CHANGES IN THE COLLEGE MOBILITY PIPELINE SINCE 1900

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Abstract

Going to college has long conferred a large wage premium. We show that the relative premium received by lower-income college-goers has halved since the 1960s. We decompose the steady rise in American higher education's regressivity using dozens of survey and administrative datasets that document 1900–2020 wage premiums and the composition and value-added of collegiate institutions and majors. Three trends explain two-thirds of rising collegiate regressivity. First, the less-selective and public institutions that disproportionately enroll lower-income students have declined in economic value. Second, lower-income students are increasingly over-represented in America's shrinking community college sector since 1990. Third, higher-income students have driven declining humanities enrollment and expanding computer science enrollment since the 2000s, increasing their degrees' value. Differential selection and shifts between four-year institutions are second-order. College-going provided equitable returns before 1960, but collegiate regressivity has curtailed higher education's potential to reduce inequality and mediates 25 percent of intergenerational income transmission.
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1 Introduction

The 20th century expansion of American higher education played a central role in reducing inequality and the intergenerational persistence of socioeconomic status in the United States (Goldin and Katz, 2009). Though average and marginal labor market returns to higher education have remained high (Autor, 2014; Zimmerman, 2014), decades of stagnated enrollments and decreasing access to high-value programs have led to concerns that the throughput of universities' pipeline to economic mobility has narrowed (Chetty et al., 2020).[1] As a result, an explosion of policy innovation and scholarship has investigated policies that widen educational access, particularly for high-value institutions and majors (e.g. Dynarski et al., 2021; Bleemer, 2022).

Our study begins by demonstrating that the high average returns to American higher education mask a swiftly-growing gap between the value of going to college for students from lower- and higher-income families. Figure 1 characterizes social mobility – the relationship between parental income rank and son's early-30s income rank – by son's educational attainment using data from 1940 and today.[2] College enrollees and graduates earned consistently higher annual wages than non-enrollees across parental income deciles in the early 20th century. In recent years, on the other hand, there is hardly any observational wage return to college-going for the lowest-income students, whereas higher-income college-goers have far higher wages than non-enrollees. Figure 1 thus suggests that the college wage premium has become regressive, constricting the college mobility pipeline.

This study documents when and why the labor market returns to college became positively correlated with childhood parental income. We compile dozens of longitudinal survey and administrative datasets spanning the 20th and 21st centuries that match high school graduates' educational attainment with pre-college academic preparation, collegiate experiences, and early-30s employment outcomes (see Figure 2). These data pin down the beginning of the rise in collegiate regressivity to the 1960s. We decompose that rise into three main components: changes in selection into college, changes in college majors, and changes in collegiate institutions. Differential

[1] Autor et al. (2020) show that after a century of increasing educational attainment that reduced US inequality, the rise in inequality since the end of the 20th century has largely occurred *between* college graduates.

[2] Mobility is measured among male children in the linked 1920–1940 US Census and in the NLSY97. The 1940 US Census was the first national census to collect both education and wages; we use LIDO, an industry-occupation-demographics-adjusted measure of SES, to proxy for parental income in 1920 (Saavedra and Twinam, 2020).

selection into college-going by parental status contributed to the observational rise in collegiate regressivity in its early years, but we find that three key causal factors explain the majority of the declining relative value of college-going for lower-income students. First, lower-income enrollment in high-value majors like engineering and (in recent years) computer science has fallen, first in the mid-20th century and again since 2000. Second higher-income enrollment in (lower-value) two-year community colleges has declined since the 1980s. Third, the economic value of more-selective and private universities – which have persistently enrolled a disproportionate share of higher-income students – rose substantially in the late 20th century.

We begin by showing that the observational returns to college enrollment and attainment were (if anything) relatively larger for male students from *lower*-income families from the 1920s to the 1950s.[3] Since that time, however, lower-income students' return to college has steadily deteriorated to such an extent that the 2000 college-going premium for students from the top parental income tercile was almost 15 percentage points greater than that of bottom-tercile students. To give a sense of scale, a simple simulation shows that contemporary university regressivity went from mediating 0 to 25 percent of the intergenerational transmission of income in the United States between 1960 and 2000.

Several high-level trends in US college enrollment, tuition, and degree program composition over the past 100 years do *not* align with this increase in collegiate regressivity. First, current proportional rates of college-going by parental income are quite similar to those of the early 20th century, though college-going was less common overall until World War II and among low-income students until the 1970s. Second, the rise in regressivity predates the late 20th century increase in college tuition, and has not closed as universities have begun increasing aid generosity in recent years. Third, the 20 percent overall enrollment share in lucrative engineering degrees has changed little over time, though business and social science programs are now more popular than in the past. Finally, the share of college degrees granted by Ivy Plus institutions fell steadily over the past century to about one percent, rendering those institutions negligible to national-level trends. These series rule out several broad-based explanations for why college-goers from higher-income families now out-earn their lower-income peers by more than in the past.

[3] We focus our main analysis on men, whose persistently high labor force participation avoids selection bias in measuring educational wage returns; we return to women's college-going at the end of our study.

Next we consider whether differential selection into college-going by parental income can explain the rise in observational regressivity. We explore how selection into college by family income has changed over time using a variety of skill and academic assessments given to high schoolers over the past century. Nearly all of these assessments reveal that higher-income college-goers have relatively higher test scores than their lower-income peers, but that the gap is largely unchanged since the 1960s. Increasing wage returns to measured pre-collegiate skills in the 1970s made this gap salient for those cohorts but did not otherwise keep pace with higher-income students' rising college wage premium, leaving substantial explanatory scope for changes in the relative treatment effect of going to college by parental income.

Instead, we demonstrate that changes in the value and composition of students' college majors and enrollment institutions ushered in an era of collegiate regressivity after 1960. To investigate the contribution of college majors, we construct the first estimates of the value of college majors from early 1930s through the present and combine them with the observed college major choices of higher- and lower-income students. We find that the relative wage values of different college majors have changed surprisingly little over time, though their dispersion widened as the overall wage return to college rose. After presenting evidence favoring the causal interpretation of our measured college major premiums, we use them to show that in two periods – the middle of the 20th century and recent years – lower-income students earned substantially lower-value majors than their higher-income peers. Major gaps since at least the late 1990s are the result of higher-income students disproportionately shifting out of humanities majors and into high-return computer science and economics majors where GPA-based barriers increasingly exclude lower-income students (Bleemer and Mehta, 2022a). In all, about one-quarter of collegiate regressivity can be explained by changes in the composition of college majors, with rising wage dispersion across major returns contributing an additional ten percent.

Finally, we turn to changes in the composition of and relative returns to different collegiate institutions. Our novel mid-century value-added estimates for over 400 colleges and universities – constructed using Project Talent following the methodology of Chetty et al. (2020) – show that mid-century institutions' value-added was far less correlated with parental income, test scores, or public control than in recent years. Universities with consistently high shares of high-income students had average value-added in the 1960s but saw large relative gains in value-added in subsequent decades. These changes are economically meaningful: extending Chetty et al. (2020)'s estimation of the forecast coefficient of college value-added statistics, we find that 70 to 80 percent of cross-institution variation in estimated value-added is likely causal.

When we combine our historical surveys with federal institution-level financial aid records to measure changes in institutions' composition by parental income over time, we find that higher-income students have steadily flowed out of relatively lower-value community colleges toward four-year universities to a far greater degree than their lower-income peers since the 1980s. Higher-income students have always enrolled at higher-value four-year institutions, yielding little change in within-sector composition in recent decades outside of an interesting spike in the 1970s. The rising value of higher-income students' institutions and lower-income students' relatively declining four-year college enrollment explains about one-third of the overall rise in higher education's regressivity over the past 60 years.

This paper provides a key link between the long literature on the relationship between education, inequality, and economic mobility with the more recent microeconomic literature documenting heterogeneity in the return to college enrollment and attainment. In the former literature, seminal work by Goldin and Katz (1999, 2009) established the centrality of relative college-educated worker supply and demand in determining 20th century inequality, but their model fails to explain the more recent rise in inequality among college-educated workers since the late 1990s (Autor et al., 2020). We document that the college wage premium is increasingly affected by college majors and institutional choices, which clarifies how wage inequality can rise in the top half of the income distribution after 1970 alongside widespread college attendance and high overall returns to post-secondary schooling (Lemieux, 2006; Autor et al., 2008; Acemoglu and Autor, 2011). Similarly, we show that the benefits of higher education were relatively homogenous by parental income in the era of high economic mobility (Ward, 2023; Jácome et al., 2024), but the rise in postsecondary regressivity likely plays an important role in America's more recent mobility decline (Aaronson and Mazumder, 2008; Chetty et al., 2017).[4]

In parallel to these studies of overall inequality and mobility in the United States, a large re-

[4]Studies have documented long-run changes in economic mobility across a wide range of demographic characteristics: race (e.g. Jácome et al., 2024; Ward, 2023; Collins and Wanamaker, 2022), gender (Craig et al., 2019; Buckles et al., 2023; Bailey and Lin, 2022), nativity (Abramitzky et al., 2021), geography (Tan, 2023), and other aspects of school quality (Card et al., 2022; Abramitzky et al., 2024; Russell and Andrews, 2022). Witteveen and Attewell (2017) document that college characteristics partly mediated intergenerational income persistence in the 2000s.

cent literature has turned from estimating the average return to higher education (Card, 1999) to measuring the substantial degree of heterogeneity in that return by institution (Chetty et al., 2020; Mountjoy, 2022) and field of study (Altonji et al., 2016). We provide some of the first characterizations of both long-run trends in the relative returns to majors and institutions and the first long-run analysis of differences in collegiate value by parental income. We leverage our longitudinal data sources to extend the earliest known prior estimates of college enrollment by parental income by 20 years (Jackson and Holzman, 2020); of institutional value-added and college major attainment by 35 years (Chetty et al., 2020; Patnaik et al., 2022); and of college major value-added and enrollment institution by parental status by a half-century (Patnaik et al., 2022; Torche, 2011).[5] The long time span of these series reveals the macroeconomic implications of the substantial heterogeneity in collegiate returns: changes in major attainment (especially the decline in humanities and rise of computer science) and both institutional returns (rising at private and more-selective institutions) and composition (especially rising lower-income community college enrollment) have meaningfully increased the intergenerational transmission of income since the 1960s. This narrowing of the college mobility pipeline dovetails with a large literature examining policies designed to widen that pipeline (e.g. Abramitzky et al., 2024; Dynarski et al., 2021; Bleemer, 2022, 2021; Black et al., 2023).

2 Data

Consider the potential employment outcomes of two high school graduates considering going to college, one from a high-income background and the other from a low-income background. The difference in each youth's potential outcome between college enrollment and non-enrollment might vary between individuals for a number of reasons. What it means to 'go to college' may vary by person due to their choice of institution or field of study, for example. Each student could also derive different value from attending the same program as a result of different baseline (pre-enrollment) academic preparation, aspirations, or social connectedness.

We compile a comprehensive collection of longitudinal individual-level survey and administra-

[5] Abramitzky et al. (2024) demonstrate that low-income students were consistently under-represented at highly-selective institutions throughout the 20th century.

tive datasets covering 1900–2020 US high school graduates' parental incomes, standardized test scores, educational attainment, collegiate institution and major, and early-30s labor market outcomes in order to measure changes over time in the value of college enrollment and attainment. We then augment those data with institution-level datasets characterizing enrollments by parental income. Figure 2 summarizes each of the datasets we compile and the age-18 cohorts for which each key characteristic is observed. All of these datasets – excluding the few marked with asterisks – include measures of parental income, the source of heterogeneity at the root of our analysis.

Our earliest records are linked 1900–1940 US Censuses. The 1940 Census was the first national census to elicit years of schooling, so we match adults back to their teenage years (and their parents) using publicly-available crosswalks (Price et al., 2021; Abramitzky et al., 2022; Helgertz et al., 2023; Ruggles et al., 2024).[6] In 1940, we observe reported 1939 wage and salary earnings and impute earlier parental income using occupation and other characteristics ('LIDO': Saavedra and Twinam, 2020). We then algorithmically link 1940 teenage boys to their AGCT test scores on World War II enlistment records and to years of education on the 1950 Census to measure differential selection into college-going.[7]

Next, we combine respondents from a series of longitudinal and retrospective surveys conducted over the past 100 years. All of these surveys record individuals' parental income, educational attainment, and early-30s wages.[8] We bring together a series of well-known federal longitudinal surveys – the three National Longitudinal Surveys (NLS), three National Center of Education Statistics longitudinal surveys (NLS72, NELS, and ELS), the ADD Health Survey (ADD), and the Panel Survey of Income and Dynamics (PSID) – with two retrospective CPS Occupational Change in a Generation (OCG) supplements, the lesser-used Wisconsin Longitudinal Survey (Wisconsin), and Project Talent, an extraordinary longitudinal survey of over 400,000 mid-century high school students.[9] Parental income is either predicted using detailed parental characteristics (Census and

[6]We use the NYSIIS standard approach from Abramitzky et al. (2022) in the baseline for men – paralleling our other linkages discussed below and in Appendix A – and the Census Tree family tree links for women to incorporate some name changes due to marriage. Appendix B provides details and robustness of these matching procedures.

[7]We do not use the full count 1950 census data on income, due to data quality issues flagged by IPUMS.

[8]The 1947 Time Magazine survey and the American Community Survey (ACS) lack parental income, but they are nevertheless valuable as the earliest and latest available measures of average wages by college major.

[9]We exclude the retrospective General Social Survey (GSS) from our main analysis due to its poorer data quality relative to contemporaneous longitudinal surveys, though Appendix A.10 and Figure AA-1 show that the GSS corroborates our finding of rising collegiate regressivity since at least the 1970s. We also exclude the American National Election Studies (ANES) due to insufficient data; see Appendix A.11.

OCG) or observed continuously or in 10–100 bins; Figure A-1 shows that there is no clear relationship between bin size and year after the earliest predicted-income datasets.[10] We construct parental and child income ranks for each cohort within each nationally representative survey or using CPS data for each cohort (Wisconsin and Project Talent). Each survey conducts different tests of high school academic aptitude; we standardize across surveys using within-sample score rank and directly measure differences in relative labor market value as discussed below. Most of the surveys include college majors for at least some cohorts, while a few – including Time, OCG, and Project Talent – include enrollment institutions.

While institution-level college enrollments by parental income are generally unavailable in recent years in the US, the federal government has published annual Pell funding and enrollments by institution since 1984. The share of students receiving federal student aid through the Pell program is a proxy for the number of lower-income students at those institutions.[11] The IPEDS database also contains detailed institutional characteristics for US universities, and we supplement those characteristics with average parental incomes of students enrolled in college in 2000, from Chetty et al. (2020). The parallel federal College Scorecard database further provides institution-by-major degree counts by Pell status for the 2015–2016 graduating cohorts.

We augment these nationally representative sources with more detailed administrative student records covering enrollees at the University of California campuses at Berkeley, Davis, Irvine, Santa Barbara, Santa Cruz, and Riverside since the mid-1970s (Bleemer, 2018). For pre-1950 enrollees, we digitize historical student registers and link them to contemporaneous US Censuses to observe parental socioeconomic status using the same methods as we do for the other historical censuses.[12] In post-1950 student records we observe field of study and home address, from which we approximate students' parental income by the average household income in their Census tract (linking to the 1980 and 1990 US Census) or Zip code (linking to 1998–2016 average adjusted gross incomes provided by the IRS).

[10]Project Talent collects only 5 parental income bins but also substantial additional parental information, including parental occupation, education, and home value. We predict continuous incomes using binned income and these other features from the 1960 Census. See Appendix A.

[11]Survey data from NPSAS shows that the median 1984–2022 Pell recipient comes from a family with income at the 20–35th percentile of US families; our estimates adjust for these changes over time (see Appendix A.13).

[12]See Appendix B for details on the formatted optical character recognition (fOCR) protocol used to digitize these historical university registers and for the linking methods (following Abramitzky et al., 2022) used to match enrollees to parental LIDO in earlier Censuses.

Finally, we collect a series of auxiliary datasets to further characterize changes in the character of US higher education in the 20th century, including sticker and net price tuition series derived from a variety of surveys, 1920–2020 enrollment series by institution level and selectivity, and several California universities' 1900–2010 faculty registers and course catalogs. More detail on our data sources can be found in Appendix A.

3 The Increasing Regressivity of Higher Education in the US

While it is well established that college-going has grown and broadened over the last century and that the college wage premium has risen in recent decades (e.g. Autor, 2014; Goldin and Katz, 2009), surprisingly little is known about the evolution of the economic value of college-going for lower- and higher-income students. We investigate changes in the relative value of higher education by parental income rank by estimating the following two linear models across the full sample of high school graduates in our combined longitudinal database:

$$Wage_{it} = \alpha_t ParInc_{it} + \gamma_{it} Coll_{it} + \zeta_t + \epsilon_{it} \tag{1}$$

$$Wage_{it} = \alpha'_t ParInc_{it} + \beta_t Coll_{it} + \delta_t\big(ParInc_{it} \times Coll_{it}\big) + \zeta'_t + \epsilon'_{it} \tag{2}$$

where young worker i from birth cohort t grew up in a family with parental income rank $ParInc_{it}$ and in their early 30s earned $Wage_{it}$ annual wages. ζ_t and α_t capture time fixed effects and annual slopes in parental income, and $Coll_{it}$ indicates completing at least one year of college.[13] The level of intergenerational income persistence (α_t) and average returns to higher education (β_t) vary flexibly for each cohort in all specifications.

In Equation 1, the main parameter of interest is the observational return to college γ, which is permitted to vary by dataset and by parental income tercile (with only the top and bottom terciles reported). The main parameter in Equation 2 is δ_t, the degree to which the observational return to college varies by parental income, which we estimate by two different specifications: a non-parametric version in which each dataset is permitted a different interaction term, and a parametric version estimating the average linear trend over time. In our baseline specification, we estimate

[13] When reporting estimates for "attainment" we parameterize $Coll_{it}$ as a matrix containing indicators for enrollment and for completing a four-year degree – isolating the attainment effect – and only report the latter coefficient.

Equation 2 over all males with at least a high school education, standardize sample weights relative to a unit weight for Census respondents, and measure $ParInc_{it}$ and $Wage_{it}$ in CPI-adjusted 2022 annual log wages for comparability across time. Standard errors are robust.

Figure 3(a) shows estimates of γ for students from the bottom and top parental income tercile. The observational return to college declined for both lower- and higher-income students in the mid-20th century, but its rise in recent decades has been driven by higher-income students, who now receive over twice the observational premium – an additional 20 percent – relative to the college enrollment premium received by their lower-income peers.[14] The time trend is clearer in Figure 3(b), which shows both the non-parametric and parametric estimates of δ_t, the correlation between the college wage premium and parental status.[15] The non-parametric coefficients provide somewhat noisy evidence that the relationship between parental income rank and the observational return to college attendance was close to zero in the early and mid-20th century.[16] These estimates turn positive in the 1960s and 1970s, and then continue to grow. By the end of the 20th century, the correlation between parental status and post-college wages was positive and large.[17]

The linear δ trend of 0.0041 confirms that over the 50-year period from 1950 to 2000, the relative return to college for students in the top parental income tercile rose considerably, by about 0.14 log points relative to those in the bottom tercile.[18] We focus on the linear trend since the 1940s – omitting the 1920s Census cohorts – because the early 20th century trend is visibly different from that in later years. Figure A-5 confirms this choice by showing that mean squared error is minimized when a single kink in the linear relationship is placed between the Census and all other observed data sources.

Higher education's declining value for lower-income students both caps its potential to generate social mobility and reduces its downward pressure on income inequality. Figure A-8 compares the

[14]Figure A-2 shows qualitatively similar patterns when child income is measured in wage ranks, which has the additional interpretative advantage of reflecting changes over time in the gap between the high-school-only and some-college rank-rank correlations visualized in Figure 1.

[15]Figure A-4 shows the β_t coefficients, which mirror the aggregate pattern of Figure 3(a).

[16]Feigenbaum and Tan (2020) use a complementary twins design to show that rich and poor twins received similar returns to schooling in the 1940 Census.

[17]Figure A-3 shows a slightly flatter trend for college attainment.

[18]When the data are restricted to only the PSID or only the NLSY, we obtain very noisy point estimates of δ – 0.0119 (0.0148) and 0.0007 (0.0078) – highlighting the advantage of our approach combining across many datasets. The point estimates in rank dollars – 0.24 (0.53) and 0.19 (0.34) – are similarly noisy but surprisingly similar to our baseline estimate of 0.222 (0.077) shown in Figure A-2.

observational wage return to college-going by parental status decile across all of our datasets. Among youths from the bottom of the parental status distribution, the income gains to college-going fell to near zero after the 1977 age-18 cohort. There was no such decline in the college wage premium for top-parental status-decile students, whose increasing collegiate returns augmented their already-strong wage advantages of family socioeconomic status. College failed to boost low income students' social standing to the same degree as their higher-income peers, increasing both income inequality and intergenerational income persistence among the college educated.

4 College Choice in the Long Run

Higher education dramatically expanded in the 70-year period between the two cohorts in Figure 1, raising the question of whether industry-wide shifts correlate with the rise in higher education's regressivity. Many features of US higher education, however, have remained largely unchanged since the 1920s, like students' choice between two- and four-year college enrollment and the extensive number of institutions spread across the country (Geiger, 2014). In this section, we leverage our student-level data to explore how two- and four-year college access has evolved over the past century to generate more earnings for rich students relative to poor ones.[19] We focus on four high-level 20th century trends in US higher education – growing enrollments, rising tuition, and shifts in degree program and institutional choice – and explain why none can meaningfully explain the documented rise in collegiate regressivity since the 1960s.

4.1 College Enrollment

The first step in understanding how post-college earnings have tilted more towards wealthier students over time is to document that college-going was sufficiently common across the parental status distribution to make regressivity meaningful over the entire timeline of our study. We plot college enrollment by parental status in Figure 4(a) in our longitudinal datasets, with the black line and diamonds indicating overall college enrollment rates and the white circles denoting enrollment among the top and bottom parental income terciles.[20] Two key insights emerge. First, college

[19] Unless stated otherwise, all student-level data are measured in the year they turn 18.

[20] Overall college enrollment rates are measured among Census respondents aged 28-42. The data closely match similar statistics reported by NCES (2021); conditioning on high school graduation reveals similar dynamics since

attendance by parental status rapidly diverged in the 1940s as higher-income veterans took up GI Bill tuition benefits (Stanley, 2003).[21] Second, this gap continued to grow until the 1970s, when higher-income sons' college enrollment stagnated and lower-income sons' enrollment ticked up. After 1990, however, the gulf in college enrollment again widened; rich sons now enroll at one-third higher rates than in the past.[22] Though the post-1960 divergence in sons' college enrollment have been documented by Bailey and Dynarski (2011) and Jackson and Holzman (2020), tracing these trends back to the 1920s reveals they began only after World War II. Moreover, Figure A-10(c) shows that the proportional rates of college-going by parental income are quite similar at the start and end of the 20th century.

4.2 College Costs

Credit constraints play a crucial role in human capital investment (Lochner and Monge-Naranjo, 2011). We construct 1920–2020 average "sticker" and "net" tuition, fees, housing, and food price series separately for four-year public, four-year private, and two-year public colleges, shown in Figure 4.[23] Overall, the relationship between (sticker or net) tuition levels and enrollments by parental income appears relatively weak over the long run. Neither the tuition increase after World War II nor its subsequent 1970s decline corresponds to a shift in college regressivity in the same direction, but net tuition's spectacular growth between 1980 and 2010 (e.g. Dynarski et al., 2023) does.[24] This latter trend lagged collegiate regressivity by two decades, though, suggesting that college costs play a second-order role in explaining regressivity's growth over time.

the 1950s (Figure A-9).

[21]Figure A-10(c) shows that older top-tercile sons disproportionately enrolled in and attained college degrees after World War II, as in Abramitzky et al. (2024) and Collins and Zimran (2024). Figure A-11 presents college enrollment and attainment by parental income in each of our datasets.

[22]Two- and four-year college enrollment grew in parallel before the 1970s (Figure A-20). We find similar trends when we examine the share of respondents who report at least four years of college education (Figure A-10b), restrict to high school graduates (Figure A-9), or look at graduate school attendance for male students (see Figure A-12). We discuss the very different patterns of female enrollment in Section 10.

