Online Appendix

Income Segregation and Intergenerational Mobility Across Colleges in the United States

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A. Sample Construction and Income Definitions

Sample Definition. Our primary sample is very similar to the "extended sample" analyzed in Chetty et al. 2014, and much of this appendix is therefore taken directly from Chetty et al. (2014, Online Appendix A).

We begin with the universe of individuals in the Death Master (also known as the Data Master-1) file produced by the Social Security Administration and housed alongside tax records. This file includes information on year of birth and gender for all persons in the United States with a Social Security Number or Individual Taxpayer Identification Number (ITIN).[1] To construct our sample of children, we begin from the set of individuals born in the 1980-1991 cohorts. We measure parent and child income, college attendance, and all other variables using data from the IRS Databank, a balanced panel covering all individuals in the Death Master file who were not deceased as of 1996.

For each child, we define the parent(s) as the person(s) who claim the child as a dependent on a 1040 tax form in the year the child turns 17. Note that the parent(s) of the child are not necessarily biological parents, as it is possible for custodians (regardless of family status) to claim a child if the child resides with them.[2] If parents are married but filing separately, we assign the child both parents. We identify children's parents at age 17 because our goal is to measure the economic resources of the child's family around the time he or she attends college. We do not match children to parents at later ages (e.g., 18 or 19) because many children leave home after age 17 (at differential rates across income groups), creating scope for selection bias.

If the child is not claimed at age 17 on any 1040 tax form, we go back one year (to the year in which the child turns 16) to identify parents. We repeat this process until we find a year when the child is claimed, up to the year in which the child turns 12. Since the tax data start in 1996, for the 1980 cohort, we only match children up to age 16; for the 1981 cohort, up to age 15, etc. In short, we use up to 6 years (from ages 12-17) to find a parental match. If no such parental match is found, then the child's record is discarded.[3]

Importantly, once we match a child to parent(s), we hold this definition of parents fixed regardless of changes in parents' marital status or who claims the child in other years. For example, a child matched to married parents at age 17 but who had a single parent at age 16 is always matched to the two married parents at age 17. Conversely, a child matched to a single parent at age 17 who had married parents at age 16 will be considered matched to a single parent.

Finally, we discard the small set of children whose parents have negative family income (as defined in Section II.A) on average over the 5 year time window when they are aged 15-19. Negative income is generally due to business losses and denotes high potential earnings ability so that ranking such parents at the very bottom is actually misleading.

Details on Income Definitions. As discussed in Section II, in our baseline analysis, we measure children's earnings as the sum of individual wage income and net self-employment income (if positive) for year 2014. Here we provide further details regarding those definitions, which are more

[1] ITINs are issued by the IRS to individuals who do not have a social security number, for example because they are undocumented immigrants.

[2] Children can be claimed as a dependent only if they are aged less than 19 at the end of the year (less than 24 if enrolled as a student) or are disabled. A dependent child is a biological child, step child, adopted child, foster child, brother or sister, or a descendant of one of these (for example, a grandchild or nephew). Children must be claimed by their custodial parent, i.e. the parent with whom they live for over half the year. Furthermore, the custodial parent must provide more than 50% of the support to the child. Hence, working children who provide more than 50% of their own support cannot be claimed as dependents. See IRS Publication 501 for further details.

[3] Very few children are unclaimed on tax returns (Chetty et al. 2014) because claiming children yields substantial refundable tax credits. Therefore, the children we exclude are almost all non US-residents when they were aged 12-17.

complex than our parent income definitions because we must apportion total income reported on the tax return across individuals to measure income at the individual level.

For a child who is a non-filer (neither a primary nor a secondary filer on any 1040 return), individual earnings are defined simply as the sum of wage income from the individual's own W-2 forms. For a child who is a single filer, individual earnings are defined as the sum of wage income on the form 1040 and self-employment income from Schedule SE on the 1040 form.[4] We use wage income as reported on Form 1040 (instead of what is reported on W-2 forms) for filers because wage income on Form 1040 includes wages earned abroad, which can be significant particularly for children at the top of the income distribution. In particular, children who move abroad (but are U.S. citizens) are required to file standard tax returns and report their worldwide income, including any foreign earnings, but those earnings do not show up on W-2s.

For a child who is a married filer, individual earnings are defined as the sum of individual self-employment income from Schedule SE form 1040, and individual wage income defined as W-2 wage income plus one half of non-W-2 wage income from Form 1040.[5] Since we do not restrict the sample to children who are alive at the point at which we measure their income, children who are deceased are assigned zero earnings.

B. College Attendance: Data Sources and Methods

In this appendix, we describe the data sources and methods we use to assign students to colleges. The appendix is divided into five subsections. First, we describe our two sources of college attendance records and the differences in how they define colleges and annual attendance. Second, we describe how we homogenize their college definitions. Third, we discuss how we homogenize their annual attendance definitions and compile annual attendance records from the two data sources. Fourth, we describe how we identify and remove a small set of colleges who have incomplete 1098-T data. Finally, we summarize annual enrollment counts for our college attendance definitions.

Data Sources. We combine two data sources to measure student-level college attendance: Form 1098-T records and National Student Loan Data System (NSLDS) Pell grant recipient records.[6] Note that neither data source relies on the student or the student's family to file a tax return, and neither data source contains information on course of study or degree attainment.

Form 1098-T is an information return that is submitted by colleges to the U.S. Treasury Department. Each calendar year, higher education institutions eligible for federal financial aid (Title IV institutions) are required to file a 1098-T form for every student whose tuition has not been waived by the college (i.e. any student who pays or is billed tuition, or who has any non-governmental third party paying tuition or receiving tuition bills on his or her behalf). The form reports tuition payments or scholarships received for the student during the calendar year. Title IV institutions include all colleges and universities as well as many vocational colleges and other postsecondary institutions, all of which we refer to as "colleges." Colleges are indexed in the 1098-T data by the

[4]Self-employment income is the amount for total tentative net earnings from self-employment. It is reported on Form 1040, Schedule SE, Section A or B, Line 3. We recode negative self-employment income as zero because negative self-employment income is generally due to business losses and is thus generally correlated with having a high level of latent income or wealth. We multiply self-employment income by 0.9235 to align treatment with wage earnings (as wage earnings are net of the 7.65% employer social security payroll tax).

[5]It is not possible to attribute to each specific spouse 1040 wage income that is not reported on the W2 forms. Hence, our decision to split such wage income equally across spouses.

[6]The full NSLDS data include data on recipients of Pell grants and federally subsidized loans. We use only the Pell grant data in our main attendance measures because almost all non-Pell students in the NSLDS data already appear in the 1098-T data, and using the non-Pell NSLDS records would likely generate more erroneous assignments due to timing inconsistencies across the two types of data (see below) than it would correct missing data.

college's Employer Identification Number (EIN) and its ZIP code. We use 1098-T data for students during calendar years 1999-2013.

Most colleges file a 1098-T for every student, regardless of whether the student's tuition has been waived. However, some colleges do not file a 1098-T for students who pay no tuition. Almost all such students with American parents are from low-income families, are eligible for a Pell grant from the federal government, and are required by their colleges to acquire a Pell grant in order to receive their tuition waiver.[7]

We therefore supplement the 1098-T records with records from the administrative NSLDS Pell records. The NSLDS contains information on every Pell grant awarded, including the college receiving the Pell payment (Pell grant payments are remitted directly from the federal government to the college the student attended). The NSLDS Pell data are indexed by award years, defined as the spring of the academic year beginning on July 1. We use NSLDS Pell data for all students in award years 1999-2014, comprising Pell awards for enrollment spells that began between the dates July 1, 1999, and June 30, 2014 (roughly academic years beginning in calendar years 1999-2013). Colleges are indexed in the NSLDS Pell data by the six-digit federal OPEID (Office of Postsecondary Education Identification) identifier.

We use the NSLDS Pell data to impute missing 1098-T data and thereby construct comprehensive student-college-year attendance records 1999-2013.[8] Doing so requires homogeneous college and time-period definitions across the two data sources, but the two data sources differ in these definitions. The next two subsections detail our methods for homogenizing those definitions and constructing comprehensive student-college-year attendance records.

Combining 1098-T and NSLDS Pell Records. Empirical work on higher education is frequently conducted at the level of the six-digit OPEID (hereafter "OPEID"). We therefore use the NSLDS Pell and loan data to construct a crosswalk between EIN-ZIP pairs from the 1098-T data (i.e. the EIN and the ZIP code of the college) and OPEIDs from the NSLDS Pell data. In almost all cases, each EIN-ZIP pair maps to a single OPEID. In the rare cases in which a single EIN-ZIP pair maps to multiple OPEIDs, we cluster the OPEIDs together and conduct our analysis as if the cluster were a single college. We refer to this unit of analysis—either an OPEID or a cluster of OPEIDs—as a "Super OPEID."

Our procedure for mapping EIN-ZIP pairs to OPEIDs relies on the fact that almost all students who receive a federally subsidized loan (and most students who receive a Pell grant) for attending a given college x in academic year t to t+1 will also have a 1098-T from college x in calendar year t or t+1 or both. Thus by merging students in the NSLDS to students in the 1098-T data within narrow time-period bands, we can infer the NSLDS OPEID that corresponds to each 1098-T EIN-ZIP pair.

Specifically, we first merge the full NSLDS data to the 1098-T data at the student level (without using any college identifiers), in order to identify records with both an OPEID (from the NSLDS data) and an EIN-ZIP (from the 1098-T data). We conduct the merge requiring that the NSLDS student's masked taxpayer identification number (TIN, i.e. her masked Social Security Number) equals the 1098-T student's masked TIN, as well as requiring the NSLDS award year equals either

[7] The vast majority of students appear in the 1098-T database. When we measure college attendance between the ages of 19 and 22 (as in our baseline analysis), 95.9% of the students in our analysis sample appear in the 1098-T records. A larger share of observations come from the NSLDS Pell records for lower income families (Online Appendix Figure IX), but even in the bottom parent income quintile, 87.1% of students appear in the 1098-T records.

[8] Our approach misses students who attend a college that does not file 1098-T's for all students and who have their tuition entirely waived despite having parental income above the Pell grant eligibility threshold. Such students could include top athletic recruits. We believe that such cases are rare, as shown by the high correlation between the counts of students in our data and total counts from IPEDS.

the 1098-T calendar year or the 1098-T calendar year plus one. Merging by year and year-plus-one is appropriate given the award year definition (see above subsection on data sources). Only rows that are successfully merged are retained.

The resulting merged dataset contains many correct matches between OPEIDs and EIN-ZIP pairs and some incorrect matches. For example, a student who uses a federally subsidized loan at UC-Berkeley and was billed tuition at both Berkeley (during the school year) and Stanford (for summer school) will have two rows in the merged data: one with Berkeley's OPEID and Berkeley's EIN-ZIP pair and another with Berkeley's OPEID and Stanford's EIN-ZIP pair. In order to correctly map Berkeley's OPEID and EIN-ZIP pair, we rely on the fact that most Berkeley students do not also attend Stanford.

To algorithmically identify the correct link between OPEIDs and EIN-ZIPs, we construct counts by OPEID, EIN-ZIP, and calendar year in the merged dataset. The distribution of counts exhibits very clear mass points and almost always stable across years: nearly all the counts of each OPEID appear in a single OPEID-EIN-ZIP cell, and almost all the counts of each EIN-ZIP appear in a single OPEID-EIN-ZIP cell. Using this algorithm, we construct a mapping of EIN-ZIP pairs to OPEIDs by identifying the OPEID(s) that appear most frequently for each EIN-ZIP pair and thus likely correspond to the same college. In the final step, OPEID-EIN-ZIP triads were confirmed to correspond to the same college via manual comparison of NSLDS college names and 1098-T college names, and the small number of discrepancies were addressed using manual adjustments to the crosswalk.

Finally, we cluster OPEIDs as follows in order to produce our final Super OPEID crosswalk, which maps every OPEID to a single Super OPEID and maps every EIN-ZIP pair to at most one Super OPEID. If an OPEID's matched EIN-ZIP pair(s) matched only to that given OPEID, then we map the OPEID and all of the OPEID's matched EIN-ZIP pairs to a Super OPEID equal to the OPEID.[9] If instead an OPEID's matched EIN-ZIP pair(s) match to multiple OPEIDs, then we map all of the matched OPEIDs and their matched EIN-ZIP pairs to a Super OPEID equal to a unique number that is smaller than the smallest OPEID so that there are no conflicts.[10] OPEIDs that did not credibly match at least one EIN-ZIP pair and EIN-ZIP pairs that did not credibly match to any OPEID are assigned Super OPEID -1 (colleges with insufficient or incomplete data). We treat Super OPEID -1 as a separate "college" and include it in our publicly released statistics, but omit it from most analyses unless otherwise specified.

We use the Super OPEID crosswalk to assign a Super OPEID to every record in the NSLDS data and every record in the 1098-T data. The crosswalk comprises 5,327 Super OPEIDs: 5,208 unaltered OPEIDs (values ranging from 1002 to 42346) and 119 newly created clusters of OPEIDs (positive values below 1002). 2.7% of NSLDS Pell records and 1.1% of 1098-T records from 1999-2013 are assigned Super OPEID -1.[11]

Imputing Calendar Year Attendance Records for Pell Recipients. The vast majority of student-college-year attendance observations appear in the 1098-T data, which measure attendance by

[9]For example, Cornell (OPEID 190415) may submit 1098-T forms from the same EIN but from two ZIPs—one ZIP corresponding to its Ithaca campus and another ZIP corresponding to its New York City campus. In this case, we map Cornell's OPEID and its two EIN-ZIP pairs to Super OPEID 190415.

[10]For example, the University of Massachusetts system comprises four undergraduate campuses, each with its own OPEID. However, all University of Massachusetts 1098-Ts are submitted from the same centralized EIN-ZIP. We therefore map all four of University of Massachusetts's OPEIDs and the University of Massachusetts EIN-ZIP to a new Super OPEID value that is smaller than 1000 (125 in the case of the University of Massachusetts). Note that all OPEIDs are larger than 1000.

[11]The rate of 1098-T assignment to Super OPEID -1 is 11.2% in 1999 and is between 0.04% and 2.2% from 2000-2013. The 1999 1098-T data lack the ZIP code of the college, so in that year only, we assign Super OPEID using the subset of EINs from the Super OPEID crosswalk that map to a single Super OPEID regardless of ZIP code.

calendar year. Therefore, after using our Super OPEID crosswalk to assign a consistent college definition to every NSLDS Pell record and ever 1098-T record, we use information from the NSLDS on dates of attendance to impute missing 1098-T data, thereby yielding comprehensive attendance records by calendar year from 1999-2013.

We map the NSLDS data to calendar years as follows. For every NSLDS Pell student at a Super OPEID x and a Pell award enrollment start date lying in calendar year t, we impute a 1098-T for the student at Super OPEID x in calendar year t. Then, for every NSLDS Pell student at a Super OPEID x and a Pell enrollment start date in the second half of calendar year t and with a Pell grant amount equal to more than 50% the student's maximum eligible Pell amount in the award year, we additionally impute a 1098-T for the student at Super OPEID x in calendar year $t+1$. Finally, we remove duplicate records. The remainder of this subsection explains the logic underlying this imputation strategy further.

The NSLDS Pell data contain the start date of the enrollment period covered by the Pell grant. If the college had submitted 1098-Ts on behalf of a given Pell student whose enrollment period began in calendar year t, the college would likely have submitted a 1098-T for the student in calendar year t (had it been required to do so). Thus, for every NSLDS Pell student with Super OPEID x and an enrollment start date in calendar year t, we impute a 1098-T for the student with Super OPEID x and calendar year t.

If the college had submitted 1098-Ts on behalf of a given Pell student, and if that student's enrollment period straddled a fall and spring term, the college would likely have submitted a 1098-T in calendar year $t + 1$ as well as in calendar year t. The NSLDS Pell data do not contain the end date of the enrollment period covered by the Pell grant. However, they do contain the share of the student's maximum eligible Pell amount in the award year that was allocated to the grant. Pell grants for a single semester typically have an amount equal to only half of the student's annual Pell maximum grant amount, even if tuition is very expensive. Hence for every NSLDS Pell student with Super OPEID x who has an enrollment start date between July and December of year t and has strictly greater than 50% of the student's maximum Pell eligibility amount allocated to the grant, we impute a 1098-T for the student with Super OPEID x and calendar year $t + 1$.

After these imputations, we drop observations that are duplicates in terms of student, Super OPEID, and calendar year. This allows students to be recorded as having attended any number of Super OPEIDs in a calendar year, but ensures that they are not recorded as having attended any Super OPEID more than once in a calendar year.

There are no public measures of calendar-year Pell attendance that can be used to directly validate the imputation procedure described above. However, indirect validation methods suggest a high degree of fidelity. The share of our students on a Pell grant in the average calendar year is very highly correlated with, and similar in levels to, approximations to annual Pell student shares based on publicly available data. Moreover, at colleges with substantial numbers of students on Pell grants, the imputation algorithm adds almost no net students to 1098-T attendance records—consistent with these colleges issuing 1098-T forms for all students regardless of their tuition billing status and with our algorithm only imputing 1098-Ts in calendar years that the student was in fact enrolled.

Removing College-Years with Incomplete 1098-T Data. A small number of college-year observations have incomplete 1098-T data, either because of errors in administrative records or because of changes in EIN's and reporting procedures.[12] We discard these defective college-years by flagging

[12]Most of these cases are college-year cells with zero 1098-Ts in the database. For example, in the years when the 1098-T first began to be collected (1999-2002), a number of small universities do not have any records at all in the database. In addition, some universities switch from reporting data separately for each campus to using a single EIN-ZIP for all their campuses, which creates inconsistencies in their data across years.

them using two methods based on counts of total students. The counts described below are constructed using the total counts of forms 1098-T and Pell grants for all children born in 1980-1991, regardless of a successful link to parents and regardless of whether the student attends several institutions.

First, for each college-year, we compare the count of individuals receiving a 1098-T form but excluding Pell grants (what we call the 1098-T-only count) versus the count of individuals receiving either a 1098-T form or a Pell grant (what we call the full count). When the 1098-T-only count is less than 10% of the full count, we conclude that there are too few 1098-T forms for the data to be complete and flag the college-year. In the vast majority of these cases, the 1098-T counts are exactly zero, implying that the college did not report any 1098-T form (most likely because the information was not transmitted correctly to the IRS or because the institution used a different EIN-ZIP in that specific year). We use the 10% threshold as a way to capture rare situations where the 1098-T counts are not exactly zero, but are clearly too small relative to the number of Pell grants to be plausibly complete.

Second, we also flag college-years when full counts are too low (less than 75%) or too high (over 125%) relative to both the preceding and subsequent years. Such abnormal changes in counts likely reflect a data reporting issue rather than true changes in enrollments, which tend to be very smooth across years.

In total, these two flags account for 2.4% of (enrollment-weighted) college-year observations and 21.9% of college-year observations when not weighing by enrollment. The rate is much higher unweighted because the data at very small colleges is much less complete.

We discard college-year records that are flagged as incomplete before assigning students to their "most-attended" college or the first college, in order to ensure that our sample accurately represents attendance at each college. Our baseline measure of a child's most-attended college uses four years of data (the years when the child turns 19, 20, 21, and 22).[13] A college which has defective (and hence discarded) data for more than 1 year out of the 4 relevant years is re-assigned to super_opeid=-1, the pool of colleges with "incomplete or insufficient data." As a result, a college is retained in our cohort-level data only if we have valid data for at least 3 years (out of the 4 years).

Enrollment Counts for Attendance Measures. The steps described above yield a student-college-year level dataset that provides a complete record of college attendance in the U.S. during calendar years 1999-2013 for children born between 1980-1991. This dataset contains 207.6 million observations.

