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

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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 a small number 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 on the entering classes of 2010-15 when analyzing attendance patterns and include earlier cohorts to analyze long-term post-college outcomes.

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 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 58% 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 ($91,000-$114,000), 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 31% 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 et al. 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 et al. (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 et al. 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 et al. (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 the 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 highly selective college in the U.S. has little impact on students' earnings (Dale et al. 2002, Dale et al. 2014, Mountjoy et al. 2021, Ge et al. 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 both) are significantly

[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.

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 8.8 percentage points, adding 145 students from

[5]This finding differs from that of Mountjoy et al. (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.

families earning less than $222,000 (the 95th percentile) to a typical Ivy-Plus college. This increase of 145 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 et al. 2004, Card 2017, Arcidiacono et al. 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 private colleges. While several studies have documented large causal effects of attending more selective public colleges using admissions thresholds to implement regression discontinuity designs in administrative data (e.g., Hoekstra 2009, Zimmerman 2014, Bleemer 2021b, Kozakowski 2023), private colleges do not use such admissions thresholds. Studies of private colleges' effects have instead exploited variation in matriculation conditional on admission and, as noted above, have found small impacts on average earnings. By linking data from multiple colleges to longitudinal data from tax records and standardized test score databases, we formulate novel research designs and study

[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.

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. Our findings serve to reconcile the results of prior studies in the U.S. and are also consistent with findings in other settings, such as Zimmerman's (2019) analysis of the impacts of 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 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 et al. 2022, Acemoglu et al. 2022, McGuirk et al. 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 spanning subsets of years from 1998-2015 from several Ivy-Plus colleges and highly selective public flagship universities, as well as data for all colleges in the University of California (UC) and California State University (CSU) systems and all four-year public colleges 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

[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.

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.

## 2.1 Sample Definitions

Our target analysis sample is college applicants who are U.S. citizens or permanent residents with parents in the U.S. who 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, we use three different samples in our analysis: one to analyze the pipeline of college enrollment across colleges, another to analyze college-specific admissions policies and their causal effects on post-college outcomes, and a third to predict 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 et al. (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

[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 et al. (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

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. We restrict the college-specific sample to the entering classes of 2010-15 when analyzing the pipeline to college attendance, but include earlier cohorts (where available) when analyzing post-college outcomes.

*Long-Term Outcomes Sample.* Because the data we have from most colleges are for relatively recent cohorts, we observe earnings when individuals are in their thirties for a relatively small sample. We address this limitation by building prediction models for later earnings based on individuals' employers at age 25, which we observe for many more students given our cohort restrictions. 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 comingled data from tax records (1098-T forms) and the National Student Loan Data System (Pell Grant records), as described in Appendix B of Chetty et al. (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 and might not receive a 1098-T form, we use data from Pell Grant records.[12] 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 most years between the ages of 19 and 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

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.

[12]Since we do not have Pell records after 2013, our approach could potentially understate college attendance rates for students receiving full financial aid in the most recent years of our sample. However, the vast majority of colleges submit 1098-T forms for all students irrespective of whether they make tuition payments or not. As a result, this missing data problem has little effect on our results: we find very similar estimates when excluding college-year cells where enrollment coverage may be incomplete.

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 et al. (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 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.[13]

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:

[13]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's internal data include scores on a separate writing section that was not administered systematically in all years of our sample.

*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.

*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 differ 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 et al. (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 parents' 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.[14] We then assign parents income ranks 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

[14] 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.

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).

*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 re-rank 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.

We validate this algorithmic approach to identifying elite and prestigious employers using two sources of external data. First, 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.

Second, we evaluate whether elite and prestigious firms serve as stepping stones to positions of political leadership by analyzing the share of senior government officials who were employed at the ten largest elite or prestigious firms identified by our algorithm. We obtain employment records of senior government officials from the OpenSecrets Revolving Door project, which uses publicly available records to identify 15,276 people who have served in both senior positions in the federal government and as registered lobbyists or political operatives at some point. We find that individuals who hold senior positions in government are several times more likely to have been employed at elite and prestigious firms than the average worker (Appendix Table 2), suggesting that these firms may serve as stepping stones to positions of leadership in society.[15]

*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 486,150 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 1374, significantly higher than the mean SAT score of 991 for test takers overall. Of these applicants, 24.2% attended an Ivy-Plus college, 11.3% 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

[15] Furthermore, we show in Section 5 below that even among Ivy-Plus students, those with higher test scores and academic ratings are far more likely to obtain jobs at elite or prestigious firms. These correlations further support the view that these firms are viewed as desirable by those who have broad options rather than simply capturing which firms happen to be more popular among Ivy-Plus graduates than flagship public graduates.

percentile of the individual income distribution on average, and 13.2% of them are in the top 1% (individual income > $261,000) 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 7.7% 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 3, 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.[16] 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 4).

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% (income > $611k) 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.[17]

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 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 resulting series to obtain measures of relative Ivy-Plus attendance rates by parental income controlling for test scores.[18]

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.[19] Students from the bottom 40% of the income distribution have slightly higher Ivy-Plus attendance rates than students from the middle class with

[16]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.

[17]See Appendix Table 5 for the dollar values corresponding to the quantiles of the parental income distribution plotted in Figure 2a.

[18]This reweighting method is analogous to that implemented in Section 5.2 of Chetty et al. (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.

[19]This statistic can be approximated from the distributions reported in Appendix Table 4: 15.8% 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.

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 (Appendix Figure 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 1.3 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 (Appendix Figure 3a).

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

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

where $N_{Top\,1\%,a}$ denotes the number of test takers with a score of $a$ from the top 1% and Attendance $\text{Rate}_{P70-80,ac}$ 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 93 students from the top 1% in the average Ivy-Plus class – 168 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 168 "extra" students from the top 1% (10.2% of total enrollment) relative to what one would expect 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 et al. 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 168 students from top 1% families. Note that in this section, we simply seek to understand what drives the presence of the additional 168 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 does each of these margins contribute to the additional 168 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).[20]

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 (Appendix Figure 5a).

[20] 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.

Application rates vary more sharply with parental income at other colleges. Selective private colleges receive 85% more applications from students in the top 1% than students with comparable test scores in the middle class, with even larger differences at certain colleges such as Georgetown and Vanderbilt (Appendix Figure 5b). 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 (Appendix Figure 3b).

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. Attendance conditional on application rises sharply in the upper tail of the income distribution at Ivy-Plus and other highly selective private colleges, but is 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 6).

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 1.7 times higher for students from the top 1% than for those from the middle class; the average of the ratio for the colleges we have internal data from is approximately 1.8.[21]

*Admissions.* Figure 4a plots admissions rates by parental income for applicants to the Ivy-Plus and public flagship colleges in our college-specific sample. As above, we reweight within each income bin to match the 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 applicants 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 44% 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 again persist after controlling for differences across racial and ethnic groups (Appendix Figure 2b).

*Matriculation.* Figure 4b plots matriculation rates of admitted students at selected Ivy-Plus colleges and highly selective public flagships by parental income, again controlling for test scores by reweighting as above. Applicants with parental incomes in the top 1% are 1.15 times more likely to attend Ivy-Plus colleges once admitted than those from the middle class. Most of this gradient arises from differences between the matriculation rates of those students admitted in early vs. regular admissions rounds; high-income students

[21]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.

are more likely to apply and be admitted in the early admissions round, where matriculation rates are higher (Appendix Figure 7). Selective public flagships display a similarly flat pattern across the income distribution, with high-income students exhibiting slightly lower matriculation rates.

*Quantification.* The preceding analysis suggests that differences in admissions—rather than application or matriculation—drive most of the gap in attendance rates by parental income at private institutions. We now quantify the relative importance of these sources in explaining the "extra" 168 students from the top 1% who attend Ivy-Plus colleges (see Appendix I for details on our methods).

We begin by focusing on students who are not recruited athletes. We do so because athletes are typically invited to apply to Ivy-Plus colleges only if they are likely to be accepted and have committed to attend, making it difficult to quantify the relative importance of application, admissions, and matriculation for them. We first calculate how many non-athletic applicants 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:

$$\begin{aligned}\text{Equal Admit CF}_c = \sum_a N_{Top\,1\%,a} &\times \text{Application Rate}_{Top\,1\%,ac} \\ &\times \text{Admission Rate}_{P70-80,ac} \times \text{Matriculation Rate}_{Top\,1\%,ac}\end{aligned} \tag{2}$$

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. Using this approach, we find that 87 out of the 168 extra top 1% students can be accounted for by the higher admissions rates of non-recruited-athletes from the top 1% (Table 2). 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 20 students. Finally, equating application rates across the income distribution would reduce the number of students from the top 1% by an additional 34 students. Together, the three components of the pipeline for non-athletes account for 141 of the 168 extra students from the top 1%.

The rest of the extra students from the top 1% come via athletic recruitment. Figure 5a illustrates this by plotting 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 13% for students from the top 1%, with a slightly steeper income gradient for females relatives to males (Appendix Figure 8). 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 colleges, there is no difference in the share of recruited athletes across the income distribution (Figure 5b).

To calculate the number of extra top 1% students due to athlete recruitment, we adjust the fraction of athletes from the top 1% to match that from the middle class in the new counterfactual student body, after equating application, admissions, and matriculation rates across parental income groups for non-athletes conditional on test scores as above. 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. We find that 27 extra students from the top 1% come from athletic recruitment, mechanically explaining the remaining number of extra students from the top 1%.

In sum, two-thirds of the extra top 1% students (114 out of 168) can be accounted for by Ivy-Plus colleges' admissions practices, with 87 coming from higher admissions rates for non-athletes and 27 coming from the disproportionate representation of high-income students among recruited athletes.[22]

## 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 whose parent(s) attended the college to which they apply – receive special consideration in college admissions (Espenshade et al. 2004, Bowen, Kurzweil, et al. 2006, Hurwitz 2011, Arcidiacono et al. 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 6a characterizes the first factor, plotting the share of legacy applicants by parental income group, reweighted on test scores across parental income groups as above.[23] Overall, legacy applicants constitute 2.5% of the applicant pool. This fraction rises monotonically with parental income, rising to more than 9% for applicants from the top 1%.

The series in green dots in Figure 6b 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 3-fold advantage at lower income levels.[24]

[22]Because (2) is multiplicative, the results of this decomposition analysis depend upon the order in which one changes each of the three margins. In Appendix Table 6 we present a decomposition that averages across the different orders in which the three margins (admissions, application, matriculation) could be changed. Averaging across orderings, admissions still account for 58% (96 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.

[23]See Appendix Figure 9b for statistics on legacy shares and admissions rates by parent income that does not control for test scores.

[24]The legacy advantage is larger for applicants from high-income families partly because there is a complementarity between the legacy boost and other factors that correlate with parental income, such as non-academic ratings, attending a private high school, etc. The 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 (Appendix Figure 10).

The higher admissions rates for legacy students in Figure 6b 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 scores. 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 of an indicator for admission on fixed effects for all combinations of students' academic and non-academic ratings, application round, entering class, race, first-generation status, gender, parental income group, high school GPA (where available), and high school fixed effects, reweighting observations so that the distribution of test scores in each parental income bin matches that distribution for students attending the relevant college. 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 high schools legacies actually attended because the set of high schools does not overlap perfectly across legacy and non-legacy applicants).

The dashed line in Figure 6b 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. This 4-fold legacy admissions advantage is comparable to the implied admissions advantage given to recruited athletes based on the same admissions model.[25]

The preceding approach relies on a "selection on observables" assumption – namely that controlling for the factors observed in application 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.[26] Figure 6c shows that legacy students are accepted at

[25]The predicted values from the admissions model imply that absent athletic preferences, only 11.1% of recruited athletes would be admitted given their application credentials; for applicants with comparable characteristics to non-athletic applicants who are currently admitted, predicted admissions rates are 4 times higher, at 44.5%.

[26]We exclude students who applied early to one of the colleges from this analysis since the decision to apply to another

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.[27] 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 52 additional students from the top 1% (Table 2).[28] Legacy preferences thus explain 52 of the 87 extra students from the top 1% who are not recruited athletes.

*Application Credentials.* To understand the source of the remaining 35 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.[29] 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 7a 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

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

[27] 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. Appendix Figure 11a 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.

[28] 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 et al. (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.

[29] 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 12 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.[30]

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 et al. 2023). Figure 7b replicates Figure 7a, 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 1.4 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 grows with students' test scores; that is, students with the strongest non-academic credentials tend to be those who have strong academic records *and* come from high-income families (Appendix Figure 13).

*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 7c) – 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.[31]

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.[32] 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 8a).[33] Since

[30] 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 7a is that these forces roughly cancel each other out on average.

[31] 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.

[32] 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 et al. 2022, Bastedo et al. 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.

[33] Although the two-fold difference in admissions rates between students who attend non-religious private high schools vs. public schools is smaller than the four-fold boost in admissions from being a legacy applicant at any given Ivy-Plus college, the effect of attending a private high school on an applicant's overall chances of attending an Ivy-Plus college may be larger than the legacy boost because applicants who attend private high schools have higher admissions rates at *all* Ivy-Plus colleges,

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 (Figure 8b).

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 8c plots the share of students with high non-academic and academic ratings by ventiles of estimated high school fixed effect, reweighting on test score.[34] 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 14). Consistent with the results in Figure 8a, 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 15). 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 from non-academic distinctions outside the classroom.

These differences in non-academic credentials turn out to 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 35 (Table 2), accounting for the remaining "extra" top 1% non-athlete students due to admissions.[35]

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.

whereas the legacy advantage applies only at the colleges that the applicant's parents attended.

[34] 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.

[35] 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 conclusions, reweighting on non-academic ratings or controlling for high school fixed effects nearly eliminates the top 1% admissions advantage conditional on test scores (Appendix Figure 16b).

# 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.[36]

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

$$Z_{ij} = \gamma_{1j} X_{1i} + \gamma_{2j} X_{2i} + \eta_i + \epsilon_{ij}, \tag{3}$$

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 $\gamma_{1j}$ and $\gamma_{2j}$, may vary across colleges. Students' ratings also depend upon two other unobserved components that are uncorrelated with potential outcomes: a component $\eta_i$ 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 $\epsilon_{ij}$ 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 $\epsilon_{ij}$ 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

[36] 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 $\epsilon_i^Y$, and college-specific value-added:

$$Y_i = \sum_{j \in J_i} D_{ij}\phi_j + \beta_1 X_{1i} + \beta_2 X_{2i} + \epsilon_i^Y, \tag{4}$$

where $\phi_j$ denotes college $j$'s causal effect (value-added) on $Y_i$. This model assumes that college value-added $\phi_j$ 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 $\eta_i$ and $\epsilon_{ij}$ in admissions ratings are orthogonal to the error term in students' long-term outcomes $\epsilon_i^Y$ ($Cov(\epsilon_{ij}, \epsilon_i^Y) = Cov(\eta_i, \epsilon_i^Y) = 0$), since unobservable factors that affect both admissions and long term outcomes are captured in $X_{2i}$.

Our goal is to estimate $\phi_A$, 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 et al. (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 $\phi_A$ 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 $\phi_A$ 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 the 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:

$$r_A = (E[Y_i|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] - E[Y_i|P_{iA} = 0, X_{1i}, \tilde{X}_{2i}])/E[D_{iA}|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] \quad (5)$$

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 $A$.[37] 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:* $\gamma_{2A} > 0 \Rightarrow \gamma_{2B} > 0$

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 (5) 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 $\tilde{C}_j = C_j - \gamma_{1j}X_{1i} - \gamma_{2j}\tilde{X}_{2i}$ 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:

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

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

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

[37] When students apply to multiple colleges, the reduced-form comparison between applicants admitted vs. rejected from college A will capture the difference between $\phi_A$ 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 $\phi_A$ , the effect of attending $A$ relative to the average highly selective public flagship college (see Section 4.2 and Figure 12a).

Since $\epsilon_{iA} \perp \epsilon_{iB}$ and $\epsilon^X_{2i} \perp \epsilon_{ij}$, it follows that if $Var(\epsilon^X_{2i}) > 0$ and $\gamma_{2A} > 0$, then $\gamma_{2B} > 0$ 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 $A$.[38]

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, $\gamma_{2B} = 0$, 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 $\gamma_{2A}$ and $\gamma_{2B}$ (the weights placed by colleges on those factors); without a restriction on $\gamma_{2A}$ relative to $\gamma_{2B}$, there is no way to identify

[38]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 $\eta_i$ that affects college admissions but is not correlated with long-term outcomes. Hence, finding $T_{B|A} = 0$ is sufficient but not necessary for (5) to yield an unbiased estimate of $\phi_A$.

the magnitude of $Var(\epsilon^X_{2i})$ 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 et al. 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 et al. (2002) and refined using richer data by Mountjoy et al. (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 et al. (2002) and Mountjoy et al. (2021) 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:

$$r_M = E[Y_i|D_{iA} = 1, X_{1i}, J_i = \{A, O\}] - E[Y_i|D_{iO} = 1, X_{1i}, J_i = \{A, O\}] \tag{6}$$

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 et al., 2021) immediately implies that $r_M = \phi_A$ (recalling that $\phi_O$ 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 et al. (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 (6) would yield downward-biased estimates of $\phi_A$. 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 et al. (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., $\phi_A$ 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 10.4% 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.[39] 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 $\epsilon_{ij}$ 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

[39]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 (3) 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.[40]

The first column of Figure 9 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 50% 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.[41] 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.[42]

In the second column of Figure 9, 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 inclusion of additional controls reduces the gap in admissions rates between accepted and rejected applicants

[40]We identify college rankings based on students' preferred choices when admitted to multiple colleges in our own sample, which accords with the revealed-preference rankings of colleges constructed by Avery et al. (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 off the waitlist in the reference college (Appendix Figure 17b). 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 (Appendix Figure 18). 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 $\epsilon_{iA}$ and $\epsilon_{iB}$), our estimator would understate the causal effect of admission to college $A$.

[41]Furthermore, academic and non-academic ratings at a given Ivy-Plus college predict admission outcomes at other Ivy-Plus colleges (controlling for SAT scores and parental income), 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 (Appendix Figure 19).

[42]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 other colleges. 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 smaller 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.

not placed on the waitlist, consistent with the larger differences in credentials between those applicants. The third column of Figure 9 presents estimates with college $B$'s ratings (rather than its 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.[43] The first row of Table 3 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 ($p = 0.93$) difference of -0.01 (relative to a standard deviation within the non-admitted group of 3.5). In the second row of Table 3, we obtain a similar result when predicting another outcome we analyze – an indicator for attending an elite graduate school attendance at age 28 – 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 differ significantly from those who are rejected: parental income and legacy status. Children of alumni (legacies) and those from the top 1% are significantly more likely to be admitted to Ivy-Plus colleges off the waitlist;

[43]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.

indeed not all Ivy-Plus schools admitted students off the waitlist in a need-blind fashion in the period that we study. 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.[44] 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 establish 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, athletes, and students with parents in the top 1% and show that we obtain very similar results to those reported below (see Appendix Table 7).

In sum, the balance tests show that admissions from the waitlist are non-random, but are 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 (5) to identify the causal effects of admission.

*Results.* Figure 10 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 (5). 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 affects the probability of reaching the top 1% of the income distribution at age 33. The first pair of bars in Figure 10a 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 this estimate is adequate to reject the null hypothesis that admission to any Ivy-Plus college has no effect on outcomes, it has three limitations: first, the estimate of the effect size is imprecise, as shown by the wide confidence interval in Figure 10a); second, the magnitude of the treatment effect is difficult to interpret because the outside option of students who are rejected has not been pinned down; and third, this analysis only captures the monetary impacts of Ivy-Plus attendance. The rest of this section addresses these three limitations.

*Increasing Precision.* The reason the estimate in Figure 10a 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 11a demonstrates this pattern within our data by

[44]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.

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 1%, consistent with the estimates in Figure 10a. 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 10b 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 Figure 10b. Further limiting the sample to exclude legacy applicants, athletes, and students with parents in the top 1% – the attributes that are unbalanced in Table 3 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 10b.

To benchmark the magnitude of these treatment effect estimates, we compare them to what 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 10b (also reported in Column 1 of Appendix Table 7) 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 7), similar to the point estimates obtained when examining actual outcomes.

Figure 10c replicates the analysis in Figure 10b 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 college has a small impact on mean income rank. We find similarly small effects when using mean income ranks at age 33 and when using observational value-added estimates of colleges' effects (Appendix Table 7). The small impacts on mean ranks arise because Ivy-Plus attendance primarily shifts outcomes within the upper tail, a finding we revisit in greater detail in Section 4.4 below when characterizing treatment effects across quantiles of the income distribution.

*Heterogeneity in Outside Options.* The magnitudes of the reduced-form estimates reported in Figure 10 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 9. 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 (Appendix Figure 20a).

To identify the causal effects of Ivy-Plus attendance relative to the fixed outside option of attending a highly selective state flagship college ($\phi_{Ivy}$), we first estimate how the 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 another 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 the strength of outside options, we classify applicants to each Ivy-Plus college into groups based on their home state, parental income, and race. We estimate the quality of outside options that applicants in each of these groups have as the mean observational value-added (estimated using a regression of outcomes on 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 2021a). For instance, if the return to attending an Ivy-Plus college were different for 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 (see Section 4.4 below), suggesting that this assumption is a reasonable approximation.

Figure 12a plots the treatment effect of being admitted from the waitlist on the share of students predicted to reach the top 1% (based on their age 25 employers) vs. the strength of their outside options, controlling for fixed effects for the Ivy-Plus college to which they applied. To construct this figure, we bin the outside options measure described above into ventiles (20 equal-sized bins) and then plot the mean treatment effect from the waitlist design vs. the predicted value of the mean outside option within each of these bins (see Appendix G for details). 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 the Ivy-Plus colleges in our college-specific sample (on the far right side of the figure) gain very little from admission to one of those Ivy-Plus colleges (a treatment effect near 0). 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 $\phi_{Ivy} = 4.58$ 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 12b evaluates this assumption by replicating Figure 12a using predicted chances of reaching the top 1% based on pre-determined characteristics (estimated as in the balance test in Table 3) 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 3. 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 12a is 0.87, 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 Appendix Table 8), we find estimates ranging from 0.81-1.05, 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 $\phi_{Ivy}$ that does not require estimating heterogeneous 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 7, 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 7, 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 4) of $\phi_{Ivy} = 4.70$ (s.e. $= 1.17$), nearly identical to that obtained from estimating heterogeneous treatment effects by outside options in Figure 12a. We use this less data-intensive estimator for $\phi_{Ivy}$ below because it yields more precise estimates, especially in smaller subgroups.

In Columns 5 and 6 of Table 4, 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 4 from the observed Ivy-Plus means in Column 6 of Table 4. 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% 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 elite (highly ranked) graduate schools as defined in Section 2. Figure 11b replicates the analysis of treatment effects by age in Figure 11a 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 13a 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 7, 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 5.5 pp, from 6.2% to 11.7% (Table 4, 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 4, Panel B).

Of course, attending an elite graduate school or working at a firm that channels many employees to the top 1% are only some of the 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 13b 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 9.8 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 18.3 pp, from 7.2% at highly selective state flagships to 25.5% at Ivy-Plus colleges.

Elite firms include many firms that are also high-paying as well as firms that are attractive for non-monetary reasons. As discussed in Section 2, we isolate the latter component by residualizing the ratio used to define elite firms with respect to the predicted top 1% measure that we use above. We then define "prestigious" firms as those that rank highly on this residual. Figure 13c 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 8.4 pp. These estimates are, as with other outcomes, insensitive to the inclusion of controls and similar to what one would predict based on observational value-added estimates. The rescaling estimator implies that Ivy-Plus attendance increases the probability of working at a prestigious firm by 16.4 pp, from 8.0% 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 et al. (2002) and Mountjoy et al. (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 et al. (2021). The y axis of Figure 14a 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 0.68. 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 14b, 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 et al. 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.[45] 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 relative to the average highly selective public flagship of approximately 4 pp.

[45] Mountjoy and Hickman focus on in-state applicants in Texas; we replicate their results restricting to that sample in Appendix Figure 21a. In-state applicants to public four-year schools in California, shown in Appendix Figure 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.

When we replicate this analysis using predicted 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 14c). The estimates on mean ranks obtained from the matriculation design are very similar to those obtained from the waitlist design. We also find small, statistically insignificant effects of Ivy-Plus attendance on log earnings (Appendix Table 7), reconciling our findings with those of Dale et al. (2014).[46] Figure 14c also explains why regression-discontinuity (RD) studies (starting from Hoekstra 2009) have found larger causal effects of attending a more selective college than matriculation-based studies (starting from Dale et al. 2002): RD studies typically compare two-year and four-year colleges that have significantly different value-added on mean earnings, whereas matriculation-based studies (with the exception of Mountjoy and Hickman's analysis of Texas colleges discussed above) typically compare elite colleges that have relatively similar value-added on mean earnings.

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 Appendix Figure 22 and Table 4), with magnitudes similar to those obtained from our first research design.

## 4.4 Heterogeneity, Quantile Treatment Effects, and Selection vs. Causal Effects

In this subsection, we compare estimates across our designs and then use them to analyze heterogeneity in treatment effects, characterize treatment effects across the income distribution, and analyze the fraction of observed variation in outcomes across colleges that is due to selection vs. causal effects.

Table 4 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 similar estimates for all of these outcomes across all three estimators. We find highly significant ($p < 0.001$) treatment effects ranging from 4.2-5.4 pp for the predicted probability of reaching the top 1% across the three estimators. Treatment effects on the probability of being in the top quartile and on mean income ranks are much smaller. Similarly, we find significant impacts on the probability of attending an elite graduate school, but much smaller, statistically insignificant effects on attending a non-elite graduate school. Finally, we find large, positive effects (exceeding 13 pp) across all the estimators on the probability of working at an elite or prestigious firm.

*Heterogeneity.* The similarity 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 significantly improves children's long-term outcomes. It also suggests that the treatment effects of Ivy-Plus attendance (after accounting for outside options) are not highly heterogeneous

[46] 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 et al. 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.

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 14d plots treatment effects on the predicted probability of reaching the top 1% by parental income group, replicating the matriculation design above separately by parent income level. Attending an Ivy-Plus college instead of a state flagship university has a similar and positive effect along the entire parental income distribution.[47] We find similar evidence of relatively homogeneous treatment effects across the parental income distribution using the waitlist design, albeit with less precision (Appendix Table 9, Column 1). Most importantly, we find that attending an Ivy-Plus college has large positive treatment effects on upper-tail success even for lower-income and middle-class students across both designs. We also find relatively similar, consistently positive treatment effects across other subgroups: by test score, academic and non-academic ratings, legacy status, and athlete status (Appendix Table 9).

*Quantile Treatment Effects.* Leveraging the similarity of the estimates across estimators, we compare the income distribution of Ivy-Plus students to students from highly selective public flagships with similar test scores to characterize the distributional impacts of Ivy-Plus attendance more precisely.[48] Figure 17b plots the ratio of the density of the individual income distribution at age 33 for students who attended Ivy-Plus vs. highly selective public flagship colleges, both unconditionally and reweighting on test scores, parent income, gender, and race. The fraction of non-working individuals (those with 0 individual income) is approximately the same (with a ratio of 1) in both groups, consistent with estimates using the waitlist design (Appendix Table 7). Ivy-Plus students are less likely to earn between the 15th and 95th percentile of the income distribution relative to peers at state flagship colleges. Ivy-Plus students are much more likely to reach the very top of the income distribution: they are 1.4 times as likely as state flagship students with comparable test scores and demographics to have incomes between the 99th and 99.5th percentiles, 2.2 times more likely to have income between the 99.9-99.99 percentiles, and nearly 4 times more likely to have incomes above the 99.99th percentile (where average incomes are $13.1 million). These differences arise because a significant number of Ivy-Plus students have exceptionally high incomes even at age 33 – 5% of Ivy-Plus students earn more than $586,000 and 1% earn more than $1.9 million (Figure 17a, Appendix Table 10) – whereas many fewer students from public flagships and other highly selective private colleges reach those income levels, highlighting the unique access provided by Ivy-Plus institutions to the very upper echelons of society.

*Selection vs. Causal Effects.* Figure 15 shows how much of the observed variation in outcomes between

[47] Although attending an Ivy-Plus college has similar treatment effects relative to a fixed outside option across the parental income distribution, 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 Figure 20b). As a result, low- and middle-income applicants stand to gain more from Ivy-Plus admission than students from the top 1%.

[48] Unfortunately, we lack adequate precision to characterize quantile treatment effects, particularly in the very upper tail, using the waitlist and matriculation designs.

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 4), and the observed mean outcome at Ivy-Plus colleges.[49] About 60% of the 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 sum, although highly selective private colleges select students with unusually high potential, much of the difference in observed outcomes across colleges is due to treatment effects. Attending an Ivy-Plus college increases a student's chance of reaching the top 1% by 60%, attending an elite graduate program by 89%, and triples their chances of working at a prestigious firm. The fact that treatment effects are largest for non-monetary outcomes echoes the finding in Figure 1 that Ivy-Plus colleges account for an even larger share of individuals in leadership positions as defined in non-monetary terms relative to those at the top of the income distribution.

# 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 factors (e.g., SAT scores and 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.[50] 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 students' post-college outcomes). If they are not, Ivy-Plus colleges may have the capacity to diversify society's leaders 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-

[49] We estimate the probability of having income in the top 1% at age 33 by multiplying 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 estimate of the actual top 1% treatment effect directly since that point estimate is larger than the estimated effect on observational VA (Figure 10a). We estimate mean income ranks at age 33 using an analogous approach.

[50] Our outcome-based tests provide an input for evaluating admissions preferences rather than a definitive test of their merits because 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.

Plus students. For example, we are interested in identifying the difference in outcomes for legacies ($X_{1i} = 1$) vs. non-legacies ($X_{1i} = 0$) among Ivy-Plus applicants:

$$\triangle Y_X = E[Y_i^{Ivy}|X_{1i} = 1] - E[Y_i^{Ivy}|X_{1i} = 0] \tag{7}$$

We cannot estimate (7) 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 (4), we can write a student's post-college outcome (e.g., earnings) as

$$Y_i = \phi_{j_D(i)} + \omega_i, \tag{8}$$

where $\phi_{j_D(i)}$ denotes the value-added of the college that student $i$ attends and $\omega_i = \beta_1 X_{1i} + \beta_2 X_{2i} + \epsilon_i^Y$ 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 $\phi_j = 0$). Note that (8) assumes that there is no heterogeneity in college value-added across students, an assumption supported by the tests for heterogeneity in treatment effects implemented in Section 4. In particular, we find no 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 9.

To estimate $Y_i^{Ivy}$ empirically, note that we can write each student's potential outcome had they attended an Ivy-Plus college as

$$Y_i^{Ivy} = \omega_i + \phi_{Ivy} = Y_i + \phi_{Ivy} - \phi_{j_D(i)} = Y_i + \Delta\phi_{j_D(i)}, \tag{9}$$

where $\phi_{Ivy}$ denotes the mean value-added of the Ivy-Plus colleges in our college-specific sample and $\Delta\phi_{j_D(i)}$ is the difference between $\phi_{Ivy}$ and the value-added of college that student $i$ attends. Intuitively, we infer students' potential outcomes at Ivy-Plus colleges by adding the difference in the value-added of the average Ivy-Plus college and the college they actually attended to their observed outcome $Y_i$.

Ideally, we would use estimates of $\phi_j$ for every college $j$ from a research design that yields unconfounded estimates, such as our waitlist-based approach, to implement (9).[51] Since we have design-based estimates only for a subset of colleges, we instead 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 $\phi^{obs}_{j_D(i)}$ are highly correlated with the causal estimates obtained from our two research designs. To adjust for the remaining selection in the

[51] The fact that the waitlist-design estimates do not change when we condition on legacy status and other factors that lead to the high-income admissions advantage (Figure 10) despite the fact that those factors are imbalanced between those admitted vs. rejected for the waitlist (Table 3) implies that within the pool of waitlisted applicants, potential outcomes must not differ significantly along those dimensions, under the maintained assumption that the waitlist design is unconfounded. Our analysis in this section seeks to generalize that conclusion by asking whether potential outcomes differ in the broader pool of applicants, including regular admits, who constitute the vast majority of admitted students and may have different potential outcomes.

observational estimates, we multiply the raw fixed effects by $\psi_Y$, the ratio of the causal effect estimates from our waitlist design to the corresponding observational estimate for outcome $Y$ (e.g., a coefficient of $\psi_Y = 0.87$ for the predicted top 1% outcome, as shown in Figure 12a).

Using these estimates of $\Delta\widehat{\phi}_{j_D(i)} = \psi_Y \left(\phi^{obs}_{Ivy} - \phi^{obs}_{j_D(i)}\right)$ to estimate $\widehat{Y}^{Ivy}_i = Y_i + \Delta\widehat{\phi}_{j_D(i)}$, we obtain the following feasible estimator for the difference in potential outcomes between applicants with different credentials:

$$\triangle Y_X = E[\widehat{Y}^{Ivy}_i | X_{1i} = 1] - E[\widehat{Y}^{Ivy}_i | X_{1i} = 0]. \tag{10}$$

This estimator is unbiased assuming that each college's value-added is homogeneous across students and that the relationship between observational VA measures and causal effects within the subsample of colleges where we have design-based estimates $\psi_Y$ is constant across all colleges. To evaluate the sensitivity of our conclusions to these assumptions, we compare the outcomes of Ivy-Plus *attendees* (rather than applicants) 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 (see Appendix H). This alternative approach yields very similar results (Appendix Figure 23), indicating that the results that follow do not rest on the specific way in which we measure and adjust for college value-added.

## 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 16a 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 11.4% 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%.

The raw comparisons of $Y_i$ in the solid bars combine differences in latent earnings potential $\omega_i$ with the fact that applicants 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 16a 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 0.8 pp of students to the top 1%, while legacies attend colleges that send an additional 0.4 pp of students to the top 1%.[52]

Finally, in Figure 16b, 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 16a. 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 1.1 pp less likely to reach the top 1% than non-legacies (holding academic/non-academic ratings and athletic status fixed), a 10% reduction relative to a baseline rate of 11.4%. By contrast, having a high (above-median) academic rating 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 16c and 16d replicate Figure 16b using indicators for attending an elite graduate school (measured at age 25 to maximize precision) 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 6.7 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 22.4%).

The preceding estimates show the effects of the four factors without controlling for any other applicant characteristics. In Appendix Table 11, 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 24 and 25, replicating the same comparison of outcomes 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. SAT/ACT scores remain strongly predictive of outcomes even conditional on high school grade point averages (Appendix Figures 24 and 25), whereas GPAs are essentially unrelated to outcomes.[53] SAT scores

[52]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 6c), whereas higher non-academic ratings lead to higher admissions rates across Ivy-Plus colleges (Appendix Figure 19).

[53]We caution that our analysis applies only to Ivy-Plus applicants and the predictive power of test scores and GPAs may differ in other settings. For example, Rothstein (2004) finds that HS GPAs predict first-year grades better than SAT scores for students at University of California colleges. One potential explanation for the difference in results is that the predictive power

remain highly predictive of outcomes even within race-gender-parent-income cells and with high school fixed effects; in contrast, high school GPA is essentially unrelated to outcomes unless one includes high school fixed effects, perhaps reflecting differences in grading rubrics or peer quality across high schools (Appendix Table 11). These results demonstrate that standardized tests reveal substantial information about student potential despite the biases that may arise from disparities in test preparation. In addition, higher academic ratings predict better post-college outcomes even conditional on standardized test scores (Appendix Table 12). 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 et al. 2020, Michelman et al. 2021, replicated here in Appendix Figure 26). 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 that 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.

of high school GPAs is weaker in the pool of Ivy-Plus applicants, who come from schools across the nation and may have GPAs closer to the maximum.

# 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.[54] 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

[54]Theory is ambiguous on the sign of such responses: for instance, lower-income students with better chances of admission 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.

parts of the pipeline are the same for marginal students as average students with the same covariates.[55] These are both strong assumptions, but lacking actual reforms from which to estimate these effects, these predictions provide a sense of potential magnitudes.

