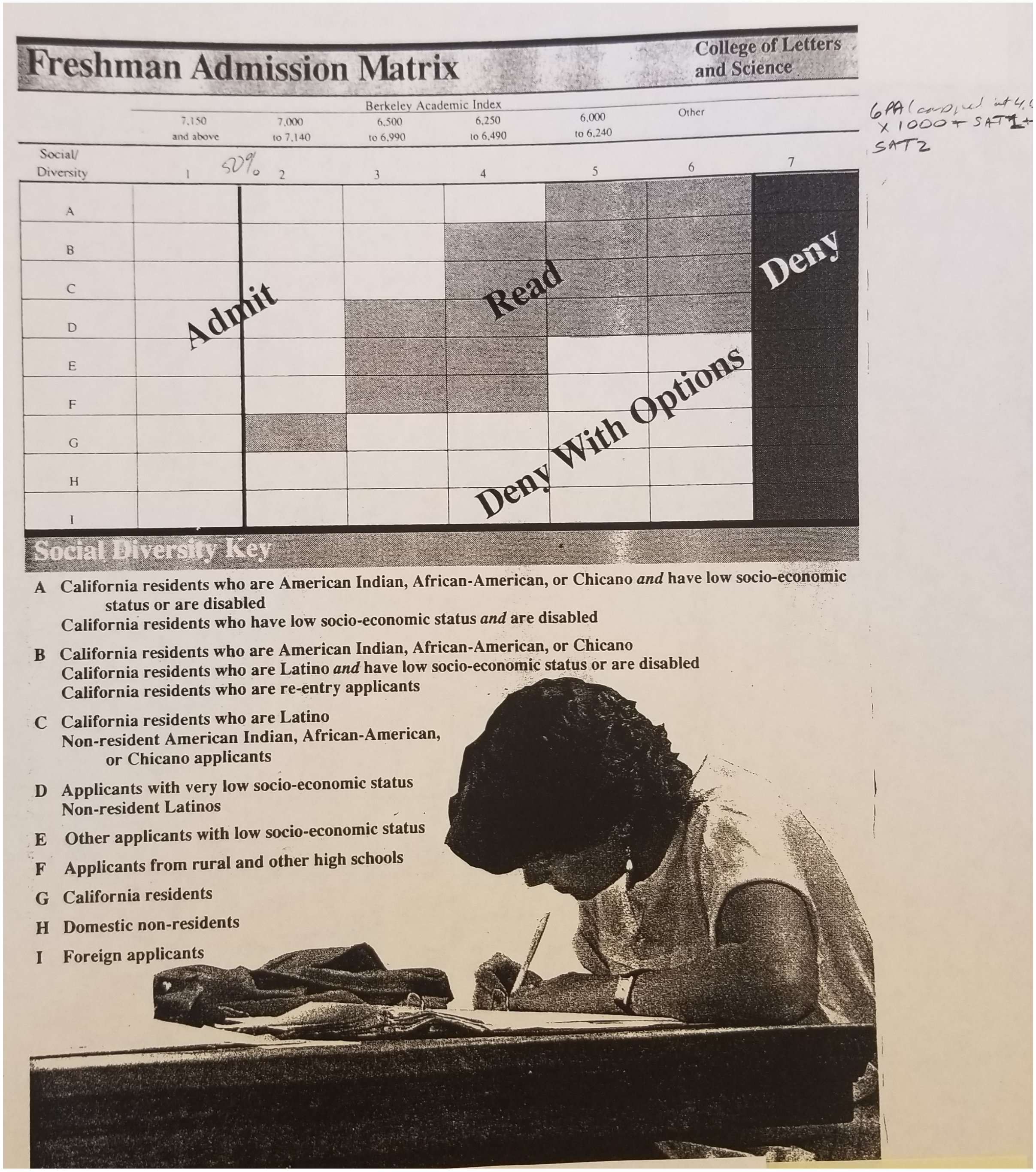


(d) California Universities



Note: This figure shows that while Prop 209 may have slightly depressed the growth of California public universities in 1997 and 1998, it had no measurable net effect on either the growth of all California institutions or the relative number of students enrolled at higher- or lower-value-added California institutions, with sharp declines in the value-added of URM students' enrollment institutions compensated for by increases among non-URM students in 1998. Year-over-year changes in freshman fall undergraduate enrollment and the enrollment-weighted average value-added of public and all California universities, overall and for URM and non-URM freshman students. Universities include all four-year institutions in California. See Appendix A.9 for methodological details and the estimated "CFSTY" value-added statistics; value-added measured relative to CSU Long Beach. Source: The Integrated Postsecondary Education Data System, UC Corporate Student System, and the CA Employment Development Department.

Figure A.13: Archival Example of UC Berkeley Pre-1998 Admissions Policy



Freshman Admission Matrix

College of Letters and Science

Social/ Diversity	1	2	3	4	5	6	7
Berkeley Academic Index	7,150 and above	7,000 to 7,140	6,500 to 6,990	6,250 to 6,490	6,000 to 6,240	Other	

Social Diversity Key

A California residents who are American Indian, African-American, or Chicano *and* have low socio-economic status or are disabled
California residents who have low socio-economic status *and* are disabled

B California residents who are American Indian, African-American, or Chicano
California residents who are Latino *and* have low socio-economic status or are disabled
California residents who are re-entry applicants

C California residents who are Latino
Non-resident American Indian, African-American, or Chicano applicants

D Applicants with very low socio-economic status
Non-resident Latinos

E Other applicants with low socio-economic status

F Applicants from rural and other high schools

G California residents

H Domestic non-residents

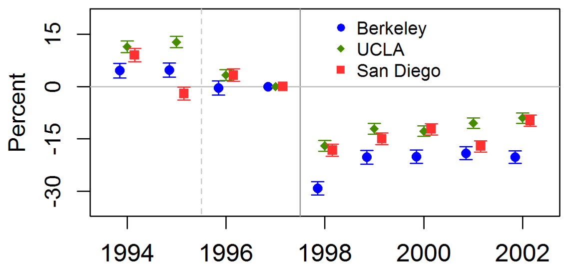
I Foreign applicants

Note: This figure presents an example of UC Berkeley's pre-1998 admissions policy. The table shows that the university guaranteed admission to all applicants above a designated Academic Index threshold, where that threshold was set every year to admit 50 percent of all Berkeley admits. The university then set lower AI guarantee thresholds for other groups of students, including disadvantaged ethnic groups, disabled students, and students with "low socio-economic status", though it is unclear how the latter were defined. The specific numbers presented at the top of the page do not match the admissions data in any specific year, suggesting that this document (found with minimal context in UC Berkeley's Bancroft Library) was presented as an example rather than a specific year's policy. Further archival documentation suggests that most other campuses used highly-comparable admissions rules. Source: UC Berkeley Bancroft Library: CU-558, Box 2, Page 8-942.

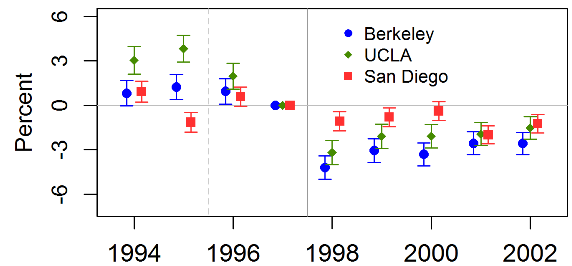
Figure A.14: Annual Difference-in-Difference Estimates of Post-1998 URM Admissions by UC Campus

Panel A: More-Selective UC Campuses

(a) Admission

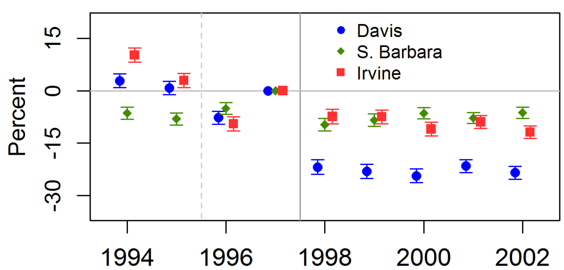


(b) Enrollment

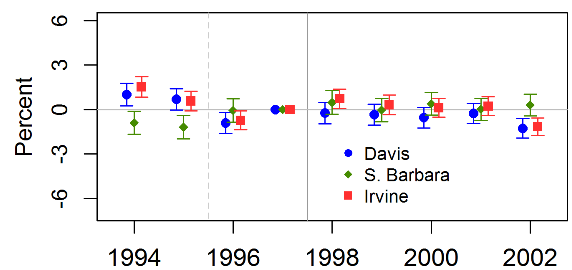


Panel B: Selectivity UC Campuses

(c) Admission

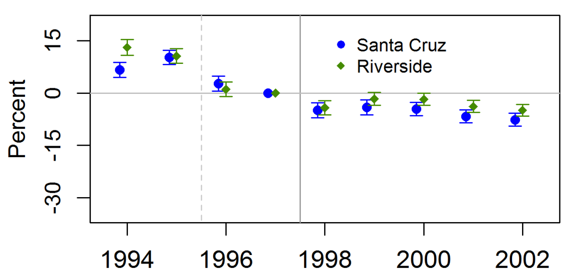


(d) Enrollment

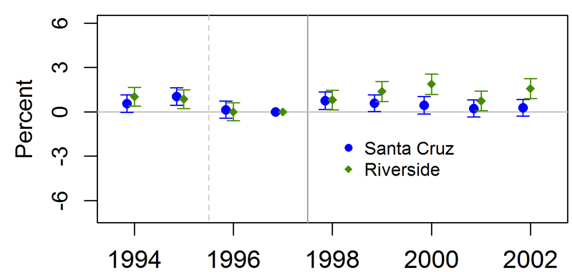


Panel C: Less-Selective UC Campuses

(e) Admission

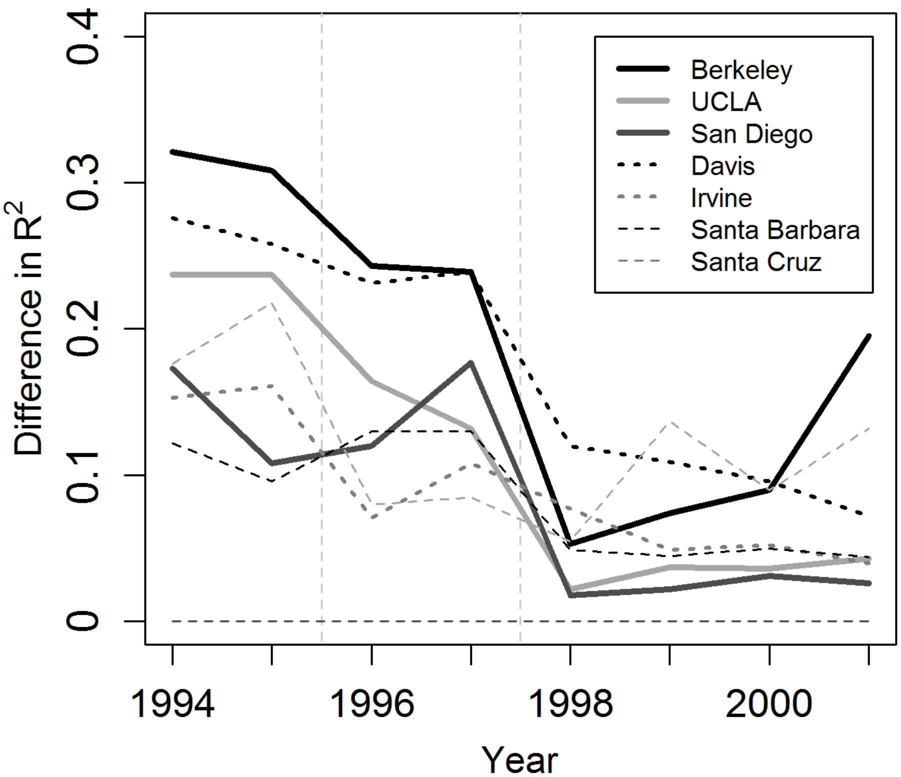


(f) Enrollment



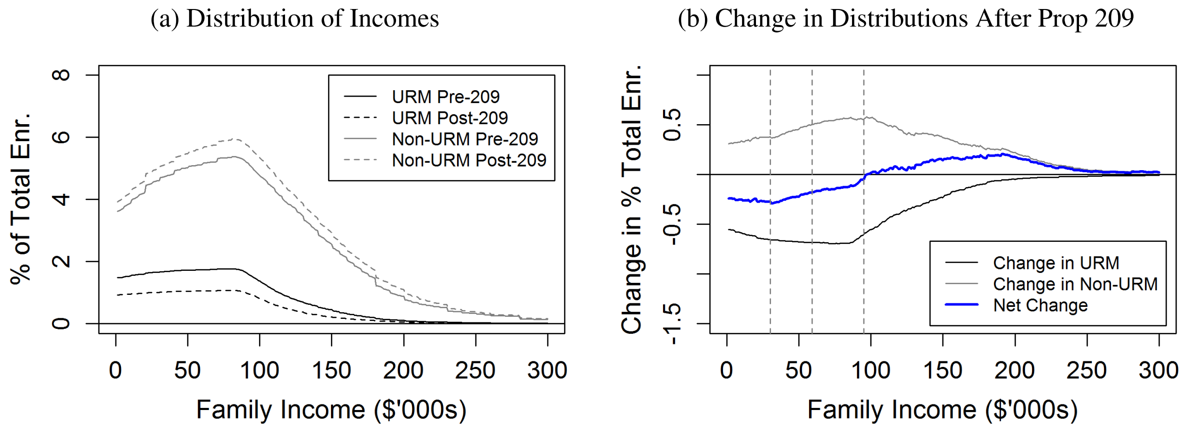
Note: This figure shows that URM UC applicants' admissions likelihood sharply and persistently declined at every UC campus in exactly 1998, but that some campuses also exhibited declines in 1996. OLS difference-in-difference coefficient estimates of the change in URM applicants' likelihood of admission or enrollment at each UC campus relative to non-URM applicants' respective likelihood, compared to the 1997 baseline. Campuses are ordered by their mid-1990s admissions rate. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Bars show 95-percent confidence intervals from robust standard errors. Admission is conditional on applying to that campus; enrollment is conditional on applying to any UC campus. Source: UC Corporate Student System.

Figure A.15: Estimated Annual First-Order Contribution of Ethnicity to UC Campuses' Admissions Decisions



Note: This figure shows that the share of variation in admissions at each UC campus that could be explained by ethnicity (above that explained by AI) fell across all campuses in 1998, though it had begun to fall at some campuses by 1996. Each point measures the difference in R^2 coefficients between two linear models of admission to each respective UC campus among 'normal' UC applicants. The first model predicts admission based on the leave-one-out likelihood of admission for students with the same academic index and ethnicity, which explains 40-70 percent of variation in most campuses' admissions decisions before 1996. The second model predicts admission based on the leave-one-out likelihood of admission for all students with the same academic index. The models are visualized separately in Figure A.11. The difference can be understood as a proxy for the annual magnitude of the first-order contribution of ethnicity to UC admission by campus. 'Normal' applicants are freshman fall California-resident applicants who (a) were UC-eligible, which means that they satisfactorily completing the required high school coursework, and (b) who selected intended majors that did not have special admissions restrictions (e.g. engineering at some campuses). UC Riverside admitted all such applicants. Source: UC Corporate Student System.

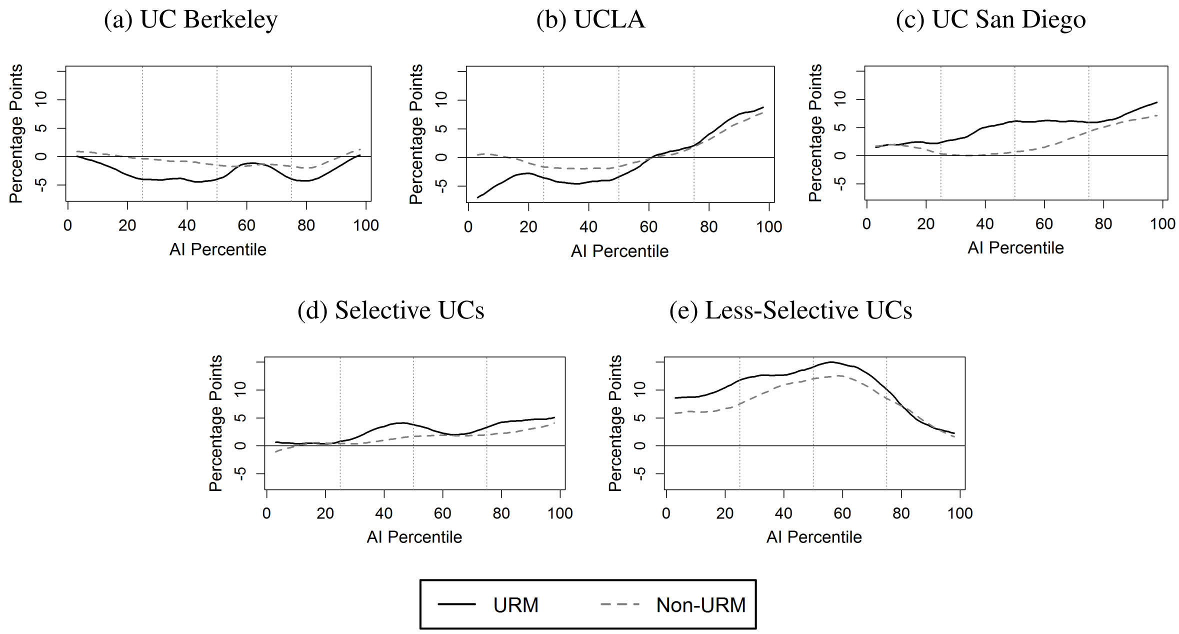
Figure A.16: Average Family Income of Berkeley and UCLA Students by Ethnicity Before and After Prop 209



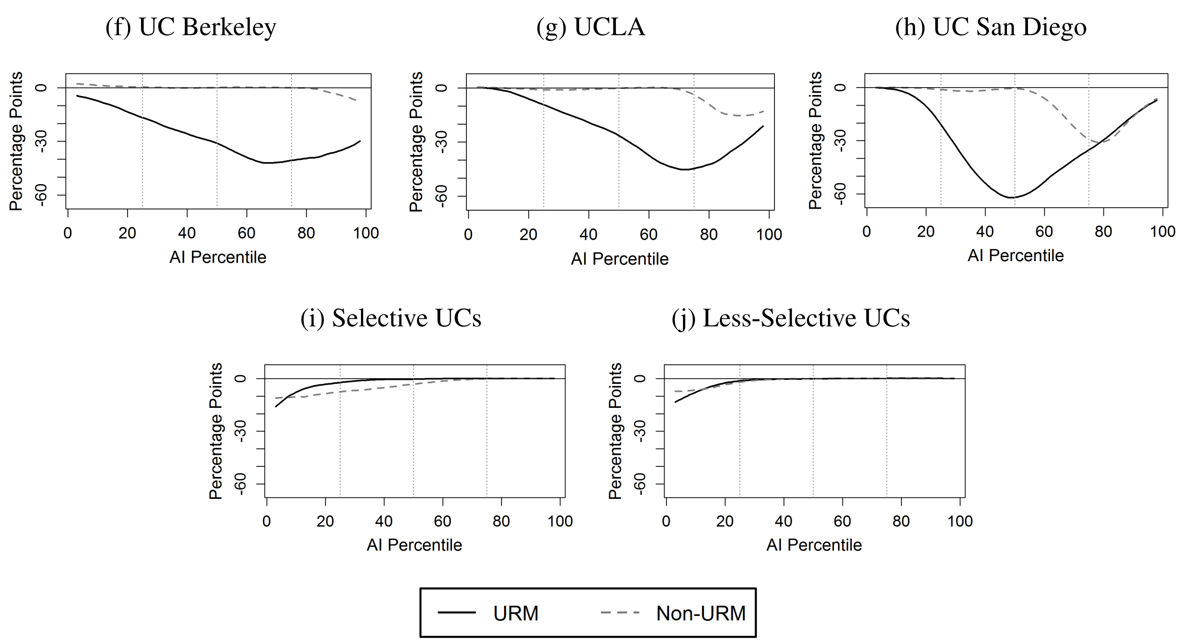
Note: This figure shows that the URM students who enrolled at UC Berkeley and UCLA under affirmative action had lower average incomes than the non-URM students who crowded into those campuses following Prop 209, leading to a net shift of students from the bottom three income quartiles (fixed in ‘96-97) to the top quartile after 1998. Shares of 1996-1999 UC Berkeley and UCLA students by income and ethnicity before and after Prop 209, differences of those shares by income and ethnicity, and the summed net enrollment change by income. The y-axis is scaled per $10,000 for readability; e.g. there was a net decline in UC Berkeley and UCLA students with family incomes of ∼$30,000 by about 0.5 percent of total enrollment after Prop 209. Dashed lines in Panel (b) show the 25th, 50th, and 75 percentiles of in-sample ‘96-97 family incomes. Figures are smoothed by a uniform kernel with bandwidth $20,000. Family incomes are not reported by 15 percent of the sample, increasing from 11 percent in ‘96-97 to 18 percent in ‘98-99; I impute incomes for these students by OLS regression of log family income on high school indicators, Zip code indicators, parental occupation indicators, max parental education indicators, standardized test scores, and gender in the full ‘96-97 CA-resident freshman UC applicant pool with observed family incomes. Imputed incomes are available for 95 percent of students with missing income; the regression's adjusted R^2 is 0.48, and the predicted values have a correlation with observed in-sample family income of 0.59. The distribution of predicted incomes among non-reporters is highly similar to the reported income distribution, with true (predicted) moments first quartile $29,500 ($41,100), median $60,000 ($60,200), mean $74,200 ($68,000), and third quartile $100,000 ($90,000). Source: UC Corporate Student System.

Figure A.17: Changes in UC App. and Admission after Prop 209 by Eth. and AI Percentile

Panel A: Changes in UC App. Likelihood by AI and Ethnicity, Among UC Applicants

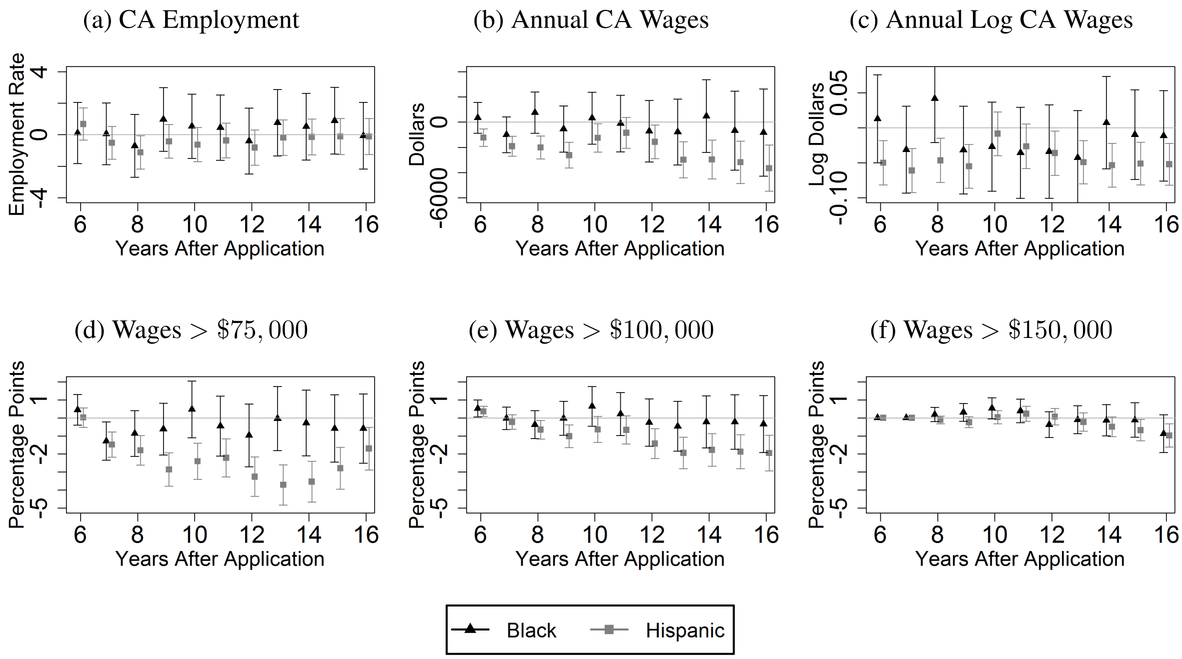


Panel B: Changes in UC Campus Admission Likelihood by AI and Ethnicity, Among Applicants



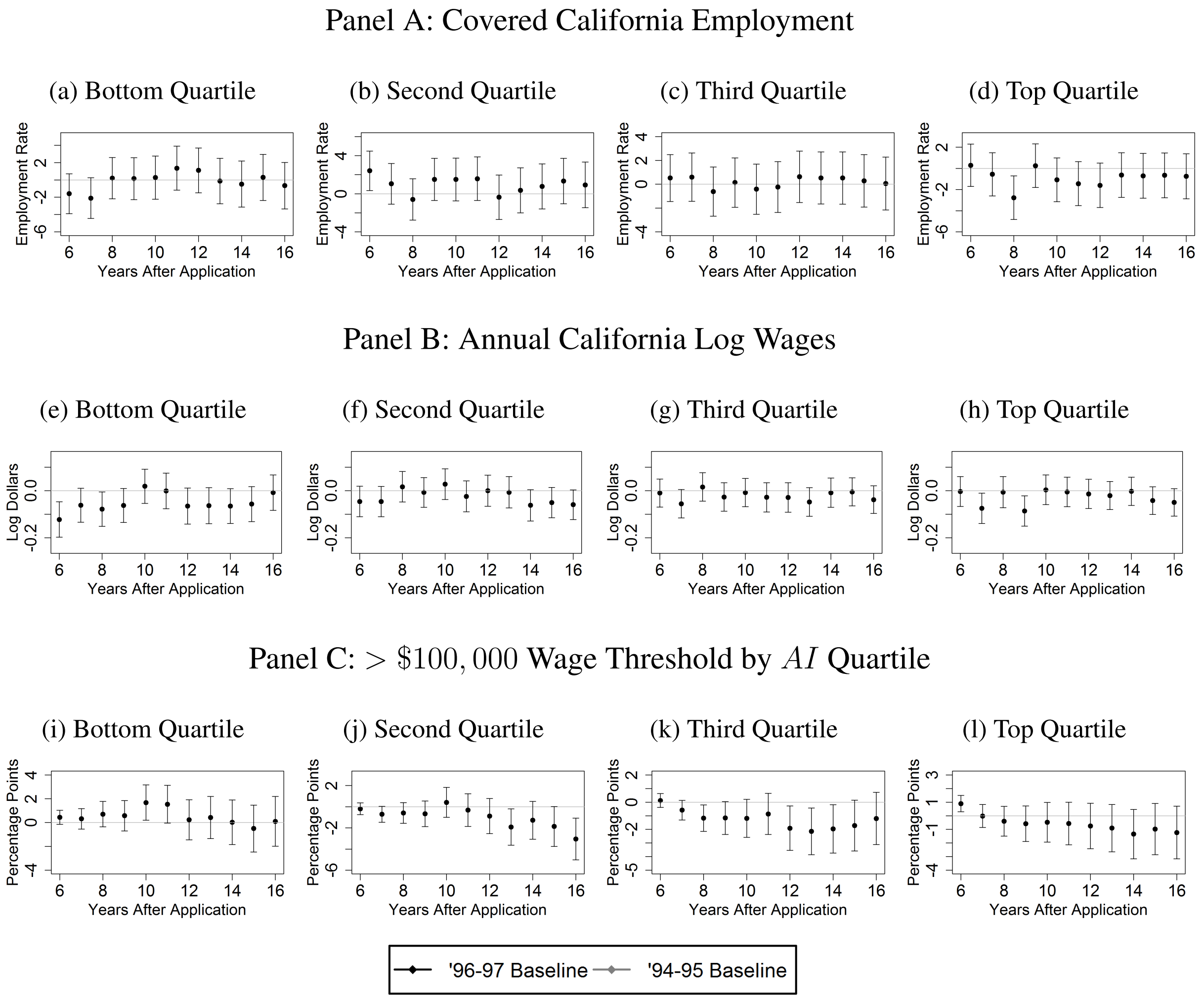
Note: This figure shows that changes in application patterns among URM UC applicants did not closely mirror changes in those applicants' UC admissions likelihood following Prop 209; for example, high-AI URM applicants were (relatively) no less likely to apply to UCLA after Prop 209 despite sharp declines in admissions likelihood at that campus. Difference in the percent of UC applicants who apply to or are admitted to each UC campus(es) between 1998-1999 and 1996-1997, by URM status and by percentile of academic index (AI) measured among all 1996-1999 URM UC applicants. Admit statistics are conditional on application to that campus. Statistics are smoothed with a triangular kernel with bandwidth 15. Source: UC Corporate Student System.

Figure A.18: Difference-in-Difference Estimates of Black and Hispanic UC Applicants' Post-1998 Wage Outcomes



Note: This figure shows that Hispanic UC applicants faced persistent labor market deterioration following Prop 209, while estimates for Black UC applicants' wage deterioration are noisy but generally appear smaller. Estimates of β_0 and $\beta_{\cdot 98-99}$ from an extension Equation 2.1 splitting the URM indicator into separate Black and Hispanic indicators interacted with post-209. The model is an OLS difference-in-difference model of 1996-1999 URM and Asian UC freshman California-resident applicants' educational outcomes compared to other non-URM students' outcomes after the 1998 end of UC's affirmative action program. Outcomes defined as non-zero California wages ("CA Employment"), California wages in dollars and log-dollars (omitting 0's), and unconditional indicators for having wages above specified wage thresholds ($75,00, $100,000, and $150,000) 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 footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. Annual wages CPI-adjusted to 2018 and winsorized at top and bottom 1 percent. Robust 95-percent confidence intervals shown. Source: UC Corporate Student System and the California Employment Development Department.

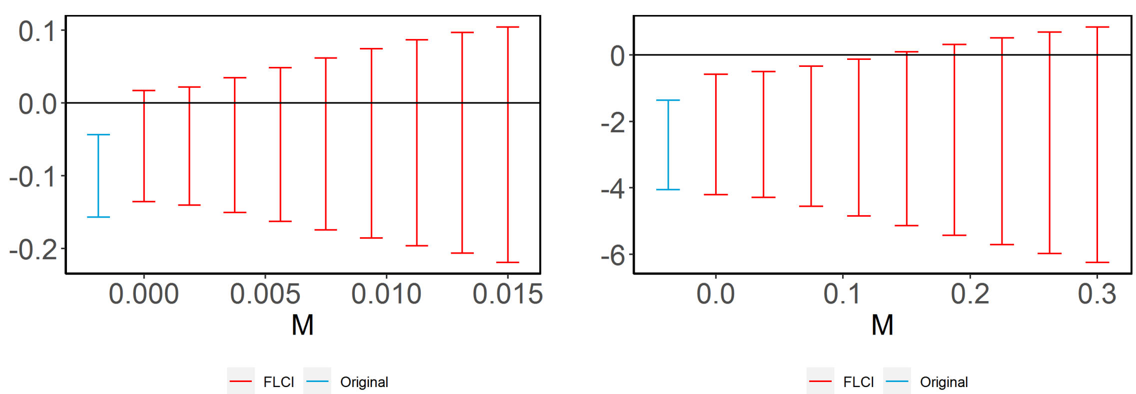
Figure A.19: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Labor Market Outcomes



Note: This figure shows that URM applicants' California employment was largely unchanged among all four AI quartiles, but that all experienced log wage declines and all but the bottom quartile became less likely to earn at least \$100,000 annual California wages, with larger estimated declines relative to the '94-95 baseline. Estimates of β_{98-99} from Equation 2.1, an OLS difference-in-difference 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. Outcomes defined as non-zero California wages ("CA Employment"), average log wages (excluding zeroes), and unconditional indicators for having wages above specified wage thresholds (\$75,00, \$100,000, and \$150,000) 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 footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. Panel C replaces the 1996-97 pre-209 UC applicants with 1994-95 UC applicants, showing coefficients from both sets of models. Annual wages CPI-adjusted to 2018 and winsorized at top and bottom 1 percent. Robust 95-percent confidence intervals shown. Source: UC Corporate Student System and the California Employment Development Department.

Figure A.20: Difference-in-Difference Robustness to Non-Parallel Trends

(a) Avg. Annual Conditional Log Wages (b) Avg. Eth-Specific Wage Percentile

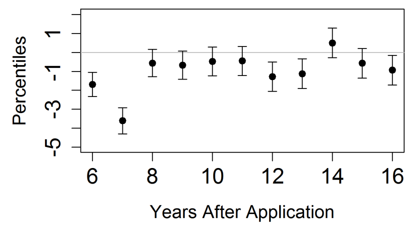


Note: This figure shows that while the difference-in-difference log wage estimates are sensitive to loosening the parallel trends assumption, replacing wages with ethnicity-specific wage percentiles generates estimates relatively insensitive to assumptions allowing bounded pre-trends of up to almost 0.15 percentiles per year. Estimates of $\beta_{`98-99}$ from Equation 2.1, an OLS difference-in-difference 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, by varying assumptions over the maximal annual degree to which the parallel trends assumption may be violated (following Rambachan and Roth, 2020). The blue bars show the baseline estimates; the black bars present fixed length confidence intervals permitting $\Delta^{SD}(M)$ (the x-axis) deviations from the parallel trends assumption. Source: UC Corporate Student System and the California Employment Development Department.

Figure A.21: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Labor Outcomes

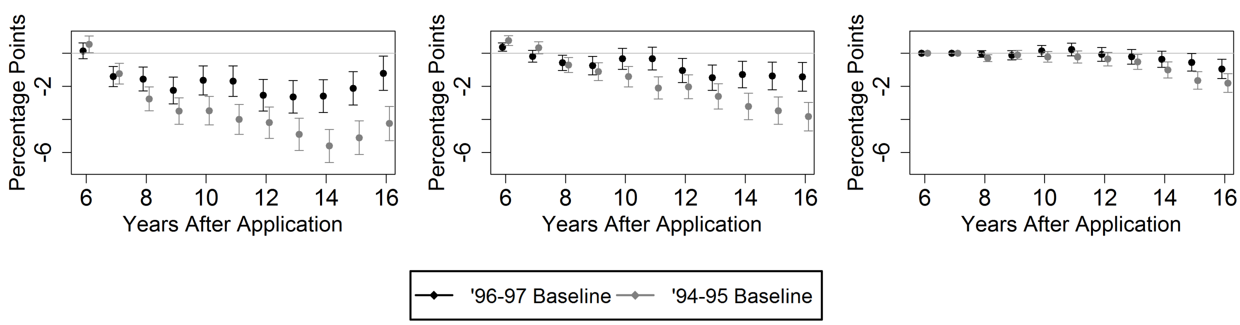
Panel A: Annual Differences in Eth-Specific Wage Percentile

(a) Wage Percentile



Panel B: Wage Threshold Estimates Using '96-97 and '94-95 Baselines

(b) Wages $> \$75,000$ (c) Wages $> \$100,000$ (d) Wages $> \$150,000$

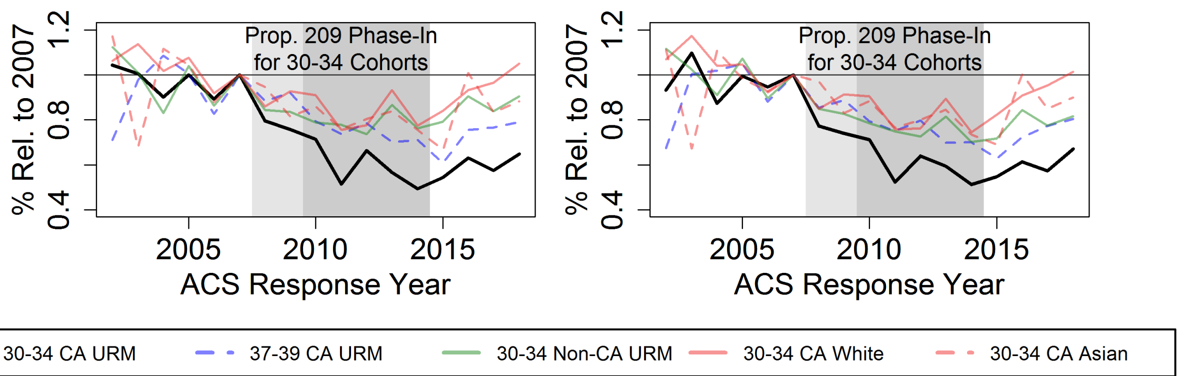


Note: This figure shows that URM UC applicants faced a long-run decline in their average wage percentile relative to same-ethnicity college-educated workers not impacted by Prop 209, and that URM UC applicants' likelihood of attaining various high-earning thresholds declined after Prop 209, and moreso relative to a '94-95 baseline. Estimates of $\beta_{'98-99}$ from Equation 2.1, an OLS difference-in-difference 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. The outcome in Panel A 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. Outcomes in Panel B defined as annual unconditional indicators for having wages above specified wage thresholds (\$75,00, \$100,000, and \$150,000) as measured in the California Employment Development Department database, which includes employment covered by California unemployment insurance. Coefficients in each model and year after UC application are estimated independently. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Academic Index (AI) is defined in footnote 25. The gray estimates replace the 1996-97 baseline with with 1994-95 UC applicants. Annual wages CPI-adjusted to 2018 and winsorized at top and bottom 1 percent. Robust 95-percent confidence intervals shown. Source: UC Corporate Student System, the California Employment Development Department, and the American Community Survey (Ruggles et al., 2018).

Figure A.22: Share of $> \$100,000$ Workers among Rolling Cohorts Before and After Prop 209's Impact

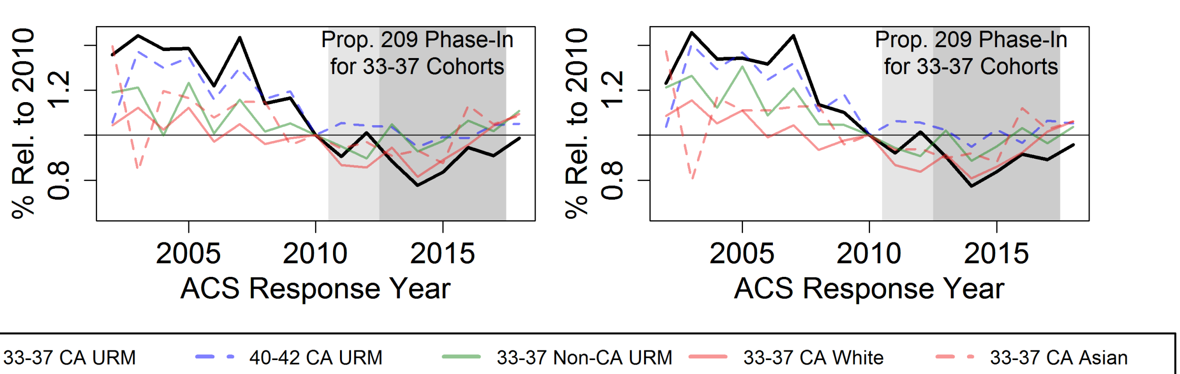
Panel A: Rolling Cohorts Age 30-34

(a) All Workers (b) College Enrollees



Panel B: Rolling Cohorts Age 33-37

(d) All Workers (e) College Enrollees

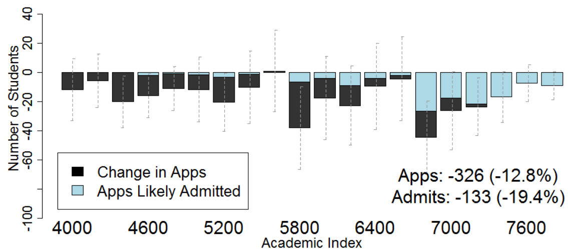


Note: This figure shows that early-career URM Californians ten to twenty years after Prop 209 were less likely to achieve high wages than a variety of reasonable comparison groups (like non-URM Californians and URM non-Californians), and that the gaps (across rolling cohorts) seem to originate and widen in the years when URM workers of that age would have been first impacted by Prop 209 (hitting age 18 around 1998). The fraction of ACS respondents earning at least \$100,000 per year in wages by ethnicity, contemporaneous age range, and either California birth or contemporaneous California residency status, normalized to 1 in 2007 or 2010 for each group. Grey lines denote the years 2010-2014 (2013-2017) in which the age 30-34 (33-37) URM cohort would have largely switched from people who graduated high school before the 1998 implementation of Prop 209 to those who graduated after implementation, assuming graduation at age 18. Some public universities began phasing out affirmative action two years earlier (in 1996), justifying the 2007 baseline. Wages are in 2018 CPI-adjusted dollars. All statistics are two-year moving averages. Source: 2001-2017 American Community Survey (Ruggles et al., 2018)

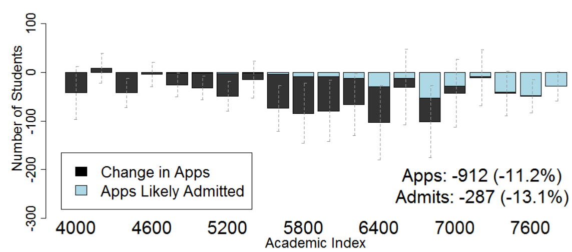
Figure A.23: Further Estimated Declines in 1998-99 Application and Admission by Ethnicity

Panel A: Changes in UC-Eligible Application Likelihood to Most-Selective UC Campuses

(a) Black

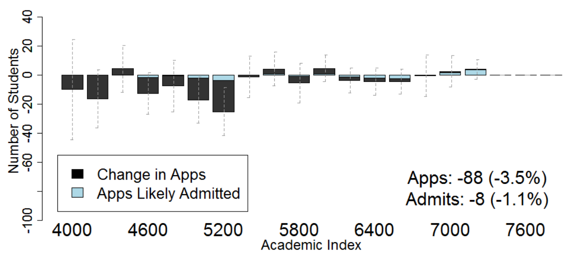


(b) Hispanic

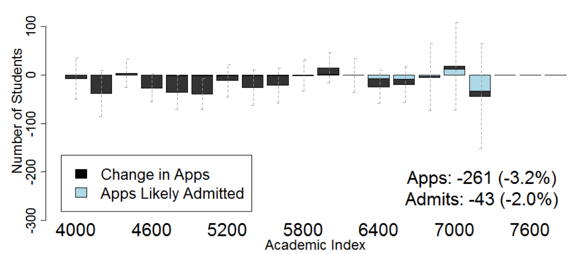


Panel B: Changes in UC-**In**eligible Application Likelihood to UC

(c) Black

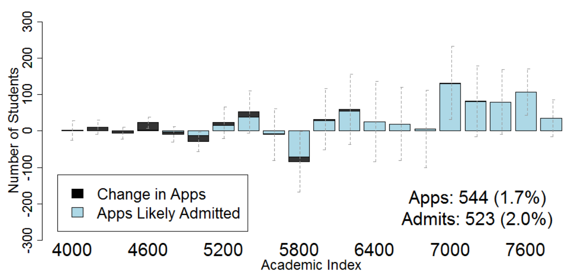


(d) Hispanic

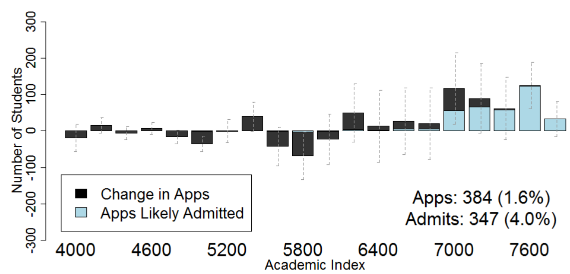


Panel C: Asian

(e) Any UC Campus



(f) Most-Selective Campuses



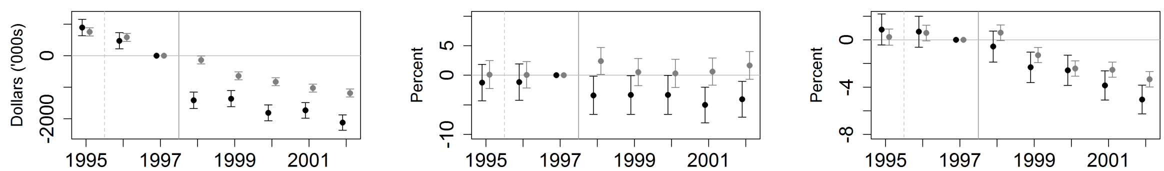
Note: This figure shows that URM application declines to the Berkeley and UCLA campuses can explain up to 20 percentage points of the decline in URM enrollment at those campuses, while application rates only slightly declined among UC-ineligible students and only slightly increased among Asian students relative to applications among white students (a sort of placebo test). Estimates of the change in the number of UC applicants (and admits) in 1998-1999 by ethnicity (e) and 200-point AI bin, relative to 1994-1995. The height of each black bar is the product of $\beta_{e,98-99,a}$ (estimated in Equation 2.2) and $\sum_s UC_{s,98-99,e}$, the average number of UC-eligible California public high school graduates of ethnicity e in 1998-1999. The height of each overlaying blue bar is the product of the black bar and the percent of 1998-1999 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. Panel A and half of Panel C re-estimate Equation 2.2 restricting to applicants to UC Berkeley or UCLA. Panels A and C are restricted to UC-eligible high school graduates and UC applicants; Panel B re-estimates Equation 2.2 for UC-ineligible graduates and applicants. 95-percent confidence intervals on the black bars from $\beta_{e,98-99,a}$ robust standard errors. Source: UC Corporate Student System and the California Department of Education.

Figure A.24: Annual Single-Difference Estimates of URM UC Applicants' Post-1998 Outcomes

(a) Institutional "Value-Added" for Wages

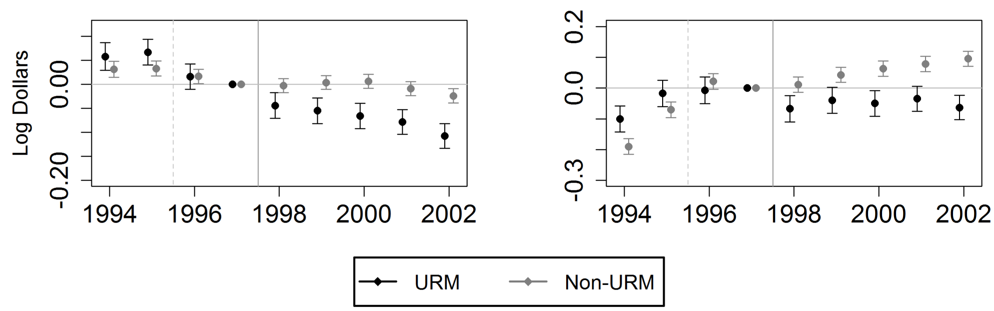
(b) Six-Year BA Attain., Bottom AI Q.

