Mobility Report Cards: The Role of Colleges in Intergenerational Mobility*

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Abstract

We characterize rates of intergenerational income mobility at each college in the United States using administrative data for over 30 million college students from 1999-2013. We document four results. First, access to colleges varies greatly by parent income. For example, children whose parents are in the top 1% of the income distribution are 77 times more likely to attend an Ivy League college than those whose parents are in the bottom income quintile. Second, children from low and high-income families have very similar earnings outcomes conditional on the college they attend, indicating that there is little mismatch of low socioeconomic status students to selective colleges. Third, upward mobility rates – measured, for instance, by the fraction of students who come from families in the bottom income quintile and reach the top quintile – vary substantially across colleges. Much of this variation is driven by differences in the fraction of students from low-income families across colleges whose students have similar earnings outcomes. Mid-tier public universities such as the City University of New York and California State colleges tend to have the highest rates of bottom-to-top quintile mobility. Elite private colleges, such as Ivy League universities, have the highest rates of upper-tail (e.g., bottom quintile to top 1%) mobility. Finally, between the 1980 and 1991 birth cohorts, the fraction of students from bottom-quintile families fell sharply at colleges with high rates of bottom-to-top-quintile mobility, and did not change substantially at elite private institutions. Although our descriptive analysis does not identify colleges' causal effects on students' outcomes, the publicly available statistics constructed here highlight colleges that deserve further study as potential engines of upward mobility.

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I Introduction

Higher education is widely viewed as a pathway to upward income mobility. However, inequality in access to colleges – particularly those that offer the best chances of success – could limit or even reverse colleges' ability to promote intergenerational mobility. Which colleges in America contribute the most to intergenerational income mobility? How can we increase access to such colleges for children from low income families?

We take a step toward answering these questions by using administrative data covering all college students from 1999-2013 to construct publicly available *mobility report cards* – statistics on students' earnings in their early thirties and their parents' incomes – for each college in America.[1] Using these mobility report cards, we document a set of descriptive results that shed light on how colleges mediate intergenerational mobility and highlight a set of colleges that warrant further study as potential engines of upward mobility.

We obtain rosters of attendance at all Title-IV accredited institutions of higher education in the U.S using de-identified data from federal income tax returns combined with data from the National Student Loan Data System. We obtain information on students' earnings in early adulthood and their parents' incomes from tax records. In our baseline analysis, we focus on children in the 1980-1982 birth cohorts – the oldest children in our data for whom we can reliably identify parents based on information on dependent claiming. We define the college each student attends as the college he or she attends for the most calendar years between the ages of 19 and 22, thereby measuring college attendance between 1999-2004 for our baseline sample. We measure parents' incomes as total pre-tax income at the household level when their children were between the ages of 15 and 19. We measure children's earnings at the individual level in 2014, when they were 32-34 years old.[2] After presenting a set of results using this baseline specification, we show that the findings are very similar using alternative specifications, such as measuring children's incomes at the household level, using alternative definitions of college attendance, and adjusting for differences in local costs of living.

Using the college-level statistics that we construct from these data, we document four sets of results.

[1] These statistics extend the statistics reported in the U.S. Department of Education's College Scorecard (2015). They generalize the statistics in the Scorecard by including all students (not just those receiving federal student aid), fully characterizing the joint distribution of parent and child income, and examining changes over time.

[2] We show that children's percentile ranks in the earnings distribution stabilize by age 32 at all colleges, which is why we use the 1980-82 birth cohorts for our baseline analysis.

First, access to colleges varies substantially across the income distribution. Among "Ivy-Plus" colleges (the eight Ivy League colleges, University of Chicago, Stanford, MIT, and Duke), more students come from families in the top 1% of the income distribution (14.5%) than the bottom half of the income distribution (13.5%). There is no evidence of a "missing middle" – the hypothesis that students from the middle class may be especially under-represented at elite private schools, since low-income students receive substantial financial aid and high-income students have ample resources. On the contrary, students from the lowest-income families have the smallest enrollment shares at the most selective private colleges, both in absolute numbers and relative to comparably ranked public schools. Only 3.8% of students come from the bottom 20% of the income distribution at Ivy-Plus colleges. As a result, children from families in the top 1% are 77 times more likely to attend an Ivy-Plus college compared to the children from families in the bottom 20%. More broadly, looking across all colleges, the degree of income segregation is comparable to income segregation across neighborhoods in the average American city. These findings challenge the perception that colleges foster interaction between children from diverse socioeconomic backgrounds.[3]

Second, children from low and high-income families have very similar earnings outcomes conditional on the college they attend. The relationship between childrens' earnings and their parents' incomes is strikingly flat within colleges especially when compared to the national relationship (Solon 1999, Chetty et al. 2014). In the nation as a whole, children from the highest-income families end up 30 percentiles higher in the distribution of individual earnings on average than those from the lowest-income families. Among students attending a given elite college (defined as one of the colleges in Tier 1 of Barron's 2009 ranking of selectivity), the gap between students from the highest- and lowest-income families is only 7.2 percentiles, 76% smaller than the national gradient.

The small gap in earnings outcomes between enrolled students from different socioeconomic backgrounds shows that colleges successfully "level the playing field" across students with different socioeconomic backgrounds, either because they select children of relatively uniform ability or because they provide greater value-added for children from low-income families (Dale and Krueger 2002). Regardless of the mechanism, the finding implies that students from low-income families are not over-placed at selective colleges, a common concern in the literature on "mismatch" (Arcidiacono and Lovenheim 2016). Since these students do nearly as well as their peers from higher

[3]These findings support the conclusions of prior research documenting that elite private colleges have a large share of students from affluent families (e.g., Bowen and Bok 1998, Pallais and Turner 2006, Hill et al. 2011, Hoxby and Avery 2013). The data we use here permit a more granular analysis than was feasible in these prior studies, allowing us to estimate statistics for the upper tail of the income distribution and report comprehensive statistics for all colleges.

socioeconomic status backgrounds, it is unlikely that they would do much better had they attended low-ranked schools, as would be the case if such mismatch were prevalent. Relatedly, this result suggests that colleges do not bear large costs in terms of student outcomes for any affirmative action that they currently grant students from low-income families in the admissions process.

In the third part of our analysis, we combine the statistics on access and outcomes to characterize how rates of intergenerational mobility vary across colleges. We measure each college's upward *mobility rate* as the fraction of its students who come from the bottom quintile of the income distribution and end up in the top quintile. Each college's mobility rate is the product of *access*, the fraction of its students who come from families in the bottom quintile, and its *success rate*, the fraction of such students who reach the top quintile. Mobility rates range from 0.9% at the 10th percentile to 3.5% at the 90th percentile across colleges. For reference, the average mobility rate in the nation as a whole is 1.7%. In a society with perfect mobility, 4% of children would make the transition from the bottom to top quintile. Relative to the 2.3 percentage point (pp) gap between perfect mobility and the observed rate of mobility in the U.S., the range of mobility rates across colleges is substantial.

Mobility rates vary substantially across colleges because there are large differences in access across colleges with similar success rates. Ivy-Plus colleges have the highest success rates, with almost 60% of students from the bottom quintile reaching the top quintile. But certain less selective universities have comparable success rates while offering much higher levels of access to low-income families. For example, 51% of students from the bottom quintile reach the top quintile at SUNY–Stony Brook. Because 16% of students at Stony Brook are from the bottom quintile compared with 4% at the Ivy-Plus colleges, Stony Brook has a bottom-to-top-quintile mobility rate of 8.4%, substantially higher than the 2.2% rate on average at Ivy-Plus colleges. More generally, the standard deviation of access conditional on success rates is 81% as large as the raw standard deviation of access. This result suggests that most of the variation in mobility rates is driven by "horizontal" variation in access across colleges that have similar outcomes rather than "vertical" selection – differences in outcomes across colleges that might differ in selectivity.

The colleges that have the highest bottom-to-top-quintile mobility rates are typically mid-tier public schools. For instance, many campuses of the City University of New York (CUNY), certain California State schools, and several campuses in the University of Texas system have mobility rates above 6%. Certain community colleges, such as Glendale Community College in Los Angeles, also have very high mobility rates; however, a number of other community colleges have very low mobility rates because they have poor outcomes. Elite private (Ivy-Plus) colleges have an average mobility rate of 2.2%, slightly above the national median: these colleges have the best outcomes but, as discussed above, a very small fraction of students from low-income families. Flagship public institutions have fairly low mobility rates on average (1.7%), as many of them have relatively low rates of access. Mobility rates are not strongly correlated with differences in the distribution of college majors, endowments, instructional expenditures, or other institutional characteristics. This is because the characteristics that correlate positively with children's earnings outcomes (e.g., selectivity or expenditures) correlate negatively with access, leading to little or no correlation with mobility rates.

If we measure "success" in earnings as reaching the top 1% of the income distribution instead of the top 20%, we find very different patterns. The colleges that channel the most children from low- or middle-income families to the top 1% are almost exclusively highly selective institutions, such as UC–Berkeley and the Ivy-Plus colleges. No college offers an upper-tail (top 1%) success rate comparable to elite private universities – at which 13% of students from the bottom quintile reach the top 1% – while also offering high levels of access to low-income students. More generally, the highest upper-tail mobility rates are concentrated at highly selective colleges with large endowments and high levels of expenditures. In this sense, the institutional model of higher education associated with the production of "superstars" is much more homogeneous than the broad variety of institutional models associated with upward mobility defined more broadly.

Our fourth and final set of results examines how access and mobility rates have changed since 2000. Overall, the number of children from low-income families attending college rose rapidly over the 2000s, both in absolute numbers and as a share of total college enrollment. Consistent with prior work, we find that the majority of this increase in college attendance occurred at two-year colleges and for-profit institutions.[4] The share of students from bottom-quintile families at four-year colleges and selective schools did not change significantly over the 2000s. Even at the Ivy-Plus colleges, which enacted substantial tuition reductions and other outreach policies during this period, the fraction of students from lower quintiles of the parent income distribution not increase significantly. Of course, this result does not imply that the increases in financial aid had no effect on access; absent these changes, the fraction of low-income students might have fallen, especially given that real incomes of low-income families fell due to widening inequality during the

[4]While our data include information on for-profit colleges, we do not focus on them in our analysis because our primary sample consists of students who attend college before age 22, and the majority of students at for-profit institutions are older.

2000s.[5] The key point is that on net, recent trends have left low-income access to elite private colleges largely unchanged.

The aggregate trends mask substantial heterogeneity across colleges within selectivity tiers. Most importantly, the fraction of students from low-income families at the institutions with the highest mobility rates – for instance, SUNY-Stony Brook and Glendale Community college – *fell* sharply over the 2000s. These changes in low-income access were not strongly associated with significant changes in students' earnings outcomes. As a result, the average student from a low-income family now attends a college with lower success rates than in 2000. In short, the colleges that may have offered many low-income students pathways to success are becoming less accessible to them.

Our analysis complements a large body of prior research that has used experimental and quasi-experimental methods to study the determinants of access and the returns to attending specific colleges.[6] Unlike this prior work, the differences in outcomes across colleges that we document here do not identify each college's causal effect on a given student ("value-added"). Much of the difference in outcomes we observe across colleges is presumably due to endogenous selection of students into colleges rather than treatment effects. However, our observational statistics highlight colleges that deserve further study as potential vehicles for upward mobility. In particular, many of the highest mobility rate colleges – such as the California State colleges or a number of community colleges – are not highly selective institutions in terms of student observables such as SAT scores or based on students' revealed preferences (Avery et al. 2013). This suggests that these colleges could potentially be "engines of upward mobility" in the sense of producing large returns for students from low-income families.[7] Conducting experimental or quasi-experimental studies – as in Zimmerman (2014) or Angrist et al. (2014) – at these high mobility rate colleges would be valuable to understand whether and how they generate substantial returns. From a policy perspective, the colleges with mobility rates in the top decile are of particular interest because their median annual instructional

[5] Our percentile-based statistics show a smaller increase in low-income access at Ivy-Plus colleges than is suggested by the increase in the fraction of students receiving federal Pell grants – a widely-used proxy for low-income access – because the Pell eligibility threshold rose in the 2000s and the real incomes of low-income families fell.

[6] Several studies have estimated the returns to attending certain selective colleges using admissions cutoffs and other quasi-experimental or matching methods (e.g., Dale and Krueger 2002, Black and Smith 2004, Hoekstra 2009, Hastings et al. 2013, Zimmerman 2014, Hoxby 2015, Kirkeboen et al. 2016). A number of studies have also analyzed how changes in tuition and other factors affect the fraction of low-income students who apply to and attend specific colleges (e.g., Avery et al. 2006, Goodman 2008, Deming and Dynarski 2009, Hoxby and Turner 2013, Marx and Turner 2015, Andrews et al. 2016, Angrist et al. 2014).

[7] Students from these colleges may have high earnings because they pursue jobs that pay more but have fewer non-pecuniary benefits. We make no attempt to assess the non-monetary impacts of attending alternative colleges in this paper, but view such an assessment as a valuable direction for future research.

expenditure is only $6,500 per student. In comparison, the median instructional expenditure at Ivy-Plus colleges – which are often the focus of efforts to increase access to high-quality higher education – exceeds $87,000, making their educational models less scalable as a pathway to upward mobility for large numbers of children.

More broadly, the college-level statistics constructed here can facilitate future quasi-experimental research on the determinants of access and outcomes in higher education. For example, researchers can use these statistics to study the impacts of tax credits, tuition changes, or outreach policies at a broader range of institutions than in prior work (Deming and Dynarski 2009).

This paper is organized as follows. Section II describes the data and key variable definitions. Section III presents results on access – the marginal distribution of parents' income at each college. Section IV studies outcomes – the distribution of children's incomes conditional on parents' incomes at each college. Section V characterizes mobility rates – the joint distribution of parents' and children's incomes across colleges. Section VI examines changes over time in access and success rates. Section VII concludes. College-level statistics by cohort and related covariates can be downloaded from the Equality of Opportunity Project website.

II Data

Our analysis builds upon the datasets used to construct the Department of Education's College Scorecard. In particular, we use data from federal income tax returns and the Department of Education spanning 1996-2014 to obtain information on college attendance, students' earnings in early adulthood, and their parents' incomes.[8] All analysis was conducted using de-identified data to protect confidentiality. In this section, we summarize the construction of our analysis sample, define the key variables we use in our analysis, and present summary statistics.

II.A Sample Definition

Our primary sample of children consists of all individuals in the U.S. who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) were born between 1980-1991, and (3) can be linked to parents with non-negative income in the tax data.[9] There

[8]Here and in what follows, the year always refers to the tax year (i.e., the calendar year in which an individual attends college or earns income).

[9]Because we limit the sample to children who can be linked to parents in the U.S. (based on dependent claiming on tax returns), our sample excludes college students from foreign countries. We limit the sample to parents with non-negative income (averaged over several years as specified in Section 2.3) because parents with negative income typically have large capital losses, which are a proxy for having significant wealth despite the negative reported income. The non-negative income restriction excludes 0.95% of children.

are approximately 48.3 million people in this sample. We provide a detailed description of how we construct this sample from the raw data (the Social Security Administration's DM-1 database housed alongside tax records) in Online Appendix A.

We identify a child's parents as the most recent tax filers to claim the child as a child dependent during the period when the child is 12-17 years old. If the child is claimed by a single filer, the child is defined as having a single parent. For simplicity, we assign each child a parent (or parents) permanently using this algorithm, regardless of any subsequent changes in parents' marital status or dependent claiming.

Children who are never claimed as dependents on a tax return cannot be linked to their parents and hence are excluded from our analysis. However, almost all parents file a tax return at some point when their child is between ages 12-17, either because their incomes fall above the filing threshold or because they are eligible for a tax refund (Cilke 1998). Thus, the number of children for whom we identify parents exceeds 98% of children born in the U.S. between 1980 and 1991 (Online Appendix Table I). The fraction of children linked to parents drops sharply prior to the 1980 birth cohort because our data begins in 1996 and many children begin to the leave the household starting at age 17 (Chetty et al. 2014, Online Appendix Table I). This is why we limit our analysis sample to children born in or after 1980.

II.B Measuring College Attendance

Data Sources. We obtain information on college attendance from two administrative data sources: federal tax records and Department of Education records spanning 1999-2013.[10] We identify students attending each college in the tax records using Form 1098-T, an information return filed by colleges on behalf of each of their students to report tuition payments.[11] Since the 1098-T tax data do not necessarily cover students who pay no tuition – who are typically low-income students receiving financial aid – we supplement them with records from the Department of Education's National Student Loan Data System (NSLDS), which cover all students receiving federal aid. Importantly, neither of these data sources relies on voluntary reporting or tax filing by students or their families. Thus, these two datasets provide a near-complete roster of college attendance at all Title IV

[10] Information on college attendance is not available in tax records prior to 1999, and the latest complete information on attendance available from the Department of Education at the point of this analysis was for 2013.

[11] All institutions qualifying for federal financial aid under Title IV of the Higher Education Act of 1965 must file a 1098-T form in each calendar year for any student that pays tuition (in order to verify students' eligibility for tax credits). In practice, many colleges file 1098-T forms for all their students, even those who pay no tuition.

institutions in the U.S.[12] The correlation between counts of students based on the 1098-T+NSLDS data and counts of students in the Department of Education's Integrated Postsecondary Education Data System (IPEDS) across colleges (from 2000-2013) is 0.97. Aggregate enrollments in our data are also well aligned with aggregate college enrollment counts from the Current Population Survey (Online Appendix Table II).

The 1098-T data and the NSLDS data use different identifiers for colleges. The NSLDS identifies each college separately using Office of Postsecondary Education Identification (OPEID) numbers, while the 1098-T forms identify colleges by their Employer Identification Number and ZIP code. Colleges frequently have multiple EINs and multiple OPEIDs, reflecting different schools or subdivisions. We develop an algorithm to map EIN-ZIPs to OPEIDs using students who received both a 1098-T form and appear in the NSLDS and then manually verify the accuracy of the resulting crosswalk for each college. See Online Appendix B for further details on the construction of this crosswalk and our methods for measuring college attendance.

Some universities file 1098-T forms for multiple campuses under a single EIN-ZIP, making it impossible to distinguish each campus. In such cases, we aggregate all colleges that share the same EIN-ZIP into a cluster and report statistics for that cluster of colleges. For example, we cannot distinguish the sub-campuses of the University of Massachusetts system in our data and therefore report statistics for the entire University of Massachusetts cluster. There are 54 such clusters, which comprise 17.6% of students and 4% of colleges in our data. We include these clusters in our baseline analysis, but also confirm that our conclusions do not change when dropping them from the sample.

A small number of college-year observations have incomplete 1098-T data either because of flaws in the administrative records or because of changes in EINs and reporting procedures. We identify such observations based on comparisons to counts in the NSLDS data and counts in the preceding and following year in the tax data (see Appendix B for details), and code them as missing data.[13] Such missing data account for 1.8% of (enrollment weighted) college-year observations.

Definition of College Attendance. Our goal is to construct statistics for the set of degree-seeking undergraduate students at each college. Since we cannot directly separate degree seekers from other students (summer school students, extension school students, etc.) in our data, we proceed in two

[12] The data would not include students who pay no tuition and receive no federal aid, but such cases are rare.

[13] 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.

steps. First, we define a student as attending a given college in a given calendar year if he appears in either the 1098-T or NSLDS data.[14] We then assign each student the college he attends for the most years between the calendar years in which he turns 19 and 22 (inclusive).[15] If a child attends two or more colleges for the same number of years (which occurs for 8% of children), we define the child's college as the first college he attended.[16]

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 173 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 first-time, full-time undergraduates in IPEDS is 0.97.[17] In addition, IPEDS data show that 97.5% of full-time undergraduate students are degree seekers, suggesting that the number of summer school or extension students in our sample is likely to be very small. We therefore conclude that our approach of focusing on students between the ages of 19 and 22 and identifying the college they attend most frequently, although not flawless, provides a good approximation of the pool of undergraduate degree seekers at each institution.[18]

To assess the sensitivity of our results, we also implement robustness checks using two alternative measures of college attendance. First, we define the *age 20 college* as an indicator for attending a given college in the calendar year that a child turns 20.[19] Second, we define the *first-attended* college as the institution that a child attends first between the calendar years in which he turns 19

[14]The 1098-T data are reported on a calendar year basis, whereas the NSLDS data report attendance on an academic year basis. We construct measures of attendance by calendar year in the NSLDS data based on the start date listed for the Pell grant and the student's Pell grant amount; see Appendix B for details.

[15]For example, we measure college attendance using data from 1999 to 2002 for children born in the 1980 cohort. Since not all children turn 19 until the end of 1999, this approach effectively measures college attendance between the ages of 18 and 22.

[16]If the student attended multiple "most attended" colleges in the first year, then a college is chosen at random from that set.

[17]Furthermore, IPEDS counts differ from our counts by 3% on average, which likely reflects international students not included in our sample.

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

[19]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 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).

and 28 (inclusive), breaking ties using the same method as in the age 20 definition.

II.C Measuring Incomes

We obtain data on childrens' and parents' incomes from federal income tax records spanning 1996-2014, following prior studies of intergenerational mobility using tax records (e.g., Chetty et al. 2014). We use income data from both income tax returns (1040 forms) and third-party information returns (e.g., W-2 forms), which contain information on the earnings of those who do not file tax returns. We measure income in 2015 dollars, adjusting for inflation using the headline consumer price index (CPI-U).

Parent Income. We measure parent income as total pre-tax income at the household level. In years where a parent files a tax return, we define family income as Adjusted Gross Income (as reported on the 1040 tax return). In years where a parent does not file a tax return, we define family income as the sum of wage earnings (reported on form W-2) and unemployment benefits (reported on form 1099-G).[20] In years where parents have no tax return and no information returns, family income is coded as zero.[21] Note that this income measure includes labor earnings and capital income. It excludes non-taxable cash transfers such as TANF and SSI, in-kind benefits such as food stamps, all refundable tax credits such as the EITC, non-taxable pension contributions (e.g., to 401(k)s), and any earned income not reported to the IRS. Income is always measured prior to the deduction of individual income taxes and employee-level payroll taxes.

We average parents' family income over the five years when the child is aged 15-19 to smooth transitory fluctuations (Solon 1992) and obtain a measure of resources available at the time when most college attendance decisions are made.[22] We then assign parents income percentiles by ranking them based on this mean income measure relative to all other parents who have children in the same birth cohort.[23]

Child Income. Our primary measure of child income is total pre-tax *individual* earnings. For single filers, individual earnings is defined as the sum of wage earnings and net self-employment

[20] The database does not record W-2s and other information returns prior to 1999, so non-filer's income is coded as zero prior to 1999. Assigning non-filing parents zero income has little impact on our estimates because only 2.9% of parents do not file in each year prior to 1999 and most non-filers have very low W-2 income (Chetty et al. 2014). For instance, in 2000, median W-2 income among non-filers was $29.

[21] Importantly, these observations are true zeros rather than missing data. Because the database covers all tax records, we know that these individuals have no taxable income.

[22] Because the data begin in 1996, we average only over the years when the child is aged 16-19 for children in the 1980 cohort.

[23] In the case of ties, we define the rank as the mean rank for the individuals in that group. For example, if 10% of a birth cohort has zero income, all children with zero income would receive a percentile rank of 5.

income (i.e., net of one-half of the self-employment tax) as reported on Form 1040. For joint filers, it is defined as the sum of wage earnings reported on the individual's own W-2 forms, net self-employment income reported on Form SE, and half of the additional wage earnings reported on Form 1040 relative to the sum of the spouses' W-2 wage earnings (see Appendix A for details). For non-filers, individual earnings is defined as the sum of wage earnings reported on the individual's W-2 forms.

We measure children's incomes in 2014 – the most recent year in which we observe earnings – to minimize the degree of "lifecycle bias" that arises from measuring children's earnings at too early an age. We show in Section IV.A below that the earnings ranks of children in our primary cross-sectional analysis sample stabilize by 2014. We assign children income percentiles by ranking them based on their individual earnings relative to all other children in the same birth cohort.

We also consider two alternative measures of child income (all measured in 2014) in sensitivity analyses. We define a child's household income in the same way as parents' household income. In addition, we define a child's household earnings as the sum of individual earnings (as defined above) for the child and his or her spouse. Household income includes capital income, while household earnings does not.

II.D College-Level Statistics

We present college-level statistics on childrens' and parents' income distributions for two samples: a *longitudinal* sample – which includes data at the college by cohort level for the 1980-1991 cohorts – and a *cross-sectional* sample – which includes data by college for children primarily in the 1980-82 cohorts. We use the cross-sectional sample for all of the empirical analysis below except when analyzing time trends in Section VI. We focus on children in the earliest birth cohorts (1980-82) in the cross-sectional sample so that their incomes can be measured at age 32 or older in 2014, the age at which children's income ranks stabilize at all colleges (see Section IV.A).

We construct the longitudinal sample simply by collapsing the primary analysis sample into college-by-cohort groups (using the college the students attends most frequently between ages 18 and 22 in our baseline analysis). We exclude colleges that have fewer than 100 students on average across the 1980-1991 birth cohorts (in years where we have data for that college) and all college-cohort observations with fewer than 50 students. Students who attend these colleges – who account for 8.6% of the primary analysis sample – are all grouped into a separate "colleges with insufficient data" category. After imposing these restrictions, we are left with a longitudinal sample that consists of 2,463 colleges that have 28.1 million students.

To construct the cross-sectional sample, we begin by extracting the data for the 1980-82 cohorts for each college in the longitudinal sample. 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_{ctv} for college c in cohort t, we first estimate the 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 6.3% of enrollment-weighted observations in the cross-sectional sample.[24] Finally, we construct the cross-sectional college-level sample by computing enrollment-weighted means of each statistic for the 1980-82 cohorts, using imputed values where necessary. We exclude colleges from the cross-sectional sample that have no data (after imputations) for the 1980-1982 cohorts.