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 (see Appendix I for details). First, we take the estimated "legacy boost" from Figure 6 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 6b. 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 down-weight 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 5 presents the impacts of this counterfactual admissions policy on 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

[55] 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.

share of students attending an elite graduate school. These predictions follow intuitively from the analysis 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 down-weight the importance of non-academic 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 "non-academic 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 applicants with strong academic credentials (SAT scores above 1500 and high academic ratings), we estimate that students from families in the top 1% are 25% more likely to be admitted than they would if they had non-academic ratings comparable to those from the middle class (based on estimates from an admissions model analogous to that used in Section 3.3; see Appendix I for details). We down-weight the number of admitted students in proportion to this non-academic ratings boost and 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 11.1%. 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.[56]

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 would further reduce the overall share of Ivy-Plus students who come from the top 1% from 11.1% to 9.9%. 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 9.9%.[57] These changes would also reduce the share of students from families with incomes between the 95th and 99th percentiles ($222-$611K) by 2.9pp, as shown in Table 5. 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 8.8pp. 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

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

[57] 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 5, 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.

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.

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 6c), 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 5 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.[58] 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

[58] 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%.

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.[59]

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 et al. 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

[59]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 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 scoresends among students who sent a score to at least one Ivy-Plus school.

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.

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 et al. 2023, Katz et al. 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 use 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 individuals are evaluated on complex criteria and thus incentivized to pursue distinctions (Bourdieu 1987), 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 the 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.) webpages. The fraction of journalists at the New York Times and the Wall Street Journal who attended an "Elite" undergraduate institution was obtained from Wai et al. (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 was obtained from the Rhodes Trust (n.d.) webpage. The fraction of Supreme Court Justices who went to Ivy-Plus colleges includes all appointments starting 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 Appendix Figure 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 Appendix Figure 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 scoresends serve as an indicator of where a student intends to apply. However, students may send their test scores to schools to which they do not ultimately apply, and thus scoresends provide an imperfect signal of true application.

To address this problem and predict true application rates from scoresend data, we estimate the fraction of scoresends 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 scoresends from a given type of student that convert into completed applications, heterogeneously based on students' characteristics. We then apply these estimated fractions to all scoresends in the data to form preliminary estimates of the total number of applications as a subset of the total scoresends to each school.

These preliminary estimates do not capture students who apply to a college without sending their test scores (since some colleges do not require standardized tests) or who send their scores 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, et al. 2014), we cannot observe reliable estimates of permanent income ranks at these ages.

We address this problem by predicting students' income ranks and probabilities of reaching the upper tail of the national earnings distribution at age 33 based on their employers (or graduate schools) at age 25. 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 W-2 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 students' 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 for 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 re-rank 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 took 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-by-test 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

$$weight_{acp} = \frac{P(SAT_a|attend_c)}{P(SAT_a|p)} \tag{11}$$

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 an 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 applications 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 et al., 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 $\epsilon$=1, we then add random noise drawn from a normal distribution with mean 0 and standard deviation 1/N.

# F Evolution of College Attendance Rates Around Financial Aid Expansions

Our 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.

In this appendix, we examine this hypothesis by studying 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 et al. 2017, Appendix Figure 27b). Families earning below $40,000 experienced a large change in federal student aid from the refundable portion of the American Opportunity Tax Credit (AOTC) and the higher full Pell Grant, while those 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, Appendix Figure 27 and Appendix Table 13 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 colleges remained nearly unchanged across all three groups around the ARRA financial aid expansions. This finding is robust to controlling for year, state, and parent income bin fixed effects and when restricting the sample to students with relatively high ACT/SAT scores (Appendix Table 13).

The rest of Appendix Figure 27 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.

# G Heterogeneity of Effect of Ivy-Plus Admissions by Outside Options

This appendix describes how we construct Figure 12 and Appendix Table 8, which show how the treatment effect of Ivy-Plus admission from the waitlist varies with the strength of an applicant's outside options.

We begin by estimating colleges' observational value-added using OLS regressions of a student's predicted top 1% probability on fixed effects for the college students attend, controlling for parental income, SAT scores, race, gender, birth cohort, and home state. We estimate these regressions using our pipeline analysis sample and normalize VA for the average highly selective state flagship public college (listed in Appendix Table 1) to 0.

We then place Ivy-Plus applicants into subgroups $j$ based on their home state, parent income, race, and the Ivy-Plus college to which they applied. Within each group $j$, we calculate each student's gain $g_j$ from attending the relevant Ivy-Plus college as the observational value-added (VA) of the Ivy-Plus college to which she applied minus the mean observational value-added of colleges attended by those rejected from the waitlist in that group. We then define the implied mean observational value-added of a student's outside options as the mean observational VA of the Ivy-Plus colleges in our college-specific sample minus the student-specific gain from Ivy-Plus attendance $g_j$; intuitively, this variable measures the strength of a student's outside options relative to the value-added of the average Ivy-Plus college for which we have data.

To construct Figure 12, we divide students into 20 bins based on the implied strength of the outside options variable. The x coordinate of each of the 20 points is the mean implied observational VA of outside options within each bin. To construct the y coordinates, we regress the predicted top 1% outcome on indicators for Ivy-Plus admission interacted with the 20 outside option strength dummies and indicators for the Ivy-Plus college to which they applied, using the sample of waitlisted Ivy-Plus applicants as in Figure 10b. In order to obtain a visual representation that is aligned with the 2SLS regression coefficient that we report in Appendix Table 8 ("visual IV"), we then divide these coefficients by the "first stage" effect of the strength of outside options on actual college VA, i.e., the coefficient on the interaction term in a regression of observational VA of the college a student actually attends on an indicator for Ivy-Plus admissions, the gain from attending an Ivy-plus college relative to outside options, the interaction of those two variables, and indicators for the Ivy-Plus college to which the student applied.

We also report the 2SLS regression slope (and the implied best fit line) corresponding to the plotted points, estimated using a 2SLS regression of the predicted top 1% outcome on the observational VA of the college a student actually attends (multiplying the coefficient by -1 since the x variable is the implied outside option rather than the gain in the figure). We instrument for observational VA with the interaction between the gain in observational VA from Ivy-Plus admission $g_j$ and an admissions indicator, controlling for the admissions indicator, $g_j$, and indicators for the Ivy-Plus college to which the student applied among waitlisted students. Column 1 of Appendix Table 8 reproduces this specification; the subsequent columns of the table report variants with different approaches to constructing the instrument $g_j$, as detailed in the notes to that table.

# H Outcome Comparisons Among Ivy-Plus Attendees

In this appendix, we replicate the outcome-based tests in Section 5 by comparing outcomes among Ivy-Plus attendees. 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):

$$\triangle Y_{X|Ivy} = E[Y_i | X_{1i} = 1, j_i = Ivy] - E[Y_i | X_{1i} = 0, j_i = Ivy] \tag{12}$$

By conditioning on attending an Ivy-Plus college, this comparison holds fixed college value-added, thereby isolating differences in students' potential $\omega_i$ 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 (3), 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 ($\triangle Y_X$) (see Rothstein 2004 for a more general discussion of this issue). 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 $\epsilon_{ij}$ 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 (12) turns out to yield estimates that are very similar to our baseline estimates that adjust for selection bias among all applicants.

*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. Appendix Figure 23 plots the coefficients obtained from this OLS regression along with 95% confidence intervals for the same three outcomes that we consider in Figure 16. 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 hold even when we make simple comparisons among Ivy-Plus attendees –

perhaps because a large portion of the variation in admissions decisions is driven by idiosyncratic factors conditional on the observable factors we consider – 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.

# I Decomposition and Counterfactual Methodology

In this appendix, we describe the methodology used in the decompositions in Table 2 and Appendix Table 6 and the policy counterfactuals in Table 5.

## I.1 Decomposition Analysis

The analysis for both Table 2 and Appendix Table 6 begins with a calculation of the total number of "extra" students from the top 1% of the income distribution implied by the higher attendance rates, conditional on test scores, in Figure 2b. We calculate counterfactual attendance assuming that each top 1% student would attend an Ivy-Plus school at the same rate as students with the same test score but from the 70th-80th percentiles of the parent income distribution. Formally, we calculate

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

from equation (1) in Section 3.1, for students with test score $a$ and for college $c$. This results in overall attendance that is 64.2% lower than the current attendance of top 1% students. We then scale this difference to an incoming class of 1650 first-year students, which is the average among Ivy-Plus schools in our sample period. In our data, 15.8% of students (or 261 out of 1650) are from top 1% families; under the counterfactual attendance rate, which is 64.2% lower than actual attendance, this falls to 93 students. The difference is 168 students.

*Athletes.* We next decompose the 168 extra students into the part due to extra recruited athletes and that due to extra non-athletes. Using our internal data from certain Ivy-Plus colleges, we calculate that 13.5% of all students (222 out of 1650) are recruited athletes, and similarly that 15.8% of top 1% students (41 of 261) are recruited athletes. This implies that there are 1,428 total students who are not recruited athletes, of which 220 (15.4%) are from the top 1%. From this point forward, we analyze the athlete and non-athlete potions of the class separately.

As discussed in Section 3.2, it is difficult to quantify the contribution of each of the three parts of the pipeline for athletes given their unique admissions process. We therefore adjust the number of top 1% athletes so that the share of athletes from the top 1% is equal to the share of the student body from the top 1% under our equal attendance counterfactual. The result is that the number of athletes from the top 1% falls by 66%, from 41 to 14; thus there are 27 extra top 1% students that result from athletic recruiting. This also implies that 141 out of the total 220 non-athletes from the top 1% are extra and that there would be 79 non-athletes from the top 1% under the counterfactual.[60]

[60]The numbers we report here are rounded to the nearest whole number, but we use the exact numbers throughout the decomposition calculations.

*Applications, Admissions, and Matriculation for Non-Athletes.* We next decompose the 141 extra non-athletes from the top 1% into the portions due to application, admission, and matriculation. Since we seek an estimate for the average across all 12 Ivy-Plus schools, but do not have internal data from each (which is required to disaggregate admissions and matriculation), we proceed in four steps:

1. Using data on all Ivy-Plus schools, we calculate the proportional contributions of applications and the joint effect of admissions and matriculation (which we can directly estimate for the full set of Ivy-Plus schools).
2. Using internal data from certain Ivy-Plus colleges, we calculate the proportional contribution of admissions and matriculation, rescaling these components to match the overall estimated effect of admissions and matriculation from Step 1.
3. Using internal data from certain Ivy-Plus colleges, we calculate the proportional contribution of preferences for legacy students and of higher non-academic ratings for top 1% students, rescaling these components to match the overall estimated effect of admissions from Step 2.
4. We translate these proportional contributions into numbers of students, using different orderings for Table 2 and Appendix Table 6. We now describe each of these four steps in more detail.

Because the first four steps operate via proportional changes in the number of top 1% students, it is useful to estimate the contribution of each part of the pipeline in log-points, thus allowing for these components to add to the whole. Among non-athletes, the extra top 1% students represent 102 log-points (220 down to 79 top 1% students).

Step 1: We calculate the total contribution of application rates following a similar approach to overall attendance above; that is, we calculate the counterfactual number of attendees from top 1% families assuming that each top 1% student applies at the same rate as students with the same test score but from the 70th-80th percentiles of the parent income distribution, while keeping admissions and matriculation rates unchanged:

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

This results in 29.8% fewer attendees from the top 1%, or 35.4 log-points. This implies that the remaining 66.6 log-points result from differences in admissions and matriculation.

Step 2: We calculate the contribution of admissions within internal data from certain Ivy-Plus institutions using a variant of the equation above:

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

That is, we calculate the counterfactual number of attending students from the top 1% assuming that each applicant from the top 1% is admitted at the same rate as students with the same test score but from the 70th-80th percentile of the parent income distribution among the pool of applicants, while keeping application rates unchanged. These calculations result in a 33.3% (40.4 log-points) reduction in the number of top 1%

students from changing admissions rates; since the overall gap in combined admissions and matriculation at these certain Ivy-Plus schools is 41.4% (53.4 log-points), this implies that a further 12.2% (13.0 log-points) reduction in the number of top 1% students stems from changing matriculation rates.

We now reconcile the components of admissions and matriculation, calculated from a particular set of Ivy-Plus schools, to produce a consistent set of estimates for the full set of 12 Ivy-Plus schools. We estimated a total contribution of 66.6 log-points for the combination of admissions and matriculation at the full set of 12 Ivy-Plus colleges. Within our set of certain Ivy-Plus schools, we estimated a contribution of 53.5 log-points from these two factors. Among our set of certain Ivy-Plus schools, there is no clear pattern that one of these components accounts for more of the overall variation between schools than the other (proportional to their overall size). Thus, we scale our estimates of admissions and matriculation up proportionally to match the overall combined effect of 66.6 log-points; this implies that the contribution of admissions is 50.4 log-points, and the contribution of matriculation is 16.2 log-points, across all 12 Ivy-Plus schools.

Step 3: We calculate the contribution of preferences for legacy students and non-academic ratings to the overall admissions component. To calculate the contribution of legacy preferences, we use the same set of certain Ivy-Plus colleges as in Step 2. We make two adjustments to remove the two channels through which top 1% students gain from legacy preferences from Section 3.3. First, we reweight the distribution of students so that the fraction of legacy students among the top 1% matches the fraction among students from the 70th-80th percentiles. This reduces the fraction of top 1% students who are legacy students from 8.9% to 1.2% (as in Appendix Figure 9a). Second, we reduce the admissions advantage enjoyed by legacy students in the top 1% to match that enjoyed by students from the 70th-80th percentiles. To do so, we use the legacy and counterfactual non-legacy admissions rates from Appendix Figure 9b, which result from estimating a linear probability model of admissions for legacy and non-legacy students using characteristics observed in the admissions data. The variables in these models are: a quintic in test score, indicators for gender, seven categories for race and ethnicity (Hispanic/Latino students of all races, plus non-Hispanic/Latino students who are white, Black, American Indian/Alaskan Native, Native Hawaiian/Pacific Islander, Asian, and unknown), first-generation status, early applicant status, 13 parent income bins, the tuple of ratings from the admissions offices, high school GPA, when available, and high school fixed effects. For each student, we define the "legacy boost" as the difference between their predicted admissions rate from the legacy admissions model and that from the non-legacy admissions model (retaining the same high school fixed effect). We then calculate the difference between the average legacy boost for top 1% applicants (32.4pp) and that for 70th-80th percentile students with the same test scores (18.8pp), subtracting the difference (13.4pp) from the modeled admission rate of each top 1% legacy applicant. Using the new counterfactual admissions rates and the counterfactual student weights, we recalculate the total number of attending top 1% students, which is 26.0% (30.1 log-points) fewer than the actual number of attending top 1% students.

Next, we calculate the contribution of removing the top 1% advantage in non-academic ratings. Due to the differences in the exact nature of ratings across schools, we calculate the contribution of non-academic ratings using data from one Ivy-Plus school (as described in Section 3.3). We begin from the counterfactual admissions rates and student weights described in the previous paragraph (which remove the top 1% additional advantage in legacy admissions) at that one school. Using the coefficients on the ratings tuple from the non-legacy admissions model, we calculate the difference between the average contribution of ratings to admissions for top 1% applicants as compared with that for 70th-80th percentile students with similar academic ratings and test scores, where we group students based on their academic rating (pooling certain rare ratings with nearby ones) and on whether their test score is equal to or greater than 1500 (SAT) or 34 (ACT). We then subtract this difference from the admissions rate of all top 1% students (including legacies) and recalculate the total number of attending students from the top 1%, keeping the model-predicted matriculation rates unchanged. Relative to the number at the end of the legacy counterfactual, removing the top 1% advantage in non-academic ratings results in 24.0% (27.5 log-points) lower top 1% attendance.

We now reconcile these estimates with the overall contribution of 50.4 log-points for non-athletic admissions from Step 2. We calculated the contribution of legacy preferences from each of our certain Ivy-Plus colleges, but we calculated the contribution of non-academic ratings from just one college. At the one college in which we calculated the contribution of non-academic ratings, the sum of the two contributions (in log-points) is just 2.2 log-points different from the entire contribution of admissions. We thus conclude that these two components together can account for the full admissions differential. Further, while the total admissions differential differs across our set of certain Ivy-Plus colleges, the contribution of legacy preferences is relatively similar across schools. We thus make the assumption that the contribution of non-academic ratings is equal to the difference between the full admissions contribution and the contribution of legacy preferences at each school. This implies that, within our set of certain Ivy-Plus schools, the total contribution of non-academic ratings is 20.3 log-points (50.4 log-points from all of admissions minus 30.1 log-points from legacy preferences).

Step 4: In this final step, we convert our proportional estimates into numbers of students out of a first-year class of 1650. Recall that the total number of top 1% students who are not recruited athletes falls from 220 in the actual class to 79 at the counterfactual attendance rates. Table 2 and Appendix Table 6 present two different approaches to decomposing the 141 extra top 1% students into the respective components.

In Table 2, we decompose the 141 extra top 1% students by stacking the components in an order reflecting one way in which schools might prioritize these changes, given the results in this paper. We first apply the changes in admissions; a 50.4 log-point (i.e., 40%) reduction in the number of top 1% students represents a reduction of 87 top 1% students, leaving 133 students (and 54 extra students) remaining from the top 1%. Within this 87, we apply the legacy component (30 log-points) first, accounting for 52 students, followed by the non-academic ratings component (20 log-points), accounting for the remaining 35 students. We next apply the matriculation component; a 16.2 log-point (i.e., 15%) reduction leaves 113 students (and 34 extra students) remaining from the top 1%. Finally, we apply the application component; a 35.4 log-point (i.e., 30%) reduction accounts for the remaining 34 extra students from the top 1%.

In Appendix Table 6, we instead apply the components proportionally based on their size in log-points. This abstracts from the order in which schools might institute these changes. Since the application component represents 35% of the total effect in log-points (35.4 out of 102 log-points), we assign 35% of the overall reduction in non-athlete top 1% students (141) to applications, for a total of 49 students. We use a similar approach for the other components.

## I.2 Policy Counterfactuals

In Table 6, we analyze the consequences of three different admissions policy counterfactuals. We perform this analysis on the set of Ivy-Plus colleges in our college-specific sample, and we then scale the intermediate policy counterfactual estimates to match the attendance gap observed across all 12 Ivy-Plus institutions as described below.

*Removing Legacy Preferences.* The second row of Table 5 considers a counterfactual in which colleges eliminate existing preferences for legacy applicants. We implement this counterfactual in two steps: we first de-admit certain students who are not athletic recruits as a result of removing the preference for legacy applicants, and then we refill the class from applicants who are not athletic recruits and who were either rejected off the waitlist in reality or who were admitted in reality and de-admitted in the first step. We keep the set of attending students who are recruited athletes the same.

In the first step, we de-admit a share of the legacy students that were currently admitted. To do so, we calculate the predicted admissions rate for each legacy student using the legacy and non-legacy linear probability models, as described earlier in this Appendix; for each student $i$, denote these predicted admissions rates $p_i^L$ and $p_i^{NL}$, respectively. Within each parent income bin $p$ and test score range $s$ (above/below SAT 1500 or ACT 34) cell, we then calculate the ratio of the average non-legacy admissions rate ($p_{ps}^{NL}$) to the average legacy admissions rate ($p_{ps}^{L}$). We then define the intermediate counterfactual admissions rate (i.e., the admissions weight after the de-admission step but before the re-admission step) for each admitted legacy student as

$$\tilde{p}_{ps} = \frac{p_{ps}^{NL}}{p_{ps}^{L}}$$

Intuitively, if legacy students from a given test score bucket and parent income group have predicted admissions rates that are three times higher than their counterfactual non-legacy admission rate, then we probabilistically de-admit two out of every three such students who are currently admitted. Admitted students who are either recruited athletes or not legacies retain $\tilde{p} = 1$, while students who were not admitted retain $\tilde{p} = 0$. Define $p_i^M$ as an indicator for whether currently admitted students chose to matriculate; then the total number of students remaining in the matriculating class after this de-admission step is

$$\tilde{N} = \sum_i \tilde{p}_i * p_i^M$$

In the second step, we admit additional students to increase the size of the class by $N^{miss} = N - \tilde{N}$ additional students, back to the original level. We admit students in this step from two pools: those students placed on the waitlist but never admitted in the data, and those students de-admitted in Step 1. We assume that students in the latter group would matriculate or not if re-admitted as they did in the data; for students in the former group, we model each student's matriculation rate using the same linear probability model as for admissions (estimated separately on students admitted in the early and regular admissions rounds) and predict matriculation rates. This process assumes that students newly admitted to the class in the counterfactual would have the same matriculation rate as do observably similar students who were actually admitted in the data, i.e. that matriculation rates for marginal students is the same as that of the average admitted student conditional on observables. For these students, define $p_i^M$ as student $i$'s predicted matriculation rate.

We now admit students from these two groups using the predicted admissions probabilities from the non-legacy model ($p_i^{NL}$). In order to fill $N^{miss}$ spots, we must re-admit students with probability

$$p_i^R = p_i^{NL} * \frac{N^{miss}}{\sum_i \left((1-\tilde{p}_i) * p_i^{NL} * P_i^M\right)}$$

where the denominator of the fraction corrects for the overall size of the pool for re-admissions. The final weight for each student $i$ after the counterfactual policy of removing legacy preferences is

$$p_i^{cf} = p_i^M * \left(\tilde{p}_i + (1-\tilde{p}_i) * p_i^R\right)$$

Note that this expression simplifies to just $p_i^M$ for non-legacy students who were originally admitted (since $\tilde{p}_i = 1$) and to $p_i^M * p_i^R$ for students not originally admitted but on the waitlist (since $\tilde{p}_i = 0$). With these new weights, we can calculate class characteristics as the average of characteristic $X_i$ among all applicants either initially accepted or placed on the waitlist using weights $p_i^{cf}$.

In practice, changes in legacy preferences may also affect which students are placed on the waitlist. In the full model from which we generate results in Table 5, we therefore implement an initial de-waitlisting and re-waitlisting step before admitting students from the waitlist in the previous calculations. Formally, this generates a probability of being on the waitlist $p_i^W$ for all students not accepted in the data, where $p_i^W = 1$ for non-legacy students actually on the waitlist and not accepted in the data. With this additional weighting, students are re-admitted with probability

$$p_i^R = p_i^{NL} * \frac{N^{miss}}{\sum_i \left((1-\tilde{p}_i) * p_i^W * p_i^{NL} * P_i^M\right)}$$

and receive final weight of

$$p_i^{cf} = p_i^M * \left(\tilde{p}_i + (1-\tilde{p}_i) * p_i^W * p_i^R\right).$$

*Removing the Influence of Privilege on Non-Academic Ratings.* Starting from the end of the previous counterfactual (i.e. having already removed preferences for legacy students), the third row considers a counterfactual in which we remove the advantage in non-academic ratings enjoyed by students above the 70th-80th parent-income percentiles. We implement the non-academic ratings counterfactual using the same two-step process as in the legacy preferences counterfactual, where now we calculate the rates at which students are de-admitted ($\tilde{p}$ above) and re-admitted ($p^R$) based on the influence of non-academic ratings. To highlight this parallel, we denote all weights in this section with $q$ (so that for instance $\tilde{q}$ is the intermediate admissions weight after the de-admission step, similar to $\tilde{p}$ before), while we maintain $p$ as the predicted admissions probability from various admissions models. Note that we now refer to "admitted" students as those probabilistically admitted after removing legacy preferences above, and similarly those on the waitlist.

In the first step, we de-admit a share of non-athlete admitted students. To do so, we use the estimates of the coefficients on indicators for each ratings-tuple in the admissions model; denote the coefficient for each rating-tuple $r$ as $\phi_r$ and so the relevant coefficient for each individual $i$ as $\phi_{r(i)}$ based on that individual's rating. Note that all individuals with the same ratings-tuple share the coefficient $\phi_{r(i)}$ (since we do not interact the ratings-tuple fixed effect with any other variables in the admissions model). Within each test score range $s$ (above / below SAT 1500 or ACT 34) and academic rating cell $d$, and for each parent income bin $p$ above the 70th-80th percentiles, we then calculate the difference $\Delta\phi_{sdp} = \bar{\phi}_{sdp} - \bar{\phi}_{sd,P70-80}$, where $\bar{\phi}_{sdp}$ is the average of $\phi_{r(i)}$ for students with test score $s$, academic rating cell $d$, and parent income bin $p$; intuitively, this difference $\Delta\phi_{sdp}$ is the admissions advantage enjoyed by students from higher income groups from non-academic ratings, as compared with students with similar test scores and academic ratings but from the 70th-80th percentiles. Defining $p_i^{NLNR} = p_i^{NL} - \Delta\phi_{sdp(i)}$ as the predicted admissions rate after removing the high-income privilege in non-academic ratings, we then define the intermediate counterfactual admissions rate for each student above the 80th percentile as

$$\tilde{q}_i = \frac{p_i^{NLNR}}{p_i^{NL}} * \frac{p_i^{cf}}{p_i^M}$$

Intuitively, if a certain group of students has an admissions rate that is only 80% as high after removing the inflated non-academic ratings, then we probabilistically de-admit one out of every five such admitted students. Because the non-academic rating counterfactual is implemented on top of the legacy counterfactual, we must apply the de-admission rate to the relative prevalence of such students in the admitted class at the end of the previous counterfactual ($\frac{p_i^{cf}}{p_i^M}$). Using the same matriculation rates $p_i^M$ as in the legacy counterfactual, the total number of students remaining in the matriculating class after this de-admission step is

$$\tilde{M} = \sum_i \tilde{q}_{i,psd} * p_i^M$$

In the second step, as in the legacy counterfactual, we admit additional students to increase the size of the class by $M^{miss} = N - \tilde{M}$ additional students, back to the original level. We draw again from the pool of waitlist rejects (after a similar de-waitlisting and re-waitlisting step as above) and those de-admitted in the first step, and we use the same matriculation rate $p_i^M$ for each student as in the legacy counterfactual. To fill $M^{miss}$ spots, we re-admit students with probability

$$q_i^R = p_i^{NLNR} * \frac{M^{miss}}{\sum_i \left((1-\tilde{q}_i) * q_i^W * p_i^{NLNR} * p_i^M\right)}$$

The final weight for each student $i$ after the counterfactual policy of removing the influence of parent income on non-academic ratings is

$$q_i^{cf} = p_i^M * \left(\tilde{q}_i + (1 - \tilde{q}_i) * q_i^W * q_i^R\right))$$

*Adjusting for the High-Income Advantage in Athletic Recruitment.* Starting from the end of the previous counterfactual (i.e. having already removed preferences for legacy students and advantages for high-income applicants due to ratings), the fourth row considers a counterfactual in which we remove the advantage enjoyed by recruited athletes in the admissions pipeline. To do this, we model the limiting case in which the characteristics of the recruited athletes in the class match those of the non-athletes. In practice, we de-admit all recruited athletes from the class and then proportionally scale up the admission rates of the non-athlete admits to refill the class. If there are $N_A$ recruited athletes in the class, we de-admit the $N_A$ recruited athletes, and calculate the new weight for each student $i$ as

$$q_i^{cf} * \frac{\sum_i q_i^{cf}}{\sum_i q_i^{cf} - N_A}$$

*Need-Affirmative Admissions.* Separately from the previous three stacked counterfactuals, the fifth row considers a counterfactual in which legacy advantages, athlete preferences, and the income gradient in non-academic ratings remain in place, but in which preference is given to low- and middle-income students with high academic ratings. We consider non-recruited athlete applicants who were either admitted or waitlisted, and we begin with the admissions rates for legacy and non-legacy applicants estimated in the admissions model above. We then increase the estimated admissions rates for each applicant with parent income below the 60th percentile with a high academic rating by a factor of $F_1$, and the estimated admissions rates for each applicant with parent income between the 60th and 95th percentile with a high academic rating by a factor of $F_2$. Admissions rates for student $i$ with parent income percentile $p$ in this counterfactual are

$$\tilde{v}_i = \begin{cases} F_1 * v_i & \text{if } p(i) < 60 \text{ and } d = HIGH \\ F_2 * v_i & \text{if } 60 \leq p(i) < 95 \text{ and } d = HIGH \\ v_i & \text{otherwise} \end{cases}$$

where $v_i = p_i^{NL}$ if student $i$ is not a legacy and $v_i = p_i^L$ if student $i$ is a legacy. Combining these admissions rates with matriculation rates $p_i^M$ (identical to those estimated above), the probability that each student attends in this counterfactual is $\tilde{v} * p_i^M$. We then select scaling parameters $F_1$ and $F_2$ such that the share of students in the attending class from the bottom 60% and from between the 60th and 95th income percentiles (each) match the shares obtained in the 4th row; these scaling factors are $F_1 = 2.3$ and $F_2 = 1.6$. Proportionally reducing admissions rates to maintain the same size of the class (overall scaling factor $F^* = \frac{N}{\sum_i \tilde{v}_i * p_i^M}$), we use an attendance weight for each student that is

$$v_i^{cf} = \tilde{v}_i * p_i^M * F^*$$

*Scaling to Ivy-Plus Distribution.* Finally, we take the parent income distributions we have calculated using the set of schools for which we have internal admissions data and scale these distributions to match the observed parent income distribution of all Ivy-Plus attendees according to the following process:

Step 1: We calculate the share of Ivy-Plus attendees with parent income in the four groups we report in Table 5: bottom 60%, 60th-95th percentiles, 95th-99th percentiles, and top 1%. In particular, the share of Ivy-Plus attendees from the top 1% is 15.8%. We then calculate the counterfactual parent income distribution of Ivy-Plus attendees that would prevail if students with parent income above the 80th percentile attended Ivy-Plus institutions at the same rate as students with parent income between the 70th and 80th percentiles with the same test scores (unlike the decomposition analysis for Table 2 and Appendix Table 6, in which we change the attendance rates only for top 1% students). We call this the "equal attendance counterfactual". In this counterfactual, the share of attendees from the top 1% is 7.2%. We calculate the same actual and counterfactual parent income distributions for the schools for which we have internal data, even-weighting across these schools.

We also calculate the intermediate parent income distributions at these schools after each of our counterfactual policy simulations. When removing advantages for legacy applicants and recruited athletes, we even-weight across the schools. When removing the influence of parent income in non-academic ratings and in calculating the need-affirmative counterfactual, we calculate the intermediate parent income distributions only for the school for which we have the most granular ratings data.

Step 2: We scale the intermediate policy counterfactual income distributions according to the overall difference between the actual and counterfactual distributions for all Ivy-Plus schools. We begin by calculating the difference between the share of students from the top 1% in the actual and attendance counterfactual classes, at all Ivy-Plus schools together (call this $\Delta top1^{ivy}_{full}$) and at each of our internal schools separately (call this $\Delta top1^{s}_{full}$). We calculate $\Delta top1^{ivy}_{full}$ as 8.6pp. For each of our internal schools, we also calculate the difference in top 1% shares between the legacy counterfactual and the full attendance counterfactual classes (call this $\Delta top1^{s}_{l}$). We then calculate the difference between $\Delta top1^{s}_{full}$ and $\Delta top1^{s}_{l}$ in log-points, even-weighting across schools. This difference is 28.3 log-points. We then apply these 28.3 log-points to the overall $\Delta top1^{ivy}_{full}$ (8.6pp) to calculate a difference of 6.5pp between the share of top 1% attendees in the full attendance counterfactual and the share of top 1% attendees in the legacy counterfactual. This implies that removing legacy preferences in admissions according to our simulation would reduce the share of Ivy-Plus attendees from the top 1% by 2.1pp, from 15.8% to 13.7%.

We then move to the second policy counterfactual: removing the influence of parent income on non-academic ratings. As in the legacy counterfactual, we calculate the difference top 1% shares between the ratings counterfactual and the full attendance counterfactual classes at the school for which we estimate the ratings counterfactual (call this $\Delta top1^{s}_{r}$). We calculate the difference between $\Delta top1^{s}_{full}$ and $\Delta top1^{s}_{r}$ as 68.4 log-points. Since we calculate this counterfactual using only one school, we translate this difference to the Ivy-Plus distributions using the difference in top 1% attendance attributable to admissions in the decomposition (50.4 log-points), the difference attributable to legacy preferences in the decomposition (30.1 log-points, even-weighting across schools), and the difference attributable to non-academic ratings (27.5 log-points, at the school for which we calculate the ratings counterfactual). We calculate the remaining log-points attributable to admissions after removing legacy preferences (50.4 − 30.1), which we had been attributing to non-academic ratings in the decomposition. We scale this number by dividing by the contribution of non-academic ratings for this particular school (27.5 log-points) and multiplying this by the 68.4 log-points above to get a difference between $\Delta top1^{ivy}_{full}$ and $\Delta top1^{ivy}_{r}$ of 50.4 log-points. We apply these log-points to the$\Delta top1^{ivy}_{l}$ (6.5pp) to calculate a difference of 3.9pp between the share of top 1% attendees in the full attendance counterfactual and the share of top 1% attendees in the ratings counterfactual. This implies that removing the influence of parent income on non-academic ratings according to our simulation would reduce the share of Ivy-Plus attendees from the top 1% by 2.6pp, from 13.7% to 11.1%.

Moving to the third policy counterfactual in which we remove athletic preferences in admissions, we calculate the share of athletes from top 1% families for the schools for which we have internal data (18.3%, even-weighting across schools), and the share of the attending class that are athletes (14.6%). Because our earlier counterfactuals have de-admitted and refilled the class only among non-athletes, the share of athletes in the ratings counterfactual class is also 14.6%, and the share of non-athletes is 85.4%. We multiply the share of athletes in the class by the share of athletes from the top 1%, subtract this number from the overall share of the class from the top 1% in the ratings counterfactual (11.1%), and divide by the share of non-athletes in the class to calculate that 9.9% of the Ivy-Plus class is from the top 1% in the athletes counterfactual.

Step 3: We calculate the shares of the remaining parent income groups in the Ivy-Plus attendee distribution in our counterfactuals. For the legacy counterfactual, we first calculate the difference, in percentage points, between the share in income group $p$ in the actual class and the share in income group $p$ in the legacy counterfactual for the schools for which we have internal data, even-weighting across schools (call this $d^{s}_{p,l}$). We then calculate a scaling factor as the ratio of the difference in top 1% shares between the actual Ivy-Plus distribution and the legacy counterfactual Ivy-Plus distribution (15.8% - 13.7%) to the ratio of this difference for the schools for which we have internal data, again even-weighting across schools. For each income group $p$, we multiply $d^{s}_{p,l}$ by this scaling factor to calculate $d^{ivy}_{p,l}$, the difference in the shares of actual Ivy-Plus attendees from parent income bin $p$ and the share of Ivy-Plus attendees from parent income bin $p$ in the legacy counterfactual. We then subtract this difference from the share of actual Ivy-Plus attendees from parent income bin $p$ to get the share of Ivy-Plus attendees from parent income bin $p$ in the legacy counterfactual. We follow a similar process for the ratings and athletes counterfactuals, except we do not average across schools for which we have internal data in calculating $d^{s}_{p,r}$ and $d^{s}_{p,a}$ but instead use the school for which we calculate the ratings counterfactual.

Step 4: We then use the distribution of Ivy-Plus attendees in the athletes counterfactual to calculate the distribution of Ivy-Plus attendees in the need-affirmative counterfactual. As we had calibrated the need-affirmative counterfactual such that the shares of the class from the bottom 95% matched the shares in the athletes counterfactual at the school for which we calculated the need-affirmative counterfactual, we similarly preserve the shares of Ivy-Plus attendees from the bottom 95% in the need-affirmative counterfactual. We then calculate the shares of students from the top 1% and from parent income percentiles 95-99. To do this, we first calculate two numbers for this select school: the share of students from the top 5% who have parent income between the 95th and 99th percentiles in the observed class and this share in the athletes counterfactual class. We calculate the difference between these shares and subtract it from the share of top 5% Ivy-Plus attendees in the athletes counterfactual who have parent income between the 95th and 99th percentiles. We apply this fraction to the share of Ivy-Plus attendees from the top 5% to calculate the share of Ivy-Plus attendees with parent income between the 95th and 99th percentiles in the need-affirmative counterfactual, so that the percentage point change in the share of the top 5% from percentiles 95-99 between the observed distribution and the athletes counterfactual distribution at the select school matches this percentage point change in the overall Ivy-Plus distributions. The remainder are from the top 1%.