(c) Grad. Degree Attainment



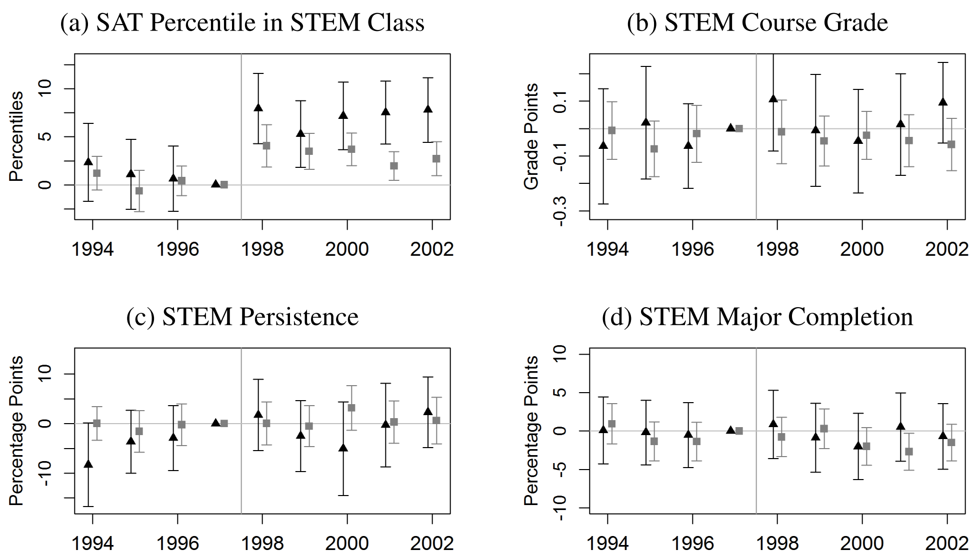
(d) Avg. Annual Conditional Log Wages

(e) # Above-$100,000 Years in Early 30s



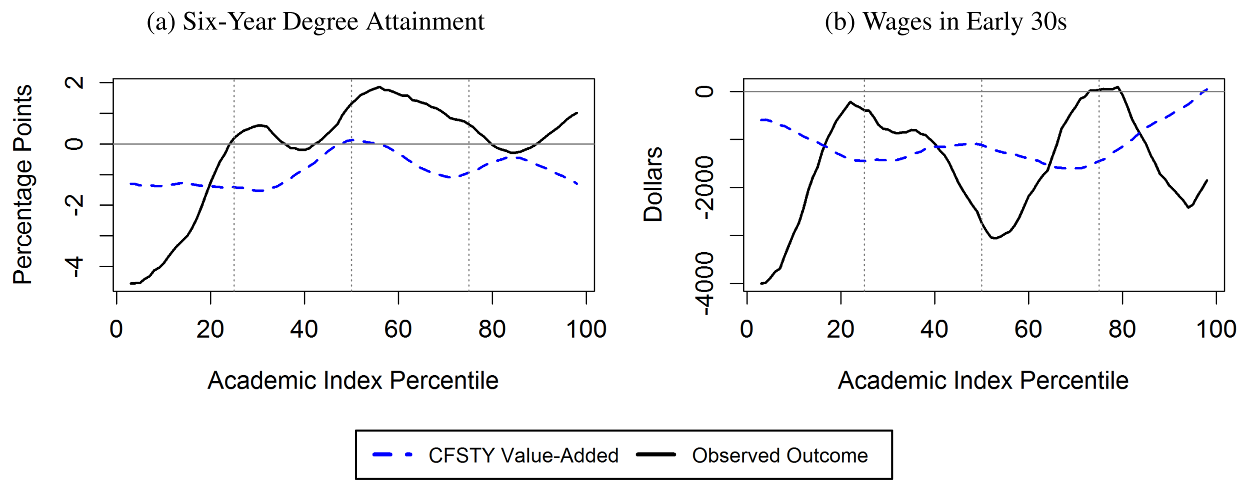
Note: This figure shows single-difference analogues to the baseline estimates, showing that the estimated effects appear largely driven by immediate 1998 declines in enrollment value-added and outcomes among URM students, not 1998 increases among non-URM students. OLS difference-in-difference coefficient estimates of the change in four URM applicant outcomes relative to non-URM applicants, compared to the 1997 baseline. Outcomes include six-year Bachelor's degree attainment in the NSC, graduate degree attainment in the NSC, average annual conditional (omitting 0's) log California covered wages 6-19 years after UC application, and the number years (6-19 years after UC application) in which California covered wages exceed $75,000. Bars show 95-percent confidence intervals from robust standard errors. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Panel (a) restricts the sample to the bottom AI quartile as measured among '96-97 URM UC applicants. Source: UC Corporate Student System, National Student Clearinghouse, and California Employment Development Department.

Figure A.25: Difference-in-Difference Estimates of URM UC Enrollees' STEM Outcomes by Ethnicity



Note: Difference-in-difference WLS regression coefficient estimates of UCB, UCSB, UCD, UCSC, and UCR enrollees' introductory STEM course performance or persistence, differencing across URM status following Equation 2.3 and interacting β_t with Black and Hispanic indicators to separately identify outcomes by URM ethnicity, 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 Appendix A.8) 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 footnote 47). UCSC is omitted from the GPA model because it did not mandate letter grades in the period. 95-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*b*).

Figure A.26: Difference-in-Difference Changes in Inst. Value-Added and Outcome by AI Quantile



Note: This figure plots unadjusted difference-in-difference averages for both VA and actual degree attainment and early-30s wages, showing that the two lines poorly mirror each other, suggesting both that VA poorly-explains and substantially underestimates the observed labor market effects of Prop 209. Raw difference-in-difference statistics of average six-year degree attainment, early-30s wages, and corresponding "CFSTY" institutional value-added measures from students' first enrollment institution, differenced among UC freshman applicants between 1998-1999 and 1996-1997 and by URM status for each percentile of academic index (AI) measured among 1996-1999 URM UC applicants. Statistics are smoothed with a triangular kernel with bandwidth 15. First enrollment measured in NSC up to six years after UC application; university groups partition possible enrollments. See note to Table 2.2 for value-added definition. Average wages measured as mean observed wages between 12 and 16 years after UC application, when most students are 30-34; VA wages are measured 15 years after UC application. Six-year degree attainment measured in the union of UC and NSC degree attainment. Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department.

Table A.14: STEM Majors in Main NSC Sample

Major	#	Major	#
BIOLOGICAL SCIENCES	8,008	EXERCISE BIOLOGY	267
BIOLOGY	6,382	ZOOLOGY	264
COMPUTER SCIENCE	6,113	STRUCTURAL ENGINEERING	251
ELECTRICAL ENGINEERING	5,110	MATERIALS SCIENCE AND ENGINEERING	250
MECHANICAL ENGINEERING	4,942	AQUATIC BIOLOGY	238
MOLECULAR AND CELL BIOLOGY	3,505	ECOLOGY BEHAVIOR & EVOLUTION	227
MATHEMATICS	3,076	INDUSTRIAL ENGINEERING AND OPERATIONS RESEARCH	225
CIVIL ENGINEERING	2,649	EARTH SCIENCES	222
CHEMISTRY	2,516	INFORMATION SYSTEMS	221
COMPUTER ENGINEERING	2,347	NUTRITIONAL SCIENCES	216
BIOCHEMISTRY	2,167	PHARMACOLOGICAL CHEMISTRY	216
PHYSICS	1,624	COMPUTER INFORMATION SYSTEMS	209
MANAGEMENT SCIENCE	1,578	CONSTRUCTION MANAGEMENT	203
GENERAL BIOLOGY	1,537	APPLIED ECOLOGY	201
CHEMICAL ENGINEERING	1,509	ASTROPHYSICS	201
ELECTRICAL ENGINEERING AND COMPUTER SCIENCES	1,502	BIOCHEMISTRY AND MOLECULAR BIOLOGY	195
BIOCHEMISTRY AND CELL BIOLOGY	1,487	MATHEMATICS/ECONOMICS	186
INFORMATION AND COMPUTER SCIENCE	1,481	COMPUTER INFO SYSTEMS	170
PSYCHOLOGY AND SOCIAL BEHAVIOR	1,462	BIOLOGICAL SYSTEMS ENGINEERING	167
PSYCHOBIOLOGY	1,451	COMPUTER ENGINEERING AND COMPUTER SCIENCE	167
INTEGRATIVE BIOLOGY	1,263	ECOLOGY AND EVOLUTION	166
COGNITIVE SCIENCE	1,088	MATERIALS ENGINEERING	165
PHYSIOLOGICAL SCIENCE	1,006	CELL AND DEVELOPMENTAL BIOLOGY	160
MICROBIOLOGY	879	ENVIRONMENTAL ENGINEERING	160
ANIMAL PHYSIOLOGY & NEUROSCI	833	BIOMEDICAL SCIENCES	159
NEUROSCIENCE	810	PHYSIOLOGY	144
MOLECULAR CELL AND DEVELOPMENTAL BIOLOGY	803	EVOLUTION AND ECOLOGY	141
BIOENGINEERING	786	MOLECULAR ENVIRONMENTAL BIOLOGY	139
APPLIED MATHEMATICS	750	ARCHITECTURAL ENGINEERING	137
AEROSPACE ENGINEERING	718	PHARMACOLOGY	136
HUMAN BIOLOGY	712	MECHANICAL ENGINEER	133
NEUROBIOPHYSIOLOGY & BEHAVIOR	639	COGN SCI W/SPECIALIZ NEUROSCI	130
GENETICS	582	ELECTRICAL ENGINEERING AND COMPUTER SCIENCE	128
COMPUTER SCIENCE AND ENGINEERING	570	GEOLOGICAL SCIENCES	127
COMPUTER SCIENCE & ENGINEERING	472	NUTRITION SCIENCE	126
BIOCHEM & MOLECULAR BIOLOGY	445	MATHEMATICS-COMPUTER SCIENCE	124
MICROBIOLOGY IMMUNOLOGY AND MOLECULAR GENETICS	403	ENGINEERING PHYSICS	122
ENGINEERING	387	BIOENGINEERING (BIOTECHNOLOGY)	119
MOLECULAR BIOLOGY	387	CLINICAL NUTRITION	117
BIOMEDICAL ENGINEERING	382	HEALTH SCIENCES	116
MATHEMATICS/APPLIED SCIENCE	350	COGN SCI W/SPEC HUM COMP INTER	115
MARINE BIOLOGY	348	ECONOMICS-MATHEMATICS	111
GEOLOGY	334	NEUROBIOLOGY	111
BIOTECHNOLOGY	332	NEUROSCIENCE AND BEHAVIOR	107
BIOLOGICAL SCIENCE	331	BIOLOGY-PHYSIOLOGY	105
INDUSTRIAL ENGINEERING	300	NATURAL SCIENCE	102
STATISTICS	295	MGMT SCI & ENGINEERING	99
BIOENGINEERING: PRE-MEDICAL	289	INDUSTRIAL AND SYSTEMS ENGINEERING	91
MICROBIOLOGY AND MOLECULAR GENETICS	288	MATHEMATICAL SCIENCES	87
BIOCHEMISTRY/CHEMISTRY	287	GENERAL ENGINEERING	85

Note: This table shows the 100 most common STEM majors earned by 1994-2002 freshman UC applicants. The 100 most common majors categorized as STEM (following the procedure described in footnote 22) among those earned by 1994-2002 freshman UC applicants at any four-year institution as reported to the National Student Clearinghouse, and the number of in-sample students who report that major. Each student is permitted up to three majors. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.15: Non-STEM Majors in Main NSC Sample

Major	#	Major	#
PSYCHOLOGY	22,896	ASIAN AMERICAN STUDIES	729
BUSINESS ADMINISTRATION	17,406	COMMUNICATIONS	709
POLITICAL SCIENCE	15,964	DESIGN	699
ECONOMICS	14,652	WOMEN'S STUDIES	682
SOCIOLOGY	12,560	LINGUISTICS	676
ENGLISH	11,634	GOVERNMENT	663
HISTORY	10,216	SOCIAL WELFARE	654
COMMUNICATION	6,964	COMPARATIVE LITERATURE	632
BUSINESS ECONOMICS	4,939	POLITICAL ECONOMY OF INDUSTRIAL SOCIETIES	626
LIBERAL STUDIES	3,878	ART STUDIO	623
ANTHROPOLOGY	3,423	INTERNATIONAL BUSINESS	622
SPANISH	3,196	ETHNIC STUDIES	576
PHILOSOPHY	2,683	ACCOUNTANCY	542
HUMAN DEVELOPMENT	2,493	RHETORIC	525
INTERNATIONAL RELATIONS	2,171	BIOPSYCHOLOGY	517
COMMUNICATION STUDIES	2,154	AMERICAN LITERATURE AND CULTURE	511
NURSING	1,966	DRAMA	497
ART	1,923	GENERAL STUDIES	493
FINANCE	1,819	ENVIRONMENTAL SCIENCES	485
MARKETING	1,786	CINEMA-TELEVISION	483
MANAGERIAL ECONOMICS	1,781	DANCE	472
ACCOUNTING	1,587	VISUAL ARTS (MEDIA)	461
INTERNATIONAL STUDIES	1,552	POLITICAL SCI/INTNTL RELATIONS	456
ARCHITECTURE	1,534	SOCIAL ECOLOGY	456
MUSIC	1,480	ENVIRONMENTAL ANALYSIS AND DESIGN	445
ART HISTORY	1,404	SOCIAL WORK	441
AMERICAN STUDIES	1,358	THEATRE ARTS	437
CRIMINOLOGY LAW AND SOCIETY	1,302	FILM AND TELEVISION	435
GLOBAL STUDIES	1,212	PHARMACY	435
LIBERAL ARTS	1,208	THEATER	416
LEGAL STUDIES	1,199	AGRICULTURAL BUSINESS	414
LAW AND SOCIETY	1,167	BUSINESS ADMINISTRATION (MARKETING)	414
SOCIAL SCIENCE	1,166	EXERCISE SCIENCE	412
ENVIRONMENTAL STUDIES	1,156	CREATIVE STUDIES	404
INTERDISCIPLINARY STUDIES	1,129	GRAPHIC DESIGN	398
MASS COMMUNICATIONS	1,097	INTERDISC COMPUTING & THE ARTS	381
KINESIOLOGY	1,070	CRIMINAL JUSTICE ADMINISTRATION	368
THEATRE	1,032	INTERNATIONAL DEVELOPMENT STUDIES	367
FILM STUDIES	999	SOCIAL SCIENCES	366
JOURNALISM	953	ECONOMICS/INTERNATIONAL AREA STUDIES	365
CRIMINAL JUSTICE	910	LATIN AMERICAN STUDIES	352
MANAGEMENT	906	CHICANO STUDIES	332
GEOGRAPHY	895	DRAMATIC	325
POLITICS	894	JAPANESE	319
FRENCH	882	LAW	312
ANIMAL SCIENCE	813	FILM AND DIGITAL MEDIA	306
BUSINESS MANAGEMENT ECONOMICS	780	LANDSCAPE ARCHITECTURE	302
RELIGIOUS STUDIES	778	HISTORY OF ART	297
STUDIO	764	SPEECH COMMUNICATION	294
CHILD DEVELOPMENT	745	INDUSTRIAL TECHNOLOGY	291

Note: This table shows the 100 most common Non-STEM majors earned by 1994-2002 freshman UC applicants. The 100 most common majors **not** categorized as STEM (following the procedure described in footnote 22) among those earned by 1994-2002 freshman UC applicants at any four-year institution as reported to the National Student Clearinghouse, and the number of in-sample students who report that major. Each student is permitted up to three majors. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.16: Descriptive Statistics of 1990s UC Admissions by Ethnicity

	Application			Admission			Enrollment		
	'94-5	'96-7	'98-9	'94-5	'96-7	'98-9	'94-5	'96-7	'98-9
Panel A: Non-URM Applicants									
Average Number or Percent of Applicants									
Berkeley	14,452	17,478	19,814	37.3	32.3	30.8	15.1	14.0	13.8
UCLA	16,738	20,272	23,965	44.3	37.3	33.9	15.3	13.3	13.5
San Diego	15,787	19,072	23,008	63.0	60.0	48.3	15.3	12.9	12.2
Davis	13,434	15,131	17,189	71.1	72.0	67.7	18.8	19.7	17.9
Irvine	11,734	13,198	16,134	76.2	71.2	64.1	19.8	19.4	17.5
Santa Barbara	12,946	14,819	18,750	84.5	74.9	57.7	18.5	18.4	14.7
Santa Cruz	7,506	8,174	9,984	85.3	85.4	81.0	16.7	18.8	17.5
Riverside	6,996	7,480	10,211	82.0	85.6	88.0	14.7	17.9	17.4
All UCs	33,602	37,972	42,268	84.8	83.5	83.9	49.6	49.4	49.6
Average SAT Score									
Berkeley	1250	1255	1262	1371	1375	1368	1344	1348	1338
UCLA	1209	1214	1228	1316	1333	1343	1262	1283	1299
San Diego	1212	1213	1222	1274	1298	1307	1224	1250	1260
Davis	1180	1184	1187	1232	1231	1230	1171	1176	1169
Irvine	1146	1151	1161	1185	1194	1213	1127	1137	1159
Santa Barbara	1141	1144	1166	1162	1182	1224	1122	1156	1189
Santa Cruz	1156	1154	1157	1177	1173	1180	1152	1151	1154
Riverside	1114	1114	1119	1137	1134	1136	1095	1091	1092
All UCs	1182	1187	1194	1207	1212	1216	1196	1208	1217
Panel B: URM Applicants									
Average Number or Percent of Applicants									
Berkeley	3,570	3,892	3,944	54.7	48.7	23.9	19.7	19.2	10.4
UCLA	4,872	5,152	5,395	55.8	42.8	24.8	21.5	16.8	11.3
San Diego	3,088	3,296	3,976	59.7	57.9	32.5	12.1	11.8	8.3
Davis	2,586	2,616	2,822	84.1	83.7	62.5	21.9	18.5	17.2
Irvine	2,884	2,752	3,238	73.4	62.7	54.8	15.7	12.9	14.3
Santa Barbara	3,197	3,542	4,008	77.0	77.2	54.3	16.3	18.1	15.4
Santa Cruz	2,235	2,096	2,291	83.7	81.3	72.9	16.0	14.5	15.6
Riverside	2,222	2,304	3,222	79.5	77.1	79.5	19.7	18.3	20.2
All UCs	9,478	9,498	9,922	81.3	79.4	73.4	47.0	44.3	39.6
Average SAT Score									
Berkeley	1072	1087	1102	1151	1168	1200	1130	1138	1143
UCLA	1030	1048	1066	1119	1155	1185	1089	1118	1140
San Diego	1059	1069	1082	1124	1151	1196	1088	1118	1163
Davis	1048	1056	1067	1083	1091	1108	1050	1070	1067
Irvine	996	1012	1025	1042	1071	1097	1004	1026	1062
Santa Barbara	1008	1021	1042	1045	1059	1102	999	1023	1075
Santa Cruz	1011	1017	1030	1033	1042	1059	990	1013	1039
Riverside	958	968	982	983	996	1009	963	960	968
All UCs	1025	1039	1048	1054	1071	1081	1052	1071	1077

Note: This table shows campus-specific descriptive statistics mirroring Table 2.1. Count and mean average descriptive statistics of 1994-1999 California-resident freshman UC applicants who are or are not underrepresented minorities (URM). URM includes African-American, Hispanic, Chicano/a, and Native American applicants. SAT score includes the Math and Verbal components and was on the 1600 scale. Percent admitted and percent enrolled are conditional on applying to that campus. Source: UC Corporate Student System.

Table A.17: Descriptive Statistics of 1990s UC Admissions by Ethnicity

	Application			Admission			Enrollment		
	‘94-5	‘96-7	‘98-9	‘94-5	‘96-7	‘98-9	‘94-5	‘96-7	‘98-9
Panel A: Black Applicants									
Average Number or Percent of Applicants									
Berkeley	1,020	1,078	1,048	50.2	50.1	23.2	17.7	20.6	10.3
UCLA	1,230	1,318	1,234	53.1	40.6	23.8	20.5	15.7	11.0
San Diego	600	681	802	50.6	53.3	23.7	8.5	9.0	5.1
Davis	608	660	666	76.6	75.5	52.9	19.1	14.7	13.7
Irvine	540	546	605	65.6	50.9	46.3	11.9	9.6	12.1
Santa Barbara	523	608	710	76.3	71.8	48.6	17.6	17.5	12.5
Santa Cruz	364	376	386	78.8	76.5	64.3	13.7	11.0	13.1
Riverside	486	490	703	74.2	67.1	71.4	19.2	16.5	18.6
All UCs	2,104	2,130	2,116	75.2	72.1	64.0	42.8	40.9	34.0
Average SAT Score									
Berkeley	1031	1049	1068	1122	1131	1157	1084	1088	1074
UCLA	1013	1027	1050	1103	1142	1176	1073	1106	1121
San Diego	1031	1040	1056	1119	1136	1210	1072	1104	1188
Davis	1009	1015	1030	1058	1064	1092	998	1015	1042
Irvine	978	994	1005	1031	1074	1090	986	1014	1048
Santa Barbara	983	999	1026	1018	1044	1096	967	979	1045
Santa Cruz	1000	1008	1027	1028	1036	1062	980	990	1019
Riverside	951	963	979	978	1006	1014	958	959	967
All UCs	1006	1018	1032	1043	1062	1078	1032	1052	1056
Panel B: Hispanic Applicants									
Average Number or Percent of Applicants									
Berkeley	2,406	2,684	2,763	55.8	47.6	24.2	20.0	18.5	10.4
UCLA	3,512	3,682	3,987	56.0	43.1	25.1	21.5	16.9	11.6
San Diego	2,338	2,470	3,006	60.8	58.3	34.8	12.7	12.1	9.2
Davis	1,821	1,830	2,002	86.3	86.3	65.6	22.3	19.2	18.2
Irvine	2,257	2,123	2,529	74.8	65.5	56.6	16.5	13.9	14.8
Santa Barbara	2,512	2,754	3,110	76.9	78.2	55.6	16.1	17.9	16.0
Santa Cruz	1,760	1,620	1,796	84.7	82.2	74.5	16.3	15.0	16.0
Riverside	1,690	1,763	2,440	81.0	79.9	81.6	19.9	18.9	20.8
All UCs	6,984	7,000	7,416	82.8	81.2	75.9	47.8	44.8	41.2
Average SAT Score									
Berkeley	1083	1098	1110	1158	1180	1212	1141	1158	1164
UCLA	1031	1051	1066	1121	1156	1184	1090	1117	1143
San Diego	1060	1072	1084	1120	1152	1189	1084	1117	1153
Davis	1054	1064	1072	1083	1094	1106	1056	1075	1069
Irvine	995	1013	1025	1039	1067	1094	1001	1025	1061
Santa Barbara	1007	1020	1040	1044	1057	1099	1001	1028	1076
Santa Cruz	1006	1012	1024	1028	1036	1052	982	1004	1036
Riverside	956	966	979	981	991	1005	962	958	965
All UCs	1025	1040	1048	1052	1068	1077	1051	1071	1077

Note: This table shows separate descriptive statistics for Black and Hispanic UC applicants, showing that the former make up only 20 percent of URM students and tend to have somewhat lower average test scores. Count and mean average descriptive statistics of 1994-1999 California-resident freshman Black and Hispanic UC applicants. SAT score includes the Math and Verbal components and was on the 1600 scale. Percent admitted and percent enrolled are conditional on applying to that campus. Source: UC Corporate Student System.

Table A.18: Descriptive Statistics of 1990s UC Admissions for White and Asian Applicants

	Application			Admission			Enrollment		
	'94-5	'96-7	'98-9	'94-5	'96-7	'98-9	'94-5	'96-7	'98-9
Panel A: White Applicants									
Average Number or % of Applications									
Berkeley	5,928	7,244	7,440	39.9	34.1	31.9	13.9	12.4	12.2
UCLA	6,612	8,294	9,156	43.9	38.0	33.1	13.9	13.5	13.2
San Diego	7,586	9,137	9,887	61.8	59.7	47.4	15.1	12.9	11.9
Davis	6,876	7,576	7,675	73.4	74.8	69.8	18.8	19.8	18.1
Irvine	3,671	3,916	4,392	79.9	74.7	69.9	14.8	15.0	15.1
Santa Barbara	7,780	9,541	10,444	86.6	75.7	59.0	21.5	21.3	17.3
Santa Cruz	4,527	5,015	5,169	88.0	87.9	83.9	19.6	21.8	20.4
Riverside	2,152	2,280	3,186	84.2	87.1	91.8	17.0	19.4	15.7
All UCs	17,060	19,486	19,304	85.4	83.0	83.8	44.9	45.4	45.1
Average SAT Score									
Berkeley	1267	1271	1277	1361	1367	1365	1332	1340	1333
UCLA	1224	1224	1239	1318	1324	1341	1268	1280	1302
San Diego	1221	1218	1229	1281	1298	1307	1248	1265	1273
Davis	1202	1202	1206	1245	1238	1242	1211	1203	1204
Irvine	1166	1169	1176	1193	1200	1208	1161	1169	1170
Santa Barbara	1160	1158	1180	1177	1196	1232	1138	1169	1196
Santa Cruz	1183	1179	1183	1198	1193	1200	1174	1169	1173
Riverside	1136	1132	1141	1151	1147	1151	1125	1120	1128
All UCs	1197	1198	1206	1217	1221	1226	1209	1217	1228
Panel B: Asian Applicants									
Average Number or % of Applications									
Berkeley	7,516	8,955	11,041	35.6	31.1	30.1	16.0	15.3	15.0
UCLA	8,970	10,548	13,200	44.8	36.8	34.3	16.4	13.0	13.7
San Diego	7,182	8,703	11,752	64.2	60.3	49.0	15.6	13.1	12.6
Davis	5,690	6,558	8,464	69.1	69.4	65.9	19.0	20.2	17.6
Irvine	7,211	8,237	10,577	74.4	69.6	61.7	22.3	21.6	18.6
Santa Barbara	4,489	4,550	7,432	81.5	73.7	56.2	13.8	13.1	11.4
Santa Cruz	2,558	2,694	4,296	81.2	81.4	78.0	11.9	13.9	14.6
Riverside	4,240	4,502	6,217	80.7	84.8	86.3	13.4	17.3	18.5
All UCs	14,488	16,148	20,548	84.4	84.3	84.1	55.1	54.1	53.6
Average SAT Score									
Berkeley	1238	1245	1254	1379	1382	1370	1352	1354	1341
UCLA	1199	1209	1223	1314	1340	1344	1258	1283	1298
San Diego	1202	1207	1218	1266	1295	1306	1201	1236	1249
Davis	1156	1166	1172	1214	1221	1219	1125	1147	1139
Irvine	1136	1143	1155	1181	1190	1215	1115	1127	1157
Santa Barbara	1112	1117	1150	1139	1156	1214	1080	1116	1177
Santa Cruz	1113	1114	1131	1139	1137	1158	1099	1102	1129
Riverside	1102	1105	1109	1128	1126	1129	1072	1074	1079
All UCs	1167	1177	1184	1196	1203	1209	1184	1198	1210

Note: This table shows descriptive statistics for white and Asian UC applicants before and after Prop 209, showing minimal evidence of differential trends among the two groups after Prop 209 (though Asian applicants' SAT scores were lower but rising faster throughout the period). Count and mean average descriptive statistics of 1994-1999 California-resident freshman non-URM UC applicants who report being either white or Asian. SAT score includes the Math and Verbal components and was on the 1600 scale. Percent admitted and percent enrolled are conditional on applying to that campus. Source: UC Corporate Student System.

Table A.19: Difference-in-Difference Estimates of Post-1998 Black and Hispanic Application by UC Campus

Campus:	UCB	UCLA	UCSD	UCSB	UCI	UCD	UCSC	UCR	Total
Admission conditional on application (%), Black									
Black	49.8	44.4	28.8	22.8	23.7	40.1	14.9	18.3	15.9
	(1.0)	(0.8)	(1.1)	(1.1)	(1.2)	(1.1)	(1.3)	(1.3)	(0.6)
Black × Prop 209	-25.4	-25.5	-20.6	-8.7	-15.3	-27.2	-17.4	-20.9	-16.8
	(1.3)	(1.1)	(1.4)	(1.5)	(1.6)	(1.5)	(1.8)	(1.5)	(0.8)
$\bar{Y}$	33.8	38.2	53.6	68.3	68.7	69.0	82.4	84.7	83.5
Obs.	71,821	85,476	79,947	65,728	57,492	62,326	36,445	35,880	160,180
Admission conditional on application (%), Hispanic									
Hispanic	39.7	34.2	21.6	8.3	19.3	31.3	13.4	14.1	12.7
	(0.7)	(0.6)	(0.6)	(0.6)	(0.6)	(0.6)	(0.6)	(0.7)	(0.3)
Hispanic × Prop 209	-29.9	-26.2	-18.8	0.1	-13.6	-23.3	-12.1	-13.4	-11.1
	(0.9)	(0.7)	(0.8)	(0.7)	(0.8)	(0.9)	(0.8)	(0.8)	(0.4)
$\bar{Y}$	34.3	38.4	53.3	68.1	68.6	69.8	82.3	84.8	83.5
Obs.	77,988	95,495	87,802	74,487	64,688	67,352	42,051	41,654	180,540

Note: This table shows that Black and Hispanic UC applicants generally faced similar declines in UC admissions likelihood after Prop 209, with Black applicants facing larger declines at some campuses. OLS coefficient estimates of β_0 and $\beta_{\cdot 98-99}$ from Equation 2.1, a difference-in-difference model of 1996-1999 URM UC freshman California-resident applicants' UC applications and enrollment compared to non-URM applicants after the 1998 end of UC's affirmative action program. Hispanic students are dropped from the sample in Panel A, and Black students are dropped from Panel B; Native American students are dropped from both panels. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47), and are estimated independently by campus or "Total" (all applicants to any UC campus). Robust standard errors in parentheses. Source: UC Corporate Student System.

Table A.20: Difference-in-Difference Estimates of Post-1998 URM Application and Enrollment by UC Campus

Campus:	UCB	UCLA	UCSD	UCSB	UCI	UCD	UCSC	UCR	Total
Application conditional on UC application (%)									
URM	11.4	8.7	-3.7	-4.8	-9.8	-4.3	-2.9	-6.3	
	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)	(0.3)	
URM × Prop 209	-2.2	-3.8	0.7	-1.0	0.4	0.7	0.3	3.5	
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.4)	
$\bar{Y}$	45.3	55.0	49.5	41.3	35.4	37.9	22.6	23.3	
Obs.	199,321	199,321	199,321	199,321	199,321	199,321	199,321	199,321	
Enrollment conditional on application (%)									
URM	13.6	8.0	2.4	0.7	-5.4	0.2	-4.9	-4.1	3.6
	(0.6)	(0.4)	(0.5)	(0.6)	(0.6)	(0.6)	(0.7)	(0.7)	(0.4)
URM × Prop 209	-9.3	-5.9	-3.3	1.6	2.8	0.2	2.1	1.8	-5.8
	(0.6)	(0.5)	(0.5)	(0.7)	(0.7)	(0.8)	(0.9)	(0.8)	(0.5)
$\bar{Y}$	16.8	14.1	12.3	16.8	17.8	18.9	17.8	18.1	50.1
Obs.	90,254	109,566	98,705	82,240	70,643	75,518	45,087	46,434	199,321
Enrollment conditional on **admission** (%)									
URM	-16.9	-17.0	-16.9	-8.1	-15.9	-14.9	-8.5	-7.0	-1.5
	(1.1)	(0.9)	(0.8)	(0.7)	(0.8)	(0.8)	(0.8)	(0.9)	(0.5)
URM × Prop 209	7.3	6.5	9.9	5.8	6.5	9.1	4.7	4.4	-2.2
	(1.5)	(1.3)	(1.2)	(1.0)	(1.1)	(1.1)	(1.1)	(1.0)	(0.6)
$\bar{Y}$	44.9	39.1	24.9	25.6	27.0	27.4	21.7	21.7	60.6
Obs.	28,755	38,037	48,268	53,513	46,299	51,777	36,850	38,581	163,967

Note: This table shows that URM students were discouraged from applying to Berkeley and UCLA after Prop 209 (though remained more likely than similarly-academically-prepared non-URM students), that URM applicants' likelihood of enrollment declined at the more-selective UCs and increased at the less-selective UCs, and that URM yield rates increased at all UCs after Prop 209 (as shown in Antonovics and Sander (2013)). OLS coefficient estimates of β_0 and $\beta_{\cdot 98-99}$ from Equation 2.1, a difference-in-difference model of 1996-1999 URM UC freshman California-resident applicants' UC applications and enrollment compared to non-URM applicants after the 1998 end of UC's affirmative action program. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47), and are estimated independently by campus or "Total" (all applicants to any UC campus). Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.21: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Enrollment

	UC Campuses by Selectivity				Comm.		CA	Non-CA	Not in
	Most	Middle	Least	CSU	Coll.	Ivy+	Priv.	Univ.	NSC
Panel A: Difference-in-Difference Coefficients									
URM	10.4 (0.4)	-4.6 (0.3)	-2.8 (0.2)	-3.6 (0.3)	-3.7 (0.3)	2.5 (0.1)	1.3 (0.3)	-0.2 (0.2)	0.7 (0.2)
URM × Prop 209	-7.6 (0.4)	1.8 (0.4)	1.8 (0.3)	1.9 (0.4)	1.1 (0.4)	0.3 (0.2)	0.8 (0.3)	1.1 (0.3)	-0.9 (0.3)
$\bar{Y}$	21.9	19.6	6.5	13.8	12.1	2.7	9.3	8.5	6.2
Obs.	199,321	199,321	199,321	199,321	199,321	199,321	199,321	199,321	199,321
Panel B: Estimates of URM × Prop 209 by AI Quartile									
Bottom Quartile	-1.7 (0.6)	-4.9 (0.9)	-0.6 (0.8)	3.4 (1.4)	2.2 (1.2)	-0.1 (0.1)	1.4 (0.8)	0.4 (0.7)	-0.0 (0.8)
Second Quartile	-12.6 (0.8)	4.4 (1.1)	3.2 (0.8)	3.1 (1.0)	1.0 (0.9)	-0.1 (0.1)	1.5 (0.8)	2.3 (0.6)	-2.4 (0.6)
Third Quartile	-16.8 (1.0)	13.0 (1.0)	2.2 (0.6)	-1.4 (0.7)	0.3 (0.7)	-0.1 (0.2)	1.6 (0.8)	1.3 (0.6)	-0.0 (0.6)
Top Quartile	-4.5 (1.1)	1.0 (0.7)	0.5 (0.4)	0.3 (0.5)	0.4 (0.5)	1.1 (0.6)	0.6 (0.7)	0.3 (0.6)	0.1 (0.6)
Panel C: Difference-in-Difference Coefficients (versus 1995)									
URM	10.2 (0.5)	-4.4 (0.5)	-1.8 (0.3)	-5.2 (0.4)	-2.6 (0.4)	2.9 (0.2)	0.8 (0.3)	-1.1 (0.3)	1.3 (0.4)
URM × Prop 209	-7.8 (0.5)	1.5 (0.5)	0.9 (0.3)	3.7 (0.5)	0.4 (0.4)	-0.1 (0.2)	1.3 (0.4)	2.0 (0.4)	-1.7 (0.4)
$\bar{Y}$	22.0	19.4	6.4	14.0	11.7	2.8	8.8	8.6	6.8
Obs.	148,980	148,980	148,980	148,980	148,980	148,980	148,980	148,980	148,980

Note: This table summarizes URM UC applicants' changed university enrollment following Prop 209, with aggregate flows from the more-selective UC campuses cascading to all other sectors of higher education, particularly among second- and third-AI-quartile applicants, and slightly larger flows compared to the '94-95 baseline. Estimates of β_0 and $\beta_{'98-99}$ from Equation 2.1, an OLS difference-in-difference model of 1996-1999 URM UC freshman California-resident applicants' enrollment outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. Outcomes defined as the first institution of enrollment by college or university type within six years of graduating high school, as measured in the NSC. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Panel C omits the years 1996-1997 because some universities preemptively curtailed their affirmative action programs in those years. "Ivy+" universities include the Ivy League, MIT, Stanford, and the University of Chicago; private and non-CA universities exclude those institutions. Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.22: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Enrollment, cont.

	UC Campuses by Selectivity				Comm.		CA	Non-CA	Not in
	Most	Middle	Least	CSU	Coll.	Ivy+	Priv.	Univ.	NSC
Panel D: Estimates with Separate Coefficients for Black and Hispanic Applicants									
Black	17.0 (0.7)	-7.6 (0.5)	-4.7 (0.3)	-6.2 (0.6)	-8.1 (0.5)	3.7 (0.3)	0.9 (0.5)	4.3 (0.5)	0.8 (0.5)
Hispanic	7.9 (0.4)	-3.8 (0.4)	-2.2 (0.2)	-2.6 (0.4)	-2.1 (0.3)	2.1 (0.2)	1.8 (0.3)	-1.8 (0.2)	0.8 (0.3)
Black × Prop 209	-10.6 (0.8)	1.9 (0.7)	1.8 (0.5)	3.2 (0.8)	0.5 (0.7)	0.7 (0.4)	1.7 (0.7)	2.5 (0.7)	-1.5 (0.6)
Hispanic × Prop 209	-6.3 (0.5)	1.8 (0.5)	1.9 (0.3)	1.4 (0.5)	0.9 (0.4)	0.1 (0.2)	0.4 (0.4)	0.8 (0.3)	-0.9 (0.3)
$\bar{Y}$	21.9	19.6	6.5	13.8	12.1	2.7	9.3	8.5	6.2
Obs.	197,804	197,804	197,804	197,804	197,804	197,804	197,804	197,804	197,804
Panel E: Estimates of Black × Prop 209 by Black AI Quartile									
Bottom Quartile	-1.2 (1.4)	-5.9 (1.6)	-0.7 (1.3)	5.7 (3.0)	2.7 (2.4)	0.0 (0.0)	1.3 (1.6)	1.1 (2.0)	-2.3 (1.6)
Second Quartile	-12.4 (1.8)	2.0 (2.1)	3.7 (1.5)	4.8 (2.0)	-2.3 (1.7)	-0.6 (0.4)	0.1 (1.7)	3.9 (1.6)	0.8 (1.2)
Third Quartile	-23.4 (2.2)	15.1 (2.0)	1.2 (1.2)	0.4 (1.3)	-1.2 (1.3)	0.2 (0.6)	4.7 (1.7)	4.5 (1.6)	-0.9 (1.1)
Top Quartile	-14.5 (2.3)	3.2 (1.4)	2.1 (0.8)	-0.0 (0.9)	2.3 (1.0)	2.9 (1.5)	4.6 (1.5)	1.7 (1.6)	-1.9 (1.2)
Panel F: Estimates of Hispanic × Prop 209 by Hispanic AI Quartile									
Bottom Quartile	-1.3 (0.6)	-5.0 (1.0)	0.1 (0.9)	2.9 (1.5)	2.0 (1.3)	-0.0 (0.0)	0.7 (0.8)	0.9 (0.6)	-0.2 (0.9)
Second Quartile	-11.2 (0.9)	6.0 (1.2)	3.0 (0.9)	1.8 (1.1)	1.3 (1.0)	0.0 (0.1)	1.3 (0.9)	1.2 (0.6)	-3.0 (0.7)
Third Quartile	-14.9 (1.1)	11.7 (1.2)	2.5 (0.7)	-1.2 (0.9)	0.1 (0.8)	0.2 (0.2)	0.8 (0.9)	0.9 (0.6)	-0.1 (0.6)
Top Quartile	-2.8 (1.2)	1.0 (0.9)	0.5 (0.4)	0.2 (0.6)	-0.1 (0.6)	0.3 (0.7)	0.3 (0.8)	-0.4 (0.7)	0.8 (0.7)

Note: This table shows that Black UC applicants were more likely to exit the more-selective UC campuses than Hispanic applicants following Prop 209, though they were also more likely to instead enroll at Ivy+ and non-California universities, especially among higher-AI applicants. This table extends Table A.21. Estimates of β_0 and $\beta_{'98-99}$ from an extension Equation 2.1 splitting the URM indicator into separate Black and Hispanic indicators interacted with post-209. The model is an OLS difference-in-difference model of 1996-1999 URM UC freshman California-resident applicants' enrollment outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. Outcomes defined as the first institution of enrollment by college or university type within six years of graduating high school, as measured in the NSC. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Models omit Native American applicants. "Ivy+" universities include the Ivy League, MIT, Stanford, and the University of Chicago; private and non-CA universities exclude those institutions. Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined separately for each ethnicity by the AI distribution of 96-97 URM UC applicants. Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.23: Estimated Change in UC URM Enrollment, '94-95 to '98-99

	Change in App. Pool	Change in Adm. and Yield		
UC Campus	Decrease	Increase†	Decrease†	Total
Berkeley	-93	4	-327	-415
UCLA	-122	0	-496	-618
San Diego	-35	127	-41	50
Santa Barbara	-32	341	-25	284
Irvine	-36	150	-50	64
Davis	-53	91	-140	-103
Santa Cruz	-46	11	-85	-119
Riverside	-38	103	-7	61
Total	-456	827	-1173	-800

Note: This table exploits year-over-year changes in URM and non-URM UC application and enrollment at each UC campus by AI bin to estimate that URM UC enrollment fell by 450 students as a result of application dissuasion and 350 students as a result of changes in UC campuses' URM admissions and yield rates (with particularly-large declines at Berkeley and UCLA), resulting in a net decline in URM UC enrollment of 800 students, or 14 percent of UC's '98-99 URM enrollment. **Change in App. Pool**: For each campus, these estimates show the sum across 200-point AI bins of the positive (increase) and negative (decrease) products of (1) the change in the number of UC applicants by AI bin (see Figure 2.7) and (2) the raw difference-in-difference in URM UC applicants' enrollment at each campus by AI bin (smoothed across bins as in Figure 2.3), where post-209 enrollment is set to 0 (since these students did not apply to UC). **Change in Adm. and Yield**: The sum across AI centiles of the positive (increase) and negative (decrease) products of (1) the number of '98-99 URM UC applicants in each bin, and (2) the raw difference-in-difference in URM UC applicants' enrollment at each campus by AI bin, smoothed across bins. **Both**: Baseline is defined as '94-95 applicants and post-209 defined as '98-99 applicants, with 1994 omitted from the difference-in-difference estimates since '94 NSC data are unreliable. Estimates reported as annual changes in '98-99. The first column is always 0 because URM UC applications declined in every relevant AI bin, resulting in enrollment increases at no campuses. † Estimates of increased and decreased URM enrollment should be interpreted as lower-bound estimates biased toward 0 by overlap in the AI distribution between students exiting and entering each campus. Source: UC Corporate Student System, National Student Clearinghouse, and the California Department of Education.

Table A.24: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Univ. Characteristics

	First Four-Year Institution			First Institution of Enrollment			
	Adm.	Avg.	6 Yr.	"MH" VA[1]		"CFSTY" VA[1]	
	Rate	SAT	BA Rate	BA 6	Earn 30s	BA 6	Earn 30s
Panel C: Difference-in-Difference Coefficients (versus 1995)							
URM	-7.2	39.8	4.1	1.7	1,910	2.8	2,923
	(0.3)	(1.5)	(0.2)	(0.2)	(101)	(0.2)	(115)
URM × Prop 209	3.9	-24.1	-2.5	-0.5	-463	-1.1	-1,085
	(0.3)	(1.7)	(0.2)	(0.2)	(114)	(0.2)	(130)
$\bar{Y}$	51.0	1,188	68.3				
Obs.	128,957	127,138	125,319	131,214	128,628	130,261	128,417
Panel D: Estimates with Separate Coefficients for Black and Hispanic Applicants							
Black	-11.0	52.8	5.4	3.4	3,149	5.2	4,815
	(0.3)	(2.1)	(0.3)	(0.2)	(142)	(0.2)	(154)
Hispanic	-6.1	31.6	2.9	1.5	1,560	2.1	2,305
	(0.2)	(1.2)	(0.2)	(0.1)	(85)	(0.1)	(95)
Black × Prop 209	4.6	-24.7	-2.6	-0.8	-455	-1.5	-1,128
	(0.5)	(2.9)	(0.4)	(0.3)	(197)	(0.3)	(214)
Hispanic × Prop 209	3.3	-17.9	-1.4	-0.5	-328	-0.7	-811
	(0.3)	(1.5)	(0.2)	(0.2)	(103)	(0.2)	(117)
Obs.	172,661	170,293	168,684	176,026	172,571	174,769	172,290

Note: This table shows that the impact of Prop 209 on proxies of UC URM applicants' university quality are generally somewhat larger when compared to the '94-95 baseline, and that Black and Hispanic UC applicants faced similar-magnitude declines in proxies of university quality after Prop 209. This table extends Table 2.2. **Panel C**: Estimates of β_0 and $\beta_{\cdot 98-99}$ from Equation 2.1, a difference-in-difference model of 1995 and 1998-1999 URM UC freshman California-resident applicants' outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. The years 1996-1997 are omitted in Panel C because some universities preemptively curtailed their affirmative action programs in those years. **Panel D**: Estimates of β_0 and $\beta_{\cdot 98-99}$ from an extension Equation 2.1 splitting the URM indicator into separate Black and Hispanic indicators interacted with post-209. The model is an OLS difference-in-difference 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. Models omit Native American applicants. **All**: For details on outcomes and specification, see Table 2.2. Robust standard errors in parentheses. Source: UC Corporate Student System, National Student Clearinghouse, the California Employment Development Department, and the Integrated Postsecondary Education Data System (IPEDS).

Table A.25: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Educational Outcomes

	Earn Bach. Degree 5-Year	Earn Bach. Degree 6-Year	Earn STEM Degree Uncondit.	Earn STEM Degree Condit.	Earn Grad. Degree All	Earn Grad. Degree STEM	Earn Grad. Degree JD
Panel C: Difference-in-Difference Coefficients (versus 1995)							
URM	-1.15	-2.46	0.09	-0.46	5.48	1.43	1.18
	(0.55)	(0.55)	(0.42)	(0.58)	(0.36)	(0.13)	(0.15)
URM × Prop 209	-1.84	-0.91	-0.61	0.25	-3.51	-2.06	-1.03
	(0.62)	(0.62)	(0.47)	(0.65)	(0.48)	(0.18)	(0.19)
$\bar{Y}$	47.33	74.23	22.37	27.43	27.99	4.30	3.76
Obs.	148,980	148,980	148,980	110,588	190,540	190,540	190,540
Panel D: Estimates with Separate Coefficients for Black and Hispanic Applicants							
Black	2.06	-0.77	3.63	4.10	12.87	1.45	3.24
	(0.74)	(0.75)	(0.53)	(0.75)	(0.78)	(0.27)	(0.38)
Hispanic	-3.14	-3.08	-0.71	-0.90	2.15	0.39	0.17
	(0.47)	(0.46)	(0.35)	(0.47)	(0.48)	(0.19)	(0.20)
Black × Prop 209	-0.83	-0.15	-1.54	-1.05	-1.50	-0.05	-0.56
	(0.99)	(1.01)	(0.70)	(1.00)	(1.05)	(0.38)	(0.49)
Hispanic × Prop 209	-0.82	-0.79	-0.62	-0.37	-1.02	-0.73	-0.06
	(0.58)	(0.57)	(0.43)	(0.58)	(0.59)	(0.23)	(0.23)
Obs.	197,804	197,804	197,804	147,795	197,804	197,804	197,804

Note: This table shows that the impact of Prop 209 on URM UC applicants' educational outcomes generally appears somewhat larger when compared to the '94-95 baseline, and that Black and Hispanic UC applicants faced similar relative declines in educational outcomes following Prop 209. This table extends Table 2.3. Estimates of β_0 and $\beta_{'98-99}$ from Equation 2.1, an OLS difference-in-difference model of 1996-1999 (or, in Panel C, 1995 and 1998-1999) URM UC freshman California-resident applicants' educational outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. For details on outcomes and specification, see Table 2.3. The years 1996-1997 are omitted in Panel C because some universities preemptively curtailed their affirmative action programs in those years; 1994 is omitted because NSC records from that year are unreliable. Panel D interacts the two coefficients with Black and Hispanic coefficients to separately estimate effects for each group; Native American applicants are omitted. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Academic Index (AI) is defined in footnote 25. Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.26: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Educational Outcomes

	Earn Bach. Degree		Earn STEM Degree	
	5-Year	6-Year	Uncondit.	Condit.
Panel E: Coefficients measured with only NSC data				
URM	-0.98	-1.33	0.34	0.12
	(0.41)	(0.41)	(0.28)	(0.46)
URM $\times$ Prop 209	-1.01	-1.06	-0.93	-0.43
	(0.51)	(0.51)	(0.35)	(0.57)
$\bar{Y}$	45.86	71.60	18.36	28.93
Obs.	199,321	199,321	199,321	126,481
Panel F: Coefficients in UC data, condit. on UC enrollment				
URM	-5.99	-2.31	0.26	0.24
	(0.63)	(0.57)	(0.52)	(0.60)
URM $\times$ Prop 209	-1.02	0.07	-0.50	-0.27
	(0.82)	(0.74)	(0.68)	(0.77)
$\bar{Y}$	46.81	80.39	29.31	29.81
Obs.	94,469	94,469	94,469	75,943

Note: This table shows that the impact of Prop 209 on URM UC applicants' undergraduate degree attainment generally appears somewhat larger when measured in NSC alone, as a result of imperfect UCSC reporting, and shrinks when the sample is restricted to UC enrollees before and after Prop 209 measured only in UC data). This table extends Table 2.3. Estimates of β_0 and $\beta_{\cdot 98-99}$ from Equation 2.1, an OLS difference-in-difference 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. For details on outcomes and specification, see Table 2.3. Outcomes are measured in NSC alone in Panel D and in UC administrative data alone in Panel E (excluding applicants who do not enroll at a UC campus). Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Academic Index (AI) is defined in footnote 25. Robust standard errors in parentheses. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.27: Difference-in-Difference Estimates of URM UC Applicants' Post-1998 Earned Majors

Major	Baseline	β_{98-99}	(s.e.)	Major	Baseline	β_{98-99}	(s.e.)
Biology	4.4	0.62	(0.25)	Economics	2.0	-0.39	(0.17)
Other Humanities	2.7	0.30	(0.18)	History	2.4	-0.32	(0.17)
International Stud.	1.2	0.23	(0.14)	Mathematics	0.9	-0.29	(0.11)
Film	0.9	0.22	(0.11)	Electrical Eng.	0.8	-0.23	(0.11)
English	3.3	0.18	(0.20)	Law	0.7	-0.20	(0.09)
Biochemistry	0.5	0.17	(0.09)	Sociology	5.0	-0.20	(0.24)
Architecture	0.3	0.15	(0.08)	Computer Science	0.7	-0.18	(0.12)
Criminology	1.0	0.14	(0.11)	Political Science	4.2	-0.18	(0.23)
Chemistry	0.4	0.13	(0.08)	Communications	2.5	-0.17	(0.18)
Environmental Stud.	0.3	0.08	(0.07)	Computer Eng.	0.3	-0.17	(0.07)

Note: This table shows the fields of study that relatively increased and decreased with greatest likelihood among URM UC applicants after Prop 209, with a mix of STEM and non-STEM fields both increasing and decreasing. Estimates of β_{98-99} from Equation 2.1, an OLS difference-in-difference model of 1996-1999 URM UC freshman California-resident applicants' unconditional likelihood (in percentage points) of earning a major in each major group compared to non-URM outcomes after Prop 209. The ten major groups with the largest and smallest β_{98-99} estimates are presented, along with the "baseline" proportion of 1996-1997 URM UC applicants who earned a major in each group. Major choice is measured only in NSC. NSC majors are categorized by the author; full categorization available upon request. The sum across all major groups' baseline values is 61.1 (reflecting URM UC applicants' likelihood of degree attainment); the sum across all major groups' β_{98-99} estimates is -1.24, reflecting the change in NSC-measured graduation after 1998. Source: UC Corporate Student System and National Student Clearinghouse.