[23]The net cost of college attendance is the enrollment-weighted sticker price minus grant and scholarship aid across all institutions by group, in contrast to prior work approximating tuition using overall revenues and expenditures (Jones and Yang, 2016; Donovan and Herrington, 2019). See Appendix A.17 for details on data and construction.

[24]Our net tuition measures do not include veterans' educational aid, which reduced college costs in post-war periods.

4.3 College Majors

The income returns to college also depend on whether colleges' degree programs align with contemporaneous skill premia and preferences (Altonji et al., 2016). Figure 4(c) shows annual major attainment shares from three retrospective surveys spanning the 20th century.[25] About 20 percent of college graduates had engineering degrees in both cohorts of Figure 1. Science and humanities degrees contracted and social science, business, and other professional (the residual) degrees expanded in that interval.[26] This suggests college degrees have been fairly responsive overall to economic development occurring between the two cohorts studied in Figure 1, but four-year universities' aggregate degree composition changed relatively little over the period when collegiate regressivity was rising.

4.4 Elite Colleges

Elite Ivy and Ivy Plus universities are prominent in discussions about college access, especially due to their propensity to enroll higher-income students and elevate average post-college wages (Chetty et al., 2023), perhaps especially among higher-income students (Michelman et al., 2022). However, Figure 4(d) shows that the share of college degrees granted by these elite institutions began has been steadily declining for a century, well before university regressivity began.[27] The gains to college tilt more towards higher-income sons now – with Ivy Plus enrollment around one percent – than in 1940, when this share was many times higher.

In sum, while the expansion of US higher education has been uneven and increasingly costly, there is no obvious correlation between the rise in collegiate regressivity since the 1960s and high-level trends in enrollments, college costs, elite college enrollment, or major choices. As a result, we turn our attention to disparities *within* college-going cohorts by parental income in order to explain rising collegiate regressivity.

[25] We complement prior work by collecting major attainment for a longer span of birth cohorts, sometimes reported closer to their college graduation date (Altonji et al., 2016; Gemici and Wiswall, 2014; Webber, 2016).

[26] Appendix C reproduces these patterns for faculty shares, course offerings, and degree distributions using UC Berkeley and Stanford University records.

[27] See Appendix A.18 for the sources used to construct these historical enrollment trends. The decline was only interrupted by a short-lived rise in elite enrollment during the post-World War II GI Bill period (Abramitzky et al., 2024).

5 The Sources of Rising Collegiate Regressivity

What explains the sharp increase in the wage return to parental status among college enrollees in the second half of the 20th century? This section decomposes rising collegiate regressivity into three components, each of which may vary by parental status: changes related to the composition of college-goers, changes related to the composition or returns to college majors, and changes related to the composition or returns to enrollment institutions.

Let $p_t(i) = p_t\big(a_i, u_i, m_i, PI_i\big)$ be college enrollee i's wage premium over not going to college given his pre-college academic aptitude a_i, enrollment institution u_i, major m_i, and parental income PI_i (capturing residual differences) in age-18 cohort t. Let D_t be the difference in average wage premiums between enrollees from the top ($q = T$) and bottom ($q = B$) income terciles:

$$D_t \equiv \Delta_q \bigg[E[p_t|q]\bigg] = E[p_t|T] - E[p_t|B] \tag{3}$$

For tractability, we make three simplifications to decompose D_t into a series of components. First, we split D_t into terms that would be zero if wage premiums were *not* independent across individuals – like changes in institutional or major peer effect – and define a residual term ϵ_t' to capture that interdependence. The remaining terms can be disaggregated by a, u, and m:

$$D_t = \Delta_q \left[\int_a \sum_u \sum_m \big(P_t(a, u, m|q) E_t[p_t|a, u, m, q]\big)\, da \right] + \epsilon_t' \tag{4}$$

Because our data generally only permit observation of a subset of (a, u, m) in any given t, we can calculate $P_t(a, u, m|q)$ and $E_t[p_t|a, u, m, q]$ for only a small set of cases. The second of three simplification is to linearize p_t in its first three terms. Third, we residualize out q from the expectation, so that the residual will capture any constant and time-varying terms reflecting differences in p_t between higher- and lower-income students *within* (a, u, m). This allows us to separate the expression into the following three measurable components:

$$D_t = \Delta_q \left[\left(\int_a P_t(a|q) v_t^a(a)\, da \right) + \left(\sum_u P_t(u|q) v_t^u(u) \right) + \left(\sum_m P_t(m|q) v_t^m(m) \right) \right] + \epsilon_t \tag{5}$$

where the scaled log-dollar values of a_i, institutions, and majors are given by v_t^a, v_t^u, and v_t^m. Notice that ϵ_t captures many residual terms in addition to ϵ'. Most innocuously, it includes all second-order and higher terms for each argument in p_t, though this linearity assumption has already been implicitly imposed in the estimation of value-added statistics above. It also includes all interactions between these terms, most problematically the joint relationships between a and each of u and m; that is, it assumes that the return to university and major value is constant in pre-college ability.[28] Finally, as noted above, it reflects any differences in p_t by parental income among students with the same a_i, u_i, and m_i, as well as any error terms arising from mismeasurement of a_i or v_t^x.

Adding and subtracting the initial valuation of each characteristic decomposes D_t into three enrollment 'composition' components and three relative 'value' components:

$$D_t = \epsilon_t + \overbrace{\int_a v_0^a(a)\Delta_q\big[P_t(a|q)\big]\,da}^{\text{Test Composition}} + \overbrace{\int_a \Delta_q\big[P_t(a|q)\big]\big(v_t^a(a) - v_0^a(a)\big)\,da}^{\text{Test Returns}} + \overbrace{v_0^u(u)\Delta_q\big[P_t(u|q)\big]}^{\text{Institutional Composition}} + \underbrace{\sum_u \Delta_q\big[P_t(u|q)\big]\big(v_t^u(u) - v_0^u(u)\big)}_{\text{Institutional Returns}} + \underbrace{\sum_m v_0^m(m)\Delta_q\big[P_t(m|q)\big]}_{\text{Major Composition}} + \underbrace{\sum_m \Delta_q\big[P_t(m|q)\big]\big(v_t^m(m) - v_0^m(m)\big)}_{\text{Major Returns}} \tag{6}$$

The following three sections discuss how we leverage our panoply of data sources to measure of each of the components of this decomposition, focusing in particular on measurement of $\Delta_q\big[P_t(x|q)\big]$ and $v_t^x(x)$ for $x \in a, u, m$ and $t \in [1920, 2020]$.

6 Selection Into College

The observed regressivity of US universities could combine the treatment effect of college enrollment with selection bias generated by differential selection into enrollment. Higher-income college-goers may increasingly out-earn their lower-income peers because of their improving pre-college capabilities (or the deteriorating relative capabilities of lower-income youths choosing to enter college). Though there is little relative growth in lower-income college-going between 1960 and 2000, Figure 4(a) may mask compositional changes in college enrollment by parental income over time. We explore this possibility by measuring changes in the return to and composition of

[28]The literature on the supermodularity of university selectivity is generally inconclusive, but Bleemer (2021, 2022) provides evidence for a negative relationship.

cognitive test scores among lower- and higher-income college enrollees.

We first approximate the return to pre-college cognitive skill by estimating the within-education-bin wage value of pre-college test scores separately in each dataset where both are observed. Figure 5(a) shows that the estimated return to pre-college cognitive skill was positive and growing in the mid-20th century. This return rose as high as 40–60 percent of wage for someone moving from the bottom to the top of the test distribution before falling in the 1990s, mirroring prior work on its rise and subsequent partial reversion in this period (Deming, 2017; Castex and Dechter, 2014).

Next, we turn to trends in college-goers' test scores by parental status over time. We re-estimate Equation 2 replacing Y_{it} with student test score outcomes; panels (b) and (c) of Figure 5 plot the resulting γ and δ coefficients. College-goers have consistently higher test scores than their non-college peers, and this held to a similar degree among high- and low-income students before 1960 in both the Wisconsin Longitudinal Study and among World War II draftees.[29] After 1960, a positive gap emerges, but stays stable at around 10 test score ranks between the highest- and lowest-income students.[30]

This exercise suggests pre-college human capital contributed to rising observational collegiate regressivity in the 1960s (as relative test scores rose) and 1970s (when cognitive skills' labor market returns increased). However, neither gap has continued to widen since the 1980 age-18 cohorts, leaving most of the long-run rise of regressivity unexplained by differential selection.

7 College Majors

7.1 Major Returns

Next we examine the potential contributions of college majors to the rise in collegiate regressivity. This necessitates measuring the relative economic value of different college majors over time. We do this by estimating simple linear models of the following form over high school graduates in each of our datasets that record both majors and early-30s incomes (Time, Wisconsin, CPS OCG,

[29] As above, we exclude the Census-linked draftees from our linear trend because that period deviates from the long-run pattern.

[30] This pattern mirrors the finding in Hendricks et al. (2021) that the correlation (conditional on family income) between academic ability and college enrollment rose in the 1940s–1950s and then remained unchanged in subsequent decades. Figure A-7 shows weak evidence of a relative *rise* in lower-income college *graduates*' pre-college cognitive skills since the 1960s.

Project Talent, the NLS's, and the ACS):

$$w_{it} = Major_{m_i} + \zeta SomeCollege_i + \alpha_t + \epsilon_{it} \tag{7}$$

where w_{it} are early-30s wages for individual i from cohort t and $Major_m$ fixed effects are estimated for either ten disciplines or sixty-six detailed majors.[31] Standard errors are robust.

Figure 6 shows the estimated coefficients for each of the six most popular disciplines spanning the 20th century.[32] Though the overall college wage premium declined in the 1950s and rose from the 1960s until the 2000s, the ordering and spread across disciplines' average wages has stayed remarkably constant over time, with humanities at the bottom and engineering and business majors earning the highest wages. Relative wages of natural sciences have slowly increased over time and are now similar to those of business majors; social science majors have steadily earned middle-of-the-pack wages. The spread in wages across disciplines widened as the observational college wage premium increased.[33]

What share of cross-discipline or cross-major wage variation is the causal treatment effect of earning that major, and what share is selection bias? Bleemer and Mehta (2022a,b) present quasi-experimental evidence from a case study favoring a forecast coefficient slightly over 1: for every 1 dollar difference in the average wages of graduates who earned major A over major B, students themselves earn just over 1 dollar less by switching from major A to major B.[34] In the absence of longitudinal quasi-experimental evidence, we proceed with a selection-on-observables analysis to quantify degree programs' labor market returns.

Table 1 presents a series of forecast coefficients using NLSY97 data (where all requisite respondent characteristics are observed). For each definition of major – coarse discipline or detailed major – we estimate multiple versions of Equation 7: a baseline version as specified above and

[31] Since majors are generally unobserved for non-graduates and two-year degree holders, $Major_m$ is 0 and $SomeCollege_i$ is 1 for anyone who attends but does not graduate college. We do not control for graduate degree attainment since such degrees are endogenous to major choice and may be part of majors' value.

[32] Major-specific observational returns have previously been observed as early as the 1990s in the National Survey of College Graduates (Patnaik et al., 2022) and the NLSY79 (Arcidiacono, 2004). See Table A-2 for coefficient estimates and Table A-3 for detailed major coefficient estimates.

[33] This growing spread across majors echoes the disappearance of middle-skill jobs in the labor market (Autor, 2014).

[34] Dahl et al. (2023) estimate a forecast coefficient of 0.96 in the context of Swedish high school majors, while Kirkeboen et al. (2016) do not estimate a forecast coefficient. Arcidiacono (2004) presents a three-discipline structural model suggesting a forecast coefficient of about 0.7.

versions with additional X_{it} covariates. We then forecast the latter estimates with the baseline estimates (weighting by enrollment):

$$\widehat{Major}_m^C = \alpha^f + \beta^f \widehat{Major}_m + \epsilon_i^f \tag{8}$$

where β^f measures the degree to which quantitative wage differences between majors are preserved conditional on covariates. Disciplines and majors earned by fewer than 20 respondents are omitted from the forecast. The first three columns of Table 1 show evidence of a forecast coefficient (β^f) close to 1: conditioning on parental income, pre-college test scores, and race does not meaningfully attenuate the relative wage gaps between coarse majors or detailed disciplines.

One reason for the absence of attenuation may be the shared ϵ_{it} terms in the second-stage (forecast) estimation. Since the same respondents are used to estimate $\widehat{Major}_m$ and $\widehat{Major}_m^C$, small-sample bias could bias β^f toward 1. Columns 4–7 in Table 1 show versions of Equation 8 where $\widehat{Major}_m^C$ and $\widehat{Major}_m$ are each estimated using half of the data (stratifying by discipline or major). Though the baseline correlations are lower due to the NLSY97's small sample size, adding covariates still has no meaningful effect on the relative wages of different disciplines or majors.[35] We conclude that $\widehat{Major}_m$ approximately captures average *causal* wage differences between college majors so long as it is estimated over a sufficiently large number of respondents. Going forward, we refer to $\widehat{Major}_m$ as a major's 'premium.'

7.2 Major Composition

We next explore changes in lower- versus higher-income students' declared majors over time, summarized using our major premium estimates from Section 7.1. We observe college major disaggregated by parental income in the OCG, Wisconsin, Project Talent, and three NLS datasets as well as by Pell status (at the institution level) in College Scorecard for the 2015–2016 graduation cohorts. We supplement those sources with annual college major choices by students in the University of California system grouped by parental income between 1920 and 1945 and again between 1975

[35] Note that in our analysis below, we focus on $\widehat{Major}_m$ estimates from Time and the ACS, both of which have much larger samples than the NLSY97. The resulting measurement error in those datasets is likely negligible.

and 2015.[36] We measure the difference in the average $\widehat{Major}_m$ of majors declared by students from the top and bottom SES tercile in each data source using three estimates of $\widehat{Major}_m$, the log income return to each major.

First consider the light gray squares and lines in Figure 7, which show that 1920–1940 college students from the top tercile earned degrees in slightly higher-paying disciplines than their bottom-tercile peers when valuing disciplines by their 1932 college cohort-based average wages (measured in the 1947 Time survey) across ten disciplinary categories.[37] This gap grew in the 1940s, may have shrunk in the 1950s, and by the 1980s had fully disappeared. At the end of the 20th century, students from lower- and higher-income backgrounds were declaring similar-value college majors. However, the gap has reopened since 2000, trending back towards a level of cross-discipline stratification similar to that of the early 20th century.

The dark gray circles and lines in Figure 7 replace the 1932 discipline premiums with 2005 premiums estimated in the ACS. While the trends look somewhat similar, these estimates' greater dispersion amplify the swings between 1950s regressivity, late 20th century recovery, and return to regressivity in recent years. These gaps are magnified further when measured across 66 detailed major categories rather than ten disciplines (in the darkest shading and triangles): bottom-tercile students now earn majors worth about 5 percentage points less than those earned by their top-tercile peers, likely the largest gap in 100 years.[38]

The two highest-value disciplines play central roles in driving this recent trend. Figure 8 visualizes the $\sum_m v_t^m(m)\Delta_q P_t(m|q)$ terms from Equation 5 by discipline since 1995 (using the UC data), separating out computer science and economics. Higher-income students' enrollment in these high-premium fields relatively rises over this period while falling in low-premium humanities fields.[39] Figure A-15 shows that the well-publicized decline in humanities enrollment (e.g. Schmidt, 2018) is disproportionately driven by higher-income students. Growth in computer science degree attainment in the same time period has left lower-income students behind. These two

[36]The California-specific data (denoted as solid lines in Figure 7) appear to reflect national trends (denoted as symbols) in the years when both sources are available.

[37]Detailed major information is largely unavailable before 1950, except among older OCG respondents.

[38]Bleemer and Mehta (2022a) show a similar trend in the value of college majors earned by underrepresented minority students, finding that the trend can be explained by the proliferation of GPA-based major restriction policies. Startz (2024) uses the College Scorecard to document contemporary Pell stratification across college majors.

[39]Figure A-14 further documents that lower-income students have not kept up with higher-income students' expanding engineering presence over time, in recent years wholly explained by their lower enrollment in computer science.

enrollment changes alone explain a 3 percentage point decline in lower-income students' value of college-going since 1995.[40]

We conclude that major composition has meaningfully contributed to the rising regressivity of US higher education both in the mid-century and in recent years, with a secondary role played by the growing dispersion of major returns as higher-income students have begun to predominate increasingly valuable high-return majors.

8 Collegiate Institutions

8.1 Institutional Returns

Turning from majors to institutions, we next quantify the distribution of labor market returns to attending specific institutions. In contrast to college majors, positive selection on student characteristics into institutions likely biases average wage differences across institutions as proxies for those institutions' wage value to their students (Chetty et al., 2020). As a result, we measure the relative value of each US postsecondary institution using linear value-added models of individuals' early-career wages (e.g. Chetty et al., 2020; Bleemer, 2021, 2022; Eller, 2023):

$$w_{it} = Inst_{u_i} + \beta_t X_i + \alpha_t + \epsilon_{it} \tag{9}$$

where w_{it} are annual log wages for individual i from cohort t and $Inst_u$ is interpreted as the wage value-added of each institution u. We allow β_t to vary by t and follow Chetty et al. (2020) in specifying X_i by fifth-order polynomials in test scores and parental income and ethnicity indicators.

We estimate sets of $\widehat{Inst}_u$ in two periods: (1) mid-century (1963) value-added estimates from Project Talent, where we observe age-29 wages by final undergraduate enrollment institution for 1963 college-goers, and (2) late 20th century (1996) value-added estimates provided in Appendix I of Bleemer (2022), which are estimated using age 31–35 average wages by first undergraduate enrollment institution for 1995–1997 college-goers who applied to at least one University of

[40] Replicating this decomposition in the 2010 College Scorecard similarly shows economics and finance to be the two largest components. It also suggests that California represents a leading indicator in the magnitude of the computer science component, as that major had yet to experience its sharp recent national growth and was still more likely to be earned by lower-income students in 2010.

California campus.[41] Because the latter value-added statistics cover only 12 percent of contemporary US enrollments, we improve their national representativeness using inverse propensity score weighting by institutional characteristics.[42]

Figure 9(a) visualizes the value-added of US two- and four-year institutions in the mid- and late 20th century. There is a 0.4 correlation for the 45 institutions with observed value-added in both periods, suggesting that institutions generate labor market value differently in these two periods. For instance, Ivy Plus institutions stand out far less in mid-century value-added estimates than they do decades later (Chetty et al., 2023). Figure A-16 shows that US higher education's average enrollment-weighted value-added declined in the mid-20th century, as aggregate enrollment growth was largely absorbed by lower-value institutions (Bleemer and Quincy, 2025), but has stabilized in recent decades and rose in the 2010s by both measures.

Table 2 characterizes high-value institutions by institutional characteristics observed in both 1962 (Blue Book) and 2021 (IPEDS). There is little overlap in the patterns observed in each time period. Private institutions' higher endowments predict higher value-added in the past, but two other measures which predict recent institutional returns – instructional expenditures and average standardized test scores – do not.[43] There is suggestive evidence that two-year institutions have experienced a decline in value-added as test scores and other institutional characteristics have become more predictive. Possibly as a result of all these changes, the relationship between parental income and value-added has also notably strengthened over time. A ten-rank increase in the average parental income of enrollees was associated with a 0.2 percent decline in wage value-added in the 1960s and a 2.1 percent increase in the 1990s.

Panels (b) and (c) of Figure 9 further explore this contrast by plotting the distribution of institutional value-added by parental income tercile in both 1963 and 1996. There was little institutional stratification by income in the 1960s, but by the end of the 20th century the value-added distribution

[41] The full set of 1963 value-added estimates are available in Appendix D. Institutions with fewer than 20 (50) assigned enrollees are omitted from the 1963 (1996) value-added estimates due to privacy restrictions, and in the former case are assigned average value-added by state and level (2- or 4-year). While our 1996 value-added estimates are only observed for California workers, they are nevertheless available for many non-California institutions.

[42] In particular, the late 20th century value-added statistics are propensity-weighted to 2015 (enrollment-weighted) institutions by interactions between control (public/private) and two/four-year status and 2021 enrollment, 2021 institutional expenditures per student, and average 2000 parental incomes of students.

[43] Figure A-17 shows that highest-collegiate-value states were in the South and Midwest in the mid-20th century, which differs dramatically from contemporary university rankings.

of universities in the top income tercile had shifted substantially upwards.[44]

There is no scholarly consensus on the causal interpretability of $\widehat{Inst}_u$ in Equation 9. Quasi-experimental studies have identified contemporary forecast coefficients in the 0.7-0.8 range for Ivy institutions (Chetty et al., 2023) and the 2–3 range for students on California public university admission margins (Bleemer, 2022, 2021).[45] We provide the first evidence on the causal interpretability of mid-century value-added statistics using the same forecast strategy as in the case of majors above, estimating versions of:

$$\widehat{Inst}_u^C = \alpha^f + \beta^f \widehat{Inst}_u + \epsilon_i^f \tag{10}$$

Table 3 shows that additional test score measures, high school grades, and indices of high school extracurricular and leadership activities absorb little of the cross-institution wage variation captured in $\widehat{Inst}_u$. However, the combination of these covariates with high school fixed effects (which also absorb geospatial wage variation) results in a β^f of 0.82. This suggests that at least 18 percent of the variation in $\widehat{Inst}_u$ reflects selection bias, mirroring Chetty et al. (2020)'s estimate of $\beta^f = 0.8$ for a set of contemporary institutional value-added estimates. Moreover, the relatively small samples used to estimate $\widehat{Inst}_u$ – ranging from 20 to 500 students per institution – result in a split-sample correlation of only 0.55, with about 30 percent of the remaining variable absorbed by selection bias on observable characteristics.[46]

We conclude that $\widehat{Inst}_u$ largely reflect treatment effect differences across institutions, maintaining Chetty et al. (2020)'s assumption of $\beta^f = 0.8$ for contemporary value-added statistics and conservatively assuming $\beta^f = 0.7$ for our noisier mid-century value-added statistics in the decomposition in Section 9.

[44]Figure A-18 shows ever starker increases value-added stratification by test score tercile. The schools which enroll high-testing students today also had high value-added in the past, but contemporaneous measures of test scores do not predict higher institutional returns in 1960, highlighting the rise of test-based meritocratic admission regimes.

[45]An alternative selection-on-observables design following Dale and Krueger (2002) provides forecast coefficients for $\widehat{Inst}_u$ of 0.5–0.8 in California but 0 in public Texas universities (Mountjoy and Hickman, 2021); see Appendix E.

[46]Note that 0.55 is a lower bound on the consistency of our full-sample baseline value-added statistics $\widehat{Inst}_u$, since those are estimated using twice the observations of either of the split-sample components.

8.2 Institutional Composition

Student enrollments shift across institutions even as those institutions' value evolves over time. We directly observe enrollment institutions by parental income tercile in the CPS OCG and in Project Talent. In order to measure changes in institution composition by parental income in more recent years, we proxy lower-income enrollment by the share of students at each institution who receive federal grant aid through the Pell grant program. Pell grants are received by students from roughly the bottom third of the parental income distribution, permitting annual 1984–2022 estimation of enrollment by parental income tercile using enrollment-weighted institution-level administrative data available from IPEDS.[47] We adjust for changes over time in Pell eligibility by family income using the 1987–2020 survey waves of the NPSAS, which provides the family income distributions of Pell and non-Pell college students.[48]

Figure 10 combines these enrollment records with the previous section's mid- and late-20th-century value-added statistics to present the difference between the average wage returns of institutions where higher- and lower-income students enroll.[49] Higher- and lower-income students enrolled at similar-value universities in both the early- and mid-20th century and in recent years when measured using 1960s value-added, suggesting that there is little scope for institutional composition to explain rising regressivity in this period.[50] But higher-income students have long enrolled at four-year, private, and (in recent years) higher-expenditure institutions where value added has increased in the late 20th century, and their enrollment at these schools has somewhat increased in recent years.[51] Bottom-tercile students now enroll at institutions with about 0.06 log points lower wage value-added than their top-tercile peers.[52]

[47] Pell recipiency is unavailable by gender; we assume Pell gender shares correspond to the institution's gender share.