Step 5: Because we are only able to calculate the ratings and need-affirmative counterfactuals using the school for which we have the most granular ratings data, Columns 5-7 report average outcomes only for this school in Rows 1-5. We use the counterfactual attendance weights for this school to calculate each weighted average.

Table 1: Summary Statistics for Analysis Samples

| | Sample | | | |
|---|---|---|---|---|
| | | | Ivy-Plus | Flagship Public |
| | Pipeline | Long Term Outcomes | College-Specific | College-Specific |
| | (1) | (2) | (3) | (4) |
| *Panel 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% |
| *Panel B: Standardized Test Scores* | | | | |
| Mean Test Score | 991 | 993 | 1374 | 1228 |
| Mean Number of Scoresends to Colleges | 4.49 | 4.86 | 9.88 | 7.46 |
| *Panel 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 | - | - | 3.5% | - |
| % Children of Faculty | - | - | 0.1% | - |
| *Panel D: Demographics* | | | | |
| Mean 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% |
| *Panel 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 |
| *Panel 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 | - | 2.0% | 13.2% | 7.7% |
| Predicted Top 1% at 33 based on Age 25 Employer | 4.4% | 4.1% | 10.4% | 7.7% |
| Predicted Income Rank at Age 33 | 71.6 | 71.4 | 77.3 | 75.8 |
| % 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 applicants to selected Ivy-Plus colleges (column 3) and highly selective public flagship colleges (column 4) from which we have internal application records and who (1) are US citizens or permanent residents, (2) can be linked to the tax data based on their SSN or ITIN, (3) can be linked to parents in the tax data, (4) were born before 1996, and (5) were 21 or younger when they applied to college. Test scores are reported in SAT points out of 1600 (with ACT scores converted to SAT points). For post-college outcomes in panel F, we further restrict to students old enough to achieve relevant outcomes. For example, row 1 of Panel F restricts to children born between before 1988. In columns 3 and 4, Panel C reports statistics counting each college application once; all others panels count each student once when constructing statistics, even if they apply to multiple schools. All monetary values are measured in 2015 dollars. See Section 2 for variable definitions and data sources.

Table 2: Sources of Additional Students from Top 1% at Ivy-Plus Colleges: 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] | Equal Attendance Counterfactual for Top 1% Students | 93 | | |
| [4] | Excess Students with Parent Income in Top 1% | 168 | | 100.0% |
| [5] | Attributable to Differences in Admissions | 114 | | 68.1% |
| [6] | Legacy Preferences | | 52 | 31.0% |
| [7] | Non-Academic Credentials | | 35 | 20.9% |
| [8] | Recruited Athletes | | 27 | 16.2% |
| [9] | Attributable to Differences in Matriculation | 20 | | 11.9% |
| [10] | Attributable to Differences in Application | 34 | | 20.1% |

*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 students' test scores fixed. Row 1 reports the average class size for incoming first-year students at the 12 Ivy-Plus colleges in our sample period. Row 2 reports the observed number of students with parents in the top 1%. Row 3 calculates the counterfactual number of students from the top 1% who would attend the average Ivy-Plus college if students from the top 1% attended such colleges at the same rate as students with the same SAT/ACT scores from the 70th-80th percentile of the parent income distribution, calculated using equation (1) in Section 3.1. Row 4 reports the difference between Rows 2 and 3, i.e. the number of “extra” students from the top 1%. The remaining rows of the table decompose these extra students into the portion attributable to various parts of the admissions process. Focusing first on students who are not recruited athletes, we estimate the numbers in Row 10 (application), the sum of Rows 6 and 7 (admissions), and Row 9 (matriculation) using a calculation similar to that in Rows 3 and 4, where we set the application/admissions/ matriculation rates of top 1% students equal to those for students with the same SAT/ACT scores from the 70th-80th percentile of the income distribution. We calculate the effect of equalizing application rates on the sample from Figure 3a; we calculate the effects of equalizing admissions and matriculation rates on the sample from Figures 4a and 4b (excluding recruited athletes). In Row 6, we equalize both the share of students who are legacy applicants (Appendix Figure 9a) and the advantage such students receive in the admissions process (Appendix Figure 9b) between the top 1% and the 70th-80th percentiles. In Row 7, we remove the effects from higher non-academic ratings received by applicants from the top 1%, relative to applicants with the same test scores and academic ratings from the 70th-80th percentiles (Figure 7b). Because we estimate these constituent effects in different subsamples of the data, we align all numbers with overall Ivy-Plus averages at the end through proportional scaling. To do so, we calculate all effects in log-points and multiply by the total number of excess students in row 4, implementing changes in the order [admissions, matriculation, application] and [legacy, non-academic ratings] within admissions. Row 8 is estimated separately from the other Rows 6-10 on the sample of applicants who are recruited athletes; to calculate the number in row 8, we reduce the overall fraction of students from the top 1% among recruited athletes to match the overall share of students from the top 1% in the equal attendance counterfactual in Row 3. Row 5 presents the sum of the numbers in Rows 6 through 8. See Appendix I for further details on the construction of this table.

Table 3: Waitlist Design: Balance Tests

| | Waitlist Reject Mean | Waitlist Reject SD | Waitlist Admit Mean | Waitlist Admit SD | Difference | SE of Diff | Difference as % of Non-Admit SD | P-Value |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A: Predicted Outcomes* | | | | | | | | |
| Placebo Predicted Top 1% at 33 based on Age 25 Employer | 12.79 | 3.54 | 12.78 | 3.45 | -0.01 | 0.11 | -0.28% | 0.93 |
| Placebo % Attending Graduate School at Age 28 | 10.76 | 4.53 | 10.74 | 4.46 | -0.02 | 0.17 | -0.37% | 0.92 |
| *Panel B: Demographics* | | | | | | | | |
| % Female | 52.09 | 49.96 | 52.08 | 49.84 | -0.01 | 1.46 | -0.02% | 0.99 |
| % Underrepresented Minority | 13.06 | 33.69 | 14.09 | 34.92 | 1.03 | 0.94 | 3.06% | 0.27 |
| % First-Generation College Student | 9.47 | 29.24 | 8.45 | 28.99 | -1.01 | 0.87 | -3.46% | 0.24 |
| *Panel C: Academic Credentials* | | | | | | | | |
| Test Score | 1450.27 | 107.31 | 1442.52 | 111.42 | -7.75 | 3.05 | -7.22% | 0.01 |
| High School Grade Point Average | 3.86 | 0.18 | 3.84 | 0.18 | -0.02 | 0.01 | -9.84% | 0.13 |
| *Panel D: High School Quality and College Applications* | | | | | | | | |
| Predicted Top 1% at 33 based on HS FE on Admissions | 13.97 | 7.77 | 14.37 | 7.07 | 0.40 | 0.30 | 5.16% | 0.19 |
| Number of Standardized Test Score Sends | 10.66 | 4.27 | 10.71 | 4.12 | 0.05 | 0.16 | 1.24% | 0.73 |
| *Panel E: Parent Income and Legacy Status* | | | | | | | | |
| % With Parent Income between 90th-95th Percentile | 15.20 | 35.91 | 15.39 | 35.97 | 0.19 | 1.06 | 0.53% | 0.86 |
| % With Parent Income between 95th-99th Percentile | 25.62 | 43.66 | 26.70 | 44.11 | 1.07 | 1.29 | 2.45% | 0.40 |
| % With Parent Income in Top 1% | 12.03 | 32.54 | 18.49 | 38.64 | 6.46 | 1.10 | 19.86% | 0.00 |
| % Legacy | 6.14 | 24.02 | 14.04 | 34.65 | 7.90 | 1.02 | 32.87% | 0.00 |

*Notes:* This table compares the application characteristics of Ivy-plus applicants who were accepted vs. rejected after being placed on the waitlist. The sample consists of all applicants in our college-specific sample who were offered a place on the waitlist at an Ivy-Plus college. To construct Columns 1 and 3, we regress the variable listed in the relevant row on an indicator for admission and fixed effects for the college at which the applicant is waitlisted. We weight applicants such that the number of matriculants is the same for each college in our sample. We calculate the waitlist reject mean as the overall waitlist mean minus the coefficient on the indicator for admissions times the share of students admitted. The waitlist admit mean is the waitlist reject mean plus the coefficient on admissions from the regression. Columns 2 and 4 report the standard deviation of each variable, separately for students rejected and admitted from the waitlist. Columns 5-8 report the coefficient on the indicator for admission, the standard error of the coefficient (clustered by student to account for students who were waitlisted at multiple schools), that coefficient as percentage of the standard deviation of the relevant variable among students rejected from the waitlist, and a p value for the null hypothesis that the difference is 0. The placebo outcomes in Panel A are predicted values from regressions of outcome variables on indicators for legacy status, parent income bin, the full tuple of admissions office ratings, gender, home state, ethnicity, and recruited athlete status, a quintic in SAT, and the college to which the student applied. In Panel D, the first variable is the predicted value from a regression of the predicted top 1% variable on high school fixed effects, and the second variable is the number of colleges to which each student sends standardized test scores. See Section 2 for other variable definitions.

Table 4: Causal Effects of Attending an Ivy-Plus Instead of State Flagship College 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 (1) | Matriculation Design (2) | Observational VA Estimate (3) | (4) | (5) | (6) | (7) |
| *Panel A: Treatment Effect on Income* | | | | | | | |
| Predicted Probability of Earning in Top 1% | 4.70 | 4.18 | 5.41 | 8.10 | 10.36 | 15.05 | 45% |
| | (1.17) | (0.36) | (0.01) | | | | |
| Predicted Probability of Earning in Top 10% | 3.98 | 4.51 | 4.87 | 41.86 | 47.47 | 51.45 | 8% |
| | (1.78) | (0.86) | (0.01) | | | | |
| Predicted Probability of Earning in Top 25% | 1.70 | 2.75 | 2.91 | 65.56 | 70.04 | 71.75 | 2% |
| | (1.31) | (0.75) | (0.01) | | | | |
| Predicted Mean Income Rank | 1.20 | 1.36 | 1.66 | 76.49 | 78.58 | 79.77 | 2% |
| | (0.74) | (0.44) | (0.00) | | | | |
| *Panel B: Treatment Effect on Non-Monetary Outcomes* | | | | | | | |
| Attend Elite Graduate School at Age 28 | 5.54 | 2.81 | 8.94 | 2.65 | 6.19 | 11.73 | 89% |
| | (2.74) | (0.82) | (0.01) | | | | |
| Attend Non-Elite Graduate School at Age 28 | -0.04 | 0.02 | -0.06 | 13.22 | 13.85 | 13.81 | -0.3% |
| | (0.02) | (1.94) | (0.01) | | | | |
| Work at Elite Firm at Age 25 | 18.30 | 13.91 | 23.66 | 3.74 | 7.19 | 25.49 | 254% |
| | (4.03) | (0.82) | (0.03) | | | | |
| Work at Prestigious Firm at Age 25 | 16.43 | 13.02 | 22.45 | 3.88 | 8.03 | 24.46 | 205% |
| | (4.18) | (0.85) | (0.02) | | | | |
| Work at Elite Firm in Occ. that Precedes Govt. Leadership | 6.08 | 4.73 | 8.02 | 0.83 | 0.36 | 6.44 | 1693% |
| | (2.17) | (0.31) | (0.01) | | | | |
| Work at Prestigious Firm in Occ. that Precedes Govt. Leadership | 4.60 | 4.27 | 6.86 | 0.76 | 0.91 | 5.52 | 505% |
| | (2.12) | (0.31) | (0.01) | | | | |

*Notes:* This table presents regression estimates of the causal effects of attending an average Ivy-Plus college relative to the mean highly selective flagship public college (listed in Appendix Table 1). The first column shows treatment effects based on the waitlist design, calculated by multiplying the waitlist TOT effect on the relevant outcome (as estimated in Figure 10 or Figure 13) by the ratio of the difference in mean observational value-added between the Ivy-Plus and nine flagship public schools and the waitlist TOT effect on value-added of college attended (for the relevant variable). In the second column, we present estimates of the causal effects of colleges based on the matriculation design, following the approach in Figure 14b; see the notes to that figure for details. The third column shows the difference in mean observational value-added (VA) between Ivy-Plus college and public flagship colleges, where observational VA is estimated using a regression of the relevant outcome on college fixed effects and controlling for a quintic parental income, a quintic in SAT scores, race, gender, and home state. Standard errors are reported in parentheses. Columns 4 and 6 show observed means of outcomes for highly selective flagship public and Ivy-Plus attendees in our pipeline sample, respectively. Column 5 shows the implied mean counterfactual outcome were Ivy-Plus students to attend the average highly selective public flagship college, calculated by subtracting the waitlist design causal effect estimates in Column 1 from Column 6. Column 7 reports the percentage difference between Columns 6 and 5. In Panel A, the outcome variables are an 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 10a for details). In the first four rows of Panel B, the outcomes are indicators for attending a highly selective (elite) graduate school at age 28, a non-elite graduate school at age 28, working at an elite firm at age 25, and working at a prestigious firm at age 25. See Section 2.5 for definitions of these outcomes. The last two rows of Panel B define an indicator for working at an elite or prestigious firm at age 25 in an occupation that commonly precedes holding a leadership position in government. We identify such occupations by looking at the top occupations that congressional members worked in prior to Congress from the Brookings Vital Statistics on Congress data (Brookings 2022), which we map to 2-digit SOC codes. We then define the outcomes in the last two rows as indicators for working at elite or prestigious firms in a (self-reported) occupation in one of these SOC codes.

Table 5: Predicted Effects of Changes in Admissions Policies on Socioeconomic Diversity and Outcomes at Ivy-Plus Colleges

| | Parent Income Distribution | | | | Post-College Outcomes | | |
|---|---|---|---|---|---|---|---|
| | 0-60 <$73,000 (1) | 60-95 $73,000-$222,000 (2) | 95-99 $222,000-$611,000 (3) | Top 1% >$611,000 (4) | Predicted Top 1% Income (5) | Share Working at Prestigious Firm (6) | Share Attending Elite Graduate School (7) |
| [1] Observed Data | 15.7% | 42.6% | 25.9% | 15.8% | 16.9% | 33.2% | 15.2% |
| *Counterfactual Policy Change* | | | | | | | |
| [2] Remove Legacy Preferences | 16.6% | 44.5% | 25.2% | 13.7% | 17.0% | 33.3% | 15.0% |
| [3] Remove Legacy + Non-Acad. Advantage | 17.8% | 47.1% | 23.9% | 11.1% | 17.0% | 33.4% | 14.8% |
| [4] Additionally Equalize Athlete Shares | 20.0% | 47.1% | 23.0% | 9.9% | 17.2% | 34.9% | 15.9% |
| [5] Implement Need-Affirmative Preferences for Students with High Academic Ratings | 20.0% | 47.1% | 20.2% | 12.7% | 17.5% | 36.3% | 16.5% |
| [6] Benchmark: Equal Attendance Rates Conditional on SAT Scores | 20.2% | 51.8% | 20.8% | 7.2% | | | |

*Notes*: This table presents estimates of the distribution of parental incomes (Columns 1-4) and post-college outcomes (Columns 5-7) at Ivy-Plus colleges under a series of counterfactual admissions policies. Row 1 presents the actual distribution of parent incomes at the 12 Ivy-Plus colleges. In Row 2, we remove legacy preferences, in two steps: first, we de-admit a fraction of legacy students based on the ratio of their modeled admissions rate as legacies to the predicted admissions rate of otherwise identical students who are not legacies; for instance, if legacy students are admitted at three times the rate of similar non-legacies, we probabilistically de-admit two of out every three legacies. We estimate the legacy and non-legacy admissions rates using our full set of admissions covariates; see the notes to Figure 6 for more details. We estimate the ratio for de-admissions separately for each group of students defined by parent-income bins and SAT/ACT score above/below 1500/34. We then re-admit students to refill the class from those either just de-admitted or those on the waitlist, using admissions rates that are proportional to students' predicted admissions rates from the non-legacy admissions model. In Row 3, we repeat this procedure (starting from the set of admitted students in Row 2) to remove the advantage to higher-income students from higher non-academic ratings. We do so in the same two steps as in Row 2: first, we de-admit students based on the ratio of their non-legacy modeled admissions rate and the admissions rate removing the admissions effect of higher non-academic ratings, and second, we re-admit admit students proportionally to refill the class. We estimate this effect by calculating the admissions rate were students from the top 20% to receive the same non-academic ratings as students from the 70th-80th percentiles with the same test scores and academic ratings; again, we calculate the de-admissions ratios within parent-income x SAT/ACT above/below 1500/34 x academic rating. The counterfactuals in Rows 2 and 3 leave the set of recruited athletes unchanged and work only through changing the admissions rates of non-athlete applicants. In Row 4, beginning from the distribution of admitted students in Row 3, we model a scenario where the characteristics of athletic recruits become identical to that for non-athlete admitted students by removing all recruited athletes and proportionally increasing the admission rates of non-athletes to refill the class. In Row 5, we present a separate counterfactual again beginning from the actual distribution of students in Row 1 and proportionally increasing the admissions rates of students from below the 95th percentile and with high academic ratings. Beginning with predicted admissions rates (modeled separately for legacy and non-legacy applicants, as in Appendix Figure 9b), we increase admissions rates for students from the bottom 60% and 60th to 95th percentiles of the parental income distribution by 2.3x and 1.6x respectively, chosen so that the fraction of students from these two lower income bins exactly matches the fraction in Row 4. We estimate the counterfactuals in Rows 2-5 on data from various subsets of the Ivy-Plus colleges and proportionally rescale the results to apply to the overall distribution of students across all 12 colleges. To predict counterfactual post-college outcomes, we first calculate the potential outcome for each student by subtracting the value-added for the college actually attended and adding back the mean value-added for the Ivy-Plus colleges (see Section 6); we then present the average of potential outcomes for the counterfactually admitted class. Row 6 presents a further separate counterfactual in which all students nationally from parent income groups above the 70-80th percentiles attend Ivy-Plus colleges at rates equal to that for students with the same SAT/ACT scores but from the 70th-80th percentiles. In order to observe outcomes through age 28, we restrict to students who applied to college in the years 2010-2013 throughout this table. See Appendix I for more details.

Appendix Table 1: List of Colleges by Group

*Panel 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 |

*Panel 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 |

*Panel 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 in alphabetical order the colleges within the three groups that we focus on in this paper.

Appendix Table 2: Share of Senior Government Officials Who Worked at Largest Elite and Prestigious Firms, by Sector

| | Elite Firms | | | Prestigious Firms | | |
|---|---|---|---|---|---|---|
| | Senior Government Officials | Population | Ratio | Senior Government Officials | Population | Ratio |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Law | 1.49% | 0.01% | 198.38 | 1.82% | 0.01% | 208.12 |
| Consulting | 0.20% | 0.01% | 18.26 | 0.20% | 0.01% | 18.26 |
| Finance | 0.65% | 0.04% | 15.31 | 0.68% | 0.05% | 14.59 |
| Universities | 0.67% | 0.09% | 7.23 | 0.68% | 0.10% | 6.54 |
| Hospitals | 0.09% | 0.08% | 1.16 | 0.09% | 0.09% | 0.98 |

*Notes:* This table compares the share of senior government officials who were employed at one of the 10 largest elite or prestigious firms, as defined by our algorithm in Section 2.5 and Appendix D, to the share of the overall working population employed at the same firms. Employment data on senior government officials are obtained from the OpenSecrets Revolving Door project, which uses publicly available records to identify 15,276 people who have served in both senior positions in the federal government and as a registered lobbyist or a political operative at some point (see https://www.opensecrets.org/revolving/methodology.php for details). OpenSecrets tracks the employment histories of these individuals, which we use to compute the share who have worked in one of the 10 largest elite or prestigious firms in each industry listed above. To calculate the population shares (columns 2 and 5), we start with the complete set of W-2s from 1999-2021. For each individual and tax year, we keep the W-2 with the highest wage, breaking ties randomly. We sum the number of W-2s at each firm and divide by the total number of W-2s in the industry to get each firm's share of the working population. We identify the 10 largest firms in each industry (Consulting, Finance, Law, Hospitals, and Universities) that we classify as "elite" and "prestigious" respectively. We define sectors using 4-digit NAICS codes: Law is 5411, Hospitals 6220, and Universities 6110. We define Consulting and Financial firms as firms where at least 10% of employees have occupation titles indicating they work in those sectors. Note that many of the largest firms in these industries are both elite and prestigious. The ratios (columns 3 and 6) are obtained by dividing column 1 by column 2 and column 4 by column 5 so that a ratio of 1 reflects proportional representation.

Appendix Table 3: Summary Statistics by College Type, Conditional on Attendance

| | Ivy-Plus | | | Public Flagship | | | Other Selective Private | |
|---|---|---|---|---|---|---|---|---|
| | Pipeline Analysis | Long Term Outcomes | College-Specific Sample | Pipeline Analysis | Long Term Outcomes | College-Specific Sample | Pipeline Analysis | Long Term Outcomes |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A: Standardized Test Scores* | | | | | | | | |
| Test Score | 1405 | 1386 | 1426 | 1211 | 1185 | 1279 | 1330 | 1299 |
| Mean Number of Scoresends | 8.42 | 7.75 | 8.23 | 6.10 | 6.05 | 6.88 | 8.43 | 7.83 |
| *Panel B: Demographics* | | | | | | | | |
| Mean 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% |
| *Panel 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 |
| *Panel 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 | - | 19.9 | 16.6 | - | 7.1 | 9.0 | - | 12.8 |
| Predicted Top 1% at 33 based on Age 25 Employer | 15.0% | 13.9% | 13.3% | 8.1% | 7.0% | 8.7% | 11.3% | 10.1% |
| Predicted Income Rank at Age 33 | 79.8 | 79.3 | 78.6 | 76.5 | 75.5 | 76.8 | 78.3 | 77.5 |
| % 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% | 3.7% | 3.6% | 5.0% | 12.9% | 12.6% |
| % Working at a Prestigious Firm | 24.5% | 26.0% | 31.2% | 3.9% | 3.9% | 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 Panels B-F of Table 1 for subsets of students who attend each of the three groups of colleges defined in Appendix Table 1. Columns 1-3 replicate Columns 1-3 of Table 1 for students attending Ivy-Plus colleges; Columns 4-6 replicate Columns 1, 2, and 4 of Table 1 for students attending highly selective public flagship colleges, and Columns 7 and 8 replicate Column 1 and 2 of Table 1 for students attending other highly selective private colleges. See notes to Table 1 for further details.

Appendix Table 4: College Attendance and Test Score Distributions by Parent Income

| | Parent Income Percentile (in National Distribution) | | | | | | | | | | | | | | | | Share of Ivy-Plus Attendees |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0-10 | 10-20 | 20-30 | 30-40 | 40-50 | 50-60 | 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) | (15) | (16) | (17) |
| *Panel A: Parental Income Distributions* | | | | | | | | | | | | | | | | | |
| **P(Parent Income in Given Range \| Attend)** | | | | | | | | | | | | | | | | | |
| Parental Income Distribution of Ivy-Plus Students | 1.4% | 1.8% | 2.1% | 2.6% | 3.3% | 4.6% | 5.7% | 8.2% | 14.9% | 13.6% | 4.2% | 5.3% | 7.1% | 9.2% | 12.7% | 3.1% | 100.0% |
| Parental Income Distribution of Highly Selective Public Flagship Students | 1.9% | 2.9% | 3.4% | 4.0% | 5.1% | 6.6% | 8.3% | 11.4% | 18.2% | 14.7% | 3.9% | 4.3% | 4.8% | 5.1% | 4.8% | 0.5% | 0.0% |
| *Panel B: 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% | 0.0% | 0.1% | 0.1% | 0.1% | 0.2% | 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% | 0.1% | 0.2% | 0.2% | 0.3% | 0.5% | 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% | 0.4% | 0.6% | 0.7% | 1.1% | 1.7% | 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) | 0.7% | 0.8% | 1.1% | 1.4% | 2.0% | 2.9% | 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) | 1.6% | 2.0% | 2.5% | 3.1% | 4.3% | 6.0% | 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) | 2.3% | 2.9% | 3.5% | 4.3% | 5.7% | 7.4% | 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) | 3.8% | 5.2% | 6.1% | 7.2% | 8.7% | 10.4% | 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) | 3.9% | 5.6% | 6.4% | 7.1% | 7.8% | 8.3% | 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) | 3.6% | 5.4% | 5.8% | 6.0% | 5.9% | 5.7% | 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) | 2.5% | 3.7% | 3.8% | 3.7% | 3.3% | 2.9% | 2.4% | 2.0% | 1.5% | 1.0% | 0.8% | 0.7% | 0.6% | 0.6% | 0.5% | 0.4% | 0.0% |
| below 600 (or ACT below 12) | 1.2% | 1.8% | 1.7% | 1.6% | 1.4% | 1.0% | 0.8% | 0.6% | 0.4% | 0.3% | 0.2% | 0.2% | 0.2% | 0.2% | 0.2% | 0.2% | 0.0% |
| Did not take SAT or ACT | 79.8% | 71.9% | 68.3% | 64.5% | 59.5% | 53.1% | 45.6% | 36.4% | 25.8% | 16.9% | 13.2% | 12.2% | 11.1% | 10.3% | 10.3% | 12.1% | 0.0% |

*Notes:* This table presents statistics on parental income distributions for Ivy-Plus vs. highly selective flagship public attendees (Panel A) and the distribution of SAT/ACT scores by parental income (Panel B). Parent income percentile is defined as parent income rank relative to other parents with children in the same birth cohort in the full national sample from tax records. The first row of Panel A presents the share of all 12 Ivy-Plus college students coming from each parent income group. The second row of Panel A replicates the same statistics for students who attend one of the highly selective public colleges listed in Appendix Table 1. Panel B presents the distribution of SAT scores (or ACT score equivalents) by parent income percentile, showing the fraction who took the test and scored in the range listed in each row conditional on having parent income in the group listed in the relevant column (including non-test-takers in the denominator when calculation fractions). The last row shows the fraction of students in each parent's income bin who did not take either the SAT or ACT. The last column of Panel B shows the share of Ivy-Plus students with test scores in each of the groups listed in the rows. The shares in each column of Panel B sum to 100%. Both panels are based on our pipeline analysis sample.

Appendix Table 5: Quantiles of Parent and Child Income Distributions

| Percentile | Parents' Household Income When Child is Aged 12-17 | Children's Individual Income at Age 33 |
|---|---|---|
| (1) | (2) | (3) |
| 10 | $14,400 | $0 |
| 20 | $23,400 | $0 |
| 30 | $32,900 | $5,400 |
| 40 | $44,200 | $17,500 |
| 50 | $57,800 | $27,000 |
| 60 | $73,200 | $36,200 |
| 70 | $91,100 | $46,700 |
| 80 | $114,400 | $61,000 |
| 90 | $158,200 | $88,300 |
| 95 | $222,400 | $120,600 |
| 96 | $251,100 | $133,100 |
| 97 | $296,500 | $151,600 |
| 98 | $380,000 | $183,200 |
| 99 | $611,400 | $261,000 |
| Top 0.1 | $2,662,300 | $879,100 |

*Notes:* This table lists the dollar amounts for both average parental household income when children are aged 12-17 and children's individual income at age 33 at specific quantiles of the national income distribution, averaging over the 1982-1988 birth cohorts. Children's income percentiles are constructed by ranking children relative to all other children in the same birth cohort and parents' percentiles are defined by ranking parents relative to all other parents with children in the same birth cohort. See Section 2.5 for details on income definitions. All monetary values are in 2015 dollars.

Appendix Table 6: Additional Students from Top 1% at 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] | Counterfactual Students with Parent Income in Top 1% | 93 | | |
| [4] | Total Excess Students with Parent Income in Top 1% | 168 | | 100.0% |
| [5] | Attributable to Application Rates | 49 | | 29.1% |
| [6] | Attributable to Admission Rates | 96 | | 57.6% |
| [7] | Legacy | | 41 | 24.7% |
| [8] | Ratings | | 28 | 16.7% |
| [9] | Athletes | | 27 | 16.2% |
| [10] | Attributable to Matriculation Rates | 23 | | 13.3% |

*Notes:* This table replicates Table 2, except that we report statistics that average over the different possible orderings of policy changes (application, admission, matriculation) rather than prioritizing admissions. To do so, 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. See Table 2 and Appendix I for details.

Appendix Table 7: Waitlist Design Treatment Effect Estimates

| | Raw Means | | With Controls | | Observational Value-Added | |
|---|---|---|---|---|---|---|
| | Pooled | Non-Advantaged | Pooled | Non-Advantaged | Pooled | Non-Advantaged |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Income at Age 33* | | | | | | |
| Actual Top 1% | 5.01 | 5.98 | 3.97 | 5.68 | 3.17 | 3.54 |
| | (2.40) | (2.92) | (2.38) | (2.92) | (0.19) | (0.25) |
| Fraction Earning No Income | 0.98 | 1.32 | 0.84 | 1.03 | 0.02 | 0.01 |
| | (0.54) | (0.69) | (0.50) | (0.64) | (0.02) | (0.03) |
| Actual Mean Income Rank | -0.67 | -1.14 | -0.92 | -1.00 | 0.78 | 0.93 |
| | (1.50) | (1.89) | (1.47) | (1.82) | (0.13) | (0.17) |
| Log(Income), Restricting to Positive Earnings | 0.16 | 0.20 | 0.09 | 0.15 | 0.09 | 0.10 |
| | (0.07) | (0.08) | (0.07) | (0.08) | (0.01) | (0.01) |
| Log(Wage Earnings) | 0.07 | 0.08 | 0.03 | 0.06 | 0.07 | 0.08 |
| | (0.06) | (0.07) | (0.06) | (0.07) | (0.01) | (0.01) |
| *Panel B: Predicted Outcomes Based on Employer at Age 25* | | | | | | |
| Predicted Top 1% Probability | 2.51 | 2.17 | 2.62 | 2.43 | 2.89 | 2.98 |
| | (0.63) | (0.73) | (0.62) | (0.72) | (0.06) | (0.08) |
| Predicted Top 10% Probability | 1.87 | 1.49 | 2.29 | 1.91 | 2.29 | 2.35 |
| | (0.84) | (1.00) | (0.83) | (0.97) | (0.09) | (0.12) |
| Predicted Top 25% Probability | 0.82 | 0.35 | 1.09 | 0.51 | 1.40 | 1.42 |
| | (0.63) | (0.77) | (0.63) | (0.75) | (0.06) | (0.08) |
| Predicted Mean Income Rank | 0.63 | 0.25 | 0.80 | 0.40 | 0.88 | 0.90 |
| | (0.39) | (0.48) | (0.39) | (0.47) | (0.04) | (0.05) |
| *Panel C: Non-Monetary Outcomes* | | | | | | |
| Attend Elite Graduate School at Age 28 | 3.17 | 3.84 | 3.23 | 4.57 | 5.11 | 5.30 |
| | (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 | 9.80 | 9.97 | 9.10 | 9.38 | 12.67 | 12.93 |
| | (2.16) | (2.55) | (2.18) | (2.57) | (0.25) | (0.30) |
| Work at Prestigious Firm at Age 25 | 8.43 | 10.18 | 7.44 | 9.11 | 11.52 | 11.76 |
| | (2.14) | (2.57) | (2.17) | (2.59) | (0.19) | (0.24) |

*Notes:* This table presents estimates of the causal effect of attending an Ivy-plus college in our college-specific sample using the waitlist design, with standard errors (clustered by student) in parentheses. The estimates in Columns 1 and 3 are constructed using the same approach as those reported in Figures 10 and 13; see notes to those figures for details. The estimates in Columns 2, 4, and 6 replicate estimates in Columns 1, 3, and 5 respectively except excluding legacy applicants, recruited athletes, and applicants with parents in the top 1% of the income distribution. Columns 5 and 6 report treatment effects on the quality of college attended, as measured by the college's observational value-added on the relevant outcome. Observational value-added estimates are based on OLS regressions of outcomes on fixed effects for the college students attend, controlling for parental income, SAT scores, race, gender, birth cohort, and home state, estimated using our pipeline analysis sample. In Panel A, we report estimates for actual earnings outcomes at age 33. In row 1, the outcome in an indicator for having earnings in the top 1% at age 33. Row 2 defines the outcome as the share earning no income at age 33. Row 3 presents estimates on mean income rank. In row 4, the outcome is log income in 2015 dollars, restricting to individuals with positive income. Row 5 presents estimates for wage earnings, defined as W-2 earnings plus self-employment earnings for single filers or W-2 earnings plus self-employment earnings plus half of the difference between total wage earnings and self-employment reported on the Form 1040 and the sum of both filers' W-2 and Form SE earnings for joint filers. In row 5, we further restrict to earnings above $15,800 to match the restriction imposed by Dale and Krueger (2014). Panel B presents estimates for predicted outcomes based on employer at age 25 and Panel C presents estimates for non-monetary outcomes. See Section 2.5 for definitions of variables in Panels B and C.

Appendix Table 8: Heterogeneity in Treatment Effects of Ivy-Plus Admission by Strength of Outside Options

| | Predicted Top 1% | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Observational VA of College Attended | 0.87<br>(0.24) | 0.98<br>(0.39) | 0.81<br>(0.34) | 0.89<br>(0.25) | 1.00<br>(0.33) | 1.05<br>(0.26) | 0.85<br>(0.24) |
| Implied effect of attending Ivy-Plus Instead of Flagship Public (pp) | 4.58<br>(1.23) | 5.12<br>(2.02) | 4.23<br>(1.78) | 4.67<br>(1.31) | 5.26<br>(1.72) | 5.48<br>(1.36) | 4.44<br>(1.27) |
| ***Grouping Instrument Construction*** | Baseline: Homestate, Race, Income School Applied | Baseline with Jackknife | CZ Only | Flexible Regression | Constructed on Regular Reject Sample | Dropping Multi-Campus Groups | Baseline, but dropping FE for Ivy-plus college to which student applied |

*Notes:* This table presents estimates of how the causal effect of being admitted to an Ivy-Plus college on a student's predicted chances of reaching the top 1% (based on age 25 firm) varies with the student's outside options. The first row of each column reports the coefficient from a 2SLS regression of the predicted top 1% outcome on the observational VA of the college the individual attends, instrumenting for observational VA with the interaction between an admissions indicator and the gain in observational VA from Ivy-plus admission relative to the mean outside option for that group of students ($g_j$). We also control directly for the admissions indicator, $g_j$, and indicators for the Ivy-Plus college to which the student applied. The regression is estimated using the sample of students waitlisted at Ivy-Plus colleges in our college-specific sample. Standard errors are clustered by student, and reported in parentheses. The second row reports the implied causal effect of attending the average Ivy-Plus college instead of the average state flagship public college listed in Appendix Table 1, estimated as the regression coefficient multiplied by the difference in observational VA between the average Ivy-plus college and the average highly selective state flagship. In Column 1, we divide students into groups g based on their home state, race, parent income, and Ivy-Plus college applied to, and estimate their outside option as the mean observational value-added of the colleges that students rejected from the waitlist in their group attend, as in Figure 12. Columns 2-7 present alternative estimates using different methods of constructing students' outside options. In Column 2, we leave out the own student when calculating the average value-added of the outside option among students rejected from the waitlist. In Column 3, we group students based only on their commuting zone (CZ) of residence, as measured in the tax data. Column 4 predicts the outside option using an OLS regression (estimated among waitlist rejects) of observational VA on the following controls: college attended interacted with the school year, parent income bin, race, dummies for test scores, home state, and gender. Column 5 estimates the outside options using the approach in Column 1, but using the pool of rejected applicants not offered a place on the waitlist. Column 6 omits large multi-campus groups for which we cannot estimate college-specific value-added of outside options (see Chetty et al. 2020 for more details on this issue). Column 7 uses the same outside option definition as in Column 1 but drops the fixed effect for the college on which a student is on the waitlist from the regression specification so that the variation in the instrument comes both from outside options and differences in observational VA across the Ivy-plus colleges in our college-specific sample.