Using this dataset, we construct the three measures of college attendance—the most-attended college (our primary measure), age-20 college, and first-attended college—following the definitions given in Section II.B. In what follows, we document the impact of the restrictions imposed in each definition on sample sizes and report the share of observations obtained from the 1098-T vs. NSLDS datasets.

To construct the most-attended definition, we first restrict the full dataset to attendance between ages 19-22, which leaves 114.6 million records. Condensing the student-college-year data to the student level using the most-attended definition (see Section II.B) leaves 33.1 million student-level records. Eliminating students we cannot match to parents or whose parents had negative income

[13] For example, we measure college attendance using data from 1999 to 2002 for children born in the 1980 cohort. We measure college attendance starting with the year the student turns 19 because the 1098-T data are only available starting in 1999, making 19 the first observed age for the 1980 birth cohort. Omitting the year in which children turn 18 is not consequential because very few children attend college only in the calendar year in which they turn 18; for instance, only 1.6% of the children in the 1982 birth cohort attended college in the year they turn 18 but not between the ages of 19-22.

leaves 31.0 million records. Finally, restricting to birth cohorts 1980-1982 (as we do in our main analysis) leaves 6.7 million records; including non-college-goers in this sample yields a sample size of 10.8 million children.

As mentioned in Section II.F, we impute income statistics and attendance for colleges with missing data for the 1980-82 cohorts using data from the 1983 and 1984 cohorts. Specifically, if a college is missing one or more years of data for the 1980-82 cohorts—either because of incomplete reporting of 1098-T forms or because the college opened more recently—we impute values for the missing cohorts using data from the 1983-84 cohorts. To impute a missing income statistic y_{ct} for college c in cohort t, we first estimate an OLS regression $y_{ct} = \alpha + \beta^y_{1983} y_{c,1983} + \beta^y_{1984} y_{c,1984} + \varepsilon$ using the sample of all colleges with non-missing data in cohort t as well as 1983 and 1984, weighting by enrollment. We then impute values for missing cohorts with the predicted values from this regression, based on each college's actual data in 1983 and 1984 (omitting colleges with missing data for 1983 or 1984). Such imputations account for 9.0% of enrollment-weighted observations in the analysis sample (1980-82 birth cohorts).[14]

We use this imputation procedure to impute data for 596 (27%), 520 (24%), and 406 (18%) colleges in cohorts 1980-1982, respectively, accounting for 570,000 additional students (9.0% of college attendees and 5.0% of all children). For the remaining 264 colleges that are missing data for either the 1983 or 1984 cohorts, we do not impute any values. This leaves us with 11.3 million children in our core sample underlying our main analysis.

9.2% of our annual attendance records for students aged 19-22 were not in the 1098-T data and appeared only in the NSLDS Pell data. Using our most-attended college attendance measure, 4.1% of the students in our analysis sample were not in the 1098-T data and originally appeared only in the NSLDS Pell data. The NSLDS Pell data has a smaller impact at the student level than the student-year level because many students attend a given college for multiple years and receive a 1098-T form in at least one of those years.

We define a child's *age 20 college* as the college the child attends in the calendar year that she turns 20.[15] To construct the age-20 definition, we restrict the full dataset to attendance at age 20, which leaves 30.5 million records. If a student attends multiple colleges at age 20, we weight the student-college-level records using the method described in Section II.B such that each student carries a total weight of one, leaving 27.3 million effective records for 26.1 million students. After bringing in non-college-goers under this definition, restricting to birth cohorts 1980-1982, and restricting to students matched to parents with weakly positive income, we have 11.0 million records for 10.8 million children. Finally, we impute income statistics and attendance for colleges with missing data for the 1980-82 cohorts as described above, leaving us with a 11.3 million person sample underlying our age-20 analysis.

We define a child's *first-attended college* as the college that a child attends first between the calendar years in which she turns 19 and 28 (inclusive), breaking ties using the same method as

[14]This imputation procedure helps increase the coverage of colleges in the analysis sample because a number of small colleges began reporting 1098-T data only in 2002. However, all of the main findings of the paper hold if we restrict attention to the set of colleges with no imputed data. The imputation leads us to slightly overstate the aggregate college attendance rate in the analysis sample, as some of the students for whom we impute college attendance from later data may already have been assigned to another college that they also attended or to the "colleges with insufficient data" category. Such double-counting turns out to be very small in practice (see later in this appendix for further details).

[15]If a student attends multiple colleges at age 20, we break ties by assigning the college that the student attended in the subsequent year, if any. For observations where ties remain (e.g., because the student attended the same multiple colleges the following year as well), we retain all colleges and weight each student-college observation by the reciprocal of the number of colleges attended (so that the total weight of each student in the analysis remains constant).

in the age 20 definition. To construct the first-attended definition, we restrict the full dataset to attendance between the ages of 19 and 28, which leaves 175.4 million records. If a student begins multiple "first-attended" colleges in the same year, we assign the student a college based on the method described in Section II.B, leaving 37.0 million records. Bringing in non-college-goers under this definition, restricting to birth cohorts 1980-1982, and restricting to students matched to parents with weakly positive income leaves 10.9 million records for 10.8 million children. Finally, we impute income statistics and attendance for colleges with missing data for the 1980-82 cohorts, leaving us with a 10.8 million person sample underlying our first-attended analysis. The reason that the first-attended definition yields slightly fewer records than the others is that we do not double-count students assigned to Super OPEID -1 (insufficient or incomplete data) in the final imputation step under this definition.

Comparisons to IPEDS Counts. We assess how well our methodology approximates the set of undergraduate degree seekers we seek to identify by comparing the count of students in our data to enrollment data from IPEDS. IPEDS does not have enrollment counts that exactly match our cohort-based definitions and age ranges, making direct comparisons difficult for many colleges, especially those where students enter at various ages. However, at highly selective colleges (defined as 176 colleges in the top two tiers of the Barron's 2009 selectivity index), the vast majority of students enter at age 18 and graduate in four years, making the number of first-time, full-time undergraduate students recorded in IPEDS a good approximation to our definition. Among these colleges, the correlation between our enrollment counts and the number of first-time, full-time undergraduates in IPEDS is 0.99.[16] In addition, IPEDS data show that 98.0% of full-time undergraduate students are degree seekers, suggesting that the number of summer school or extension school students in our sample is likely to be very small.[17]

C. Comparison of Incomes in Tax Data vs. American Community Survey Data

In this appendix, we compare the income distribution of parents in the tax data to incomes reported in the American Community Survey. Most prior work on the income distribution of families in the United States focuses on money income (defined as pre-tax market income plus cash transfers from the government) and uses the household unit (defined as all individuals living in the same dwelling). In contrast, in the tax data, we define income as Adjusted Gross Income (pre-tax income excluding government cash transfers) and define the unit of observation as the tax filing unit (either a single person or a married couple, excluding other household members).

We show that income distributions in the ACS are very similar to those in the tax data if we use the same household unit and income definitions. We focus on the annual family income of children aged 15 in 2000 (Panel A of Online Appendix Table II) and children aged 15 in 2006 (Panel B). We begin by describing how we define income and family units in the two datasets.

Tax Data. The unit of observation for family income is the tax unit. As described in Online Appendix A, the tax unit, i.e. the child's parents, are defined as the person(s) who claim the child as a dependent on a 1040 form in the year the child turns 17. Children are either assigned to married parents or a single parent, and income is defined as Adjusted Gross Income (AGI), which

[16] The IPEDS counts are 3% larger than our counts on average, which likely reflects international students not included in our sample.

[17] Our methodology could be further tested and refined by linking external data on college attendance—for instance, from the National Student Clearinghouse—to the tax records, as in Hoxby (2015).

is pre-tax and pre-transfer cash income when the child is 15.[18] AGI is rounded to the nearest $250 for disclosure purposes.

ACS Data. To illustrate how family and income definitions affect the results, in the ACS data, we consider both standard household definitions and construct a concept analogous to tax units. Similarly, we also consider both the traditional total money income measure and a concept analogous to AGI.

To define households, we simply use the household ID that uniquely identifies each household in our ACS sample. We then restrict the sample to children aged 15, excluding a small number of individuals aged 15 who are listed as the head of the household. To create the tax unit claiming the child, we determine who claims the child for tax purposes as follows. A child is assigned to married parents if the child lives in the same household with parents who are married and one of the parents has non-zero income. If both parents' incomes are zero, we use instead the income of the head of household as the head would most likely claim the child for tax purposes in this case. For children not living with married parents, we assign the mother as the parent if she is present in the household and has non-zero income. We assign the father as the parent if the mother is absent or has zero income. Finally, if both father and mother are absent, we define the parent as the head of household.

We obtain total money income for each household member directly from the ACS. We define AGI starting from total money income and subtracting social security income (retirement and disability benefits), veterans benefits, supplementary security income, welfare (public assistance) income, and other non-taxable cash transfers. AGI for the tax unit is defined as the income earned by each of the parent(s), summing across the two parents when the child is assigned to married parents.

Results. Column 1 of Online Appendix Table II presents statistics on the income distribution in the tax data. Column 2-4 present analogous statistics using the ACS data. Column 2 presents ACS statistics using the tax unit and adjusted gross income, which replicates the concepts we use in the tax data. The statistics in columns 1 and 2 are very similar. Notably, the quantiles of the earnings distribution (P10, P25, P50, P75, P90) are very similar across the two datasets. Mean incomes are higher in the tax data by about 15% because the ACS data are top-coded whereas the tax data are not. The fraction of children with zero tax unit income is slightly higher in the ACS (5.0%) than in the tax data (3.4%), perhaps because survey respondents fail to report very small income amounts.

Column 3 replicates the ACS income statistics at the household level instead of the tax unit level. Because a household can be larger than a tax unit (e.g., a child living with both a parent and a grandparent), adjusted gross incomes at the household level are substantially higher than incomes at the tax unit level. Column 4 then expands the income definition to total money income. This further increases income, particularly in the lower percentiles. The fraction with zero incomes falls to 0.4% and the bottom percentiles (P10, P25, P50) are now substantially higher than in the tax data because of cash transfers.

In sum, a naive comparison between survey data using typical money income and household definitions and tax data using the tax unit and adjusted gross income definitions would mistakenly suggest that tax data incomes are substantially lower than survey income. However, this discrepancy is entirely due to the differences in household unit and income definition. Once one uses the same definitions, the distribution of incomes reported in the tax data are well aligned with those in standard surveys.

[18]This is different from our main income concept used throughout the paper, which averages the parent's income when the child is 15-19. We use annual income here for comparison with the ACS data, where we only observe annual income.

D. Stability of Children's Earnings Ranks

In our analysis sample comprising the 1980-1982 birth cohorts, we measure earnings at ages 32-34. Measuring children's earnings ranks when they are too young can potentially yield misleading estimates of lifetime earnings ranks because children with high lifetime earnings have steeper earnings profiles (e.g., Haider and Solon 2006; Solon 1999). This issue may be especially acute for analyses of earnings outcomes at elite colleges, where many students go on to pursue advanced degrees. In this appendix, we show that ages 32-34 are sufficiently late in a child's life to obtain a reliable measure of children's ranks at all colleges. Of course, individuals' earnings *levels* continue to rise sharply during their thirties, but a rank-preserving fanning out of the earnings distribution does not affect the rank-based analysis of Section IV.

To evaluate when children's earnings stabilize, we examine how the earnings of children evolve by age at each college. In order to examine the profile of earnings over the broadest range of ages, we go back to the 1978 birth cohort for this analysis. For children born in 1978, we can observe college attendance starting at age 21 in 1999 and earnings up to age 36 in 2014.[19] We assign each child a college based on the college he or she attends most frequently in 1999 and 2000, following the same approach as we use in our baseline college definition described in Section II.B. We assign children percentile ranks at each age by ranking them relative to other children in the 1978 cohort in each calendar year.

Online Appendix Figure IIa plots the mean earnings ranks of children from ages 25 to 36 for children who attended colleges in four mutually exclusive tiers: Ivy-Plus, Other Elite (Barron's Tier 1 colleges, excluding the Ivy-Plus group), other 4-year colleges, and 2-year colleges. For individuals who attended elite colleges, and especially Ivy-Plus colleges, earnings ranks rise sharply from age 25 to 30. If we were to measure children's earnings at age 25, we would find that children at Ivy-Plus colleges have *lower* income ranks than those who attend less selective 4-year colleges. Mean ranks at elite colleges stabilize at approximately the 80th percentile after age 30, with very little change starting at age 32. In contrast, the age profiles at lower-tier colleges are virtually constant from ages 25 to 36, at approximately the 60th percentile for 2-year colleges and the 70th percentile for non-elite 4-year colleges.

The stabilization of mean earnings ranks once children reach their early thirties holds not just across college tiers, but also across individual colleges. To characterize the college-level patterns, we examine the mean ranks of students who attend each college at each age from 25-36. Online Appendix Figure IIb plots the (enrollment-weighted) correlation of the mean ranks at each age with the mean ranks at age 36 across colleges. Consistent with the patterns in Online Appendix Figure IIa, this correlation rises sharply between ages 25 and 30, when it reaches 0.98 and stabilizes. We find analogous stabilization across all quantiles of the distribution by the early 30s, including the probability that children reach the top quintile or the top 1% of their age-specific income distribution (Online Appendix Figure IIc-d).[20]

E. SAT/ACT Data

For individuals who took either test multiple times, we use the individual's maximum composite score. The mean SAT and ACT scores for children in our sample for whom we observe a score

[19] We do not use the 1978 cohort for our primary analysis of intergenerational mobility because we cannot link children in the 1978 cohort to their parents based on dependent claiming. However, linking children to their parents is not necessary to analyze the unconditional distribution of children's earnings as we do here.

[20] At the vast majority of colleges, earnings ranks stabilize by age 25, implying that one can reliably analyze earnings outcomes for the 1980-89 cohorts with our publicly available data for most colleges.

is 989 and 21.8, which are each roughly comparable to the mean scores of 1026 (reported by the College Board) and 20.9 (reported by ACT) for the high school graduating class of 2004.

We combine the SAT and ACT data as follows into a single test score, which lies on the SAT's 400-1600-point scale. For the 47.6% of college students with an SAT score, their SAT/ACT score equals their SAT score – including for the 14.3% of college students with both an SAT score and an ACT score. To facilitate non-parametric matching, we coarsen SAT scores into 20-point bins throughout our analysis. For the 26.2% of college students with an ACT score but not an SAT score, we convert ACT scores to SAT using the 2016 ACT/SAT concordance table (Summit, 2016) in which each ACT score is mapped to a range of SAT scores. For each person with an ACT score, we randomly select a 20-point SAT score bin from the range of possible scores.

F. Estimation Algorithm for College-Level Statistics

This paper builds upon the Department of Education's College Scorecard by constructing estimates of student and parent income distributions at higher education institutions in the U.S. The College Scorecard reports exact statistics on student earnings by college. The Scorecard's student population is the subset of enrollees who receive federal financial aid, as recorded in the Education Department's National Student Loan Data System (NSLDS) data. We extend the Scorecard by reporting estimates of student and parent incomes at higher education institutions for the full population of enrollees by combining NSLDS enrollment data with data from Form 1098-T. Following established disclosure standards such as the standard of aggregating over 10 or more tax units when disclosing statistics, we report *estimates* for each college that are based on tabulations that aggregate across several colleges. This appendix describes our methodology for constructing these college-specific estimates in detail.

We begin by reporting statistics for groups of ten or more similar colleges, for instance average student earnings for colleges in different selectivity tiers and states. This aggregation over ten (or more) colleges is a direct application of established disclosure standards, used for instance in the production of county-to-county migration data by the Internal Revenue Service. We report statistics by birth cohort, defining each child's college as the college he or she attends most between the ages of 19 and 22. For example, the average earnings for students in the 1980 birth cohort who attended community colleges in Illinois—a group of 29 colleges—is \$36,316. Because we measure college attendance between the ages of 19 and 22, this statistic is based on an aggregate of $29 \times 4 = 116$ college-years of data (and several thousand students).

Although simple tabulations based on state and college selectivity tier provide some information on college outcomes, colleges differ on many dimensions as well. For instance, large colleges might differ from small colleges, public institutions might differ from private institutions, and differences in the mix of majors chosen by students might affect their incomes after graduation. To study how these factors are associated with students' and parents' incomes at each college, we use multivariable regression models to relate college-level outcomes to a set of publicly available college characteristics and report the coefficient estimates obtained from these regression models. We estimate these models by pooling data from several colleges, so that—just like the raw averages—the models provide estimates based on aggregate tabulations without directly revealing any individual data from a given college.

An important consideration when estimating such regression models is to preserve the same degree of confidentiality as the raw group mean of \$36,316 reported above. A raw mean over the group of ten colleges in a particular selectivity tier preserves confidentiality because ten underlying data points are aggregated to construct one statistic that is disclosed. That is, there are nine more underlying data points than the number of statistics disclosed. To preserve the same degree of confidentiality as we include additional predictive characteristics, we add one college to the group for every additional predictive characteristic that we include. This procedure ensures that there are always at least nine more underlying data points than aggregate statistics, exactly as in the construction of the raw mean. For example, suppose we include two additional characteristics (e.g., total college enrollment and the fraction of students in STEM majors) to explain differences across colleges. In this case, we would estimate a regression model using at least 12 colleges and disclose 3 aggregate statistics (the intercept and coefficients on college enrollment and STEM majors from the regression). Since there are 9 more underlying data points than the number of aggregate statistics disclosed, this method preserves the same degree of confidentiality as a raw mean based on 10 colleges.

There are numerous characteristics that could be used to understand differences in outcomes across colleges. We begin with data on outcomes from the (publicly available) College Scorecard, such as average earnings for students receiving federal student aid and other statistics on the distribution of earnings, such as the 10th and 75th percentiles. To model differences between students receiving federal aid (those covered by the Scorecard) and the full set of students enrolled at each college, we use three additional broad categories of college-level characteristics. First, we include measures of the type of the education at each institution, such as instructional expenditures per student, the fraction of faculty that are part time, and the net price of attendance for the average student. Second, we include variables that characterize the mix of fields of study chosen by students, such as the fraction of students pursuing STEM majors. Third, we include various measures of students' demographic characteristics.

To determine which of the large number of available characteristics to use in the regressions models, we use a covariate selection approach similar to that used in the machine learning literature. We begin by partitioning colleges into groups, where each group g corresponds to a manually-selected set of 20-50 colleges with similar characteristics. This partitioning is useful because the best predictors of outcomes in one type of colleges (e.g., elite private colleges) are typically not the same for other types of colleges (e.g., community colleges). We then let the data tell us which characteristics are the most important predictors of outcomes in each group g using a forward-search algorithm, choosing the characteristics that add the greatest explanatory power sequentially. In each group g, we first regress the outcome of interest (e.g., mean student earnings) y on each available characteristic $c \in C$.[21] We retain the characteristic c_i that explains the most variation in outcomes across colleges (i.e. the variable that generates the highest R-squared or, equivalently, the lowest mean-squared error). We then repeat this procedure adding a second explanatory variable to the regression, cycling through the remaining characteristics, and retaining the characteristic that explains the greatest amount of the residual variation. We continue this procedure of selecting explanatory variables until either (1) the number of characteristics used reaches the limit of the number of observations in each college group minus 9 or (2) the standard deviation of the prediction errors falls below 3% of the (enrollment-weighted) population-wide standard deviation of y, which is on the order of the standard errors of the college-by-cohort estimates.[22]

[21] We clean the set of covariates to exclude variables with observations more than three standard deviations from the (within group) mean and all variables with missing observations. We also drop covariates that are binary indicators and variables that contain five or more observations of exactly 0 or 1 (within a given group).

[22] To allow for flexibility in functional forms, we allow the algorithm to select between logarithmic and quadratic forms for each eligible covariate. We incorporate a functional form test to ensure that logarithmic terms are not added to a model in which the same variable appears in level or quadratic terms, level terms are not added to a model with logarithmic terms, and quadratic terms are not added unless a level term is in the model. When predicting a probability, we perform an OLS regression and recode predicted values that are greater than 1 or less than 0 to 1 or 0, respectively.