[48] For example, the median recipient (non-recipient) in 1993 came from a family earning about \$18,500 (\$50,000), the 32nd (70th) percentile in that year. As a result, we assume that the top/bottom tercile gap is equal to the Pell/non-Pell gap multiplied by $\frac{0.833-0.167}{0.7-0.32}$, where the numerator is the approximate median income rank difference of top- and bottom-tercile students. Figure AA-2 plots Pell and non-Pell recipients' median family income rank over time.

[49] Late-century value-added statistics are only available for a subset of schools comprising about 12 percent of contemporary enrollment. Figure A-19 shows that when universities are valued by average wages – which are available for nearly all institutions (Chetty et al., 2020) – enrollment gaps between income terciles look somewhat more regressive when using all institutions. This is also true among the subset of institutions with observed late-century value-added, but the gap is larger because raw wage differences exceed value-added differences by as 200–300 percent.

[50] The dramatic expansion of community college in the 1960s generated a large but short-lived gap in mid-century value-added observed in the NLS72 among both two- and four-year institutions; see Figures A-20 and A-16.

[51] Baker et al. (2018) show that Black and Hispanic college enrollees shifted toward relatively less selective institutions in the 1990s and 2000s.

[52] Our wage value-added statistics predate the rise in for-profit university enrollment (Deming et al., 2012), which have

Figure 11 visualizes the rise in institutional stratification – $\sum_u v_t^u(u)\Delta_q P_t(u|q)$ from Equation 5 – by splitting them into four components, focusing especially on changes over time in the share of top- and bottom tercile students who first enroll at four-year $(4Y)$ or two-year $(2Y)$ institutions:

1. "4-Year Composition", $\sum_{u\in 4Y} v_{63}^u(u)\Delta_q\Big(P_t(u|q,4Y)\times P_{84}(4Y|q)\Big)$, the four-year university components holding mid-century value-added and four-year enrollment share fixed;

2. "4-Year Returns", $\sum_{u\in 4Y}\Big(v_t^u(u)-v_{63}^u(u)\Big)\Delta_q\Big(P_t(u|q,4Y)\times P_{84}(4Y|q)\Big)$, the contribution of the change in four-year universities' value-added from the mid- to late-20th century;[53]

3. "2-Year", $\sum_{u\in 2Y} v_t^u(u)\Delta_q\Big(P_t(u|q,2Y)\times P_{84}(2Y|q)\Big)$, the two-year composition and return components, holding only the four-year enrollment share fixed; and

4. "2/4-Year Transitions", the residual terms, measuring the effect of changes in relative enrollments in two- and four-year institutions since the IPEDS data began in 1984.[54]

The figure shows that institutions have primarily contributed to rising regressivity since 1980 by two means: growing relative four-year enrollment among higher-income students and increasing relative value-added of the institutions where higher-income students enroll. Changes in the composition of two- and four-year universities played a substantial but temporary role in rising regressivity during the community college expansion of the 1970s but have played smaller and offsetting roles in recent decades. We conclude that the growing relative value of higher-cost and high average test score institutions decreased the relative value of college enrollment for lower-income students in the late 20th century, while higher-income students' decreasing community college enrollment has steadily contributed to rising regressivity since the 1980s.

9 Decomposing Rising Regressivity in US Higher Education

Figure 12 visualizes the combined contributions of pre-college academic selection, college major, and enrollment institution to the rise in collegiate regressivity over the past century by presenting

likely further decreased the value of lower-income students' institutions *beyond* these measured declines.

[53] We define $v_t^u(u)$ as v_{63}^u until 1963, a linear weighted average with $v_{96}^u(u)$ until 1996, and is $v_{96}^u(u)$ thereafter.

[54] Figure A-20 shows that two-year enrollment was less common across the SES distribution before the 1960s, so we use 1984 as the base year and combine components (3) and (4) into a single "2-Year" component prior to that year.

direct measurements of D_t and each of the six components named in Equation 6. The solid line reflects that $D_t \approx (\frac{5}{6} - \frac{1}{6})(t - 1950) \times \delta$ when $t \geq 1950$, the best-fit line from Figure 3 (estimated in log dollars) multiplied by the approximate difference in parental income ranks between the top and bottom terciles. Between 1920 and 1950, Figure 3 suggests that $D_t \approx 0$, but it has grown to be about 0.15 log dollars in recent years.[55] The gray diamonds are the dataset-specific estimates of D_t. We also summarize the contributions of each component by period in Table 4.

The first two components measure the magnitude of selection in explaining the rise in collegiate regressivity. The first term, Test Composition, holds the estimated wage return to standardized test rank differences fixed at its earliest estimate – 1957, in Wisconsin – and then plots the product between that value and the gap between top- and bottom-tercile college-goers' test scores as estimated in each of the datasets shown in Figure 5, linearly smoothing between estimates.[56] The resulting component, plotted in orange in Figure 12, explains part of the initial 1960s regressivity but has played essentially no role since that time in perpetuating the rise

The second term, Test Returns, measures the difference between the Test Composition component and what that component would have been if the labor market value of higher test scores were also allowed to vary over time. Using the test score valuations presented in Figure 5(b), Figure 12 shows that the rising value of higher test scores explains much of the differential growth of the observational return to college for top-tercile college students from the mid-1960s until 1980, when the observed return to pre-college human capital peaked.[57] The contribution of test returns has shrunk since that time, however. Table 4 shows that differential selection into college-going explains almost 10 percent of the rise in regressivity since the 1960s.

Next we turn to college major's contributions, shown in blue. The Discipline Composition component fixes the return to each of ten disciplines in 1932 (using 1947 Time data) and measures the degree to which changes in disciplinary composition by parental income have contributed to rising regressivity. As shown in Figure 7, most of the growth in regressivity in the 1950s is explained by disciplinary stratification, but its contribution then receded until recent years.

[55]Rather remarkably, Figures 5b, 7, and 10 each show that $\Delta_q \left[v_0^x(x) P_0(x|q) \right] \approx 0$ for $x \in (a, u, m)$, respectively, so all six terms are null in the start year.

[56]This component ignores differences in test content between tests; any such differences that contribute to collegiate regressivity appear in the Test Returns component.

[57]These trends could reflect both the rising value of pre-college human capital in the US labor market and improvements in test scores' capabilities of measuring that value.

The Discipline Returns component measures the importance of changes in the relative returns to the disciplines in which lower- and higher-income students earned degrees. Figure 6 shows that the spread of relative returns has slightly widened since 1960, which contributes a small portion to rising regressivity due to higher-income students' greater representation in high-return majors.

The third major component, Major Composition, measures the further degree to which within-discipline shifts between detailed majors contribute to the rise in higher education stratification since the earliest measurement of detailed majors in the late 1950s. As suggested by Figure 7, within-discipline major stratification has played an important role in rising regressivity, explaining all remaining growth in the early 1960s and an additional 2–3 percentage points in recent years, with a period of milder stratification in the 1980s. Overall, the composition and returns of college majors explain about one-third of the aggregate rise in regressivity over the past century.

Finally, we turn to the contributions of institutions, shown in green. The Institutional Composition component measures the degree to which variation over time in the enrollment institutions of lower- and higher-income students contributes to the growth in collegiate regressivity, fixing institutional value-added in 1963. Given the relatively weak correlation between mid-century value-added and contemporary measures of selectivity, it may be unsurprising that SES-based composition alone explains only a small share of rising regressivity since the 1980s, though (as discussed above) it plays an important if temporary role in explaining stratification during the community college boom of the 1970s.

The Institutional Returns component linearly re-values universities to their 1996 value-added between 1963 and 1996 and employs the latter valuation thereafter, revealing little contribution to increasing regressivity until about 1980. Since then, this channel has played a substantial role in higher education's rising regressivity, outstripping even the rise in major stratification. Over 5 percentage points of the rise in collegiate regressivity in Figure 12 can be explained by compositional changes across institutions when those institutions are valued using recent value-added estimates,

In sum, the relative decline in American higher education's value for lower-income Americans can largely be explained by changes in the relative value of two- and four-year institutions and the composition of college majors, with differential selection into college playing a secondary role.

10 College-Going Among Women

Measuring changes in the labor market value of college for female students is more complex than for men due to the large SES-dependent changes in female labor force participation since the 1940s (e.g. Goldin, 2006). However, Figure 13 shows that college-going appears to have become regressive for women as female enrollment patterns converged to those of men.

As with men, Figure 13(a) shows there were relatively small income-based differences in female college enrollment in the early 20th century which widened as the aggregate share of women attending college nearly doubled between 1950 and 1960 (Goldin et al., 2006). The income gap in female college-going still persists, echoing male enrollment patterns.[58]

Figure 13 shows little evidence of meaningful contributions from selection, major, and institutions to collegiate regressivity for women until the latter two factors turned regressive in the 1990s.[59] Figure 13(b) suggests that test score-based selection does not appear to have changed by parental status over the last half of the 20th century. Lower-income women earned *higher*-premium majors than their higher-income peers for most of the 20th century, though the past 20 years' patterns mirror men's trend toward regressivity.[60] Similarly, Figure 13(d) provides evidence of relatively declining institutional value-added among lower-income female college students in recent years.[61] Together, these results indicate that college has likely become more regressive for both male and female students in the past 100 years, perhaps beginning more recently for women.

11 Discussion

Colleges and universities in the US have provided over a century of high average wage premiums to college-goers, but those gains are no longer equally shared by enrollees from higher- and lower-income backgrounds. Lower-income students have become less likely to enroll in the four-year university sector than their peers, experienced declines in the relative value of the public four-

[58] College-going has risen more among women across income terciles than men over time (see Goldin et al., 2006; Bailey and Dynarski, 2011); Figure A-21 shows the same trend in college attainment.

[59] Several of our main data sources either do not include women (e.g. CPS OCG, WWII draft records) or are more limited in their usage for measuring outcomes for women (linked Censuses).

[60] Figure CC-2 uses UC Berkeley undergraduate degrees by gender to show that women's major declaration has trended toward that of men, though women still earn far lower-paying majors on average (Sloane et al., 2021).

[61] As above, we assume Pell gender shares correspond to the institution's gender share due to data availability.

year universities where they have long enrolled, and become less likely to earn computer science and other high-value college majors than their higher-income peers. These forces – moreso than changes in high-quality university access and net tuition costs – now lead students from the bottom parental income tercile to earn half the enrollment wage premium received by top-tercile students.

The declining relative value of college-going for lower-income students since 1960 has significantly disrupted those students' upward mobility. We simulate the effect of reversing collegiate regressivity by continuously adjusting NLSY97 respondents' early-30s rank wages to equalize their rank return to college-going. Under the assumption that 10 percent of the rise in regressivity is the result of differential selection (see Table 4), we find that equalizing the college-going wage premium across the parental status distribution would causally lower the prevailing intergenerational rank-rank correlation of 0.265 to 0.203. The arrival and growth of collegiate regressivity after 1960 can thus explain 25 percent of the current intergenerational transmission of income, whereas it explained nothing prior to 1960.[62] For comparison, a more aggressive policy that equalizes collegiate *attainment* by parental income – such that all students are equally likely to enroll in and attain college degrees, and assuming that 80 percent of the return to college is causal – would reduce the rank-rank correlation to 0.189, though such a policy would require increasing university capacity rather than largely reallocating and adjusting existing programs.[63]

Inequitable access to high-value college majors and institutions is not a permanent feature of American higher education. Our long-run approach illustrates that education provided high- and low-income students similar labor market value for decades before 1960. Given the wide range of policy changes affecting both college supply and demand since then – like the expansion and subsequent contraction of the two-year college sector, phase-in and phase-out of race-based affirmative action; the phase-in of grade-based restrictions on lucrative college major access; the growth of for-profit institutions and federally-subsidized financial aid; and rising variation across four-year universities in per-student expenditures – there is significant scope for future work to disentangle the importance of each policy to the various channels that have generated regressivity in American higher education.

[62] Chetty et al. (2020) estimate a somewhat larger contribution of university regressivity among similar cohorts in administrative tax data, suggesting that closing the value-added gap for male and female students' modal enrollment institutions alone would decrease intergenerational income transmission by 25 percent.

[63] Bloome et al. (2018) discuss similar simulations equalizing educational attainment by parental income.

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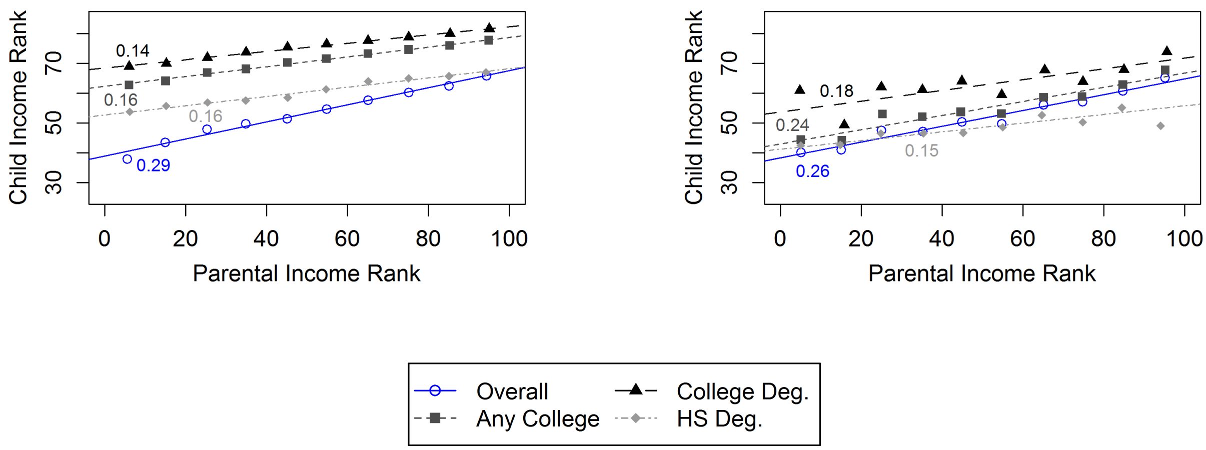
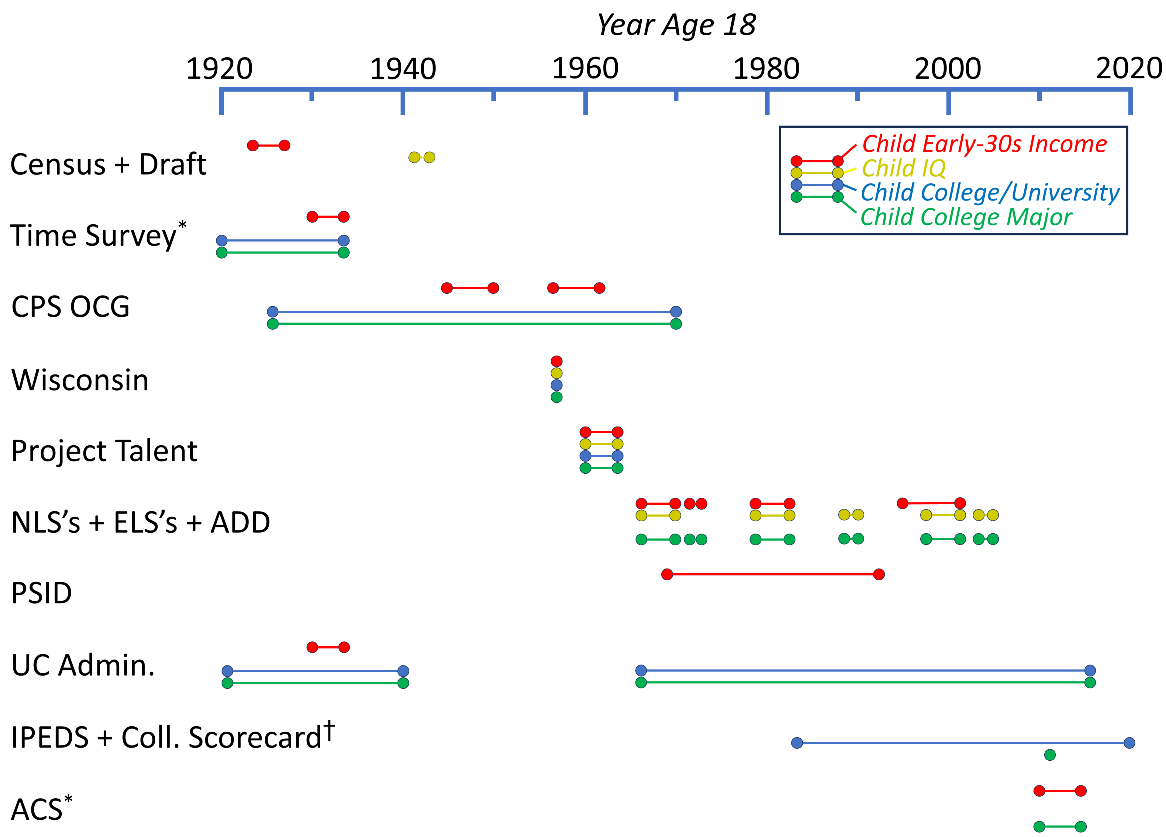
Figure 1: Rank-Rank Income Correlation for Age 31–35 Children

(a) 1923–1928 (b) 1998–2002



Note: Binned scatterplots and slopes of income rank among employed age 31–35 men by father's predicted income rank at age 11–15 (a) or parental income rank at age 14–17 (b) overall, for college graduates, for people who had completed at least one year of college, and for high school graduates who had not completed any years of college. Panel (a) is measured among father-son pairs matched in the 1920 and 1940 Censuses following Abramitzky et al. (2012) with predicted LIDO father's incomes from Saavedra and Twinam (2020); Panel (b) is measured among youths in the NLSY97 using survey weights. Headers refer to age-18 years. Child incomes below the contemporaneous half-time federal minimum wage are omitted. Source: US Census and NLSY97.

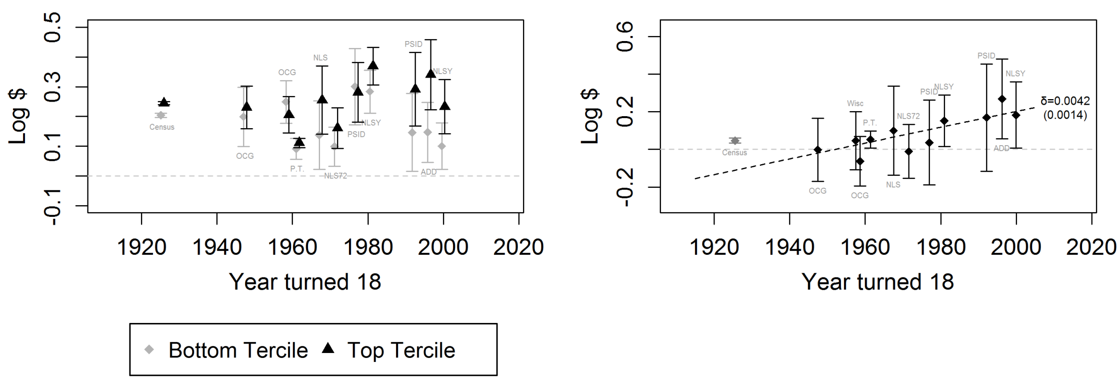
Figure 2: Summary of Data Sources



Note: Available cohorts of longitudinal, retrospective, or cross-sectional datasets used to measure changes in the relative value of college-going, test performance, institutions, and majors in the US labor market since 1920. Except where noted, each data source is nationally representative. Parental income measured between ages 10 and 15 (or earlier if necessary) is available for all datasets except those with asterisks (*). 'Child Income' refers to measuring the individual's income between 30 and 35; 'Child IQ' refers to observing a standardized test score like the AFQT or ASVAB; 'Child College/University' refers to observing the individual's first postsecondary institution; and 'Child College Major' refers to observing the individual's college major. 'Census + Draft' refers to the linked 1920–1940 Census (following Abramitzky et al., 2012) and the 1940 Census linked to 1943 AFQT on draft cards (denoted in a dotted line since AFQT is measured post-college); 'Time Magazine' to Time's 1947 College Graduate Survey; 'CPS OCG' to the retrospective Occupational Changes in a Generation CPS supplements; 'Wisconsin' to the Wisconsin Longitudinal Study (restricted to Wisconsin high school graduates); 'Project Talent' to the AIR study by that name; 'NLS + NLSY + ELS + ADD' and 'PSID' to the seven federal longitudinal studies by those acronyms; 'UC Admin' to administrative University of California records (restricted to enrollees of those institutions, and replacing parental income with average local residential income); 'IPEDS + VA' to institution-level university enrollment characteristics from federal IPEDS data matched to value-added statistics from Bleemer (2022); and 'ACS' refers to cross-sectional wage and major data from the American Community Survey.

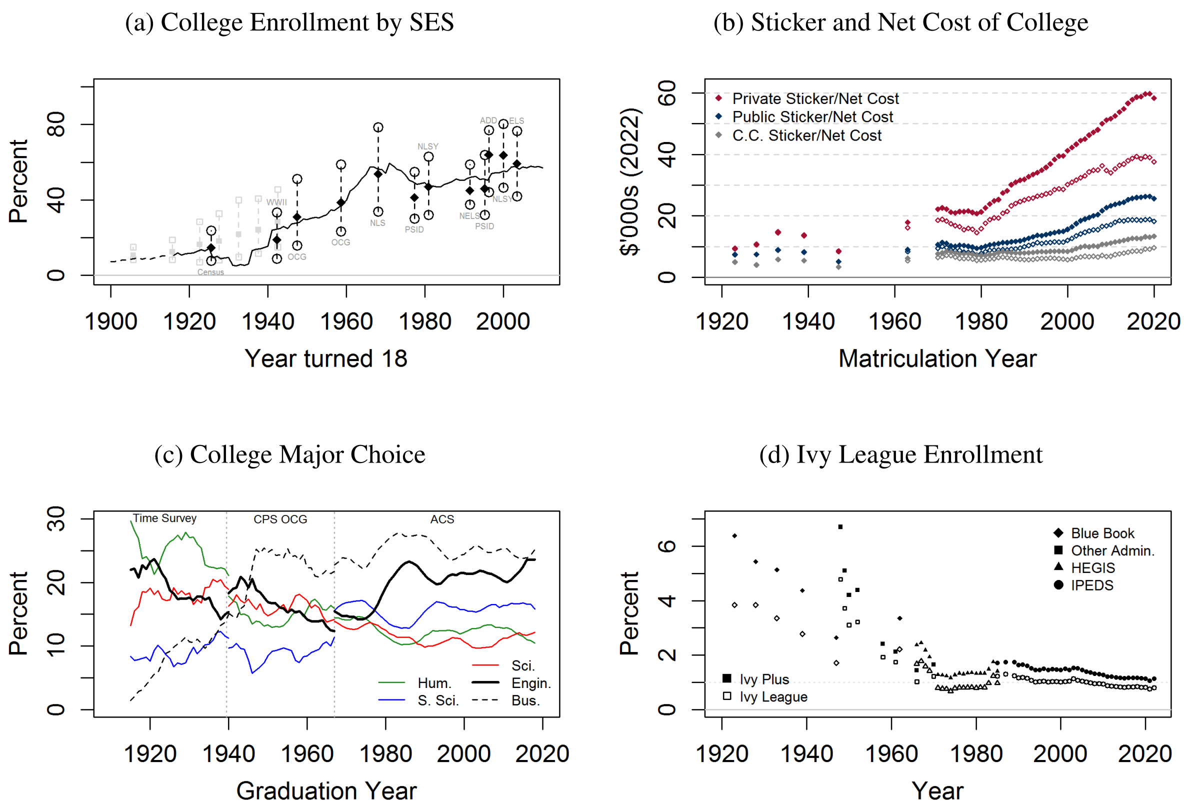
Figure 3: Regressivity of US Higher Education Over Time

(a) Average Return to Enrollment by Tercile (b) Increased Return per Parent Income Rank (δ)



Note: **Panel (a)**: The estimated observational annual wage return to at least one year of college enrollment at age 31-35 among high school graduates by survey dataset and contemporaneous parental income tercile (displaying only the top and bottom terciles), measured in CPI-adjusted 2022 log dollars and conditional on dataset-cohort-tercile fixed effects. **Panel (b)**: Estimated regressivity of male college enrollment over time in the United States, where the trend line is the estimated δ and standard error from Equation 2, parameterizing $Coll_{it}$ as indicating at least one year of college. Dataset-specific estimates and 95-percent confidence intervals are from a version of Equation 2 estimated with separate δ_t terms for each dataset; the linear slope (and standard error) is from a version with δ_t permitted only a linear trend over time, excluding Census respondents, and can be interpreted as the annual increased relative log wage value of college-going per 100 family income wage ranks. Child incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. See Appendix A for details on data construction. Source: US Census, CPS OCG, Wisconsin, Project Talent, NLSM, NLS72, PSID, NLSY79, ADD Health, and NLSY97.