Appendix Table 9: Heterogeneity in Causal Effects of Ivy-Plus Attendance on Predicted Top 1% Earnings Probability

| | Effects on Predicted Top 1% Probability | |
|---|---|---|
| | Waitlist Design (1) | Matriculation Design (2) |
| *Panel A: Pooled Sample Estimate* | 2.51 | 4.18 |
| | (0.63) | (0.36) |
| *Panel B: Heterogeneity by Parental Income* | | |
| P0-P60 | 1.42 | 4.29 |
| | (1.45) | (0.87) |
| P60-P95 | 3.17 | 2.95 |
| | (1.08) | (0.62) |
| P95-P99 | 1.27 | 2.55 |
| | (1.20) | (1.02) |
| Top 1% | 2.68 | 5.38 |
| | (1.83) | (4.22) |
| P-Value from F-test of null of no heterogeneity | 0.29 | 0.98 |
| *Panel C: Heterogeneity by Test Score* | | |
| $<$ 1300 | 3.11 | 1.24 |
| | (2.64) | (0.79) |
| 1300-1400 | 2.07 | 3.43 |
| | (1.21) | (1.56) |
| 1400-1500 | 1.56 | 2.12 |
| | (0.99) | (5.12) |
| 1500-1600 | 4.74 | 3.63 |
| | (1.30) | (15.40) |
| P-Value from F-test of null of no heterogeneity | 0.18 | 0.92 |
| *Panel D: Heterogeneity by Academic Rating* | | |
| High Academic Rating | 3.55 | |
| | (1.69) | |
| Low Academic Rating | 3.37 | |
| | (2.09) | |
| P-Value from F-test of null of no heterogeneity | 0.97 | |
| *Panel E: Heterogeneity by Athlete Status* | | |
| Athlete | 6.36 | |
| | (5.61) | |
| Non-Athlete | 2.54 | |
| | (0.68) | |
| P-Value from F-test of null of no heterogeneity | 0.48 | |
| *Panel F: Heterogeneity by Non-Academic Rating* | | |
| High Non-Academic Rating | 2.81 | |
| | (1.51) | |
| Low Non-Academic Rating | 5.36 | |
| | (2.96) | |
| P-Value from F-test of null of no heterogeneity | 0.31 | |
| *Panel G: Heterogeneity by Legacy Status* | | |
| Legacy | 6.30 | |
| | (1.65) | |
| Non-Legacy | 2.25 | |
| | (0.74) | |
| P-Value from F-test of null of no heterogeneity | 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 using the waitlist design, following the estimator in the first pair of bars in Figure 10b; see notes to that figure for details. Column 2 presents estimates using the matriculation design, following the approach in Figure 14b; again, see notes to that figure for details. Panel A shows estimates for the full sample, while Panels B-G report estimates among subgroups of applicants with certain observable characteristics. The last row of each panel reports p-values for the null hypothesis of homogeneous treatment effects across the subgroups shown in each panel.

Appendix Table 10: Distribution of Earnings at Age 33 by College Type

| Percentile | Ivy-Plus | Uneweighted Highly Selective Public Flagship | Reweighted Highly Selective Public Flagship | Unweighted Other Highly Selective Private | Reweighted Other Highly Selective Private |
|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) |
| 20 | $50,300 | $35,400 | $42,300 | $41,900 | $45,500 |
| 40 | $87,900 | $59,800 | $73,700 | $74,600 | $79,100 |
| 60 | $141,100 | $88,600 | $112,800 | $113,700 | $121,700 |
| 70 | $182,500 | $108,900 | $141,500 | $143,400 | $154,500 |
| 80 | $248,500 | $139,200 | $188,400 | $189,800 | $206,800 |
| 90 | $385,700 | $206,100 | $286,800 | $287,500 | $310,000 |
| 95 | $586,700 | $297,100 | $401,900 | $404,600 | $432,700 |
| 96 | $687,700 | $330,000 | $443,000 | $451,500 | $483,200 |
| 97 | $847,100 | $376,000 | $511,500 | $525,300 | $554,200 |
| 98 | $1,162,900 | $448,100 | $651,400 | $660,600 | $701,200 |
| 99 | $1,902,000 | $642,900 | $985,700 | $1,041,900 | $1,100,000 |
| Top 0.1 | $12,203,700 | $3,003,100 | $4,147,100 | $5,839,600 | $5,529,100 |

*Notes:* This table presents quantiles of the distributions of earnings at age 33 for individuals who attended Ivy-Plus, Highly Selective Public Flagship, or Other Highly Selective Private colleges (listed in Appendix Table 1). Column 2 reports the distribution of earnings for individuals who attended Ivy-Plus colleges. Columns 3 and 5 report distributions of earnings among students who attended public flagship and other selective private colleges, respectively. Columns 4 and 6 replicate columns 3 and 5 after reweighting on parent income bin, gender, race, and test score to match Ivy-Plus matriculants. The sample consists of all students in our long-term outcomes sample for whom we observe college attendance, SAT/ACT scores, and income at age 33.

Appendix Table 11: Association Between Ivy-Plus Students' Post-College Outcomes and ACT/SAT Scores vs. High School GPA

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *Panel A: Predicted Top 1% Earnings Probability* | | | | | | |
| SAT (or converted ACT) Percentile | 0.082 | | 0.084 | 0.071 | 0.070 | 0.044 |
| | (0.007) | | (0.007) | (0.008) | (0.009) | (0.013) |
| High School GPA Percentile | | 0.005 | -0.011 | -0.001 | -0.001 | 0.033 |
| | | (0.008) | (0.008) | (0.009) | (0.009) | (0.018) |
| Legacy | | | | -1.582 | -1.582 | -1.859 |
| | | | | (0.607) | (0.614) | (0.829) |
| Recruited Athlete | | | | 0.718 | 0.641 | -0.031 |
| | | | | (0.670) | (0.679) | (1.042) |
| $R^2$ | 0.018 | 0.000 | 0.019 | 0.036 | 0.055 | 0.282 |
| Mean of Dependent Variable | 16.918 | 16.918 | 16.918 | 16.918 | 16.927 | 17.473 |
| Implied Difference Between SAT Score of 1400 and 1600 | 5.825 | | 5.933 | 5.038 | 4.953 | 3.067 |
| Implied Difference Between GPA of 3.75 and 4.0 | | 0.267 | -0.534 | -0.068 | -0.026 | 1.701 |
| Race, Gender, and Parent Income FEs | No | No | No | Yes | Yes | Yes |
| Race x Gender x Parent Income FEs | No | No | No | No | Yes | Yes |
| High School FE | No | No | No | No | No | Yes |
| Number of Observations | 7,067 | 7,067 | 7,067 | 7,067 | 7,056 | 5,270 |
| *Panel B: Elite Graduate School Attendance at Age 25* | | | | | | |
| SAT (or converted ACT) Percentile | 0.149 | | 0.142 | 0.135 | 0.136 | 0.095 |
| | (0.015) | | (0.016) | (0.018) | (0.019) | (0.029) |
| High School GPA Percentile | | 0.072 | 0.046 | 0.032 | 0.032 | 0.133 |
| | | (0.017) | (0.017) | (0.018) | (0.018) | (0.037) |
| Legacy | | | | -2.280 | -2.127 | -2.421 |
| | | | | (1.336) | (1.351) | (1.800) |
| Recruited Athlete | | | | -4.671 | -4.331 | -0.566 |
| | | | | (1.252) | (1.269) | (1.955) |
| $R^2$ | 0.013 | 0.002 | 0.014 | 0.028 | 0.047 | 0.267 |
| Mean of Dependent Variable | 15.718 | 15.718 | 15.718 | 15.718 | 15.700 | 16.073 |
| Implied Difference Between SAT Score of 1400 and 1600 | 10.577 | | 10.107 | 9.563 | 9.668 | 6.770 |
| Implied Difference Between GPA of 3.75 and 4.0 | | 3.693 | 2.338 | 1.649 | 1.651 | 6.761 |
| Race, Gender, and Parent Income FEs | No | No | No | Yes | Yes | Yes |
| Race x Gender x Parent Income FEs | No | No | No | No | Yes | Yes |
| High School FE | No | No | No | No | No | Yes |
| Number of Observations | 7,081 | 7,081 | 7,081 | 7,081 | 7,070 | 5,282 |
| *Panel C: Employment at Prestigious Firm at Age 25* | | | | | | |
| SAT (or converted ACT) Percentile | 0.226 | | 0.219 | 0.172 | 0.178 | 0.152 |
| | (0.024) | | (0.024) | (0.029) | (0.030) | (0.047) |
| High School GPA Percentile | | 0.090 | 0.049 | 0.043 | 0.042 | 0.161 |
| | | (0.027) | (0.027) | (0.028) | (0.029) | (0.063) |
| Legacy | | | | -0.879 | -0.800 | -3.578 |
| | | | | (2.133) | (2.172) | (2.947) |
| Recruited Athlete | | | | -7.249 | -6.965 | -4.844 |
| | | | | (2.185) | (2.217) | (3.633) |
| $R^2$ | 0.017 | 0.002 | 0.017 | 0.023 | 0.051 | 0.306 |
| Mean of Dependent Variable | 35.767 | 35.767 | 35.767 | 35.767 | 35.764 | 37.598 |
| Implied Difference Between SAT Score of 1400 and 1600 | 15.858 | | 15.376 | 12.013 | 12.415 | 10.552 |
| Implied Difference Between GPA of 3.75 and 4.0 | | 4.461 | 2.439 | 2.098 | 2.110 | 8.287 |
| Race, Gender, and Parent Income FEs | No | No | No | Yes | Yes | Yes |
| Race x Gender x Parent Income FEs | No | No | No | No | Yes | Yes |
| High School FE | No | No | No | No | No | Yes |
| Number of Observations | 5,164 | 5,164 | 5,164 | 5,164 | 5,156 | 3,580 |

*Notes:* This table presents OLS regression estimates of students' post-college outcomes on various student characteristics at the time of application. In each panel, each column presents results from a single regression on the variables listed in that column. SAT (or converted ACT) scores and high school grade point average (GPA) are converted to percentile ranks among Ivy-plus attendees so that the coefficients on those variables can be interpreted as the effect of a 1 percentile increase in the explanatory variable on the outcome. The dependent variable in Panel A is the predicted top 1% share based on firm at age 25 (see Section 2.5 for more details); in Panels B and C, it is an indicator variable for attending an elite graduate school and working at a prestigious firm at age 25, respectively (see Section 2.5 for more details). The sample consists of all students enrolled at Ivy-plus colleges in our college-specific sample for whom we have high school GPAs and other requisite variables to estimate the regressions.

## Appendix Table 12: Differences in Post-College Outcomes by Applicant Characteristics at Ivy-Plus Colleges

| | No Controls | | Controlling for Test Score | | Controlling for Parent Income Bin | | Controlling for Test Score Gender, and Race | | Controlling for All Observables | | Controlling for All Observables & 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: Predicted Top 1% Earnings Probability* | | | | | | | | | | | | |
| High Academic Rating vs. Low Academic Rating | 4.41 | 3.69 | 1.10 | 0.75 | 4.20 | 3.41 | 0.97 | 0.54 | 1.69 | 1.17 | 1.68 | 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 | 0.31 | -0.67 | 2.92 | 1.45 | -0.01 | -0.96 | 2.88 | 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 | -0.73 | -0.81 | -0.51 | -0.62 | -1.97 | -1.85 | -0.08 | -0.38 | -0.84 | -1.04 | -1.21 | -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 | 0.22 | -0.49 | 0.88 | 0.07 | 0.21 | -0.48 | 1.13 | 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 | 2.19 | 1.71 | 2.19 | 1.71 | 1.96 | 1.47 | 2.00 | 1.28 | 0.99 | 0.55 | 0.92 | 0.49 |
| | (0.16) | (0.17) | (0.16) | (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 | 4.20 | 3.42 | 4.20 | 3.42 | 3.87 | 3.07 | 3.74 | 2.62 | 2.03 | 1.34 | 1.93 | 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 | 6.26 | 5.16 | 6.26 | 5.16 | 5.93 | 4.80 | 5.37 | 3.92 | 2.96 | 2.10 | 2.88 | 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: 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: Employment at Prestigious Firm at Age 25* | | | | | | | | | | | | |
| 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) |
| *Panel D: Employment at Elite Firm at Age 25* | | | | | | | | | | | | |
| 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) |

*Notes:* This table replicates estimates from Figure 16 using other student characteristics and additional controls. In each pair of columns, the first (odd-numbered) column presents "raw" differences in outcomes between the two groups listed in the relevant row, controlling for certain variables but without adjusting for differences in the observational value-added of the colleges' applicants attended (as in the left bar in each pair in Figure 16a). The second (even-numbered) column presents the same estimate, subtracting out the difference in the observational value-added of college attended 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 7 (as in Figures 16b-d). Each row reports an estimate from a separate regression. Each pair of columns includes a different set of controls when estimating both the raw effects and differences in observational VA: in Columns 1-2, no additional controls; Columns 3-4, controls for a quintic in test scores; Columns 5-6, controls for the 13 parent income group indicators (the same bins used in Figure 2); Columns 7-8, controls for a quintic in test scores as well as indicators for gender and race/ethnicity; Columns 9-10, controls for indicators for gender and race/ethnicity, indicators of the combination of student's academic and non-academic ratings, the year applied, an indicator for early applications, first-generation status, a quadratic of high school GPA, and teacher, guidance counselor, and alumni ratings; and Columns 11-12, controls for all variables used in 9-10 as well as the parent income bins. The four panels each consider different outcomes; see Section 2.5 for definitions of these outcomes. The sample consists of students either admitted or offered a place on the waitlist at the Ivy-plus college for which we have internal data in our college-specific sample. The first row and fourth rows of each panel, which compare academic and non-academic ratings, limit the sample to students admitted or offered a place on the waitlist at the Ivy-Plus college with the most granular rating data. Baseline rates of outcomes for low academic rating, non-athlete, non-legacy, and low non-academic rating students with SATs in the first quartile (below 1400) are 10.4% (Panel A), 6.8% (Panel B), 17.4% (Panel C), and 19.2% (Panel D).

Appendix Table 13: 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) | -0.15 | -0.03 | 0.57 | 0.65 | -0.13 |
| | (0.05) | (0.05) | (0.10) | (0.10) | (0.28) |
| Diff-in-Diff Estimate (Middle vs High Income) | 0.07 | 0.07 | 0.56 | 0.40 | 0.64 |
| | (0.05) | (0.05) | (0.10) | (0.10) | (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) | -0.06 | 0.25 | 0.19 | 0.28 | 0.51 |
| | (0.10) | (0.10) | (0.13) | (0.13) | (0.19) |
| Diff-in-Diff Estimate (Middle vs High Income) | 0.17 | 0.15 | 0.26 | 0.16 | 0.40 |
| | (0.10) | (0.10) | (0.13) | (0.13) | (0.17) |
| State Fixed Effects | | X | | X | X |
| Year Fixed Effects | | X | | X | X |
| Parent Income Controls | | X | | X | X |
| Race Fixed Effects | | | | | X |
| *Panel C: Four-Year College Attendance* | | | | | |
| | All Students | | | | |
| Diff-in-Diff Estimate (Low vs High Income) | -0.64 | -0.64 | | | |
| | (0.12) | (0.12) | | | |
| Diff-in-Diff Estimate (Middle vs High Income) | 0.29 | 0.26 | | | |
| | (0.12) | (0.12) | | | |
| State Fixed Effects | | X | | | |
| Year Fixed Effects | | X | | | |
| Parent Income Controls | | X | | | |

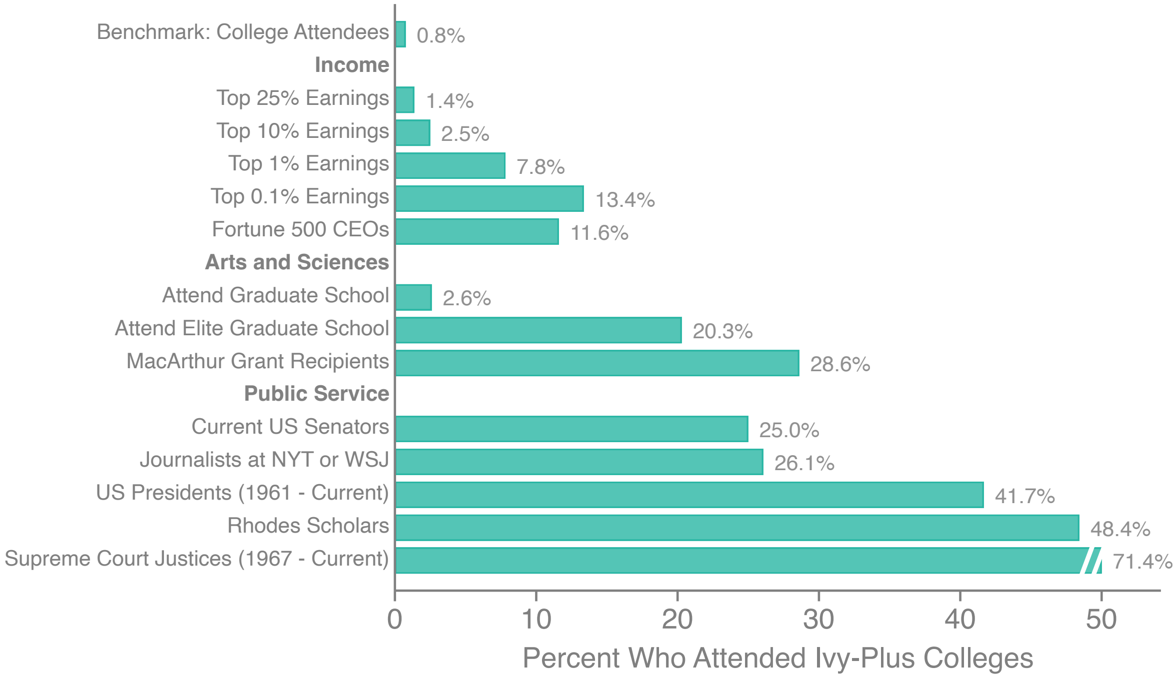
*Notes:* This table presents regression estimates of the effects of a difference-in-differences analysis comparing college attendance rates before and after the expansion of Pell and AOTC in 2009. The treated groups are children with parental incomes between $0 and $40,000 (low-income) and between $40,000 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.

Appendix Table 14: Examples of High Schools by High School Type, by Metro Area

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

*Notes:* This table presents illustrative examples of high schools from each of the four categories in Figure 8a in twelve large metropolitan areas in the U.S. 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. We classify high schools that fall in the top 20% of this index of advantage as "advantaged." The schools in these examples were identified using publicly available data; they were not chosen based on their presence in any of our confidential datasets or based on their estimated fixed effects.

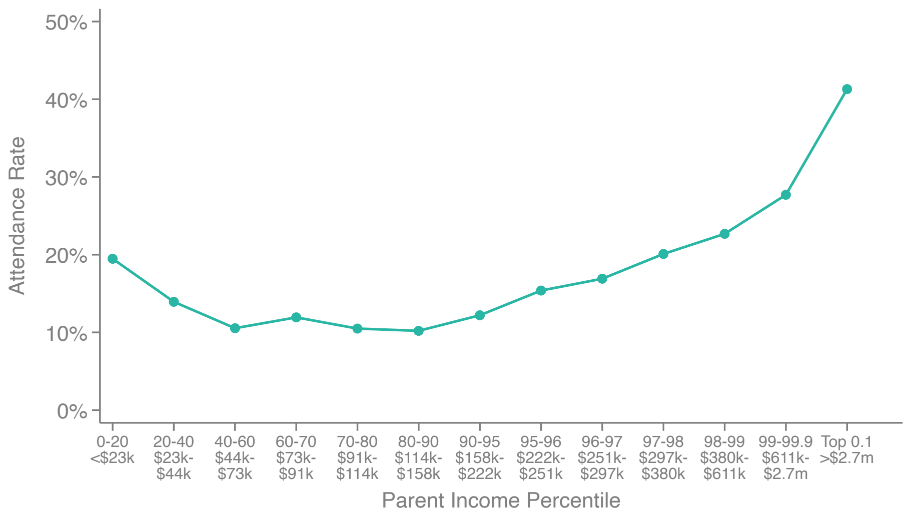
Figure 1: Share of Individuals in Leadership Positions who Attended Ivy-Plus Colleges



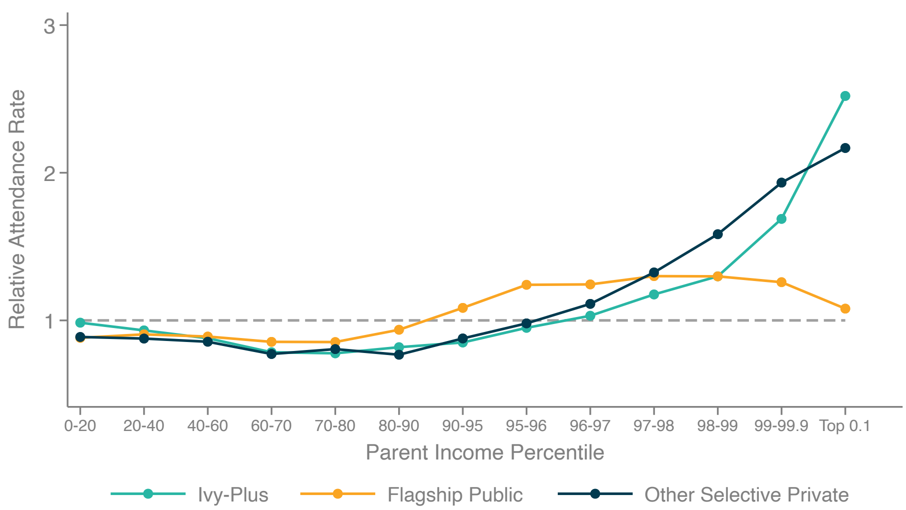

*Notes:* Figure 1 shows the proportion of individuals in various subgroups who attended an Ivy-Plus college (the eight Ivy-League colleges, Chicago, Duke, Stanford, and MIT) as an undergraduate. See Appendix A for definitions and sources for each of these outcome variables.

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



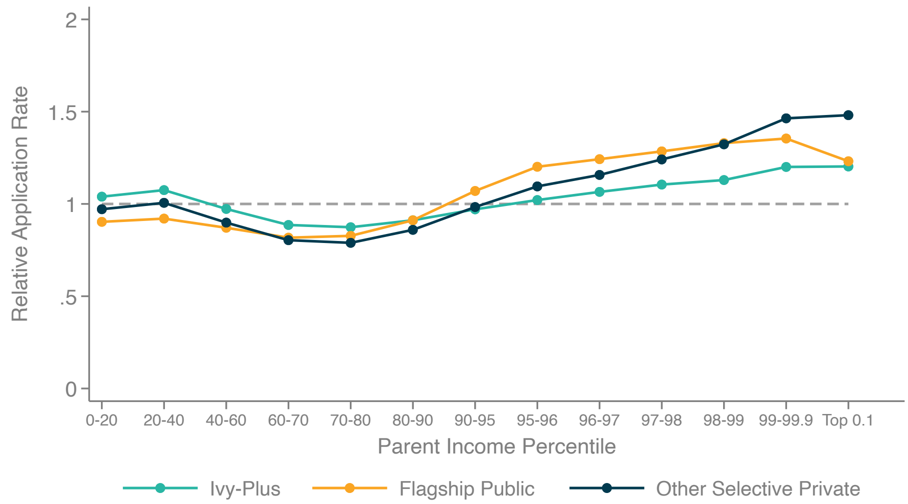

(b) Attendance Rates at Selective Colleges by Parental Income, Controlling for Test Scores



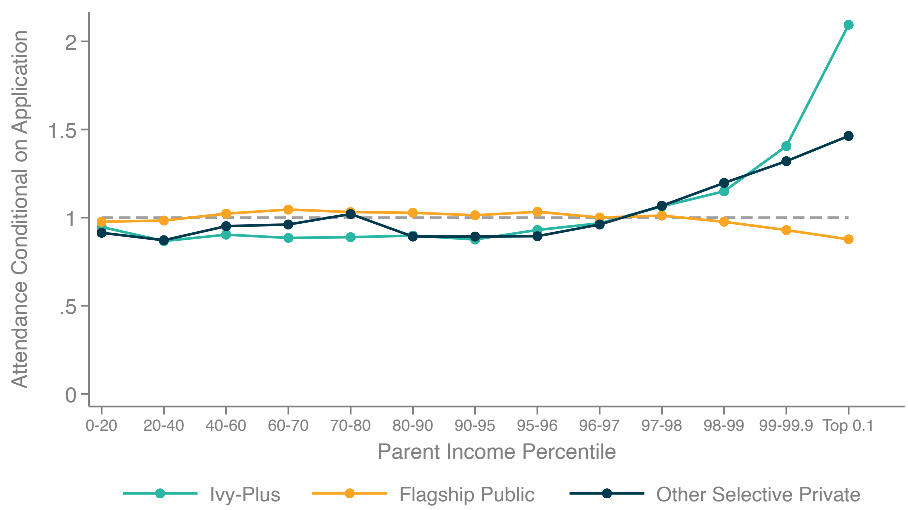

*Notes:* Figure 2a plots the share of individuals with an SAT score of 1510 (out of 1600) or an ACT composite score of 34 (out of 36) who attend an Ivy-Plus college, by parental income. To construct the series for Ivy-Plus colleges in Figure 2b, we calculate the attendance rate at each Ivy-Plus college (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 overall distribution of test scores for students attending each college. We then calculate the relative attendance rate at each college by dividing the resulting test-score-reweighted attendance rate by the mean test-score-reweighted average attendance rate (across students from all parent-income bins). Finally, we take an unweighted average of the 12 college-specific series. Figure 2b plots this relative attendance rate series for the twelve Ivy-Plus colleges, as well as similarly constructed relative attendance rates for the 12 other highly selective private colleges and 9 highly selective public flagship colleges listed in Appendix Table 1. The sample for both panels 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



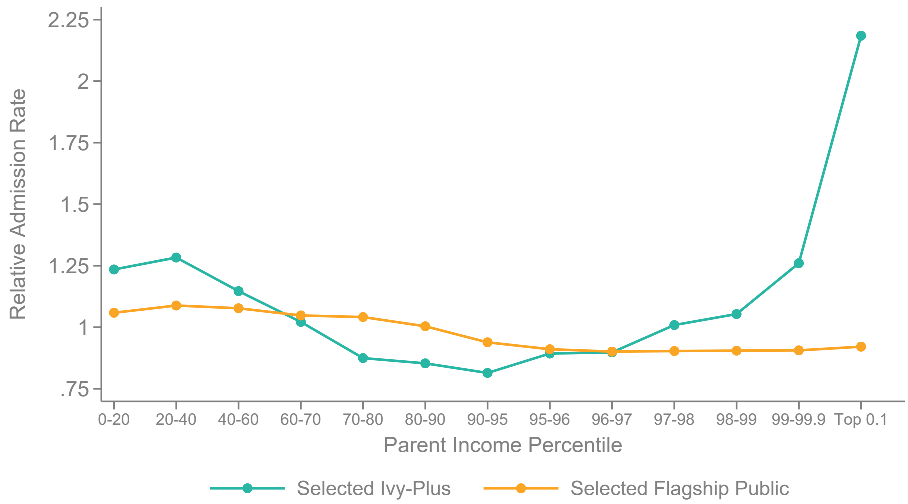

(b) Attendance Rates Conditional on Application



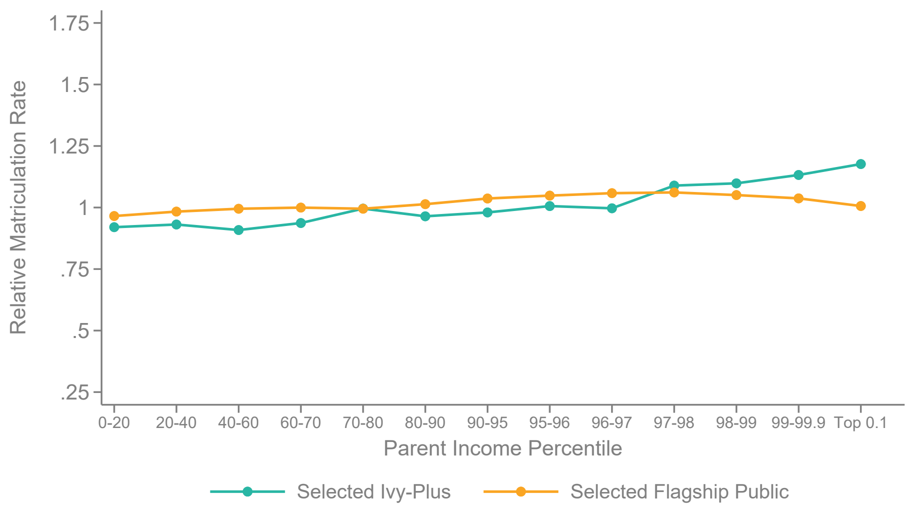

*Notes:* Figure 3a replicates Figure 2b but with application rates rather than attendance rates, where application rates are predicted using score sending data as described in Appendix B. Figure 3b replicates Figure 2b but with attendance rates conditional on application, defined as the ratio of attendance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

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

(a) Admissions Rates



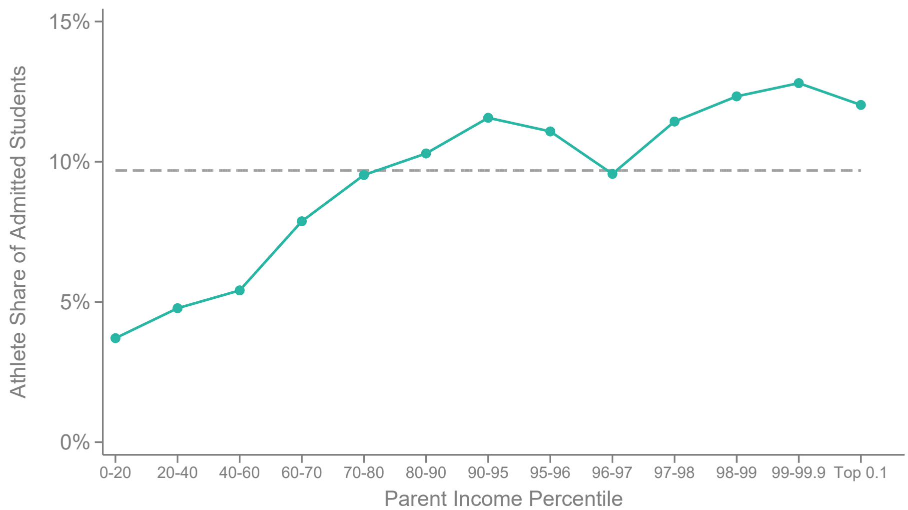

(b) Matriculation Rates



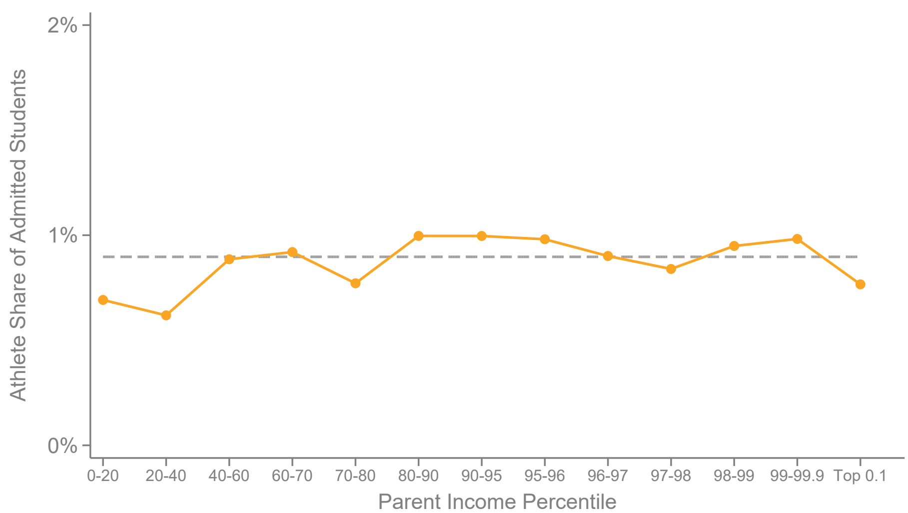

*Notes:* Figure 4a and Figure 4b plot admissions and matriculation (or yield) rates by parental income, controlling for test scores. We reweight students within each parent income bin on test scores using the same method as in Figure 2; see notes to Figure 2 for details. The sample for these figures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

Figure 5: Share of Admitted Students who are Recruited Athletes, by Parental Income

(a) Selected Ivy-Plus Colleges



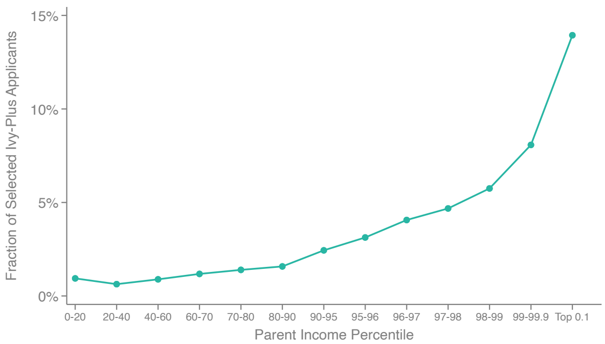

(b) Selected Flagship Public Colleges



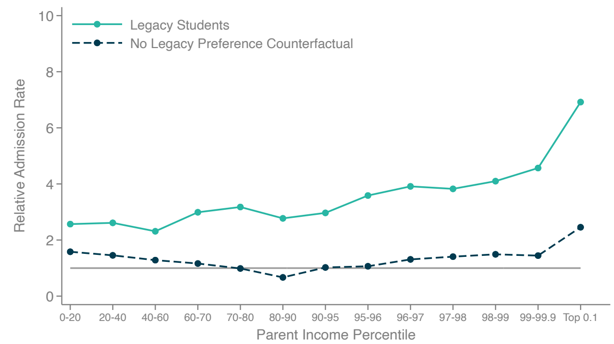

*Notes:* Figure 5a and Figure 5b plot the fraction of admits who are recruited athletes by parent income bin at Ivy-Plus and highly selective public flagship schools, respectively. The dashed lines show the mean share of admits who are recruited athletes. The sample for these figures is our college-specific sample, a selected subset of Ivy-Plus and highly selective public flagship schools for which we have linked internal admissions data.

