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Faculty Research Working Paper Series

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Diversifying Society's Leaders? The Causal Effects of Admission to Highly Selective Private Colleges

Raj Chetty, David J. Deming, and John N. Friedman

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ABSTRACT

Leadership positions in the U.S. are disproportionately held by graduates of a few highly selective private colleges. Could such colleges — which currently have many more students from high-income families than low-income families — increase the socioeconomic diversity of America's leaders by changing their admissions policies? We use anonymized admissions data from several private and public colleges linked to income tax records and SAT and ACT test scores to study this question. Children from families in the top 1% are more than twice as likely to attend an Ivy-Plus college (Ivy League, Stanford, MIT, Duke, and Chicago) as those from middle-class families with comparable SAT/ACT scores. Two-thirds of this gap is due to higher admissions rates for students with comparable test scores from high-income families; the remaining third is due to differences in rates of application and matriculation. In contrast, children from high-income families have no admissions advantage at flagship public colleges. The high-income admissions advantage at private colleges is driven by three factors: (1) preferences for children of alumni, (2) weight placed on non-academic credentials, which tend to be stronger for students applying from private high schools that have affluent student bodies, and (3) recruitment of athletes, who tend to come from higher-income families. Using a new research design that isolates idiosyncratic variation in admissions decisions for waitlisted applicants, we show that attending an Ivy-Plus college instead of the average highly selective public flagship institution increases students' chances of reaching the top 1% of the earnings distribution by 60%, nearly doubles their chances of attending an elite graduate school, and triples their chances of working at a prestigious firm. Ivy-Plus colleges have much smaller causal effects on average earnings, reconciling our findings with prior work that found smaller causal effects using variation in matriculation decisions conditional on admission. Adjusting for the value-added of the colleges that students attend, the three key factors that give children from high-income families an admissions advantage are uncorrelated or negatively correlated with post-college outcomes, whereas SAT/ACT scores and academic credentials are highly predictive of post-college success. We conclude that highly selective private colleges currently amplify the persistence of privilege across generations, but could diversify the socioeconomic backgrounds of America's leaders by changing their admissions practices.

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1 Introduction

Leadership positions in the United States are held disproportionately by graduates of highly selective private colleges. Less than half of one percent of Americans attend Ivy-Plus colleges (the eight Ivy League colleges, Chicago, Duke, MIT, and Stanford). Yet these twelve colleges account for more than 10% of Fortune 500 CEOs, a quarter of U.S. Senators, half of all Rhodes scholars, and three-fourths of Supreme Court justices appointed in the last half-century (Figure 1).[1] Ivy-Plus colleges also enroll a disproportionate share of students from high income families: students from families in the top 1% of the income distribution are more than twice as likely to attend an Ivy-Plus college than students with comparable SAT or ACT scores from the middle class (Figure 2).

These two facts motivate our central questions: Do highly selective private colleges amplify the persistence of privilege across generations by taking students from high-income families and helping them obtain high-status, high-paying leadership positions? Conversely, to what extent could such colleges diversify the socioeconomic backgrounds of society's leaders by changing their admissions policies?

The answers to these questions depend on two sub-questions, the first related to the inputs into colleges and the second related to their outputs. First, how much of the disproportionate representation of students from high-income families at highly selective private colleges is driven by preferential admissions practices vs. student choices about where to apply and matriculate? Second, do such colleges have a causal effect on students' post-college outcomes, or would the students they admit have done equally well if they had attended other colleges?

We study these questions using a new anonymized panel dataset that links several sources of administrative data: (1) information from parents' and students' federal income tax records; (2) college attendance information from the Department of Education; (3) data from the College Board and ACT on standardized test scores; and (4) application and admissions records from several highly selective public and private colleges covering 2.4 million students. This dataset provides longitudinal information on a rich set of pre-college characteristics (parental income, students' SAT and ACT scores, high school grades, academic and non-academic credentials) as well as post-college outcomes (earnings, employers, occupations, graduate school attendance). Within this dataset, we focus on the 12 Ivy-Plus colleges, 12 other highly selective private colleges (e.g., Northwestern University and Washington University), and 9 highly selective state flagship public institutions (e.g., University of California Berkeley and University of Michigan Ann Arbor). We focus primarily on the entering classes of 2010-15, who are just old enough to observe post-college outcomes in currently available tax records.

We divide our analysis into four parts. We begin by examining why children from high-income families are more likely to attend Ivy-Plus colleges by analyzing the pipeline to college enrollment, from application to admission to matriculation (enrollment conditional on admission). Conditional on pre-college academic

[1] Ivy-Plus colleges are distinctive in this respect: a far smaller share of individuals in these leadership positions attended other highly selective colleges (e.g., public state flagship universities or other highly-ranked private colleges) despite the fact that those institutions enroll many more students (Appendix Figure A.1).

qualifications – as measured by SAT and ACT scores – students from high-income families apply to highly selective private colleges at slightly higher rates than those from lower-income families.[2] These differences in application rates explain 20% of the income gap in attendance conditional on SAT/ACT scores.[3]

Two-thirds of the difference in enrollment rates at Ivy-Plus colleges by parental income can be explained by higher *admissions* rates for students from high-income families. Conditional on SAT/ACT scores, applicants from families in the top 1% (incomes > $611,000) are 55% more likely to be admitted to Ivy-Plus colleges than applicants from middle class families, which we define in this study as those with parental incomes between the 70th and 80th percentiles of the national income distribution ($83,000-$116,000), roughly the middle decile of the parental income distribution for applicants to highly selective colleges. Conditional on admission, children from high-income families are slightly more likely to enroll (matriculate) at an Ivy-Plus college, explaining the remaining 12% of the gap in attendance rates.

To understand why admissions rates differ so much by parental income, we analyze the admissions process in greater detail at the subset of colleges where we have linked admissions records. 24% of the admissions advantage for students from top 1% families can be explained by the recruitment of athletes, who tend to come from higher-income families. Another 46% of the admissions advantage comes from preferential admission for students whose parents attended the same college ("legacies"). This is both because legacy students are disproportionately likely to come from families in the top 1% and because the legacy advantage is particularly large among high-income families. Legacy students from families in the top 1% are 5 times as likely to be admitted as the average applicant with similar test scores, demographic characteristics, and admissions office ratings; legacy students from families below the 90th percentile are 3 times as likely to be admitted as peers with comparable credentials. The legacy advantage does not transfer across colleges. The children of alumni of a given Ivy-Plus college have no higher chance of being admitted to other Ivy-Plus colleges (conditional on their other credentials), indicating that legacy status does not simply proxy for other unobservable credentials that lead to higher admissions rates.

The remaining 30% of the admissions advantage for students from families in the top 1% is explained by the fact that they are judged to have stronger *non-academic* credentials (e.g., extracurricular activities, leadership traits, etc.) than students from lower-income families. The relationship between parental income and non-academic credentials is mediated by high schools. Comparing non-legacy applicants with the same test scores, demographics, and parental income, Ivy-Plus applicants who attend non-religious private high schools are twice as likely to be admitted as those who attend public high schools in affluent neighborhoods.

[2]Throughout this paper, we use SAT/ACT scores as a baseline measure of pre-college academic qualifications. Standardized test scores may not be pure measures of academic "merit" insofar as children from high-income families may have access to additional test preparation or other resources that allow them to obtain higher scores (e.g., Goodman, Gurantz, and J. Smith 2020). Such factors would only lead us to understate the disparities in college attendance by parental income conditional on academic merit, amplifying the arguments made below.

[3]Our findings differ from those of Hoxby and Avery (2013), who identify differences in application rates as a key factor that explains why selective private colleges have fewer low-income students, because we measure parental income at the individual level rather than using geographic imputations (see Chetty, Friedman, Saez, Turner, and Yagan 2020) and because we study a more recent time period, after private colleges and non-profits had expanded programs to recruit applicants from lower-income backgrounds. Our findings are consistent with those of Dynarski, Libassi, Michelmore, and Owen (2021), who focus on public colleges, in that we too find significant gradients in application rates by parental income at many public institutions.

Conditional on SAT/ACT scores, the academic ratings of students from private high schools with high admissions rates are no higher than those from public high schools, but their non-academic ratings are much higher. Since children from the top 1% are much more likely to attend private high schools, these differences in non-academic credentialing across high schools contribute to the income gap in admissions rates to Ivy-Plus colleges.

The results described above for private colleges differ sharply from those at highly selective public institutions. Conditional on SAT/ACT scores, admissions rates are virtually identical for students from low- and high-income families at all of the highly selective public colleges we study. However, there are large differences in application rates by parental income at public institutions, especially for out-of-state students. For example, children from the top 1% are 62% more likely to apply to selective public flagship universities than children with parents between the 70th and 80th percentiles of the national income distribution. The stark difference in admissions gradients by parental income between selective public and private institutions suggests that highly selective private colleges may have the capacity to change the composition of their student bodies by changing their admissions practices to emulate those used by highly selective public colleges.

In light of these findings, the second part of the paper estimates the causal impact of admission to an Ivy-Plus college on post-college outcomes. Would admitting more low- and middle-income students increase their chances of reaching the upper tail after college and ultimately increase socioeconomic diversity among society's leaders? We estimate the causal effect of attending an Ivy-Plus college instead of the average highly selective public flagship university using two research designs: one that we introduce in this paper and a second that replicates designs used in prior work and serves to reconcile our findings with prior results.

Our first research design exploits admissions decisions from multiple colleges to isolate variation in admissions that is plausibly orthogonal to candidates' outcomes. Following Dale and Krueger (2002), we consider a statistical model in which admissions decisions are a function of (1) students' latent abilities, defined as factors that are correlated with long-term outcomes and (2) other idiosyncratic factors that matter for admissions at a particular college but do not affect long-term outcomes (e.g., whether the student plays a musical instrument that is needed to fill a college orchestra or happens to get a high essay rating from a particular reader). We isolate the latter source of variation in two steps. We first focus on the subset of applicants who are waitlisted at a given college and are thus on the margin for admission. We then develop a test for whether applicants who are admitted vs. rejected from the waitlist are selected based on their latent abilities or idiosyncratic factors by examining whether probabilities of admission to *other* Ivy-Plus colleges vary across students who are admitted vs. rejected from the waitlist at a given Ivy-Plus college. Intuitively, if colleges with similar admissions practices make uncorrelated admissions decisions, then the residual variation in admissions conditional on being on the waitlist must be due to idiosyncratic factors uncorrelated with students' long-term potential outcomes; but if admissions decisions are correlated across different colleges, they must reflect latent abilities.

Implementing this test using data from several Ivy-Plus colleges, we find that admissions outcomes among

waitlisted applicants at any given Ivy-Plus college are indeed uncorrelated with the admissions decisions and internal ratings of other Ivy-Plus colleges. Under the identification assumption that different college admission committees' assessments of a candidate's underlying merit (i.e., the component that predicts long-term outcomes) are positively correlated with each other, comparisons of students who are admitted vs. rejected from the waitlist can therefore be used to identify the causal effect of admission for marginal applicants.

Using this design, we find that being admitted to an Ivy-Plus college increases students' chances of achieving upper-tail success on both monetary and non-monetary dimensions. Relative to those rejected from the waitlist, applicants admitted from the waitlist are significantly more likely to reach the top 1% of the income distribution, attend an elite graduate school, and work at a prestigious firm.[4] In contrast, we find a small and statistically insignificant impact of admission from the waitlist on mean earning ranks and the probability of reaching the top quartile of the income distribution; the causal impacts of Ivy-Plus colleges are concentrated entirely in reaching the upper tail of the distribution, consistent with the predominance of students from such colleges in positions of leadership that motivated this study.

To quantify the gain from attending an Ivy-Plus college relative to the average highly selective public flagship institution, we exploit heterogeneity in students' fallback options if they are not admitted to an Ivy-Plus college. To do so, we first establish that our causal effect estimates are closely aligned with what one would predict based on observational value-added (VA) models that compare students who attend different colleges, controlling for SAT/ACT scores, parent incomes, race, gender, and home state. We then show that the causal effect of admission to an Ivy-Plus college is much larger for students with weaker (lower VA) fallback options – e.g., whose colleges in their home state channel fewer students to the top 1% after college. Based on the relationship between causal effects of admission from the waitlist and the VA of students' outside options, we predict the causal effect of attending an average Ivy–Plus college relative to an average highly selective public flagship institution. We find that the marginal student who is admitted to and attends an Ivy-Plus college instead of the average highly selective public flagship is about 60% more likely to reach the top 1% of the income distribution at age 33, nearly twice as likely to attend a highly-ranked graduate school, and three times as likely to work at a prestigious firm.

These findings differ from a well-known set of studies which conclude that attending a more selective college in the U.S. has little impact on students' earnings (Dale and Krueger 2002, Dale and Krueger 2014, Mountjoy and Hickman 2021, Ge, Isaac, and A. R. Miller 2022). To investigate why our conclusions differ, we replicate the research design used in those studies by comparing earnings outcomes for students who attend different colleges, controlling for the set of colleges to which they were admitted. This design yields estimates very similar to and statistically indistinguishable from those obtained from our first research design: students who choose to attend Ivy-Plus colleges instead of state flagship colleges (conditional on being admitted to

[4] We define "prestigious" firms as those that employ a particularly large fraction of graduates from Ivy-Plus colleges despite not paying exceptionally high wages. The top of our list of prestigious firms overlaps closely with external measures of top-ranked hospitals, research institutions, and other non-profits (see Section 2.5). Our revealed-preference approach to identifying prestigious firms allows us to expand this list beyond the handful of large institutions identified in typical public rankings.

both) are significantly more likely to reach the top 1% of the income distribution, attend an elite graduate school, and work at prestigious firms. However, once again, we find very small impacts of attending an Ivy-Plus on average earnings, consistent with the findings of Dale and Krueger (2002) who only estimated impacts on average earnings, perhaps due to smaller sample sizes. The magnitudes of our causal effect estimates from variation in matriculation conditional on admission are also highly correlated with observational VA estimates.[5] In sum, our findings on mean earnings impacts are fully consistent with prior work, and both of our designs show that attending an Ivy-Plus college instead of a state flagship public college substantially increases an individual's chances of reaching the upper tail. Furthermore, within the current set of applicants, we find no significant heterogeneity in the causal effects of attending an Ivy-Plus college instead of a state flagship institution by parental income, SAT scores, and other applicant characteristics – suggesting that diversifying access to these institutions has the potential to improve outcomes across many subgroups.[6]

In the third part of the paper, we analyze whether the credentials underlying the high-income admissions advantage (legacy, athlete status, high non-academic ratings) and other factors (e.g., SAT scores, academic ratings) are associated with better post-college outcomes. We combine our estimates of colleges' causal effects with students' observed outcomes to infer students' potential outcomes if they were to attend an Ivy-Plus college among the pool of Ivy-Plus applicants. We find that recruited athletes, students with higher non-academic ratings, and legacy students have equivalent or *lower* chances of reaching the upper tail of the income distribution, attending an elite graduate school, or working at a prestigious firm than comparable Ivy-Plus applicants once we adjust for the fact that they are admitted to better colleges. By contrast, academic ratings and SAT/ACT scores are highly predictive of post-college outcomes. These findings show that application files contain significant information about students' long-term potential, but the factors that currently lead to higher admissions rates for students from high-income families are not very informative about post-college success.

Finally, we combine the estimates from our pipeline and causal effects analyses to answer our motivating question: how much could Ivy-Plus colleges diversify society's leaders by changing their admissions practices? We first consider a counterfactual admissions scenario in which colleges eliminate the three factors that drive the admissions advantage for students from high-income families – legacy preferences, the advantage given to those with higher non-academic ratings, and the differential recruitment of athletes from high-income families – and then refill the newly opened slots with students who have the same distribution of SAT scores as the current class. Under such an admissions policy, the share of students attending Ivy-Plus colleges from the bottom 95% of the parental income distribution would rise by 8.7 percentage points, adding 144 students

[5]This finding differs from that of Mountjoy and Hickman (2021), who find that estimates that condition on admissions portfolios are uncorrelated with observational VA measures when focusing on colleges in Texas. We replicate Mountjoy and Hickman's findings, but show that Texas colleges are an outlier in this respect; for all the other colleges for which we have data (e.g.,the UC system, Cal State system, other flagship state colleges, and Ivy-Plus colleges), estimates that condition on admissions portfolios are strongly correlated with observational VA measures. Most importantly for the purposes of the present study, we find that attending an Ivy-Plus college has a large impact on measures of upper-tail success conditional on the set of colleges one is admitted to, irrespective of which colleges one uses in the admission portfolio set.

[6]We find no heterogeneity relative to the fixed outside option of a state flagship; in practice, students from lower-income families tend to have weaker fallback options, and thus the reduced-form gain from being admitted to an Ivy-plus college is larger for students from lower-income families.

from families earning less than $240,000 (the 95th percentile) to a typical Ivy-Plus college. This increase of 144 students from lower-income and middle class families is similar to the reduction in the number of Black and Hispanic students that would arise from eliminating race-based affirmative action policies absent any other changes in admissions practices (as estimated by Card (2017)). Hence, eliminating the admissions practices that benefit students from high-income families would increase socioeconomic diversity by a magnitude comparable to the effect of racial preferences on racial diversity. Importantly, the increase in socioeconomic diversity would not come at the cost of reducing class quality as judged by post-college outcomes: the share of students from Ivy-Plus colleges who reach the upper tail of the income distribution would remain similar and the share who work at prestigious firms would increase because the factors leading to admissions advantages for students from high-income families are not predictors of better outcomes.[7]

We then consider an alternative "need affirmative" admissions policy in which low-income students with high academic ratings are given an admissions preference. We show that such a policy could generate increases in socioeconomic diversity comparable to those obtained from eliminating the three high-income admissions advantages with admissions "boosts" for highly qualified low-income students that are smaller than those currently given to legacy applicants. Moreover, it would increase the share of students reaching the upper tail of the income distribution and working at prestigious firms. These results demonstrate that there are a substantial number of low- and middle-income students with strong chances of success – in particular, students with high SAT/ACT scores – who apply but are not currently admitted to Ivy-Plus colleges. The availability of this pool of applicants implies that such colleges could meaningfully diversify the socioeconomic origins of society's leaders by changing their admissions practices.

Our study builds on and contributes to an extensive literature studying diversity in and the impacts of higher education. The literature on diversity has focused primarily on racial disparities and affirmative action; data on socioeconomic diversity, particularly in the very upper tail of the income distribution, have been much more scarce (e.g., Bowen and Bok 2000). Here, we study socioeconomic diversity by taking advantage of the detailed information on parental income available from tax records. While we show that our findings hold conditional on race, we do not study the role of race in admissions directly because it has been examined extensively in other recent work (e.g., Espenshade, Chung, and Walling 2004, Card 2017, Arcidiacono, Kinsler, and Ransom 2022). The literature on the impacts of higher education has likewise been hampered by an inability to follow large numbers of students over time after college, particularly at elite colleges. By linking data from multiple colleges to longitudinal data from tax records and standardized test score databases, we formulate novel research designs and study a richer set of outcomes that illuminate the role of highly selective private colleges in the United States as gateways to positions of influence in society, consistent with the findings of Zimmerman (2019) regarding elite colleges in Chile.

We caution that changes in admissions policies at highly selective private colleges cannot by themselves increase economic mobility substantially, for two reasons. First, disparities in outcomes by parental income

[7] These predictions rely on the assumption that changes in admissions policies induce no behavioral responses in the colleges to which students apply and that colleges' causal effects do not change with the composition of their student bodies.

emerge well before college application – issues that can be most effectively addressed through interventions at earlier stages (e.g., in primary schools, neighborhoods, and families). Second, because Ivy-Plus colleges account for less than 1% of total college enrollment and have little impact on average incomes, creating more social mobility through higher education requires changes at the colleges that serve most students (e.g., community colleges). Nevertheless, even holding fixed pre-college factors and their small scale, our analysis shows that a handful of highly selective colleges have the capacity to change the backgrounds of society's leaders meaningfully – an outcome of particular significance given that leaders' personal backgrounds and experiences shape decisions that influence many people's lives (e.g., Washington 2008, Einio, Feng, and Jaravel 2022, Acemoglu, He, and Maire 2022, McGuirk, Hilger, and N. Miller 2023).

The rest of this paper is organized as follows. The next section describes the data we use. Section 3 characterizes the pipeline to college enrollment by parental income. Section 4 presents evidence on the causal effects of attending Ivy-Plus colleges. Section 5 combines these results to examine how post-college outcomes vary with students' application credentials. Section 6 shows how counterfactual changes in admissions practices would affect the diversity of the student body at Ivy-Plus colleges and society's leaders. Section 7 concludes.

2 Data

We construct a de-identified dataset on parent characteristics and student outcomes by linking five sources of data: (1) federal income tax records on parents and children's incomes from 1996-2021; (2) 1098-T tax forms on college attendance from 1999-2015; (3) Pell grant records from the Department of Education's National Student Loan Data System from 1999-2013; (4) standardized test score data from the College Board from 2001-2005 and every other year from 2007-15 and ACT from 2001-15; and (5) applications and admissions records for undergraduate first-year student admissions from several Ivy-Plus colleges and highly selective public flagship universities, as well as data for all schools in the University of California (UC) and California State University (CSU) systems and all four-year publics schools in Texas from the Texas Higher Education Coordinating Board (THECB). We include data from UC-Berkeley, UCLA, and UT-Austin among others in our sample of highly selective public flagship universities with internal data. These five sets of data were linked to each other at the individual level by social security number and/or identifying information such as name, date-of-birth, and gender.[8] All analyses were then conducted using the linked individual-level dataset after it was stripped of personally identifiable information.

In this section, we describe our analysis samples, define the key variables we use, and present summary statistics.

[8] Within our target sample of U.S. citizens or permanent residents, we link more than 90% of the individuals who appear in datasets 2-5 to the income tax records.

2.1 Sample Definitions

Our target analysis sample is children who (1) are U.S. citizens or permanent residents with parents in the U.S., (2) apply to college between 2001-15 and, (3) took either the SAT or ACT. We focus on U.S. citizens and permanent residents with parents in the U.S. because those are the students for whom we observe parental income. Virtually all students who apply to highly selective colleges take either the SAT or ACT over the period we study; we restrict attention to college applicants who took one of these tests when defining our sample frame because we use those scores as baseline measures of pre-college academic preparation and are interested in the disparities that emerge thereafter.

Due to differences in data availability across colleges and datasets, we construct approximations of this target sample in different ways in different parts of our empirical analysis. We use three different samples: one to analyze the pipeline of college enrollment across colleges, another to analyze college-specific admissions policies and causal effects, and a third to examine long-term post-college outcomes. We define each in turn.

Pipeline Analysis Sample. When characterizing the pipeline to college enrollment by college (as in Section 3), we construct our analysis sample by starting from the raw income tax data (described in Appendix A of Chetty, Friedman, Saez, Turner, and Yagan (2020)) and retaining the subset of individuals who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) can be linked to parents, and (3) appear in either the SAT or ACT data in 2011, 2013, or 2015.[9] We define each child's "parent" as the person who most recently claimed the child as a dependent between child ages 12–17. If the child is claimed by a single filer, the child is defined as having a single parent; if the child is claimed by joint filers, both filers are defined as parents. Children who are not claimed as dependents on any tax return are not linked to parents and are excluded from our analysis.[10] Because our sample only includes children whose parents file taxes at least once in the United States, it excludes virtually all international students.

College-Specific Analysis Sample. When studying admissions and matriculation at specific colleges (Section 3.2), admissions decisions (Section 3.3), and the causal effects of colleges on outcomes (Section 4), we focus on the subset of Ivy-Plus and highly selective public colleges for which we have internal admissions data. In these analyses, we define the analysis sample as all permanent residents or citizens in the college-specific dataset who applied to the college over the years for which we have data who (1) can be linked to the tax data based on their SSNs or ITINs and (2) can be linked to parents in the tax data.[11] We do not need to impose further restrictions on having SAT or ACT scores for these college-specific analyses because we have data on standardized test scores from the colleges themselves.

[9] The first two restrictions are intended to isolate citizens and permanent residents of the U.S. (as we do not observe citizenship status in the tax records). The SAT and ACT data are organized by the year in which students would graduate from high school if they graduate in four years; for the vast majority of students, this corresponds to the year in which they apply to college. We focus on 2011, 2013, and 2015 because SAT data are available only in odd years.

[10] Because almost all U.S. residents file at least one tax return in a year when their child is between ages 12 and 17, we are able to link more than 98 percent of children born in the U.S. to parents (Chetty, Friedman, Saez, Turner, and Yagan (2020), Appendix Table I).

[11] We also exclude a small number of applicants who are born after 1996 or are older than 21 in the year they would enter the college to ensure that individuals have had adequate time to complete a four year degree when we measure post-college outcomes at age 25.

Long Term Outcomes Sample. Because the samples defined above focus on relatively recent cohorts, they do not allow us to examine earnings and other outcomes after age 25. We address this limitation by building prediction models for long-term outcomes based on the shorter-term proxies (e.g., initial employer) that we observe at age 25 in our main analysis samples. Because these predictions of labor market trajectories do not require information on college attendance or parental income, we estimate these models using data from the 1974-88 birth cohorts, including all individuals with valid SSNs or ITINs in the tax data (irrespective of whether they can be linked to parents).

2.2 College Attendance

We measure college attendance for children in our samples using two methods.

The first definition is constructed using commingled data from tax records (1098-T forms) and the National Student Loan Data System (Pell grant records), as described in Appendix B of Chetty, Friedman, Saez, Turner, and Yagan (2020). 1098-T forms are filed by all Title IV-accredited institutions of higher education in each calendar year for all tuition-paying students. To identify students who do not pay tuition because they receive financial aid, we supplement the 1098-T data with Pell grant records. In combination, these two data sources provide a near-comprehensive roster of domestic student college attendance at higher education institutions in the United States. For each student and each calendar year, we define a student as attending a college if she appears in either the 1098-T or Pell grant data for that school and year. We then assign each student to one college by defining the college attended as the college a student is matched to in the most years in ages 19–22. If multiple schools are matched to a student for the same number of years, we define a student's college as the first college she attends.

Our second method of measuring college attendance is to use colleges' own attendance records. In these data, we define college attendance using a college-provided indicator for whether a student matriculates to a given university as a first-year undergraduate student.

The two measures of attendance each have certain advantages. The measures based on federal administrative data are imperfect in that they sometimes do not distinguish between specific campuses of multi-campus state universities or distinguish summer school students from regular full-time undergraduates. Attendance measures based on college's own datasets are more accurate, but are available only for the subset of colleges and years for which we have admissions data. When both attendance measures are available, they are typically well aligned; moreover, the attendance measures based on federal data have a correlation of 0.99 with enrollment counts from IPEDS (Chetty et al. 2020, Appendix B). However, there are certain exceptions to this pattern; in cases with such discrepancies in enrollment counts (or where we cannot obtain campus-specific measures), we use college-specific data to measure attendance where available. Note that we do not observe degree completion in either dataset, so students are assigned to colleges based on attendance without regard to graduation.

We focus in this paper on three groups of colleges in our primary analysis: (1) Ivy-Plus colleges, which includes the Ivy League, Stanford, Duke, MIT, and Chicago (12 colleges); (2) other highly selective private

colleges (the 12 highest ranked private colleges according to the 2022-2023 U.S. News and World Report for National Universities, excluding the Ivy-Plus); and (3) 9 highly selective public flagship colleges for which we have data. These colleges are listed in Appendix Table 1. In the main text, we focus on comparisons of means across these three groups. We additionally provide data on attendance, application, and conditional attendance patterns for another 106 colleges in our Online Data Appendix.

2.3 Standardized Test Scores and Score Sending

Virtually all students who apply to highly selective colleges take standardized college entrance tests during the period we study (as they were required by most colleges). For the pipeline and long-term outcome samples, we obtain data on standardized test scores from the College Board and ACT. As in Chetty, Friedman, Saez, Turner, and Yagan (2020), we focus on a student's composite SAT score, defined as the mathematics score plus the critical reading score, and the composite ACT score (ranging from 1 to 36). We map ACT scores into equivalent SAT scores using published concordance tables (ACT, 2016), prioritizing SAT scores when both scores are available. We only use the subset of years for which we have both SAT and ACT data (2001-2005 and odd years from 2007-15). We use students' most recent test scores if they have taken a test multiple times.

The college-specific datasets also contain data on students' standardized test scores as part of their applications. In the college-specific analysis sample, we prioritize the test score reported in student applications (and analyze all available years of data); if that score is missing, we use data from the College Board and ACT files.[12]

The College Board and ACT report student test scores to colleges at students' request. Since sending one's score to a college indicates an intention to apply to that college, we use this score-send data to construct a prediction model for application to colleges for which we do not have internal applications records in our pipeline analysis (see Appendix B).

2.4 College-Specific Application and Admissions Information

For the college-specific analysis sample, we observe additional information from colleges' application and admissions records:

Application. Students are defined as applying to a college if they submit a first-year undergraduate application. If a student applies multiple times, we keep only their last application.

Admission and Matriculation. Students are defined as admitted to and matriculating to a college based on indicators in the data.

High School and GPA. The data contain information on students' high schools and their high school GPAs as reported in their applications.

[12]For colleges in the University of California system, we always prioritize the scores reported by the College Board and ACT because the SAT scores in the UC system internal data include scores on a separate writing section that was not administered systematically in all years of our sample.

Race and Ethnicity. The data contain information on students' self-reported race and ethnicity. We group all students reporting Hispanic ethnicity, and then group non-Hispanic students into racial categories corresponding to those used by the Census Bureau: American Indian and Alaskan Native, Asian, Black, Hawaiian and Pacific Islander, White, multiple, and unknown (i.e., unreported).

Gender. The data contain an indicator for self-reported gender, with the options Female, Male, and unknown (i.e., unreported).

Recruited Athletes. At all colleges except UNC-Chapel Hill, we observe indicators for recruited athletes which flag that a student was actively recruited by the college to apply and join an athletic team.

In addition, we observe a more detailed set of admissions variables in the internal data from Ivy-Plus universities.

Early vs. Regular Application. During the period we study, Ivy-Plus colleges received two rounds of applications. In the first round, candidates applied in the late fall and received a college decision by December. The second round of applicants submitted applications in the winter and received decisions in the Spring, under a "Regular Decision" timeline. The data contain an indicator for whether students apply under the Early or Regular timelines. Some students who apply in the Early round are deferred to the Regular Decision admissions cycle by the admissions office. These deferrals are also indicated in the data.

Waitlist. Some students who are not admitted in either of the two rounds are added to a waitlist by the admissions office. After the two rounds of admissions decisions and student matriculation decisions, colleges offer additional available positions in their first-year undergraduate classes to certain students on the waitlist. A student is defined as a waitlist admit if they are placed on the waitlist and then ultimately admitted. A student is defined as a waitlist reject if they are placed on the waitlist but not admitted.

Legacy and Faculty Children. The data contain indicators for legacy (one or more of the child's parents obtained an undergraduate degree from the college to which the student applied) and faculty child (one of the parents is currently a tenure-track faculty member at the college) status.

Parental Education. Some colleges' records contain a flag indicating that neither of the student's parents completed a four-year college degree, as well as information on the highest level of education obtained by a student's parents.

Ratings. Admissions officers assign each applicant numerical ratings on certain materials within their application. While the exact set and scaling of ratings differs by college, the ratings are integer-valued and typically measure academic and non-academic aspects of an application separately.

2.5 Demographics, Parent Incomes, and Post-College Outcomes

We obtain data on children's and parents' incomes from income tax returns (1040 forms) and third-party information returns (e.g., W-2 forms), which contain information on the earnings of those who do not file tax returns. We measure income in 2015 dollars, adjusting for inflation using the consumer price index (CPI-U).

Parental Income. Our primary measure of parental income is total household-level pre-tax income. In years in which a child's parent files an income tax return, we define household income as the Adjusted Gross

Income reported on the 1040 tax return. In years in which a parent does not file an income tax return, we define household income as the sum of wage earnings (reported on form W-2) and unemployment benefits (reported on form 1099-G) for all parents linked to a child. In years in which parents neither file tax returns nor receive information returns, household income is coded as zero. Chetty, Friedman, Saez, Turner, and Yagan (2020) show that these income definitions yield an income distribution similar to that in the American Community Survey (ACS) under the same income definitions.

We average parent household income over the years in which their child is between 12 and 17 years old to smooth year-to-year income fluctuations and estimate the resources available to a household when a child chooses to attend college.[13] Parents are then assigned to income ranks using this household income measure relative to all other parents with children in the same birth cohort.

Child Income. We define children's incomes in adulthood as total pre-tax *individual* income. For single filers, we define individual income as the sum of wage earnings and net self-employment income if positive as reported on 1040 tax returns. For those who file jointly, we define individual income as the sum of (1) individual wage earnings reported on W-2 forms; (2) individual net self-employment income (if positive) reported on Form SE; and (3) the difference between total wage earnings and self-employment income reported on Form 1040 and the sum of both filers' W-2 wage earnings and form SE income. For non-filers, we define income as the sum of wage earnings reported on W-2 forms; if an individual does not receive W-2 forms, we report income as zero. We then rank children based on this income measure relative to the national distribution for their birth cohort.

Graduate Schools. We use data from 1098-T forms to measure graduate school attendance at various ages. The 1098-T forms include a flag for graduate school attendance, but they do not include information on the type of graduate school attended (e.g. medicine, law, business, etc). We define "elite" graduate schools as Ivy-Plus institutions, as well as UC-Berkeley, UCLA, UCSF, University of Michigan, and University of Virginia, all of which have multiple programs that are consistently ranked in the top 10 or 15 by U.S. News and World Report.

Early-Career Employers. Because income ranks do not stabilize until graduates are in their early thirties, we use data on individuals' employers and graduate schools to predict incomes at age 33. We first assign individuals who receive 1098-T forms in the year they turn 25 to graduate institutions, based on what is indicated by the 1098-T form. For those who do not receive a graduate 1098-T in the year they turn 25, we assign them to the firm from which they receive their highest earnings at age 25, based on W-2 forms. If a student does not receive either a W-2 or a graduate 1098-T at age 25, we use graduate 1098-Ts and W-2s at age 26 (if age 26 data are observed) and then W-2s at age 24 to measure school attendance and employers. We designate individuals with no graduate 1098-T form and no firm in these years as "unable to be classified." We then use these age 25 assignments to predict income rank and the probability of having earnings in the top 1% of the birth cohort at age 33 (see Appendix C for further details).

[13] We limit the sample to parents with non-negative income because those with negative income typically have large business losses, which are a proxy for having significant wealth. The non-negative income restriction excludes less than 1% of children from our sample.

Elite and Prestigious Employers. We construct measures of "elite" and "prestigious" employers that expand upon conventional lists of high-status jobs based on the revealed preferences of Ivy-Plus graduates. In particular, we define elite firms as those that disproportionately employ students from Ivy-Plus colleges. We first calculate the share of all Ivy-Plus attendees in the 1979 to 1996 birth cohorts that work at each firm when they are age 25. We then calculate the same share for the highly selective public colleges, and compute a ratio of those shares, restricting the sample to firms that employ at least 25 college attendees from the 1979-96 birth cohorts and excluding each individual's own college from the ratio. We rank firms using this metric and define a firm as "elite" by pulling firms from the top of the list until we have accounted for 25% of Ivy-Plus attendee employment (see Appendix D for further details).

Many of the elite firms by this definition also have high predicted income ranks. To measure high-status jobs that do not necessarily lead to high earnings, we regress the ratio of the shares defined above on a quintic function of the firm's predicted top 1% probability defined above. We then calculate the residual from this regression and rerank firms accordingly. We call the top firms which account for 25% of Ivy-Plus employment "prestigious" employers. Intuitively, this outcome measures firms that disproportionately employ non-sample Ivy-Plus attendees conditional on their salaries.

To validate our approach to identifying elite and prestigious employers, we compare the firms identified by our algorithm to publicly available rankings of firms in various industries. We find a high degree of overlap. Among the 10 largest law firms that we identify as "prestigious", 5 are also ranked among the top 10 most prestigious law firms by an external (Vault.com) ranking. Similarly, 4 of the 5 largest consulting firms we identify as "prestigious" are among the top 5 most prestigious as well according to the same (Vault.com) ranking. Of the 10 largest prestigious hospitals by our definition, 5 are ranked among the 10 top hospitals that treat patients (by the institutional research ranking site Scimagoir.com). 7 of the 10 largest prestigious universities we identify are Ivy-Plus institutions.

Children's Demographics. We obtain information on children's year of birth and gender from the Death Master (also known as the Data Master-1) file produced by the Social Security Administration and housed alongside tax records. We obtain information on children's self-reported race and ethnicity from the College Board and ACT datasets. For the college-specific analysis sample, we prioritize information on applicants' race and ethnicity as reported by students in their college applications.

2.6 Summary Statistics

Table 1 presents descriptive statistics for the three analysis samples defined in Section 2.1.

Column 1 lists summary statistics for the pipeline analysis sample, which consists of 5.1 million individuals who took the SAT or ACT in 2011, 2013, or 2015. 93% of these test-takers attended a college at some point between the ages of 19 and 22. A small share of test takers attended one of the highly selective colleges we focus on in this study: 0.7% attended an Ivy-Plus college, 2.4% a flagship public college, and 0.9% another highly selective private college.

Column 2 lists summary statistics for our long-term outcomes sample, subset to those who took the SAT

or ACT for comparability to the pipeline analysis sample. This restriction limits the sample to people in the 1982-88 cohorts who took the SAT or ACT in 2001-2005 or in 2007. The proportions of students from Ivy-Plus, flagship public, and other highly selective private colleges are very similar to the pipeline sample. The samples are also similar in terms of race, gender, parent income, and other demographics.

Column 3 lists summary statistics for our college-specific Ivy-Plus sample, which consists of 489,178 applicants to the subset of Ivy-Plus colleges for which we have internal admissions records. Applicants to Ivy-Plus colleges have a mean SAT score of 1373, significantly higher than the mean SAT score of 991 for test takers overall. Of these applicants, 24.1% attended an Ivy-Plus college, 11.2% attended a flagship public college, and 11.7% attended another highly selective private college.

Column 4 lists summary statistics for the 1.88 million applicants to the flagship public colleges for which we have internal admissions records. Applicants to flagship public colleges have a mean SAT score of 1228 and are demographically similar to the Ivy-Plus sample.

The mean parental household income ranks of children who applied to flagship public and Ivy-plus colleges in our college-specific sample are 72.3 and 78.0, respectively. We therefore define individuals with parental income between the 70th and 80th percentile of the national parental income distribution as the "middle class" for the purposes of our analysis, since we focus on applicants to highly selective colleges.

Post-college outcomes differ substantially for the Ivy-Plus and state flagship applicant samples relative to the broader long-run outcomes sample. Applicants to the Ivy-Plus colleges in our sample are at the 80th percentile of the individual income distribution on average, and 15.4% of them are in the top 1% (individual income > \$220,000) among 33 year olds. State flagship applicants have a mean income rank at age 33 of 77.0, only slightly less than Ivy-Plus applicants, and 10.3% have incomes in the top 1%. 7.1% of Ivy-plus applicants and 2.9% of state flagship applicants attend an elite graduate school at age 28.

The preceding statistics include all individuals who *applied* to the three sets of colleges we analyze. In Appendix Table 2, we present analogous summary statistics for the pipeline and long-term outcomes samples separately for individuals who *attended* the same three sets of colleges. Conditioning on attendance amplifies the differences in characteristics and outcomes between the three groups. The characteristics and outcomes of individuals in the college-specific analysis samples are generally similar to those in the broader pipeline samples (which include all colleges in the relevant groups), indicating that the colleges for which we have internal admissions records are broadly representative of the colleges in their tier.

3 College Attendance Rates by Parental Income: Pipeline Analysis

Why are children from high-income families more likely to attend highly selective private colleges? In this section, we answer this question in three steps. We first characterize how attendance rates vary with parental income for children with similar pre-college qualifications, as measured by their SAT/ACT scores. We then decompose the college attendance pipeline into three parts—applications, admissions, and matriculation—

and quantify how much each contributes to income gaps in attendance rates. Finally, after establishing that differences in admissions rates are a key driver of the gaps, we characterize the specific admissions practices that lead to higher admissions rates for children from high-income families. Throughout this section, we use our pipeline analysis sample – students who are on pace to graduate high school in 2011, 2013, or 2015.

3.1 Attendance Rates Conditional on Test Scores

Students' credentials at the point of college application depend on many factors that are associated with parental income, such as the quality of K-12 schools, the neighborhoods in which they grow up, and family inputs. Since highly selective colleges typically seek to admit students with the strongest credentials, these disparities in childhood environment contribute to differences in children's chances of attending highly selective colleges. While such differences in childhood environment are important drivers of upward mobility in their own right, they are not directly shaped by institutions of higher education or higher education policies. In contrast, differences in application, admissions, or matriculation rates between students with equivalent academic credentials could be addressed directly by changes in the higher education system. To understand the degree to which colleges can change the socioeconomic diversity of their student bodies by changing policies within their control, we therefore begin by separating disparities that emerge prior to college application from those that emerge during the college application and admissions process.

To do so, we follow the prior literature and use standardized (SAT and ACT) test scores as a proxy for academic credentials at the point of college application. Although test scores are not perfect measures of either past academic achievement or students' future potential, they provide a consistent baseline measure of pre-college qualifications that is available for nearly all students during the period we study, when standardized tests were required by most selective colleges.[14] Consistent with the existence of disparities by parental income prior to college application, SAT/ACT scores differ sharply by parental income, with children from high-income families having much greater chances of scoring at the top of the distribution than those from lower-income families (Appendix Table 3).

Even holding fixed test scores, there are still large differences in students' chances of attending Ivy-Plus colleges by parental income. Figure 2a illustrates this point by plotting Ivy-Plus attendance rates for students scoring at the 99th percentile on standardized tests (an SAT score of exactly 1510 or an ACT score of 34). Among these high-scoring students, more than 30% who come from families in the top 1% (> \$677k) attend Ivy-Plus colleges. In contrast, just 10% of students scoring at the 99th percentile from families in the "middle class" of the applicant pool (between the 70th to 80th percentile of the national income distribution) attend Ivy-Plus colleges.

One can construct a series analogous to that in Figure 2a for every SAT and ACT score level and every Ivy-Plus college separately. To obtain a single summary measure of how attendance rates vary with parental

[14] Although we start with test scores as a baseline measure of pre-college qualifications, the conclusions we draw below do not rely on the assumption that test scores fully capture student potential. We examine how other measures of student credentials vary with parental income in Section 3.3 and then test whether those credentials and test scores predict students' post-college outcomes in Section 6.

income controlling for SAT scores, we average these score-specific series together, putting greater weight on scores that are represented more frequently in the current distribution of Ivy-Plus attendees. More precisely, we take a weighted average of attendance rates by test score in each parental income bin, weighting by the distribution of test scores of students who attend each Ivy-Plus college. We then combine these measures for each of the 12 Ivy-Plus colleges into a single overall mean by taking an enrollment-weighted mean across the 12 colleges and dividing by the overall mean of the result series to obtain measures of relative Ivy-Plus attendance rates by parental income controlling for test scores.[15]

The series in green circles in Figure 2b plots the resulting test-score-reweighted average Ivy-Plus college attendance rates using our pipeline analysis sample. Consistent with the pattern in Figure 2a, students from the top 1% are 2.3 times more likely to attend an Ivy-Plus college than students from the middle class (p70-80) with comparable test scores, averaging across test score levels.[16] Students from the bottom 40% of the income distribution have slightly higher Ivy-Plus attendance rates than students from the middle class with the same test scores. The result is a "missing middle" pattern where attendance rates are lowest conditional on SAT/ACT scores for middle-class students. Note that these differences in attendance rates by parental income do not arise from differences in attendance rates by race and ethnicity: we find very similar results when reweighting to hold both the distribution of test scores and race and ethnicity constant across parent income bins (Figure A.2a).

For comparison, Figure 2b also plots test-score-controlled attendance rates, constructed using the same reweighting approach, for the 12 other highly selective private colleges in our sample and the nine state flagship colleges (listed in Appendix Table 1). Other highly selective private colleges exhibit a similar pattern to Ivy-Plus colleges, with much higher attendance rates for students from high-income families and the lowest attendance rates for families from the middle class.

In contrast, attendance at highly selective public colleges exhibits a different shape: attendance is roughly constant up to the 80th percentile conditional on SAT scores, then rises by a factor of 1.4 from the 80th to 95th percentiles, and is roughly constant thereafter – not exhibiting the very sharp spike at the top observed at private colleges. The gradient in attendance by parental income at public colleges is driven primarily by differences in the attendance rates of out-of-state students, whose attendance rates vary with parental income in a manner that is similar to that at Ivy-Plus colleges (Figure A.3).

We find qualitatively similar patterns at each of the colleges within our three groups, although the magnitudes of the gradients differ across colleges (Figure A.4). With the exception of MIT – which exhibits relatively constant attendance rates by parental income - attendance rates at every Ivy-Plus college are

[15] This reweighting method is analogous to that implemented in Section 5.2 of Chetty, Friedman, Saez, Turner, and Yagan (2020), who document differences in attendance rates conditional on SAT scores. Here, we provide a more detailed college-by-college analysis, which we then use as a starting point to analyze the sources of disparities in attendance.

[16] This statistic can be approximated from the distributions reported in Appendix Table 3: 15.7% of students at Ivy-Plus colleges come from the top 1%, whereas only 7.3% of students with SAT scores comparable to those of current Ivy-Plus students come from the top 1% – a two-fold over-representation. On the other hand, middle class (p70-80) families account for 8.2% of Ivy-Plus students but 9.8% of SAT scores comparable to those of current Ivy-Plus students – a 30% under-representation. These numbers do not match those reported in Figure 2 exactly because they are based on coarse (100 SAT point) test score bins rather than exact test score values.

significantly higher conditional on test scores for students from families in the top 1%. The same is true among highly selective private colleges, with the exception of Cal Tech and Carnegie Mellon (Figure A.4b). Public colleges all exhibit shallower gradients, with the exception of the University of Michigan, Ann Arbor, where attendance rates rise sharply for high-income students (Figure A.4c), primarily driven by out-of-state enrollment (Figure A.4e). Given the similarity of the patterns across colleges within each group, we focus on analyzing the sources of differences across the groups of colleges (especially the Ivy-Plus vs. highly selective state flagship colleges) rather than within-group variation across colleges.