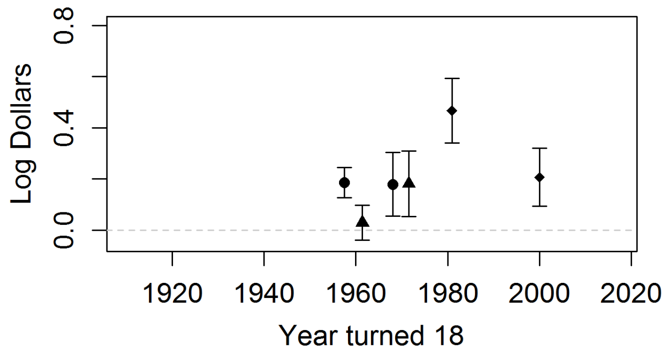
Figure 4: Trends in US Higher Education



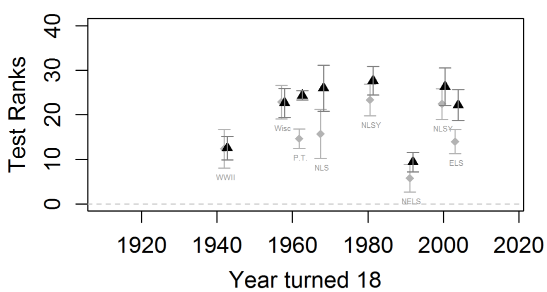
Note: **Panel (a)**: Points in black show the share of men between ages 30 and 35 who had completed at least one year of college overall (black diamond) or among those from the bottom or top tercile of parental incomes when age 14–17 (circles). The solid line reports the same overall average educational outcome for 1940–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, and 2021 American Community Survey respondents between the ages of 28 and 42. Points in gray show the same for older men when other data are unavailable: linked 1900–1940 Census respondents (age 50–55), 1910–1940 Census respondents (age 40–45), and 1962 CPS OCG respondents for every 5-year age range from 35–40 to 55–60. **Panel (b)**: Enrollment-weighted average sticker and net price of attendance – including tuition, fees, room, and board – at public and private four-year institutions and public two-year institutions (now community colleges, or C.C.) for full-time undergraduates. Net price is equal to sticker price minus governmental, institutional, and (after 1960) private grant aid for students who only enrolled at that institution. Sticker and net prices omit non-resident fees and military or educational tax benefits; room and board refer to on-campus accommodations and are omitted for universities without residential facilities prior to 1960. **Panel (c)**: The share of college degrees awarded to men in five disciplines – humanities, social sciences, natural sciences, engineering, and business – among respondents to the Time Survey of College Graduates, the 1973 CPS OCG, or the 2009, 2016, or 2021 ACS by year of degree attainment (smoothed over 5 years) or (in the ACS) birth year plus 23. The share earning (mostly professional) degrees is omitted. OCG respondents with graduate degrees report their graduate degree field. **Panel (d)**: The share of undergraduate degrees awarded by universities in the Ivy League (open points) or Ivy Plus (solid points) institutions (which adds Chicago, Duke, MIT, and Stanford), as reported in various sources. See Appendix A for details on data construction and sources for (a) and (c); Appendix A.17 for data construction and sources for (b); and Appendix A.18 for data construction and sources for (d).

Figure 5: Pre-College Human Capital Trends

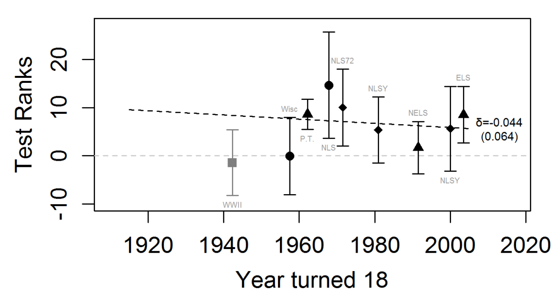
(a) Return to Pre-College Human Capital



(b) Average HS-College IQ Gap by Tercile

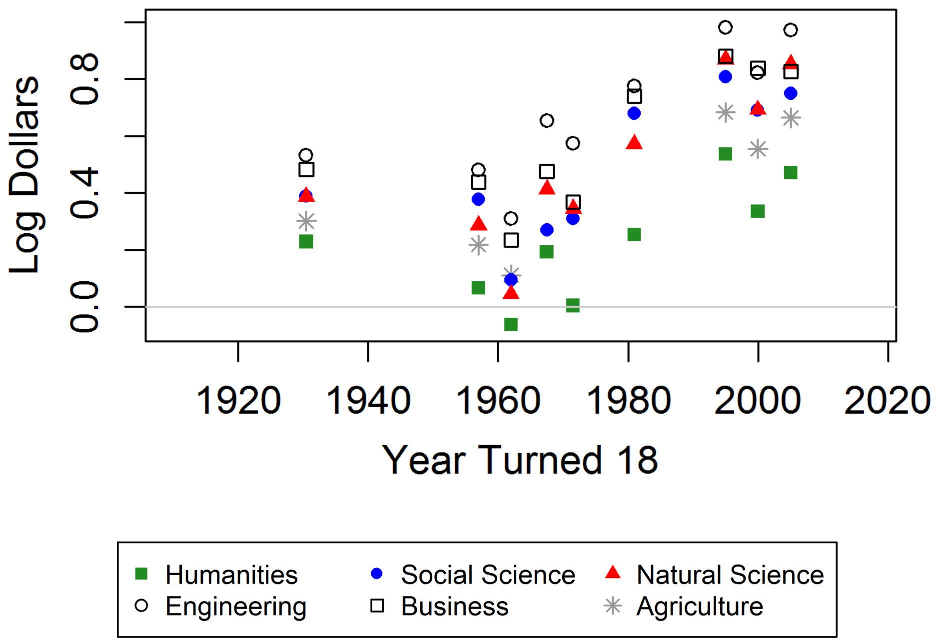


(c) Differential Selection into College Enrollment



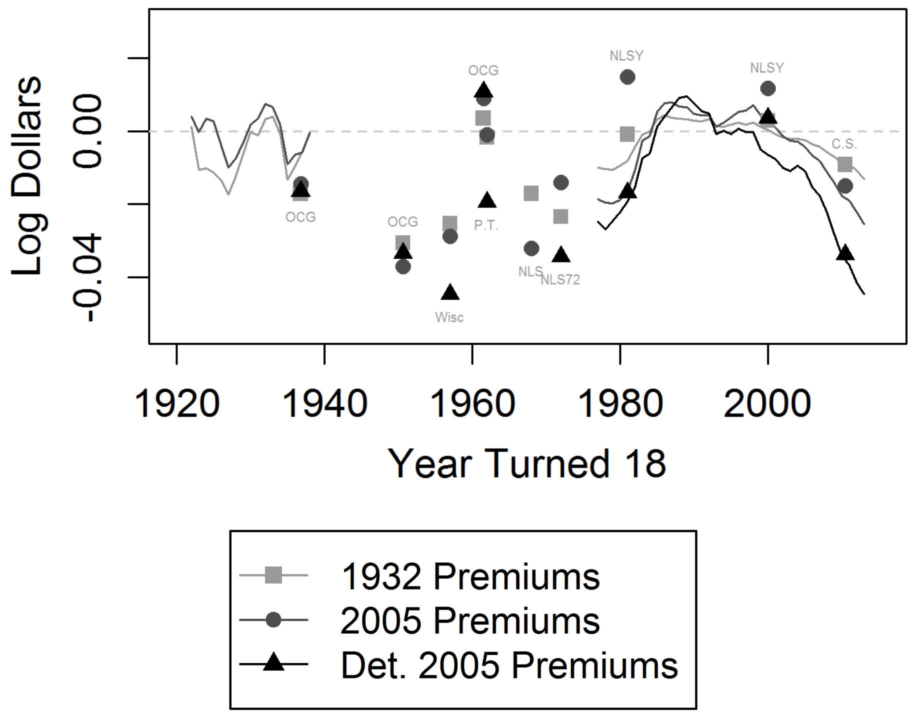
Note: **Panel (a)** plots the estimated log income return to male pre-college human capital scores over time in the United States, estimated separately in each dataset conditional on parental income and education level among high school graduates and shown with 95-percent confidence intervals. **Panel (b)** plots the estimated difference in within-cohort high school rank test score between students with at least one year of college enrollment at age 31-35 and high school graduates who do not go to college (γ), by survey dataset and contemporaneous parental income tercile (displaying only the top and bottom terciles) and conditional on dataset-cohort-tercile fixed effects. **Panel (c)** plots estimated differential selection into male college enrollment over time in the United States, with dataset-specific coefficients (δ) and 95-percent confidence intervals from a version of Equation 2 estimated with separate β's in each dataset and replacing $Wage_{it}$ with measures of pre-college cognitive skills. The linear slope (and standard error) is from a version of Equation 2 with δ_t permitted only a linear trend over time, excluding Census respondents, and can be interpreted as the annual increase in average test score rank of college-going per 100 family income wage ranks. All regressions are weighted using standardized survey weights; standard errors are robust. See Appendix A for details on variable definition and data construction. Source: US Census, WWII draft cards, Wisconsin, Project Talent, NLSM, NLSY79, and NLSY97.

Figure 6: Observational Returns by College Major Over Time



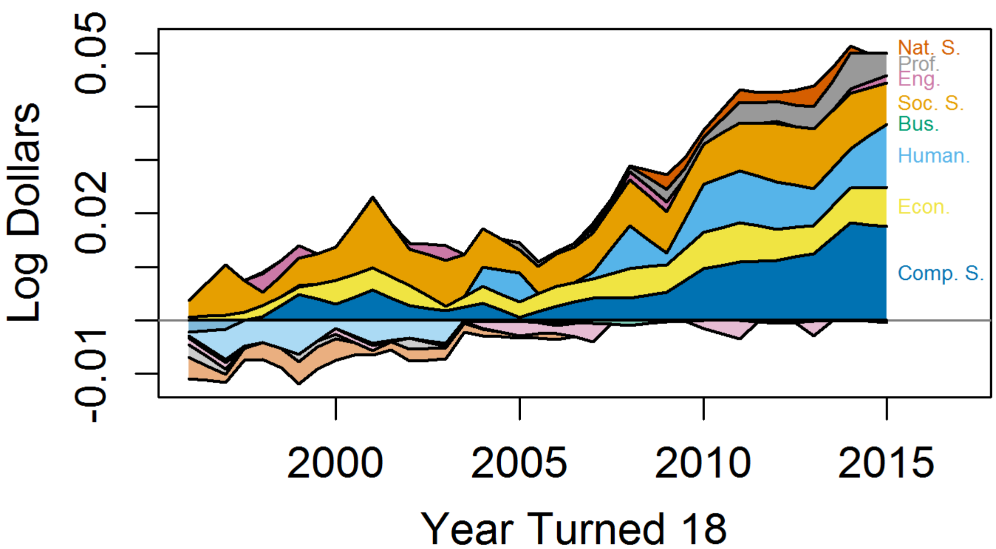
Note: Average log wage return to college discipline relative to only having a high school degree, measured at age 30–35 (or 30–39 in the 1930s) in all available survey datasets that contain major discipline and individual wages. Estimated by OLS regression of log wages on discipline indicators among male workers with positive earnings who either report a college major or report having never gone to college, with covariates for birth year, gender, and an indicator for enrolling at college but never earning a four-year degree. Estimates for Time survey (in which all respondents are college graduates) are linearly scaled so that their weighted average equals the average observational return to college among similar-aged workers in the US Census. Estimates for other health and professional degrees are omitted. Source: Time survey, US Census, Project Talent, Wisconsin, NLS, NLSY79, NLSY97, and ACS.

Figure 7: Difference in Major Premiums Between Students from Bottom- and Top-Tercile Parental Incomes



Note: The difference in average major premiums declared between male University of California enrollees (lines) or nationally-representative male respondent graduates (symbols) from the bottom and top parental income tercile in that year, where major premiums are estimated for ten discipline categories – humanities, social sciences, natural sciences (the three of which are grouped into letters and sciences before 1945), agriculture, business, chemistry, engineering, pre-medicine, other health professions, and other professional degrees – or 66 'detailed' categories in either the 1947 Time Magazine Survey or the 2019–2021 American Community Survey (Figure 6). Annual parental income terciles were measured by Census-linked fathers' estimated income (LIDO) 2–11 years prior to their first year of enrollment (UC 1920–1940), by average income in students' residential Census tract (UC 1975–1995) or Zip code (UC 1996–2016), by reported parental income at ages 14–17 (non-UC surveys), or by Pell status (2015–2016 degree recipients in the College Scorecard; see Appendix A.13). University of California enrollees exclude those from UCLA, UCSD, and UCM. See Appendix A for details on data construction. Source: University registers, Saavedra and Twinam (2020), US Census, UC-CHP administrative student records, IRS SOI, Wisconsin, NLSM, NLSY79, NLSY97, College Scorecard, and the ACS (Ruggles et al., 2024).

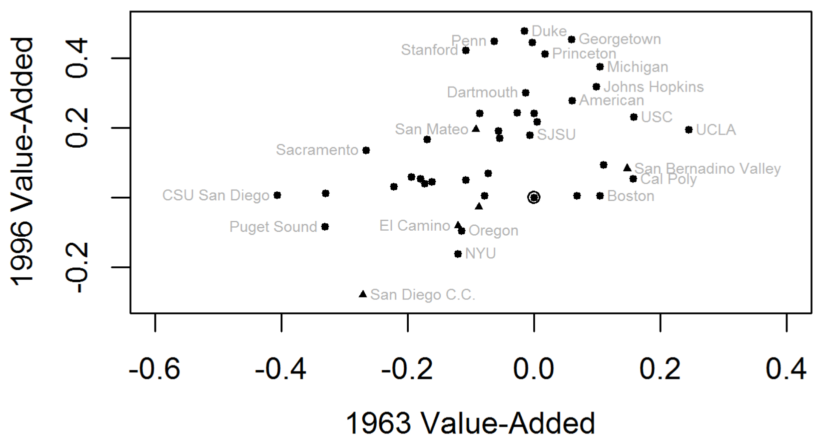
Figure 8: Decomposition of Recent Rising Major Stratification by Discipline



Note: The annual 1996–2015 contribution of each discipline to the rising gap in the average premium of majors declared by male University of California enrollees from the bottom and top parental income tercile, separating out the two most-contributing detailed majors: computer science (including computer engineering) and economics (including finance). We measure each of the 66 detailed majors' contributions by $\widehat{Major}_m \Delta_q \left[P_t(m|q)\right]$ from Equation 5, where $\widehat{Major}_m$ is measured in the 2019–2021 American Community Survey and demeaned, and aggregate by discipline (combining all professional disciplines). Annual parental income terciles were measured by average income in students' residential Zip code. University of California graduates exclude those from UCLA, UCSD, and UCM. See Appendix A for details on data construction. Source: UC-CHP administrative student records and the ACS (Ruggles et al., 2024).

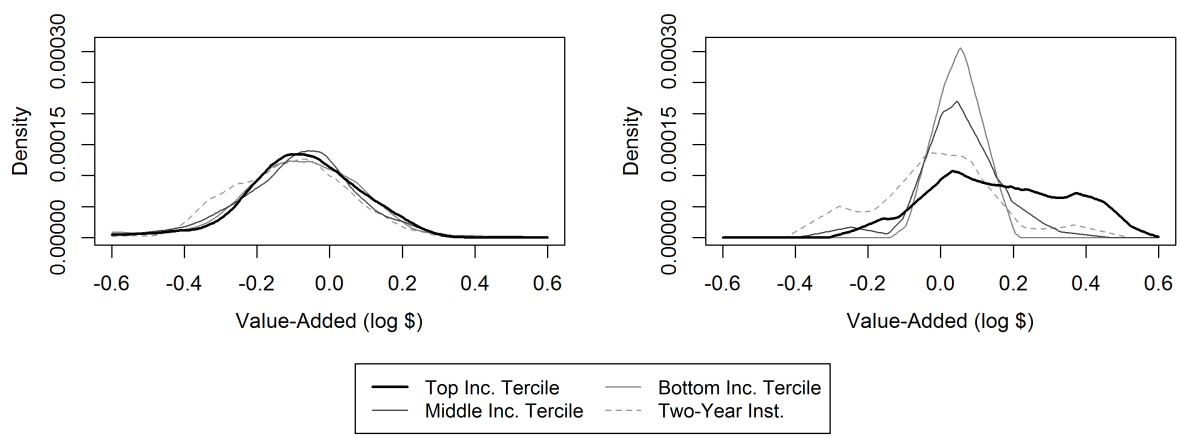
Figure 9: Institutional Value-Added in the 1960s and 1990s

(a) Comparison Between 1963 and 1996 Value-Added



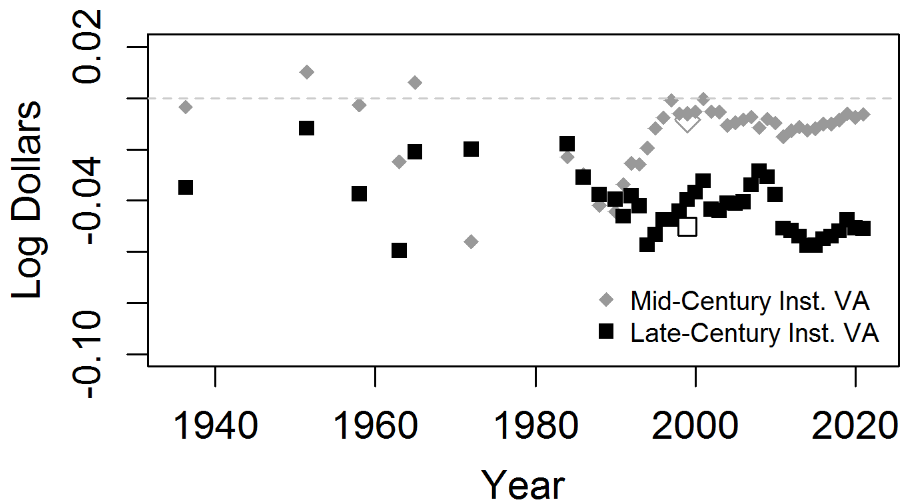
(b) Distribution of 1963 Value-Added

(c) Distribution of 1996 Value-Added



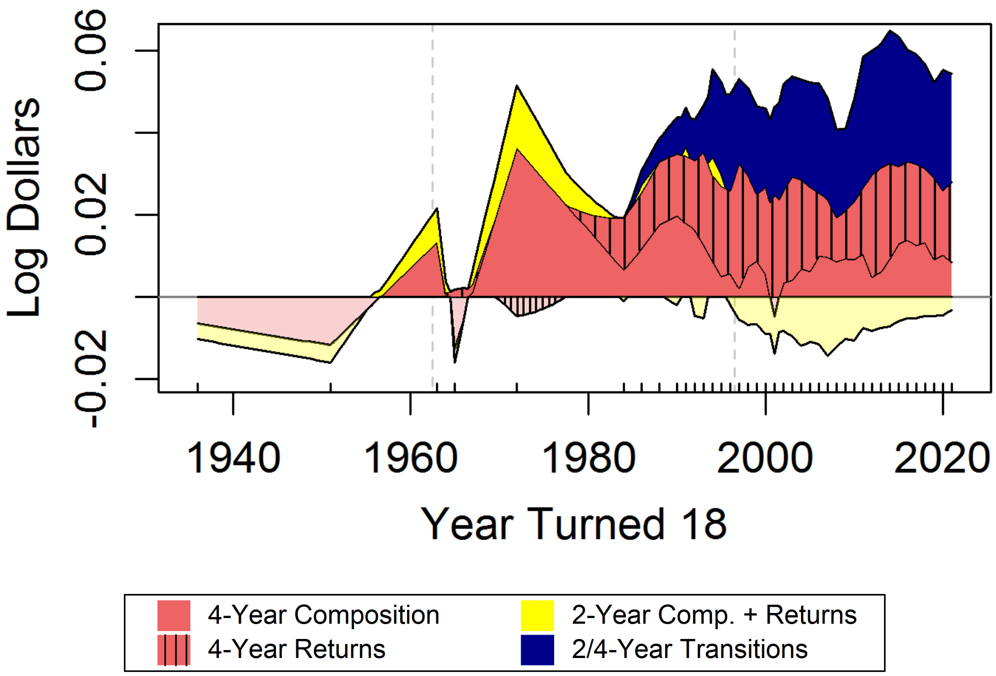
Note: The institutional value-added of US colleges and universities in log dollars estimated from 18-year-olds in 1963 ("mid-century", using Project Talent) and 1996 ("late-century", from University of California applicant records), estimated relative to CSU Long Beach (which is set to 0) and visualized as a scatterplot and as a kernel density plot by four- or two-year institution type and (for the former) tercile of contemporaneous average parental income. Value-added is estimated by OLS with fifth-order polynomials in test scores (measured academic aptitude or SAT), parental income rank, and race indicators as controls (following Chetty et al., 2020). Project Talent value-added estimates are restricted to men and include the 523 last-enrollment institutions with at least 20 employed male respondents, with wages measured at age 29; the 1996 value-added estimates include the 136 first-enrollment institutions where at least 50 1995–1997 University of California applicants enrolled who were employed in California between ages 31 and 35 (using average wages measured at those ages). In the density plots, the 1996 value-added estimates are propensity-weighted to 2015 (enrollment-weighted) institutions by interactions between control and two/four-year status and 2021 enrollment; 2021 instructional, research, and student service expenditures per student; and average 2000 parental incomes of students. The triangular kernel bandwidth is 0.15. See Appendix A for details on data construction. Source: Project Talent, IPEDS, Bleemer (2022), and Chetty et al. (2020).