Following established disclosure standards, we report estimates rather than exact values of the statistics for each college. Online Appendix C describes the procedure that we use to construct these estimates. The estimates are generally very accurate. For example, the estimates of average student earnings by college have a mean (enrollment-weighted) absolute error of $266; for reference, the standard deviation of average earnings across colleges is $17,061 and the interquartile range is $19,500.[25] Hence, the estimation error does not meaningfully affect comparisons across colleges. As another benchmark, the estimation error is comparable to the fluctuation in the true statistics across years for a typical college that arises due to sampling error. To facilitate replication, we use the publicly available estimates wherever possible in our analysis. However, we show in Appendix Table 4 that using the exact values for our analysis yields virtually identical conclusions.

For certain analyses, we report statistics for groups of colleges rather than individual colleges. We classify colleges as "4-year" or "2-year" based on the highest degree they offer using IPEDS

[24] This imputation procedure is helpful to maximize the coverage of colleges in the cross-sectional sample that we use for most of the college-level analysis, as 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. In addition, note that the imputation leads us to slightly overstate the aggregate college attendance rate in the cross-sectional 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 Online Appendix B for details).

[25] Analogous statistics for the estimation error in other key statistics analyzed below are provided in Appendix Table IV.

data.[26] Following prior work (e.g., Deming et al. 2015), we use data from the Barron's 2009 index (Barron's Educational Series, College Division 2008) to classify 4-year colleges into five tiers based on their selectivity: Ivy-Plus (the Ivy League plus Stanford, MIT, Chicago, and Duke), elite (Barron's Tier 1 excluding the Ivy-Plus; 64 colleges), highly selective (Barron's Tier 2; 97 colleges), selective (Barron's Tiers 3-5; 970 colleges), and non-selective (Barron's Tier 9 and all four year colleges not included in the Barron's selectivity index; 232 colleges).[27] Finally, we also obtain information on college characteristics, such as instructional expenditures, endowments, and the distribution of majors from the 2000 IPEDS. We also use information on net cost of attendance and admissions rate from Department of Education's (DoE) College Scorecard, as measured in 2013 (College Scorecard 2015). Online Appendix E provides sources and definitions for all of the variables we use from the IPEDS and College Scorecard data.

II.E Summary Statistics

Table I reports summary statistics for children in the cross-sectional sample (1980-82 cohorts).[28] Online Appendix Table I reproduces these statistics for the longitudinal sample (1980-1991 cohorts). We report statistics for three groups: all children (Column 1), children who attend college between the ages of 19 and 22 (Column 2), and children who do not attend college between the ages of 19 and 22 (Column 3).

61.8% of the 11.3 million children in 1980-82 birth cohorts attend college at some point between the ages of 19 and 22. Another 12% attend college at some point by age 28; and 27% of children do not attend college at all before age 28. Among those who attend college between 19 and 22, 55% attend a four-year college, 48% attend a selective four-year college, and 0.79% attend an Ivy-Plus college.

Children who attend college both come from richer families and also earn more themselves. 12% of students who attend college come from families in the bottom quintile, while 28% come from the top quintile and 1.53% come from the top 1%. 28% of college-goers have earnings in the top quintile and 1.55% have earnings in the top 1% (in 2014, between the ages of 32-34). Conditional on having parents in the bottom quintile, 15.9% of college-goers reach the top quintile of the distribution,

[26] Since many colleges offer both 2-year and 4-year programs, many students attending a "4-year" college may be enrolled in a 2-year program.

[27] The statistics reported for groups of colleges are exact values rather than estimates because the groups aggregate data over multiple colleges.

[28] For simplicity, we include only children born between 1980-82 without using imputations based on the 83-84 cohorts when constructing these summary statistics.

compared with 4.1% of non-college-goers.

III Access: Parental Income Distributions

We begin by analyzing the marginal distributions of parental household incomes at each college. We characterize these distributions by ranking parents relative to other parents with children in the same birth cohort. For parents of children in the 1980 birth cohort, median income is $58,000 (in 2015 dollars), while the 20th and 80th percentiles are $25,000 and $111,000, respectively (Appendix Figure Ia). The income distribution is highly skewed: the 99th percentile is $512,000 and the 99.9th percentile is $2.2 million.

Figure Ia plots the parental income distribution at four colleges that are representative of the broader variation across colleges: Harvard, UC-Berkeley, the State University of New York–Stony Brook, and Glendale Community College in Los Angeles. The bars show the fraction of parents in each quintile of the national income distribution; the share of families coming from the top 1% is shown by the cross-hatched bars within the top quintile.

We estimate that approximately 3% of children at Harvard come from the lowest income quintile of families, compared with more than 70% from the top quintile.[29] 15.4% of students at Harvard come from families in the top one percent of the income distribution – about the same number as from the bottom three quintiles combined. This highly skewed parental income distribution is representative of other elite private colleges. Figure Ib presents the distribution of parent income at the twelve Ivy-Plus colleges (the Ivy League plus Stanford, MIT, Chicago, and Duke). Each of the 100 dots represents the fraction of students at those schools with parents in a specific income percentile. By definition, a nationally representative student body would have 1% of students from each of the 100 parent income percentiles, displayed as the horizontal dashed line in the figure. There are more students who come from families in the top one percent (14.5%) than the bottom half of the parent income distribution (13.5%). Only 3.8% of students at these colleges come from parents in the bottom quintile, implying that children born into top 1% households are 77 times more likely to attend an Ivy-Plus school than children born into the bottom 20%. This ratio is even larger in the extreme upper tail, where children born into top 0.1% households are 122 times more likely to attend such colleges than those in the bottom quintile.

Returning to Figure Ia, now consider UC-Berkeley. Berkeley, a highly selective public college,

[29]These numbers and all other college-specific statistics reported below are estimates of the underlying values, following the procedure described in Section II.D and Online Appendix C.

has fewer students from high-income families than Harvard. However, even at Berkeley, more than 50% of students come from the top quintile, as compared with only 8.8% from the bottom quintile. The fraction of students in each quintile at Berkeley falls monotonically relative to the fraction at Harvard as parent income rises. This pattern rejects the hypothesis that elite private universities have a "missing middle class" because low-income students receive substantial financial aid and high-income students can afford to pay high tuition. Rather, students from the lowest-income families are significantly less likely to attend the most selective private colleges relative to the most selective public colleges. This suggests that the cost of attending is not the primary reason for the under-representation of low- and middle-income students at elite private colleges, consistent with the findings of other work examining the determinants of students' application decisions (Hoxby and Avery 2013).

The other colleges in Figure Ia have many more students from low-income families. SUNY-Stony Brook, a second-tier public institution according to the Barron's rankings, has a much more even distribution of parental incomes, though there are still significantly more students from the top quintile (30.1%) than the bottom quintile (16.4%). Glendale Community College has a monotonically declining fraction of students across the income quintiles, with 32.4% students coming from a family in the bottom income quintile and only 13.6% from the top quintile.

To characterize the variation in income distributions more generally across all colleges, we focus on the share of parents from the bottom quintile at each college as a simple summary statistic to measure low-income access. Figure Ic plots the (enrollment-weighted) distribution of the bottom quintile parental share across all 2,199 colleges in our sample. The fraction of students coming from low-income families varies greatly across colleges. Approximately 9.3% of students come from the bottom quintile at the median college, similar to the level of low-income access at UC-Berkeley. Ten percent of colleges (like Harvard) draw fewer than 3.7% of their students from the bottom quintile, while 10% of colleges have more than 21.0% of such students.

Figure I indicates that there is substantial income segregation across colleges, with students from rich families predominantly attending some institutions while students from poor families attend others. To quantify the degree of income segregation relative to other benchmarks, we use a two-group Theil (1972) index (Reardon and Firebaugh (2002); Reardon (2011)). Formally, we define entropy (income diversity) within the college-going population as a whole as $E = p\log_2 \frac{1}{p} + (1-p)\log_2 \frac{1}{1-p}$, where p is the fraction of college-goers from the bottom quintile of the parent income distribution. Letting $j = 1, \ldots, N$ index colleges in the U.S., we analogously measure entropy within each college as $E_j = p_j \log_2 \frac{1}{p_j} + (1-p_j)\log_2 \frac{1}{1-p_j}$, where p_j denotes the fraction of individuals at college j from the bottom quintile. We then define the degree of income segregation across colleges as

$$H = \sum_j \left[\frac{N_j}{N} \times \frac{E - E_j}{E} \right]$$

where N_j/N is the fraction of students who attend college j. We estimate that $H = 0.077$ across the colleges in our cross-sectional sample. For comparison, the median level of income segregation (at the 25th percentile) across Census tracts within the 100 largest commuting zones in America is $H = 0.084$, with an interquartile range of 0.067 to 0.099 (Chetty et al. 2014, Online Data Table 8). The degree of income segregation across colleges is thus comparable to income segregation across census tracts in the average American city. Contrary to the common perception that children interact with a more socioeconomically diverse group of peers when they reach college, colleges in America are just as segregated as the neighborhoods in which children grow up.

IV Outcomes: Children's Earnings Distributions

We now shift our focus to children's earnings outcomes.[30] As with parents, we assign each child a percentile rank by comparing his or her individual earnings to the earnings of all other children in the same birth cohort matched to parents with non-negative income. For children in the 1980 cohort, median individual earnings in 2014 (at age 34) are \$28,000. Roughly 20% of children have 0 individual earnings. The 80th percentile is \$58,000, and the 99th percentile is \$197,000 (Appendix Figure IVb).

We first examine the age profile of children's earnings across colleges to determine the point at which children's incomes provide stable measures of lifetime income.[31] We then characterize the distribution of children's earnings conditional on their parents' incomes within each college, restricting attention to children who are old enough that we can obtain reliable estimates of their lifetime income.

[30] Ideally, one would measure a child's earnings *potential*, which may differ from his or her realized earnings. For instance, children of wealthy parents may choose not to work or may choose lower-paying jobs that offer non-pecuniary benefits, which would reduce the persistence of income across generations relative to the true persistence of underlying opportunities. Lacking a measure of earnings potential, we follow the prior literature on intergenerational mobility and focus on realized earnings.

[31] This issue does not arise for parents because we measure most parents' incomes in their forties and fifties, when their children are between 15 and 19.

IV.A Lifecycle Profiles of Earnings Ranks by College

In our primary sample, which begins with the 1980 birth cohort, we cannot observe earnings after age 34. Measuring children's incomes when they are too young can potentially yield misleading estimates of lifetime income because children with high lifetime incomes have steeper earnings profiles (e.g., Haider and Solon, 2006, Solon 1999). For example, children who are on a path to having high lifetime earnings may still be in graduate school in their late twenties and thus have temporarily lower incomes than those who did not pursue advanced training. This issue may be especially acute at elite colleges, where many students go on to pursue advanced degrees.

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.[32] 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 all other children in the 1978 cohort in each calendar year.

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.[33]

[32] 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.

[33] The slight decline from ages 30 to 36 for those who attend lower-tier colleges is a consequence of two factors: (1) the increasing earnings of children who attended elite colleges, which pushes down other individuals' ranks (since the ranks must average to 50 by definition at all ages) and (2) the entry of higher-earning immigrants at older ages. These factors lead to very small changes in ranks for children who attend lower-tier colleges because the fraction of children attending elite colleges and the fraction of high-skilled immigrants entering the U.S. in their thirties are both relatively small.

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. 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 Figure IIa, this correlation rises sharply between ages 25 and 30, when it reaches 0.98. The correlation exceeds 0.99 starting at age 32, showing that one would reach very similar conclusions about the earnings ranks of students at each college if one were to measure their earnings at any point after age 32.

In sum, individuals' relative positions in the income distribution stabilize by age 32 for children at all colleges.[34] Of course, individuals' earnings *levels* continue to rise sharply during their thirties and forties, but this rank-preserving fanning out of the income distribution does not affect the rank-based analysis that follows. We therefore focus on children in the 1980-82 birth cohorts in our baseline analysis, for whom we measure earnings at ages 32-34 in 2014.

IV.B Children's Earnings Distributions by College

We now turn to characterizing the distribution of children's earnings conditional on their parents' incomes within each college. We focus primarily on differences in earnings outcomes between children from low- and high-income families *within* colleges (i.e., relative mobility) in this section. We present a more comprehensive analysis of differences in the level of outcomes *between* colleges in the next section.

We begin by examining the conditional expectation of children's ranks given their parents' ranks at the national level, pooling all children in the 1980-82 birth cohorts. The series in circles in Figure IIIa presents a scatter plot of the mean percentile rank of children (based on their individual earnings in 2014) vs. their parents' percentile rank (based on their mean household earnings when the children were aged 15-19). This relationship is almost perfectly linear, consistent with the findings of Chetty et al. (2014). Using an OLS regression, we estimate that a one percentage point (pp) increase in parent rank is associated with a 0.288 pp increase in the child's mean rank.[35] That is, children from the highest-income families end up 29 percentiles higher in the income distribution on average relative to children from the poorest families in the nation as a whole.

Next, we examine the rank-rank relationship among students who attend a given college. Figure

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

[35]This estimate is smaller than the baseline rank-rank slope of 0.34 reported in Chetty et al. (2014) because we use individual earnings rather than household income. We present estimates using household income below.

IIIa shows the rank-rank relationship among students at three of the colleges examined above: UC-Berkeley, SUNY-Stony Brook, and Glendale Community College.[36] To increase precision, we plot the mean rank of children in each college by parent ventile (5 pp bins) rather than percentile. The relationship between children's earnings and parents' incomes is much flatter within each of these colleges than in the nation as a whole. The rank-rank slopes, estimated using OLS regressions on the underlying microdata, are less than or equal to 0.06, one-fifth as large as the national slope of 0.30. This illustrates the main result of this subsection: children from low-income and high-income families who attend the same college have very similar earnings outcomes. That is, parent income is no longer predictive of children's outcomes conditional on college attendance.

Figure IIIb shows that this result holds more generally across all colleges. It plots the relationship between children's ranks and parents' ranks conditional on which college a child attends for colleges in three tiers: elite four-year (Barron's Tier 1), all other four-year, and two-year. To construct each series in this figure, we regress children's ranks on parent ventile indicators and college fixed effects and plot the coefficients on the twenty ventile indicators. The slopes are estimated using OLS regressions of children's ranks on their parents' ranks in the microdata, with college fixed effects. Among elite colleges, the average rank-rank slope is 0.065 on average within each college. The average slope is slightly higher for colleges in lower tiers – 0.095 for other four-year colleges and 0.11 for two-year colleges – but is still only one-third as large as the national rank-rank slope. The steeper slope could potentially arise because colleges in lower tiers are less selective and hence admit a broader spectrum of students in terms of abilities or because there is substantial heterogeneity in completion rates at lower-tier colleges, which may correlate with parent income.

Children from low- and high-income families at a given college not only have similar mean rank outcomes but also a similar distribution of earnings outcomes across all percentiles. Appendix Figure III replicates Figure III, replacing the outcome used to measure children's earnings by an indicator for being in the top quintile (earnings above approximately $58,000 at ages 32-34). Nationally, children from the highest-income families are 40 pp more likely to be in the top quintile than children from the poorest families. Conditional on attending an elite college, this gap shrinks to approximately 12 pp, and at certain colleges, such as UC-Berkeley, SUNY-Stony Brook, and Glendale Community College, the gap is even smaller, at 6-9 pp.

[36] We omit Harvard from this figure because the very small fraction of low-income students at Harvard makes estimates of the conditional rank for children from low-income families very noisy; the estimated rank-rank slope for Harvard is 0.112 (s.e. = 0.018). For the same reason, we combine the Ivy-Plus category with other elite colleges in Figure IIIb below.

Robustness Analysis. In Table II, we explore the robustness of these results using alternative income definitions and subsamples. Each cell of the table reports an estimate from a separate regression of children's outcomes on parents' ranks (estimated in the microdata), with standard errors clustered by parent income percentile. The first row of the table shows estimates from univariate OLS regressions in the full sample, as in the series in circles in Figure IIIa. The second row shows estimates from regressions that include college fixed effects, including only children who attend college at some point between ages 19-22. The remaining rows show estimates analogous to those in the second row, restricting the sample to students who attended elite colleges, other four-year colleges, or two-year colleges. As a reference, the first column reproduces our baseline specification, pooling all children in the 1980-82 birth cohorts, replicating the slopes reported in Figure 3. The remaining columns present variants of this specification that assess the robustness of our findings.

The intergenerational persistence of income might be low especially within elite colleges if children from high-income families at such colleges choose not to work (e.g., because they marry a high-earning college classmate). To assess this concern, Column 2 of Table II replicates the baseline specification, using an indicator for working (having positive individual earnings in 2014) as the dependent variable. In practice, children from high-income families are slightly *more* likely to work, even within elite colleges. To assess whether differences in rates of part-time work – which we cannot measure directly as we do not observe hours of work – might matter, we replicate the specification in Column 1 separately for sons and daughters in Columns 3 and 4 of Table II. Even for men, for whom the hours of work margin is likely much less important, the rank-rank slope is 0.09 within elite colleges, much lower than the national slope of 0.33. Conditioning on which college a child attends similarly reduces the rank-rank slope for women substantially, although women have lower rank-rank slopes both nationally and within colleges than men. Together, these results suggest that differences in labor force participation rates do not mask latent differences in the earnings potentials of children from low vs. high income families within elite colleges.

Next, we explore the sensitivity of the findings to using household-level measures of income instead of individual measures. Column 5 of Table 2 replicates Column 1 using household earnings (own plus spousal earnings) instead of individual earnings ranks for children. We continue to find a substantial decrease in the correlation between child and parent income within colleges when measuring earnings at the household level, but the degree of intergenerational persistence rises in all subgroups. Nationally, the rank-rank slope rises to 0.374 (from 0.288); within elite colleges, the slope rises to 0.131 (from 0.065). To understand the mechanism underlying this effect, in Column 6, we regress an indicator for the child being married (in 2014, between the ages of 32-34) on parents' income ranks. Nationally, children from the richest families are 37.5 pp more likely to be married than those from the poorest families. Within elite colleges, the gap in marriage rates remains at 15.1 pp. Hence, much of the increase in intergenerational persistence when measuring income at the household level is driven by the fact that children from high-income families are more likely to be married, even conditional on attending the same college. Put differently, colleges (especially elite ones) largely level the playing field between students from low and high-income families in terms of their individual earnings outcomes, but do not level the playing field in terms of rates of marriage to the same degree.

Finally, in Column 7 of Table II, we replicate the baseline specification using household income (Adjusted Gross Income), which adds capital income to household earnings. We find very similar results when using this broader income measure, as capital income is small for the vast majority of individuals.[37]

Discussion. The result that students from low- and high-income families within a given college have very similar earnings outcomes has several implications, especially for highly selective colleges. First, these results provide evidence against the concern that students from low socioeconomic status backgrounds may be "mismatched" at selective colleges. The mismatch hypothesis predicts that students from low-income families are made worse off by attending a higher-ranked college, for which they are ill-prepared or otherwise mismatched. To see how the results in this section weigh against this hypothesis, consider a student from a poor family attending an elite college. It is implausible that this student, had she attended a lower-ranked institution, would earn more than the students from high-income families at the elite college. Hence, the earnings of the students from high-income families at a given college provide an upper bound for the potential earnings of a low-income student at a lower-ranked school. Since students from low- and high-income families have very similar earnings outcomes, especially at elite colleges, this bounding logic suggests that low-income students who are admitted to elite colleges cannot be significantly over-placed on average.[38]

[37] Appendix Figure II demonstrates this result non-parametrically by replicating the national rank-rank series in Figure III for the household income and household earnings concepts. Below the 98th percentile of parental income, the mean household income and household earnings' ranks of children are virtually identical, showing that the difference relative to individual earnings is entirely due to spousal income except in the upper tail.

[38] These findings are consistent with prior research using survey data showing that the association between children's and parents' incomes or occupational status is much weaker among college graduates (e.g., Hout (1988); Torche (2011)). Our data show that conditioning on the specific college a child attends further reduces the correlation between children's and parents' incomes, and that this holds true even at elite colleges, where concerns about mismatch are most acute.

Second, these findings suggest that colleges do not pay a large cost, in terms of reduced earnings outcomes, for any affirmative action policies currently in place that favor the admission of low income students. There are two possible explanations for this conclusion. One explanation is that the marginal low-income student admitted because of affirmative action has very similar ability to other students admitted under regular admissions standards. That is, the "quality" supply curve for low-income students may be very flat in terms of potential earnings outcomes. Another possibility is that many elite colleges do not offer students from low income families a substantial admissions advantage relative to students from higher-income families in practice.[39]

V Differences in Mobility Rates Across Colleges

In this section, we combine the distribution of parents' incomes and children's earnings to characterize how rates of intergenerational mobility vary across colleges in our cross-sectional sample (1980-82 cohorts). We begin by presenting a case study comparing mobility rates at two universities in New York, Columbia and the SUNY-Stony Brook. We then show how the lessons from this case study generalize to other colleges, evaluate the robustness of the findings to alternative income and sample definitions, and explore what types of colleges have the highest mobility rates.

V.A Case Study: Columbia vs. SUNY-Stony Brook

We characterize intergenerational mobility at each college using a *mobility report card* that depicts the marginal distribution of parent incomes and the conditional distribution of students' earnings given their parents' incomes. Figure IVa presents mobility report cards for Columbia University, an Ivy-Plus college, and SUNY-Stony Brook, a large public university that is in the second tier of the Barron's selectivity rankings. The bars show estimates of the fraction of parents in each quintile of the income distribution for children in the 1980-82 birth cohorts, as in Figure Ia. The lines show estimates of the fraction of students from each of those quintiles who have individual earnings in the top quintile (i.e., above $58,000 at age 34).[40]

These mobility report cards echo the key findings from Sections III and IV above. Parent income distributions vary substantially across these colleges: a much larger number of students come from the top one percent at Columbia (13.7%) than Stony Brook (0.4%). Children's earnings outcomes

[39]For instance, students from high-income families may benefit indirectly from admissions preferences for legacy students or other non-academic preferences, offsetting other preferences that may be given to low-income students.

[40]We view reaching the top quintile as a plausible definition for "upward mobility" for much of the population, especially children from low-income families. We show how using different income thresholds to define upward mobility affects our conclusions below.

do not vary significantly with their parents' incomes: approximately 60% of students at Columbia and 50% of students at Stony Brook reach the top quintile across the parental income distribution.

We combine these statistics to construct measures of intergenerational mobility by defining each college's upward *mobility rate* as the fraction of its students who come from the bottom quintile of the income distribution and end up in the top quintile. A college's mobility rate is the product of low-income *access*, the fraction of its students who come from families in the bottom quintile, and its *success rate*, the fraction of such students who reach the top quintile:

$$\underbrace{P(\text{Child in Q5 and Parent in Q1})}_{\text{mobility rate}} = \underbrace{P(\text{Parent in Q1})}_{\text{access}} \times \underbrace{P(\text{Child in Q5} \mid \text{Parent in Q1})}_{\text{success rate}}$$

For instance, at Columbia, access is 5.0% and the success rate is 61.2%. Therefore, the mobility rate at Columbia is $5.0\% \times 61.2\% = 3.1\%$. That is, 3.1 out of 100 students at Columbia come from a family in the bottom quintile and reach the top quintile. Stony Brook (51%) has a slightly lower success rate (51%) than Columbia, but a much higher level of low-income access (16.4%). As a result, Stony Brook has a bottom-to-top-quintile mobility rate of 8.4%, channeling nearly 3 times as many children from the bottom to the top of the income distribution as Columbia.

This comparison illustrates that bottom-to-top-quintile mobility rates vary substantially across colleges because there are large differences in access across colleges with similar success rates. Highly selective colleges such as Columbia – where the admission rate was 7% and students' average SAT scores were 1480 in 2013 – generally have the highest success rates. However, certain less selective universities such as Stony Brook – where the admission rate was 40% and average SAT score was 1250 – have comparable top-quintile success rates while offering much higher levels of access to low-income families. These differences cannot be explained purely by observable differences in institutional characteristics such as differences in students' majors. For instance, approximately one-third of students at both Columbia and Stony Brook major in science, technology, engineering, or mathematics (STEM) or business, two of the highest-paying fields.

The relative similarity of students' outcomes despite the differences in the socioeconomic background of students at these colleges suggests that Stony Brook either (a) admits more students than Columbia from low-income backgrounds who have high earnings potential or (b) generates causal effects on earnings comparable to Columbia for a much larger number of low-income students. Although the descriptive comparison in Figure IVa cannot distinguish between these selection and value-added effects, the fact that students at Stony Brook have lower SAT scores and other observable characteristics than those at Columbia at least calls for careful consideration of the second explanation. More broadly, our analysis below highlights a number of high-mobility-rate colleges like Stony Brook that deserve further study as potential "engines of upward mobility."

In Figure IVb, we present analogous mobility report cards for upper-tail success. Here, we examine children's chances of reaching the top 1% of the income distribution (earning more than $197,000 at age 34) instead of the top quintile; the statistics on the parent income distribution are the same as in Figure IVa. Unlike with top-quintile success rates, Columbia has a much higher rate of upper-tail success than Stony Brook: 15% of students at Columbia from the bottom quintile reach the top 1%, compared with 2% at Stony Brook. As a result of this 7-fold difference in success rates, Columbia's upper-tail mobility rate – the fraction of students it channels from the bottom quintile to the top 1% – is 0.75%, more than twice as large as at Stony Brook, where the upper-tail mobility rate is 0.32%. Hence, while Stony Brook is a pathway to the top quintile for many low-income students – which is presumably a reasonable metric for "upward mobility" for much of the population – it does not offer a pathway to upper-tail success for a large number of students. More broadly, we find that upper-tail success is highly concentrated at elite schools like Columbia with very high levels of instructional expenditure and large endowments, and no university in the U.S. currently offers both high rates of upper-tail success and substantial low-income access.

In the rest of this section, we show how the results from this case study generalize across the 2,199 colleges in our cross-sectional sample.