Quantifying the Number of Extra High-Income Students. We quantify the impact of these differences in attendance rates on the socioeconomic composition of the student body by considering a counterfactual scenario in which students from the top 1% attend Ivy-Plus colleges at the same rates as students from the 70th-80th percentiles with the same test scores. For each college c, we define

$$\text{Counterfactual Attendance Rate}_c = \sum_a N_{Top\,1\%,a} \times \text{Attendance Rate}_{P70-80,ac}\,,$$

where $N_{Top\,1\%,a}$ denotes the number of test takers with a score of a from the top 1% and Attendance Rate$_{P70-80,ac}$ denotes the fraction of students who attend college c among students with score a from the 70th-80th percentiles. We then scale the resulting counterfactual attendance rate to a class of 1650 students, which is approximately the average number of entering first-year students at Ivy-Plus colleges in Fall 2022.

Under this counterfactual of "income neutral" attendance conditional on SAT/ACT scores, there would be 104 students from the top 1% in the average Ivy-Plus class – 157 fewer than the 261 students from the top 1% we observe in our data for the average Ivy-Plus college in our pipeline analysis sample (Table 2). Put differently, there are 157 "extra" students from the top 1% (9.3% of total enrollment) than one would expect purely based on their SAT and ACT scores. Insofar as the test scores of students from high-income families may be biased upward relative to their latent potential because of test preparation or taking the test more times (as in Goodman, Gurantz, and J. Smith 2020), this gap understates the true number of "extra" students from high-income families relative to their academic qualifications.

The rest of this section seeks to understand what part of the college application and admissions process accounts for the additional 157 students from top 1% families. Note that in this section, we simply seek to understand what drives the presence of the additional 157 students, taking no normative stance on whether their presence is warranted or not. If students from high-income families have other characteristics that make them stronger applicants, their higher attendance rates conditional on SAT scores may be fully merited. After identifying the reasons that high-income students attend Ivy-Plus colleges at higher rates, we use data on post-college outcomes to assess whether the "extra" students from high-income families have better outcomes in Section 6.

3.2 Applications, Admissions, and Matriculation Rates

We now decompose the differences in attendance rates by parental income conditional on test scores into the effects of application, admissions, and matriculation. How much do each of these margins contribute to

the additional 157 students from the top 1%? This diagnostic analysis helps identify at what point in the process socioeconomic gaps in attendance emerge and what types of policies might address them.

Applications. We measure application rates in our full pipeline analysis sample using data from testing companies on the colleges to which students sent their test scores. Sending a test score to a college is an informative but noisy proxy for application, as some students may send test scores but not ultimately apply and vice versa. We adjust for the noise in score sending by building a prediction model using the subset of colleges in our college-specific sample for which we observe true application data from colleges' internal records (see Appendix B).[17]

Figure 3a shows predicted application rates by parental income group (normalized by the overall mean), controlling for test scores by reweighting students in each income bin to match the distribution of attendees' test scores as above. Children from high-income families apply at 37% higher rates to Ivy-Plus colleges than those from middle class families with comparable test scores, while those at the lowest income levels apply to Ivy-Plus colleges at 19% higher rates than those in the middle class. While there are some colleges where application rates vary more sharply with parent income – such as Dartmouth and Duke – at every Ivy-Plus college, the difference in application rates by parental income is considerably smaller than the difference in attendance rates (Figure A.6a).

Application rates vary more sharply with parental income at other colleges. Selective private colleges receive 85% more applications from students in the top 1% as students with comparable test scores in the middle class, with even larger differences at certain colleges such as Georgetown and Vanderbilt (Figure A.6b). Highly selective flagship public colleges receive 62% more applications from students in the top 1% than students in the middle class, a gradient that is again driven primarily by out-of-state applicants (A.5).

Since Ivy-Plus application rates do not exhibit the same spike at the top of the income distribution that attendance rates do, the gradient in attendance rates must be driven by differences in attendance rates among those who apply to Ivy-Plus colleges. Figure 3b confirms that this is indeed the case, with attendance conditional on application rising sharply in the upper tail of the income distribution at Ivy-Plus colleges and other highly selective private colleges, but virtually flat or even slightly downward sloping at flagship public colleges. This pattern again holds systematically across colleges within each of the three groups (Appendix Figure A.7).

The difference in conditional attendance rates must arise either from differences in admissions or matriculation rates. Unfortunately, one cannot distinguish between admissions and matriculation in our full sample of colleges; we therefore turn to our college-specific subsample, where we have data from admissions offices on admissions and matriculation decisions, for the rest of our pipeline analysis. These colleges are representative of Ivy-Plus colleges in their attendance patterns: the (equal-weighted) average attendance rate conditional on application is 1.69 times higher for students from the top 1% than for those from the

[17] We validate these predictions using a hold-out approach, verifying that the gradients they generate for application rates by parental income closely match the gradients of actual applications by parental income in colleges held out when estimating the prediction model.

middle class; the average of the ratio for the colleges we have internal data from is approximately 1.7.[18]

Admissions. In what follows, we first focus on students who are not recruited athletes, as the admissions process is very different for athletes; we return to athletes below. Figure 4a shows admissions rates by parental income of applicants to the selected Ivy-Plus and public flagship colleges in our college-specific sample. As above, we reweight within each income bin to match the standardized test score distribution at each college and divide the resulting rates by each college's overall mean admission rate.

Admissions rates are substantially higher for students from the highest-income families at Ivy-Plus colleges. Students with parental incomes in the top 0.1% are 2.5 times more likely to be admitted than students from the middle class (p70-80) with comparable SAT scores. Students with parental incomes in the 99-99.9 percentile are 43% more likely to be admitted than students from the middle class. In contrast, admission rates at the five highly selective public universities in our college-specific sample are essentially constant across the income distribution. The differences in admissions rates by parental income are again unrelated to differences across racial and ethnic groups (Figure A.2b).

Matriculation. Figure 4b plots matriculation rates of admitted students at selected Ivy-Plus colleges and highly selective flagships by parental income, again controlling for test scores by reweighting as above. Students with parental incomes in the top 1% are 1.13 times more likely to attend Ivy-Plus schools once admitted than students from the middle class. Most of this gradient arises from differences between matriculation rates from those students admitted in early vs. regular admissions rounds; high-income students are more likely to apply and be admitted in the early admissions round, where matriculation rates are higher (Figure A.8). Selective public flagships display a similarly flat pattern across the income distribution, with high-income students exhibiting slightly lower matriculation rates

The finding that matriculation rates do not vary significantly by parental income suggests that financial barriers are not the key driver of differences in attendance rates at Ivy-Plus colleges by parental income at present. However, Ivy-Plus colleges typically offer generous financial aid packages, so financial aid (and matriculation) may be a more important margin that determines college attendance more generally. To examine this possibility, we study the effects of large changes in financial aid as part of the American Recovery and Reinvestment Act (ARRA) of 2009. These changes resulted in a substantial increase in aid for tuition, especially for students from families earning less than $80,000 (see Chetty, Friedman, Saez, Turner, and Yagan 2017, Appendix Figure 9b). Families earning below $40,000 experienced a large change in federal student aid from the refundable portion of the AOTC and the higher full Pell grant, while those in between $40,000 and $80,000 experienced large increases in eligibility for Pell grants.

To assess the effects of the increase in aid on college attendance, Figure 5 and Table 3 compare college attendance rates for students from treated groups to those less affected from higher-income ($100,000-$120,000) families. Panel A shows that the share of individuals who attend highly selective college remained nearly unchanged across all three groups around the ARRA financial aid expansions. This finding is robust

[18]Furthermore, each of the colleges from which we have data exhibits similar patterns individually to the average results we report below, further supporting the view that the findings from this sample apply across Ivy-Plus colleges.

to controlling for year, state, and parent income bin fixed effects and when restricting the sample to students with relatively high ACT/SAT scores (Table 3).

The rest of Figure 5 repeats this analysis expanding the set of colleges under consideration to all selective colleges (Panel B) and all four-year colleges (Panel C). In all cases, the attendance trends for the lower- and middle-income students who benefitted most from the 2009 expansion are nearly identical to those for higher-income students, suggesting that the expansions of federal financial aid for students from middle-income families had little impact on where they went to college. These findings support the view that financial barriers are not the key reason that children from lower-income families are less likely to attend highly selective private colleges than children from high-income families. We caution, however, that further research is needed to determine whether other types of financial aid expansions might be more effective in increasing socioeconomic diversity. For instance, it is possible that the ARRA increases in federal financial aid simply crowded-out other sources of financial aid, and thus did not ultimately increase the net financial aid available to students.

Recruited Athletes. Recruited athletes are invited to apply to Ivy-Plus colleges if they are likely to be accepted and to attend, making it difficult to draw sharp distinctions between application, admissions, and matriculation as above. We therefore examine this group of applicants separately in Figure 6a, which plots the fraction of students admitted to Ivy-Plus colleges who are recruited athletes by parent income level. The share of recruited athletes rises from just 5% for students from the bottom 60% of the parental income distribution to more than 13% for students from the top 1%. Since students from high-income families are already admitted at higher rates than others (as documented above), the disproportionate share of athletes from high-income families among admitted students contributes to the presence of "extra" top 1% students. In contrast, at highly-selective public schools, there is no difference in the share of recruited athletes across the income distribution (Figure 6b).

Quantification. The preceding analysis suggests that differences in admissions—rather than application or matriculation—drive most of gap in attendance rates by parental income at private institutions. We quantify the relative importance of these sources in explaining the "extra" 157 students from the top 1% who attend Ivy-Plus colleges in Table 2. Beginning with students who are not recruited athletes, we first calculate how many students from the top 1% would attend Ivy-Plus colleges if their admissions rates conditional on SAT scores were the same as those for middle class students:

$$\begin{aligned}\text{Equal Admit CF}_c = \sum_a N_{Top\,1\%,a} \times \text{Application Rate}_{Top\,1\%,ac} \\ \times \text{Admission Rate}_{P70-80,ac} \times \text{Matriculation Rate}_{Top\,1\%,ac}\end{aligned} \tag{1}$$

We then further equalize matriculation rates, and then application rates, at which point (mechanically) the attendance rate for top 1% students is equal to that of middle class students with the same test scores. Finally, we calculate the number of "extra" top 1% students coming from athlete recruitment by adjusting the fraction of athletes from the top 1% to match that from the middle class in the new counterfactual student

body. This effectively assumes that colleges recruit athletes across the income distribution in proportion to the number of students with SAT scores comparable to those currently enrolled at Ivy-Plus colleges.

Using this approach, we find that 103 out of the 157 extra top 1% students can be accounted for by college admissions practices, with 78 coming from higher admissions rates for non-athletes and 25 coming from the disproportionate representation of high-income students among recruited athletes. If colleges were to then further eliminate the differences in matriculation rates by income – e.g., by addressing differences that arise between early and regular application rounds – the number of students from the top 1% would fall by a further 19. Finally, equating application rates across the income distribution would reduce the number of students from the top 1% by an additional 35, fully closing the gap relative to the middle class. Hence, two-thirds of the extra students from the top 1% come from admissions practices.[19]

3.3 Determinants of Admissions Rates at Ivy-Plus Colleges

Why are applicants from high-income families admitted to highly selective private colleges at higher rates, conditional on having the same test scores? In this section, we identify the mechanisms underlying this admissions advantage – an analysis of interest both in its own right and as a step toward determining where these advantages are merited from an outcome-based perspective. We exclude recruited athletes throughout this subsection, since their path to admission is distinct from other students as discussed above.

Legacy Preferences. It is well established that legacy students – students who had one or more parent attend the college to which they apply – receive special consideration in college admissions (Espenshade, Chung, and Walling 2004, Bowen, Kurzweil, and Tobin 2006, M. Hurwitz 2011, Arcidiacono, Kinsler, and Ransom 2022). However, prior studies have not measured the extent to which legacy preferences contribute to higher admissions (and ultimately attendance) rates for students from high-income families.

The effect of legacy preferences on differences in admissions rates by parent income depends on two factors: (1) the extent to which these students come from high-income families and (2) the extent of the admissions preference for legacy students. Figure 7a characterizes the first factor, plotting the share of legacy applicants by parental income group. Overall, legacy applicants constitute 3.2% of the applicant pool. This fraction rises monotonically with parental income, rising to more than 10 percent for applicants from the top 1%.

The series in green dots in Figure 7b characterizes the second factor by plotting admission rates for legacy students, reweighted to match the test score distribution of Ivy-Plus attendees as above, divided by the mean (test-score-reweighted) admission rate for all applicants. Admissions rates are considerably higher for legacy applicants relative to an average applicant with comparable test scores, especially at higher income levels: legacy applicants from the top 1% have more than a 5-fold advantage in admissions, as compared with a

[19] Because (1) is multiplicative, the results of this decomposition analysis depend upon the order in which one changes each of the three margins. In Appendix Table 4, we present a decomposition that averages across the different orders that the three margins (admissions, application, matriculation) could be changed. Averaging across orderings, admissions still account for 57% (90 students) of the overall gap in attendance. Note that these decompositions should be viewed as accounting exercises rather than predictions about how the parental income distribution of students would change if admissions policies were changed; we consider the impacts of policy changes in Section 6.

3-fold advantage at lower income levels.[20]

The higher admissions rates for legacy students in Figure 7b could be driven by a preference for children of alumni themselves or because legacy students have stronger academic or non-academic credentials relative to other students with the same test score. We use two approaches to distinguish between these hypotheses and isolate the effect of legacy status itself.

We first use the rich set of variables in our admissions data to account for observable differences in legacy applicants' credentials. To do so, we first predict admissions separately for non-legacy and legacy students using OLS regressions with quintics for SAT/ACT scores and high school GPA, fixed effects for all combinations of students' academic and non-academic ratings, entering class, race, first-generation status, gender, parental education, parental income group, and high school fixed effects. We then assign legacy applicants a counterfactual non-legacy admissions rate by predicting their admissions rates using admissions model coefficients estimated on the non-legacy sample (retaining the high school fixed effects for the schools legacies actually attended).

The dashed line in Figure 7b plots the resulting counterfactual admissions rates for legacy students, taking into account their different credentials but ignoring their legacy status. Even absent legacy preferences, children of alumni would be admitted at slightly higher rates than non-legacy students because of their favorable observable characteristics (stronger academic credentials, etc.). However, these counterfactual admissions rates are only slightly higher than for non-legacy students, implying that most of the roughly 4-fold difference on average between the observed admissions rates of legacy and non-legacy students is due to the effect of legacy preferences themselves.[21]

The preceding approach relies on a "selection on observables" assumption – namely that controlling for observable factors in admissions files is adequate to account for the different attributes of legacy applicants. Of course, one may be concerned that there are other characteristics of legacy applicants not recorded in the data (such as the nature of their recommendation letters or activities) that explain their higher admissions rate. To address this concern and evaluate our predictions based on observables, we turn to a second approach: comparing admissions rates for legacy applicants at the college their parents attended to their admissions rates at *other* Ivy-Plus colleges. The logic underlying this test is that if legacy applicants have stronger unobserved credentials, they should have higher admissions rates at all Ivy-Plus colleges, not just the particular college their parents attended.

To implement this test, we focus on the subset of individuals who applied to at least two Ivy-Plus colleges in our college-specific sample and compare admissions rates controlling for the same vector of variables as those used in the admissions model described above.[22] Figure 7c shows that legacy students are accepted at

[20] These differences in admissions rates lead to similar differences in attendance rates between legacy and non-legacy students, taking differences in matriculation rates across students into account (Figure A.10).

[21] The same model implies similarly large preferences for recruited athletes. Absent athletic preferences, the estimates imply that only 11.1% of recruited athletes would be admitted given their application credentials; for applicants with comparable characteristics to non-athletic admitted students, admissions rates are 4 times higher, at 44.5%.

[22] We exclude students who applied early to one of the colleges from this analysis since the decision to apply to another college is endogenous to the admissions decision at the college to which the student applied early. We control parametrically for test scores instead of reweighting here to maximize precision in the smaller sample of legacy applicants who apply to two or

four times the rate of non-legacy applicants with comparable test scores at the college their parents attended, but are only slightly more likely to be admitted than other applicants at other colleges.[23] Furthermore, the predicted counterfactual admissions rate for legacy students at other colleges is very similar to the actual admissions rate for those students, providing an out-of-sample validation of our predictions based on observable characteristics.

Using our counterfactual predictions, we estimate that legacy preferences (holding fixed all other credentials) lead to 47 additional students from the top 1% (Table 2).[24] Legacy preferences thus explain 47 of the 78 extra students from the top 1% who are not recruited athletes.

Application Credentials. To understand the source of the remaining 31 extra students from the top 1%, we examine how application credentials vary with parental income. To separate the effects of other factors from legacy preferences, we exclude legacy applicants (and children of faculty).

At most highly selective private colleges, applicants receive several integer-valued numerical ratings on the strength of various aspects of their application, including both academic and non-academic credentials.[25] We begin by analyzing how academic credentials vary with parental income. One plausible explanation for the higher admissions rates of high-income students is that they have stronger overall academic credentials conditional on their SAT/ACT scores. For example, students from high-income families may have higher grade point averages, taken a more difficult curriculum in high school, achieved higher scores on Advanced Placement exams, or had other significant academic achievements, such as winning a science fair or math competition.

To test this hypothesis, Figure 8a plots the fraction of students who obtain a high academic rating – defined as having ratings in the top 40% of the applicant pool – by parental income, again reweighting observations so that the distribution of test scores in each parental income bin matches that distribution for attending students. The share of applicants who obtain high academic ratings is essentially constant across the parental income distribution, and is in fact slightly *lower* for students from the top 1% of the income distribution than for those from the upper-middle class. Admissions committees evidently do not rate the academic credentials of children from high-income families as being any higher than those from lower-income families with comparable test scores, suggesting that differences in academic credentials do not explain the

more colleges; using parametric controls instead of non-parametric reweighting yields very similar estimates in the full sample analyses above.

[23] One concern with this test is that colleges may choose not to admit applicants whose parents attended higher-ranked colleges, since such a student is less likely to attend if admitted. Figure A.9a addresses this concern by testing whether legacies at a lower-ranked reference school (based on a revealed-preference ranking) are admitted to higher-ranked colleges. The average admissions rates are lower in this more selective sample of other colleges, but the gap between admissions rates for students who are legacies and non-legacies at the lower-ranked reference school remains similar.

[24] We also replicate the preceding analysis on students who are children of faculty at the institution to which they apply. Although these students are admitted at even higher rates than legacy students with comparable credentials (as shown by Arcidiacono, Kinsler, and Ransom (2022)), the admissions advantage for faculty children results in less than half an extra student on average from the top 1% because children of faculty account for only 0.1% of applicants Ivy-Plus colleges (Table 1). Because of these very small sample sizes, we are unable to publish additional analyses for this subgroup.

[25] The granularity of these ratings varies across colleges – some colleges use a 3 point scale (high, medium, low) while others use finer gradations – as do the specific measures. For simplicity, we focus in what follows on one Ivy-Plus college where we have the most granular information on ratings. We show in Appendix Figure A.11 that when we coarsen the ratings at this college to match the data available elsewhere, we obtain similar qualitative results at all colleges.

higher rates of admission for high-income applicants.[26]

An alternative explanation for the admissions advantage of high-income students is that they have stronger *non-academic* credentials, such as participation in extracurricular activities or leadership traits (Park, Kim, Wong, Zheng, Breen, Lo, Baker, Rosinger, Nguyen, and Poon 2023). Figure 8b replicates Figure 8a, showing the fraction of students with high non-academic ratings by parental income, controlling for test scores. Unlike with academic ratings, students from the top 1% of the income distribution have significantly higher non-academic ratings than those from low and middle income families; students from the top 0.1% are 1.5 times as likely to have strong non-academic ratings as compared with students from the bottom 99 percent. The gap in non-academic ratings by parental income is especially large among students with high test scores, i.e., among students who have sufficiently strong academic qualifications to be plausible candidates for admission (Figure A.14).

High School Effects. Students from the top 1% are also more likely to obtain higher ratings on the strength of their teacher recommendation and guidance counselor letters (Figure 8c) – two factors that contribute to non-academic ratings – suggesting that high schools may play a key role in explaining why students from high-income families have higher non-academic ratings. To investigate the role of high schools more directly, we estimate high school effects on admissions and examine their association with academic and non-academic ratings. To do so, we regress an indicator for Ivy-Plus admission on high school fixed effects, controlling a quintic in test scores, and indicators for race, gender, and parental income group, excluding the student herself to avoid mechanical biases. The resulting high school fixed effects can be interpreted as the difference in Ivy-Plus admissions rates across high schools for students with comparable test scores and demographics.[27]

High school admissions fixed effects vary significantly across types of high schools. We divide high schools into four categories: non-religious private, religious private, and "advantaged" (typically affluent) vs. "disadvantaged" (typically lower-income) public high schools.[28] Advantaged public high schools – the types of schools most Ivy-Plus applicants from the middle class or upper middle class attend – have the lowest fixed effects; disadvantaged public high schools and religious schools are in the middle; and private high schools have the most positive fixed effects. The differences are substantial in magnitude: students attending non-religious private high schools are twice as likely to be admitted to an Ivy-Plus college as those who attend advantaged public schools with comparable test scores and demographics (Figure 9a). Since

[26]This result may be surprising insofar as students from high-income families tend to attend better-resourced (e.g., private) high schools that offer more advanced coursework and opportunities to participate in activities such as science fairs, etc. However, there is a countervailing force when conditioning on test scores, which is that the students from lower-income families who score at say the 99th percentile on a standardized test may be particularly strong academically relative to higher-income peers who reach the same score. One way to interpret Figure 8a is that these forces roughly cancel each other out on average.

[27]To obtain estimates with adequate precision, we restrict attention to schools that have at least 40 non-legacy, non-athlete Ivy-Plus applicants in our sample. Note that these high school effects cannot be interpreted as the causal effects of high school attendance on college admissions because we do not attempt to fully control for selection of students across schools.

[28]We break public high schools into two groups based on their percentile on high school challenge indicators that capture educational opportunities or disadvantages in the high school environment, variables that feed into the CollegeBoard Landscape tool (Mabel, M. D. Hurwitz, J. Howell, and Perfetto 2022, Bastedo, Bell, J. S. Howell, Hsu, M. Hurwitz, Perfetto, and Welch 2022). We classify high schools that fall in the top 20% of this index of advantage as "advantaged." 75% of applicants to our Ivy-Plus colleges come from advantaged high schools with this definition.

students from the top 1% are more likely to attend private high schools, they attend schools that have much more positive admissions fixed effects than middle-class students (9b).

Tying these high-school-level differences back to admissions office ratings, we find that the higher admissions rates at the schools attended by children from high-income families arise entirely from differences in *non-academic* rather than academic factors. Figure 9c plots the share of students with high non-academic and academic ratings by ventiles of estimated high school fixed effect, reweighting on test score.[29] About 61% of children receive high academic ratings, irrespective of whether they attend a high school with a small or large admissions fixed effect. In contrast, the share of students receiving high non-academic ratings rises from 15% to nearly 40% going from the schools with the lowest to highest admissions fixed effects, partly because schools with higher admissions fixed effects generate more positive teacher recommendation and guidance counselor letters (Appendix Figure A.15). Consistent with the results in Figure 9a, students at private high schools have much higher non-academic ratings (but no higher academic ratings) than peers with comparable test scores and demographics at other schools (Appendix Figure A.13). In short, the admissions advantage for students from the private high schools typically attended by higher-income families appears to arise not from having a stronger academic program (e.g., more advanced classes), but rather non-academic factors outside the classroom.

These differences in non-academic credentials turn out fully account for the remaining high-income advantage in Ivy-Plus admissions. We quantify the contribution of non-academic ratings by returning to the parametric admissions model used to quantify the legacy effect above. We use the model estimated on non-legacy students to calculate how the number of admitted students from the top 1% would change if they received the same distribution of ratings as students from the middle class (p70-80) with the same standardized test scores. This further lowers the number of admitted students from the top 1% of the income distribution by 31 (Table 2), accounting for the remaining "extra" top 1% non-athlete students due to admissions.[30]

To summarize the findings of this section: two-thirds of the higher Ivy-plus attendance rates of students from the top 1% relative to the middle class is explained by an admissions advantage that arises from three factors: athletic recruitment, legacy preferences, and higher non-academic ratings.

[29] To adjust for noise in the fixed effect estimates, we shrink the estimates by multiplying each fixed effect by the ratio of signal variance to the high-school-specific total variance. We estimate the signal variance by splitting the sample of applicants into even and odd application years, estimating high school fixed effects separately for each sample, and then calculating the signal variance as the covariance between these separately-estimated fixed effects. We estimate the total variance of each high school's fixed effect as the sum of signal variance and that school's noise variance, the squared standard error of its fixed effect when estimated on the full sample of students.

[30] The fact that legacy preferences and higher non-academic ratings fully account for the non-athlete high-income admissions advantage is not the result of a mechanical decomposition; it just turns out empirically that these two factors fully explain the observed difference in admissions rates. Consistent with these conclusion, reweighting on non-academic ratings or controlling for high school fixed effects nearly eliminates the top 1% admissions advantage conditional on test scores (Figure A.12b).

4 Causal Effects on Post-College Outcomes

How would increasing the representation of low- and middle-income students at Ivy-Plus colleges – perhaps by changing the admissions practices identified above – affect their post-college outcomes? In this section, we estimate the causal effect of attending an Ivy-Plus college instead of an average highly selective public flagship college on students' post-college outcomes using two research designs. The first isolates idiosyncratic variation in admissions decisions, while the second exploits variation in where students choose to matriculate conditional on their admissions portfolios. Throughout this section, we focus on the subset of Ivy-Plus colleges for which we have internal admissions records, which appear to be representative of Ivy-Plus colleges more broadly in terms of their causal effects.[31]

We begin by presenting a statistical model to specify the two research designs and their identification assumptions and then present empirical results from each of the designs in turn.

4.1 Statistical Model

4.1.1 Setup

As discussed above, enrolling in a selective college in the U.S. involves three steps: application, admission, and matriculation. Because our research designs start by conditioning on the set of colleges to which students apply, we take the application set as exogenous and begin by modeling colleges' admissions decisions.

College Admissions. Each college j assigns applicant i a rating

$$Z_{ij} = \ _{1j}X_{1i} + \ _{2j}X_{2i} + \eta_i + \epsilon_{ij}, \tag{2}$$

where X_{1i} denotes a characteristic of student i that we observe in our data (e.g., her SAT score) and X_{2i} denotes an unobservable characteristic (e.g., an admissions committee's assessment of a student's motivation) that may be correlated with the student's post-college outcome Y_i (e.g., earnings). The relative weights placed on these components, controlled by $_{1j}$ and $_{2j}$, may vary across colleges. Students' ratings also depend upon two other unobserved components that are uncorrelated with potential outcomes: a component η_i that is common across colleges (e.g., having a guidance counselor who writes an especially strong letter of support for a student with given characteristics) and a component ϵ_{ij} that is uncorrelated across colleges (e.g., idiosyncratic noise in different reviewers' assessments of the same letters, or whether the student happens to play a musical instrument needed in college j's orchestra in the year they apply). Assume that ϵ_{ij} has infinite support, a regularity condition that ensures that any candidate has some non-zero probability of admission to a college j.

Colleges admit student i if $Z_{ij} > C_j$, where C_j denotes a college-specific cutoff for admissions. Note that this structure assumes that colleges do not condition their admissions decisions for student i on his or

[31] Observational value-added models estimated (as described below) in the pipeline analysis sample imply a 5.0 pp increase in the predicted probability of reaching the top 1% from attending an Ivy-Plus college instead of the average highly selective state flagship, averaging across all 12 Ivy-Plus colleges; the corresponding difference in value-added for the subset of Ivy-plus colleges we study below relative to state flagships is approximately 5.4 pp.

her admissions outcomes at other schools. Let P_{ij} denote an indicator for whether student i is admitted to college j. Let J_i denote the set of colleges to which student i is admitted and D_{ij} denote an indicator for whether student i chooses to enroll in college j, so that $D_{ij} = 1$ for one college $j \in J_i$ and $D_{ij} = 0$ for all others.

Post-College Outcomes. Let Y_i denote the post-college earnings or other outcomes for student i. Students' outcomes are a function of the same characteristics that enter colleges' ratings (X_1 and X_2), idiosyncratic noise ϵ_i^Y, and college-specific value-added:

$$Y_i = \sum_{j \in J_i} D_{ij}\phi_j + \ {}_1X_{1i} + \ {}_2X_{2i} + \epsilon_i^Y, \tag{3}$$

where ϕ_j denotes college j's causal effect (value-added) on Y_i. This model assumes that college value-added ϕ_j is homogeneous across students; we present evidence that this is a good approximation for the set of Ivy-Plus colleges and applicants we study by showing that colleges' causal effects are similar across subgroups and different margins of admission or enrollment.

We normalize the value-added of the outside option (denoted by college O) to $\phi_O = 0$ and assume for simplicity that everyone in the sample applies and is admitted to the outside option college ($P_{iO} = 1$ for all i). Note that by definition, the error terms η_i and ϵ_{ij} in admissions ratings are orthogonal to the error term in students' long-term outcomes ϵ_i^Y ($Cov(\epsilon_{ij}, \epsilon_i^Y) = Cov(\eta_i, \epsilon_i^Y) = 0$), since unobservable factors that affect both admissions and long term outcomes are captured in X_{2i}.

Our goal is to estimate ϕ_A, the treatment effect of attending an Ivy-Plus college (denoted by college A) instead of the outside option (college O), which we define as the average highly selective public flagship college in our college-specific sample (i.e., the 9 colleges listed in Appendix Table 1).

As discussed in Dale and Krueger (2002), a naive comparison of earnings between students who attend college A vs. college O conditional on observable characteristics X_{1i}, $E[Y_i|D_{iA} = 1, X_{1i}] - E[Y_i|D_{iO} = 1, X_{1i}]$, will typically yield a biased estimate of ϕ_A because the omitted variable X_{2i} affects both the probability of admission to college A and earnings. We now discuss two research designs that yield unbiased estimates of ϕ_A by making use of additional data under different identification assumptions.

4.1.2 Research Design 1: Isolating Idiosyncratic Variation in Admissions

Our first research design makes use of additional information $\tilde{X}_{2i}$ from college A's admissions files – in particular, whether the admissions committee places the candidate on waitlist for admission – to isolate idiosyncratic variation in admissions decisions that is plausibly orthogonal to students' long-term potential outcomes. We view $\tilde{X}_{2i}$ as a potentially imperfect proxy for the (unobserved) student characteristic X_{2i}:

$$X_{2i} = \tilde{X}_{2i} + \epsilon_{2i}^X$$

Consider the difference in expected earnings between students admitted vs. rejected by college A, controlling now for both X_{1i} and $\tilde{X}_{2i}$, divided by the probability of matriculating conditional on admission:

$$r_A = (E[Y_i|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] - E[Y_i|P_{iA} = 0, X_{1i}, \tilde{X}_{2i}])/E[D_{iA}|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] \tag{4}$$

If $Var(\epsilon_{2i}^X) = 0$, then $X_{2i} = \tilde{X}_{2i}$, and it follows that this rescaled difference in conditional means is an unbiased estimate of the causal effect of attending college A instead of O (i.e., $r_A = \phi_A$) for a student who applies only to colleges O and A.[32] The key question is therefore whether the proxy $\tilde{X}_{2i}$ fully captures the variance in X_{2i}. We can test whether this is the case by exploiting the fact that we observe independent admissions decisions at other colleges under the following assumption.

Assumption 1 (**Correlated Admissions Criteria**). *Any unobserved component of students' applications associated with long-term outcomes that affects admissions at college A affects admissions at another college B with the same sign:* ${}_{2A} > 0 \Rightarrow {}_{2B} > 0$

Let the difference in an applicant's probability of admission to college B conditional on being accepted vs. rejected at college A be given by:

$$T_{B|A} = E[P_{iB} = 1|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] - E[P_{iB} = 1|P_{iA} = 0, X_{1i}, \tilde{X}_{2i}].$$

Claim. Under Assumption 1, if admissions decisions at college B are orthogonal to those at college A conditional on X_{1i} and $\tilde{X}_{2i}$, then (4) yields unbiased estimates of the causal effect of admission to A:

$$T_{B|A} = 0 \Longrightarrow r_A = \phi_A.$$

Proof. We will establish that if $Var(\epsilon_{2i}^X) > 0$, then the probability of admission to college B is correlated with whether a student is admitted to college A under Assumption 1. To simplify notation, let $\tilde{C}_j = C_j - {}_{1j}X_{1i} - {}_{2j}\tilde{X_{2i}}$ denote the threshold for admission adjusting for observable characteristics at college j, and $\tilde{X}_i = (X_{1i}, \tilde{X_{2i}})$ denote the vector of observable characteristics.

The probability of admission to college B conditional on admission to college A for a student with characteristics $\tilde{X}_i$ is:

$$\begin{aligned} E[P_{iB} &= 1|P_{iA} = d, X_{1i}, \tilde{X}_{2i}] = E[Z_{iB} > C_B|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}]. \\ &= E[\ {}_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B|P_{iA} = 1, \tilde{X}_i]. \\ &= E[\ {}_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B|\ {}_{2A}\epsilon_{2i}^X + \eta_i + \epsilon_{iA} > \tilde{C}_A, \tilde{X}_i] \end{aligned}$$

The difference in the probability of admission to college B conditional on being accepted vs. rejected at college A is therefore:

$$T_{B|A} = E[\ {}_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B|\ {}_{2A}\epsilon_{2i}^X + \eta_i + \epsilon_{iA} > \tilde{C}_A, \tilde{X}_i] - E[\ {}_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B|\ {}_{2A}\epsilon_{2i}^X + \eta_i + \epsilon_{iA} < \tilde{C}_A, \tilde{X}_i].$$

[32] When students apply to multiple colleges, the reduced-form comparison between applicants admitted vs. rejected from college A will capture the difference between ϕ_A and the average value-added of the college that students attend when rejected from A. In our empirical application, we address this complication by estimating reduced-form treatment effects heterogeneously by students' outside options in order to estimate ϕ_A , the effect of attending A relative to the average highly selective public flagship college (see Section 4.2 and Figure 13a).

Since $\epsilon_{iA} \perp \epsilon_{iB}$ and $\epsilon_{2i}^{X} \perp \epsilon_{ij}$, it follows that if $Var(\epsilon_{2i}^{X}) > 0$ and $_{2A} > 0$, then $_{2B} > 0$ implies that $T_{B|A} > 0$, i.e., the probability of admission to B differs depending upon whether a student is admitted to A.

The intuition underlying this result is straightforward: if colleges' decisions are based on the same latent factors that predict long-term outcomes, any residual variation in such latent factors (conditional on the controls $\tilde{X}_i$) will lead to correlations in admissions decisions. If no such correlation exists, then we can conclude that the variation in admissions decisions A in the marginal pool (i.e., conditional on the controls) is due to idiosyncratic factors unrelated to long-term outcomes and therefore can be used to identify the causal effects of admission to A.[33]

It is instructive to consider two cases where the key correlated admissions assumption fails. First, suppose that college B follows a rule-based admissions procedure that considers only the observable factor X_1 (e.g., SAT scores), whereas college A takes a more holistic approach that considers unobservable factors X_2 that may be correlated with long-term outcomes. For example, as discussed above, admissions decisions at certain public institutions appear to be based more on observable factors than holistic review. In this case, $_{2B} = 0$, and our test fails: even though admissions decisions at B may be uncorrelated with those at A, those who are admitted at A may have different potential outcomes from those who are rejected. To address this issue, we focus on admissions decisions at other Ivy-Plus colleges with similar admissions procedures to estimate the test statistic $T_{B|A}$.

As a second example of a potential violation of Assumption 1, consider a situation with two highly selective private colleges that both consider unobservable criteria but put weight on different factors. For example, suppose that college A puts weight on unobserved measures of mathematical ability, while college B puts weight on unobserved measures of artistic ability, and assume those two factors are uncorrelated with each other but are both correlated with long-term outcomes. In this case, our test fails once again, because students admitted to college A may have higher mathematical ability and better long-term potential outcomes, yet may not have any better chances of being admitted to college B. While we cannot directly measure all the latent factors that colleges may consider to rule out such a scenario, we find that for the subset of variables $\tilde{X_{2i}}$ that we do observe, attributes that are correlated with long-term outcomes and are positively associated with admissions at one college are also associated with admissions at the peer elite private colleges we analyze, supporting the validity of our assumption.

Although testing whether $T_{B|A} = 0$ allows us to test whether controlling for $\tilde{X_{2i}}$ is adequate to purge selection bias when estimating the treatment effects of admission to a college on long-term outcomes, estimating $T_{B|A}$ does not provide a way to correct for selection bias if it exists. Intuitively, this is because the correlation in admissions decisions across colleges is driven by both the degree of residual variance in the latent factors that affect long-term outcomes and the relative magnitudes of $_{2A}$ and $_{2B}$ (the weights placed by colleges on those factors); without a restriction on $_{2A}$ relative to $_{2B}$, there is no way to identify

[33] The converse of this claim generally does not hold, since a positive correlation in admissions decisions across colleges could be driven by the common component η_i that affects college admissions but is not correlated with long-term outcomes. Hence, finding $T_{B|A} = 0$ is sufficient but not necessary for (4) to yield an unbiased estimate of ϕ_A.

the magnitude of $Var(\epsilon_{2i}^{X})$ if it is non-zero. Our approach therefore relies on having sufficiently rich data to identify a control vector $\tilde{X_{2i}}$ that can be used to purge selection bias entirely – an approach that proves to be feasible with the detailed admissions records we now have, but was infeasible in prior work with more limited data (e.g., Dale and Krueger 2002).

4.1.3 Research Design 2: Isolating Idiosyncratic Variation in Matriculation

Our second research design isolates variation in matriculation decisions that may be orthogonal to students' potential outcomes by controlling for the set of colleges to which students are admitted. This design follows the approach originally proposed by Dale and Krueger (2002) and refined using richer data by Mountjoy and Hickman (2021). We present a brief summary of the design here, focusing in particular on how the identification assumptions and parameters it identifies differ from our first design; see Dale and Krueger and Mountjoy and Hickman for further discussion.

Consider the difference in expected earnings (controlling for X_{1i}) between students admitted to the same set of colleges, but who choose to attend different colleges:

$$r_M = E[Y_i|D_{iA} = 1, X_{1i}, J_i = \{A, O\}] - E[Y_i|D_{iO} = 1, X_{1i}, J_i = \{A, O\}] \tag{5}$$

Assume that controlling for the set of colleges to which a student is admitted eliminates any correlation between a student's potential outcomes and her choice of which college to attend.

Assumption 2 (**Admissions Portfolios Capture Selection**). *Conditional on the set of colleges to which a student is admitted and her observable characteristics X_{1i}, unobserved determinants of student i's long-term potential outcomes are orthogonal to which college she chooses to attend: $E[X_{2i}|D_{ij}, J_i, X_{1i}] = E[X_{2i}|J_i, X_{1i}]$.*

Assumption 2 (which is equivalent to Assumption 1 in Mountjoy and Hickman, 2021) immediately implies that $r_M = \phi_A$ (recalling that ϕ_O has been normalized to 0), since $E[X_{2i}|D_{iA} = 1, X_{1i}, J_i] = E[X_{2i}|D_{iO} = 1, X_{1i}, J_i]$. Intuitively, under this assumption, two students i and i' who are both admitted to colleges A and O but choose to attend different colleges have comparable potential outcomes, and thus the difference in their expected earnings reveals the relative value-added of college A.

It is instructive to compare the identification assumption underlying this design to that underlying our first research design by again considering examples where it might fail. As discussed by Dale and Krueger (2002), if students select colleges in a manner correlated with their potential outcomes – e.g., if students who expect to have better long-term outcomes forego paying the potentially higher cost of attending the more selective college A even after being admitted – then Assumption 2 would fail and (5) would yield downward-biased estimates of ϕ_A. While we cannot directly test this assumption, we can use additional observables $\tilde{X}_{2i}$ to assess its validity, examining whether $\tilde{X}_{2i}$ is balanced across students who choose different colleges within a given application set. Mountjoy and Hickman (2021) present evidence for such balance in the context of colleges in Texas and we likewise find balance in the set of colleges we analyze, supporting the validity of the identification assumption.

The two research designs are closely related in that they use data on admissions decisions at multiple colleges to address selection bias, leveraging the fact that admissions officials observe the factors that are unobservable to the econometrician. The idiosyncratic admissions design uses other admissions decisions to test for selection and isolate idiosyncratic variation in admissions, while the matriculation design uses other admissions decisions to control for selection and isolate idiosyncratic variation in matriculation decisions.

When both Assumption 1 and Assumption 2 hold, the two designs yield the same estimates in the simple model above where the returns to college attendance are not heterogeneous (i.e., ϕ_A is constant across students). However, when returns are heterogeneous across students, the two designs identify different local average treatment effects. Our first design identifies the treatment effect of attending college A for students who are narrowly admitted to vs. rejected at college A because of idiosyncratic variation in college A's assessment of their applications. The second design identifies the return to attending college A instead of O for the subset of students admitted to both colleges who choose to make different choices because of idiosyncratic variation in their preferences over colleges. While both of these treatment effects are relevant depending upon the margin of interest, our pipeline analysis above shows that the admissions margin is most central in driving the under-representation of lower-income students at highly selective private colleges. We therefore begin our empirical analysis by reporting estimates from the idiosyncratic admissions research design and then turn to the matriculation design to reconcile our findings with those from the prior literature.

4.2 Estimates Based on Idiosyncratic Variation in Admissions

Isolating Idiosyncratic Variation. We identify the treatment effect of attending an Ivy-Plus college for students who would be affected by marginal changes in admissions policies by focusing on applicants placed on admissions waitlists. On average, the Ivy-Plus colleges in our college-specific analysis sample place 10.1% of the applicant pool on the waitlist; of the waitlisted students, 3.3% are ultimately admitted. Admissions decisions from the waitlist are typically made on the basis of differences between expected and actual yield within specific categories where colleges may seek balance, such as by gender, region, in a specific activity such as the orchestra or a sports team, etc. (Clinedinst, 2019).

The logic of focusing on waitlisted applicants is similar to that underlying a regression discontinuity design: waitlisted students are close to the margin of admission and may have similar potential outcomes (i.e., comparable X_{2i}), potentially permitting identification of causal effects of admission by comparing the outcomes of those who are admitted with those who are not.[34] However, since waitlisted applicants are not admitted randomly, there is no guarantee that those who are admitted from the waitlist have the same distribution of unobservables correlated with outcomes X_{2i} as those who are not.

We therefore begin by evaluating whether the variation in admissions decisions among those on the waitlist is driven by idiosyncratic factors ϵ_{ij} that do not affect outcomes or systematic factors X_{2i} that do using the multiple-rater admissions test developed above. Formally, we treat an indicator for being placed on

[34] One cannot directly implement a regression discontinuity estimator in this setting because there is no exogenous running variable that colleges use to determine admissions from waitlists.

the waitlist as an observable control $\tilde{X_{2i}}$ and test whether the residual variation in admissions conditional on being on the waitlist at a given Ivy-Plus college A is correlated with admissions outcomes at other Ivy-Plus colleges B.

A practical complication in implementing this test is that some colleges make strategic decisions to admit students from their waitlists to manage yield. In particular, a student on the waitlist at a lower-ranked college A may not get in if she was admitted to a higher-ranked college B purely as a result of the admissions decision at college B. This violates the assumption embedded in (2) that colleges make admissions independently and can lead to $T_{B|A} < 0$ even though admission from the waitlist at any given college is orthogonal to potential outcomes. To address this issue, we implement our test using other Ivy-Plus colleges B that are ranked lower (based on revealed preference) by most students relative to the college A whose waitlist decisions we are seeking to evaluate.[35]

The first column of Figure 10 plots the probability of admission to a given Ivy-Plus college vs. an applicant's admission status at another (lower-ranked) Ivy-Plus college. Individuals who are regular admits at one college have a 55% chance of being admitted at another Ivy-Plus college, while those who are rejected have less than 10% chance of being admitted at another Ivy-Plus college, supporting the correlated admissions criteria assumption underlying our test.[36] Waitlisted candidates' chances of admission to other colleges fall between these two extremes. Among waitlisted candidates, the probability of admission to other colleges does not vary with the admissions outcome: that is, we do not reject the hypothesis that $T_{B|A} = 0$ among waitlisted students.[37]

In the second column of Figure 10, we probe the robustness of this conclusion by controlling for a set of additional observables: a quintic in SAT scores, parental income indicators (13 dummies corresponding to the income groups shown in Figure 2), race/ethnicity indicators, home state indicators, gender, recruited athlete status, and legacy status. The inclusion of these additional controls does not change the gap in admissions rates at other Ivy-plus colleges among accepted vs. rejected students on the waitlist. In contrast, the

[35]We identify college rankings based students' preferred choices when admitted to multiple colleges in our own sample, which accords with the revealed-preference rankings of colleges constructed by Avery, Glickman, Hoxby, and Metrick (2013). When implementing the test using all colleges rather than just lower-ranked ones, we find, as expected, that the probability of admission to the other college is lower for students who are admitted of the waitlist in the reference college (Appendix Figure A.16b). The causal effect estimates we report below using the full sample remain very similar when limiting to the subsample of colleges that pass at least one multiple-rater test with another college (Figure A.17). Furthermore, note that if students admitted from the waitlist at college A are less likely to be admitted to college B than those rejected from the waitlist at college A because they have lower levels of X_{2i} (rather than because of a negative correlation between ϵ_{iA} and ϵ_{iB}), our estimator would understate the causal effect of admission to college A.

[36]We further evaluate the validity of the correlated admissions criteria assumption in Appendix Figures A.18 and A.19 by correlating the residual variation in the various ratings that we observe in a given Ivy-Plus college's admissions records (controlling for SAT scores and parental income) with admissions outcomes and overall ratings at other Ivy-Plus colleges. We find positive correlations in all cases, supporting the view that the Ivy-Plus colleges in our college-specific sample place weight on the same latent factors in assessing a candidate's potential.

[37]At the upper bound of the 95% confidence interval, our estimates imply that students admitted from the waitlist are at most 2% more likely to be admitted to others college. To gauge the potential bias that could arise from a 2% higher admission rate at other colleges among the admitted pool, note that admitted students at other colleges (among all waitlisted or accepted applicants at those colleges) have a 2 pp higher predicted probability of reaching the top 1%. This 2 pp estimate is an upper bound on the degree to which potential outcomes differ between accepted and rejected applicants on average insofar as the causal effects of admission to any Ivy-plus college are weakly positive. A 2% higher admission rate would therefore translate to a 0.04 pp upward-biased estimate of the treatment effect on the predicted probability of reaching the top 1% – two orders of magnitude than our actual estimate of the treatment effect below. These calculations suggest that the multiple-rater test is adequately powered to detect meaningful degrees of bias.

inclusion of additional controls reduces the gap in admissions rates between accepted and rejected applicants not placed on the waitlist, consistent with the larger differences in credentials between those applicants. The third column of Figure 10 presents estimates with college B's ratings (rather than it's ultimate admissions decision, which includes noise from idiosyncratic factors such as available slots) as the outcome. Once again, we find no evidence of differences in other colleges' ratings between candidates admitted vs. rejected from college A. Under Assumption 1, these tests imply that the variation in admissions decisions between waitlisted candidates is due to idiosyncratic factors rather than differences in underlying student quality and is thus orthogonal to their potential outcomes.