Table A.28: Difference-in-Difference Est. of URM UC Applicants' Post-1998 CA Wage Outcomes, cont.

	Average 6-16 Years after UC App.				Average 12-16 Years after UC App.			
	# Years CA Emp.	Total Wages	Log Wages	# > \$100K Wages	# Years CA Emp.	Total Wages	Log Wages	# > \$100 Wages
Panel C: Estimates of URM $\times$ Prop 209 by AI Quartile								
Bottom Quartile	-0.02 (0.11)	-1,095 (995)	-0.06 (0.03)	0.06 (0.06)	0.00 (0.06)	-1,964 (1,430)	-0.09 (0.04)	0.00 (0.04)
Second Quartile	0.10 (0.10)	-1,824 (936)	-0.05 (0.03)	-0.11 (0.06)	0.03 (0.05)	-1,935 (1,361)	-0.04 (0.03)	-0.09 (0.04)
Third Quartile	0.02 (0.09)	-1,595 (935)	-0.03 (0.02)	-0.14 (0.06)	0.02 (0.05)	-2,077 (1,374)	-0.02 (0.03)	-0.09 (0.04)
Top Quartile	-0.10 (0.09)	-1,468 (1,041)	-0.02 (0.02)	-0.06 (0.06)	-0.04 (0.05)	-2,024 (1,553)	-0.03 (0.03)	-0.05 (0.04)
Panel D: Difference-in-Difference Coefficients (versus 1995)								
URM	0.19 (0.04)	343 (391)	0.04 (0.01)	-0.00 (0.02)	0.11 (0.02)	-387 (580)	0.01 (0.01)	0.02 (0.01)
URM $\times$ Prop 209	-0.22 (0.05)	-2,555 (462)	-0.08 (0.01)	-0.19 (0.03)	-0.11 (0.02)	-3,184 (676)	-0.07 (0.01)	-0.15 (0.02)
$\bar{Y}$	7.05	61,107	10.69	1.39	3.07	79,331	10.90	0.95
Obs.	190,540	158,989	158,989	190,540	190,540	136,341	136,341	190,540

Note: This table shows that the labor market deterioration faced by URM UC applicants following Prop 209 was somewhat-larger among low-AI applicants and somewhat-larger when estimated relative to the '94-95 baseline. This table extends Table 2.4. Estimates of β_0 and $\beta_{'98-99}$ from Equation 2.1, an OLS difference-in-difference model of 1996-1999 (or, in Panel D, 1994-1995 and 1998-1999) URM UC freshman California-resident applicants' educational outcomes compared to non-URM outcomes after the 1998 end of UC's affirmative action program. Outcomes are defined as number of years of non-zero California wages, average wages and log wages across years with non-zero wages, and number of years with wages above \$100,000, among the years 6-16 or 12-16 years after initial UC application. Outcomes measured in the California Employment Development Department database, which includes employment covered by California unemployment insurance. The years 1996-1997 are omitted in Panel D because some universities preemptively curtailed their affirmative action programs in those years. Models include high school fixed effects and the components of UC's Academic Index (see footnote 47). Academic Index (AI) is defined in footnote 25; models by AI quartile are estimated independently, with quartiles defined by the AI distribution of 96-97 URM UC applicants. Annual wages CPI-adjusted to 2018 and winsorized at top and bottom 1 percent. Robust standard errors in parentheses. Source: UC Corporate Student System and the California Employment Development Department.

Table A.29: 1994-2001 Change in UC Application Rates in Public CA High Schools by Ethnicity

		All Campuses				Most-Selective Campuses		
		Unweighted	Weighted			Unweighted	Weighted	
Black	1995	0.034	0.023	0.014		0.021	0.016	0.011
		(0.021)	(0.014)	(0.014)		(0.019)	(0.013)	(0.013)
	1996	-0.024	-0.005	-0.012		-0.038	-0.011	-0.011
		(0.021)	(0.015)	(0.015)		(0.019)	(0.013)	(0.013)
	1997	-0.011	-0.016	-0.020		-0.014	-0.025	-0.033
		(0.022)	(0.015)	(0.015)		(0.020)	(0.014)	(0.013)
	1998	-0.013	-0.008	-0.014		-0.029	-0.031	-0.031
		(0.021)	(0.014)	(0.014)		(0.019)	(0.013)	(0.013)
	1999	0.003	-0.024	-0.026		-0.034	-0.054	-0.053
		(0.022)	(0.016)	(0.015)		(0.020)	(0.014)	(0.013)
	2000	-0.005	-0.013	-0.012		-0.018	-0.037	-0.035
		(0.021)	(0.015)	(0.015)		(0.019)	(0.013)	(0.013)
	2001	-0.000	-0.019	-0.023		-0.025	-0.051	-0.051
		(0.021)	(0.016)	(0.015)		(0.019)	(0.013)	(0.013)
Hispanic	1995	0.006	-0.004	-0.004		0.002	-0.005	-0.009
		(0.013)	(0.011)	(0.010)		(0.012)	(0.009)	(0.009)
	1996	-0.016	-0.020	-0.026		-0.011	-0.010	-0.012
		(0.013)	(0.011)	(0.011)		(0.012)	(0.010)	(0.009)
	1997	-0.018	-0.033	-0.035		-0.014	-0.029	-0.036
		(0.014)	(0.011)	(0.011)		(0.013)	(0.009)	(0.009)
	1998	-0.021	-0.026	-0.022		-0.031	-0.029	-0.027
		(0.014)	(0.011)	(0.010)		(0.012)	(0.009)	(0.009)
	1999	-0.036	-0.040	-0.037		-0.051	-0.048	-0.046
		(0.014)	(0.011)	(0.011)		(0.012)	(0.009)	(0.009)
	2000	-0.021	-0.028	-0.029		-0.037	-0.039	-0.036
		(0.014)	(0.011)	(0.011)		(0.013)	(0.010)	(0.009)
	2001	-0.029	-0.026	-0.024		-0.027	-0.029	-0.029
		(0.014)	(0.012)	(0.011)		(0.012)	(0.010)	(0.010)
Asian	1995	0.046	0.018	0.018		0.023	0.002	0.009
		(0.016)	(0.012)	(0.011)		(0.014)	(0.010)	(0.010)
	1996	0.010	0.022	0.021		0.019	0.025	0.026
		(0.017)	(0.012)	(0.011)		(0.014)	(0.010)	(0.010)
	1997	0.018	0.021	0.020		0.029	0.014	0.015
		(0.016)	(0.012)	(0.012)		(0.014)	(0.010)	(0.010)
	1998	0.036	0.025	0.024		0.035	0.009	0.015
		(0.017)	(0.012)	(0.012)		(0.015)	(0.011)	(0.010)
	1999	0.032	0.016	0.009		0.023	-0.004	-0.000
		(0.017)	(0.012)	(0.011)		(0.015)	(0.011)	(0.010)
	2000	0.042	0.017	0.025		0.045	0.004	0.015
		(0.017)	(0.012)	(0.011)		(0.015)	(0.011)	(0.010)
	2001	0.043	0.026	0.029		0.052	0.024	0.025
		(0.017)	(0.012)	(0.012)		(0.015)	(0.012)	(0.011)
HS×Eth.		X	X	X		X	X	X
HS×Year		X	X	X		X	X	X
by Eth.×Gender				X				X
R^2	0.72	0.90	0.82		0.72	0.90	0.83	
Obs.	20,311	20,311	37,622		21,191	21,191	39,008	

Note: This table provides the underlying regression statistics (estimated at the annual level) behind Figure 2.7, showing that URM application rates following Prop 209 declined by between 4 and 6 percent of all UC-eligible URM public high school graduates while Asian application rates remained unchanged after Prop 209 in the main 'weighted' specifications. Estimates of the change in the proportion of California public high school graduates by ethnicity who applied to UC or to UC's more-selective Berkeley and UCLA campuses, relative to 1994. Coefficients are estimates of $\beta_{e,y,a}$ from different specifications Equation 2.2, with annual coefficients and across all AI bins. Columns 1 and 4 are unweighted, columns 2 and 5 are weighted by the number of graduates in each high-school-year (main specification), and columns 3 and 6 disaggregate observations by gender (as well as school-year-ethnicity) and weight by number of graduates. Standard errors in parentheses clustered by high school. Source: UC Corporate Student System and the California Department of Education.

Table A.30: Difference-in-Difference Estimates of URM Students' Post-1998 STEM Grades and Persistence

	SAT %tile	GPA	Persist.	STEM Deg.	SAT %tile	GPA	Persist.	STEM Deg.
URM	-19.0	-0.42	-11.2	-10.3	-7.3	-0.06	-2.0	0.1
	(1.7)	(0.06)	(1.5)	(0.6)	(1.2)	(0.05)	(1.6)	(0.6)
URM $\times$ Prop 209	2.7	0.02	1.5	1.2	4.0	-0.01	0.6	-0.1
	(1.4)	(0.05)	(1.7)	(0.9)	(0.9)	(0.04)	(1.5)	(0.8)
AI Cov. And HS FE					X	X	X	X
$\bar{Y}$	48.9	2.59	59.3	26.0	48.9	2.59	59.3	26.0
# of Obs.	109,489	105,550	85,206	56,160	109,489	105,550	85,206	56,160

Note: This table shows that URM students across five UC campuses had lower STEM class rank, performance, persistence, and STEM major completion before Prop 209, but that these latter three gaps are fully explained by the students' prior academic opportunities and preparation; ending affirmative action had no estimable impact on any of them. Difference-in-difference WLS regression coefficient estimates of 1996-1999 UC enrollees' introductory STEM course rank, performance, or persistence, differencing across URM status and post-1998 following Equation 2.3. In all but the 'STEM Deg' columns, each observation is a student-course pair in an introductory biology, chemistry, physics, or computer science course (see Appendix A.8) 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 the 'STEM Degree' models each observation is a student; the outcome indicates completing any UC STEM degree. Academic preparation covariates include high school fixed effects, and the components of UC's Academic Index (see footnote 47); all models include cohort fixed effects. The sample is restricted to CA-resident freshmen students at UCB, UCSB, UCD, UCSC, or UCR. UCSC is omitted from the GPA model because it did not mandate letter grades in the period. Standard errors (in parentheses) are two-way clustered by student and course, or robust ('STEM Deg'). Source: UC Corporate Student System and UC-CHP Database (Bleemer, 2018*b*).

Table A.31: Difference-in-Difference Estimates of URM UC Enrollees' Post-1998 STEM Outcomes

	Chemistry				Biology		Physics		Comp. Science		
	1	2	3	4	1	2	1	2	1	2	3
Grade in Course (if earned grade)											
URM	0.06 (0.02)	-0.11 (0.04)	-0.22 (0.05)	-0.09 (0.06)	-0.02 (0.04)	-0.18 (0.06)	-0.06 (0.04)	0.04 (0.07)	-0.11 (0.09)	0.15 (0.15)	0.12 (0.15)
URM × Prop 209	-0.09 (0.03)	0.08 (0.05)	0.27 (0.07)	0.07 (0.08)	-0.02 (0.05)	0.09 (0.08)	-0.00 (0.06)	-0.18 (0.09)	-0.02 (0.13)	-0.29 (0.22)	0.01 (0.22)
Acad. Prep.	X	X	X	X	X	X	X	X	X	X	X
$\bar{Y}$	2.53	2.54	2.49	2.65	2.46	2.65	2.73	2.91	2.57	2.61	2.89
Obs.	22,330	14,415	10,632	7,610	12,436	7,639	11,719	6,059	6,027	3,708	2,975
Indicator for Persistence to Next Course (%)											
URM	-1.7 (1.4)	5.1 (1.7)	-10.2 (2.1)		-4.1 (1.9)		-6.3 (2.1)		-8.4 (3.5)	4.1 (5.0)	
URM × Prop 209	1.5 (1.8)	-2.9 (2.3)	8.7 (2.9)		-0.9 (2.5)		5.1 (2.7)		-3.2 (4.6)	-2.9 (6.9)	
Acad. Prep.	X	X	X		X		X		X	X	
$\bar{Y}$	59.9	64.6	68.1		54.0		48.5		55.3	68.7	
Obs.	23,384	14,933	10,954		12,858		12,291		6,638	4,148	

Note: This table shows course-specific regression coefficients mirroring the sixth and seventh columns of Table A.30, showing that URM students at the five observed UC campuses tended to earn lower grades in most STEM courses following Prop 209, with both positive and negative estimates on persistence across different courses. Difference-in-difference OLS regression coefficient estimates across 1996-1999 CA-resident freshman UCB, UCSB, UCD, UCSC, or UCR enrollees' introductory STEM courses, differencing across URM status and post-1998 using Equation 2.3. Persistence indicates completing the subsequent course in the introductory STEM course sequence; course grade is the grade points received in completed courses. Academic covariates include high school fixed effects and the components of UC's Academic Index (see footnote 47). Standard errors (in parentheses) are robust. The specific courses comprising each sequence can be seen in Appendix A.8; courses taken after the first 2.5 years of matriculation are omitted. UCSC is omitted from the GPA model because it did not mandate letter grades in the period. Source: UC Corporate Student System and UC-CHP Database (Bleemer, 2018*b*).

Appendix B

Appendix to Chapter 3

B.1 The Impact of ELC on UC Admissions after 2012

In 2011, the University of California "expanded" its Eligibility in the Local Context program to the top nine percent of graduates from each California high school. It also began calculating each high school's GPA thresholds at each percentile from first to ninth, instead of only the fourth percentile, and each UC campus was directly provided with its applicants' ELC percentiles for admissions purposes.[1] As a result, campuses were subsequently able to provide admissions advantages to their choice of applicant GPA centile.

I analyze the post-2011 impact of ELC eligibility on UC admissions and enrollment by employing the same regression discontinuity research design described in the main text to estimate the effect of barely achieving each GPA centile threshold on admission and enrollment at each UC campus. I follow Equation 3.2 and employ the conservative local linear running variable specification with bias-corrected cluster-robust standard errors (Calonico et al., 2019).[2] The data cover UC admissions from 2012 to 2017 and are restricted to students from the bottom half of California high schools by SAT (B50).

Between 2001 and 2011, the four Absorbing UC campuses provided admissions advantages to the top four percent of B50 graduates from each high school of between 12 and 33 percentage points, leading to 3-5 percentage point enrollment increases at Davis, San Diego, and Irvine (see Table B.15). After 2012, the impact of ELC eligibility on campus admissions was far smaller. Table B.1 shows that only UC's least-selective Merced campus provided an estimable admissions advantage to ELC-eligible students, with eligible students becoming about 10 percentage points more likely to be admitted. There is further evidence that Irvine provided a small (4 p.p.) advantage to students in the top four percent of their graduating class, and perhaps some evidence that San Diego provided an advantage to applicants from the top 1 or 2 percent of their classes. In short, ELC ceased providing meaningful admissions advantages to any campus other than UC Merced.

As a result of these inestimably small admissions advantages provided by ELC eligibility after 2012, ELC ceased substantially shifting near-threshold applicants' UC enrollment decisions.

[1] UC also ceased calculating special "ELC GPA"s, instead relying on high-school-provided grade point averages, and switched to only updating each schools' centile thresholds every three years. It also ceased informing students of their ELC eligibility prior to UC application.

[2] I calculate each of the annual centile thresholds for each high school using the same minimum-error method described in the main text.

Table B.2 replicates the structure of the previous table, replacing each outcome with unconditional enrollment by centile and UC campus. It shows no evidence of any enrollment increases at any UC campus among barely ELC-eligible applicants or applicants barely in the top four percent of their graduating classes. There is not even a measurable enrollment increase at UC Merced among ELC-eligible applicants, despite its small admissions advantage for ELC-eligible applicants. Estimating Equation 3.2 for enrollment at *any* Absorbing UC campus between 2012 and 2017 results in a $\hat{\beta}$ of 1.1 p.p. (s.e. 1.4 p.p.) — and a coefficient of 0.4 (s.e. 1.3) at the fourth percentile threshold — rejecting any ELC-generated enrollment increase that is even a third of the magnitude of the pre-2012 policy's; in fact, the post-2011 policy appears no larger than one-tenth the size of its predecessor in terms of spurring more-selective university enrollment.

As a result of these findings, I assume that ELC played no substantial role in post-2011 UC admissions when constructing the structural model of university decision-making in Section 6 above.

Table B.1: The 2012-2017 Impact of ELC Percentile on Admission to Each UC Campus

	UCB	UCLA	UCSB	UCD	UCSD	UCI	UCR	UCSC	UCM
First Centile	-4.19	-1.94	-0.83	-0.08	3.44 *	1.12	-1.66	-3.35	-2.60
Second Centile	1.36	0.32	-2.70	0.19	5.69 **	1.45	-1.85	1.04	0.85
Third Centile	1.44	2.30	-1.56	-1.58	0.59	-1.98	-1.75	2.80	0.96
Fourth Centile	-0.63	-0.30	0.72	1.48	-0.71	4.05 †	1.57	-0.65	-0.07
Fifth Centile	0.38	0.87	0.22	-0.26	3.78	0.62	-0.62	-0.07	2.04
Sixth Centile	-1.01	-0.07	0.35	-1.86	-1.84	-1.49	-1.00	0.74	-0.04
Seventh Centile	0.51	1.45	0.42	2.57	2.04	0.10	0.71	0.21	1.11
Eighth Centile	-0.77	0.46	0.36	1.76	1.27	0.04	-1.22	-0.61	0.38
Ninth Centile	-0.19	0.44	1.00	0.89	0.44	1.18	-4.83 *	1.47	10.54 **

Note: This table shows that achieving post-2011 ELC eligibility or any of the first to ninth centiles of (within-high-school) ELC GPA rank provided negligible admissions advantages at all UC campuses except for UC Merced, which provided a small admissions advantage to ELC-eligible students. Estimated $\hat{\beta}$ (treatment) coefficients on applicants' likelihood of admission to each UC campus (conditional on application) at each 2012-2017 ELC GPA centile threshold from local linear regression discontinuity estimation, with indicated statistical significance (from 0) estimated by bias-corrected cluster-robust standard errors by school-year (Calonico et al., 2019) following Equation 3.2. Sample restricted to applicants from the bottom half of California high schools by SAT (B50). Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Statistical significance: † 10 percent, * 5 percent, ** 1 percent. Source: UC Corporate Student System

Table B.2: The 2012-2017 Impact of ELC Percentile on Enrollment at Each UC Campus

	UCB	UCLA	UCSB	UCD	UCSD	UCI	UCR	UCSC	UCM
First Centile	-1.60	-0.83	2.25 **	1.46	2.18	-1.31	0.79	-0.16	-0.64
Second Centile	-0.51	-1.14	0.66	-3.07 *	0.26	0.93	0.73	0.20	-0.58
Third Centile	-0.35	0.68	-0.31	-1.27	-0.58	-0.31	0.21	-0.66	0.20
Fourth Centile	-0.02	-0.70	-0.13	0.41	-1.58 †	0.58	0.40	0.16	-0.02
Fifth Centile	0.67	0.26	-0.13	0.37	-0.33	-0.26	0.64	0.16	0.14
Sixth Centile	0.29	0.06	-0.39	-2.40 †	0.51	1.20	-0.07	0.15	0.43
Seventh Centile	0.09	0.47	0.85	0.14	0.50	-0.47	0.51	0.17	0.60
Eighth Centile	-0.15	0.52	0.71	0.08	1.31 *	0.40	-0.16	-0.48	-0.71
Ninth Centile	-0.10	0.00	-0.23	0.78	-0.04	0.51	-0.10	0.45	-0.03

Note: This table shows that achieving post-2011 ELC eligibility or any of the first to ninth centiles of (within-high-school) ELC GPA rank caused no meaningful measurable changes in students' likelihood of enrollment at any UC campus, including UC Merced. Estimated $\hat{\beta}$ (treatment) coefficients on applicants' unconditional likelihood of enrollment at each UC campus at each 2012-2017 ELC GPA centile threshold from local linear regression discontinuity estimation, with indicated statistical significance (from 0) estimated by bias-corrected cluster-robust standard errors by school-year (Calonico et al., 2019) following Equation 3.2. Sample restricted to applicants from the bottom half of California high schools by SAT (B50). Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Statistical significance: † 10 percent, * 5 percent, ** 1 percent. Source: UC Corporate Student System

B.2 Robustness of Regression Discontinuity Design

This appendix discusses several tests of the key smoothness assumption justifying the regression discontinuity design presented in the main text.

B.2.1 Sample Selection Bias

The most common sample selection concern in regression discontinuity settings arises from individuals observing their running variable value and changing their position to end up across the threshold. In the present setting, such "cheating" would involve high school students inflating their grades — for example, by studying harder for certain exams — in order to end up above their school's ELC eligibility threshold. However, ELC's centrally-organized policy structure makes such behavior impossible. Thresholds were recalculated every year using special centrally-calculated ELC GPAs, so students would have been unable to know their own or their peers' UC-calculated ELC GPA ranks prior to being informed of their eligibility. Indeed, even students who strategically switched high schools in order to achieve ELC eligibility (as some students appear to have done in Texas (Cullen, Long and Reback, 2013)) could not have known where they would fall on the ELC GPA running variable, for which reason their presence is not a threat to the research design as presented.

However, the observed data do not contain every ELC-eligible or -ineligible California high school student, but are instead limited to students who apply to at least one of the nine undergraduate UC campuses. There is good reason to think that all students within 0.3 GPA points of their high schools' thresholds — the relevant sample in this study — would apply to at least one UC campus whether or not they were ELC eligible. These are students in the top $\sim$5% of their high school classes, and by 2000 over 14 percent of the average California high school's graduates applied to at least one UC campus. Even bottom-quartile schools by SAT score had an average UC application rate of 9.4 percent, and 9.5 percent of the average school's URM graduates applied to UC.

Moreover, nearly all of the sample's students would be virtually guaranteed to be admitted to several highly-regarded UC campuses. For example, Appendix Table B.15 shows that barely ELC-ineligible students had admissions rates of 91.9 percent at UC Santa Barbara, 98.0 percent at Santa Cruz, and 96.6 percent at Riverside, which had U.S. News & World Report national rankings of 44^{th}, 79^{th}, and 96^{th} in 2008. Even at bottom-quartile (B25) high schools, the admissions rates would be 77.3 percent, 93.0 percent, and 94.1 percent; among URM students, 83.5 percent at Santa Barbara, 94.6 percent at Santa Cruz, and 93.1 percent at Riverside. There are no public research universities in California outside the UC system; students' next-best alternative paying in-state tuition (which in 2008 was $6,200, a small fraction of the cost of comparable alternatives) would be local comprehensive universities in the California State University system.

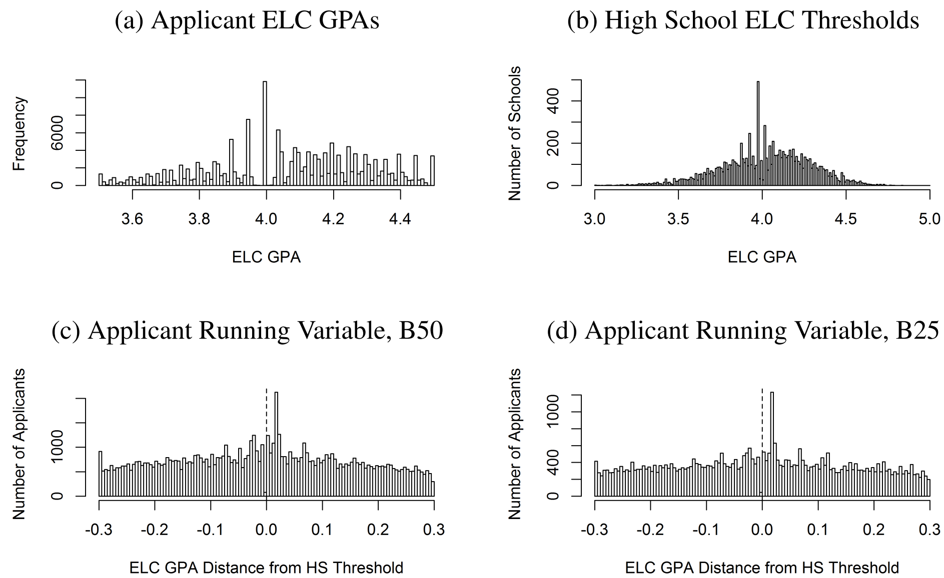
Also, though ELC-eligible students received letters notifying them that they would be guaranteed admission to at least one UC campus if they applied, 94.5 percent of ELC-ineligible students within 0.3 ELC GPA points of their high schools' thresholds received similar letters notifying them that they too would likely be guaranteed admission to at least one UC campus under UC's "Eligibility in the Statewide Context" program, which guaranteed admission to students in the top 12.5% of California high school graduates by a publicly-available linear combination of GPA and SAT score.[3] There is thus little reason to expect that the ELC eligibility letter would cause barely above-threshold students to become meaningfully more likely to apply to UC relative to below-threshold students.

However, a peculiarity of the University of California's ELC eligibility threshold-setting rule interferes the clearest test of the presence of selection into application, the McCrary (2008) test of distributional discontinuity at the eligibility boundary. Because ELC eligibility was determined using ELC GPAs rounded to the nearest hundredth, any students who 'tied' for the 4th percentile GPA were also deemed ELC-eligible. Moreover, because the distribution of ELC GPAs is highly lumpy — see Panel (a) in Figure B.1 — popular ELC GPAs were likely to be chosen as the fourth-percentile threshold, which algorithmically generates a particular 'bunching' pattern immediately above the ELC eligibility threshold. The distribution of thresholds across high schools is shown in Panel (b) of Figure B.1. Note that these figures are not histograms, but complete reflections of the counts of the discrete running variable.[4]

[3] The letter to ELC-ineligible students was somewhat speculative, because UC administrators could not yet observe the students' SAT scores.

[4] In the 1.3% of high-school-years when the estimated ELC eligibility thresholds are not in these discrete bins, as

Figure B.1: Distribution of ELC GPAs, Overall and Around High School Thresholds



Note: This figure shows that the uneven discrete nature of the ELC GPA running variable combines with UC's GPA threshold selection rule (providing ELC eligibility to GPA 'ties') to cause a discrete mass of applicants with running variables exactly 0.02 GPA points above their high schools' thresholds, but there is no other evidence of applicants bunching above the eligibility threshold. (a) Discrete distribution of observed ELC GPAs by hundredth across years. ELC GPAs only observed for California high school seniors in the top 10 percent of their class who did not allow their ELC-participating high school to share their transcript with UC, and who applied to at least one UC campus. (b) Discrete distribution of high schools' estimated ELC eligibility thresholds (see the Data section for estimation details). (c and d) Discrete distribution of the running variable (difference between high school threshold and own ELC GPA), within 0.3 GPA of the threshold, for applicants from the bottom half and quartile of California high schools by SAT (see Footnote 28 for definition of SAT quartiles). Source: UC Corporate Student System.

As a result, Panel (c) of Figure B.1 shows a discrete mass of B50 applicants exactly 0.02 GPA points above the eligibility threshold. There is no evidence of bunching at any other level of the running variable, nor evidence of a decline in the number of applicants below the threshold (a tell-tale pattern of selection); indeed, there appears to be slightly elevated numbers of applicants just below the threshold as well, another artifact of the threshold determination rule (which usually selects the mean between the lowest eligible ELC GPA and the highest ineligible ELC GPA). Panel (d) shows an even more extreme pattern among B25 applicants, since those schools tend to have fewer honors- and AP-level courses available and thus coarser grade point averages. The 0.02 mass point largely reflects schools with minimum eligible ELC GPAs at exactly 3.94, 4.0, and 4.06 (with the next-highest GPA exactly 0.04 points lower). There is no other evidence of distributional discontinuity in the sample.

While both of these distributions fail the McCrary test, it appears unlikely that they do so as a result of selection into UC application among ELC-eligible students, which would generate a more distributed pattern of increased applications just above high schools' thresholds. Sample selection

a result of noise in eligibility reporting around the threshold discussed in the Data section above, I round to the nearest 0.005 for Figure B.1.

Table B.3: Baseline Balance Versus Post-2011 Applicants within 0.1 GPA Points of 4th Percentile Threshold

	Pre-Treatment Dependent Variable						Predicted Values[2]	
	Female (%)	URM (%)	Max. Parent Ed. (Index)[1]	Log Fam. Income	Missing Inc. (%)	SAT Score	Graduation Rate (%)	California Earnings ($)
All	0.55	-0.06	-0.12	-0.072	-1.13	-8.80	-0.32	86.44
	(1.92)	(1.47)	(0.07)	(0.055)	(1.36)	(6.36)	(0.29)	(411)
B50	0.84	1.26	-0.01	-0.030	-0.11	-15.41	-0.22	160
	(3.21)	(2.72)	(0.14)	(0.088)	(1.75)	(12.04)	(0.52)	(566)
B25	3.24	2.50	-0.34	0.051	-3.16	-35.06	-0.88	-75.66
	(4.35)	(3.73)	(0.20)	(0.123)	(2.00)	(17.10)	(0.70)	(706)

Note: Reported coefficients are from difference-in-difference balance estimates of permanent applicant characteristics on an indicator for pre-2012 and above the 4th percentile ELC eligibility threshold, among 2010-2013 UC applicants within 0.1 ELC GPA points of their high schools' ELC eligibility threshold. Sample covers all students and applicants and applicants from the bottom half (B50) or quartile (B25) of California high schools by SAT. Covariates include interactions between a pre-2012 indicator, a 4th percentile indicator, and the running variable (applicants' ELC GPA distance from their high school's threshold), along with high school and year fixed effects. Standard errors (in parentheses) are clustered by school-year. [1]Integer index of reported maximum parental education (across two parents), from 1 (no high school) to 7 (graduate degree). [2]Dependent variable is the predicted values from an OLS regression (across the full sample of 1995-2013 UC freshman California-resident applicants, excluding the study's primary sample) of either five-year NSC graduation or 6-to-8 year average California covered wages (see text for definitions) on gender by ethnicity indicators, maximum parental education indicators (7 categories), family income, missing income indicator, SAT score, and year indicators. UC Corporate Student System.

bias would also likely lead to different observable student characteristics across the eligibility threshold, of which no evidence can be seen in the baseline coefficient estimates presented in Table B.14 despite substantial power and highly-detailed observed demographics.

Difference-in-Difference Comparison with Comparable Post-2011 Applicants

I conduct one further test of selection into application, investigating whether the characteristics of UC applicants in the top four percent of the graduating classes change after the ELC program's admissions advantages ceased in 2012 (see Appendix B.1). While UC continued to identify the fourth percentile of applicants from each school-year, though it made a number of changes after 2011: (a) eligibility was determined using grades submitted by applicants on their UC applications instead of being calculated from high school records, including ninth grade grades; (b) applicants were no longer informed by letter of their eligibility; and (c) GPAs were no longer rounded to the nearest hundredth, and thresholds were only calculated every three years.[5] Nevertheless, if being notified of ELC eligibility encouraged UC application, then the identification of a group of fourth-percentile applicants who did *not* receive notification suggests a test of whether the informed applicants differ from the non-informed on detailed observables. I restrict the sample to 2010-2013 applicants within 0.1 ELC GPA points of their schools' fourth percentile threshold and estimate difference-in-difference regressions of the form:

[5]Because of the change in GPA and threshold calculations implemented by policy-makers, the distributions of students around the ELC eligibility thresholds also changes somewhat, prohibiting direct tests of running-variable distributional similarity.

$$Y_{iy} = \alpha_{h_i} + \gamma_y + \beta_1 Above_i + \beta_2 Above_i Pre_y + \gamma_{1y} GPA_{iy} Above_i + \gamma_{2y} GPA_{iy}(1 - Above_i) + \epsilon_{iy} \tag{B.1}$$

where Y_{iy} is an observed permanent characteristic of individual i who applied to UC in y, $Above_i$ indicates having an ELC GPA above the fourth percentile threshold, Pre_y indicates $y \in (2010, 2011)$, and GPA_{iy} is that year's ELC GPA.[6] Standard errors are clustered by school-year.

Table B.3 shows estimates of Equation B.1's β_2 for the highly detailed set of observable characteristics described above. Estimates are shown for the full sample and for B50 and B25 applicants. Despite substantial precision (e.g. standard errors of 2 percentage points for gender and 6 points for the SAT), the only estimate with a t-statistic greater than 1.5 suggests that under the ELC program, above-threshold B25 applicants had somewhat-*lower* average SAT scores by about 35 points, or 0.15 standard deviations. As in the main text, I construct predicted measures of degree attainment and early-career earnings and find that the estimates of predicted income are precisely estimated 0's, while those of college graduation suggest slight negative selection, with pre-2012 above-threshold B25 applicants having lower expected likelihoods of graduation by 0.9 (s.e. 0.7) percentage points. These evidence suggest that the composition of near-threshold UC applicants did not change after ELC's letter-sending and admissions advantages ceased.

The first panel of Figure B.2 estimates Equation B.1 annually relative to 2012, adding the covariates used in the main analysis: gender-ethnicity indicators and a quadratic of SAT score. It suggests that there is little evidence of a meaningful change in near-threshold applicant characteristics around the ELC eligibility threshold, providing important evidence against sample selection driving the present study's main reduced-form findings. Indeed, the small degree of selection appears to be negative selection, suggesting that those estimates may be slightly conservative.

Panel B provides an additional robustness check on the study's findings by replacing Y_{iy} in Equation B.1 with indicators for enrolling at an Absorbing UC campus and five-year degree attainment. It shows that despite no evidence of differential selection, above-threshold applicants prior to 2012 were significantly and substantially more likely than their post-2011 peers to enroll at Absorbing campuses and earn college degrees (though the latter estimates are somewhat noisy). The estimated coefficients are slightly larger than the regression discontinuity results, though the two are statistically indistinguishable. In short, these difference-in-difference findings provide additional support for a causal interpretation of the study's main reduced-form findings.

B.2.2 Alternative Discontinuities in the Running Variable

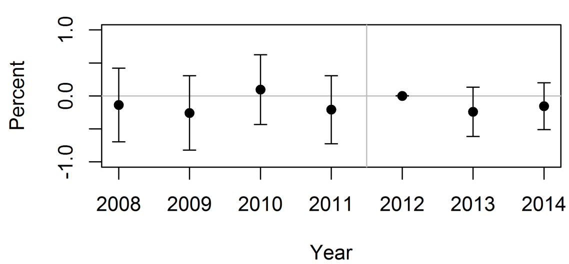
The distribution of ELC GPAs shown in Figure B.1 also suggests a second possible threat to the research design resulting from the apparent non-continuity of the running variable as a measure of student preparedness. The large mass point at 4.00 GPA, for example, could indicate that students at that GPA level are qualitatively different academic performers than those with slightly lower GPAs, perhaps because GPAs above 4.00 are effectively top-censored for students not taking honors-level classes (since even the best students are unable to earn more than 4.00 GPA points in

[6] I allow the γ terms to differ before and after 2011 in order to account for the 2012 change in GPA calculation.

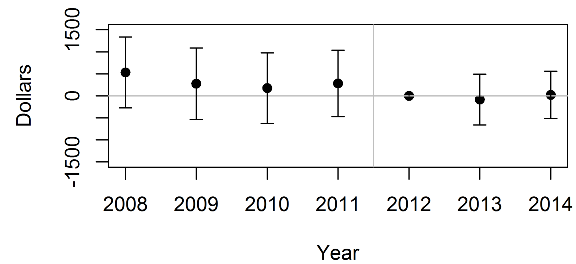
Figure B.2: Difference-in-Difference Estimates after 2011 for Near-Threshold B50 Applicants

Panel A: Predicted Outcomes

(a) Predicted Grad.

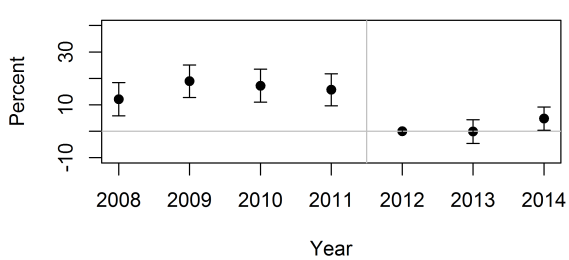


(b) Predicted Post-Grad. Wage

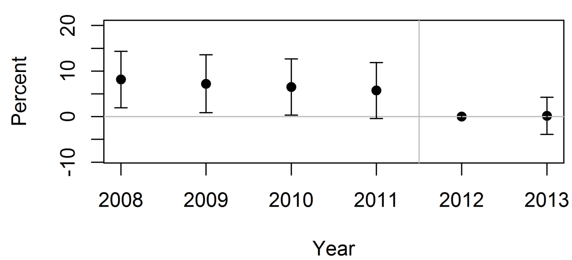


Panel B: Actual Outcomes

(c) Enroll at Abs. UC Campus



(d) Five-Year Degree Attainment



Note: This table replicates the main reduced-form findings in the study in a difference-in-difference design following the end of admissions advantages for top-four-percent applicants after 2011, showing that above-threshold B50 applicants were of similar socioeconomic composition but faced declines in Absorbing UC campus enrollment and degree attainment as would have been anticipated by the regression-discontinuity estimates. Difference-in-difference estimates of applicant characteristics and outcomes on annual indicators interacted with an indicator for being above the 4th percentile ELC eligibility threshold, among 2008-2014 UC applicants within 0.1 ELC GPA points of their high schools' ELC eligibility threshold. Sample restricted to applicants from the bottom half of California high schools by SAT (B50). Covariates include interactions between pre-2012 indicator, a 4th percentile indicator, and the running variable (applicants' ELC GPA distance from their high school's threshold), along with high school and year fixed effects, gender-ethnicity indicators, and a quadratic in SAT score. Predicted graduation rate and wages from an OLS regression (across the full sample of 1995-2013 UC freshman California-resident applicants, excluding the study's primary sample) of either five-year NSC graduation or 6-to-8 year average California covered wages on gender by ethnicity indicators, maximum parental education indicators (7 categories), family income, missing income indicator, SAT score, and year indicators. Absorbing UC Campus enrollment includes San Diego, Davis, Irvine, and Santa Barbara, and is measured from National Student Clearinghouse, as is whether the student has earned a college degree within five years of high school graduation. Degree attainment for the 2014 cohort is not yet observed. Standard errors (in parentheses) are clustered by school-year. Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

such courses). Similar (though lesser) concerns may be present at other GPA mass points. Because UC's ELC eligibility threshold-setting rule tended to set thresholds just below these mass points — since greater mass at a given GPA leads to a greater likelihood of the fourth-percentile student having that GPA — these qualitative differences could positively bias the study's main reduced-form results: GPA mass points tend to be censored from above, suggesting that students at mass points are higher-performing than their GPA evinces.

These arguments justify the removal of high schools with ELC eligibility thresholds at 4.00, which I omit from all reduced-form analysis.[7] Having removed those schools, the clearest test of the presence of running variable discontinuities at the eligibility threshold is the baseline estimates presented in Table B.14, along with the covariate comparisons with post-2011 applicants (for whom mechanical threshold-bunching no longer occurred) presented in Appendix Table B.3. These estimates suggest the absence of positive selection just above the eligibility threshold on detailed demographic and socioeconomic characteristics.

The binned scatterplots in Figure 3.2 provide additional evidence that running variable discontinuities are not biasing the estimates; a discontinuity just above the threshold would lead the fit line to spike upwards approaching the threshold from above, but there is no such pattern in any of the figures.

I further test the continuity of applicant preparedness along the running variable at the 4.00 mass point using the test presented by Caetano (2015), directly estimating the degree of mass-point non-continuity by comparing conditional regression estimates at the mass point to those of local linear regressions around the mass point. The tests include the same covariates as in Equation 3.2, with third-order polynomials in distance to the ELC eligibility threshold, and provide bounds on the bias induced by the running variable's non-continuities. Bandwidths are optimally chosen following Calonico, Cattaneo and Titiunik (2014) as in the local linear estimates presented above, and range from 0.07 to 0.09 GPA points.

4.00 is chosen because it is almost-certainly the mass point with the largest type-discontinuity in the running variable; students with 4.00 GPAs might have been able to perform better if grades above A were available to be earned, and thus may be qualitatively different from students just below that GPA. As a result, the generated estimates are not only upper bounds for the possible degree of bias caused by type discontinuities at the eligibility threshold, but would be expected to be *far* higher than the true observed bias, since high schools with 4.00 eligibility thresholds are omitted from the analysis and most high schools' thresholds are not located at similar discontinuities.

Table B.4 shows the resulting estimates. Unsurprisingly, 4.00 students in the bottom SAT quartile of high schools have significantly upwardly biased SAT scores and predicted likelihoods of graduation, though their expected postgraduate wages are substantially negatively biased. These estimates suggest that if type-discontinuities were driving the observed results, we would expect to observe them in discontinuous student characteristics at the eligibility threshold, though no such differences are observed (see Table B.14).

The remaining columns suggest the possibility for modest bias in enrollment behavior, graduation rate, and early-career California wages, the latter of which appears to be negatively biased. The most-troubling of these results is the possible two percentage-point upward bias in five-year graduation rates. However, consider a worst-case scenario in which half of students' high schools' eligibility thresholds were set at type discontinuities just as biased as the 4.00 bias (an extremely unlikely scenario). In this case, we would expect a 1 percentage point upward bias in our estimate of the impact of ELC eligibility on graduation. In fact, Table B.23 shows that the comparable local-linear estimate of the increase in five-year graduation rate for bottom-quartile applicants is 6.29 p.p. This implies a maximum possible upward bias of 16 percent. In fact, there

[7] In particular, I omit high schools with estimated ELC eligibility thresholds between 3.96 and 4.00. Estimates are highly similar — with no important changes to estimate magnitude or significance — when those schools are included.

Table B.4: Caetano (2015) Endogeneity Test Coefficients at 4.0 ELC GPA

	SAT	Predicted Values Grad. Rate	Predicted Values Earnings	Abs. UC Enr.	Five-Year Grad. Rate	Grad. In Five Years	Early-Career CA Earnings
All	-9.3	-0.01	-17.68	-0.39	0.39	1.42	-2,085
	(0.66)	(0.01)	(31.99)	(0.19)	(0.08)	(0.15)	(419)
B25	12.24	0.06	-243.32	0.52	1.05	2.05	-2,336
	(1.47)	(0.02)	(53.12)	(0.40)	(0.19)	(0.37)	(764)
Bandwidth	0.09	0.09	0.09	0.08	0.07	0.09	0.07

Note: This table shows the potential for small upward biases in reduced-form estimates of barely-eligible applicants' degree attainment resulting from a possible potential-outcome discontinuity in applicants' GPA running variable at exactly 4.0, though as the text explains, the actual biases resulting from such discontinuities is far smaller than those shown in this table (which assume that *every* ELC eligibility threshold is set at exactly 4.0, whereas in fact all such applicants are removed from the main estimation sample). Reported coefficients are endogeneity test coefficients estimated by two-step procedure described in Caetano (2015) around a 4.0 ELC GPA on various outcomes defined in previous tables, with same controls as main specification (indicator for above ELC eligibility threshold, polynomial in distance from threshold, polynomial in SAT, gender/URM indicators, and high school and year FEs), for the full sample and for students from the bottom SAT quartile of high schools. Coefficients are normally distributed with standard errors in parentheses; statistical significance rejects the null hypothesis that the outcome is conditionally continuous at GPA 4.0. Coefficients can be interpreted as the bias induced by endogeneity in the running variable at 4.0. Bandwidths are optimally chosen following Calonico, Cattaneo and Titiunik (2014), and range from 0.07 to 0.09 GPA points. B25 applicants are those from the bottom quartile of high schools by SAT score. Source: UC Corporate Student System, National Student Clearinghouse, and CA Employment Development Department.

is good reason to think that the bias is substantially smaller:

1. Bias of such magnitude would be observable in the detailed characteristics observed for each applicant, but there is no evidence of positive selection on observables (if anything, there is slight evidence of negative selection on SAT score).

2. Most type discontinuities are likely to impose less bias than the discontinuity at 4.00, which is omitted from the sample.

3. Most high schools' thresholds are likely *not* set at meaningfully discontinuous points in the running variable.

As a result of these estimates and others described in the text, I report uncorrected estimates and assume that type discontinuities play a very minor (if any) role in driving this study's results.

B.3 National Student Clearinghouse Data Quality

The National Student Clearinghouse's StudentTracker database contains enrollment and graduation records for nearly all two- and four-year postsecondary institutions in the United States. A nonprofit and nongovernmental organization founded in 1993, NSC collects postsecondary student records and provides degree verification and other services back to contributing universities. Participating universities, including the University of California, are permitted to match their applicants and enrollees by name and date of birth (using NSC's proprietary match algorithm) in order to observe those students' enrollment and degrees at other institutions.[8]

Individual students' enrollment or graduation records may fail to match in the NSC for three reasons: (1) because the student's institution does not report records to NSC; (2) because the student has blocked their record from being shared through NSC; or (3) the student's name and date of birth fail to match using the NSC's match algorithm. NSC reports that about 4 percent of records are censored due to student- or institution-requested blocks for privacy concerns (National Student Clearinghouse Research Center, 2017), and that the only public university in California with censorship greater than 10 percent is UC Berkeley. Dynarski, Hemelt and Hyman (2015) compare aggregate NSC enrollment to aggregate enrollment reported in the federal Integrated Postsecondary Education Data System (IPEDS) and find that enrollment coverage has been greater than 90 percent in California since at least 2003, the first year of data used in the present study, and is near-comprehensive for public institutions. Coverage is shown to generally be poorest at for-profit institutions.

I directly test the quality of NSC coverage for the institutions at which UC applicants tend to enroll in two ways. Using the complete linked UC-NSC database since 1994, I measure institution's NSC participation by identifying the first recorded year in which each institution appears in the NSC records. Table B.5 presents a complete list of California public four-year universities along with all private California four-year universities with at least 500 enrolled students in 1998. The largest institution that still fails to report enrollment to NSC in 2003 was the private 4,400-student University of San Diego, but all California public universities were reporting both enrollment and degree attainment by that year. The largest university to begin reporting degree attainment after 2007, the first year of degree receipience for the first cohort in the present study, was the 648-student San Diego Christian College.

Table B.6 shows similar statistics for the California Community Colleges. As with the private universities, many community colleges did not begin reporting enrollment until the late 1990s or early 2000s, though they reported degree attainment in earlier years. However, by 2003 nearly-all extant schools were reporting enrollment.

Unfortunately, because I only observe enrollment for UC applicants, I cannot directly measure the proportion of enrollees at each California university that appear in the NSC. However, I *can* estimate NSC's data quality for the UC campuses themselves. I first focus on degree attainment, measuring the proportion of UC graduates by campus who are observed as such in the NSC records. The most likely reason for match failure is students' decision to censor their records, as permitted under federal FERPA guidelines, though universities may also choose to censor student records. Table B.7 presents type 2 error rates (that is, false negative rates) by campus and application year. Censorship rates are persistently highest at UCLA and UC Riverside, which had NSC error rates around 5-10 percent annually between 1995 and 2012. The only school to face large non-reporting bias is UC Santa Cruz, which had error rates between 50 and 80 percent from 1995 until the 2000 entering class, suggesting substantial censorship of degrees from that campus. Interestingly, it does not appear that coverage rates are improving over time — indeed, several campuses' error rates were higher in 2012 than in 1995 — nor does it appear that more-selective campuses systematically have lower error rates than less-selective campuses. In general, however, failure rates are very low

[8]For additional documentation, see NSC's "StudentTracker for Systems of Institutions User Manual": https://studentclearinghouse.info/onestop/wp-content/uploads/STSOI_User_Manual.pdf.