V.B Bottom-to-Top Quintile Mobility Rates

In Figure Va, we characterize the variation in mobility rates across colleges by plotting each college's success rate (P(Child in Q5 | Parent in Q1)) vs. its level of low-income access (P(Parent in Q1)). These two measures can be interpreted as the quantity (access) and quality (success rate) of work that each college does – either in terms of selection of students or value-added – in contributing to intergenerational mobility.[41] There is substantial variation in both of these dimensions across colleges. On the "vertical" dimension, the 10th percentile of the (enrollment-weighted) distribution of success rates is 7.1%, while the 90th percentile is 32.8%. On the "horizontal" dimension, the 10th percentile of the distribution of the distribution of access is 3.7%, while the 90th percentile is 21.0%.

[41] We stress that the "contributions" to intergenerational mobility should be interpreted as an accounting measure, not the causal effect of a given institution on intergenerational mobility.

In general, greater access is correlated with lower success rates, as the institutions which admit a large number of low-income students, such as community colleges, tend to be the least selective. However, the (enrollment-weighted) correlation between success rates and access is only -0.5, leading to considerable variation in mobility rates – the product of access and success rates – across colleges. Mobility rates are higher for colleges in the upper right quadrant of the figure. To illustrate the magnitude of the variation, we plot isoquants representing the set of colleges that have mobility rates at the 10th percentile (0.9%), median (1.6%), and 90th percentile (3.5%) of the enrollment-weighted distribution across colleges. The enrollment-weighted standard deviation (SD) of mobility rates of 1.30%. As a benchmark, note that the average mobility rate in the nation as a whole is 1.7%. If all colleges had mobility rates comparable to those at the 10th percentile, we would have half as much bottom-to-top-quintile income mobility among those who attend college as we currently do in the U.S. If in contrast all colleges has mobility rates comparable to those at the 90th percentile, we would have mobility rates comparable to a society with perfect relative mobility, where children's outcomes are unrelated to their parents' incomes and 4% of children would make the transition from the bottom to top quintile. Hence, the range of mobility rates across colleges is substantial relative to plausible benchmarks.

Which colleges have the highest mobility rates? Table IIIa lists the colleges with the ten highest mobility rates among colleges with 300 or more students per year (excluding the smallest 5% of colleges in our sample). The college with the highest mobility rate is California State University–Los Angeles, where nearly 10% of students come from a family in the bottom quintile of the income distribution and reach the top quintile themselves. Cal State achieves this high mobility rate by combining a success rate of 29.9% – close to the 90th percentile across all colleges – with low-income access of 33% – well above the 95th percentile across all colleges. SUNY-Stony Brook ranks third at 8.4%, while the City of University New York system ranks sixth, with an average mobility across across its 17 campuses of 7.2%.[42] Eight out of the ten are public institutions, with Pace University in New York as the only private not-for-profit school and Technical Career Institutes as the only for-profit school.

As Table IIIa shows, the colleges with the highest mobility rates tend to be mid-tier public colleges that combine fairly high success rates with high levels of access. Colleges that have the highest success rates tend to have very low levels of access and thus channel relatively few children from the bottom to the top. For instance, the twelve Ivy-Plus colleges, highlighted in blue circles in

[42] When broken out separately by campus, six of the CUNY campuses have mobility rates in the top 10.

Figure Va, have a mean success rate of 58%, but average access of 3.8%, leading to a mean mobility rate of 2.2%, slightly above the national median. Flagship public universities, such as UC-Berkeley and the University of Michigan–Ann Arbor, have somewhat higher access (5.2%), but on average considerably lower success (), so that their average mobility rate is lower than that of Ivy-plus schools.[43]

The preceding examples illustrate one of the central results of this section: much of the variation in mobility rates across colleges is driven not by "vertical selection" – differences in students' success rates across colleges that might simply admit students with different levels of ability – but rather "horizontal" variation in access across colleges that have similar success rates. We quantify the degree of horizontal variation in access by measuring the standard deviation (SD) of access conditional on success rates. We compute these SDs non-parametrically by dividing colleges into 50 equal-sized (enrollment-weighted) bins based on their success rates. For instance, consider the highlighted schools in Figure Vb, which are at the 75th of success (i.e., 37th to 39th bin) with success rates of roughly 21%. Among others, this group includes schools such as Bowling Green State University (access = 3.6%), Louisiana Tech University (access = 10.7%), and Glendale Community College (access = 32.4%). Across this swath of colleges, the SD of access is 6.9%. We calculate a similar conditional standard deviation within each slice and then average across the full sample.

The raw unconditional SD of access is 7.6% across all colleges. The average SD of access conditional on success rates is 6.2%; that is, 80% of the variation in access comes *within* narrowly defined success tiers. Part of this horizontal variation in access comes from the mass of schools at the bottom of Figure Va, which have very low rates of success and hence do not contribute significantly to upward mobility. But even among colleges with above-median success rates (success rates above 14.4%), the conditional SD of access remains at 5.41%, two-thirds as large at the unconditional SD. Indeed, even among schools with success rates comparable to the Ivy-Plus colleges, the SD of access is 3.3%, as there are a number of colleges (such as SUNY-Stony Brook) that have success rates comparable to these colleges, but offer much higher levels of access. Table IV also shows that the large degree of horizontal variation in Figure Vb holds even within CZ, and even after correcting for measurement errors in access and success.

The considerable variation in access even among colleges with comparably high success rates

[43] As discussed in Section II.B, in some cases (e.g., the University of Illinois) we cannot separate the flagship campus (Urbana) from other campuses. We exclude such institutions for these calculations.

is an encouraging result because it shows that there are educational models that achieve good outcomes while offering access to a large number of low-income students. The existence of institutions such as SUNY–Stony Brook and California State–Los Angeles provides a template for other colleges seeking to increase their mobility rates, whether via changes in their admissions criteria or through changes in their educational model. If all of the variation in mobility rates were instead due to "vertical" variation in success rates, then replicating the success of the highest mobility rate institutions would potentially be much more challenging. For example, it may be more feasible for Bowling Green to move "horizontally" and increase access to the levels observed at colleges with comparable success rates – e.g., Louisiana Tech or Glendale Community College – than for Bowling Green to move "vertically" and increase the success rates of its student body to match those of much more selective colleges with higher success rates such as Harvard or UC-Berkeley.

V.C Sensitivity Analysis

In this section, we explore the sensitivity of the preceding results to various potential concerns.

First, it is possible that the variation in mobility rates is driven by characteristics of a college's location – for instance, the quality of the local labor market, or local price levels – rather than characteristics of the college itself. To investigate this possibility, we examine the extent to which mobility rate varies within vs. between commuting zones. We begin in Figure Vc by directly looking at the variation in access, success, and mobility rate within the Los Angeles CZ (where two of the top ten from Table 3 are located). There is substantial variation in both access and success between colleges in this one city. More generally, we find that the standard deviation of mobility rate within CZs is 0.87%, as compared with 1.30% nationally, so that 67% of the variation in mobility rate is within CZ. This figure is a lower bound on the effect of city-specific characteristics such as labor market strength on mobility rates, since some of the between-CZ variation may be driven by differences in college quality. Figure Vb also reinforces the extent to which the variation in mobility rate exists even between schools with very similar levels of success, rather than across schools with different levels of success. For instance, even within Los Angeles, Pepperdine (access =4.3%, success = 43.1%) has a much lower mobility rate than UC-Riverside (access = 14.7%, success = 41.0%).

We also address concerns about differing local price levels directly by adjusting both parents' and kids' incomes for differences in local price levels using a price index based on local house prices and the ACCRA price index (see Chetty et al. 2014). In practice, this adjustment has small effects on our estimates: the (enrollment-weighted) correlation of the raw and cost-of-living-adjusted mobility rates is 0.96. Intuitively, it is not surprising that local price levels do not drive variation in mobility rates, since upward mobility is fundamentally about the difference in incomes between kids and parents. Local differences in price levels instead move both parents and kids up or down together. We therefore conclude that characteristics of a college's location explain a relatively small share of variation in mobility rates.

Second, it is possible that differences in areas of study across colleges explain the variation in mobility rate. This force is clearly important for certain colleges in our data; for instance, Vaughn College of Aeronautics and Technology has a very high mobility rate, probably because it trains students in a highly technical and well-compensated field. Choice of field is an important element of student outcomes, and the causal effect of attending Vaughn, relative to a liberal arts school, is probably positive. Still, the data suggest that differences in major account for relatively little of the variation across schools in mobility rates overall. To demonstrate this, Figure VIa shows the distribution across fields of study at schools with a mobility rate in the top 10%, and other schools. The distribution is remarkably similar. The share of students in STEM fields is slightly larger at top mobility rate schools, but by just 3 percentage points. There are actually more students studying fields in business at lower mobility rate schools. More generally, the distribution of majors has very low explanatory power for mobility rate; the adjusted R-squared of a regression of mobility rate on a eight variables measuring the share of students in each of the fields in Figure VIa is just 1.8%. Figure VIb replicates Figure VIa, restricting to Ivy-plus schools and top 10% mobility rate schools with success rates comparable to the Ivy-plus; here too the distribution of majors is very similar. The explanatory power of the distribution of majors is higher within these top schools, but still 90% of the variation in mobility rates is unrelated to field of study. We conclude that field of study may be an important factor a certain institutions, but that it explains a relatively small share of variation in mobility rates.

Third, it is possible that variation in the mobility rate is driven by the details of our choice of individual labor earnings as the primary measure of income. For instance, individuals who focus on household production may have far lower income, by our definition, than was available to them had they entered the labor market. Measuring incomes at ages 32-34 may make this an especially important concern, since those years are prime child-bearing years for highly educated women. We address this concern in two ways. First, Appendix Table VII replicates our main findings but restricting the sample to men; second, Appendix Table VII uses household income (AGI) instead of individual earnings to calculate success rates and mobility rates. These adjustments are very important at certain schools – for instance, Brigham Young University has a substantially higher mobility rate using either of these two alternatives due to the relatively large fraction of female students who do not participate in the labor market in their mid-thirties. Nevertheless, the broad patterns in student outcomes and mobility rates still hold, and the correlation between our baseline measures and these alternative measures are above 0.95.

Fourth, it is possible that outstanding mobility rates at certain colleges reflects the large presence of children from immigrant families. These households may have incomes that are particularly low relative to other household characteristics - for instance, a parent with a Ph.D. who works in a relatively low-skill job. Such children would of course also appear in national statistics on intergenerational mobility as highly upwardly mobile, but if they cluster into certain colleges they may have an outsized influence on our statistics. To investigate this possibility, we analyze the relationship between our statistics the share of students from different racial and ethnic groups (as reported in IPEDS) as an imperfect proxy. Indeed, the share of students who Asian at each school is highly correlated with the mobility rate, as is the share of Hispanics.[44] However, further investigation suggests that higher upward mobility from immigrant children is unlikely to explain a large fraction of differences across colleges in mobility rates. More likely, the correlation between racial and ethnic shares and mobility rates reflects the fact that the children of immigrants are disproportionately likely to attend certain schools which have, for other reasons, very high mobility rates.

To see this, consider the case of Asians, a demographic subgroup that has particularly high levels of upward mobility. When we regress the success rate on the fraction of Asians at each school (excluding outliers beyond the 90th percentile of % Asian), the coefficient is 1.4 and significantly greater than 1. Since the mechanical effect of each extra Asian student can be at most 1 extra success story, this coefficient must be generated in part by an ecological correlation between Asian share and the characteristics of the school or other students. To further explore what fraction of variation in mobility rates can be directly explained by the presence of Asian students, we note that Asians are 11 percentage points more likely than individuals of other races to be in the top 20% of the income distribution for 30-34 year-olds in the 2015 Census.[45] We then calculate an

[44] IPEDS does not identify children from immigrant families directly. Among the ethnic and racial groups reported, Asians and Hispanics have the highest share of foreign-born US residents aged 50-75 in the 2013 Current Population Survey.

[45] These statistics are derived from 2015 Census Table PINC-01, Subtables 1.1.1 and 1.1.7.

"adjusted" success rate by subtracting 0.11 times the Asian share from the raw success rate at each school, and calculate an adjusted mobility rate as the product of the access and the adjusted success rate. The correlation between the original and adjusted mobility rates across colleges is greater than 0.99. The presence of Asian students is largest in California; but still, the correlation between the adjusted and original mobility rates within California schools is 0.94. We therefore conclude that the demographic mix of students does not mechanically drive a large share of the variance in mobility rates.

Finally, does the mobility rate place too much emphasis on access, as opposed to outcomes? For instance, a hypothetical school that admitted only poor students (access = 100%) would rank among the top 10% of colleges on mobility rate if just 4% of students reached the top quintile. While this hypothetical would be perverse, in practice schools do not seem to achieve high mobility rates by combining very high access with very low mobility. For instance, nine of the top ten schools in Table 3 have success rates above the enrollment-weighted median success rate of 17.9% in our sample. Another way to assess the sensitivity of our measure to such perverse cases is consider alternative measures of mobility rate. A generalization of our baseline statistic would calculate the mobility rate for each school as

$$mobility\,rate = access \times (success - X)$$

where X is the "benchmark" against which outcomes at a given school are measured. Our measure implicitly uses a benchmark of 0%, which admits the hypothetical scenario above. In Appendix Table VI, we present similar results using two alternative benchmarks: the success rate of 3.9%, and the average community college success rate in a given commuting zone (which is 11.7% on average). As a reference, students who do not attend college by age 28 have success rates of 3.9%, lower than 97% of colleges. 34.5% of these students come from the bottom quintile, which is at the 99th percentile of the distribution of access across colleges. These higher benchmarks would rule out a high mobility rate among schools with very low success rates. Empirically, though, these alternative mobility rates are once again highly correlated with our baseline measure; the "no college" benchmark mobility rate has an enrollment-weighted correlation of 0.98 with the baseline measure, and even the more conservative "local community college" benchmark has a correlation of 0.54.

V.D Correlates of Mobility Rates

Having established that the variation in mobility rates is a robust phenomenon that is not sensitive to measurement issues or mechanical differences across colleges such as variation in fields of study, we now explore whether there are other characteristics of colleges that systematically predict variation in mobility rates. Table 5 shows univariate correlations between mobility rate and various college covariates. We weight all statistics by enrollment. In the first panel, we study differences across college types. Although public schools dominate the top ten mobility rate schools in Table 3, they have only slightly higher average mobility rates on average than private schools because there are many public schools that also have lower mobility rates than private schools.

Figure VId demonstrates the source of this null result by separating public, private, and for-profit schools in our basic scatterplot of access vs. success rates. Intuitively, public control does not correlate strongly with mobility rates because it correlates in opposite directions with access and success. Private schools tend to have higher success rates and lower access; public schools have lower success rates but higher access. These correlations nearly cancel each other out in the full sample. The same is true for four-year schools, which have slight higher average mobility rates. Notably, for-profit schools in this sample have higher average mobility rates, though there is great variance (as for-profits make up some of the best and worst mobility rate schools).

In contrast to college type, college selectivity (as measured by one minus the admissions rate) is positively and significantly correlated with mobility rates. This correlation is driven entirely by selective public schools; private schools do not, on average, have a mobility rate that covaries with selectivity. Mobility rates have a small yet significantly negative correlation with endowment per capita. There is essentially no correlation at all with enrollment.

Finally, we explore the correlations between mobility rates and measures of spending and college cost. First, we find a *positive* correlation between the average net cost for poor students and the mobility rate, perhaps as a reflection of an institution's selectivity; we find no correlation with "sticker" price tuition, however. Second, we find positive correlations between both average faculty salaries and per-student instructional expenditures. These results are consistent with a literature linking instructional inputs to student outcomes. Importantly, these correlations hold within both the public and private schools, so that the relationship between spending and mobility rate is driven not by the large spending gaps between public and private schools (which we do not find correlates with mobility rates).[46]. For example, the median instructional expenditure per student

[46] Correlations with instructional expenditure are 0.158 and 0.107 for public and private schools, respectively, and

across schools in the top 10% of mobility rates is just $5,500, as compared with $80,700 in ivy-plus schools. This disparity highlights the extent to which high mobility rate schools may represent a scalable model of education to increase intergenerational mobility.

The key takeaway from this correlational analysis is that differences in bottom-top-quintile mobility rates across colleges cannot be systematically predicted based on observable characteristics. This underscores the importance of using data on observed student outcomes and parent incomes in order to assess which colleges have the highest mobility contributions.

V.E Upper Tail Mobility Rates

Figure VIIa plots upper tail success against access for our full group of schools, mirroring Figure Va. As illustrated by the case study of Columbia in Section V.A, it is clear that Ivy-plus colleges have distinctly high upper-tail success rates. In fact, despite the far lower access at these schools, a number of them appear in the top ten upper-tail mobility rate list shown in Table IIIb.

The patterns in Figure VIIa and Table IIIb differ from those for the top quintile measure of success in two important ways. First, elite schools (including both elite private and public schools) stand out with very high rates of upper-tail success shared by few other schools. In contrast, elite schools have high top quintile success rates, but so do many other schools. Quantitatively, there are 49 other schools within the range of Ivy-Plus upper-tail success rates (6.9% to 18.5%), while 135 schools have top 20% success rates comparable to those of Ivy-Plus colleges.

Second, only schools with very low access generate very high upper-tail success rates (including both Ivy-Plus and other schools). For instance, no schools with access greater than 6.2% generates upper-tail success rates greater than 10% (which is roughly the 2nd lowest level of upper-tail success rates across all ivy-plus schools). Other schools outside the Ivy-Plus group with upper-tail success rates above 10%, such as Harvey Mudd College, Rice University, and Northwestern, are also elite private schools with similar levels of access. To formalize this point, we calculate the SD of access among schools with similar upper success rates to Ivy-plus colleges. This SD is just 1.03%, which is essentially the SD of access within the ivy-plus schools themselves (equal to 0.94%). As noted above, the corresponding statistic for access conditional on ivy-plus levels of top quintile success is much higher at 3.42%.

Table IVb shows that four-year and private schools have much higher upper-tail mobility rates, and selectivity has a univariate correlation of 0.56. As shown in Figure 8B, the increased success

with average faculty salary are 0.158 and 0.279, respectively.

rates in private schools are far stronger for the top 1% outcome, more than counterbalancing the lower access rates. Schools that are smaller, with larger endowments, higher completion rates, and greater STEM shares, have higher mobility rates. Spending is also strongly related to upper-tail mobility rates, as measured by higher tuition, instructional cost per student, and average faculty salary. Given the very high tuition at these schools, it is not surprising that net cost of attendance for poor students is negatively correlated with mobility rates. Ivy-Plus schools are well described by all of these characteristics. However, these correlations hold even excluding Ivy-Plus (or even all selective institutions) from the sample, as shown in Figure VIIb, which presents a binned scatterplot of the relationship between top 1% mobility rates and instructional expenditures per student in 2000. It is clear from this figure that schools (like the Ivy-plus) which spend a very large amount per student have far higher top 1% mobility rates than schools spending less, but there is also a clear (and perhaps stronger) relationship among schools spending less than $10,000 per year (roughly the 90th percentile of the count-weighted distribution of costs). We conclude that these correlations with the top 1% mobility rate are stable throughout the distribution of schools.

Despite this finding, it is important to note that elite private colleges are far from the only path to the top 1% for children from poor families. Figure VIII shows the distribution of "success stories" – that is, children from the poorest quintile of households that make it to the top of the earnings distribution – across college types, for both top quintile and top 1% outcomes. Even focusing children from poor families that reach the top 1%, just 5.0% of "success stories" come from Ivy-Plus colleges. This small share arises because Ivy-Plus colleges are small, especially in terms of the number of low-income students they enroll. Thus, larger colleges and those with more poor students account for many more success stories even though their upper-tail success rate is much lower. Ivy-Plus schools (and elite schools more generally) account for an even lower share of top 20% success stories, as shown in Figure VIIIb, simply by virtue of their relatively small size.

VI Trends in Access and Mobility Rates

The cross-sectional sample that we have used for our analysis thus far consists of children in the 1980-82 cohorts, who typically attended college in the early 2000s. Since then, there have been a number of important changes that may have affected access and success rates across colleges. For instance, certain elite private schools have enacted changes intended to increase the fraction of students from low-income families, for instance through "need blind" admissions policies and increased financial aid budgets. At the same time, "sticker price" tuition has increased sharply at these same schools, which may have discouraged qualified students from applying. Meanwhile, at many public institutions, funding levels did not keep pace with the rising numbers of students, leading to tuition increases and budget cuts that may have limited access (Deming and Walters (2017)).

In this section, we present a descriptive analysis of trends in access and mobility rates for students born between 1980 and 1991. Because we cannot observe earnings outcomes at a sufficiently old age for students in more recent cohorts, we focus primarily on changes in access. We then show how these changes in access correlate with changes in success rates in early cohorts and make predictions about changes in mobility rates on this basis.

VI.A Changes in Access

We begin by describing key changes in access over this time period for different types of institutions. To reduce noise, we focus on trend changes, defined as the coefficient from a count-weighted regression of access on a linear cohort trend. For ease of interpretation, we then multiply these coefficients by 11 to provide estimates of the change between the 1980 and 1991 cohorts.

We begin by summarizing trends in access for different groups of colleges during our sample period, in Figure IXa. The solid line shows that access has risen on average at colleges in the U.S. by 1.95 percentage points from 10.7% to 12.6%. This reflects the sharp increase in college attendance for children from low-income families, relative to children from richer families. Across different college tiers, this rising average access is most apparent among two-year and for-profit schools. At more selective institutions, however, access has increased only slightly. For instance, access increased by just 0.65 percentage points at Ivy-plus schools, and access actually fell by 0.46 percentage points at elite schools outside the Ivy-plus group. Statistics for changes in the fraction of students from the bottom 60% of the income distribution (in Appendix Figure V) show similar patterns.

Our data paint a less sanguine picture for expanding access at elite schools than do statistics on the share of Pell-eligible students. For instance, Pell share statistics show an increase from 11.3% to 16.7% at Ivy-plus schools, in comparison to just an increase of 0.5 percentage points in the fraction of students from households in the bottom 40% of the income distribution in our data. The reason for the discrepancy is two-fold. First, Congress raised the effective income eligibility threshold for Pell Grants significantly in both 2001 and 2009, expanding the family income levels that qualified for Pell. Second, bottom quintile income thresholds fell sharply over time, expanding the family income ranks that qualified for Pell even holding policy thresholds constant in real terms.[47] Importantly, our data do not imply that the financial aid expansions and other efforts from elite private schools to expand access for low-income students were ineffective – indeed, it is possible that access at Ivy-plus colleges would have fallen were it not for these policies. It is clear, however, that economic diversity did not expand as much as Pell shares suggest during our sample period.

Perhaps more important than changes in average access by college tier has been the heterogeneity of changes within group. We illustrate these key patterns in Figure IXb, which looks at access over time in selected colleges. Harvard was one of the most aggressive elite schools in pushing to expand access during our sample period, and it resulted in broad pattern of increasing access. The fraction of students from the poorest fifth of families increased from an average of 3.1% in the first half of our sample to 4.8% in the second half; the gains were even larger at Harvard for students from the bottom 60%, which increased from 16.1% to 20.9% over the same interval. Our data also show a large jump in access between the 1986 and 1988 cohorts, which corresponds with Harvard's large financial aid expansion (as previously studied in Hoxby et al. 2006). Still, even these gains at Harvard are small when compared to the large existing differences in access, for instance between Harvard and schools such as UC-Berkeley and SUNY-Stony Brook. These high mobility-rate schools saw steadily declining access over our sample period; UC-Berkeley fell from 9.0% to 7.9% from the first to the second half of our sample, and SUNY-Stony Brook saw access fall from more than 15% in the earliest years of our sample to just 10% by the end. And unlike at other community colleges, which in Figure IXa saw increasing access, high mobility-rate community colleges like Glendale Community College experienced a large decline in access.

Figure IXc generalizes the pattern of falling access at high mobility-rate schools from Figure IXb. The middle series plots the changes in access over time for the schools in the highest decile of mobility rates.[48] Despite the overall increase of 1.95 percentage points in access, the highest mobility schools saw a 1.3 percentage point decline in access over our sample period. Figure IXc also shows that these declines were not driven simply by broader trends affecting schools with high success rates, or schools with high average access. Schools in the upper half of success rates showed no change in access over our sample period, and schools in the top half of average access in

[47]We discuss these changes, and their consequences for reported Pell-share statistics, in more detail in Appendix D.

[48]Defining these top mobility-rate schools based on access in the first cohorts of our sample, as we did in Section 5 of this paper, could lead to mechanical downward trends in access over time due to mean reversion. We therefore define the mobility rate for Figure 9C using the average access over our sample period.

fact showed a 2 percentage point increase in the representation of low-income students, mirroring national trends.[49]

Table VI further explores the pattern of declining access at high mobility-rate schools. Column 1 replicates the main result in Figure IXc by regressing access on a cohort trend, a dummy for the top-decile mobility-rate schools, and the interaction. Relative to other schools, top mobility-rate schools saw access fall by 2.6 percentage points over our sample. Columns 2-4 control independently for average access and success rates (and their interactions with a cohort trend) to show that the pattern in top mobility-rate schools is really a phenomenon specific to that group and not simply driven by general trends affecting high access or high success-rate schools. When controls for both average access and success rate are included in Column 4, the trend change in access at top mobility rate is still a highly significant 2.2 percentage points. Column 5 add CZ fixed effects to the specification in Column 4; the declining access pattern is, if anything, stronger comparing only schools within the same city. Finally, Column 6 shows that adding fixed effects for college tier to the specification in Column 5 does not affect the results.

VI.B Changes in Mobility Rates

How did the changing rates of access across colleges during our sample period affect mobility rates? Since the youngest cohorts in our data are not yet old enough for reliable measurements of success rates, we exploit changes in access in the first years of our panel in order to project how changes in access over the whole period might have affected mobility rates.

We begin by correlating changes in success rates and access in the first five years of our data, the 1980-84 cohorts. These children are aged 30-34 at the time of income measurement in 2014, which is essentially old enough (even at the most elite schools) to measure permanent income ranks quite accurately (Figure IIa). Then, to exploit broad trends in access and success rates rather than sharp year-to-year fluctuations (which may be driven by sampling fluctuations in the number of students from low-income families at each college), we first estimate college-specific trends in access and success rates over the 1980-84 cohorts. We then regress the trend in success rate on the trend in access, including college-tier fixed effects so that the correlation is identified from comparisons in trends across broadly similar colleges.