Online Appendix Table XVIII provides an example of one such model estimation, studying the relationship between students' average incomes (between the ages 32 and 34) and college characteristics within the 29 community colleges in Illinois. The forward-search algorithm selects several variables from the College Scorecard, which is not surprising given that these data measure the same outcomes for the subset of students receiving federal aid at each college. The estimated relationships are intuitive: for instance, colleges with higher student earnings on the College Scorecard (by several measures) are predicted to have higher earnings overall. The regression model also includes a number of variables that capture other aspects of the student body and educational characteristics at each college that predict earnings. For instance, colleges with higher faculty salaries have higher earnings, perhaps because they offer higher quality instruction. The percentage of students receiving financial aid is correlated with lower earnings, while colleges with higher total enrollment generally have higher average earnings. Overall, the model estimated in Online Appendix Table XVIII includes 12 aggregate statistics—the mean level of earnings (the intercept) and 11 coefficients on explanatory variables—to describe average incomes of students in a group of 29 colleges. Hence, there are 17 more data points than the number of aggregate statistics disclosed, in adherence with established disclosure standards.

Using the estimated regression coefficients in Online Appendix Table XVIII, we produce college-specific estimates of average outcomes. Intuitively, we begin with average earnings for this group of community colleges in Illinois ($36,316). We then adjust this average based on publicly available college characteristics using the model estimated in Online Appendix Table XVIII. For instance, we adjust estimates upward for colleges with higher levels of earnings in the College Scorecard. Similarly, we adjust earnings upward for colleges with higher faculty salaries. We make analogous adjustments for each of the other 11 characteristics listed in Online Appendix Table XVIII. Since each college's estimate is adjusted according to its own characteristics, this procedure results in college-specific estimates of mean earnings that are based entirely on the aggregate estimates from the regression rather than any one college's own data.

The college-specific estimates provide fairly accurate estimates without disclosing exact college-specific data for two reasons. First, the College Scorecard already contains considerable information about the earnings of students at each college, as the earnings of students receiving federal aid are highly predictive of the earnings of the student body more broadly. For example, regressing median earnings in our data on median earnings in the College Scorecard (the main earnings measure reported in the Scorecard) yields an R^2 of 0.92 (Looney 2017). Second, the discrepancy between the earnings estimates from the College Scorecard and the earnings for the full set of students is well explained by differences in observable characteristics.

Row 1 of Online Appendix Table XIX summarizes the precision of the estimates of mean earnings (across all colleges) by showing summary statistics for the distribution of errors (the difference between our estimate and the true value of mean earnings at each college). The mean absolute error is $266. 1% of colleges have errors exceeding $1,846, and 5% have errors exceeding $965 in magnitude. Hence, the estimates we construct are informative about broad differences in outcomes between colleges—and thus will be useful both for education researchers and prospective students—without disclosing data about any single college.

We use analogous regression models to calculate other statistics beyond mean earnings at each college, such as the fraction of students at a given college that reach the top 20% of the student earnings distribution conditional on having parents in the bottom quintile of the parents' income distribution. Again, we aggregate colleges and estimate regression models based on colleges' observable characteristics to understand the factors that predict these other outcomes and construct college-specific estimates. As with mean earnings, the estimates provide valuable college-specific information about these outcomes, as shown in Online Appendix Table XIX.

G. Construction of College-Level Characteristics

This appendix provides definitions and sources for the college-level characteristics we use in our correlational analysis.

Public. This indicator provides a classification of whether a college is operated as public institution or as a private college that derives its funding from private sources. We use the Integrated Postsecondary Education Data System's (IPEDS) Institutional Characteristics survey in 2013 to create this indicator. For colleges aggregated in a cluster, we assign the cluster the type of the institution with the largest enrollment in that cluster.

Tier. This variable is based on Barron's Educational Series, College Division (2008), and is defined as follows. Tier 1 includes "Ivy Plus" colleges (the eight Ivy League colleges plus Chicago, Duke, MIT, and Stanford). Tier 2 includes all other colleges coded as "Elite" in Barron's. Tier 3 includes highly selective public colleges, while tier 4 includes highly selective private colleges. Tiers 5 and 6 are selective public and private colleges, respectively. Tiers 7 and 8 are nonselective four-year public and private colleges, respectively. Tier 9 includes two-year public and private not-for-profit colleges. Tiers 10 and 11 are private for-profit colleges (four-year and two-year, respectively), and tier 12 includes less than two-year colleges of any kind. In certain Online Data Tables, tier 13 is used to present counts of students attending college with insufficient or incomplete data and tier 14 is used to present counts of students attending college between the ages of 23 and 28 (outside our baseline age range).

SAT Scores. We compute average SAT scores as the mean of the 25th and 75th percentile SAT scores on the math and verbal sections reported by colleges in IPEDS in 2001 and 2013, scaled to 1600. For colleges aggregated in a cluster, we compute this and all other measures below as the enrollment-weighted mean of the variable for the colleges in the cluster.

Graduation Rate. We measure the graduation rate as of the year 2002. This variable comes from the IPEDS Delta Cost Project Database, which is a longitudinal database derived from IPEDS survey data. It measures the percentage of full-time, first-time, degree/certificate-seeking undergraduate students graduating within 150 percent of normal time at four-year and two-year institutions.

Net Cost for Low-Income Students. The net cost for low-income variable is taken from Department of Education's College Scorecard for the year 2013. This variable captures the average net cost of attendance for full-time, first-time degree/certificate seeking undergraduates who receive Title IV aid and are in the bottom quintile of the income distribution ($0-$30,000 family income). Note that this metric is only available in the Scorecard starting in the academic year 2009-10.

Sticker Price. We compute this measure as the sum of tuition for in-state undergraduate full-time, full-year students and in-state undergraduate fees from IPEDS for the academic year 2000-01.

Endowment per Student. We compute the endowment per student by dividing the ending value of endowment assets in 2000, which are taken from IPEDS' Delta Cost Project Database, by the total undergraduate enrollment in the fall of 2000, taken from IPEDS Fall Enrollment survey.

Expenditures per Student. Following the approach of Deming and Walters (2017), we compute the instructional expenditure per student for a college in 2000 as the total expenditure for instruction excluding operations and maintenance and interest for the year divided by the total enrollment in the fall of 2000 using data from IPEDS.

Enrollment. We measure enrollment as the sum of total full-time and part-time undergraduate students enrolled in the fall of 2000 using data from the IPEDS Fall Enrollment survey.

Average Faculty Salary. This variable measures the average salary for full-time faculty members on 9-month equated contracts in the academic year 2001-02, as reported in the IPEDS Delta Cost Project Database.

STEM Major Share. This variable measures the percentage of degrees awarded in communication technologies, computer and information services, engineering, engineering related technologies, biological sciences, mathematics, physical sciences and science technologies in the year 2000, using data from IPEDS.

College Demographics. College-level demographic shares are calculated from the IPEDS Fall Enrollment survey in 2000. The black share is defined as the number of undergraduate students enrolled in a college who are black alone divided by the total undergraduate enrollment. To compute the Hispanic share, we use the number of students of any race who are Hispanic in the numerator instead. For the Asian and Pacific Islander share, the numerator is the number of students who are of Asian origin or have origins in the Pacific Islands.

Average CZ Income. We compute this measure as the population-weighted average household income from the ACS 5-year 2012-2016 estimates. We define household income simply as the household income of those above 25 years old and not living in group quarters.

H. Sensitivity Analysis of Heterogeneity in Earnings Outcomes across Colleges

This appendix explores the robustness of Section IV.B's conclusions about the heterogeneity in earnings outcomes and mobility rates across colleges. First, one may be concerned that the variation in Figure IVa is largely driven by a college's geographic location – for instance, the quality of the local labor market or local price levels. We find that controlling for a college's Commuting Zone (CZ) reduces the standard deviation of mobility rates across colleges from 1.30% to 0.97%. There is substantial variation in earnings and mobility rates even within CZs.

Directly adjusting for differences in local price levels or for local parental income distributions using the norming approach in Section III.B also has small effects on the college-level estimates. For example, the (enrollment-weighted) correlation of our baseline and cost-of-living-adjusted mobility rates is 0.96 (Online Appendix Table IV). Online Appendix Table XX similarly shows that many of the top mobility rate colleges listed in Table IV remain in the top ten when adjusting for the local costs of living. Intuitively, since most children stay in the same area as their parents, differences in price levels move both parents and children up or down in the distribution together, leaving mobility rates unchanged.

One may also be concerned that the use of individual earnings to measure students' incomes might lead us to overstate the heterogeneity in mobility rates across colleges. For instance, if individuals' propensities to participate in the labor force vary across colleges, this would create more variation in observed earnings outcomes than in the underlying earnings potential of students. We address this concern using two approaches. First, we construct separate estimates of mobility rates for male and female students at each college, noting that labor force participation rates are less likely to vary for men. Second, we use household income (AGI) instead of individual earnings to measure students' incomes. The correlation between our baseline estimates of mobility rates and all of these alternative measures exceed 0.92 (Online Appendix Table IV). The colleges that have the highest mobility rates when we focus just on male students or use household earnings also remain very similar (Online Appendix Table XXI). Hence, the broad patterns in mobility rates are not sensitive to using income measures that are less influenced by labor force participation choices.[23]

[23] Mobility rates at certain colleges where a large fraction of female students do not participate in the labor market in their mid-thirties, such as Brigham Young University in Utah, do change significantly when we use these alternative measures.

I. Ecological Bounding Exercise

Table Va found that a college's Asian share is highly correlated with its top-quintile outcome rate and its mobility rate. We bound the degree to which high mobility rates can be explained simply by colleges enrolling a large share of Asian students who would attain top-quintile outcomes regardless of college—i.e., by the ecological (group-level) correlation between Asian share and top-quintile outcome rate.

In Online Appendix Figure X, we present a binned scatter plot of the relationship between the fraction of students who reach the top quintile of the income distribution and Asian shares across colleges.[24] As Asian shares rise from 0% to 5%, the percentage of students who reach the top quintile rises by nearly 15 percentage points (pp). Even if every Asian student ended up in the top quintile of the earnings distribution, the fraction of students in the top quintile would rise by a maximum of 5 pp over this range (a non-parametric upper bound, depicted by the solid line on the figure). Hence, non-Asian students at colleges with larger Asian shares must also have higher top-quintile outcome rates, either because they are also more positively selected or because such colleges have higher value-added.

To gauge the extent to which individual-level differences in top-quintile outcome rates drive the correlation between Asian shares and mobility rates, we use Census data to estimate that Asian students from low-income families have top-quintile outcome rates that are at most 23.5 pp higher than non-Asians.[25] An "Asian-adjusted" measure of top-quintile outcome rates that subtracts 0.235 times the Asian share from the raw top-quintile outcome rate at each college yields mobility rates that have a correlation of more than 0.98 with our baseline estimates.[26] The Asian-adjusted mobility rates continue to have a correlation of 0.43 with Asian shares, implying that most of the baseline correlation of 0.54 between mobility rates and Asian shares is due to ecological factors.

Similarly, although part of the correlation between Black and Hispanic shares and fraction low-income is due to the lower incomes of Hispanics and Blacks themselves, these associations are also partly driven by differences in parental income among other students. For instance, we estimate that a 1 pp increase in the share of Hispanic students is associated with a 0.34 pp increase in the share of students from the bottom quintile using an OLS regression across colleges. But Hispanic parents are only 14.8 pp more likely than non-Hispanics to be in the bottom income quintile (based on the 2003 Current Population Survey), implying that only 43% of the association between Hispanic shares and the fraction of low-income students is explained by the lower incomes of Hispanics themselves.

In sum, differences in the racial and ethnic makeup of colleges' student bodies are highly predictive of their mobility rates. However, these correlations are not just driven by individual-level differences across racial and ethnic groups. Understanding the mechanisms underlying these strong ecological correlations—which could include peer effects, differences in instructional methods at col-

[24]We use the fraction of *all* students who reach the top quintile (rather than the top-quintile outcome rate among students from bottom-quintile families) here because we do not have data on racial shares by income group. This limitation is unlikely to affect our conclusions since the results in Section IV.A show that the fraction of students who reach the top quintile is fairly invariant to parent income within each college.

[25]29% of Asians had earnings in the top income quintile among 30-34 year olds in 2015 (Census Table PINC-01, 1.1.7). Assuming that the distribution of income for Asians (relative to other individuals) is stable across cohorts and that the intergenerational persistence of income is weakly positive, we can infer that at most 29% of Asian students with parents in the bottom quintile reach the top quintile. Given that 7.5% of children born to parents in the bottom quintile in the 1980-82 birth cohorts reach the top quintile on average in the U.S. (Chetty et al. 2014) and that Asians make up 6.5% of children born in these cohorts, it follows that Asian students have top-quintile outcome rates that are at most 23.5 pp higher than non-Asians.

[26]To compute these correlations, we make the conservative assumption that the share of Asian students at each college does not vary across income quintiles, as we do not have data on racial shares by college and income group.

leges that attract certain demographic groups, or selection on unobservables correlated with these demographics—may be a fruitful approach to uncovering why certain colleges have particularly high mobility rates.

J. Methods for Counterfactuals

This appendix provides further details on how we implement the counterfactual allocations of students to colleges and construct counterfactual earnings distributions in Section V.

For the income-neutral allocations counterfactual, we first rank all college students by their SAT/ACT scores, breaking ties with idiosyncratic noise, and record the ranks of the students actually enrolled at each college c, $\overrightarrow{r}_c$. We then re-rank students by their original test scores, *breaking ties with new idiosyncratic noise*, and allocate ranks $\overrightarrow{r}_c$ to college c. By breaking ties with new idiosyncratic noise, we randomly assign students to each college c within test score bins, including students who were not admitted to or did not apply to college c.

The need-affirmative allocations counterfactual employs the same procedure with one change: students are re-ranked by their original SAT/ACT scores plus the increment corresponding to their parent income quintile, as defined in Section V.B. By applying each college's actual test score ranks $\overrightarrow{r}_c$ to the post-test-score-bonus distribution, we effectively re-norm the post-test-score-bonus distribution to align with the actual SAT distribution.

We construct a counterfactual earnings distribution for children at each college based on the observed distribution of earnings for children in each parent income quintile, SAT/ACT score level, and college. Children are randomly assigned the earnings of another child who is observed as attending their counterfactually assigned college and who has the same parent income quintile and SAT/ACT score. For example, suppose that Harvard actually enrolled 10 bottom-quintile children with a 1400 SAT/ACT score and that the counterfactual assigned 30 bottom-quintile children with a 1400 SAT/ACT score to Harvard. Each of those 30 students would be randomly assigned the earnings of one of the 10 actual enrollees. In 1.1% of observations in income-neutral allocations and 1.4% in need-affirmative allocations below, a student is allocated to a college in which no student of the same parent income quintile and same SAT/ACT score actually enrolled (e.g., a bottom-quintile 1400 student is allocated to Harvard but Harvard enrolled no bottom-quintile 1400-scorer in actuality). In those cases, we assign those students a earnings rank of an actually enrolled student from the same parent income quintile with the nearest SAT/ACT score, which is on average 31 points away.

K. Replication of Dale and Krueger (2014)

In this appendix, we show that our data yield estimates of the return to attending a highly selective college that are similar to those estimated by Dale and Krueger (2014), and in particular exhibit higher rates of return to attending more selective colleges for students from lower-income families.

We replicate the key specifications estimated by Dale and Krueger in Online Appendix Table XV. We focus on a set of 31 highly selective colleges in the College and Beyond sample and estimate a set of specifications that parallel those in Columns 3 and 4 in Table 2 of Dale and Krueger (2014).[27] The dependent variable is log earnings, excluding observations with earnings below $13,822 in 2007 dollars ($15,800 in 2015 dollars), as in Dale and Krueger's analysis. The key independent variable is the mean SAT score (divided by 100) of the college in 2001, a proxy for the college's selectivity.

[27] The original College and Beyond sample includes 34 colleges; we do not have data for Morehouse, Tulane, and Williams.

Column 1 presents OLS regression estimates controlling only for $f(S_i)$ and $f(p_q)$, the quintics in test scores and parent income rank. The coefficient of 0.016 implies that a 100 point increase in SAT score increases earnings by 1.6% on average. In Column 2, we add fixed effects for the set of colleges to which students sent their test scores (among the 30 colleges in the sample). The coefficient on mean SAT scores remains similar at 1.2% in this specification, with 1.6% lying in the 95% confidence interval. For comparison, Dale and Krueger (2014, Table 2, Column 2) report an estimate of −0.001 (s.e. 0.012) in an analogous specification in their data. Our estimates – both in the baseline specification and the specification that controls for college fixed effects thus lie within one standard error of their point estimate and hence are not statistically distinguishable from it.

This result should not be taken to mean that colleges have no causal effects: as Dale and Krueger emphasize, there are substantial differences in earnings across colleges that are orthogonal to mean SAT scores within the small set of highly selective colleges they study, even after controlling for selection on observables and unobservables. Moreover, we find much larger differences in earnings outcomes when we consider all colleges, looking beyond the highly selective institutions in the College and Beyond sample.

In Columns 3-7 of Online Appendix Table XV, we investigate how the return to attending a more selective college varies with parental income by replicating the specification in Column 1 by parent income quintile. The coefficients on mean SAT scores decline across the columns as we examine higher income groups: attending a college with 100 point higher average SAT scores is associated with a 4% increase in earnings for students from the bottom quintile, but only a 1% increase in earnings for students from the top quintile. Again, we find similar results using specifications that control for the college application set (not shown). These findings match Dale and Krueger's conclusions that the returns to attending a more selective college are larger for students from low-income families.

L. Relationship between ACT/SAT Scores and Earnings

We follow Dale and Krueger (2002), Hoxby and Avery (2013), and many others in using standardized test scores as a proxy for academic credentials. Test scores are widely used as proxies for academic qualifications because they are widely available and because previous work has shown that they are predictive of later outcomes (e.g., Sackett et al. 2012, Kurlaender and Cohen 2019), although the extent to which that predictive power comes simply from correlations with demographics such as parental income and race is debated (Rothstein 2004). In this appendix, we re-evaluate the relationship between SAT scores and earnings using our longitudinal data. We use our baseline 1980-1982 cohorts sample for this analysis, omitting students who do not take either the SAT or ACT, and rescale ACT to SAT scores as discussed in Appendix E.

The series in circles in Appendix Figure XI presents a binned scatter plot of the relationship between earnings ranks in adulthood (measured at ages 32-34) and test scores. There is a strong positive, linear relationship across the distribution. Column 1 of Appendix Table XXII presents an OLS regression corresponding to this binned scatter plot. We estimate that a 100-point higher SAT score (out of 1600) is associated with earning $6,744 more annually at agess 32-34, or 2.73 percentile ranks higher in the income distribution.

In subsequent columns of Appendix Table XXII, we assess how this relationship changes as we add additional controls for demographic factors. We find that the relationship between SAT scores and earnings falls by about 20% when we control for parental income, race, gender, and high school. The series in triangles in Appendix Figure XI present a binned scatter plot analogous to the specification in Column 4 by replacing the linear SAT score term with 20 bins for SAT scores. It confirms that there is a strong relationship between SAT scores and later earnings even conditional on demographics throughout the test score distribution.

Columns 5-9 examine this relationship within individual colleges. These coefficients must be interpreted with caution, since the college a student attends is endogenous to their SAT scores. Nevertheless, a 100-point difference in SAT scores between students at the same college, and of the same demographic background, still predicts substantial differences in earnings. This relationship holds within each of the selective tiers of colleges.

We conclude that SAT scores provide an informative proxy for qualifications at the point of college application for our purposes in the sense that it predicts earnings above and beyond demographics. We note, however, that our analysis provides no evidence on how standardize test scores compare to other potential proxies for academic credentials, such as high school grades or other forms of assessment, and hence does not speak to the question of whether standardized tests provide good measures of qualifications more broadly.