Table B.5: Maximum Years that Four-Year Universities in California Began Contributing to National Student Clearinghouse

Institution	1998 Enroll.	In NSC Data Enroll.	In NSC Data Grad.	Univ.	1998 Enroll.	In NSC Data Enroll.	In NSC Data Grad.
University of California							
UC Los Angeles	24,101	1995	1995	UC Irvine	14,336	1995	1995
UC Berkeley	22,259	1995	1996	UC Santa Cruz	9,921	1995	1996
UC Davis	19,258	1995	1995	UC Riverside	9,125	**2000**	1996
UC Santa Barbara	17,048	1996	1995	UC Merced (2005)		2008	2006
UC San Diego	15,818	1995	1995				
California State University							
San Diego State Univ.	25,773	1995	1996	CSU San Bernardino	9,636	1995	1996
CSU Long Beach	22,868	1995	1996	CSU East Bay	9,626	1996	1996
CSU Fullerton	21,279	1996	1997	CSU Dominguez Hills	7,834	1996	1996
San Francisco State Univ.	21,044	1994	1995	Humboldt State Univ.	6,534	1995	1997
CSU Northridge	20,955	1995	1995	Sonoma State Univ.	5,856	**1998**	1996
San Jose State Univ.	20,681	1995	1996	CSU Stanislaus	4,992	1997	1995
CSU Sacramento	18,702	1995	1995	CSU Bakersfield	4,223	**2003**	1996
CA State Poly. Univ.	15,351	1996	1995	CSU San Marcos	4,103	1995	1996
CA Poly. State Univ.	15,347	1995	1996	CSU Monterey Bay	1,716	1995	1997
CSU Fresno	14,518	1995	1996	CA State Univ. Maritime Academy	436	**2006**	1998
CSU Los Angeles	13,935	**2003**	1996	CSU Channel Islands (2002)		2006	2003
CSU Chico	13,196	1996	1997				
Private Universities in California (Undergraduate enrollment $\geq$ 500 in 1998)							
Univ. of Southern CA	15,218	1995	1996	Golden Gate Univ.	1,235	**1998**	1996
Stanford Univ.	6,391	1994	1996	Vanguard Univ. of Southern CA	1,180	**2003**	1996
Univ. of San Francisco	4,570	1995	1996	La Sierra Univ.	1,148	1997	1997
Univ. of San Diego	4,439	**2007**	1997	Loma Linda Univ.	1,137	1995	1998
National Univ.	4,393	1995	1997	Claremont McKenna College	1,024	1996	1996
Loyola Marymount Univ.	4,327	1995	1996	Simpson Univ.	1,021	1996	**2003**
Santa Clara Univ.	4,311	**1999**	1997	CA College of the Arts	1,004	**2006**	1997
Academy of Art Univ.	4,023	1997	1998	Notre Dame de Namur Univ.	983	1997	1996
Saint Mary's College of CA	3,234	1996	1997	The Master's Univ. and Seminary	959	1997	1999
Pepperdine Univ.	3,233	1995	1996	Dominican Univ. of CA	946	**2001**	1998
Univ. of La Verne	3,168	**2005**	1995	Woodbury Univ.	931	1996	1998
Univ. of the Pacific	2,802	1996	1996	Marymount CA Univ.	923	**1998**	1995
Azusa Pacific Univ.	2,795	1996	1996	CA Institute of Technology	901	**2004**	1997
Univ. of Redlands	2,737	1997	1997	Pitzer College	880	1997	1997
Chapman Univ.	2,486	**2001**	1996	CA Institute of the Arts	777	1998	1997
Biola Univ.	2,341	1996	1997	Scripps College	776	1996	1997
Point Loma Nazarene Univ.	2,301	1996	1997	Otis College of Art and Design	763	**2004**	1998
Brandman Univ.	2,125	**2011**	**2003**	Fresno Pacific Univ.	754	1997	1997
CA Lutheran Univ.	1,750	1996	1996	Mills College	741	1996	1997
Mount Saint Mary's Univ.	1,687	1996	1996	Hope International Univ.	706	**1998**	1997
CA Baptist Univ.	1,653	1995	1997	Harvey Mudd College	705	1996	1997
Pomona College	1,571	1996	1995	Concordia Univ.	694	1996	1999
Pacific Union College	1,554	1997	1997	San Diego Christian College	648	**2015**	**2015**
Occidental College	1,529	**1999**	1995	Musicians Institute	559	**2011**	**2011**
Art Center College of Design	1,308	**2008**	1998	Ashford Univ.	555	**2000**	**2001**
Westmont College	1,304	1997	1998	Menlo College	534	**2015**	1997
Whittier College	1,279	1995	1996				

Note: This table shows that all public California universities were reporting enrollment and degree attainment throughout the ELC study period. The largest private California university that did not report degree attainment by the beginning of the study period was the 648-student San Diego Christian College. For all four-year public and private (with more than 500 students in 1998) higher education institutions in California, the earliest year in which any 1995-2016 applicant to any UC campus was recorded in the National Student Clearinghouse as being enrolled at that university or having graduated from that university. Years that might interfere with inference in a study of 1996 (or later) UC enrollees — that is, any years that suggest uniformly missing enrollment records after 1997 or missing graduation records after or in 1996+4=2000 — are in bold. Source: UC Corporate Student System and National Student Clearinghouse

at most campuses for the 2003-2011 cohorts.

Finally, I conduct a similar exercise for STEM major choice, conditional on being recorded as having earned a degree in both the NSC and UC records. Students are defined as studying STEM if their stated major is included on a federally designated list of 278 “fields involving research, innovation, or development of new technologies using engineering, mathematics, computer science, or natural sciences (including physical, biological, and agricultural sciences)” (U.S. Department of Homeland Security, 2016). While six-digit CIP codes are available for UC majors, permitting direct matching to the STEM list, the frequent absence of CIP codes in the NSC required hand-coding of each observed major in the NSC dataset (omitting majors ever earned by fewer than 20 UC applicants). A complete crosswalk is available from the author.

Table B.8 shows the Type 1 and Type 2 error rates in STEM major attainment for each UC graduate by campus and application year. Type 1 errors tend to occur because the UC campus records a major in NSC that was not recorded as STEM, but its CIP code recorded by UC *is* designated as STEM; these cases are very rare at most campuses. Type 2 errors tend to occur because either no major is recorded in the NSC file or a different major is recorded; this appears most prevalent among double-majors, with sometimes only a single major reported to NSC (although NSC allows multiple fields for major reporting). UC Berkeley has remarkably low error rates, never higher than 0.4 percent, while most campuses have Type 2 error rates around 1-5 percent. As in the case of degrees, these very low error rates serve to increase confidence in the reliability of the major-specific estimates reported in the study.

B.4 NSC-Estimated Five-Year Graduation Rates

This appendix describes the novel institutional five-year graduation rate and average SAT score statistics produced to index colleges’ and universities’ selectivity in this study. As discussed in the text, these statistics are calculated for all two- and four-year postsecondary institutions at which at least 100 UC applicants first enroll, making them a much more useful proxy than many alternative selectivity statistics that are unavailable for community colleges (or fail to account for many students’ transferring from those colleges after two years). Specifically, I restrict the sample to 2001-2011 California-resident freshman UC applicants outside this study’s primary sample — that is, applicants without ELC GPAs or with ELC GPAs more than 0.3 GPA points from their high schools’ eligibility threshold — which leaves 618,116 applicants. I assign each applicant to their institution of first enrollment using NSC enrollment records from July of their year of high school graduation to six years later.[9]. I then define each institution’s average SAT score as the average SAT score of assigned applicants, and its five-year graduation rate as the percent of assigned applicants who are reported to have earned a degree in the NSC within five years of high school graduation. 3.0 percent of applicants in this study’s sample do not have any enrollment institution reported within six years of high school graduation, and another 3.0 percent enroll at institutions that fewer than 100 applicants from the full sample had enrolled at in the sample period, for which reason they are omitted (since the university characteristics are noisily estimated).

This appendix contains five tables, covering UC, CSU, California community colleges, and the top and bottom half of private (and out-of-state) universities. Each table presents each in-

[9]If an applicant enrolls at a two-year institution but has changed enrollment to a four-year institution within six months, I assign them to the latter institution

sample institution's 'NSC-measured' graduation rate and average SAT score, along with the same measures from 2008 IPEDS where available. These rates differ for three primary reasons: the UC applicant pool is positively selected relative to other California public institutions (though perhaps negatively selected at some highly-selective private institutions), the NSC-measured graduation rates include degrees obtained at other institutions (following transfer), and they do not include degrees censored from NSC by the institutions. The most notable feature of these new statistics is their inclusion of community colleges, which have NSC-measured graduation rates ranging from 6.6 to over 40 percent.

Table B.9 shows the estimated selectivity statistics for the nine undergraduate University of California campuses, ordered by their NSC-calculated graduation rates. The third and fourth columns show 2008 IPEDS measures of the campuses' average SAT score and five-year graduation rates. The most-selective UC campuses had published graduation rates over 80 percent and average SAT scores over 1900 on the 2400 scale, more than a standard deviation above the median SAT test-taker. The least-selective UC campuses have substantially lower SAT scores and graduation rates, with UC Riverside and Merced each reporting average SAT scores of 1568.[10]

These statistics are relatively closely mirrored in the NSC-calculated statistics shown in the first and second columns. Average SAT scores run from 1942 at UC Berkeley down to 1548 at UC Merced, and graduation rates run from 87.0 to 64.9. The Absorbing UC campuses have five-year graduation rates between 74 and 79 percent.

Table B.10 shows an even greater degree of variation variation in average SAT scores and graduation rates among the California State Universities, California's public comprehensive university system. According to IPEDS, the two institutions with the strongest statistics are the CSU Maritime Academy and California Polytechnic State University in San Luis Obispo (Cal Poly), with average SAT scores between 1575 and 1780 and five-year graduation rates above 55 percent. That graduation rate is on par with the UC Riverside and UC Merced campuses, though Cal Poly's SAT scores are closer to those of the middle UC campuses. Meanwhile, the CSU Los Angeles and Dominguez Hills campuses have far lower measured statistics, with average SAT scores under 1300 and five-year graduation rates around 25 percent.

The institutional quality measures estimated from the UC-applicant NSC database are generally higher than those available from IPEDS, likely as a result of selection into UC application: the CSU enrollees who had also chosen to apply to at least one University of California campus tend to have higher SAT scores and were otherwise more likely to ultimately earn a college degree. Graduation rates are also higher because of high transfer rates between and out of the CSU system, such that more students who first enroll at a given institution end up earning a college degree than the number of students who earn degrees from that particular university. Average SAT scores are only modestly higher, by between 20 and 120 points, but graduation rates exceed IPEDS-reported rates by as much as 20 percentage points (at Sonoma State University).

As a result, the five-year graduation rates observed at a few top CSU institutions are comparable to those of the middle-selectivity University of California campuses, with a 73 percent graduation rate at the small CSU Maritime Academy and graduation rates above 60 percent at Cal Poly, Sonoma State, and San Diego State. The median CSU campus had a five-year graduation rate around 44 percent, while the least-selective CSU campuses had graduation rates just above 30 percent.

[10]Since UC Merced was founded in 2005, it did not yet report a five-year graduation rate in 2008.

Table B.11 does not present IPEDS statistics for the California Community Colleges because graduation rates and average SAT scores are unavailable for two-year institutions. The first two columns show the average SAT score and five-year graduation rates of enrollees at each California Community College, omitting colleges with fewer than 100 UC-applicant enrollees in the sample period. As in the case of the CSU system, these statistics are likely upward-biased snapshots of the actual student body of each college, since CC enrollees who chose to apply to a UC campus after graduating high school were likely positively selected relative to the average CC enrollee. Nevertheless, these selectivity statistics are relevant for the UC applicants who comprise the main estimation sample in this study.

UC-applicant enrollees at many California community colleges are strikingly prepared for university enrollment. About half of all community colleges have measured average SAT scores that are higher than the average SAT score of enrollees at UC Riverside or UC Merced. The college with the highest average observed SAT score is the Foothill College (in California's high-income Silicon Valley), which has an average SAT score among UC applicants of 1739, higher than all but one CSU institution and approximately equal to the average SAT score of enrollees at UC Davis. Indeed, more than a quarter of the 93 observable community colleges have average SAT scores above 1600 among UC applicants, higher than nearly all CSU campuses.

Moreover, the community colleges have relatively high five-year college graduation rates, despite their not awarding Bachelor's degrees themselves. Seventeen community colleges have graduation rates above 35 percent, comparable to the bottom quartile of CSU institutions. One college — Moorpark College, near the Simi Valley outside of Los Angeles — has a graduation rate of almost 45 percent. While some colleges' graduation rates are low, some even below 10 percent, these calculations suggests that large numbers of UC applicants who choose to enroll at community colleges ultimately earn college degrees, making some colleges of comparable selectivity to lower-tier public universities.

Finally, Tables B.12 and B.13 presents statistics for the 200 private and out-of-state univerities with at least 100 UC-applicant enrollees. The schools with the highest graduation rates tend to be private institutions on the East Coast with graduates rates (over 93) and average SAT scores (2000+) considerably higher than the most-selective UC campuses. The median private or out-of-state university in the sample has a graduation rate and average SAT scores comparable to the middle-selectivity UC campuses.

The less-selective private and out-of-state universities, however, shows a small set of outliers — including Harvard University and Mount Holyoke College — that appear to have extremely low graduation rates. These institutions likely do not report degree attainment to National Student Clearinghouse, such that the only reported degrees earned by their enrolled students are from students who transferred and earned degrees elsewhere. While this could be concerning for the graduation rate measures discussed in this study, none of the impacted schools enroll more than a tiny handful of students near their high schools' ELC eligibility thresholds, and (as shown in Table 3.2) their enrollment is unimpacted by (and largely irrelevant to) ELC eligibility. The other schools that actually have the lowest reported graduation rates include out-of-state public universities and several for-profits (like the University of Phoenix and DeVry University), and have SAT scores comparable to the lower-tier CSU campuses. As a result of these outliers (and also because of the other differences discussed above), the correlation between IPEDS and NSC-measured graduation rates is only about 0.56, while the correlation between average SAT scores is over 0.95.

Table B.6: Maximum Years that California Community Colleges Began Contributing to National Student Clearinghouse

Institution	1998 Enroll.	In NSC Data Enroll.	In NSC Data Grad.	Univ.	1998 Enroll.	In NSC Data Enroll.	In NSC Data Grad.
California Community Colleges							
Pasadena City College	16272	**1998**	1995	West Valley College	4952	**2001**	1995
Orange Coast College	15759	**2000**	1995	Mt San Jacinto C.C. District	4805	1997	1995
Cerritos College	15703	1995	1997	Irvine Valley College	4793	1995	1995
Mt San Antonio College	15073	1996	1995	College of the Desert	4768	1996	1998
San Diego Mesa College	14527	**1998**	2004	Skyline College	4687	1996	1995
City College of San Francisco	13679	**2001**	1995	Ohlone College	4667	1997	1996
Riverside City College	13542	1996	1995	Merced College	4601	**1999**	1995
El Camino C.C. District	13379	1997	1995	Allan Hancock College	4593	1995	1995
American River College	13031	**1999**	1995	MiraCosta College	4307	1996	1995
Santa Monica College	12801	1996	1995	Coastline C.C.	4157	**2000**	2001
Fullerton College	12390	**1998**	1995	Imperial Valley College	4103	**2001**	1995
Palomar College	12338	**1998**	1995	Hartnell College	4093	1995	1997
Diablo Valley College	12229	1997	1995	Mission College	3963	**2001**	1995
De Anza College	11919	1995	1995	San Diego Miramar College	3905	**1998**	2004
Santa Rosa Junior College	11727	**1998**	1995	Victor Valley College	3680	**2001**	1998
Fresno City College	11491	**1998**	1995	Los Medanos College	3632	**2000**	1995
Long Beach City College	11247	1995	1995	Las Positas College	3508	1996	1995
Grossmont College	10976	**1999**	1998	Cuyamaca College	3463	**1999**	2003
Sacramento City College	10273	**1999**	1995	College of the Redwoods	3445	**2000**	1995
Sierra College	10113	1996	1995	Los Angeles Harbor College	3375	**1999**	1995
Modesto Junior College	9790	**2000**	1996	Los Angeles Trade Tech. College	3362	**1999**	1995
Southwestern College	9620	**2001**	1995	Contra Costa College	3237	**2001**	1995
San Diego City College	9574	**1998**	2004	Copper Mountain C.C.	2942	**1999**	2000
Chaffey College	9408	1997	1995	West Los Angeles College	2929	**1999**	1995
Citrus College	9317	**2000**	1995	Monterey Peninsula College	2913	**1998**	2002
Glendale C.C.	8672	**2001**	2001	Napa Valley College	2886	**1998**	1995
San Joaquin Delta College	8432	**1998**	1995	College of Marin	2881	**2000**	1995
Chabot College	8418	1996	1995	Oxnard College	2728	1996	1995
Rio Hondo College	8146	**2001**	1995	Crafton Hills College	2514	1995	1995
Cosumnes River College	7843	**1999**	1995	College of Alameda	2246	1997	1995
College of the Sequoias	7788	**2006**	1995	Los Angeles Southwest College	2112	**1999**	1995
Bakersfield College	7762	**2000**	1995	Los Angeles Mission College	2097	**1999**	1995
Cypress College	7718	**1998**	1995	Canada College	2094	1996	1995
Santa Barbara City College	7689	**1998**	1995	West Hills College	2086	**2001**	1995
Saddleback College	7673	1995	1995	Merritt College	1969	1997	1997
Santa Ana College	7629	1996	1995	Cerro Coso C.C.	1889	**2000**	1998
Moorpark College	7414	1996	1995	Porterville College	1692	**2000**	1998
East Los Angeles College	7151	**1999**	1995	Gavilan College	1650	**2010**	1995
Los Angeles Pierce College	6984	**1999**	1995	Mendocino College	1647	**1998**	1997
Golden West College	6961	**2000**	1997	Berkeley City College	1528	1997	2000
Butte College	6804	**1998**	2000	Barstow C.C.	1434	**1998**	1995
Los Angeles City College	6772	**1999**	1995	Columbia College	1328	**2000**	1997
Cuesta College	6644	1995	1995	College of the Siskiyous	991	**1998**	1998
Evergreen Valley College	6461	**2002**	1998	Lake Tahoe C.C.	910	**2001**	1995
College of San Mateo	6349	1996	1995	Lassen C.C.	837	**1998**	1995
Los Angeles Valley College	6337	**1999**	1995	College of the Canyons	637	**1998**	1995
San Jose City College	6230	**2002**	1995	Taft College	578	**2012**	1995
Foothill College	5836	1996	1995	Feather River C.C. District	486	**1998**	1995
Cabrillo College	5820	1996	1995	Palo Verde College	370	**2009**	2010
Solano C.C.	5602	**1998**	1995	Santiago Canyon College (2001)		2009	2001
Shasta College	5462	**1999**	1997	Folsom Lake College (2004)		2005	2004
Yuba College	5358	**2001**	1995	West Hills College (2006)		2007	2006
Antelope Valley College	5156	**1998**	1998	Woodland C.C. (2009)		2010	2009
Reedley College	5004	**1998**	1995	Moreno Valley College (2010)		2011	2010
Ventura College	4980	1996	1995	Norco College (2010)		2011	2010
Laney College	4978	1997	1997	Clovis C.C. (2016)		2016	2016
San Bernardino Valley College	4968	1995	1996				

Note: This table shows that nearly all California Community Colleges were reporting enrollment to NSC by the start of the study period. For all community colleges in California, the earliest year in which any 1995-2016 applicant to any UC campus was recorded in the National Student Clearinghouse as being enrolled at that college or having graduated from that college. Years that might interfere with inference in a study of 1996 (or later) UC enrollees — that is, any years that suggest uniformly missing enrollment records after 1997 or missing graduation records after or in 1996+4=2000 — are in bold. Source: UC Corporate Student System and National Student Clearinghouse

Table B.7: National Student Clearinghouse Degree Data Quality for UC Graduates

Year	UCB	UCD	UCLA	UCR	UCSD	UCSC	UCSB	UCI	UCM
1995	1.3	1.9	5.0	11.6	1.5	79.1	1.7	3.5	
1996	2.5	2.3	6.0	13.1	1.5	78.1	1.6	2.8	
1997	0.9	1.7	6.1	8.2	1.1	74.4	1.6	2.4	
1998	1.5	2.0	6.1	5.2	1.9	69.1	1.4	2.2	
1999	1.2	1.3	5.9	7.3	2.1	70.1	1.2	1.9	
2000	1.4	1.5	7.8	8.6	1.9	55.9	1.1	1.8	
2001	1.2	1.9	6.8	9.2	1.3	5.7	0.9	2.6	
2002	1.1	1.6	6.7	10.5	1.6	2.1	1.8	2.6	
2003	0.3	1.8	6.7	9.9	1.8	2.8	1.9	1.7	
2004	0.8	2.7	6.0	9.5	1.9	2.9	1.7	1.9	
2005	1.2	2.2	6.6	9.0	2.0	2.1	2.5	2.1	1.5
2006	1.4	2.2	8.4	8.5	2.0	3.5	2.4	1.8	0.5
2007	1.1	2.6	8.9	8.7	2.1	3.4	2.0	1.6	3.0
2008	1.1	2.8	7.6	9.4	2.8	3.0	2.2	1.9	1.3
2009	1.2	3.2	7.7	8.1	2.0	3.5	2.9	2.4	1.5
2010	1.5	2.7	8.1	7.6	2.6	2.7	3.1	2.4	1.4
2011	2.4	2.7	6.5	9.7	2.8	3.3	3.5	2.4	0.9
2012	0.4	2.1	4.4	7.3	1.7	2.0	2.5	2.2	1.6

Note: This table shows low levels of missing NSC degree attainment records for UC graduates identified in administrative data throughout the study period. The proportion of UC graduates (within five years of first enrollment), among freshman California-resident enrollees, who are not recorded as having graduated within five years of graduating in their matched National Student Clearinghouse record, by UC campus and year of first enrollment. Source: UC Corporate Student System and National Student Clearinghouse

Table B.8: National Student Clearinghouse STEM Major Data Quality for UC Graduates

Year	UCB		UCD		UCLA		UCR		UCSD		UCSC		UCSB		UCI		UCM	
Err. Type:	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2	1	2
1996	0.2	0.0	0.7	6.3	1.2	3.2	3.0	1.7	3.0	2.7	11.7	2.0	2.6	2.8	1.3	3.1		
1997	0.4	0.1	1.6	5.6	0.4	2.5	3.1	2.2	2.0	4.0	6.8	2.1	1.9	3.5	1.5	3.4		
1998	0.3	0.4	0.9	5.8	0.4	3.0	5.8	2.4	1.6	3.1	8.1	0.5	2.3	2.9	0.7	2.7		
1999	0.1	0.1	0.6	6.0	0.2	2.8	3.7	1.4	1.6	2.9	5.3	2.2	2.8	1.8	0.6	2.1		
2000	0.3	0.2	1.2	6.9	0.4	2.4	6.0	1.6	1.0	4.6	10.1	5.1	2.3	2.4	0.9	2.9		
2001	0.2	0.2	0.9	4.7	0.3	2.6	6.2	1.1	1.7	4.4	6.6	4.9	2.3	1.2	1.7	1.8		
2002	0.1	0.2	0.8	5.2	0.3	2.2	3.8	1.7	1.1	3.5	6.3	6.3	1.7	2.0	1.4	2.7		
2003	0.1	0.0	1.1	5.2	0.3	2.7	5.0	1.1	1.0	4.2	4.2	11.5	1.7	1.9	1.2	1.9		
2004	0.2	0.2	1.1	4.7	0.3	2.5	3.7	1.3	1.0	3.3	5.9	15.3	1.9	1.9	1.2	2.5		
2005	0.1	0.2	1.5	4.5	0.7	2.6	6.4	1.1	1.3	4.0	5.2	8.3	2.5	2.7	1.4	2.5	4.8	0.6
2006	0.0	0.1	1.0	4.5	0.4	2.2	5.0	0.5	1.9	3.1	4.3	7.1	2.4	1.7	0.8	2.5	5.9	0.0
2007	0.2	0.0	1.0	2.9	0.1	2.6	3.8	0.7	1.1	4.4	2.9	6.1	1.9	2.0	1.1	2.6	11.0	0.0
2008	0.1	0.1	0.7	4.0	0.3	2.0	4.0	0.8	0.8	3.0	3.7	5.8	1.5	2.0	1.1	2.2	2.6	0.5
2009	0.0	0.1	0.5	3.7	0.1	2.4	3.9	1.0	0.8	2.9	2.5	2.8	1.5	2.2	1.0	2.4	4.1	0.3
2010	0.1	0.1	0.2	3.2	0.1	1.6	4.0	0.4	0.7	2.2	3.5	2.5	1.5	1.1	0.6	2.0	2.7	0.2
2011	0.1	0.3	0.6	2.5	0.1	1.7	2.7	0.5	0.9	2.1	2.3	2.0	0.8	1.8	1.0	2.7	2.7	0.9
2012	0.1	0.7	0.2	4.1	0.2	2.9	3.3	0.9	0.5	1.8	2.3	1.6	1.4	2.9	0.8	2.9	4.4	1.1

Note: This table shows NSC's very low error rates in identifying UC students who earned STEM degrees throughout the study period. The Type 1 and Type 2 error rate in measurement of STEM major (among students denoted as graduates in base-truth UC records and linked to NSC degree records within five years of first enrollment) among freshman California-resident enrollees. Type 1 error (false positive) indicates non-STEM graduates listed with STEM majors in NSC; Type 2 error (false negative) indicates STEM graduates listed without STEM majors in NSC. STEM defined in U.S. Department of Homeland Security (2016), with NSC majors hand-coded in the absence of CIP codes. Source: UC Corporate Student System and National Student Clearinghouse

Table B.9: University of California Campuses

	NSC		IPEDS			NSC		IPEDS	
Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT	Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT
UC Berkeley	82.3	1941	87	1995	UC Davis	74.3	1756	77	1740
UCLA	80.2	1886	88	1928	UC Santa Cruz	72.7	1715	68	1702
UC San Diego	79.4	1884	80	1868	UC Riverside	63.7	1586	60	1568
UC Irvine	79.3	1773	78	1755	UC Merced	58.0	1547		1568
UC Santa Barbara	78.5	1791	76	1778					

Note: This table presents selectivity statistics for the nine undergraduate University of California campuses, showing that the Absorbing UC campuses fall relatively in between the most-selective Berkeley and UCLA campuses and the less-selective Santa Cruz, Riverside, and Merced campuses. University of California estimated graduation rates and average SAT scores. 'NSC' statistics measured from 2001-2011 UC freshman California-resident applicants assigned by first institution of enrollment (using National Student Clearinghouse data), with '5-Yr. G.R.' measuring the percent of those applicants who had earned a Bachelor's degree within five years of high school graduation (according to NSC records) and 'Avg. SAT' measuring their average SAT score. 'IPEDS' presents statistics as publicly reported in 2008. Institutions are ordered by NSC graduation rate. Source: National Student Clearinghouse, UC Corporate Student System, and Integrated Postsecondary Education Data System (IPEDS).

Table B.10: California State University Campuses

	NSC		IPEDS			NSC		IPEDS	
Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT	Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT
CA State Univ. Maritime Academy	73.2	1673	57	1575	CSU Fullerton	44.0	1531	38	1470
CA Poly. State Univ.	67.4	1796	60	1778	CA State Poly. Univ.	42.1	1590	38	1530
Sonoma State Univ.	63.0	1611	43	1522	CSU Northridge	39.9	1463	29	1410
San Diego State Univ.	62.4	1627	53	1575	San Jose State Univ.	39.0	1549	26	1492
CSU Chico	59.1	1607	45	1515	CSU East Bay	38.7	1433	35	1365
CSU Monterey Bay	51.4	1519	30	1470	Humboldt State Univ.	38.1	1595	32	1552
CSU San Marcos	47.7	1503	34	1455	CSU Sacramento	37.4	1489	30	1440
CSU Long Beach	47.6	1570	40	1515	CSU San Bernardino	37.2	1393	34	1328
CSU Fresno	46.9	1480	37	1388	CSU Bakersfield	36.7	1427	33	1380
San Francisco State Univ.	45.6	1541	32	1500	CSU LA	30.6	1373	23	1298
CSU Stanislaus	45.3	1464	45	1425	CSU Dominguez Hills	30.1	1340	24	1222
CSU Channel Islands	44.1	1509							

Note: This table presents selectivity statistics for the California State University system, showing that the campuses range in selectivity from schools that look similar to the least-selective UC campuses to schools that have considerably lower graduation rates. California State University estimated graduation rates and average SAT scores. 'NSC' statistics measured from 2001-2011 UC freshman California-resident applicants assigned by first institution of enrollment (using National Student Clearinghouse data), with '5-Yr. G.R.' measuring the percent of those applicants who had earned a Bachelor's degree within five years of high school graduation (according to NSC records) and 'Avg. SAT' measuring their average SAT score. 'IPEDS' presents statistics as publicly reported in 2008. Institutions are ordered by NSC graduation rate. Source: National Student Clearinghouse, UC Corporate Student System, and Integrated Postsecondary Education Data System (IPEDS).

Table B.11: CA Community Colleges

Institution	NSC 5-Yr. G.R.	NSC Avg. SAT	IPEDS 5-Yr. G.R.	IPEDS Avg. SAT	Institution	NSC 5-Yr. G.R.	NSC Avg. SAT	IPEDS 5-Yr. G.R.	IPEDS Avg. SAT
Moorpark C.	43.5	1674	-	-	Cuesta C.	25.1	1678	-	-
Saddleback C.	41.0	1689	-	-	Cuyamaca C.	25.0	1545	-	-
Las Positas C.	40.1	1677	-	-	Reedley C.	24.9	1512	-	-
C. of San Mateo	40.1	1623	-	-	Berkeley City C.	24.9	1673	-	-
Ohlone C.	39.8	1644	-	-	El Camino C.	24.8	1511	-	-
Folsom Lake C.	38.6	1718	-	-	Yuba C.	24.8	1508	-	-
C. of Marin	38.5	1723	-	-	San Joaquin Delta C.	24.6	1499	-	-
Diablo Valley C.	37.9	1651	-	-	Cabrillo C.	23.9	1628	-	-
Santa Barbara City C.	37.7	1637	-	-	Mission C.	23.7	1592	-	-
De Anza C.	37.4	1660	-	-	San Jose City C.	22.9	1545	-	-
Shasta C.	37.0	1652	-	-	C. of the Redwoods	22.6	1665	-	-
Skyline C.	36.8	1563	-	-	LA Valley C.	22.5	1515	-	-
MiraCosta C.	36.7	1683	-	-	Laney C.	22.5	1495	-	-
Irvine Valley C.	36.4	1678	-	-	Merritt C.	22.4	1467	-	-
Foothill C.	36.1	1739	-	-	Los Medanos C.	22.3	1497	-	-
Glendale C.C.	35.7	1568	-	-	Bakersfield C.	22.0	1557	-	-
West Valley C.	35.0	1698	-	-	Cosumnes River C.	21.9	1531	-	-
Orange Coast C.	34.5	1624	-	-	Coastline C.C.	21.8	1613	-	-
Sierra C.	34.0	1663	-	-	Antelope Valley C.	21.5	1511	-	-
Canada C.	32.2	1633	-	-	Modesto Junior C.	21.2	1554	-	-
Santa Rosa Junior C.	31.7	1703	-	-	Citrus C.	20.6	1505	-	-
Palomar C.	31.7	1642	-	-	Long Beach City C.	20.0	1499	-	-
C. of the Canyons	30.9	1599	-	-	Allan Hancock C.	19.5	1543	-	-
City C. of San Francisco	30.5	1573	-	-	Grossmont C.	19.2	1557	-	-
Butte C.	30.2	1616	-	-	LA Mission C.	19.0	1430	-	-
Santa Monica C.	30.0	1583	-	-	Crafton Hills C.	18.5	1522	-	-
Sacramento City C.	30.0	1562	-	-	Oxnard C.	18.5	1439	-	-
Santiago Canyon C.	29.8	1652	-	-	C. of the Sequoias	17.9	1448	-	-
Contra Costa C.	29.8	1464	-	-	LA Harbor C.	16.5	1465	-	-
Golden West C.	29.2	1594	-	-	West Hills C.	16.5	1400	-	-
LA Pierce C.	29.2	1585	-	-	Cerritos C.	16.2	1460	-	-
San Diego Miramar C.	29.2	1623	-	-	Imperial Valley C.	16.1	1401	-	-
Napa Valley C.	28.2	1571	-	-	San Diego City C.	15.8	1449	-	-
American River C.	28.1	1608	-	-	Hartnell C.	15.6	1477	-	-
Solano C.C.	28.1	1574	-	-	Chaffey C.	15.5	1489	-	-
San Diego Mesa C.	27.9	1587	-	-	Southwestern C.	15.2	1443	-	-
Ventura C.	27.7	1554	-	-	Merced C.	15.2	1422	-	-
Pasadena City C.	27.4	1586	-	-	Rio Hondo C.	14.8	1463	-	-
Chabot C.	27.3	1519	-	-	Mt San Jacinto C.C.	14.3	1500	-	-
C. of Alameda	27.0	1440	-	-	Victor Valley C.	13.5	1473	-	-
Fullerton C.	26.8	1619	-	-	West LA C.	13.5	1479	-	-
Evergreen Valley C.	26.5	1526	-	-	C. of the Desert	13.3	1430	-	-
Mt San Antonio C.	26.4	1559	-	-	Riverside City C.	12.5	1452	-	-
Santa Ana C.	26.0	1533	-	-	East LA C.	11.7	1401	-	-
Fresno City C.	25.5	1494	-	-	LA City C.	11.1	1463	-	-
Monterey Peninsula C.	25.5	1632	-	-	LA Trade Tech. C.	7.1	1293	-	-
Cypress C.	25.4	1610	-	-	San Bernardino Valley C.	6.6	1422	-	-

Note: This table presents selectivity statistics for the California Community College system, showing that many community colleges have average SAT scores comparable to middle-selective public universities, though their five-year graduation rates tend to be comparable only to the least-selective universities. California Community College estimated (Bachelor's) graduation rates and average SAT scores, among colleges with at least 100 enrollees among applicants in the NSC sample. 'NSC' statistics measured from 2001-2011 UC California-resident freshman applicants assigned by first institution of enrollment (using National Student Clearinghouse data), with '5-Yr. G.R.' measuring the percent of those applicants who had earned a Bachelor's degree within five years of high school graduation (according to NSC records) and 'Avg. SAT' measuring their average SAT score. 'IPEDS' statistics unavailable for community colleges. Institutions are ordered by NSC graduation rate. Source: National Student Clearinghouse, UC Corporate Student System, and Integrated Postsecondary Education Data System (IPEDS).

Table B.12: Top Half of Private and Out-of-State Universities (by Grad. Rate)

	NSC		IPEDS			NSC		IPEDS	
Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT	Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT
Bates C.	96.7	1893	89		Santa Clara Univ.	87.1	1819	84	1822
Swarthmore C.	95.4	2103	91	2152	Kenyon C.	87.0	1965	88	2002
Williams C.	94.7	2085	95	2130	Univ. of San Diego	86.8	1798	74	1785
Bowdoin C.	94.4	2017	89	2108	Macalester C.	86.8	2015	87	2040
Haverford C.	94.3	2061	94	2085	Univ. of Portland	86.3	1794	70	1792
Northwestern Univ.	93.6	2110	93	2152	Whitworth Univ.	86.3	1804	75	1808
Claremont McKenna C.	93.5	2002	94	2100	Johns Hopkins Univ.	86.0	2085	88	2100
Pomona C.	93.1	2099	94	2212	Univ. of Southern CA	85.9	1961	86	2055
Princeton Univ.	93.0	2167	95	2228	Univ. of North Carolina at Chapel Hill	85.4	2003	83	1958
Wesleyan Univ.	92.6	2063	92	2092	Stanford Univ.	85.4	2142	92	2152
Middlebury C.	92.6	2036	93	2092	Univ. of Virginia	84.9	2019	92	1995
Carleton C.	92.4	2052	92	2100	Bryn Mawr C.	84.4	1944	85	1958
Brown Univ.	92.4	2098	92	2145	Colorado C.	84.3	1966	86	1972
Yale Univ.	92.4	2180	95	2242	Pepperdine Univ.	84.1	1816	80	1860
Tufts Univ.	92.3	2068	91	2130	Seattle Univ.	84.0	1796	68	1718
Duke Univ.	92.3	2127	88	2160	Southern Methodist Univ.	84.0	1854	72	1868
Amherst C.	92.2	2093	93	2130	New York Univ.	83.9	1992	83	2018
Colby C.	92.0	1981	90	2032	Brandeis Univ.	83.6	1991	88	2055
Univ. of Pennsylvania	91.6	2126	94	2138	Miami Univ.	83.5	1787	40	1770
Wellesley C.	91.3	2062	90	2051	Lehigh Univ.	83.2	1922	83	1972
Dartmouth C.	91.2	2099	94	2160	Boston Univ.	83.0	1894	79	1905
Wheaton C.	90.9	1792	81		Brite Divinity School	83.0	1769	67	1748
Connecticut C.	90.5	1870	87	1988	Clark Univ.	83.0	1830	72	1800
Georgetown Univ.	90.4	2050	92	2032	Loyola Marymount Univ.	82.7	1749	78	1755
Skidmore C.	90.4	1882	81	1890	Trinity Univ.	82.6	1886	80	1935
Whitman C.	90.4	2006	91	1980	George Washington Univ.	82.4	1932	80	1935
Davidson C.	90.2	2022	93	2046	Univ. of Wisconsin Extension	82.0	1842	78	1905
Univ. of Chicago	90.2	2115	91	2130	Point Loma Nazarene Univ.	81.8	1726	69	1680
Villanova Univ.	90.2	1881	88	1958	Grinnell C.	81.8	1929	85	2010
Washington Univ. in St Louis	90.2	2131	92	2190	Univ. of Denver	81.6	1790	72	1792
Vanderbilt Univ.	90.1	2038	89	2122	Baylor Univ.	81.1	1819	71	1808
Boston C.	89.7	1988	90	2010	American Univ.	81.1	1907	75	1890
CA Inst. of Tech.	89.6	2219	87	2272	Indiana Univ.	81.0	1813	69	1725
Rice Univ.	89.5	2111	92	2138	Seattle Pacific Univ.	81.0	1774	61	1725
Oberlin C.	89.2	2021	82	2032	Tulane Univ. of Louisiana	80.9	1969	73	2010
Bucknell Univ.	89.1	1932	88	1965	Sarah Lawrence C.	80.8	1872	71	
Harvey Mudd C.	89.0	2144	89	2242	Emerson C.	80.7	1864	75	1838
Univ. of Michigan	88.7	1952	85	1988	Univ. of Puget Sound	80.3	1883	75	1860
Rhode Island School of Design	88.5	1903	85	1838	Willamette Univ.	80.3	1860	69	1838
Wake Forest Univ.	88.4	1962	88	1980	Carnegie Mellon Univ.	80.1	2047	84	2092
Scripps C.	88.4	1994	82	2025	Syracuse Univ.	80.0	1781	79	1755
Barnard C.	88.3	2066	88	2018	Fordham Univ.	79.5	1879	78	1838
Massachusetts Inst. of Tech.	88.2	2161	92	2205	Lewis & Clark C.	79.4	1891	70	1965
Smith C.	88.1	1905	88	1920	Case Western Reserve Univ.	79.3	1992	78	1965
Columbia Univ.	88.1	2096	92	2152	Univ. of Vermont	79.2	1829	69	1785
Dickinson C.	88.0	1720	84	1935	Univ. of Maryland	79.1	1944	80	1912
Wheaton C.	87.9	2004	83	1950	Marquette Univ.	79.0	1766	74	1755
Occidental C.	87.5	1868	85	1912	Brandman Univ.	78.9	1789	62	1837
Gonzaga Univ.	87.2	1790	78	1770	Univ. of Washington	77.9	1865	73	1608
Emory Univ.	87.2	2009	87	2078	Univ. of Miami	77.7	1870	75	1928

Note: This table presents selectivity statistics for the top half of private and out-of-state universities, showing that many of these schools tend to be even more selective than the most-selective UC campuses. Estimated graduation rates and average SAT scores of the private and out-of-state universities with at least 100 enrollees among applicants in the UC-NSC sample. 'NSC' statistics measured from 2001-2011 UC freshman California-resident applicants assigned by first institution of enrollment (using National Student Clearinghouse data), with '5-Yr. G.R.' measuring the percent of those applicants who had earned a Bachelor's degree within five years of high school graduation (according to NSC records) and 'Avg. SAT' measuring their average SAT score. 'IPEDS' presents statistics as publicly reported in 2008. Institutions are ordered by NSC graduation rate. Source: National Student Clearinghouse, UC Corporate Student System, and Integrated Postsecondary Education Data System (IPEDS).

Table B.13: Bottom Half of Private and Out-of-State Universities (by Grad. Rate)

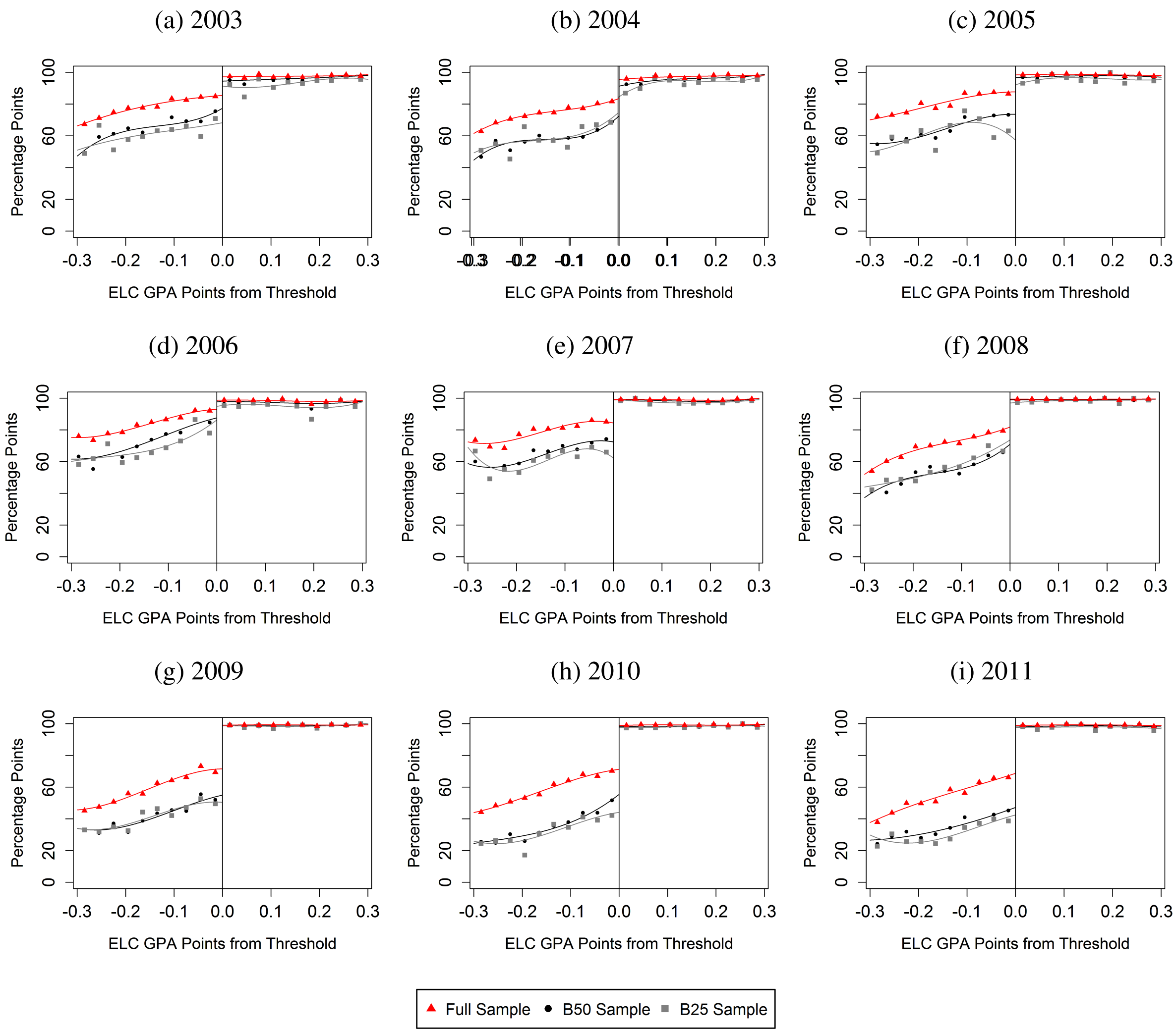
	NSC		IPEDS			NSC		IPEDS	
Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT	Institution	5-Yr. G.R.	Avg. SAT	5-Yr. G.R.	Avg. SAT
The Univ. of Texas at Austin	77.0	1924	73	1838	Oregon State Univ.	63.5	1715	57	1605
Univ. of Oregon	76.4	1736	61	1635	Notre Dame de Namur Univ.	63.0	1507	53	1446
Rensselaer at Hartford	76.2	1958	81	2002	The Evergreen State C.	62.6	1790	59	1695
Spelman C.	75.8	1618	0	1605	Arizona State Univ.	62.5	1659	50	1612
Vassar C.	75.7	2029	91	2070	Concordia Univ.	62.0	1555	59	1740
Pitzer C.	75.7	1822	69		Univ. of the Pacific	61.8	1769	62	1740
Univ. of Rochester	75.6	1918	82	1980	Colgate Univ.	61.5	1986	91	2048
Univ. of Illinois at Urbana	75.0	1943	80	1942	Hofstra Univ.	61.5	1769	52	1762
Saint Mary's C. of CA	75.0	1646	63	1612	Pacific Union C.	61.2	1706	36	1492
Univ. of Redlands	75.0	1686	73	1725	Pace Univ.	60.8	1725	53	1605
Reed C.	74.7	2059	76	2070	Washington State Univ.	60.5	1705	62	1665
CA Lutheran Univ.	74.5	1671	68	1642	St John's Univ.	59.4	1667	50	1605
Univ. of Missouri	74.4	1892	65	1792	Rutgers Univ.	59.3	1807	56	1660
Ithaca C.	74.3	1803	77	1778	Dominican Univ. of CA	59.2	1583	46	1538
CA C. of the Arts	74.2	1694	56		Univ. of Iowa	58.8	1778	0	1808
Whittier C.	74.0	1614	54	1568	Northern Arizona Univ.	58.4	1647	48	1582
Ohio State Univ. Ag. Tech. Inst.	73.7	1828	35	1845	George Mason Univ.	58.0	1754	55	1672
Creighton Univ.	72.5	1778	75	1755	Morehouse C.	57.9	1589	62	1530
Arizona Board of Regents	72.0	1690	52	1650	Saint Louis Univ.	57.3	1849	73	1800
Hampshire C.	71.8	1884	0	1882	Univ. of Hawaii at Manoa	55.9	1649	40	1635
Pennsylvania State Univ.	71.4	1771	48	1463	Clark Atlanta Univ.	53.3	1362	42	1350
Virginia Poly. Inst. and State Univ.	71.4	1771	75	1808	Yeshiva Univ.	53.3	1925	69	1815
Biola Univ.	71.3	1723	68	1680	Embry	52.4	1699	53	1631
Azusa Pacific Univ.	70.3	1681	60	1605	Univ. of Minnesota	52.1	1842	61	1868
Texas A & M Univ.	70.0	1872	73	1785	Art Center C. of Design	52.1	1731	86	
Drexel Univ.	69.6	1853	56	1800	Boise State Univ.	51.7	1580	19	1545
Loyola Univ. Chicago	69.6	1771	64	1768	CA Inst. of the Arts	50.9	1739	61	
Univ. of Pittsburgh	69.3	1900	56	1557	Univ. of Nevada	48.8	1660	39	1575
Mills C.	69.2	1693	61	1688	Rochester Inst. of Tech.	48.3	1854	54	1800
Univ. of Colorado Boulder	69.1	1755	62	1762	Holy Names Univ.	46.9	1399	11	1397
Univ. of San Francisco	68.8	1682	65	1718	Univ. of New Mexico	46.7	1658	35	1598
Univ. of Massachusetts	68.4	1770	67	1732	Univ. of Utah	46.4	1706	39	1661
The New School	68.0	1780	60	1665	Marymount CA Univ.	45.3	1497		
Vanguard Univ. of Southern CA	67.8	1523	51	1455	Univ. of Nevada	42.0	1546	31	1522
Pratt Inst.	67.6	1772	45	1725	La Sierra Univ.	41.5	1496	25	1478
Northeastern Univ.	67.5	1925	64	1905	Tuskegee Univ.	41.4	1362	39	1312
DePaul Univ.	67.3	1748	60	1702	Southern Oregon Univ.	40.7	1686	33	1500
Purdue Univ.	66.8	1811	66	1725	Fresno Pacific Univ.	38.1	1549	60	1522
Loyola Univ. New Orleans	66.7	1781	61	1778	DeVry Univ.	36.6	1402		
Howard Univ.	66.4	1587	61	1710	Portland State Univ.	35.7	1711	27	1568
Hampton Univ.	66.2	1475	48	1589	Brigham Young Univ.	34.0	1859	53	1845
Georgia Inst. of Tech.	66.1	1982	70	1995	Brigham Young Univ.	33.3	1579	39	1635
Univ. of Notre Dame	66.0	2019	96	2115	Academy of Art Univ.	28.9	1596	24	
Michigan State Univ.	66.0	1756	72	1725	Woodbury Univ.	27.0	1472	54	1395
Western Washington Univ.	65.9	1767	63	1672	Univ. of Phoenix	12.1	1529	4	
Otis C. of Art and Design	65.8	1652	52	1545	Mount Holyoke C.	10.2	1819	82	
Univ. of La Verne	65.5	1514	57	1470	Westmont C.	8.6	1809	78	1822
Colorado State Univ.	64.8	1729	58	1680	Harvard Univ.	5.7	2186	96	2228
Mount Saint Mary's Univ.	64.0	1429	57	1380	CA Baptist Univ.	4.5	1492	45	1574
Cornell Univ.	63.5	2065	92	2100	Soka Univ. of America	2.3	1773	93	1750

Note: This table presents selectivity statistics for the bottom half of private and out-of-state universities, showing that these schools exhibit a comparable selectivity range to the CSU system, though there are a small number of universities that have erroneously-low NSC graduation rates as a result of non-reporting. Estimated graduation rates and average SAT scores of the private and out-of-state universities with at least 100 enrollees among applicants in the UC-NSC sample. 'NSC' statistics measured from 2001-2011 UC freshman California-resident applicants assigned by first institution of enrollment (using National Student Clearinghouse data), with '5-Yr. G.R.' measuring the percent of those applicants who had earned a Bachelor's degree within five years of high school graduation (according to NSC records) and 'Avg. SAT' measuring their average SAT score. 'IPEDS' presents statistics as publicly reported in 2008. Institutions are ordered by NSC graduation rate. Source: National Student Clearinghouse, UC Corporate Student System, and Integrated Postsecondary Education Data System (IPEDS).