Balance Tests. To further assess the validity of our design we test whether the characteristics of applicants admitted vs. rejected from the waitlist are balanced, pooling all Ivy-Plus colleges in our sample. We begin with an omnibus test of balance on the characteristics that matter for our post-college outcomes of interest. We regress the primary outcome we analyze – the predicted probability of reaching the top 1% based on firm at age 25 (see below for details) – on the following observable characteristics: a quintic in SAT/ACT scores, parent income dummies (the 13 bins shown in Figure 2), indicators for race and ethnicity, gender, home state, recruited athlete status, legacy status, fixed effects for academic and non-academic ratings, and college by cohort fixed effects. We then compare the predicted values from this regression among admitted vs. rejected students by regressing the predicted outcome on an indicator for admissions and fixed effects for the college to which students applied.[38] The first row of Table 4 shows the predicted probability of reaching the top 1% in the rejected group and the admitted group (adding the coefficient from the regression to the rejected group mean). We find virtually identical predicted values in the two groups, with a small, statistically insignificant ($p = 0.72$) difference of -0.05 (relative to a standard deviation within the non-admitted group of 3.6). The second row of Table 4 that we obtain a similar result when predicting another outcome we analyze – an indicator for attending an elite graduate school attendance at age 25 – essentially using a different weighting of the same observables.

To further probe balance and obtain insight into the factors that influence admissions from the waitlist, we next compare the observable characteristics of those admitted vs. rejected from the waitlist. Consistent with the results from the omnibus test, we find balance on most of these variables, including student demographics, academic credentials, and a measure of high school quality, defined as the average predicted probability of reaching the top 1% based on the high school a student attends. We find a small imbalance on standardized test scores: SAT/ACT scores are 0.07 SD lower for admitted students relative to rejected students, an imbalance that, if anything, would work against finding a positive effect of Ivy-Plus attendance since test scores are positively correlated with outcomes (see Section 5).

The final group of variables shows one key dimension on which students admitted from the waitlist do differ significantly from those who are rejected: parental income and legacy status. Children of alumni

[38] We estimate these and all subsequent regressions in this section in a dataset with one observation per student per Ivy-Plus college at which they were waitlisted, clustering standard errors by student to account for the fact that some students appear on multiple waitlists. We weight the regressions to obtain an average treatment effect that weights each Ivy-Plus college in our sample equally.

(legacies) and those from the top 1% are significantly more likely to be admitted to Ivy-Plus colleges. In short, the same factors identified above that lead to an admissions advantage for high-income applicants in general also lead to an admissions advantage from the waitlist.[39] This imbalance turns out to not matter for potential outcomes, however, because legacy status and the other factors that lead to higher admissions rates for students from high-income families are uncorrelated with post-college outcomes, a result that we develop further in Section 5 below. To further verify that the imbalance related to parental income and legacy status does not affect our conclusions, we replicate our main causal estimates excluding legacies and students with parents in the top 1% and show that we obtain very similar results to those reported below (see Appendix Table 6).

In sum, the balance tests show that admissions from the waitlist is non-random, but is driven by idiosyncratic factors orthogonal to potential outcomes, consistent with the results of our multiple-rater tests. We therefore proceed to compare the outcomes of students accepted vs. rejected from the waitlist using the estimator in (4) to identify the causal effects of admission.

Baseline Estimates. Figure 11 plots treatment effects of waitlist admissions on various outcomes. To construct these charts, we first estimate the treatment effect of attending an Ivy-Plus college using the estimator in (4). We estimate this treatment effect by regressing the outcome on an indicator for being admitted (along with fixed effects for the college to which the student applied and, in certain specifications, additional controls). We then divide the coefficient on the admission indicator by the probability of attendance conditional on admission to obtain a treatment-on-the-treated (TOT) estimate of the causal effect of attendance for those admitted from the waitlist. Finally, we plot two values: the observed mean for those rejected from the waitlist and the same mean plus the estimated treatment effect.

We begin by examining how admission to an Ivy-Plus college affect the probability of reaching the top 1% of the income distribution at age 33. The first pair of bars in Figure 11a shows that students admitted from the waitlist are 5 percentage points more likely to reach the top 1% at age 33 than those who are rejected. Although we reject the null hypothesis that admission to any Ivy-Plus college has no effect on the share of students reaching the top 1% at age 33, the confidence interval for the point estimate (shown by the whiskers in Figure 11a) is quite wide. The reason the estimate is imprecise is that we observe outcomes at age 33 for relatively few cohorts in our sample.

In principle, one could include younger cohorts to increase precision; however, individuals' incomes change sharply during their late twenties, especially for graduates of highly selective colleges, many of whom attend graduate school or undertake clerkships or internships that have relatively low wages in their twenties. Figure 12a demonstrates this pattern within our data by plotting the share of students in the top 1% of the income distribution (relative to others of the same age), separately for students accepted vs. rejected from the waitlist. In both groups, the fraction in the top 1% of the income distribution rises sharply between ages 25 and 33. At age 33, those admitted from the waitlist are approximately 5 pp more likely to be in the top

[39] Consistent with these findings, Golden (2006) presents case studies from several selective colleges that identify pressure to admit legacy and high-income students as factors in waitlist admissions; other than these factors, waitlist admissions appear to be driven primarily by idiosyncratic factors.

1%, consistent with the estimates in Figure 11a. The difference is near 0 at age 25 and grows steadily with age, indicating that those admitted to Ivy-Plus colleges are placed on a different wage trajectory relative to those who are rejected.

In light of these differences in wage trajectories, we cannot directly measure earnings impacts at age 25, where we have a much larger sample size. Instead, we predict individuals' probabilities of reaching the top 1% at age 33 using their employers or graduate schools at age 25, as described in Section 2. Figure 11b shows that at age 25, waitlist admits work at firms whose workers have a 2.5 percentage point higher probability of reaching the top 1% at age 33 than those rejected from the waitlist. As expected due to the larger sample size, this estimate is considerably more precise, with a standard error of 0.6, allowing us to reject the null hypothesis of no treatment effects with $p < 0.001$. Controlling for observable characteristics does not change these estimates significantly, consistent with the balance in characteristics between those admitted vs. rejected from the waitlists, as shown in the second set of bars in 11b. Further limiting the sample to exclude legacy applicants, athletes, and parents in the top 1% – the attributes that are unbalanced in Table 4 and are associated with admissions advantages for high-income applicants – also does not change the estimates, as shown in the third set of bars in Figure 11b.

To benchmark the magnitude of these treatment effect estimates, we compare them to one would predict based on observational estimates of college value-added (VA), constructed by regressing individuals' predicted probabilities of reaching the top 1% on fixed effects for the college they attended and a quintic in SAT scores, 13 parent income bins, indicators for race, gender, and home state. We replicate the same specification as that used to estimate the treatment effects in Figure 11b (also reported in Column 1 of Appendix Table 6) using the observational VA of the college that students attend as the outcome instead of their observed outcomes. Students admitted from an Ivy-plus waitlist attend colleges that are predicted to send an additional 2.9 percentage points of students to the top 1% based on the observational VA model (Column 5 of Appendix Table 6), similar to the point estimates obtained when examining actual outcomes.

Figure 11c replicates the analysis in Figure 11b using the predicted mean income rank (based on employer at age 25) rather than the probability of reaching the top 1%. Being admitted to an Ivy-Plus colleges has a small impact on mean income rank, both when comparing those admitted vs. rejected from the waitlist and when using observational value-added estimates of college's effects (Appendix Table 6). This is because Ivy-Plus attendance primarily shifts outcomes within the upper tail; for instance, we find much smaller, statistically insignificant impacts on predicted probability of reaching the top 25% of the income distribution (Appendix Table 6).

Heterogeneity in Outside Options. The magnitudes of the reduced-form estimates reported in Figure 11 are difficult to interpret because they depend on the outside options of students who are rejected from the waitlist. In particular, many students who are rejected from the waitlist at one Ivy-Plus college are admitted to other Ivy-Plus colleges, as shown in Figure 10. More generally, students rejected from Ivy-Plus colleges tend to attend colleges that have higher levels of value-added (based on observational estimates) relative to

the highly selective public flagship institutions that are our target outside option, as shown in Figure A.20a.

To identify causal effects of Ivy-Plus attendance relative to the fixed outside option of attending a highly selective state flagship college (ϕ_{Ivy}), we first estimate how causal effects of admission to Ivy-Plus colleges vary with students' outside options. For example, observational estimates indicate that students at Penn State – the flagship public university in Pennsylvania – have much lower chances of reaching the top 1% of the income distribution than those at UC-Berkeley (controlling for SAT scores). Consider two students who apply to an Ivy-Plus college, one of whom is from Pennsylvania and applies to Penn State as a fallback option, and other who is from California and applies to Berkeley as a fallback option. Is the causal effect of admission to an Ivy-Plus college larger for the student who has Penn State as a fallback compared to UC-Berkeley?

To operationalize this examination of heterogeneity in treatment effects by strength of outside options, we classify students at each Ivy-Plus college into groups based on their home state, parental income, and race. We estimate the quality of outside options that students in each of these groups have as the mean observational value-added (estimated using a regression of outcomes of college fixed effects, controlling for parental income, SAT scores, race, gender, and home state as above) among non-waitlisted rejected applicants in that group. We then estimate the treatment effect of being admitted vs. rejected from an Ivy-Plus college for students in groups with high vs. low value-added outside options.

This grouping instrument approach to estimating the effect of differences in outside options relies on the assumption that there is no essential heterogeneity in the causal effect of attending an Ivy-Plus college for students in different groups (as in Bleemer (2021)). For instance, if the return to attending an Ivy-Plus college were different from students from California vs. Pennsylvania, even holding fixed their fallback option, then our approach would not yield a consistent estimate of the effect of attending an Ivy-Plus college relative to an average highly selective public flagship institution. While we cannot directly test this assumption, we find little heterogeneity in treatment effects across other observable dimensions such as parental income and test scores (Appendix Table 7), suggesting that this assumption is a reasonable approximation.

Figure 13a plots the treatment effect of being admitted from the waitlist on the share of students who are predicted to reach the top 1% based on their age 25 firm vs. the strength of their outside options, controlling for fixed effects for the Ivy-Plus college to which students applied. To construct this figure, we bin the outside options measure described above into ventiles (20 equal-sized bins) and then plot the mean waitlist treatment effect vs. the predicted value of the mean outside option within each of these bins. There is a clear downward-sloping relationship between the treatment effects of admission and the strength of students' outside options. Students whose outside options are on average as good in terms of value-added as Ivy-Plus colleges (on the far right side of the figure) gain very little from admission to an Ivy-Plus college (a treatment effect near 0), as one would expect. At the other end of the quality spectrum, students whose mean outside option is comparable to the value-added of the average highly selective public flagship institution have a $\phi_{Ivy} = 4.40$ percentage point (s.e. = 1.20) higher predicted chance of reaching the top 1%.

Identifying heterogeneous treatment effects by outside options requires that students admitted vs. rejected from the waitlist have comparable potential outcomes not just on average but also within each outside options subgroup. Figure 13b evaluates this assumption by replicating Figure 13a using predicted chances of reaching the top 1% based on pre-determined characteristics (estimated as in the balance test in Table 4) as the outcome variable. There is no relationship between the predicted outcomes of admitted students and the strength of their outside option: we find placebo treatment effect estimates close to 0 across the entire distribution, consistent with the balance test in Table 4. The fact that the actual outcomes plotted in Panel A diverge so sharply from the predicted outcomes in Panel B further supports the view that the differences in outcomes observed between those admitted vs. rejected from the waitlist reflect the causal effect of attending an Ivy-Plus college.

The slope of the relationship plotted in Figure 13a is 0.86, indicating that most of the variation in observational value-added is driven by differences in causal effects of colleges rather than selection. In Appendix Table 8, we evaluate the sensitivity of this estimate to alternative specifications for students' outside options, such as defining individuals' groups purely based on geographic area (commuting zone), using a jackknife approach to exclude a student's own observation when estimating her outside options, or excluding fixed effects for the colleges to which students apply so that differences between Ivy-Plus colleges are also used to identify the coefficient. Across a range of specifications (described in the notes to the Table 8), we find estimates ranging from 0.73-0.98, and as a result the implied causal impact of attending an Ivy-Plus college instead of a state flagship is robust to the measure used to predict a student's fallback option.

An alternative approach to estimating ϕ_{Ivy} that does not require estimating heterogenous treatment effects by outside options is to multiply the reduced-form estimate obtained from the waitlist design (plotted in the figures above and reported in Appendix Table 6, Column 1) by the ratio of the difference in observational VA between the average Ivy-Plus and highly selective public flagship college and the difference in observational VA for those admitted vs. rejected from Ivy-Plus colleges (reported in Appendix Table 6, Column 5). This rescaling estimator extrapolates from the local difference in mean outcomes for waitlist admits vs. rejects to what one would observe if the outside option were the average highly selective public flagship based on differences in observational VA. This approach yields a point estimate (reported in Column 1 of Table 5) of $\phi_{Ivy} = 4.63$ (s.e. $= 1.18$), nearly identical to that obtained from estimating heterogenous treatment effects by outside options in 13a. We use this less data-intensive estimator for ϕ_{Ivy} below because it yields more precise estimates especially in smaller subgroups.

In Columns 5 and 6 of Table 5, we summarize the treatment effects by reporting the mean outcome for Ivy-Plus attendees and the implied mean outcome had those students attended average highly selective public flagship colleges instead by subtracting the waitlist design treatment effect reported in Column 1 of Table 5 from the observed Ivy-Plus means in Column 6 of Table 5. We estimate that attending an Ivy-Plus college instead of a highly selective state flagship increases a student's predicted chance of reaching the top

1% based on their age 25 employer from 10.4% to 15.0%.

Non-Monetary Outcomes. Our analysis thus far has focused solely on monetary outcomes. As Figure 1 shows, however, Ivy-Plus colleges appear to have an even greater share in other non-monetary measures of upper-tail success, such as attending elite graduate schools or achieving positions of influence in public service. While we cannot directly measure all the outcomes in Figure 1 because we can only analyze outcomes at relatively young ages and because of the rarity of outcomes such as becoming a senator, we can examine treatment effects on other non-monetary outcomes that are likely to be predictors of such long-term success.

We begin by examining treatment effects on attending graduate schools, in particular elite (highly ranked) graduate schools as defined in Section 2. Figure 12b replicates the analysis of treatment effects by age in 12a using elite graduate school attending instead of top 1% earnings as the outcome. We see a mirror image pattern, with larger treatment effects of approximately 3-4 pp between the ages of 25-28 – the peak ages of graduate school attendance – and then smaller treatment effects in the late 20s and early 30s, precisely when earnings impacts begin to appear (presumably as students have completed graduate and now earn high incomes). Figure 14a shows that the estimated treatment effects on elite graduate school attendance (at age 28) are insensitive to controls. They are also similar to what one would predict based on observational estimates of value-added (Appendix Table 6, Column 5). Using the rescaling estimator described above, we estimate that attending an Ivy-Plus college increases the chance of attending an elite graduate school at age 28 by 5.6 pp, from 6.2% to 11.7% (Table 5, Panel B). Consistent with our findings for monetary outcomes, the treatment effects are confined to measures of upper-tail success on the graduate school dimension as well: admission to an Ivy-Plus college has no significant impact on the probability of attending a non-elite graduate school (Table 5, Panel B).

Of course, attending an elite graduate school or working in a firm that channels many employees to the top 1% are only some of many potential pathways to success and influence. To capture such pathways more broadly, we use a revealed preference approach, inferring how "elite" a firm is based on whether it attracts many students from Ivy-Plus colleges. As discussed in Section 2, we define an "elite" firm as one that has a particularly high ratio of Ivy-Plus graduates relative to graduates of state flagship institutions (excluding the college that the student herself attended, to avoid bias from finite-sample noise and any mechanical effects of higher probabilities of working at certain firms, e.g. due to geographic proximity). Figure 14b shows the reduced-form impact of admission from the waitlist on the probability of working at an elite firm. Attending an Ivy-Plus college increases the chance that a student works at an elite firm by 9.4 pp. Applying the rescaling approach described above, we estimate that attending an Ivy-Plus college increases the probability of working at an elite firm by 17.6 pp, from 7.9% at highly selective state flagships to 25.5% at Ivy-Plus colleges.

Elite firms include many firms that are also high-paying (and thus overlap with those predicted to channel many employees to the top 1%) as well as firms that are attractive for non-monetary reasons. To isolate the latter component, we residualize the ratio used to define elite with respect to the predicted top 1% measure

that we use above. We then define as "prestigious" firms that rank highly on this residual, intuitively measuring a firm's attractiveness to Ivy-Plus graduates above and beyond the extent to which it generates a trajectory towards earning in the top 1%. These firms – which include many leading hospitals, research institutions, non-profits, etc. – attract individuals who presumably have many employment opportunities despite not paying exceptionally high wages and thus are prestigious in non-monetary terms in a revealed-preference sense. Figure 14c shows that being admitted to an Ivy-Plus college significantly increases students' chances of working at a prestigious firm after college, with a reduced-form treatment effect of 7.9 pp. These estimates are, as with other outcomes, insensitive to the inclusion of controls and similar to what one would predicted based on observational value-added estimates. The rescaling estimator implies that Ivy-Plus attendance increases the probability of working at a prestigious firm by 15.5 pp, from 9.0% at highly selective state flagships to 24.5% at Ivy-Plus colleges.

4.3 Estimates Based on Idiosyncratic Variation in Matriculation

We now present results from our second research design, which exploits idiosyncratic variation in matriculation conditional on admissions offers, following Dale and Krueger (2002) and Mountjoy and Hickman (2021).

We begin by regressing students' predicted probability of reaching the top 1% on indicators for the college they attended and indicator variables for the portfolio of colleges to which they were admitted, replicating the baseline specification in Mountjoy and Hickman (2021). The y axis of Figure 15a reports these fixed effect estimates when estimating this model using the Ivy-Plus colleges and state flagship institutions in our college-specific analysis sample. We plot these estimates against observational estimates of value-added, constructed as above.

The observational VA estimates and the estimates that condition on admissions portfolios are strongly positively correlated, with a slope of 0.67. The point estimate for the Ivy-Plus colleges (pooled together to preserve confidentiality) implies that attending an Ivy-Plus college instead of the average highly selective public flagship (whose VA is normalized to 0) increases a student's predicted chance of reaching the top 1% by approximately 4 pp, similar to the estimate obtained from our waitlist admissions design.

In Figure 15b, we expand the sample of colleges we consider to include several other colleges for which we have admissions data: University of California colleges, California State colleges, and public colleges in Texas (the sample used by Mountjoy and Hickman (2021)). We continue to find a strong relationship between observational estimates of college VA and estimates that condition on admissions portfolios, with the exception of colleges in Texas, where, consistent with the results of Mountjoy and Hickman, we find essentially no variation in outcomes conditional on admissions portfolios.[40] Most importantly for our purposes, Ivy-Plus colleges remain well above all the other colleges in terms of their causal effects, with an estimated impact

[40] Mountjoy and Hickman focus on in-state applicants in Texas; we replicate their results restricting to that sample in Figure A.21a. In-state applicants to public four-year schools in California, shown in Figure A.21b, again show a different pattern, showing that Texas is unique in exhibiting small differences in outcomes when comparing matriculants to different colleges. Why colleges in Texas exhibit a different pattern is an interesting question that we defer to future work; what is clear is that the same design implies that Ivy-Plus colleges have large positive causal effects on upper-tail outcomes.

relative to the average highly selective public flagship of approximately 4 pp.

When we replicate this analysis using mean income ranks instead of the probability of reaching the top 1%, we find much smaller differences between Ivy-Plus colleges and other institutions (Figure 15c). This result echoes the results from our waitlist design and reconciles our findings with those of Dale and Krueger (2002), who find little difference in mean earnings between students who chose to attend highly selective vs. less selective colleges once they condition on the set of colleges to which they were admitted. Our estimates of impacts on mean earnings fall within their confidence intervals.[41]

Finally, replicating this design using the other non-monetary outcomes considered above, we find large positive effects of attending an Ivy-Plus college on the probability of attending an elite graduate school, working at an elite firm, and working at a prestigious firm (see Figure A.22 and Table 5), with magnitudes similar to those obtained from our first research design.

4.4 Summary of Estimates

Table 5 summarizes our estimates of the treatment effects of attending an Ivy-Plus college instead of a highly selective public flagship college using our three estimators – the waitlist idiosyncratic admissions design, the matriculation design, and observational estimates of differences in outcomes conditional on SAT scores and parent income. We obtain broadly similar estimates for all of these outcomes across all three estimators. We find highly significant ($p < 0.001$) treatment effects ranging from 4.6-5.4 pp for the predicted probability of reaching the top 1% across all three estimators. We find significantly smaller impacts on the probability of being in the top quartile and on mean income ranks. Similarly, we find significant impacts on attending an elite graduate school of 5.6-8.8 pp across the three estimators, but much smaller, statistically insignificant effects on attending a non-elite graduate school. Finally, we find large, positive effects exceeding 12 pp across all the estimators on the probability of working at an elite or prestigious firm.

The consistency of the estimates between the two quasi-experimental research designs and the observational estimates – each of which relies on different assumptions – strengthens the view that attending an Ivy-Plus college has significant outcomes. It also suggests that the treatment effects of Ivy-Plus attendance (after accounting for outside options) are not highly heterogeneous across students on different margins of choice: those on the margin of being admitted, on the margin of choosing where to enroll conditional on admission, or for the average student attending different colleges.

Consistent with the similar treatment effects across different margins, we find similar treatment effects of attending an Ivy-Plus college instead of a state flagship across various observable subgroups as well. For example, Figure 15d plots treatment effects on the predicted probability of reaching the top 1% by parental income group, replicating the matriculation design above separately for different parental income bins. Attending an Ivy-Plus college instead of a state flagship university has a similar and positive effect

[41] A further methodological difference is that Dale and Krueger proxy for college quality by the average SAT scores of admitted students rather than estimating college fixed effects directly. Within the set of highly selective colleges Dale and Krueger consider, average SAT scores turn out to be weakly associated with post-college earnings (Chetty, Friedman, Saez, Turner, and Yagan 2017). Hence, it is not that these colleges have no impact on earnings, but rather that mean SAT scores are not highly predictive of value-added within a sample of highly selective colleges.

along the entire parental income distribution. We find similar evidence of similar treatment effects across the parental income distribution using the waitlist design and based on observational VA estimates (Appendix Table 7). We also find relatively similar, positive treatment effects across other subgroups: students with different test scores, academic ratings, legacies vs. non-legacies, etc. using all three estimators (Appendix Table 7).

Although these results suggest that attending an Ivy-Plus college has significant benefits for many types of students relative to a fixed outside option, it is important to recognize that students' outside options are highly heterogeneous. Most notably, students from high-income families who are rejected from Ivy-Plus colleges tend to attend higher-value added colleges – perhaps because they apply more widely or live in areas with better fallback public options (Appendix A.20b). As a result, the gain from being admitted to an Ivy-Plus college is larger for low- and middle-income applicants than it is for students from the top 1%.

Figure 16 summarizes our causal effect estimates and shows how much of the raw variation in outcome between Ivy-Plus colleges and highly selective state flagship institutions is driven by causal effects vs. selection. For each outcome, we plot three estimates: the observed mean outcome at state flagships, the implied mean outcome had Ivy-Plus attendees attended state flagships instead (estimated by rescaling the waitlist design estimates, as in Column 5 of Table 5), and the observed mean outcome at Ivy-Plus colleges.[42]

Attending an Ivy-Plus college instead of a state flagship is estimated to increase mean income ranks by only 3%. However, attending an Ivy-Plus college increases a student's chance of reaching the top 1% by 59%, attending an elite graduate program by 90%, and nearly triples their chances of working at a prestigious firm. These estimates imply that about 60% of difference in the share who reach the top 1% and attend elite graduate programs between individuals who attended Ivy-Plus colleges vs. highly selective state flagships is due to the causal effect of Ivy-Plus colleges, with the remaining 40% driven by the fact that Ivy-Plus colleges select stronger students. The causal share of the difference is even larger for our measures of working at elite and prestigious firms.

In short, although elite private colleges clearly select students with unusually high potential, much of the difference in observed outcomes is due to the treatment effects of the colleges themselves. The fact that these treatment effects are largest for non-monetary outcomes echoes the finding in Figure 1 that Ivy-Plus colleges play a particularly outsize role in educating society's leaders as defined by measures of influence beyond income.

5 Differences in Outcomes by Admissions Credentials

In this section, we combine our admissions and outcomes results to analyze whether the credentials underlying the high-income admissions advantage (legacy, athlete status, and high non-academic ratings) and other

[42] We estimate the probability of having income in the top 1% at age 33 by rescaling the difference in the observational VA estimate at age 33 (8.4 pp) by the ratio of the waitlist design to observational VA estimate for predicted incomes based on the age 25 employer, which we are able to estimate with greater precision. This approach yields a smaller estimate than using our estimated of the actual top 1% treatment effect directly since that point estimate is larger than the estimated effect on observational VA (Figure 11a). We estimate mean income ranks at age 33 using an analogous approach.

factors (e.g., SAT scores, academic ratings) are associated with better post-college outcomes. These outcome-based tests provide an input into assessing the merits of weighting these credentials in the admissions process.[43] They are also critical for understanding whether diversifying the Ivy-Plus student body would diversify society's leaders. If legacy status, higher non-academic ratings, and being a recruited athlete are associated with greater chances of success after college, colleges may face a tradeoff between admitting more students from middle class families and class quality as judged by the share of students who achieve upper-tail success. If they are not, Ivy-Plus colleges may have the capacity to diversify the leaders of society by changing whom they admit.

5.1 Methodology

Let Y_i^{Ivy} denote student i's post-college outcome (e.g., earnings) if she attends an average Ivy-Plus college. Our goal is to identify the average difference in Y_i^{Ivy} for applicants with different credentials X_{1i}, in order to understand how changing who is admitted would affect the average level of post-college outcomes for Ivy-Plus students. For example, we are interested in identifying the difference in outcomes for legacies ($X_{1i} = 1$) vs. non-legacies ($X_{1i} = 0$):

$$\triangle Y_X = E[Y_i^{Ivy}|X_{1i} = 1] - E[Y_i^{Ivy}|X_{1i} = 0] \tag{6}$$

We cannot estimate (6) directly because we do not observe Y_i^{Ivy} for students who do not attend Ivy-Plus colleges. To make progress, we infer students' potential outcomes had they attended Ivy-Plus colleges by combining our estimates of colleges' causal effects with students' observed outcomes. Under the model in (3), we can write a student's post-college outcome (e.g., earnings) as

$$Y_i = \phi_{j_D(i)} + \omega_i, \tag{7}$$

where $\phi_{j_D(i)}$ denotes the value-added of the college that student i attends and $\omega_i = \ _1X_{1i} + \ _2X_{2i} + \epsilon_i^Y$ denotes the student's latent potential, defined here as the student's outcome if she were to attend the average highly selective public flagship college (for which $\phi_j = 0$).

We estimate each college's value-added $\phi_{j_D(i)}$ using the observational model used in Section 4, regressing the relevant outcome (e.g., an indicator for reaching the top 1%) on college fixed effects and a quintic in SAT scores, 13 parent income bins, and indicators for race, gender, and home state. As discussed in Section 4, these observational estimates are highly correlated with the causal estimates observed from our two research designs. To adjust for the remaining selection in the observational estimates, we multiply the raw fixed

[43] Our outcome-based tests provide an input for evaluating admissions preferences rather than a definitive test of their merits for two reasons. First, colleges' objective functions include many factors beyond maximizing post-college student success, such as success in athletics or contributions to the student body or society that are not captured by the outcomes we study. Without taking a stance on a college's objective function, we cannot make normative claims about the desirability of current admissions criteria. Second, the outcomes we study may not directly measure students' latent merit at the point of college application because post-college outcomes are themselves a product of decisions made at subsequent stages (by firms, graduate schools, etc.). Any biases or preferences that deviate from meritocracy in downstream decisions will filter directly into our outcome-based tests.

effects by the ratio of the causal effect estimates from our waitlist design to the corresponding observational estimate (e.g., a coefficient of 0.86 for the predicted top 1% outcome, as shown in Figure 13a).[44]

We then estimate each student's latent potential as $\widehat{\omega}_i = Y_i - \widehat{\phi}_{j_D(i)}$ and predict their potential outcome had they attended an Ivy-Plus college as

$$\widehat{Y}_i^{Ivy} = \widehat{\omega}_i + \widehat{\phi}_{Ivy} = Y_i - \widehat{\phi}_{j_D(i)} + \widehat{\phi}_{Ivy}, \tag{8}$$

where $\widehat{\phi}_{Ivyplus}$ is the mean value-added of the Ivy-Plus colleges in our college-specific sample. Intuitively, we infer students' potential outcomes at Ivy-Plus colleges by adding the difference in fixed effects between the college they actually attended and the average Ivy-Plus college to their observed outcome Y_i.

Using this estimate of $\widehat{Y}_i^{Ivy}$, we obtain the following feasible estimator for the difference in potential outcomes between applicants with different credentials:

$$\triangle Y_X = E[\widehat{Y}_i^{Ivy}|X_{1i} = 1] - E[\widehat{Y}_i^{Ivy}|X_{1i} = 0]. \tag{9}$$

To maximize precision, the estimator in (8) assumes that there is no heterogeneity in causal effects across students who are currently admitted. The tests for heterogeneity in treatment effects implemented in Section 4 support this assumption. In particular, there is no significant heterogeneity in treatment effects along the key dimensions that we focus on here (legacy status, academic/non-academic ratings, and recruited athlete status), as shown in Appendix Table 7. Furthermore, we show in Figure A.23 that we obtain very similar results to those reported below simply by comparing the outcomes of Ivy-Plus attendees with different application credentials, an approach that does not require any use of college value-added estimates (but instead relies on other assumptions to rule out selection bias among admitted students).

5.2 Results

We regress outcomes on four binary indicators of academic credentials: indicators for legacy status, being a recruited athlete, having high non-academic ratings, and having high academic ratings. We estimate these regressions in the sample of all waitlisted applicants and admitted students, excluding rejected applicants not placed on the waitlist (who are not close to the margin of admission).

We first examine how students' predicted chances of reaching the top 1% of the income distribution vary with their credentials. To illustrate how our estimator works, we begin by simply regressing observed predicted top 1% rates based on employers at age 25 (Y_i) on the four indicators. The solid bars in Figure 17a plot the coefficients from this regression (along with 95% confidence intervals in the vertical lines). Ivy-Plus applicants' chances of reaching the top 1% after college are essentially unrelated to legacy status or their non-academic ratings. Recruited athletes are 2.5 pp more likely to reach the top 1% (relative to a baseline rate of 11.3% among non-athlete, non-legacy applicants with low academic and non-academic ratings). Those with high academic ratings are 4.4 pp more likely to reach the top 1%.

[44] We use the corrected observational estimates rather than directly using college fixed effect estimates from our waitlist or matriculation design because our data do not include all colleges.

The raw comparisons of Y_i in the solid bars combine differences in latent earnings potential ω_i with the fact that those with certain credentials are more likely to be admitted to Ivy-Plus colleges, which channel more students to the top 1%, as shown above. The second (cross-hatched) set of bars in Figure 17a show how much of the difference in outcomes is due to differences in the quality of colleges by regressing the value-added $\widehat{\phi}_{j_D(i)}$ of the colleges that students actually attend on the same four indicators. The estimates confirm that recruited athletes, legacies, and students with higher academic and non-academic ratings attend colleges that increase their students' chances of reaching the top 1%. The difference in college VA is especially large for recruited athletes (2.3 pp) relative to others in the applicant pool because virtually all athletes recruited to apply to Ivy-Plus colleges are ultimately admitted. Those with high non-academic ratings attend colleges that we estimate send an additional 0.7 pp of students to the top 1%, while legacies attend colleges that send an additional 0.3 pp of students to the top 1%.[45]

Finally, in Figure 17b, we regress potential outcomes if students had attended Ivy-Plus colleges $\widehat{Y}_i^{Ivy} = Y_i - \widehat{\phi}_{j_D(i)} + \widehat{\phi}_{Ivy}$ on the same four factors. These estimates correspond to the difference between the two sets of bars plotted in Figure 17a. After adjusting for differences in college quality, we find that athletic recruitment and non-academic ratings have no significant association with students' predicted chances of reaching the top 1%. Legacy status is negatively associated with children's chances of reaching the top 1%: legacy students are 1 pp less likely to reach the top 1% than non-legacies (holding academic/non-academic ratings and athletic status fixed), a 9% reduction relative to a baseline rate of 11.3%. By contrast, having a high (above-median) academic rating is increases one's chances of reaching the top 1% by 3.6 pp (32%), a magnitude similar to the causal effect of attending an Ivy-Plus college relative to a state flagship college.

Figures 17c and 17d replicate Figure 17b using indicators for attending an elite graduate school and working at a prestigious firm as outcomes. We again find no association between legacy status and non-academic ratings with these outcomes. Recruited athletes are substantially less likely to attend elite graduate schools and work at prestigious firms than their peers. Students with high academic ratings are substantially more likely to achieve success on these non-monetary outcomes, with a 6.6 pp higher chance of attending an elite graduate school (relative to a baseline rate of 8.4%) and a 7.3 pp higher chance of working at a prestigious firm (relative to a baseline rate of 22.1%).

The preceding estimates show the effects of the four factors without controlling for any other applicant characteristics. In Appendix Table 9, we replicate the same analysis with various sets of controls – controlling for SAT scores, parent income, additional demographics, and all observables in our admissions model. In virtually every specification and for all outcomes, we find the same pattern as above: academic ratings are highly predictive of outcomes, whereas the three factors that underlie the high-income admissions advantage are all negatively associated or uncorrelated with post-college outcomes.

To further investigate the predictive power of academic ratings, we examine how test scores predict post-college outcomes in Appendix Figures A.25 and A.26, replicating the same comparison of outcomes

[45] Legacy status has a smaller impact on average college VA than non-academic ratings because legacy preferences are beneficial only at a single college (Figure 7c), whereas higher non-academic ratings lead to higher admissions rates across Ivy-Plus colleges (Figure A.18).

among applicants with different SAT/ACT scores. Among applicants to Ivy-Plus colleges, students with higher SAT/ACT scores have substantially better post-college outcomes, adjusting for the quality of colleges they attend. These results demonstrate that standardized tests reveal substantial information about student potential despite the biases that may arise from disparities in test preparation.[46] In addition, higher academic ratings predict better post-college outcomes even conditional on standardized test scores (Appendix Table 9). Hence, admissions processes that take into account the strength of a student's coursework and other qualifications help identify student potential above and beyond standardized measures when focused on academic assessment.

In summary, our findings show that college admissions committees have considerable information at their disposal to distinguish applicants with different potential outcomes. Both objective and subjective measures of academic qualifications are highly predictive of students' post-college success, with predictive power comparable in magnitude to the causal effects of attending an Ivy-Plus college instead of a state flagship college. However, the other (non-academic) factors that are responsible for the higher admissions rates of students from high-income families do not predict (or, if anything, negatively predict) the measures of post-college success we consider.

5.3 Outcomes by Parental Income

Prior work has shown that children from families in the top 1% have a substantially higher chance of reaching the top 1% of the income distribution after college than those from lower-income families even among Ivy-Plus attendees (Chetty, Friedman, Saez, Turner, and Yagan 2020, Michelman, Price, and Zimmerman 2021, replicated here in Figure A.27). These patterns raise the possibility that children from high-income families have better outcomes (and thus may merit higher admissions rates) than those from lower income families. How do these results fit with our finding that the factors which lead to higher admissions rates for children from high-income families are not actually associated with outcomes?

Figure 18a shows that the difference in post-college incomes among Ivy-Plus attendees appears to be driven at least in part by career choices rather than differences in students' latent potential. Children from higher-income families are much more likely to work in (typically high-paying) business sectors (finance, consulting, or technology) and less likely to work in lower-paying non-profit or public sector positions (health, education, government, or civic organizations). Consistent with these findings, Figure 18b shows that the probability of attending an elite graduate school or working at a prestigious firm does not vary with parent income. These results suggest that the cross-sectional differences in observed incomes by parental income among Ivy-Plus attendees may largely be due to career choices rather than differences in the underlying potential outcomes among children from high- vs. low-income families.

[46] SAT/ACT scores remain strongly predictive of outcomes even conditional on high school grade point averages (Figures A.25 and A.26), whereas GPAs are essentially unrelated to outcomes.

6 Impacts of Changes in Admissions Practices: Counterfactual Predictions

In this section, we combine the results from the preceding analyses to answer our motivating question: how would changes in admissions practices at Ivy-Plus colleges affect the diversity of society's leaders? We first consider a set of changes that would undo the factors currently leading to higher admissions rates for high-income students. We then consider "need affirmative" policies that would instead provide explicit preferences for students from lower-income families. For each policy change, we predict the effect on the socioeconomic diversity of the student body at Ivy-Plus colleges and the post-college outcomes of students who would gain admission under these alternative scenarios.

The policy counterfactual analysis in this section differs from the decomposition of the extra students from the top 1% reported above in Table 2 in two ways. First, that analysis focused exclusively on students from families in the top 1%, rather than all students who might be affected by changes in a given admissions policy. For instance, removing legacy preferences would affect all legacy applicants, not just those from high-income families. Second, the decomposition analysis did not attempt to "refill" the slots opened up by eliminating the admissions advantages given to students from high-income families, which would further change the socioeconomic composition of the student body in practice. The policy counterfactuals below address both of these issues.

6.1 Reducing High-Income Admissions Advantages

Motivated by our findings in Section 3.3, we first analyze the effects that three changes in admissions practices would have on the socioeconomic backgrounds of Ivy-Plus college students: (1) eliminating legacy preferences, (2) eliminating the admissions advantage arising from the higher non-academic ratings obtained by students from high-income families, and (3) eliminating the over-representation of students from high-income families in athletic recruitment.

Assumptions. Our policy counterfactuals yield unbiased estimates of the impacts of changes in admissions practices on socioeconomic diversity under two key assumptions. First, we assume that there are no behavioral responses by students to changes in admissions policies upstream in the college application process. For example, we assume that students do not change their application patterns or aspects of their applications (e.g., investments in academic or non-academic qualifications) in response to the change in admissions practices.[47] Second, we assume that students affected by the admissions policy change (i.e, those newly admitted or not) have matriculation rates that are the same as the average matriculation rates for currently admitted students with the same characteristics. Abstractly, this assumption requires that all downstream parts of

[47] Theory is ambiguous on the sign of such responses: for instance, lower-income students with better chances of admissions after a change in admissions policy might be more likely to apply to an Ivy-Plus college (since applying is more likely to result in admission, analogous to a "price" effect) or they might be less likely to apply (since they now need to apply to fewer colleges to achieve a given chance of admission at a top college, analogous to an "income" effect). We envision a scenario where all Ivy-Plus colleges made these changes, which might lessen some changes purely due to "competition" effects, but further research is needed to assess the potential changes to application patterns that might result from these changes.

the pipeline are the same for marginal students as average students with same covariates.[48] These are both strong assumptions, but lacking actual reforms from which to estimate these effects, these predictions provide a sense of the magnitude of these potential changes.

To predict how student outcomes would change in these counterfactual scenarios, we further assume that colleges' causal effects do not change with the composition of the student body. If, for instance, having fewer students from families in the top 1% limits students' networks, and those connections are the reason that attending an Ivy-Plus college currently has large causal effects, then the causal effect of attending an Ivy-Plus college would fall as diversity rises and our predictions would overstate the impacts of diversifying the student body on the diversity of those who reach the upper tail after college.

Legacy Preferences. We begin with considering a policy that removes legacy preferences for all students. As above, we exclude recruited athletes entirely in this analysis since they are not admitted through the same process and return to them at the end of this subsection.

We model the impacts of eliminating legacy preferences in two steps. First, we take the estimated "legacy boost" from Figure 7 and proportionally de-admit a corresponding number of currently admitted legacy students, separately by parental income and SAT score to allow for the heterogeneity in the legacy advantage across subgroups shown in Figure 7b. For example, among students from families in the top 1% with SAT scores above 1500, legacy students are admitted to an Ivy-Plus college at roughly 4 times the rate as non-legacy applicants with comparable credentials. We therefore downweight the number of legacy students in the admitted class who are from the top 1% and have SAT scores above 1500 by 75%.

The de-admission step releases 112 slots, which can now be filled by other students. We then refill the number of slots released by admitting students from the waitlist (as well as the pool of newly rejected legacies from the first step) in proportion to their predicted admissions probability from the non-legacy admissions model in Section 3.3.

Table 6 presents the impacts of this counterfactual admissions policy on the parental income distributions. Eliminating legacy preferences and refilling the class as described above would reduce the fraction of students with parents in the top 1% from 15.8% in the actual data for Ivy-Plus colleges to 13.7%. It would also reduce the share of students from families between the 95th and 99th percentiles of the parent income distribution by another 0.7 pp, with a corresponding increase of 2.8 pp in the fraction of students from the bottom 95% of the income distribution.

Following the methodology in Section 5, we also predict the impacts of our admissions counterfactuals on outcomes by calculating the mean of Y_i^{Ivy} for the students who attend an Ivy-Plus under the counterfactual admissions policy and comparing it to the mean of Y_i among actual Ivy-Plus attendees in our data. Eliminating legacy preferences increases the share of students predicted to reach the top 1% based on their employer at age 25, the share of students working at prestigious firms (as defined in Section 4.2), and the share of students attending an elite graduate school. These predictions follow intuitively from the analysis

[48] Although we cannot directly verify this assumption in the data for students who are admitted or de-admitted, matriculation rates vary very little across subgroups once we condition on SAT scores and basic demographics; for instance, conditional on these factors, students' academic and non-academic ratings are uncorrelated with matriculation rates among admitted students.

in Section 5.2; legacy students are less likely to work at prestigious firms or attend elite graduate schools, and thus reducing the share of legacy admits improves average outcomes.

Non-Academic Ratings. Next, consider a policy that eliminates the admissions advantage that arises from the higher non-academic ratings enjoyed by students from high-income families. For instance, admissions readers could downweight the importance of nonacademic accomplishments for high-income students or could place greater weight on nonacademic factors for students from lower SES backgrounds, taking into account the context of their schools and childhood environments. Similar to the "legacy boost" in the previous counterfactual, we estimate the "ratings boost" as the difference in admissions rates between students from families with incomes above the 80th percentile and those with similar academic credentials (measured both by test scores and academic ratings) from the middle class, allowing for heterogeneity by parent income and academic credentials. For example, among students from families in the top 1% and with top academic credentials (SAT scores above 1500 and high academic ratings), applicants are 25% more likely to be admitted due to higher non-academic ratings. We then downweight the number of admitted students in proportion to this ratings boost. We then refill the class in the same proportional manner as in the legacy counterfactual.

We estimate that eliminating the influence of higher non-academic ratings among students from high-income families on admissions would further reduce the fraction of students from the top 1%, from 13.7% to 11.2%. Even though non-academic ratings accounted for a smaller share of the decomposition in Table 2 than legacy preferences, the effect in this counterfactual is of a similar magnitude. This is because the benefits of higher non-academic ratings are more concentrated in the top 1%, allowing less scope for new top 1% students to be admitted to replace de-admitted students from lower income families. This change would also increase the fraction of students working at prestigious firms and attending elite graduate schools, although it would slightly lower the fraction of students predicted to reach the top 1% based on their employer at age 25 because students from middle income families are less likely to have earnings in the top 1% than those from high-income families (potentially because of the occupational choice effect discussed in Section 5.3).

Recruited Athletes. Finally, consider a policy that would remove the disproportionate representation of high-income students among recruited athletes, so that the distribution of parental income among recruited athletes matches the parental income distribution of students with SAT scores comparable to non-athlete Ivy-Plus college attendees. While universities could in principle eliminate their athletic programs entirely and admit other students with skills comparable to the present student body, such a sharp change in athletic offerings may not be practical. A more feasible alternative might be to recruit athletes from more diverse backgrounds. One approach to doing so might be to recruit athletes with characteristics more similar to that of non-athletes, since even before the policy changes discussed above (and especially after them) there is greater socioeconomic diversity among non-athletes.[49]

To measure the potential impact of such changes, we model the limiting case in which athletes are recruited in such a way that their characteristics match those of the non-athletes in the class. Such a policy

[49]For instance, Ivy League schools could increase the Academic Index thresholds so that athletes differed less from the non-athlete population.

would further reduce the overall share of Ivy-Plus students who come from the top 1% from 11.2% to 10%. While the share of students predicted to reach the top 1% based on their employer at age 25 would increase only slightly – athletes are on average as financially successful as other students – the share of students attending elite graduate schools at age 28 or working at prestigious firms at age 25 would increase sharply.

In summary, the three changes in admissions practices would reduce the share of students from the top 1% at Ivy-Plus colleges by approximately 40%, from 15.8% to 10.0%.[50] These changes would also reduce the share of students from families with incomes between the 95th and 99th percentiles ($239-$611K) by 2.9pp, as shown in Table 6. The net result of eliminating the factors underlying the admissions advantage for high-income students is that the share of students from families in the bottom 95% of the parental income distribution would increase by 8.7pp. Average student outcomes would not change or, if anything, improve along all three dimensions we consider.

Intuitively, because the three factors that lead to higher admissions rates for students from high-income families are either uncorrelated or negatively associated with post-college outcomes, there is scope for changes in admissions policies that could substantially increase the socioeconomic diversity of students without changing the share of students who reach the upper tail of society after college. Given that the pathway to many leadership positions runs through Ivy-Plus colleges (Figure 1), these results imply that Ivy-Plus colleges could significantly increase the diversity of the country's leaders by changing their admissions practices to be more income-neutral.

6.2 Need-Affirmative Admissions

The three counterfactual policies modeled above directly address the sources of advantage in admissions for students from high-income families from our analysis in Section 3. In practice, there may be constraints to making such changes that are outside the scope of our analysis, such as a need to maintain alumni relations or athletic teams. An alternative, potentially more feasible approach to addressing these imbalances is to simply offer students from low- and middle-income families their own offsetting advantage in admissions, an approach sometimes termed "need-affirmative" admissions. We now analyze the level of such preferences that would be required to achieve a similar level of socioeconomic diversity as adjusting the three specific policies above.

Unlike the previous analysis – which involves de-admitting a specific subset of students and then refilling from the pool – this approach requires admitting a substantially new class. We therefore begin with all students either admitted or placed on the waitlist, estimating their chances of admissions as the rate predicted by our admissions model from Section 3. We preserve all admissions preferences as we observe in the data, including the reliance on the three factors discussed above. As above, we assume that students not previously admitted would matriculate at the same rates as other students with similar characteristics.