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ONLINE APPENDIX TABLE I

Counts in Administrative vs. Survey Data by Birth Cohort

Birth Cohort	Size of Birth Cohort Based on Vital Stats.	Number of Citizens in Our Sample	Number of 20 Year Olds in CPS	Number of 20 Year Olds in Our Sample	CPS College Attendees	College Attendees at Age 20 in Our Sample
	(1)	(2)	(3)	(4)	(5)	(6)
1980	3,612	3,189	3,840	3,385	1,839	1,526
1981	3,629	3,403	3,829	3,482	1,845	1,601
1982	3,681	3,493	3,938	3,545	1,998	1,689
1983	3,639	3,470	3,926	3,575	2,009	1,794
1984	3,669	3,664	3,981	3,835	2,030	1,952
1985	3,761	3,776	4,222	3,939	2,187	1,987
1986	3,757	3,764	4,057	3,922	2,022	1,986
1987	3,809	3,836	4,006	4,061	2,078	2,080
1988	3,910	3,960	4,007	4,212	2,147	2,175
1989	4,041	4,103	4,087	4,361	2,254	2,316
1990	4,158	4,227	4,399	4,498	2,389	2,415
1991	4,111	4,178	4,281	4,484	2,433	2,402
1980-1991	45,776	45,062	48,573	47,298	25,231	23,922

Notes: This table compares aggregate counts in our administrative data sample to aggregate counts from the National Vital Statistics System and the Current Population Survey (CPS). All counts are reported in thousands. Column 1 reports the size of the birth cohort according to Vital Statistics in each birth cohort. Column 2 lists the number of citizens in the given birth cohort in our administrative data sample. Values in column 2 can be larger than values in column 1 because Column 1 excludes naturalized citizens. Column 3 reports the number of people in the CPS who are age 20 in each birth cohort. Column 4 reports analogous counts of 20 year olds in our sample of children linked to parents in the tax data. Column 5 reports the number of people enrolled in college at age 20 in each cohort. Column 6 reports analogous counts in our sample. Statistics in Columns 2, 4, and 6 of this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE II

Income Distributions in the Tax Data vs. the American Community Survey

	(1)	(2)	(3)	(4)
Data source	Tax Data	ACS	ACS	ACS
Family Unit	Tax unit	Tax unit	Household	Household
Income definition	Adjusted gross income	Adjusted gross income	Adjusted gross income	Total money income
Panel A. Families of Children aged 15 in 2000				
Count	4,006,698	4,083,218	4,083,218	4,083,218
Mean	93,041	78,456	86,989	91,254
Fraction with zero income	3.4%	5.0%	3.4%	0.4%
10th Percentile	$12,000	$9,634	$15,140	$21,265
25th Percentile	$26,500	$28,903	$35,785	$40,602
Median	$56,750	$59,183	$68,817	$72,809
75th Percentile	$100,500	$99,097	$108,731	$112,860
90th Percentile	$156,750	$151,398	$163,372	$165,161
Fraction with married parents	64.0%	64.9%	64.9%	64.9%
Panel B. Families of Children aged 15 in 2006				
Count	4,531,577	4,347,184	4,347,184	4,347,184
Mean	92,635	80,219	86,662	90,779
Fraction with zero income	4.7%	6.3%	4.7%	1.1%
10th Percentile	$9,500	$7,055	$11,758	$18,225
25th Percentile	$23,250	$27,161	$32,923	$37,626
Median	$50,000	$58,791	$66,434	$70,549
75th Percentile	$96,750	$104,130	$111,702	$114,642
90th Percentile	$158,250	$160,499	$168,141	$170,493
Fraction with married parents	59.5%	62.5%	62.5%	62.5%

Notes: This table compares the parental income distributions of children aged 15 in 2000 (Panel A) and in 2006 (Panel B) in the tax data vs. American Community Survey (ACS) data. Column 1 presents statistics from the tax data, where the unit of observation for family income is the tax unit (married parents or single parent) and income is defined as Adjusted Gross Income, which is pre-tax and pre-transfer cash income. Column 2 replicates this in the ACS data, excluding the few children who are heads of household or spouses of heads of household. For children living with married parents, we sum the income of the two parents (if both parents' incomes are zero, we use instead the income of the head of household as the head would most likely claim the child for tax purposes in this case). For children not living with two married parents, we take the income of the mother if present and non-zero and father if the mother's income is zero or the mother is absent. If both father and mother have zero income or are absent, we define the child's parent as the head of household. Column 3 considers adjusted gross income summed across all household members aged 15 or older (instead of just parents). Column 4 considers total household money income (instead of adjusted gross income). Total money income is the standard income definition used in the ACS and is broader than adjusted gross income, as it includes cash government transfers, retirement and disability benefits. All dollar values are expressed in 2015 dollars, adjusting for inflation using the CPI-U. The counts in the first row are actual counts for the tax data and implied population counts corresponding to the ACS sample based on the sampling weights. Statistics in Column 1 of this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE III

Additional Summary Statistics for Analysis Sample

	Sample		
		College-Goers in Data Release	
	All Children in 1980-82 cohorts	80-82 cohorts only	Including data imputed from 83-84 cohorts
	(1)	(2)	(3)
A. College Attendance Rates			
% Attending College Between Age 19-22	61.83	-	-
% Attending a College in Data Release (based on 80-82 cohorts)	53.07	-	-
% Attending an Ivy-Plus College	0.49	0.95	0.84
% Attending an Other Elite College	1.71	3.31	3.02
% Attending an Other 4-year College	31.59	59.63	58.08
% Attending a 2-Year or Less College	19.28	36.11	38.06
% Not Attending any College by Age 28	26.65	-	-
B. Parents' Household Income (When Child is Aged 15-19)			
Mean Income ($)	87,335	117,080	114,306
Median Income ($)	59,100	77,100	N/A
% with Parents in Bottom 20%	20.00	10.63	11.12
% with Parents in Top 20%	20.00	30.93	29.92
% with Parents in Top 1%	1.00	1.70	1.62
C. Children's Individual Earnings (in 2014, Ages 32-34)			
Mean Earnings ($)	35,526	47,048	46,179
Median Earnings ($)	26,900	35,800	N/A
% Employed	81.68	88.72	88.60
% in Top 20%	20.00	29.66	28.87
% in Top 1%	1.00	1.73	1.63
% in Top 20% | Parents in Bottom 20%	8.65	18.33	17.44
% in Top 1% | Parents in Bottom 20%	0.22	1.00	0.92
% in Top 20% and Parents in Bottom 20%	1.73	1.95	1.94
% in Top 1% and Parents in Bottom 20%	0.04	0.07	0.06
Number of Children	10,757,269	5,535,694	6,244,162
Percentage of College Students Covered	-	83.2%	93.9%

Notes: The table presents additional summary statistics. Column 1 includes all children in the 1980-82 birth cohorts and replicates Column 1 of Table I. Column 2 limits this sample to students who attend a college (between the ages of 19-22) that is included in the public data release using data purely from the 1980-82 birth cohorts. This is the set of colleges for which we observe a sufficient number of students and have complete attendance records for the 1980-82 cohorts, as described in Section II and Online Appendix B. Column 3 adds imputed data from the 1983-84 birth cohorts for colleges with insufficient data in the 1980-82 birth cohorts (see Section II.F for details), replicating Column 2 of Table I. This is the sample used for most of our analyses. See notes to Table I for definitions. Statistics in Column 1 are constructed based on Online Data Table 6 and statistics in Columns 2 and 3 are based on Online Data Table 2, with the exception of median income and earnings, which are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE IV

Sensitivity of Key Intergenerational Mobility Statistics to Alternative Definitions

	(1) Baseline	(2) Excluding Clusters of Colleges	(3) Sons	(4) Daughters	(5) Household Earnings	(6) Household Income	(7) Income Adjusted for Local Prices	(8) College at Age 20	(9) First college by Age 28
A. Key Descriptive Statistics									
Standard Deviation of Fraction Low-Income	7.59	7.68	6.84	8.25	7.59	7.60	8.43	7.15	8.59
Percentage of Parents from Top 1% at Ivy-Plus colleges	14.52	14.52	14.60	14.43	14.52	14.52	12.95	14.51	14.38
Rank-Rank Slope Within All Colleges	0.10	0.10	0.12	0.06	0.14	0.15	0.10	0.09	0.11
Rank-Rank Slope Within Elite Colleges	0.07	0.06	0.09	0.04	0.11	0.13	0.06	0.06	0.07
SD of Bottom-to-Top Quintile Mobility Rate	1.30	1.35	1.54	1.20	0.99	0.97	1.25	1.36	1.28
B. Correlation of College-Level Statistics with Baseline Estimates									
Correlation with Baseline Mobility Rate			0.94	0.93	0.93	0.92	0.96	0.99	0.98
Correlation with Baseline Upper-Tail Mobility Rate			0.93	0.86	0.86	0.83	0.91	0.98	0.94
Correlation with Baseline Top-Quintile Outcome Rate			0.95	0.96	0.94	0.93	0.86	0.99	0.98
Correlation with Baseline Top-1% Outcome Rate			0.94	0.88	0.90	0.87	0.89	0.98	0.96
Correlation with Baseline Fraction Low-Income			0.99	0.99	1.00	1.00	0.92	0.99	0.99

Notes: This table replicates the main results reported in the paper using alternative subsamples (columns 2-4), alternative child income definitions (columns 5-7), and alternative definitions of college attendance (columns 8-9). All statistics reported are based on the analysis sample (primarily the 1980-82 birth cohorts; see Section II for details). Column 1 replicates statistics reported for the baseline definitions and sample as a reference. Column 2 excludes colleges that cannot be individually identified and are grouped into "Super OPEIDs" (see Online Appendix B). Columns 3 and 4 divide the main sample into male and female children, respectively. In columns 5 and 6, we use household earnings (wage earnings plus self-employment income) and household income (AGI) instead of individual earnings to define children's ranks. In Column 7, we compute parents' and children's ranks after deflating incomes by a local cost-of-living price index based on their locations when their incomes are measured. In Column 8, children are assigned to colleges based on the college they attend at age 20; those who do not attend college at age 20 are excluded. In Column 9, they are assigned to the first college they attend before age 28. Columns 8 and 9 use the baseline income definitions. Panel A reports key descriptive statistics discussed in the main text. The standard deviation (SD) of fraction low-income is the enrollment-weighted standard deviation of the fraction of parents in the bottom income quintile across colleges. Rank-rank slopes are the coefficients from a regression of child income rank on parent income rank with college fixed effects, as in Panels D-G of Table III. The SD of the mobility rate is the enrollment-weighted SD of the fraction of students who have parents in the bottom quintile and who are in the top quintile themselves. Panel B reports enrollment-weighted correlations between the baseline estimates and the alternative estimates for the key college-level statistics reported in Table II; see notes to Table II for definitions of these variables. See Section II for further details regarding income and college definitions. Statistics in this table are constructed based on Online Data Tables 2 and 4.

ONLINE APPENDIX TABLE V

Income Segregation across Colleges vs. Pre-College Neighborhoods

A. Income Segregation across Pre-College Residential Neighborhoods (ZIP Codes)

	Fraction of residential ZIP-code peers from each parental income group..					
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%	Top 1%
	(1)	(2)	(3)	(4)	(5)	(6)
..for children from						
Bottom 20%	29.7%	24.1%	19.1%	15.5%	11.5%	0.5%
Quintile 2	24.1%	23.0%	20.6%	18.1%	14.2%	0.6%
Quintile 3	19.1%	20.6%	21.7%	21.0%	17.5%	0.7%
Quintile 4	15.5%	18.1%	21.0%	23.3%	22.2%	0.9%
Top 20%	11.5%	14.2%	17.5%	22.2%	34.5%	2.4%
Top 1%	9.9%	11.5%	13.8%	17.6%	47.2%	7.3%

B. Income Segregation across Colleges

	Fraction of college peers from each parental income group..					
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%	Top 1%
	(7)	(8)	(9)	(10)	(11)	(12)
..for children from						
Bottom 20%	26.8%	23.9%	20.6%	17.0%	11.8%	0.4%
Quintile 2	23.9%	22.5%	20.7%	18.4%	14.6%	0.6%
Quintile 3	20.6%	20.7%	20.6%	20.2%	17.9%	0.7%
Quintile 4	17.0%	18.4%	20.2%	22.0%	22.5%	1.0%
Top 20%	11.8%	14.6%	17.9%	22.5%	33.3%	2.3%
Top 1%	8.2%	11.1%	14.7%	20.2%	45.9%	5.6%

Notes: This table presents parental income segregation measures across the neighborhoods (ZIP codes) where children lived before college in Panel A and across colleges in Panel B. The sample includes all children in our analysis sample (1980-82 birth cohorts), pooling non-college goers into a single group in Panel B. Each row corresponds to a group of children based on their own parents' income. For each row, each column reports the average composition of peers in the 1980-82 birth cohorts (in the same ZIP code in Panel A, in the same college in Panel B) using the same parent income quintile groups in columns 1-5 and the top 1% group in column 6. Peer composition is computed using leave-out means. The first five columns and the first five rows each sum to 100% by definition in each panel. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE VI

Income Segregation across Colleges vs. Pre-College Neighborhoods for Ivy-Plus Students

A. Income Segregation across Pre-College Residential Neighborhoods (ZIP Codes)

	Fraction of residential ZIP-code from each income group..					
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%	Top 1%
	(1)	(2)	(3)	(4)	(5)	(6)
..for children from						
Bottom 20%	24.4%	20.2%	16.8%	15.1%	23.5%	2.3%
Quintile 2	19.5%	19.3%	17.8%	17.6%	25.9%	2.2%
Quintile 3	16.4%	17.7%	18.0%	19.0%	29.0%	2.4%
Quintile 4	13.7%	15.7%	17.3%	20.0%	33.3%	2.8%
Top 20%	9.7%	11.5%	13.5%	16.9%	48.5%	7.1%
Top 1%	9.1%	10.1%	11.5%	14.1%	55.2%	12.1%

B. Income Segregation across Colleges

	Fraction of college peers from each income group..					
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%	Top 1%
	(7)	(8)	(9)	(10)	(11)	(12)
..for children from						
Bottom 20%	3.9%	6.0%	9.0%	13.7%	67.4%	13.7%
Quintile 2	3.9%	5.9%	9.0%	13.7%	67.5%	13.8%
Quintile 3	3.9%	5.9%	9.0%	13.7%	67.5%	13.9%
Quintile 4	3.8%	5.9%	8.9%	13.6%	67.8%	14.1%
Top 20%	3.7%	5.7%	8.6%	13.3%	68.7%	14.8%
Top 1%	3.5%	5.5%	8.4%	13.0%	69.7%	15.6%

Notes: This table replicates Appendix Table V for the subset of children in the analysis sample who attend Ivy-Plus colleges. See notes to Appendix Table V for details. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE VII

Income Segregation across Colleges vs. Pre-College Neighborhoods for College Goers

A. Income Segregation across Pre-college Neighborhoods (ZIP Code)

	Fraction of residential ZIP-code peers from each parental income group..					
	(1)	(2)	(3)	(4)	(5)	(6)
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%	Top 1%
..for children from						
Bottom 20%	27.5%	23.2%	19.3%	16.3%	13.7%	0.6%
Quintile 2	22.5%	22.0%	20.5%	18.7%	16.3%	0.7%
Quintile 3	18.1%	19.9%	21.4%	21.3%	19.3%	0.8%
Quintile 4	14.9%	17.6%	20.7%	23.3%	23.4%	1.0%
Top 20%	11.2%	13.9%	17.2%	22.0%	35.6%	2.5%
Top 1%	9.6%	11.3%	13.6%	17.4%	48.1%	7.5%

B. Income Segregation across Colleges

	Fraction of college peers from each parental income group..					
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%	Top 1%
..for children from						
Bottom 20%	15.7%	18.6%	20.6%	22.6%	22.5%	0.9%
Quintile 2	13.5%	17.3%	20.6%	23.8%	24.8%	1.0%
Quintile 3	11.5%	15.9%	20.5%	24.9%	27.2%	1.2%
Quintile 4	10.0%	14.5%	19.7%	25.6%	30.2%	1.4%
Top 20%	7.9%	12.1%	17.1%	24.0%	38.8%	2.7%
Top 1%	5.8%	9.3%	14.0%	20.7%	50.2%	6.3%

C. Income Segregation across Colleges Using Normed Quintiles

	Fraction of college peers from each parental income group..				
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%
..for children from					
Bottom 20%	14.9%	18.1%	20.9%	23.2%	22.9%
Quintile 2	13.6%	17.2%	20.6%	23.8%	24.8%
Quintile 3	12.3%	16.2%	20.2%	24.4%	26.9%
Quintile 4	11.0%	15.0%	19.6%	24.7%	29.6%
Top 20%	8.7%	12.5%	17.3%	23.7%	37.9%

Notes: Panels A and B of this table replicate Appendix Table V, restricting the sample to college goers. See notes to Appendix Table V for details. Panel C repeats Panel B using normed parent income shares at each college. To construct these normed shares, we norm each college's parent income quintile shares by the parent income quintile shares of its pool of potential students. We assume that most elite colleges (i.e., the top two selectivity tiers excluding public colleges from tier 2) draw students from a nationwide pool, the remaining selective colleges (i.e., the next four tiers and tier 2 public colleges) draw students from a state-specific pool, and unselective colleges (i.e., tiers 7-12) draw students from their local Commuting Zone. We construct locally normed measures by first dividing each college's parent income quintile shares by the parent income quintile shares of its potential pool of students. For each college, we then divide these five values by the sum of the five values so that the final normed shares sum to 1. The resulting statistics can be interpreted as the parental income distributions that would arise at each college if every college had the same (national) pool of applicants. Statistics in Panels A and B of this table are constructed directly from the individual-level microdata and in Panel C based on Online Data Table 14.

ONLINE APPENDIX TABLE VIII

Sensitivity of Mobility Rate to Alternative Definitions

Alternative Measure of Mobility Rate	Correlation with Baseline Mobility Rate
Mobility Rate Adjusted for Non-College Top-Quintile Outcome Rate	0.98
Percent of Students who start in Bottom 20% and end up in Top 40%	0.87
Percent of Students who start in Bottom 40% and end up in Top 40%	0.85
Percent of Students who moved up Two or More Income Quintiles	0.82

Notes: This table presents enrollment-weighted correlations between alternative measures of colleges' mobility rates and our baseline mobility rate estimates. In the Mobility Rate Adjusted for Non-College Top-Quintile Outcome Rate measure, we first define each college's adjusted top-quintile outcome rate as its observed top-quintile outcome rate (the fraction of children who reach the top quintile conditional on having parents in the bottom quintile) minus 3.9%, which is the top-quintile outcome rate of those who do not attend college by age 28. The adjusted mobility rate is then computed as the product of the adjusted top-quintile outcome rate and the share of children at a college with parents in the bottom quintile of the income distribution (fraction low-income). The Percent of Students who start in the Bottom 20% and end up in the Top 40% measure is the share of students whose parents were in the bottom quintile of the income distribution and whose own earnings are in the top two quintiles in adulthood. The Percent of Students who start in the Bottom 40% and end up in the Top 40% measure is defined analogously. The Percent of Students who moved up Two or More Income Quintiles is the fraction of students whose own incomes placed them two or more quintiles above their parents' income quintiles. Each of the alternative measures is constructed using our analysis sample (primarily the 1980-82 birth cohorts), as is our baseline measure. As in our baseline analysis, children are ranked based on their individual earnings relative to other children in the same birth cohort in all measures and parents, and are ranked based on their household income relative to other parents with children in the same cohort. Statistics in this table are constructed based on Online Data Table 2.