Figure 6: Ivy-Plus Legacy Applicant Shares and Admissions Rates, by Parental Income

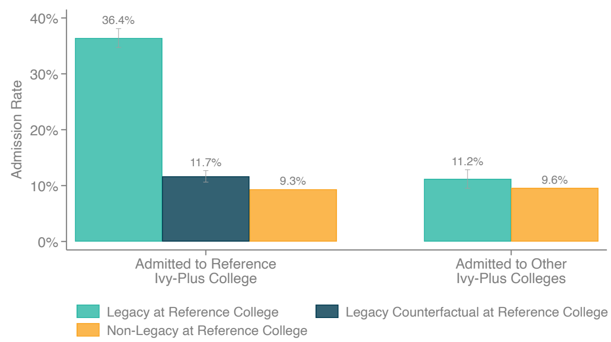
(a) Share of Legacy Applicants by Parental Income, Controlling for Test Scores




(b) Ivy-Plus Admissions Rates for Legacy Applicants, Controlling for Test Scores



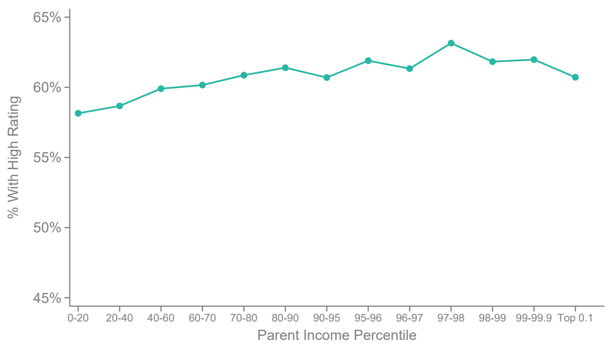

(c) Admission Rate by Legacy Status at Parent's College vs. Other Ivy-Plus Colleges, Controlling for Test Scores



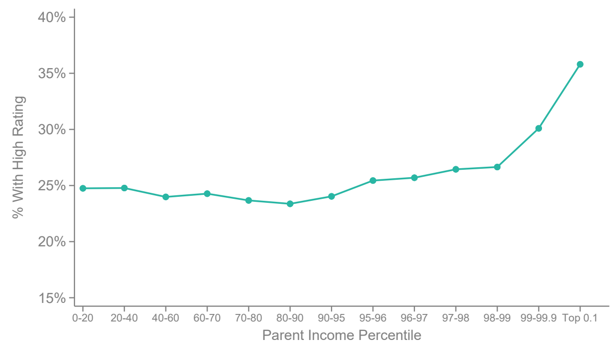

*Notes:* Figure 6a plots the share of non-athlete applicants to selected Ivy-Plus colleges who are children of alumni (i.e., legacy students) by parent income level, controlling for test scores using the same reweighting procedure as in Figure 2b. Figure 6b plots two series. The solid (green) series plots admissions rates for legacy applicants in each parent income bin, reweighting those applicants across test score bins to match the distribution of test score for all attendees (as in Figure 2b). The dashed (dark blue) series replicates the solid series using a counterfactual admissions rate for legacy students if they did not benefit from legacy preferences in admissions but had otherwise identical application credentials. We divide the admissions rates in both series by the mean test-score-reweighted predicted counterfactual admissions rate for all applicants, so that the values can be interpreted as admissions rates relative to the average applicant absent legacy preferences. To calculate counterfactual admissions rates absent legacy preferences, we first estimate a linear probability model to predict admissions of non-legacy students using indicators for race, gender, first-generation status, entering cohort, and application round, fixed effects for the full tuple of admissions office ratings, high-school GPA (where available), parent income bin, and high-school fixed effects, reweighting students to match all attendees on test score. We then apply the coefficients from this admissions model for non-legacies to predict a counterfactual admissions rate based on the individual characteristics of each legacy student. Figure 6c compares admissions rates for legacy vs. non-legacy students across colleges for non-athletes who apply regular decision to multiple Ivy-Plus colleges in our college-specific sample. The first bar plots mean admissions rates at Ivy-Plus colleges for applicants whose parents are alumni of that college. The second bar plots the mean counterfactual non-legacy admissions rate (constructed as above) for the same group of students. To construct the remaining bars, we regress admissions rates at each Ivy-Plus college on indicators for legacy status at that college, legacy status at other Ivy-Plus colleges, and a quintic in SAT/ACT scores. The third bar plots the implied admissions rate for non-legacy applicants (controlling for SAT scores) based on this regression. The fourth bar plots the admissions rate at other Ivy-Plus colleges (i.e., a college the applicant's parents did not attend) for legacy students at a given college. The fifth bar plots the admissions rate for non-legacy applicants at other Ivy-Plus colleges. All results are based on the selected set of Ivy-Plus colleges for which we have linked internal admissions data; see Section 2 for details.

Figure 7: Admissions Office Ratings of Applicants by Parental Income, Controlling for Test Score

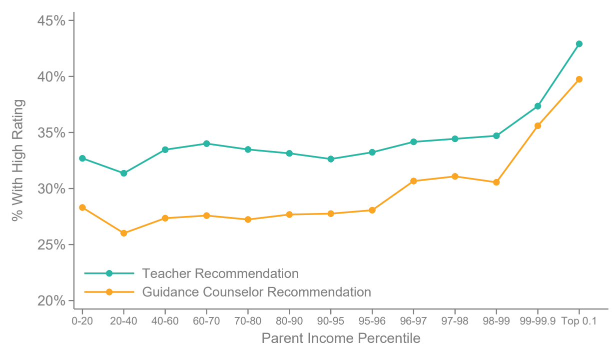
(a) Academic Ratings




(b) Non-Academic Ratings



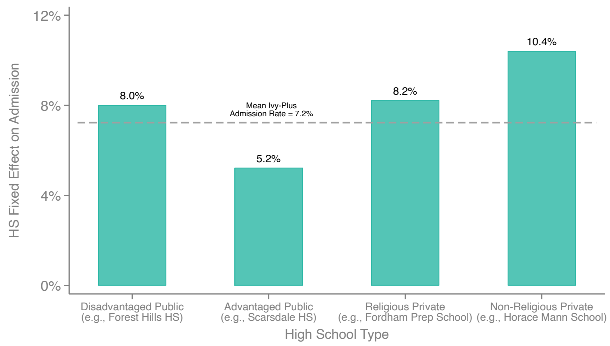

(c) Teacher and Guidance Counselor Ratings



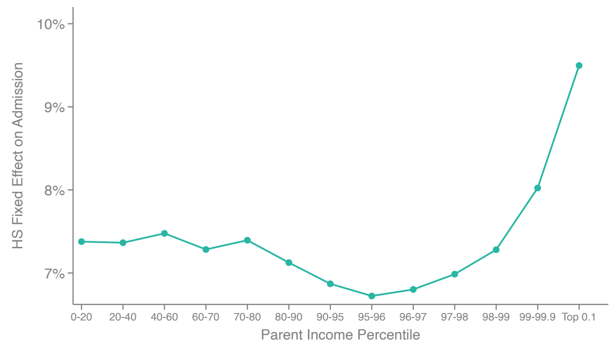

*Notes:* Figure 7 plots the proportion of Ivy-Plus applicants receiving high admissions ratings on various dimensions by parent-income bin, reweighting applicants to control for test scores as in Figure 2. Figure 7a considers academic ratings; Figure 7b non-academic ratings; and Figure 7c ratings of letters of recommendation from teachers (green) and school guidance counselors (orange). All figures exclude recruited athletes, faculty children, 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 12 for analogous figures that pool all Ivy-Plus colleges in our sample and use coarser ratings.

Figure 8: Differences in Ivy-Plus Admissions Across High Schools

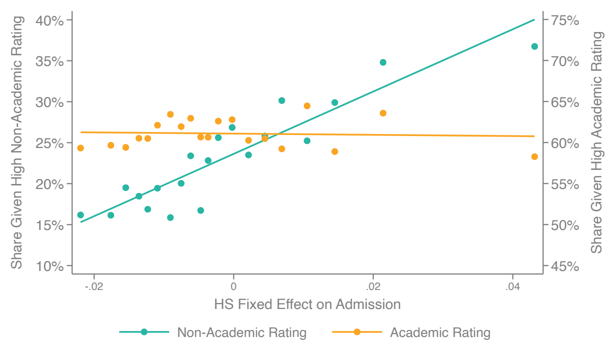
(a) High School Fixed Effect on Admissions by High School Type




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

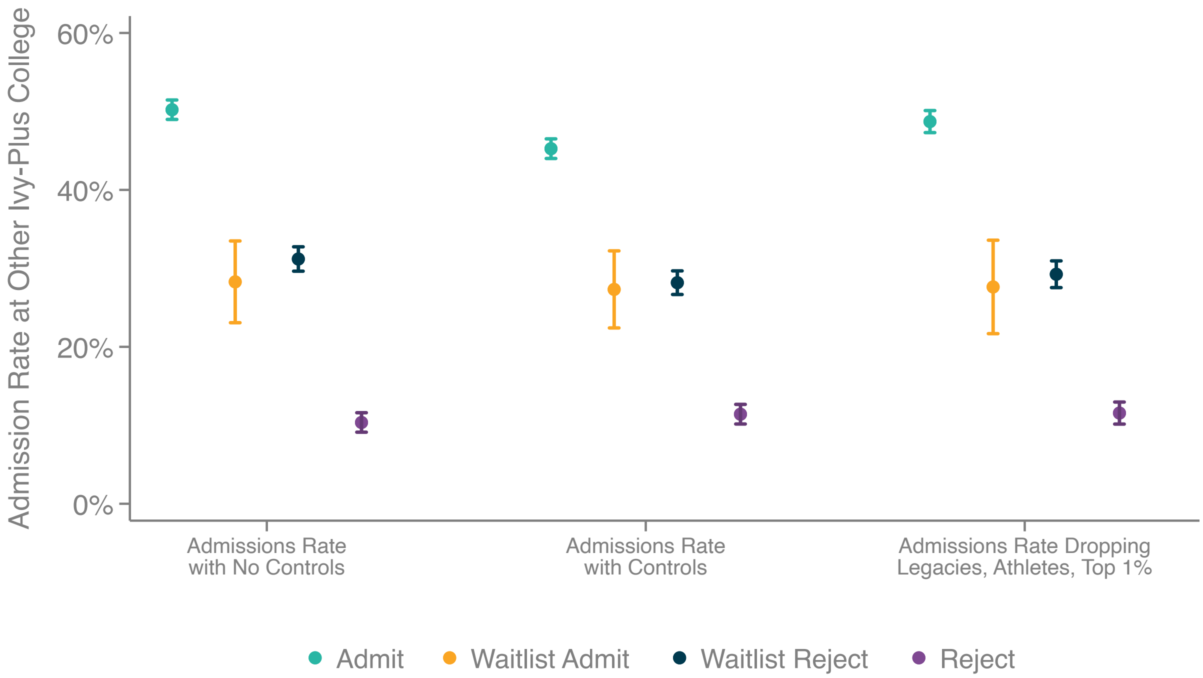



(c) Share of Ivy-Plus Students with High Ratings by High School Fixed Effect on Admissions




*Notes:* Figure 8 shows how Ivy-Plus admissions rates vary across high schools, focusing on high schools with at least 40 Ivy-Plus applicants across the years of our sample. We first estimate high school fixed effects on Ivy-Plus admissions using a linear probability model omitting recruited athletes, legacy applicants, and faculty children. The 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 each high school. We estimate a jackknife fixed effect for each student $i$ that excludes his/her own observation from the high school fixed effect estimate. Figure 8a plots the mean high-school admissions fixed effect (adding back the mean admissions rate) for four mutually exclusive sets of high schools. 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. We classify high schools that fall in the top 20% of this index of advantage as "advantaged." Figure 8b plots the mean high school fixed effect on admissions by parental income bin. We give examples of high schools in each of the four groups from the New York City metro area for illustrative purposes; see Appendix Table 14 for analogous examples from other metro areas. Figure 8c is a binned scatterplot showing the share of applicants given high academic or non-academic ratings (as defined in Figure 7) by ventile of high school fixed effect on admission; to adjust for attenuation bias, in this panel, we shrink each high school fixed effect estimate towards zero by multiplying it by its reliability. 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 9: Multiple-Rater Test for Idiosyncratic Variation in Admissions Decisions

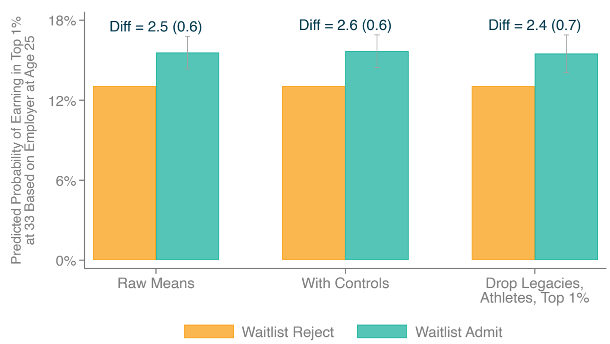



*Notes:* Figure 9 tests whether admissions decisions are driven by idiosyncratic variation by examining the relationship between admissions decisions at a given Ivy-Plus college and other Ivy-Plus colleges. Each block of four dots plots admissions rates at a lower-ranked Ivy-Plus college (based on revealed preference) by the admissions outcome at another higher-ranked Ivy-Plus college (admitted directly, admitted off the waitlist, rejected off the waitlist, and rejected without being waitlisted). The first block includes no additional controls. The second block of four dots repeats the first block but controls for a quintic function of test scores and includes 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 are 95% confidence intervals. All estimates are based on individuals who applied to at least two Ivy-Plus colleges in our college-specific sample.

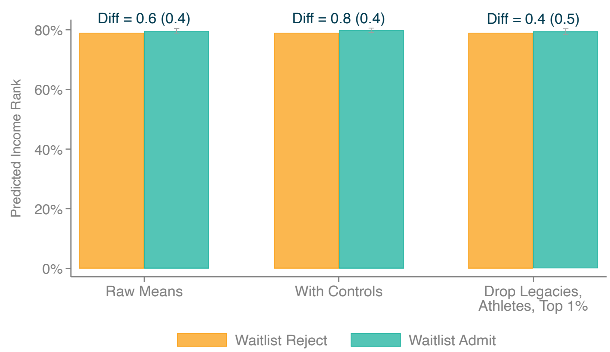
Figure 10: Treatment Effects of Ivy-Plus Admissions on Income for Waitlisted Applicants

(a) Earnings in Top 1% at Age 33


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



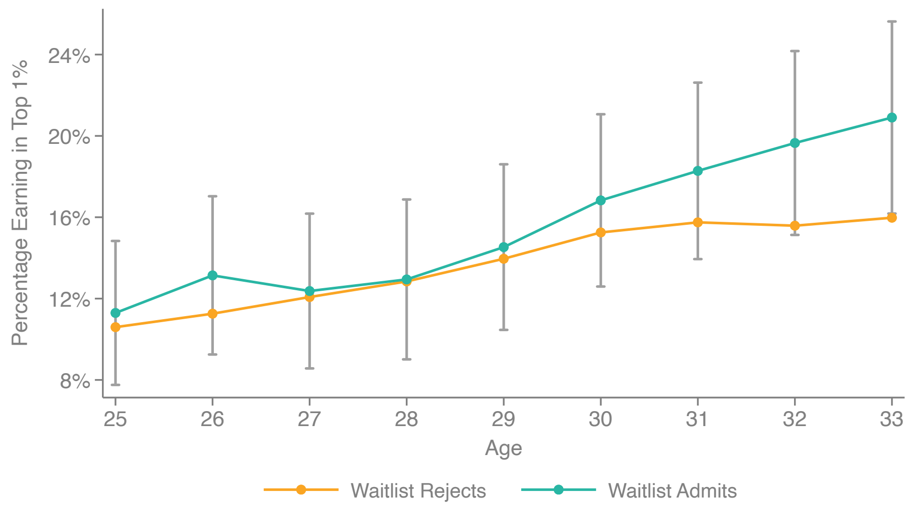

(c) Predicted Mean Income Rank Based on Firm at Age 25



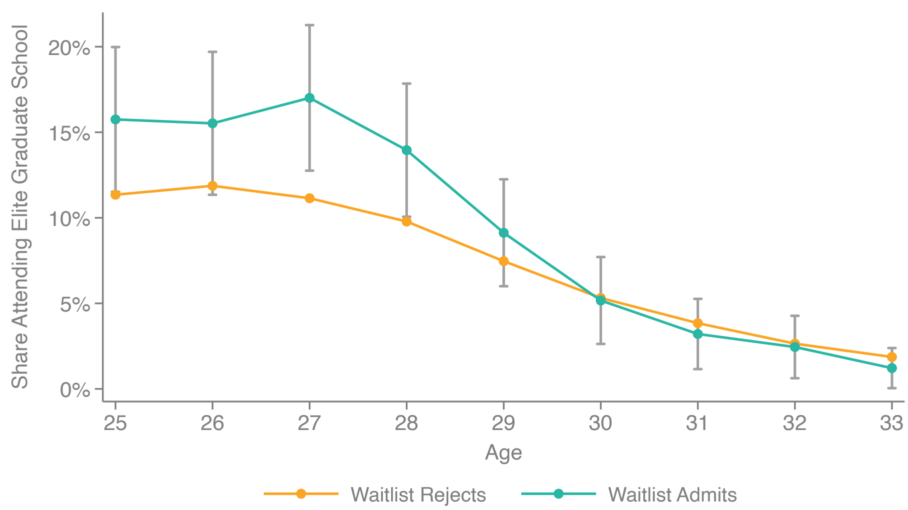

*Notes:* Figure 10a shows the treatment effect of Ivy-Plus admission on the probability of reaching the top 1% of the income distribution at age 33 by plotting outcomes for students admitted vs. rejected from the waitlist at an Ivy-Plus college. Income is individual income, defined as household income from the 1040 return minus spousal wage and self-employment earnings (if married). 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-on-the-treated (TOT) effect. We estimate the TOT of attending an Ivy-Plus college in the sample of waitlisted applicants by regressing an indicator for reaching the top 1% on matriculation, instrumenting for matriculation using an indicator for admission, with varying control vectors. In the first pair of bars, we estimate the TOT using only fixed effects for the college at which the student was waitlisted as controls. The second pair of bars further includes controls for a quintic in test scores, indicators for parent income bins, gender, race, state, recruited athlete, and legacy status. The third pair of bars replicates second pair of bars, except excluding legacies, athletes, and applicants with parental income in the the top 1%. Figure 10b replicates Figure 10a using the predicted top 1% share based on the firm at which the individual works at age 25 (see Section 2.5 for details); Figure uses mean predicted income rank based on firm at age 25 instead. Standard errors are clustered by individual, with whiskers denoting 95% confidence intervals for the TOT estimates. All estimates are based on the set of Ivy-Plus colleges in our college-specific analysis sample.

Figure 11: Treatment Effects of Ivy-Plus Admissions, by Age

(a) Share in Top 1%



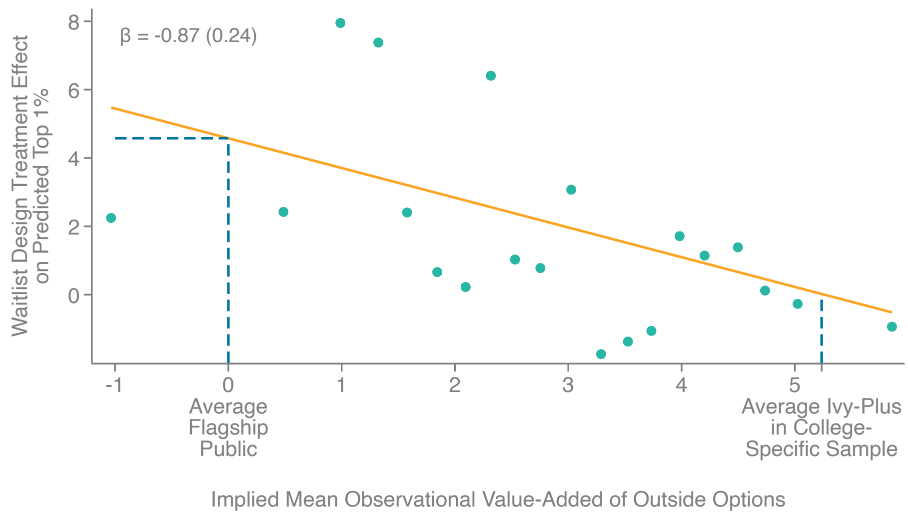

(b) Elite Graduate School Attendance



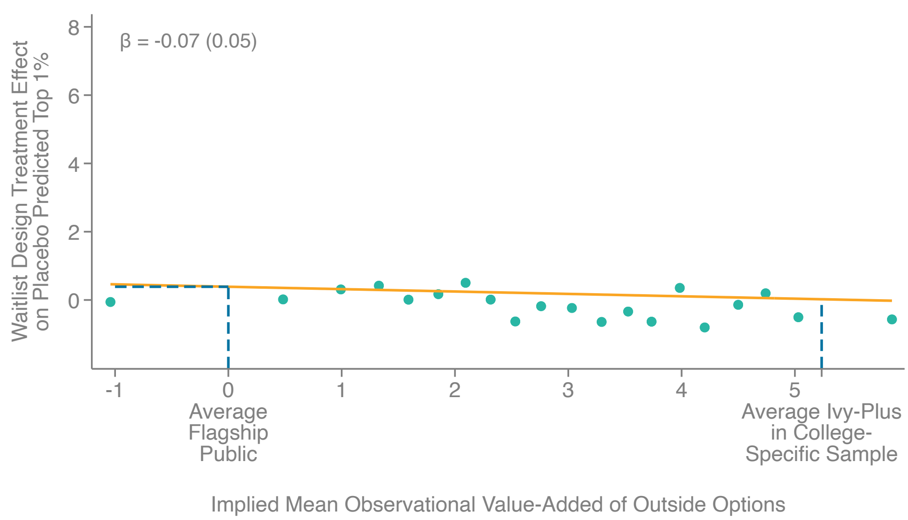

*Notes*: Figure 11a shows the estimated treatment-on-the-treated effect of Ivy-Plus admission for waitlisted applicants on the probability of having individual income in the top 1% of the age-specific income distribution at various ages, from 25 to 33. The orange line plots the average outcome for waitlist rejects; green bars plot the orange bar plus the estimated treatment effect at each age. The treatment effect is calculated using separate two-stage-least-squares regressions for each age using the same specification as in the leftmost pair of bars in Figure 10a; the estimates at age 33 in this figure replicate the estimate from the left pair of bars in Figure 10a. figure 11b replicates Figure 11a using an indicator variable for attending an elite graduate school at each age as the outcome variable. All estimates are based on the set of Ivy-Plus colleges in our college-specific analysis sample and are based on a balanced panel of individuals observed until age 33.

Figure 12: Heterogeneity in Treatment Effects by Strength of Outside Options

(a) Predicted Earnings in Top 1%



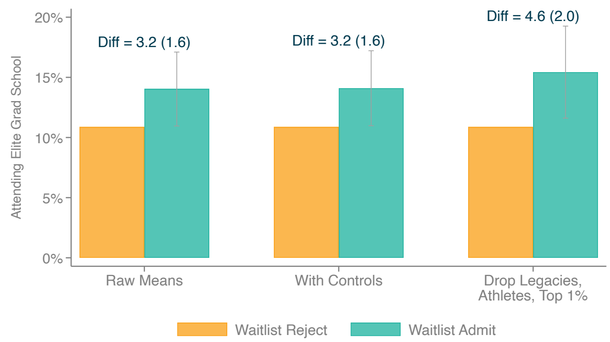

(b) Placebo Predicted Earnings in Top 1%



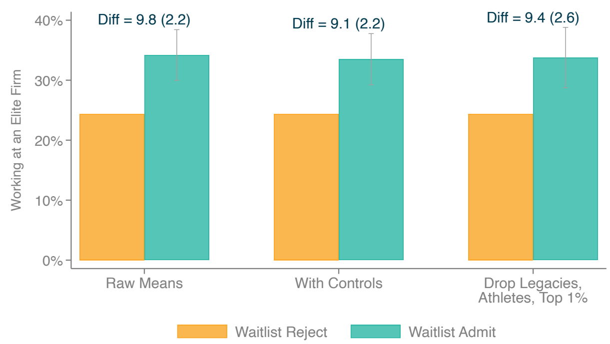

*Notes:* Figure 12 shows how the treatment effect of Ivy-Plus admission from the waitlist varies with the strength of an applicant's outside options. We place Ivy-Plus applicants into subgroups $j$ based on their home state, parent income, race, and the Ivy-Plus college to which they applied. Within each group $j$, we measure the strength of students' outside options based on the average observational value-added of the colleges that students who are rejected from the waitlist attend (see Appendix G for details). We then divide students into 20 bins based on this strength of outside options variable. The x coordinate of each of the 20 points is the mean implied observational VA of outside options within each bin. To construct the y coordinates, we regress the predicted top 1% outcome on indicators for Ivy-Plus admission interacted with the 20 outside option strength dummies and indicators for the Ivy-Plus college to which they applied, using the sample of waitlisted Ivy-Plus applicants as in Figure 10b. We then divide these coefficients by the "first stage" effect of the strength of outside options on actual college VA (see Appendix G). We also report the 2SLS regression slope (and the implied best fit line) corresponding to the plotted points, estimated using the specification in Column 1 of Appendix Table 8. Figure 12b replicates Figure 12a using placebo predicted top 1% outcomes based on a set of predetermined application characteristics (listed in Appendix G) as the y variable. All estimates are based on the set of Ivy-Plus colleges in our college-specific analysis sample.

Figure 13: Treatment Effects of Ivy-Plus Admission on Non-Monetary Outcomes

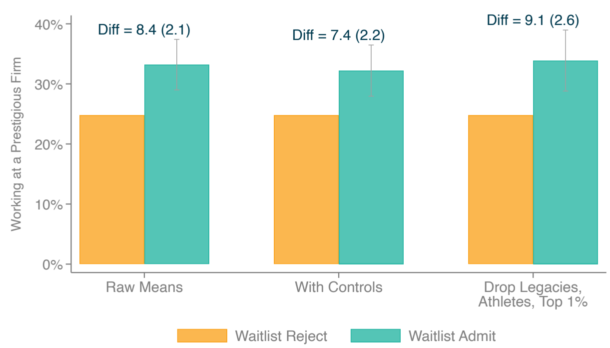
(a) Elite Graduate School Attendance




(b) Employment at an Elite Firm



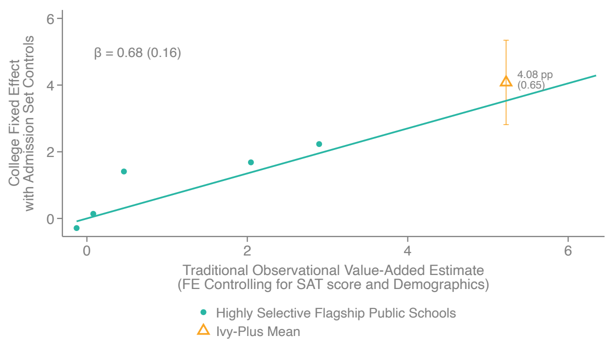

(c) Employment at a Prestigious Firm



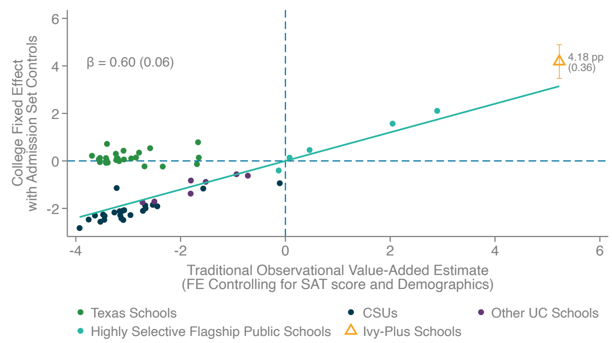

*Notes:* Figures 13 replicates Figure 10 using non-monetary outcomes: attending an elite graduate school at age 28 (Figure 13a), working at an elite firm at age 25 (Figure 13b), and working at a prestigious firm (Figure 13c). Elite graduate schools are defined as Ivy-Plus institutions, as well as UC-Berkeley, UCLA, UCSF, University of Michigan, and University of Virginia. Elite firms are defined as firms that employ the highest share of Ivy-Plus graduates relative to graduates of highly selective publish flagship colleges (leaving out the individual's own college). Prestigious firms are identified based on the same ratio, controlling for the share of individuals with income in the top 1% at that firm. See Section 2.5 for more details on the definitions of these variables.

Figure 14: Causal Effects of Ivy-Plus Attendance: Matriculation Design

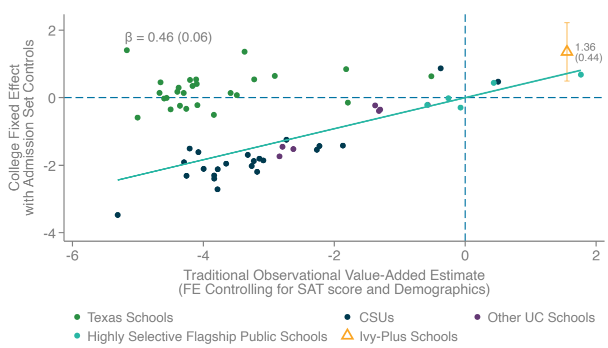
(a) Causal Effects vs. Observational VA for Predicted Top 1%, Highly Selective Colleges



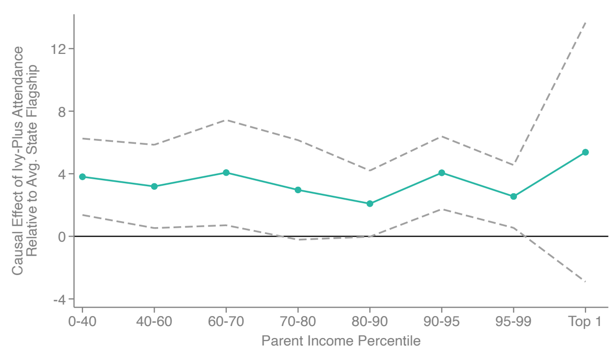

(b) Causal Effects vs. Observational VA for Predicted Top 1%, by College Group




(c) Causal Effects vs. Observational VA for Predicted Mean Income Rank, by College Group

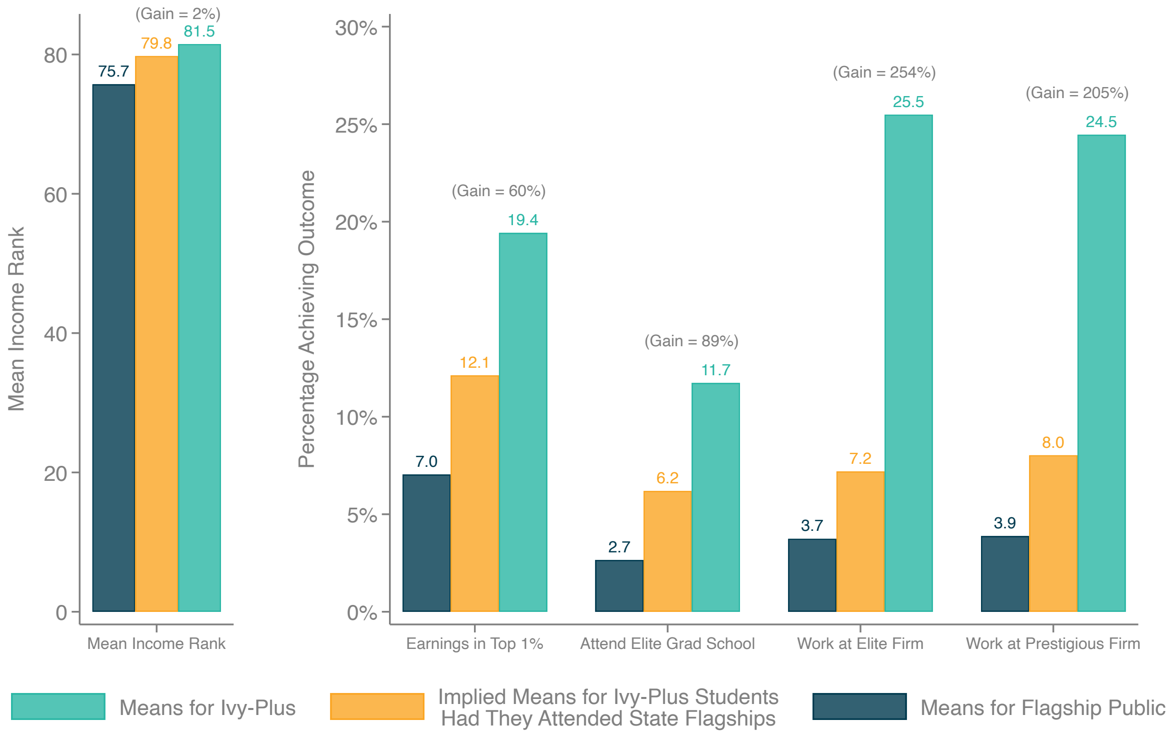



(d) Heterogeneity in Causal Effects of Ivy-Plus Attendance by Parent Income



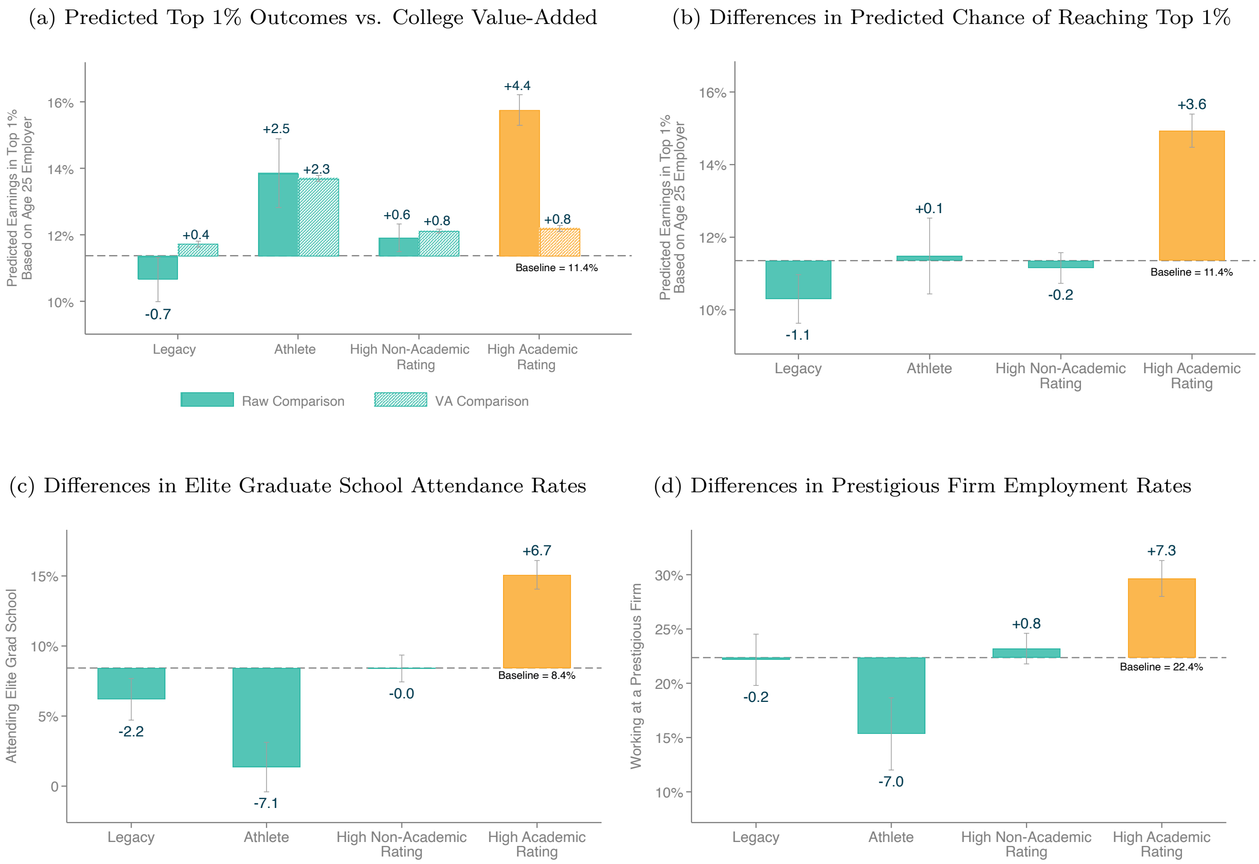

*Notes:* This figure presents estimates of colleges' causal effects based on variation in where students choose to attend conditional on the set of colleges to which they were admitted. The first three panels are scatter plots of colleges' causal effects vs. observational value-added estimates. Causal effects are estimated using 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. Observational value-added estimates are based on OLS regressions of outcomes on fixed effects for the college students attend, controlling for parental income, SAT scores, race, gender, birth cohort, and home state, estimated using our pipeline analysis sample. 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 mean of the estimates for the Ivy-Plus colleges in our college-specific sample in a single point (denoted by a triangle), along with the point estimate and standard error for that causal effect. We also plot a best-fit line based on a regression on the plotted points, as well as the slope and standard error for that line. Figure 14a presents estimates 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 14b replicates Figure 14a, 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. We use data from Texas for 1999-2008 school years to align with the sample used by Mountjoy and Hickman (2021). Figure 14c replicates Figure 14b, but using predicted mean income rank based on firm at age 25 as the outcome variable. Figure 14d reports the causal effect of attending Ivy-Plus colleges (relative to the highly selective public flagship schools) as in Figure 14b, but separately for students from each of eight parent income bins; the dashed lines present 95% confidence intervals. All estimates in this figure are based on our college-specific analysis sample.