Figure Xa shows this relationship for all colleges. The coefficient is just -0.082; to put this in

[49] We use the "upper half" cutoffs since roughly 10% of schools are in the upper half of both the success rate and the average access distribution. More generally, however, these facts hold for other definitions of "high access" or "high success rate" schools.

perspective, the decline in access of 2.6 percentage points for high mobility-rate schools within tier (in Table 6, Column 6) predicts a decrease of just 0.2 percentage points in success rates (relative to a base in the pooled sample of 23.5%). Even for some of the high mobility-rate schools like Glendale Community College, in which we see declines of more than 10 percentage points in access, we predict that success rates increase by just 2 percentage points. It therefore does not appear that declining access was offset by increases in success rates. Therefore, in order to predict changes in mobility rates over time, we simply assume a constant success rate.

Figure Xb demonstrates how mobility rates have changed over time in two key sets of schools. Ivy-Plus schools have seen small increases in access that increase the average mobility rate from 2.17% to 2.24%. The ten colleges with the highest mobility rates, as ranked by their mobility rate based on mean access from 1980-1991, have seen large declines in access that decrease the average mobility rate from 8.3% to 6.1%. At the beginning of our sample period, these colleges - for instance, CUNY, SUNY-Stony Brook, Laredo Community College, and Glendale Community College - stood out with exceptionally high success rates for a very high share of poor students. Over time, however, whatever made these schools exceptional has faded. These results suggest that the colleges that may have offered many low-income students pathways to success are becoming less accessible to them.

VII Conclusion

Using new data covering all college students from 1999-2013, this paper has characterized the income distributions of parents and children at each college in the U.S. Both parents' incomes and students' earnings outcomes vary significantly across colleges, leading to substantial variation in rates of upward mobility across colleges. These differences in mobility rates raise the possibility that increasing low-income access to colleges with good student outcomes could increase the overall contribution of higher education to upward mobility. Although our descriptive analysis does not shed light on specific policies to achieve that goal, it does yield a set of broader lessons that can help guide future work on these issues.

First, low-income students admitted to selective colleges do not appear over-placed, as their earnings outcomes are similar to those of their peers from higher income families. This result mitigates the concern that attending a selective institution may be detrimental for students from disadvantaged backgrounds, providing support for policies that seek to bring more such students to selective colleges.

Second, efforts to expand low-income access often focus on elite colleges, such as Ivy League universities. Although these highly selective colleges have excellent outcomes, expanding access to the high-mobility-rate colleges identified here – such as California State–Los Angeles, the City University of New York, and the University of Texas–El Paso – may be more critical. These colleges have very good outcomes while admitting large numbers of low-income students. Since they are not exceptionally selective (e.g., in terms of SAT scores), it is plausible that they have high value-added relative to other colleges with similar applicant pools – a hypothesis that can be tested using quasi-experimental or experimental research designs in future work. If these colleges do have high value-added, they could provide a scalable model for increasing upward mobility for large numbers of students, as they have median annual instructional expenditures of \$6,500 per student, far lower than median instructional expenditure of \$87,000 per student at elite private colleges.

Finally, recent trends in access – a decline at colleges with the highest mobility rates and little change at elite private colleges despite their efforts to increase financial aid – call for a re-evaluation of policies at the national, state, and college level. For example, it may be worth considering changes in admissions criteria, expansions of transfers from the community college system, or outreach efforts targeted at promising students in primary school before they begin applying to college. We hope the new statistics constructed in this study will help researchers develop and test such interventions.

ONLINE APPENDICES

A. Sample Construction and Income Definitions

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. This file includes information on year of birth and gender for all persons in the United States with a Social Security Number or an Individual Taxpayer Identification Number.[50] Our sample of children are all individuals born in cohorts 1980-1991. 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 (not yet deceased by 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 the year the child turns 17.[51] If the child is not claimed that year on any 1040 tax form, then we go back one year (when the child turns 16), and so on until we find a year when the child is claimed (up to the year when the child turns 12). Hence, we use up to 6 years (from age 17 to age 12) to find a parental match. If no such parental match is found, then the child record is discarded.[52]

The matching parent(s) can be either married in which case the child is defined as having two parents, or the matching parent(s) can be single in which case the child is defined as having a single parent. If the matching parents are married but filing separately, we assign the child both parents. Importantly, once we match a child to parent(s), we hold this definition of parents fixed regardless of subsequent dependent claims or changes in marital status. 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, though spouse income will be included in our definition of parent income because we measure parent income at the family

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

[51] 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 support themselves for more than 50% cannot be claimed as dependents. See IRS Publication 501 for further details.

[52] Very few children are unclaimed on tax returns (as children generate generous refundable credits to claimers). Therefore, the discarded children were almost all non US-residents when they were aged 12-17. As the tax data start in 1996, for the 1980 cohort, we can only match up to age 16; for the 1981 cohort, up to age 15, etc.

level in our baseline analysis (see below). 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 the child if the child resides with them. Our goal is to measure the economic resources of the family the child was in shortly before attending college, hence our choice of age 17 for matching. We do not pick a later age to find parents (such as 18 or 19) because a significant fraction of children leave home at that age and differentially so across income groups.

Finally, we discard children whose parents have negative income on average over the 5 year time window when they are aged 15-19 (the exact parental income definition used is discussed below).

*Income Definitions.*We measure parent income at the family (household) level using the standard concept of Adjusted Gross Income from the 1040 form that sums all forms of income (pre-tax) including both labor and capital income. In the case of a single parent associated with the child, parental income is defined as the 5-year average Adjusted Gross Income of the parent when the child is aged 15 to 19. Note that this income obviously includes only the income of the sole parent when the sole parent is single but it also includes the income of the spouse in any year the sole parent is married. In the case of two parents, we take the average of the income of each of the two parents (and then average over the 5-year span). If the two parents stay married together all 5 years, this produces the straight family income of the two parents (as each parent has the same AGI). If there are marital changes, our definition follows each parent separately.

In any year a parent is not a primary or secondary filer on a 1040 form (non filing parent), we estimate his or her income as the sum of W-2 wage income and unemployment benefits from form 1099-G, which are the most common forms of income for non-filers.[53] We discard children whose parents have strictly negative average income over the five year window (as 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). Such cases are very rare. Note that we do not impose any restriction on the death status of children. To be in our data, the child needs to be alive at age 12-17 so that a successful match to parents can be done. However, a child can die at any later age. We do not impose any further restriction because death is highly correlated with socioeconomic status. Naturally, a deceased child will have zero earnings.

In our baseline specification, childrens' earnings are measured as the sum of individual wage

[53]We do not include Social Security and Disability Benefits (which are other common income sources of non-filers) because such benefits are basically tax exempt for low income parents and hence are not included in AGI. We do not include this form of income in our income definition because it is not reported systematically on the 1040 form when such income is non taxable. Information returns on such income start in 1999 (instead of 1996 for the 1040 form). Hence, we would not be able to measure such income well for the earliest cohorts of children we consider).

income and self-employment income for year 2014. For a child who is a non-filer (neither a primary nor a secondary filer on any 1040 return), individual earnings are defined as the sum of wage income from W2-forms. For a child who is a single filer, individual earnings are simply defined the sum of wage income on the form 1040 and self-employment income from Schedule SE on the 1040 form.[54] For filers, we use 1040 wage income (instead of W2 wage income) because 1040 wage income also includes wages earned abroad, which can be significant for children who attend elite schools. In particular, children who move abroad (but are US citizens) are required to file standard tax returns and report their worldwide income, including any foreign earnings. 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 W2-wage income plus one half of non-W2 wage income from the form 1040.[55]

Note that a child who does not work but is married to a working spouse is assigned zero earnings. To evaluate the sensitivity of our results, we also consider family income measures for children, defined in the same way as for parents.

B. College Data and Definitions

In this appendix, we describe the data sources and methods we use to define college attendance, in four subsections. First, we describe our two sources of college attendance records and the differences in how they define colleges and annual attendance. Second, we document how we homogenize their college definitions. Third, we document how we homogenize their annual attendance definitions and compile full annual attendance records from the two data sources. Finally, we summarize enrollment counts for our college attendance definitions.

College Data Sources. We combine two data sources in order to measure student-level college attendance: Form 1098-T records and National Student Loan Data System (NSLDS) Pell grant recipient records. 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

[54] 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. In this study, negative self-employment income is set equal to zero (as negative self-employment income is generally due to business losses and is actually a marker for high earning ability). We multiply self-employment income by .9235 to align treatment with wage earnings (as wage earnings are net of the 7.65% employer social security payroll tax).

[55] 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.

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 schools and other postsecondary institutions, all of which we refer to as "colleges". Colleges are indexed in the 1098-T data by the college's Employer Identification Number (EIN) and its ZIP code. We use 1098-T data for all students during calendar years 1999-2013.

A large share of 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 tuition-waived students. Almost all such students with American parents have low-income parents, are eligible for a Pell grant from the federal government, and required by their colleges to acquire a Pell grant in order to receive their tuition waiver.[56]

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 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. 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.

*Reconciling 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 construct a crosswalk between EIN-ZIP pairs from the 1098-T data (i.e. the EIN and the ZIP code of the

[56]It is possible for a student to have her tuition waived but for her parental income to lie above the Pell grant eligibility threshold and thus for her to be ineligible for a Pell grant. Such students could include top athletic recruits. We believe that such students are small in number.

college) and OPEIDs from the NSLDS Pell data. In almost call 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 the "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-(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 without using any college identifiers, in order to find as many records with both an OPEID (from the NSLDS data) and an EIN-ZIP (from the 1098-T data).[57] 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 to equal either 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 Subsection A above). 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.

We do so by computing counts by OPEID-EIN-ZIP-CALENDARYEAR in the merged dataset. The counts are always extremely skewed 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. By algorithm and by hand, we construct a final mapping

[57] The full NSLDS data include data on recipients of Pell grants and federally subsidized loans. We use only the Pell data in our main attendance measures since 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 Subsection C below) than it would correct missing data.

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 real-world college. OPEID-EIN-ZIP triads were confirmed to correspond to the same real-world college via manual comparison of NSLDS college names and 1098-T college names.

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.[58] 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.[59] 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; we treat Super OPEID -1 as a college and include in our publicly released data 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 5,335 Super OPEIDs: 5,222 unaltered OPEIDs (values ranging from 1002 to 42346) and 113 newly created clusters of OPEIDs (positive values below 1002) for credible matches). Non-credible matches are assigned a value -1. 2.8% of NSLDS Pell records 1999-2013 (especially at Puerto Rican and foreign colleges) and 2.2% of 1098-T records with a valid ZIP code 1999-2013 are assigned Super OPEID -1.[60]

*Imputing 1098-T Records for Pell Recipients.*The vast majority student-college-year attendance instances appear in the 1098-T data, which use a calendar year convention. Thus after using our Super OPEID crosswalk to assign a consistent college definition to every NSLDS Pell record and ever 1098-T record, we use time period information from the NSLDS to impute missing 1098-T data, thereby yielding comprehensive student-college-year attendance records 1999-2013. We do so as follows. For every NSLDS Pell student at a Super OPEID X and a Pell award enrollment start

[58]For example, Cornell (OPEID 190415) may submit 1098-T forms from the same EIN but from two ZIPs – one ZIP corresponding to its Ithica 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.

[59]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). (All OPEIDs are larger than 1000.)

[60]The rate of 1098-T assignment to Super OPEID -1 is 9.0% in 1999 and is 1.3%-2.3% in each year 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.

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. We then remove duplicate records. The remainder of this subsection explains 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 in calendar year t. 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 this given Pell student, and if the Pell 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 and an enrollment start date inclusively between July and December of year t and with 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 duplicates by STUDENT-SUPEROPEID-CALENDARYEAR. Thus students can be recorded as having attended any number of Super OPEIDs in a calendar year but cannot be recorded as having attended any Super OPEID more than once in a calendar year. The resulting dataset constitutes our full student-college-year attendance records.

9.4% percent of our full annual attendance records during years 1999-2013 and for student ages 18-22 were not in the 1098-T data and thus originally appeared only in the NSLDS Pell data. Using our most-attended college attendance measure (see subsection D below), 4.2% of the students in our full analysis sample during years 1999-2013 were not in the 1098-T data and thus originally appeared only in the NSLDS Pell data. Our primary college attendance measure is at the student-level rather than the student-year level, explaining the smaller impact of the NSLDS Pell data on the size of the full analysis sample of students.

There are no public measures of calendar-year Pell attendance that can be used to directly validate our imputation procedure. However, indirect validations suggest a high degree of fidelity. At a large share of 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 particular 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. Furthermore, 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.

We believe remaining infidelities to be small and to bias our statistics in no particular a priori direction. Recruited athletes from high-income families may have tuition waived and thus not be issued a 1098-T while also not being eligible for a Pell grant. This possibility would imply that our data could over-represent students from low-income families. On the other hand, our procedure typically does not impute a 1098-T to year t+1 for Pell grant recipients whose Pell grant covered only the fall semester of year t. If a college's billing for the fall term in year t stretches into year t+1, non-Pell recipients attending only the fall term may receive a 1098-T in both t and t+1. This possibility would imply that our data could under-represent students from low-income families. Importantly, our most-attended definition combines attendance information across multiple years, likely reducing the impact of any such infidelities on our statistics.

College-year data cleaning: A small number of college-year observations have incomplete data, either because of errors in administrative records or because of changes in EIN's and reporting procedures. [61]We discard defective college-years by flagging them in two ways. The flags are constructed using the total counts of forms 1098-T and Pell grants for all children born in 1980-1991. These total counts are not restricted to our main sample but including the universe of all students born in 1980-1991 regardless of successful link to parents, and 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 1098T 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, this indicates that there are too few 1098-T forms and the college-year is flagged. In the vast majority of these cases, the 1098-T counts

[61]For example, 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.

are exactly zero, implying that the college did not report any 1098-T form (likely because the information was not transmitted correctly to the IRS or 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 strictly zero but clearly too small relative to the number of Pell grants. Second, we also flag college-years when full counts are too low (less than 75%) or too high (over 125%) relative to both adjacent years. Such abnormal changes in counts likely denote a data issue. We discard the defective college-year records before assigning students to colleges as we do below. In total, such missing data account for 1.8% of (enrollment weighted) college-year observations (15% when not weighing by enrollment as flags are concentrated in very small schools). We discard such records before defining college attendance because defective 1098-T counts imply that the college-year records are biased toward Pell grants and would lead to overestimating the fraction of students from a disadvantaged background attending the college in that year.

Our baseline measure of most attended college uses 4 year of data (the years when the child turns 19, 20, 21, and 22). A college which has defective (and hence discarded) data for more than 1 year out of the 4 is re-assigned to super_opeid=-1 (the pool of colleges where we do not provide separate information). As a result, for a given birth cohort, 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). Using various placebo tests, we have checked that having 3 years of valid data is enough to ensure accurate numbers (having only 1 or 2 years of valid data is not sufficient to generate fully reliable results).

Enrollment Counts for Attendance Measures. Section II.B fully defined our three measures of college attendance: most-attended college (our primary measure), age-20 college, and first-attended college. Here, we document the quantitative impact of each definition's restrictions on sample sizes.

Our annual attendance records are at the student-college-year level and encompass years 1999-2013, restricting to students who have a valid Social Security Number or Individual Taxpayer Identification Number and to those who were born between 1980-1991. This leaves 207.8 million records remaining, prior to proceeding separately with each of the three definitions.

For the most-attended definition, restricting to attendance at ages 18-22 leaves 114.7 million records. Condensing the student-college-year data to the student level using the most-attended definition (see Section 2.2) leaves 38.1 million records. Eliminating students we could not match to parents or whose parents had negative income leaves 31.1 million records. Restricting to birth cohorts 1980-1982 (as we do in our main analysis) leaves 6.7 million records. After bringing in non-college goers under this definition, we have 10.8 million records.

Finally, we impute income statistics and attendance for cohorts with missing 1098-T school-years using data from the 1983 and 1984 cohorts using the procedure described in Section II.D. We use this procedure to impute data for 668 (29%), 564 (25%), and 461 (20%) colleges in cohorts 1980-1982, respectively, accounting for 625,000 additional students (8.6% of college attendees and 5.5% of all children). For the remaining roughly 90 schools that are missing data for either the 1983 or 1984 cohorts, we do not impute any data values. This leaves us with 11.4 million children in our core sample underlying our main analysis.

For the age-20 definition, restricting to attendance at age 20 leaves 30.6 million records remaining. If a student attends multiple schools at age 20, we weight the student-college-level records using the method described in Section 2.2 such that each student carries a total weight of one, leaving 27.4 million effective records (i.e. records on 27.4 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 attendance at schools with missing cohorts as described above, leaving us with the 11.4 million person sample underlying our age-20 analysis.

For the first-attended definition, restricting to ages 18-28 leaves 175.6 million records. If a student begins multiple "first-attended" colleges in the same year, we assign schools based on the method described in Section 2.2, leaving 36.9 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 11.0 million records for 10.8 million children. Finally, we impute attendance at schools with missing cohorts, leaving us with the 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, because of the way that missing 1098-T school-years are handled, we do not double-count students assigned to Super OPEID -1 in the imputation.

C. Description of 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 the highest-ranked private colleges based on SAT scores. 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, we find that the average student earnings at age 34 for students born in 1980 who attended one of the 30 top-ranked schools is \$134,206. The average earnings for students in the 1980 birth cohort who attended community colleges in Illinois – a group of 39 colleges – is \$35,239. Because we measure college attendance between the ages of 19 and 22, these statistics are based on aggregates of $30 \times 4 = 120$ and $39 \times 4 = 156$ school-years of data (and several thousand students), respectively.

Although simple tabulations by mean SAT score provide some information on college outcomes, colleges differ on many dimensions beyond the average SAT score of their student body. For instance, large schools might differ from small schools, 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 \$134,206 reported above. A raw mean over the group of ten colleges with the highest mean SAT scores 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 to 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 82 groups, where each group g corresponds to a manually-selected set of 20-50 schools with similar characteristics. This partitioning is useful because the best predictors of outcomes in one type of schools (e.g., elite private schools) are typically not the same for other types of schools (e.g., community colleges in Texas). 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 $c \in C$.[62] We retain the characteristic c_i that explains the most variation in

[62] We clean the set of covariates to exclude variables with observations more than three standard deviations from the

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.[63]

Appendix Table II 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 39 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, as are colleges with greater earnings growth. The regression model also includes a number of variables that capture other aspects of the student body and academic offerings at each school that predict earnings. For instance, schools with higher tuition prices for poor students have higher earnings, perhaps because they provide more educational resources. The distribution of degrees across majors at a college also predicts earnings; for instance, colleges with a higher share of degrees in Mechanic and Repair Technologies have lower earnings. Finally, a number of student demographic characteristics predict earnings in intuitive ways. For instance, the percentage of students receiving financial aid (both overall, as well as for three specific income groups) is correlated with lower earnings, while larger schools have higher average earnings. Overall, the model estimated in Appendix Table II includes 21 aggregate statistics (the mean level of earnings in the group and 20 coefficients on explanatory variables) to describe average incomes of students in a group of 39 colleges. Hence, there are 18 more data points than the number of aggregate statistics disclosed, in adherence with established disclosure standards.

(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).

[63]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 with 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.

Using the estimated regression coefficients in Appendix Table II, we produce college-specific estimates of average outcomes, shown in Appendix Table III. Intuitively, we begin with average earnings for this group of community colleges in Illinois ($35,239). We then adjust this average based on publicly available college characteristics using the model estimated in Appendix Table II. For instance, we adjust estimates upward for colleges with higher median earnings in the College Scorecard. Similarly, we adjust earnings upward for colleges with higher net tuition rates. We make analogous adjustments for each of the other 19 characteristics listed in Appendix Table II. 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 in Appendix Table III provide fairly accurate estimates without disclosing exact college-specific data for two reasons. First, the College Scorecard already contains considerable information about the average earnings of students at each college, as the mean earnings of students receiving federal aid are highly predictive of the mean earnings of the student body more broadly. Predicting average earnings based using information only on average earnings in the College Scorecard yields estimates with an average absolute error of $3,268, which is small relative to the standard deviation of mean earnings across colleges ($16,374). Second, the discrepancy between the earnings estimates from the College Scorecard and the mean earnings for the full set of students is well explained by differences in observable characteristics. Row 1 of Appendix Table IV summarizes the precision of the estimates of mean earnings at each college 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 range of errors is significant, with 1% of colleges having errors exceeding $1,815 and 5% having errors exceeding $986 in magnitude. The average absolute error is approximately $264. 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 average 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 schools 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 average earnings, the estimates provide valuable college-specific information about this probability while making use of only aggregate statistics.

D. Changes in Low-Income Access: Pell Shares vs. Percentile-Based Measures

Shares of students receiving federal Pell grants have been widely used as a proxy to measure low-income access to colleges because they are publicly available. For example, many elite private schools have cited their rising Pell share as evidence of success in attracting a more economically diverse student body. Ivy-Plus colleges, for instance, have seen their average Pell share increase from 11.3% to 16.7% between 2000 and 2011. The Pell share has also been used to measure access more broadly across colleges, for instance in the New York Times College Access Index.

Although the Pell data suggest that low-income access is increasing at highly selective schools, the data from our mobility report cards paint a different picture. In these data, the fraction of students from the bottom 40% of the parental income distribution at Ivy-Plus colleges increased by just 0.5 percentage points (as the predicted trend increase) between 2000 and 2011. Why do the two sources of data paint such different pictures of trends in access?

We show in this appendix that the discrepancy between the changes over time in Pell share and the fraction of low income students in our data arise due to nationwide increases in the fraction of children who (if they attended college) would be Pell-eligible. One major driver of this expansion in eligibility was the increase in the income threshold for Pell eligibility. Congress increased this ceiling twice, first in 2001-2003 and then again in 2008-2011. The largest increases in Pell share at Ivy-plus schools came at exactly the same time as these policy changes, as shown in Appendix Figure VIIa. Quantitatively, our back-of-the-envelope calculation suggests that these policy changes resulted in at least 3.4 percentage points more students at Ivy-plus schools becoming Pell-eligible, holding constant the set of students enrolled. This represents roughly two-thirds of the actual increase in Pell share at Ivy-plus schools over our sample period.

The remaining difference between the increases in Pell share and in low-income students can be attributed to falling real household incomes among low-income parents. Even holding program eligibility rules constant in real terms, more students became eligible. The remainder of this appendix describes in more detail the expansion of Pell eligibility and falling real incomes and how they affect Pell shares mechanically.

Pell Grant Program Expansions. Pell grant amounts are calculated based on the Expected Family Contribution (EFC), which depends on a family's income, household size, and assets. Students receive a Pell Grant equal to the difference between the Maximum Pell Grant and their EFC. In our sample period, students are eligible for a Pell Grant if their EFC is less than 95% of the Maximum Pell Grant.[64] As a consequence, increases in the Maximum Pell Grant increase both the size of Pell award for any given eligible student and also the set of students who are eligible.

Congress has changed both the Maximum Pell Grant and the EFC formula as a function of income over time, although in practice the changes to the Maximum Pell Grant have been most consequential. During the past fifteen years, two major revisions to the Pell formula have substantially increased the parental incomes at which students are still eligible. In 2001, Congress increased the Maximum Pell Grant from $3,300 in 2001 to $4,000 in 2003. After several years of erosion in real terms, Congress acted in 2007 to fund Pell Grants on a more sufficient basis, following by a large increase in the Maximum Pell Grant in 2009 as part of the American Recovery and Reinvestment Act (ARRA). As a result, the Pell Grant Maximum increased from $4,310 in 2007 to $5,500 in 2010.

Putting these changes together, Appendix Figure VIIa above shows the Maximum Pell Grant (in real 2015 dollars) over our sample period. The cap increased by more than 33% between 2000 and 2011, with a correspondingly large increase in the maximum EFC with which students could qualify. These increases, in turn, expanded significantly the range of household incomes at which students could qualify.

The mapping from household AGI to EFC relies on a complicated set of factors, including household size, assets, and tax liabilities. The EFC formula also varies across years. Nevertheless, we can confirm the broad expansion in eligibility in our data.

Appendix Figure VIIb shows the proportion of students who received Title IV aid of any kind who qualified for a Pell Grant, at each level of parental AGI in different years.[65] While eligibility is relatively stable in the early years (time series not shown), it expands substantially to 2011. For instance, 72% of students from households with $40K of AGI qualified for a Pell Grant in 2000, compared with 84% in 2011. This increase is even larger for students with $60K of AGI, whose Pell chances increased from 31% to 55%. Altogether, these changes amounted to a substantial expansion in Pell eligibility for households at any given income level over our sample period.

How much of the observed increase in Pell shares does the change in Pell criteria explain? We conduct a back-of-the-envelope estimate based on the observed changes in fraction Pell-eligible in

[64] Beginning in 2012, Congress lowered the EFC ceiling from 95% to 90% of the Maximum Pell Grant.

[65] We use all students receiving Title IV aid as the denominator of the Pell eligibility fraction to be conservative, since it is plausible that Title IV take-up (as a share of students who would be eligible if they applied) did not change as much for students at Ivy-plus schools than nationally.

Appendix Figure VIIb above and our observed distribution of parent incomes at Ivy-plus schools in the 1991 cohort. We make this calculation in four steps. First, we calculate the change in fraction Pell eligible between 2000 and 2011, for each $1000 AGI bin (in real 2015 $) up to $100,000. Second, we calculate the fraction of students at Ivy-plus institutions within each of these AGI bins. Third, we multiply the change in fraction Pell-eligible (from step 1) by the fraction of students in each bin (from step 2). For instance, the fraction of students in the $40,000 AGI bin increased by 12% from 2002 to 2011, and 0.26% of students at Ivy-plus institutions are in the $40,000 AGI bin. For this slice of students, therefore, we calculate that the changes in Pell eligibility increased aggregate Pell shares by 12% * 0.26% = 0.03 percentage points. Aggregating up over all of the bins yields our estimate.