ONLINE APPENDIX TABLE IX
Parent Income Distributions by SAT/ACT Score for College Students

	Parent Income Quintile					Share of all college goers
	1	2	3	4	5	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Share of SAT/ACT Bin from Each Parent Income Quintile						
1500-1600	2.5%	4.7%	8.8%	16.9%	67.2%	0.6%
1400-1490	3.2%	6.4%	12.4%	21.1%	56.8%	2.3%
1300-1390	4.0%	7.7%	14.2%	23.3%	50.9%	5.3%
1200-1290	5.0%	9.4%	16.3%	25.1%	44.2%	10.4%
1100-1190	6.4%	11.4%	18.5%	26.4%	37.4%	16.7%
1000-1090	8.5%	13.4%	19.9%	26.6%	31.7%	19.8%
900-990	11.3%	16.1%	21.0%	25.7%	25.8%	19.2%
800-890	15.4%	19.3%	21.6%	23.4%	20.2%	13.7%
700-790	21.0%	23.1%	21.1%	20.0%	14.8%	7.7%
600-690	25.8%	26.1%	20.3%	16.8%	11.1%	3.1%
500-590	30.8%	28.0%	18.9%	13.9%	8.5%	1.1%
400-490	34.5%	28.3%	18.7%	11.1%	7.4%	0.2%
B. Share of Cumulative SAT/ACT Bin from Each Parent Income Quintile						
≥1500	2.5%	4.7%	8.8%	16.9%	67.2%	0.6%
≥1400	3.1%	6.1%	11.7%	20.3%	58.9%	2.9%
≥1300	3.7%	7.1%	13.3%	22.2%	53.7%	8.2%
≥1200	4.4%	8.4%	15.0%	23.8%	48.4%	18.5%
≥1100	5.3%	9.8%	16.6%	25.0%	43.2%	35.3%
≥1000	6.5%	11.1%	17.8%	25.6%	39.0%	55.0%
≥900	7.7%	12.4%	18.6%	25.6%	35.6%	74.2%
≥800	8.9%	13.5%	19.1%	25.3%	33.2%	87.9%
≥700	9.9%	14.3%	19.3%	24.8%	31.7%	95.6%
≥600	10.4%	14.6%	19.3%	24.6%	31.1%	98.7%
≥500	10.6%	14.8%	19.3%	24.5%	30.8%	99.8%
≥400	10.7%	14.8%	19.3%	24.4%	30.8%	100.0%

Notes: Panel A reports the parent income distribution by SAT/ACT score bin among college goers in our analysis sample. SAT scores for 47.6% of college goers are obtained directly from the College Board; ACT scores for another 26.2% of college goers are obtained from ACT and converted to an SAT score. We impute an SAT/ACT score for the other 26.2% of college-goers using the SAT/ACT score of the student from the same parent income quintile, same pre-college state, and same college tier with the nearest child earnings and non-missing race. Each cell of Columns 1-5 reports the share of students in a given SAT/ACT bin who have parents in the parent income quintile defined in the column heading. The sixth column reports the total share of college goers who fall into the corresponding SAT/ACT bin. Panel B weights the distributional statistics in Columns 1-5 by the overall college-goer shares reported in Column 6 to compute the joint cumulative distribution function of SAT/ACT scores and parent income. For example, Panel B reports that 3.6% of college goers with an SAT/ACT score of at least 1300 have bottom-quintile parents. Online Appendix Table X shows that similar results are obtained when excluding college goers with an imputed SAT/ACT score, while Online Appendix Table XI shows that similar results are obtained using data from the National Postsecondary Student Aid Study. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE X

Parent Income Distributions by SAT/ACT Score, Excluding Students with Imputed SAT/ACT Scores

	Parent Income Quintile					Share of all college goers
	1	2	3	4	5	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Share of SAT/ACT Bin from Each Parent Income Quintile						
1500-1600	2.2%	4.4%	8.4%	16.8%	68.1%	0.8%
1400-1490	2.8%	5.9%	11.8%	20.9%	58.6%	2.9%
1300-1390	3.3%	6.9%	13.5%	23.1%	53.2%	6.5%
1200-1290	4.0%	8.2%	15.3%	25.1%	47.4%	12.1%
1100-1190	5.0%	9.9%	17.3%	26.7%	41.1%	18.4%
1000-1090	6.6%	11.6%	18.8%	27.2%	35.8%	20.3%
900-990	8.8%	14.1%	20.2%	26.9%	30.0%	18.2%
800-890	12.2%	17.2%	21.1%	25.2%	24.2%	11.8%
700-790	16.8%	21.1%	21.3%	22.3%	18.6%	6.0%
600-690	20.7%	24.2%	20.9%	19.5%	14.8%	2.2%
500-590	25.2%	26.3%	20.0%	16.8%	11.8%	0.7%
400-490	27.7%	27.8%	19.8%	13.9%	10.8%	0.1%
B. Share of Cumulative SAT/ACT Bin from Each Parent Income Quintile						
≥1500	2.2%	4.4%	8.4%	16.8%	68.1%	0.8%
≥1400	2.7%	5.6%	11.1%	20.1%	60.5%	3.6%
≥1300	3.1%	6.4%	12.6%	22.0%	55.9%	10.2%
≥1200	3.6%	7.4%	14.1%	23.7%	51.2%	22.3%
≥1100	4.2%	8.5%	15.5%	25.1%	46.6%	40.6%
≥1000	5.0%	9.6%	16.6%	25.8%	43.0%	60.9%
≥900	5.9%	10.6%	17.4%	26.0%	40.0%	79.2%
≥800	6.7%	11.5%	17.9%	25.9%	38.0%	91.0%
≥700	7.3%	12.1%	18.1%	25.7%	36.8%	97.0%
≥600	7.6%	12.3%	18.2%	25.6%	36.3%	99.2%
≥500	7.8%	12.4%	18.2%	25.5%	36.1%	99.9%
≥400	7.8%	12.4%	18.2%	25.5%	36.1%	100.0%

Notes: The table replicates Online Appendix Table IX, omitting college goers with an imputed SAT/ACT score. See the notes to that table for details. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XI

Parent Income Distributions by SAT/ACT Score Using NPSAS Data

	Parent Income Quintile					Share of all college goers
	1	2	3	4	5	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Share of SAT/ACT Bin from Each Parent Income Quintile						
1500-1600	2.6%	6.8%	16.1%	20.4%	54.0%	0.9%
1400-1490	5.1%	7.2%	12.9%	27.0%	47.9%	3.1%
1300-1390	3.7%	7.1%	14.5%	25.2%	49.4%	7.2%
1200-1290	5.9%	11.1%	16.1%	25.7%	41.2%	12.2%
1100-1190	7.1%	13.1%	18.1%	26.1%	35.6%	18.4%
1000-1090	8.3%	13.1%	22.0%	26.4%	30.2%	20.3%
900-990	15.8%	19.6%	20.2%	21.9%	22.4%	19.8%
800-890	18.0%	19.8%	22.8%	23.1%	16.3%	11.5%
700-790	20.1%	22.6%	19.6%	19.5%	18.1%	5.2%
600-690	18.8%	18.7%	23.2%	20.2%	19.0%	1.2%
500-590	22.4%	21.7%	11.7%	24.2%	20.0%	0.3%
400-490	44.8%	17.4%	4.7%	33.1%	0.0%	0.0%
B. Share of Cumulative SAT/ACT Bin from Each Parent Income Quintile						
≥1500	2.6%	6.8%	16.1%	20.4%	54.0%	0.9%
≥1400	4.5%	7.1%	13.6%	25.6%	49.2%	4.0%
≥1300	4.0%	7.1%	14.2%	25.4%	49.4%	11.2%
≥1200	5.0%	9.2%	15.2%	25.5%	45.1%	23.4%
≥1100	5.9%	10.9%	16.5%	25.8%	40.9%	41.8%
≥1000	6.7%	11.6%	18.3%	26.0%	37.4%	62.1%
≥900	8.9%	13.6%	18.7%	25.0%	33.8%	81.9%
≥800	10.0%	14.3%	19.2%	24.8%	31.7%	93.4%
≥700	10.6%	14.8%	19.3%	24.5%	30.9%	98.5%
≥600	10.7%	14.8%	19.3%	24.4%	30.8%	99.7%
≥500	10.7%	14.8%	19.3%	24.4%	30.8%	100.0%
≥400	10.7%	14.8%	19.3%	24.4%	30.8%	100.0%

Notes: This table replicates Online Appendix Table IX using data from the National Postsecondary Student Aid Study (NPSAS) instead of the tax data. The NPSAS contains coarse information on college-goers' parent income and SAT or ACT score. To overcome this problem, we norm NPSAS parent income to match the true distribution of college-goers' parent income quintiles from our analysis sample in the tax data, convert ACT scores to SAT scores, and use parent income and tier to impute missing SAT/ACT scores. Specifically, we use information gleaned from FAFSA AGI and survey questions to generate an observed distribution of parent income within tier and SAT/ACT quartile or missing SAT/ACT score. We then randomly assign incomes to students with unobserved parent income to match the observed distribution within these cells. Next, we assign parental income quintiles to this NPSAS income variable such that the quintile distribution matches that from our main analysis sample. Finally, we impute missing SAT/ACT scores such that the distribution of observed SAT/ACT scores within parent income quintile and tier is preserved. See the notes to Online Appendix Table IX for details on the statistics reported in this table. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XII
Ivy-Plus Attendance Rate by Parent Income Group and SAT/ACT Score

A. Share attending an Ivy-Plus college with SAT/ACT score…

	1200	1300	1400	1500	1600
Bottom 20%	0.8%	2.8%	7.3%	21.4%	60.0%
Quintile 2	0.7%	2.2%	4.7%	19.3%	43.9%
Quintile 3	0.7%	2.0%	4.5%	14.5%	54.3%
Quintile 4	0.5%	1.7%	4.4%	15.6%	41.7%
Top 20%	1.0%	3.8%	10.8%	30.9%	60.5%
P80-P90	0.6%	2.0%	5.5%	17.0%	46.0%
P90-P95	0.9%	2.9%	8.4%	24.9%	50.0%
P95-P99	1.4%	5.0%	14.6%	38.8%	71.0%
P99-P100	2.6%	10.0%	26.2%	53.8%	76.7%

B. Share attending an Ivy-Plus college within SAT/ACT score range…

	1200-1290	1300-1390	1400-1490	1500-1590	1600
Bottom 20%	1.3%	3.9%	11.3%	29.7%	60.0%
Quintile 2	1.1%	3.2%	8.6%	27.3%	43.9%
Quintile 3	1.0%	2.8%	7.6%	22.1%	54.3%
Quintile 4	0.8%	2.6%	7.2%	23.5%	41.7%
Top 20%	1.7%	5.9%	16.4%	38.8%	60.5%
P80-P90	0.9%	3.1%	8.7%	24.6%	46.0%
P90-P95	1.3%	4.5%	12.6%	32.4%	50.0%
P95-P99	2.4%	8.0%	21.5%	45.3%	71.0%
P99-P100	4.8%	14.6%	34.4%	60.7%	76.7%

C. Share attending an Ivy-Plus college with at least SAT/ACT score…

	1200+	1300+	1400+	1500+	1600
Bottom 20%	3.4%	7.0%	14.5%	31.0%	60.0%
Quintile 2	2.9%	5.7%	11.7%	27.9%	43.9%
Quintile 3	2.6%	5.0%	10.0%	23.5%	54.3%
Quintile 4	2.5%	5.0%	10.1%	24.4%	41.7%
Top 20%	6.7%	12.0%	21.9%	40.0%	60.5%
P80-P90	3.3%	6.2%	12.0%	25.7%	46.0%
P90-P95	5.2%	9.3%	17.3%	33.4%	50.0%
P95-P99	9.8%	16.3%	28.0%	46.8%	71.0%
P99-P100	17.4%	26.8%	42.0%	61.6%	76.7%

Notes: This table shows the Ivy-Plus attendance rate for college-goers in our analysis sample by parent income group and SAT/ACT score. In addition to parent income quintiles, the top 20% is broken down further into 80-90th percentiles, 90-95th percentiles, 95-99th percentiles, and the top 1%. Panel A reports the Ivy-Plus attendance rate for individual SAT/ACT scores. For example, of all college-goers with parents in the bottom 20% and with a 1600 SAT/ACT score, 60.0% attended an Ivy-Plus college. Panel B repeats Panel A for 100-point SAT/ACT score ranges. Panel C reports the Ivy-Plus attendance rate for those with at least a certain SAT/ACT score. See notes to Online Appendix Table IX for details about SAT/ACT scores and imputation. See Table I for a definition of Ivy-Plus colleges. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XIII

Counterfactual Income Segregation across Colleges for College-Goers

A. Actual Income Segregation across Colleges

	(1)	(2)	(3)	(4)	(5)
	Fraction of college peers from each income group..				
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%
..for children from					
Bottom 20%	15.7%	18.6%	20.6%	22.6%	22.5%
Quintile 2	13.5%	17.3%	20.6%	23.8%	24.8%
Quintile 3	11.5%	15.9%	20.5%	24.9%	27.2%
Quintile 4	10.0%	14.5%	19.7%	25.6%	30.2%
Top 20%	7.9%	12.1%	17.1%	24.0%	38.8%

B. Income-Neutral Student Allocations

	Fraction of college peers from each income group..				
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%
..for children from					
Bottom 20%	12.8%	16.5%	19.5%	23.3%	27.8%
Quintile 2	11.9%	15.8%	19.6%	23.9%	28.8%
Quintile 3	10.8%	15.0%	19.6%	24.6%	30.0%
Quintile 4	10.2%	14.5%	19.4%	25.0%	30.9%
Top 20%	9.6%	13.9%	18.8%	24.6%	33.2%

C. Need-Affirmative Student Allocations

	Fraction of college peers from each income group..				
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%
..for children from					
Bottom 20%	12.2%	15.9%	19.2%	23.3%	29.3%
Quintile 2	11.4%	15.4%	19.4%	23.9%	29.8%
Quintile 3	10.6%	14.9%	19.5%	24.6%	30.4%
Quintile 4	10.2%	14.5%	19.4%	24.9%	31.0%
Top 20%	10.1%	14.4%	19.1%	24.6%	31.8%

Notes: Panel A reprints Online Appendix Table VIIb; see the notes to that table for details. Panels B and C replicate Panel A under the two counterfactuals discussed in the notes to Table VI. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XIV

Parental Income Distributions Under Counterfactual Student Allocation Rules without Racial and Geographic Constraints

	Parent Income Quintile					Share of all college goers
	1 (Bottom 20%)	2	3	4	5 (Top 20%)	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Actual Distributions						
Ivy-Plus	3.8%	5.7%	8.7%	13.4%	68.4%	0.9%
Other elite colleges	4.3%	6.8%	10.2%	15.8%	62.8%	3.3%
Highly selective public	5.5%	9.2%	14.3%	23.4%	47.6%	7.0%
Highly selective private	4.1%	7.6%	12.2%	19.7%	56.5%	2.4%
Selective public	8.4%	12.9%	18.6%	26.1%	34.1%	34.4%
Selective private	7.1%	12.0%	18.2%	25.5%	37.2%	8.6%
Nonselective 4-year public	17.0%	20.4%	22.1%	22.7%	17.7%	4.6%
Nonselective 4-year private non-profit	10.7%	14.7%	19.8%	24.6%	30.2%	1.0%
2-year public and non-profit	14.6%	18.6%	22.2%	24.7%	19.9%	35.5%
4-year for-profit	17.8%	22.3%	22.5%	21.1%	16.3%	1.7%
2-year for-profit	21.5%	23.9%	23.1%	19.5%	12.0%	0.7%
Less than two-year colleges	20.7%	23.2%	21.3%	21.0%	13.8%	0.2%
All colleges	10.7%	14.8%	19.3%	24.4%	30.8%	100.0%
Underrepresentation in selective tiers	31.3%	21.6%	11.4%	-0.6%	-27.9%	
B. Counterfactual Distributions Under Income-Neutral Student Allocations						
Ivy-Plus	3.8%	7.1%	12.6%	21.6%	54.9%	0.9%
Other elite colleges	5.1%	9.2%	15.4%	24.2%	46.1%	3.3%
Highly selective public	7.1%	11.4%	17.6%	25.0%	38.9%	7.0%
Highly selective private	6.6%	11.0%	17.7%	25.1%	39.6%	2.4%
Selective public	9.5%	14.0%	19.2%	25.0%	32.3%	34.4%
Selective private	9.2%	13.5%	18.9%	24.9%	33.5%	8.6%
Nonselective 4-year public	13.1%	16.9%	20.4%	23.9%	25.7%	4.6%
Nonselective 4-year private non-profit	10.5%	14.9%	19.4%	24.8%	30.3%	1.0%
2-year public and non-profit	13.2%	17.1%	20.2%	23.9%	25.6%	35.5%
4-year for-profit	13.2%	17.2%	20.0%	23.9%	25.6%	1.7%
2-year for-profit	16.0%	19.1%	20.4%	22.5%	21.9%	0.7%
Less than two-year colleges	14.2%	18.3%	20.2%	23.1%	24.1%	0.2%
All colleges	10.7%	14.8%	19.3%	24.4%	30.8%	100.0%
Underrepresentation in selective tiers	18.5%	11.7%	3.7%	-1.9%	-12.9%	
C. Counterfactual Distributions Under Need-Affirmative Student Allocations						
Ivy-Plus	11.3%	16.8%	19.9%	23.9%	28.0%	0.9%
Other elite colleges	10.3%	14.7%	19.6%	24.0%	31.4%	3.3%
Highly selective public	10.1%	14.5%	19.3%	24.9%	31.2%	7.0%
Highly selective private	9.9%	14.5%	19.3%	24.9%	31.4%	2.4%
Selective public	10.4%	14.6%	19.3%	24.8%	30.8%	34.4%
Selective private	10.4%	14.6%	19.3%	24.7%	31.0%	8.6%
Nonselective 4-year public	11.1%	15.1%	19.2%	24.1%	30.6%	4.6%
Nonselective 4-year private non-profit	10.7%	14.7%	18.8%	24.8%	31.0%	1.0%
2-year public and non-profit	11.0%	15.0%	19.2%	24.1%	30.7%	35.5%
4-year for-profit	10.9%	15.3%	19.0%	24.0%	30.7%	1.7%
2-year for-profit	11.4%	15.3%	19.0%	23.5%	30.8%	0.7%
Less than two-year colleges	11.5%	14.9%	18.8%	24.2%	30.6%	0.2%
All colleges	10.7%	14.8%	19.3%	24.4%	30.8%	100.0%
Underrepresentation in selective tiers	2.7%	1.0%	-0.3%	-1.2%	-0.3%	

Notes: This table replicates Table VI, but reallocates students to colleges randomly conditional on their SAT/ACT scores (or adjusted SAT/ACT scores), without holding fixed the racial composition or pre-college-state distribution of the student body. See notes to Table VI for details. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XV

Replication of Specifications in Dale and Krueger (2014)

Dep. Var.: Log earnings	All Quintiles	All Quintiles, College Application Set FEs	Bottom Quintile Only	Quintile 2 Only	Quintile 3 Only	Quintile 4 Only	Top Quintile Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average SAT of College Attended	0.016	0.012	0.038	0.032	0.025	0.016	0.010
	(0.002)	(0.003)	(0.009)	(0.007)	(0.005)	(0.004)	(0.003)
Adj. R-squared	0.060	0.099	0.079	0.072	0.062	0.052	0.025

Notes: This table replicates specifications in Dale and Krueger (2014) estimating the relationship between students' earnings outcomes and college selectivity, as measured by students' average SAT scores. It uses the sample of students attending one of the 31 colleges for which we have data in the College and Beyond Survey used in Dale and Krueger (2002, 2014), restricting to students with earnings that exceed $13,822 in 2007 dollars ($15,800 in 2015 dollars). Column 1 reports estimates from a regression of log earnings on the average SAT score of college attended, controlling for a quintic in parent income rank and a quintic in SAT. Column 2 adds fixed effects for the exact set of these 31 schools that each student sent scores to. Columns 3-7 replicate Column 1 using only children from the parent income quintile specified in the column title. Robust standard errors are in parentheses. Statistics in this table are constructed directly from the individual-level microdata. See Online Appendix K for further details.