[50] If one were to eliminate income disparities (conditional on SAT scores) in all parts of the college attendance pipeline (application, admissions, and matriculation), the share of students from the top 1% would fall to 7.2% (Table 6, row 6). This is slightly larger than the impacts of changing the three admissions practices, confirming that changes in admissions practices could achieve most of the attainable gains from a scenario in which Ivy-Plus attendance rates did not differ by parental income conditional on SAT/ACT scores.

We then implement a need-affirmative policy by proportionally increasing the admissions rates for all students below the 95th percentile of the parent income distribution who have high academic ratings. This focus on admitting students with high academic ratings is motivated by the analysis in Section 5, which identified academic ratings as particularly predictive of post-college outcomes among the various factors considered in holistic admissions. We then scale down admissions rates proportionally for all students to preserve the size of the attending class.

Colleges could achieve any level of socioeconomic diversity by increasing the intensity of the need-affirmative preferences. Here, we choose preferences to match exactly the shares of students from the bottom 60% and 60th to 95th percentiles of the parent income distribution produced by the three policies analyzed above. Matching the income shares produced by eliminating high-income admissions advantages requires admissions rates that are roughly 60% higher for students between the 60th and 95th percentiles than for students in the top 5% with comparable admissions credentials and 130% higher for students from the bottom 60% relative to those from the top 5% with comparable credentials. These admissions boosts are smaller than the preferences currently given to legacy students (which are approximately 300%, as shown in Figure 7c), suggesting that it may be feasible to achieve increases in socioeconomic diversity comparable to those above with plausible need-based preferences in admissions.

Row 5 of Table 6 reports the characteristics of attending students under this need-affirmative counterfactual. By construction, the share of students from the lowest two income groups matches that in Row 4 of the table, following the three specific policy counterfactuals.[51] Most importantly, we find that the additional students admitted under this need-affirmative counterfactual have *better* post-college outcomes than current Ivy-Plus attendees: they are more likely to be predicted to reach the top 1% based on their age 25 employer, work at a prestigious firm, and attend an elite graduate school.

The preceding calculations apply to a single Ivy-plus college changing its admissions practices by itself. When such changes are scaled across all colleges, one may be concerned about supply constraints: are there enough high-achieving low- and middle-income students who apply to Ivy-plus colleges for such policies to remain feasible if all Ivy-Plus colleges were to admit more such students? The need-affirmative counterfactual calls for increasing the total number of enrolled students with high academic ratings from the bottom 95% of the parent-income distribution across all 12 Ivy-Plus colleges from 7,000 to 10,000 (250 per college). We estimate that there are 24,500 students in each graduating cohort of high school students who would attain high academic ratings if they applied to the schools for which we have admissions data. Within this group, we then calculate that 11,050 currently apply to at least one Ivy-Plus institution – suggesting that there is likely an adequate supply of high-achieving, low-income students even among the current applicant pool.[52]

[51] The corresponding reduction in students from the top 5% falls more heavily on students between the 95th and 99th percentiles than the top 1% relative to our previous set of counterfactuals. Intuitively, those three policies directly target the sources of the spike in admissions rates for students from the top 1%; absent need-affirmative policies that benefit all students from the bottom 99%, need-affirmative policies will not address the sharp differences in admissions rates between students within the top 5%.

[52] More precisely, we use the internal admissions records to calculate the share of students with a high academic rating at each test score level, and then we multiply this share by the total number of scores at each level above an SAT of 1400 or ACT of 31 from students in the bottom 95% of the parent-income distribution. To calculate the total number of such students who

We conclude that there are a substantial number of low- and middle-income students with strong potential outcomes – students with high academic ratings or simply high SAT/ACT scores – who apply to Ivy-Plus colleges but are presently not admitted. Admitting more of these students could allow colleges to diversify their student body while improving class quality, as measured by the set of outcomes we analyze here.

7 Conclusion

This paper has shown that highly selective private colleges serve as gateways to the upper echelons of society in the United States. Because these colleges currently admit students from high-income families at substantially higher rates than students from lower-income families with comparable academic credentials, they perpetuate privilege across generations. At the same time, these colleges have the capacity to substantially diversify society's leaders in terms of their socioeconomic backgrounds (and presumably other characteristics as well) by changing their admissions policies. Importantly, our findings reveal that class-based affirmative action (favoring students from more disadvantaged backgrounds) is not necessary to increase socioeconomic diversity at such colleges; simply removing the admissions advantages currently conferred to students from high-income families (or offsetting them with corresponding advantages for students from lower-income families) could increase socioeconomic diversity by an amount comparable to the impacts of race-based affirmative action on racial diversity.

Highly selective public colleges also have a larger share of students from very high-income families than middle-class families, but the gap there is driven by disparities in application rates rather than admissions rates. Thus, increasing socioeconomic diversity going forward will require different approaches at different types of colleges. At elite private institutions, revisiting three key aspects of the admissions process – preferences given to children of alumni, to students from certain high schools that produce strong non-academic credentials, and to recruited athletes – could significantly increase socioeconomic diversity. At public colleges, interventions to increase application rates from qualified students, such as the HAIL intervention at the University of Michigan (Dynarski, Libassi, Michelmore, and Owen 2021), changes in out-of-state tuition, and outreach policies are likely to be more impactful.

Despite substantial initiatives to increase socioeconomic diversity, the share of students from the top 1% vs. the middle class at highly selective private colleges in America has remained essentially unchanged over the past 20 years, both unconditionally and conditional on SAT/ACT scores (Figure 19). Understanding what aspects of the pipeline to college enrollment lead to an under-representation of children from low- and middle-income families at a given college and addressing the relevant barriers directly may be a more fruitful approach to expanding access. To that end, the pipeline data produced in this study – which are publicly available on a college-by-college basis at www.opportunityinsights.org/data – can be used to determine which part of the pipeline one should focus on (applications vs. admissions or matriculation) at a given college going forward to increase socioeconomic diversity.

apply to at least one Ivy-Plus school, we calculate the total number of applications across all schools and divide by the average number of Ivy-Plus score sends among students who sent a score to at least one Ivy-Plus school.

Beyond their implications for highly selective colleges, the results of this study also may have implications for policies to increase social mobility in other settings. First, as in a number of recent studies in very different settings (e.g., Bergman, Chetty, DeLuca, Hendren, Lawrence F Katz, and Palmer 2023, Lawrence F. Katz, Roth, Hendra, and Schaberg 2022), our findings show that simply providing financial resources is insufficient to improve economic opportunity. Most of the colleges in our dataset offered extensive financial aid for lower-income applicants during the period we studied yet had much lower attendance rates among those groups as a result of other factors. These results underscore the importance of coupling financial support (which may be a necessary condition for lower-income students to attend expensive colleges) with other policy changes to increase economic mobility.

Second, our results raise questions about the equity implications of holistic evaluation policies. Highly selective public colleges that follow more standardized processes to evaluate applications exhibit smaller disparities in admissions rates by parental income than private colleges that use more holistic evaluations. While holistic evaluations permit broader evaluations of diverse candidates in principle, in practice, they appear to create incentives and scope for students from high-income families to further differentiate themselves from others (e.g., by enrolling at private high schools that provide non-academic credentialing). Similar challenges may arise in many other settings where applicants are evaluated on complex criteria, from internships to job applications to memberships in selective clubs.

Finally, our results illustrate how economic advantage is passed down across generations through highly selective colleges, one of many selective groups in modern societies. Similar dynamics may be at play in other selective groups – from K-12 schools to employers – and contribute to the persistence of intergenerational inequality. Studying entry and outcomes in other such groups using the longitudinal data and research designs developed in this paper may yield further insights into how opportunities can be distributed more widely.

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Supplementary Appendix

A Statistics on Colleges Attended by Society's Leaders

This Appendix describes how we construct Figure 1, which presents calculations of the share of elite occupations held by Ivy-Plus college attendees. The first row shows that Ivy-Plus attendees are 0.8 percent of all college students. We construct this statistic using the long-term outcomes sample and dropping test-takers who didn't go to college, as described in Section 2.

Income. The first set of outcomes focuses on the fraction of individuals in various upper quantiles of the individual income distribution at age 33 who attended Ivy-Plus colleges. We measure income as total pre-tax individual earnings using data from tax records and college attendance using the 1098-T data, Pell grant data, and colleges' own attendance records, as described in Section 2. We measure the fraction of Fortune 500 CEOs who attended Ivy-Plus colleges using hand-collected data from Fortune (n.d.) webpage and various publicly available biographical resources for these CEOs in January 2023. If an individual received multiple undergraduate degrees and one of them is from an Ivy-Plus institution, they are categorized as an Ivy-Plus attendee.

Arts and Sciences. We measure graduate school attendance as the proportion of individuals who attended Ivy-Plus colleges, among test-takers who attended graduate school by age 28 (as described in detail in Section 2). We define elite graduate schools as all Ivy-Plus institutions plus UC Berkeley, UCLA, UCSF, University of Virginia, and the University of Michigan, since these schools consistently rank highly across graduate programs in medicine, business, science, law, and other fields. We obtain data on the undergraduate institutions of individuals who were granted MacArthur Fellowships between 1981 and 2014 from Conrad (2015).

Public Service. The last set of outcomes shows the fraction of individuals in various public service leadership positions who attended Ivy-Plus colleges. We measured the proportion of current US Senators from the 117th Congress who attended Ivy-Plus colleges by combining information from Buchholz (2021) and Congress.gov (n.d.) webpage. The fraction of journalists at the New York Times and the Washington Post who attended an "Elite" undergraduate institution was obtained from Wai and Perina (2018). In their data, the "Elite" undergraduate institution includes the Ivy League as well as 20 other colleges where the average SAT score exceeded 1400 in 2013. Data on US Presidents from 1961-2023 who attended Ivy-Plus colleges were collected from publicly available biographical resources by referring to the list of US Presidents from The Library of Congress (n.d.) webpage. We also computed the fraction of all Rhodes scholarship winners from 2014 to 2021 who attended Ivy-Plus schools as undergraduates. This information is web-scraped from Rhodes Trust (n.d.) webpage. Finally, the fraction of Supreme Court Justices who went to Ivy-Plus colleges includes all appointments from Thurgood Marshall in 1967 to Ketanji Brown Jackson in 2022. We collected information on undergraduate institutions of Supreme Court Justices from publicly available biographical resources by referring to the list of appointments from Supreme Court of the United States (n.d.) webpage.

We also replicate these statistics for attendees of highly selective private and flagship public colleges in Figure A.1. The list of highly selective private and public colleges used in this figure can be found in Appendix Table 1. All the outcomes in Figure A.1 use the same definition and come from the same data sources as Figure 1.

B Predicting Application Rates Using Scoresend Data

Our data provide two sources of information on students' applications to colleges. First, we observe applications to colleges for which we have linked internal data in our college-specific sample. Second, we observe colleges to which students send their standardized test scores (up to 33 colleges, although in practice students rarely hit this limit). These score-sends serve as an indicator for where a student intends to apply. However, students may send their test scores to schools to which they do not ultimately apply, and thus score sends provide an imperfect signal of true application.

To correct for this problem and predict true application rates from score send data, we estimate the fraction of score sends that result in actual applications at the subset of colleges for which we have internal application data. Among those students who sent a score, we regress an indicator for a completed application on quintic polynomials of parental income, student SAT/ACT score, and distance between a student's home zip code and the college the student applied to. The predicted values from this regression give the estimated fraction of score-sends from a given type of student that convert into completed applications, heterogeneously based on students' characteristics. We then apply these estimated fractions to all score-sends in the data to form preliminary estimates of the total number of applications as a subset of the total score-sends to each school.

These preliminary estimates do not capture students who apply to a college without sending their test score (since some colleges do not require standardized tests) or who send their score in a manner that we do not capture in our data (an issue that can arise in the ACT data since we do not see scores sent after students take the test whose score we record). This leads us to undercount the true number of applications. To adjust for this issue, we calculate the ratio of the total number of applications reported in the Integrated Postsecondary Education Data System (IPEDS) to our preliminary estimate of the total number of applications separately for highly selective private and public colleges, and multiply our preliminary estimates by these ratios.

C Predicting Income Trajectories Using Initial Employers

Many of the students in the cohorts we study are in their mid to late twenties when we observe their post-college outcomes in 2021. Because individuals' income ranks do not stabilize until their thirties (see Chetty, Hendren, Kline, and Saez (2014)), we cannot observe reliable estimates of permanent income ranks at these ages. We address this problem by predicting earnings ranks at later ages using data on individuals' initial employers.

We therefore predict students' income rank and probability of reaching the upper tail of the national earnings distribution at age 33 based on their employer (or graduate school) at age 25 and their birth cohort. We estimate predictions using students who attended colleges in selectivity tiers 1-4 (Ivy-Plus schools, Other Elite Schools, Highly Selective Public, and Highly Selective Private, totaling 176 colleges), and apply these predictions to all members of their birth cohort.

More specifically, each student's firm is identified as the payer from which students receive the highest earnings in a given year. If a student attends graduate school in a given year, we identify the firm as the graduate institution. Firms with less than 7 students in our prediction sample are pooled together using the firm's 2-digit NAICS industry code, a flag for if there is only 1 employee, and the ventile of the fraction of age 33 employees in the top 1%. We use student's firms when they are age 25, filling in with firm at age 26 and then at age 24 if the age 25 firm is missing. College tier is based on the modal college attended by a student over multiple years, as reported in Pell records and Forms 1098-T. For students in the 1974-1988 birth cohorts, we calculate the mean age 33 income rank and probability of reaching the top 1% of the income distribution for their cohort-firm combination. We then predict each student's income as the mean for their firm across all cohorts, excluding their own cohort. For students in the 1989-1996 birth cohorts, we predict their income as the mean for their firm pairing across all birth cohorts in 1974-1988. Finally, we re-rank predicted incomes within cohorts to have comparable income ranks across years.

We assign firms to the "Finance/Consulting/Tech" and "Non-Profit/Public" categories using NAICS codes. Finance/Consulting/Tech includes firms with NAICS codes beginning with 51, 52 and 54. Non-Profit/Public includes the 2-digit NAICS codes 61, 62, and 92.

D Defining Elite and Prestigious Firms

In this appendix, we describe how we construct our "elite" and "prestigious" firm definitions. We begin with the list of firms (corresponding to EINs in W-2s), firm names, North American Industry Classification System (NAICS) codes, and flags for government and nonprofit firms from the IRS Business Returns Transaction File metadata. Firms are identified using their names. In cases where the same firm appears multiple times under similar names, we pool the firms together by eliminating common qualifiers (e.g. LLC, Corporation, etc.).

We first calculate the share of all Ivy-Plus attendees in the 1979 to 1996 birth cohorts that work at each firm when they are age 25. We remove the attendee's own college from the calculation of the firm-level shares. When students do not have firms at age 25, we fill them in using age 26, and then age 24. We then calculate the same share for the Highly Selective Public colleges. In instances where a firm employs zero Highly Selective Public attendees, we calculate the share as if there were one. We then compute a ratio of those shares to form a measure of disproportionate Ivy-Plus employment, restricting the sample to firms that employ at least 25 students and leaving the student's own observation out of the share calculation altogether. We rank firms using this metric and define a firm as "elite" by pulling firms from the top of the

list until we have accounted 25% of Ivy-Plus attendee employment.

To measure high-status jobs that do not necessarily lead to high earnings, we regress each individual's "elite" firm ratio (described above) on the predicted top 1% probability of the individual's age 25 firm, which is described in more detail in Appendix C. We then calculate the residual from this regression and rerank firms from highest to lowest according to the residual. Finally, we pull the firms with the highest residuals in order until we have accounted for 25% of Ivy-Plus employment, and we call these firms with the highest residual ranks "prestigious" employers.

To validate our approach, we identify law firms, hospitals and universities using NAICS codes. Consulting firms cannot be reliably identified with NAICS firms, instead we identify firms as consulting if greater than 25% of Ivy-Plus applicants employed at a firm have occupational titles that indicate they are consultants.

E Pipeline Statistics by College

In this appendix we describe the methods we use to calculate attendance and application rates at the college-specific level, which we release publicly along with this study. We calculate and report attendance and application rates for all Ivy-Plus institutions, the schools used in our elite public and elite private school samples, the members of the New England Small College Athletic Conference, flagship public universities, and nearly all of the remaining colleges ranked in the top 100 national universities by U.S. News and World Report in 2022.

We begin with the merged dataset, as described in Section 2, for students who take a standardized test and were on track to graduate from high school in the classes of 2011, 2013, or 2015. We calculate attendance rates (fraction of students who attend each college) separately for students in each college-by-parent income bin-bytest score cell. We use 13 parent income bins, corresponding to parent income rank percentiles 0-20, 20-40, 40-60, 60-70, 70-80, 80-90, 90-95, 95-96, 96-97, 97-98, 98-99, 99-99.9, and the top 0.1%. We convert ACT scores into SAT scores using concordance tables published by the College Board and the ACT. We then aggregate these attendance rates to the college by parent income bin level using the distribution of test scores for students attending a given institution. More specifically, in calculating the attendance rates for a given college c, we weight test-takers with score a from parent income group p with

$$weight_{acp} = \frac{P(SAT_a|attend_c)}{P(SAT_a|p)} \tag{10}$$

This ensures that the distribution of test scores matches that of attending students at a given college for all parent income groups. For public universities, we also calculate attendance rates separately for in-state and out-of-state students using a very similar process but with two differences. First, in each case the test-score distribution used for the final weighting remains the distribution of test scores for all attending students rather than of in-state or out-of-state students; second, in order to avoid statistics based on very few students in our school-specific analyses, we calculate a single statistic for attendance and application for in-state students from the top 1% (thereby combining those from the 99-99.9 and top 0.1% parent income

bins).

We cannot directly observe applications for schools for which we do not have internal admissions data. However, our data from testing companies includes a subset of schools to which students submitted their scores. We observe up to 33 score submissions for each student who took the SAT or the ACT. Sending test scores to a college was generally required as part of the application process during the years for which we calculate attendance and application rates; however, a record of score submission ("scoresend") to a school does not guarantee that the student applied to that school, nor does the absence of scoresend rule out application.

We therefore predict application rates at the college by parent income bin by score level using scoresend rates, supplemented with other data sources. We begin by predicting actual application conditional on observed scoresends within the subset of schools for which we can observe completed applications. We then regress this indicator for completed application at the student level on quintics of SAT score, parent income rank, and distance from the college, defined as the distance from the college's address to the centroid of the student's home zip code.

We further validate our estimates using data from the Integrated Postsecondary Education Data System (IPEDS), which includes the number of applicants per year to many colleges that have published this information, for the years 2002-2020. We first adjust the total number of applicants in the IPEDs data to account for unlinked students (primarily international students) using the ratio of total number of applications in the internal data to the total number of applications in the IPEDS data for each year. We then take the average of these scaling factors, weighted by the number of applicants to each school, for each year, separately for public and private schools. We then apply these scaling factors to the IPEDS data for all schools. Then at the college by year level, we calculate an "application gap" as the difference between the scaled number of applicants in the IPEDS data and the total number of predicted applicants from the scoresend model. We then scale each student's predicted probability of having completed an application by the ratio of this application gap to the total number of scoresends, distributing the application gap proportionally across all students who send scores. We then collapse these predicted application rates by parent income bin, using the same SAT weighting process as in the calculation of the attendance rates. As with attendance rates, we also calculate the predicted application rates separately for in-state and out-of-state students for public schools.

To protect the confidentiality of individuals in the tax data, we add a small amount of random noise to each statistic following a differentially private algorithm (Dwork, McSherry, Nissim, and A. D. Smith, 2006 and Chetty and Friedman, 2019). Because each published statistic is a fraction, the global sensitivity of each number is 1/N, where N is the number of individuals in our data in a given parent income bin. (For in-state estimates for public colleges, N is the number of individuals in our data in a given state and parent income bin; for out-of-state estimates, N is the number of individuals in our data excluding those from a specific state.) Using ϵ=1, we then add random noise drawn from a normal distribution with mean 0 and standard

deviation 1/N.

F Outcome Comparisons Among Ivy-Plus Attendees

In this appendix, we replicate the outcome-based tests in Section 5, directly comparing outcomes among Ivy-Plus attendees instead of comparing outcomes among applicants, and adjusting for differences in the value-added of the colleges they attend. We show that this approach – which does not rely on assumptions about colleges causal effects – yields results very similar to our baseline estimates that correct for selection using college value-added estimates.

Methods and Assumptions. We estimate $\triangle Y_X$ by comparing the observed outcomes of Ivy-Plus attendees with different credentials (e.g., legacy vs. non-legacy students):

$$\triangle Y_{X|Ivy} = E[Y_i|X_{1i} = 1, j_i = Ivy] - E[Y_i|X_{1i} = 0, j_i = Ivy] \tag{11}$$

By conditioning on attending an Ivy-Plus college, this comparison holds fixed college value-added, thereby isolating differences in students' potential ω_i independent of college fixed effects. However, because the set of students who are admitted to Ivy-Plus colleges is endogenously selected based on their overall rating as in (2), this estimator does not necessarily yield an unbiased estimate of the average difference in outcomes among legacy and non-legacy students in the applicant pool had they all attended Ivy-Plus colleges ($\triangle Y_X$). Intuitively, given legacy preferences, non-legacy students who are admitted must have a more positive draw on some other attribute on average (e.g., academic credentials) in order to gain admission to an Ivy-Plus college. If those attributes are correlated with long-term outcomes (X_{2i}), we will obtain an estimate $\triangle Y_{X|Ivy} < \triangle Y_X$, since we are effectively comparing non-legacy students with stronger academic credentials on average to legacy students within the admitted pool of students. To obtain an unbiased estimate of $\triangle Y_X$ by comparing outcomes among matriculants, we must therefore make the following strong assumption, which rules out the preceding example and assumes that all residual variation in admissions decisions comes from idiosyncratic factors unrelated to long-term outcomes.

Assumption 3 (**Idiosyncratic Admissions Conditional on Observables**). *Conditional on X_1, differences in admissions decisions are driven entirely by idiosyncratic factors ϵ_{ij} rather than latent unobservables correlated with long term outcomes X_{2i}: $Var(X_{2i}) = 0$.*

Although this assumption may not hold exactly, the estimator in (11) turns out to yield estimates that are very similar to our baseline estimates that adjust for selection bias among all applicants, perhaps because a large portion of the variation in admissions decisions is driven by idiosyncratic factors conditional on the observable factors we consider.

Results. We regress outcomes on indicators for legacy status, being a recruited athlete, having high non-academic ratings, and having high academic ratings in our college-specific sample of Ivy-Plus attendees. Figure A.23 plots the coefficients obtained from this OLS regression along with 95% confidence intervals for

the same three outcomes that we consider in Figure 17. We find very similar results, with nearly identical magnitudes. Legacy status and non-academic ratings have no significant association with any of the three outcomes. Athletic recruitment is unrelated to income but negatively associated with the probability of attending an elite graduate school or working at a prestigious firm. By contrast, having a high (above-median) academic rating is strongly associated with significantly better outcomes, with magnitudes similar to the causal effect of attending an Ivy-Plus college instead of a state flagship college. These findings show that our baseline results are apparent even with simple comparisons among Ivy-Plus attendees and thus do not depend on the way in which we adjust for college value-added or on assumptions about the heterogeneity of colleges' causal effects across students.

Table 1: Summary Statistics for Analysis Samples

	Sample			
	Pipeline	Long Term Outcomes	Ivy-Plus College-Specific	Flagship Public College-Specific
	(1)	(2)	(3)	(4)
A: College Attendance				
% Attending Any College	93.0%	96.3%	97.9%	99.0%
% Attending Ivy+ College	0.7%	0.9%	24.2%	4.5%
% Attending Flagship Public College	2.4%	2.6%	11.3%	24.9%
% Attending Other Selective Private College	0.9%	1.0%	11.7%	5.6%
B: Standardized Test Scores				
Test Score	991	993	1374	1228
Number of Scoresends	4.49	4.86	9.88	7.46
C: Admission and Matriculation				
% Admitted	-	-	13.0%	34.4%
% Matriculated	-	-	7.4%	15.7%
% Applied Early	-	-	12.4%	29.1%
% Waitlisted	-	-	10.4%	4.3%
% Athlete	-	-	1.7%	0.3%
% Legacy	-	-	3.3%	-
% Children of Faculty	-	-	0.1%	-
D: Children Demographics				
Year of Birth	1994	1985	1990	1989
Mean Age at Matriculation	18	18	18	18
% Female	53.4%	54.1%	55.2%	46.5%
% White	57.4%	66.1%	50.9%	45.8%
% Black	13.1%	10.6%	7.6%	6.3%
% Hispanic	14.3%	7.4%	8.9%	14.2%
% Asian	5.8%	5.1%	20.9%	26.3%
% American Indian/ Native American	0.7%	0.8%	1.3%	0.6%
% Native Hawaiian/ Pacific Islander	0.1%	0.0%	0.1%	0.0%
% Unknown Race	8.6%	10.0%	10.4%	6.7%
E: Parents' Incomes				
Mean Parent Household Income	$76,360	$86,030	$151,627	$123,027
Mean Parent Income Rank	61.8	62.5	78.0	72.3
F: Post-College Outcomes				
Median Income at Age 33	-	$45,632	$83,209	$72,197
Mean Income Rank at Age 33	-	65.8	79.8	77.0
% in Top 1% at Age 33	-	5.2%	15.4%	10.3%
Predicted Top 1% at 33 based on Age 25 Employer	4.5%	4.1%	10.3%	7.7%
Predicted Income Rank at Age 33	71.3	71.1	76.6	75.3
% Attending Graduate School at Age 28	7.3%	8.3%	23.2%	16.1%
% Attending an Elite Graduate School at Age 28	0.4%	0.5%	7.1%	2.9%
% Working at an Elite Firm at Age 25	3.3%	3.3%	18.3%	5.3%
% Working at a Prestigious Firm at Age 25	4.1%	4.1%	19.6%	5.7%
Number of Children	5,063,263	9,849,734	486,150	1,877,770

Notes: The table presents summary statistics for the samples defined in Section 2.1. Column 1 includes children who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) can be linked to parents, and (3) appear in either the SAT or ACT data in 2011, 2013, or 2015. Column 2 includes children who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) can be linked to parents, (3) were born in 1982-1988, and (4) appear in either the SAT or ACT data in 2001 to 2005 or 2007. Columns 3 and 4 show statistics for children in our internal admissions data from selected Ivy-Plus colleges (column 3) and highly selective public flagship colleges (column 4) who (1) are US citizens or permanent residents, (2) can be linked to the tax data based on their SSN or ITIN, and (3) can be linked to parents in the tax data. Test Scores are reported in SAT points out of 1600. In columns 3 and 4, Panel C reports statistics counting each college application once; panels A, B, D-F report one statistic per unique student, even if they apply to multiple schools. For post-college outcomes in panel F, we further restrict to students old enough to achieve relevant outcomes. Specifically, Panel F Column 1 restricts to children born in 1991-1996. Columns 3 and 4 restrict to kids born before 1996 who were 21 or younger when they applied to college. All monetary values are measured in 2015 dollars. See Section 2 for more detail on variable definitions and data sources.

Table 2: Additional Students Admitted from Top 1% at Selected Ivy-Plus Colleges: Sequential Decomposition Analysis

		Total	Subtotal	Share of Excess Top 1% Students
[1]	Class Size	1650		
[2]	Total Students with Parent Income in Top 1%	261		
[3]	Total Excess Students with Parent Income in Top 1%	157		100.0%
[4]	Attributable to Admission Rates	103		65.7%
[5]	Legacy		47	30.0%
[6]	Ratings		31	19.7%
[7]	Athletes		25	16.0%
[8]	Attributable to Matriculation Rates	19		12.4%
[9]	Attributable to Application Rates	35		21.9%

Notes: This table quantifies the sources of the gap in attendance rates between students from families in the top 1% of the income distribution and students from families between the 70th and 80th percentile of the national income distribution (the "middle class") by considering a series of counterfactuals, all of which hold fixed students' test scores. Row 1 reports the average class size at Ivy-Plus colleges, while Row 2 reports the observed number of students with parents in the top 1%. We then calculate the share of students from the top 1% who would attend these colleges if they attended at the same rate as students with the same test score but from the 70th to 80th percentiles of the parent-income distribution. Row 3 reports the difference between the number in Row 2 this number, i.e. the "excess" students from the top 1% relative to the equal-attendance-rate benchmark, equal to the share of students with parental income rank between 70 and 80 who attend these colleges in order to calculate the "excess" students from the top 1%. We then calculate the number in Rows 9, 8, and the sum of Rows 5 and 6 as the shares of the attendance pipeline (for non-athletes) due to application, matriculation, and admissions, respectively. We estimate the share due to applications as the log-change in top 1% share from equalizing application rates in the full sample of Ivy-Plus schools (see Figure 3a for more details). We also estimate the share due to the combination of admissions and matriculation in the full sample of Ivy-Plus schools (see Figure 3b for details). We then apportion this between admissions and matriculation using the log-share for each in the subset of selected Ivy-Plus schools for which we have internal data (as in Figure 4). Within admissions, we estimate the log-point contribution of legacy preferences again in the subset of selected Ivy-Plus schools; in a certain Ivy-Plus school (see Figure 8 for more details), we estimate the contribution of non-academic ratings. Finding that non-academic ratings account for all of the remaining admissions share (after legacy preferences) at the certain school, we assume that non-academic ratings accounts for the remainder of the admissions share at all other Ivy-Plus schools as well (a share which differs by school). We then calculate the absolute numbers of students reported in Rows 4-9 by first reducing the total number of top 1% students by the admissions share (sum of Rows 5 and 6), and within that based proportionally on the legacy (Row 5) and non-academic rating (Row 6) shares, then by the matriculation share (Row 8), and finally by the application share (Row 9). In Row 7, the number of excess top 1% athletes is calculated as the difference between the total number of athletes from the top 1% and the counterfactual number of top 1% athletes if top 1% athletes attended Ivy-Plus colleges at the same counterfactual rate as top 1% non-athletes, holding fixed the total number of athletes in the class. Row 4 is the sum of the numbers in Rows 5-7.

Table 3: Effects of AOTC and Pell Grant Policy Changes

	(1)	(2)	(3)	(4)	(5)
Panel A: Tier 1 and 2 College Attendance					
	Four Year College Goers		Four Year College Goers, SAT ≥ 1200		
Diff-in-Diff Estimate (Low vs High Income)	-0.17	-0.06	0.03	0.40	-0.13
	(0.06)	(0.06)	(0.03)	(0.29)	(0.29)
Diff-in-Diff Estimate (Middle vs High Income)	0.09	0.10	0.49	0.93	0.56
	(0.06)	(0.06)	(0.26)	(0.23)	(0.23)
State Fixed Effects		X		X	X
Year Fixed Effects		X		X	X
Parent Income Controls		X		X	X
Race Fixed Effects					X
Panel B: Tiers 1-3 (Plus Flagship) College Attendance					
	Four Year College Goers		Four Year College Goers, SAT ≥ 1000		
Diff-in-Diff Estimate (Low vs High Income)	-0.34	0.07	0.46	0.89	0.52
	(0.12)	(0.12)	(0.02)	(0.20)	(0.19)
Diff-in-Diff Estimate (Middle vs High Income)	0.15	0.18	0.49	0.69	0.41
	(0.12)	(0.12)	(0.18)	(0.17)	(0.17)
State Fixed Effects		X		X	X
Year Fixed Effects		X		X	X
Parent Income Controls		X		X	X
Race Fixed Effects		X			X
Panel C: Four-Year College Attendance					
	All Students				
Diff-in-Diff Estimate (Low vs High Income)	-0.72	-0.73			
	(0.12)	(0.12)			
Diff-in-Diff Estimate (Middle vs High Income)	0.22	0.2			
	(0.11)	(0.12)			
State Fixed Effects		X			
Year Fixed Effects		X			
Parent Income Controls		X			

Notes: This table presents regression estimates of the effects of a difference-in-differences analysis comparing college attendance rates before and after the expansion of Pell and AOTC in 2009. The treated groups are children with parental incomes between $0 and $40,000 (low-income) and between $40,000 to $80,000 (middle income). The control group is children from higher-income families ($100,000-$120,000). Panel A reports attendance by parent income at Tier 1 and Tier 2 schools as classified by Barron's, which correspond to Ivy-Plus and other highly selective schools. Column 1 reports the raw difference-in-differences estimate, and Column 2 reports the estimate with controls for state, year, and parent income. Columns 3, 4, and 5 restrict the sample to test-takers with standardized test scores equivalent to at least 1200 points on the SAT. Column 3 reports a raw difference-in-difference estimate, Column 4 adds state, year, and parental income controls, and Column 5 additionally adds controls for student race. Panel B reports attendance rates, conditional on attending any four-year college, at schools in Tiers 1–3, which additionally include highly selective colleges. Columns 1-5 are defined identically to those in Panel A, except the sample for columns 3-5 is restricted to test takers with scores equivalent to at least 1000 on the SAT. Panel C reports unconditional attendance rates at any four-year college, with and without controls.

Table 4: Waitlist Design: Balance Test

	Waitlist Non-Admits		Waitlist Admits		Difference	Difference as % of Non-Admit SD	SE	P-Value
	Mean	SD	Mean	SD				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Outcomes								
Placebo Predicted Top 1% at 33 based on Age 25 Employer	12.84	3.56	12.80	3.39	-0.05	-1.32%	0.13	0.72
Placebo Predicted Income Rank at 33 based on Age 25 Employer	78.24	1.77	78.15	1.74	-0.09	-4.86%	0.07	0.19
Placebo % Attending Graduate School at Age 28	10.86	4.52	11.02	4.37	0.16	3.44%	0.22	0.48
Placebo % Attending Graduate School at Age 28	10.86	4.52	11.02	4.37	0.16	3.44%	0.22	0.48
Demographics								
% Female	52.09	49.96	52.08	49.84	-0.01	-0.02%	1.46	0.99
% Underrepresented Minority	13.06	33.69	14.09	34.92	1.03	3.06%	0.94	0.27
% First-Generation College Student	9.47	29.24	8.45	28.99	-1.01	-3.46%	0.87	0.24
Academic Credentials								
Test Score	1450	107	1443	111	-7.75	-7.22%	3.05	0.01
High School GPA	3.86	0.18	3.84	0.18	-0.02	-9.84%	0.01	0.13
Teacher Rating (Standardized)	-2.47	0.58	-2.41	0.53	0.06	9.62%	0.03	0.10
Academic Rating (Standardized)	76.88	42.16	69.05	46.32	-7.83	-18.57%	2.93	0.01
Nonacademic Rating (Standardized)	43.53	49.58	78.57	41.11	35.04	70.66%	2.62	0.00
High School Quality and College Applications								
Predicted Top 1% at 33 based on High School Fixed Effect on Admissions	13.88	7.79	14.27	7.12	0.38	4.93%	0.30	0.21
Number of Scoresends	10.66	4.27	10.71	4.12	0.05	1.24%	0.16	0.73
Parent Income and Legacy Status								
Parent Income Percentile 90-95	15.20	35.91	15.39	35.97	0.19	0.53%	1.06	0.86
Parent Income Percentile 95-99	25.62	43.66	26.70	44.11	1.07	2.45%	1.29	0.40
Parent Income in Top 1%	12.03	32.54	18.49	38.64	6.46	19.86%	1.10	0.00
% Legacy	6.14	24.02	14.04	34.65	7.90	32.87%	1.02	0.00

Notes: This table displays comparisons between students who were admitted vs. not admitted in our waitlist sample. Within a pooled dataset including all students offered a place on the waitlist at each of our selected Ivy-Plus schools, we regress each covariate on an indicator for admission, including fixed effects for the exact set of schools on whose waitlist a student applies. We weight applicants such that the number of matriculants is the same for each school in our sample and cluster standard errors at the applicant level.

Table 5: Effects of Ivy-Plus College Attendance on Post-College Outcomes

	Treatment Effect of Attending Ivy-Plus Relative to Public Flagship			Observed Means For Highly Selective State Flagship Attendees	Implied Means Had Ivy-Plus Students Attended State Flagships	Observed Means For Ivy-Plus Attendees	Percentage Gain from Attending Ivy-Plus
	Rescaled Waitlist Admissions Design	Matriculation Design	Observational VA Estimate				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Treatment Effect on Income							
Predicted Top 1% Probability	4.63	4.07	5.38	8.08	10.41	15.03	44%
	(1.18)	(0.43)	(0.01)				
Predicted Top 10% Probability	3.87	4.33	4.71	41.79	47.25	51.12	8%
	(1.75)	(0.95)	(0.01)				
Predicted Top 25% Probability	1.89	2.77	2.77	65.14	69.12	71.01	3%
	(1.28)	(0.81)	(0.01)				
Predicted Mean Income Rank	1.38	1.38	1.51	76.00	77.65	79.02	2%
	(0.72)	(0.48)	(0.00)				
Panel B: Treatment Effect on Non-Monetary Outcomes							
Attend Elite Graduate School at Age 28	5.56	6.19	8.85	2.65	6.17	11.73	90%
	(2.75)	(2.12)	(0.01)				
Attend Non-Elite Graduate School at Age 28	-0.04	2.98	-0.06	13.22	13.85	13.81	-0.3%
	(0.02)	(4.04)	(0.01)				
Work at Elite Firm at Age 25	17.59	13.75	23.64	3.75	7.91	25.49	222%
	(4.02)	(1.02)	(0.03)				
Work at Prestigious Firm at Age 25	15.54	12.57	22.42	3.88	8.97	24.51	173%
	(4.19)	(1.05)	(0.02)				

Notes: This table presents regression estimates of the causal effects of attending an Ivy-Plus college relative to the mean highly selective flagship public school. The first column shows the treatment effect calculated using a rescaling of the waitlist design treatment effect, following the approach in Figure 16. In the second column, we present estimates of the causal effects of colleges based on the matriculation design, following the approach in Figure 15b. The third column shows the difference in traditional value-added estimates between Ivy-Plus schools and the most selective public flagship schools. In each row, Column 4 and 6 shows the observed means for highly selective flagship public and Ivy-Plus attendees, respectively. Column 5 shows the implied mean counterfactual should Ivy-Plus students attend one of the highly selective public flagship school, calculated by subtracting the causal estimates in Column 1 from Column 6. Column 7 reports the percentage change of means between Ivy-Plus and flagship public schools outcome. In Panel A, the dependent variables are applicant's predicted likelihood of reaching the top 1%, top 10%, and top 25% based on their firm at age 25 and predicted income rank based on their firm at age 25 (see notes to Figure 11 for more details). In Panel B, the dependent variable is a dummy variable for attending a highly selective graduate school at age 28, a non-selective graduate school at age 28, working at elite firm at age 25, and working at prestigious firm at age 25 (see notes to Figure 14 for more details). Statistics in this table are constructed directly from the individual-level microdata. Standard errors are reported in parentheses. See Section 2 of the paper for more details on the data sources.

Table 6: Effect of Changes in Admissions Policies at Selected Ivy-Plus Colleges

	Parent Income Percentile						
	0-60 <$68,000	60-95 $68,000-$239,000	95-99 $239,000-$611,000	Top 1% >$611,000	Predicted Top 1% Income	Share Working at Prestigious Firm	Share Attending Elite Graduate School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Observed Data	15.7%	42.6%	25.9%	15.8%	16.8%	33.3%	15.2%
Policy Change							
Remove Legacy Advantage	16.6%	44.5%	25.2%	13.7%	16.9%	33.4%	15.0%
Remove Legacy and Ratings Advantage	17.8%	47.1%	24.0%	11.2%	16.9%	33.4%	14.8%
Additionally Equalize Athlete Shares	20.0%	47.0%	23.0%	10.0%	17.1%	34.9%	15.9%
Implement Need-Affirmative Preferences for Students with High Academic Ratings	20.0%	47.0%	21.4%	11.7%	17.4%	36.2%	16.6%
Benchmark: Equal Attendance Rates Conditional on SAT Scores	20.2%	51.8%	20.8%	7.2%			

Notes: This table reports the distribution of parent income and average expected outcomes for attendees of Ivy-Plus colleges under different admissions policy scenarios. See Section 6 for details of how these counterfactuals are constructed.

Appendix Table 1: List of Colleges by Group

A. Ivy-Plus Colleges

No	Name	Location
1	Brown University	Providence, RI
2	Columbia University	New York, NY
3	Cornell University	Ithaca, NY
4	Dartmouth College	Hanover, NH
5	Duke University	Durham, NC
6	Harvard University	Cambridge, MA
7	Massachusetts Institute of Technology	Cambridge, MA
8	Princeton University	Princeton, NJ
9	Stanford University	Stanford, CA
10	University of Chicago	Chicago, IL
11	University of Pennsylvania	Philadelphia, PA
12	Yale University	New Haven, CT

B. Other Highly Selective Private Colleges

No	Name	Location
1	California Institute of Technology	Pasadena, CA
2	Carnegie Mellon University	Pittsburgh, PA
3	Emory University	Atlanta, GA
4	Georgetown University	Washington, DC
5	Johns Hopkins University	Baltimore, MD
6	New York University	New York, NY
7	Northwestern University	Evanston, IL
8	Rice University	Houston, TX
9	University of Notre Dame	Notre Dame, IN
10	University of Southern California	Los Angeles, CA
11	Vanderbilt University	Nashville, TN
12	Washington University in St. Louis	St. Louis, MO

C. Highly Selective Public Flagship Colleges

No	Name	Location
1	The Ohio State University	Columbus, OH
2	University of California, Berkeley	Berkeley, CA
3	University of California, Los Angeles	Los Angeles, CA
4	University of Florida	Gainesville, FL
5	University of Georgia	Athens, GA
6	University of Michigan - Ann Arbor	Ann Arbor, MI
7	University of North Carolina at Chapel Hill	Chapel Hill, NC
8	University of Texas at Austin	Austin, TX
9	University of Virginia	Charlottesville, VA

Notes: This table lists the specific institutions within three particular subsamples that we analyze at various points in the paper.

Appendix Table 2: Summary Statistics by College Type, Conditional on Attendance

	Ivy-Plus			Public Flagship			Other Selective Private	
	Pipeline Analysis (1)	Long Term Outcomes (2)	College-Specific Sample (3)	Pipeline Analysis (4)	Long Term Outcomes (5)	College-Specific Sample (6)	Pipeline Analysis (7)	Long Term Outcomes (8)
A: Standardized Test Scores								
Test Score	1405	1386	1426	1211	1185	1279	1330	1299
Number of Scoresends	8.42	7.75	8.23	6.10	6.05	6.88	8.43	7.83
B: Children Demographics								
Year of Birth	1994	1985	1989	1994	1985	1989	1994	1985
Mean Age at Matriculation	18	18	18	18	18	18	18	18
% Female	48.6%	49.0%	51.3%	54.1%	54.5%	43.5%	51.1%	52.3%
% White	50.3%	56.0%	53.9%	59.5%	61.7%	49.9%	53.9%	58.6%
% Black	7.1%	6.5%	8.5%	5.2%	5.6%	5.0%	5.7%	5.0%
% Hispanic	10.0%	6.6%	8.7%	10.5%	7.0%	9.4%	9.3%	6.5%
% Asian	19.2%	15.6%	17.2%	14.9%	14.1%	27.7%	18.5%	15.2%
% American Indian/ Native American	0.7%	0.6%	1.8%	0.3%	0.3%	0.6%	0.2%	0.3%
% Native Hawaiian/ Pacific Islander	0.1%	0.0%	0.1%	0.1%	0.0%	0.0%	0.1%	0.0%
% Unknown Race	12.6%	14.6%	9.9%	9.4%	11.2%	7.4%	12.3%	14.5%
C: Parents' Incomes								
Median Parent Household Income	$184,356	$177,990	$183,366	$125,610	$122,355	$131,277	$181,475	$166,772
Mean Parent Income Rank	82.2	81.2	80.8	74.9	73.4	74.2	81.1	80.1
D: Post-College Outcomes								
Median Income at Age 33	-	$107,974	$90,014	-	$70,949	$76,887	-	$89,155
Mean Income Rank at Age 33	-	83.5	81.4	-	77.0	78.7	-	80.5
% in Top 1% at Age 33	-	21.7	18.5	-	9.9	11.6	-	15.2
Predicted Top 1% at 33 based on Age 25 Employer	15.0%	13.7%	13.2%	8.1%	6.9%	8.7%	11.2%	9.9%
Predicted Income Rank at Age 33	79.1	78.5	77.8	76.0	75.1	76.3	77.7	76.9
% Attending Graduate School at Age 28	23.7%	26.2%	27.0%	15.1%	16.2%	17.8%	18.2%	19.7%
% Attending an Elite Graduate School at Age 28	10.8%	11.9%	12.3%	2.5%	2.7%	4.1%	4.6%	4.7%
% Working at an Elite Firm	25.5%	25.7%	30.4%	3.8%	3.6%	5.0%	12.9%	12.6%
% Working at a Prestigious Firm	24.5%	26.0%	30.9%	3.9%	4.0%	5.3%	13.4%	14.1%
Number of Children	37,352	89,785	41,212	123,548	255,705	387,835	45,047	94,548

Notes: The table replicates Table 1 but for subsets of students who attend particular groups of schools (as defined in Appendix Table 1). Columns 1-3 replicate Columns 1-3 of Table 1 for students attending Ivy-Plus schools; Columns 4-6 replicate Columns 1, 2, and 4 of Table 1 for students attending highly selective public flagship schools, and Column 7 and 8 replicate Column 1 and 2 of Table 1 for students attending other highly selective private schools.