Our back-of-the-envelope calculation suggests that rising Pell eligibility thresholds increased the Pell share at Ivy-plus schools by 3.4 percentage points. This is roughly two-thirds of the 5.4 percentage point observed increase in Pell shares at these schools (from 11.3% to 16.7%) during our sample period.

Our back-of-the-envelope calculation relies many key assumptions. For instance, we assume that the observed fraction of students within each AGI bin that are Pell-eligible nationally is similar to the fraction within each bin at Ivy-league schools. If students at Ivy-league schools had systematically different household structures or levels of assets, conditional on income, then this assumption would fail. In order to test our method, we can use a similar approach (combining the distribution of parent income at Ivy-plus schools with the fraction Pell eligible at each income level) to estimate that 16.3% of students at Ivy league schools were Pell-eligible in 2011. This estimate matches closely the 16.7% figure that one would get from using Pell data specifically from these schools. It therefore appears that the fraction of students who are Pell-eligible within each income bin at Ivy-plus schools is very similar to that nationally.

Another assumption implicit in the calculation is that pool of Title IV students remained constant other than the expansion of Pell Grants. In fact, eligibility for other Title IV aid (such as Stafford and Perkins loans) expanded over this period as well (Looney and Yannelis 2015). These expansions increase the denominator of the Pell-eligible fractions, leading to a downward-biased estimate of the increase in fraction Pell-eligible within each AGI bin over time. As a result, the calculation above presents a lower bound on the true impact of the Pell expansions on Pell shares at Ivy-plus schools.

Declining Real Household Incomes. Rising Pell eligibility thresholds explain roughly two-thirds of the difference between the trends in low-income attendance in our data and the Pell share from the 2000-2011 cohorts. The remaining share can be attributed to falling real incomes among parents of college-going age. It is well known that income growth has stagnated in the bottom half of the income distribution in recent years, but our data suggest that real incomes are actually falling at the household level for the parents of kids in our sample. For instance, the 40th percentile falls from $47,000 for parents of children in the 1980 cohort to $37,000 in the 1991 cohort. This implies that the fraction of parents with incomes below any given real threshold has fallen; for instance, 48% of parents of children in the 1991 cohort had incomes below $47,000, up from exactly 40% in 1980.

E. Construction of College-Level Covariates

This appendix provides definitions and sources for the college covariates used in Section V.D. Online Data Table 10 contains descriptions for each covariate and briefly describes the source of data for each variable. Here, we provide additional details on the construction of these covariates.

Public. This indicator provides a classification of whether a college is operated as public institution or as a private college, which 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.

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, scaled to 1600.

Rejection Rate. We compute this measure as one minus the admissions rate at a school, where the admissions rate is taken from the Department of Education's (DoE) College Scorecard for the year 2013. For colleges aggregated in a cluster, we compute this measure as a one minus the enrollment weighted mean of the admission rate for all 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 poor variable is taken from DoE'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). This metric is only available in the Scorecard starting with 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. We assign the cluster of colleges the enrollment-weighted mean of the sticker price for the colleges in that cluster.

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, with the total undergraduate enrollment in the fall of 2000, which are taken from IPEDS' Fall Enrollment survey.

Expenditure per Student. This variable measures the instructional expenditure for undergraduate student in the year 2000. We take the total instructional expenditure from IPEDS' Delta Cost Project Database and divide it by the total undergraduate enrollment in the fall of 2000. We define instructional expenditures per student in 2013 in the same way.

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

Average Faculty Salary. We take this variable from IPED's Delta Cost Project Database. This variable captures the average salary for full-time faculty members on 9-month equated contracts in the academic year 2001-02.

STEM Major Share. This variable captures 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.

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 enrollment in a college who are black alone divided by the total undergraduate enrollment; For the Hispanic share, the numerator is the number of students of any race who are Hispanic. We also calculate a Asian and Pacific Islander share category where the numerator is the number of students that are of Asian origin or have origins in the Pacific Islands. Lastly, we compute the share of international students where the numerator is the number of students who are non-resident aliens.

Commuting Zone Characteristics. We obtain data on demographic characteristics of the commuting zone in which each college is located from Chetty et al. (2014, Online Data Table 8).

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TABLE I

Summary Statistics for Cross-Sectional Sample

	Sample		
	All Children	College Goers	Non-Goers
	(1)	(2)	(3)
A. College Attendance			
% Attending College	61.84	100	-
% Attending College in Publicly Available Dataset	52.77	85.33	-
% Attending Ivy-Plus College	0.49	0.79	-
% Attending an Other Elite College	1.71	2.77	-
% Attending an Other 4-year College	31.32	50.65	-
% Attending a 2-Year or Less College	19.24	31.12	
% Not Attending by Age 28	26.64	-	69.81
B. Parents' Household Income			
Mean Earnings ($)	87,346	110,162	50,380
Median Earnings ($)	59,100	76,200	37,400
% with Parents in Bottom 20%	20.00	12.43	32.27
% with Parents in Top 20%	20.00	28.39	6.41
% with Parents in Top 1%	1.00	1.53	0.14
C. Children's Individual Earnings			
Mean Earnings ($)	35,528	44,950	20,261
Median Earnings ($)	26,900	36,400	13,600
% employed	81.68	88.29	70.96
% in Top 20%	20.00	27.76	7.43
% in Top 1%	1.00	1.55	0.12
% in Top 20% | Parents in Bottom 20%	8.65	15.93	4.11
% in Top 1% | Parents in Bottom 20%	0.23	0.49	0.06
% in Top 20% and Parents in Bottom 20%	1.73	1.98	1.33
% in Top 1% and Parents in Bottom 20%	0.05	0.06	0.02
Number of Children	10,755,222	6,650,665	4,104,557
Number of Colleges	1,803	1,803	

Notes: The table presents summary statistics for the cross-sectional sample (1980-82 birth cohorts); see Online Appendix Table I for analogous summary statistics for the longitudinal sample (1980-91 cohorts). College goers are defined as children attending college at some point between the ages of 19-22. Colleges in the publicly available dataset are those for which we observe a sufficient number of students, data is unreliable, or estimates cannot be produced. Ivy-Plus colleges are defined as the eight Ivy-League colleges as well as the University of Chicago, Stanford University, MIT, and Duke University. Elite colleges are defined as those in categories 1 or 2 in Barron's Profiles of American Colleges (2009). 4-year Colleges are defined using the highest degree offered by the institution as recorded in IPEDS (2013). Parent income is defined as mean pre-tax Adjusted Gross Income in 2015 dollars during the period in which the child was ages 15-19. Parent income percentiles are constructed using the parents' rank in the national income distribution among parents with a child in the same birth cohort. Children's earnings are measured as the sum of individual wage earnings and self-employment income in the year 2014. At each age, children are assigned percentile ranks based on their rank relative to children born in the same birth cohort. A child is defined as employed if they have positive income. Children are assigned to colleges using the college that they attended for the most years between ages 19 and 22, breaking ties by taking the college which a child first attends.

TABLE II

Relationship Between Children's and Parent's Income Ranks With Colleges

Dependent Variable:	Individual Earnings Rank	Working	Individual Earnings Rank		HH Inc. Rank	HH Earn. Rank	Married
Sample:	Full Sample	Full Sample	Male Kids	Female Kids	Full Sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Full Population							
Parent Rank	0.288	0.192	0.334	0.240	0.365	0.357	0.372
	(0.002)	(0.005)	(0.000)	(0.000)	(0.002)	(0.002)	(0.005)
B. All College-Goers (with College FE)							
Parent Rank	0.100	0.030	0.118	0.064	0.149	0.142	0.175
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)
C. Elite Colleges (with College FE)							
Parent Rank	0.065	0.023	0.090	0.036	0.131	0.107	0.151
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)
D. Other 4-Year Colleges (with College FE)							
Parent Rank	0.095	0.024	0.114	0.064	0.147	0.139	0.170
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
E. 2-Year Colleges (with College FE)							
Parent Rank	0.110	0.042	0.125	0.067	0.154	0.149	0.190
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

Notes : This table presents results of individual-level rank-rank WLS (count-weighted) regressions on various samples from the 1980-1982 birth cohorts. Each cell reports the coefficent on parent rank for the given model. Panel A uses the full population of children. Panel B restrictes to all children that attend college under the baseline definition and includes college fixed effects. Panels C, D, and E further restrict to children that attended particular types of colleges, all including college fixed effects. Column 1 presents results for the full sample of children where the dependent variable is the child's income rank. Column 2 presents results for the full sample of children where the dependent variable is in indicator for whether the child is working in the year 2014. Columns 2 and 4 repeat the specification in column 1 but restrict to male and female children, respectively. Column 5 uses all household adjusted gross income as the dependent variable and Column 6 uses household wage plus self-employment income as the dependent variables, respectively. Column 7 uses an indicator for whether the child is married as the dependent variable. Columns 5-7 use the full sample of children. College type definitions, parent income ranks, child income ranks, and college assignment are described in the notes to Table I.

Table III
Top 10 Colleges by Mobility Rate

Panel A: Top 10 Colleges by Mobility Rate (Bottom to Top 20%)

Rank	Name	Mobility Rate =	Access	X	Success Rate
1	Cal State, LA	9.9%	33.1%		29.9%
2	Pace University – New York	8.4%	15.2%		55.6%
3	SUNY – Stony Brook	8.4%	16.4%		51.2%
4	Technical Career Institutes	8.0%	40.3%		19.8%
5	University of Texas – Pan American	7.6%	38.7%		19.8%
6	CUNY System	7.2%	28.7%		25.2%
7	Glendale Community College	7.1%	32.4%		21.9%
8	South Texas College	6.9%	52.4%		13.2%
9	Cal State Polytechnic – Pomona	6.8%	14.9%		45.8%
10	University of Texas – El Paso	6.8%	28.0%		24.4%

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

Rank	Name	Mobility Rate =	Access	X	Upper-Tail Success Rate
1	University of California – Berkeley	0.76%	8.8%		8.6%
2	Columbia University	0.75%	5.0%		14.9%
3	MIT	0.68%	5.1%		13.4%
4	Stanford University	0.66%	3.6%		18.5%
5	Swarthmore College	0.61%	4.7%		13.0%
6	Johns Hopkins University	0.54%	3.7%		14.7%
7	New York University	0.52%	6.9%		7.5%
8	University of Pennsylvania	0.51%	3.5%		14.5%
9	Cornell University	0.51%	4.9%		10.4%
10	University of Chicago	0.50%	4.3%		11.5%

Notes: This table presents the top 10 colleges as measured by the mobility rate (Panel A) and upper tail mobility rate (Panel B). The mobility rate is defined as the product of the share of children at a college with parents in the bottom quintile of the income distribution ("Access") and the share of children with parents in the bottom quintile of the income distribution that reach the top quintile of the income distribution ("Success Rate"). In other words, the mobility rate is the joint probability of a child being from the bottom quintile and reaching the top quintile. Parent income ranks, child income ranks, and college assignment are described in the notes to Table I. CUNY System includes all CUNY undergraduate campuses but the recently founded William E. Macaulay Honors College and Guttman Community College.

Table IV
Distribution of Access Conditional on Success Rates

	All Colleges (1)	Colleges with Above Median Success Rate (2)
Panel A: Variation in Access Conditional on Success Rate		
Unconditional SD Access	7.59%	5.97%
Residual SD Access	6.16%	5.41%
SD Access with Parametric Controls	6.31%	5.48%
Residual SD Access with Parametric Controls	4.50%	3.88%
SD Access | Success Rate of Ivy Plus	3.42%	
Panel B: Variation in Access Conditional on Upper-Tail Success Rate		
Unconditional SD Access	7.59%	6.55%
Residual SD Access	6.46%	5.46%
SD Access with Parametric Controls	6.82%	5.83%
Residual SD Access with Parametric Controls	5.31%	4.17%
SD Access | Upper-Tail Success Rate of Ivy Plus	1.03%	

Notes : This table presents information on the distribution of access given success rates (Panel A) and upper-tail success rates (Panel B). Column 1 gives statistics on the sample of all colleges and column 2 restricts to colleges with above median success rates (weighted by the number of students with parents in the bottom income quintile). The residual standard devation of access is constructed by taking the root-mean-square error of an OLS regression of access on indicators for each of 50 quantiles of success rates (weighted by the number of students with parents in the bottom income quintile). The standard deviation of access with parametric controls is calculated as the root-mean-square error of an OLS regression of access on a third-order polynomial of success rates. The residual standard deviation of access with parametric controls is calculated as the root-mean-square error of an OLS regression of access on a third-order polynomial of success rates including indicators for each commuting zone. The standard deviation of access conditional on upper-tail success rate of Ivy-Plus colleges is calulated by creating indicators for 50 quantiles of success rates (weighted by the number of students with parents in the bottom quintile) and reporting the root-mean-square error of an OLS regression of access on the quantile indicators, restricting to colleges with success rates between the minimum and maximum success rates of the Ivy-Plus colleges. All standard deviations are count-weighted. Parent income ranks, child income ranks, and college assignment are described in the notes to Table I.

TABLE V

Correlations of College Characteristics with Mobility Statistics

Covariate	Mobility Rate		Access		Success Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Correlates of Access and Success Rate						
Public	0.03	(0.026)	0.20	(0.024)	-0.19	(0.033)
SAT Scores	-0.13	(0.063)	-0.58	(0.046)	0.69	(0.035)
Rejection Rate	0.32	(0.052)	-0.02	(0.039)	0.42	(0.049)
Rejection Rate, Public Schools	0.51	(0.080)	0.14	(0.059)	0.41	(0.071)
Rejection Rate, Private Schools	0.13	(0.044)	-0.19	(0.046)	0.44	(0.068)
Graduation Rate	0.06	(0.034)	-0.52	(0.027)	0.63	(0.036)
Net Cost for Poor	-0.05	(0.030)	-0.29	(0.027)	0.25	(0.031)
Sticker Price	-0.02	(0.025)	-0.38	(0.019)	0.48	(0.029)
Endowment per Student	0.02	(0.047)	-0.23	(0.056)	0.38	(0.107)
Expenditure per Student	0.16	(0.032)	-0.19	(0.077)	0.35	(0.010)
Enrollment	0.14	(0.048)	-0.21	(0.029)	0.41	(0.051)
Avg. Faculty Salary	0.20	(0.040)	-0.43	(0.028)	0.68	(0.034)
STEM Major Share	0.12	(0.035)	-0.24	(0.024)	0.40	(0.039)
Panel B: Correlates of Access and Upper-Tail Success Rate						
Public	-0.24	(0.038)	0.20	(0.024)	-0.25	(0.035)
SAT Scores	0.55	(0.065)	-0.58	(0.046)	0.72	(0.053)
Rejection Rate	0.56	(0.064)	-0.02	(0.039)	0.44	(0.061)
Rejection Rate, Public Schools	0.48	(0.126)	0.14	(0.059)	0.36	(0.117)
Rejection Rate, Private Schools	0.60	(0.062)	-0.19	(0.046)	0.57	(0.074)
Graduation Rate	0.48	(0.050)	-0.52	(0.027)	0.53	(0.046)
Net Cost for Poor	0.10	(0.034)	-0.29	(0.027)	0.17	(0.027)
Sticker Price	0.40	(0.044)	-0.38	(0.019)	0.51	(0.047)
Endowment per Student	0.38	(0.078)	-0.23	(0.056)	0.49	(0.130)
Expenditure per Student	0.46	(0.146)	-0.19	(0.077)	0.36	(0.119)
Enrollment	0.23	(0.063)	-0.21	(0.029)	0.25	(0.048)
Avg. Faculty Salary	0.57	(0.061)	-0.43	(0.028)	0.54	(0.052)
STEM Major Share	0.33	(0.050)	-0.24	(0.024)	0.32	(0.043)

Notes : This table presents college-level correlations of various college characteristics on mobility statistics, with standard errors in parentheses. "Public" is an indicator for whether a school is public or not based on the control of the institution reported by IPEDS (2013). SAT scores are measured as the mean of the 25th and 75th percentile of math and verbal reading scores reported by IPEDS (2001) multiplied by two. Rejection rate is one minus the admissions rate based on the College Scorecard for the year 2013. The graduation rate is measured as the graduation rate for fulltime undergraduates that graduate in 150% of normal time in IPEDS (2002). Net cost for poor is measured as the average net cost of attendence for the academic year 2009-2010 from the College Scorecard (2013). Sticker price is the sum of tuition and fees for the academic year 2000-01 from IPEDS. Endowment per student is the ending value of endowment asssets in 2000 divided by the number of students in IPEDS (2000). Expenditure per student is defined as the instructional expenditure for undergraduates divided by total undergraduate enrollment in IPEDS (2000). Average faculty salary is the average faculty salary for full-time faculty in the academic year 2001-02 in IPEDS. STEM major share is the percentage of degrees awarded in science, technology, engineering, and mathematics fields in IPEDS (2000). Correlations with the mobility rate or access are count-weighted. Correlations with the success rate are weighted by the number of students with parents in the bottom income quintile. Parent income ranks, child income ranks,

TABLE VI
Mobility Rates and Access Over Time

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Fraction of Parents from Bottom Quintile					
Cohort	0.001	0.000	0.001	0.001	0.001
High Mobility Rate	4.816	5.489	3.908	4.199	4.705
Cohort * High Mobility Rate	-0.026	-0.030	-0.021	-0.023	-0.026
High Access		-3.175	-1.397	-1.508	-1.247
Cohort * High Access		0.002	0.001	0.001	0.001
High Success Rate			2.554	2.001	1.756
Cohort * High Success Rate			-0.001	-0.001	-0.001
CZ Fixed Effects	No	No	No	Yes	No
College Tier Fixed Effects	No	No	No	No	Yes

Notes: This table analyzes the relationship between mobility rates and changes in access over time. Each column presents a separate regression run at the college X cohort level. The dependent variable in all regressions is the fraction of students in a college X cohort cell from the bottom quintile of the parents' income distribution, as defined in Section 2. For this table, we calculate the mobility-rate as the product of success (as calculated in the pooled sample of the 1980-82 cohorts) and the average access over all years that a college in present in our sample. We define High Mobility Rate as an indicator equal to 1 if a college's mobility rate is above the 90th percentile of the count-weighted distribution of that variables (using the average count per cohort for each school as the weight). In Column 1, the independent variables are cohort, High Mobility Rate, and the interaction of these variables. Column 2 includes the independent variables in Column 1, plus High Access, an indicator variable equal to 1 if the average access (across all cohorts) is above the median of the count-weighted distribution of this variable (using total counts across all cohorts within a school for the weights), and an interaction of High Access with cohort. Column 3 includes the independent variables in Column 2, plus High Success, an indicator variable equal to 1 if the success rate (from the pooled 1980-82 cohorts) is above the median of the count-weighted distribution of this variable (using the average count in the 1980-82 cohorts as the weight), and an interaction of High Success indicator with cohort. Column 4 includes the independent variables in Column 3, plus fixed effects at the commuting zone (CZ) level. Column 5 includes the independent variables in Column 4, plus fixed effects at the college tier level (using tiers as defined in Section 2). All regressions are weighted by the counts within each college X cohort cell.

ONLINE APPENDIX TABLE I
Summary Statistics for Longitudinal Sample

	Sample		
	All Children	College Goers	Non-Goers
	(1)	(2)	(3)
Panel A: College Attendance			
% Attending College	64.43	100	-
% Attending College in Publicly Available Data Set	58.56	90.88	-
% Attending 4-year College	35.05	54.40	-
% Attending a Selective College	25.98	40.00	-
% Attending Ivy-Plus College	0.44	0.68	-
Panel B: Parents' Household Income			
Mean Earnings ($)	88,546	111,083	47,727
Median Earnings ($)	55,900	73,000	34,300
% with Parents in Bottom 20%	19.98	13.09	32.47
% with Parents in Top 20%	20.02	27.78	5.98
% with Parents in Top 1%	1.00	1.48	0.14
Panel C: Children's Individual Earnings			
Mean Earnings ($)	26,632	32,504	15,993
Median Earnings ($)	19,600	25,500	9,900
% employed	81.80	89.40	68.10
% in Top 20%	20.02	26.29	8.67
% in Top 1%	1.01	1.41	0.27
% in Top 20% | Parents in Bottom 20%	8.80	14.37	4.74
% in Top 1% | Parents in Bottom 20%	0.28	0.49	0.13
% in Top 20% and Parents in Bottom 20%	1.76	1.88	1.54
% in Top 1% and Parents in Bottom 20%	0.06	0.06	0.04
Number of Children	48,286,824	31,110,322	17,176,502
Number of Colleges	2,434	2,434	

Notes: The table presents summary statistics for the longitudinal sample (1980-91 birth cohorts); see Table I for analogous summary statistics for the cross-sectional sample (1980-82 cohorts) and definitions.

ONLINE APPENDIX TABLE II

Sample Sizes vs Survey Counts

	Size of Birth Cohort	Number of Citizens in Our Sample	Ratio of (2)/(1)	CPS Number Aged 20	Kids Aged 20 in Our Sample	CPS College Attendees	College Attendees at Age 20 in Our Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1980	3,612	3,189	88.3%	3,840	3,385	1,839	1,526
1981	3,629	3,403	93.8%	3,829	3,482	1,845	1,601
1982	3,681	3,493	94.9%	3,938	3,545	1,998	1,689
1983	3,639	3,470	95.4%	3,926	3,575	2,009	1,794
1984	3,669	3,664	99.9%	3,981	3,835	2,030	1,952
1985	3,761	3,776	100.4%	4,222	3,939	2,187	1,987
1986	3,757	3,764	100.2%	4,057	3,922	2,022	1,986
1987	3,809	3,836	100.7%	4,006	4,061	2,078	2,080
1988	3,910	3,960	101.3%	4,007	4,212	2,147	2,175
1989	4,041	4,103	101.5%	4,087	4,361	2,254	2,316
1990	4,158	4,227	101.7%	4,399	4,498	2,389	2,415
1991	4,111	4,178	101.6%	4,281	4,484	2,433	2,402
1980-1991	45,776	45,062	98.4%	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 report the size of the birth cohort according to Vital Statistics in each birth cohort. Column 4 reports the number of people in the CPS in the given birth cohort during the year in which the birth cohort was aged 20. Column 6 reports the number of people in the CPS in the given birth cohort who attended college during the year in which the birth cohort was aged 20. 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 due to naturalized citizens. Column 5 reports the number of children in our sample in each birth cohort. Column 7 reports the number of people in our sample in the given birth cohort who in our data attended college at age 20.

ONLINE APPENDIX TABLE III

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

Covariate	Regression Coefficient (1)	Standard Error (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 coefficients obtained by running the forward-search algorithm for student earnings (in dollars) for thirty-nine community colleges in Illinois. The enrollment-weighted mean income in this group is $36,316. See Online Appendix C for more details.

ONLINE APPENDIX TABLE IV
Predicted Values of Regression by College

College Name	Average Student Earnings (1)
Southwestern Illinois College	$34,374.85
Black Hawk College	$36,054.35
Danville Area Community College	$32,044.41
Elgin Community College	$38,913.03
Joliet Junior College	$40,069.05
Illinois Valley Community College	$36,517.58
Morton College	$33,212.01
Rock Valley College	$35,638.91
Sauk Valley Community College	$32,710.15
South Suburban College Of Cook County	$32,624.32
Ancilla Domini College	$34,025.91
Harper College	$39,706.07
College Of Du Page	$32,051.02
Illinois Central College	$37,142.11
Waubonsee Community College	$36,920.35
Parkland College	$37,870.46
Rend Lake College	$30,631.38
Coyne College	$34,652.55
Lake Land College	$32,185.69
Kishwaukee College	$35,987.57
Mchenry County College	$39,589.18
Moraine Valley Community College	$41,418.54
College Of Lake County	$39,427.34
Oakton Community College	$38,451.26
Lewis And Clark Community College	$32,937.13
Richland Community College	$33,033.31
Northwestern College	$32,820.03
Le Cordon Bleu College Of Culinary Arts In Chicago	$35,592.93
Heartland Community College	$35,894.98
Observations	29
Number of Statistics Estimated	12

Notes: This table reports college-specific estimates for average student earnings in twenty-nine two-year colleges located in the state of Illinois, computed using the regression coefficients estimated in Online Appendix Table III. See Online Appendix C for more details.