Figure 10: Institutional Enrollment by Student Socioeconomic Status



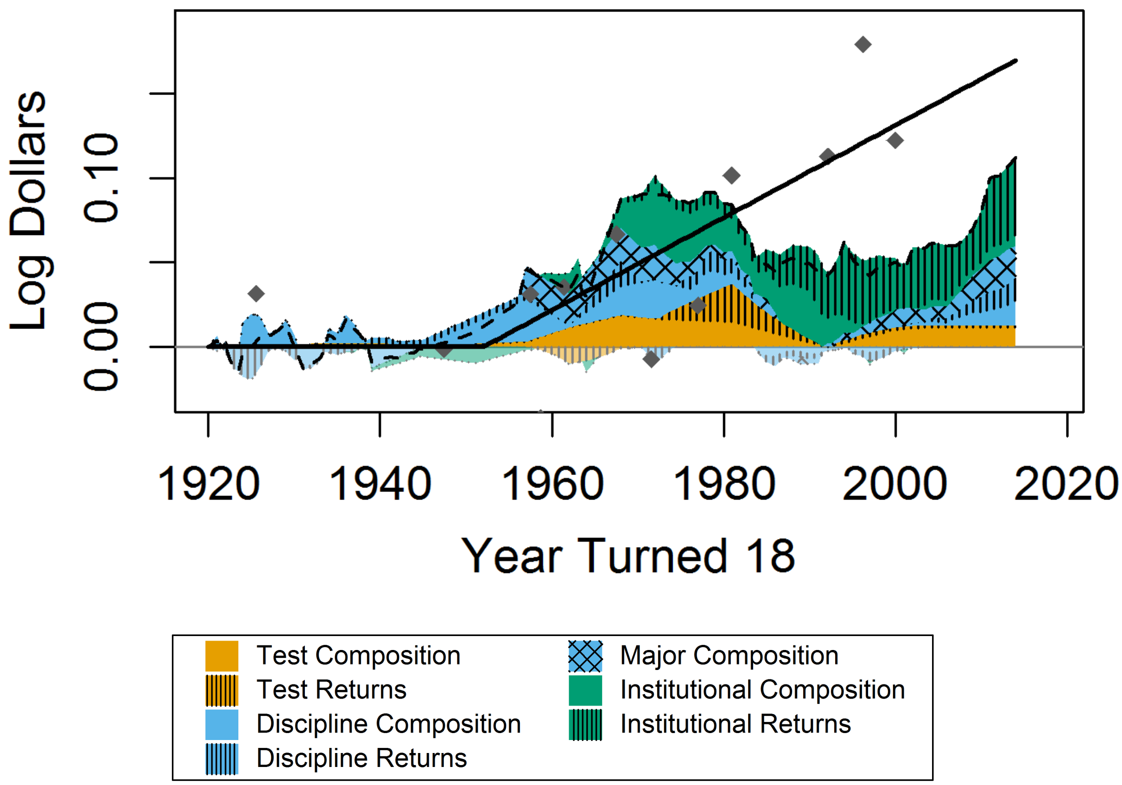
Note: The difference in average institutional value-added of the enrollment institutions between students from the bottom and top parental income tercile. The institutional value-added of US colleges and universities estimated from 18-year-olds in the mid-20th century (using Project Talent) and the late 20th century (from University of California applicant records) by OLS with fifth-order polynomials in test scores (measured academic aptitude or SAT), parental income rank, and race indicators as controls (following Chetty et al., 2020). Project Talent value-added estimates are restricted to men and include the 474 last-enrollment institutions or institution-groups with at least 20 employed male respondents, with wages measured at age 29; the 1996 value-added estimates include the 136 first-enrollment institutions where at least 50 1995–1997 University of California applicants enrolled who were employed in California between ages 31 and 35 (using average wages measured at those ages). Enrollments are measured in the CPS OCG (split into birth cohort terciles), Project Talent, and in more recent years by institution-level Pell and non-Pell degree recipients (IPEDS) adjusted for changes over time in the average family income rank of Pell (and non-Pell) recipients; see Appendix A.13 for details. Late 20th century Pell and non-Pell enrollments are reweighted to match total enrollments by degree level, sector, and year due to missing value-added statistics. The large open square and triangle validate the Pell approximation by replacing Pell with enrollment measurements of 1980–1982-birth-cohort students from the top two or bottom two income quintiles as reported by Chetty et al. (2020), adjusting for the comparison between top and bottom terciles. Source: CPS OCG, WLS, Project Talent, IPEDS, NPSAS, Bleemer (2022) Appendix I (late-century value-added estimates), and Chetty et al. (2020).

Figure 11: Decomposition of Rising Institutional Stratification



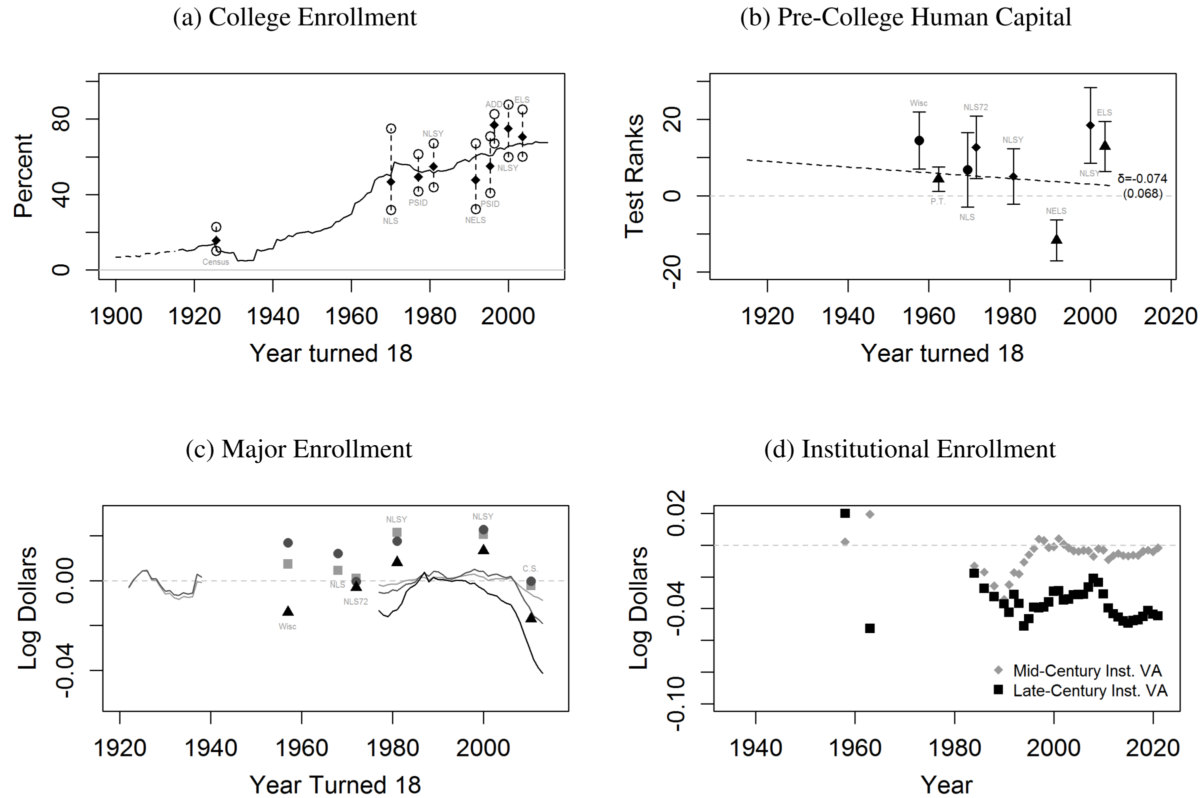
Note: This figure shows the annual 1935–2021 contribution of each of four components to the rise in institutional regressivity: that is, the difference in average institutional value-added faced by college-goers from top- and bottom-tercile parental incomes, $\sum_u \widehat{Inst}_{tu} \Delta_q \left[P_t(u|q)\right]$, as shown in Figure 10. The ticks at the bottom represent years in which data are available: three terciles of 1973 OCG by birth cohort, Project Talent, and annual IPEDS data. We measure $\widehat{Inst}_{tu}$ by mid-century value-added prior to 1963, late-century value-added after 1996, and linearly interpolate in between (marked by gray dotted lines). First, we split the summation into two – one for four-year universities, the other for community colleges – and starting in 1984 (because two-year enrollment is small in prior years) we hold relative overall enrollments in each segment fixed, with the residual (capturing changes between two- and four-year enrollment by lower- and higher-income students) reflected in **2/4-Year Transitions**. Among four-year universities, we hold mid-century value-added fixed for **4-Year Composition** and capture residual four-year variation resulting from changes toward late-century value-added in **4-Year Returns**. Finally, **2-Year Comp. + Returns** measures the full community college component (reflecting both composition and returns), along with the 2/4-Year transition components before 1984. The institutional value-added of US colleges and universities estimated from 18-year-olds in the mid-20th century (using Project Talent) and the late 20th century (from University of California applicant records) by OLS with fifth-order polynomials in test scores (measured academic aptitude or SAT), parental income rank, and race indicators as controls (following Chetty et al., 2020). Project Talent value-added estimates are restricted to men and include the 474 last-enrollment institutions or institution-groups with at least 20 employed male respondents, with wages measured at age 29; the 1996 value-added estimates include the 136 first-enrollment institutions where at least 50 1995–1997 University of California applicants enrolled who were employed in California between ages 31 and 35 (using average wages measured at those ages). Institution-level Pell and non-Pell degree recipients (IPEDS) are adjusted for changes over time in the average family income rank of Pell (and non-Pell) recipients; see Appendix A.13 for details. Late 20th century Pell and non-Pell enrollments are reweighted to match total enrollments by degree level, sector, and year due to missing value-added statistics. See Appendix A for details on data construction. Source: CPS OCG, Project Talent, IPEDS, NPSAS, and Bleemer (2022).

Figure 12: Contributions to the Rise in Collegiate Regressivity in the US



Note: This figure decomposes the rise in regressivity of US college enrollment – shown by scaling the non-parametric (gray dots) and parametric (black lines) estimates from Figure 3 to a comparison between students from top- and bottom-tercile parental incomes – into seven components. **Test Composition** holds the estimated wage return to standardized test rank differences fixed in 1957 (Wisconsin) and approximates the effect of the shifting composition of test scores among college-goers by parental income over time, measured from 1932 to 2003. **Test Returns** approximates the additional effect of changes in the average return to high test scores over time, measured from 1957 to 2000. **Discipline Composition** holds the estimated wage return to discipline fixed in 1931 (Time) and approximates the effect of the shifting composition of disciplines among (national and UC) college-goers by parental income over time, measured from 1920 to 2014. **Discipline Returns** approximates the additional effect of changes in the average return to disciplines over time, measured from 1931 to 2005. **Major Composition** approximates the additional effect of changes in the composition of *within-discipline majors* among (national and UC) college-goers by parental income over time, measured from 1957 to 2014 and holding returns fixed in 2005 (ACS). **Institutional Composition** holds the estimated wage return enrollment institution fixed in 1996 (Bleemer, 2022) and approximates the effect of shifting composition of institutions among college-goers by parental income over time, measured from 1959 to 2014. **Institution Returns** approximates the additional effect of changes in the average return to institutions over time, measured in 1963 and 1996. The dotted line shows the sum of the components. See Section 9 for details on construction of this decomposition. Source: See Appendix A.

Figure 13: Parental Status-Based Differential Trends in US Women's College Choices



Note: **Panel (a)**: Points in black show the share of women between ages 30 and 35 who had completed at least one year of college overall (black diamond) or among those from the bottom or top tercile of parental incomes when age 14–17 (circles). The solid line reports the same overall average educational outcome for 1940–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, and 2021 American Community Survey respondents between the ages of 28 and 42. **Panel (b)**: Each coefficient plots estimated differential selection into female college enrollment over time in the United States, with dataset-specific coefficients (δ) and 95-percent confidence intervals from a version of Equation 2 estimated with separate β's in each dataset and replacing $Wage_{it}$ with measures of pre-college cognitive skills. Further information can be found in Figure 5(b). **Panel (c)** The difference in average major premiums earned by University of California female graduates (lines) or nationally-representative respondent female graduates (symbols) from the bottom and top parental income tercile in that year, where major premiums are estimated for ten discipline categories or 66 'detailed' categories in either the 1947 Time Magazine Survey or the 2019–2021 American Community Survey (Figure 6). Figure 7 provides further detail. **Panel (d)**: The difference in average institutional value-added of the enrollment institutions of female students from the bottom and top parental income tercile using male-specific institutional value-added statistics estimated from 18-year-olds in 1963 ("mid-century", using Project Talent) and 1996 ("late-century", from University of California applicant records). The Wisconsin estimates are top-coded at 0.02. See Figure 10 for sources and definitions. See Appendix A for details on data construction and sources for all panels. Source: See Appendix A.

Table 1: Selection-on-Observables Forecast Coefficients of Average Wages by Major

Major Type:	Disciplines				Detailed Majors			
Add'l Cov.:	None	Fam. Inc.	+AFQT	+Race	None	Fam. Inc.	+AFQT	+Race
***Panel A**: Full Sample*								
Full-Sample		1.02	1.03	1.03		1.00	1.01	1.00
β		(0.02)	(0.02)	(0.03)		(0.06)	(0.08)	(0.08)
Obs.			7				14	
1st Stg. Obs.			842				753	
***Panel B**: Split Sample*								
Split-Sample	0.83	0.88	0.92	0.91	0.66	0.68	0.68	0.67
β	(0.09)	(0.09)	(0.09)	(0.09)	(0.28)	(0.27)	(0.28)	(0.28)
Obs.		7				14		
1st Stg. Obs.		418				372		
***Panel C**: Students with Below-Median Parental Income*								
Full-Sample	0.75	0.75	0.75	0.78	0.81	0.83	0.83	0.84
β	(0.33)	(0.28)	(0.25)	(0.26)	(0.19)	(0.16)	(0.14)	(0.18)
Obs.		7				14		
1st Stg. Obs.		266				235		

Note: Each cell in this table displays the result of an OLS regression of average earnings in each college discipline (or detailed major), adjusted for successively more individual-level controls, on average earnings in the same college disciplines (or majors). The first-stage regressions in which we regress average earnings on college discipline fixed effects and the additional covariates are available from the authors; they include birth cohort fixed effects and use survey weights. The individual-level controls (in order of the column header) are: a third-order polynomial in family income rank, a third-order polynomial pre-college test score rank, and indicators for Black and other non-white races. Panel A uses the same baseline sample to estimate both sets of college major fixed effects; Panel B splits the sample evenly (within major) for each first-stage estimation; and Panel C restricts the left-hand-side set of major fixed effects to be estimated in a first-stage regression of only graduates from families with below-median family incomes. '1st Stg. Obs.' reports the number of observations in the first-stage regression that produces the left-hand-side fixed effects. Regressions are weighted by the total number of respondents who select each major, or the number of below-median-income respondents in Panel C. The sample is restricted to male respondents with at least a college degree and to majors or disciplines reported by at least 20 such respondents. Standard errors are robust and do not correct for first-stage sampling error. The baseline first-stage fixed effect regression for disciplines (detailed majors) has an adjusted R^2 of 0.07 (0.09), while the fully-controlled regression has an adjusted R^2 of 0.12 (0.14). See Appendix A for the categorizations of all fields into ten disciplines and 66 detailed majors.

Source: NLSY97.

Table 2: Characteristics of High- and Low-Value Institutions

Dep. Var:	1963 VA		1996 VA	1996 VA - 1963 VA
Year Cov. Measured:	1962	2021	2021	2021
Public Institution	-0.0376 (0.0206)		-0.0281 (0.0265)	-0.1575 (0.0681)
Two-Year Institution	-0.0184 (0.0453)	0.0230 (0.0356)	-0.0190 (0.0249)	-0.1162 (0.0620)
Normalized Average Test Score	0.0089 (0.0084)	0.0557 (0.0192)	0.0288 (0.0103)	0.0336 (0.0200)
Average Parent Income Rank	-0.0002 (0.0007)	0.0004 (0.0010)	0.0021 (0.0011)	0.0060 (0.0025)
Log Instructional Expend. Per Student	0.0137 (0.0197)	0.0637 (0.0173)	0.1052 (0.0234)	0.0871 (0.0441)
Log Enrollment	0.0159 (0.0126)	0.0090 (0.0104)	-0.0109 (0.0128)	-0.0499 (0.0388)
Log Cost of Attendance	-0.0014 (0.0063)	0.0145 (0.0117)	0.0078 (0.0071)	0.0436 (0.0184)
Ivy+	0.1061 (0.0589)		0.3026 (0.1160)	0.2034 (0.1381)
Log Endowment	0.0075 (0.0032)			
1963 VA			0.2949 (0.1281)	
Combined Adj. R^2	0.01	0.05	0.14	0.08
# of Obs.	393	357	124	44

Note: The observational relationships (estimated by OLS) between 1963 (mid-century) and 1996 (late 20th century) estimates of two- and four-year institutions' log wage value-added and institutional characteristics, with each correlation *measured separately* (not conditional on other characteristics). Mid-century value-added is estimated following Equation 9 using age 29 wages from Project Talent; late 20th century value-added estimated following Equation 9 using age 31–35 California wages among 1995–1997 University of California applicants as reported in Appendix I of Bleemer (2022). Two-year institutions are measured in the 1962 Blue Book (column 1) or 2021 IPEDS (columns 2–4); 'Norm Avg. Test Score' is academic score (Project Talent, column 1), SAT (summed 25th and 75th math and reading scores in IPEDS, column 2), or SAT (averaged across 1995–1997 UC applicants who first enroll at that institution) score standardized across observed enrollment-weighted institutions; 'Average Parent Income Rank' is measured across family incomes in Project Talent or reported for the 1989–1991 birth cohorts from IRS data by Chetty et al. (2020); 'Log Inst. Exp. Per Stud.' is measured as total annual income per enrolled student (Blue Book) or instructional expenditure per FTE student (IPEDS); 'Log Enrollment' is measured as total undergraduate enrollment (Blue Book or IPEDS); 'Log Cost of Attend.' is total posted tuition, required fees, room, board (Blue Book or IPEDS); Ivy+ institutions include the contemporary Ivy League, Chicago, Duke, MIT, and Stanford; and 'Log Endowment' is only measured in the Blue Book. Combined R^2 is measured from a regression of each dependent variable on all covariates, omitting 1963 VA in column 3 (since it is frequently unobserved). 1996 value-added estimates are propensity-score-weighted to represent US higher education; see Appendix A. Value-added estimates are unshrunk and estimates are unadjusted for VA sampling error.

Source: 1962 Blue Book, Project Talent, IPEDS, Chetty et al. (2020), and Bleemer (2022).

Table 3: Selection-on-Observables Forecast Coefficients of Institutional Value-Added

Sample:	Same Sample				Split Sample				
Add'l Cov.:	+Tests	+Grades	+HS FE	+Extra.	Base.	+Tests	+Grades	+HS FE	+Extra.
Inst. FE	0.97 (0.004)	0.95 (0.006)	0.82 (0.027)	0.82 (0.026)	0.552 (0.041)	0.523 (0.041)	0.511 (0.040)	0.385 (0.051)	0.389 (0.049)
Obs.	396				396				
1st Stg. Obs.	22,099				10,956				

Note: Each cell in this table displays the result of an OLS regression of average 1963 institutional value-added, adjusted for successively more individual-level controls, on average baseline institutional value-added (which control for fifth-order polynomials in family income rank and academic performance and indicators for Black and other non-white races). We omit the first-stage regressions in which we regress average earnings on institution fixed effects and baseline controls (and additional controls where labeled) from the table. The additional individual-level controls are: verbal, quantitative, technical, and scientific "Test" score components; high school "Grade" point average, high school fixed effects, and self-reported indices of 'Extracurricular' participation, reading, hobbies, sports, leadership, and 'socialness'. The 'Same Sample' columns use the same baseline sample in the first stage to estimate both sets of institution fixed effects; the 'Split Sample' columns split the sample evenly (within institution) for each first-stage estimation, with '1st Stg. Obs.' reporting the number of observations in the baseline fixed effect estimation. Regressions are weighted by the total number of respondents who enroll at each institution. The sample is restricted to male respondents with at least a college degree and to institutions reported by at least 20 such respondents. All first-stage regressions include birth cohort fixed effects and use survey weights. Standard errors are robust and do not correct for first-stage sampling error. The baseline first-stage fixed effect regression for has an adjusted R^2 of 0.06, while the fully-controlled regression has an adjusted R^2 of 0.09.

Source: Project Talent.

Table 4: Tabular Decomposition of Collegiate Regressivity in the US in Log Dollars

Year:	1960	1980	2000	2014
Obs. Regressivity	0.022	0.077	0.131	0.169
Test Selection	0.004	0.035	0.011	0.013
	[18.4]	[45.9]	[8.7]	[7.5]
Net Regressivity	0.018	0.041	0.120	0.157
Major Composition	0.029	0.013	0.010	0.034
	[131.3]	[16.4]	[8.0]	[20.0]
Major Returns	0.001	0.008	-0.002	0.013
	[3.9]	[11.0]	[-1.7]	[7.8]
Inst. Composition	0.006	0.021	0.001	0.006
	[25.8]	[27.9]	[0.9]	[3.7]
Inst. Returns	0.000	0.008	0.029	0.046
	[0.0]	[9.8]	[22.4]	[27.2]

Note: This table summarizes the contributions of selection (on pre-college academic preparation), college majors, and collegiate institutions to the rise in collegiate regressivity since 1950 in four year cross-sections. “Obs. Regressivity” approximates the overall log-wage increase in the value of college-going for students from the top tercile of parental incomes relative to those from the bottom tercile (see the solid line in Figure 12). “Test Selection” combines the two selection components visualized in Figure 12; “Net Regressivity” is the remaining regressivity not explained by test selection. The remaining four components are defined in the note to Figure 12; they are presented in overall magnitude and (in brackets) as a share of total Observational Regressivity. Institutional returns cannot contribute prior to 1960 because we have no measure of institutional value-added from before 1963. See Section 9 for details on the construction of this decomposition.

Source: See Appendix A.

Online Appendix

Changes in the College Mobility Pipeline Since 1900

Zachary Bleemer and Sarah Quincy

December 2024

Table of Contents

Appendix A: Data Appendix

This study employs a large set of survey and administrative data sources. This appendix documents the main data sources used to measure changes in regressivity, selection, major attainment, and institution attainment in the US since 1920 (see Figure 2) along with additional information on historical data sources used to construct several auxiliary figures.

A.1 US Census and World War II Draft Cards

Previous studies of pre-1940 higher education in the United States have almost exclusively used data from post-1940 US Censuses.[64] Before 1940, the US Census asked no questions about individuals' education; starting in that year, the Census asked respondents for the "highest grade of school completed," with responses ranging from 0 ("None") to 17 ("College, 5th or subsequent year"). Using automated record linkage techniques (e.g. Abramitzky et al. (2012)) for men and Price et al. (2021); Buckles et al. (2023) for women), we match individuals' pre-college information with adult earnings. However, census data do not distinguish between the partial completion of a Bachelor's degree and partial or full completion of a two-year junior or teaching college degree, and do not ask about college type or field of study.[65] Appendix B discusses link quality and robustness to alternative linking strategies.

In Figure 3 and associated figures, we restrict the sample to men born 31–35 years before the 1940 Census with positive wage and salary income who resided with at least one parent with positive LIDO (Saavedra and Twinam, 2020) – socioeconomic status predicted by occupation, industry, age, sex, race, state, census region and their interactions using LASSO – in the 1920 Census.[66] We define (log) wage income by (log) wage income reported in the 1940 Census and rank incomes within-sample. We define parental income rank using LIDO within-sample. We define college enrollment, attainment, and graduate school enrollment by people who report at least 1, 4, or 5 years of college in 1940, respectively. In Figure 4a and associated figures, we broaden our sample to men born 30–35, 40–45, and 50–55 years before 1940 who resided with at least one parent with positive LIDO in the 1920, 1910, and 1900 Census.

We also link the 1940 US Census to 1943 World War II draft cards by name, birth year, and birth state following the linkage methodology of Abramitzky et al. (2012). As described in Ferrie et al. (2012), staffers mistakenly included AGCT test scores in the weight field at many enlistment centers over ten weeks between March and June 1943.[67] We then link matched draft cards to

[64] See, e.g., Smith (1984), Mare (1991), and Goldin et al. (2006). Exceptions include Goldin and Katz (2000), which studies the 1915 Iowa Census, Goldin and Katz (1999), which uses data on universities from the US Department of Education. and Andrews (2023), who adds in patent and yearbook information to observe students' names and innovation.

[65] This measure reflects "years of schooling" rather than completed educational attainment, which may not be strictly equivalent, especially in the South (Margo, 1986).

[66] Fathers are identified as the household head in non-group quarters only if they report they have a child living in the household and they are male. Census enumerators included college student-aged children with their parents' household regardless of student age (Bureau of the Census, 1930).

[67] The AGCT is the World War II equivalent of the modern AFQT. Additionally, Ferrie et al. (2012) show that the

the 1950 census using the Helgertz et al. (2023) crosswalk from the 1940 to 1950 censuses. We restrict the sample to men matched between all three sources who were born between 1923 and 1928 (age 15–20 in 1943) to identify young men whose 1943 test scores would largely represent pre-college academic aptitude.[68] We further restrict to men who resided with at least one parent with observable wage income in 1940, and use that parent's income as our measure of family income.[69] Parental income and AGCT rank are measured within-sample.

Census data are available from Ruggles et al. (2024), with names attached using Ruggles et al. (2021). WWII draft card data are available from the National Archives.

A.2 1947 Time Magazine College Graduate Survey

Time Magazine conducted its 'US College Graduate Study' in the spring of 1947 (Havemann and West, 1952). The names and addresses of all bachelor's degree recipients with last names beginning with the letters 'FA' were solicited from all colleges, universities, and teachers' colleges, with 95 percent of student-weighted institutions providing this information. Out of 17,053 such graduates, 9,064 mailed back a completed questionnaire (in one of two attempts) and 419 more were interviewed, with some underrepresentation of non-white respondents (1 vs 4 percent) but reported representativeness on other measurable characteristics. The survey responses were digitized for this study by the Roper Center at Cornell University and will soon be made publicly available on that institution's website.

The survey includes respondents' gender, age (in ten-year bins), current annual wage income (8 bins), and college major (15 bins); these data are used to construct the 1930s college major premiums shown in Figure 6. The survey also includes the institutions from which individuals earned their undergraduate (and graduate) degrees.