B.5 Annual Relationship between ELC GPA and UC Admissions

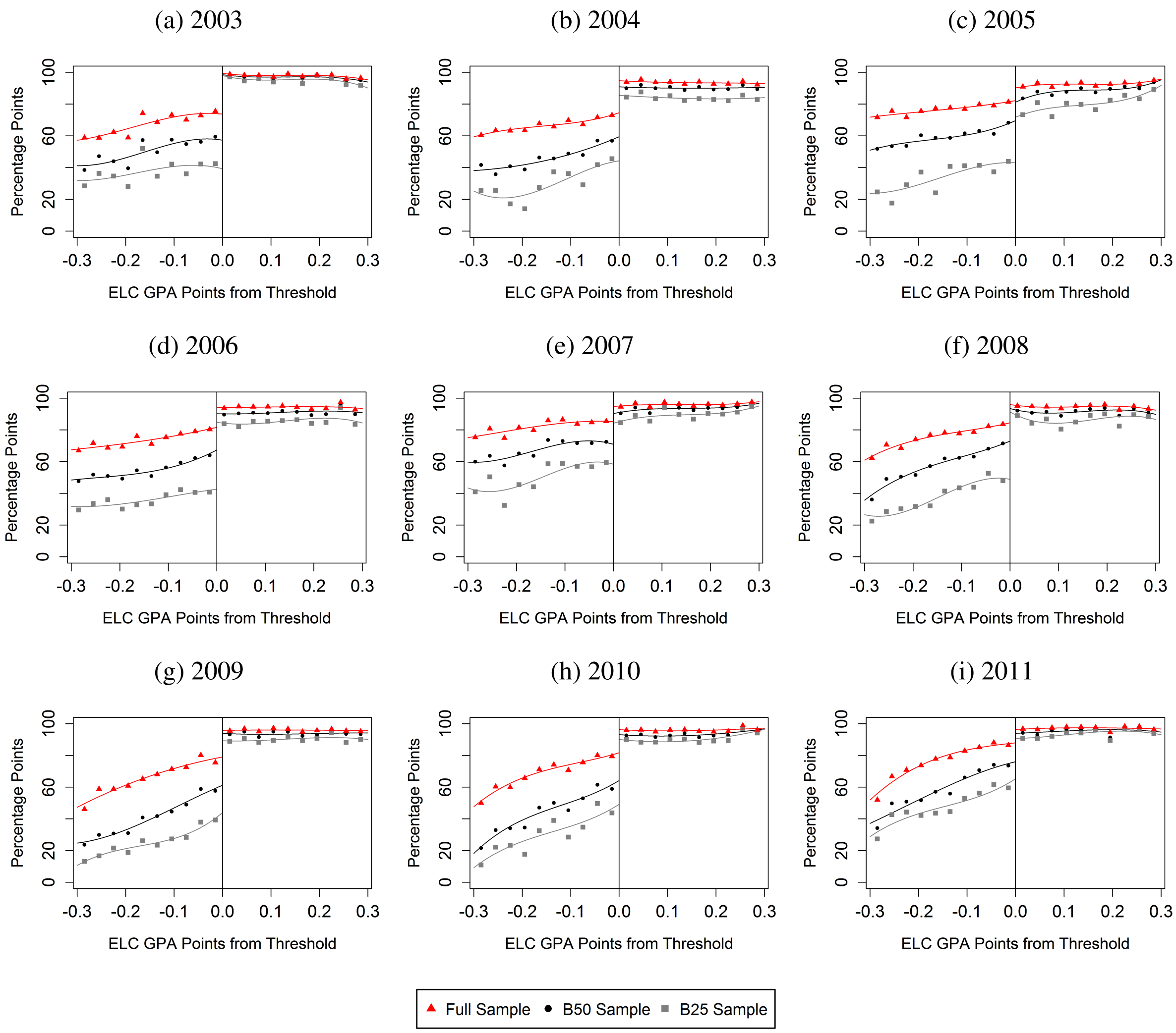
Figures B.3 to B.11 show annual break-outs of the effect of ELC eligibility on applicants' likelihood of admission to each campus. They show that the general admissions patterns remain highly persistent across the nine observed years: applicants receive large admissions advantages in most years at the Absorbing UC campuses and negligible admissions advantages at the other UC campuses. Some Absorbing UC campuses' admissions advantages grow somewhat over time, largely driven by the campuses' increasing selectivity in the period (decreasing near-threshold applicants' admissions likelihood through non-ELC admissions).

Figure B.3: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC Davis



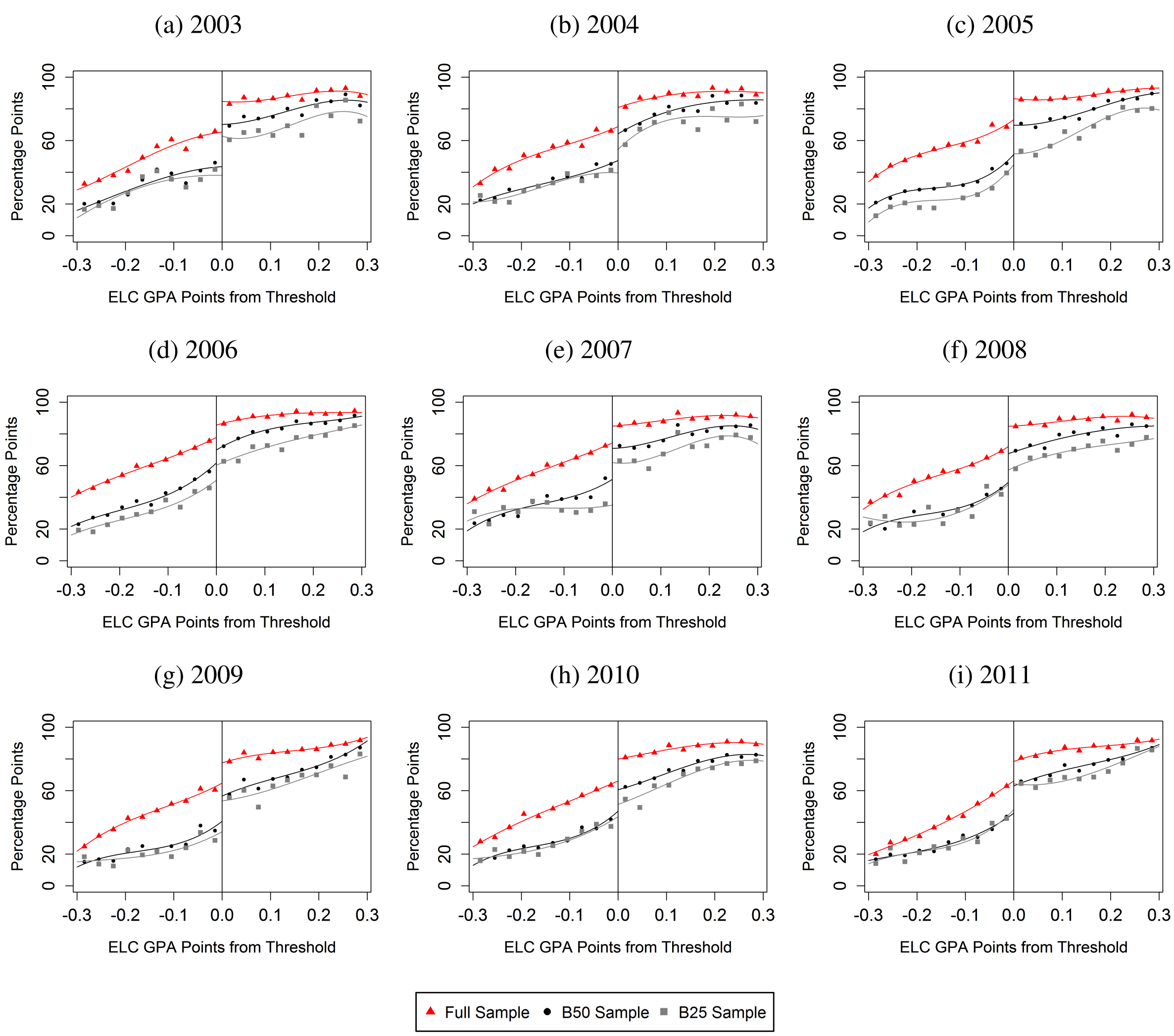
Note: Applicants' annual likelihood of admission to UC Davis by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

Figure B.4: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC Irvine



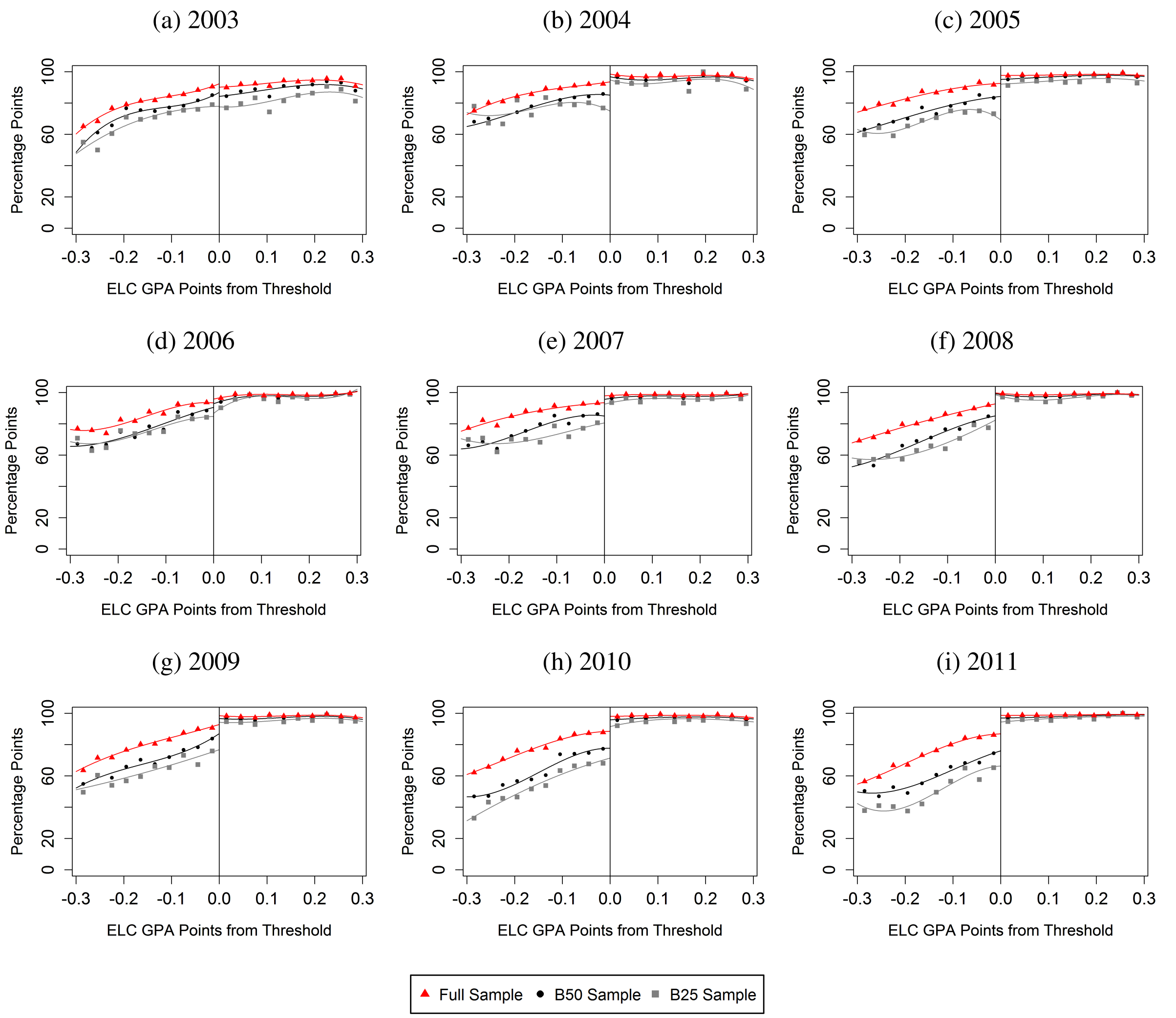
Note: Applicants' annual likelihood of admission to UC Irvine by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

Figure B.5: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC San Diego



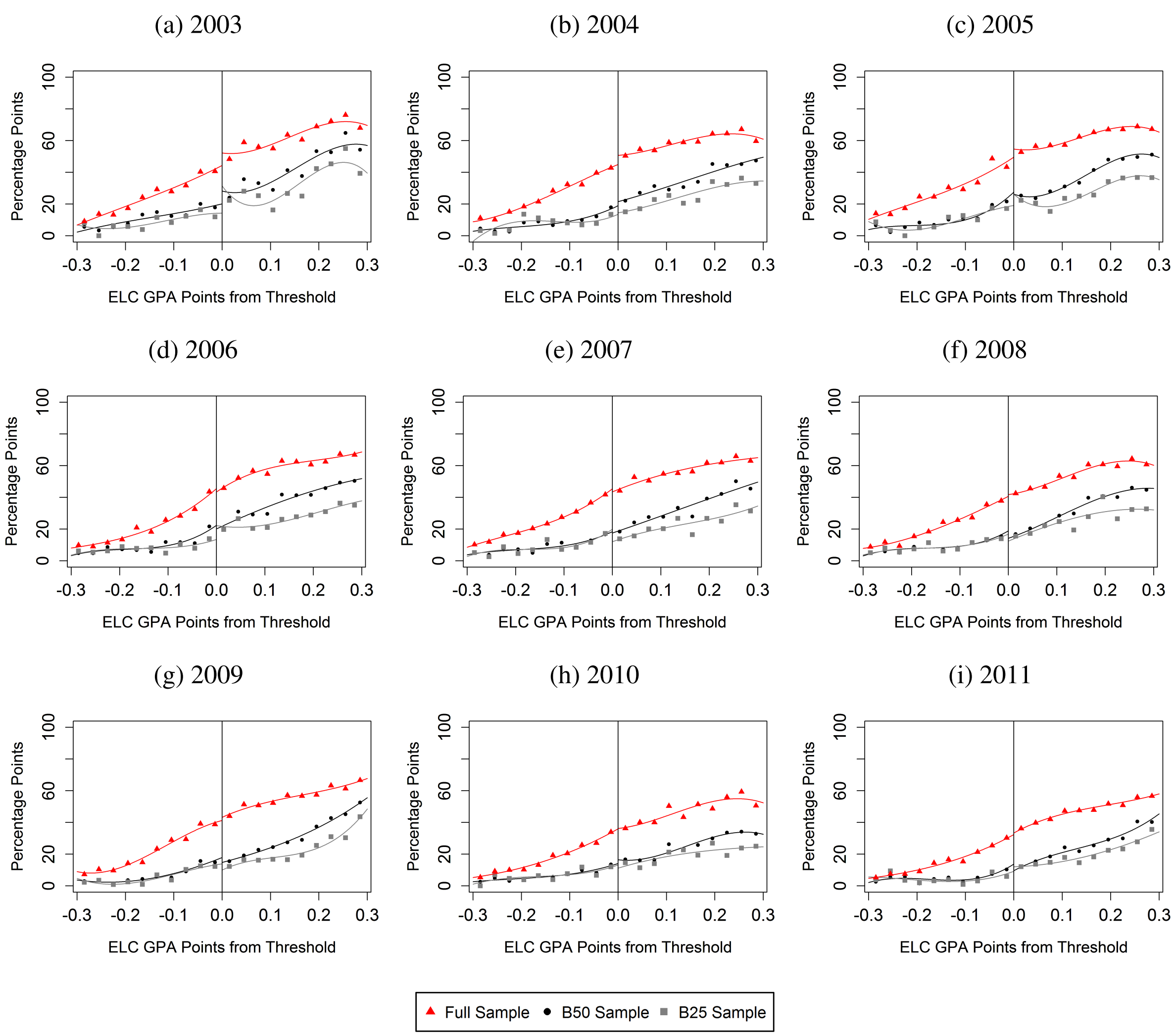
Note: Applicants' annual likelihood of admission to UC San Diego by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

Figure B.6: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC Santa Barbara



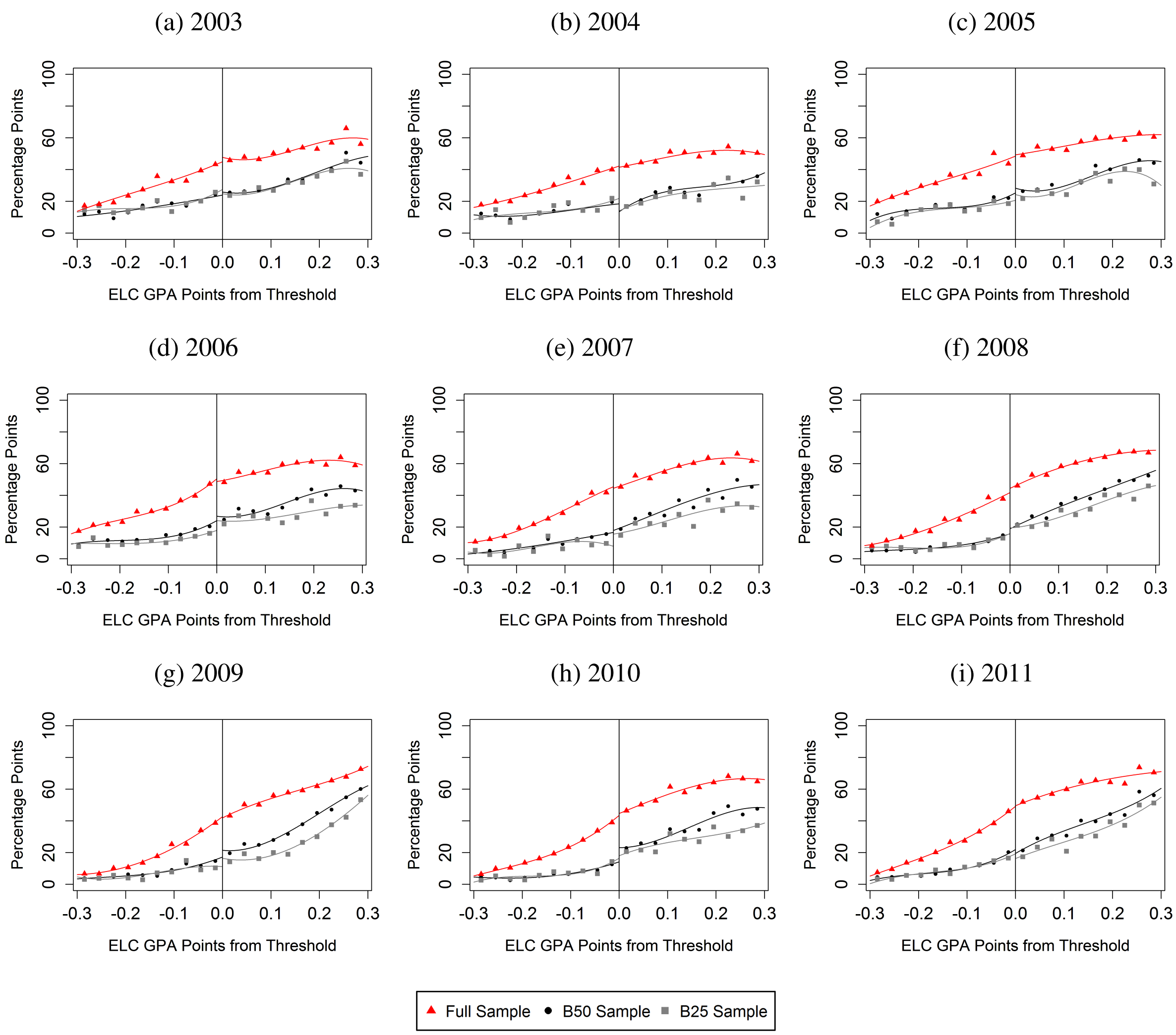
Note: Applicants' annual likelihood of admission to UC Santa Barbara by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

Figure B.7: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC Berkeley



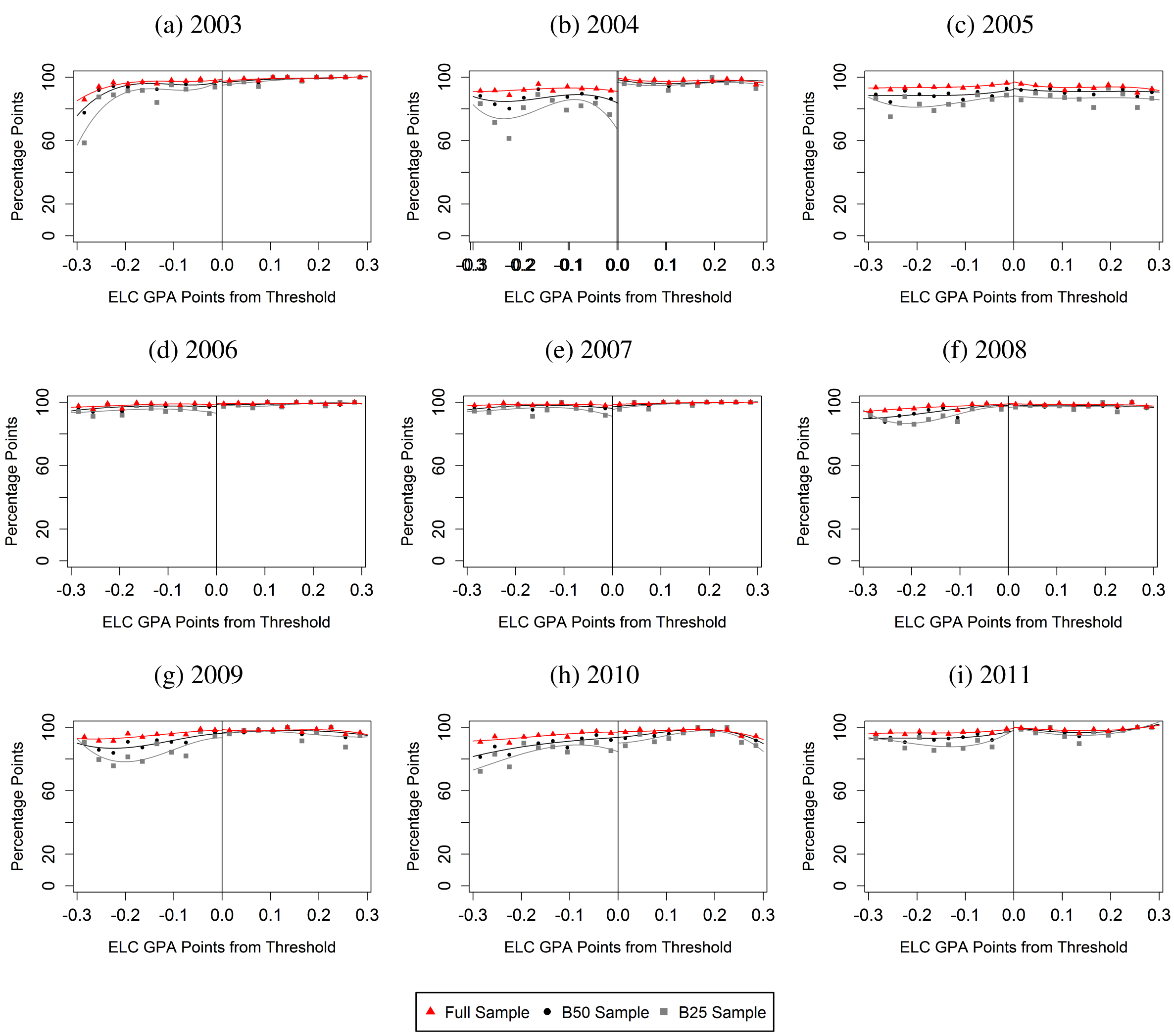
Note: Applicants' annual likelihood of admission to UC Berkeley by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

Figure B.8: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UCLA



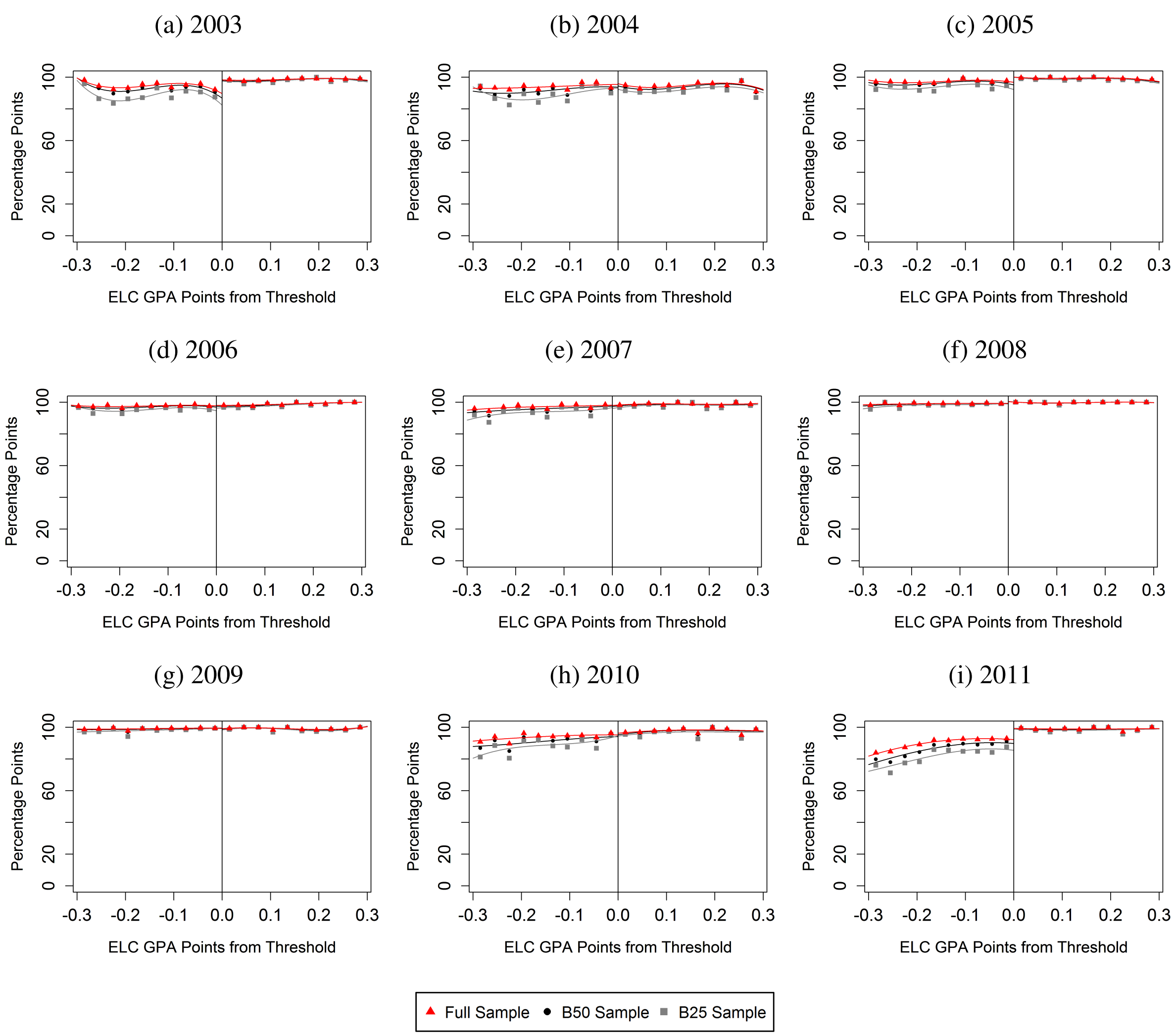
Note: Applicants' annual likelihood of admission to UCLA by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

Figure B.9: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC Santa Cruz



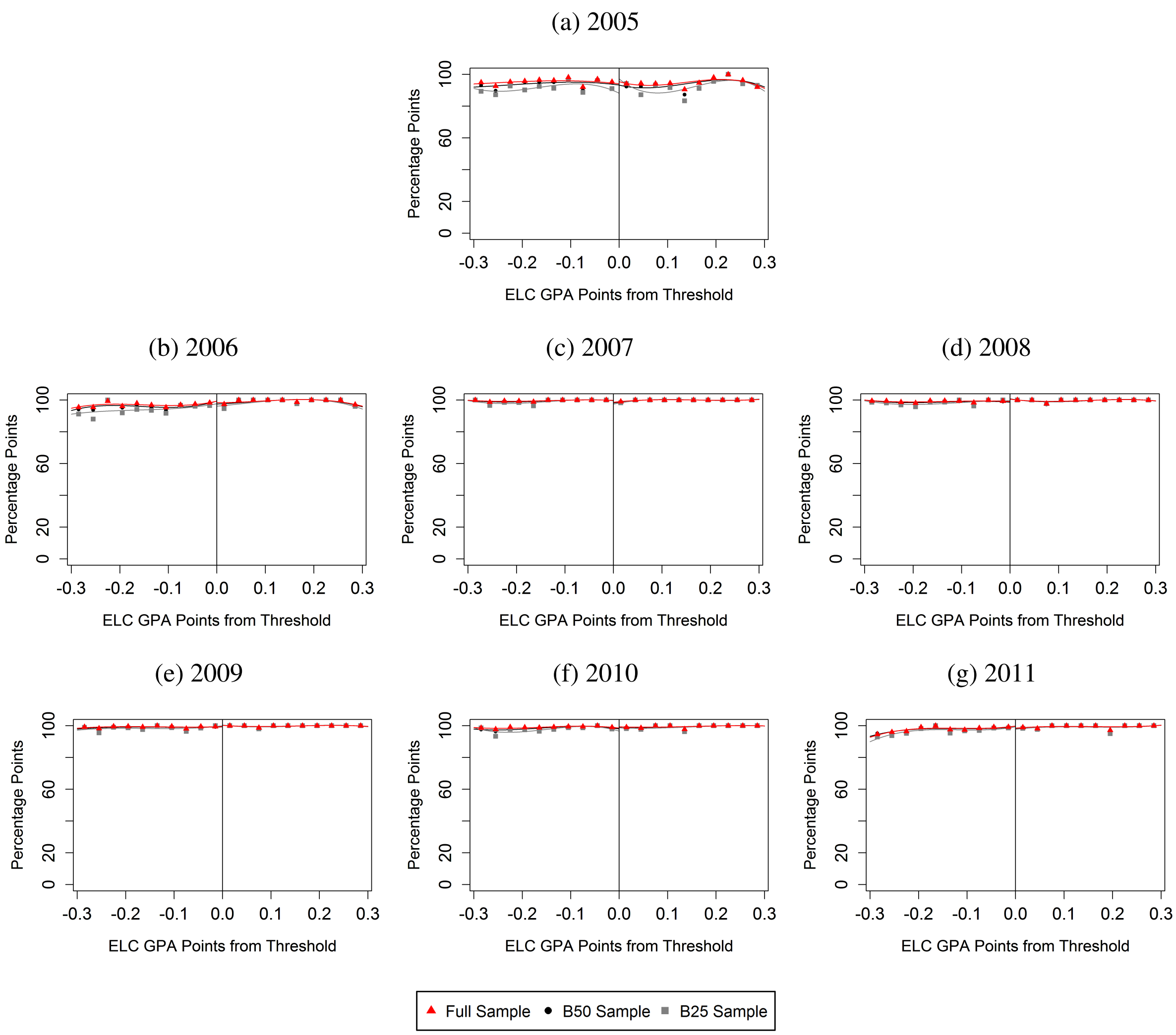
Note: Applicants' annual likelihood of admission to UC Santa Cruz by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

Figure B.10: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC Riverside



Note: Applicants' annual likelihood of admission to UC Riverside by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

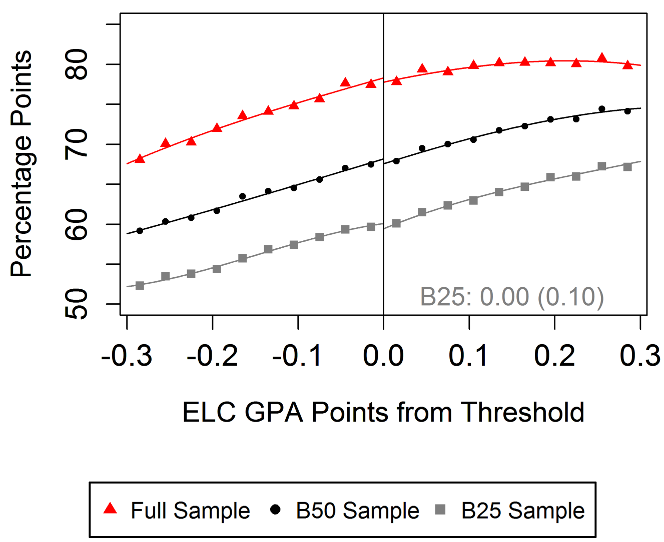
Figure B.11: Local Effect of ELC Eligibility on Applicants' Likelihood of Admission to UC Merced



Note: Applicants' annual likelihood of admission to UC Merced by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Each panel conditions on applying to that UC campus in that year. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System.

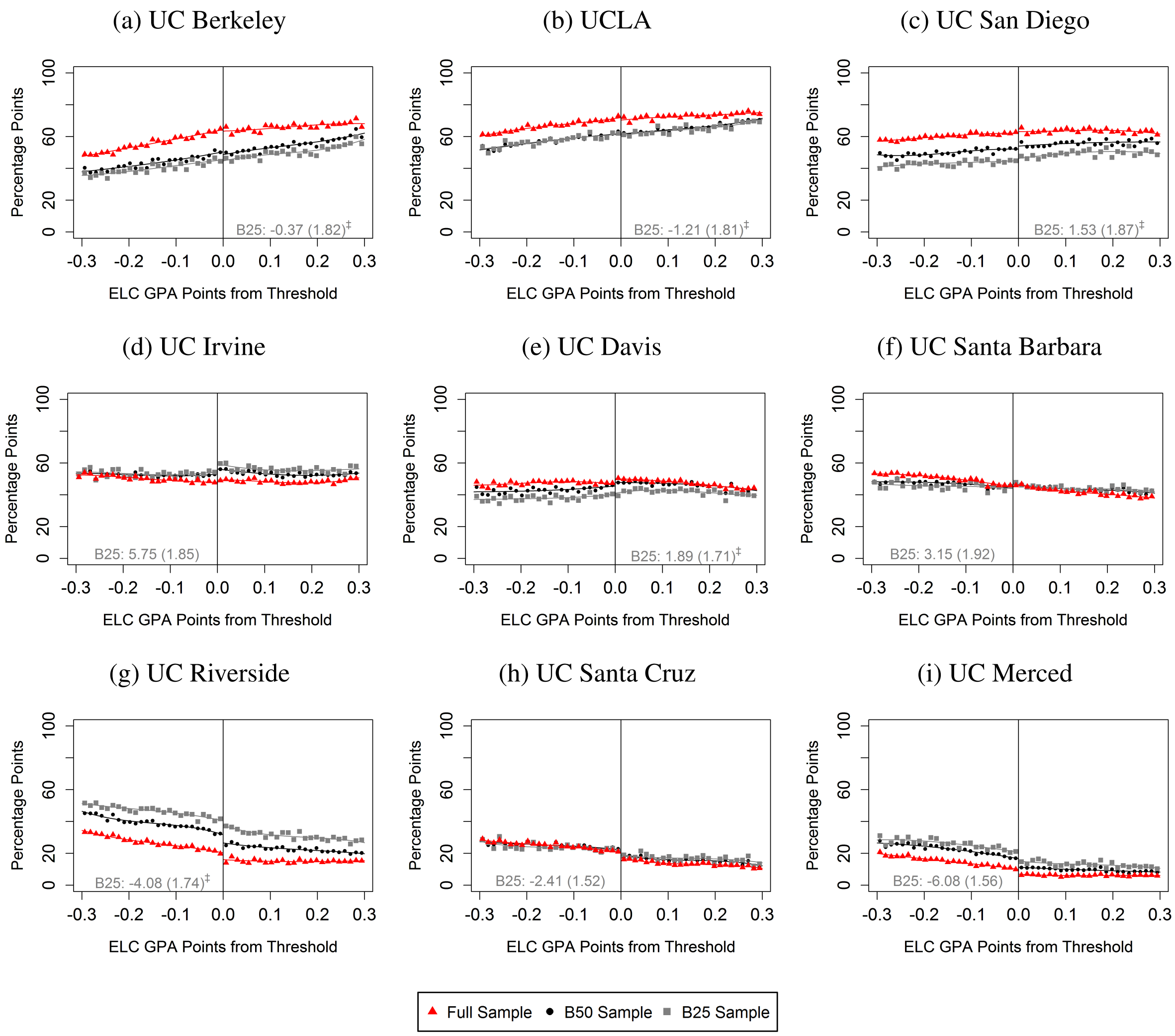
B.6 Other Appendix Figures and Tables

Figure B.12: Socioeconomic-Predicted Five-Year Degree Attainment



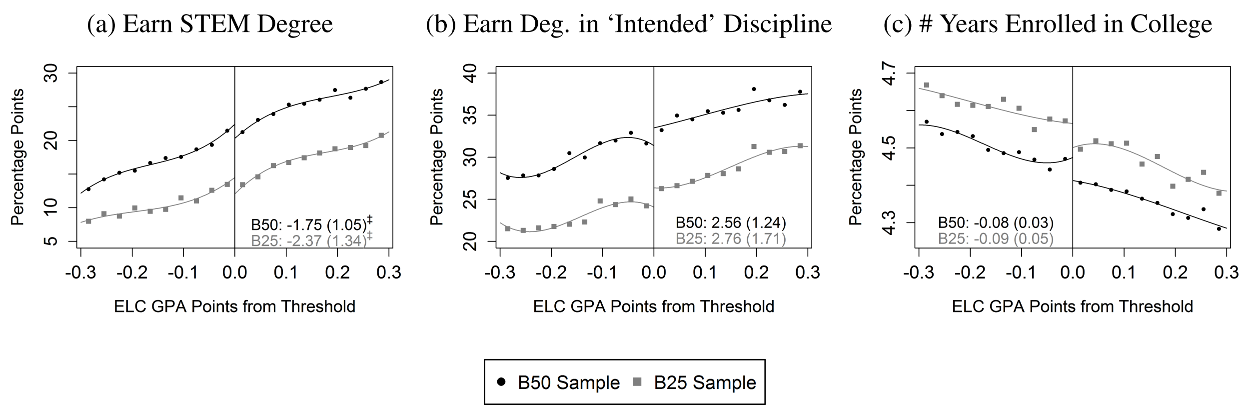
Note: This figure summarizes baseline sample balance across the ELC eligibility threshold using applicants' predicted five-year degree attainment (on the basis of socioeconomic characteristics). Regression discontinuity plot of applicants' predicted likelihood of five-year degree attainment by ELC GPA distance from their high school's ELC eligibility threshold, among all applicants or applicants from the bottom half or quartile of California high schools by SAT. Points are binned averages; lines are unconditional cubics. Beta estimate from cubic parameterization of Equation 3.2 for the B50 sample, with standard error clustered by school-year in parentheses. Predicted graduation from an OLS regression (across 1995-2013 UC freshman California-resident applicants outside the study's primary sample) of five-year NSC degree-attainment on gender-ethnicity indicators, maximum parental education indicators (7 categories), family income, missing income indicator, SAT score, high school GPA, and year indicators. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Source: UC Corporate Student System and National Student Clearinghouse.

Figure B.13: Local Effect of ELC Eligibility on Applicants' Likelihood of Application to each UC Campus



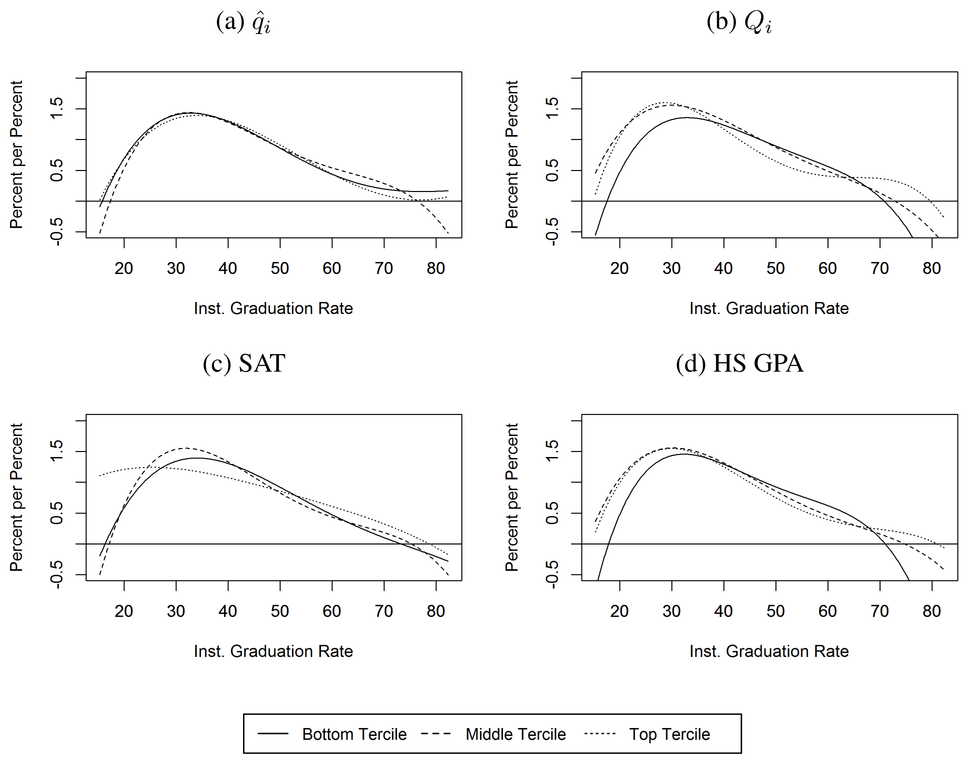
Note: This figure shows that barely ELC-eligible applicants responded to their Absorbing UC campus admissions advantages by becoming slightly more likely to apply to those campuses and slightly less likely to apply to the Dispersing campuses, though the magnitudes are far smaller than the shifts in those applicants' admissions likelihoods. UC applicants' likelihood of application to each UC campus by ELC GPA distance from their high school's ELC eligibility threshold, among all UC applicants and those from the bottom half (B50) or quartile (B25) of California high schools by SAT. Points are binned averages; lines are cubic fits. Beta estimates are from cubic regression discontinuity models following Equation 3.2 for the B25 sample, with standard errors in parentheses clustered by high-school-year. Each panel conditions on applying to that UC campus. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ indicates reduced-form estimates with $p > 0.1$ for the null hypothesis ($\hat{\beta} = 0$) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). Source: UC Corporate Student System.

Figure B.14: Local Effect of ELC Eligibility on UC Applicants' Other Education Outcomes



Note: Regression discontinuity plots of applicants' measured outcomes by ELC GPA distance from their high school's ELC eligibility threshold, among applicants from the bottom half (B50) or quartile (B25) of high schools by SAT. Points are binned averages; lines are cubic fits. Beta estimates are from cubic regression discontinuity models following Equation 3.2, with standard errors in parentheses clustered by high-school-year. Degree attainment by discipline is unconditional on overall attainment. See footnote 22 for definitions of STEM and other disciplines; intended discipline is applicants' most-selected prospective major discipline reported to UC campuses. Number of years enrolled in college is the number of academic years within seven years of high school graduation in which the applicant is observed enrolled at a postsecondary institution but has not yet earned a Bachelor's degree. Applicants from high schools with approximated ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ indicates reduced-form estimates with $p > 0.1$ for the null hypothesis ($\hat{\beta} = 0$) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). Source: UC Corporate Student System and National Student Clearinghouse.

Figure B.15: Estimated Return per Inst. Grad Rate for Applicants by q_i, Q_i, and SAT Tercile



Note: This figure shows that the return to more-selective university enrollment (per one point of graduation rate) for universities in the support of the data (which is strongest from about 30 to 75) is similar for top-, middle-, and bottom-tercile UC applicants by model-defined caliber (q_i), application merit (Q_i), SAT score, or high school GPA, where returns are measured by five-year degree attainment. Estimated change in applicants' likelihood of five-year degree attainment per additional percentage point in institutional graduation rate, by tercile of $\hat{q}_i$, $\hat{Q}_i$, SAT score, or high school GPA. Estimates from an extension of equation 3.8 in which GR_i is a fifth-order polynomial in institutional graduation rate and $\hat{q}_i$ is replaced by tercile indicators, omitting the constant term; the plot shows the *derivative* of the resulting estimated polynomials. Applicants' $\hat{q}_i$ estimated using the posterior distribution of q_i's resulting from the model parameters described in the text, and $\hat{Q}_i = z_i\hat{\beta}^z + \hat{q}_i$. Covariates include gender-ethnicity indicators, SAT score, HS GPA, log income, parental education and occupation indicators, ELC eligibility, and high school, zip code, and year fixed effects, as well as admissions portfolio indicators for every combination of UC campuses to which the applicant applies and UC campuses to which they are admitted. See Appendix B.4 for definition of institutional graduation rates. Source: UC Corporate Student System and the National Student Clearinghouse.

Table B.14: Baseline Characteristic Balance at ELC GPA Threshold, 2003-2011

	Permanent Applicant Characteristics						Predicted Values[2]	
	Female (%)	URM (%)	Max. Parent Ed. (Index)[1]	Log Fam. Income	Missing Inc. (%)	SAT Score	Graduation Rate (%)	California Earnings ($)
All	0.0	1.0	-0.022	0.02	-0.0	-5.7	0.06	83.5
	(0.8)	(0.6)‡	(0.030)	(0.02)	(0.7)	(2.9)‡	(0.06)	(63.2)
URM	-0.6	-	-0.061	-0.04	-0.1	-2.0	-0.07	-105.2
	(1.8)	-	(0.078)	(0.05)	(1.1)	(6.6)	(0.20)	(111.5)
High School Quartiles by SAT Score								
Top Quartile	0.6	0.2	-0.017	0.05	0.9	-0.6	0.00	188.1
	(1.6)	(0.8)	(0.038)	(0.05)	(1.5)	(4.1)	(0.12)	(136.3)
Third Quartile	0.5	2.1	-0.020	0.06	-0.2	-10.6	0.16	61.7
	(1.6)	(1.1)‡	(0.055)	(0.04)	(1.5)	(5.3)	(0.12)	(121.3)
Second Quartile	-0.6	0.7	0.014	-0.01	-1.5	-5.9	0.04	129.0
	(1.7)	(1.4)	(0.068)	(0.04)	(1.3)	(6.1)	(0.11)	(117.0)
Bottom Quartile	-1.4	0.8	-0.085	-0.01	0.4	-3.3	0.05	-126.6
	(1.9)	(1.6)	(0.082)	(0.05)	(1.0)	(7.4)	(0.10)	(105.3)
Male[3]	-	1.0	-0.221	0.00	2.3	-7.2	-0.01	-200.8
	-	(2.7)	(0.146)	(0.09)	(1.8)	(13.0)	(0.26)	(218.4)
Female[3]	-	0.2	0.060	0.00	-0.1	-0.3	0.01	-376.9
	-	(2.1)	(0.111)	(0.06)	(1.2)	(9.1)	(0.16)	(151.1)
Baseline[4]	60.5	21.9	5.15	11.01	22.7	1877	75.6	87,477

Note: This table shows baseline sample balance across the ELC eligibility threshold. Reported coefficients are estimated changes in various applicant characteristics across the ELC eligibility threshold. Estimates from 2SLS cubic fuzzy regression discontinuity models, with standard errors in parentheses clustered by high-school-year. Additional covariates (where not colinear with the outcome variable) include gender-ethnicity indicators and a quadratic term in SAT score, along with year and high school fixed effects. Coefficients estimated overall, for URM (Black, Hispanic, and Native American) applicants, by high school SAT quartiles (see the text for definition), and for male and female applicants (among applicants from the bottom two high school SAT quartiles). SAT score on a 2400 point scale; converted from ACT score or 1600-point SAT score if otherwise unavailable. Family income is not reported by 12 percent of applicants. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ Indicates estimates with $p < 0.1$ for the null hypothesis such that $p \not< 0.05$ (*insignificant* at conventional levels) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). [1]Integer index of reported maximum parental education (across two parents), from 1 (no high school) to 7 (graduate degree). [2]Dependent variable is the predicted values from an OLS regression (across the full sample of 1995-2013 UC freshman California-resident applicants, excluding the study's primary sample) of either five-year NSC graduation or 6-to-8 year average California covered wages (see text for definitions) on gender-ethnicity indicators, maximum parental education indicators (7 categories), family income, missing income indicator, SAT score, and year indicators. [3]Conditional on graduating from the bottom two SAT high school quartiles. [4]The estimated baseline (ELC-ineligible) mean characteristic of barely below-threshold UC applicants, estimated following Abadie (2002).