ONLINE APPENDIX TABLE V
Statistics on Prediction Errors

	Standard Deviation of Variable	Mean Absolute Error	95th Percentile of Absolute Error	99th Percentile of Absolute Error	99.9th Percentile of Absolute Error
Mean Student Earnings ($)	17061	266	965	1846	3186
Median Student Earnings ($)	12068	181	640	1352	2289
Median Student Earnings - Positive Earners($)	12097	187	691	1257	2353
Mean Parent Household Income ($)	52296	829	3025	5993	11288
Mean Parent Rank (pp)	10.24	0.15	0.56	1.04	1.79
Parents in Top 10% (%)	10.63	0.16	0.58	1.11	2.13
Parents in Top 5% (%)	7.15	0.11	0.39	0.75	1.32
Parents in Top 1% (%)	2.20	0.03	0.12	0.24	0.45
Parents in Top 0.1% (%)	0.31	0.004	0.02	0.04	0.08
Kid in Top 10% (%)	10.09	0.16	0.54	1.18	1.99
Kid in Top 5% (%)	6.83	0.11	0.38	0.75	1.32
E[Kid Rank | Parents in Q1] (pp)	8.63	0.13	0.45	1.06	1.92
E[Kid Rank | Parents in Q2] (pp)	7.43	0.13	0.45	0.98	1.56
E[Kid Rank | Parents in Q3] (pp)	7.20	0.11	0.39	0.77	1.45
E[Kid Rank | Parents in Q4] (pp)	7.15	0.11	0.38	0.73	1.34
E[Kid Rank | Parents in Q5] (pp)	8.09	0.13	0.48	0.96	1.85
P(Kid in Q1, Parents in Q1) (%)	1.49	0.03	0.1	0.19	0.36
P(Kid in Q1, Parents in Q2) (%)	1.28	0.02	0.09	0.21	0.44
P(Kid in Q1, Parents in Q3) (%)	1.11	0.02	0.08	0.18	0.33
P(Kid in Q1, Parents in Q4) (%)	1.13	0.02	0.09	0.21	0.47
P(Kid in Q1, Parents in Q5) (%)	1.79	0.03	0.1	0.21	0.43
P(Kid in Q2, Parents in Q1) (%)	2.19	0.04	0.13	0.25	0.50
P(Kid in Q2, Parents in Q2) (%)	1.68	0.03	0.11	0.22	0.41
P(Kid in Q2, Parents in Q3) (%)	1.41	0.03	0.11	0.22	0.51
P(Kid in Q2, Parents in Q4) (%)	1.27	0.02	0.09	0.18	0.37
P(Kid in Q2, Parents in Q5) (%)	1.52	0.03	0.11	0.24	0.49
P(Kid in Q3, Parents in Q1) (%)	2.20	0.04	0.14	0.3	0.55
P(Kid in Q3, Parents in Q2) (%)	2.00	0.03	0.11	0.27	0.52
P(Kid in Q3, Parents in Q3) (%)	1.84	0.03	0.12	0.23	0.47
P(Kid in Q3, Parents in Q4) (%)	1.90	0.04	0.14	0.35	0.62
P(Kid in Q3, Parents in Q5) (%)	1.68	0.03	0.12	0.26	0.53
P(Kid in Q4, Parents in Q1) (%)	1.81	0.03	0.11	0.24	0.47
P(Kid in Q4, Parents in Q2) (%)	1.61	0.03	0.12	0.26	0.52
P(Kid in Q4, Parents in Q3) (%)	1.62	0.03	0.11	0.25	0.46
P(Kid in Q4, Parents in Q4) (%)	2.12	0.04	0.14	0.30	0.55
P(Kid in Q4, Parents in Q5) (%)	2.96	0.05	0.16	0.33	0.60
P(Kid in Q5, Parents in Q1) (%)	1.56	0.02	0.09	0.17	0.36
P(Kid in Q5, Parents in Q2) (%)	1.62	0.03	0.1	0.21	0.40
P(Kid in Q5, Parents in Q3) (%)	1.94	0.03	0.13	0.28	0.46
P(Kid in Q5, Parents in Q4) (%)	3.03	0.05	0.16	0.33	0.65
P(Kid in Q5, Parents in Q5) (%)	8.89	0.13	0.49	0.92	1.65
P(Kid in Top 1%, Parents in Q1) (%)	0.22	0.003	0.012	0.024	0.056
P(Kid in Top 1%, Parents in Q2) (%)	0.15	0.003	0.011	0.023	0.046
P(Kid in Top 1%, Parents in Q3) (%)	0.21	0.004	0.013	0.030	0.063
P(Kid in Top 1%, Parents in Q4) (%)	0.33	0.006	0.021	0.044	0.077
P(Kid in Top 1%, Parents in Q5) (%)	1.57	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. Column 1 lists the outcome variables we report for each college. Column 2 reports the (enrollment-weighted) standard deviation across colleges of each variable. Column 3 reports the mean absolute error of our 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 C for more details.

ONLINE APPENDIX TABLE VI

Panel A

Top 10 MR Colleges, Adjusting for Non-College Success Rate

Rank	Name	Mobility Rate =	Access	X	Adjusted Success Rate
1	Cal State – Los Angeles	8.6%	33.1%		26.1%
2	Pace University – New York	7.8%	15.2%		51.7%
3	SUNY – Stony Brook	7.8%	16.4%		47.4%
4	Technical Career Institutes	6.4%	40.3%		16.0%
5	University of California – Irvine	6.3%	12.2%		51.4%
6	Cal State Poly – Pomona	6.3%	14.9%		41.9%
7	Saint John's University – New York	6.2%	14.3%		43.5%
8	University of Texas – Pan American	6.2%	38.7%		15.9%
9	CUNY System	6.1%	28.7%		21.3%
10	Saint Francis College	6.1%	13.5%		45.3%

Panel B

Top 10 MR Colleges, Adjusting for Local Community College Mean Success Rate

Rank	Name	Mobility Rate =	Access	X	Adjusted Success Rate
1	Pace University – New York	5.7%	15.2%		37.3%
2	SUNY – Stony Brook	5.4%	16.4%		33.0%
3	New Jersey Institute of Technology	4.9%	10.1%		48.3%
4	University of California – Irvine	4.9%	12.2%		39.7%
5	Cal State – Los Angeles	4.7%	33.1%		14.3%
6	Cal State Poly – Pomona	4.5%	14.9%		30.2%
7	SUNY – Binghamton	4.2%	9.4%		44.8%
8	Saint John's University – New York	4.2%	14.3%		29.1%
9	Saint Francis College	4.2%	13.5%		30.9%
10	College of Mount Saint Vincent and Manhattan College	4.1%	9.2%		44.3%

Notes: This table replicates Table IIIa under two alternative formulations of the mobility rate. In each panel, we compute each college's mobility rate as the product of its access and its adjusted success rate. For Panel A, each college's adjusted success rate equals its actual success rate minus 3.9%, which is the success rate of those who do not attend college by age 28. For Panel B, each college's adjusted success rate equals its actual success rate minus the average community college success rate in the college's commuting zone (which is 11.7% on average). See the notes to Table IIIa and Section V.C for additional detail.

ONLINE APPENDIX TABLE VII

Robustness Checks: Key Statistics and Correlations with Main Measure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Men Only	Women Only	Household Earnings	Household Income	Local Price Adjustment	College at Age 20	First college before Age 28
Panel A: Statistics on Access							
SD of Access	6.84	8.25	7.59	7.60	8.44	7.15	8.59
Share of Top 1% Kids at Ivy Plus	14.60	14.43	14.52	14.52	12.97	14.51	14.38
Rank-Rank Slope all College Goers	0.12	0.06	0.14	0.15	0.10	0.09	0.11
Rank-Rank Slope Elite Schools	0.09	0.04	0.11	0.13	0.06	0.06	0.07
SD of Access | Success	5.72	6.73	5.49	5.43	6.54	6.02	6.45
Residual SD of Access | Success above Median	5.24	5.82	4.50	4.38	5.60	4.93	5.95
Residual SD of Access | Success of Ivy Plus	2.66	3.84	2.71	2.92	1.67	2.99	3.43
Residual SD of Access | Tail Success of Ivy Plus	1.24	1.18	1.34	2.11	1.16	0.96	1.04
Panel B: Correlation with Baseline Measures							
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	0.92	0.98	0.94
Correlation with Baseline Success Rate	0.95	0.96	0.94	0.93	0.86	0.99	0.98
Correlation with Baseline Upper-Tail Success Rate	0.94	0.88	0.90	0.87	0.89	0.98	0.96
Correlation with Baseline Access	0.99	0.99	1.00	1.00	0.92	0.99	0.99

Notes : This table replicate main results under alternative samples (columns 1-2), alternative child income definitions (columns 3-5), and alternative definitions of college attendance (columns 6-7). See Sections II.B and II.C for details of the alternative definitions. The rank-rank slopes are the coefficients from a regression of child income on parent income after controlling for college fixed effects.

ONLINEL APPENDIX TABLE VIII

Correlations of College Characteristics with Mobility Statistics

Panel A: Correlates of Access and Success Rate

Covariate	Mobility Rate		Access		Succes Rate	
Share Asians and Pacific Islanders	0.53	(0.032)	-0.02	(0.030)	0.54	(0.054)
Share Black	0.20	(0.025)	0.47	(0.034)	-0.21	(0.026)
Share Hispanic	0.54	(0.035)	0.53	(0.029)	0.01	(0.027)
Share Non-resident Alien	0.22	(0.036)	-0.11	(0.032)	0.34	(0.044)
Frac. Foreign Born	0.59	(0.041)	0.26	(0.035)	0.29	(0.042)
Poor Share	0.34	(0.042)	0.43	(0.052)	-0.08	(0.025)
Log Population Density	0.28	(0.037)	0.02	(0.030)	0.25	(0.031)
Income Segregation	0.27	(0.031)	0.02	(0.027)	0.24	(0.029)
Income Level	0.09	(0.037)	-0.15	(0.038)	0.24	(0.033)

Panel B: Correlates of Access and Upper-Tail Success Rate

Covariate	Mobility Rate		Access		Upper-Tail Succes Rate	
Share Asians and Pacific Islanders	0.56	(0.077)	-0.02	(0.030)	0.37	(0.069)
Share Black	-0.09	(0.020)	0.47	(0.034)	-0.15	(0.018)
Share Hispanic	0.10	(0.020)	0.53	(0.029)	-0.06	(0.010)
Share Non-resident Alien	0.30	(0.046)	-0.11	(0.032)	0.24	(0.044)
Frac. Foreign Born	0.29	(0.053)	0.26	(0.035)	0.10	(0.034)
Poor Share	0.08	(0.031)	0.43	(0.052)	-0.06	(0.022)
Log Population Density	0.20	(0.042)	0.02	(0.030)	0.12	(0.030)
Income Segregation	0.18	(0.034)	0.02	(0.027)	0.11	(0.024)
Income Level	0.15	(0.050)	-0.15	(0.038)	0.14	(0.036)

Notes: The table extends Table V to list more correlations with college mobility rates, access, and success rates. The share variables are at the college level and drawn from IPEDS in year 2000. The fraction foreign born, log population density, income segregation, and income level are computed using the 2000 Census. See the notes to Table V for additional detail.

ONLINE APPENDIX TABLE IX

Panel A

Top 10 Colleges by Mobility Rate (Bottom to Top 20%), Men Only

Rank	Name	Mobility Rate =	Access	X	Success Rate
1	Cal State – Los Angeles	11.6%	31.8%		36.4%
2	South Texas College	11.1%	51.4%		21.5%
3	Southern Careers Institute	11.0%	50.2%		22.0%
4	University of Texas – Pan American	10.8%	38.4%		28.1%
5	University of Texas – Brownsville	10.1%	45.5%		22.3%
6	Laredo Community College	10.1%	42.3%		23.8%
7	Technical Career Institutes	9.5%	37.7%		25.2%
8	SUNY – Stony Brook	9.5%	16.8%		56.4%
9	Southwest Texas Junior College	9.4%	38.8%		24.3%
10	CUNY System	8.9%	28.1%		32.2%

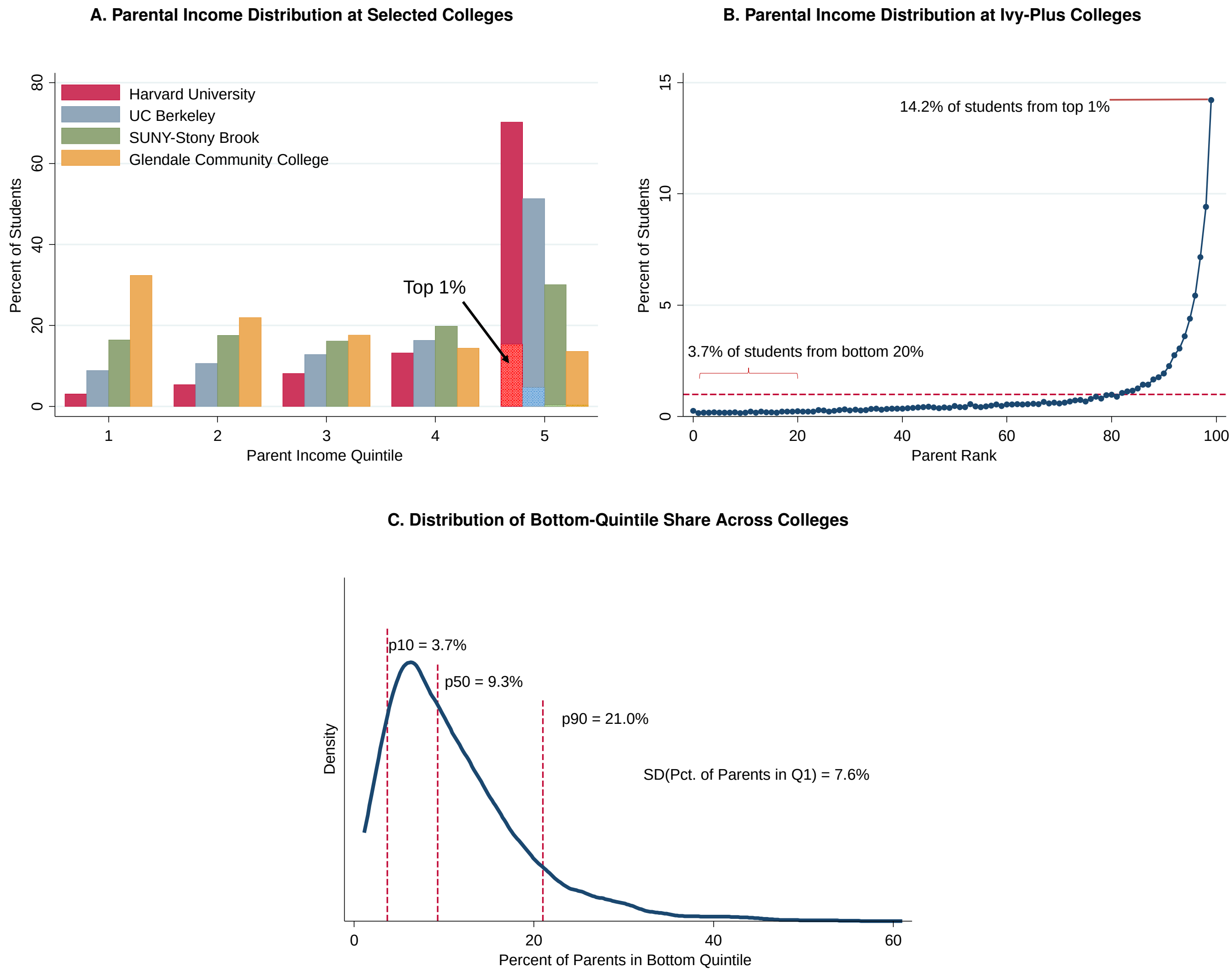
Panel B

Top 10 Colleges by Mobility Rate (Bottom to Top 20%), Household Labor Earnings

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

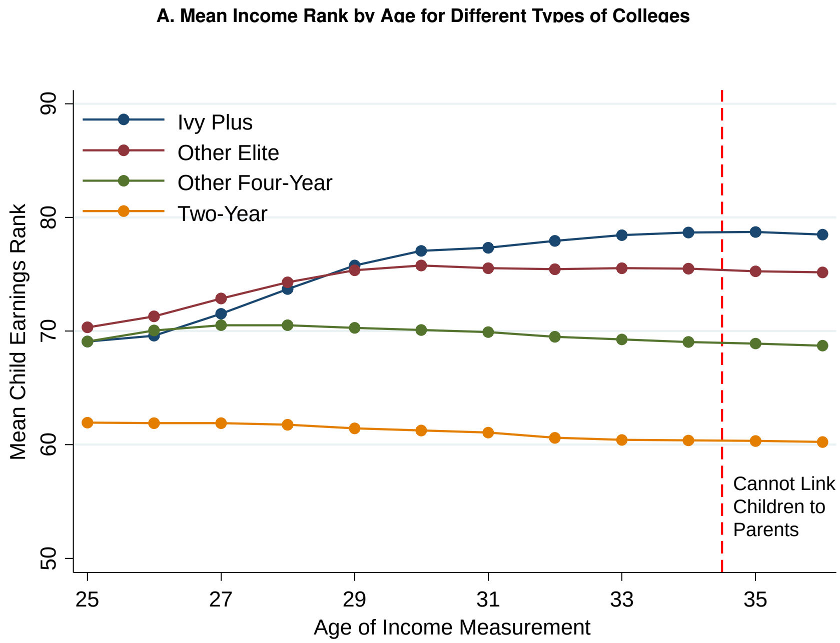
Notes: Panel A replicates Table IIIa for men only. Panel B replicates Table IIIa when measuring child income as household labor earnings. See the notes to Table IIIA for additional detail.

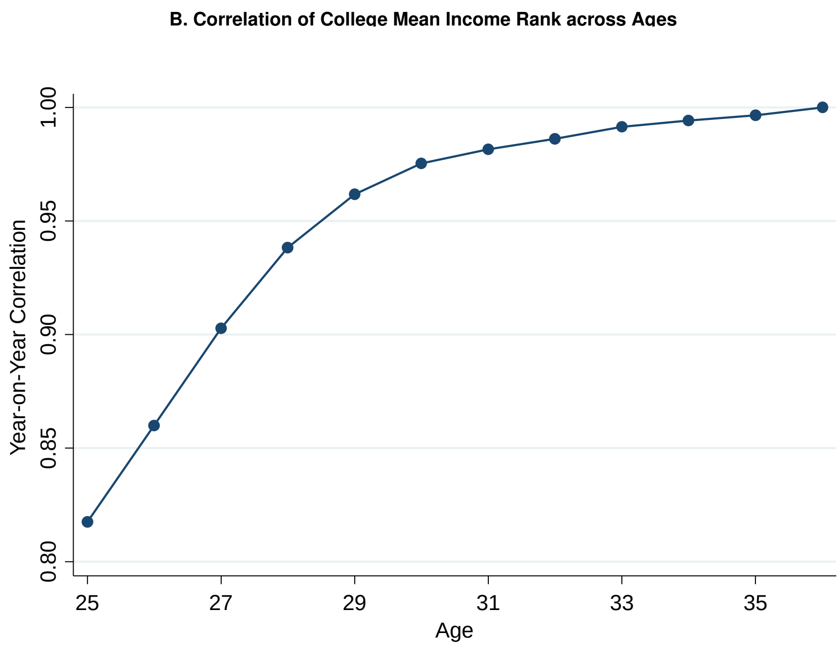
FIGURE I: Distributions of Parent Income by College



Notes: This figure presents the distribution of parent incomes for children in the 1980-1982 birth cohorts. Panel A plots the percentage of students with parents in each income quintile at Harvard University, University of California at Berkeley, State University of New York at Stony Brook, and Glendale Community College, as well as the percentage of students with parents in the top income percentile for each school. Panel B plots the percentage of students with parents in each income percentile across all Ivy-Plus colleges, which include the eight Ivy-League colleges as well as the University of Chicago, Stanford University, MIT, and Duke University. Panel C plots the (enrollment-weighted) distribution of the fraction of children with parents in the lowest income quintile across all colleges. Parent income is defined as mean pre-tax Adjusted Gross Income in 2015 dollars during the period in which the child was ages 15-19. Parent income percentiles are constructed using the parents' rank in the national income distribution among parents with a child in the same birth cohort. Children are assigned to colleges using the college that they attended for the most years between ages 19 and 22, breaking ties by taking the college which a child first attends.

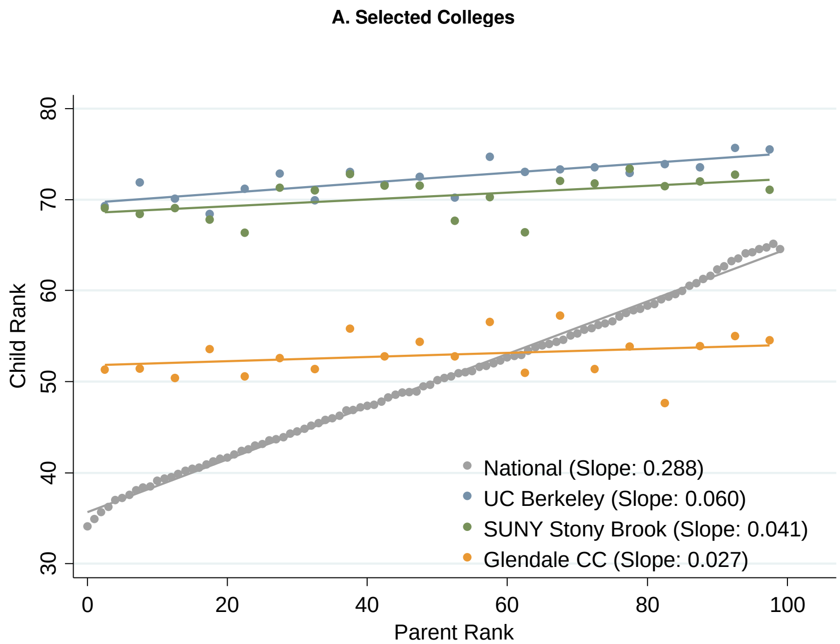
FIGURE II: Children's Income Ranks by Age of Income Measurement

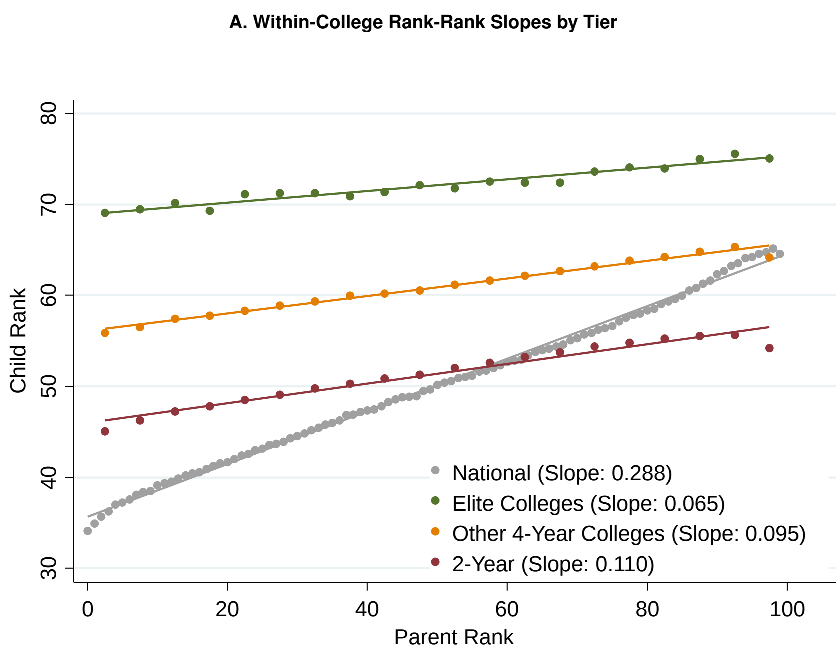




Notes: Panel A plots the mean income rank by age for students who attended colleges in various tiers. Children's earnings are measured as the sum of individual wage earnings and self-employment income. We measure childrens' incomes at each age 25-36 and then assign percentile ranks based on their position in age-specific distribution of incomes for children born in the same birth cohort. "Ivy-Plus" includes the Ivy-League colleges as well as the University of Chicago, Stanford University, MIT, and Duke University. "Other Elite" is defined using all other colleges (excluding the Ivy-Plus group) classified as "Most Competitive" (Category 1) by Barron's Profiles of American Colleges (2009). "Other 4-Year" includes all other 4 year institutions excluding the "Ivy plus" and "Other Elite" groups, measured based on highest degree offered by the institution as recorded in IPEDS (2013). "2-Year" includes all two-year institutions. Panel B plots the (enrollment-weighted) correlation between the college-level mean rank at age 36 and the college-level mean rank at ages 25-36. The sample for both panels of this figure comprise the 1978 birth cohort, with individuals assigned to the college they were attending at age 22. Note that children cannot be linked to parents before the 1980 birth cohort.

FIGURE III: Children's vs. Parents' Ranks within Colleges

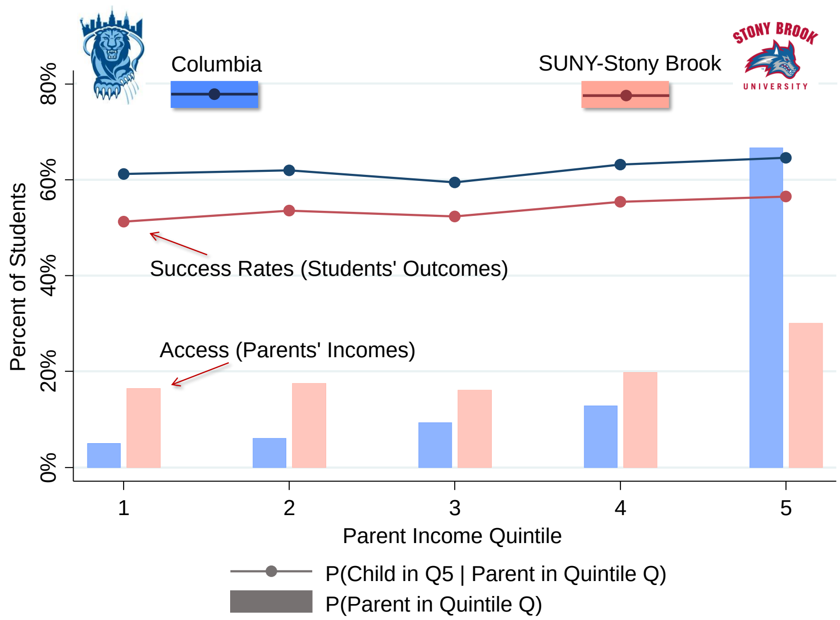




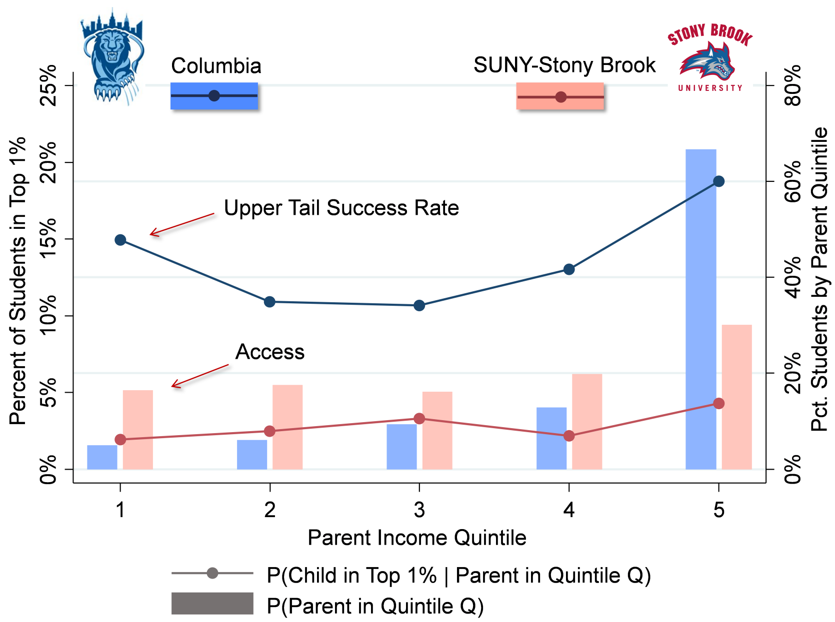
Notes: This figure plots within-college rank-rank slopes for various tiers of colleges as well as the national rank-rank slope, for children from the 1980-82 birth cohorts. We measure all child incomes in 2014, ranked relative to the incomes of other children from the same birth cohort in this year. Panel A presents rank-rank slopes for children at University of California at Berkeley, State University of New York at Stony Brook, and Glendale Community College. Panel B present within-college slopes for various types of schools. The national rank-rank slope is the coefficient on parent rank in a regression of child rank on parent rank for all children, not including college dummies. Points in Panel A are constructed by taking the mean child rank by parent income ventile within the college. Within-college slopes are constructed as the coefficient on parent rank in a regression of child rank on parent rank including an indicator for each college, restricting the sample to children in colleges of a particular tier. Points in Panel B are constructed by taking a (count-weighted) mean of mean child rank by college tier including college fixed effects. See the notes to Figure I for details on the sample specifications and the definition of parent income ranks and college assignment. See the notes to Figure II for the definition of college tiers.