We also match respondents to US Census records among respondents with 'FA' last names and at least 3 years of post-secondary education as of 1940. We further restrict our 1940 census pool to those we successfully find as children in an earlier census.[70] We use childhood location, probable age (year of college graduation - 22 in Time), and race to identify which survey respondent is which census observation by modifying Abramitzky et al. (2022) linking strategies separately for men and women:

1. We find all unique matches based on birth year, birth state, and race, first by exact birth year, then within one- and then two-year bands, and keep links that are unique within that five year

overall AGCT test score distribution looks quite similar to the enlistees in their sample which is linked to the 1930 5% IPUMS sample. These enlistment cohorts had test scores similar to other periods of the conflict as seen in Maier and Sims (1986). We follow Aaronson and Mazumder (2011) in classifying which observations in the "weight" field actually recorded test scores.

[68] Draft cards also report years of education; 26 percent of the linked sample had completed at least one year of college prior to enlisting.

[69] Results are similar if we use parental LIDO in 1940 instead of observed income.

[70] We use Abramitzky et al. (2022) NYSIIS-standard crosswalks for men. For women, we use their childhood census reported last name found with Price et al. (2021) crosswalks to define those eligible under the Time sampling method but otherwise use the same criteria.

band. We remove these unique links from the pool of potential matches for the next step.

2. In the next step, we include information on "pre-college" city size and state in the linking criteria. In the Time survey, this corresponds to the question asking where people spent most of their pre-college years. In the census data, we use the state and city information recorded in the childhood link.[71] Again, we iteratively link using the age bands in Step 2, and only keep unique observations from that pool as links. We append these links to those from Step 2 and remove them from the pool of potential matches.

3. Finally, we add "post-college" characteristics to the linking criteria: city size, state, and marital status.[72] The Time survey asks where people spent most of their post-college years. We use 1940 location to determine the census equivalent. We iteratively link these datasets using the five-year age band and keep only those matches which are unique within the age band.

This process yields 1,102 unique matches out of 4,808 male Time magazine respondents, a 23 percent match rate, and 719 out of 3,384 female Time magazine respondents, a 21 percent match rate. This match rate is consistent with other historical linking rates; recall that we condition on having pre-1940 census information in the pool of potential census matches. Our sample will be tilted towards those who moved less as children and adults, as we consider census location to be people's usual pre- or post-college location. Other linking biases are likely smaller, due to the high human capital required to be in the sample.

A.3 CPS Occupational Changes in a Generation Supplement

The Current Population Survey (CPS) included an Occupational Changes in a Generation supplement in March 1962 and March 1973. The questionnaires augmented the standard labor market questions elicited in the CPS – including the male household head's wage income in the prior year, all household wage income in the prior year, and his level of education (including codes for college enrollment, attainment, and graduate school attainment) – with questions about the socioeconomic status of the male household head's parents' socioeconomic status. While survey respondents varied in age, we restrict analysis to individuals aged 30 to 35 for our main analysis and 30 to 60 for supplementary analysis of variation in college-going, major, and institution among earlier birth cohorts.

In order to construct more accurate, continuous, and commensurable measures for both parental and (in 1962) respondent income, we follow the approach of Collins and Wanamaker (2022).[73] Collins and Wanamaker (2022) non-parametrically impute incomes as the average earnings of

[71] The Time survey city size categories separately identify farm residents, rural non-farm residents, and 4 city population bins. We use $FARM$, $URBAN$, and $CITYPOP$ to assign this in the census records.

[72] In both datasets, marital status separately identifies married, separated, divorced, widowed, and never-married individuals.

[73] Respondent incomes are only observable for a small fraction of employed 1962 respondents but are available in the 1973 survey.

individuals in the same occupation, race, region, and gender cell from the nearest decennial census. Parental income and respondent income are winsorized at 1 percent and CPI-adjusted to 2022.

We group respondents of both samples into one of five educational attainment categories: less than high school, completed high school, some college (no degree), college (with a degree), and graduate school.

The 1973 OCG sample also recorded which college respondents attended. Institutions were reported with 1971 FICE codes, which we extract from a digitized NCES codebook and then link to modern IPEDS institution codes by institution name (manually corrected in case of name changes or institutional closures). Our FICE-IPEDS crosswalk is available in the article's supplementary files. College majors are reported for all enrollees, not only graduates, in 141 categories; we construct a dictionary matching these to both our 10-code disciplines and our 66-code detailed majors.

OCG data are available from ICPSR. Replication files for Collins and Wanamaker (2022) are also available from ICPSR.

A.4 Wisconsin Longitudinal Survey

The Wisconsin Longitudinal Survey (WLS) was a multi-decade longitudinal survey of the 1957 class of Wisconsin high school graduates. We focus on the baseline surveys and the 1975 follow-up eliciting information about 1974 (when respondents were about 35), restricting the sample to individuals born 1937–1945. We observe parental income in 1957–1960 and child income in 1974. Income ranks are derived from Current Population Survey respondents: parental incomes are ranked relative to 1962–1963 CPS respondents (the earliest available years) with at least one child aged 16-17 and child incomes are ranked relative to 1974 respondents between ages 33 and 36. Respondents' Henmon-Nelson test score (a standard cognitive test) was measured in their junior year of high school; we produce test score ranks within-sample. We code college enrollment as people who report in 1964 or 1975 that they are attending college or attended college but have no degree, but it is insufficient for the respondent to report an indicator that they have 'ever attended' college, since many such students may not have even completed one year of college (our minimum bar). College attainment is measured by reporting a Bachelor's degree; graduate school is measured by reporting at least one year of post-baccalaureate study. Parental income and respondent income are winsorized at 1 percent and CPI-adjusted to 2022.

We construct dictionaries matching Wisconsin respondents' enrollment institutions to IPEDS *UnitID*'s and 799 college majors to our 10-code disciplines and 66-code detailed majors.

Most of these data are available from the Wisconsin Longitudinal Study website. Researchers should contact the study office to obtain respondents' first enrollment institution.

A.5 Project Talent

Project Talent was a massive longitudinal survey of 1960 high school students. We combine the baseline survey with the 11-year follow-up survey, when respondents were approximately age

29. Respondents are assigned sample weights in both surveys; a small number of follow-up non-respondents were insistently surveyed, with multiple in-person visits, and upweighted to account for non-response. Eleventh and twelfth grade students are omitted from our baseline analysis because they may be positively selected (since dropouts are excluded) but are included in our value-added analysis (since dropouts would have been unlikely to enroll in college, mitigating selection bias). Students' measured "general academic aptitude" is measured by their composite performance on a battery of math (38%), reading (48%), vocabulary (4%), abstract reasoning (4%), and creativity (6%) exams; the correlation with IQ is 0.94. Wage earnings are defined as wages derived from main job.

Parental income is observed in six bins for about two-thirds of children. In order to add nuance to observed parental income, we use the 1960 1% US Census sample to predict parental income using race (five codes), region (four codes), home value (five codes) or rent level (five codes), mother's and father's education (ten codes), mother's and father's occupation (fifty codes), number of children (when observed), and six parental income bins (e.g. <3000, 3000–6000,... when observed). We restrict the Census to households with at least one member aged 30–64 and at least one member aged 13–20.

The institution recorded in Project Talent is the final undergraduate institution where the student enrolled. We reassign any institution with fewer than 20 male enrollees to an aggregate institution by state and level (2- or 4-year), and then exclude remaining institutions with fewer than 20 enrollees. We construct dictionaries matching Project Talent respondents' enrollment institutions to IPEDS *UnitID*'s and 44 college majors to our 10-code disciplines and 66-code detailed majors.

A.6 National Longitudinal Surveys

We employ data from four cohorts of National Longitudinal Surveys: the Young Men and Young Women of the original National Longitudinal Survey (NLS), the National Longitudinal Survey of Youth 1979 (NLSY79), and the National Longitudinal Survey of Youth 1997 (NLSY97). The data are publicly available from the NLS Investigator. In each of these samples, we restrict to individuals who are first observed at ages 14-18 with non-missing parental income and eventual educational attainment. Income and test ranks are measured using sample weights within sample and (except for family income) within gender.

Male (female) NLS respondents are in the 1948–1952 (1950–1954) birth cohorts. Family income is measured in the first year of observation (or 1 or two years later if otherwise unavailable) – 1966 for men and 1968 for men – in 11 bins. IQ test scores were measured on a 40–160 scale in the first year of observation. Child income is CPI-adjusted, continuous, and averaged across all observed years between ages 30 and 35 conditional on employment. Education is measured as the highest level of education reported in the union of survey responses between 1975–1980 (male) or 1972–1978 (female). College majors are observed in 31 categories; we construct a dictionary matching these to our 10-code disciplines.

NLSY79 respondents are in the 1961–1965 birth cohorts. Family income is measured continuously in 1979, the first year of observation. AFQT test scores were measured on a 1–99 scale in 1979.[74] Child income is CPI-adjusted, continuous, and averaged between observed incomes in 1994, 1996, 1998, and 2000 conditional on having non-zero income and being aged 30–35; sample weights are similarly averaged. Education is measured as the highest completed year of schooling using the concatenation of all survey responses. College majors are observed in 380 categories; we construct a dictionary matching these to both our 10-code disciplines and our 66-code detailed majors.

NLSY97 respondents are in the 1980–1984 birth cohorts. Family income is averaged over non-zero CPI-adjusted continuous parental incomes in 1997, 1998, and 1999. ASVAB test scores were measured on a continuous 1–100 scale in 1999. Child income is CPI-adjusted, continuous, and averaged between observed incomes in 2011, 2013, 2015, 2017, and 2019 conditional on having non-zero income and being aged 30–35; baseline sample weights are used.[75] Education is measured as the highest completed year of schooling across all survey responses. College majors are reported in 34 categories; we construct a dictionary matching these to both our 10-code disciplines and our 66-code detailed majors.

A.7 National Center for Education Statistics Surveys

We employ data from three cohorts of National Center for Education Statistics longitudinal studies: the National Longitudinal Survey of 1972 (NLS72), the National Educational Longitudinal Study of 1988 (NELS) and the Educational Longitudinal Study of 2002 (ELS). Each of these survey datasets is available from ICPSR: the NLS72, the NELS and the ELS. Two other NCES high school studies – HS&B and and HSLS-09 – do not permit unit-level data to be accessed from non-NCES computers, prohibiting our combining them with the other datasets in our sample into a comprehensive analysis.

The final follow-up with NELS and ELS participants occurred at age 26, when respondents were still too young for their annual wages to be representative of their career outcomes; as a result, we exclude them from all employment and wage analysis. The final follow-up of the NLS72, on the other hand, was conducted 14 years later, when respondents were about 32; we measure educational attainment and wages in that year. The NELS and ELS surveys are only included in our analysis of selection into college by parental income and test scores. In each sample, we restrict to individuals with observed positive family income and either observed age-26 or age-32 level of education, respectively. Family income, child wages, and test ranks are measured using sample weights within sample and (for wages and tests) gender.

NLS72 respondents are mostly in the 1954 birth cohort, with smaller numbers between 1950 and 1956. Family income is measured in 1967; child income is measured in 1986. Sample weights for age-32 respondents are used. The age-32 follow-up does not elicit whether the respondent

[74] While renormed AFQT scores are available – using alternative unit-level weights constructed in 1989 and 2006 – the strength of the relationship between those renormed scores and later-life income is so strong as to suggest potential overfitting; we use the original scores in our analysis.

[75] Over half of reported survey weights for 30s respondents with incomes are null, so we use baseline weights rather than dropping all those observations.

graduated high school, so we define high school graduation using responses from the second survey wave (1973) and otherwise define education by years of completed academic schooling; people who completed two years of 'vocational/technical' schooling are coded as completing an Associate's degree. Students' measured "academic aptitude" is measured by their composite performance on a battery of math (25%), reading (25%), vocabulary (25%), and logic (25%) exams. Enrollment institution is defined as the earliest FICE code that matches an institution in HEGIS as reported for enrollment 1-4 years following high school graduation, conditional on completing at least one year of college. College major is defined as the earliest college major code associated with the student's college enrollment, as measured 1-4 years following high school graduation.

NELS respondents are mostly in the 1974 birth cohort, with smaller numbers between 1972 and 1975. Family income is measured in 15 bins in 1988. Test scores are the sum of standardized math and reading scores on exams taken in 1988. Sample weights for age-26 respondents are used. Education is measured as the number of completed years of school as of age 26.

ELS respondents are mostly in the 1985–1986 birth cohorts, with smaller numbers between 1983 and 1987. Family income is measured in 13 bins in 2001. Test scores are the sum of standardized composite math and reading scores on exams taken in 2002. Sample weights for age-26 respondents are used. Education is measured as the number of years of education completed, requiring students who report having enrolled in at least one year of college to have completed at least one year of course credits to be recorded as having *completed* at least one year of college.

A.8 National Longitudinal Study of Adolescent to Adult Health

The National Longitudinal Study of Adolescent to Adult Health (ADD) is a longitudinal survey of adolescents conducted by the Eunice Kennedy Shriver National Institute of Child Health and Human Development, one of the National Institutes of Health. It is a nationally-representative survey of people who were in grades 7–12 during the 1994–1995 school year. We restrict to the 1974–1980 birth cohorts (though most respondents were born after 1976) because child's income is measured in 2008, when later birth cohorts would have been under the age of 28 and were thus too young for their incomes to be included. The sample is also restricted to respondents with positive recorded family income in 1994, recorded education in 2008, and non-missing survey weight.[76] The data are available from ICPSR.

Family income is CPI-adjusted and measured continuously in 1994. There are no test scores that are comprehensively available in ADD. Child income is CPI-adjusted, continuous, and measured in 2008. Education is measured as the number of years of education completed; some respondents report having "some vocational/technical training (after high school)," which we assume is less than one year of completed college (and thus is just high school completion), whereas "completed vocational/technical training (after high school)" is assumed to be equivalent to Associate's Degree attainment (and two years of college completion). Students who report "some graduate school" or "some post baccalauraete professional education (e.g., law school, med school, nurse)"

[76] About 1,400 respondents have missing survey weights but non-missing family income. We drop these respondents.

are recorded as enrolling in graduate school.

A.9 Panel Study of Income Dynamics

The Panel Study of Income Dynamics (PSID) is a longitudinal survey of American families conducted by the University of Michigan's Survey Research Center. While the original sample was approximately nationally representative, the unique survey structure – following family members across generations, with new cohorts added to represent immigrant households – representativeness has become challenging to establish, and survey weights are unavailable. We treat the data as if they are nationally representative, though estimates should be interpreted with caution. Surveys were conducted annually from 1968 to 1997 and biennially from 1999 to 2019, but income is generally unavailable in all surveys in and after 2009 for unknown reason. The data are available directly from the Survey Research Center.

We restrict the sample to individuals for whom we observe non-zero family incomes between ages 14–17 (the sum of all household members' reported incomes, excluding households with more than 14 members) and non-zero child incomes between ages 30–35, defining each by the average CPI-adjusted continuous non-zero income observed in those age windows. Birth year is defined as the modal reported birth year across surveys. No test scores are available in this survey. Education is measured as the years of completed schooling, defined by the highest value that respondents provide as a response to the years of schooling completed question; college-going is defined by having completed at least 13 years of schooling, and individuals without observed education (less than 1 percent) are omitted. College majors are observed for a subset of some recent cohorts but are presently omitted from our analysis.

The full span of PSID birth years with observable family and child income is from 1951 to 1988, with few respondents with observed child income after the 1977 cohort because of missing post-2007 incomes. The median birth cohort is 1965, so we visualize the PSID data by splitting it into two halves, the 1951–1965 birth cohorts and the 1966–1988 birth cohorts.

A.10 General Social Survey

The General Social Survey (GSS) is a nationally-representative cross-sectional survey of Americans conducted by NORC at the University of Chicago annually from 1974–1991 and biannually thereafter.[77] While parental income is not directly observed, we follow and extend Jácome et al. (2024) and impute household family income by parental occupation, parental education, region, and race bin using the nearest Census to when the respondent was 10. We restrict to respondents between the ages of 30 and 35 with observed imputed family income and observed education, resulting in 150–400 male and female respondents per survey year. Survey weights adjust for over-sampling of Black respondents in some years; weights are normalized across years to sum to the number of not-omitted respondents in that year. The data are available from NORC.

[77] There were no surveys in 1979 and 1981, and the 2020 survey was conducted in 2021. The survey was also provided in 1972 and 1973, but respondent income was not solicited in those years.

Test scores are unavailable. Child income is measured in 12–25 discrete bins, with an increasing number of bins over time. Education is measured as the number of years of education completed or defined by degrees received. Family and child incomes are CPI-adjusted; ranks are defined within-sample among GSS respondents within 5 birth cohorts of the respondent.

The GSS parental income methodology is comparable (though coarser) to the parental income imputation conducted in the CPS OCG survey described above, but covers a similar time period to longitudinal surveys in which parental income is directly observed. It also employs coarse wage bins with relatively low top-coding (e.g. $100,000 in the 2000s and $160,000 in recent years), unlike those surveys' continuous non-top-coded wages. Perhaps for these or other reasons, however, the relative returns to college-going measured in the GSS overall and by parental income appear quite different than those measured in other contemporaneous surveys.

Figure AA-1 splits the combined GSS into weighted birth year quartiles and reports jointly-estimated β_t and δ_t coefficients and standard errors from Equation 2 estimated only over GSS respondents. In the first quartile of birth cohorts, there is an approximately 0 return overall to college enrollment, and no differentiation in this return by parental income. The 1970s and 1980s cohorts appear to enroll in a period of substantial *progressivity* in American higher education, where the observational return to college is far higher for lower- than higher-income studnets. That period ends in recent years, when the noisily-estimated point estimate suggests that American higher education has become slightly regressive.

Excluding the early years in which the GSS reports a null observational return to college-going, Panel (b) of Figure AA-1 suggests that American higher education has become far more regressive over time, with a slope substantially higher than that reported in Figure 3. However, the figure implies meaningful different in the level of collegiate regressivity: while regressivity appears to have been rising in the GSS since the early 1970s, it has risen from strong progressivity to parity (though the recent years' point estimates from Figure 3 cannot be rejected by the last point estimate in Figure AA-1b). While we are unsure why the level of collegiate regressivity appears so much lower in the GSS, its lower-quality data (unobserved parental income and coarsely-observed child income) and the fact that the rising regressivity trend is replicated over the past five decades leads us to exclude GSS from our main analysis, though we do not believe that it meaningfully undermines our presented findings.

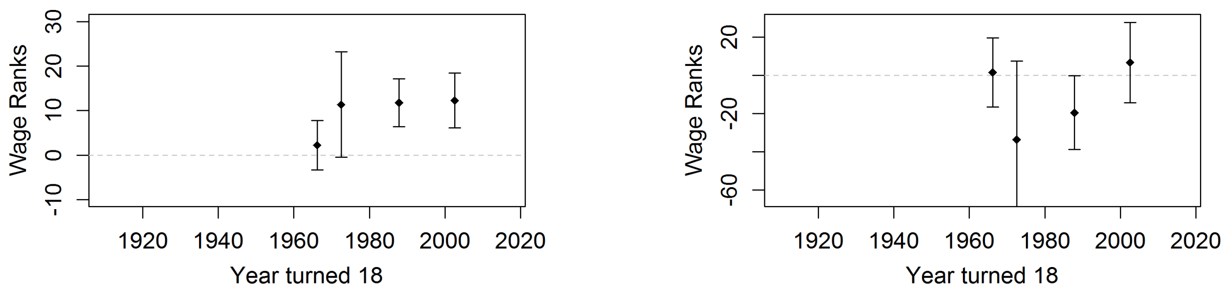
A.11 American National Election Studies

The American National Election Studies (ANES) is a nationally-representative cross-sectional survey of Americans conducted by a collaboration of universities biannually from 1948–2022. While parental income is not directly observed, Jácome et al. (2024) impute household family income by parental occupation, parental education, region, and race bin using the nearest Census to when the respondent was 10. They then use the ANES survey to measure changes over time in income mobility in the US.

Unfortunately, the ANES does not include sufficient information for us to include it in our analysis. First, the ANES survey does not elicit child income, instead eliciting the income of the

Figure AA-1: Regressivity of US Higher Education Over Time in the GSS

(a) Main Effects (β_t) (b) Interactions (δ_t)



Note: This figure shows that apart from the survey's earliest years (when there was *no* observational return to college in the GSS), college-going has become sharply more regressive over time among GSS respondents, although from a much lower base than in contemporaneous longitudinal surveys. Estimated regressivity of male college enrollment over time in the United States following Equation 2, parameterizing $Coll_{it}$ as indicating at least one year of college and estimating dataset-specific β_t and δ_t coefficients and 95-percent confidence intervals with separate terms for each quartile of birth years among GSS respondents. Child incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights; standard errors are robust. Source: GSS.

child's family (including spouse and other immediate family members). Second, since 1952 the education question has not differentiated between less than one year of college and more than one year of college. We define college-going as completing at least one year of college, and are thus unable to cleanly distinguish college-goers from non-college-goers in the ANES. For these reasons, we do not incorporate the ANES in any of our analysis.

A.12 University of California Administrative Data

To characterize the selection into college majors before World War II, we digitize the annual 1920–1940 university student registers of the University of California and match them to the 1900–1940 full count censuses. These annual records detail students' names, hometowns (residence when not at school), year in school (we characterize students by their initial matriculation year), and discipline of study (whether or not they completed their degree) for all enrollees at any University of California campus, which at the time included UC Berkeley, UCLA, UC San Francisco (which enrolled Bachelor's students in health science fields), and UC Davis (which enrolled Bachelor's students in agriculture).[78] The data are available from the UC ClioMetric History Project.

We use Abramitzky et al. (2012) style algorithmic linking commands from Abramitzky et al. (2022) modified to use the identifying information in the registers. This requires two departures from the Abramitzky et al. (2012) approach for finding a likely match between two datasets. Because the registers do not report age, only year in school, we first assign a birth year based on the student being 18 at the first observation in a register. Unlike census records, registers did not

[78] We group disciplines into our ten discipline categories, though in this period humanities, social sciences, and natural sciences are all combined into "Letters and Sciences".

contain information on birthplace, so we use students' first observed hometown in the register data to assign state of residence.[79] Then we take a census record and a register record as matched if they report the same NYSIIS-standardized first and last names (replacing last name with maiden name for women married before college), reported sex, census year state of residence, and estimated birth year iteratively widening to a two-year band around birth year.[80] We conduct this exercise for each census year in our dataset. We discard any linked student with more than seven combined years of college to balance the historical reality of students' movement between campuses and the potential for false matches. Any unlinked record in either a register or the census is discarded.[81]

For matched students, we assign their family income based on the observed father's LIDO score discussed in Section A.1. Parental income terciles are defined by the national family LIDO percentile of their matched father's LIDO.

To characterize college major selection after 1970, we combine the complete annual student administrative records of six of the nine undergraduate University of California campuses (Bleemer, 2018): UC Berkeley (1975–2015), UC Davis (1980–2015), UC Irvine (1975–2015), UC Riverside (1982–2015), UC Santa Barbara (1988–2015), and UC Santa Cruz (1983–2015). The data are only available with restricted access from each campus's Office of the Registrar. For each undergraduate student, we observe their first matriculation year, their gender, their home address, and their final declared majors (whether or not they completed their degree). We geolocate each address into its 1980 and 1990 Census tract and assign each student the average 1979 income of households in their 1980 Census tract as reported in the 1980 Census (for 1975–1984 student cohorts), the average 1989 income of households in their 1990 Census tract as reported in the 1990 Census (for 1985–1994 student cohorts), or the average contemporaneous income (in their year of matriculation) of households in their Zip code as reported in the Internal Revenue Service Statistics of Income (for 1995–2015 cohorts).[82] Income terciles are defined nationally across population-weighted tracts and tax-return-weighted Zip codes. College majors are observed in a large set of raw university-designated categories; we construct a dictionary matching these to both our 10-code disciplines and our 66-code detailed majors. Data from the other three undergraduate UC campuses – UCLA, UC San Diego, and UC Merced (which opened in 2005) – are unavailable.