Appendix Table 3: Ivy-Plus College Attendance and Test Score Distribution by Family Income

	Parent Income Percentile (National Distribution Parent Income Bins)													Share of Ivy-Plus Attendees
	0-20	20-40	40-60	60-70	70-80	80-90	90-95	95-96	96-97	97-98	98-99	99-99.9	Top 0.1%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Parental Income Distribution of Ivy-Plus Students														
P(Parent Income in Given Range | Ivy Plus Students)														
Share of Ivy-Plus Students	3.2%	4.7%	8.2%	6.0%	8.4%	14.9%	13.5%	4.1%	5.2%	7.1%	9.1%	12.8%	2.9%	100.0%
Ivy Plus Attendance Shares Relative to Bin Size	0.1%	0.1%	0.2%	0.2%	0.3%	0.6%	1.0%	1.6%	2.0%	2.7%	3.5%	5.4%	11.9%	100.0%
Panel B: Parental Income Distribution of Highly Selective Public Flagship Students;														
P(Parent Income in Given Range | Highly Selective Flagship Public Students)														
Share of Highly Selective Public Flagship Students	4.8%	7.4%	11.7%	8.4%	11.5%	18.3%	14.7%	3.9%	4.3%	4.8%	5.2%	4.9%	0.5%	0.0%
Highly Selective Public Flagship Attendance Shares Relative to Bin Size	0.3%	0.5%	0.8%	1.1%	1.4%	2.3%	3.6%	4.9%	5.3%	6.0%	6.3%	6.7%	6.0%	0.0%
Panel C: Distribution of Test Scores Conditional on Parent Income;														
P(Test Score in Given Range | Parent Income Group)														
1500-1600 (or ACT of 34 - 36)	0.0%	0.1%	0.1%	0.3%	0.4%	0.8%	1.7%	2.5%	2.8%	3.4%	3.9%	4.6%	6.8%	32.6%
1400-1490 (or ACT of 32 - 33)	0.1%	0.2%	0.4%	0.8%	1.3%	2.2%	3.8%	5.0%	5.6%	6.4%	7.3%	8.6%	11.6%	27.0%
1300-1390 (or ACT of 29 - 31)	0.4%	0.7%	1.4%	2.4%	3.6%	5.6%	8.3%	10.5%	11.3%	12.1%	13.1%	14.3%	14.7%	20.9%
1200-1290 (or ACT of 27 - 28)	0.8%	1.2%	2.4%	4.0%	5.7%	8.3%	11.4%	13.2%	14.0%	14.5%	15.2%	15.6%	14.7%	11.1%
1100-1190 (or ACT of 24 - 26)	1.8%	2.8%	5.1%	8.1%	10.7%	14.1%	17.1%	18.2%	18.5%	18.8%	19.1%	18.4%	16.2%	6.0%
1000-1090 (or ACT of 22 - 23)	2.6%	3.9%	6.5%	9.4%	11.6%	13.9%	15.2%	15.1%	14.8%	14.4%	13.6%	12.7%	10.6%	1.7%
900-990 (or ACT of 19 - 21)	4.5%	6.7%	9.6%	12.3%	14.0%	14.7%	14.0%	12.8%	12.2%	11.4%	10.6%	9.6%	8.3%	0.6%
800-890 (or ACT of 17 - 18)	4.8%	6.7%	8.1%	8.8%	9.0%	8.6%	7.3%	6.3%	5.7%	5.3%	4.7%	4.2%	3.8%	0.1%
700-790 (or ACT of 15 - 16)	4.5%	5.9%	5.8%	5.3%	4.8%	4.1%	3.0%	2.4%	2.2%	2.0%	1.8%	1.5%	1.3%	0.0%
600-690 (or ACT of 13 - 14)	2.9%	3.5%	2.9%	2.3%	1.8%	1.4%	1.0%	0.7%	0.7%	0.6%	0.5%	0.5%	0.4%	0.0%
below 600 (or ACT below 12)	1.7%	1.9%	1.4%	0.9%	0.7%	0.5%	0.3%	0.2%	0.2%	0.2%	0.2%	0.2%	0.2%	0.0%
Did not take SAT or ACT	75.9%	66.4%	56.3%	45.6%	36.4%	25.8%	16.9%	13.2%	12.2%	11.1%	10.3%	10.3%	12.1%	0.0%
Total	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%	100.0%

Notes: This table presents distributional statistics for Ivy-Plus college attendance and SAT/ACT scores by parental income. The data include all SAT/ACT test takers in years 2010-2015. Parent income percentile is defined as the position of the test-takers family income in the national distribution as estimated in tax data. The first row of Panel A presents the share of all Ivy-Plus college students coming from each parent income percentile. The second row, "Attendance Rate Relative to Bin Size," divides the attendance share in the first row by the size of each bin (e.g. divide by 60 for 0-60, divide by 10 for 60-70, etc.). The first row of Panel B presents the share of all highly selective public flagship students coming from each parent income percentile, and the second row divides the attendance share in the first row by the size of each bin. Panel C presents the distribution of SAT scores (or ACT score equivalents) by parent income percentile, including the fraction of students in each parent-income bin who do not take the test. The last column of this table present the share of Ivy-Plus students by test score group.

Appendix Table 4: Additional Students Admitted from Top 1% at Selected Ivy-Plus Colleges: Simultaneous Decomposition Analysis

		Total	Subtotal	Share of Excess Top 1% Students
[1]	Class Size	1650		
[2]	Total Students with Parent Income in Top 1%	261		
[3]	Total Excess Students with Parent Income in Top 1%	157		100.0%
[4]	Attributable to Application Rates	46		29.1%
[5]	Attributable to Admission Rates	90		57.5%
[6]	Legacy		39	25.0%
[7]	Ratings		26	16.4%
[8]	Athletes		25	16.0%
[9]	Attributable to Matriculation Rates	21		13.4%

Notes: This table replicates Table 2, except that we proportionally allocate students across the stages of the pipeline based on the ratio of the log-point difference in attendance rates from each stage (application, admission, and matriculation) and the total log-point difference (in contrast with Table 2 which stacks the stages in a particular order). See Table 2 for more details.

Appendix Table 5: List of Examples for High Schools by Type

Area	Private		Public	
	Non-Religious	*Religious*	*Advantaged*	*Non-Advantaged*
New York	Horace Mann School	Fordham Preparatory School	Scarsdale High School	Forest Hills High School
Boston	Milton Academy	Boston College High School	Newton South High School	Somerville High School
Atlanta	Pace Academy	Westminster School	Northview High School	Midtown High School
Chicago	Lake Forest Academy	St. Ignatius College Prep	New Trier High School	Crete-Monee High School
Los Angeles	Harvard-Westlake School	Loyola High School	Palos Verdes High School	Pasadena High School
San Fransisco Bay	Harker School	Archbishop Mitty High School	Palo Alto High School	George Washington High School
Houston	Kinkaid School	Strake Jesuit College Preparatory	Clements High School	Jersey Village High School
Miami	Pine Crest School	Belen Jesuit Preparatory School	Cypress Bay High School	Southwest Miami Senior High School
Phoenix	Brophy College Preparatory	Northwest Christian Academy	Desert Vista High School	Mountain View High School
Philadelphia	Germantown Friends School	Archbishop Ryan High School	North Penn High School	Coatesville High School
Washington DC	Sidwell Friends School	National Cathedral School	Thomas Jefferson High School	Springbrook High School
Dallas-Ft Worth	The Hockaday School	St. Marks School of Texas	Carroll High School	Rowlett High School

Notes: This table presents examples of high schools from each of the four categories in Figure 9a within twelve large metropolitan areas in the US. The schools in these examples were not chosen based on their presence in any of our datasets or based on their actual estimated fixed effects.

Appendix Table 6: Waitlist Design Treatment Effect Estimates for Pooled and Non-Advantaged Samples

	Raw Means		With Controls		Observational VA Estimate	
	Pooled	Non-Advantaged	Pooled	Non-Advantaged	Pooled	Non-Advantaged
	(1)	(2)	(3)	(4)	(5)	(6)
Predicted Top 1% Probability	2.46	2.12	2.58	2.38	2.86	2.95
	(0.63)	(0.73)	(0.62)	(0.72)	(0.06)	(0.08)
Predicted Top 10% Probability	1.83	1.42	2.23	1.84	2.23	2.29
	(0.83)	(0.99)	(0.82)	(0.96)	(0.09)	(0.12)
Predicted Top 25% Probability	0.92	0.37	1.16	0.54	1.34	1.37
	(0.62)	(0.76)	(0.62)	(0.75)	(0.06)	(0.08)
Attend Elite Graduate School at Age 28	3.17	3.84	3.23	4.57	5.04	5.22
	(1.57)	(1.92)	(1.59)	(1.95)	(0.10)	(0.12)
Attend Non-Elite Graduate School at Age 28	1.31	-0.01	1.18	-0.37	-0.89	-0.87
	(1.70)	(1.97)	(1.71)	(1.97)	(0.08)	(0.10)
Work at Elite Firm at Age 25	9.44	9.88	8.71	9.25	12.69	12.94
	(2.16)	(2.55)	(2.18)	(2.57)	(0.25)	(0.30)
Work at Prestigious Firm at Age 25	7.92	9.56	6.93	8.46	11.43	11.68
	(2.14)	(2.56)	(2.17)	(2.58)	(0.19)	(0.23)

Notes: This table presents estimates of the causal effect of attendance at Ivy-Plus schools using the waitlist design. The estimates in Columns 1, 3, and 4 match the estimates in Figures 11 and 14. The estimates in Columns 2, 4, and 6 replicate estimates in column 1, 3, and 5 respectively except excluding legacy applicants, recruited athletes, and students from the top 1% of parental income. Column 5 and 6 estimates the raw treatment effect on the quality of college attended, as measured by a value-added model on the panel-specific outcome. For each outcome, we calculate value-added as the fixed effect of each college in a regression, controlling for test scores, parental income bin, race, gender and state.

Appendix Table 7: Heterogeneity in Causal Effects of Ivy-Plus Admission on Predicted Top 1% Probability

	Effects on Predicted Top 1% Probability		
	Waitlist Design	Matriculation Design	Observational VA Estimate
	(1)	(2)	(3)
Panel A: Pooled Sample Estimate	2.46	4.07	2.86
	(0.63)	(0.43)	(0.00)
Panel B: Heterogeneity by Parental Income			
P0-P60	4.37	5.79	3.66
	(2.63)	(0.98)	(0.17)
P60-P95	3.68	6.57	3.84
	(1.26)	(0.72)	(0.10)
P95-P99	1.29	5.51	2.98
	(1.42)	(1.52)	(0.10)
Top 1%	5.44	6.51	1.97
	(2.74)	(6.64)	(0.26)
P-Value	0.29	0.98	
Panel C: Heterogeneity by Test Score			
< 1300	2.57	3.77	4.16
	(1.60)	(0.87)	(0.23)
1300-1400	2.01	6.98	3.53
	(1.10)	(1.58)	(0.13)
1400-1500	1.59	5.64	3.17
	(1.01)	(4.69)	(0.09)
1500-1600	5.00	12.09	2.46
	(1.40)	(12.41)	(0.09)
P-Value	0.18	0.92	
Panel D: Heterogeneity by Academic Rating			
High Academic Rating	4.15		3.49
	(1.53)		(0.10)
Low Academic Rating	4.39		3.75
	(5.52)		(0.14)
P-Value	0.97		
Panel E: Heterogeneity by Athlete Status			
Athlete	10.43		3.14
	(10.67)		(0.79)
Non-Athlete	2.88		3.37
	(0.80)		(0.06)
P-Value	0.48		
Panel F: Heterogeneity by Non-Academic Rating			
High Non-Academic Rating	2.08		3.38
	(1.52)		(0.08)
Low Non-Academic Rating	5.31		3.48
	(2.83)		(0.12)
P-Value	0.31		
Panel G: Heterogeneity by Legacy Status			
Legacy	5.30		4.11
	(1.95)		(0.19)
Non-Legacy	2.67		3.32
	(0.88)		(0.07)
P-Value	0.22		

Notes: This table presents estimates of the causal effects of attending an Ivy-Plus college on an applicant's predicted likelihood of reaching the top 1% based on their firm at age 25, separately by student characteristics. Column 1 presents estimates calculated using the waitlist design, following the approach in Figure 16. Column 2 presents estimates calculated using the matriculation design. In Column 3, we present estimates of the difference in the observational value-added measures of the colleges attended by waitlisted admits vs. waitlist rejects. Panel A displays pooled sample estimates, while Panels B through G display estimates calculated among applicants with different observable characteristics. We report p-values for the null hypothesis of homogeneous treatment effects across each group of characteristics.

Appendix Table 8: Robustness in Outside Option Heterogeneity in Waitlist Admission Effects Design

	Predicted Top 1%						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mean Predicted Top 1% Value-Added of Outside Options	-0.86	-0.94	-0.73	-0.87	-0.97	-0.95	-0.84
	(0.23)	(0.37)	(0.33)	(0.25)	(0.32)	(0.25)	(0.24)
Implied Ivy-Plus minus Elite Public Treatment Effect (pp)	4.45	4.85	3.79	4.50	5.05	4.91	4.33
	(1.20)	(1.93)	(1.72)	(1.29)	(1.65)	(1.32)	(1.24)
Grouping Instrument Construction	Baseline: Homestate, Race, Income School Applied	Baseline with Jackknife	CZ Only	Flexible Regression	Constructed on Regular Reject Sample	Dropping Multi-Campus Group	Without School Fixed Effect

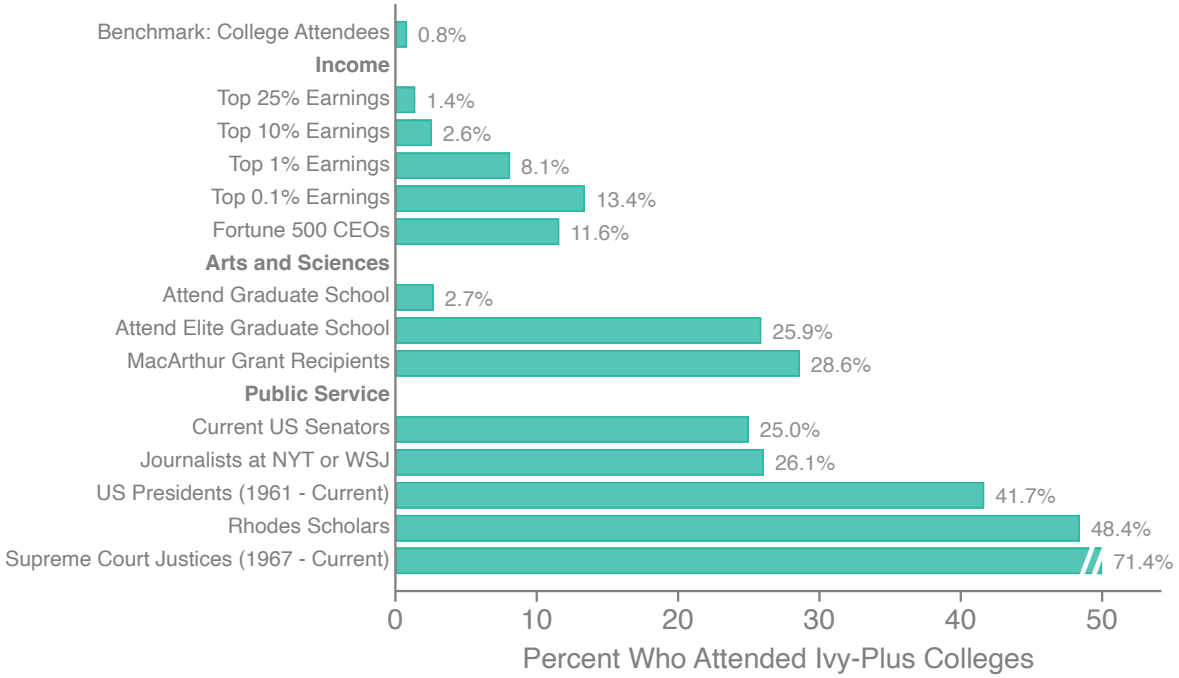
Notes: This table presents estimates of the causal effect of attending an Ivy-Plus college, relative to attending a highly selective public flagship school, following the approach in Figure 13. Column 1 replicates exactly the specification from Figure 13. Columns 2-7 present alternative estimates using different approaches to estimating a student's outside option. In Column 2, we leave out the own student when calculating average value-added of the outside option among waitlist non-admits. In Column 3, we group students only on the commuting zone (CZ) of residence, as measured in the tax data. Column 4 estimates the outside option using a flexible regression of controls (controls included in this regression are schools attended by waitlist rejects interacted with school year, parent income bin, race, dummies for test scores, homestate, and gender) instead of the means within fully interacted bins. Column 5 estimates the outside options using the approach in Column 1 but in the pool of rejected applicants not offered a place on the waitlist. Column 6 omits large multi-campus groups for which we cannot estimate school-specific value-added of outside options (see Chetty et al. 2020 for more details on this issue). Column 7 drops the fixed effect for the school on which a student is on the waitlist.

Appendix Table 9: Differences in Post-College Outcomes Among Ivy-Plus Admits and Waitlist Rejects

	No Controls		Controlling for Test Score		Controlling for Parent Income Bin		Controlling for Test Score Gender, and Race		Controlling for All Observables Model Vars		Controlling for All Observables Var & Parent Income	
	Difference	Controlling for VA	Difference	Controlling for VA	Difference	Controlling for VA	Difference	Controlling for VA	Difference	Controlling for VA	Difference	Controlling for VA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Differences in Predicted Top 1% Probability												
High Academic Rating vs. Low Academic Rating	4.42	3.69	1.06	0.75	4.21	3.41	0.93	0.54	1.72	1.17	1.71	1.14
	(0.24)	(0.23)	(0.33)	(0.33)	(0.24)	(0.24)	(0.33)	(0.33)	(0.38)	(0.38)	(0.38)	(0.39)
Athlete vs. Non-Athlete	0.34	-0.67	2.95	1.45	0.02	-0.96	2.90	1.10	1.93	0.48	2.01	0.53
	(0.29)	(0.29)	(0.30)	(0.30)	(0.29)	(0.29)	(0.30)	(0.30)	(0.35)	(0.35)	(0.35)	(0.35)
Legacy vs. Non-Legacy	-0.72	-0.81	-0.50	-0.62	-1.95	-1.85	-0.08	-0.38	-0.86	-1.04	-1.23	-1.34
	(0.23)	(0.23)	(0.23)	(0.23)	(0.24)	(0.24)	(0.23)	(0.23)	(0.25)	(0.25)	(0.26)	(0.26)
High Non-Academic Rating vs. Low Non-Academic Rating	0.19	-0.49	0.85	0.07	0.18	-0.48	1.11	0.31	0.47	0.08	0.47	0.08
	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.32)	(0.32)	(0.32)	(0.32)
Second Quartile of SAT Distribution vs. First Quartile	2.16	1.71	2.16	1.71	1.95	1.47	1.97	1.28	0.98	0.55	0.92	0.49
	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.20)	(0.20)	(0.20)	(0.20)
Third Quartile of SAT Distribution vs. First Quartile	4.21	3.42	4.21	3.42	3.89	3.07	3.74	2.62	2.04	1.34	1.95	1.25
	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.19)	(0.19)	(0.23)	(0.23)	(0.23)	(0.23)
Fourth Quartile of SAT Distribution vs. First Quartile	6.29	5.16	6.29	5.16	5.98	4.80	5.40	3.92	2.99	2.10	2.92	2.03
	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.20)	(0.20)	(0.26)	(0.26)	(0.26)	(0.26)
Panel B: Differences in Elite Graduate School Attendance at Age 25												
High Academic Rating vs. Low Academic Rating	7.48	7.30	4.30	4.18	6.96	6.74	4.57	4.44	4.46	4.33	4.40	4.26
	(0.52)	(0.53)	(0.72)	(0.73)	(0.53)	(0.53)	(0.72)	(0.73)	(0.86)	(0.87)	(0.86)	(0.87)
Athlete vs. Non-Athlete	-8.35	-8.73	-3.92	-4.38	-8.76	-9.14	-2.41	-2.93	-2.13	-2.53	-2.18	-2.60
	(0.47)	(0.47)	(0.51)	(0.51)	(0.48)	(0.48)	(0.53)	(0.53)	(0.69)	(0.70)	(0.70)	(0.70)
Legacy vs. Non-Legacy	-2.26	-2.37	-1.90	-2.02	-3.56	-3.65	-1.16	-1.32	-0.91	-1.10	-1.42	-1.61
	(0.52)	(0.53)	(0.52)	(0.52)	(0.54)	(0.54)	(0.52)	(0.53)	(0.60)	(0.61)	(0.60)	(0.61)
High Non-Academic Rating vs. Low Non-Academic Rating	0.14	-0.08	1.09	0.84	0.25	0.04	0.97	0.72	-0.50	-0.61	-0.47	-0.57
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.72)	(0.73)	(0.72)	(0.73)
Second Quartile of SAT Distribution vs. First Quartile	3.42	3.44	3.42	3.44	2.94	2.95	4.41	4.38	2.41	2.39	2.23	2.21
	(0.40)	(0.41)	(0.40)	(0.41)	(0.41)	(0.41)	(0.41)	(0.42)	(0.47)	(0.48)	(0.47)	(0.48)
Third Quartile of SAT Distribution vs. First Quartile	6.47	6.40	6.47	6.40	5.81	5.72	7.83	7.68	4.40	4.29	4.14	4.03
	(0.42)	(0.43)	(0.42)	(0.43)	(0.43)	(0.44)	(0.44)	(0.45)	(0.54)	(0.54)	(0.54)	(0.55)
Fourth Quartile of SAT Distribution vs. First Quartile	10.40	10.18	10.40	10.18	9.62	9.38	11.85	11.55	7.11	6.87	6.84	6.59
	(0.44)	(0.45)	(0.44)	(0.45)	(0.46)	(0.46)	(0.47)	(0.48)	(0.61)	(0.62)	(0.61)	(0.62)
Panel C: Differences in Prestigious Firm Employment												
High Academic Rating vs. Low Academic Rating	7.91	6.66	4.12	3.51	7.41	5.90	4.22	3.42	3.44	2.05	3.51	2.06
	(0.86)	(0.85)	(1.19)	(1.18)	(0.88)	(0.87)	(1.20)	(1.18)	(1.49)	(1.50)	(1.50)	(1.50)
Athlete vs. Non-Athlete	-5.85	-10.31	-1.34	-6.57	-6.31	-10.73	-1.93	-7.69	-1.62	-6.19	-1.56	-6.21
	(0.91)	(0.91)	(0.95)	(0.95)	(0.91)	(0.91)	(0.97)	(0.97)	(1.18)	(1.18)	(1.18)	(1.18)
Legacy vs. Non-Legacy	0.74	-0.87	1.14	-0.54	-0.82	-2.15	0.49	-1.44	-2.22	-3.52	-2.28	-3.52
	(0.83)	(0.82)	(0.83)	(0.82)	(0.85)	(0.84)	(0.84)	(0.83)	(0.94)	(0.94)	(0.94)	(0.94)
High Non-Academic Rating vs. Low Non-Academic Rating	2.57	0.34	3.75	1.31	2.57	0.43	3.42	1.02	0.47	-0.69	0.45	-0.69
	(0.74)	(0.73)	(0.74)	(0.74)	(0.74)	(0.73)	(0.75)	(0.74)	(1.13)	(1.13)	(1.13)	(1.13)
Second Quartile of SAT Distribution vs. First Quartile	3.37	2.84	3.37	2.84	3.02	2.34	3.12	2.08	1.32	0.69	1.36	0.68
	(0.65)	(0.64)	(0.65)	(0.64)	(0.66)	(0.66)	(0.68)	(0.67)	(0.77)	(0.78)	(0.78)	(0.78)
Third Quartile of SAT Distribution vs. First Quartile	6.58	5.42	6.58	5.42	6.11	4.76	6.34	4.46	2.79	1.71	2.84	1.72
	(0.66)	(0.65)	(0.66)	(0.65)	(0.68)	(0.67)	(0.71)	(0.70)	(0.87)	(0.87)	(0.87)	(0.87)
Fourth Quartile of SAT Distribution vs. First Quartile	10.69	8.82	10.69	8.82	10.20	8.09	10.71	8.02	5.40	3.70	5.45	3.68
	(0.67)	(0.67)	(0.67)	(0.67)	(0.70)	(0.70)	(0.74)	(0.73)	(0.97)	(0.97)	(0.97)	(0.97)
Panel D: Differences in Elite Firm Employment												
High Academic Rating vs. Low Academic Rating	10.54	8.87	5.12	4.22	9.87	7.87	5.35	4.20	5.87	4.21	5.91	4.14
	(0.86)	(0.85)	(1.20)	(1.18)	(0.88)	(0.87)	(1.20)	(1.19)	(1.51)	(1.51)	(1.51)	(1.51)
Athlete vs. Non-Athlete	-4.04	-9.80	1.73	-5.10	-4.75	-10.45	1.22	-6.33	0.50	-5.43	0.59	-5.44
	(0.94)	(0.94)	(0.98)	(0.98)	(0.94)	(0.94)	(1.00)	(1.00)	(1.20)	(1.19)	(1.20)	(1.20)
Legacy vs. Non-Legacy	0.71	-1.25	1.17	-0.89	-1.86	-3.45	0.66	-1.74	-2.60	-4.27	-2.99	-4.58
	(0.84)	(0.83)	(0.84)	(0.82)	(0.86)	(0.84)	(0.85)	(0.83)	(0.94)	(0.94)	(0.95)	(0.94)
High Non-Academic Rating vs. Low Non-Academic Rating	4.15	0.98	5.69	2.23	4.12	1.05	5.39	1.98	2.12	0.24	2.09	0.25
	(0.75)	(0.74)	(0.75)	(0.74)	(0.75)	(0.74)	(0.76)	(0.75)	(1.15)	(1.14)	(1.15)	(1.15)
Second Quartile of SAT Distribution vs. First Quartile	4.93	4.15	4.93	4.15	4.35	3.38	4.83	3.36	1.95	1.15	1.92	1.06
	(0.64)	(0.64)	(0.64)	(0.64)	(0.66)	(0.65)	(0.67)	(0.67)	(0.77)	(0.77)	(0.77)	(0.77)
Third Quartile of SAT Distribution vs. First Quartile	8.85	7.27	8.85	7.27	8.06	6.24	8.79	6.25	3.75	2.39	3.72	2.29
	(0.66)	(0.65)	(0.66)	(0.65)	(0.68)	(0.67)	(0.71)	(0.70)	(0.86)	(0.86)	(0.87)	(0.87)
Fourth Quartile of SAT Distribution vs. First Quartile	13.52	10.98	13.52	10.98	12.73	9.87	13.67	10.03	6.39	4.23	6.37	4.12
	(0.67)	(0.67)	(0.67)	(0.67)	(0.70)	(0.69)	(0.74)	(0.73)	(0.97)	(0.97)	(0.97)	(0.97)

Notes: This table replicates estimates from Figure 17 with various outcomes, student characteristics, and additional controls. In each pair of columns, the first (odd-numbered) column presents differences in outcomes controlling for certain variables but without adjusting for the observational value-added of the colleges applicants attended (as in the left bar in each pair in Figure 17a). The second (even-numbered) column presents the estimate further adjusting for the value-added of college attended (as in Figures 17b-d). Across pairs of columns we change the set of other variables for which we control when estimating differences in outcomes predicted by the focal variable (labeled on the left of each row). Across panels we change the student outcome variable. See notes to Figure 17 and Section 5 for more details. Baseline rates of outcomes (i.e. for admitted or waitlist students with none of the characteristics in the table) are 10.47% (Panel A), 6.77% (Panel B), 19.23% (Panel C), and 17.37% (Panel D).

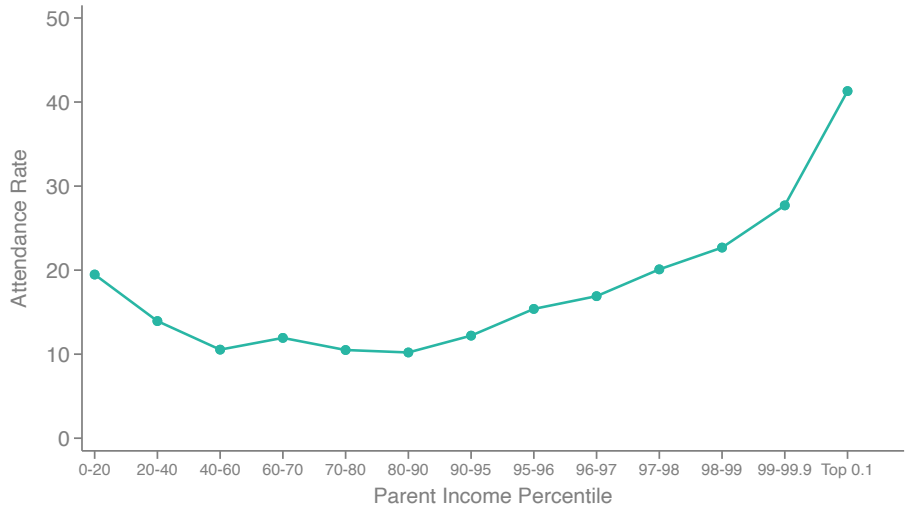
Figure 1: Share of Individuals in Leadership Positions who Attended Ivy-Plus Colleges



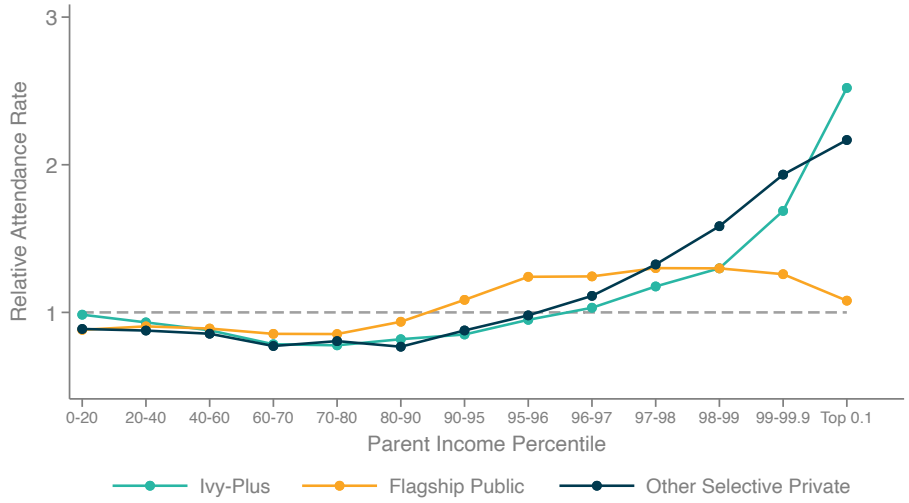
Notes: Figure 1 shows the proportion of individuals who attended an Ivy-Plus college as an undergraduate for different subsets of the population. The definition and source for each outcome are described in Appendix A.

Figure 2: Attendance Rates at Selective Colleges by Parental Income

(a) Ivy-Plus Attendance Rates for Students Scoring at 99th Percentile of SAT/ACT, by Parental Income



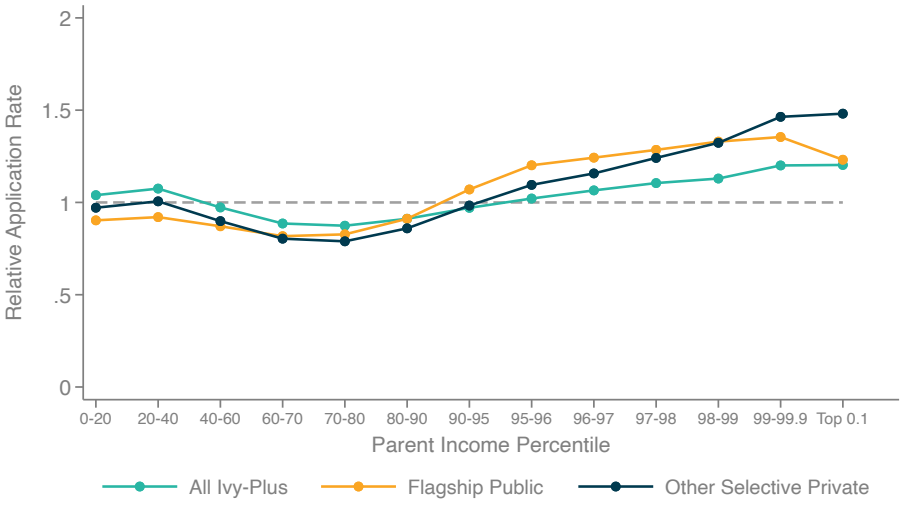
(b) Attendance Rates at Selective Colleges by Parental Income, Controlling for Test Scores



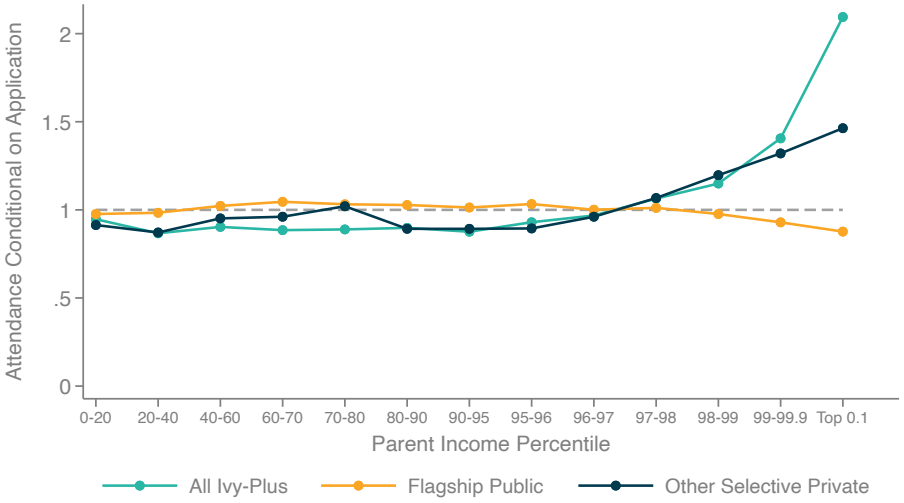
Notes: Figure 2a plots the Ivy-Plus attendance rate for students with exactly an SAT score of 1510 (out of 1600) or an ACT composite score of 34 (out of 36), by parental income. To construct the series for Ivy-Plus colleges in Figure 2b, we calculate the attendance rate at each Ivy-Plus college (separately) for students in each parent income bin and at each test score level. For each college and within each parent-income bin, we then then average together the attendance rates from different test score levels, where the weight on each test score level is the fraction of attending students at that specific college with that specific test score. This procedure reweights the distribution of test scores at each parent-income level to match the distribution of test scores for all attending students at each school. We then average together the twelve college-specific series, weighting by the number of attendees. Finally, we calculate the relative attendance rate by dividing this attendance rate by the mean test-score-reweighted average attendance rate (across students from all parent-income bins). Figure 2b plots this relative attendance rate series for the twelve Ivy-Plus colleges, as well as similarly constructed relative attendance rate series for the 12 other highly selective private schools and the 9 highly selective public flagship schools listed in Appendix Table 1. The sample for each panel is our pipeline analysis sample: the set of students who were on pace to graduate from high school and took either the SAT or the ACT in 2011, 2013, or 2015 and whom we can link to parent incomes in the tax data. See Section 2 for details on sample construction and variable definitions.

Figure 3: Application and Conditional Attendance Rates at Selective Colleges by Parental Income, Controlling for Test Scores

(a) Application Rates by Parental Income, Controlling for Test Scores



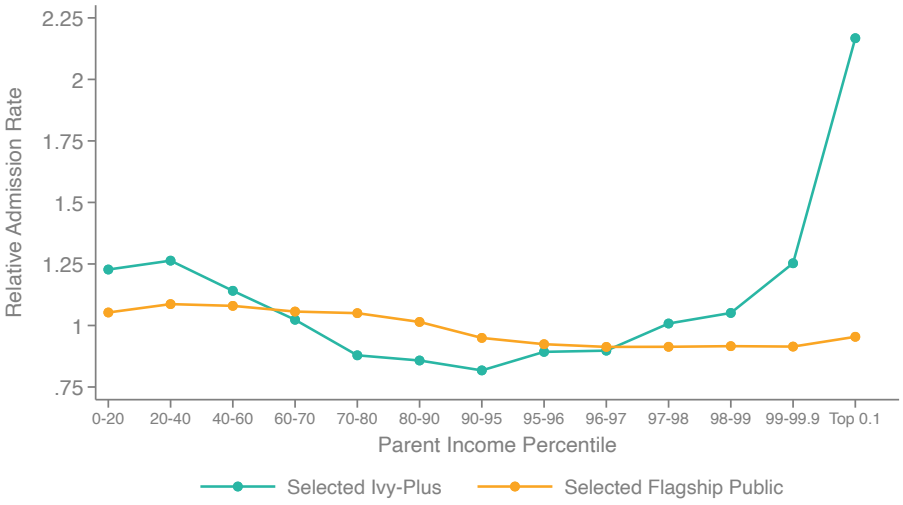
(b) Attendance Rates Conditional on Application by Parental Income, Controlling for Test Scores



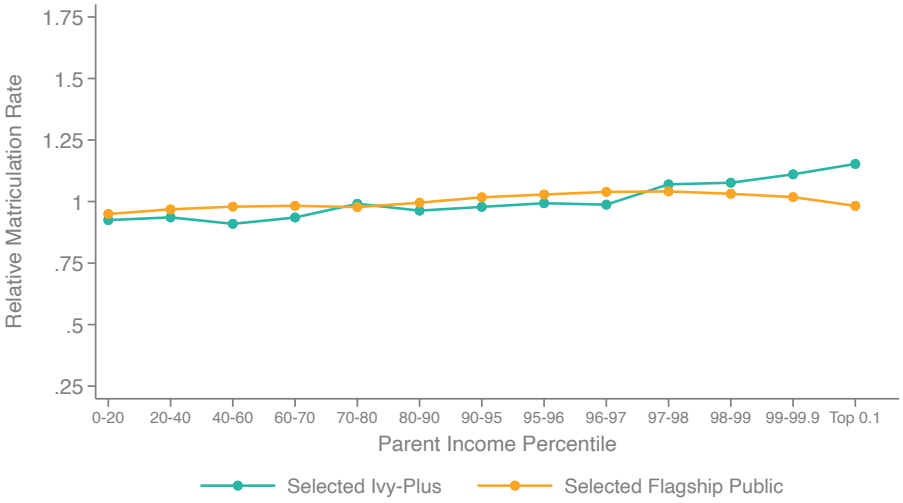
Notes: Figure 3a replicates Figure 2b but with application rates rather than attendance rates, where application rates are predicted using score sends as described in Appendix B. Figure 3b replicates Figure 2b but with attendance rates conditional on application, defined as the ratio of attendance rates to application rates.

Figure 4: Admissions and Matriculation Rates at Selected Private and Public Colleges, Controlling for Test Scores

(a) Admissions Rates by Parental Income



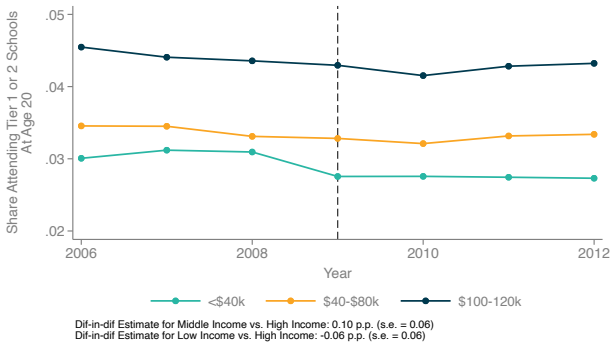
(b) Matriculation Rates by Parental Income



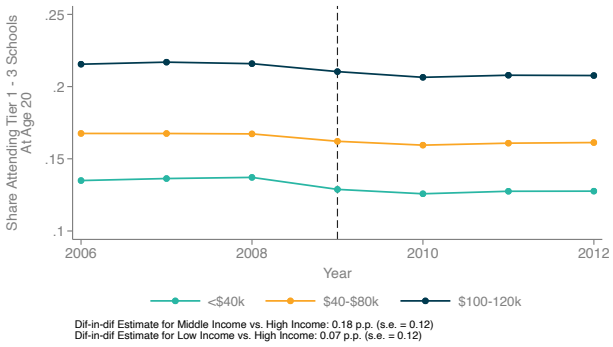
Notes: Figures 4a and 4b plot admissions and matriculation (or yield) rates by parental income (normalized by college-level means) by parental income. We reweight students within each parent income bin on test scores using the same method as that in Figure 2 (see notes to Figure 2 for details). The sample for these figures is our college-specific sample, a selected subset of Ivy-Plus and highly selective public flagship schools for which we have linked internal admissions data. See Section 2 for further details on this sample.

Figure 5: Evolution of College Attendance Rates Over Time, by Parental Income

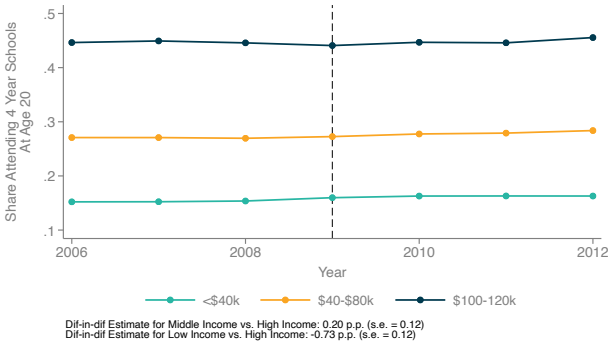
(a) Attendance Rates at Highly Selective Colleges, by Parental Income



(b) Attendance Rates at Selective Colleges



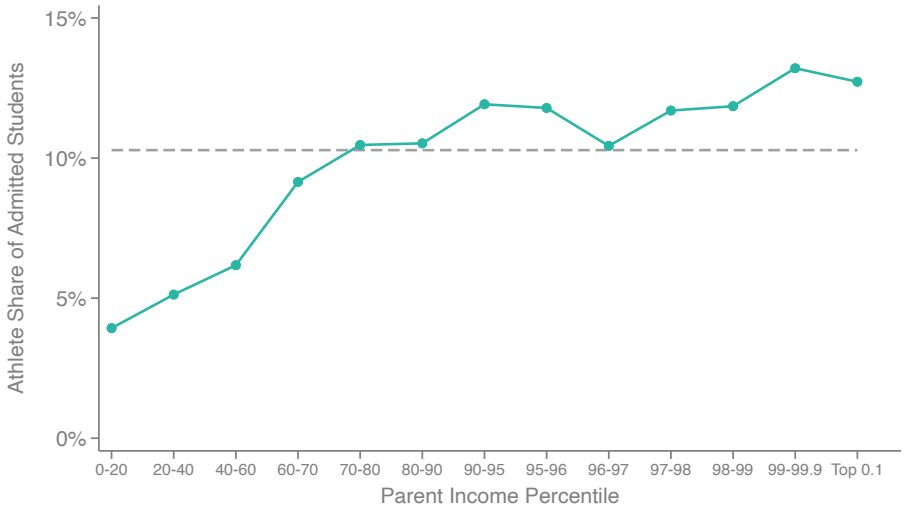
(c) Attendance Rates at Four-Year Colleges, by Parental Income



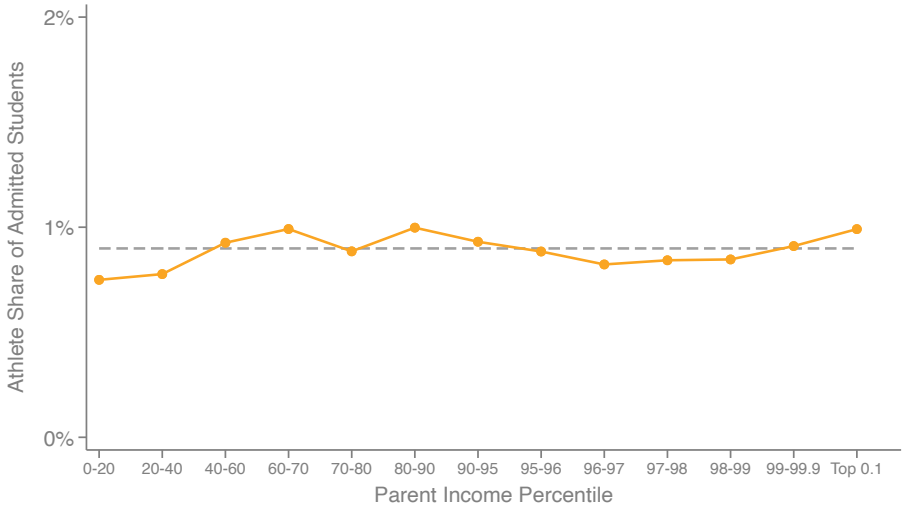
Notes: Figure 5 plots the fraction of standardized test takers who attend different types of four-year colleges by high school graduation cohort, disaggregated by parental income. Figure 5a reports attendance by parent income at Tier 1 and Tier 2 schools as classified by Barron's, which correspond to Ivy-Plus and other extremely selective schools. Figure 5b reports attendance rates at schools in Tiers 1–3, which additionally include highly selective public colleges. Figure 5c reports attendance rates at any four-year college. Each figure also reports a difference-in-difference estimate comparing post-2009 changes in college attendance for middle- and low-income children, each relative to that for high-income children.

Figure 6: Share of Students who are Recruited Athletes, by Parental Income

(a) Selected Ivy-Plus Colleges



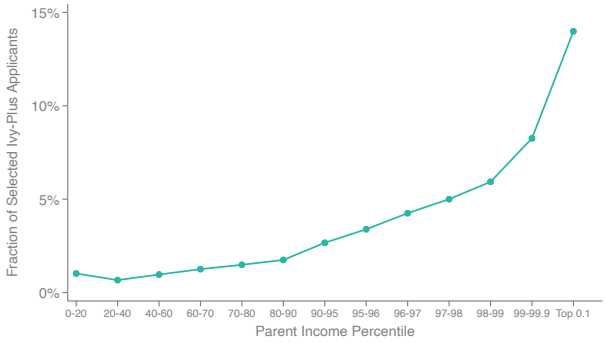
(b) Selected Flagship Public Colleges



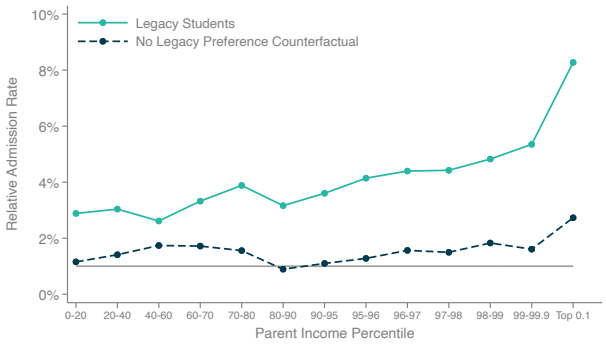
Notes: Figures 6a and 6b plot the fraction of admitted students who are recruited athletes by parent income bin at Ivy-Plus and highly selective public flagship schools, respectively. The sample for these figures is our college-specific sample, a selected subset of Ivy-Plus and highly selective public flagship schools for which we have linked internal admissions data.