FIGURE IV: Mobility Report Cards for Columbia vs. SUNY Stony Brook

A. Bottom-to-Top-Quintile Mobility

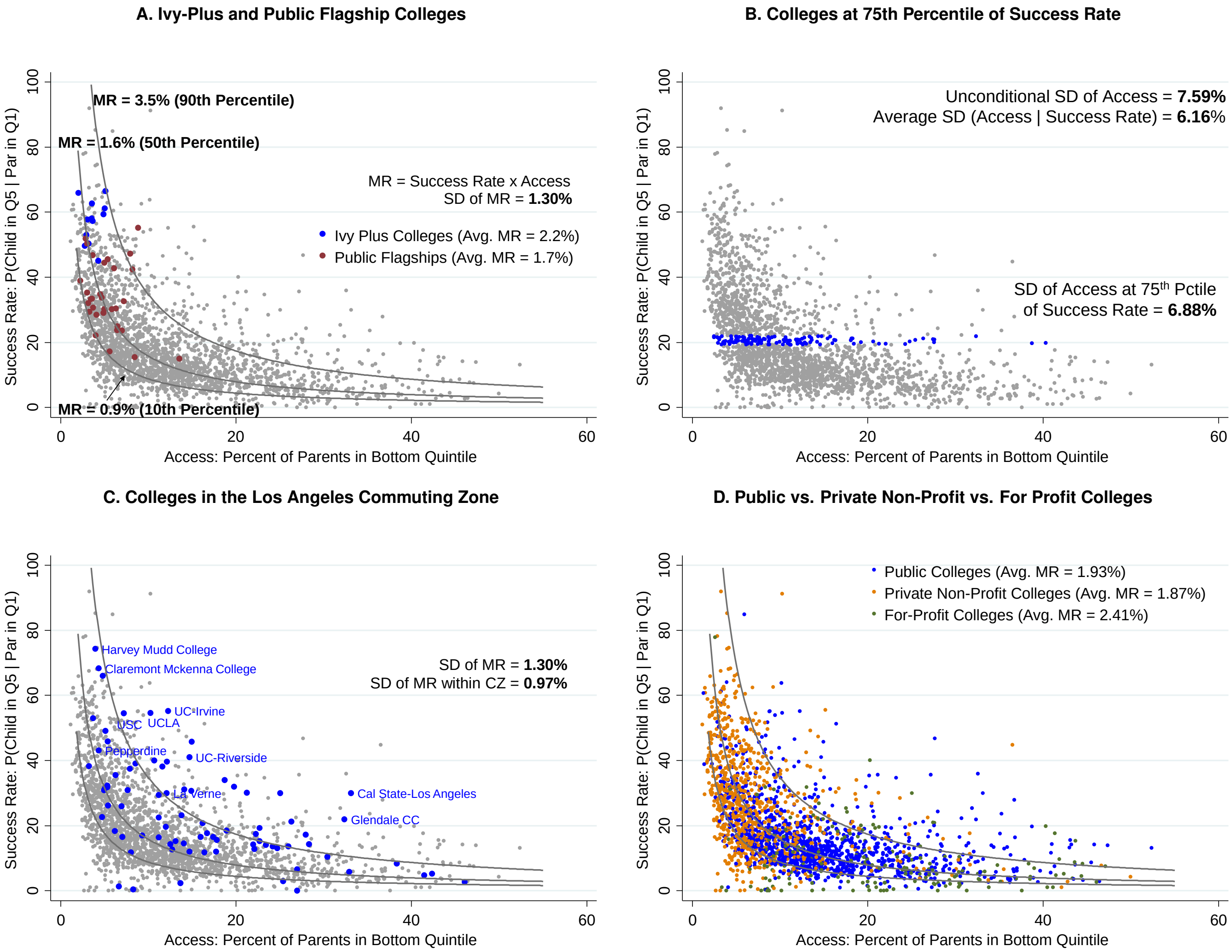


B. Upper Tail (Top 1%) Mobility



Notes: This figure presents both the parent income distribution and the share of children reaching a given rank by parent income quintile (together termed the "Mobility Report Card") for children born in the 1980-1982 cohorts attending Columbia University and State University of New York at Stony Brook. Parent rank distributions are presented by income quintile, analogous to Panel A of Figure I. In addition to the parent income distribution, Panel A also plots the share of children reaching the top quintile of the child income distribution, conditional on parent income quintile. In addition to the parent income distribution, Panel B plots the share of children reaching the top 1% of the child income distribution, conditional on parent income quintile. Formally, points are defined as the empirical probability of a child reaching either the top quintile or the top 1% conditional on parent income quintile multiplied by 100. The probability of a child reaching the top quintile or the top 1% of the income distribution conditional on having parents in the lowest income quintile are termed the "Success Rate" and "Upper-Tail Success Rate", respectively. See the notes to Figure I for details on the sample specifications and the definition of parent income ranks and college assignment; see the notes to Figure III for details on the measurement of children income ranks.

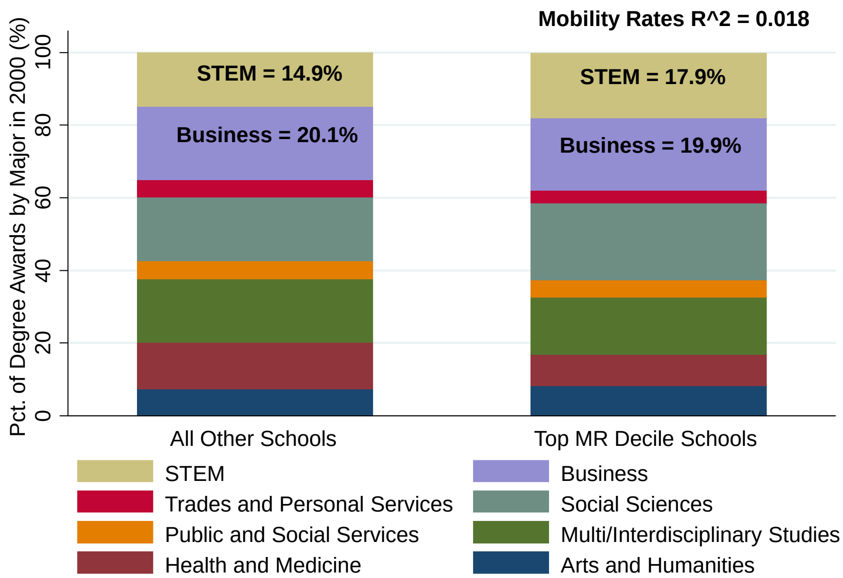
FIGURE V: Mobility Rates: Success Rate vs. Access by College



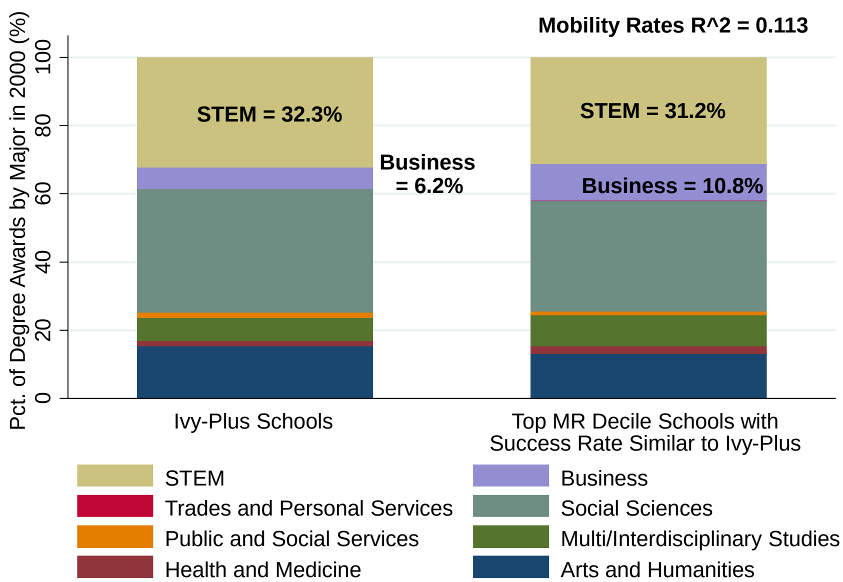
Notes: All four panels in this figure plot the share of children reaching the top quintile of the national income distribution conditional on having parents in the bottom income quintile (termed the "Success Rate") against the probability of having a parent income in the bottom quintile (termed "Access"), by college, for the 1980-1982 child birth cohorts. Multiplying these quantities produces the joint probability of a child having parents in the bottom quintile and reaching the top quintile of the national income distribution, termed the "Mobility Rate". Panels A, C, and D overlay isoquants representing the 10th, 50th, and 90th percentiles of the (count-weighted) distribution of mobility rates across colleges. The panels differ in the schools which are highlighted and the statistics provided. Panel A highlights the Ivy-Plus and public flagship colleges, in blue and red respectively, where Ivy-Plus colleges are defined in the Figure II notes and public flagships are defined using the College Board Annual Survey of Colleges (2016). We omit any state public flagship school that we identify as part of a super-OPEID with multiple schools. Panel B highlights all colleges in the Los Angeles commuting zone. The standard deviaton of the distribution of mobility rates is calculated as the root-mean-square error in a (count-weighted) regression of the mobility rate on indicators for each commuting zone. Panel C highlights schools around the 75th percentile of success rates (weighted by the count of children with parents in the bottom income quintile). The average standard deviation of access conditional on the success rate is constructed by partitioning schools into 50 quantiles (weighted by the count of children with parents in the bottom quintile) and reporting the root-mean-square error of a regression the (count-weighted) share of children with parents in the bottom quintile on indicators for each quantile. The standard deviation of access at the 75th percentile of success rates is the (count-weighted) standard deviation of children with parents in the bottom income quintile for the 37th-39th quantile. Panel D highlights public, private non-profit, and for-profit colleges, defined using the college type in IPEDS (2013). Parent income quartiles and college assignment are defined in the Figure I notes, and child income ranks are defined in the Figure II notes.

FIGURE VI: Distribution of Majors

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

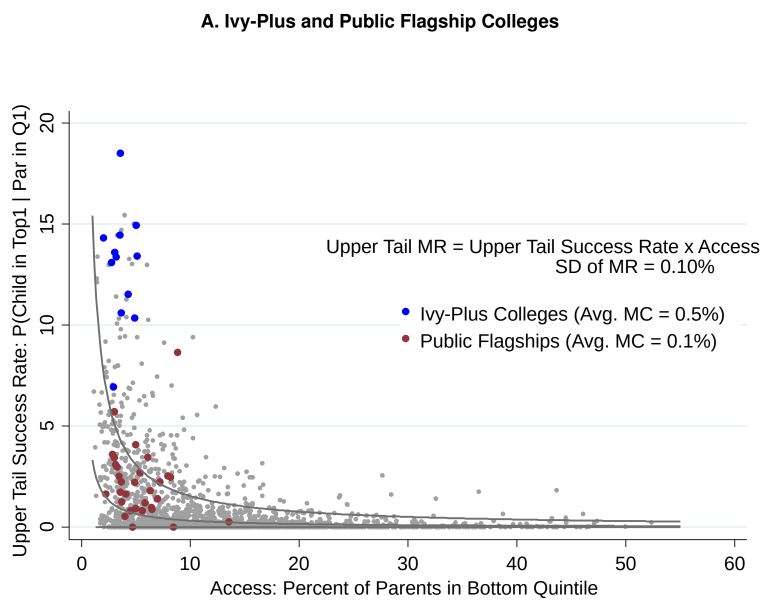


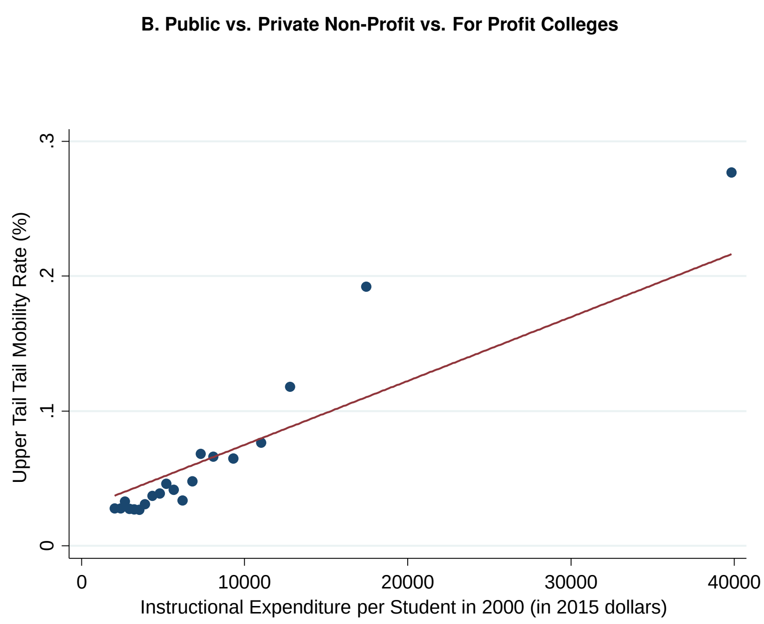
B. Ivy-Plus Colleges vs. High-Mobility-Rate Colleges with Comparable Success Rates



Notes: Panel A presents the fraction of majors at high-mobility-rate colleges compared to all other colleges. Panel B presets the fraction of majors at Ivy-Plus colleges compared to high-mobilty-rate colleges with comparable success rates for the 1980-1982 cohorts. Major shares are defined by categorizing the share of degrees awarded by college in IPEDS (2000) according to the College Board's classification of major categories. High-mobility-rate colleges are defined as colleges with a mobility rate above the 90th percentile (count-weighted) for the 1980-1982 cohorts. High-mobility rate colleges with success rates similar to Ivy-Plus colleges are defined as high-mobility-rate colleges with success rates between the those at the Ivy-Plus colleges with the second-highest and the second-lowest success rates. Ivy-Plus colleges and college assignment are defined in the Figure I notes. Mobility rates are defined in the notes to Figure V.

FIGURE VII: Top 1% Mobility Rates: Success Rate vs. Access by College

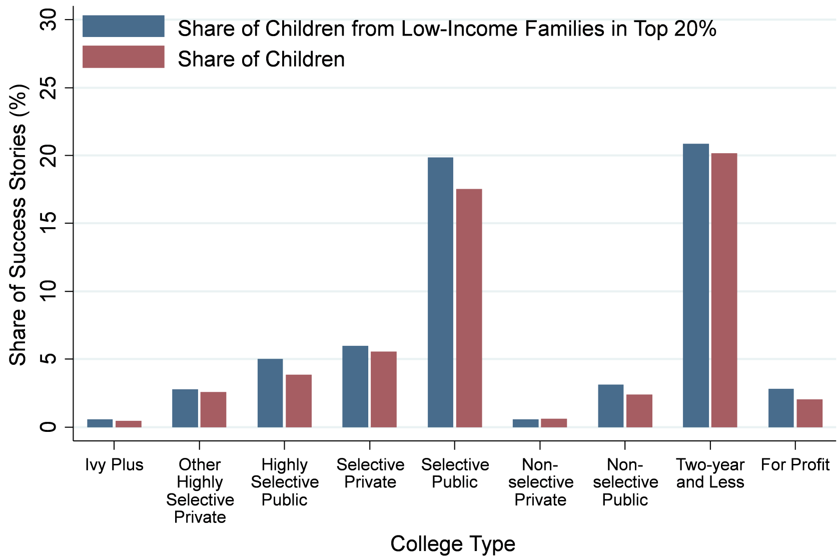




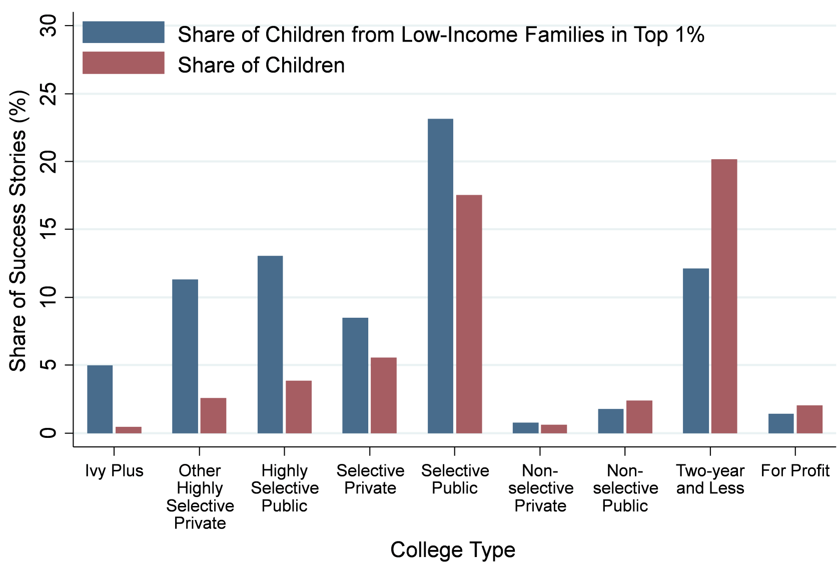
Notes: Panel A plots the share of children with parents in the bottom quintile of the income distribution that reach the top 1% of the income distribution (termed the "Upper Tail Success Rate") against the share of students with parents in the bottom quintile by college. The sample and plot is analogous to Panel A of Figure V, except that this figure plots the probability of a child reaching the top 1% conditional on having parents in the bottom quintile rather than plotting the probability of a child reaching the top 20% conditional on having parents in the bottom quintile. Panel B plots a (count-weighted) binscatter of the joint probability that a child has parents in the bottom quintile and reaches the top quintile against the college's instructional expenditure in IPEDS (2000). Instructional expenditures are inflated to 2015 dollars using the CPI-U-RS. Parent income quartiles and college assignment are defined in the Figure I notes, and child income ranks are defined in the Figure II notes.

FIGURE VIII: Fraction of Success Stories, by School Type

A. Success Rate (Top 20%)

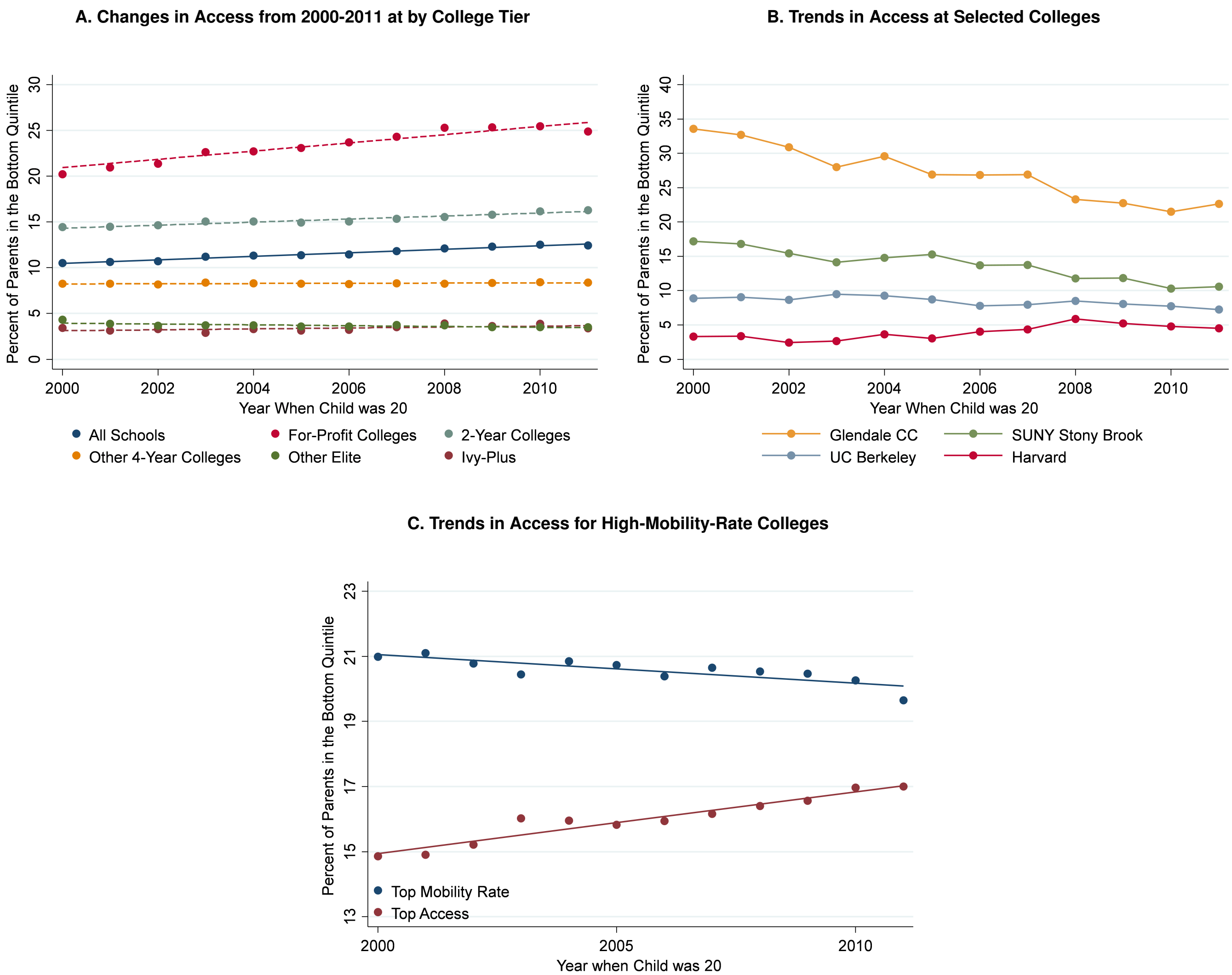


B. Upper-Tail Success Rate (Top 1%)



Notes: This figure plots the fraction of individuals in the 1980-1982 birth cohorts with parents in the bottom quintile that reach a given rank (termed "Success Stories") who attended a particular type of college. In other words, the figure plots the PMF of children that went from the bottom quintile to a particular quantile across college types, excluding non-college-goers for simplicity. Panel A plots the PMF for children in the top quintile and Panel B plots the PMF for children in the bottom quintile. Ivy-Plus colleges are defined in the Figure I notes. Highly selective colleges are those with a categorization of 2 or less in Barron's Profiles of American Colleges (2009). Selective colleges are defined as those with a Barron's categorization of 5 or less. Non-selective colleges are defined as those colleges with a Barron's categorization of 9 or missing. Two-year colleges are defined using the highest degree offered by the institution and public, private non-profit, and for-profit colleges are defined using the control of the institution as recorded in IPEDS (2013). College assignments and parent income are defined in the notes to Figure I. Child income is defined in the notes to Figure II.

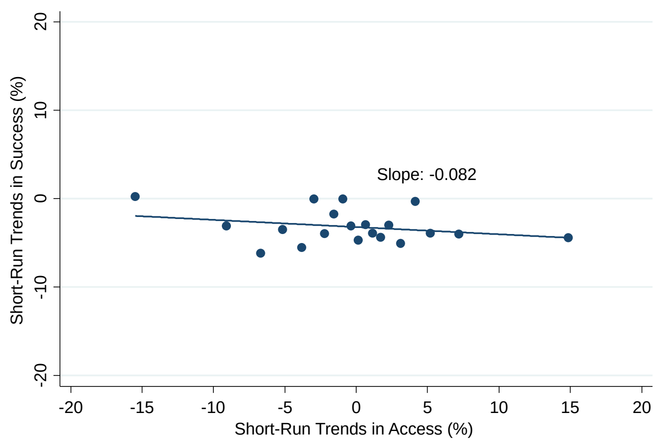
FIGURE IX: Changes in Access over Time



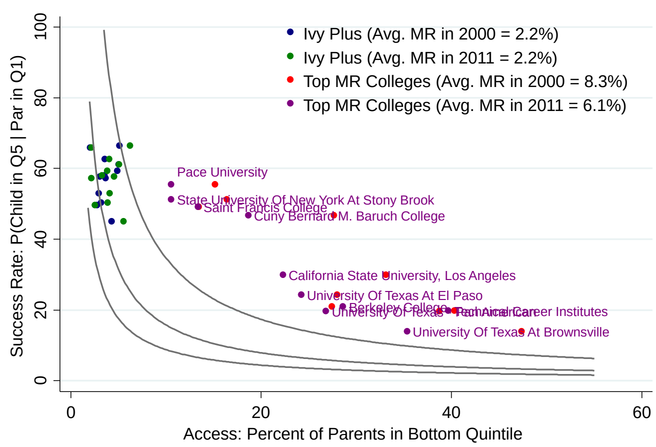
Notes: This figure shows the fraction of students from the bottom quintile of the children-cohort-specific parents' income distribution (i.e., access) over time for various groups of colleges. In all three panels, the x-axis measures the birth cohort of the children, and the y-axis measures access in a school or the count-weighted average access across a given set of schools. Panel A shows average access over time for five mutually exclusive tiers of colleges (as defined in Section 2): Ivy-plus, other elite schools, other 4-year schools, two-year schools, and for-profit schools. We also plot the best-fit linear trend, estimated by a count-weighted regression at the college X cohort level of access on a linear trend. The estimate reported on the figure is the coefficient from this regression multiplied by 11, which is the predicted trend increase in access in each group over our sample period. Panel B shows access over time for four specific schools: Harvard, UC-Berkeley, SUNY-Stony Brook, and Glendale Community College. Panel C shows access over time for three mutually exclusive groups of schools: high mobility-rate, high access but not high mobility-rate, and high success but not high mobility-rate. Each of these groups can be defined using the High Mobility-Rate, High Access, and High Success indicators as defined in the notes to Table 6. The high mobility-rate group are schools with the High Mobility-Rate indicator equal to 1. The high access but not high mobility-rate group comprises schools with High Mobility-Rate equal to 0 and High Access equal to 1. The high success but not high mobility-rate group comprises schools with High Mobility-Rate equal to 0 and High Success equal to 1.

