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On the Meritocratic Allocation of Higher Education

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Access to higher education is a key determinant of lifetime earnings in the United States. Since the 1960s, selective public universities have admitted students mostly on the basis of standardized test scores and other measures of academic preparation, on the theory that highly prepared students can best take advantage of universities' rigorous curricula. I employ quasi-experimental and structural research designs to investigate the efficiency and economic mobility ramifications of these "meritocratic" admissions policies. 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.

Essay 1

Affirmative Action, Mismatch, and Economic Mobility after Prop 209

Educational attainment, income, wealth, and economic mobility exhibit racial disparities in the United States. Access to selective universities is a key determinant of economic success and intergenerational mobility (Chetty et al. 2020a). As a result, many selective universities provide admissions advantages to applicants from disadvantaged racial and ethnic groups. Proponents of affirmative action argue that it offsets applicant qualification gaps that result from systemically unequal educational opportunities (Johnson 2019). Detractors argue that affirmative action limits opportunity for Asian and white applicants and may have unintended consequences for targeted students. This study examines three questions at the basis of this disagreement. First, which students are targeted by affirmative action, and to what degree does affirmative action 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 studies have shown that applicants with test scores and grades at selective universities' minimum admissions thresholds are benefited by admission.[1] 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 links 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.[2] 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 s mostly flowed to less-selective public colleges and universities. Overall, Prop 209 resulted in a net outflow of lower-income students from highly selective public universities.

How did less-selective enrollment affect URM UC applicants? I estimate the average effect of Prop 209 using a difference-in-difference design 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.[3] High school fixed effects and AI covariates absorb spurious variation and observable selection bias into UC application.[4] I also estimate effect heterogeneity by URM AI quartile and by URM ethnicity. Implementing this model, I show that Prop 209 led URM UC applicants to enroll at relatively lower-quality colleges and universities on average, measured both by traditional metrics like graduation rate and by institutional value-added.[5] 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.[6] 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.[7]

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.[8] 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.[9] 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.[10]

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

Next, I turn to mechanisms explaining URM UC applicants' deteriorated educational outcomes after Prop 209. Several prior studies have suggested that URM students' STEM course performance and persistence would improve absent affirmative action, which likely would have led to the opposite of Prop 209's effect on STEM attainment.[11] 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. 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.[12] 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.[13] Second, the 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).[14] This evidence suggests that affirmative action both promotes socioeconomic mobility among URM youths and improves higher education's allocative efficiency.

This study makes three main contributions. First, while previous studies have analyzed the intermediate effects of universities' affirmative action policies—sometimes coming to conflicting conclusions—they share common limitations. Several studies have exploited cross-state policy variation to estimate the educational impact of banning affirmative action, but out-of-state enrollment confounds identification of the policies' effects on impacted students.[15] 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.[16] 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.[17] 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.[18] 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.[19] On the other hand, while most studies of heterogeneous university return 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. 2020b; Derenoncourt and Montialoux 2021), but find that it improved Black students' educational attainment and relatively increased (mostly lower-income) Hispanic youths' wages.

Essay 2

Top Percent Policies and the Return to Postsecondary Selectivity

Since the 1960s, selective public universities in the United States have admitted students mostly using test scores and other measures of academic preparation.[20] 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" admissions 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 United States. 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 UC between 2001 and 2011. Eligibility in the Local Context (ELC) 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.[21] 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 9 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).[22] 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 essay 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 10-campus UC system 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.

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 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 10–35 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 12th 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 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 30–34 percentage points and graduate school enrollment by 22–47 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 1, 2, and up to the top 9 percent of applicants from each California high school. The simulations show that top percent policies are indeed "access-oriented": the 9 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. (2020a). These findings suggest that the first-order net effect of top percent policies is to reallocate educational resources to high-GPA (and perhaps high noncognitive 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.[23] 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.[24] This evidence suggests that broadening selective research university access to many high school graduates with low socioeconomic status, as through low-cost 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. 2020a; 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 2020a).

Second, this study provides evidence on the impact of a college admissions 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).

Essay 3

Major Choice Restrictions and Student Stratification

Undergraduate major selection has long-run labor market implications: students earn higher postgraduate wages if they earn degrees in "high-return" professional degrees (Deming and Noray 2020; Bleemer and Mehta 2020b) or degrees in their preferred field of study (Kirkeboen, Leuven, and Mogstad 2016; Daly and Le Maire 2019). 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).[25] However, major restrictions are widely implemented at selective public universities in the United States. Consider five of the highest-wage college majors at the 25 top-ranked U.S. public universities (according to *U.S. News & World Report*), shown in the chapter's Table 1. These universities enroll about 750,000 undergraduates, or half of all students at top-100 American universities (and 7 percent of *all* American undergraduates).[26] Half of these schools restrict their computer science majors—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.[27]

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 a case study that compares students' persistence by socioeconomic characteristics at a restricted and an unrestricted university.