Figure 15: Differences in Outcomes at Ivy-Plus vs. Highly Selective State Flagship Colleges: Causal Effects vs. Selection




*Notes:* Figure 15 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 across colleges. In each triplet of bars, the first bar plots mean observed outcomes for the nine highly selective flagship public schools listed in Appendix Table 1. The last of the three bars plots mean observed outcomes for the Ivy-Plus colleges listed in Appendix Table 1. The middle bar shows implied means for Ivy-Plus students had they attended state flagships by subtracting treatment effect estimates from the mean observed outcomes for Ivy-Plus attendees. 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 on the relevant outcome (as estimated in Figure 10 or Figure 13) by the ratio of the difference in mean observational value-added between the Ivy-Plus and nine flagship public schools and the waitlist TOT effect on value-added of college attended (for the relevant variable). For mean income ranks and earnings in top 1% at 33, we estimate the treatment effect by rescaling the difference in the observational VA estimates at age 33 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. 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 Figure 10 and Figure 13 for more detail on the variables, sample, and waitlist-based estimates of the TOT effects.

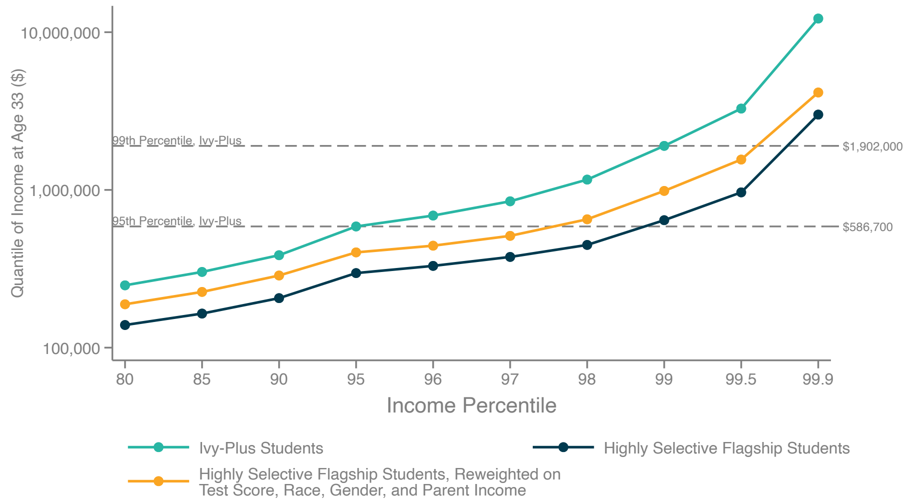
Figure 16: Post-College Outcomes by Application Credentials Among Ivy-Plus Applicants



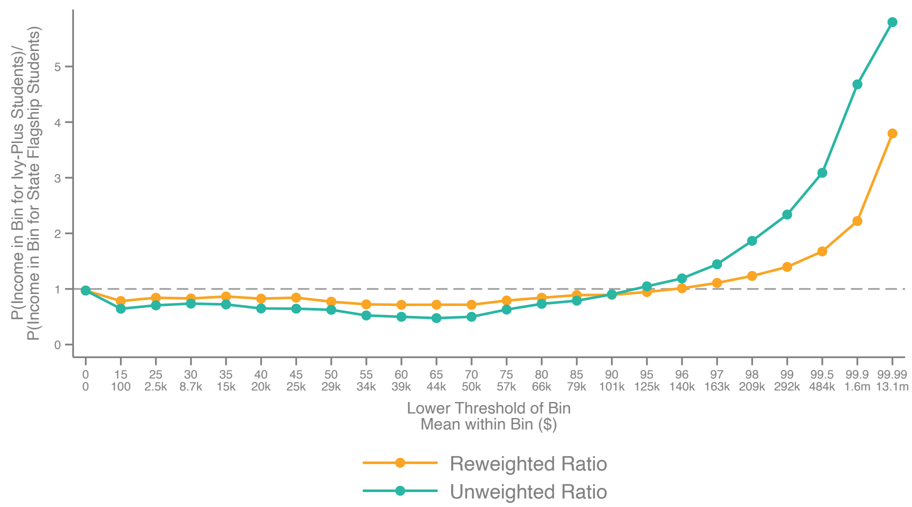

*Notes:* In Figure 16a, the bars on the left in each pair report estimates from regressing the predicted probability of reaching the top 1% based on age 25 employer on four explanatory variables: indicators for whether a student is a legacy, is a recruited athlete, has a high non-academic rating, and 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 in each pair, we replace the dependent variable with the observational value-added of college attended 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 7. Figure 16b plots the difference between the Raw Outcome Comparison and VA Comparison in Figure 16a for the four explanatory variables plotted in Figure 16a. These estimates show the difference in outcomes for applicants by their credentials, netting out differences in the value-added of the college they attend. See Appendix Figure 28 for an illustration of the levels underlying Panels A and B for those with low vs high non-academic ratings. Figure 16c and Figure 16d replicate Figure 16b 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 17: Income Distributions of Ivy-Plus vs. Public Flagship Students at Age 33

(a) Quantiles of Income Distribution at Age 33 for Ivy-Plus vs. Highly Selective Public Flagship Students



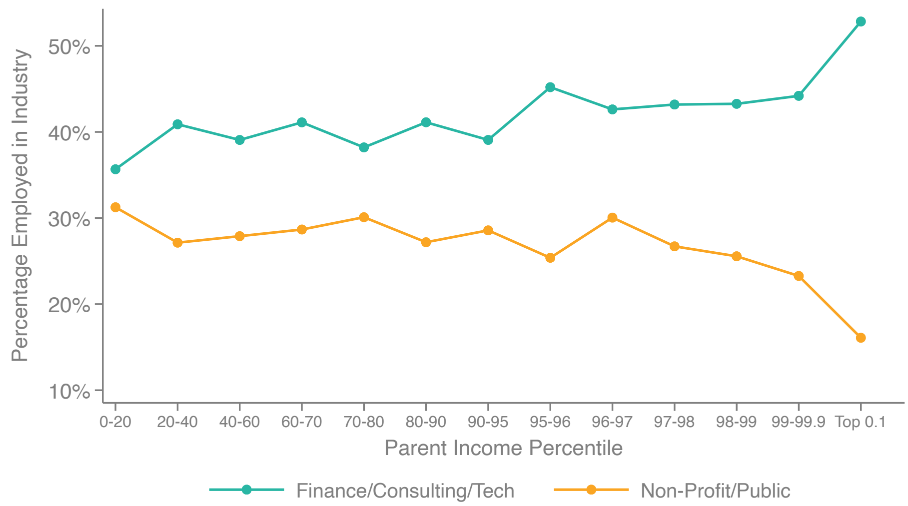

(b) Ratio of Density of Income Distribution at Age 33 for Ivy-Plus vs. Highly Selective Flagship Students



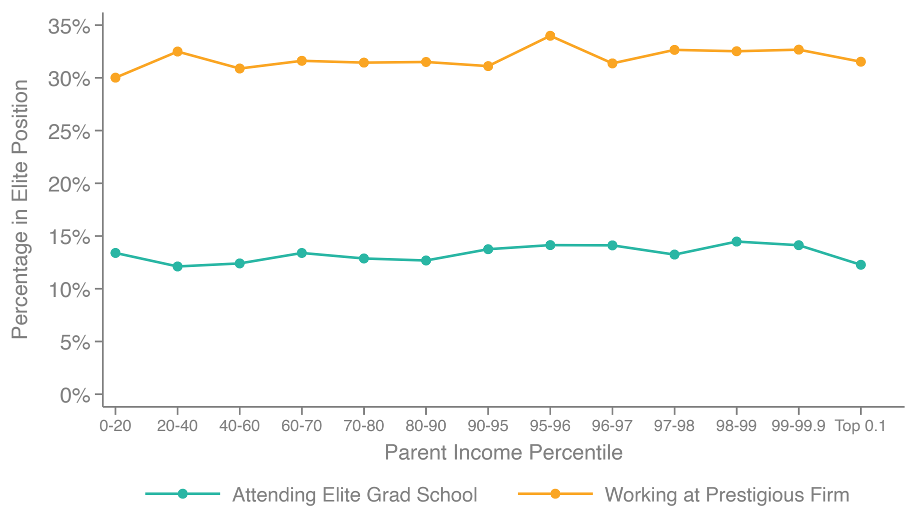

Notes: Figure 17 compares the distribution of total pre-tax individual income (as defined in Section 2.5) at age 33 for students who attended an Ivy-Plus vs. one of the nine highly selective flagship public colleges listed in Appendix Table 1. Figure 17a plots quantiles of the distribution of earnings for Ivy-Plus students (green), public flagship students (dark blue), and public flagship students reweighted to match Ivy-Plus students on test score, race, gender, and parent income bin (orange), following the method in Figure 2. Figure 17b plots the ratio of the share of Ivy-Plus students in each bin of the national income distribution shown on the x axis to the corresponding share for students who attended flagship public colleges. We label the bottom percentile threshold for each bin on the x axis. For example, the point labeled 15 on the x axis plots the previously described ratio for individuals with earnings between the 15th and 25th percentiles of the national income distribution. Note that individuals with earnings below the 15th percentile all have zero earnings. The green line plots the raw ratio; the orange line plots the same ratio after reweighting flagship public students to match Ivy-Plus students on test score, race, gender, and parent income bin. The values listed below the x axis labels in this figure show the mean income within each bin. The sample consists of all students in our long-term outcomes sample for whom we observe college attendance, SAT/ACT scores, and income at age 33.

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

(a) Industry of Employment



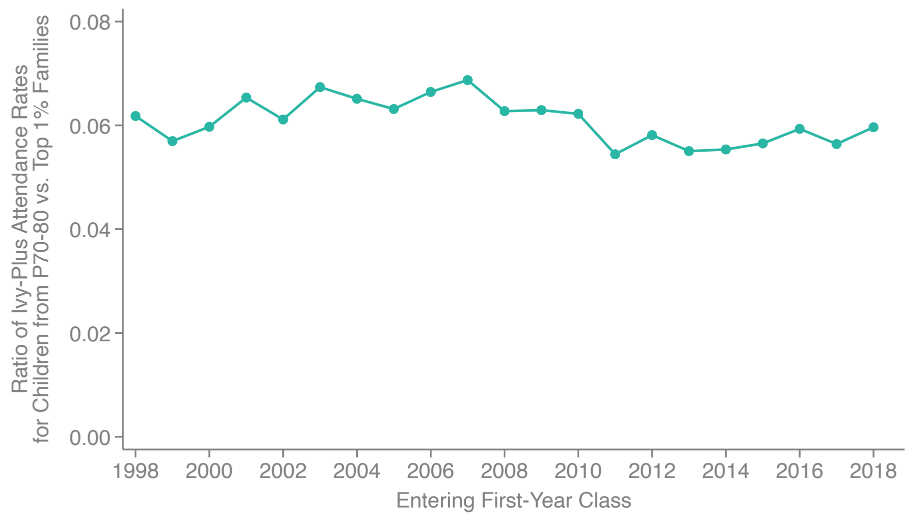

(b) Elite Graduate School Attendance and Prestigious Firm Employment




*Notes*: Figure 18a plots the share of Ivy-Plus matriculants working in Finance/Consulting/Tech or Non-Profit/Public industries at age 25 vs. parental income, controlling for a quintic in test scores. Industries are identified based on the employer from which individuals received the largest payment (based on W-2 forms) at age 25. 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. Figure 18b plots the share of matriculants who attend an elite graduate school or work at a prestigious firm at age 25 vs. parental income, controlling for a quintic in test scores. Elite graduate schools are defined as Ivy-Plus institutions, as well as UC-Berkeley, UCLA, UCSF, University of Michigan, and University of Virginia. Elite firms are defined as firms that employ the highest share of Ivy-Plus graduates relative to graduates of highly selective publish flagship colleges (leaving out the individual's own college); prestigious firms are identified based on the same ratio, controlling for the share of individuals at the firm with income in the top 1%. See Section 2.5 for more details on the definitions of these variables.

Figure 19: Trends in Ivy-Plus Attendance Rates for Children from Middle Class vs. Top 1% Families

(a) Ratio of P70-80 to Top 1% Ivy-Plus Attendance Rates, 1998-2018

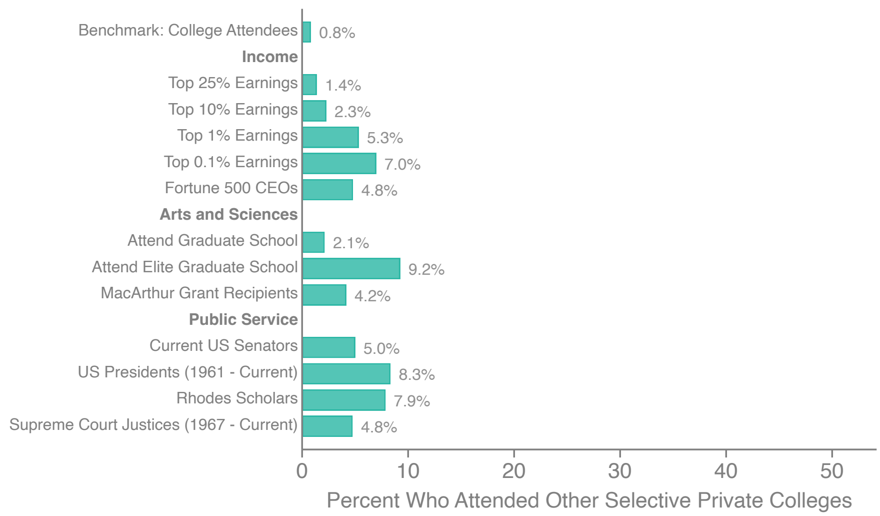



(b) P70-80 / Top 1% Ivy-Plus Attendance Rates Controlling for Test Scores, 2001-2015

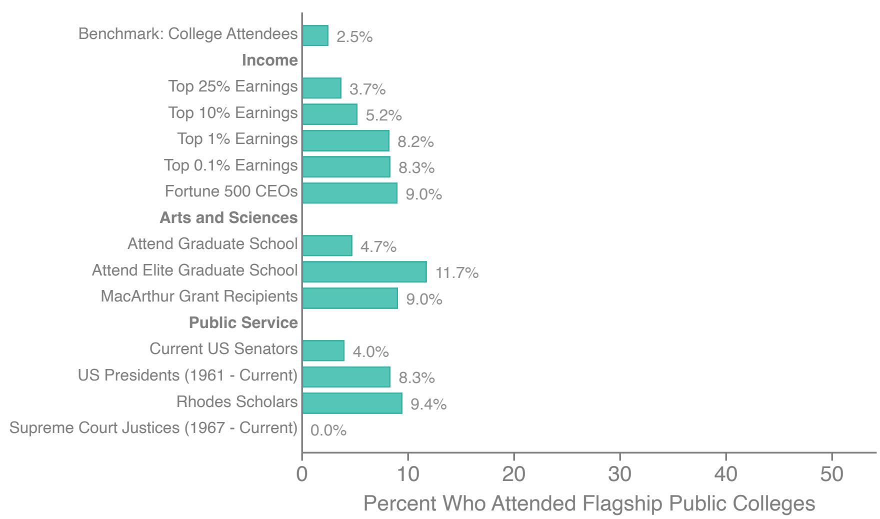

*Notes:* Figure 19 plots the ratio of the share of students with parental incomes between the 70th and 80th percentile who attend Ivy-Plus colleges to the share of students with parental incomes in the top 1% who attend Ivy-Plus colleges by year for students who turn 18 in the years from 1998-2018. Figure 19b plots the same ratio reweighting on test scores to match the distribution of test scores attending each Ivy-Plus college in each year, as in Figure 2b.

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

(a) Other Highly Selective Private Colleges



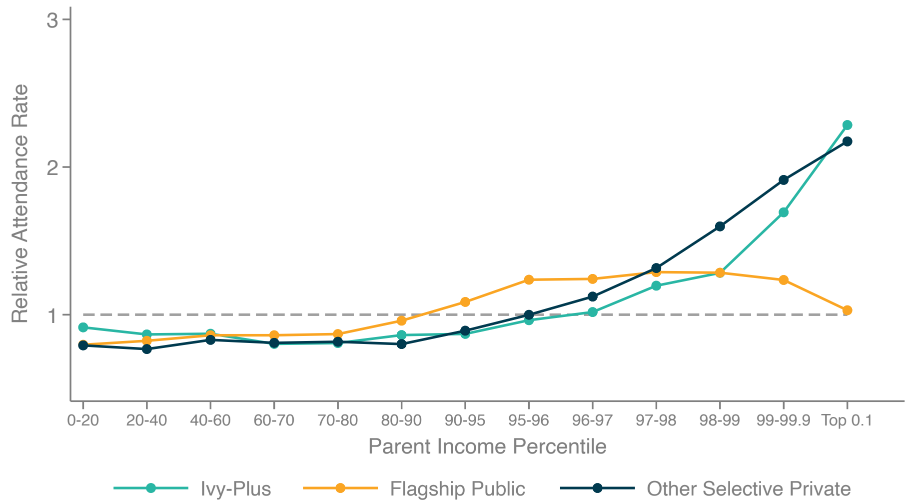

(b) Highly Selective Flagship Public Colleges



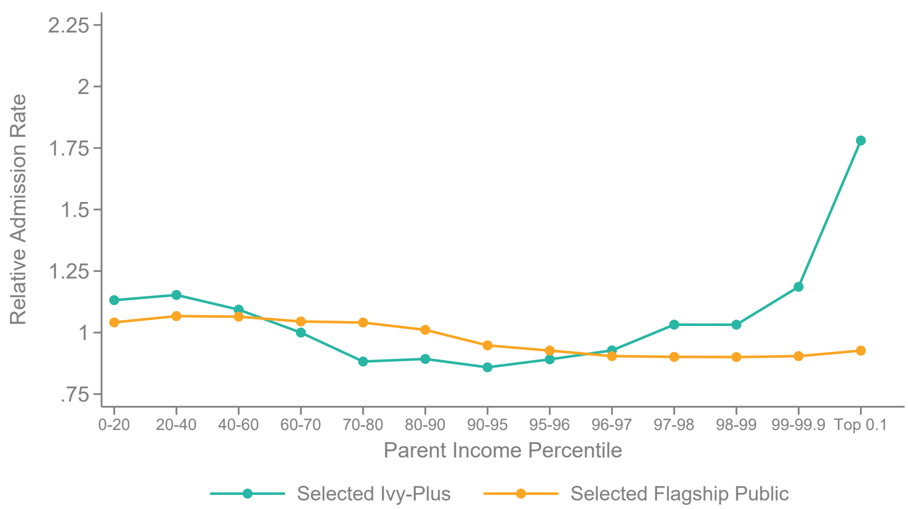

*Notes:* This figure replicates Figure 1, but showing the share who attended one of the other most selective private colleges (in Panel A) or one of the nine most selective flagship public colleges (in Panel B) listed in Appendix Table 1. See notes to Figure 6for further details and Appendix A for the definitions and sources for each outcome variable.

Appendix Figure 2: Attendance and Admission Rates by Parental Income, Controlling for Test Scores and Race

(a) Attendance Rates



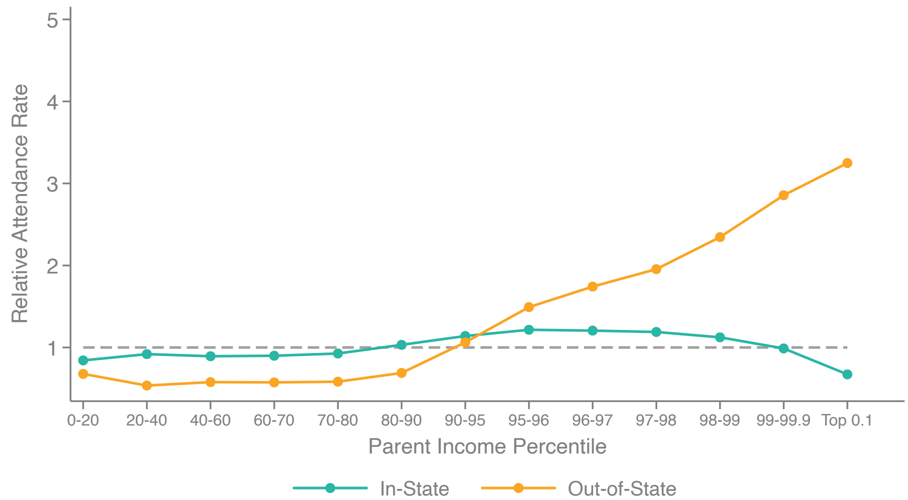

(b) Admission Rates



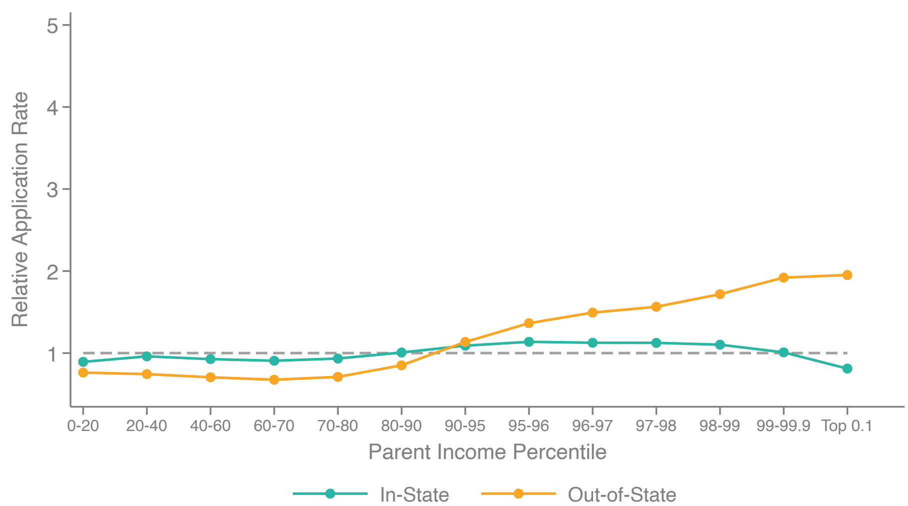

*Notes:* Appendix Figure 2a replicates Figure 2b, but reweighting so that the joint distribution of race/ethnicity (categorized as described in Section 2.4) and test scores within each parent income bin matches the distribution for attending students overall. Appendix Figure 2b replicates Figure 4a, similarly reweighting on both test scores and race.

Appendix Figure 3: Highly Selective Public Flagship College Attendance and Application Rates by Parental Income and In-State Status, Controlling for Test Scores

(a) Attendance Rates



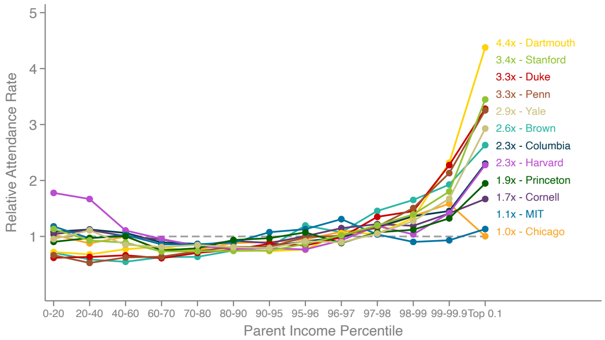

(b) Application Rates



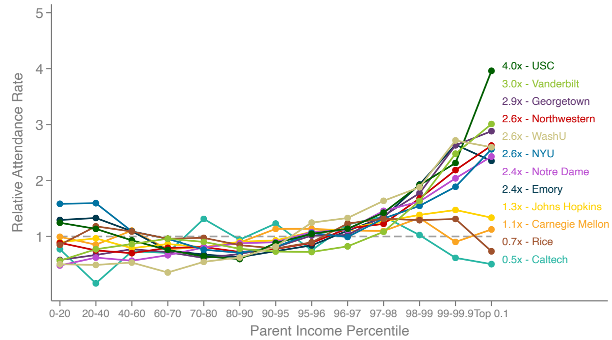

*Notes:* Appendix Figure 3a replicates the estimates from Figure 2b for the nine highly selective public flagship universities listed in Appendix Table 1, splitting students into those living in state vs. out of state. We measure in-state status using the students' state of residence when they take a standardized test. Appendix Figure 3b replicates Appendix Figure 3a replacing attendance rates with application rates. Application rates are predicted using score sending data as described in Appendix B.

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

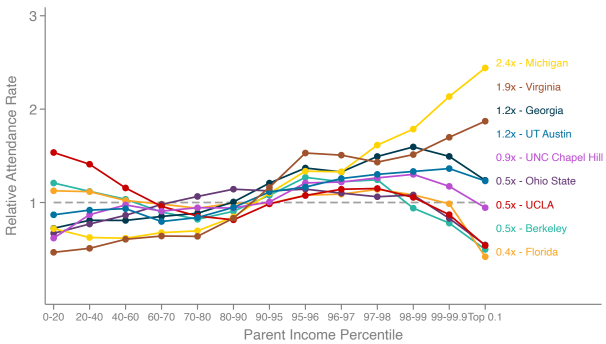
(a) Ivy-Plus Colleges



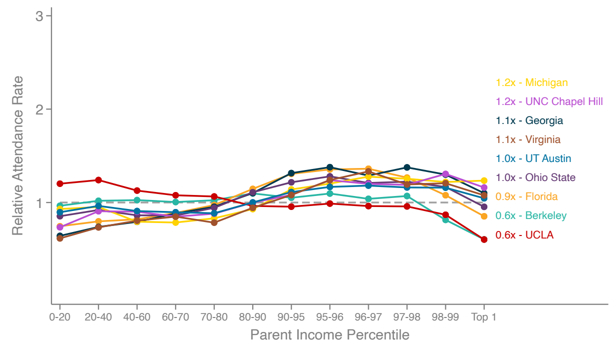

(b) Other Selective Private Colleges



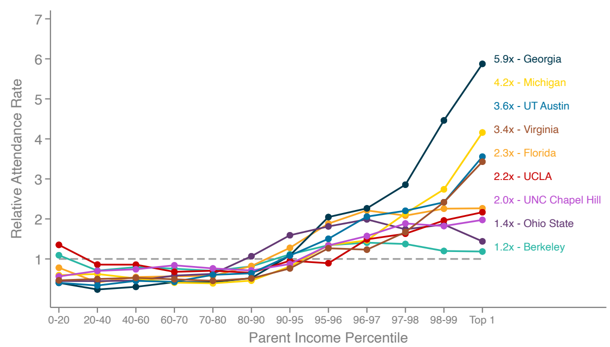

(c) Selective Public Flagships




(d) Selective Public Flagships: In-State



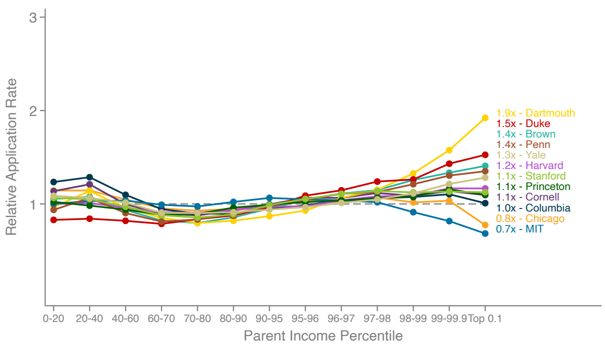

(e) Selective Public Flagships: Out-of-State



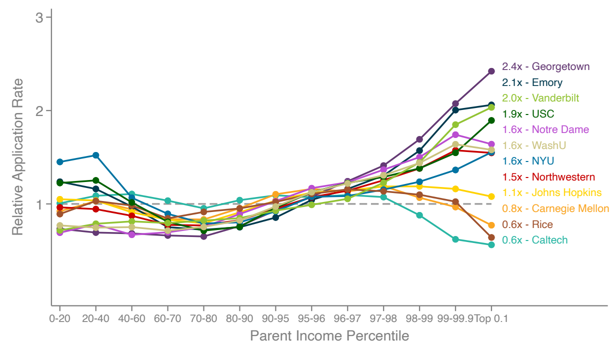

*Notes:* This figure replicates the estimates in Figure 2b and Appendix Figure 3a, but separately by college for all colleges listed in Appendix Table 1 using our pipeline analysis sample. Appendix Figure 4a plots relative attendance rates for each Ivy-Plus college; Appendix Figure 4b plots relative attendance rates for each other highly selective private college; Appendix Figure 4c plots relative attendance rates for of the highly selective public flagship universities pooling in-state and out-of-state students, while Appendix Figures 4d and 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 $\theta$ and $\varepsilon$ 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 $\varepsilon = 1$. See notes to Figure 2 and Appendix Figure 3 for details on variable definitions and methods.

Appendix Figure 5: Application Rates by Parental Income and College, Controlling for Test Score

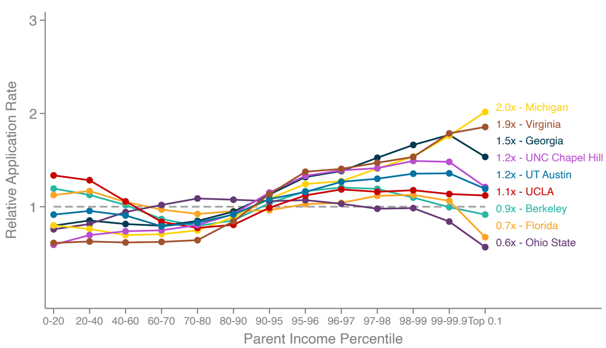
(a) Ivy-Plus Colleges



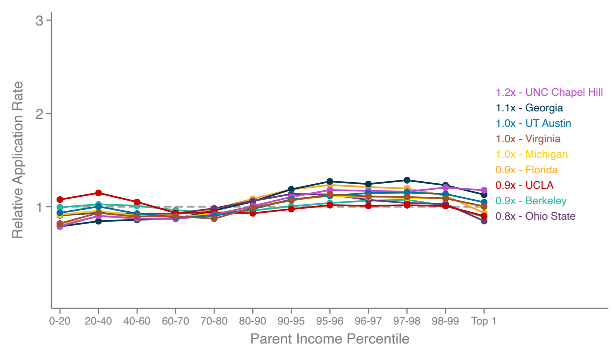

(b) Other Selective Private Colleges



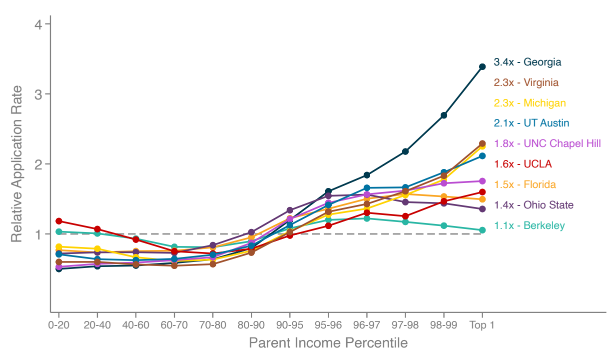

(c) Selective Public Flagships




(d) Selective Public Flagships: In-State



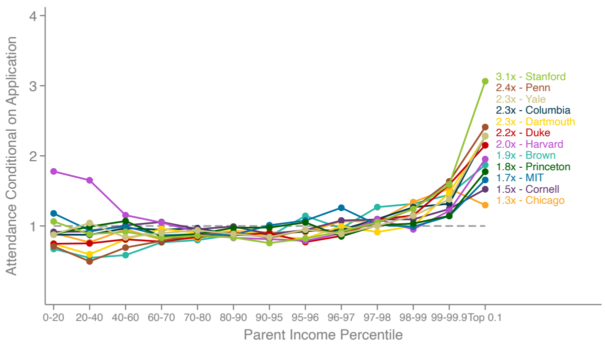

(e) Selective Public Flagships: Out-of-State



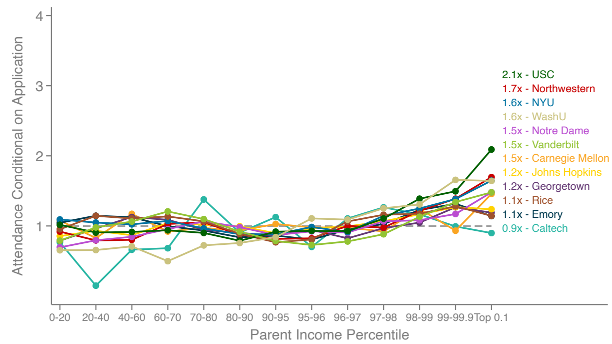

*Notes:* This figure replicates Appendix Figure 4, replacing attendance rates with application rates. Application rates are predicted using score sending data as described in Appendix B.

Appendix Figure 6: Attendance Rates Conditional on Application by Parental Income and College, Controlling for Test Score

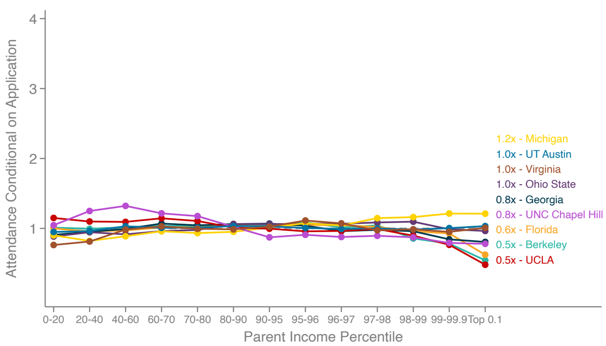
(a) Ivy-Plus Colleges



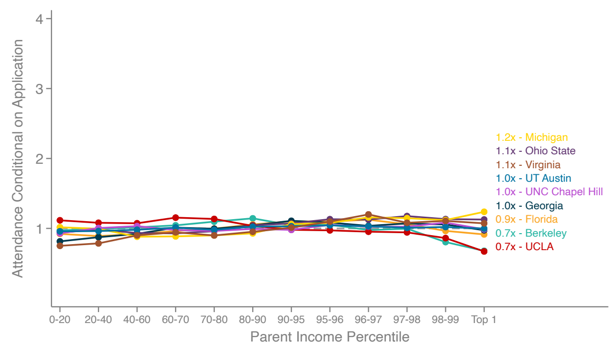

(b) Other Selective Private Colleges



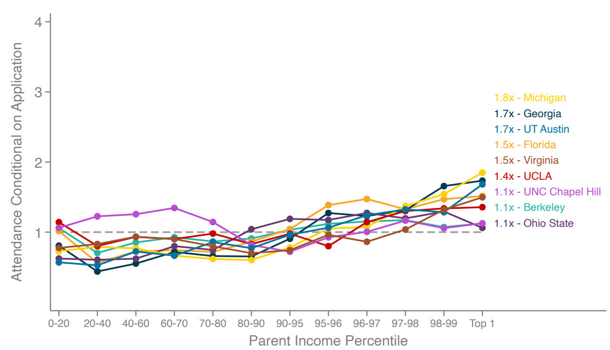

(c) Selective Public Flagships




(d) Selective Public Flagships: In-State



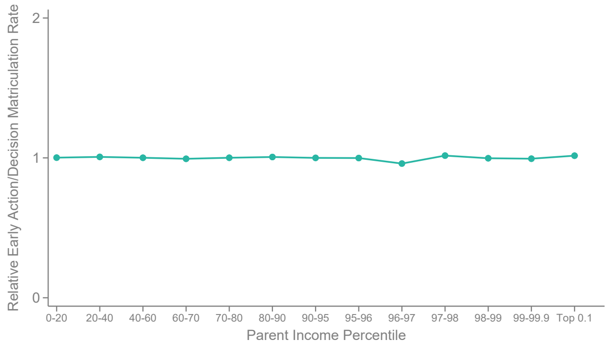

(e) Selective Public Flagships: Out-of-State



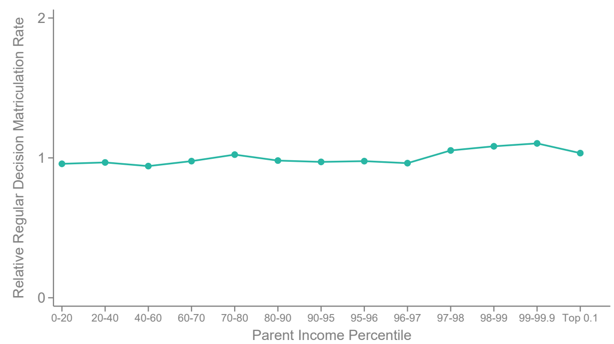

*Notes*: This figure replicates Appendix Figure 4, replacing attendance rates with the ratio of attendance rates to application rates. Application rates are predicted using score sending data as described in Appendix B.