FIGURE X: Changes in Mobility Rates over Time

A. Short Run Trends in Success Rates vs. Access



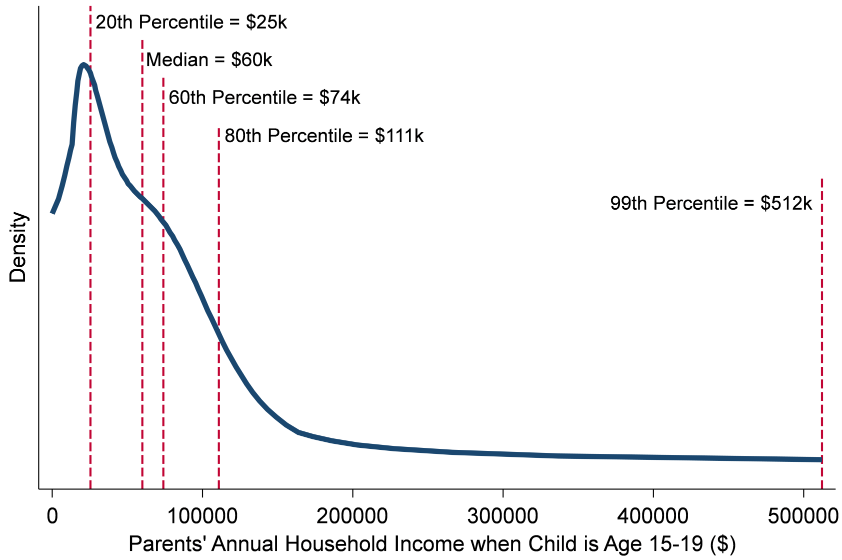
B. Projected Changes in Mobility Rates For Selected Colleges



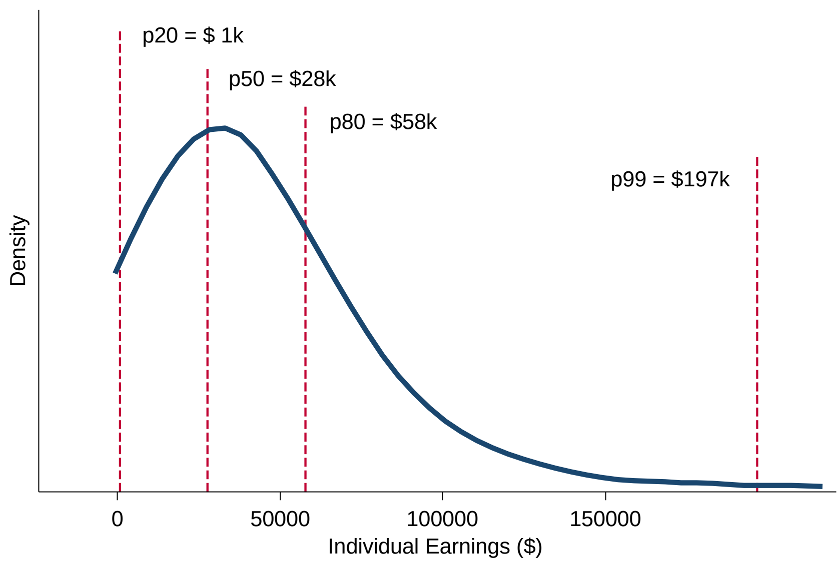
Notes: Panel A presents a binned scatterplot of the relationship between changes in access and changes in success over time. The dependent variable is the trend change in success rates at each school, estimated as the coefficient on cohort in a college-specific regression of success rate on cohort using data from cohorts 1980-84. The independent variable is the trend change in access at each school, estimated as the coefficient on cohort in a college-specific regression of access on cohort using data from cohorts 1980-84. The binned scatterplot then constructs the plot in two steps as in a residual regression. First, we regress the independent and dependent variables on college tier fixed effects, calculate residuals (though we add back the constant terms). Second, we sort the data into twenty equal-sized bins, sorted on the residualized independent variable, and then, for each bin, plots as each dot the mean of the residualized dependent variable against the mean of the residualized independent variable. The best-fit line and reported coefficient comes from the coefficients from a regression of the dependent variable on the independent variables with college tier fixed effects. Panel B presents a scatterplot of success on access for each college, as in Panel A of Figure V, but for a selected set of schools: Ivy-plus schools, and the top 10 schools as ranked on mobility-rate, calculated as using average access for all cohorts as in Table 6. For each of these 22 schools, we plot two dots. Each dot uses the success rate from the pooled 1980-82 cohorts as the y-axis coordinate. The first dot uses access for that school in the 1980 cohort as the x-axis variable; the second dot uses access for that school in the 1991 cohort as the x-axis variable. We also present the average count-weighted mobility rate for each group of schools in 2000 (as the product of success, as measured in the pooled 1980-82 cohorts, and access in the 1980 cohort with success) and as projected in 2011 (as the product of success, as measured in the pooled 1980-82 cohorts, and access in the 1991 cohort with success).

APPENDIX FIGURE I: Income Distributions

A. Parent Income Distribution

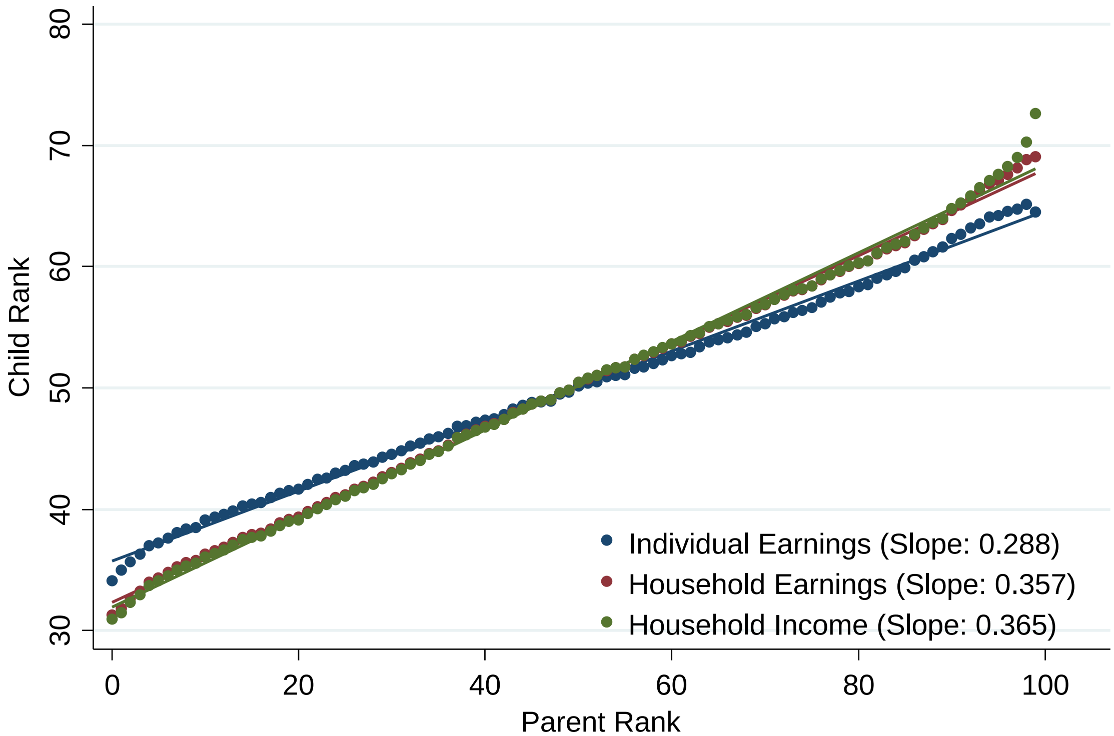


B. Child Income Distribution



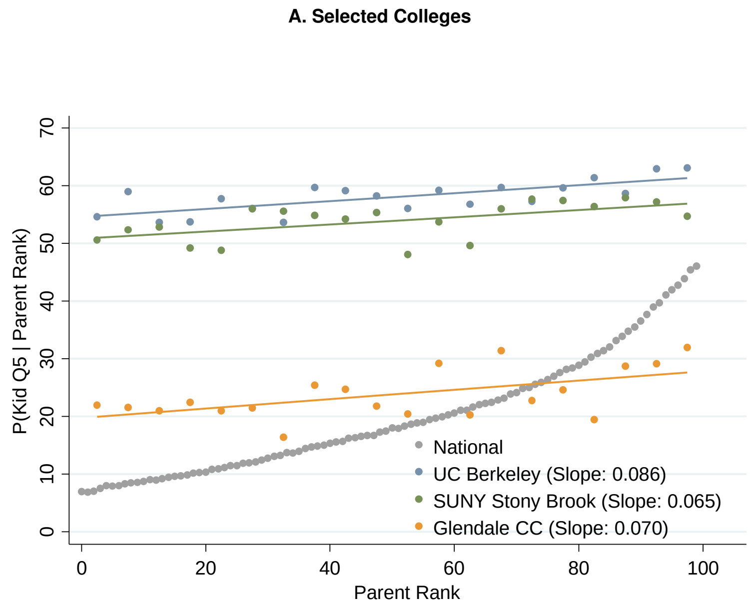
Notes: Panel A plots the distribution of parent income for the 1980 birth cohort in our analysis sample. Panel B plots the distribution of child individual earnings for the 1980 birth cohort in our analysis sample. See Section II.A for our analysis sample construction and Section II.C for income definitions.

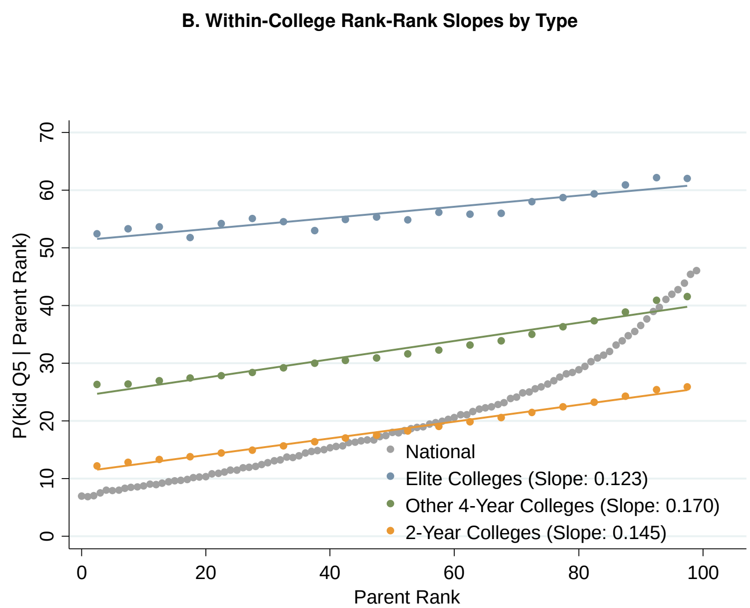
APPENDIX FIGURE II: Rank-Rank Slopes by Child Income Definition



Notes: This figure replicates the national rank-rank series in Figure Ia for the household income and household earnings concepts. See Section II.C for these alternative income definitions and the notes to Figure III for additional detail.

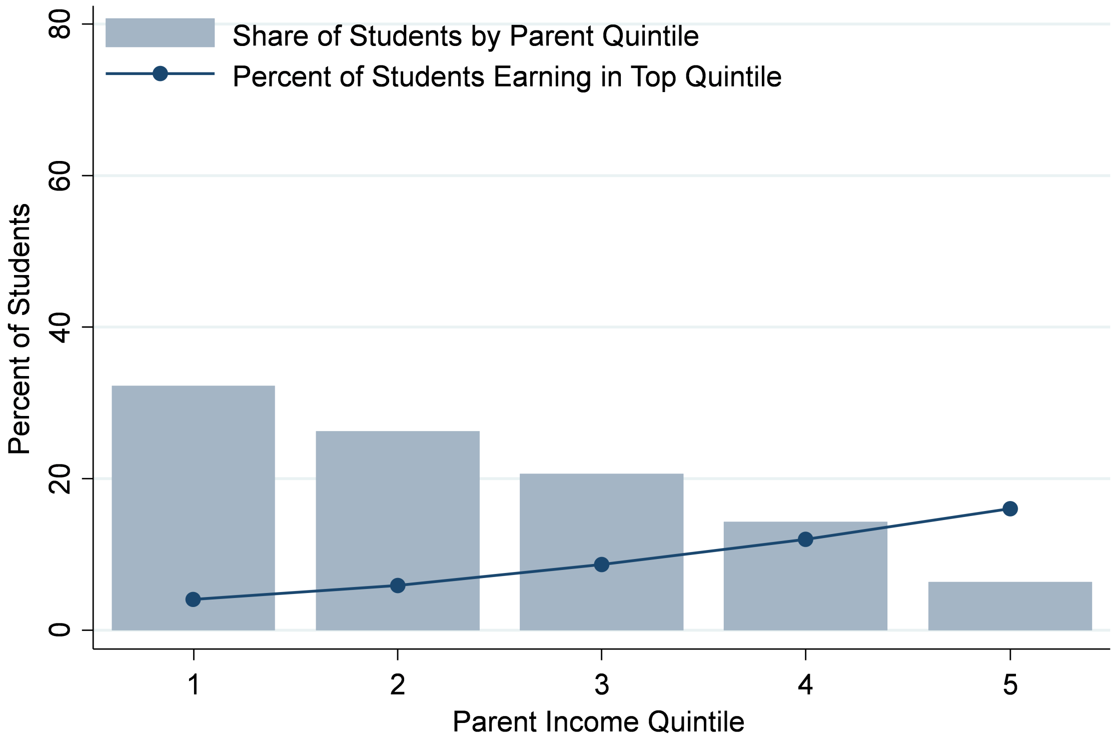
APPENDIX FIGURE III: Share of Children Reaching the Top Quintile by Parent Income Rank





Notes: This figure replicates Figure III with an alternative outcome on the y-axis: the share of children with individual earnings lying in the top quintile of their birth cohorts' (1980-1982) individual earnings in 2014. See the notes to Figure III for additional detail.

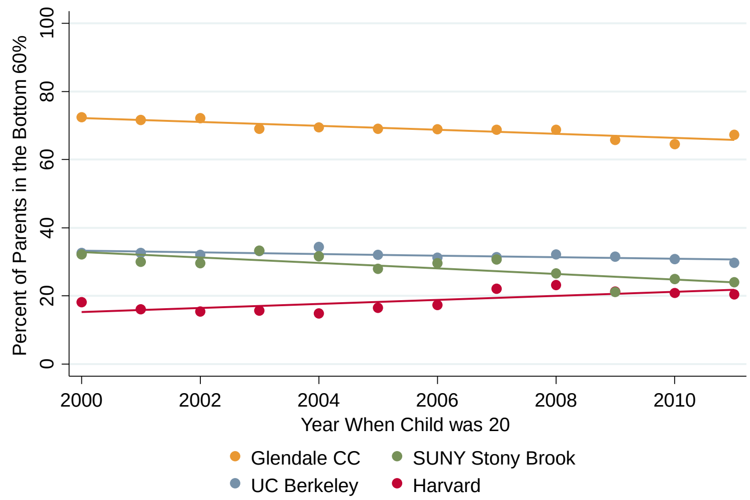
APPENDIX FIGURE IV: Mobility Report Card for Non-College Goers



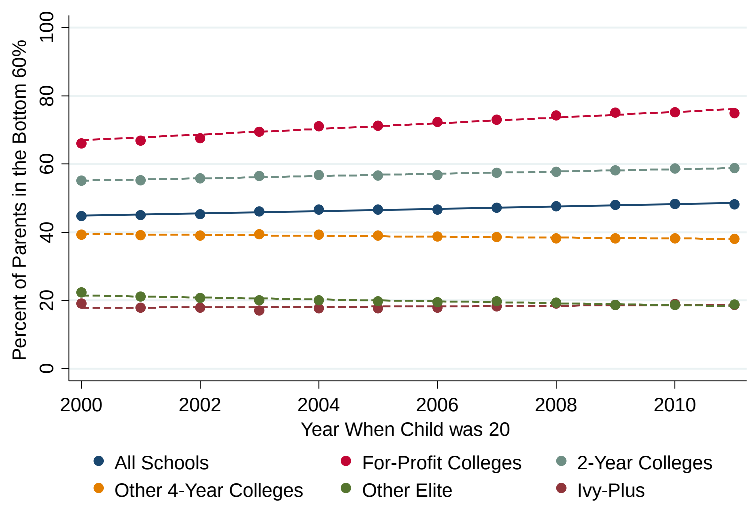
Notes: This figure replicates Columbia University's mobility report card in Figure IVa for children in the 1980-1982 cohorts who did not attend college between the ages of 18 and 22. See Section II.A for detail on our analysis sample construction (including both college-goers and non-college-goers) and the notes to Figure IVa for additional detail.

APPENDIX FIGURE V: Changes in Bottom 60% Access over Time

A. Trends at Selected Colleges

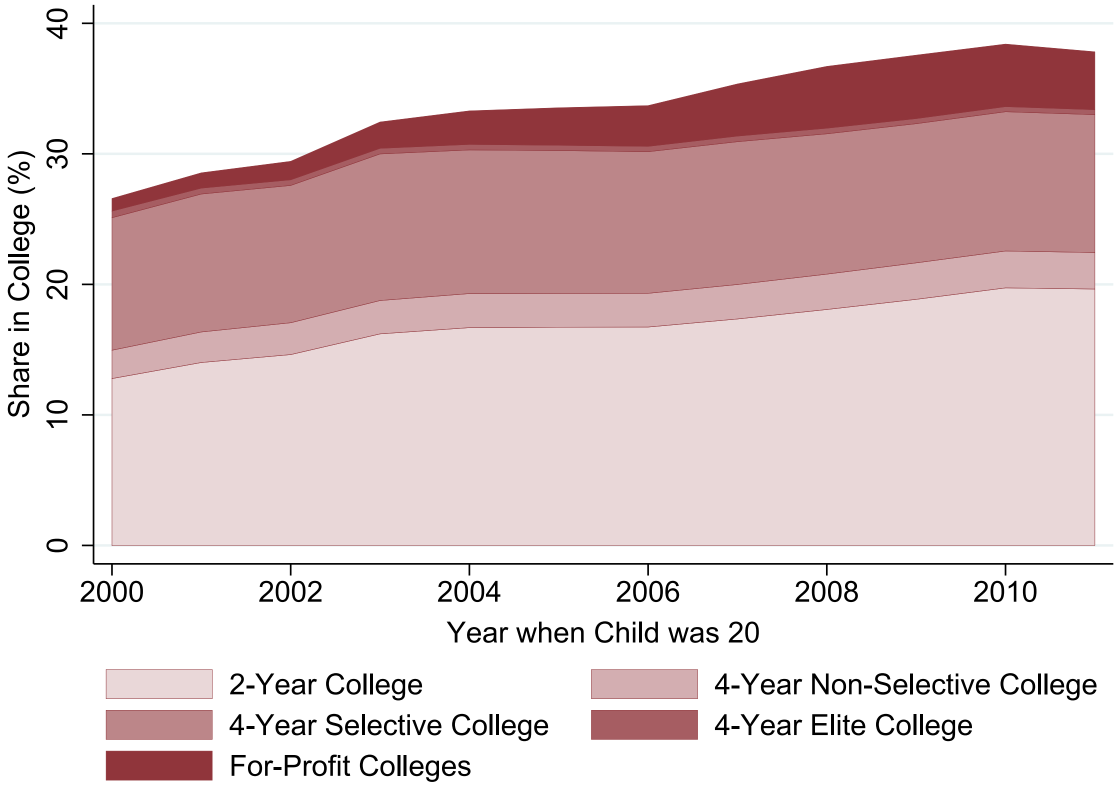


B. Changes from 2000-2011 at by School Tier



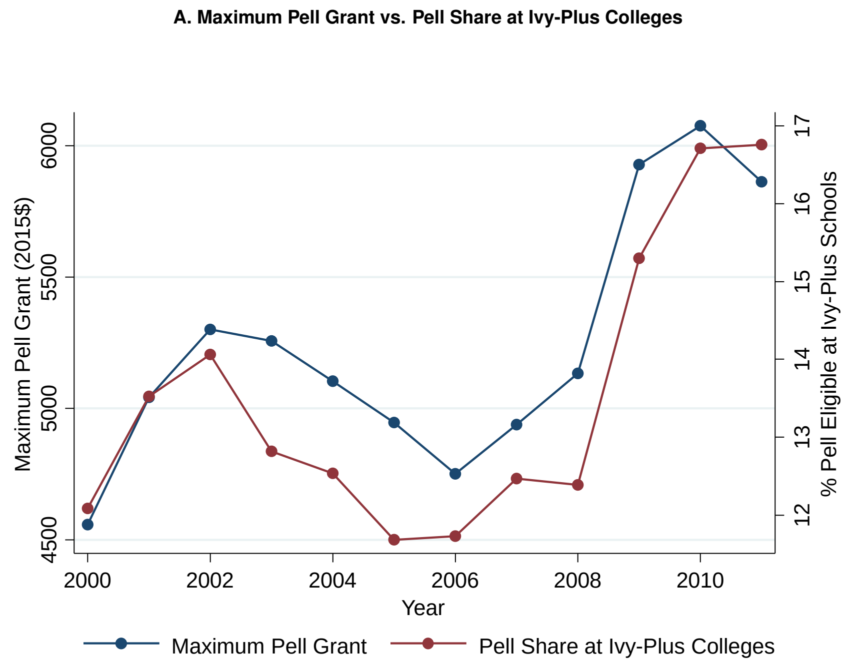
Notes: Panel A replicates Figure IXb for the alternative outcome of the percent of parents in the bottom three quintiles. Panel B replicates Figure IXa for the alternative outcome of the percent of parents in the bottom three quintiles. See the notes to Figure IX for additional detail.

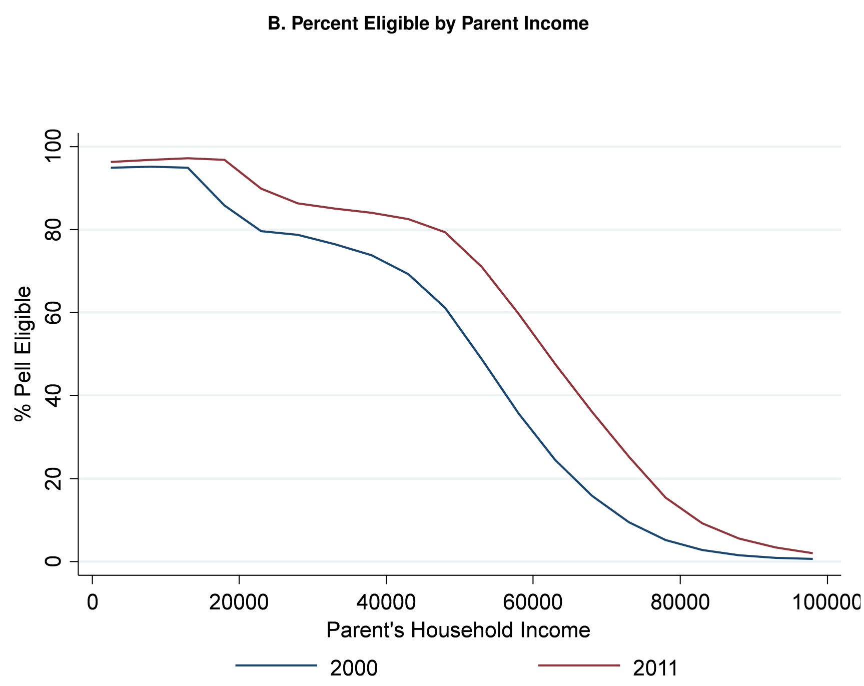
APPENDIX FIGURE VI: Share of Children in College by Type over time



Notes: This figure plots the share of children in college in our analysis sample by year and college type. College attendance here is defined as college attended at age 20. See Section II.B for details of the age-20 college attendance definition.

APPENDIX FIGURE VII: Pell Eligibility over Time





Notes: The left axis of Panel A plots the maximum Pell grant amount available in the fall of each academic year 2000-2011. The right axis of Panel A plots the Pell share of students at Ivy-plus colleges in the fall of the academic year, equal to total Pell grants awarded to students at these colleges in the academic year as reported in the Department of Education's Federal Pell Grant Program Data Books divided by total degree-seeking undergraduates at these colleges in the academic year as reported in IPEDS. Panel B plots the proportion of students in the administrative NSLDS microdata (comprising students who received Title IV aid of any kind) who qualified for and received a Pell Grant, at each level of parental AGI in years 2000 and 2011.