ONLINE APPENDIX TABLE XVI

Counterfactual Gaps in Mobility Rates: Sensitivity to Assumptions about Colleges' Causal Effects

Assumed causal share of differences in earnings across colleges, conditional on SAT/ACT scores and parent income (%)	Increment to the SAT Scores of Bottom-Quintile College-Goers				
	0	100	160	200	300
100	18.2%	26.7%	33.1%	37.6%	46.2%
90	16.4%	24.1%	29.8%	33.8%	41.7%
80 (baseline)	14.6%	21.4%	26.5%	30.1%	37.1%
70	12.8%	18.8%	23.2%	26.4%	32.5%
60	11.0%	16.1%	20.0%	22.7%	27.9%
50	9.3%	13.5%	16.7%	18.9%	23.3%
40	7.5%	10.9%	13.4%	15.2%	18.7%
30	5.7%	8.2%	10.1%	11.5%	14.1%
20	3.9%	5.6%	6.9%	7.8%	9.5%
10	2.1%	3.0%	3.6%	4.0%	4.9%
0	0.3%	0.3%	0.3%	0.3%	0.3%

Notes: Consider the difference between the fraction of college students with parents in the bottom vs. top quintile who reach the top earnings quintile. Each cell reports the fraction of this gap that is closed under alternative assumptions about colleges' student allocations rules (columns) and colleges' causal effects (rows). The columns vary the increment to the SAT/ACT scores of bottom-quintile college-goers, with smaller proportional increments to second-quintile (80% of the bottom-quintile constant), third-quintile (60%), and fourth-quintile (40%) college-goers. The first column (0 addition) corresponds to the income-neutral student allocations counterfactual reported in Table VIII; the third column (160 points) corresponds to the need-affirmative student allocations counterfactual. The rows vary the share of the differences in earnings ranks across colleges conditional on SAT/ACT scores and parental income quintile that are assumed to be causal (λ). The estimates in the third row assume that λ=80% and replicate the analysis in Table VIII. In the remaining rows, for each assumed causal share λ, we report a weighted average with θ weight on the counterfactual earnings distribution that assumes 100%-causality and *1-*λ weight on the actual (0%-causal) distribution of child earnings. We then recompute quintile earnings thresholds so that each child earnings quintile has 20% of children, taking as given the outcomes of non-college-goers. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XVII

Actual vs. Counterfactual Intergenerational Transition Matrices with Heterogeneous Causal Effects

	Fraction of Children with Earnings in Each Group					
	Bottom 20%	Quintile 2	Quintile 3	Quintile 4	Top 20%	Top 1%
	(1)	(2)	(3)	(4)	(5)	(6)
A. Actual Outcomes						
..for children with parents from						
Bottom 20%	16.0%	21.1%	23.1%	21.7%	18.2%	0.6%
Quintile 2	14.0%	18.1%	22.4%	24.1%	21.4%	0.7%
Quintile 3	12.8%	15.7%	21.0%	25.6%	24.9%	0.9%
Quintile 4	11.5%	13.7%	18.9%	26.3%	29.6%	1.2%
Top 20%	11.1%	11.6%	14.3%	22.8%	40.2%	3.4%
B. Income-Neutral Student Allocations						
..for children with parents from						
Bottom 20%	15.6%	20.4%	22.7%	22.0%	19.4%	0.7%
Quintile 2	13.8%	17.7%	22.2%	24.2%	22.2%	0.8%
Quintile 3	12.7%	15.5%	20.7%	25.6%	25.5%	1.0%
Quintile 4	11.4%	13.7%	18.9%	26.2%	29.8%	1.3%
Top 20%	11.4%	12.1%	14.9%	22.9%	38.8%	3.3%
Share of rich-poor top-quintile outcome gap narrowed	11.7%					
C. Need-Affirmative Student Allocations						
..for children with parents from						
Bottom 20%	15.3%	19.8%	22.3%	22.1%	20.5%	0.9%
Quintile 2	13.6%	17.3%	21.9%	24.3%	22.8%	0.9%
Quintile 3	12.6%	15.5%	20.6%	25.6%	25.7%	1.1%
Quintile 4	11.5%	13.7%	18.8%	26.2%	29.8%	1.3%
Top 20%	11.6%	12.4%	15.3%	22.9%	37.9%	3.1%
Share of rich-poor top-quintile outcome gap narrowed	21.3%					

Notes: This table replicates Table VIII, but varies the causal effect of colleges on children's earnings based on parental income quintile, tier attended, and counterfactually assigned tier so that children from lower-income families gain more from attending more selective colleges. The counterfactual allocation of students to colleges is the same as in Table VIII. Children's earnings, however, are assigned differently. Mechanically, children are first randomly assigned the earnings of another child who is observed as attending their counterfactually assigned college and who has the same parent income quintile, race, and SAT/ACT score. After that counterfactual earnings level is calculated, children whose parents are in the bottom 20%, attend a school in one of the six most selective tiers (first six rows of Table II), and are counterfactually assigned to a school in one of the six most selective tiers have a causal share of 40%. This means the earnings outcome is with 40% probability their counterfactually assigned earnings and with 60% probability it is their empirically observed earnings. Children whose parents are not in the bottom 20%, attend a school in one of the six most selective tiers, and are counterfactually assigned to a school in one of the six most selective tiers have a causal share of 0%. This means that those children's counterfactual earnings is the same as their observed earnings. All other children—regardless of parental income quintile and observed and counterfactual tier—have a causal share of 80%. See the notes to Table VIII and Section V for more details on the counterfactuals and earnings allocation. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XVIII

Predictors of Mean Student Earnings, 2-year Colleges in Illinois

Dependent Variable: Mean Student Earnings in 2014		
Covariate	Regression Coefficient	Standard Error
	(1)	(2)
College Scorecard Measures		
Mean earnings of male students working and not enrolled 10 years after entry (log)	11460.3	(3023.1)
Median earnings of students working and not enrolled 8 years after entry (log)	8743.4	(3778.8)
75th percentile of earnings of students working and not enrolled 6 years after entry in 2011 (log)	3592.4	(3904.7)
College-Specific Inputs		
Average Faculty Salary (log)	2871.2	(873.4)
Student Demographics		
Percentage of students receiving financial aid (log)	-7129.4	(844.4)
Number of full-time undergraduate students (ages 18 and 19)	2.264	(0.429)
Number of full-time undergraduate students (ages 25 to 34)	-14.78	(1.752)
Number of part-time undergraduate students (ages 18 and 19)	-5.466	(1.217)
Number of part-time undergraduate students (ages 35 to 49)	7.328	(1.129)
Number of part-time undergraduate students (ages 65 and over)	-37.47	(3.884)
Independent students with family incomes between $30,001-$48,000 in nominal dollars	-20964.1	(1694.7)
Observations	29	
Number of Statistics Estimated	12	

Notes: This table reports the regression coefficients and standard errors obtained by running the forward-search algorithm described in Online Appendix F to predict mean student earnings (in dollars) for the group of 29 community colleges in Illinois. The enrollment-weighted mean income in this group is $36,316. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XIX

Prediction Errors in Publicly Released College-Level Statistics

	Mean Across Colleges	Std. Dev. Across Colleges	Absolute Error of Prediction			
			Mean	95th Percentile	99th Percentile	99.9th Percentile
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Student Earnings ($)	49327	19846	266	965	1846	3186
Median Student Earnings ($)	41304	12908	181	640	1352	2289
Median Student Earnings - Positive Earners ($)	45941	13022	187	690	1257	2353
Mean Parent Household Income ($)	116093	64479	829	3025	5993	11288
Mean Parent Rank (pp)	60.46	10.50	0.15	0.56	1.04	1.79
Parents in Top 10% (%)	16.44	12.43	0.16	0.58	1.11	2.13
Parents in Top 5% (%)	8.42	8.75	0.11	0.39	0.81	1.25
Parents in Top 1% (%)	1.72	2.83	0.03	0.12	0.24	0.45
Parents in Top 0.1% (%)	0.17	0.40	0.004	0.02	0.04	0.08
Kid in Top 10% (%)	15.83	11.21	0.16	0.54	1.18	1.99
Kid in Top 5% (%)	8.24	7.85	0.11	0.39	0.75	1.32
E[Kid Rank | Parents in Q1] (pp)	54.52	8.62	0.13	0.45	1.06	1.92
E[Kid Rank | Parents in Q2] (pp)	56.73	7.92	0.13	0.45	0.98	1.56
E[Kid Rank | Parents in Q3] (pp)	58.33	7.53	0.11	0.39	0.77	1.45
E[Kid Rank | Parents in Q4] (pp)	60.10	7.29	0.11	0.38	0.73	1.34
E[Kid Rank | Parents in Q5] (pp)	61.28	7.69	0.13	0.48	0.96	1.85
P(Kid in Q1, Parents in Q1) (%)	1.70	1.37	0.03	0.1	0.19	0.36
P(Kid in Q1, Parents in Q2) (%)	2.08	1.21	0.02	0.09	0.21	0.44
P(Kid in Q1, Parents in Q3) (%)	2.48	1.11	0.02	0.08	0.18	0.33
P(Kid in Q1, Parents in Q4) (%)	2.86	1.14	0.02	0.09	0.21	0.47
P(Kid in Q1, Parents in Q5) (%)	3.56	1.88	0.03	0.1	0.21	0.43
P(Kid in Q2, Parents in Q1) (%)	2.19	1.96	0.04	0.13	0.25	0.50
P(Kid in Q2, Parents in Q2) (%)	2.63	1.67	0.03	0.11	0.22	0.41
P(Kid in Q2, Parents in Q3) (%)	2.99	1.45	0.03	0.11	0.22	0.51
P(Kid in Q2, Parents in Q4) (%)	3.32	1.29	0.02	0.09	0.18	0.37
P(Kid in Q2, Parents in Q5) (%)	3.62	1.46	0.03	0.11	0.24	0.49
P(Kid in Q3, Parents in Q1) (%)	2.44	2.06	0.04	0.14	0.3	0.55
P(Kid in Q3, Parents in Q2) (%)	3.28	1.95	0.03	0.11	0.27	0.52
P(Kid in Q3, Parents in Q3) (%)	3.96	1.93	0.03	0.12	0.23	0.47
P(Kid in Q3, Parents in Q4) (%)	4.52	1.91	0.04	0.14	0.35	0.62
P(Kid in Q3, Parents in Q5) (%)	4.34	1.56	0.03	0.12	0.26	0.53
P(Kid in Q4, Parents in Q1) (%)	2.33	1.66	0.03	0.11	0.24	0.47
P(Kid in Q4, Parents in Q2) (%)	3.59	1.56	0.03	0.12	0.26	0.52
P(Kid in Q4, Parents in Q3) (%)	4.91	1.72	0.03	0.11	0.25	0.46
P(Kid in Q4, Parents in Q4) (%)	6.37	2.21	0.04	0.14	0.3	0.55
P(Kid in Q4, Parents in Q5) (%)	7.00	3.06	0.05	0.16	0.33	0.6
P(Kid in Q5, Parents in Q1) (%)	2.02	1.42	0.02	0.09	0.17	0.36
P(Kid in Q5, Parents in Q2) (%)	3.26	1.40	0.03	0.1	0.21	0.4
P(Kid in Q5, Parents in Q3) (%)	4.85	1.59	0.03	0.13	0.28	0.46
P(Kid in Q5, Parents in Q4) (%)	7.24	2.84	0.05	0.16	0.33	0.65
P(Kid in Q5, Parents in Q5) (%)	12.48	10.40	0.13	0.49	0.92	1.65
P(Kid in Top 1%, Parents in Q1) (%)	0.07	0.12	0.003	0.012	0.024	0.056
P(Kid in Top 1%, Parents in Q2) (%)	0.11	0.17	0.003	0.011	0.023	0.046
P(Kid in Top 1%, Parents in Q3) (%)	0.19	0.25	0.004	0.013	0.030	0.063
P(Kid in Top 1%, Parents in Q4) (%)	0.31	0.39	0.006	0.021	0.044	0.077
P(Kid in Top 1%, Parents in Q5) (%)	1.08	2.03	0.025	0.088	0.164	0.284

Notes: This table reports statistics on the prediction errors for estimates of the parent and student income distributions across U.S. colleges. Columns 1 and 2 report the (enrollment-weighted) mean and standard deviation of the estimates of each variable across colleges. Column 3 reports the mean absolute error (relative to the true values) of the estimates. Columns 4, 5 and 6 report the 95th percentile, 99th percentile and 99.9th percentile of the absolute error distribution, respectively. See Online Appendix F for further details on the algorithm used to estimate these statistics. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX TABLE XX

Colleges with the Highest Mobility Rates – Cost of Living Adjusted

A. Top 10 Colleges by Bottom-to-Top-Quintile Mobility Rate (Bottom 20% to Top 20%)

Rank	Name	Fraction Low-Income	x	Top-Quintile Outcome Rate	=	Mobility Rate
1	University of Texas – Pan American	27.4%		35.1%		9.6%
2	South Texas College	38.4%		24.7%		9.5%
3	Pace University – New York	22.1%		42.0%		9.3%
4	SUNY – Stony Brook	23.8%		36.6%		8.7%
5	University Of Texas At Brownsville	34.8%		22.9%		7.9%
6	New Jersey Institute Of Technology	13.7%		56.1%		7.7%
7	Laredo Community College	33.5%		22.9%		7.7%
8	Texas State Technical College Harlingen	31.8%		23.8%		7.6%
9	St. John's University – Queens, NY	21.5%		34.3%		7.4%
10	University of Texas – El Paso	22.0%		31.2%		6.9%

B. Top 10 Colleges by Upper-Tail Mobility Rate (Bottom 20% to Top 1%)

Rank	Name	Fraction Low-Income	x	Top-1% Outcome Rate	=	Upper-Tail Mobility Rate
1	MIT	6.3%		13.2%		0.84%
2	Columbia University	6.3%		12.8%		0.80%
3	Stanford University	4.8%		13.3%		0.64%
4	University Of California, Berkeley	13.0%		4.7%		0.61%
5	New York University	9.7%		6.2%		0.60%
6	University Of Pennsylvania	4.4%		12.5%		0.55%
7	Cornell University	6.1%		8.7%		0.53%
8	University Of Chicago	5.1%		9.8%		0.50%
9	University Of California, Los Angeles	15.3%		3.1%		0.48%
10	Pace University – New York	22.1%		2.1%		0.47%

Notes: This table replicates Table IV using cost-of-living adjusted income measures. We compute parents' and children's ranks after deflating incomes by a local cost-of-living price index based on their locations when their incomes are measured. See Section IV.A for further details on the cost-of-living adjustment and the notes to Table IV for further details on the construction of the tables. Statistics in this table are constructed based on Online Data Table 4 and 15, excluding colleges that have been closed as of September 2019.

ONLINE APPENDIX TABLE XXI

Colleges with the Highest Mobility Rates: Sensitivity Analysis

A. Top 10 Colleges by Bottom-to-Top-Quintile Mobility Rate for Sons

Rank	Name	Fraction Low-Income	x	Top-Quintile Outcome Rate	=	Mobility Rate
1	Cal State – Los Angeles	31.8%		36.4%		11.6%
2	South Texas College	51.4%		21.5%		11.1%
3	Southern Careers Institute	50.2%		22.0%		11.0%
4	University of Texas – Pan American	38.4%		28.1%		10.8%
5	University of Texas – Brownsville	45.5%		22.3%		10.1%
6	Laredo Community College	42.3%		23.8%		10.1%
7	SUNY – Stony Brook	16.8%		56.4%		9.5%
8	Southwest Texas Junior College	38.8%		24.3%		9.4%
9	CUNY System	28.1%		32.2%		8.9%
10	University of Texas – El Paso	26.7%		33.4%		8.9%

B. Top 10 Colleges by Bottom-to-Top-Quintile Mobility Rate for Household Earnings

Rank	Name	Fraction Low-Income	x	Top-Quintile Outcome Rate	=	Mobility Rate
1	University of Texas – Pan American	38.8%		20.2%		7.8%
2	Cal State – Los Angeles	33.2%		20.9%		6.9%
3	Pace University – New York	15.1%		42.9%		6.5%
4	SUNY – Stony Brook	16.4%		38.8%		6.4%
5	Laredo Community College	43.2%		14.6%		6.3%
6	University of Texas – Brownsville	47.3%		13.3%		6.3%
7	Southwest Texas Junior College	42.9%		14.2%		6.1%
8	South Texas College	52.3%		11.7%		6.1%
9	University of Texas – El Paso	28.0%		21.2%		5.9%
10	University of California – Irvine	12.3%		46.8%		5.8%

Notes: Panel A replicates Table IVa for male children. Panel B replicates Table IVa, measuring children's income as household (instead of individual) earnings. See the notes to Table IV for details. Statistics in this table are constructed based on Online Data Tables 2 and 15, excluding colleges that have been closed as of September 2019.

ONLINE APPENDIX TABLE XXII

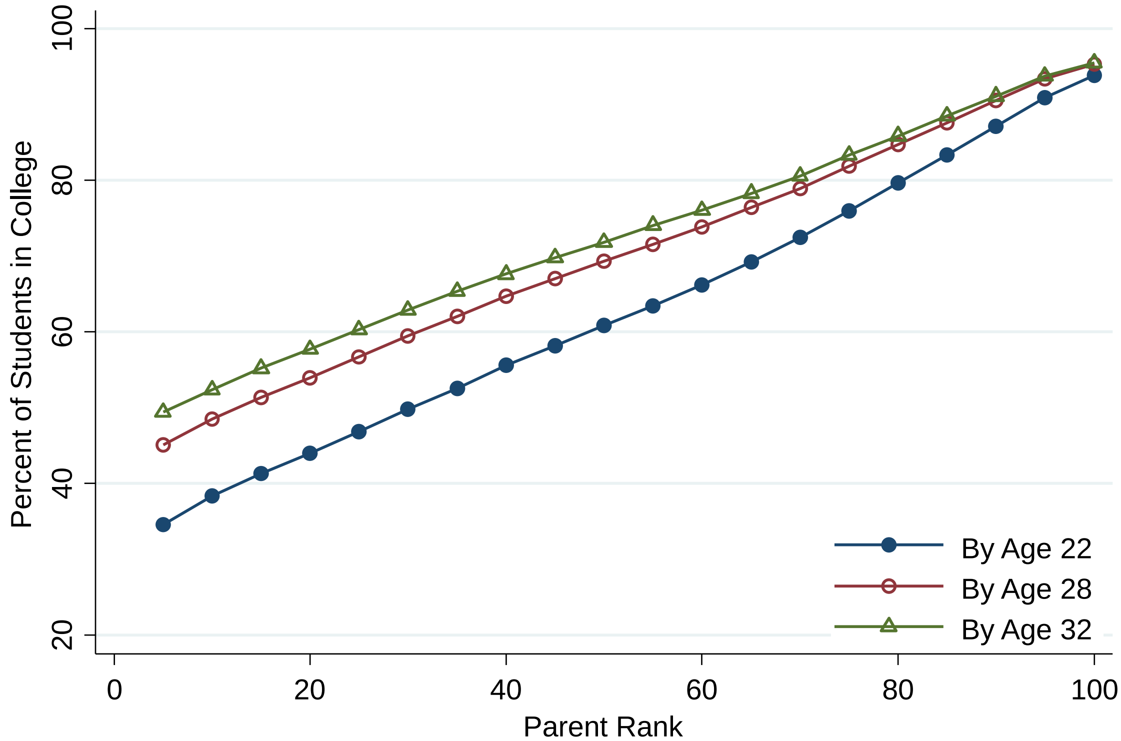
Relationship between SAT/ACT Scores and Earnings at Ages 32-34

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Dep. Var.: Individual Earnings in 2014 ($)									
SAT/ACT Score (100 points)	6,744	5,941	5,617	5,307	2,732	5,414	3,944	4,437	3,015
	(21)	(20)	(21)	(21)	(20)	(709)	(214)	(89)	(29)
B. Dep. Var.: Individual Income Rank (Percentiles)									
SAT/ACT Score (100 points)	2.73	2.41	2.24	2.23	1.27	1.26	1.23	1.45	1.44
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.12)	(0.06)	(0.03)	(0.01)
N	4,180,853	4,180,853	4,180,853	4,127,173	4,180,853	51,843	179,723	379,202	1,767,357
Indicator for SAT, ACT, or both taken	X	X	X	X	X	X	X	X	X
Cubic in Parent Income Rank		X	X	X	X	X	X	X	X
Interactions of cubic in Parent Rank, Race, and Gender			X	X	X	X	X	X	X
High School FE's				X					
College FE's					X	X	X	X	X
Restricted to Colleges in Tier						1	2	3	5

Notes: This table reports estimates from OLS regressions of students' earnings in 2014 on standardized test scores (SAT and ACT). The sample includes all college-goers in our 1980-1982 cohorts for whom we have either SAT or ACT scores. We convert ACT scores to the SAT 1600-point scale. Coefficients reported are multiplied by 100 so that they can be interpreted as the effect of a 100 point increase in the SAT score on the outcome. In Panel A, the left-hand side variable is individual wage earnings (2015 dollars), winsorized at $0 and $1 million; in Panel B, the left-side variable is individual income rank. Each column in each panel reports the coefficient on test scores from a different regression. In Column 1, we regress the outcome on only test scores and an indicator for whether the student took the SAT, the ACT, or both. Column 2 adds a cubic polynomial in parent income rank. Column 3 adds interactions between the parent income cubic, race, and gender. Columns 4 and 5 add high school and college fixed effects respectively. Columns 6, 7, 8, and 9 replicate column 5, restricting the sample to students attending colleges in tiers 1 (Ivy-Plus), 2 (other elite colleges), 3 (highly selective public), and 5 (selective public) respectively. Statistics in this table are constructed directly from the individual-level microdata.