A.13 IPEDS and NPSAS

We use data from the Integrated Postsecondary Education Data System (IPEDS) to construct annual first-year and undergraduate enrollment by Pell status annually since 1984 (excluding 1985

[79]This biases us towards finding students who did not move between a census and their first year of college. Therefore, we only look for students in the closest pre-college census among youths between the ages of 6 and 20, which allows for a two-year band in age reporting.

[80]The age band allows us to find transfer students who are over 18 when they first enroll in a four-year school, for example. Junior college transfers were already common in California by this time (Greenleaf, 1939).

[81]Appendix B provides balance tables for the linked and unlinked target populations and a comparison of linkage methods for our main findings. Though these data often under-represent Black sons (Ward, 2023), college-goers are largely white. We show below that our results are similar across linking approaches as well.

[82]Due to limited IRS SOI data availability, the 1995–2000 cohorts are assigned to the average family income in their Zip code in 1998; the 2001–2003 cohorts to 2001; and the 2008 cohort to 2007.

and 1987–1989, when Pell data are unavailable). We exclude institutions outside the 50 states and schools that only award degrees completed in less than two years.

Prior to 2009, IPEDS provides the total Pell funding paid to each university but not the number of students who received that funding. Starting in 2009, both numbers are provided. We impute the number of students receiving Pell funding at each university prior to 2009 as:

$$IP_{iyg} = FEnr_{iyg} \times \frac{\frac{TotalP_{iy}}{MaxP_y}}{FTE_{iy}} \times \left(\left(\frac{1}{|U_{sc}|} \sum_{j \in U_{sc}} PercP_j \right) \times \left(\frac{1}{|U_{sc}|} \sum_{j \in U_{sc}} \frac{FTE_j}{\frac{TotalP_j}{MaxP_y}} \right) \right)^{-1} \tag{AA-1}$$

where $FEnr_{ig}$ is the full-time first-time domestic undergraduate enrollment of gender g at institution i in year y, FTE_{iy} is the full-time-equivalent total domestic undergraduate enrollment at i in y, $TotalP_{iy}$ is the total amount of Pell funding provided to i in y, $MaxP_y$ is the maximum (and modal) amount of Pell funding available to a single student in y, U_{sc} is the set of institution-years in i's state s and control (public, private, or for-profit) c, and $PercP_j$ is the share of 2009–2021 undergraduate students at institution-year j who receive any Pell grant funding. $TotalP_{iy}$ is unobserved in about 11 percent of institution-years with positive domestic enrollment, primarily at community colleges in the 1990s; we linearly interpolate Pell *enrollment shares* (not total funding, preserving observed enrollment fluctuations) between observation years to approximate Pell recipience in those years.

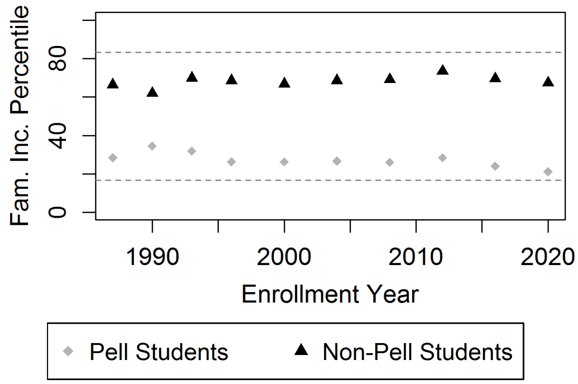
The composition of students who receive or do not receive Pell grants has changed over time due to changes in Pell eligibility criteria. We use survey data from the 1987–2020 National Postsecondary Student Aid Study – accessed through the NCES DataLab – to measure the median parental income of (dependent) male Pell grant recipients and non-Pell college-goers triennially. We convert these median parental incomes to ranks using the household income distribution of survey-weighted households with 16- or 17-year-olds in the contemporaneous March supplement of the Current Population Survey (Ruggles et al., 2024); Figure AA-2 shows the resulting triennial median income ranks of Pell and non-Pell students.

We linearly smooth these estimated median income ranks across years and use them to convert differences in the enrollments of Pell and non-Pell students into the differences in enrollments of top- and bottom-tercile students by assuming linearity in the parental income gaps. Notice that the parental income of students from the top (bottom) parental income tercile is approximately $\frac{5}{6}$ ($\frac{1}{6}$), so the average difference in ranks between students in the top and bottom parental income terciles is approximately $\frac{200}{3}$. If the difference in average institutional value-added of first enrollment institutions between Pell and non-Pell students in year y is x_y, then we impute the difference between bottom- and top-tercile students to be $\frac{200}{3} \times \left(AvgNonPell_y - AvgPell_y\right)^{-1}$, where $AvgNonPell_y$ ($AvgPell_y$) is the average family income rank of non-Pell (Pell) students in y.

A.14 College Scorecard

The College Scorecard (C.S.) has released large annual data files since 2015. Somewhat confusingly, the fields recording field of study by institution and Pell status – which are present in each

Figure AA-2: Average Parental Income of Pell and Non-Pell Students



Note: This figure shows changes over time in the average parental income gap between Pell and non-Pell college-goers, an important factor for our analysis to adjust for when using Pell and non-Pell enrollments to impute the enrollments of lower- and higher-income students. The median family income rank of Pell and non-Pell college enrollees by year of enrollment. Income ranks are defined across CPS households with aged 16–17 children between one year before and after the NPSAS survey. The horizontal dotted lines represent the median incomes of households in the top and bottom tercile of the family income distribution. Source: NPSAS and CPS.

year's report – are recorded as "Privacy Suppressed" in *every* cell (that is, for every institution-major pair) in all but the 2018–2019 report. As a result, we are only able to use C.S. data to measure differences in major attainment by parental income in a single cross-section: students who graduated in 2015 or 2016 (and who thus started college around 2010–2011).

We restrict the data to Bachelor's degrees and use the "EARN_COUNT_PELL_NE_3YR" and "EARN_COUNT_NOPELL_NE_3YR" fields to measure total Pell and non-Pell enrollment by institution and four-digit CIP code. We also measure male and female enrollment by institution-CIP. Counts below 10 are suppressed, but can sometimes be reconstructed as the difference between total degrees and the variable's converse.[83] Merging in total 2016 degree counts by institution-CIP from IPEDS (excluding double-majors, since it appears C.S. assigns each student to their first major), we find that about 78 percent of all degrees are accounted for in the C.S. data, with most remaining degrees likely awarded in suppressed institution-CIP's.[84] When counts by gender are unavailable in C.S. (but Pell counts are available), we preserve the gender proportions observed in that institution-CIP in IPEDS. This results in 1.5 million degree observations. We assign Pell and non-Pell shares of male and female students proportionately to the male and female shares in those institution-CIP's.

College majors are reported in 379 four-digit CIP categories; we construct a dictionary matching these to both our 10-code disciplines and our 66-code detailed majors.

Because the composition of students who receive or do not receive Pell grants has changed

[83] For example, if a institution-CIP pair has 15 total degrees and 12 Pell degrees, we infer 3 non-Pell degrees despite its being privacy-suppressed.

[84] IPEDS and C.S. are often slightly misaligned on the number of awarded degrees by institution-CIP. When IPEDS has a higher value, we assume that remaining students are non-Pell but proportionately split by gender. When C.S. has a higher value, we scale down proportionately to the IPEDS number.

over time due to changes in Pell eligibility criteria, we follow the same procedure as in Appendix A.13 to adjust differences in major attainment by parental income using triennial median parental income measures from the NPSAS survey.

A.15 American Community Survey

We access survey responses to the American Community Survey using Ruggles et al. (2024). College major returns (in log annual wage earnings) are estimated using 2009–2011 and 2019–2021 male respondents between ages 31–35 with at least a high school degree across 8, 10, or 66 major categories; categorizations are available from the authors. All estimates employ sample weights. Data can be accessed at https://usa.ipums.org/usa/.

A.16 Blue Books

The *College Blue Books* were statistical records originally collected by Huber William Hurt, a professor of education at Columbia University. They include detailed institution-level information on universities across the United States. We digitize the Blue Books from the years 1923, 1928, 1933, 1939, 1947, and 1962, available through HathiTrust.

We impute undergraduate enrollment as the product between total enrollment (including graduate students) and the share of undergraduate degrees among all degrees (weighting Masters by half, since they take half as long to complete on average relative to undergraduate or doctoral degrees). Income per student is total annual income divided by total enrollment. The sticker price is the sum of tuition, fees, room, and board for each institution; the net price subtracts the product of tuition plus fees and the proportion of enrolled undergraduates who were reported to have scholarships. The Blue Books also identify two- and four-year institutions by whether or not they are junior colleges, and identify public institutions as those controlled by the federal, state, city, or other local government.

A.17 Tuition Data

We define the annual average enrollment-weighted sticker (or net) price of college attendance for each institution type as the tuition, fees, room, and board (less average total grant aid). The data presented in Figure 4b estimate this annually for 2-year public schools, 4-year public schools, and 4-year non-profit private schools.

Prior to 1960, we measure sticker and net tuition using the 1923–1947 Blue Books (see Appendix A.16). We restrict to universities in the 50 states and weight averages by undergraduate enrollment.

Two-year public college estimates between 1963 and 2022 are from the 2022 Digest of Educational Statistics Table 331.10 converted to 2022 dollars. All 1963 tuition, fees, room, and board data for 1963 are from the 2022 Digest of Educational Statistics Table 330.10, converted to 2022 dollars.

From 1970 onward, four-year tuition, fees, room and board cost estimates by institutional control are from College Board's Trends in College Pricing Table CP-2 from 1970 onward. The IPEDS tuition, fees, room, and board definition departs from the College Board series, which surveys colleges annually for the charges to full-time first-year undergraduate students over the course of a nine-month academic year of 30 semester hours or 45 quarter hours, weighting in and out of state + resident/commuter student tuitions within responding school, and then enrollment weighting across all respondents in each year. The IPEDS series, instead, enrollment weights all institutions' average charges for full-time students reported for the entire academic year.

We measure total annual undergraduate aid as the sum total of all state, institutional and private aid programs. We include Pell, FSEOG, LEAP, Academic Competitiveness grants, and SMART federal grant programs; veteran aid and tax benefits are not included. We source these categories from the College Board Trends in Student Aid 2023 Table 1 between 1970 and 1990 and use the 2002 College Board Trends in College Aid Appendix B figure for 1963, inflated back to 2022 dollars using College Board Trends in Student Pricing Table CP-A1 CPI converter. From 1990 to the present, we use undergraduate-specific statistics from College Board Trends in Student Aid 2023 Table 3. We also use this source to adjust prior years' total post-secondary aid. Specifically, we subtract the 1990 ratio of graduate to total post-secondary aid from each prior year.

We use the triennial 1987–2020 National Postsecondary Student Aid Study to measure average grant aid per student. In particular, we use TOTGRT for full-time undergraduate students enrolled in only one type of institution, including those receiving zero aid, split by level and control. For all other years, we linearly interpolate grant aid between NPSAS observations using annual enrollment shares by level and control weighted shares of total undergraduate aid (see below) and a linear interpolation of the two nearest NPSAS ratios of institution aid to 4-year private grant aid. Before 1987, we fix the NPSAS institution ratios at the 1987 level.

We measure total annual undergraduate enrollment using the total undergraduate fall enrollment by level and control from NCES Digest of Educational Statistics 2022 Table 303.70 for 1970, 1975, 1980, and every year from 1985 onward. In prior years, we linearly interpolate total undergraduate fall enrollment using total fall enrollment by level and control in each year via NCES Digest of Educational Statistics 2022 Table 303.10. The only exception is for 1988 and 1989 4-year non-profit private enrollment, which grows out the 4-year private enrollment total using the 4-year non-profit private share of 4-year private enrollment using 1987 and 1990 observation, respectively.

A.18 Ivy League Enrollment Data

Figure 4d presents annual undergraduate degree shares for each Ivy League university from a number of sources. First, we impute degree counts by gender from the 1923–1962 Blue Books (see Appendix A.16) as the product between total Bachelor's degrees awarded in a year and the share of enrolled students who were male or female. We do not measure total overall degree counts using the Blue Books.

Second, the *Earned Degrees Conferred by Higher Educational Institutions* were a series of annual reports issued by the Federal Security Agency Office of Education. We use reports from 1948, 1949, 1950, and 1952. These reports included detailed degree conferral counts from a near-census of roughly 1,300 universities, with conferrals broken down by gender, level, and field of study. We use the number of "Bachelor's and First Professional Degrees" awarded by each Ivy Plus institution (broken down by gender), divided by the total counts of these reported in each book. Since these are degree conferral shares, they are the most directly commensurable with the HEGIS and IPEDS datasets.

Third, *Opening Enrollment in Higher Education* was a series of institutional data reports released annually by the US Department of Health, Education, and Welfare. We use reports from 1958, 1961, 1963, 1966, and 1970. These reports include Fall enrollments of all degree-credit students across institutions, as well as enrollments of first-time degree credit students, both broken down by gender. We extract Ivy degree shares by dividing first-time Fall enrollment at each institution with the total first-time Fall enrollment listed in the books. We note that these shares should be interpreted a little differently from those of other sources, both because first-time enrollments include students who drop out and do not graduate (potentially biasing Ivy shares upward, since these schools tend to have higher completion rates), and because the denominator, total first-time enrollment, includes junior colleges, which is not true in some of our other data (biasing shares downward).

Fourth, *120 Years of American Education: A Statistical Portrait* is a book published by Thomas D. Snyder for the National Center for Education Statistics in 1993.[85] The book contains annualized enrollment and degree conferral records for all levels of education in the United States, dating back to 1869. We utilize these total counts as denominators to calculate Ivy Plus degree shares for data sources that do not include totals in their records.

Fifth, we use the Higher Education General Information Survey (HEGIS) – the federal precursor to IPEDS, to measure annual Ivy Plus and overall enrollment between 1966 and 1985 (omitting 1970) by gender.[86] We restrict to institutions in the 50 US states.

Finally, we use IPEDS to measure Ivy Plus enrollments by institution since 1984. See A.13.

There are several instances in which data for a given institution-year are either missing, unreadable, or clearly erroneous (more than ten times larger than in years immediately before or after). In these cases, we impute the missing values by estimating a linear regression of degree conferral over time for all observed (accurate) data points for that institution, and use the fitted value.

A.19 College Enrollment by Level

Figure A-20(a) displays US college enrollment separately for four- and two-year colleges over the past century. Where possible, we use NCES 2022 Digest of Educational Statistics Table 305.10 (found here), which contains the total fall enrollment of all first-time degree/certificate-seeking students in degree-granting postsecondary institutions by level. These series start in 1960 and

[85] We thank Lucas Marron and Joseph Altonji for suggesting this source,

[86] We thank Lucas Marron, John Eric Humphries, and Joseph Altonji for providing a unified HEGIS dataset that allows us to calculate degree shares by institution and gender for all sample years.

run continuously through 2021, with the exception of 1963. We extend these series back to 1931 using *120 Years of American Education: A Statistical Portrait* Table 24 (see this link. Before that period, we digitize two-year college enrollment from the 1944/46 Digest of Educational Statistics Table 3 and 1919 Department of the Interior Bureau of Education Bulletin 35 on junior colleges (for 1914-15 and 1915-16), available through here and here, respectively. We generate four-year enrollment as the difference between all college enrollment (via *120 Years of American Education: A Statistical Portrait* Table 24) and two-year college enrollment. These earlier data do not adjust for returning students as in the 2022 Digest of Educational Statistics and earlier sources, so we adjust the earlier data downward using the 1960s average ratio between the series for each level.

Appendix B: Robustness of Historical Record Linking

Automated record linking can induce meaningful biases in mobility estimates due to false positives and unrepresentative samples (Bailey et al., 2020; Ward, 2023). Further issues may arise due to mis-measurement of socioeconomic status in samples without income (Feigenbaum, 2015). Such issues may overstate social mobility if measurement error and false positives push intergenerational correlations towards 0. Since this paper investigates why mobility was higher in the past than now for college attendees, these biases are a major concern. This appendix examines whether the NYSIIS-standard linked, LIDO rank-income rank baseline correlation is an artifact of the specific linking method and status measures used. We demonstrate that the relatively progressivity of higher education in 1940 is consistent across linkage methods and status measures.

First, Table BB-1 estimates rank-rank correlations across several automated linking methods. Each panel uses a different linking method from either Abramitzky et al. (2022) or Helgertz et al. (2023) both with and without weights for 1940 representativeness. The sample is restricted in every case to men between the ages of 31 and 35 in 1940 who are observed living with their parents in 1920 and who have non-zero, non-missing incomes in 1940. Standard errors are robust. There are four correlations for each of these specification-linkage pairs: the overall correlation, high school graduates only, college enrollees who do not attain four years of college, and four-year college graduates. In each case, we find that college graduates' social mobility is more than twice as high as the overall population. The baseline measure has the lowest correlations, especially when linked, but estimates are close to each other. The linking method in the baseline sample, therefore, does not artificially induce high mobility between 1920 and 1940.

We next investigate the extent to which our choice of social status measures influences our results. Using our baseline NYSIIS standard linked sample of men ages 31 to 35 in 1940 living at home in 1920, we modify both the rank-rank form and status variables used. Table BB-2 shows that using the same type of measure for both fathers and sons, e.g. $OCCSCORE$ in both periods, does not alter the pattern of college inducing more mobility than in the present. We also are able to link some 1920 fathers forwards to 1940 to see a (much later) continuous measure of labor earnings. Again, the results are unchanged. Finally, we use these income data to conduct a log-log regression, and again find the same results. The type of socioeconomic status coding, therefore,

Table BB-1: Historical record linkage mobility robustness

	Unweighted				Weighted			
	All	HS	Some coll	Coll grad	All	HS	Some coll	Coll grad
A: NYSIIS standard								
Father rank	0.335***	0.166***	0.183***	0.151***	0.337***	0.170***	0.188***	0.154***
	(0.00113)	(0.00265)	(0.00404)	(0.00358)	(0.00117)	(0.00281)	(0.00431)	(0.00384)
R-sq	0.11	0.03	0.04	0.03	0.11	0.03	0.04	0.03
N	686,334	128,245	51,521	57,869	670,791	125,264	49,863	55,903
B: NYSIIS conservative								
Father rank	0.344***	0.175***	0.197***	0.160***	0.357***	0.182***	0.209***	0.168***
	(0.00139)	(0.00322)	(0.00489)	(0.00433)	(0.00142)	(0.00343)	(0.00529)	(0.00477)
R-sq	0.12	0.03	0.05	0.04	0.13	0.03	0.05	0.04
N	450,455	86,456	35,279	39,793	441,042	84,585	34,228	38,533
C: Exact standard								
Father rank	0.339***	0.171***	0.185***	0.156***	0.349***	0.178***	0.194***	0.162***
	(0.00120)	(0.00278)	(0.00422)	(0.00374)	(0.00122)	(0.00294)	(0.00450)	(0.00405)
R-sq	0.11	0.03	0.04	0.03	0.12	0.03	0.04	0.04
N	609,144	116,877	46,989	53,439	595,729	114,204	45,531	51,655
D: Exact conservative								
Father rank	0.340***	0.176***	0.189***	0.161***	0.364***	0.189***	0.207***	0.175***
	(0.00140)	(0.00321)	(0.00487)	(0.00431)	(0.00142)	(0.00343)	(0.00532)	(0.00481)
R-sq	0.11	0.03	0.04	0.04	0.13	0.04	0.05	0.04
N	448,626	88,120	35,745	40,725	439,526	86,244	34,717	39,465
E: MLP panel								
Father rank	0.336***	0.186***	0.204***	0.169***	0.367***	0.190***	0.212***	0.169***
	(0.00138)	(0.00328)	(0.00524)	(0.00497)	(0.00144)	(0.00346)	(0.00556)	(0.00539)
R-sq	0.11	0.03	0.05	0.04	0.14	0.04	0.05	0.04
N	480,070	946,57	33,509	33,772	464,621	91,389	32,190	32,219

Note: All men between 31 and 35 years old (inclusive) in 1940 reporting positive wage and salary income who lived with a parent with a positive LIDO in 1920 included. Some college refers to reporting between 12 and 16 years (not inclusive) of education in the 1940 census. College graduate applies to those reporting at least 16 years of education. Dependent variable is the son's rank of 1940 wage and salary income in this sample. Explanatory income is the rank of father's LIDO in 1920 in this sample. Standard errors are heteroskedasticity robust only. Linking methods classified by name cleaning method and age band required for uniqueness. Weights defined using son's 1-digit occupation category, region, and urban status, all interacted with an indicator for reporting race as Black. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Ruggles et al. (2024), Abramitzky et al. (2022), Helgertz et al. (2023), and Saavedra and Twinam (2020).

does not drive our historical motivating facts.

Finally, we provide evidence in Table BB-3 that our unweighted NYSIIS-standard linked sample is positively selected from the 1940 population of men aged 31 to 35. We use 1940 to demonstrate the extent to which our male samples are more educated and higher earning than their peers. Despite this positive selection, we demonstrate that re-weighting erases much of the difference. Because we find such similar mobility results in Table BB-1 Panel A regardless of weights, we believe our census linking-based results are unlikely to be driven solely by biases induced by linking method.

Table BB-2: Historical status measure mobility robustness

	All	HS	Some coll	Coll grad
A: Inc Rank - LIDO Rank				
Father LIDO Rank	0.337***	0.169***	0.188***	0.156***
	(0.00113)	(0.00271)	(0.00415)	(0.00371)
R-sq	0.11	0.03	0.04	0.03
N	686,334	128,245	51,521	57,869
B: Inc Rank - Inc Rank				
Father Income Rank	0.284***	0.154***	0.162***	0.111***
	(0.00335)	(0.00687)	(0.0108)	(0.00916)
R-sq	0.08	0.03	0.03	0.02
N	80,776	17,117	6,632	7,606
C: Occscore Rank - Occscore Rank				
Father Occscore Rank	0.381***	0.249***	0.197***	0.147***
	(0.00131)	(0.00300)	(0.00446)	(0.00373)
R-sq	0.09	0.04	0.03	0.02
N	928,550	167,389	66,561	77,490
D: Log Inc - Log Inc				
Log Father Income	0.140***	0.0623***	0.0856***	0.0765***
	(0.00305)	(0.00582)	(0.00868)	(0.00717)
R-sq	0.03	0.01	0.01	0.01
N	95,900	19,991	7,868	9,555

Note: All men between 31 and 35 years old (inclusive) in 1940 reporting positive wage and salary income who lived with a parent with a positive LIDO in 1920 included. Some college refers to reporting between 12 and 16 years (not inclusive) of education in the 1940 census. College graduate applies to those reporting at least 16 years of education. Dependent variable is measured for sons in 1940. Explanatory income is father's status measured in 1920 except for income, which is reported in 1940. Income is 1939 wage and salary income only. Ranks constructed within this sample. Standard errors are heteroskedasticity robust only. NYSIIS standard links used to follow all men over time. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Ruggles et al. (2024), Abramitzky et al. (2022), and Saavedra and Twinam (2020).