We find that major restrictions lead to a 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' preenrollment educational opportunity and demographically stratify students across majors by average wages.

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. 2020a; Arcidiacono, Kinsler, and Ransom 2019a,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 (2020b) show that falling just below an economics department's GPA major restriction substantially decreases rejected students' early-career wages.[28]

Second, this study documents an important determinant of student major selection that has been largely omitted from the large academic literature on major choice.[29] 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.[30]

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 2015a,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.

Essay 4

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

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 nongraduates (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.

Estimating the causal effects of earning specific college majors is challenged by students' nonrandom 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).[31] 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 observational average wage differences between economics and other majors as a proxy for the effect of majoring in economics.

The specific case we analyze is the department of economics at UC 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. 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 41st 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 and accounting. 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.

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 United States.[32] 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 United States 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. 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, 2020a). 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). 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.

Notes

1. 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) provide evidence of their return to selectivity in other contexts.
2. URM includes African American (Black), Chicano and Latino (Hispanic), and Native American students.
3. 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 the essay, 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.
4. AI and ethnicity explained 40–70 percent of admissions variation at most UC campuses in the mid-1990s. Cortes (2010) uses a similar design to compare student outcomes between Texas's affirmative action and Top Ten policies.
5. 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. (2020a).
6. 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 and 34, and URM applicants' employment remained unchanged overall and in each AI quartile.
7. This finding is in line with Chetty et al.'s (2020b) argument that educational differences cannot explain the Black-white wage gap in the United States, although that study does not discuss the role of university selectivity.
8. Other potential threats—including nonreported applicant ethnicity, imperfect National Student Clearinghouse degree reporting, and some campuses' preemptive implementation of Prop 209—are discussed in the essay.
9. 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.
10. 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.
11. See Loury and Garman (1993); Holzer and Neumark (2000); Arcidiacono, Aucejo, and Hotz (2016).
12. I show that relative enrollment at high- and low-value-add California universities was unchanged by Prop 209.
13. 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.
14. 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 upward.
15. See Backes (2012); Hinrichs (2012, 2014); Blume and Long (2014); Hill (2017); Long and Bateman (2020).
16. See Alon and Tienda (2005); Howell (2010); Arcidiacono, Aucejo, and Hotz (2016); Kapor (2020).
17. 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.
18. See, e.g., 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.
19. Two recent studies of affirmative action "mismatch" also analyze UC in the 1990s (Arcidiacono et al. 2014; Arcidiacono, Aucejo, and Hotz 2016). Bleemer (2020b) discusses the limitations of that previous research in the specific context of Prop 209 and reconciles his analysis with my baseline findings. Dillon and Smith (2020) and Barrow, Sartain, and de la Torre (2020) find evidence of test- and income-based "mismatch" at U.S. undergraduate institutions and elite Chicago public high schools, respectively.
20. 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).
21. Top percent policies have been implemented in Texas, Florida, and Georgia, and have been considered in several other states.
22. As I discuss below, ELC was indeed "expanded" in 2012 to the top 9 percent of applicants from each high school, but I show 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.
23. Bertrand, Hanna, and Mullainathan (2010) estimate a positive wage return to caste-based affirmative action programs at engineering colleges in India. Subsequent to this study, Bleemer (2020a) 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.
24. Zimmerman (2014) and Smith, Goodman, and Hurwitz (2020) show positive returns to less- or nonselective 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.
25. Stange (2015), Andrews and Stange (2019), and Denning and Turley (2017) discuss major-specific price discrimination and payments, which are important—though presently less-common—supply-side contributors to major choice.

26. Wage statistics as reported by Altonji, Blom, and Meghir (2012).
27. 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 capable students.
28. Griffith (2010) shows that students with lower measured preparedness are less likely to earn STEM majors, while Arcidiacono, Aucejo, and Hotz (2016) and Bleemer (2020a) come to different conclusions about whether enrollment at more-selective universities under affirmative action decreases URM students' STEM degree attainment.
29. See Altonji, Blom, and Meghir (2012) and Altonji, Arcidiacono, and Maurel (2016) for surveys.
30. This study also documents a source of selection bias in the estimation of major-specific returns (Arcidiacono 2004).
31. This design was suggested by both Altonji, Blom, and Meghir (2012) and Altonji, Arcidiacono, and Maurel (2016).
32. The only known quasi-experimental study to previously identify heterogeneous returns by college major in the United States 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.

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