ONLINE APPENDIX FIGURE I

College Attendance Rates by Parent Income and Age

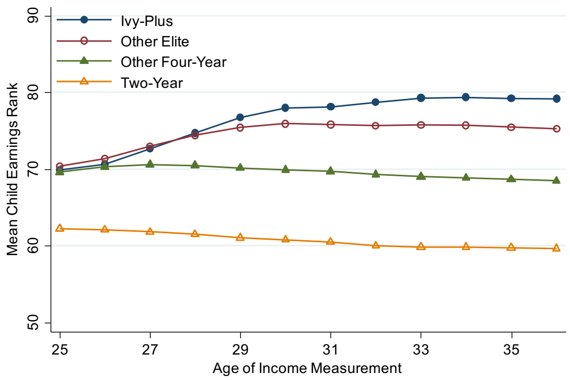


Notes: This figure plots the fraction of children in the 1980-82 birth cohorts in our analysis sample who attend college at any time during or before the year in which they turn ages 22, 28, and 32, by parent income ventile. This figure is constructed directly from the individual-level microdata.

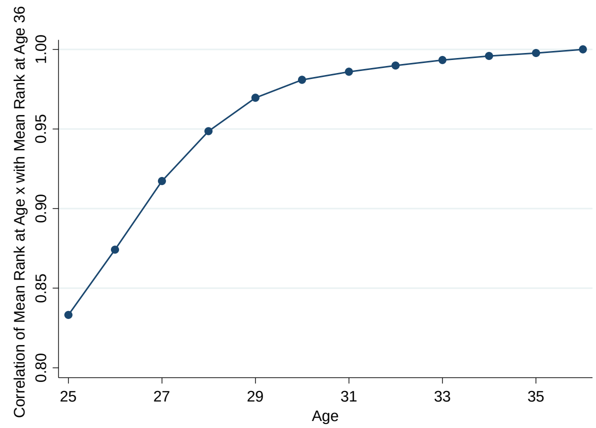
ONLINE APPENDIX FIGURE II

Children's Earnings Ranks by Age of Earnings Measurement

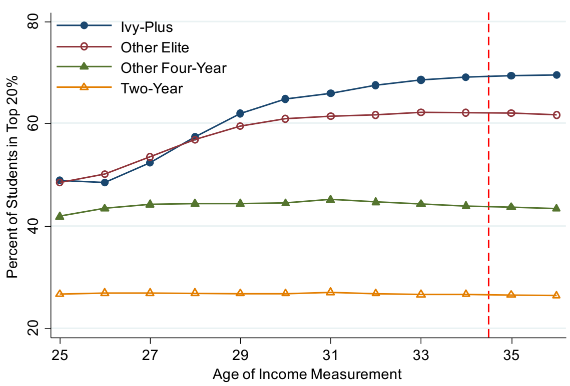
A. Mean Earnings Rank by Age and College Tier



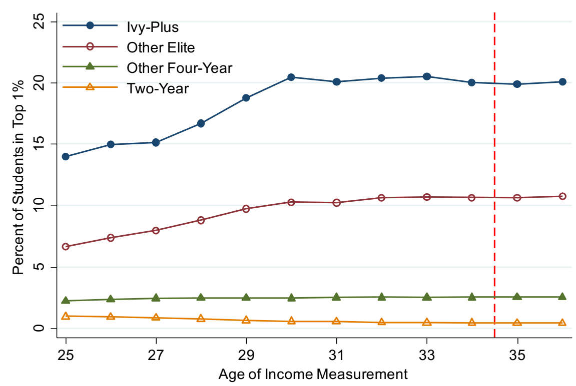
B. Correlation of College Mean Earnings Rank across Ages



C. Fraction of Children in Top Quintile



D. Fraction of Children in Top 1% by Age and College Tier

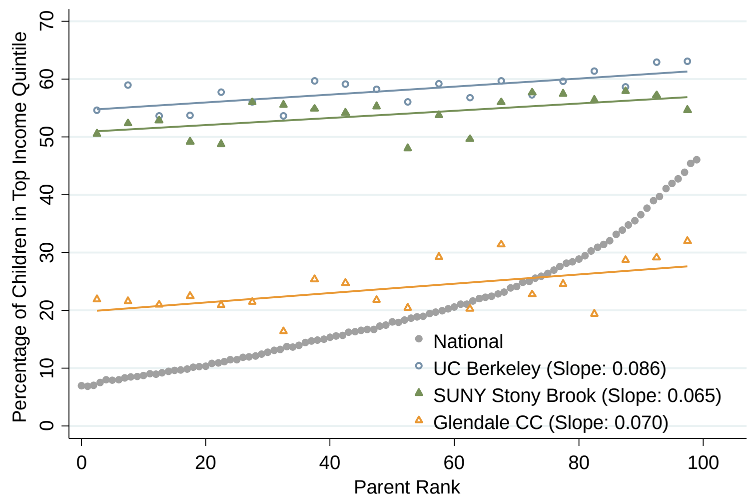


Notes: Panel A plots the mean income rank by age for students who attended colleges in various tiers. Children's incomes are defined as the sum of individual wage earnings and self-employment income. We measure children's incomes at each age from 25 to 36 and assign them percentile ranks at each age based on their positions in the age-specific distribution of incomes for children born in the same birth cohort. See notes to Figure III for definitions of these college tiers. Elite colleges are split into Ivy-Plus (the eight Ivy-League colleges as well as the University of Chicago, Stanford University, MIT, and Duke University) and Other Elite (all other elite colleges). Panel B plots the (enrollment-weighted) correlation between the college-level mean income rank of students at age 36 with the college-level mean income rank at ages 25-36. Panels C and D replicate Panel A, changing the outcome variable to the percentage of children who reach the top quintile (Panel C) or top 1% (Panel D) of their age- and cohort-specific earnings distribution. To maximize the age range at which incomes are observed, we use data for children in the 1978 birth cohort in this figure, with individuals assigned to the college they attended at age 22 (in 2000). Because children cannot be linked to parents before the 1980 birth cohort, we use data starting with the 1980 cohort and only observe income up to age 34 in our main analysis. This figure is constructed directly from the individual-level microdata.

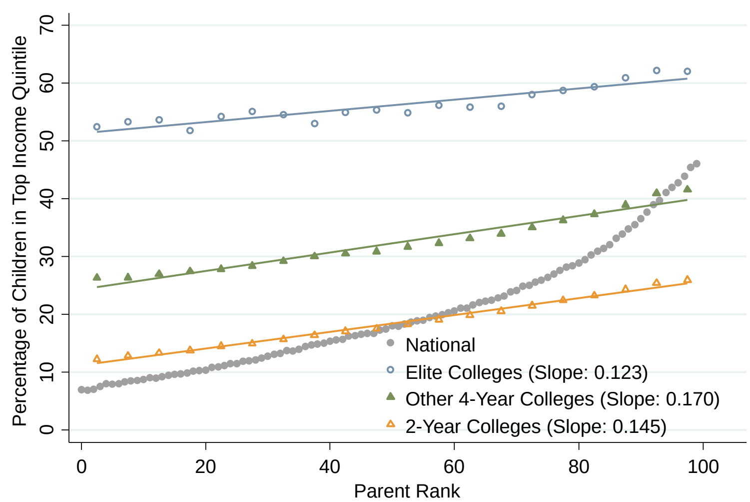
ONLINE APPENDIX FIGURE III

Fraction of Children who Reach Top Quintile by Parent Income Rank

A. At Selected Colleges



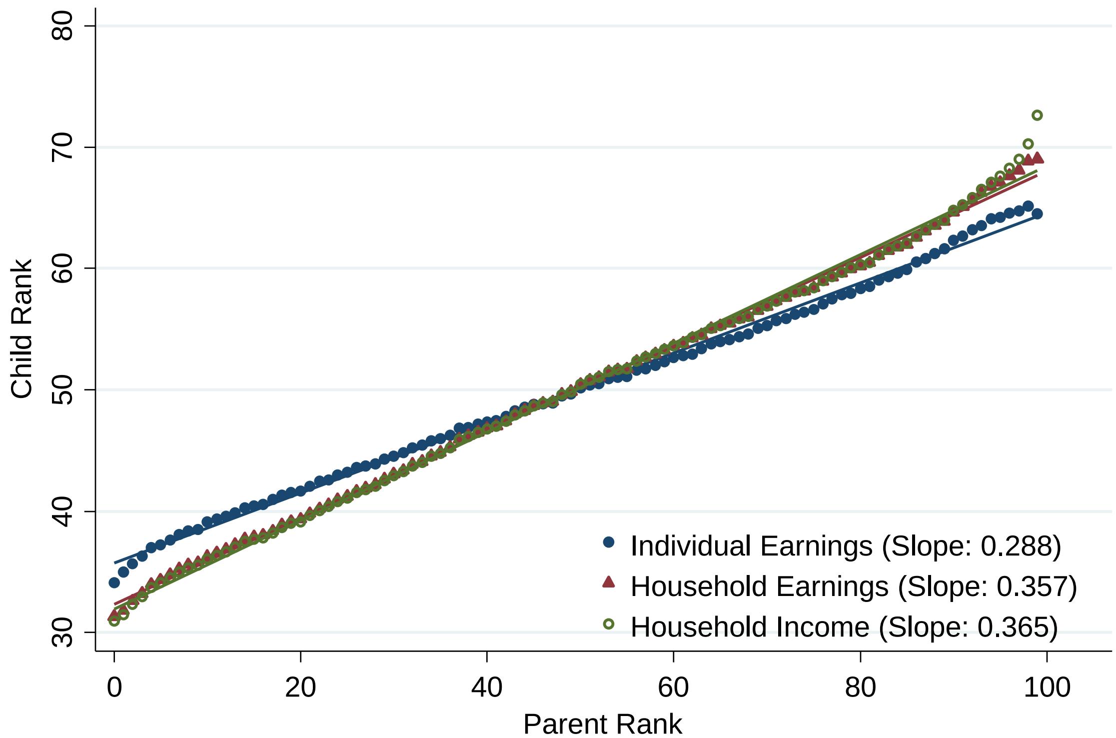
B. At Selected College Tiers



Notes: This figure replicates Figure IIIb-c using the fraction of children with individual earnings in the top income quintile as the outcome on the y-axis instead of children's mean ranks. Children's income quintiles are defined based on their individual earnings rank relative to all other children in the same birth cohort. We report slopes for each college or group of colleges, estimated using an OLS regression on the twenty plotted points, weighting by the count of observations in the microdata in each parent ventile. See the notes to Figure III for details. This figure is constructed directly from the individual-level microdata.

ONLINE APPENDIX FIGURE IV

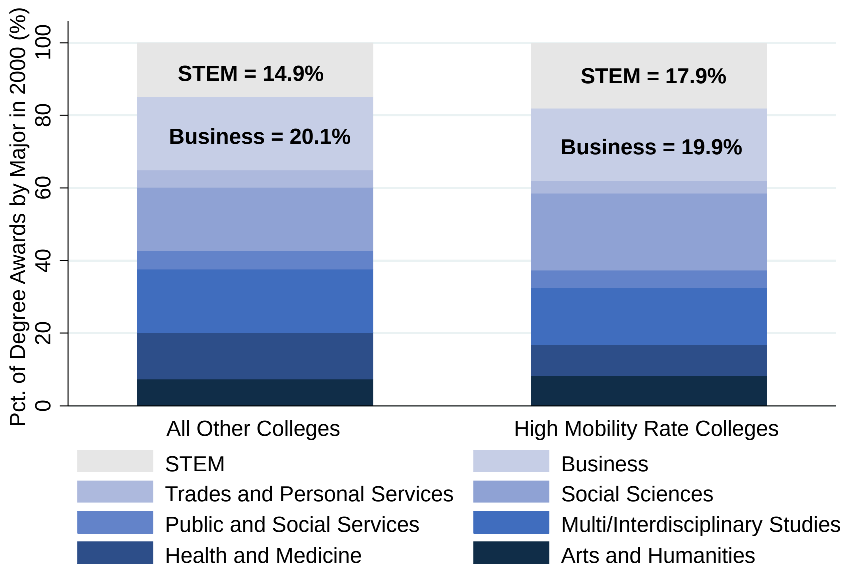
Sensitivity of Relationship between Children's and Parents' Ranks to Alternative Income Definitions



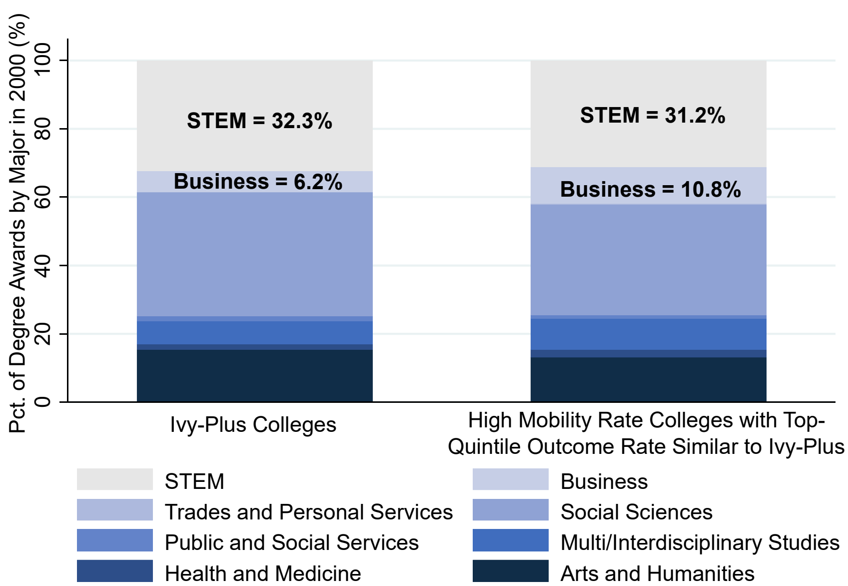
Notes: The series in solid circles replicates the national rank-rank series shown in Figure IIIa, plotting the mean children's individual income rank for each parents' household income percentile. The other two series present analogous estimates using alternative measures of children's incomes. The series in triangles measures children's labor earnings at the household rather than individual level, defined as the sum across both spouses (where present) of wage earnings and self-employment income. The series in open circles measures children's household income (including all sources of income). See Online Appendix A for further information on the income definitions and notes to Figure III for details on the construction of this figure. This figure is constructed directly from the individual-level microdata.

ONLINE APPENDIX FIGURE V
Distribution of Majors

A. High-Mobility-Rate Colleges vs. All Other Colleges

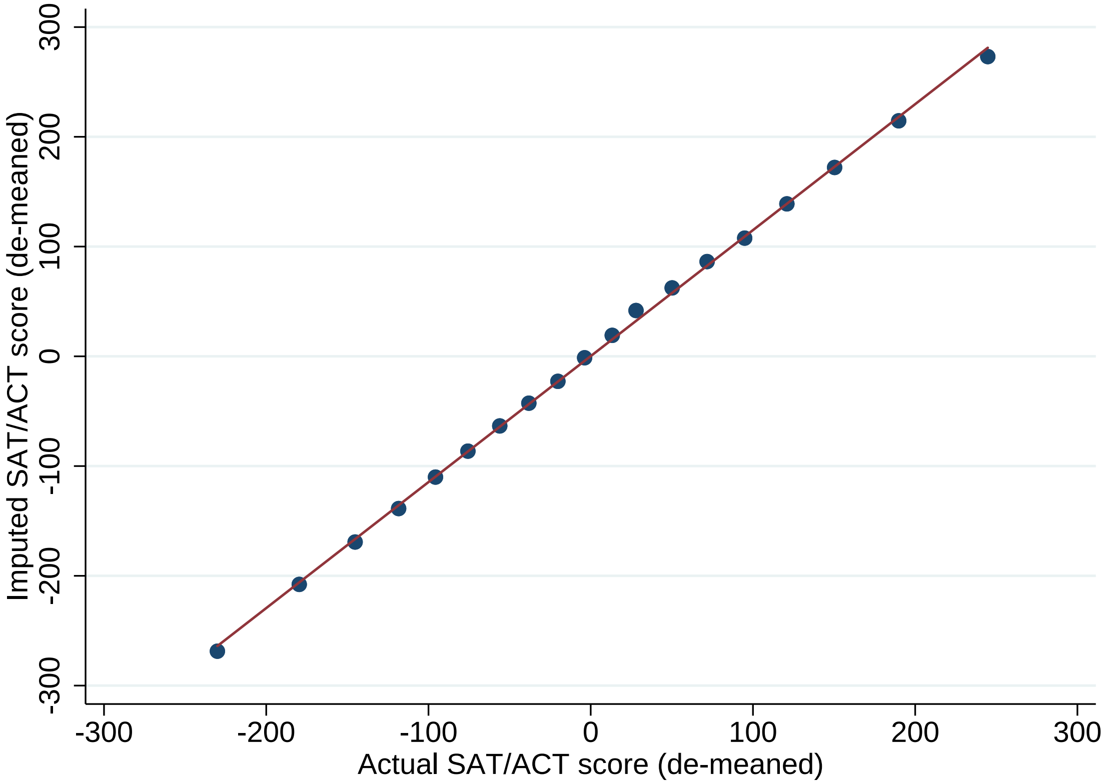


B. Ivy-Plus Colleges vs. High-Mobility-Rate Colleges with Comparable Top-Quintile Outcome Rates



Notes: Panel A shows the distribution of majors among students at high-mobility-rate colleges, defined as colleges in the analysis sample with a mobility rate above the 90th percentile of the enrollment-weighted distribution, vs. all other colleges. Panel B shows the distribution of majors at Ivy-Plus colleges compared to high-mobility-rate colleges with comparable top-quintile outcome rates, i.e. those with top-quintile outcome rates between the second-lowest and second-highest Ivy-Plus college. The share of students in each major is estimated by categorizing the share of degrees awarded by college in IPEDS (2000) according to the College Board's classificationof major categories. See notes to Figure IV for definition of mobility rates and notes to Figure I for definition of Ivy-Plus colleges. This figure is constructed from Online Data Tables 2 and 10.