Source: UC Corporate Student System.

Table B.15: Impact of ELC on Admissions and Enrollment for Barely ELC-Eligible Applicants by Campus

	Admission (%)				Enrollment (%)			
	All		B25[1]		All		B25[1]	
	Baseline	β	Baseline	β	Baseline	β	Baseline	β
Unimpacted Campuses								
Berkeley	42.6	2.0 (0.9)‡	15.7	1.8 (1.9)	13.5	0.8 (0.6)	4.8	0.9 (0.8)
UCLA	46.0	1.3 (0.8)	17.4	1.8 (1.8)	12.5	-0.6 (0.5)	6.7	0.2 (1.0)
Absorbing Campuses								
Davis	79.6	19.8 (0.8)	57.9	40.1 (2.4)	6.6	2.7 (0.5)	7.9	4.3 (1.1)
San Diego	70.1	13.9 (0.8)	42.7	17.3 (2.5)	7.3	2.8 (0.5)	5.2	2.7 (0.9)
Santa Barbara	91.9	6.7 (0.6)	77.3	17.4 (1.9)	6.1	-0.5 (0.4)	8.0	1.2 (1.1)
Irvine	81.9	14.9 (0.8)	49.1	41.8 (2.1)	5.9	0.9 (0.4)‡	6.8	7.4 (1.1)
Dispersing Campuses								
Riverside	96.5	2.4 (0.6)	94.1	4.0 (1.2)‡	2.5	-0.9 (0.2)	8.5	-2.5 (1.0)‡
Santa Cruz	98.0	1.4 (0.6)‡	93.0	4.5 (2.2)‡	1.7	-0.4 (0.2)	2.7	-1.7 (0.6)
Merced	95.6	0.4 (1.3)	95.5	0.6 (1.8)	0.7	-0.4 (0.1)	1.7	-0.8 (0.5)

Note: This table presents the impact of near-threshold ELC eligibility on each UC campus's admissions and enrollment, showing that the Absorbing UC campuses provided large admissions advantages to eligible students (especially those from less-competitive high schools) that translated into increased likelihood of enrollment. Reported coefficients are the estimated baseline (ELC-ineligible) proportion of below-threshold students at their high school's ELC eligibility threshold admitted or enrolled at each UC campus 2003-2011, and the estimated change in admission or enrollment for barely ELC-eligible applicants (β), overall and for students from the bottom SAT quartile of high schools. Values in percentages. Estimates from 2SLS cubic fuzzy regression discontinuity models; standard errors are clustered by school-year, and omitted for baseline estimates (which are estimated following Abadie (2002)). Additional covariates include gender-ethnicity indicators and a quadratic term in SAT score, along with year and high school fixed effects. 'Admission' estimates are conditional on applying to that campus; 'Enrollment' estimates are conditional on applying to *at least one* UC campus. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ Indicates estimates with $p < 0.1$ for the null hypothesis such that $p \not< 0.05$ (*insignificant* at conventional levels) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). [1]Bottom quartile of high schools by SAT scores of students within 0.3 GPA points of the ELC eligibility threshold.

Source: UC Corporate Student System and National Student Clearinghouse.

Table B.16: Local Effect of ELC Eligibility on Application to Each UC Campus

	UCB	UCLA	UCSD	UCI	UCD	UCSB	UCSC	UCR	UCM
Panel A: Baseline Application Likelihood (%)									
All	64.6	72.2	62.4	47.8	47.0	45.5	20.0	20.4	9.9
B50	50.3	62.0	52.4	53.2	45.9	44.5	31.8	21.6	16.6
B25	45.1	61.7	44.8	55.0	39.9	43.4	40.9	20.4	20.7
Panel B: Local Change in App. Likelihood Caused by ELC Eligibility (p.p.)									
All	-0.2	-0.9	1.7	1.9	3.3	1.0	-4.9	-3.8	-3.5
	(0.8)	(0.7)	(0.8)	(0.8)	(0.8)	(0.9)	(0.6)	(0.7)	(0.5)
B50	-1.0	0.4	2.5	4.2	2.6	2.4	-6.2	-3.7	-6.1
	(1.2)	(1.2)	(1.2)	(1.3)	(1.2)‡	(1.3)‡	(1.1)	(1.0)	(1.0)
B25	-0.4	-1.2	1.5	5.8	1.9	3.1	-4.1	-2.4	-6.1
	(1.8)	(1.8)	(1.9)	(1.8)	(1.7)	(1.9)	(1.7)‡	(1.5)	(1.6)

Note: This table shows that barely ELC-eligible applicants responded to their Absorbing UC campus admissions advantages by becoming slightly more likely to apply to those campuses and slightly less likely to apply to the Dispersing campuses, though the magnitudes are far smaller than the shifts in those applicants' admissions likelihoods. Reported coefficients are the estimated baseline (ELC-ineligible) proportion of near-threshold UC applicants who apply to each UC campus, and the estimated change in application likelihood for barely above-threshold ELC-eligible applicants (β). Values in percentages; estimates overall and for students from the bottom half (B50) and quartile (B25) of high schools by SAT. Estimates from cubic regression discontinuity models following Equation 3.2; standard errors are clustered by school-year and omitted for baseline estimates (which are estimated following Abadie (2002)). ‡ Indicates estimates with $p < 0.1$ for the null hypothesis such that $p \not< 0.05$ (*insignificant* at conventional levels) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). Source: UC Corporate Student System.

Table B.17: Change in Characteristics of ELC-Eligible Students' University of First Enrollment

Sample:	First Four-Year Inst.			First Two- or Four-Year Institution[1]				
	Admit Rate	Avg. SAT	Four-Year Grad. Rate	Five-Year Grad. Rate	Avg. SAT	Med. Fam. Income	Sticker Price	Est. Net Price[2]
Overall								
Baseline	43.0	1,820.9	55.4	1,821.0	75.0	111,874.0	29,977.7	18,601.7
β	-0.70	15.46	1.58	12.66	1.63	766.79	225.61	113.65
	(0.35)‡	(3.17)	(0.35)	(2.27)	(0.32)	(393.49)‡	(238.18)	(231.97)
IV: Enroll	-9.9	208.4	21.8	209.6	27.1	12,313.4	3,297.3	1,672.0
Abs. UC	(5.4)	(47.3)	(5.1)	(43.6)	(5.4)	(6,414.6)	(3,609.6)	(3,444.8)
Bottom High School Quartile (B25)								
Baseline	52.8	1,678.6	42.3	1,682.0	64.2	95,371.0	26,794.2	11,536.9
β	-2.17	36.71	5.59	39.46	5.24	2,612.37	902.14	305.00
	(0.85)	(8.33)	(0.90)	(5.68)	(0.83)	(906.39)	(417.07)‡	(382.89)
IV: Enroll	-12.2	201.9	30.8	249.1	33.1	15,285.5	4,259.2	1,505.8
Abs. UC	(4.8)	(41.9)	(4.4)	(35.4)	(4.8)	(4,964.1)	(2,003.7)	(1,903.4)
Source:	IPEDS	IPEDS	IPEDS	NSC	NSC	OI	IPEDS	IPEDS

Note: This table shows that ELC caused barely-eligible applicants to enroll at more-selective universities using a host of selectivity measures, and but those universities had similar net prices for students with their family incomes. Reported coefficients are the estimated characteristics of applicants' first-enrollment university or post-secondary institution at the barely ELC-ineligible baseline, the change in those characteristics across the ELC eligibility threshold (β), and the estimated change in those characteristics for Absorbing-UC-campus compliers estimated using ELC eligibility as an instrumental variable. Estimates from 2SLS cubic fuzzy regression discontinuity models; standard errors are clustered by school-year, and omitted for baseline estimates (which are estimated following Abadie (2002)). Additional covariates include gender-ethnicity indicators and a quadratic term in SAT score, along with year and high school fixed effects. Enrollment measured as first four-year (columns 1-3) or two- or four-year (columns 4-8) college or university of enrollment between July following high school graduation and six years later. IPEDS and Opportunity Insights (OI) data linked by OPE ID (and year in IPEDS case) to NSC enrollment. NSC-measured average SAT scores and five-year graduation measured only for 2001-2011 UC applicants (excluding applicants within 0.3 GPA points of their high school's ELC eligibility threshold) and are time-invariant; see the text for the definition. Also see the text for definition of high school quartiles. IPEDS Average SAT score calculated for each school as the sum of the mean of the 25th and 75th percentiles of each SAT section, converting scores from 1600 scale to 2400 scale when necessary. Sticker price is defined using on-campus residency unless unavailable, in which case it is defined using off-campus non-family residency. IPEDS admission rate unavailable prior to 2005, and price information unavailable prior to 2008. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ Indicates reduced-form estimates with $p < 0.1$ for the null hypothesis such that $p \not< 0.05$ (*insignificant* at conventional levels) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). [1] If the applicant enrolls at a community college but then enrolls at a four-year university within 6 months, the latter is defined as her first institution of enrollment. [2] Net price includes tuition and fees, expected room and board, books and supplies, and other expenses net of federal, state, local, or institutional grant aid. Calculated as the IPEDS average net price for Title-IV-aid-awarded enrollees in the applicant's family income bin, where the observed bins are \$0-30,000, \$30,000-48,000, \$48,000-75,000, \$75,000-110,000, and above \$110,000. Applicants with unobserved family incomes are omitted, and likely paid the sticker price (as they did not apply for federal financial aid, including loans).

Source: UC Corporate Student System, National Student Clearinghouse, the Integrated Postsecondary Education Data System (IPEDS), and Opportunity Insights (Chetty et al., 2020*a*).

Table B.18: Change in Characteristics of ELC-Eligible Students' Degree-Providing Universities

Sample:	First Four-Year Inst.			First Two- or Four-Year Institution[1]				
	Admit Rate	Avg. SAT	Four-Year Grad. Rate	Five-Year Grad. Rate	Avg. SAT	Med. Fam. Income	Sticker Price	Est. Net Price[2]
Overall								
Baseline	42.5	1,826.7	56.2	80.0	1,840.2	113,380.8	29,974.5	18,801.1
β	-0.56 (0.38)	16.26 (3.39)	1.53 (0.37)	0.93 (0.21)	10.13 (2.28)	623.98 (400.77)	325.98 (249.42)	364.73 (246.87)
IV: Enroll Abs. UC	-8.9 (6.4)	247.7 (61.3)	23.9 (6.3)	17.2 (4.1)	187.5 (50.7)	11,556.1 (7,608.6)	4,877.1 (3,996.2)	5,886.1 (4,258.6)
Bottom High School Quartile (B25)								
Baseline	52.9	1,687.7	44.0	1,704.0	70.6	97,577.2	26,812.8	11,526.0
β	-2.05 (0.99)	33.56 (10.13)	4.43 (1.05)	35.26 (7.03)	3.87 (0.76)	2,172.27 (971.84)	991.20 (473.61)‡	570.78 (436.75)
IV: Enroll Abs. UC	-12.4 (6.1)	200.5 (56.2)	26.2 (5.7)	222.6 (44.6)	24.4 (4.6)	13,830.3 (5,876.4)	5,058.3 (2,521.4)	3,165.8 (2,495.1)
Source:	IPEDS	IPEDS	IPEDS	NSC	NSC	OI	IPEDS	IPEDS

Note: This table shows that ELC caused barely-eligible applicants to earn degrees from more-selective institutions using a host of selectivity measures (conditional on degree attainment). Reported coefficients are the estimated characteristics of applicants' Bachelor's graduation university or post-secondary institution (conditional on BA graduation) at the barely ELC-ineligible baseline, the change in those characteristics across the ELC eligibility threshold (β), and the estimated change in those characteristics for Absorbing-UC-campus compliers using an IV estimator instrumenting with ELC eligibility. Estimates from instrumental variable 2SLS cubic fuzzy regression discontinuity models; standard errors are clustered by school-year, and omitted for baseline estimates (which are estimated following Abadie (2002)). Graduation measured as first Bachelor's degree earned between July following high school graduation and six years later. IPEDS and Opportunity Insights (OI) data linked by OPE ID (and year in IPEDS case) to NSC enrollment. NSC-measured average SAT scores and five-year graduation measured only for 2001-2011 UC applicants (excluding applicants within 0.3 GPA points of their high school's ELC eligibility threshold) and are time-invariant; see the text for the definition. Also see the text for definition of high school quartiles. IPEDS Average SAT score calculated for each school as the sum of the mean of the 25th and 75th percentiles of each SAT section, converting scores from 1600 scale to 2400 scale when necessary. Sticker price is defined using on-campus residency unless unavailable, in which case it is defined using off-campus non-family residency. IPEDS admission rate unavailable prior to 2005, and price information unavailable prior to 2008. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. ‡ Indicates reduced-form estimates with $p < 0.1$ for the null hypothesis such that $p \not< 0.05$ (*insignificant* at conventional levels) when estimated using a local linear model with bias-corrected and cluster-robust confidence intervals following Calonico et al. (2019). [1]If the applicant enrolls at a community college but then enrolls at a four-year university within 6 months, the latter is defined as her first institution of enrollment. [2]Net price includes tuition and fees, expected room and board, books and supplies, and other expenses net of federal, state, local, or institutional grant aid. Calculated as the IPEDS average net price for Title-IV-aid-awarded enrollees in the applicant's family income bin, where the observed bins are $0-30,000, $30,000-48,000, $48,000-75,000, $75,000-110,000, and above $110,000. Applicants with unobserved family incomes are omitted, and likely paid the sticker price (as they did not apply for federal financial aid, including loans).

Source: UC Corporate Student System, National Student Clearinghouse, the Integrated Postsecondary Education Data System (IPEDS), and Opportunity Insights (Chetty et al., 2020*a*).

Table B.19: Instrumental Variable Estimates of Near-Threshold ELC Eligibility Outcomes by Campus, with Unadjusted Log-Distance Instrumental Variables

	UCD	UCSD	UCSB	UCI	F^1
Predicted Grad.[2]	-0.32 (0.54)	-0.45 (0.78)	1.46 (1.61)	-0.50 (0.64)	0.583
Institution's 5-Year Grad. Rate	24.0 (4.94)	33.6 (7.21)	38.3 (14.8)	23.4 (6.20)	0.266
Grad. Within 5 Years (%)	32.48 (11.89)	27.40 (17.44)	53.47 (35.76)	27.75 (14.90)	0.817
Earn STEM Degree (%)	-9.06 (11.71)	-5.27 (17.87)	-71.07 (35.29)	-19.63 (14.52)	0.100
Enr. At Grad School within 7 Yrs. (%)	31.88 (12.85)	18.24 (18.58)	83.13 (40.46)	37.06 (16.08)	0.413
Num. Yrs. Pos. CA Wages[3]	0.33 (0.35)	0.19 (0.48)	-0.14 (1.01)	0.43 (0.45)	0.898
Avg. Early-Career Wages[3]	24,819 (10,581)	16,095 (13,836)	1,555 (28,973)	7,788 (12,635)	0.049
Avg. Early-Career Log Wages[3]	0.71 (0.31)	0.13 (0.48)	-0.87 (0.86)	0.19 (0.33)	0.006
First Stage F	107.1	12.8	7.2	61.8	
Conditional F	41.6	51.7	16.1	46.4	

Note: This table replicates Table 3.4 without adjusting the UCSB distance-to-campus instrument, showing that the reduction in that instruments' predictive power does not substantially change the main presented results. Estimates of the effect of Absorbing UC campus enrollment on educational and labor market outcomes for near-threshold ELC-eligible students following Equation 3.3. Log distance to Santa Barbara is unadjusted, resulting in poor instrument strength; see Table 3.4 for adjusted estimates. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Conditional F statistic estimated following Sanderson and Windmeijer (2016). [1] F-test of the null hypothesis of equality among the four campus enrollment coefficients. [2] The predicted values from an OLS regression (across the full sample of 1995-2013 UC freshman California-resident applicants, excluding the study's primary sample) of five-year NSC graduation on gender by ethnicity indicators, maximum parental education indicators (7 categories), family income, missing income indicator, SAT score, HS GPA, and year indicators. [3] The number of years between 6 and 8 years after high school graduation in which the applicant has positive covered California wages, and the applicants' unconditional average annual wages in the period.

Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

Table B.20: First-Stage IV Estimates of Near-Threshold ELC Eligibility Outcomes by Campus

	UCD	UCSD	UCSB	UCI
Log Distance to UC Davis × ELC Elig.	-0.063	0.021	0.016	0.021
	(0.003)	(0.004)	(0.003)	(0.003)
Log Distance to UC San Diego × ELC Elig.	0.018	-0.055	0.001	0.049
	(0.005)	(0.011)	(0.006)	(0.008)
Log Distance to UC Santa Barbara × ELC Elig.	0.019	0.009	-0.034	0.017
	(0.004)	(0.004)	(0.004)	(0.004)
Log Distance to UC Irvine × ELC Elig.	0.043	0.034	0.014	-0.091
	(0.005)	(0.010)	(0.006)	(0.008)

Note: This table shows first-stage OLS regression coefficients for the four instrumental variables used in the IV specifications in Table 3.4, showing that near-threshold enrollment at each UC campus is much more strongly predicted by distance to that campus as by distance to the other campuses. Estimates of the effect of the interaction between ELC eligibility and log distance to each Absorbing UC campus on enrollment at each of those campuses, representing the four first-stage regressions implied by instrumental variable OLS estimation of Equation 3.3. Each column presents estimates from a separate OLS regression predicting enrollment at the stated Absorbing UC campus; covariate coefficients are not reported. Log distance to Santa Barbara is set to 0 after 2010 to increase instrument strength. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted.

Source: UC Corporate Student System and National Student Clearinghouse.

Table B.21: Baseline Changes in Intended Major Selection

	Undec.	Art	Hum.	Soc. Sci.	Nat. Sci.	Engin.	Profess.	Bus.	STEM[1]
B50 Sample									
Baseline	14.9	-1.4	8.4	2.6	52.0	16.2	11.8	10.0	69.4
$\hat{\beta}$	-0.1	-0.1	-0.3	2.1	-2.1	0.8	-0.4	0.1	-1.3
	(1.0)	(0.5)	(0.8)	(1.0)	(1.3)	(1.0)	(0.6)	(0.7)	(1.3)
B25 Sample									
Baseline	19.5	-2.4	4.9	6.4	56.4	17.5	10.7	6.7	73.7
$\hat{\beta}$	0.0	-0.4	-0.4	1.6	-2.6	0.3	0.2	1.2	-2.0
	(1.5)	(0.6)	(1.2)	(1.6)	(1.9)	(1.5)	(0.9)	(1.1)	(2.0)

Note: This table shows that barely ELC-eligible applicants were somewhat less likely to report intended majors in natural science and broader STEM fields, with some switching toward social science, potentially suggesting that eligible students felt less pressure to earn lucrative majors if they attended more-selective universities. Reported coefficients are the estimated distribution of intended majors reported on UC applications at the barely ELC-ineligible baseline (estimated following Abadie (2002) with Absorbing UC campus enrollment as the endogenous variable), and the change in those characteristics across the ELC eligibility threshold ($\hat{\beta}$) estimated following Equation 3.2. Estimates from 2SLS cubic fuzzy regression discontinuity models; standard errors are clustered by school-year. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Statistical significance: † 10 percent, * 5 percent, ** 1 percent. [1]STEM includes all Natural Science and Engineering majors as well as some Professional majors (e.g. Agriculture and Architecture); see U.S. Department of Homeland Security (2016).

Source: UC Corporate Student System.

Table B.22: ELC Impact on Intended Major to Earned Major Transitions, B50 Sample

	No Degree	Art	Human.	Soc. Sci.	Nat. Sci.	Engin.	Profess.	Bus.	STEM[1]
Undeclared	-2.5	0.5	1.2	4.5	-4.3*	-0.2	0	4.2*	-3.5
Hum.	-1.4	-4.5*	1.8	2.8	1.9	0.1	-1.8	0.6	0.6
Soc. Sci.	-8.6**	0.6	-0.2	10**	-0.7	0.2	0.1	-1	-2.8
Nat. Sci.	-2.9	0.2	-1.2	5.3**	0.6	-2**	0.3	-0.6	-1.9
Engin.	0.4	0.7	-0.1	-0.6	0	-2.7	1.1	2	-1.8
Profess.	-10.1**	0.6	-4†	5.9	0.3	-0.9	4.9	2.1	1.1
Bus.	-5.9	-0.5	-0.2	1.9	3	-1.2	3.5	-1.9	1.2
STEM	-2	0.2	-1.4†	3.8**	-0.3	-1.2	0.5	0.4	-1.7

Note: This table shows that barely ELC-eligible intended STEM majors tended to switch into social science majors, though the estimates are too noisy to precisely estimate intended STEM majors' transition out of STEM fields. Reported coefficients are the estimated change in likelihood for barely ELC-eligible applicants ($\hat{\beta}$) to earn a major by discipline conditional on their intended major's discipline, among applicants from the bottom half of California high schools by SAT. Estimates from polynomial specification of Equation 3.2; hypothesis tests (from 0) conducted with standard errors clustered by school-year. Degree attainment measured five years after initial enrollment. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. [1]STEM includes all Natural Science and Engineering majors as well as some Professional majors (e.g. Agriculture and Architecture); see U.S. Department of Homeland Security (2016).

Source: UC Corporate Student System and National Student Clearinghouse

Table B.23: Impact of ELC Eligibility on Schooling and Labor Market Outcomes

	B50 Sample						B25 Sample					
	Reduced Form			Abs. UC	Potential Out.		Reduced Form			Abs. UC	Potential Out.	
	Base.	CCT	Min. GPA	IV	Below	Above	Base.	CCT	Min. GPA	IV	Below	Above
Inst. Five-Year	3.33	3.05	3.57	26.78	49.89	76.68	5.41	5.27	5.74	34.14	44.51	78.65
Grad. Rate (%)	(0.51)	(0.75)	(0.51)	(3.83)	(3.67)	(1.15)	(0.83)	(0.94)	(0.81)	(4.84)	(4.51)	(1.52)
Grad. within	3.50	4.50	2.66	28.59	46.41	75.00	4.83	6.29	3.13	31.00	34.98	65.98
Five Years (%)	(1.20)	(1.59)	(1.19)	(9.92)	(8.27)	(5.82)	(1.96)	(2.55)	(1.91)	(12.63)	(10.51)	(7.13)
Number of	-0.08	-0.12	-0.08	-0.62	4.99	4.37	-0.09	-0.12	-0.07	-0.55	4.96	4.41
Year Enrolled	(0.03)	(0.04)	(0.03)	(0.24)	(0.20)	(0.14)	(0.05)	(0.06)	(0.05)	(0.31)	(0.26)	(0.17)
STEM	-1.75	-1.27	-1.14	-14.28	37.32	23.04	-2.37	-1.80	-1.44	-15.22	26.46	11.24
Degree (%)	(1.05)	(1.23)	(1.04)	(8.81)	(6.52)	(6.05)	(1.34)	(1.56)	(1.29)	(8.95)	(6.46)	(6.03)
Deg. in Intended	2.56	3.60	3.24	20.92	25.08	46.00	2.76	4.31	3.28	17.68	7.89	25.57
Field of Study (%)	(1.24)	(1.47)	(1.24)	(10.20)	(7.73)	(6.65)	(1.71)	(2.12)	(1.66)	(11.01)	(8.71)	(7.13)
Enr. Grad. School	2.56	3.87	1.76	20.94	4.00	24.94	3.87	3.77	2.13	24.82	-1.27	23.54
Within 7 Years	(1.17)	(1.65)	(1.17)	(9.78)	(7.91)	(6.27)	(1.66)	(1.83)	(1.62)	(11.02)	(9.02)	(6.48)
# Early-Career	0.05	0.04	0.05	0.47	2.17	2.64	0.07	0.05	0.03	0.47	2.21	2.68
Years Employed	(0.03)	(0.04)	(0.03)	(0.30)	(0.24)	(0.18)	(0.05)	(0.05)	(0.05)	(0.35)	(0.30)	(0.20)
Average Early-Career	2,356	1,253	2,254	20,341	27,351	47,692	2,243	2,227	1,400	16,205	27,145	43,350
Covered Earnings	(901)	(1,206)	(865)	(8,199)	(6,322)	(4,891)	(1,184)	(1,262)	(1,139)	(8,884)	(7,164)	(5,231)
Average Early-Career	0.10	0.08	0.06	0.76	10.04	10.81	0.08	0.06	0.03	0.56	10.19	10.75
Log Covered Earnings	(0.04)	(0.05)	(0.03)	(0.33)	(0.25)	(0.21)	(0.06)	(0.06)	(0.04)	(0.39)	(0.32)	(0.25)

Note: This table summarizes a series of robustness checks and extensions of the main regression discontinuity findings, showing that they are generally robust to alternative specifications and also highlighting the changes in potential outcomes resulting from Absorbing UC campus enrollment because of ELC eligibility. Reported coefficients are the estimated change in various outcome measures for barely ELC-eligible applicants estimated by Equation 3.2: reduced-form 2SLS polynomial and 'CCT' local linear (Calonico, Cattaneo and Titiunik, 2014) regression discontinuity models; reduced-form polynomial regression discontinuity model using an alternative measurement of each high school's ELC eligibility threshold (just below the lowest ELC-eligible applicant's ELC GPA); polynomial models with Absorbing UC campus enrollment ($\mathbb{1}_{Abs.}$) as the endogenous variable; and estimated average potential outcomes for barely ELC-ineligible and barely ELC-eligible Absorbing UC campus compliers (following Abadie (2002)). Standard errors in parentheses are clustered by high-school-year. Coefficients estimated for applicants from the bottom half (B50) and quartile (B25) of California high schools by SAT; see text for details, and for definitions of the outcome variables. Early-career employment outcomes for 7-9 years after high school graduation. See Appendix Table B.26 for annual specifications 6-10 years after high school graduation. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted.

Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

Table B.24: Impact of ELC Eligibility on Schooling and Labor Market Outcomes by Subgroup

	Reduced-Form Polynomial Estimates						Five-Year Graduation Rate IV Estimates					
	B50	B25	Female	Male	URM	Non-URM	B50	B25	Female	Male	URM	Non-URM
Inst. Five-Year Grad. Rate (%)	3.33 (0.51)	5.41 (0.83)	3.97 (0.67)	2.27 (0.82)	3.59 (0.93)	3.03 (0.62)						
Grad. within Five Years (%)	3.50 (1.20)	4.83 (1.96)	3.45 (1.49)	3.89 (2.05)	3.97 (2.12)	3.08 (1.45)	1.01 (0.35)	0.85 (0.34)	0.89 (0.35)	1.57 (0.93)	1.08 (0.57)	0.98 (0.46)
Number of Year Enrolled	-0.078 (0.030)	-0.092 (0.050)	-0.058 (0.037)	-0.120 (0.051)	-0.103 (0.055)	-0.070 (0.035)	-0.024 (0.010)	-0.018 (0.010)	-0.016 (0.010)	-0.048 (0.025)	-0.030 (0.016)	-0.022 (0.012)
STEM Degree (%)	-1.75 (1.05)	-2.37 (1.34)	-3.74 (1.25)	0.54 (1.90)	-1.96 (1.45)	-1.61 (1.48)	-0.52 (0.33)	-0.44 (0.27)	-0.87 (0.36)	0.11 (0.84)	-0.55 (0.44)	-0.50 (0.52)
Deg. in Intended Field of Study (%)	2.56 (1.24)	2.76 (1.71)	1.49 (1.60)	4.09 (2.08)	3.06 (1.93)	2.19 (1.62)	0.79 (0.38)	0.54 (0.32)	0.42 (0.40)	1.85 (1.06)	0.89 (0.55)	0.78 (0.53)
Enr. Grad. School Within 7 Years	2.56 (1.17)	3.87 (1.66)	3.63 (1.58)	1.06 (1.81)	4.15 (1.90)	1.47 (1.51)	0.77 (0.36)	0.63 (0.31)	0.94 (0.41)	0.43 (0.81)	1.12 (0.58)	0.48 (0.50)
# Early-Career Years Employed	0.054 (0.034)	0.065 (0.048)	0.053 (0.042)	0.055 (0.057)	0.012 (0.056)	0.074 (0.044)	0.016 (0.011)	0.014 (0.010)	0.014 (0.011)	0.023 (0.030)	-0.003 (0.020)	0.025 (0.014)
Average Early-Career Covered Earnings	2,356 (901)	2,243 (1,184)	2,422 (1,079)	2,575 (1,616)	704 (1,349)	3,392 (1,207)	728 (310)	478 (258)	670 (298)	1,082 (938)	104 (488)	1,111 (437)
Average Early-Career Log Covered Earnings	0.100 (0.04)	0.083 (0.06)	0.080 (0.05)	0.146 (0.07)	-0.008 (0.06)	0.176 (0.05)	0.033 (0.02)	0.021 (0.01)	0.018 (0.01)	0.146 (0.17)	0.000 (0.02)	0.063 (0.03)

Note: This table summarizes heterogeneity in the estimated relative return to university selectivity for near-threshold ELC-eligible participants, generally suggesting similar treatment effect magnitudes for male, female, URM, and non-URM applicants. Reported coefficients are the estimated change in various outcome measures for barely ELC-eligible applicants from the polynomial specification of Equation 3.2, with the IV estimates replacing the endogenous variable with applicant's first institution's five-year graduation rate. Sample is restricted to the bottom half (B50) of California high schools by SAT; second column is further restricted to bottom quartile (B25) of high schools, and other columns are restricted to female, male, URM, or non-URM applicants. Standard errors in parentheses are clustered by high-school-year. URM includes black, Hispanic, and Native American applicants. See the text for definition of high school SAT quartiles and the outcome variables. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. Early-career employment outcomes are for 7-9 years after high school graduation.

Source: UC Corporate Student System, National Student Clearinghouse, and the California Employment Development Department (Bleemer, 2018*c*).

Table B.25: Tests of Treatment Effect Linearity in University Graduation Rate

	Number of HS Quantiles				
	2	4	6	8	10
Panel A: 2SLS Over-ID Tests on Graduation Rate					
IV β	1.05	0.88	1.04	1.04	1.16
	(0.35)	(0.33)	(0.33)	(0.32)	(0.31)
Sargan's S	0.15	1.89	1.85	2.58	2.31
p	0.698	0.595	0.870	0.921	0.986
Panel B: LIML Estimates on Graduation Rate					
IV β	1.06	0.95	1.21	1.33	1.47
	(0.36)	(0.57)	(0.53)	(0.67)	(0.59)
Panel C: 2SLS Estimates of Quadratic in Grad. Rate					
GR^2 β	0.369	0.326	0.091	0.091	0.052
	(1.332)	(0.336)	(0.063)	(0.056)	(0.039)

Note: This table reports the results of three series of tests of whether the changes in outcomes caused by barely ELC-eligible students' Absorbing UC campus enrollment could be usefully projected onto their change in university selectivity (indexed by five-year graduation rates). Interacting ELC eligibility and the running variable terms with applicants' high school quantiles, Panel A shows that over-id tests cannot reject linear returns to selectivity; Panel B shows that the LIML IV estimates do not shrink as the number of instruments increase; and Panel C shows that a quadratic term in graduation rate is not statistically significantly different from 0. Reported coefficients are coefficient estimates and test statistics from regressions of an indicator for applicants' five-year university graduation on their institution of first enrollment's NSC-calculated five-year graduation rate, instrumented by ELC eligibility interacted with high school SAT quantile indicators. Sample restricted to UC applicants in the bottom half of California high schools by near-threshold SAT score, and regressions include third-order polynomials in the ELC running variable interacted with quantile dummies along with high school and year fixed effects, gender-ethnicity indicators, and a quadratic in SAT score. Standard errors in parentheses clustered by high-school-year. **Panel A:** Coefficients and statistics from 2SLS regression estimation. Reported "IV β" is the second-stage term on five-year graduation rates; Sargan's S tests for over-identification and is distributed χ^2 with degrees of freedom equal to the number of high school quantiles minus 1 (p estimates model's likelihood under the null hypothesis). **Panel B:** Coefficients on graduation rate from Limited Information Maximum Likelihood estimation. **Panel C:** Coefficients on the square of graduation rate when both linear and squared rates are instrumented by ELC-interactions.

Source: UC Corporate Student System and National Student Clearinghouse

Table B.26: Impact of ELC Eligibility on Observed Annual California Wages

	B50 Sample						B25 Sample					
# Years after HS Grad:	6	7	8	9	10	11	6	7	8	9	10	11
Non-Zero Wage Indicator	1.65 (1.15)	1.34 (1.18)	1.81 (1.28)	1.82 (1.39)	1.76 (1.53)	2.13 (1.70)	2.55 (1.66)	2.23 (1.69)	1.52 (1.79)	1.53 (1.99)	0.53 (2.20)	1.25 (2.42)
Average Wages	734 (690)	1,758 (814)	2,150 (964)	2,068 (1,154)	2,237 (1,383)	2,063 (1,637)	1,341 (917)	1,637 (1,059)	2,062 (1,261)	1,652 (1,554)	1,249 (1,897)	2,597 (2,248)
Average Log Wages	0.017 (0.037)	0.083 (0.038)	0.070 (0.038)	0.042 (0.040)	0.040 (0.042)	-0.005 (0.048)	0.061 (0.052)	0.053 (0.050)	0.052 (0.054)	0.036 (0.056)	0.059 (0.060)	0.082 (0.069)
Latest Year Inc.[1]	2011	2011	2011	2010	2009	2008	2011	2011	2011	2010	2009	2008
Number of Observations	85,725	85,725	85,725	75,334	65,462	54,811	42,904	42,904	42,904	37,435	32,121	26,669
Percent of Total[1]	100	100	100	87.9	76.4	63.9	100	100	100	87.3	74.9	62.2

Note: This table shows that ELC eligibility appears to persistently increase wages for barely-eligible applicants as they age (from about age 24 to 29, though the number of observations declines in years further from high school graduation), suggesting that the main estimates are unlikely to be short-lived in applicants' very early careers. Estimated reduced-form changes ($\hat{\beta}$) in annual covered California employment and covered California wages and log wages 6-11 years after high school graduation caused by near-threshold ELC eligibility. Estimates from polynomial specification of Equation 3.2, restricting the sample to the bottom half (B50) or quartile (B50) of California high schools by SAT (see text for details); standard errors are clustered by school-year. Covered wages exclude wages not covered by unemployment insurance, including federal and self-employment. Applicants from high schools with ELC eligibility thresholds between 3.96 and 4.00 are omitted. [1]The latest observed high school cohort is 2011, and the latest observed wages are 2019. As a result, every year more than 8 years following high school graduation requires dropping one cohort of applicants from the estimation sample. These figures show the latest cohort included in estimation, the number of within-sample applicants in those cohorts, and the percent of all in-sample applicants in available cohorts.

Source: UC Corporate Student System and the California Employment Development Department. (Bleemer, 2018*c*).

Table B.27: Other Model Parameters

Parameter	Value	Parameter	Value	Parameter	Value
$\sigma^2_{\nu_{CSU}}$	1.54 (0.71)	γ^s_{Const}	-0.59 (0.04)	β^x_{Dist}	-0.37 (0.02)
$\sigma^2_{\nu_{Unimp}}$	3.05 (0.76)	γ^s_{LogInc}	-0.11 (0.02)	$\beta^x_{Dist^2}$	0.02 (0.01)
$\sigma^2_{\nu_{Abs1}}$	1.58 (0.72)	γ^s_{Female}	-0.14 (0.04)	$\beta^x_{Dist \times LogInc}$	0.08 (0.01)
$\sigma^2_{\nu_{Abs2}}$	1.78 (0.71)	γ^s_{Asian}	0.13 (0.05)	$\beta^x_{Dist \times Female}$	0.02 (0.01)
$\sigma^2_{\nu_{Disp}}$	2.22 (0.71)	γ^s_{URM}	-0.61 (0.06)	$\beta^x_{Dist \times Asian}$	-0.10 (0.02)
				$\beta^x_{Dist \times URM}$	-0.03 (0.02)
γ^c_{Const}	0.05 (0.001)	$\gamma^{q\|s}_{Const}$	-2.58 (0.72)		
γ^c_{LogInc}	0.02 (0.001)	$\gamma^{q\|s}_{LogInc}$	0.03 (0.31)		
γ^c_{Female}	0.00 (0.001)	$\gamma^{q\|s}_{Female}$	-0.55 (0.66)		
γ^c_{Asian}	-0.03 (0.001)	$\gamma^{q\|s}_{Asian}$	0.13 (0.65)		
γ^c_{URM}	-0.02 (0.001)	$\gamma^{q\|s}_{URM}$	-0.53 (1.06)		

Note: This table presents estimates of the remaining structural model parameters not presented in the main tables; see text for details. Parameter estimates from maximum simulated likelihood (maximized by the BFGS Quasi-Newton algorithm) of Equation 3.7. Reported standard errors from the inverse of the empirical Hessian matrix. Missing family incomes are imputed; see footnote 50. Continuous variables are standardized in-sample. Sample restricted to 2010-2013 UC freshman California-resident applicants who enroll at a public California institution in the fall after high school graduation.

Source: UC Corporate Student System and the National Student Clearinghouse.

Table B.28: Adjusted Admissions Thresholds in in Counterfactual Simulations

	Unimpacted	UCSD/UCSB	UCD/UCI	Dispersing
Baseline $\underline{\pi}_j$'s	1.95	0.46	0.15	-1.63
Counterfactual 1: Setting $\beta^{ELC}_{Abs1} = \beta^{ELC}_{Abs2} = 0$ before 2012				
	1.95	0.41	0.07	-1.64
Counterfactual 2: Setting $ELC = 1$ above threshold after 2011				
Threshold:				
1	1.95	0.48	0.17	-1.63
2	1.94	0.50	0.20	-1.62
3	1.94	0.52	0.23	-1.62
4	1.94	0.54	0.27	-1.62
5	1.94	0.57	0.32	-1.62
6	1.94	0.60	0.38	-1.61
7	1.94	0.63	0.46	-1.61
8	1.94	0.67	0.54	-1.61
9	1.94	0.70	0.63	-1.61

Note: This table shows how each UC campus's $\underline{\pi}_j$ admissions threshold adjusts to preserve expected enrollment in the counterfactual presence or absence of a top percent admissions policy; the Absorbing UC campuses' thresholds relax (tighten) when the top percent policy is removed (added), reflecting their trade-off between admitting students through regular admissions or the top percent policy. Parameter estimates from maximum simulated likelihood (maximized by the BFGS Quasi-Newton algorithm) minimizing changes in each UC campus's expected enrollment in counterfactual exercises removing the ELC policy before or adding the policy after 2011 (providing admissions advantages to students at or above the specified GPA rank admissions threshold). Standard errors are omitted. Sample restricted to 2010-2013 UC freshman California-resident applicants who enroll at a public California institution in the fall after high school graduation. See text for details on the counterfactual exercises.

Source: UC Corporate Student System and the National Student Clearinghouse.

Appendix C

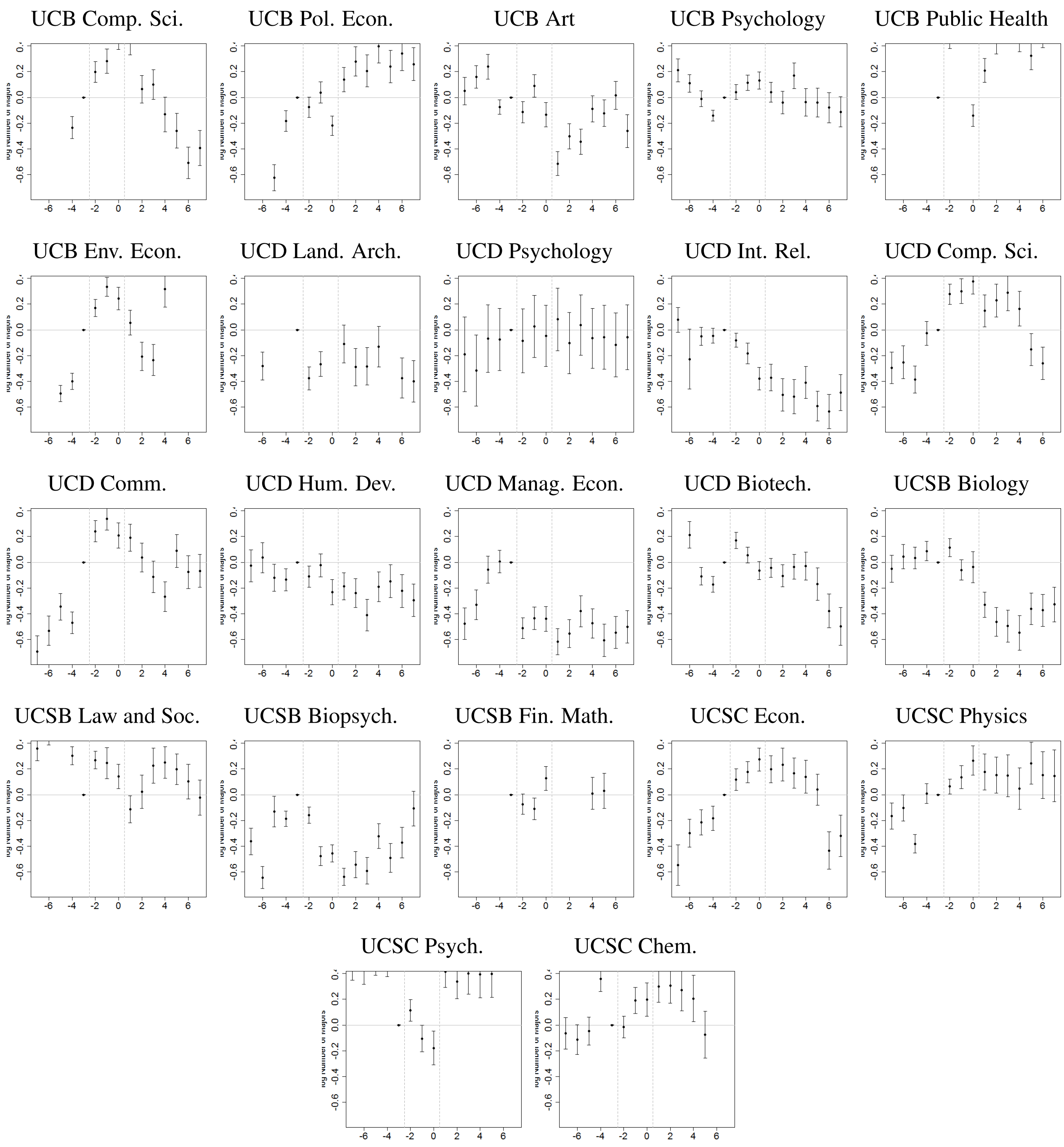
Appendix to Chapter 4

C.1 Department-Specific Event Study Estimates

This study estimates the average effects of newly-imposed major restrictions by averaging across every restriction imposed by the University of California campuses at Berkeley, Davis, Santa Barbara, and Santa Cruz, conditional on data availability in the surrounding years. However, heterogeneity in implementation timing, grandfathering provisions, strictness, and other characteristics of majors generate substantial heterogeneity in how majors' student composition shifted as a result of their new policies. This appendix presents estimates of β_t from Equation 4.2 separately for each estimable restriction event, decomposing the average into its many heterogeneous components.

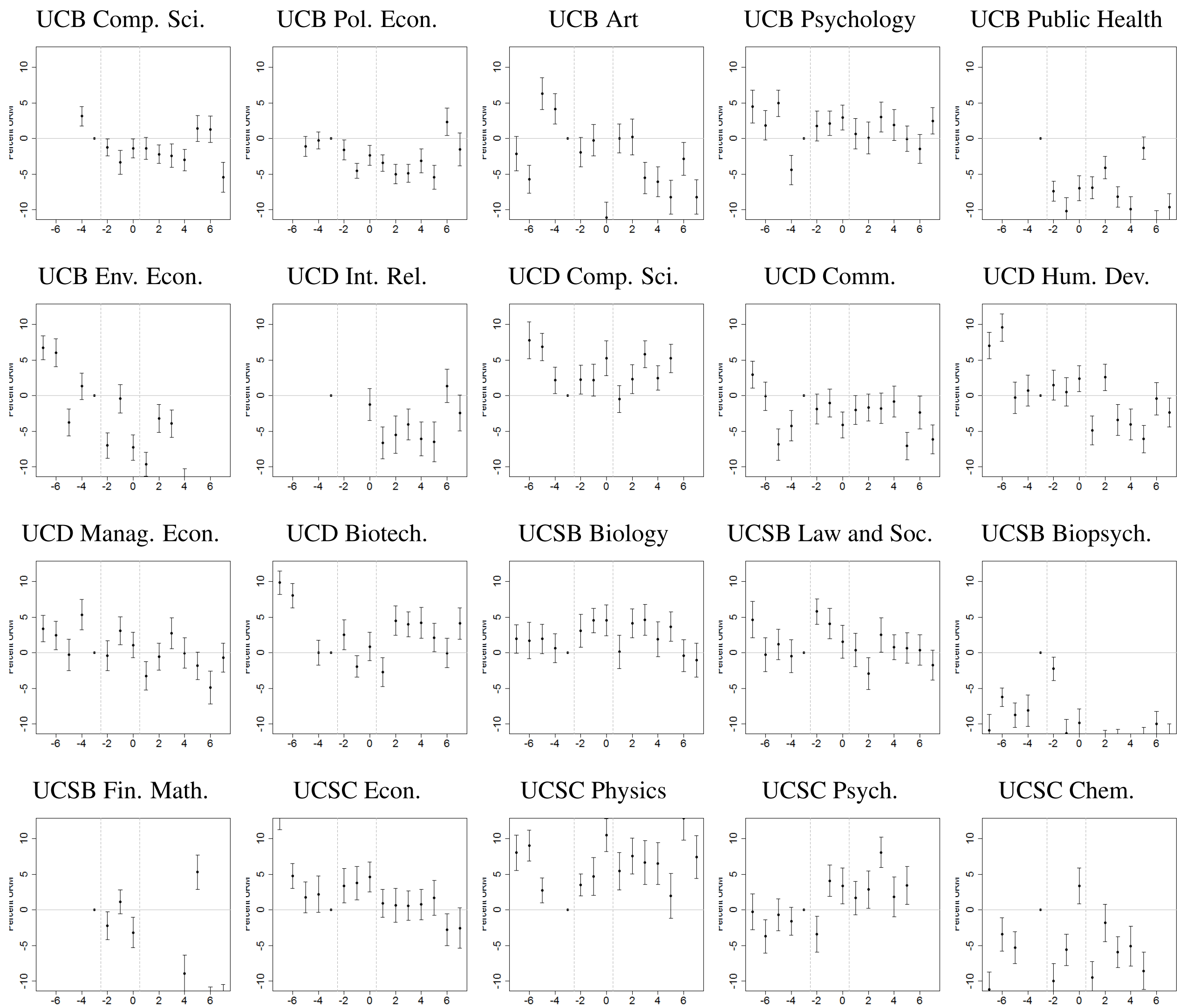
C.2 Other Appendix Figures and Tables

Figure C.1: Individual Department Event Studies: Log Number of Students



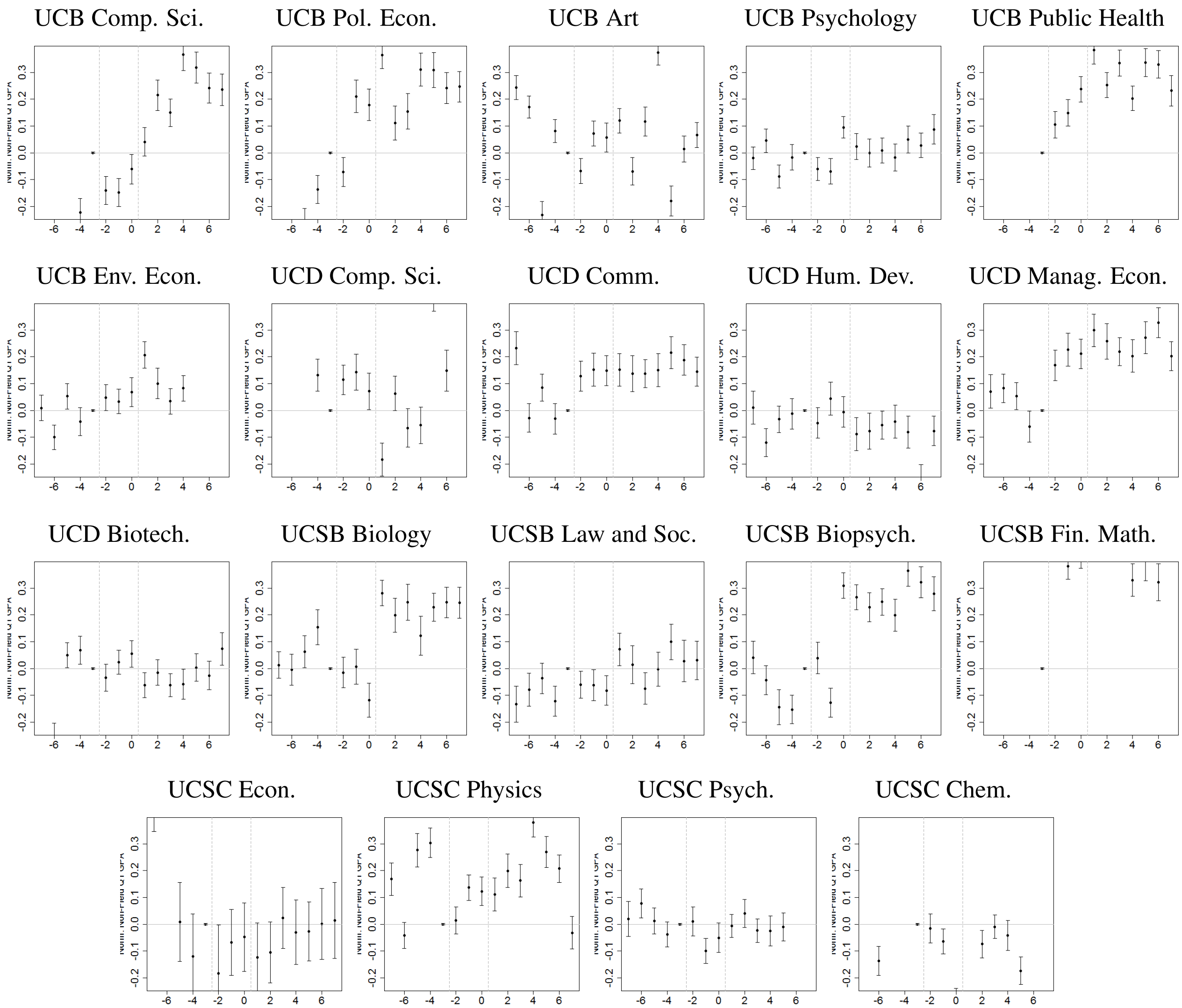
Note: Event study β estimates of the log number of students in each respective major before and after the implementation of its restriction, relative to other majors in that campus-year. Estimated over the full sample of campus-major-cohorts, but only including one 'event' per figure. β_{-1} is omitted and standard errors are clustered by campus-major. Source: UC ClioMetric History Project Student Database.