Figure 7: Admissions Rates for Legacy Students, by Parental Income

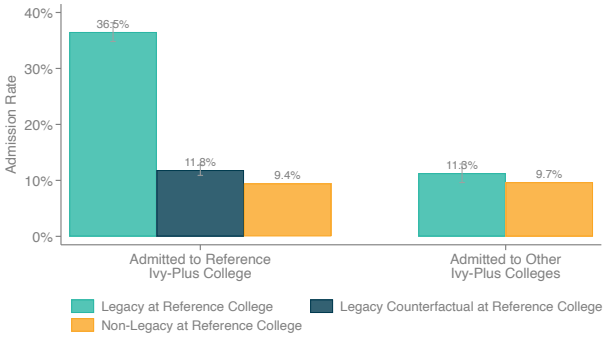
(a) Share of Applicants at Selected Ivy-Plus Colleges who are Legacies



(b) Admissions Rate at Selected Ivy-Plus Colleges for Legacy Applicants



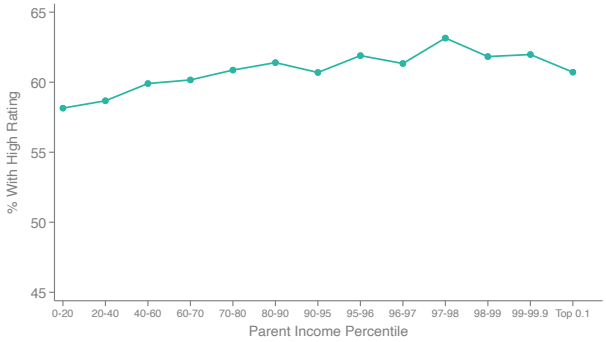
(c) Admission Rate by Legacy Status



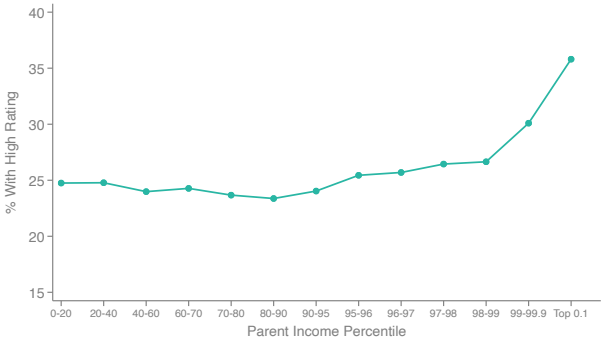
Notes: Figure 7a reports the share of non-athlete attendees of selected Ivy-Plus colleges who are children of alumni (i.e., legacy students), by parent income level. Figure 7b plots two series. The solid (teal) series plots the admissions rate of legacy students in each parent income bin, reweighting those students to match all attendees on test score (as in Figure 2) and normalizing by the average test-score-reweighted admissions rate of all students. The dashed (dark blue) series plots the same test-score reweighted and normalized average but of the counterfactual admissions rate for legacy students if they did not benefit from legacy preferences in admissions but were otherwise identical. To calculate this counterfactual, we fit a linear probability model to predict admissions of non-legacy students using a quintic in SAT/ACT scores, indicator for race, gender, first-generation status, entering cohort, and parent-income bin, fixed effects for the full tuple of admissions office ratings, high-school GPA (where available), and high-school fixed effects. We then apply the coefficients from this model to predict a counterfactual admissions rate based on the individual characteristics of each legacy student. Figure 7c compares the admissions rates for legacy and non-legacy students across colleges for students who apply to multiple schools in our sample. To construct this figure, we calculate the following within each ordered pair of schools in our data, denoting the first of the two as the "reference school." The first bar plots the admissions rate at the reference school for legacy applicants at that school. The second bar plots the mean counterfactual non-legacy admissions rate (as above in Figure 7b) at the reference school for the same group of students. The third bar repeats the first bar for non-legacy applicants. The fourth bar plots the admissions rate at the non-reference school (i.e., a college the applicant's parents did not attend) for legacy students at the reference school. The fifth bar plots the admissions rate for non-legacy applicants at the non-reference school. The differences in admissions rates between legacy and non-legacy applicants control for a quintic in SAT/ACT scores. All estimates in this figure are from the selected subset of Ivy-Plus colleges for which we have linked internal admissions data; see Section 2 for details.

Figure 8: Admissions Office Ratings of Applicants by Parental Income, Controlling for Test Score

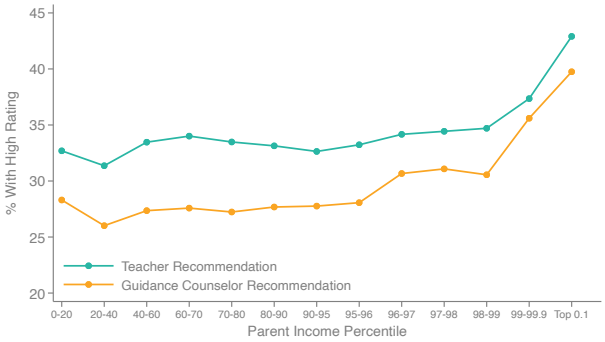
(a) Academic Ratings



(b) Non-Academic Ratings



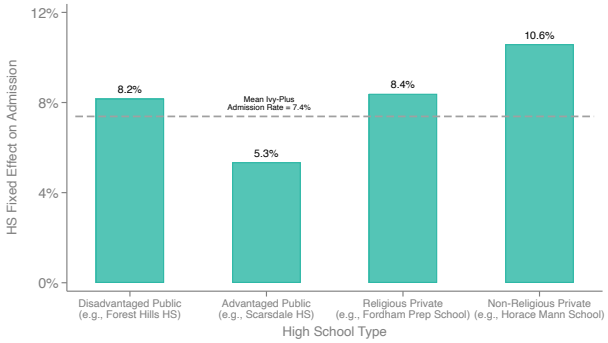
(c) Teacher and Guidance Counselor Ratings



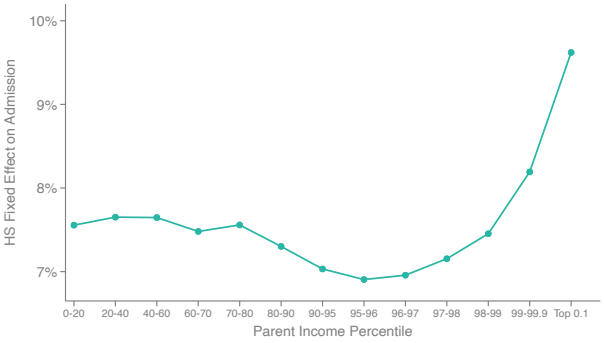
Notes: Figure 8 plots the proportion of applicants receiving admissions office ratings on various dimensions by parent-income bin, reweighting applicants to control for test scores as in Figure 2. Figure 8a considers academic ratings; Figure 8b non-academic ratings; and 8c ratings of letters of recommendation from teachers (teal) and school guidance counselors (orange). All figures exclude recruited athletes and legacy applicants and are estimated using data from the Ivy-plus college in our college-specific sample that records the most granular ratings information. See Appendix Figure A.11 for analogous figures that pool all Ivy-plus colleges in our sample and use coarser ratings.

Figure 9: Effects of High Schools on Ivy-Plus Admissions

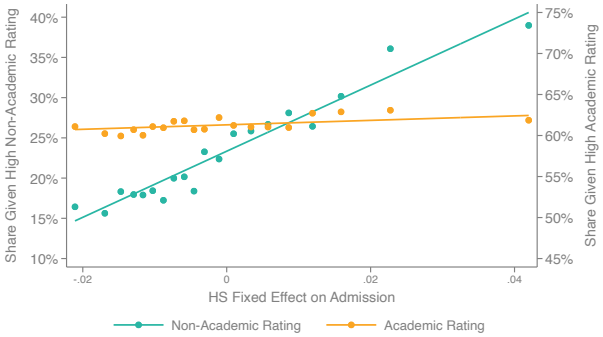
(a) High School Fixed Effect on Admissions, by High School Type



(b) High School Fixed Effect on Admissions by Parental Income

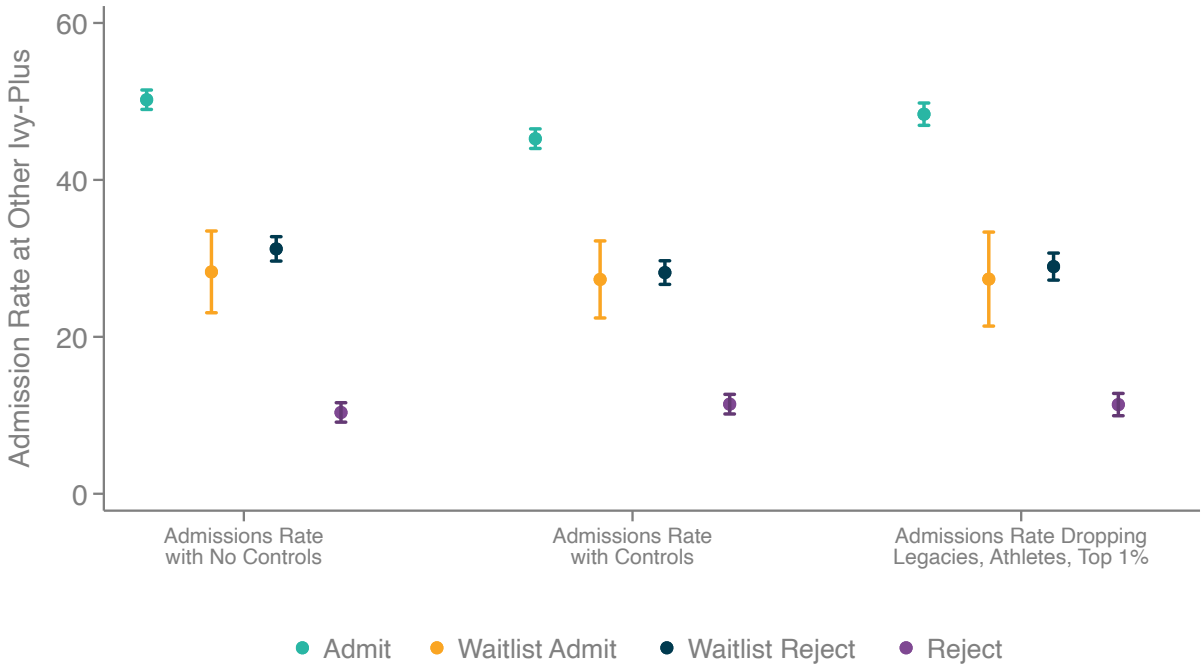


(c) Share of Ivy-Plus Students with High Ratings by High School Fixed Effect



Notes: Figure 9 presents various analysis of high-school fixed effects on admissions, focusing on high schools with at least 40 Ivy-plus applicants across the years of our sample. To calculate these fixed effects, we estimate a linear probability model to predict Ivy-plus admissions for non-legacy applicants, omitting recruited athletes. This admissions model includes fixed effects for exact SAT/ACT score, fixed effects for the interaction of race, gender, and parent income, and fixed effects for high school. We then calculate a jack-knife fixed effect estimate for each student i that excludes his/her own observation from the high school fixed effect estimate. Figure 9a plots the mean high-school admissions fixed effect (adding back the sample mean admissions rate) for four mutually exclusive sets of high schools. We classify public high schools as advantaged (disadvantaged) if they above above the 20th percentile on the aggregation of high school challenge indicators that capture educational opportunities or disadvantages in the high school environment, variables that feed into the CollegeBoard Landscape tool. Figure 9b plots the mean high school fixed effect on admissions by parental income bin. Figure 9c is a binned scatterplot showing the share of applicants given high academic or non-academic ratings (as in Figure 8) by ventile of high school fixed effect on admission; since the high-school fixed effect is on the x-axis, we shrink each estimate towards zero by multiplying it by its reliability to adjust for attenuation bias. To calculate reliability, we estimate the aggregate noise variance for the high school fixed effects as the average of the standard errors squared, and the signal variance as the total variance minus the aggregate noise variance; the reliability for each fixed effect is the signal variance divided by the sum of the signal variance plus the standard error of the school-specific estimate squared. All estimates are based on data from the Ivy-plus college in our college-specific sample that records the most granular ratings information.

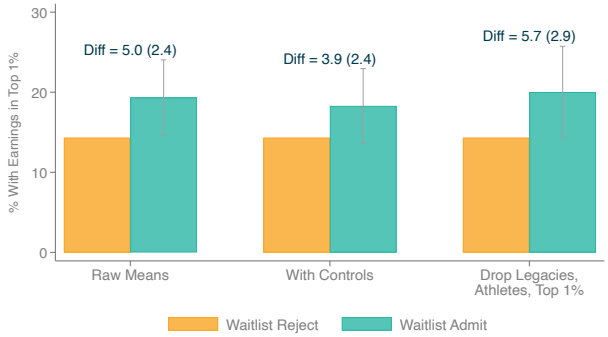
Figure 10: Multiple-Rater Test for Idiosyncratic Variation in Admissions



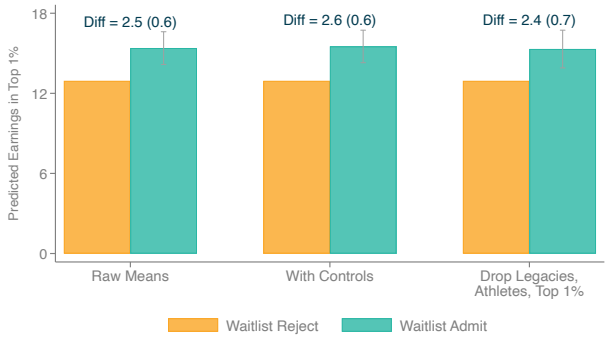
Notes: Figure 10 implements a multiple-rater test for whether admissions decisions are driven by idiosyncratic variation. Each block of four dots plots admissions rates at a lower-ranked Ivy-plus college by admissions outcome at another higher-ranked Ivy-plus college (from left to right: admitted directly, admitted off the waitlist, rejected off the waitlist, and rejected without the waitlist). The first block includes no additional controls. The second block of four dots repeats the first block but with fixed effects for parent-income bin, race, gender, recruited athlete status, legacy status, and home state. The third block of four dots repeats the second block, but dropping all students who are legacies, recruited athletes, or with parental incomes in the top 1%. The intervals show 95% confidence intervals. All estimates are based on individuals who applied to at least two Ivy-plus colleges in our college-specific sample.

Figure 11: Treatment Effects of Ivy-Plus Admissions on Income for Waitlisted Applicants

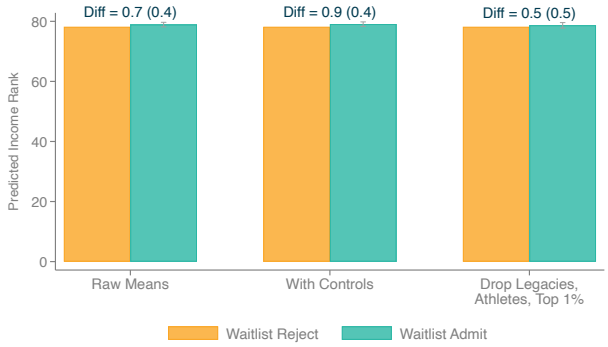
(a) Earnings in Top 1% at Age 33



(b) Predicted Earnings in Top 1% Based on Firm at Age 25



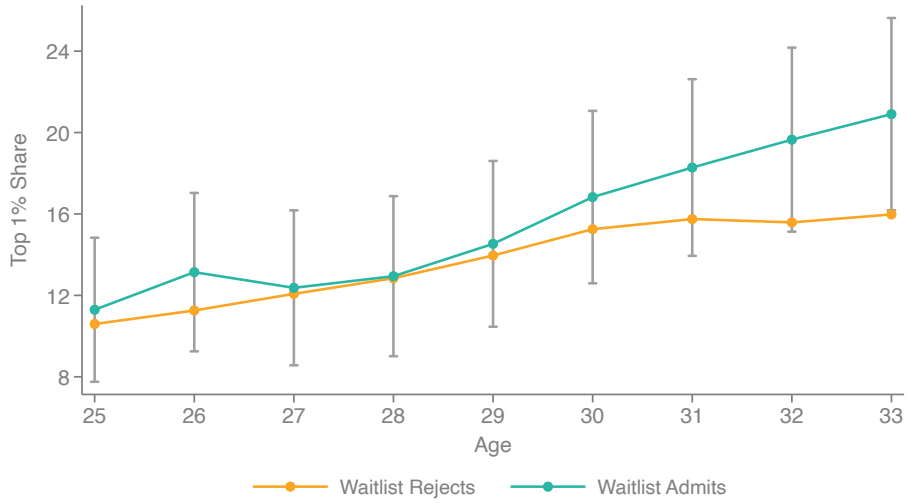
(c) Predicted Mean Income Rank



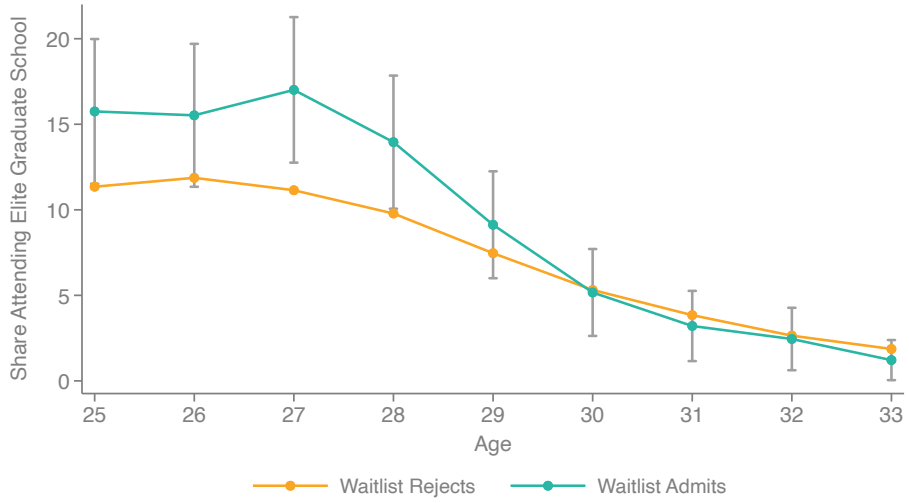
Notes: Figure 11 shows the treatment effects of Ivy-Plus admissions for waitlisted applicants on income by comparing mean outcomes between waitlist admits and rejects for Ivy-Plus colleges. Orange bars (left side in each pair) plot the average outcome for waitlist rejects; green bars (right side in each pair) plot the orange bar plus the estimated treatment effect. We present treatment on the treated (TOT) effects, adjusting for take-up (matriculation) by using waitlist admission as an instrument for attendance. In each panel, the first pair of bars presents the treatment effect with only fixed effects for the school on which a student is on the waitlist; the second pair of bars includes controls for test scores, parent-income bins, gender, race, state, recruited athlete, and legacy status; and the third pair of bars replicates second pair of bars except excluding legacies, athletes, and applicants with top 1% parental income. The outcome variable is an indicator variable for income (defined as household income, minus spousal wage and self-employment earnings if married) in the top 1% of the age- and cohort-specific distribution at age 33 in Panel 11a, the predicted top 1% share based on firm at age 25 (see Section 2.5 for details) in Panel 11b, and mean predicted income rank based on firm at age 25 in Panel 11c. Standard errors are clustered by individual, with whiskers denoting 95% confidence intervals. All estimates are from applicants to a selected subset of Ivy-Plus schools for which we have linked internal admissions data.

Figure 12: Treatment Effects of Ivy-Plus Admissions, by Age

(a) Share in Top 1%



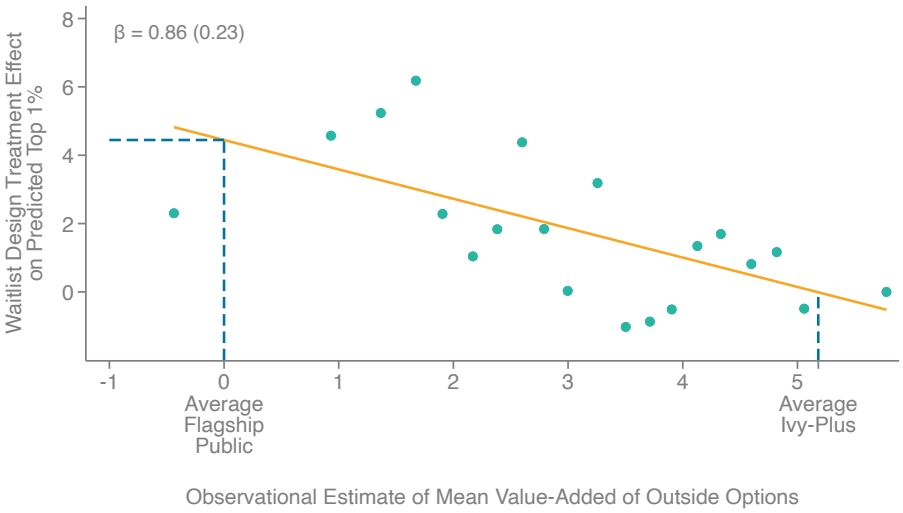
(b) Elite Graduate School Attendance



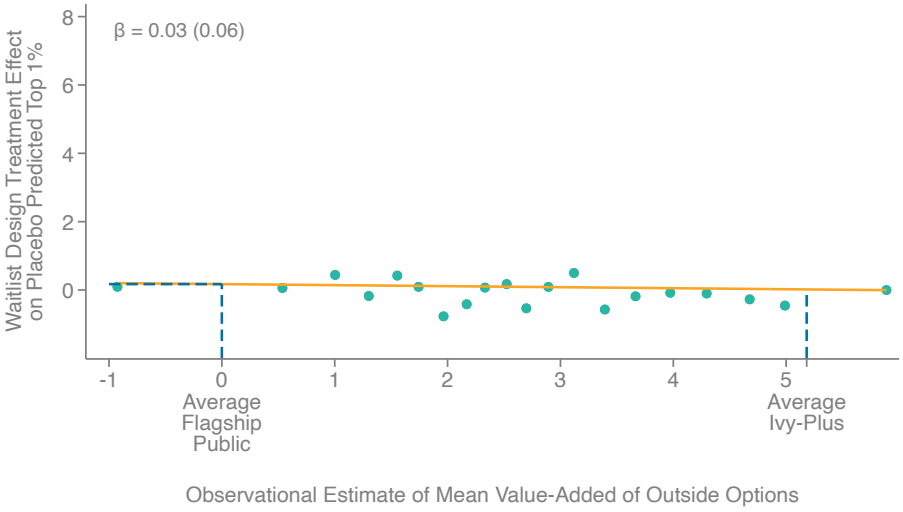
Notes: Figure 12a shows the estimated treatment-on-the-treated effect of Ivy-Plus admissions for waitlisted applicants on income without controls. The orange line plots the average outcome for waitlist rejects; teal bars plot the orange bar plus the estimated treatment effect for each age of measurement from 25 through 33. The treatment effect is calculated using separate instrumental regressions for each age using the same specification as in Figure 11a; the estimates at age 33 in this figure replicate the estimate from the left pair of bars in Figure 11a. Figure 12b replicates Figure 12a but with an indicator variable for attendance at an elite graduate school at each age as the outcome variable. All estimates in this figure are from the selected subset of Ivy-Plus colleges for which we have linked internal admissions data and are based on a balanced panel of individuals observed until age 33.

Figure 13: Heterogeneity in Treatment Effects by Strength of Outside Options

(a) Predicted Earnings in Top 1%



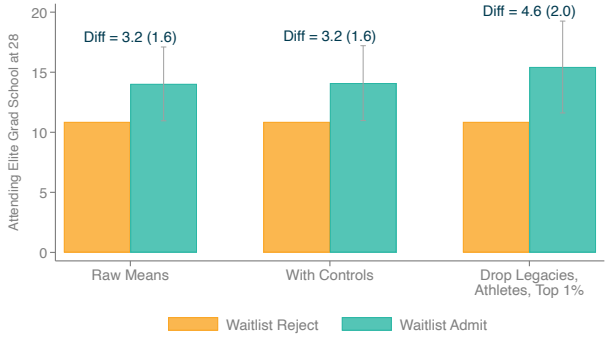
(b) Placebo Predicted Earnings in Top 1%



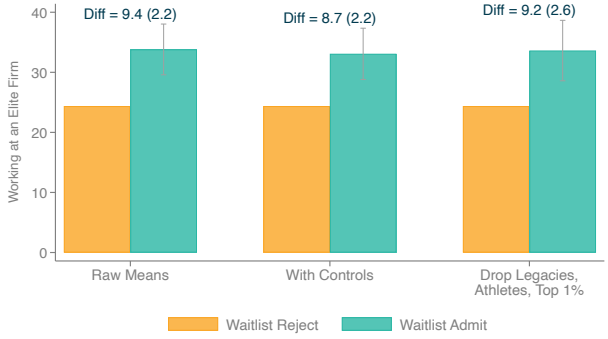
Notes: Figure 13 shows how the treatment effect estimate for waitlist admission varies with the strength of an Ivy-Plus applicant's outside options. To construct this figure, we define Ivy-plus applicants into subgroups g by their home state, parent income, race, and the Ivy-Plus school to which they applied. Within each group g, we calculate the average outside option as the observational value-added of colleges attended by those rejected from the waitlist, which is estimated as described in Section 4.2. We then pool students into twenty bins defined by ventiles of outside options, which correspond to the twenty dots on the figure. For each dot, the x-axis coordinate is the mean predicted value-added of the outside option. The y-axis coordinate is the TOT estimate for predicted top 1% outcome within that ventile for Figure 13a and the TOT estimate for placebo predicted top 1% outcome for Figure 13b. Placebo outcomes are predicted by regressing the predicted probability of reaching the top 1% on the following characteristics: a quintic in test scores, parent income bins, indicators for race, gender, home state, recruited athlete status, legacy status, fixed effects for academic and nonacademic ratings using the sample of waitlist rejects. We report the coefficient (and the implied best fit line) on the interaction of outside option with attendance at the waitlisted school, using the interaction between the outside option and admissions on the waitlist as the instrument, in a 2SLS regression estimated on the microdata. All estimates are from applicants to a selected subset of Ivy-Plus schools for which we have linked internal admissions data.

Figure 14: Treatment Effects of Ivy-Plus Admissions on Non-Monetary Outcomes

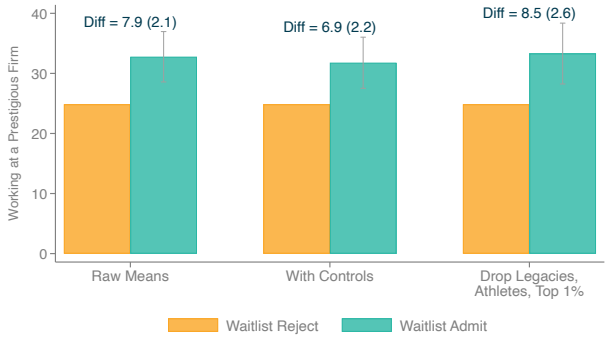
(a) Elite Graduate School Attendance



(b) Employment at an Elite Firm



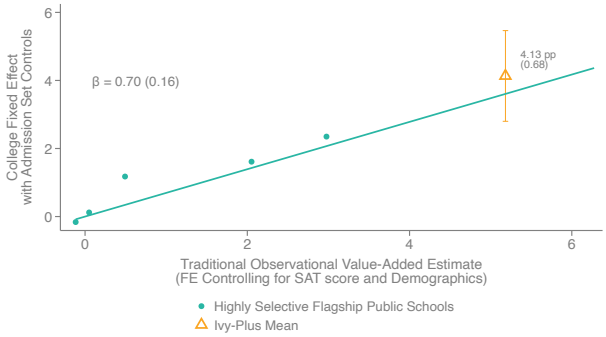
(c) Employment at a Prestigious Firm



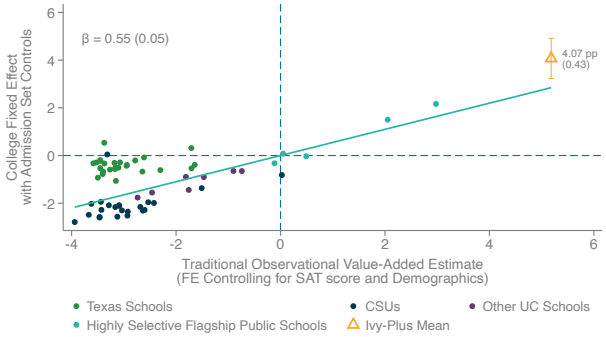
Notes: This figure replicates Figure 11 using non-monetary outcomes. Panel 14a shows the share of waitlisted applicants who attend an elite graduate school at age 28. Panel 14b shows the proportion of waitlisted applicants who work at an elite firm at age 25. Panel 14c shows the proportion of waitlisted applicants who work at a prestigious firm at age 25. See Section 2.5 for more details on the definition of these outcome variables.

Figure 15: Causal Effects of Ivy-Plus Attendance: Matriculation Design

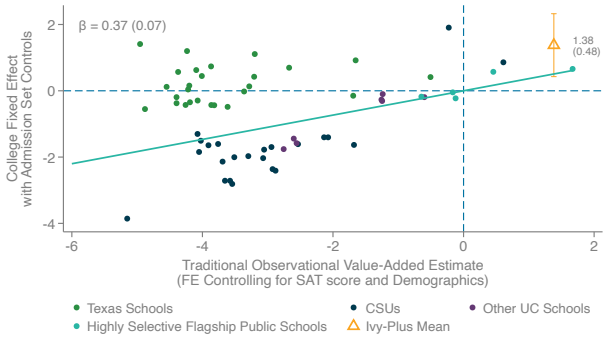
(a) College Fixed Effects on Predicted Top 1% for Highly Selective Colleges



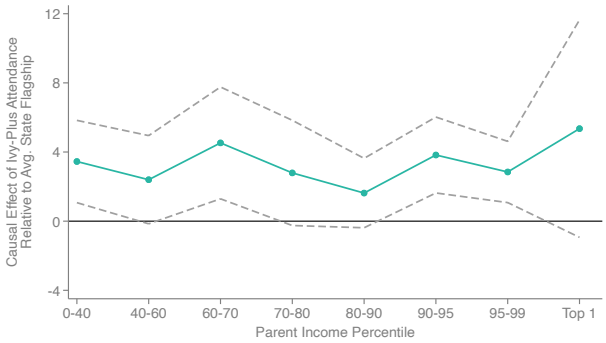
(b) College Fixed Effects on Predicted Top 1% by College Group



(c) College Fixed Effects on Predicted Mean Income Rank by College Group

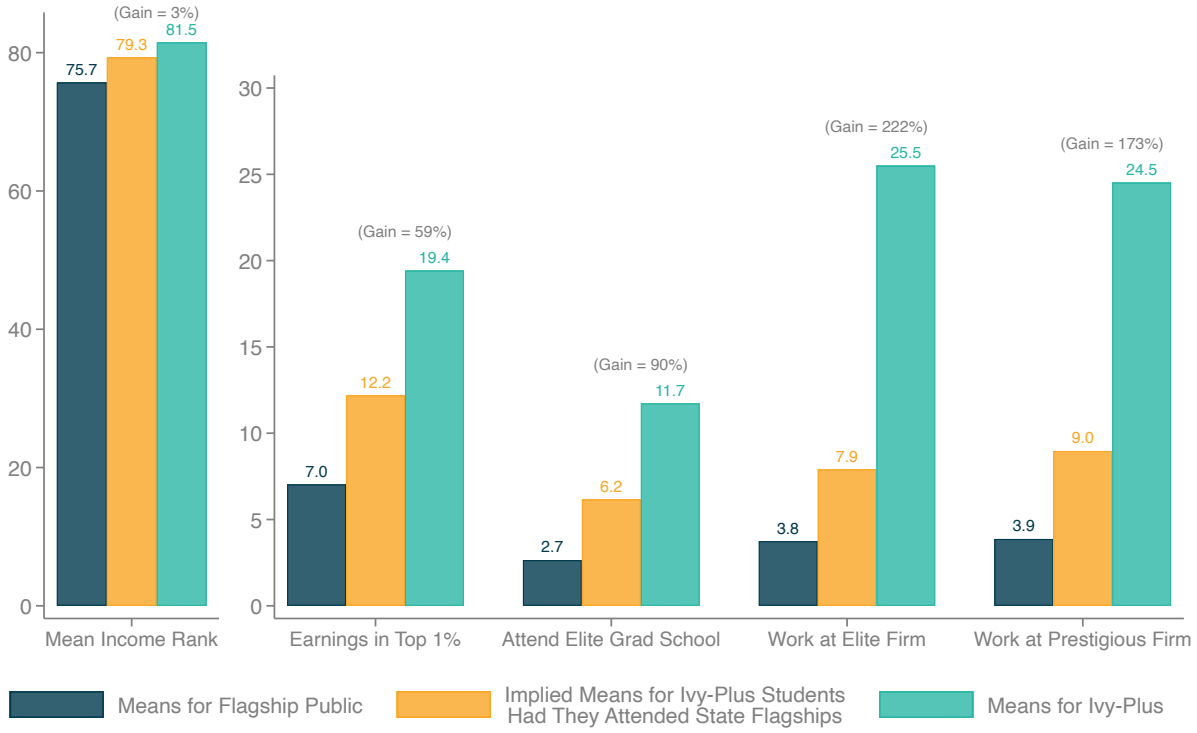


(d) Heterogeneity in Effects of Ivy-Plus Attendance by Parent Income



Notes: This figure presents estimates of the causal effects of attending different colleges using the design based on variation in matriculation conditional on the set of colleges to which a student was admitted. See Section 4.1.3 for more details on this design. In each of the first three panels, we plot the causal effect estimates for each college as the y-axis variable, estimated from regressions of outcomes on indicators for school attended with fixed effects for the exact set of schools to which the student is admitted (among the set of schools plotted in each panel) as controls. The x-axis variable is the observational value-added estimate for each college, estimated as described in Section 4.2. The value-added estimates are normed such that the value-added of highly selective public flagship schools (listed in Appendix Table 1) is 0. Each dot represents a different college, except that we report the estimates for the Ivy-Plus colleges in our college-specific sample in a single triangle, along with the point estimate and standard error for the causal effect. We also plot the best-fit line based on a regression on the plotted points, as well as the slope and standard error for that line. Figure 15a presents results from this design for the predicted top 1% based on age 25 firm outcome, using only the Ivy-Plus and highly selective public flagship colleges in our college-specific sample. Figure 15b replicates Figure 15a, but additionally includes data from all other schools in the UC system, all schools in the CSU System, and all other 4-year public schools in Texas. Figure 15c replicates Figure 15b, but using predicted mean income rank based on firm at age 25 as the outcome variable. Figure 15d reports the causal effect of attending Ivy-Plus colleges (relative to the highly selective public flagship schools) as in Figure 15b, but separately for students from each of eight parent income bins; the dashed lines present 95% confidence intervals.

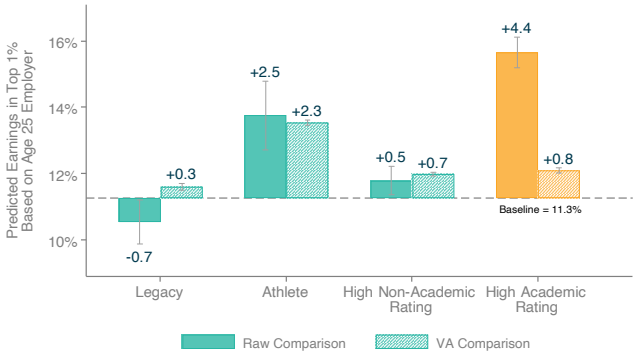
Figure 16: Differences in Outcomes at Ivy-Plus vs. Highly Selective State Flagship Colleges: Causal Effects vs. Selection



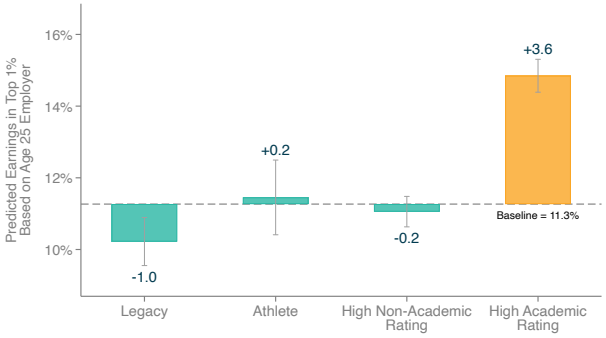
Notes: Figure 16 shows how much of the difference in observed post-college outcomes between Ivy-Plus and highly selective state flagship students is due to causal effects of colleges vs. selection. For elite graduate school attendance, elite firm employment, and prestigious firm employment, we estimate the causal effect of Ivy-Plus attendance by multiplying the waitlist TOT effect (as estimated in Figures 11 or 14) by the ratio of the difference in value-added between Ivy-Plus and the nine flagship public schools and the waitlist TOT effect for value-added of college attended (for the relevant variable). For mean income ranks and earnings in top 1% at 33, we use the difference in observational value-added for the probability of reaching the top 1% between Ivy-Plus and the nine flagship public schools instead of the waitlist TOT effect to maximize precision, and rescale the by ratio for the predicted top 1% and predicted mean rank estimates as discussed in Section 4.2. We then calculate the implied means for Ivy-Plus students had they attended state flagships (orange bar) by subtracting the implied treatment effects from the mean outcomes for Ivy-Plus attendees (teal bar). Mean observed outcomes for the nine highly selective flagship public schools are shown in the blue bar. The difference between the first and second bars in each triplet can be interpreted as the part of the difference in observed outcomes between Ivy-plus and state flagship students that is due to selection, while the difference between the second and third bars is the causal effect of Ivy-plus attendance. See notes to Figures 11 and 14 for more detail on the variables, sample, and waitlist estimate of the TOT effects.

Figure 17: Post-College Outcomes by Application Credentials Among Ivy-Plus Applicants

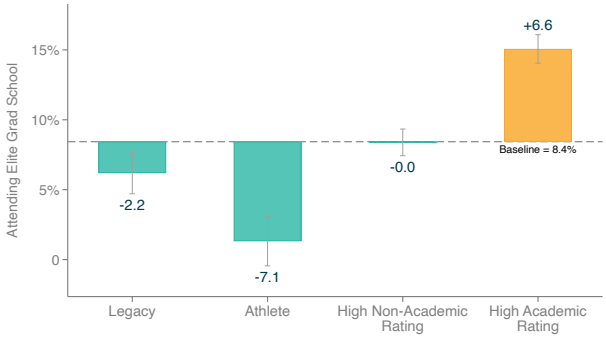
(a) Predicted Top 1% Outcomes vs. College Value-Added



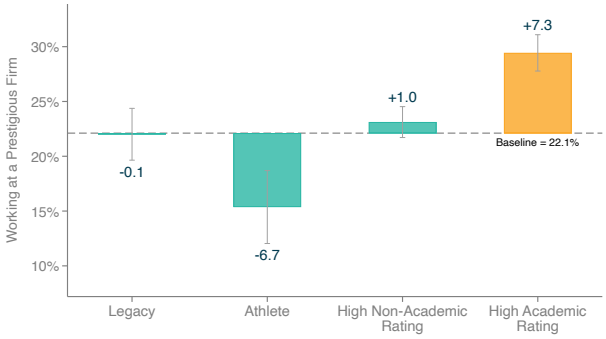
(b) Differences in Predicted Chance of Reaching Top 1%



(c) Differences in Elite Graduate School Attendance Rates



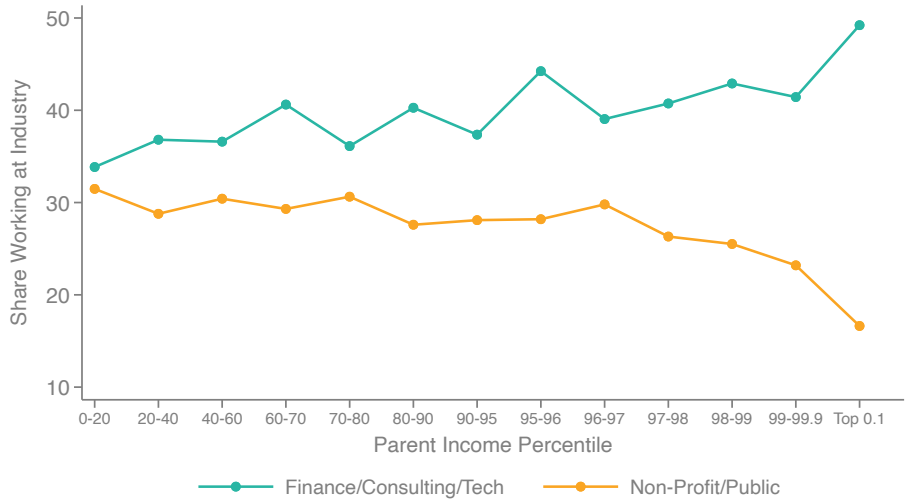
(d) Differences in Prestigious Firm Employment Rates



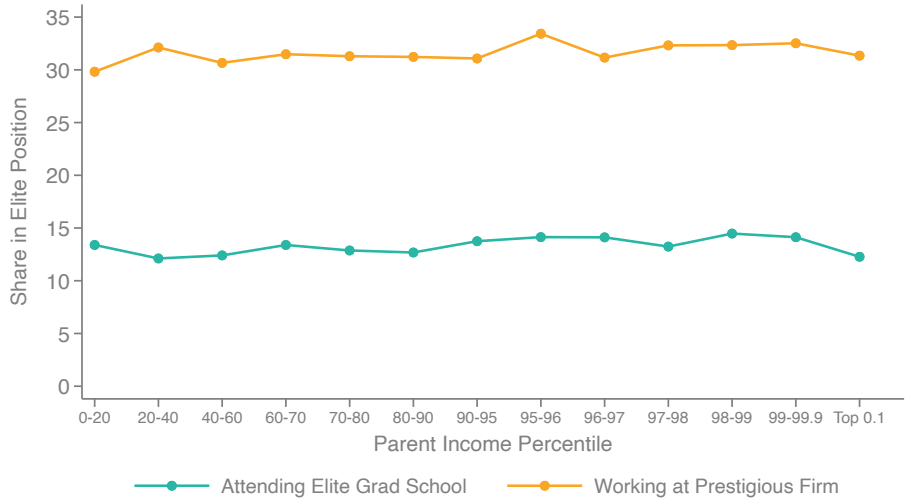
Notes: In Figure 17a, the bars on the left in each pair report estimates from regressing the predicted probability of reaching the top 1% based on age 25 employer on four explanatory variables: indicators for whether a student is a legacy, is a recruited athlete, has a high non-academic rating, and has a high academic rating. The sample consists of students either admitted or offered a place on the waitlist at the Ivy-plus college with the most granular ratings data in our sample. We plot the regression coefficients plus the baseline rate for the outcome in the sample, defined as the mean of the outcome non-legacy, non-athlete applicants with low academic and non-academic ratings. In the bars on the right, we replace the dependent variable with the observational value-added of college attended (based on the same predicted top 1% outcome) multiplied by the ratio of the waitlist-design treatment effect estimate to the observational VA estimate reported in Columns 1 and 5 of Appendix Table 6. See Figure A.24 for an illustration of the levels underlying the coefficients in this figure for students with low vs. high non-academic ratings. Figure 17b plots the difference between the Raw Outcome Comparison and VA Comparison in Figure 17a for the four explanatory variables plotted in Figure 17a. These estimates show the difference in outcomes for applicants by their credentials, netting out differences in the value-added of the college they attend. Figures 17c and 17d replicate Figure 17b using an indicator for attending an elite graduate school at age 25 and working at a prestigious firm at age 25 as the dependent variables.

Figure 18: Post-College Outcomes for Ivy-Plus Matriculants by Parental Income

(a) Share of Ivy-Plus Matriculants by Industry



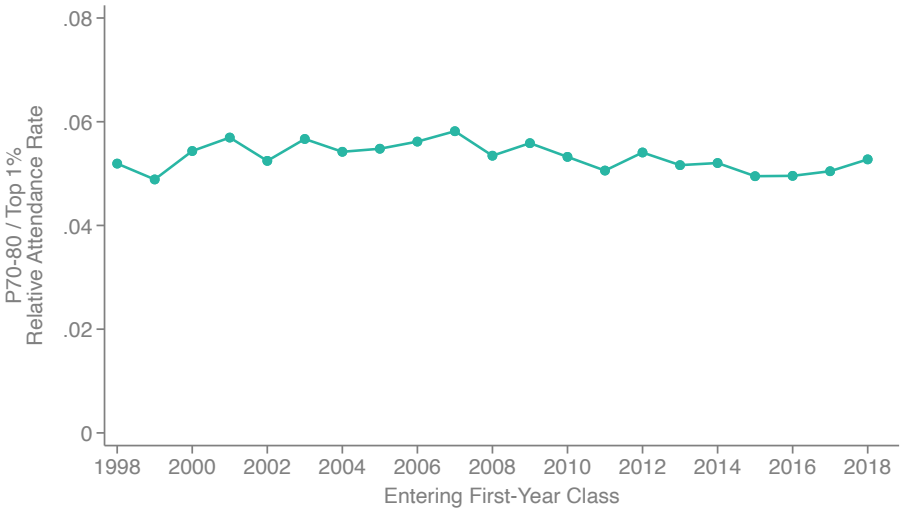
(b) Share of Ivy-Plus Matriculants in Graduate School and Prestigious Firms



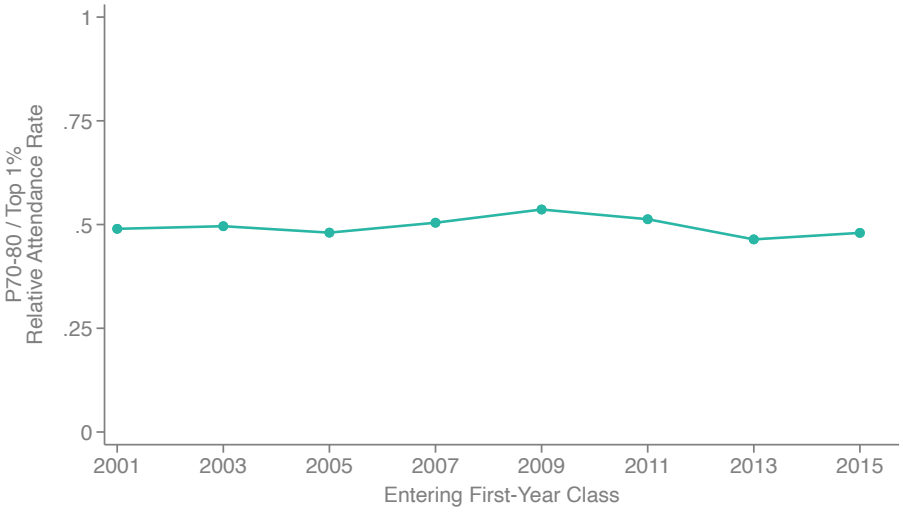
Notes: Figure 18a plots the share of Ivy-Plus matriculants working in Finance/Consulting/Tech (in green line) vs. Non-Profit/Public (in orange line) vs. parental income for Ivy-plus attendees. Figure 18b plots the share of matriculants who attend elite graduate school at age of 25 (green line) and who work at a prestigious firm at age 25 (orange line).