ONLINE APPENDIX FIGURE VI
Validation of SAT/ACT Imputation

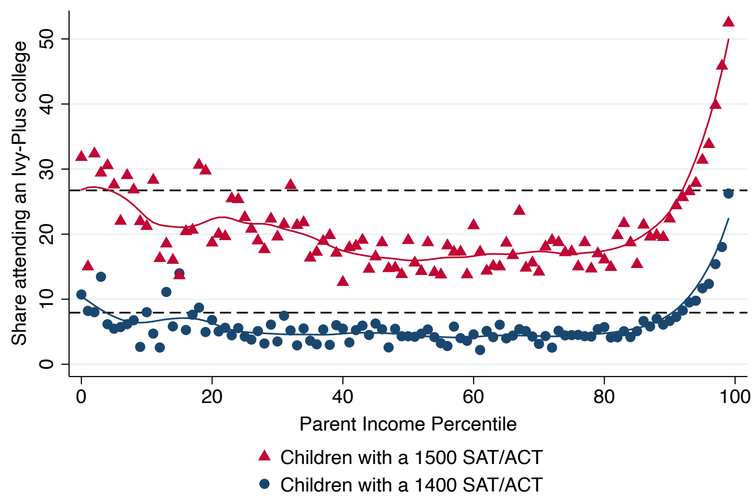


Notes: As noted in Section V.A, we impute an SAT/ACT score to the 26.2% of college goers missing an SAT and ACT score using the SAT/ACT score of the college student from the same parent income quintile, same college selectivity tier, and same state who has the closest level of earnings in adulthood. This figure presents a quantile-quantile plot of imputed SAT/ACT score versus actual SAT/ACT score, within college-parent income quintile, using data from five states where the SAT or ACT is administered to essentially all students. We construct the graph as follows. First, we set actual SAT/ACT scores to missing for college goers in the five states (measured using the college-goer's parent ZIP code) with the highest SAT/ACT coverage rates in our data (which range from 89% to 91% of college goers). We then run the imputation procedure described above by parent income quintile and tier (not state). Then, within each college-parent income quintile cell and *restricting to students whose actual SAT/ACT scores were set to missing*, we de-mean imputed SAT/ACT scores and compute ventile thresholds (i.e., the 5th percentile, 10th percentile,..., 95th percentile). We similarly compute de-meaned ventile thresholds for these students' actual SAT/ACT scores. Finally, we restrict attention to the ten colleges with the highest enrollment of these students and plot unweighted mean imputed quantiles versus unweighted mean actual quantiles (e.g., the bottom-left dot is mean imputed 5th percentile versus mean actual 5th percentile). The slope of the best-fit line (1.15) is near one with a constant (0.20) near zero. Hence, within college by parent income quintile cells, the distribution of imputed SAT/ACT scores nearly matches the distribution of actual SAT/ACT scores. Recall that the graph pools across college-parent income quintiles. When repeating the analysis separately for each parent income quintile, the slopes range from 1.12 to 1.20 and constants range from -0.8 to 1.1. When repeating the analysis using the top-10 selective colleges and separately using the top-10 unselective colleges, the slopes range from 1.14 to 1.23 and the constants range from 0.2 to 0.4. This figure is constructed directly from the individual-level microdata.

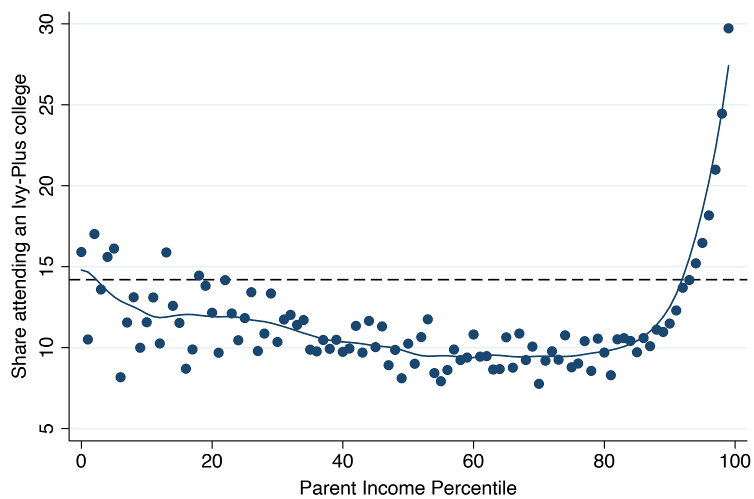
ONLINE APPENDIX FIGURE VII

Ivy-Plus Attendance Rates by Parental Income Conditional on SAT/ACT Scores

A. Students with Scores of 1400 or 1500 on SAT/ACT



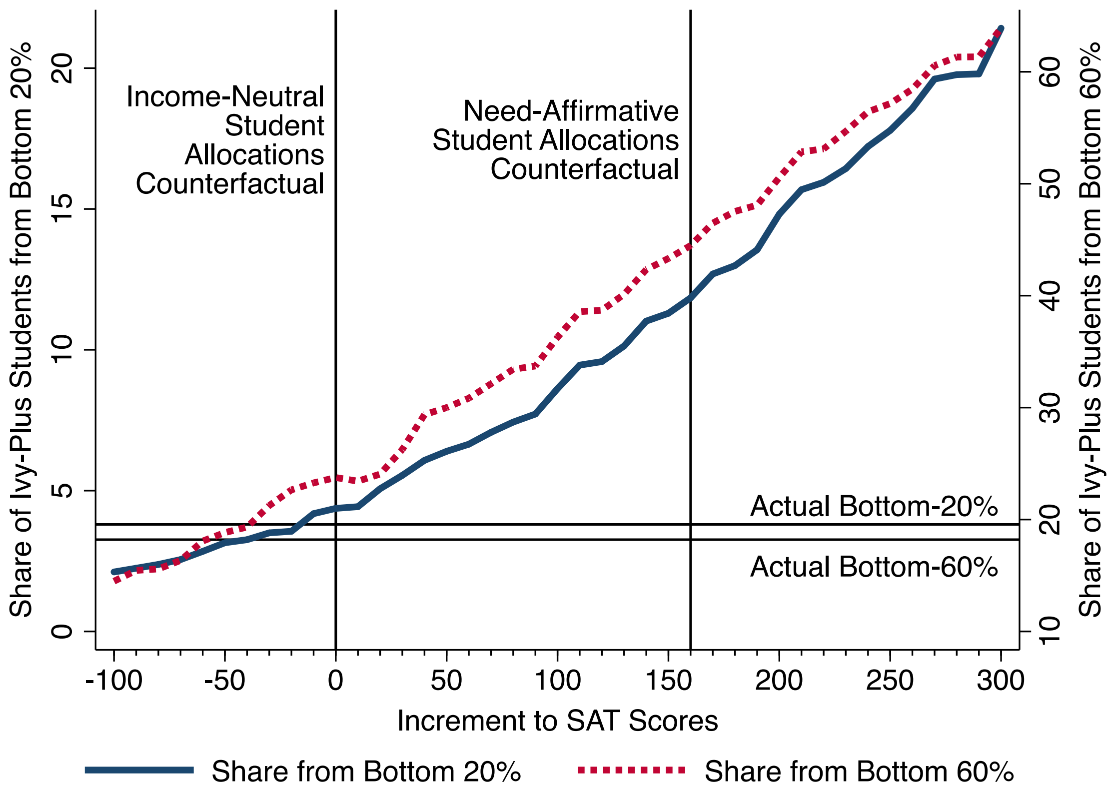
B. All SAT/ACT Scores, Reweighted to Match Ivy-Plus SAT/ACT Score Distribution



Notes: Panel A plots Ivy-Plus college attendance rates by parental income percentile for students with a 1400 or 1500 SAT/ACT score. The 1400 series exactly replicates Figure Vb. Because relatively few low-income students have a 1500 test score, the 1500 series pools students with an SAT/ACT score between 1480-1520. Panel B replicates Figure Vb pooling all SAT/ACT scores, weighted by the SAT/ACT score distribution of actual Ivy-Plus attendees. That is, Panel B plots the share of students who would attend an Ivy-Plus college by parent income percentile if each percentile's test score distribution matched the test score distribution of Ivy-Plus students. See notes to Figure Vb for additional details. This figure is constructed directly from the individual-level microdata.

ONLINE APPENDIX FIGURE VIII

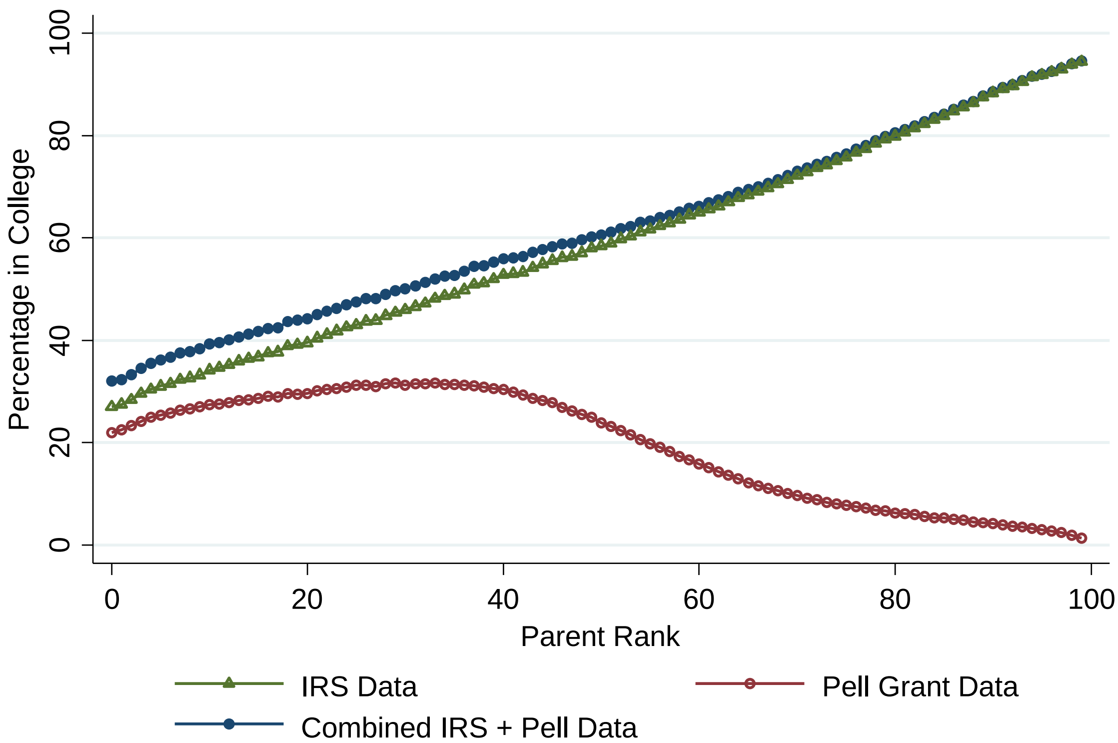
Counterfactual Low-Income Shares at the Ivy-Plus



Notes: This figure plots the share of students from the bottom-20% (left y-axis scale) and bottom-60% (right y-axis scale) in the Ivy-Plus tier, varying the constant added to bottom-20% college-goers' SAT/ACT on the x-axis. Second, third, and fourth quintile college goers' SAT/ACT scores are incremented upward by 80%, 60%, and 40% of the bottom-20%'s upward bonus, respectively. These shares are computed following the method used to construct the need-affirmative student allocations counterfactual described in Section V.B. The vertical lines show the shares that result from our baseline income-neutral student allocations counterfactual (0 point increment for low-income students) and baseline need-affirmative student allocations counterfactual (160 point increment). The horizontal lines show the actual shares of students from the bottom 20% and bottom 60% at Ivy-Plus colleges in our analysis sample. This figure is constructed directly from the individual-level microdata.

ONLINE APPENDIX FIGURE IX

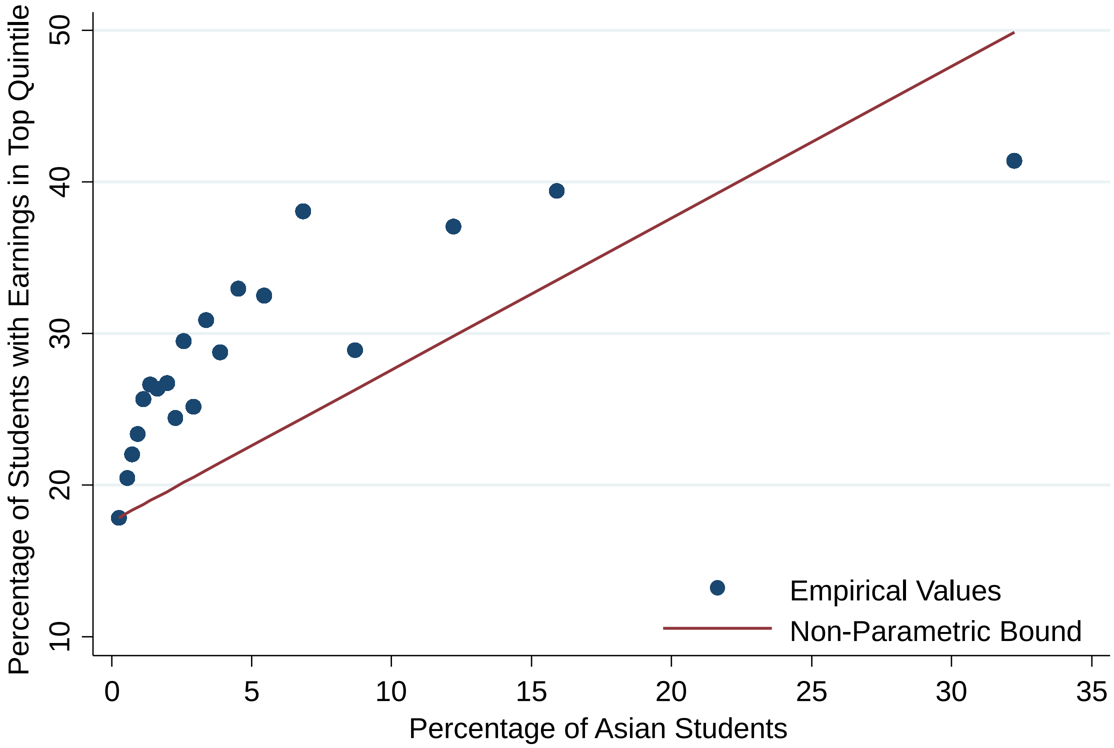
College Attendance Rates in 1098-T and Pell Records by Parent Income



Notes: This figure plots the fraction of students in the 1980-82 birth cohorts in our analysis sample who attend college at any time during the years in which they turn 19-22 by parental income percentile. The series in open circles plots the fraction of students in each parental income percentile with a college attendance record in the NSLDS data only. The series in triangles plots the fraction of students in each parental income percentile with a college attendance record in the 1098-T data only. The series in solid circles plots the fraction who attend college based on the union of the NSLDS and 1098-T data, the measure of attendance we use in our empirical analysis. This figure is constructed directly from the individual-level microdata.

ONLINE APPENDIX FIGURE X

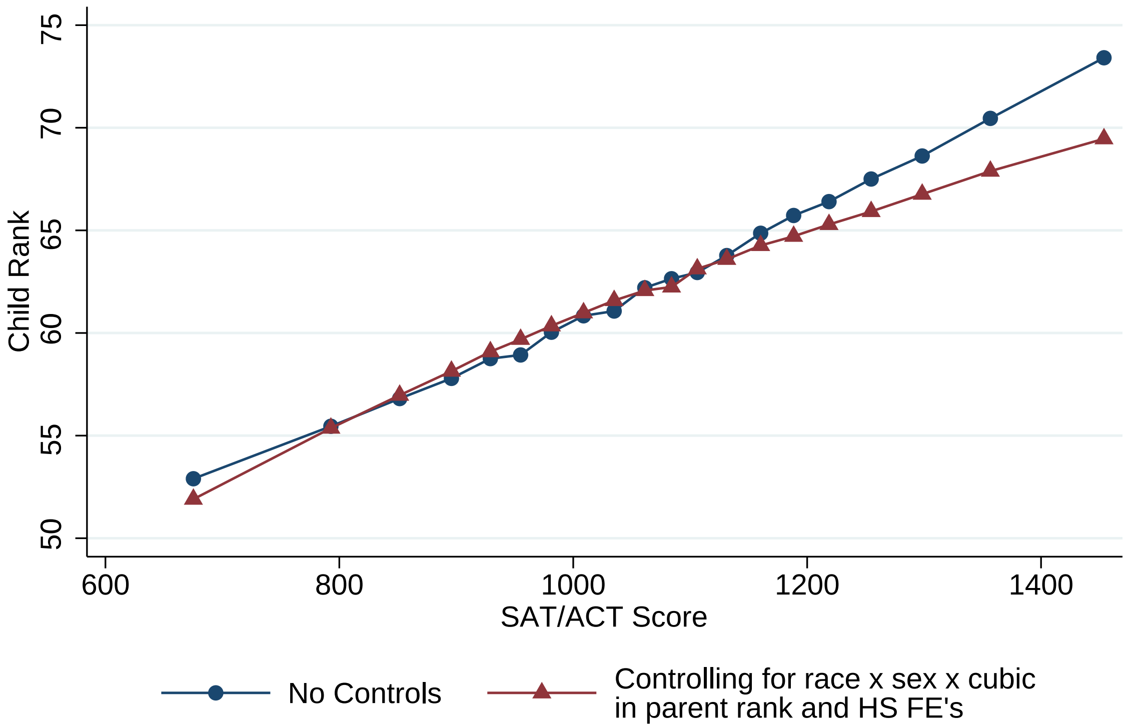
Ecological Association Between Top-Quintile Outcome Rates and Share of Asian Students



Notes: The points on this figure are a binned scatter plot of the fraction of students at a college with earnings in the top income quintile vs. the fraction of students who are Asian. To construct the binned scatter plot, we divide the x variable (the Asian share) into twenty equal-sized bins, weighting by college enrollment, and plot the (enrollment-weighted) means of the y and x variables within each bin. This series is constructed using the analysis sample. The solid line shows a non-parametric upper bound on the change in top-quintile outcome rates one would obtain if the association between Asian shares and top-quintile outcome rates across colleges was entirely driven by the higher top-quintile outcome rate of Asian students. This upper bound, which is obtained by assuming that every Asian student reaches the top quintile whereas every non-Asian student does not, has a slope of 1 and an intercept that coincides with the top-quintile outcome rate when the Asian share is zero. This figure is constructed from Online Data Tables 2 and 10.

ONLINE APPENDIX FIGURE XI

Relationship Between SAT Scores and Earnings in Adulthood



Notes: This figure shows the association between students' earnings ranks in 2014 and their standardized test scores (SAT and ACT). The sample includes all college-goers in our 1980-1982 cohorts for whom we have either SAT or ACT scores. We convert ACT scores to the SAT 1600-point scale. We construct binned scatter plots by first regressing children's ranks on twenty indicators (5 percentile point bins) for their test scores and a set of controls. In the series in blue circles, the only control is an indicator for whether the student took the SAT, the ACT, or both tests; in the series in red triangles, we additionally control for a cubic in parental income rank interacted with race and sex as well as high school fixed effects. We then plot the estimated child ranks and mean SAT scores within each of the twenty bins, recentering both variables so that their means match the overall sample means. This figure is constructed directly from the individual-level microdata.