Figure C.2: Individual Department Event Studies: Percent of Majors URM



Note: Event study β estimates of the percent of declared students in each respective major who are underrepresented minorities (URM) before and after the implementation of the major's restriction, relative to other majors in that campus-year. Estimated over the full sample of campus-major-cohorts, but only including one 'event' per figure. β_{-1} is omitted and standard errors are clustered by campus-major. Source: UC ClioMetric History Project Student Database.

Figure C.3: Individual Department Event Studies: Outside-Discipline Normed GPA

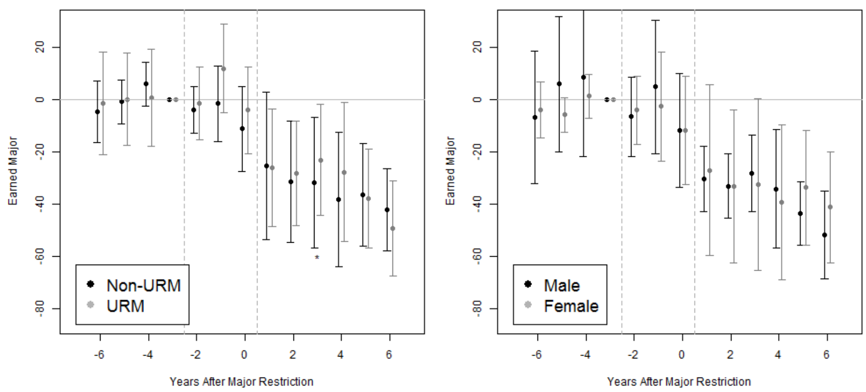


Note: Event study β estimates of each major's declared students' first-quarter normed GPA in courses taken outside of the major's division (and excluding Mathematics and Statistics courses) before and after the implementation of its restriction, relative to other majors in that campus-year. Estimated over the full sample of campus-major-cohorts, but only including one 'event' per figure. β_{-1} is omitted and standard errors are clustered by campus-major. Source: UC ClioMetric History Project Student Database.

Figure C.4: Estimated Changes in Major Choice and Composition of Students Who Intend Restricted Majors

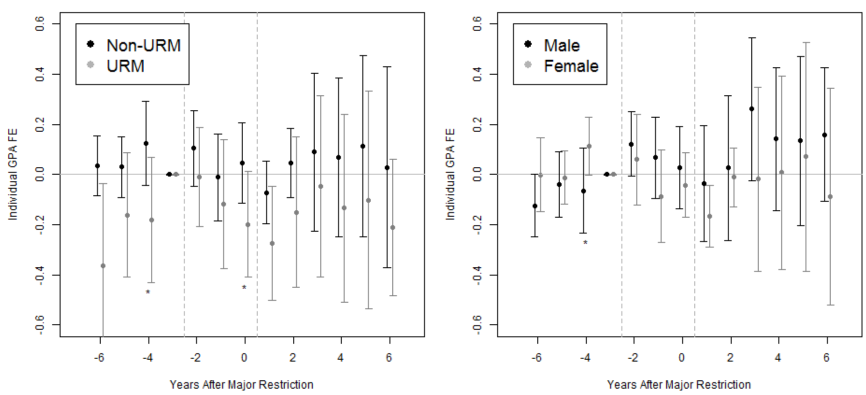
Panel A: Earn the Restricted Major

(a) By Ethnicity (b) By Gender



Panel B: Student GPA FE

(c) By Ethnicity (d) By Gender



Note: Difference-in-difference event study β_{it} estimates by gender and ethnicity of the relationship between students' intending the restricted major ($\hat{M}_{im}$) and their major choice or student characteristic before and after the implementation of the restriction, following Equation 4.5 and estimated over a stacked dataset of students i's major intentions in field m. Outcomes are defined as whether the student earns the restricted major and the student's GPA FE, their individual fixed effect from a two-way fixed effect model of GPA on student and course effects. β_{-3} is omitted, and standard errors are two-way clustered by campus-majors m and by students i. Models include campus-major-cohort fixed effects. Source: UC ClioMetric History Project Student Database.

Table C.1: Wage-by-Major Estimates from the American Community Survey

Major Code and Name		β	s.e.	Major Code and Name		β	s.e.
2419	Petroleum Engineering	0.7593	0.0842	2503	Industrial Production Technologies	0.2283	0.0293
6202	Actuarial Science	0.7476	0.0510	3202	Pre-Law and Legal Studies	0.2251	0.0285
6106	Health and Medical Preparatory Programs	0.7300	0.0377	6100	General Medical and Health Services	0.2227	0.0278
2404	Biomedical Engineering	0.7220	0.0388	6102	Communication Disorders Sciences and Services	0.2200	0.0245
3611	Neuroscience	0.7141	0.0498	6402	History	0.2197	0.0217
4006	Cognitive Science and Biopsychology	0.6462	0.0515	2602	Common Foreign Language Studies	0.2178	0.0238
6108	Pharmacy, Pharm. Sciences, and Admin.	0.6265	0.0239	5004	Geology and Earth Science	0.2138	0.0259
2405	Chemical Engineering	0.6226	0.0232	5401	Public Administration	0.2100	0.0314
3603	Molecular Biology	0.6188	0.0306	6103	Health and Medical Administrative Services	0.2076	0.0254
3601	Biochemical Sciences	0.6040	0.0256	5006	Oceanography	0.1965	0.0427
2418	Nuclear Engineering	0.6033	0.0587	2107	Computer Networking and Telecommunications	0.1954	0.0301
2407	Computer Engineering	0.5982	0.0226	2500	Engineering Technologies	0.1948	0.0349
4005	Mathematics and Computer Science	0.5890	0.0621	1902	Journalism	0.1932	0.0231
4003	Neuroscience	0.5828	0.2293	2106	Computer Information Management and Security	0.1886	0.0280
5008	Materials Science	0.5724	0.0380	5299	Miscellaneous Psychology	0.1881	0.0366
2408	Electrical Engineering	0.5585	0.0213	6006	Art History and Criticism	0.1880	0.0294
5501	Economics	0.5467	0.0218	1901	Communications	0.1865	0.0215
2415	Metallurgical Engineering	0.5406	0.0480	6104	Medical Assisting Services	0.1838	0.0309
2401	Aerospace Engineering	0.5377	0.0268	6110	Community and Public Health	0.1833	0.0313
3607	Pharmacology	0.5360	0.0650	1103	Animal Sciences	0.1771	0.0275
2414	Mechanical Engineering	0.5299	0.0213	2101	Computer Programming and Data Processing	0.1754	0.0384
5402	Public Policy	0.5226	0.0479	5206	Social Psychology	0.1697	0.0601
3701	Applied Mathematics	0.5190	0.0356	2303	School Student Counseling	0.1597	0.0380
2412	Industrial and Manufacturing Engineering	0.5069	0.0244	1301	Environmental Science	0.1551	0.0244
2416	Mining and Mineral Engineering	0.5039	0.0648	5200	Psychology	0.1515	0.0211
6207	Finance	0.4990	0.0214	3301	English Language and Literature	0.1504	0.0214
3605	Genetics	0.4972	0.0434	5000	Physical Sciences	0.1476	0.0532
2102	Computer Science	0.4874	0.0212	4002	Nutrition Sciences	0.1422	0.0324
3600	Biology	0.4854	0.0213	3201	Court Reporting	0.1401	0.0714
5003	Chemistry	0.4770	0.0224	4801	Philosophy and Religious Studies	0.1362	0.0239
6205	Business Economics	0.4695	0.0310	5507	Sociology	0.1356	0.0218
5505	International Relations	0.4671	0.0271	5301	Criminal Justice and Fire Protection	0.1327	0.0212
2417	Naval Architecture and Marine Engineering	0.4610	0.0504	5502	Anthropology and Archeology	0.1312	0.0247
5007	Physics	0.4564	0.0235	5503	Criminology	0.1302	0.0288
2410	Environmental Engineering	0.4547	0.0324	1101	Agriculture Production and Management	0.1154	0.0286
3702	Statistics and Decision Science	0.4516	0.0344	4007	Interdisciplinary Social Sciences	0.1123	0.0304
3606	Microbiology	0.4505	0.0276	2601	Linguistics and Comparative Language and Lit.	0.1068	0.0311
6212	Management Information Systems and Statistics	0.4399	0.0227	5201	Educational Psychology	0.1051	0.0414
2501	Engineering and Industrial Management	0.4375	0.0410	5504	Geography	0.0954	0.0252
5801	Precision Production and Industrial Arts	0.4307	0.1720	3604	Ecology	0.0903	0.0297
2403	Architectural Engineering	0.4297	0.0440	5202	Clinical Psychology	0.0799	0.0576
2413	Materials Engineering and Materials Science	0.4278	0.0344	2308	Science and Computer Teacher Education	0.0782	0.0271
2406	Civil Engineering	0.4277	0.0222	3401	Liberal Arts	0.0775	0.0224
5506	Political Science and Government	0.4257	0.0215	1903	Mass Media	0.0768	0.0244
2409	Engineering Mechanics, Physics, and Science	0.4251	0.0429	4000	Interdisciplinary and Multi-Disciplinary Studies	0.0748	0.0288
2105	Information Sciences	0.4219	0.0252	2310	Special Needs Education	0.0727	0.0236
2400	General Engineering	0.4136	0.0221	2399	Miscellaneous Education	0.0710	0.0246
2499	Miscellaneous Engineering	0.4034	0.0296	1303	Natural Resources Management	0.0704	0.0256
3608	Physiology	0.4015	0.0305	1302	Forestry	0.0607	0.0313
5599	Miscellaneous Social Sciences	0.4005	0.0486	4101	Physical Fitness, Parks, Recreation, and Leisure	0.0605	0.0224
3609	Zoology	0.3946	0.0306	2305	Mathematics Teacher Education	0.0592	0.0275
5001	Astronomy and Astrophysics	0.3867	0.0617	6004	Commercial Art and Graphic Design	0.0573	0.0228
3700	Mathematics	0.3745	0.0225	2603	Other Foreign Languages	0.0567	0.0347
6107	Nursing	0.3690	0.0210	3302	Composition and Speech	0.0555	0.0306
6210	International Business	0.3644	0.0270	3402	Humanities	0.0515	0.0331
6201	Accounting	0.3628	0.0211	2001	Communication Technologies	0.0498	0.0309
6204	Operations, Logistics and E-Commerce	0.3624	0.0277	5500	General Social Sciences	0.0479	0.0289
5005	Geosciences	0.3446	0.0454	1106	Soil Science	0.0433	0.0555
5601	Construction Services	0.3385	0.0264	2313	Language and Drama Education	0.0390	0.0238
5901	Transportation Sciences and Technologies	0.3342	0.0249	2300	General Education	0.0327	0.0212
2402	Biological Engineering	0.3332	0.0384	6211	Hospitality Management	0.0327	0.0251
3801	Military Technologies	0.3284	0.0867	6199	Miscellaneous Health Medical Professions	0.0317	0.0301
2100	Computer and Information Systems	0.3184	0.0221	1105	Plant Science and Agronomy	0.0256	0.0301
1104	Food Science	0.3078	0.0402	6005	Film, Video and Photographic Arts	0.0225	0.0279
2502	Electrical Engineering Technology	0.2903	0.0272	2311	Social Science or History Teacher Education	0.0197	0.0254
6200	General Business	0.2880	0.0211	2309	Secondary Teacher Education	0.0164	0.0235
5098	Multi-disciplinary or General Science	0.2810	0.0231	2306	Physical and Health Education Teaching	0.0034	0.0233
6206	Marketing and Marketing Research	0.2800	0.0215	5404	Social Work	0.0028	0.0223
2301	Educational Administration and Supervision	0.2798	0.0304	1100	General Agriculture	0.0000	0.0000
4008	Multi-disciplinary or General Science	0.2747	0.0353	5203	Counseling Psychology	-0.0041	0.0341
3699	Miscellaneous Biology	0.2740	0.0315	2304	Elementary Education	-0.0239	0.0211
4001	Intercultural and International Studies	0.2737	0.0297	2901	Family and Consumer Sciences	-0.0239	0.0236
5205	Industrial and Organizational Psychology	0.2694	0.0467	3501	Library Science	-0.0277	0.0502
6105	Medical Technologies Technicians	0.2667	0.0253	2312	Teacher Education: Multiple Levels	-0.0295	0.0262
1102	Agricultural Economics	0.2539	0.0373	5701	Electrical and Mechanic Repairs and Technologies	-0.0303	0.0463
6299	Misc. Business and Medical Admin.	0.2527	0.0281	1199	Miscellaneous Agriculture	-0.0310	0.0753
5102	Nuclear, Industrial Radiology, and Bio. Tech.	0.2522	0.0499	3602	Botany	-0.0335	0.0479
2599	Miscellaneous Engineering Technologies	0.2498	0.0272	5403	Human Services and Community Organization	-0.0529	0.0274
1501	Area, Ethnic, and Civilization Studies	0.2468	0.0260	2314	Art and Music Education	-0.0531	0.0239
5002	Atmospheric Sciences and Meteorology	0.2461	0.0381	6000	Fine Arts	-0.0577	0.0232
6209	Human Resources and Personnel Management	0.2446	0.0242	6001	Drama and Theater Arts	-0.0745	0.0257
6203	Business Management and Administration	0.2440	0.0208	6002	Music	-0.0791	0.0240
1904	Advertising and Public Relations	0.2429	0.0251	6003	Visual and Performing Arts	-0.0959	0.0349
1401	Architecture	0.2427	0.0227	2307	Early Childhood Education	-0.1104	0.0246
6109	Treatment Therapy Professions	0.2418	0.0226	6007	Studio Arts	-0.1463	0.0301
2504	Mechanical Engineering Related Technologies	0.2343	0.0354	6099	Miscellaneous Fine Arts	-0.1543	0.0844
2411	Geological and Geophysical Engineering	0.2339	0.1412	2201	Cosmetology Services and Culinary Arts	-0.1603	0.0385
6403	United States History	0.2288	0.0535	4901	Theology and Religious Vocations	-0.2731	0.0240

Note: Estimates from a linear regression of annual log income on major indicators across all employed college graduates in the 2009-2019 American Community Surveys (Ruggles et al., 2020), conditioning on gender-ethnicity-age and year indicators. Individuals with two majors are randomly assigned to one of their majors. Standard errors are robust. Source: American Community Survey.

Appendix D

Appendix to Chapter 5

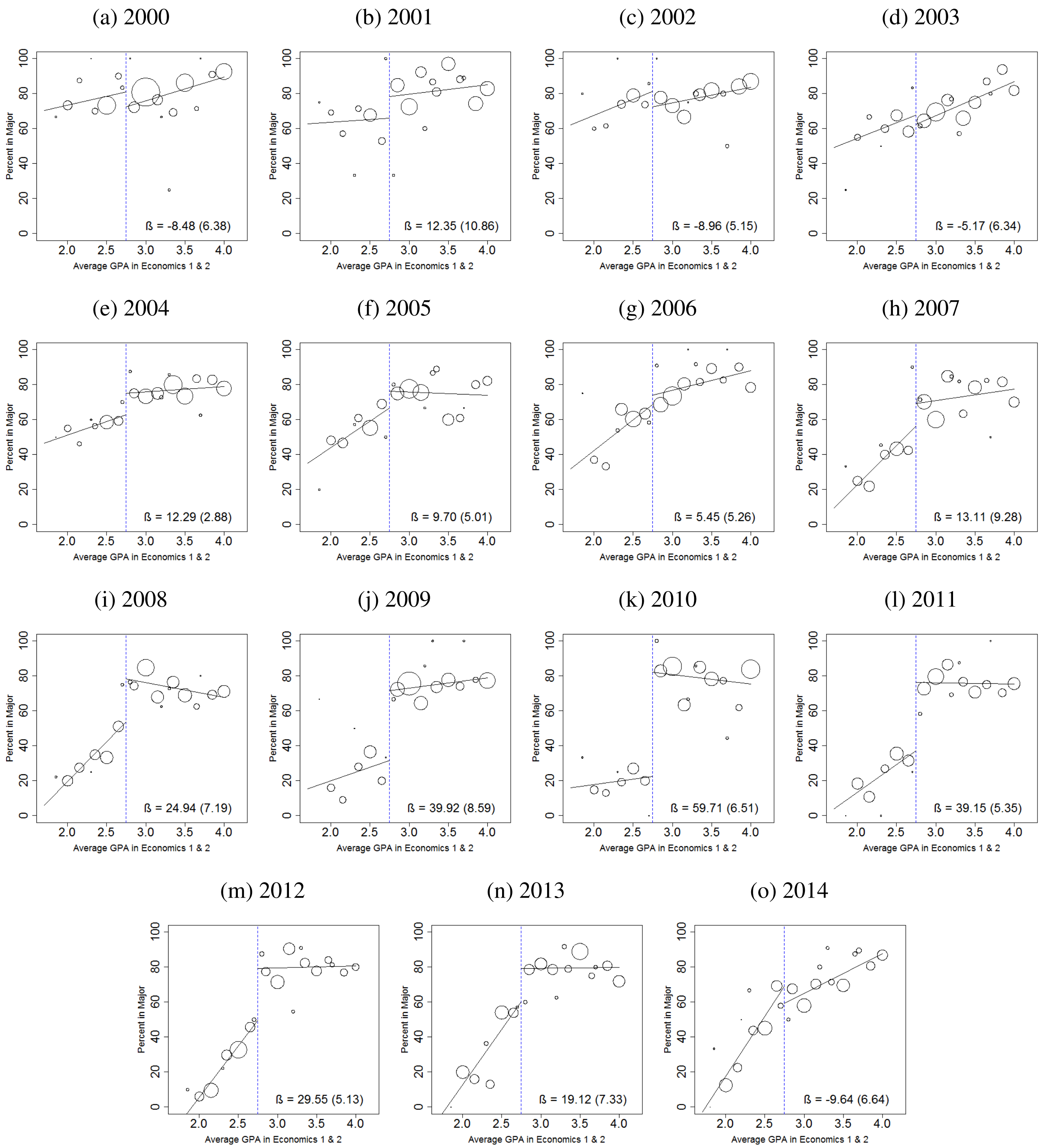
D.1 Survey Appendix

We analyze students' responses to two UCUES survey questions. The first question asks: "How many hours: -Studying and other academic activities outside of class," and respondents are provided eight radio-button alternatives: "0; 1-5; 6-10; 11-15; 16-20; 21-25; 26-30; More than 30". We code each range to its mean, and code "More than 30" to 35.

The second question asks: "Career hope to eventually have after education complete". Students available responses are: "Agricultural/agribusiness; Artistic, creative professions; Business, finance-related professions; Civil service/government; Education; Engineering, computer programming; Law; Medicine, health-related professions; Military; Psychology, helping professions; Researcher, scientist; I have no idea whatsoever; Other". Our analysis uses an indicator for whether the student selected the third response, "Business, finance-related professions".

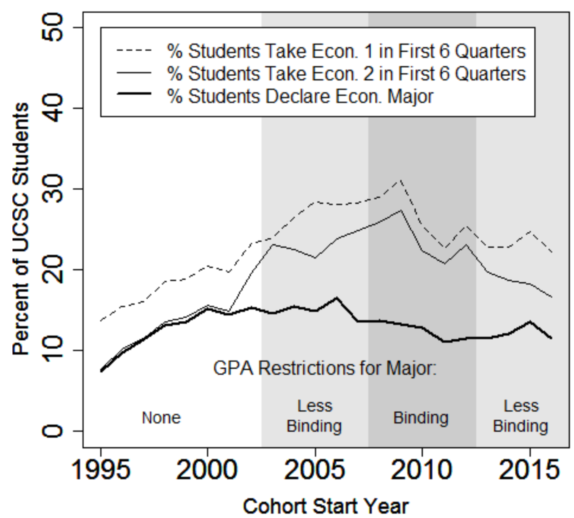
D.2 Other Appendix Figures and Tables

Figure D.1: UCSC Economics Major Declaration at the Admission Threshold by Year



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

Figure D.2: Trends in UCSC Economics

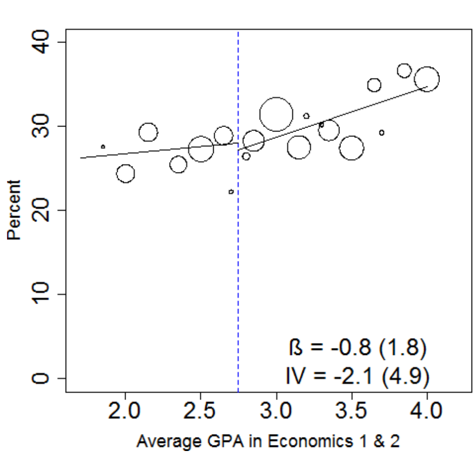


Note: This figure shows that the UCSC major restriction became binding following a substantial increase in student demand for the economics major leading up to and after the 2007-2008 financial crisis. This figure shows the annual proportion of UCSC freshman-admit students who enroll in Economics 1 or Economics 2 prior to the last quarter of their second year, and the proportion of those students who declare the economics major. UCSC formalized its economics major restriction in 2003; the "binding" period is defined as the years in which barely below-threshold students are estimated to be more than 20 percentage points less likely to declare the economics major than barely above-threshold students (see Figure D.1). Sources: The UC-CHP Student Database.

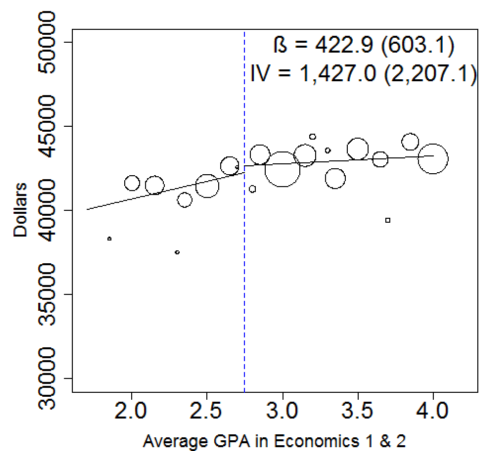
Figure D.3: Selection into Completing the Biannual UCUES Survey

Panel A: Sophomore/Junior Year Survey

(a) Completed Survey

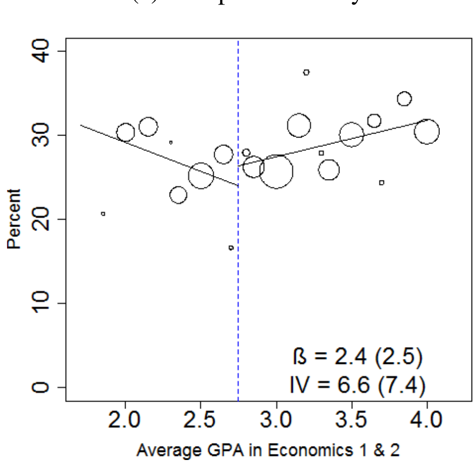


(b) Predicted Wages by Demographics

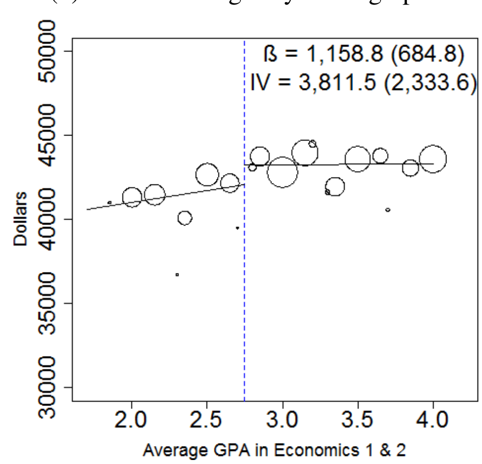


Panel B: Junior/Senior Year Survey

(c) Completed Survey



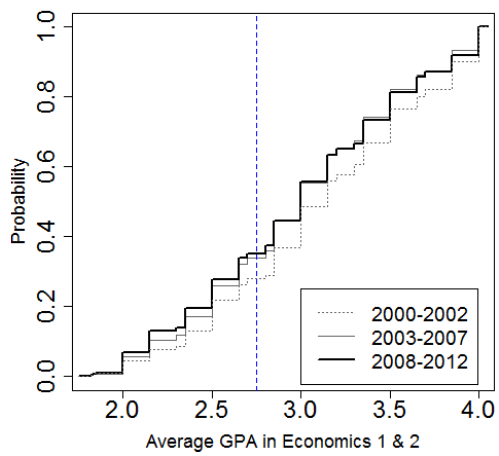
(d) Predicted Wages by Demographics



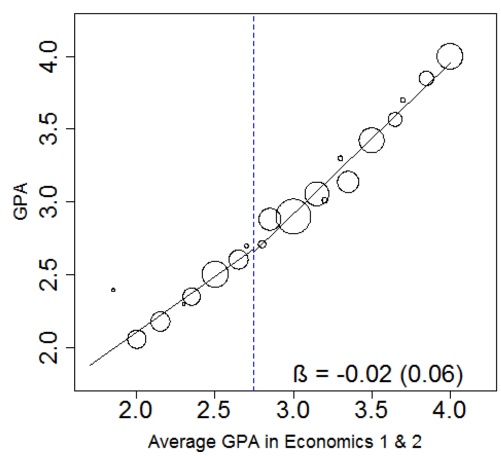
Note: This figure shows that UCUES survey response rates (among sophomore/junior respondents and junior/senior respondents) are smooth across the threshold, as are respondents' demographic and socioeconomic characteristics projected onto predicted postgraduate wages. Each circle represents the percent of students who completed the UCUES survey (for different survey timing) or respondents' predicted wages by demographic and socioeconomic background (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. "Predicted Wages by Demographics" estimates each student's predicted wages by a linear regression (among 2008-2012 UCSC students outside the main sample) of 2017-2018 wages on gender-ethnicity indicators, residency status, and third-order polynomials in SAT score and mean ZIP Code income. 2017-2018 wages are the mean in EDD-covered California wages in those years, omitting zeroes. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database and the CA Employment Development Department.

Figure D.4: Grade Distribution of Potential Economics Majors

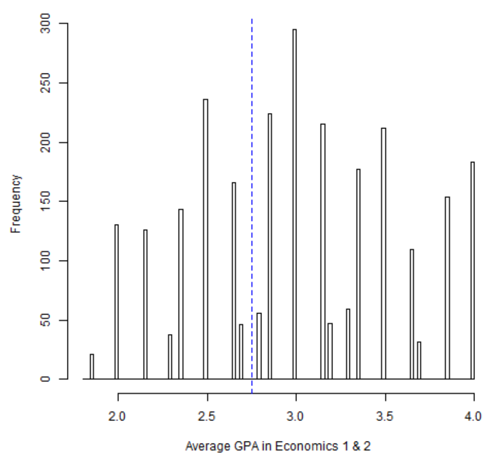
(a) Cumulative $EGPA$ Distribution



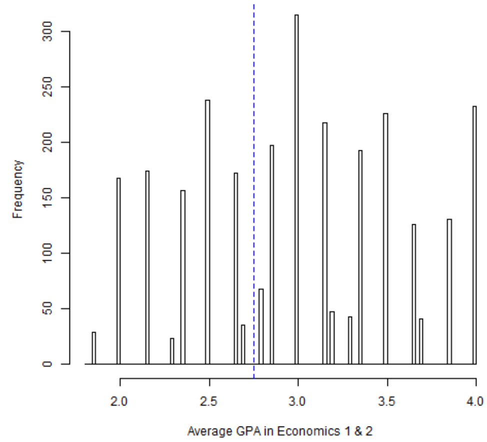
(b) 2008-2012 Grades in ECON 2



(c) 2003-2007

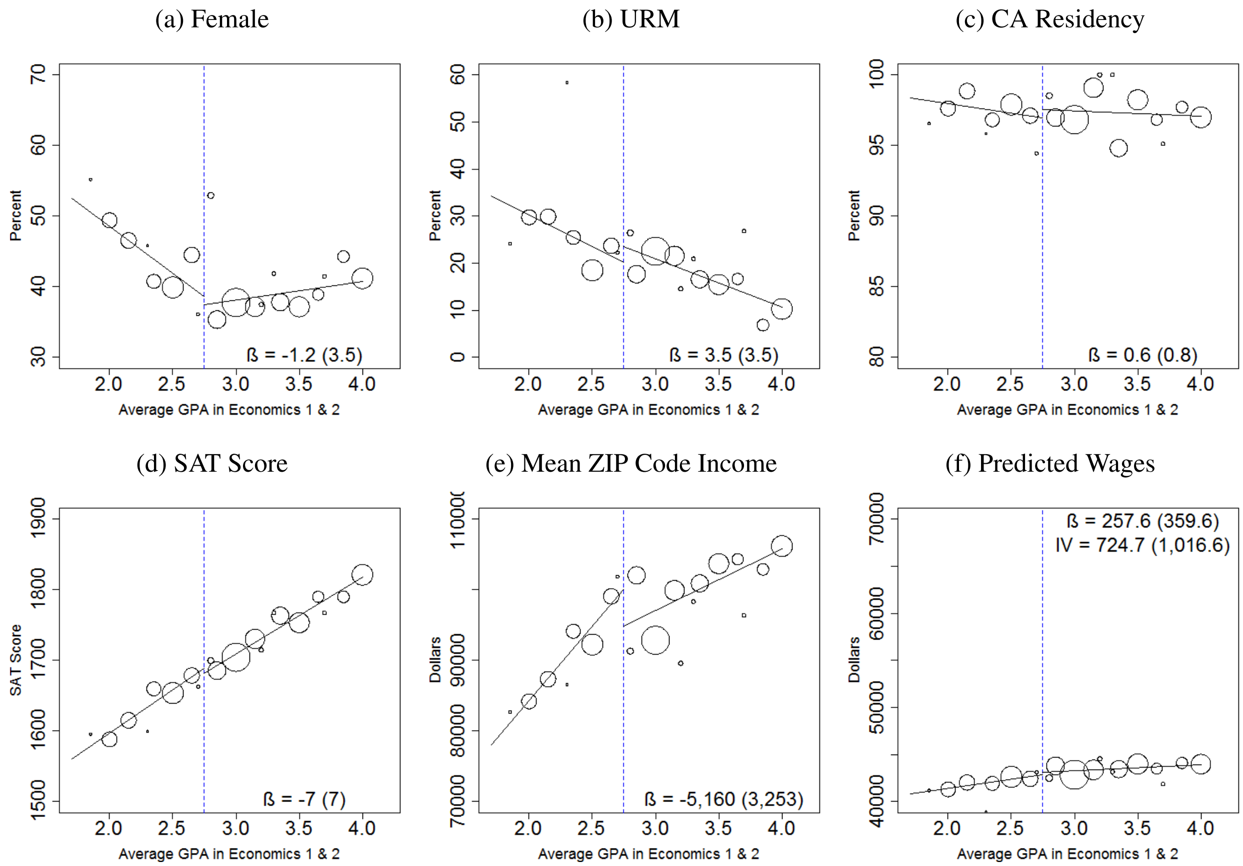


(d) 2008-2012



Note: This figure shows the distribution of UCSC Economics 1 and 2 grades ($EGPA$s), showing the absence of a pattern suggesting that students manipulated their grades above the GPA threshold. **Panel (a)** shows the cumulative distribution of Economics 1 and 2 $EGPA$s for three cohorts of freshman-admit UCSC students: 2000-2002, 2003-2007, and 2008-2012. In **Panel (b)**, each circle represents the average Economics 2 grade (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. **Panels (c) and (d)** show the distribution of $EGPA$s among the 2003-2007 cohorts (when the major restriction policy was less-binding) and the 2008-2012 cohorts. Source: The UC-CHP Student Database.

Figure D.5: Baseline Balance at the Economics Major Eligibility Threshold

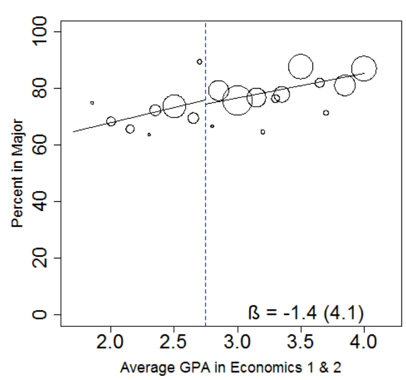


Note: This figure shows that 2008-2012 UCSC students' socioeconomic characteristics were smooth across the economics GPA threshold, separately and together in a one-dimensional prediction of early-career earnings. Each circle represents the mean demographic or socioeconomic characteristic (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. For the 4 percent UC students who submit ACT test scores instead of SAT scores, or SAT scores on a 1600 point basis, the scores are converted to 2400-point SAT scores using standard concordance tables. ZIP Codes are from students' applications, and are matched to reported mean adjusted gross income in their application year. "Predicted Wages" estimates each student's predicted wages by a linear regression (among 2008-2012 UCSC students who did *not* complete Economics 1 and 2) of 2017-2018 wages on gender-ethnicity indicators, residency status, and third-order polynomials in SAT score and mean ZIP Code income. Predicted wages are restricted to students with observed 2017-2018 wages. 2017-2018 wages are the mean in EDD-covered California wages in those years, omitting zeroes; wages are CPI-adjusted to 2018 and winsorized at 2% above and below. $EGPA$s below 1.8 are omitted, leaving 2,839 students in the sample (2,446 with observed wages). Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification; standard error (clustered by $EGPA$) in parentheses. Sources: The UC-CHP Student Database, IRS SOI, and the CA Employment Development Department.

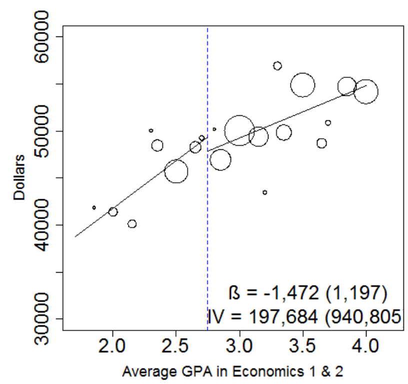
Figure D.6: Placebo Tests: Treatment Effect on Major and Wages with No Restriction or Less-Binding Restriction

Panel A: 2000-2002 Cohorts (No Restriction)

(a) Economics Major

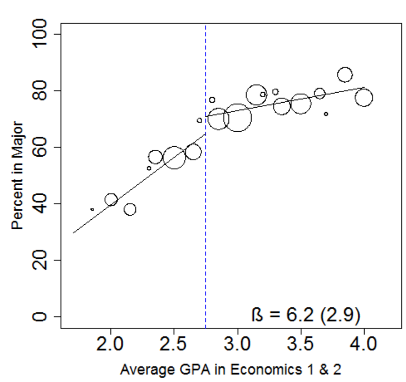


(b) Annual 2008-2009 Wages

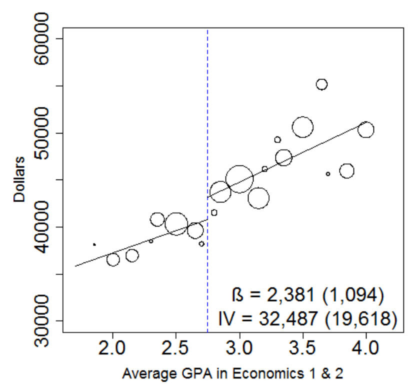


Panel B: 2003-2007 Cohorts (Less-Binding Restriction)

(c) Economics Major

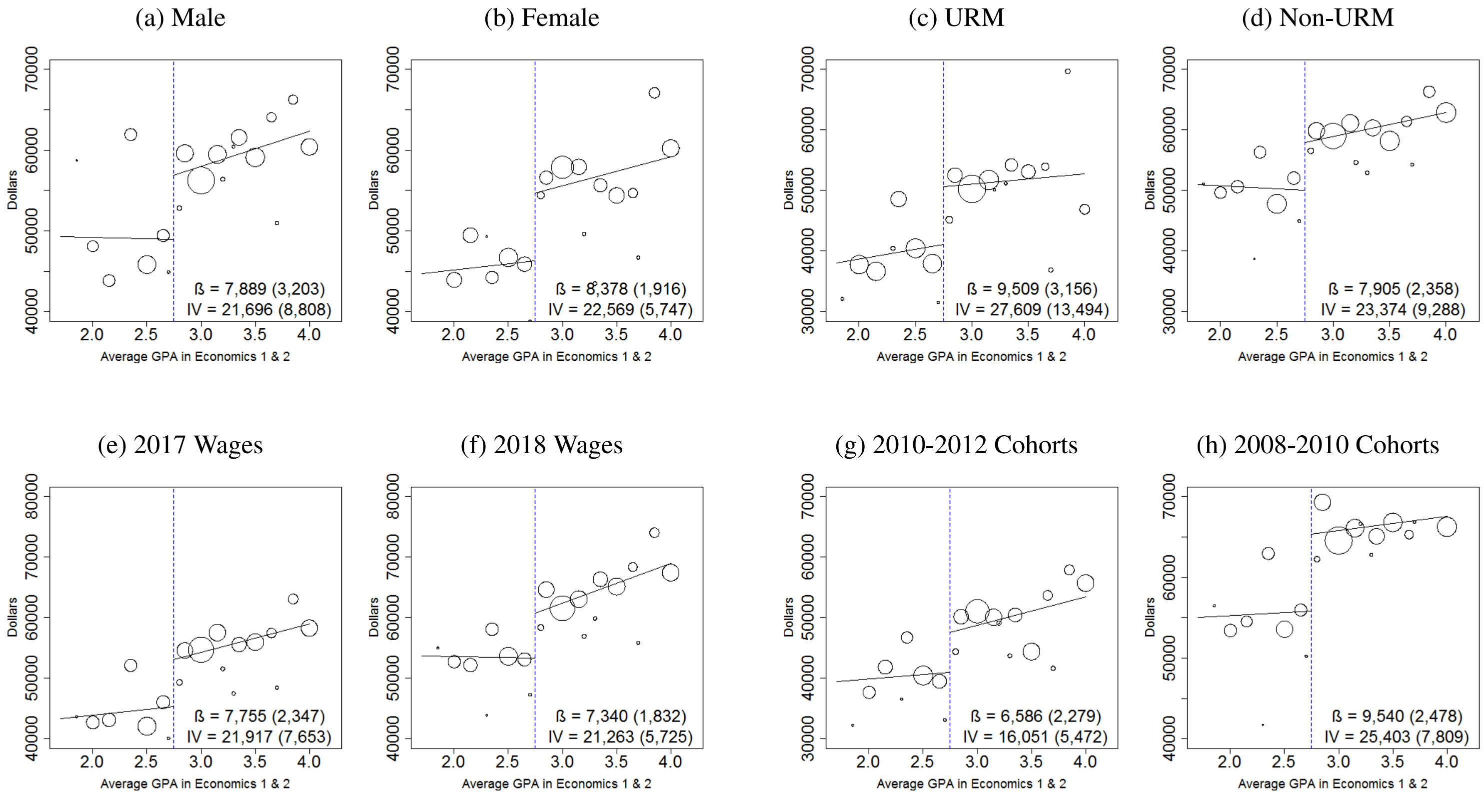


(d) Annual 2012-2013 Wages



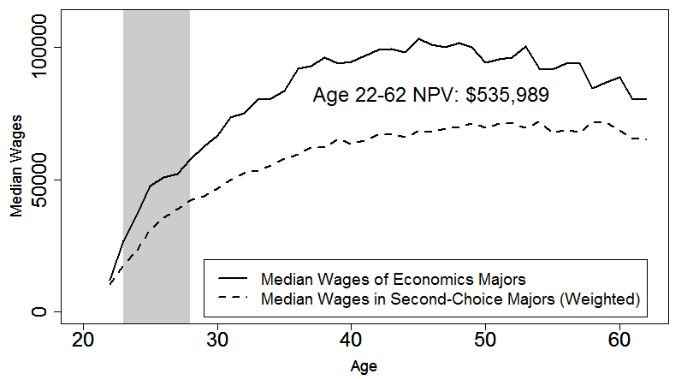
Note: This figure presents two placebo tests showing (A) that major choice and wages were smooth across the 2000-2002 2.8 $EGPA$ threshold (prior to the policy's initial implementation) and (B) both slightly discontinuous in 2003-2007 (during the policy's less-binding phase), generating a similar (but noisy) instrumental variable estimate of the impact of economics major choice on early-career wages. Each circle represents the proportion of economics majors or mean annual wages of UCSC students (y axis) among those who earned a given $EGPA$ in Economics 1 and 2 (x axis), restricted to the 2000-2002 or 2003-2007 UCSC cohorts. The size of each circle corresponds to the proportion of students who earned that $EGPA$. $EGPA$s below 1.8 are omitted. UCSC did not restrict the economics department to the 2000-2002, and only maintained a loosely-binding major restriction for the 2003-2007 cohorts. Wages are presented for each cohort when they were approximately the same age as in the main analysis. 2008-2009 and 2012-2013 wages are the mean in EDD-observed California wages in those years; individuals with no wages in one year are assigned the other year's wages, and those with no observed wages in either are omitted. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database and the CA Employment Development Department.

Figure D.7: Earnings Effect Heterogeneity at the Economics GPA Threshold, 2008-2012



Note: This figure shows that the wage return to majoring in economics is of similar magnitude when measured among male and female students or among underrepresented minority (URM) and non-URM students, is of similar magnitude when measured in 2017 or 2018, and appears somewhat larger for earlier (and thus older) cohorts. Each circle represents the mean annual wages of UCSC students (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Panels (a) to (d) restrict the sample to male, female, URM (Black, Hispanic, or Native American), and non-URM students, respectively. Panels (e) and (f) measure wages in 2017 or 2018, respectively; all other panels measure wages as the mean between EDD-observed 2017 and 2018 California wages in those years, where individuals with no wages in one year are assigned the other year's wages. Panels (g) and (h) restrict the sample to only the 2010-2012 and the 2008-2010 cohorts, respectively. $EGPA$s below 1.8 are omitted. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database and the CA Employment Development Department.

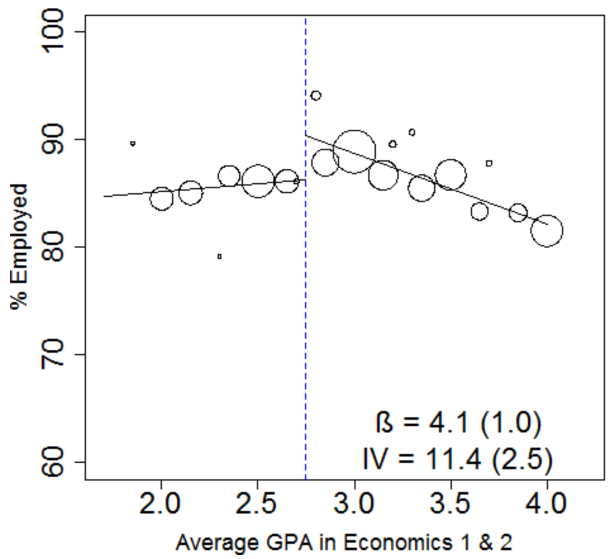
Figure D.8: Lifetime Earnings Difference for Economics Majors in the ACS



Note: This figure shows that the relative observational return to majoring in economics increases with age in workers' 20s and 30s and remains large throughout workers' careers, resulting in a $536,000 observational net present value of majoring in economics (relative to barely above-threshold UCSC students' distribution of second-choice majors). This figure shows annual median wages of economics majors and other majors (weighted by policy compliers' counterfactual likelihood of earning that major; see Figure 5.6) by age among all 22-62 ACS respondents between 2009 and 2018, CPI-adjusting wages to 2018 dollars. The "Age 22-62 NPV" is the net present value (at age 22) of majoring in economics, assuming that a worker working full-time and full-year would receive the median economics wage at each age between 22 and 62 if she majors in economics and the weighted other majors' median wage at each age otherwise (and assuming a 3 percent discount rate). The shaded area overlaps with our observed sample, enabling empirical validation. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Sources: The UC-CHP Student Database and the American Community Survey (Ruggles et al., 2020).

Figure D.9: California Employment at the Economics GPA Threshold, 2008-2012

(a) 2017-2018

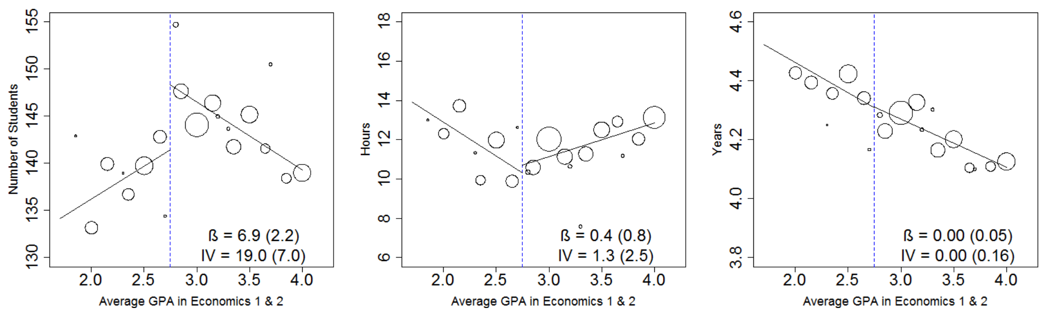


Note: This figure shows that 2017-2018 California employment is high (over 85 percent) for UCSC students near the GPA threshold, with some evidence (depending on specification) of slightly increased employment likelihood just above the economics GPA threshold. Each circle represents the percent of 2017-2018 California employment (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Employment is defined as earning non-zero EDD wages in either 2017 or 2018. $EGPA$s below 1.8 are omitted. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database and the CA Employment Development Department.