Appendix Figure 7: Matriculation Rates at Ivy-Plus Colleges by Parental Income and Early vs. Regular Round Application, Controlling for Test Scores

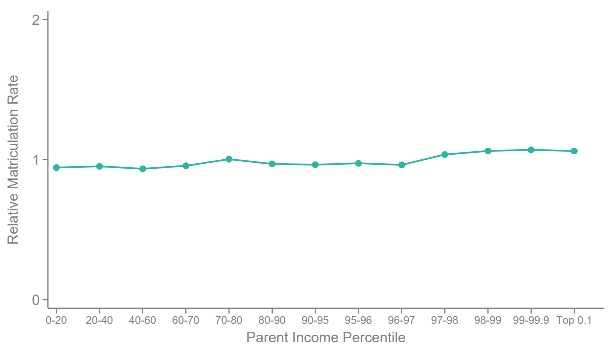
(a) Matriculation Rates: Early Action/Decision Applicants



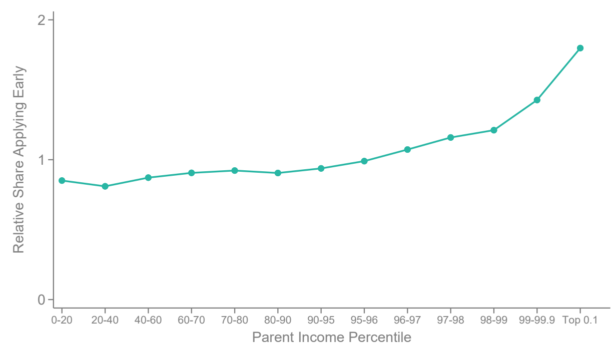

(b) Matriculation Rates: Regular Decision Applicants




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

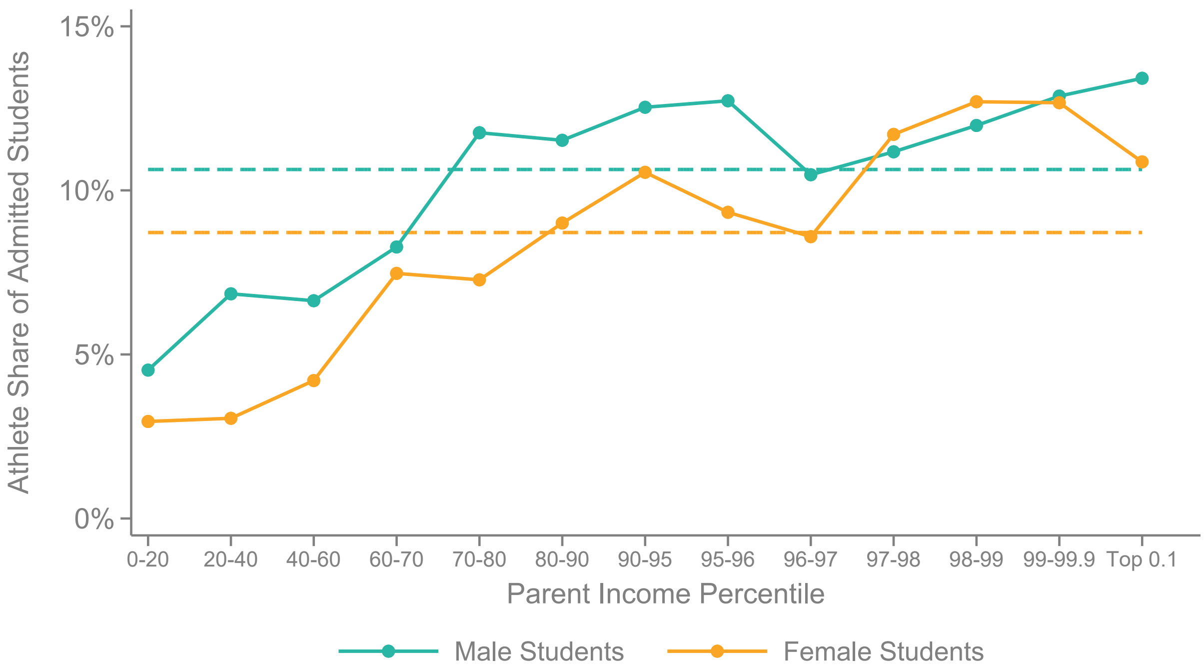



(d) Share Applying Early




*Notes*: Appendix Figures 7a and 7b 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. Appendix Figure 7c replicates Figure 4b, reweighting students on both test scores and application round to equalize the share of students in early vs. regular decision rounds across parent income bins. Appendix Figure 7d plots the share of early action/decision applicants by parental income reweighted by test score. Estimates are based on the college-specific sample of Ivy-Plus colleges; see Section 2 for details.

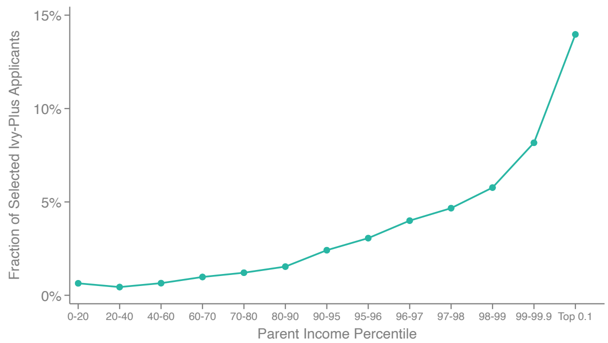
Appendix Figure 8: Share of Ivy-Plus Admitted Students who are Recruited Athletes, by Parental Income and Gender



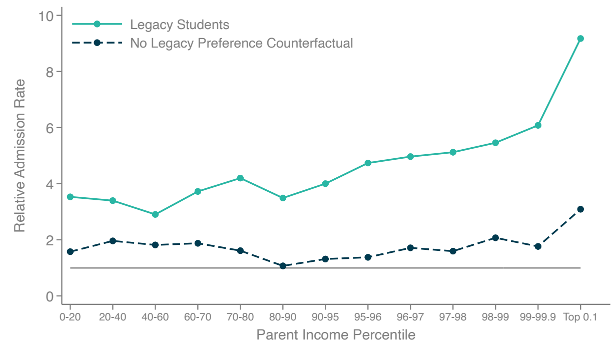

*Notes:* Appendix Figure 8 replicates Figure 5 separately by gender.

Appendix Figure 9: Ivy-Plus Legacy Applicant Shares and Admissions Rates by Parental Income

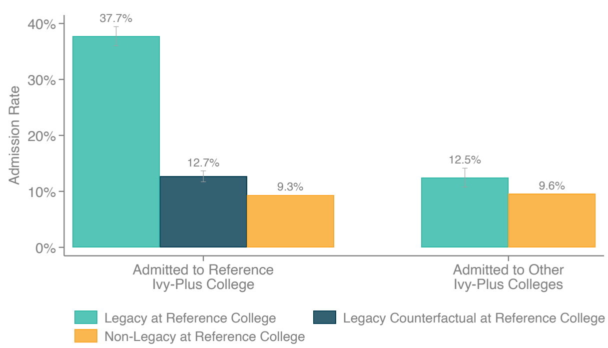
(a) Share of Legacy Applicants by Parental Income



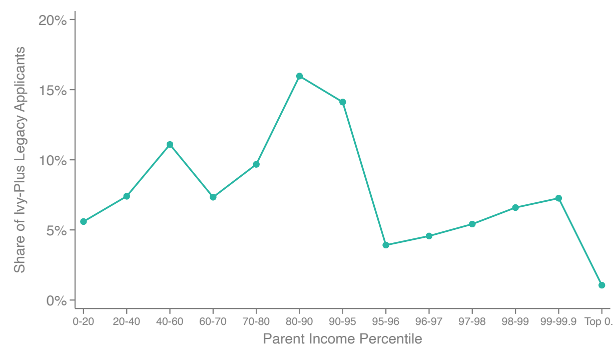

(b) Admissions Rates for Legacy Applicants




(c) Admissions Rates by Legacy Status at Parents' College vs. Other Ivy-Plus Colleges

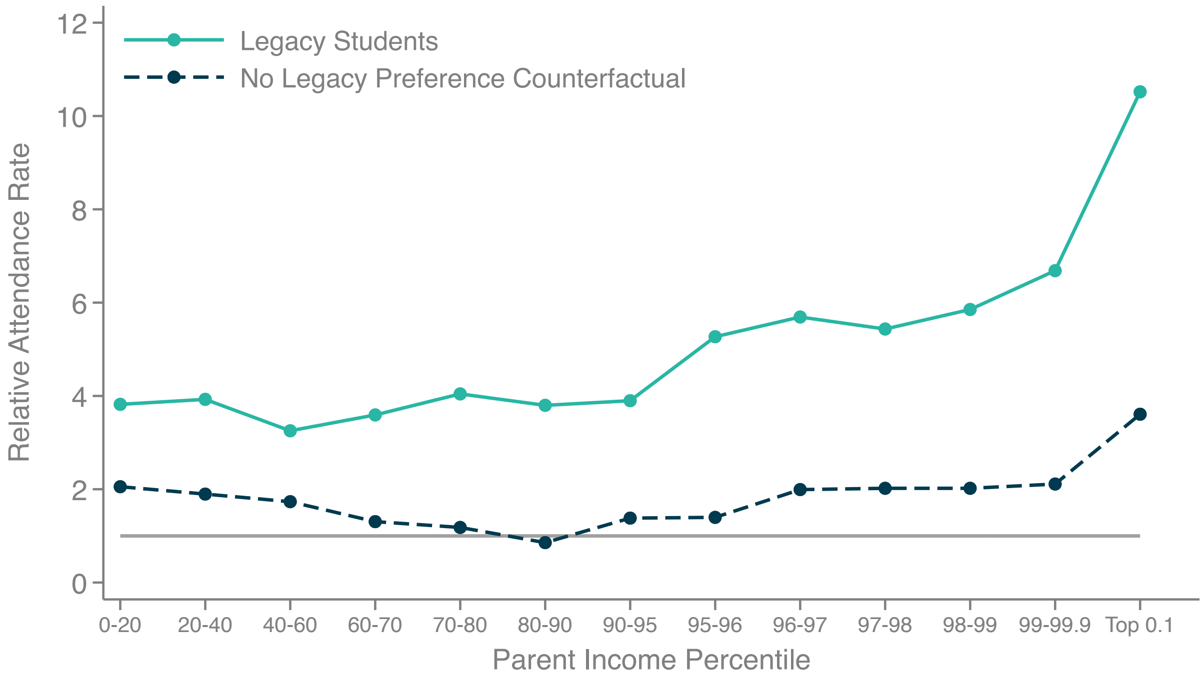



(d) Parental Income Distribution of Ivy-Plus Applicants




*Notes:* Appendix Figures 9a - 9c replicate Figures 6a - 6c without reweighting on test scores. Appendix Figure 9d plots the distribution of parent income among all applicants (without test score reweighting) in the same sample as in Appendix Figures 9a and 9b.

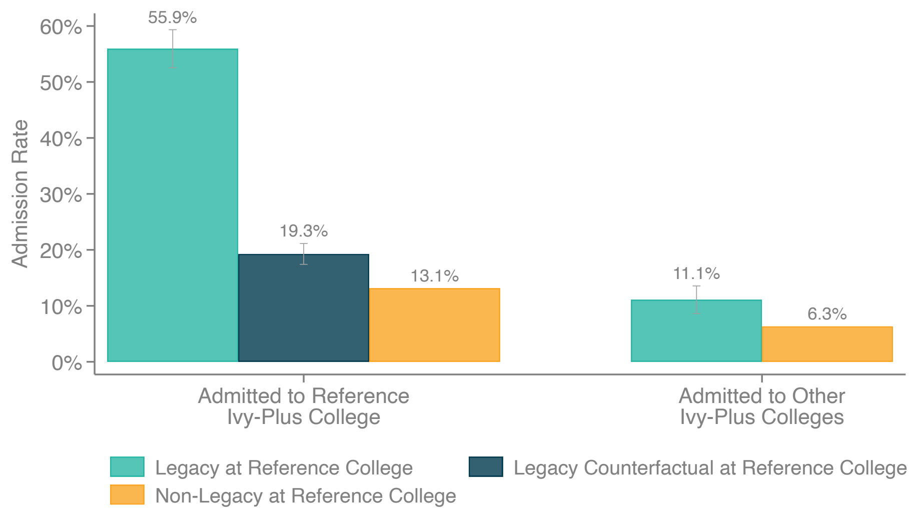
Appendix Figure 10: Actual vs. Counterfactual Attendance Rates for Ivy-Plus Legacy Students, by Parental Income



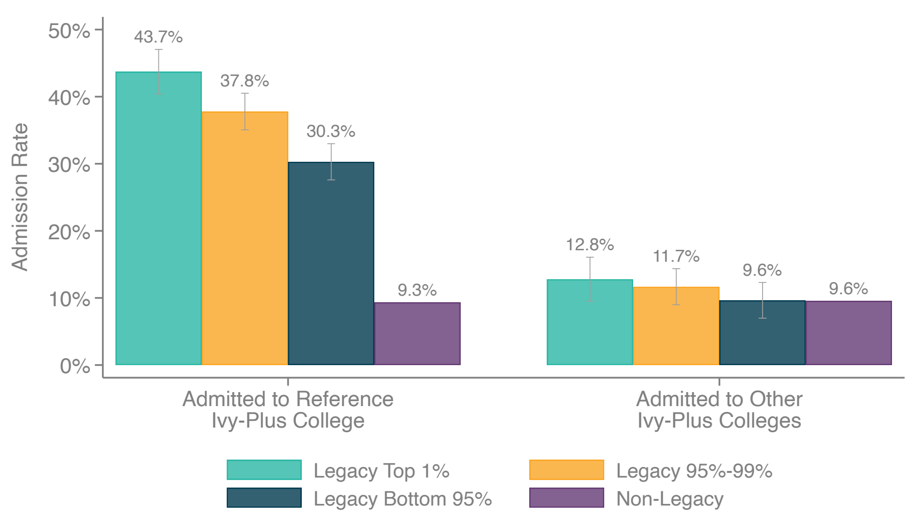

*Notes:* Appendix Figure 10 replicates Figure 6b, except that we plot actual and counterfactual attendance rather than admissions rates.

Appendix Figure 11: Admission Rate by Legacy Status

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



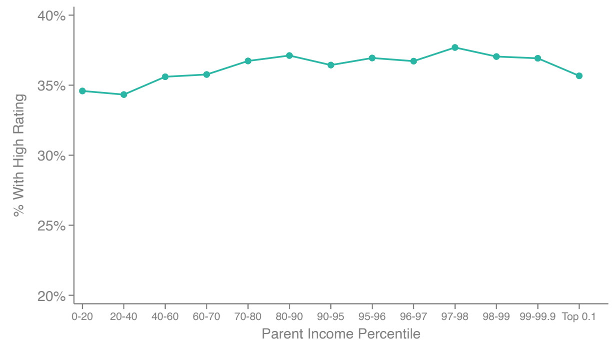

(b) Admission Rate by Legacy Status and Parental Income



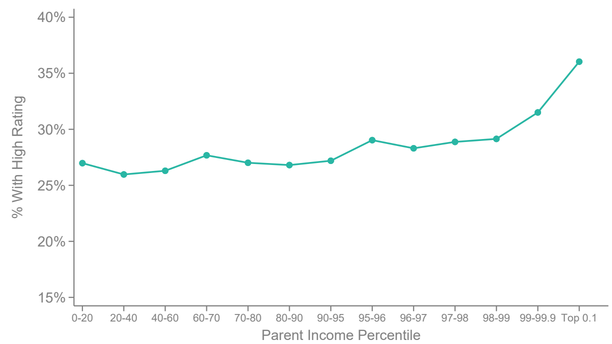

*Notes:* Appendix Figure 11a replicates Figure 6c, except that we limit the sample to reference colleges that are lower-ranked within the relevant pair of colleges (based on revealed preference), to mitigate concerns about strategic admission by other Ivy-Plus colleges. Appendix Figure 11b replicates Figure 6c, but dividing legacy applicants into thee groups based on parental income (top 1%, 95%-99%, and bottom 95%).

Appendix Figure 12: Admissions Office Ratings of Applicants by Parent Income: Sensitivity Analysis

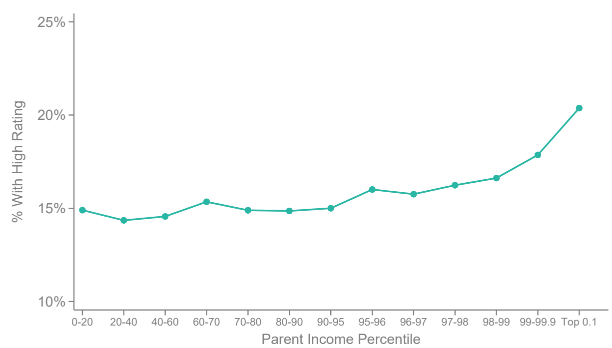
(a) Academic Ratings, Pooling Multiple Ivy-Plus Colleges




(b) Coarse Non-Academic Ratings, Focal Ivy-Plus College

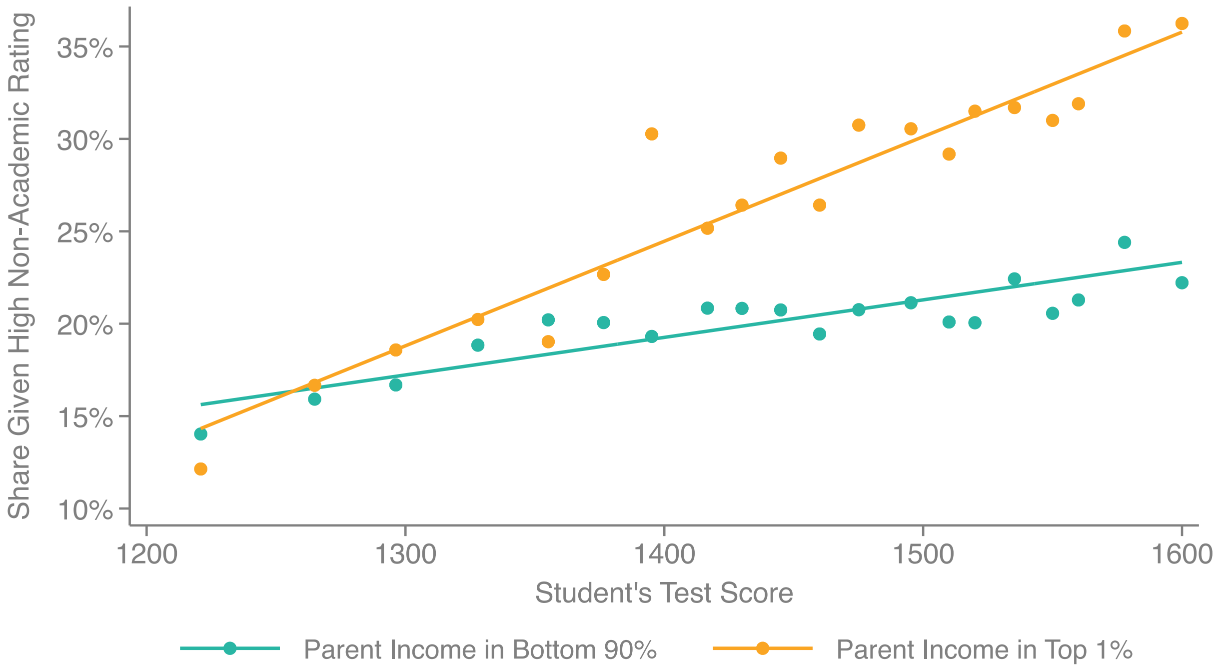



(c) Coarse Non-Academic Ratings, Pooling Multiple Ivy-Plus Colleges




*Notes:* Appendix Figure 12 replicate the results on Figures 7a and 7b using a broader set of Ivy-Plus colleges. Panel A replicates Figure 7a with data from multiple Ivy-Plus colleges. Panel B replicates Figure 7b with data only from the Ivy-Plus college used in Figure 7b, but coarsening the measurement of non-academic rating to match the measurements available in datasets from other Ivy-Plus colleges. Panel C then replicates Panel B using data from all the Ivy-Plus colleges in our college-specific sample for which we have ratings information.

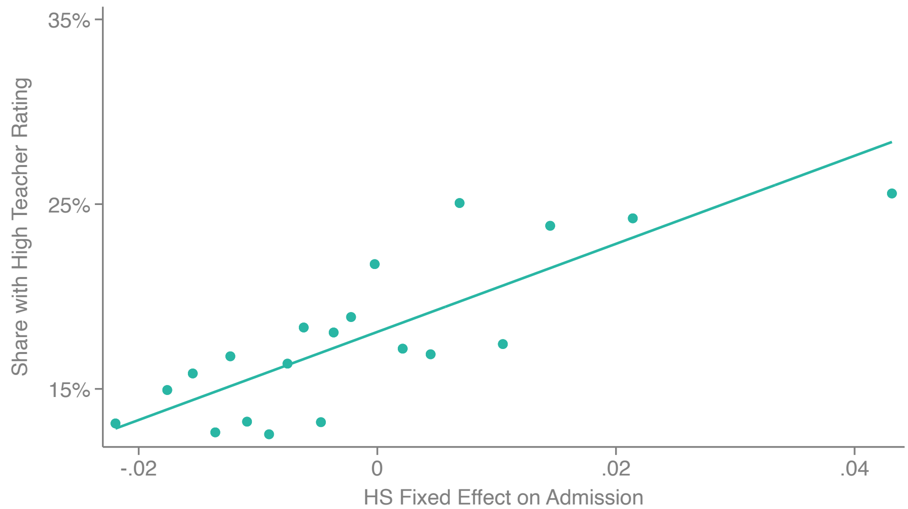
Appendix Figure 13: Non-Academic Ratings vs. Test Scores, by Parental Income



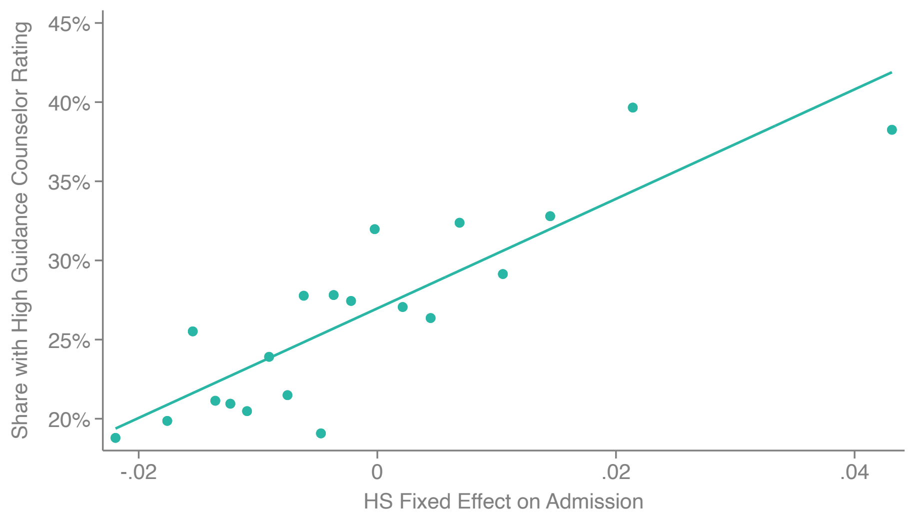

*Notes:* Appendix Figure 13 presents a binned scatter plot of the share of applicants given high non-academic ratings by student test score ventile, separately for students with parents in the bottom 90 percent of the income distribution vs. those with parents in the top 1 percent. Estimates are based on applicants to the Ivy-Plus college for which we have the most granular ratings data. The sample includes all applicants with SAT scores greater than 1200 (or, equivalently, ACT scores greater than 21), excluding recruited athletes, legacy students, and faculty children.

Appendix Figure 14: Teacher and Guidance Counselor Ratings by High School Fixed Effect on Ivy-Plus Admissions

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



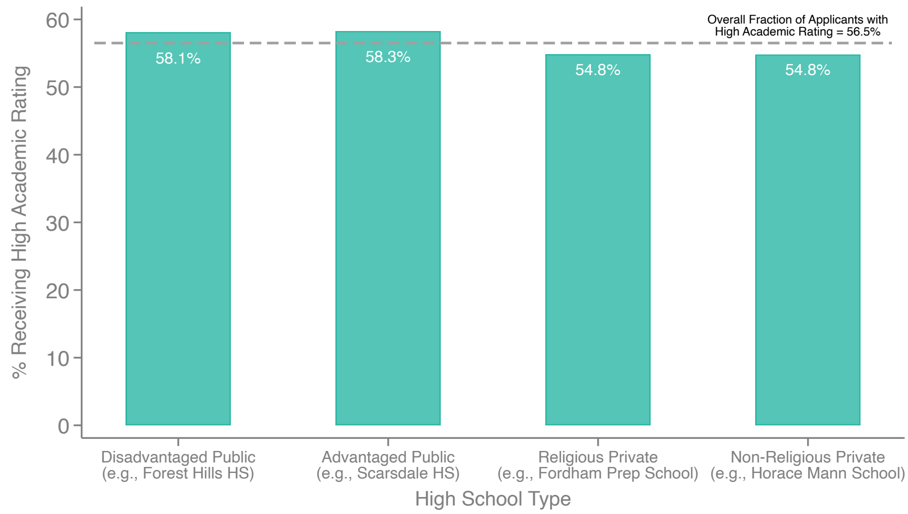

(b) Share with High Guidance Counselor Rating by High School Fixed Effect



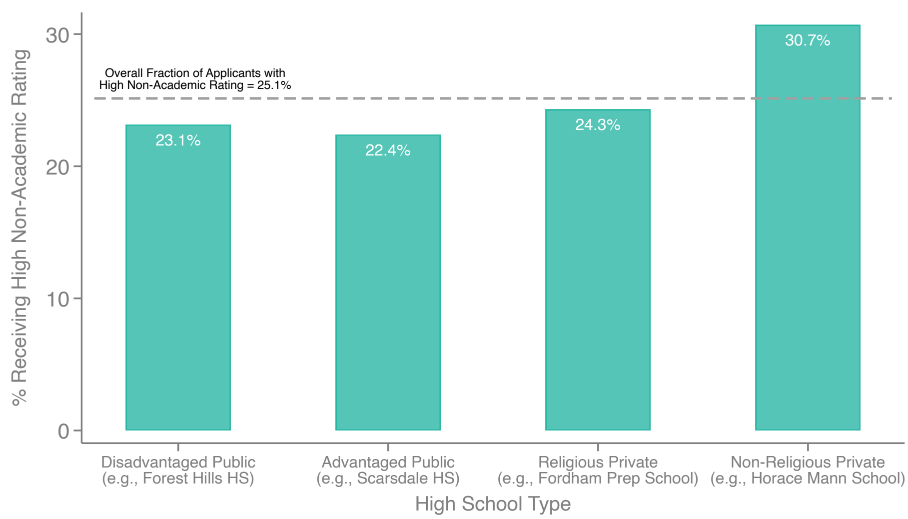

*Notes:* Appendix Figure 14a replicates Figure 8c using the share of students with high ratings of teacher recommendation letters (Panel A) and guidance counselor letters (Panel B) as the outcome variables. See the notes to Figure 8 for a description of how the high school fixed effects are constructed.

Appendix Figure 15: Admissions Office Ratings of Ivy-Plus Applicants by High School Type, Controlling for Test Scores

(a) Academic Ratings by High School Type



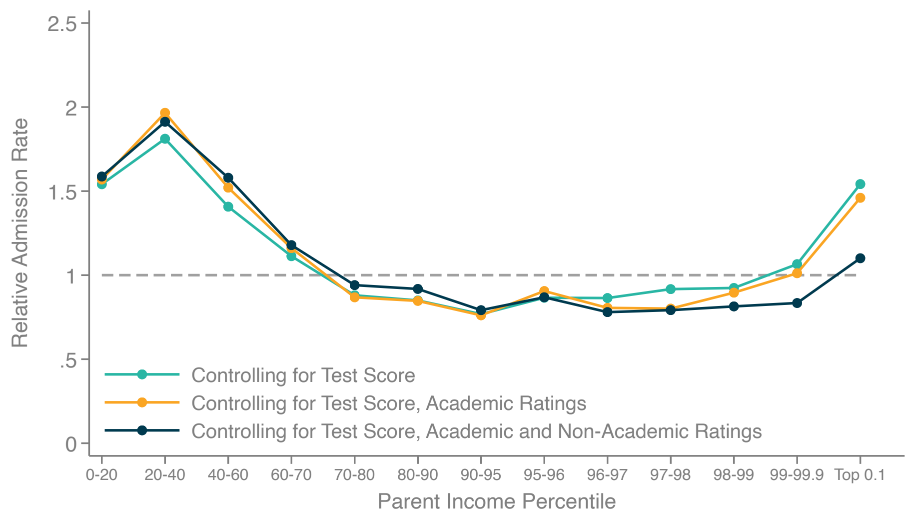

(b) Non-Academic Ratings by High School Type



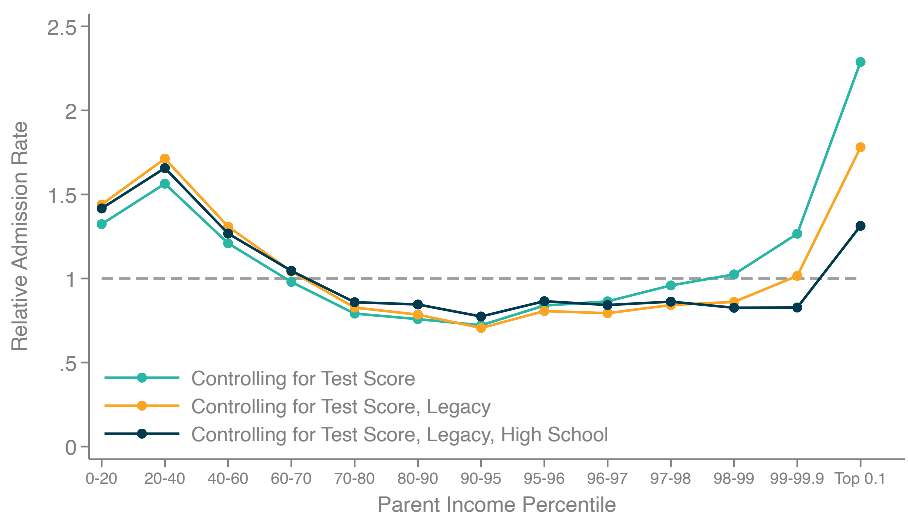

*Notes:* Appendix Figure 15 plots the proportion of non-recruited-athlete applicants who receive high academic (Panel A) and non-academic (Panel B) ratings by high school type, controlling for test scores. We regress an indicator for receiving a high rating on indicators for the four high school types and a quintic in test scores and plot the coefficients on the indicators (normalized so that the weighted average of the four coefficients matches the unconditional mean share of students who receive a high rating).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. We classify high schools that fall in the top 20% of this index of advantage as “advantaged.” Estimates are based on applicants to the Ivy-Plus college for which we have the most granular ratings data.

Appendix Figure 16: Admissions Rate by Parental Income, Controlling for Ratings and High School

(a) Effect of Controlling for Ratings



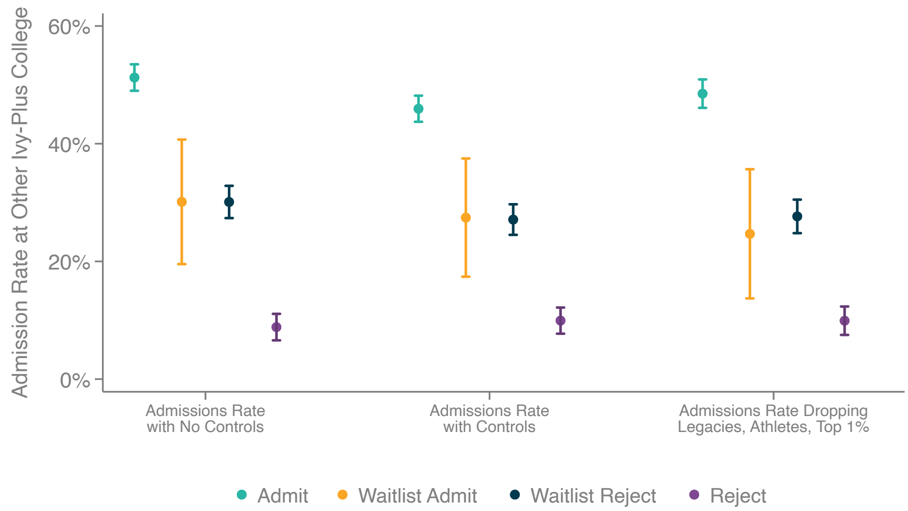

(b) Effect of Controlling for High School



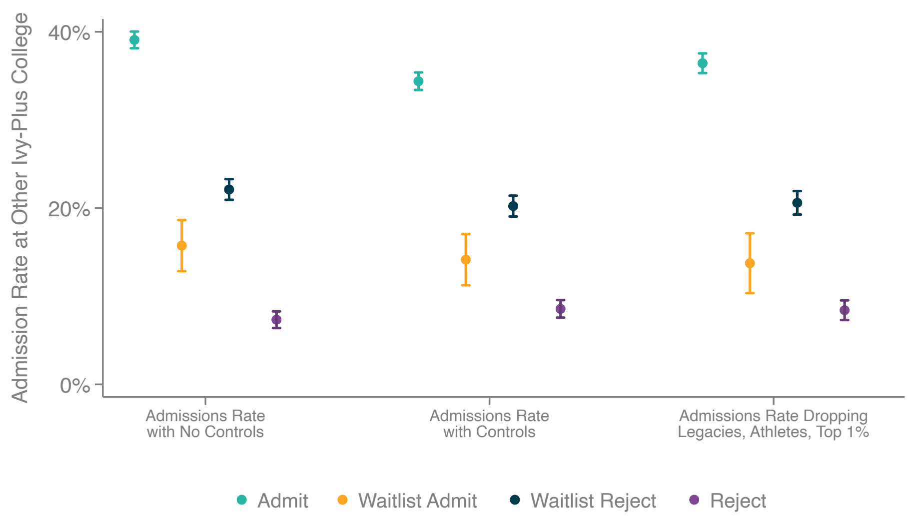

*Notes:* Appendix Figure 16a plots admissions rates for non-legacy, non-recruited-athlete applicants to the Ivy-Plus college for which we have the most granular ratings information with three set of weights. The green line reweights on test score, so that the distribution of test scores within each parent income bin matches that of attending students, as in Figure 2b. 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. Appendix Figure 16b plots admissions rates for non-recruited-athletes at the same Ivy-Plus college by parental income bin reweighting on test scores (green line), adding regression controls for legacy status (orange line), and finally adding high school fixed effects (dark blue line).

Appendix Figure 17: Multiple-Rater Test for Idiosyncratic Variation in Admissions: Sensitivity Analyses

(a) Excluding Private High School Attendees



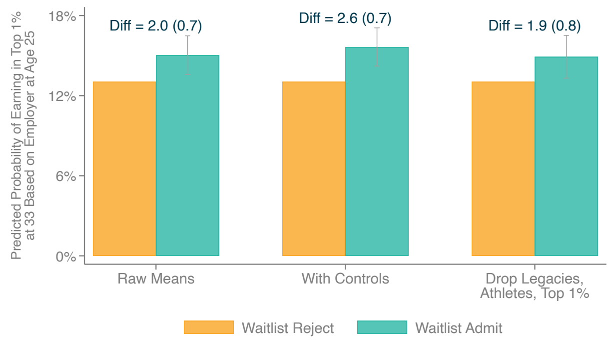

(b) Comparing Admissions Outcomes at all Ivy-Plus Colleges



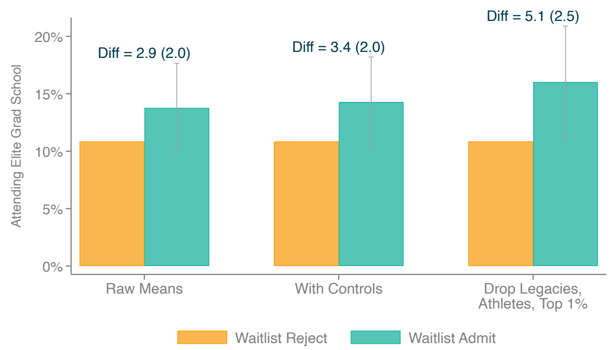

*Notes*: Appendix Figure 17a replicates Figure 9 except excluding students from private high schools. Appendix Figure 17b replicates Figure 9 except that we consider admissions outcomes at all other Ivy-Plus colleges in our college-specific sample, not just outcomes at higher-ranked colleges. See Figure 9 for further details.