Figure 19: P70-80 / Top 1% Attendance Shares for First-Year College Students, 1998-2018

(a) P70-80 / Top 1% Attendance Shares for First-Year College Students, 1998-2018



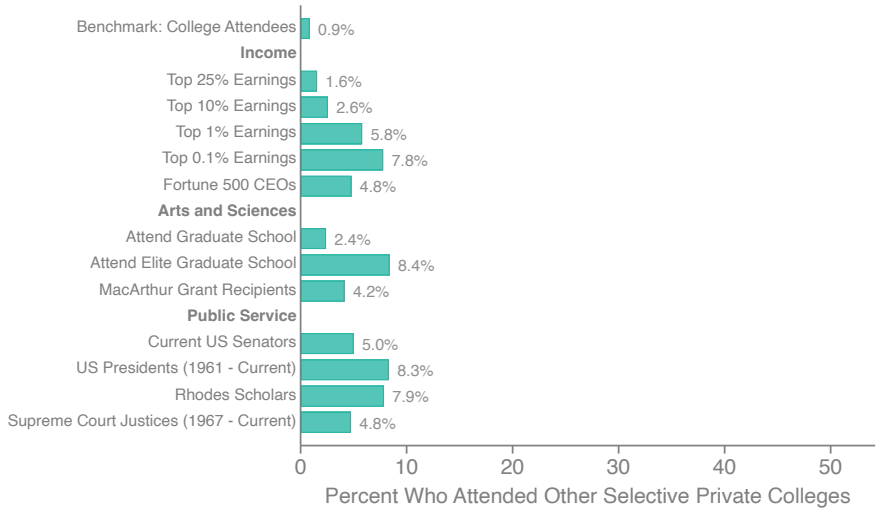
(b) P70-80 / Top 1% Attendance Shares for First-Year College Students, Controlled by Test Score, 2001-2015



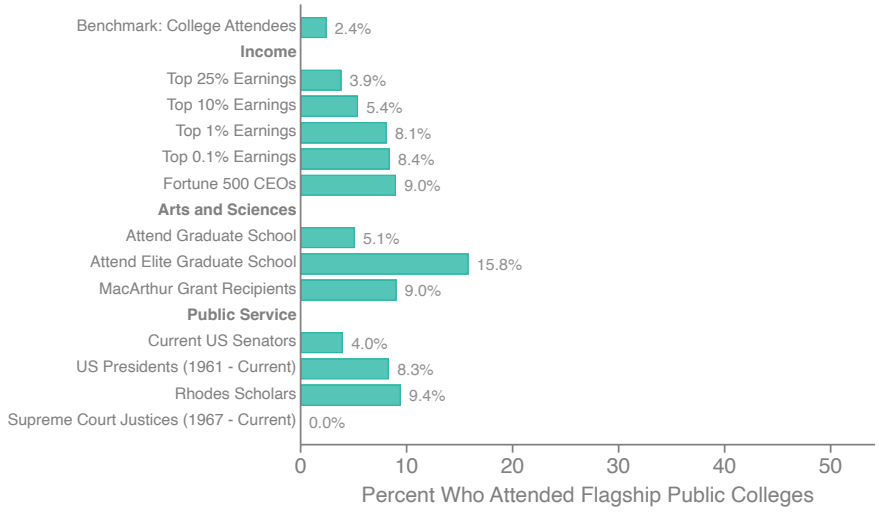
Notes: Figure 19 reports the attendance share ratio between students with parental incomes between the 70th and 80th percentile and students with parental incomes in the top 1% who turn 18 in the years from 1998-2018, pooling all 12 Ivy-Plus colleges. The attendance share ratio is calculated as the share of college attendees from p70-p80 divided by the share of attendees from the top 1%, divided by 10 (since there are 10x more students between the 70th and 80th percentiles than in the top 1%). Figure 19b plots the same ratio controlling for test scores by reweighting as in Figure 2.

Figure A.1: Share of Individuals in Leadership Positions by Colleges

(a) Share of Individuals in Leadership Positions who Attended Other Selective Private



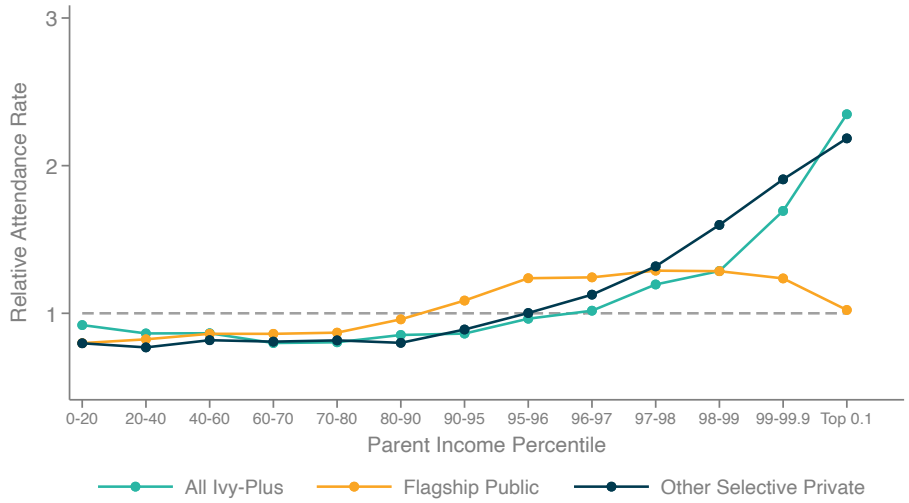
(b) Share of Individuals in Leadership Positions who Attended Flagship Public



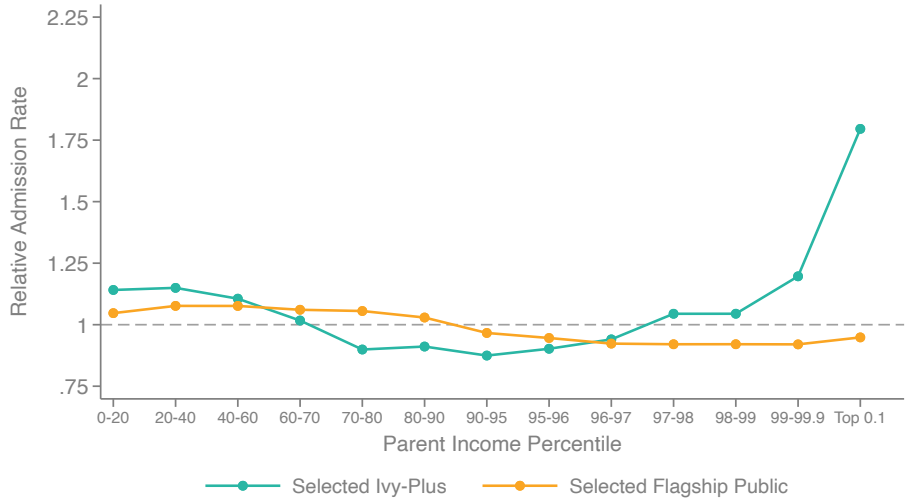
Notes: This figure replicates Figure 1 but with students who attended one of the other most selective private universities (in Panel a) or one of the nine most selective flagship public schools (in Panel b). See Appendix Table 1 for the list of schools, and Appendix A for the definitions and sources for each variable.

Figure A.2: Attendance and Admission Rates by Parental Income, Controlling for Race

(a) Attendance Rates

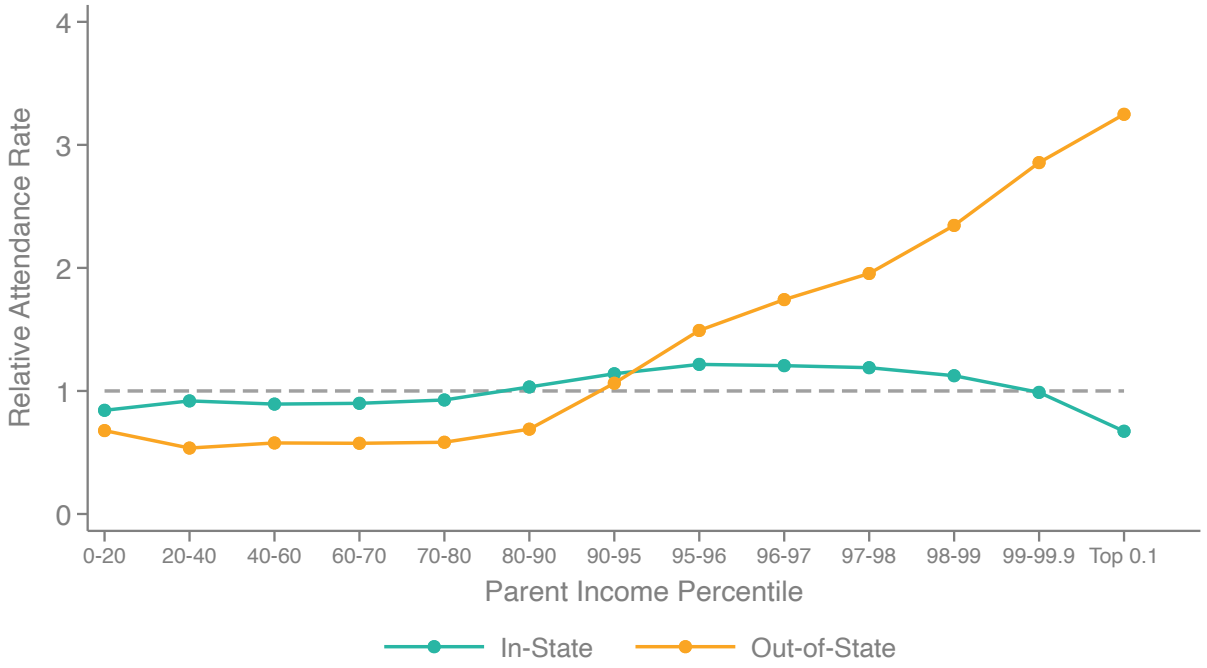


(b) Admission Rates



Notes: Figure A.2a replicates Figure 2b, but reweighting so that the joint distribution of race and test scores within each parent-income bin matches the distribution for attending students. Figure A.2b replicates Figure 4a, but similarly reweighting on both test score and race.

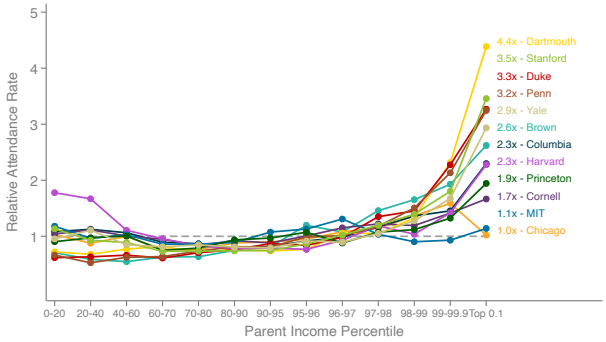
Figure A.3: Attendance Rates at Selective Public Flagship Universities, by Parental Income and In-State Status



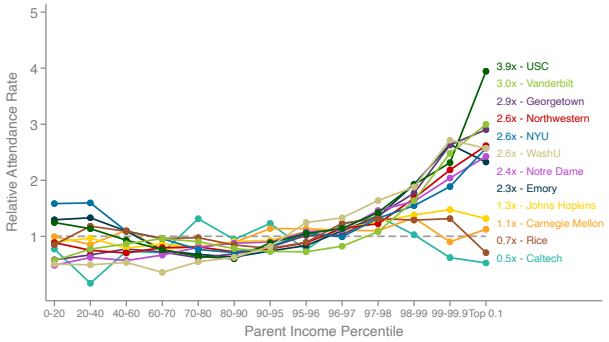
Notes: This figure replicates the estimates from Figure 2 for the nine highly selective public flagship universities , but splitting students into those applying in-state and out-of-state. We measure in-state status using the student's state of residence when they take a standardized test.

Figure A.4: Attendance Rates by Parental Income and College, Controlling for Test Score

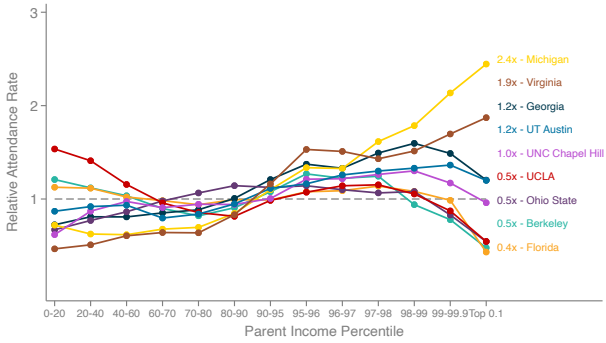
(a) Attendance at Ivy-Plus Colleges



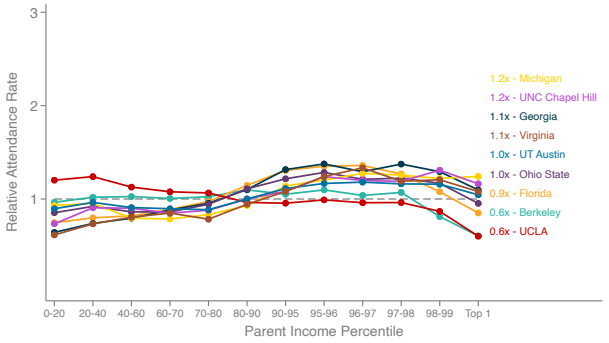
(b) Attendance at Selective Private Colleges



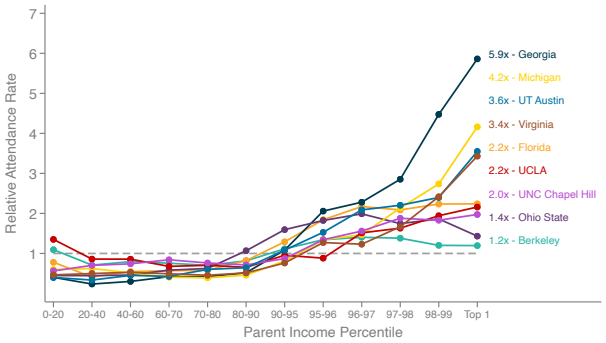
(c) Attendance at Selective Public Flagships



(d) In-State Attendance at Selective Public Flagships

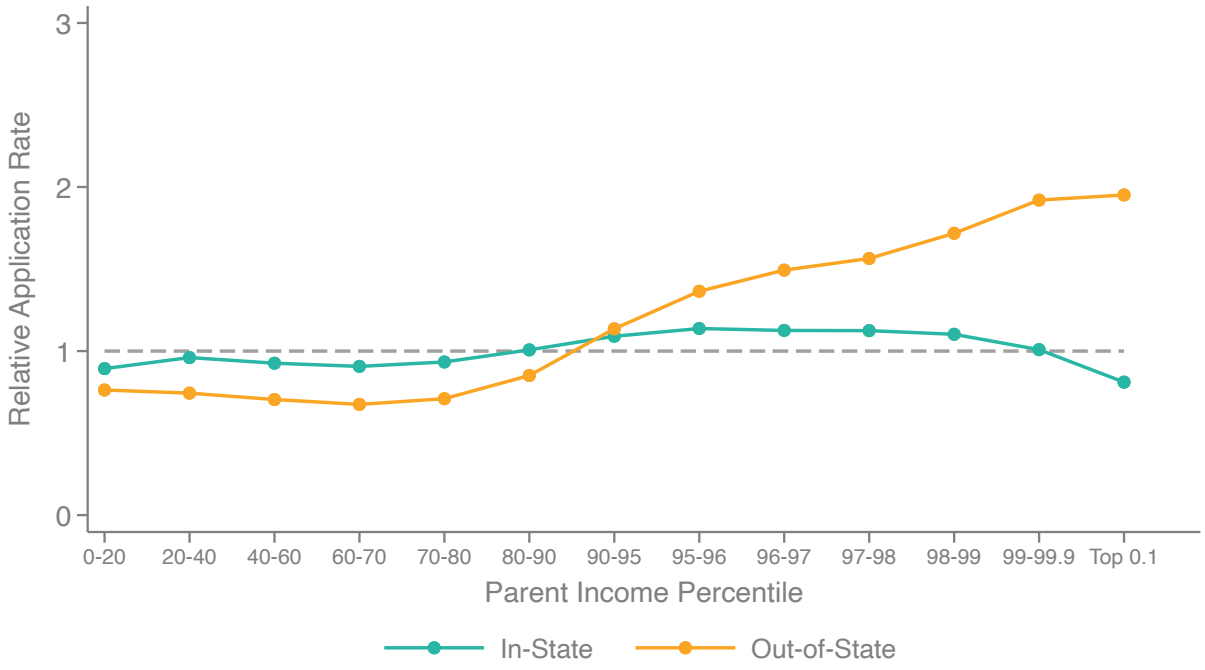


(e) Out-of-State Attendance at Selective Public Flagships



Notes: This figure replicates the estimates in Figures 2 and A.3, but separately by college. We follow the same procedure as in these other figures, except that we normalize the test-score-weighted attendance rate series for each school by the mean test-score-weighted attendance rate at that school to create each series. See notes to Figures 2 and A.3 for more details. Figure A.4a plots relative attendance rates for each Ivy-Plus colleges; Figure A.4b plots relative attendance rates for each other highly selective private college; Figure A.4c plots relative attendance rates for of the highly selective public flagship universities pooling in-state and out-of-state students, while Figures A.4d and A.4e repeat this for in-state and out-of-state students respectively. In all panels we follow a differential privacy approach and add random noise distributed $N\left(0, \frac{\Delta\theta}{\varepsilon}\right)$ to each estimate, where $\Delta\theta$ is the global sensitivity of statistic θ and ε is the privacy loss parameter. Since the outcome is a binary variable, $\Delta\theta = \frac{1}{N}$ (where N is the number of observations behind a given estimate); we set $\varepsilon = 1$.

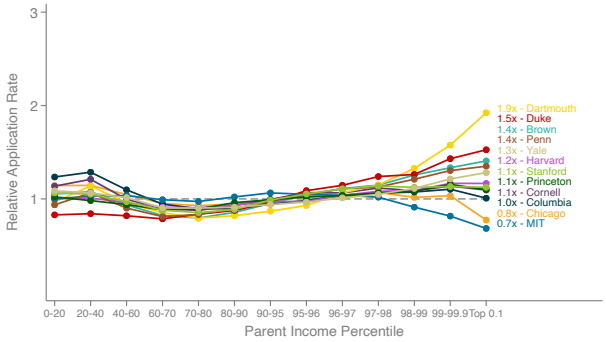
Figure A.5: Application Rates at Selective Public Flagship Colleges



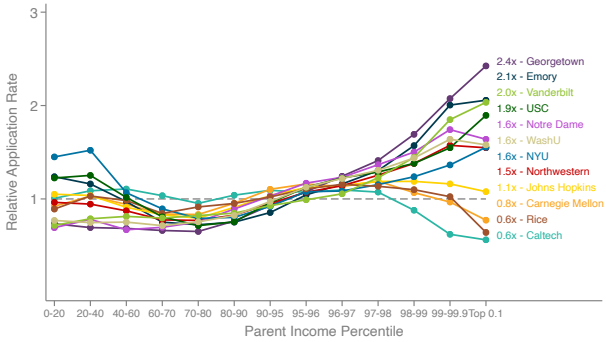
Notes: This figure replicates Figure A.3, but replacing attendance rates with application rates.

Figure A.6: Application Rates by Parental Income and College, Controlling for Test Score

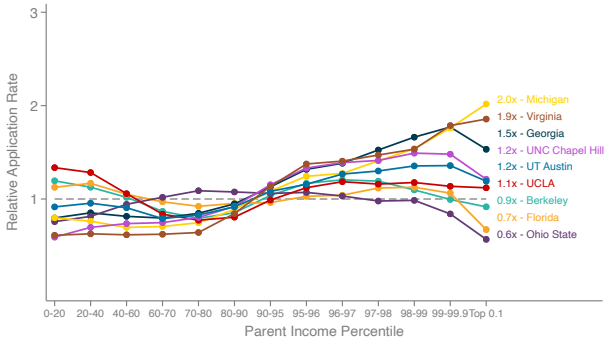
(a) Application to Ivy-Plus Colleges



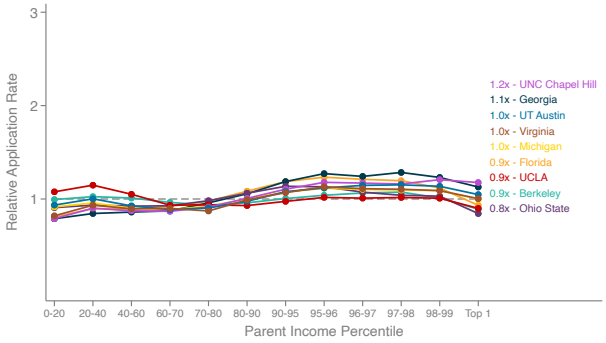
(b) Application to Selective Private Colleges



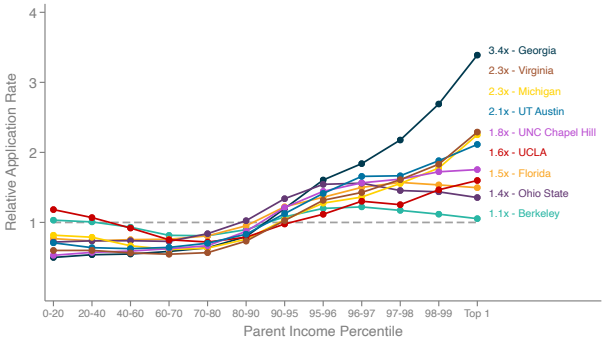
(c) Application to Selective Public Flagships



(d) In-State Application to Selective Public Flagships



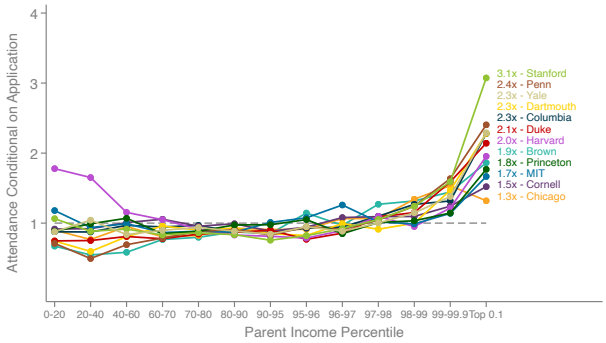
(e) Out-of-State Application to Selective Public Flagships



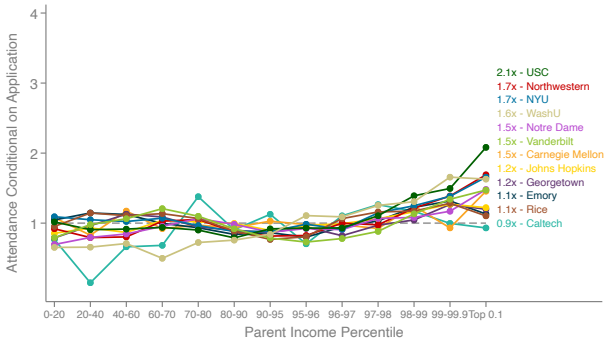
Notes: This figure replicates A.4, but replacing attendance rates with application rates.

Figure A.7: Attendance Rates Conditional on Application by Parental Income and College, Controlling for Test Score

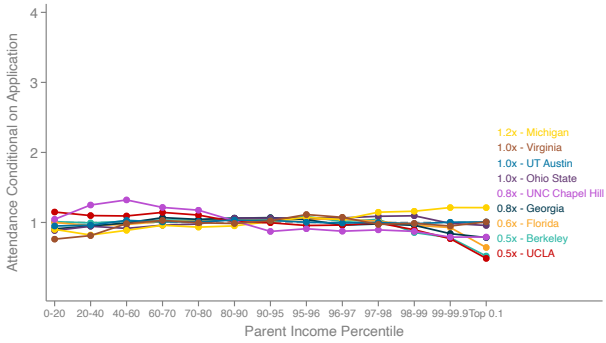
(a) Conditional Attendance to Ivy-Plus Colleges



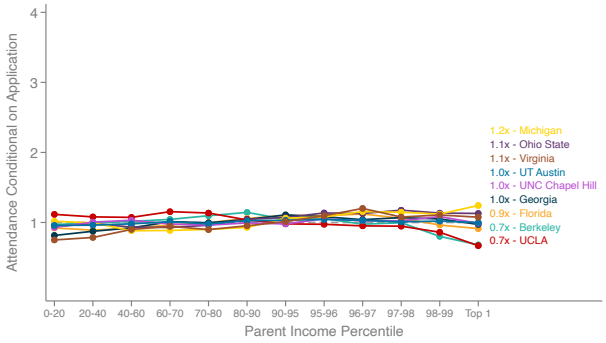
(b) Conditional Attendance to Selective Private Colleges



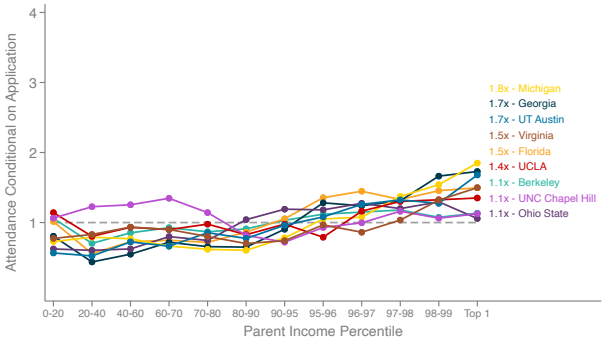
(c) Conditional Attendance to Selective Public Flagships



(d) In-State Conditional Attendance to Selective Public Flagships



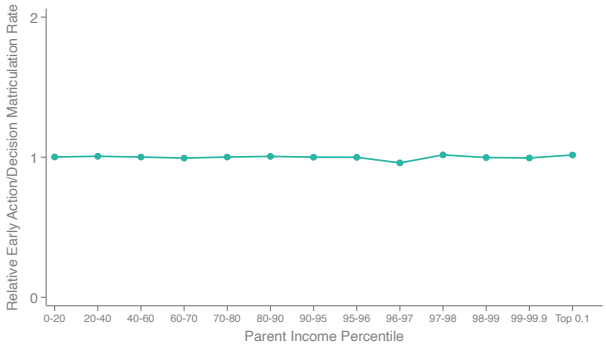
(e) Out-of-State Conditional Attendance to Selective Public Flagships



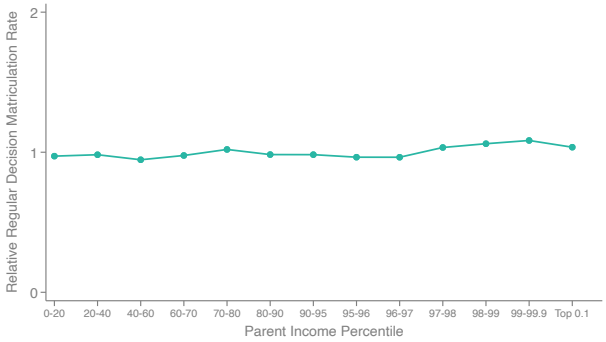
Notes: This figure replicates A.4, but replacing attendance rates with attendance-conditional-on-application rates.

Figure A.8: Early Action/Early Decision Applications and Matriculation Rates

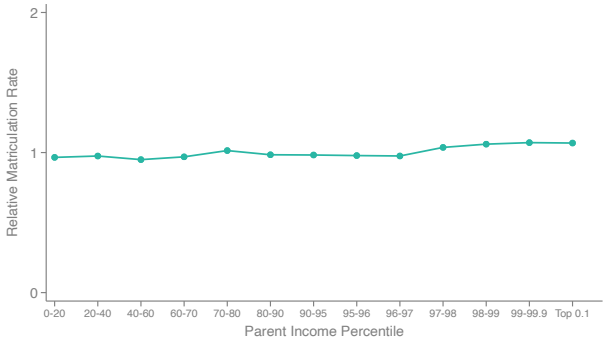
(a) Matriculation Rate, Early Action/Decision Applicants



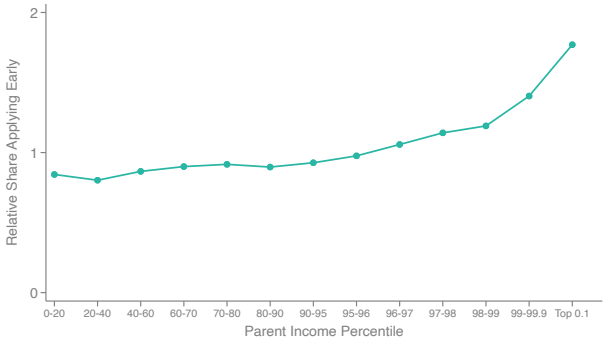
(b) Matriculation Rate, Regular Decision Applicants



(c) Matriculation Rate, All Admitted Students, Weighted by Application Round



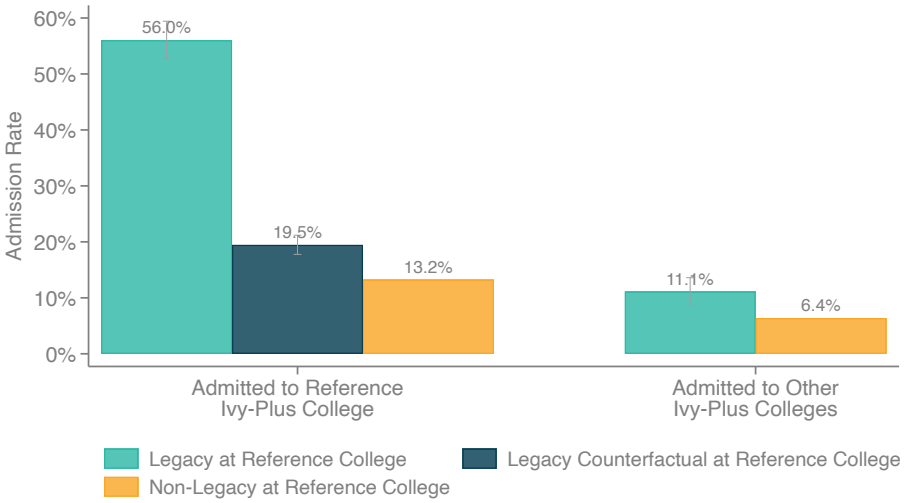
(d) Share Applying Early



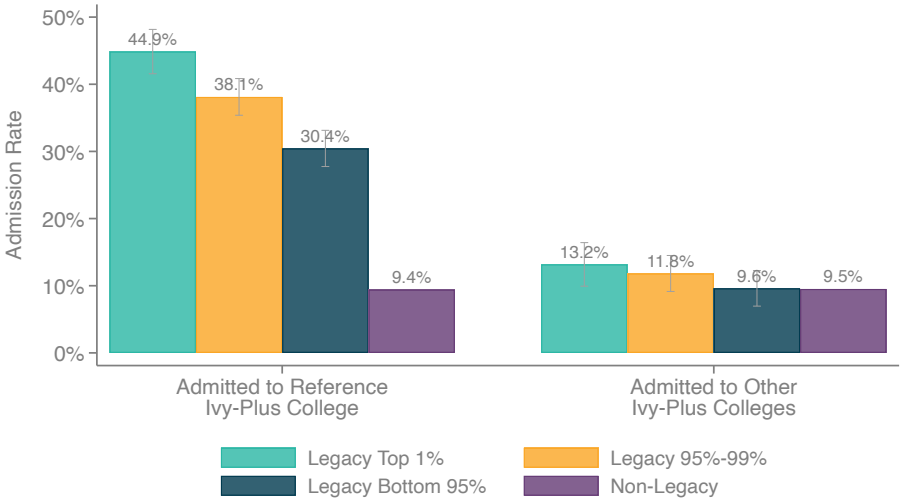
Notes: Figures A.8a and Panel A.8b replicate Figure 4b but for students who were admitted to Ivy-Plus schools in the early action / early decision round or in the regular decision round, respectively. Figure A.8c replicates Figure 4b, reweighting students not only on test-score but to equalize the share of students in early vs. regular decision rounds across parent-income bin. Figure A.8d plots the share of early action/decision applicants by parental income. We calculate these figures in a selected subset of Ivy-Plus schools for which we have linked internal admissions data; see Section 2 for more details on data sources.

Figure A.9: Admission Rate by Legacy Status

(a) Admission Rate by Legacy Status at Lower Ranked Colleges

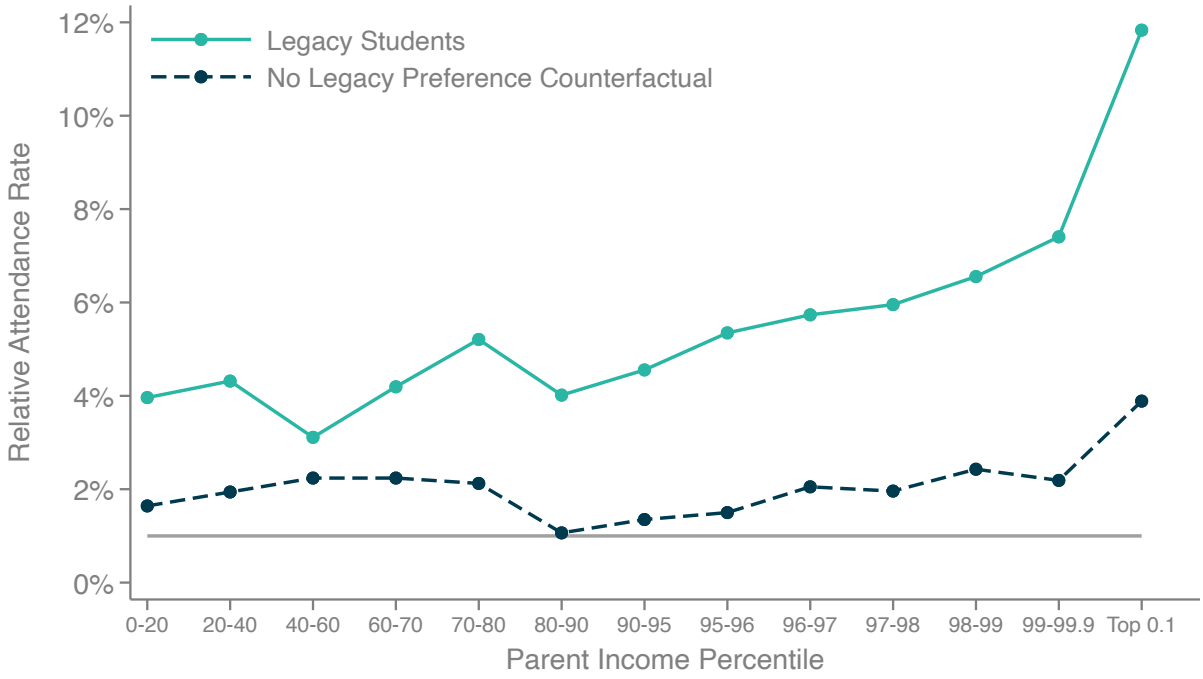


(b) Admission Rate by Legacy Status and Parental Income



Notes: Figure A.9a replicates Figure 7c, except that we only allow the reference school in each pair to be the lower ranked school. Figure A.9a estimates admissions rate of legacy and non-legacy applicants in both reference and other colleges as in Figure 7c but separately by parental income group (top 1% and bottom 95%).

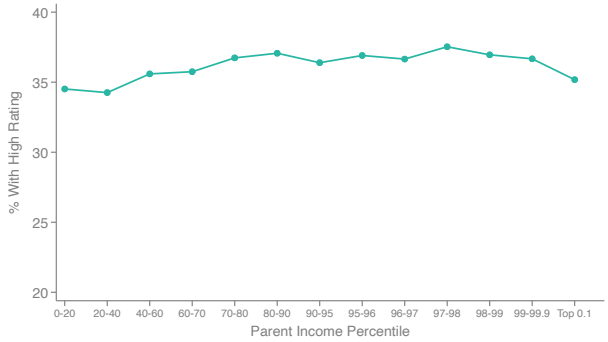
Figure A.10: Counterfactual Attendance Rates, Ivy-Plus Legacy Students



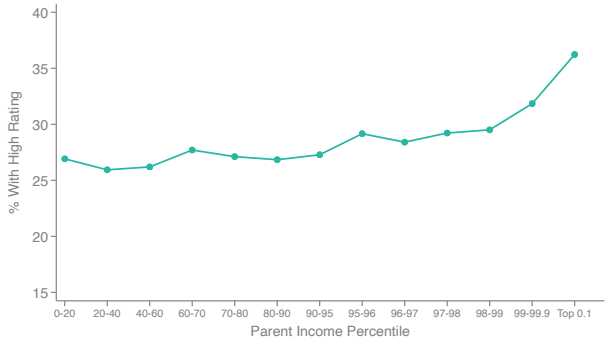
Notes: Figure A.10 replicates Figure 7b, except that we plot actual and counterfactual attendance rates rather than admissions rates for legacy students, by parent-income bin.

Figure A.11: Admissions Office Ratings of Applicants by Parent Income: Robustness

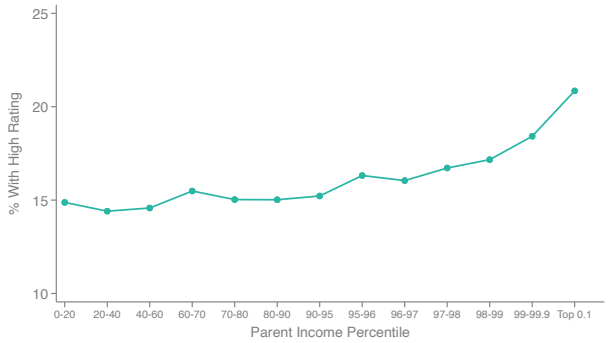
(a) Academic Ratings, All Schools



(b) Coarse Non-Academic Ratings, Focal School



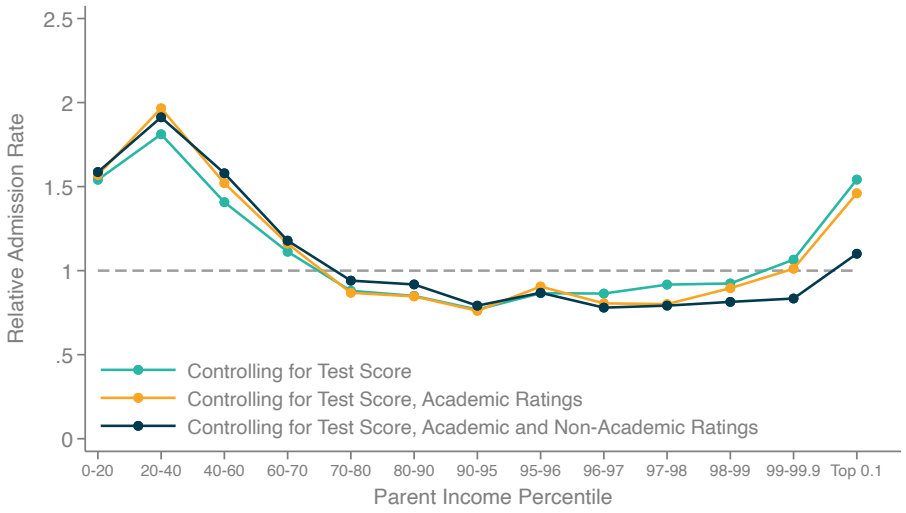
(c) Coarse Non-Academic Ratings, All Schools



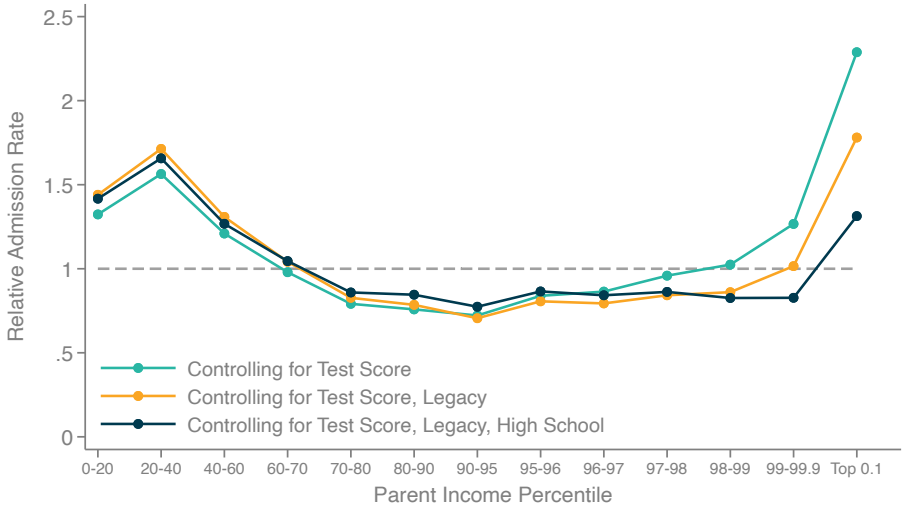
Notes: Figure A.11 replicate the results on Figures 8a and 8b, but with a broader set of schools. Panel A replicates Figure 8a but with data from multiple Ivy-Plus schools. Panel B replicates Figure 8b with data only from a certain Ivy-Plus college used in Figure 8b, but coarsening the measurement of non-academic rating to be more similar to the measurement of non-academic ratings elsewhere. Panel C then replicates Panel B but including data from multiple Ivy-Plus schools.

Figure A.12: Admissions Rate by Parental Income

(a) Relative Admissions Rate by Parental Income, with Ratings Controls



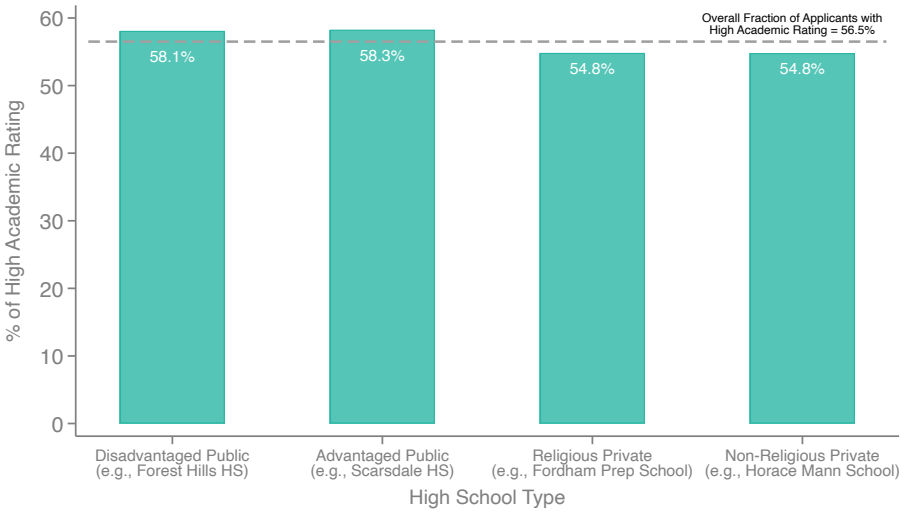
(b) Admissions Rate by Parent Income, with High School Controls



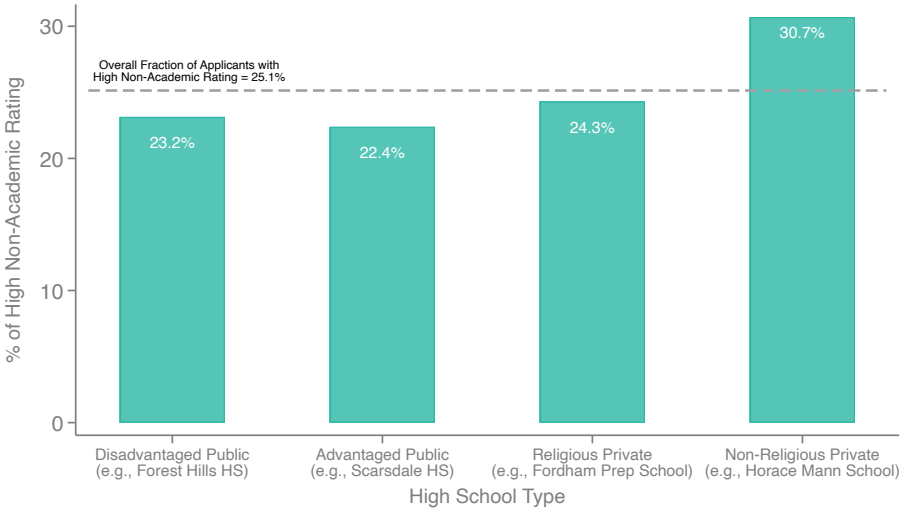
Notes: Figure A.12a plots admissions rates for non-legacy non-recruited-athlete applicants to a certain Ivy-Plus college with three set of weights. The teal line reweights on test score, so that the distribution within each parent income bin matches that of attending students. The orange line reweights on the joint distribution of test score and academic rating. The dark blue line reweights on the joint distribution of test score, academic rating, and non-academic rating. Figure A.12b plots admissions rates at Ivy-Plus colleges by parental income bin reweighting for SAT/ACT scores (teal line), adding regression controls for legacy status (orange line), and finally adding high school fixed effects (dark blue line). The sample includes all domestic applicants in 2010–2015, excluding recruited athletes and attending high schools with at least 10 applicants to the college.

Figure A.13: Admissions Office Ratings of Applicants by High School Type

(a) Academic Ratings by High School Type

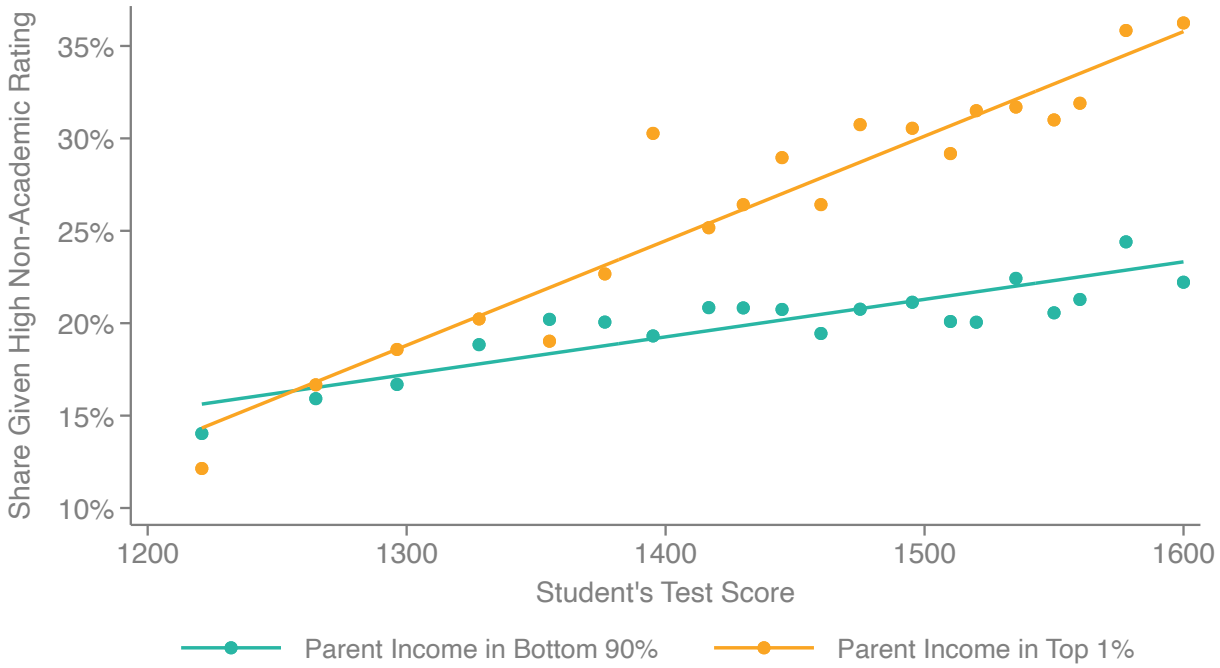


(b) Non-Academic Ratings by High School Type



Notes: Figures A.13 plot the proportion of applicants receiving high ratings on various dimensions by high school type adjusting for the test scores of applicants using a quintic in test score. Figure A.13a plots this for academic ratings and FigureA.13b plots this for non-academic. All estimates exclude recruited athletes and legacy applicants. All estimates are calculated in data from a certain Ivy-Plus college and for high school with at least 40 applicants across the years of our sample.