Figure D.10: Effect of Economics Major Access on Other Educational Outcomes

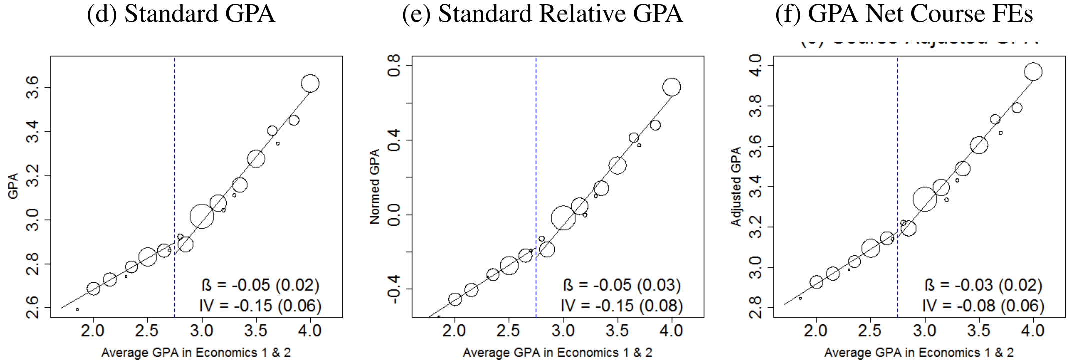
Panel A: Educational Experience

(a) Median Class Size (b) # Hours/Week Studying (c) Years Enrolled



Panel B: Student Grade Point Average Measures

(d) Standard GPA (e) Standard Relative GPA (f) GPA Net Course FEs

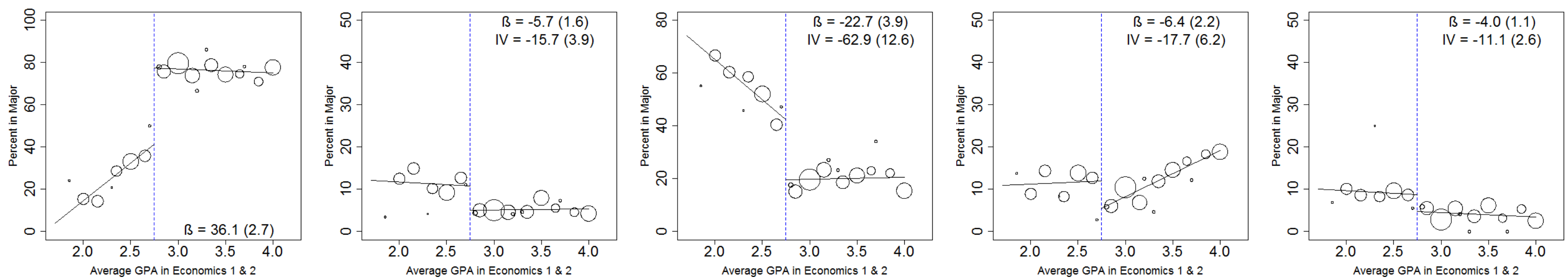


Note: This figure shows that barely above-threshold UCSC students had larger classes but spent similar time studying when compared to below-threshold peers. They also had smooth (or slightly lower) average grades, average grades compared to their peers, and average grades partialing out course fixed effects (from a two-way FE model). This suggests both both that students' educational intensity and performance cannot explain their labor market success and that the students hardly (if at all) struggled in the courses they were nearly restricted from. Each circle represents the mean educational characteristic (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Median class size measured by course department, number, and term. Number of hours studying per week measured among 789 in-sample UCUES survey respondents in their third or fourth year (the survey is biannual). Years enrolled measures the number of academic years (of the seven following high school graduation) in which the student is observed as enrolled in NSC but has not yet earned a Bachelor's degree. Standard GPA is a weighted average over students' grades by units. Standardized Relative GPA is the credit-unit-weighted average over students' within-course standardized grades (using course grade means and standard deviations). GPA Net Course FEs is calculated as each student's credit-unit-weighted mean of the differences between students' grades and each course's fixed effect from a two-way fixed effect model of UCSC course grades on student and course effects, with a 2013 writing course as the omitted course. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Sources: The UC-CHP Student Database and the Student Experience in the Research University (SERU) database.

Figure D.11: Major Choice at the Economics GPA Threshold, 2008-2012

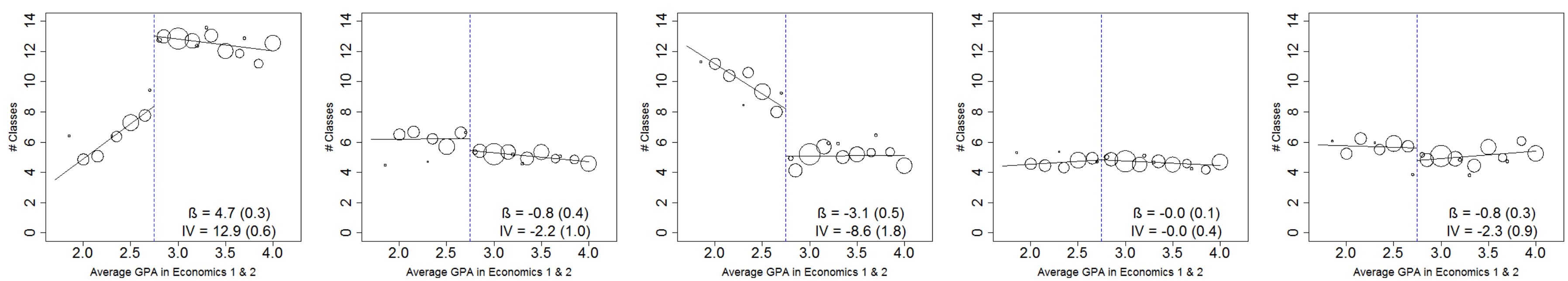
Panel A: Change in Major Choice

(a) Economics (b) Humanities (c) Other Social Sciences (d) Natural Sciences (e) Engineering



Panel B: Change in Course Enrollment

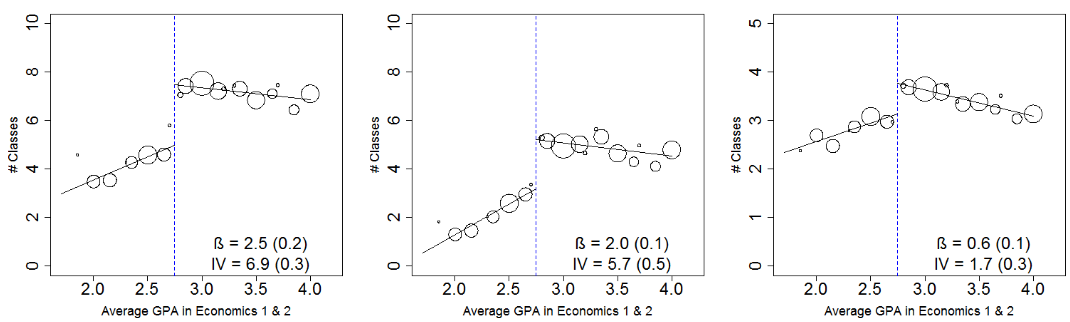
(f) Economics (g) Humanities (h) Other Social Sciences (i) Math/Statistics (j) Other Natural Sciences



Note: This figure shows that about two-thirds of barely above-threshold policy compliers would have otherwise earned degrees in the other social sciences, and that about 8.5 of economics majors additional 13 economics courses would have otherwise been in other social science departments (though there is no net change in their number of completed mathematics and statistics courses). Each circle represents the mean percent of students in the major area or the mean number of courses taken in an area (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Major indicators include students with multiple majors. Majoring in economics indicates declaring any of UCSC's three economics major tracks: economics, global economics, or business management economics. "Other social sciences" includes all social sciences other than economics. "Math/Statistics" includes all courses in the Mathematics or Applied Mathematics and Statistics departments; "other natural sciences" includes all other natural sciences. Source: The UC-CHP Student Database.

Figure D.12: Detailed Economics Course Completions at the Economics GPA Threshold, 2008-2012

(a) Economics Sub-Disciplines (b) Business Sub-Disciplines (c) Quant. Method. Courses

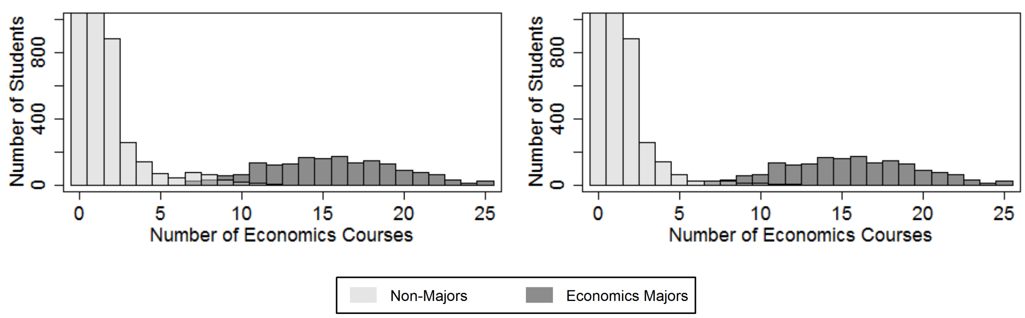


Note: This figure shows that the 13 additional economics courses taken by barely above-threshold economics majors were split between traditional economics sub-disciplines and business and finance sub-disciplines, and that economics majors took two additional quantitative methodology courses across departments. Each circle represents the mean number of courses taken in an area (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Business sub-disciplines include all accounting or "business management upper division electives" as designated by UCSC, which include courses in management, finance, and marketing; traditional economics subdisciplines include all other courses in offered by the Department of Economics. Quantitative methodology courses include any course that mentions 'statistics', 'econometrics', 'psychometrics' or 'quantitative/math/research/information methods' in its title. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database.

Figure D.13: Histograms of Economics Courses taken by UCSC Economics Majors and Non-Majors

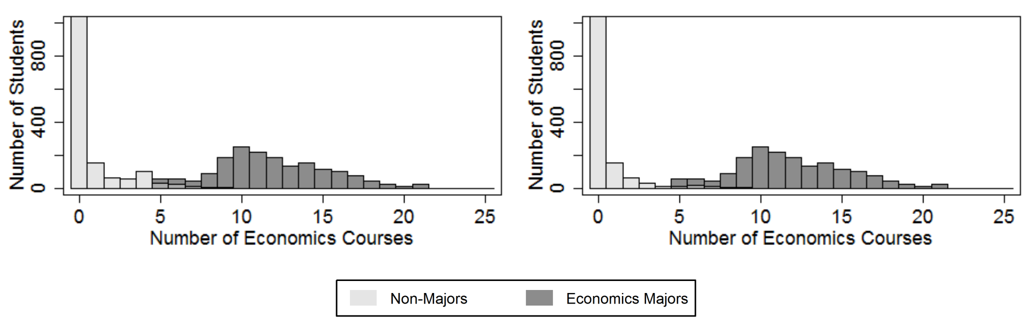
Panel A: All Economics Courses

(a) All Non-Economics Majors (b) Excluding TIM Majors



Panel B: Upper-Division Economics Courses

(c) All Non-Economics Majors (d) Excluding TIM Majors

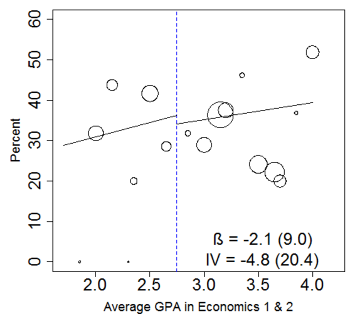


Note: Histograms showing the number of freshman-admit UCSC graduates from the 2008-2012 cohorts by the number of economics courses they completed. The sample is split by whether the student earned a major in economics, with ‘non-majors’ including (excluding) Technology and Information Management (TIM) majors in panels a and c (b and d). Panel A includes all economics courses; Panel B includes only upper-division economics courses (that is, with course numbers above 99). Course counts are winsorized at 25 for all courses and 21 for upper-division courses, with fewer than 25 students having taken more such courses. Some bars are taller than the chosen y-axis. Source: The UC-CHP Student Database.

Figure D.14: Additional Specifications of the Intended Career in Business/Finance Survey Responses at the 2008-2012 Economics GPA Threshold

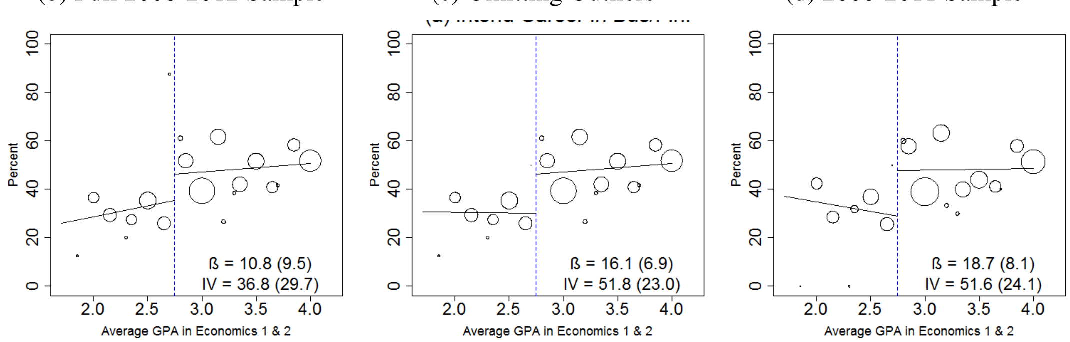
Panel A: Freshman UCUES Survey Responses on Intend Career in Bus/Fin.

(a) First-Year Respondents



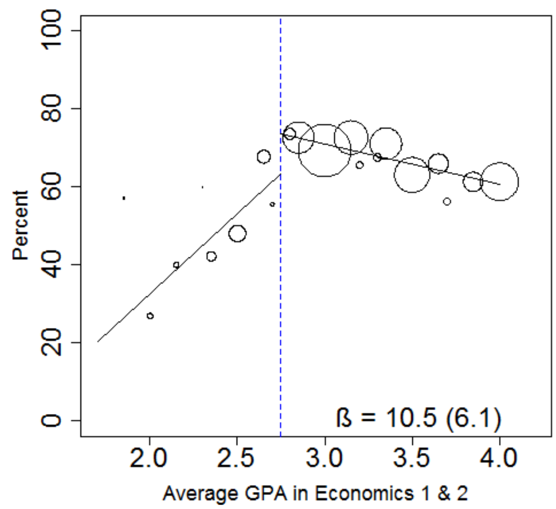
Panel B: Alternative Sample Specifications of Sophomore/Junior Responses

(b) Full 2008-2012 Sample (c) Omitting Outliers (d) 2008-2011 Sample



Note: This figure shows that (A) there was no difference in *first-year* survey respondents' baseline business/finance career intentions (prior to taking many economics courses), and (B) estimated differences in sophomore-junior responses are sensitive to six 2.7-$EGPA$ 2012 sophomore economics major "outliers" (who make up 75% of all 2.7-$EGPA$ UCUES respondents, and all intend business/finance careers). Each circle represents the percent of students in different samples who report intending business/finance careers (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Panel A is restricted to the 338 in-sample students who completed the survey in the spring of their first year; Panel B is restricted to the 874 students who completed in it in their second or third year. Panel (c) further omits six "outlier" students easily-observable in (b): they are all 2012 second-year respondents with 2.7 (below-threshold) $EGPA$s, economics majors, and report intending business/finance careers, which given their closeness to the threshold strongly shifts the estimated effect of majoring in economics despite their non-compliance and small number. Panel (d) instead omits all 2012 respondents, showing a similar pattern to (c). Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database.

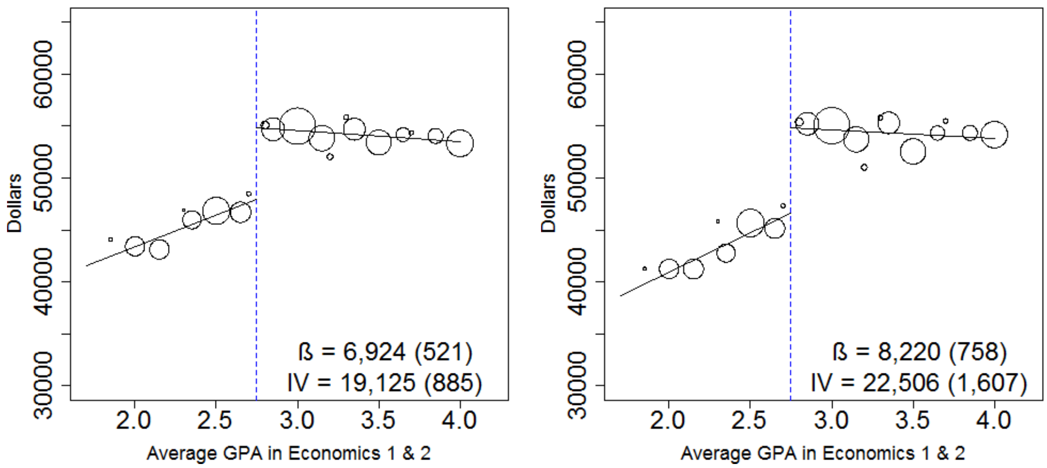
Figure D.15: Share of 2008-2012 UCSC Economics Majors on the "Business Management Economics" Track



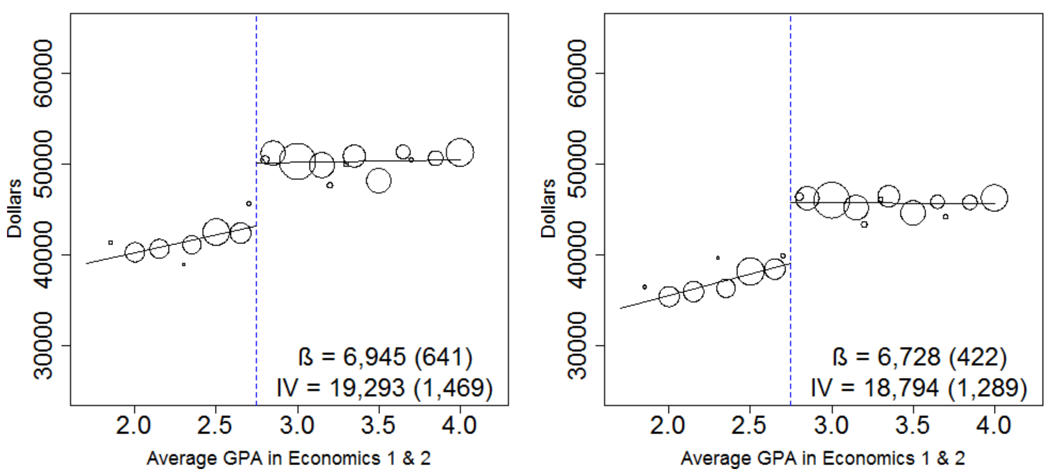
Note: This figure shows that the proportion of economics majors on the business economics track is relatively smooth across the GPA threshold, implying that the wage returns at the threshold are unlikely to arise as a result of access specifically to the business economics track changing at the GPA threshold. Each circle represents the percent of economics majors on the business management economics track (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. $EGPA$s below 1.8 are omitted, leaving 1,671 economics majors. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Source: The UC-CHP Student Database.

Figure D.16: Median Wages in the 2008-2012 UCSC Cohorts' Chosen Majors, Imputed from Different Samples

(a) UCSC Economics 1&2 Students (b) All UCSC Students

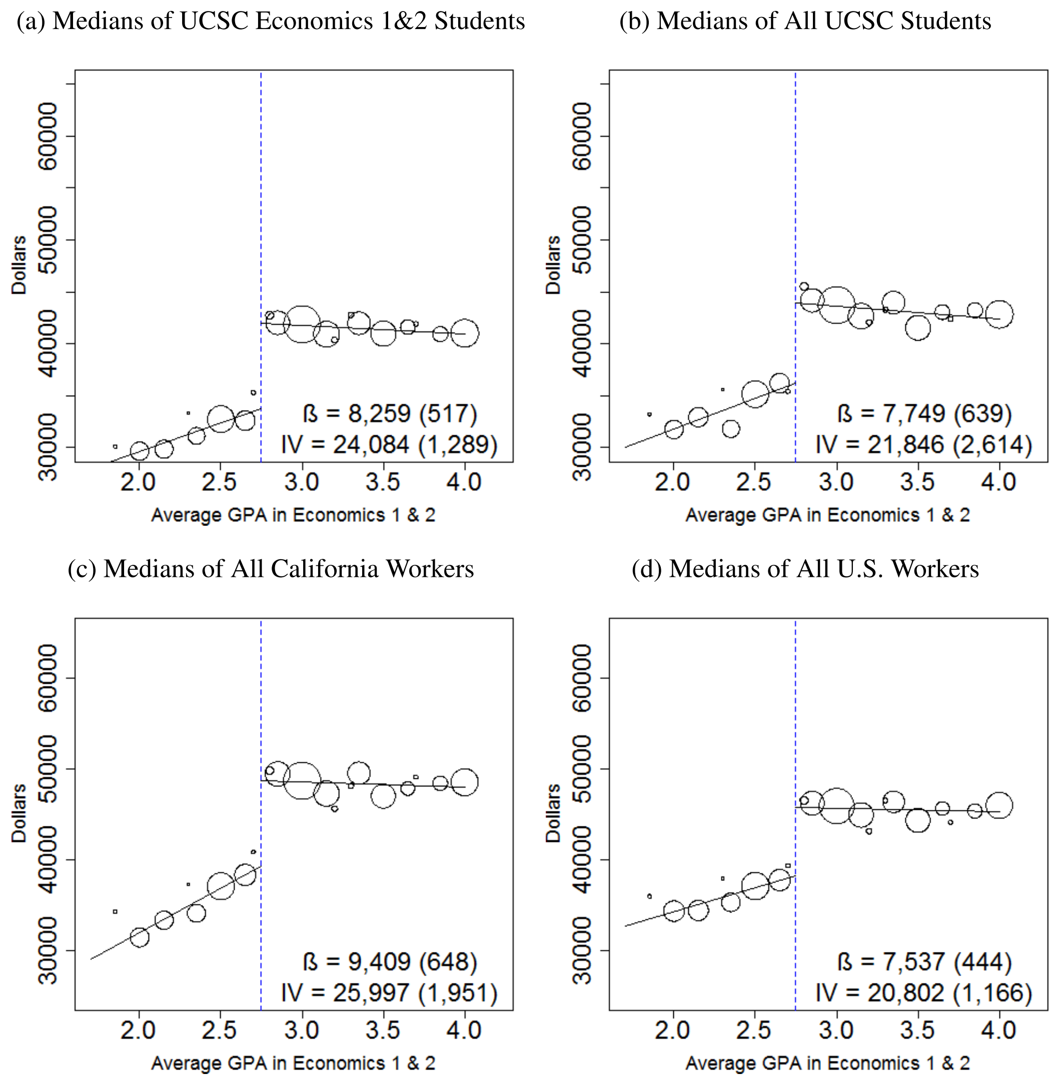


(c) All California Workers (d) All U.S. Workers



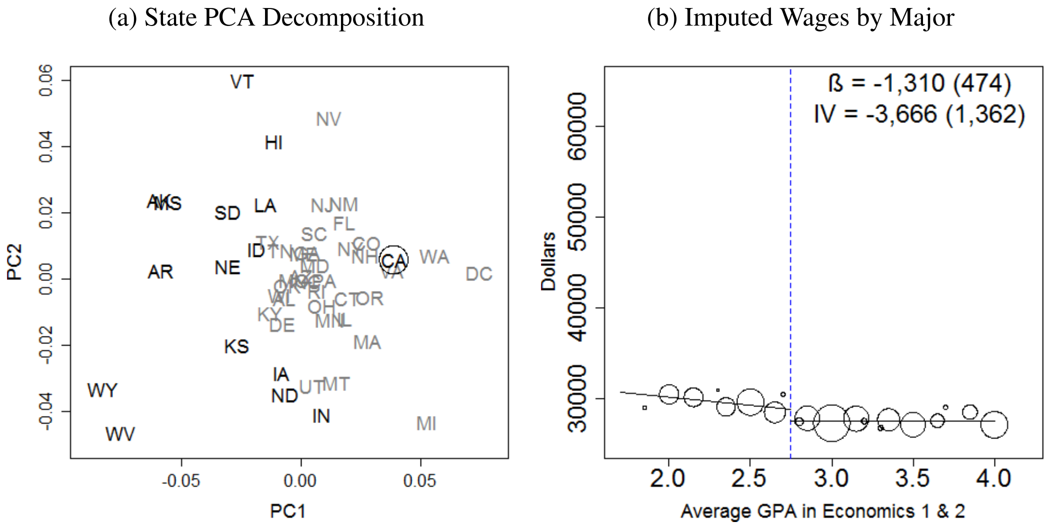
Note: This figure shows that when wages are imputed for each student by the median wages of similar-age workers with their same major choice – among the 2008-2012 main UCSC sample, among all 2008-2012 UCSC students, among all similar-age California-residing ACS respondents, or among all similar-age ACS respodents – the imputed wages increase across the GPA threshold by \$6,700 to \$8,200, similar (or slightly smaller) magnitude to the true change in students' early-career wages. Each circle represents the imputed wages associated with students' chosen majors (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Wage-by-major medians are calculated using 2017-2018 wages for four groups: (a) 2008-2012 freshman-admit UCSC students who completed Economics 1 and 2; (b) all 2008-2012 freshman-admit UCSC students; (c) 23-to-27-year-olds in the 2017 ACS and 24-to-28-year-olds in the 2018 ACS employed in California; and (d) all employed ACS respondents of those same ages. Students with double majors are characterized by that double-major (irrespective of order) in both data sets, with independent wage medians for each major pair. ACS medians are weighted by sample weights. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. $EGPA$s below 1.8 are omitted, leaving 2,839 students. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Sources: The UC-CHP Student Database, the CA Employment Development Department, and the American Community Survey (Ruggles et al., 2020).

Figure D.17: Median Early-Career 2009-2010 Wages of the Majors Chosen by the 2008-2012 UCSC Cohorts



Note: This figure shows that imputing wages using wage-by-major medians (as in Figure D.16), but using 2009-2010 CPI-adjusted medians from the 2000-2004 cohorts, provides highly similar estimates, implying average wage differences across majors are relatively persistent over time. Each circle represents the imputed wages associated with students' chosen majors (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Wage-by-major medians are calculated using 2009-2010 wages for four groups: (a) 2000-2004 freshman-admit UCSC students who completed Economics 1 and 2; (b) all 2000-2004 freshman-admit UCSC students; (c) 23-to-27-year-olds in the 2009 ACS and 24-to-28-year-olds in the 2010 ACS employed in California; and (d) all employed ACS respondents of those same ages. Students with double majors are characterized by that double-major (irrespective of order) in both data sets, with independent wage medians for each major pair. ACS medians are weighted by sample weights. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. $EGPA$s below 1.8 are omitted, leaving 2,839 students. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Sources: The UC-CHP Student Database, the CA Employment Development Department, and the American Community Survey (Ruggles et al., 2020).

Figure D.18: Median Wages in the 2008-2012 UCSC Cohorts' Chosen Majors, Imputed from States Dissimilar to California



Note: This figure shows that when wages are imputed for each UCSC student by the median wages of similar-age workers with their same major choice **from states with highly-dissimilar college-educated labor markets from California's**, economics majors do not have higher average wages than college graduates with the second-choice majors chosen by policy compliers below UCSC's GPA threshold. Panel (a) shows the 15 states most-dissimilar from California in distance on the first two principal components of college-educated employment shares by industry, measured using the full ACS industry codes of the 23-to-27-year-old respondents in the 2017 ACS and 24-to-28-year-olds in the 2018 ACS. In Panel (b), each circle represents the imputed wages associated with students' chosen majors (y axis) among 2008-2012 UCSC students who earned a given $EGPA$ in Economics 1 and 2 (x axis). The size of each circle corresponds to the proportion of students who earned that $EGPA$. Wage-by-major medians are calculated using the 2017-2018 wages of all employed ACS respondents of those same ages who reside in one of the fifteen states most-dissimilar from California. Students with double majors are characterized by that double-major (irrespective of order) in both data sets, with independent wage medians for each major pair. ACS medians are weighted by sample weights. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. $EGPA$s below 1.8 are omitted, leaving 2,839 students. Fit lines and beta estimate (at the 2.8 GPA threshold) from linear regression discontinuity specification and instrumental variable specification (with majoring in economics as the endogenous variable); standard error (clustered by $EGPA$) in parentheses. Sources: The UC-CHP Student Database and the American Community Survey (Ruggles et al., 2020).

Table D.1: Alternative RD Model Specifications for Figures 5.1 and 5.2

	Major in Economics	Predicted Wages by Demographics All	Emp. 17-18	UCUES	2017-2018 Wages	2017-2018 Log Wages	2017-2018 CA Employ.
Baseline	36.1	-15.0	998.9	-15.0	7,989	0.21	4.1
	(2.7)	(392.3)	(733.9)	(392.3)	(1,885)	(0.05)	(1.0)
Quadratic Run. Var.	31.8	-114.6	405.9	-114.6	12,584	0.29	2.8
	(5.5)	(661.4)	(839.2)	(661.4)	(2,979)	(0.07)	(1.8)
Detailed Covariates	35.2	-288.1	-159.4	-288.1	8,579	0.19	4.7
	(4.4)	(258.2)	(504.8)	(258.2)	(2,599)	(0.08)	(2.8)
Narrow Bandwidth	37.5	-346.2	-766.2	-346.2	12,336	0.31	3.9
	(4.3)	(821.1)	(951.6)	(821.1)	(3,242)	(0.07)	(2.2)
"Honest" Local Lin.	29.4	554.3	2,590.3	554.3	10,977	0.18	4.3
	(7.9)	(1,047.5)	(2,357.2)	(1,047.5)	(5,020)	(0.15)	(5.5)

Note: This table shows that the results presented in Figures 5.1 and 5.2 are highly robust to alternative regression specifications, though the conservative "honest" local linear estimation on log wages estimates a statistically-insignificant effect on log wages (because its wide bandwidth just includes $EGPA = 2.35$, which has unexpectedly high wages). Regression discontinuity specifications estimating the reduced-form effect of economics major access on major choice and labor market outcomes for 2008-2012 UCSC students who completed Economics 1 and 2. Baseline specification is the beta coefficient from a regression discontinuity OLS model linear in the running variable (Econ $EGPA$). The second specification includes quadratic terms in the running variable on either side of the threshold. The third specification includes linear running variable terms along with gender-ethnicity indicators, cohort indicators, and high school indicators. The fourth specification includes linear running variable terms but restricts the sample to within 0.5 $EGPA$ points of the threshold, resulting in 10 available $EGPA$s. The fifth specification estimates "honest" local linear RD coefficients with optimal bandwidth, triangular kernel, and an assumed constant bound on the second derivative of the conditional expectation function following Kolesar and Rothe (2018). "Major in economics" indicates declaring any of UCSC's three economics major tracks: economics, global economics, or business management economics. "Predicted Wages by Demographics" estimates each student's predicted wages by a linear regression (among 2008-2012 UCSC students outside the main sample) of 2017-2018 wages on gender-ethnicity indicators, residency status, and third-order polynomials in SAT score and mean ZIP Code income. The effects on predicted wages are included for three samples: the full sample, those who are employed in 2017-2018, and those who complete the UCUES survey in their junior or senior year (see Figure D.3). 2017-2018 wages are the mean in EDD-covered California wages in those years, omitting zeroes. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Employment is defined as earning non-zero EDD wages in either 2017 or 2018. $EGPA$s below 1.8 are omitted, leaving 2,839 students in the sample (2,446 with observed wages). All standard errors are clustered by the 20 available $EGPA$s earned by students in Economics 1 and 2.

Sources: The UC-CHP Student Database and the CA Employment Development Department

Table D.2: Alternative RD Model Specifications for Figure 4

	College GPA	Degree Attain.	Years Enr.	Grad. Deg. Enr.	Median Class Size	# Hours/Week Studying	# Econ. Courses
Baseline	-0.05 (0.02)	-0.4 (1.5)	0.00 (0.05)	-2.3 (2.2)	7.0 (2.3)	0.4 (0.8)	4.7 (0.3)
Quadratic Run. Var.	0.00 (0.03)	-3.8 (2.1)	-0.07 (0.08)	-2.8 (4.1)	6.5 (4.0)	0.8 (1.3)	4.0 (0.6)
Detailed Covariates	-0.05 (0.02)	-1.6 (1.9)	-0.06 (0.05)	-2.5 (4.8)	9.1 (2.3)	0.3 (0.8)	4.6 (0.5)
Narrow Bandwidth	-0.02 (0.03)	-2.6 (2.0)	-0.09 (0.06)	-1.4 (3.8)	7.2 (3.4)	-0.0 (1.3)	4.4 (0.4)
“Honest” Local Lin.	-0.00 (0.05)	1.3 (3.6)	0.07 (0.13)	1.3 (6.2)	12.0 (6.8)	0.5 (2.7)	2.9 (1.4)

Note: This table shows that the results presented in Figure 4 are highly robust to alternative regression specifications. Regression discontinuity specifications estimating the reduced-form effect of economics major access on educational outcomes for 2008-2012 UCSC students who completed Economics 1 and 2. Baseline specification is the beta coefficient from a regression discontinuity OLS model linear in the running variable (Econ $EGPA$). The second specification includes quadratic terms in the running variable on either side of the threshold. The third specification includes linear running variable terms along with gender-ethnicity indicators, cohort indicators, and high school indicators. The fourth specification includes linear running variable terms but restricts the sample to within 0.5 $EGPA$ points of the threshold, resulting in 10 available $EGPA$s. The fifth specification estimates “honest” local linear RD coefficients with optimal bandwidth, triangular kernel, and an assumed constant bound on the second derivative of the conditional expectation function following Kolesar and Rothe (2018). College GPA includes all courses and is weighted by units. Degree attainment measured in 2019 and includes degrees earned at other institutions (by students who transfer away from UCSC) measured in NSC. Years enrolled measures the number of academic years (of the seven following high school graduation) in which the student is observed as enrolled in NSC but has not yet earned a Bachelor’s degree. Graduate degree enrollment indicates having enrolled in a graduate degree (measured in NSC) within seven years of high school graduation. Median class size measured by course department, number, and term. Number of hours studying per week measured among 789 in-sample UCUES survey respondents in their third or fourth year (the survey is biannual). Number of economics courses measures the number of courses listed on the student’s transcript as having been taught in the Department of Economics. All standard errors are clustered by the 20 available $EGPA$s earned by students in Economics 1 and 2, with the sample restricted to $EGPA$s above 1.8.

Sources: The UC-CHP Student Database, the Student Experience in the Research University (SERU) database, and the National Student Clearinghouse

Table D.3: Alternative RD Model Specifications for Figure 5

	Intend. In Bus/Fin†	Intend. In Bus/Fin	FIRE and Account.	FIRE	Account.	Imp. UCSC Wages by Ind.
Baseline	16.1 (6.9)	10.8 (9.5)	9.1 (2.3)	6.3 (2.3)	3.4 (1.1)	3,937 (1,166)
Quadratic Run. Var.	24.7 (7.7)	12.3 (17.5)	11.4 (3.2)	10.0 (2.9)	3.2 (1.7)	6,431 (1,473)
Detailed Covariates	17.0 (6.9)	12.5 (8.6)	9.6 (3.7)	7.1 (4.0)	2.4 (1.3)	3,471 (1,604)
Narrow Bandwidth	18.4 (10.1)	8.9 (16.7)	6.8 (2.9)	4.3 (2.5)	3.6 (1.5)	7,374 (1,053)
"Honest" Local Lin.	36.9 (15.9)	-13.0 (14.6)	11.0 (5.3)	8.9 (5.2)	5.1 (3.6)	9,498 (3,387)

Note: This table shows that the results presented in Figure 5 are highly robust to alternative regression specifications, though some specifications find larger estimates on imputed wages by industry. Regression discontinuity specifications estimating the reduced-form effect of economics major access on educational outcomes for 2008-2012 UCSC students who completed Economics 1 and 2. Baseline specification is the beta coefficient from a regression discontinuity OLS model linear in the running variable (Econ $EGPA$). The second specification includes quadratic terms in the running variable on either side of the threshold. The third specification includes linear running variable terms along with gender-ethnicity indicators, cohort indicators, and high school indicators. The fourth specification includes linear running variable terms but restricts the sample to within 0.5 $EGPA$ points of the threshold, resulting in 10 available $EGPA$s. The fifth specification estimates "honest" local linear RD coefficients with optimal bandwidth, triangular kernel, and an assumed constant bound on the second derivative of the conditional expectation function following Kolesar and Rothe (2018). Intended career in business/finance indicates selecting "Business, finance-related professions" on a survey asking "Career hope to eventually have after education complete" (see Appendix A) among 834 in-sample UCUES survey respondents in their second or third year (the survey is biannual). Employment in FIRE and accounting indicates 2017 or 2018 employment in the finance, insurance, and real estate (NAICS codes 52 and 531) or accounting (541211) industries, both of which employ large shares of UCSC economics majors; see Figure D.5. Imputed wages by industry (6-digit NAICS) are calculated as the mean 2017-2018 wages of all 2008-2012 freshman-admit UCSC students. Imputed wages are CPI-adjusted to 2018 and winsorized at 2% above and below. All standard errors are clustered by the 20 available $EGPA$s earned by students in Economics 1 and 2, with the sample restricted to $EGPA$s above 1.8. † Six 2012 sophomore respondents – economics majors with 2.7 $EGPAs$ – were omitted from estimation; see Figure D.14.

Sources: The UC-CHP Student Database, the Student Experience in the Research University (SERU) database, and the CA Employment Development Department

Table D.4: Alternative RD Model Specifications for Figure 5.6

	UCSC OLS Coef.		Median Wages		
	No Cont.	Controls	UCSC	CA	U.S.
Baseline	7,178	5,579	8,065	6,945	6,728
	(547)	(1,333)	(599)	(641)	(422)
Quadratic Run. Var.	7,731	7,491	8,100	7,250	6,969
	(715)	(1,475)	(996)	(1,151)	(620)
Detailed Covariates	6,693	1,778	7,727	7,082	6,592
	(823)	(2,123)	(830)	(1,018)	(683)
Narrow Bandwidth	8,156	8,111	9,106	7,590	7,557
	(674)	(1,360)	(861)	(1,001)	(603)
"Honest" Local Lin.	8,072	6,873	8,404	7,075	6,868
	(1,894)	(2,269)	(1,753)	(1,437)	(1,252)

Note: This table shows that the reduced-form versions of the RD IV estimates presented in Figure 5.6 are highly robust to alternative regression specifications. Regression discontinuity specifications estimating the reduced-form effect of economics major access on imputed wages (by college majors) for 2008-2012 UCSC students who completed Economics 1 and 2. Baseline specification is the beta coefficient from a regression discontinuity OLS model linear in the running variable (Econ $EGPA$). The second specification includes quadratic terms in the running variable on either side of the threshold. The third specification includes linear running variable terms along with gender-ethnicity indicators, cohort indicators, and high school indicators. The fourth specification includes linear running variable terms but restricts the sample to within 0.5 $EGPA$ points of the threshold, resulting in 10 available $EGPA$s. The fifth specification estimates "honest" local linear RD coefficients with optimal bandwidth, triangular kernel, and an assumed constant bound on the second derivative of the conditional expectation function following Kolesar and Rothe (2018). The outcome variables assign each 2008-2012 UCSC student to their corresponding majors' average wage – partitioning students by their set of majors, and in the UCSC no-controls sample using leave-one-out models – and estimates the linear RD IV model on the resulting imputed wages. OLS coefficients from a linear regression of wages on major dummies with or without covariates (gender-ethnicity, cohort year, and high school), partitioning students by majors and omitting Business Management Economics. Median wages calculated by majors for UCSC sample, for the ACS sample of California residents, and for the full ACS sample. See the appendix for UCSC-ACS major mapping. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. All standard errors are clustered by the 20 available $EGPA$s earned by students in Economics 1 and 2, with the sample restricted to $EGPA$s above 1.8.

Sources: The UC-CHP Student Database, the CA Employment Development Department, and the American Community Survey (Ruggles et al., 2020).

Table D.5: Changes in 2017-18 Industry

Two-Digit NAICS Industry	IV Est. (β)	(s.e.)	Econ. Maj. Share UCSC	Econ. Maj. Share U.S.	Young Coll. Work. Share UCSC	Young Coll. Work. Share U.S.
FIRE	17.2	(5.4)	14.0	24.0	4.9	7.3
Accounting	9.3	(2.8)	10.8	3.1	1.6	1.7
Professional Services	5.7	(10.0)	32.6	18.8	20.5	12.9
Public Administration	4.2	(4.3)	4.2	5.8	5.3	5.2
Construction	4.0	(2.3)	2.0	1.5	1.3	1.9
Transportation	4.0	(2.9)	2.2	2.5	1.6	1.6
Management Firms	3.5	(1.5)	0.5	0.4	0.3	0.2
Agriculture	2.1	(2.3)	1.6	0.5	1.2	0.6
Manufacturing	1.8	(6.0)	7.6	4.5	6.5	6.6
Utilities	1.2	(1.3)	0.6	0.4	0.3	0.5
Admin. Support	0.5	(4.3)	10.9	2.6	10.2	2.6
Rental/Leasing	0.0	(1.3)	0.7	0.4	0.5	0.4
Arts and Entertainment	-0.7	(3.7)	2.4	1.6	4.3	2.8
Other Services	-1.0	(2.8)	2.0	2.7	4.8	3.3
Information	-1.3	(10.0)	9.9	3.8	7.2	3.4
Accomodation and Food	-4.1	(2.9)	5.3	3.2	8.4	4.8
Retail Trade	-5.1	(8.8)	8.2	6.8	9.9	7.9
Education	-8.1	(4.0)	6.6	10.8	19.5	18.3
Wholesale Trade	-8.5	(6.6)	5.2	2.2	3.3	1.8
Healthcare and Social Assist.	-8.6	(3.4)	4.6	3.9	15.1	15.6

Note: This table shows the two-digit-NAICS industries of 2017-2018 employment most impacted across the 2008-2012 UCSC economics GPA threshold, with workers flowing most into FIRE and out of education, healthcare and social assistance, and (noisily) wholesale trade, along with the worker shares at UCSC and across the country (for economics majors and all college graduates). Columns one and two show estimates from instrumental variable regression discontinuity specifications of indicators for 2017 or 2018 employment in each two-digit NAICS industry on economics major choice (instrumented by the 2.8 $EGPA$ threshold; standard error (clustered by $EGPA$) in parentheses. The remaining columns show the proportion of 2008-2012 UCSC students or 23-to-28-year-old 2017-2018 ACS respondents employed (in 2017-2018) in each industry, overall and among economics majors. The following NAICS codes are combined for similarity: 52/531 (FIRE), 31/32/33 (manufacturing), 44/45 (retail trade), and 48/49 (transportation). Accounting (541211, or 5412 in the ACS) is separated out from professional services. Employment industry is the reported NAICS code of an individual's highest-paying position in the year's fourth quarter.

Sources: The UC-CHP Student Database, the CA Employment Development Department, and the American Community Survey (Ruggles et al., 2020).

Table D.6: Counterfactual Major Choice and Average Wages by Major

Major	% of Grads UCSC	U.S.	Δ Among Comp. (%)	UCSC OLS Coef. No Cont.	Controls	Median Wages UCSC	CA	U.S.
Psychology	12.9	6.4	-20.4	-26,088	-24,160	33,875	30,661	30,000
			(4.3)	(1,146)	(1,253)			
Environmental Studies	6.1	0.8	-14.1	-24,602	-23,561	38,135	40,606	33,915
			(6.8)	(1,473)	(1,609)			
Tech. & Info. Mgmt.	1.2	0.2	-11.6	3,410	1,183	61,672	48,000	49,871
			(1.5)	(2,682)	(2,698)			
Sociology	6.0	1.7	-9.8	-22,014	-19,316	37,024	35,055	32,000
			(2.4)	(1,341)	(1,543)			
Film and Dig. Media	3.4	0.7	-8.0	-28,599	-25,241	30,685	30,594	28,617
			(2.7)	(1,638)	(1,845)			
Legal Studies	2.6	0.2	-7.7	-14,636	-13,140	42,500	46,828	34,749
			(1.8)	(1,897)	(2,054)			
Mathematics	2.0	1.4	-6.5	-17,446	-12,911	44,577	50,000	38,899
			(3.0)	(2,256)	(2,590)			
Latin Amer. Studies	2.0	0.7	-5.1	-28,369	-21,465	35,112	32,007	30,661
			(1.2)	(2,846)	(3,160)			
Art	3.6	1.0	-3.9	-34,687	-31,265	25,641	30,661	28,000
			(1.5)	(1,809)	(1,932)			
Anthropology	4.7	0.7	-3.6	-26,810	-26,426	32,032	26,711	25,551
			(1.8)	(1,556)	(1,854)			
...								
Economics	3.4		4.0	-8,071	-7,085	50,317		
		2.4	(8.9)	(1,623)	(1,737)		55,560	50,000
Global Economics	0.9		5.9	-5,848	-7,788	53,689	55,560	
			(1.7)	(2,947)	(3,085)			
Bus. Mgmt. Economics	7.1	0.2	90.1	-	-	61,872	54,538	48,025
			(8.2)					
Weighted Sum by UCSC Major Shares				20,039	18,073	21,287	17,436	15,385
RD IV Estimate on Imputed Wages by Majors				19,247	17,461	22,171	19,293	18,794

Note: This table presents the statistics used to generate Figure 5.6, showing that observational average differences in early-career earnings – at the university, state, or national level, and in the presence or absence of control variables – well-approximate the causal estimate of the wage return to economics for policy compliers near the GPA threshold. This table presents shares and average wages by major among 2008-2012 UCSC students (in 2017-2018) and 2017-2018 ACS respondents (age 23-28), along with estimates of the difference between the average wages of majors chosen by above-threshold policy compliers and average wages of their counterfactual majors. Columns 1 and 2 present the proportion of students who choose each major in each sample. The third column shows the change in major choice at the GPA threshold estimated using the linear RD IV specification described in the text; majors are ordered by this column, with those outside the top ten (and bottom three) omitted from the table. OLS coefficients from a linear regression of wages on major dummies with or without covariates (gender-ethnicity, SAT score, ZIP Code average AGI, cohort year, and high school), partitioning students by major (choosing higher-earning major among in-sample single majors for multi-major students) and omitting Business Management Economics. Median wages calculated by higher-earning major for UCSC sample and full ACS sample. "**Weighted Sum Using UCSC Major Shares**" shows the difference between the weighted sum of Econ wage values by the share of UCSC students in that major (using highest-earning majors) and that of non-Econ wage values. "**RD IV Estimate on Imputed Wages**" assigns each 2008-2012 UCSC student to their corresponding majors' average wage – now partitioning students by their set of majors (not their higher-earning major), and in the UCSC no-controls sample using leave-one-out averages – and estimates the linear RD IV model on the resulting imputed wages. The ACS does not have separate major categories for Economics and Global Economics; see the appendix for UCSC-ACS major mapping. Wages are CPI-adjusted to 2018 and winsorized at 2% above and below. Sources: The UC-CHP Student Database, the CA Employment Development Department, and the American Community Survey (Ruggles et al., 2020).

Table D.7: UCSC Major to ACS Major Mapping

Major	ACS Field	Prop. in Sample
American Studies	1501	0.6
Anthropology	5502	1.1
Applied Ling and Multiling	2601	0.0
Art	6000	1.7
Art History	6006	0.6
Biochemistry&Molecular Bio	3603	0.4
Bioengineering	2402	0.1
Bioinformatics	2402	0.0
Biology	3600	1.3
Business Mgmt Economics	6205	50.9
Chemistry	5003	0.4
Cognitive Science	4006	0.6
Community Studies	5403	0.4
Comp Sci Computer Game Des	2407	0.4
Computer Engineering	2407	0.4
Computer Science	2102	2.4
Critical Race&EthnicStudies	1501	0.1
Earth Sciences	5004	0.6
Ecology and Evolution	3604	0.1
Economics	5501	20.2
Electrical Engineering	2408	0.2
Environmental Studies	1301	9.5
Feminist Studies	4007	0.3
Film and Digital Media	6005	2.9
German Studies	2602	0.1
Global Economics	5501	5.5
Health Sciences	6100	0.2
History	6402	3.4
Human Biology	3699	0.1
Individual	4000	0.1
Information Systems Management	2106	1.7
Jewish Studies	1501	0.0
Language Studies	2601	0.7
Latin Amer & Latino Studies	1501	2.1
Legal Studies	3202	3.6
Linguistics	2601	0.4
Literature	3301	1.5
Marine Biology	3699	0.3
Mathematics	3700	3.5
Molec Cell Develop Biology	3603	2.1
Music	6002	0.3
Neuroscience	3611	0.2
Philosophy	4801	1.1
Physics	5007	0.2
Plant Sciences	1105	0.1
Politics	1105	5.0
Psychology	5200	8.5
Sociology	5507	4.1
Spanish Studies	2602	0.3
Technology&Info Management	2106	6.0
Theater Arts	6001	0.5
Women's Studies	4007	0.0

Note: This table shows the employed mapping between UCSC majors and ACS "Detailed Field of Degree" codes, along with the proportion of students in the 2008-2012 main UCSC sample in each major. Multiple UCSC majors may be mapped to the same ACS degree field. See https://usa.ipums.org/usa-action/variables/DEGFIELD.

Sources: The UC-CHP Student Database and the American Community Survey (Ruggles et al., 2020).