Appendix Figure 18: Treatment Effects of Ivy-Plus Admissions on Post-College Outcomes for Waitlisted Applicants in the Higher-Ranked Multiple-Rater Test Subsample

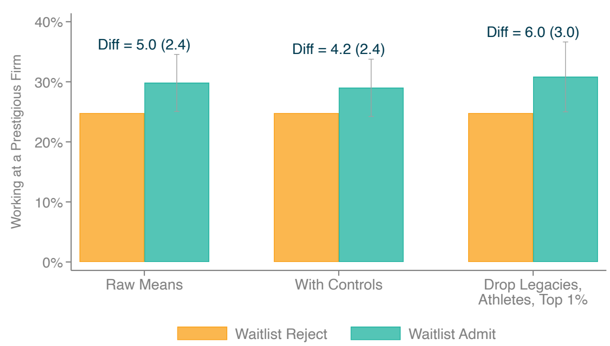
(a) Predicted Earnings in Top 1% Based on Firm at Age 25




(b) Elite Graduate School Attendance



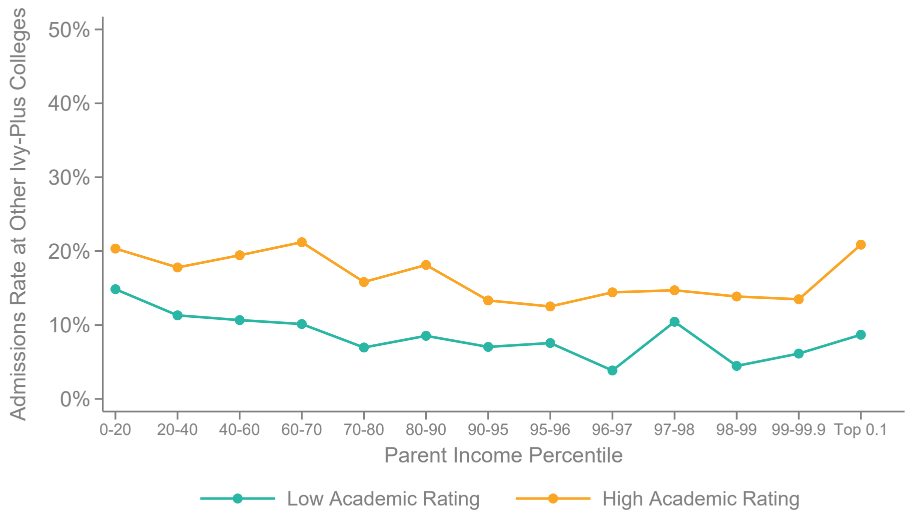

(c) Employment at a Prestigious Firm



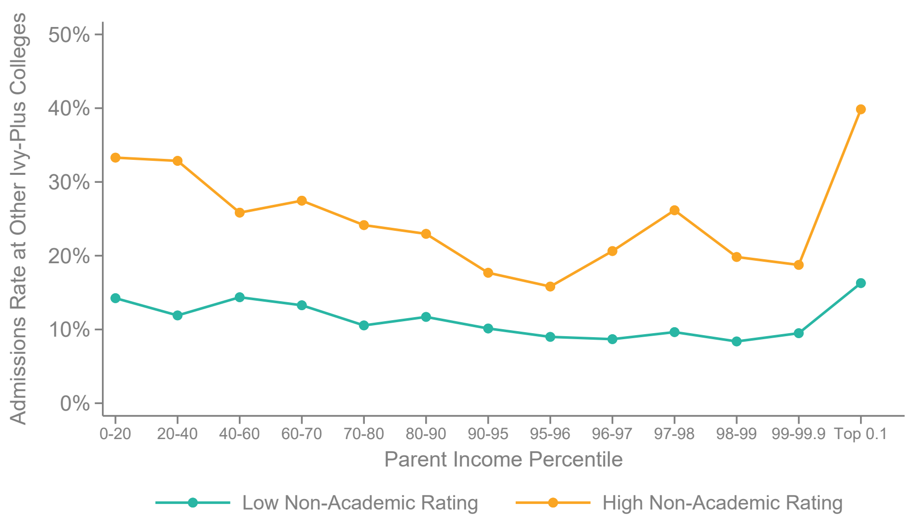

Notes: Appendix Figure 18 replicates Figure 10b, Figure 13a, and Figure 13c for the subset of Ivy-Plus applicants to colleges that pass at least one multiple-rater test with another (lower-ranked) Ivy-Plus college; see Section 4.2 for details on the test and the resulting subset of colleges.

Appendix Figure 19: Admission Rates at Other Ivy-Plus Colleges by Parental Income and Ratings at a Given Ivy-Plus College

(a) Academic Ratings, Controlling for Test Score



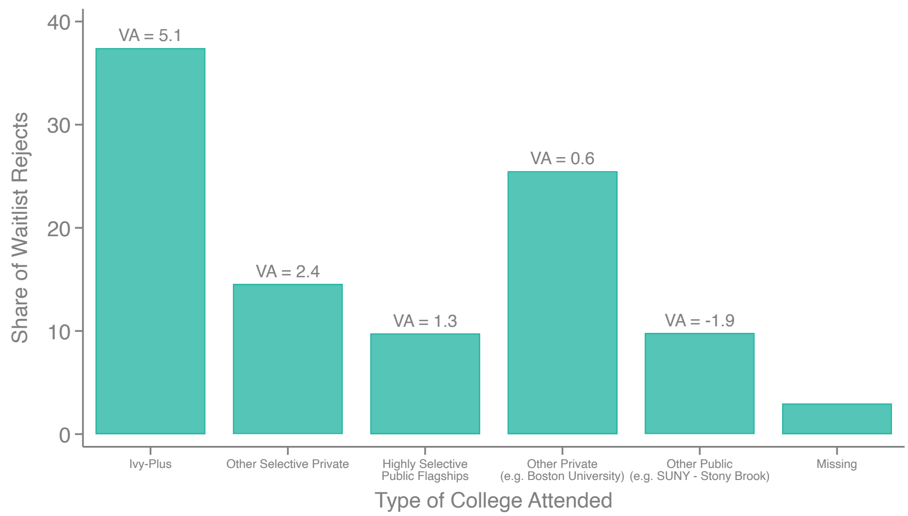

(b) Non-Academic Ratings, Controlling for Test Score



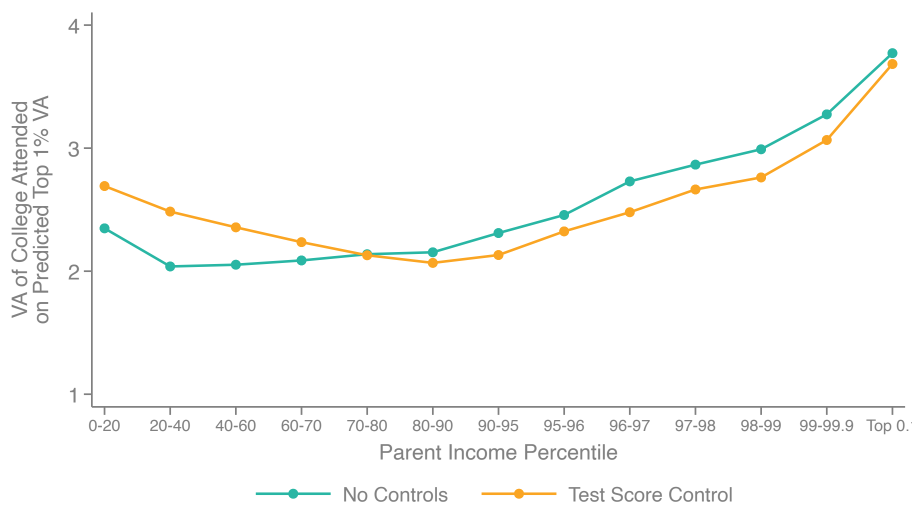

*Notes:* Appendix Figure 19 shows how admissions rates (reweighted by test score, as in Figure 2b) at other Ivy-Plus colleges in our college-specific sample vary with academic and non-academic ratings for applicants to the Ivy-Plus college for which we have the most granular ratings information, by parent income bin. We exclude legacies, recruited athletes, and faculty children, as well as students with missing ratings from this figure.

Appendix Figure 20: Distribution of Outside Options for Ivy-Plus Applicants Rejected from Waitlist

(a) Distribution of Colleges Attended by Rejected Applicants



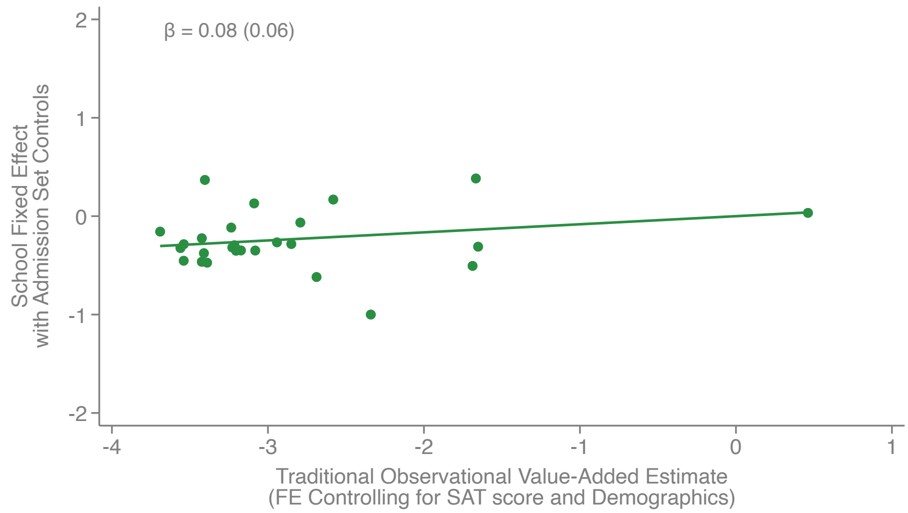

(b) Value-Added of Colleges Attended by Rejected Applicants, by Parental Income



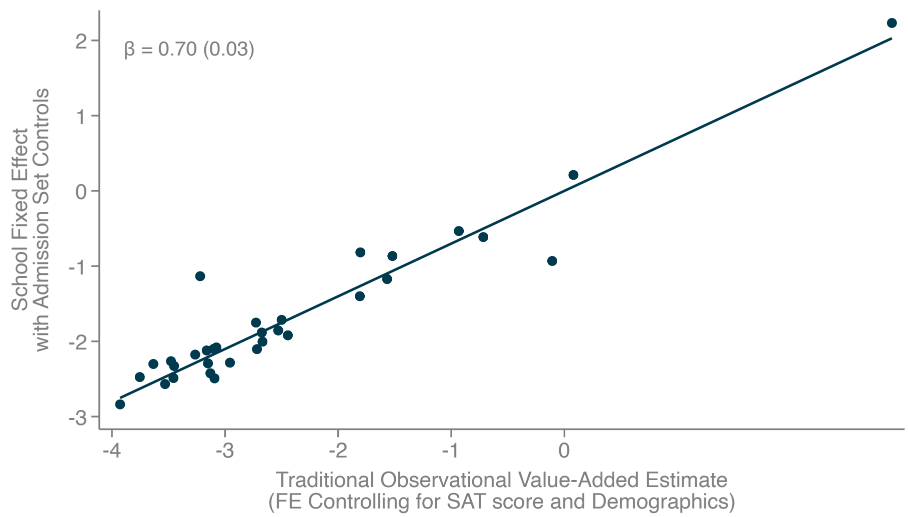

*Notes*: Appendix Figure 20a shows the distribution of colleges attended by applicants to Ivy-Plus colleges in our college-specific sample who were rejected from the waitlist. We divide colleges into five groups: Ivy-Plus, Other Selective Private, and the Highly Selective Public Flagships listed in Appendix Table 1 and then other private and other public colleges. The VA estimate listed on top of each of the bars reports the mean observational value-add (VA) estimate for colleges in each group on students' predicted probability of reaching the top 1% based on their age 25 employer, estimated as described in the notes to Figure 12. Appendix Figure 20b shows how students' outside options vary across the parent income distribution. It plots the mean observational VA of the college attended by applicants rejected from the waitlist at Ivy-Plus colleges by parent income bin. The green line plots raw means (i.e., without any controls), while the orange line reweights applicants within each parent income bin to match the test score distribution of all Ivy-Plus attendees.

Appendix Figure 21: Estimates of College Effects Using Matriculation Design in Texas vs. California Public Colleges

(a) Texas



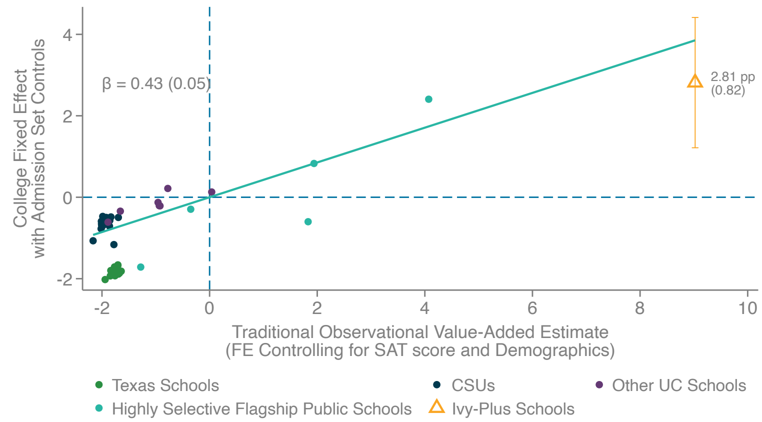

(b) California



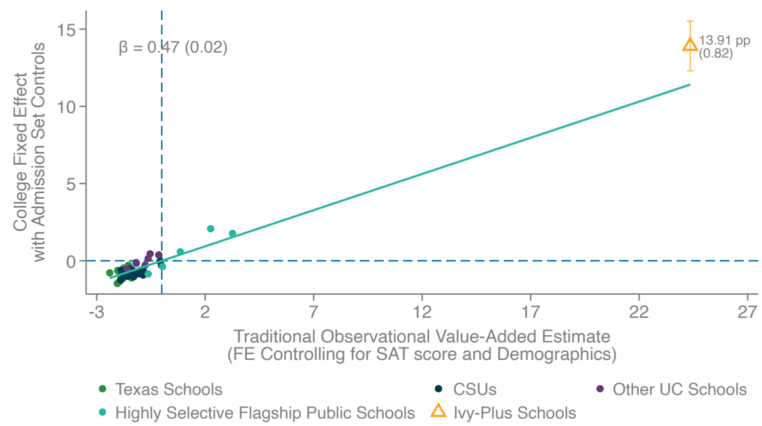

*Notes:* Appendix Figure 21 replicates Figure 14b restricting to in-state applicants to public colleges in Texas in Panel A and public colleges in California in Panel B.

Appendix Figure 22: Causal Effects of Ivy-Plus Attendance on Non-Monetary Outcomes Based on Matriculation Design

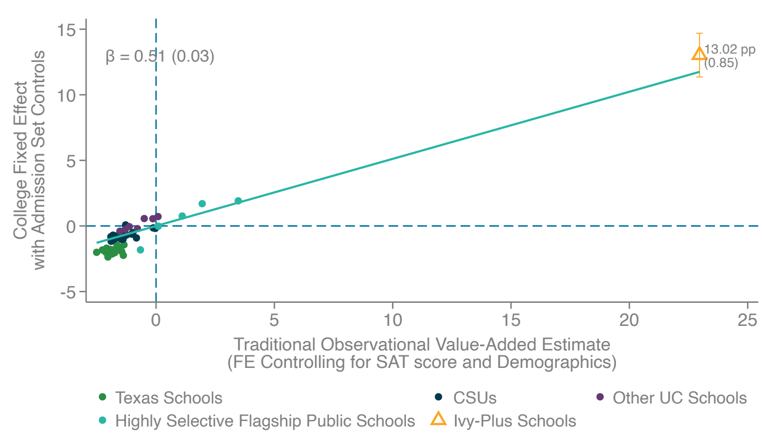
(a) Elite Graduate School Attendance




(b) Employment at Elite Firm



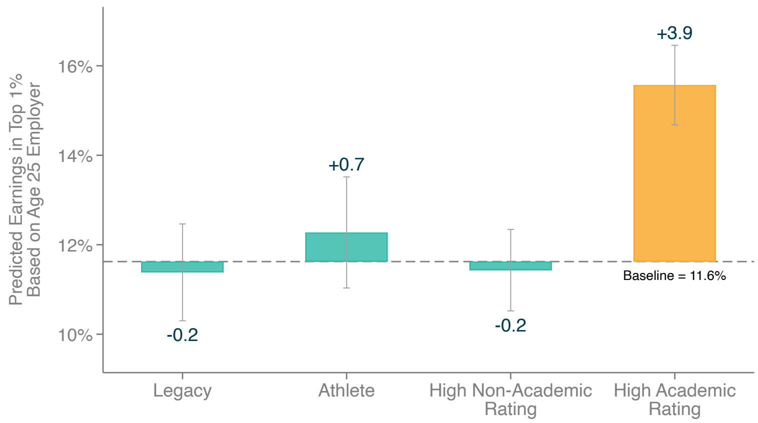

(c) Employment at Prestigious Firm



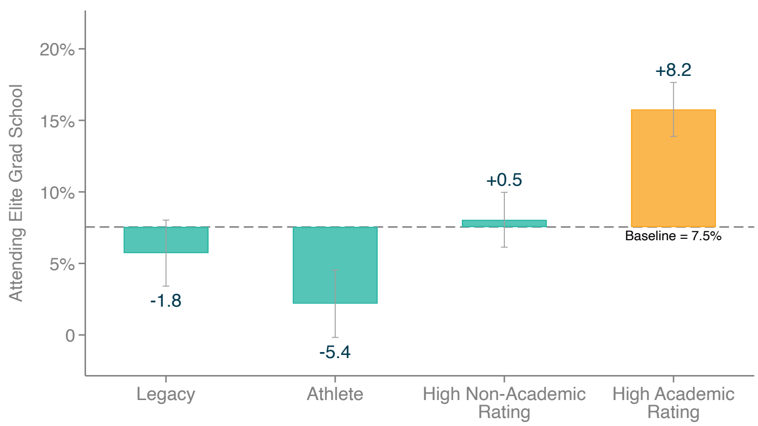

*Notes:* Appendix Figure 22 replicates Figure 14b using non-monetary outcomes: elite graduate school attendance at age 28 (Panel A), employment at an elite firm at age 25 (Panel B), and employment at a prestigious firm at age 25 (Panel C). See notes to Figure 13 for definitions of these outcomes.

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

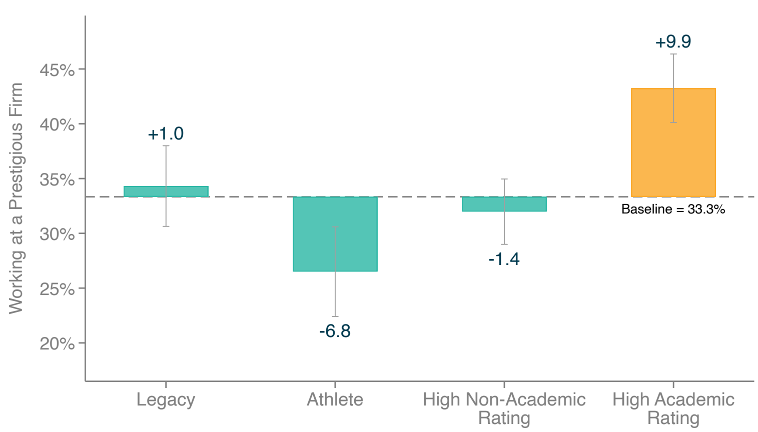
(a) Association Between Predicted Top 1% Earnings and Admissions Criteria




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



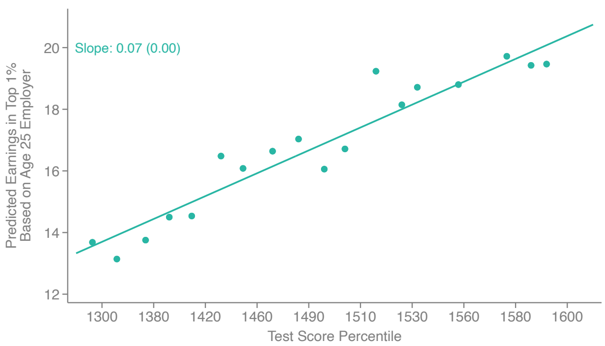

(c) Association Between Prestigious Firm Employment and Admissions Criteria



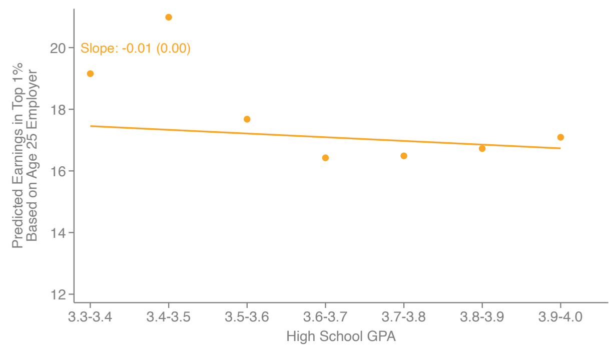

*Notes:* Appendix Figure 23 replicates the "Raw Comparison" estimates from Figure 16a, except restricting to the students who attended the Ivy-Plus college with the most granular ratings information (i.e., focusing on attendees, not all applicants). Panels B and C replicate Panel A using other post-college outcomes.

Appendix Figure 24: Ivy-Plus Matriculants' Outcomes by Test Score and High School GPA

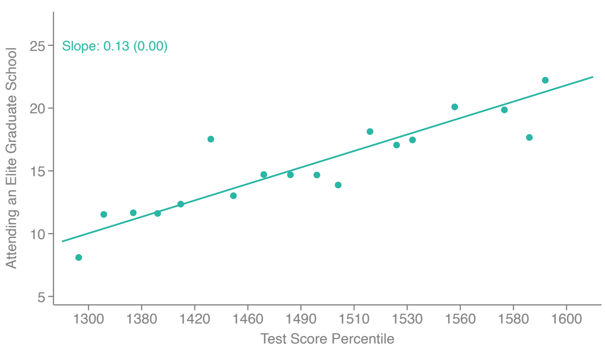
(a) Predicted Top 1% by Test Score



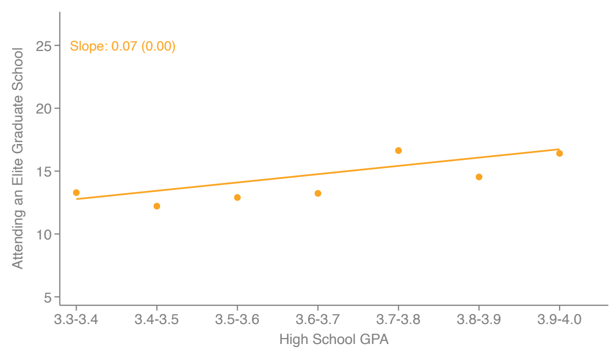

(b) Predicted Top 1% by High School GPA



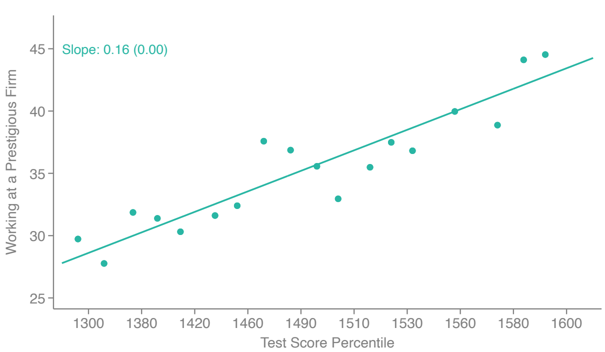

(c) Attending Elite Graduate School by Test Score



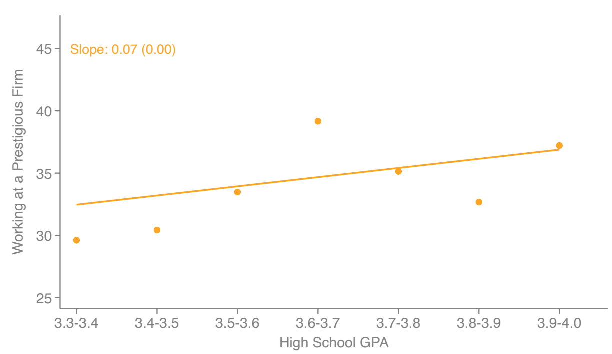

(d) Attending Elite Graduate School by High School GPA




(e) Working at Prestigious Firm by Test Score



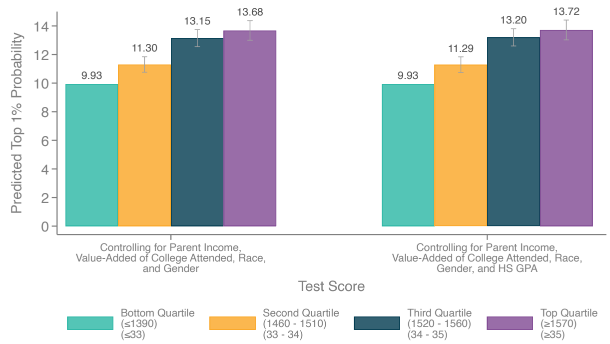

(f) Working at Prestigious Firm by High School GPA



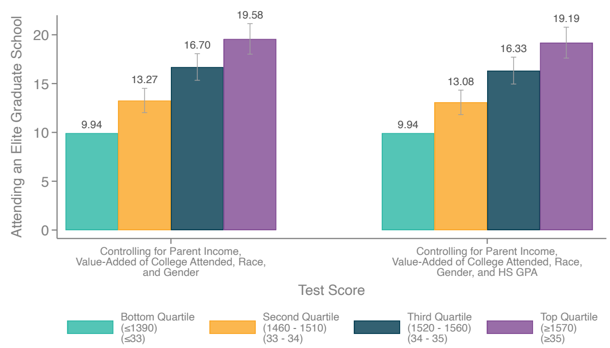

*Notes*: Appendix Figure 24 presents binned scatter plots of outcomes vs. ACT/SAT test scores or high school GPA for students enrolled at Ivy-Plus colleges in our college-specific sample where we have high school GPA information. Panels A, C, and E present binned scatter plots of students' post-college outcomes on test scores, controlling for parent income bin, indicators for 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. 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 estimated on exactly the same sample. We also report slopes based on OLS regressions (with standard errors in parentheses) estimated in the underlying microdata. Outcome measures are predicted top 1% earnings 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). See notes to Figure 13 for definitions of these outcomes.

Appendix Figure 25: Ivy-Plus Applicants' Outcomes by Test Score Quartile, Adjusting for Value-Added of College Attended

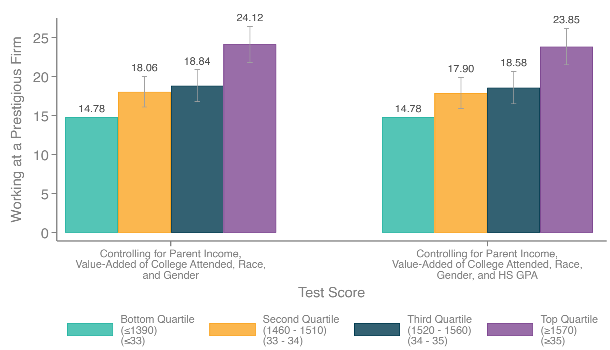
(a) Predicted Top 1%




(b) Elite Graduate School Attendance




(c) Employment at Prestigious Firm



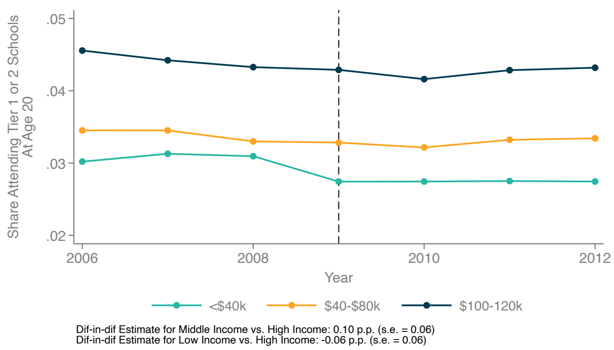

*Notes*: Appendix Figure 25 plots the relationship between students' post-college outcomes and their SAT/ACT test score quartile among waitlisted and admitted Ivy-Plus applicants, adjusting for the value-added of college attended. To construct Appendix Figure 25a, we regress the predicted probability of having earnings in the top 1% based on age 25 firm on indicators for test score quartile, race, gender, and parent income bin in the bars on the left and additionally for indicators for high school GPA on the right. We then repeat this regression with the observational VA of college attended (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 7) as the dependent variable. Each quartet of bars plots the four test-score-bin coefficients from the first regression minus those from the second. Appendix Figures 25b and 25c replicate 25a but using indicators for attending an elite graduate school at age 25 or working at a prestigious firm at age 25, respectively, as the outcome variables. Whiskers show 95 percent confidence intervals. See notes to Figure 13 for definitions of these outcomes.

Appendix Figure 26: Share of Ivy-Plus Attendees in Top 1% of Income Distribution at Age 33 by Parental Income, Controlling for Test Scores

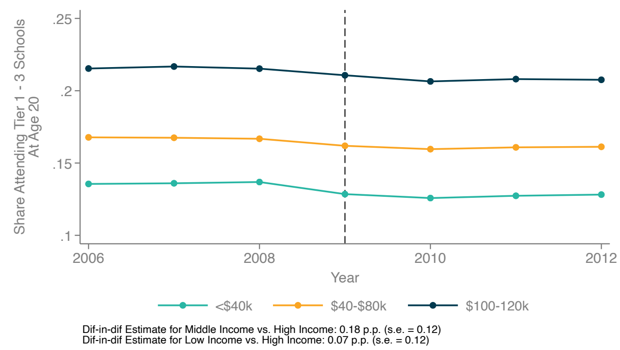

*Notes:* Appendix Figure 26plots the share of Ivy-Plus students in the top 1% at age 33 based on total individual income (as defined in Section 2.5) or W-2 wage earnings by parent income percentile, controlling for a quintic in test scores.

Appendix Figure 27: Evolution of College Attendance Rates Over Time, by Parental Income

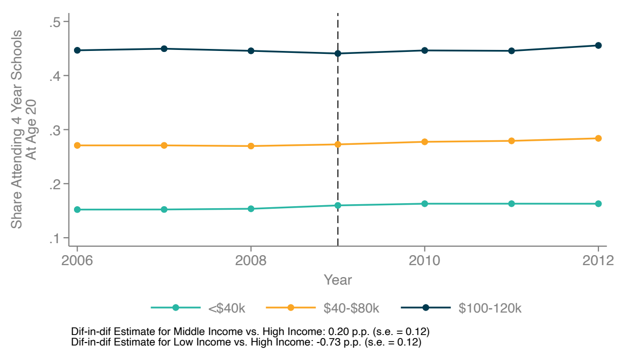
(a) Attendance Rates at Highly Selective Colleges, by Parental Income




(b) Attendance Rates at Selective Colleges

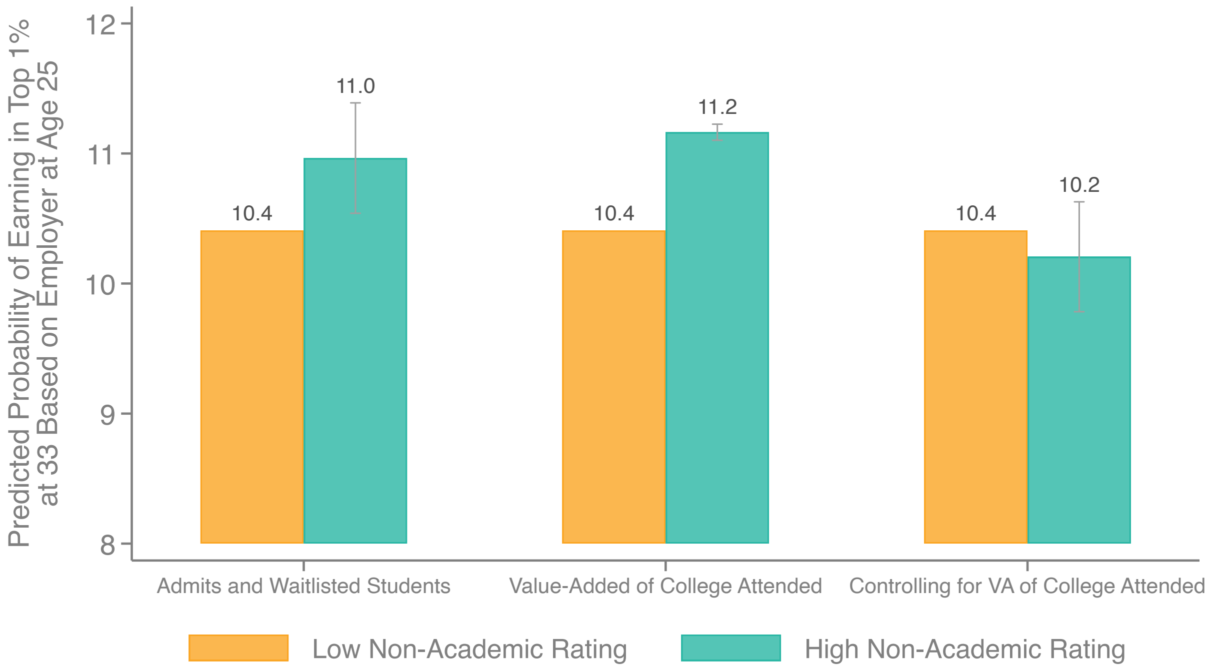



(c) Attendance Rates at Four-Year Colleges, by Parental Income




*Notes:* Appendix Figure 27 plots the fraction of children born in the 1986-1992 birth cohorts who attend different types of four-year colleges by high school graduation cohort, disaggregated by parental income. Appendix Figure 27a 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. Appendix Figure 27b reports attendance rates at schools in Tiers 1–3, which additionally include highly selective and flagship public colleges. Appendix Figures 27a and 27b restricts to students who attended any four-year college. Appendix Figure 27c reports attendance rates at any four-year college among the total population. Each figure also reports a difference-in-difference estimate, regressing college attendance on income buckets, a post-2009 indicator, and the interaction between the two for middle- and low-income children, each relative to that for high-income children, controlling for home state, year, and a quintic of parent income rank.

Appendix Figure 28: Post-College Outcomes Among Ivy-Plus Applicants by Non-Academic Ratings




*Notes*: Appendix Figure 28 compares the predicted top 1% share based on firm at age 25 of applicants who receive high v.s low non-academic ratings. The sample consists of applicants who were either admitted or offered a place on the waitlist from the Ivy-Plus college with the most granular ratings information in our college-specific sample. The first, third, and fifth bars show the mean predicted top 1% share of applicants with a low non-academic rating. The second bar adds the coefficient on high non-academic rating to the first bar, estimated using a regression of predicted top 1% on an indicator for high non-academic rating, high academic ratings, legacy status, and being a recruited athlete. The fourth bar repeats the second bar, except using the observational value-added of the college the student attended (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 7) as the dependent variable in the regression. The last bar adds the difference between the 2nd and 4th bars to the level in the 5th bar to obtain the difference in outcomes by non-academic rating adjusted for the value-added of the college the student attends.