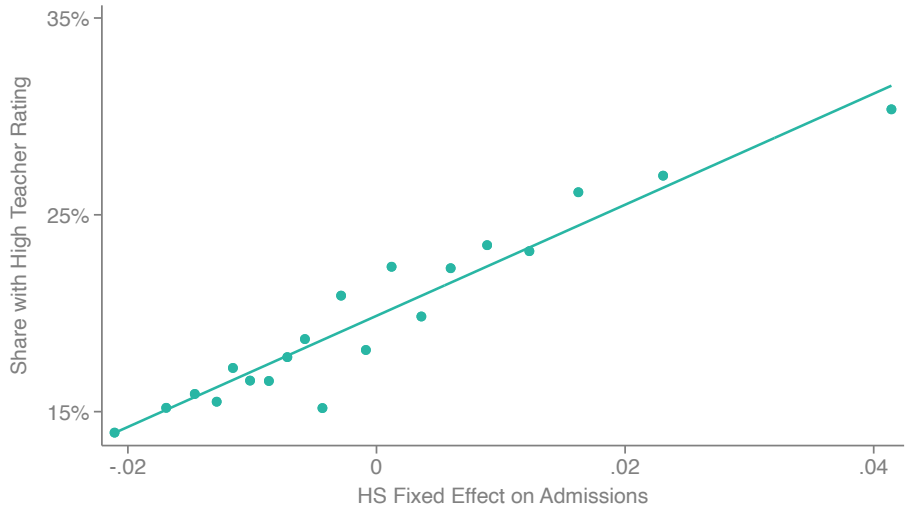
Figure A.14: Admissions Office Ratings vs. Test Scores, by Parental Income



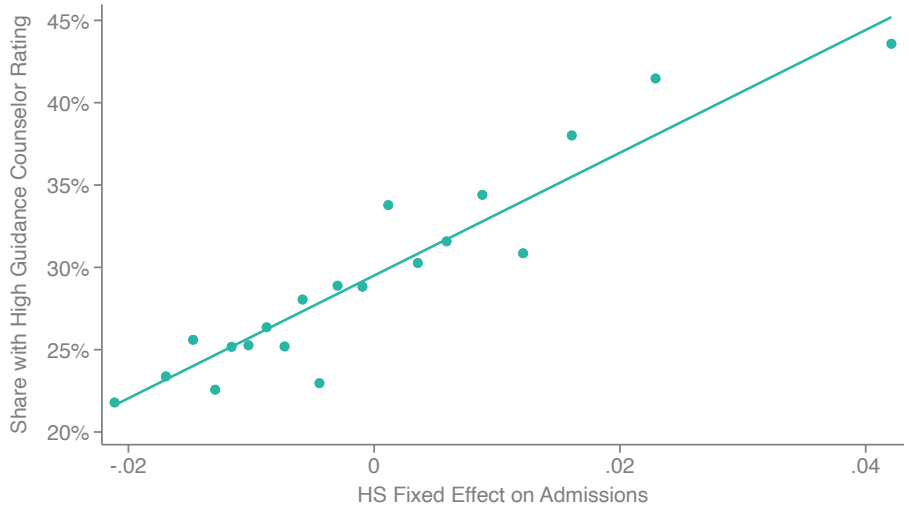
Notes: Figure A.14 is a binned scatterplot plotting the share of applicants given high non-academic ratings by student test score ventile, separately for students in the bottom 90 percent of the income distribution and those in the top 1 percent. The sample includes all domestic applicants applying in 2010–2015 with SAT scores greater than 1100 or ACT scores greater than 21, excluding recruited athletes, legacy students, and faculty children at a certain Ivy-Plus institution.

Figure A.15: Non-Academic Ratings by High School Fixed Effect on Ivy-Plus Admissions

(a) Share of High Teacher Rating by High School Fixed Effect



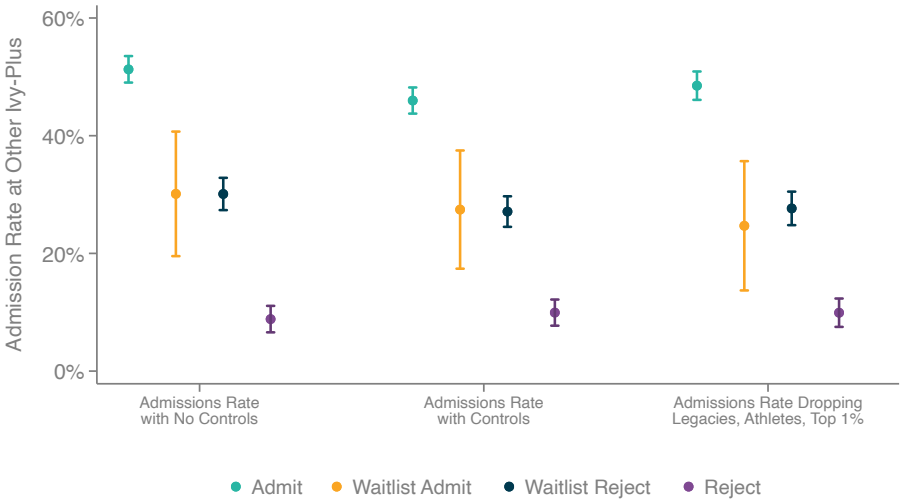
(b) Share of High Guidance Counselor Rating by High School Fixed Effect



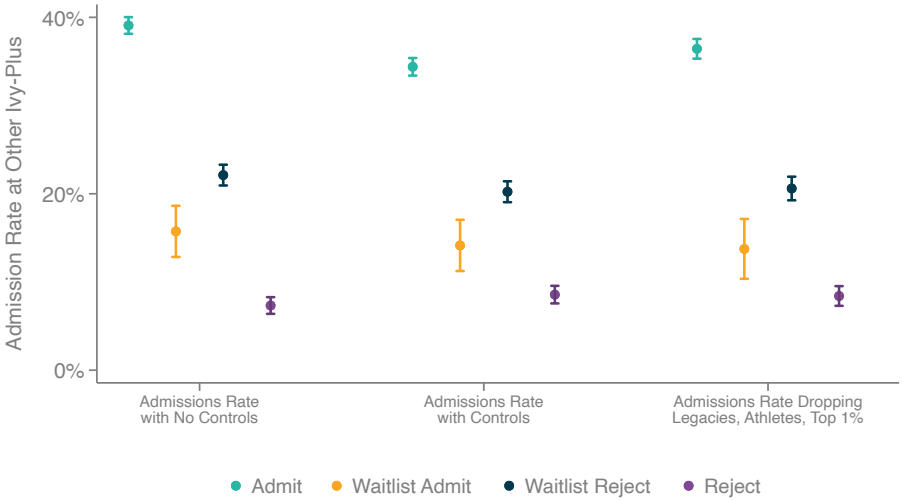
Notes: Figure A.15a replicates the teal line from Figure 8c but by high school fixed effect on Ivy-Plus admissions. Figure A.15b replicates the teal line from Figure 8c but by high school fixed effect on Ivy-Plus admissions. All estimates exclude recruited athletes and legacy applicants. All estimates are calculated in data from a certain Ivy-Plus college and for high school with at least 40 applicants across the years of our sample.

Figure A.16: Multiple-Rater Test for Idiosyncratic Variation in Admissions for All School Comparison

(a) Multiple-Rater Test without Private High School Attendees



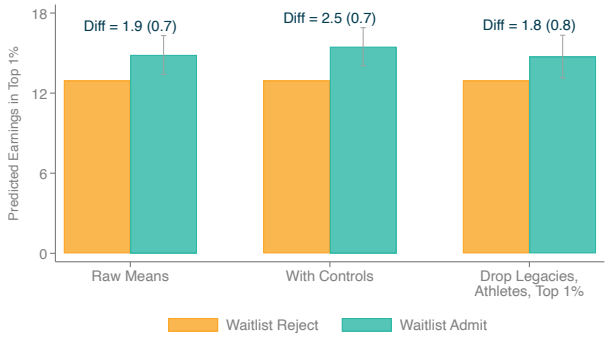
(b) Multiple-Rater Test for All School Comparison



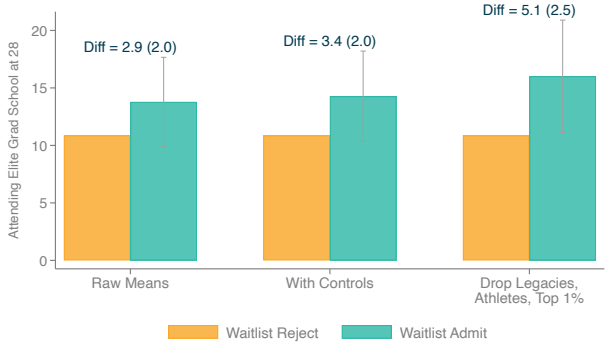
Notes: Figure A.16a replicates Figure 10 except excluding students from private high schools. Figure A.16b replicates Figure 10 except for all school comparisons, not only the higher ranked. See Figure 10 notes for more details on multiple-rater test.

Figure A.17: Treatment Effects of Ivy-Plus Admissions on Post-College Outcomes for Waitlist Applicants in the Higher-Ranked Multiple-Rater Test Subsample

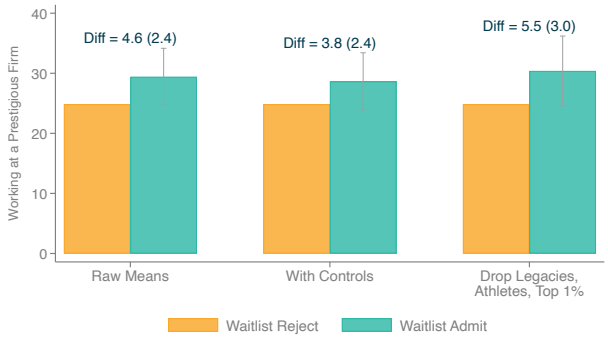
(a) Predicted Earnings in Top 1% Based on Firm at Age 25



(b) Elite Graduate School Attendance at Age 28



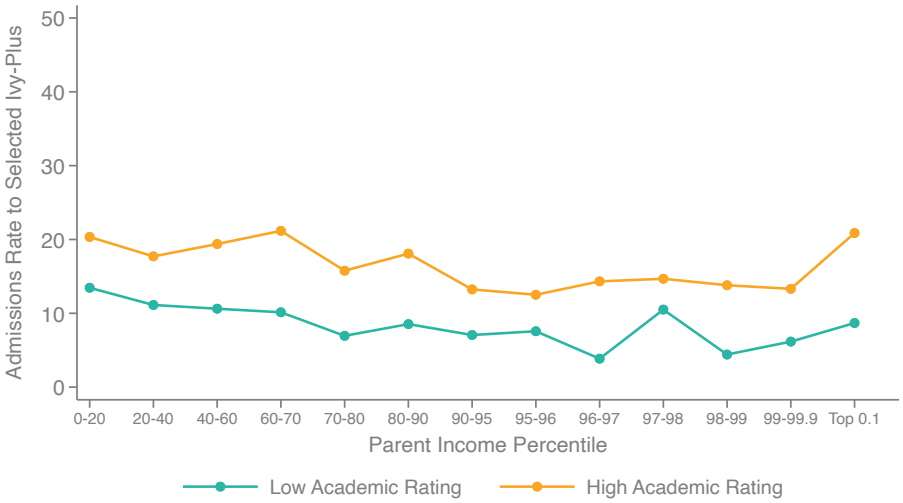
(c) Prestigious Firms Employment at Age 25



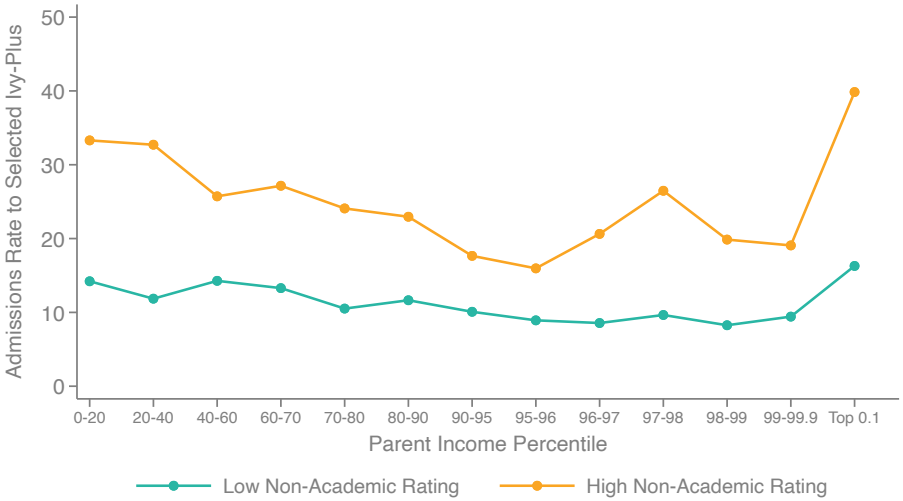
Notes: Figure A.17 replicates Figure 11b, Figure 14a, and Figure 14c for a subsample of passing multiple-rater test (higher ranked colleges). See Section 4.1 for more details on the definition of higher-ranked colleges and see Section 2.5 for more details on the definition of these outcome variables.

Figure A.18: Admissions to Other Ivy-Plus Institutions by Ratings, Controlling for Test Score

(a) Academic Ratings

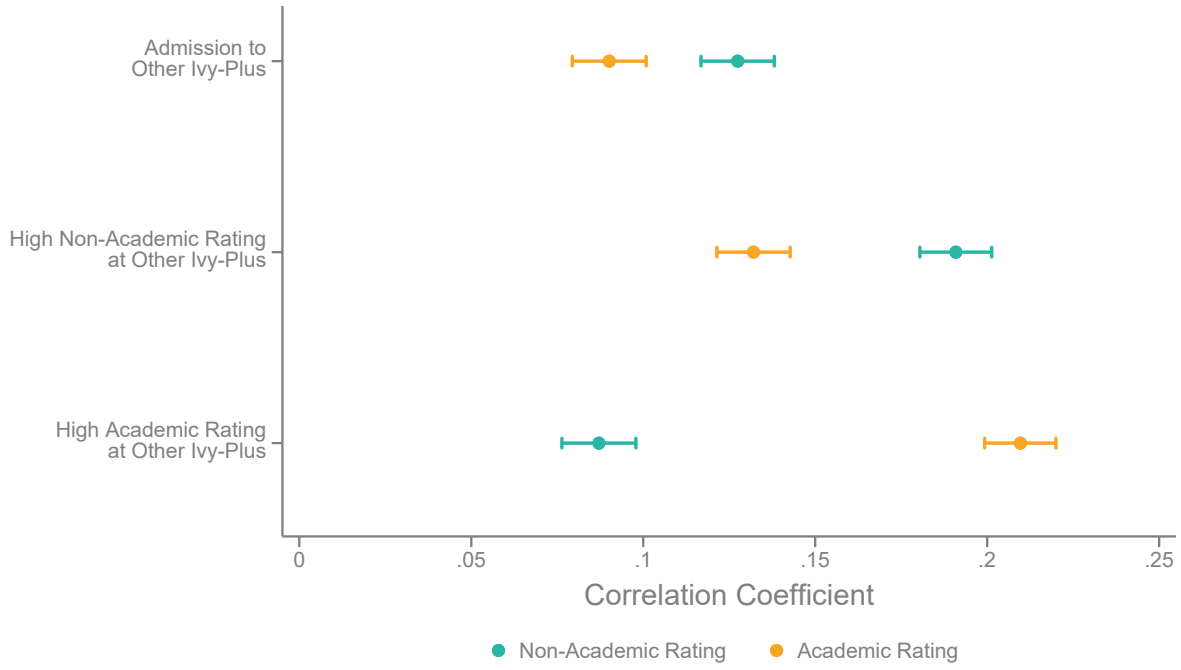


(b) Non-Academic Ratings



Notes: Figure A.18 shows how admissions rates at other Ivy-Plus institutions (reweighted by test score) vary with academic and non-academic ratings for student at a certain Ivy-Plus college, by parent income bin. We exclude legacies, recruited athletes, and faculty children, as well as students missing ratings.

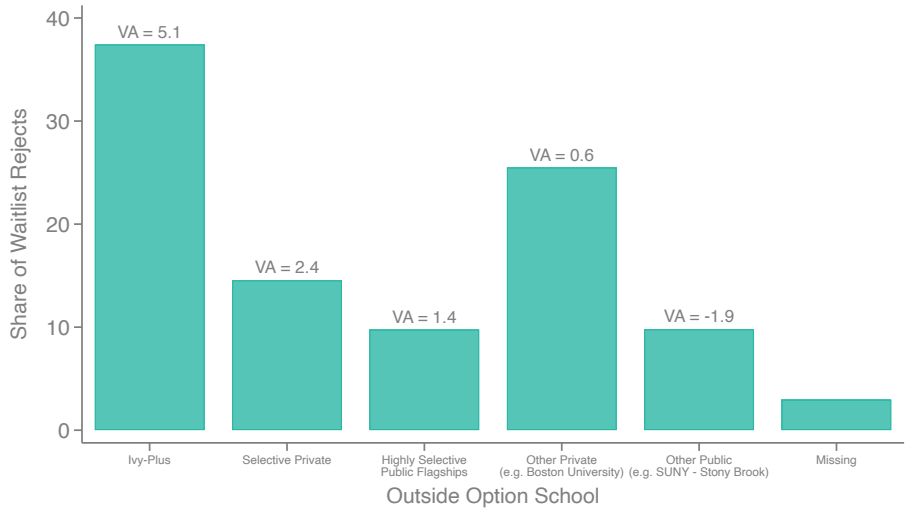
Figure A.19: Correlation of Ratings Between Ivy-Plus Colleges



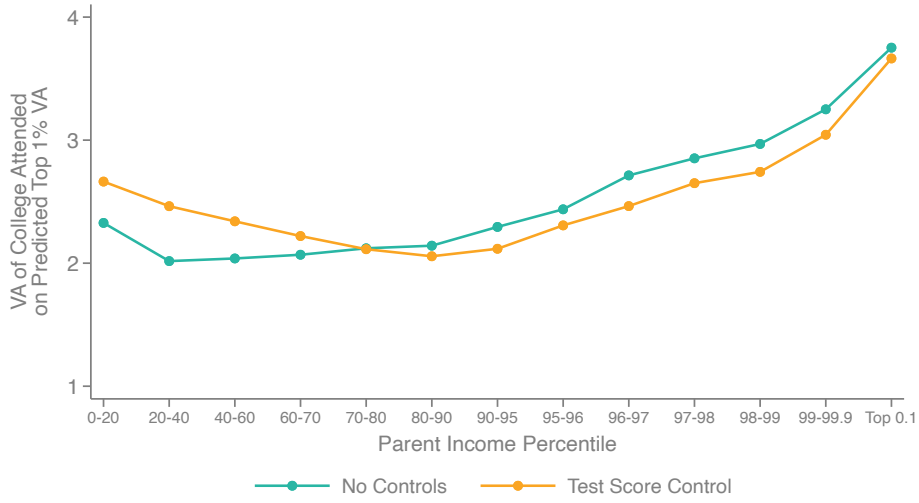
Notes: Figure A.19 plots the correlation coefficient between the residual variation in the academic and non-academic ratings that we observe in a certain Ivy-Plus colleges' internal data with admissions outcomes, overall ratings, and dummy variables of whether the applicants receive high ratings at other Ivy-Plus colleges, controlling for SAT scores and parental income. Lines show 95 percent confidence intervals, estimated using Fisher's transformation. We exclude early applicants, recruited athletes, faculty children, and legacy applicants.

Figure A.20: Distribution of Outside Options for Waitlist Rejects by Parent Income

(a) Value-Add of Schools Attended by Ivy-Plus Waitlist Rejects on Elite Graduate Schools Attendance



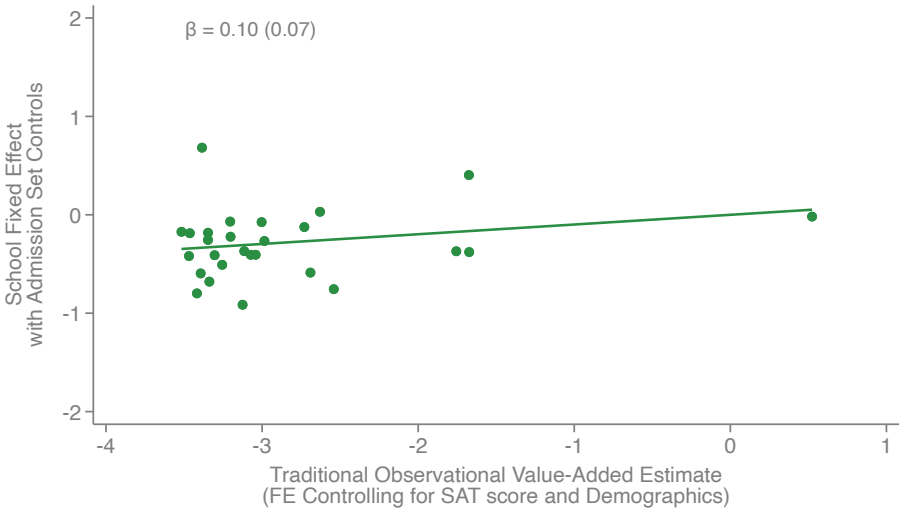
(b) Value-Add of Schools Attended by Ivy-Plus Waitlist Rejects on Predicted Top 1%



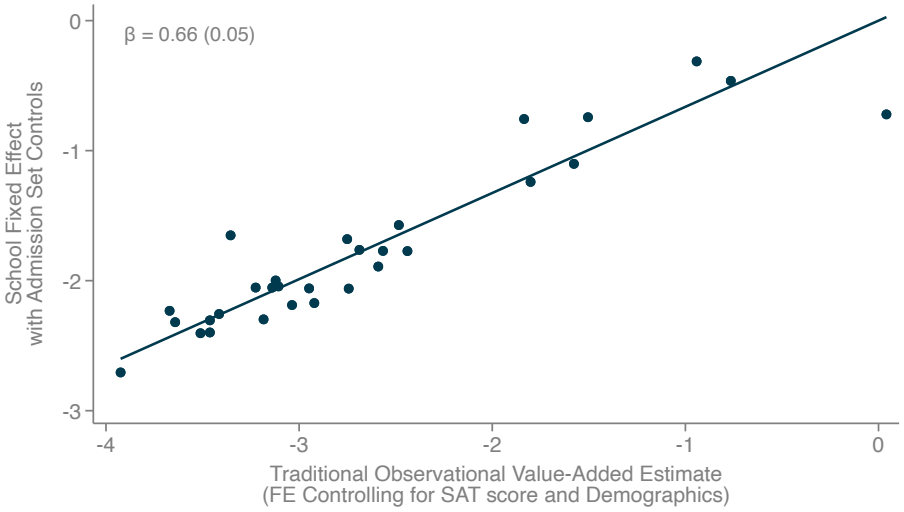
Notes: Figure A.20a shows the distribution of college tier attended by waitlist-rejected applicants to selected Ivy-Plus colleges for which we have internal records. The VA label in each top of the bars report the mean of value-add (VA) estimate of each school group for the predicted top 1% outcome. Figure A.20b shows students' outside options to the Ivy-Plus schools in our sample vary across the parent income distribution. It plots the mean value-added (VA) of college attended by applicants who were rejected off the waitlist at selected Ivy-Plus colleges for which we have internal records, by parent-income bin. The green line plots this series as the raw means (i.e., without any controls), while the orange line adjusts for the test scores of applicants using a quintic in test score. We calculate the VA for each college using predicted top 1% income based on the firm of employment at age 25. See the notes to Figures 11 and 14 for more details on the value-added models and outcome variables; see Section 2 for more detail on data sources.

Figure A.21: Matriculation Design for Different Set of Schools

(a) Causal Effects of Attendance to Texas Colleges on Predicted Top 1%, In-State Applicants



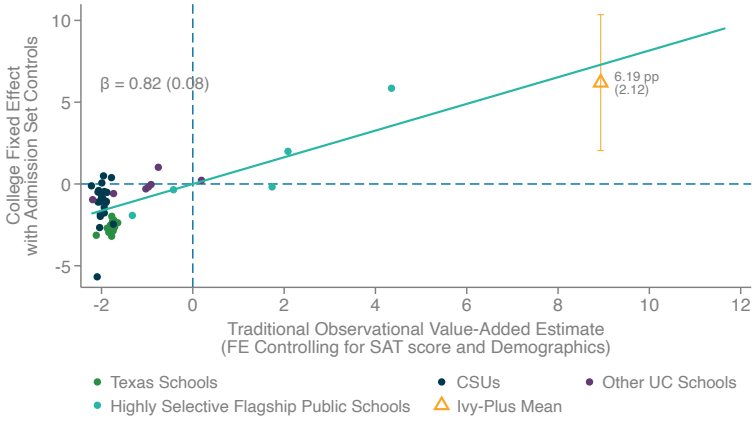
(b) Causal Effects of Attendance to California Colleges on Predicted Top 1%, In-State Applicants



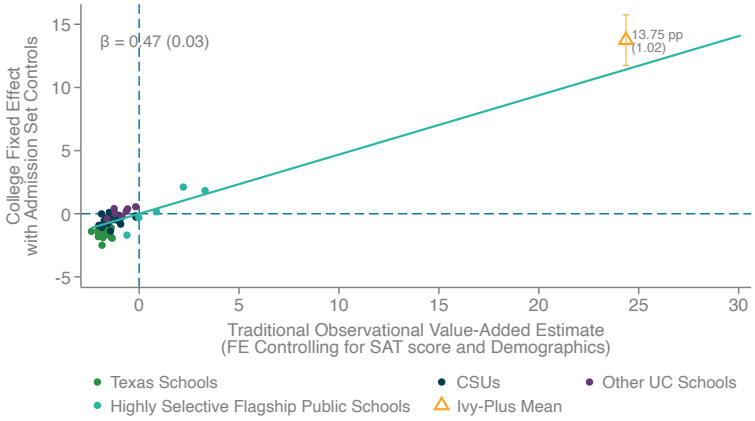
Notes: Figure A.21 replicates the matriculation-based design from Figure 15b, except restricting in each panel to a subset of the schools.

Figure A.22: Matriculation Design for Non-Monetary Outcomes

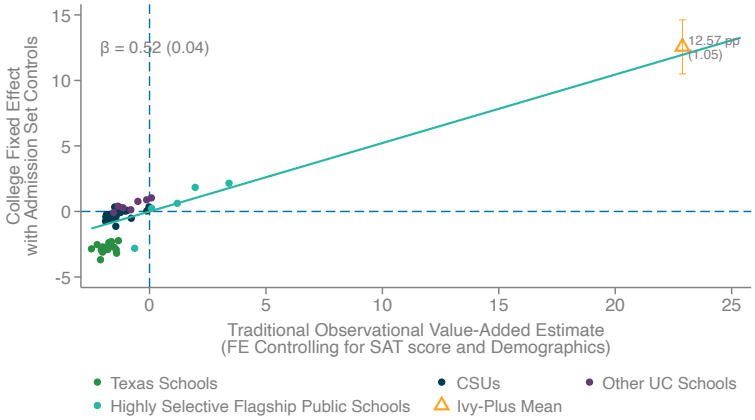
(a) Causal Effects of Ivy-Plus Attendance on Elite Graduate Schools Attendance



(b) Causal Effects of Ivy-Plus Attendance on Working at an Elite Firm



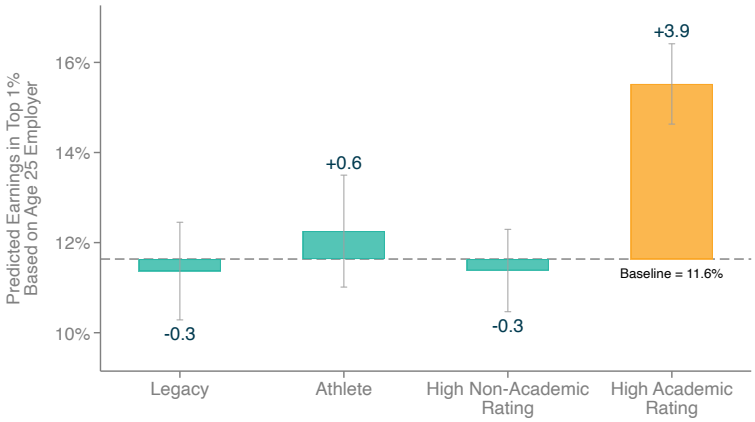
(c) Causal Effects of Ivy-Plus Attendance on Working at a Prestigious Firm



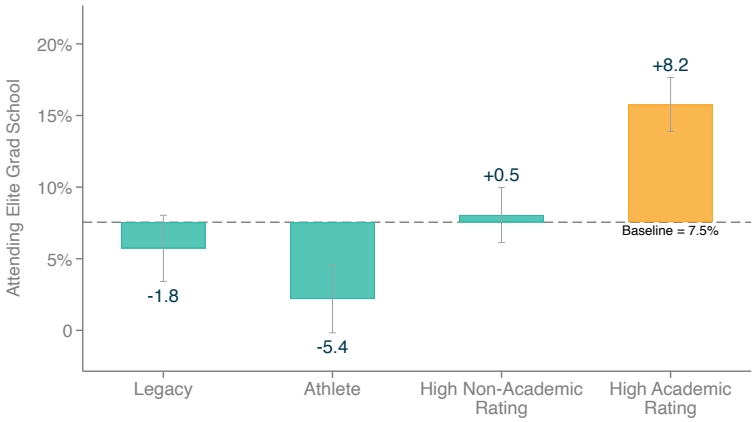
Notes: Figure A.22 replicates the matriculation-based design from Figure 15b, except with different student outcomes.

Figure A.23: Association Between Post-College Outcomes and Admissions Criteria among Ivy-Plus Matriculants

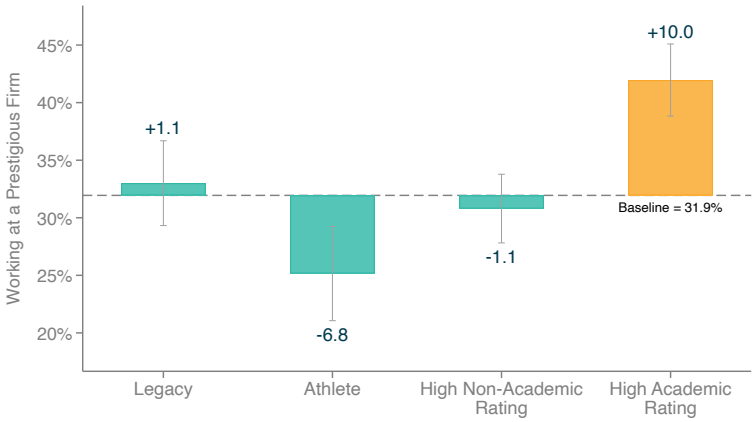
(a) Association Between Predicted Top 1% and Admissions Criteria



(b) Association Between Elite Graduate School Attendance and Admissions Criteria

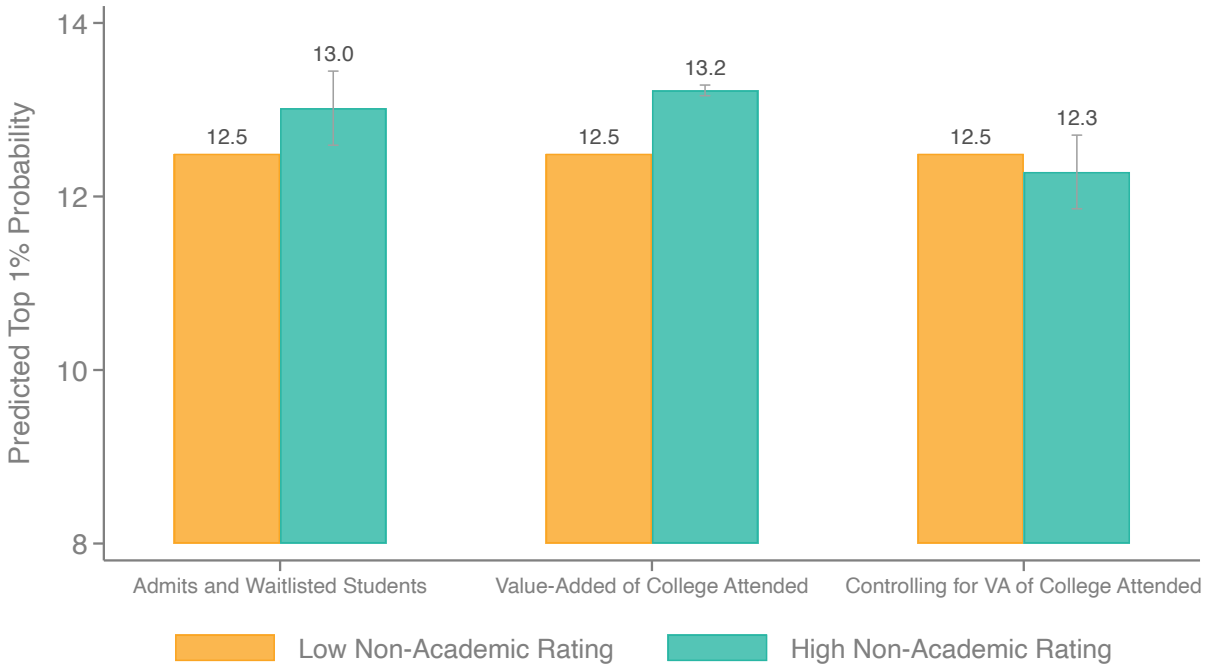


(c) Association Between Prestigious Firms Employment and Admissions Criteria



Notes: Figure A.23 replicates the "Raw Comparison" estimates from Figure 17a, except restricting to the students who attended a certain Ivy-Plus institution and (in Panels b and c) varying the student outcome measure.

Figure A.24: Post-College Outcomes Among Ivy-Plus Applicants by Non-Academic Ratings

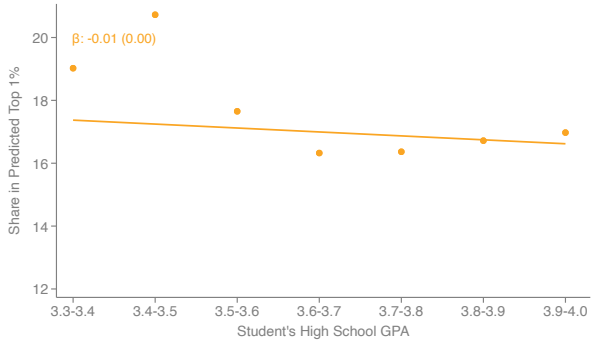


Notes: Figure A.24 compares the predicted top 1% share based on firm at age 25 of students either admitted or offered a place on the waitlist who receive high v.s low non-academic ratings. The first column shows the average predicted top 1% share of students with a low non-academic rating. The second column adds the coefficient on high non-academic rating to the first column, where this coefficient is estimated in a regression of predicted top 1% on an indicator for high academic ratings, high non-academic ratings, legacy status, and being a recruited athlete. The second pair of columns repeats the first set, except the fourth column add the estimated coefficient on high non-academic ratings from a regression of the VA of college attended on the same variables from the second column (see Section 4 for more details on the VA measure). The third pair of columns replicates the first pair, except adding the difference between the coefficients from the 2nd and 4th columns to the level in the 5th column to calculate the 6th column.

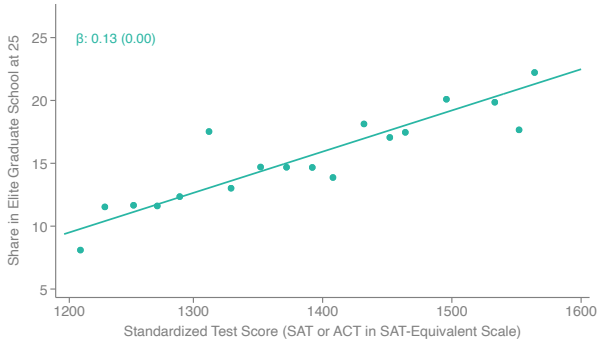
Figure A.25: Ivy-Plus Matriculants' Outcomes by Test Score and High School GPA

(a) Predicted Top 1% by Test Score

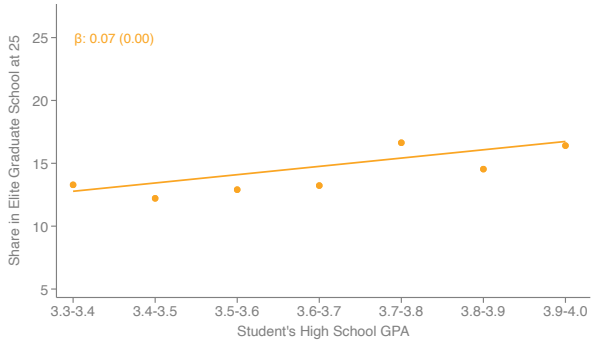
(b) Predicted Top 1% by High School GPA



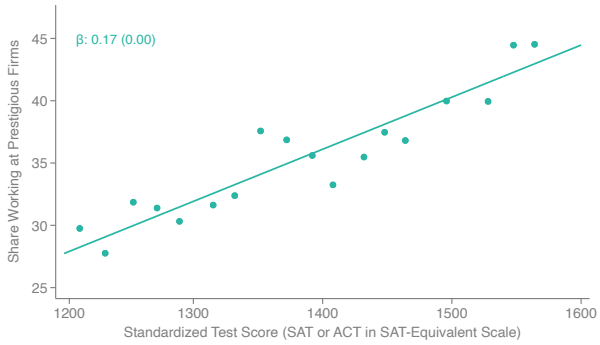
(c) Attending Elite Graduate School by Test Score



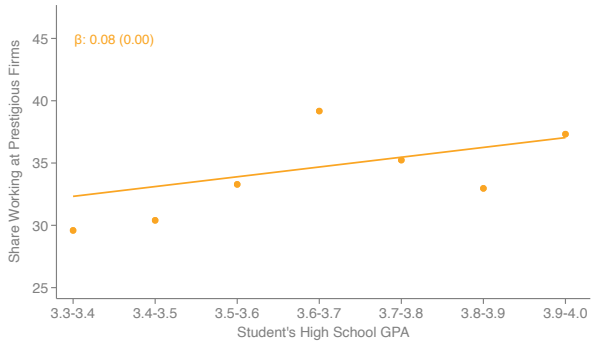
(d) Attending Elite Graduate School by High School GPA



(e) Working at Prestigious Firm by Test Score



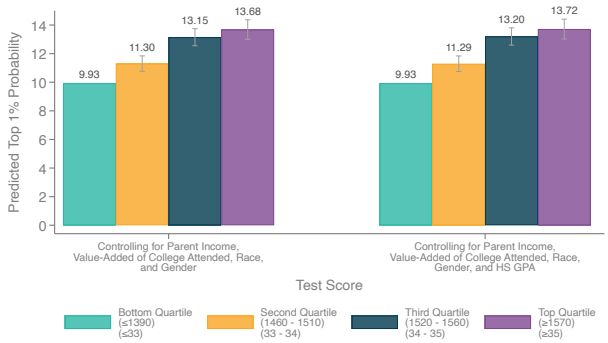
(f) Working at Prestigious Firm by Test Score



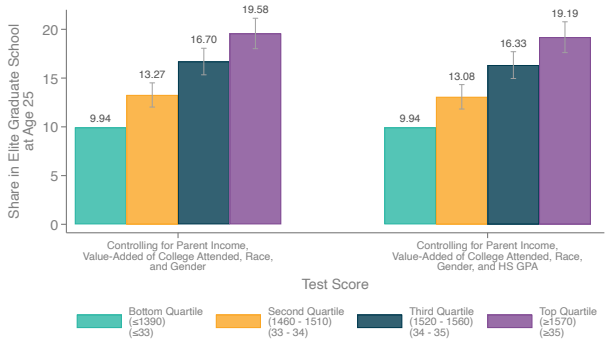
Notes: Figure A.25 plots the outcomes for matriculants to a selected subset of Ivy-Plus colleges by level of test score or high school GPA. Panels a, c, and e plot a binscatter of test score of a measure of student outcomes on test scores, controlling for parent income bin, race, gender, legacy status, recruited athlete status, and high school GPA. Panels b, d, and f repeat this but using high school GPA as the x-axis variable and controlling for the same set of variables plus test score (and without high school GPA). In these panels we plot one dot per tenth-point GPA bin, rather than by ventile. Our outcome measures are predicted top 1% based on age 25 firm (Panels a and b), attendance at elite graduate school at age 25 (Panels c and d), and working at a prestigious firm at age 25 (Panels e and f). In all panels we restrict the sample to domestic matriculants with test scores at or above an SAT of 1200 or ACT of 27 and GPA at or above 3.3; each pair of binscatters with the same outcome variable are on the same sample. See Section 2 for more details on the outcome variables and data sources.

Figure A.26: Ivy-Plus Applicants' Outcomes by Test Score

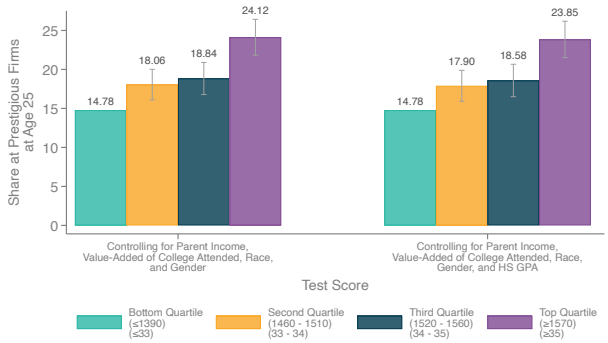
(a) Predicted Top 1% by Test Score



(b) Attending Elite Graduate School by Test Score

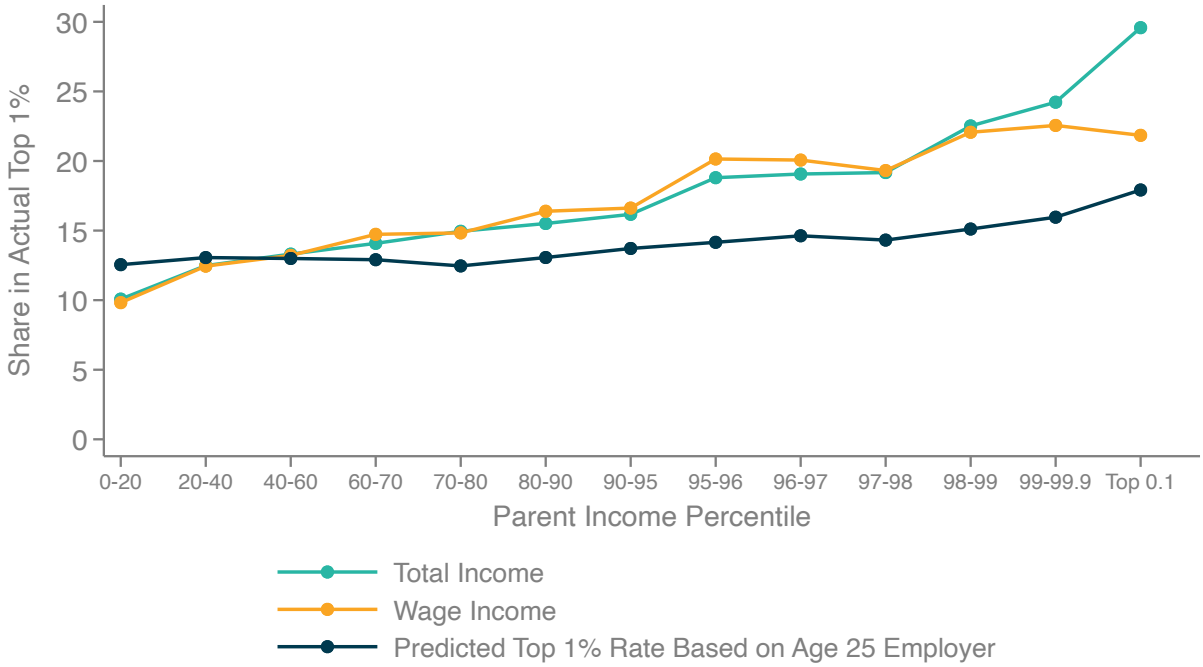


(c) Working at a Prestigious Firm by Test Score



Notes: A.26 plots the outcomes for waitlist applicants and admitted students to a selected subset of Ivy-Plus colleges by level of test score, and controlling for race, gender, home state, and college attended. To construct Figure A.26a, we regress our predicted top 1% predictor based on age 25 firm on indicators for each of the four indicated score score levels, as well as fixed effects for parent income bin, race, gender, and state. We repeat this regression with the VA of college attended (for predicted top 1% based on age 25 firm) as the dependent variable. The leftmost quartet of bars plots the four test-score-bin coefficient from the first regression minus those from the second; this procedure is the same as that in Figure A.23a, but for test score rather than other student characteristics. The rightmost quartet of bars replicates the leftmost procedure, but adding high school GPA as dummy variables to the set of controls in each regression. Figures A.26b and A.26c replicate A.26a but using indicators for attendance at elite graduate school at age 25 and working at a prestigious firm at age 25, respectively, as the outcome variables.The sample includes all domestic applicants to a certain Ivy-Plus college. Estimates are reported with 95 percent confidence intervals. Predicted Top 1%, elite firm, and prestigious firm definition is described in Section 2.5.

Figure A.27: Share of Ivy-Plus Attendees in Top 1% of Income Distribution at Age 33 by Parental Income



Notes: Figure A.27 replicates Figure 18b but using the share of students in the top 1% at age 33 (by total income and wage income) as the y-axis variable.