Appendix C: Longitudinal Changes in US Higher Education: Degrees, Faculty, and Courses

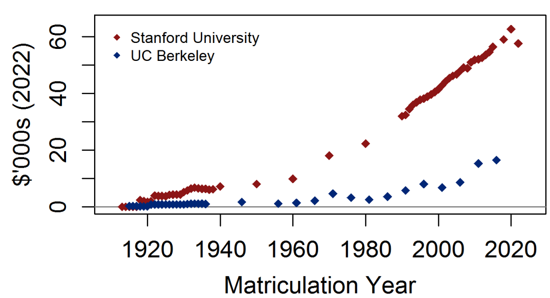
The dramatic growth of American higher education over the long span of the 20th century suggests that the fundamental character of US college-going could have sharply changed over this period. This appendix assesses whether US colleges experienced fundamental changes in this period that would prohibit reasonable and smooth estimation of changes over time in the relative value of college-going. We focus on two very different institutions – the University of California, Berkeley and Stanford University – that have evolved over the past 100 years and collect detailed records on degree completion, college costs, faculty teaching, and course availability. This detailed look into two California universities that complements the birds' eye view of higher education in the main text by demonstrating the same long-run trends hold even when holding cross-institutional

Table BB-3: Unweighted Balance Table for 1920–40 NYSIIS Standard Links

	Age	Occscore	Black	Urban	W/S	1(Own)	1(HS Grad)	1(8th Grade)	1(4yr Grad)
A: Baseline									
1(Matched)	0.0246	0.673	0.00708	0.000567	58.38	-0.0606	0.0251	0.00779	0.0115
	(0.00204)	(0.0164)	(0.000319)	(0.000720)	(1.440)	(0.000715)	(0.000667)	(0.000605)	(0.000373)
Constant	32.95	24.06	0.0490	0.552	1232.3	0.443	0.298	0.773	0.0679
	(0.00170)	(0.0136)	(0.000261)	(0.000601)	(1.195)	(0.000600)	(0.000552)	(0.000506)	(0.000304)
Observations	2,248,757	2,248,757	2,248,757	2,248,757	1,578,862	2,248,757	2,248,757	2,248,757	2,248,757
B: Weighted									
1(Matched)	0.0241	0.0490	0.00000101	-0.000775	48.11	-0.0645	0.0113	0.00465	0.00635
	(0.00206)	(0.0164)	(0.000308)	(0.000725)	(1.445)	(0.000718)	(0.000667)	(0.000610)	(0.000368)
Constant	32.95	24.06	0.0490	0.552	1232.3	0.443	0.298	0.773	0.0679
	(0.00170)	(0.0136)	(0.000261)	(0.000601)	(1.195)	(0.000600)	(0.000552)	(0.000506)	(0.000304)
Observations	2,248,757	2,248,757	2,248,757	2,248,757	1,578,862	2,248,757	2,248,757	2,248,757	2,248,757

Note: Standard errors are heteroskedasticity robust only.Topline sample includes all men between ages 31 and and 35 in 1940. Matched is a dummy variable for being in the ABE NYSIIS-Standard linked sample. The second panel weights based on 1940 characteristics, as in Table BB-1. Source: Ruggles et al. (2024) and Abramitzky et al. (2022).

Figure CC-1: California University Tuition, 1920–2020



Note: Real (CPI-adjusted) annual sticker tuition and fee costs for California-resident undergraduates at the University of California, Berkeley and students at Stanford University. Room and board, non-resident fees, and incidental costs are not included. Source: UC and Stanford course catalogues.

variation constant.[87] Though our description below focuses on faculty allocation, we emphasize that these characteristics hold true for (male) students and course enrollments as well.

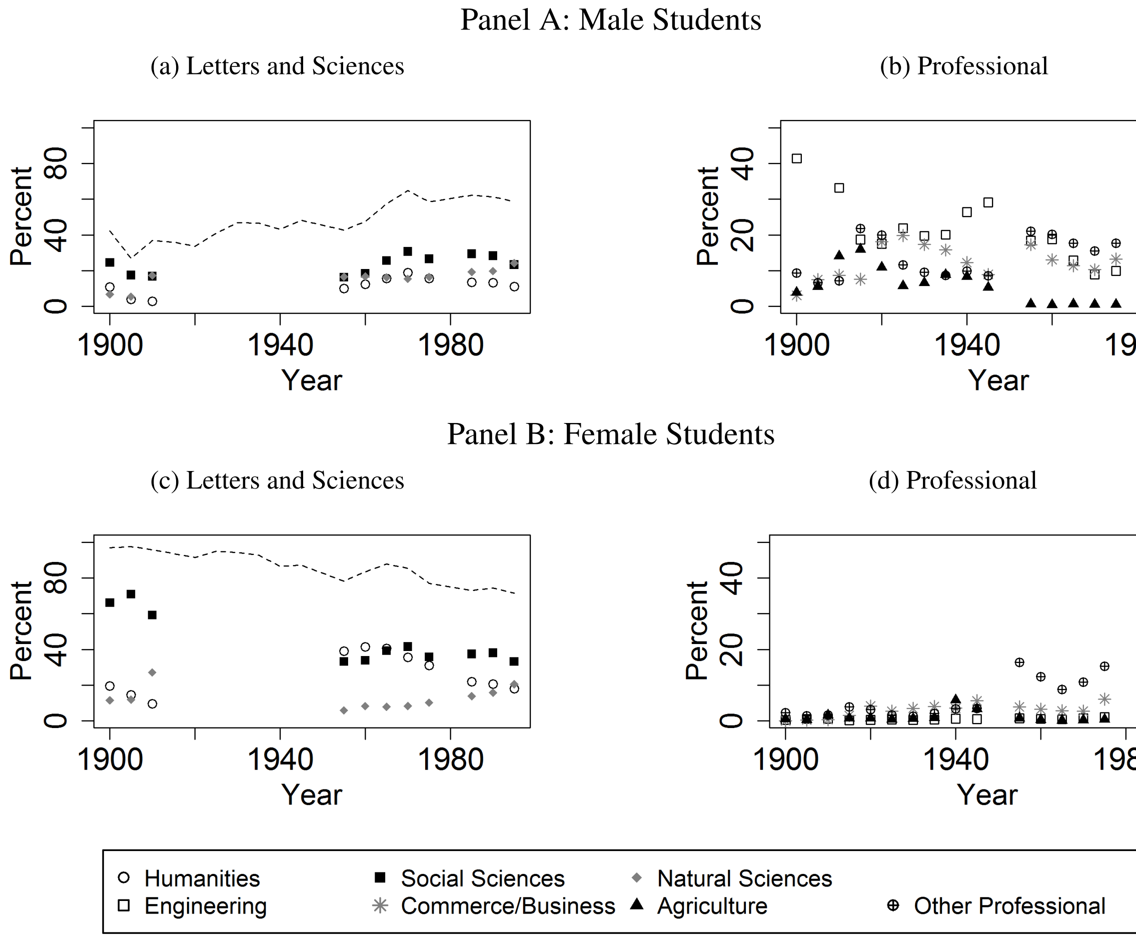
C.1 College Before World War II

The first period of our dataset spans the two decades before World War II. Low tuition rates likely contributed to the relatively even dispersion of college attendance across the socio-economic status distribution. University of California resident tuition was under 10 percent of state per capita personal income in 1929, and Stanford tuition was 25 percent.[88] One study found that half of college men and one-quarter of college women relied on wages during the school year to pay tuition

[87]Our analysis in this appendix is made possible by the student, faculty, and course records collected by the UC Cliometric Project (Bleemer, 2018).

[88]The average public co-ed institution charged \$63 in 1933, higher than the \$50 in California (Richtmyer and Willey, 1936). Per capita income from $CAPCPI$ available from the BEA through FRED.

Figure CC-2: UC Berkeley Degree Distribution in the 20th Century



Note: The annual share of undergraduate UC Berkeley and Stanford enrollees in a partition of university disciplines, averaged over five-year periods. The dotted line in panels (a) and (c) shows snnual measures of summed Letters and Sciences degree attainment. Five-year averages are reported prior to 1950; i.e. 1915 indicates the average of 1915–1919. “Other Professional” fields include fields like Education, Architecture, and Public Health. Sources: UC-CHP Course Database (Bleemer, 2018) and the UC Berkeley and Stanford University course catalogs.

at both private and land-grant schools in 1927, suggesting students could allay most financial constraints (Richtmyer and Willey, 1936).[89]

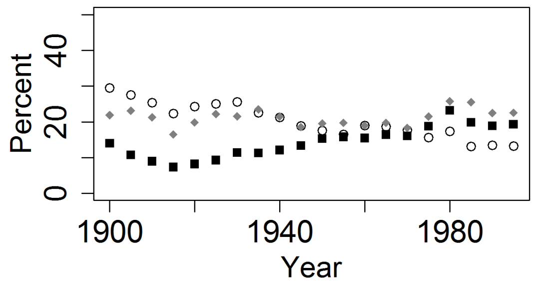
Once in college, students could expect to encounter an array of faculty specialities not dissimilar to what we see now (see Figure CC-3). The largest concentrations of faculty were in natural science and humanities over the entire 1900 to 1940 period, though humanities staffing fell fairly steadily from 1900 onward. Other disciplines experienced rapid changes over that time. Faculty shares in agriculture contracted as professional, engineering, and social sciences staffing rose steadily after 1920, reversing the trends of the prior decade. By 1940, half of faculty were in natural science and professional schools, as they are today.

[89]Employment included work for room and board, either with a family or the institution, undergraduate work at the university (on average $0.30 an hour). The most common occupations were clerks, teachers, and waiters (Greenleaf, 1939).

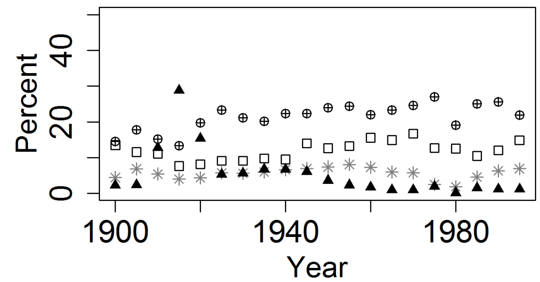
Figure CC-3: University Faculty Distribution in the 20^{th} Century

Panel A: Faculty Distribution at UC Berkeley

(a) Letters and Sciences

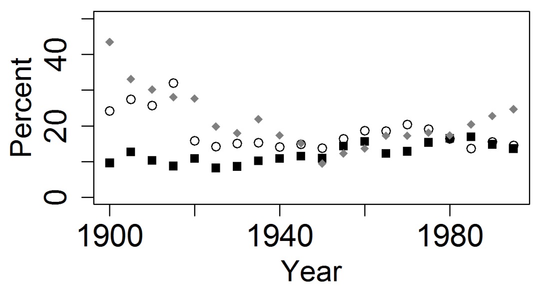


(b) Professional

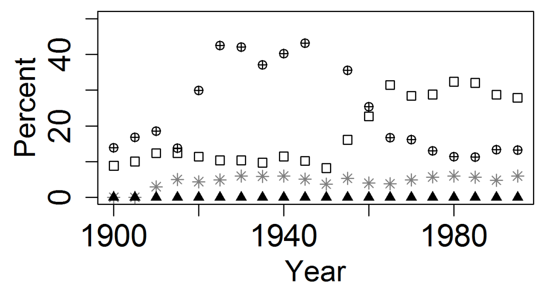


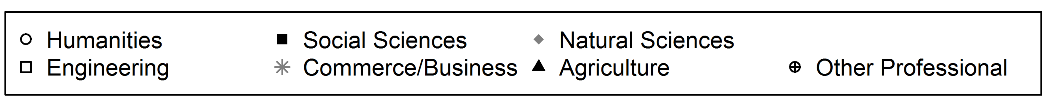
Panel B: Faculty Distribution at Stanford University

(c) Letters and Sciences



(d) Professional





Note: The annual share of undergraduate UC Berkeley instructors in a partition of university disciplines, averaged over five-year periods. Statistics are *not* weighted for student enrollment. Faculty include those teaching any course listed in either university's course catalog, restricted to courses taught in that year (in any term) numbered between 1 and 199 (which omits graduate student courses) and omitting physical education courses. "Other Professional" fields include fields like Education, Architecture, and Public Health. Sources: UC-CHP Course Database,(Bleemer, 2018) and UC Berkeley course catalogs.

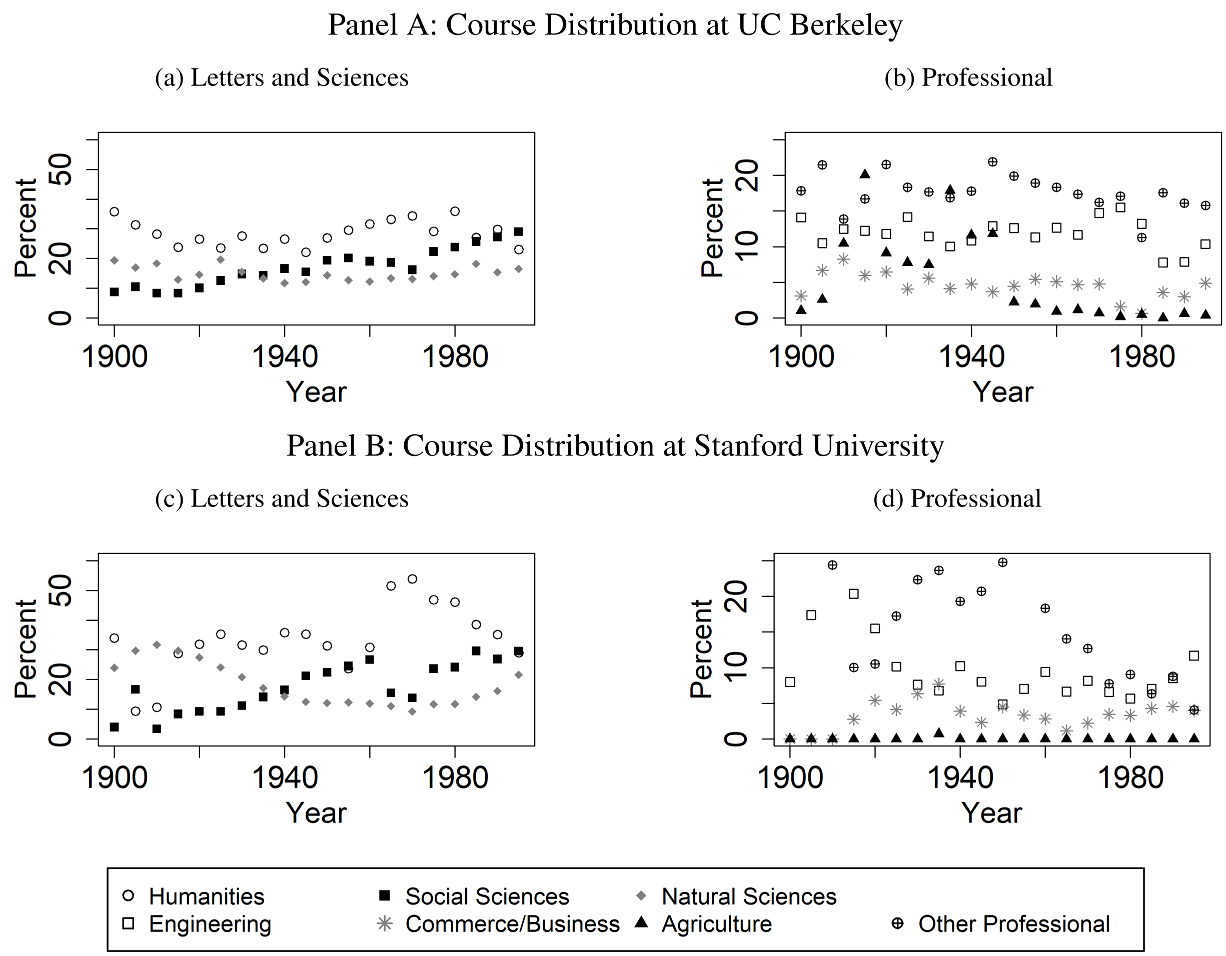
C.2 The Post-War Boom

There is no evidence rising post-war enrollment differences by parental status were due to rising college tuition. Figure CC-1 is flat in these decades at both public and private institutions. Though this figure only captures the direct cost of college attendance, it is clear that college-going differed by social class before more recent increases in tuition.

Faculty composition suggests that agriculture and humanities became less of a university priority between 1940 and 1960 as commerce degrees grew in importance. Staffing in business-related departments offset the fall in agriculture and humanities at UC Berkeley.[90] Figure CC-3 reveals that science, engineering, and professional schools were consistently popular in the 1940 to 1960

[90]During this period, the agriculture campus of US Berkeley, now UC Davis, became independent.

Figure CC-4: University Course Distribution in the 20^{th} Century



Note: The annual share of undergraduate UC Berkeley and Stanford courses taught in a partition of university disciplines, averaged over five-year periods. Statistics are *not* weighted for student enrollment. Courses include any course listed in either university's course catalog, restricted to courses taught in that year (in any term) numbered between 1 and 199 (which omits graduate student courses) and omitting physical education courses. "Other Professional" fields include fields like Education, Architecture, and Public Health. Sources: UC-CHP Course Database (Bleemer, 2018) and the UC Berkeley and Stanford University course catalogs.

window.

C.3 Divergence After 1960

In this period, tuition costs rose faster at Stanford than at UC Berkeley, though this also marked the beginning of federal college aid for non-veterans. There was a marked shift towards social and natural sciences in faculty, courses, and degrees. These faculty shares each rose by 50 percent, while humanities and agriculture faculty continued their decades-long fall. Natural sciences and professional staffing made up half of faculty by 1980, as they had for decades, but social science surpassed humanities to become the third largest group of faculty.

Table DD-1: 1963 Value-Added of Small Institutions, Aggregated to States

Institution	$	$ VA	N	Institution	$	$ VA	N
Other AL Univ.	-0.39	-0.18	21	Other VT Univ.	-0.18	-0.22	20
Other CA Univ.	-0.12	-0.14	115	Other WI Univ.	-0.02	-0.02	47
Other CT Univ.	-0.09	-0.13	21	Other CA Comm. Coll.	-0.04	-0.03	98
Other FL Univ.	-0.20	-0.14	31	Other CT Comm. Coll.	-0.19	-0.19	27
Other GA Univ.	-0.01	-0.04	36	Other FL Comm. Coll.	-0.18	-0.23	55
Other IA Univ.	0.08	0.09	35	Other GA Comm. Coll.	-0.10	-0.15	53
Other IL Univ.	-0.67	-0.64	48	Other IA Comm. Coll.	-0.31	-0.34	47
Other IN Univ.	-0.61	-0.64	28	Other IL Comm. Coll.	-0.09	-0.09	78
Other KS Univ.	0.61	0.53	22	Other MA Comm. Coll.	-0.30	-0.30	50
Other KY Univ.	-0.25	-0.25	23	Other MD Comm. Coll.	-0.17	-0.05	23
Other MA Univ.	-0.52	-0.46	50	Other MI Comm. Coll.	-0.35	-0.36	51
Other MD Univ.	-0.57	-0.51	31	Other MO Comm. Coll.	-0.34	-0.32	34
Other MI Univ.	0.11	0.07	57	Other NC Comm. Coll.	-0.15	-0.12	44
Other MO Univ.	-0.25	-0.22	67	Other NJ Comm. Coll.	0.00	0.02	29
Other NC Univ.	-0.22	-0.18	25	Other NY Comm. Coll.	-0.17	-0.15	57
Other NY Univ.	-0.09	-0.13	81	Other OH Comm. Coll.	-0.04	-0.08	26
Other OH Univ.	-0.20	-0.23	50	Other OK Comm. Coll.	-0.07	-0.05	39
Other OK Univ.	-0.49	-0.43	20	Other OR Comm. Coll.	-0.31	-0.33	22
Other PA Univ.	-0.09	-0.11	67	Other PA Comm. Coll.	-0.29	-0.28	40
Other TN Univ.	-0.20	-0.20	40	Other TX Comm. Coll.	-0.27	-0.25	64
Other TX Univ.	-0.07	-0.08	58	Other WA Comm. Coll.	0.00	-0.06	33
Other VA Univ.	-0.11	-0.09	34				

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each set of four- and four-year institutions (aggregated by state across institutions with fewer than 11 employed enrollees) with at least 11 employed male enrollees in the Project Talent database, and the number of (weighted) respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured academic aptitude rank and parental income rank and race indicators. University names and states from matched universities in IPEDS.

Source: Project Talent and IPEDS.

C.4 Gaps Widen, 1980 to today

Unlike earlier decades, this divergence in college enrollment attendance came in a period of rapidly rising college costs in every sector in Figure CC-1. Net college costs have grown sharply in every sector since 1980. They rose first at 4-year private schools, doubling between 1980 and 2000, then 4-year public schools, doubling between 2000 and 2010). These attendance costs outpaced income growth: for instance Stanford's per-year tuition is now 75 percent of California's per capita personal income.

Figure CC-3 shows that commerce and engineering degrees have surged to their mid-century peaks in the years following 1980. In contrast, natural science and professional school faculty shares are approximately where they were before World War II. Continuing on their respective century-long trajectories, social science faculty shares are higher than they have ever been while humanities staffing is now at its lowest point. As a result, almost two-thirds of faculty are now in natural or social science, engineering, or business fields, compared to half in 1920.

Table DD-2: 1963 Value-Added of Public Universities 1

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Auburn Univ.	AL	0.05	0.01	129	Iowa State Univ.	IA	-0.01	-0.01	106
Jacksonville State Univ.	AL	-0.03	-0.09	33	Univ. of Iowa	IA	-0.58	-0.63	69
The Univ. of Alabama	AL	0.07	0.04	76	Univ. of Northern Iowa	IA	-0.20	-0.19	39
Troy Univ.	AL	-0.10	-0.14	26	Univ. of Idaho	ID	-0.13	-0.13	30
Univ. of AL-Birmingham	AL	0.20	0.18	22	Eastern Illinois Univ.	IL	0.11	0.05	43
Univ. of Montevallo	AL	-0.29	-0.31	20	Illinois State Univ.	IL	-0.20	-0.20	65
Arkansas State Univ.	AR	-0.31	-0.32	30	Northern Illinois Univ.	IL	0.06	0.03	110
Southern Arkansas Univ.	AR	-0.29	-0.27	42	South. IL U.-Edwardsville	IL	-0.28	-0.30	200
Univ. of Arkansas	AR	-0.08	-0.12	42	Univ. of Illinois Chicago	IL	0.03	0.02	312
ASU-Campus Immersion	AZ	0.12	0.08	49	Western Illinois Univ.	IL	-0.25	-0.26	100
Northern Arizona Univ.	AZ	-1.07	-1.15	42	Ball State Univ.	IN	-0.15	-0.16	127
Univ. of Arizona	AZ	-0.19	-0.11	73	IU-Bloomington	IN	-0.08	-0.11	263
Cal State Poly-Pomona	CA	0.19	0.16	43	Purdue Univ.	IN	0.24	0.19	163
CSU-Chico	CA	-0.17	-0.17	51	Purdue Univ. Northwest	IN	0.12	0.10	41
CSU-Fresno	CA	-0.10	-0.07	28	Fort Hays State Univ.	KS	-0.13	-0.15	58
CSU-Fullerton	CA	-0.30	-0.33	22	Kansas State Univ.	KS	-0.19	-0.19	158
CSU-Long Beach	CA	0.00	0.00	87	Pittsburg State Univ.	KS	-0.15	-0.14	32
CSU-Los Angeles	CA	0.03	0.01	81	Univ. of Kansas	KS	-0.37	-0.42	98
CSU-Sacramento	CA	-0.23	-0.27	30	Washburn Univ.	KS	0.35	0.38	23
CSU-San Bernardino	CA	-0.17	-0.18	96	Wichita State Univ.	KS	-0.13	-0.15	37
San Diego State Univ.	CA	-0.39	-0.41	93	Eastern Kentucky Univ.	KY	0.21	0.20	58
San Francisco State Univ.	CA	-0.07	-0.08	82	Morehead State Univ.	KY	-0.16	-0.16	46
San Jose State Univ.	CA	-0.02	-0.01	78	Murray State Univ.	KY	-0.09	-0.05	50
Shasta Coll.	CA	0.06	0.08	30	Univ. of Louisville	KY	-0.17	-0.20	34
UC-Berkeley	CA	0.02	0.00	66	Western Kentucky Univ.	KY	0.08	0.11	73
UC-Los Angeles	CA	0.17	0.25	81	Louisiana Tech Univ.	LA	0.11	0.06	25
UC-Santa Barbara	CA	0.14	0.11	20	Nicholls State Univ.	LA	-0.32	-0.34	31
Colorado S. Univ.-Fort Collins	CO	-0.20	-0.19	80	Univ. of LA-Lafayette	LA	-0.03	-0.03	111
Comm. Coll. of Denver	CO	-0.23	-0.28	21	Univ. of LA-Monroe	LA	0.32	0.39	39
Met. State Univ. of Denver	CO	-0.36	-0.36	36	Salem State Univ.	MA	-0.15	-0.18	32
United States Air Force Acad.	CO	0.31	0.22	29	UMass-Amherst	MA	-0.20	-0.21	113
Univ. of Colorado Boulder	CO	-0.03	-0.06	176	UMass-Boston	MA	-0.08	-0.11	63
Univ. of Northern Colorado	CO	-0.16	-0.20	49	Morgan State Univ.	MD	-0.21	-0.08	57
Central Connecticut S. Univ.	CT	-0.01	-0.02	89	St. Mary's Coll. of MD	MD	-0.53	-0.54	43
Univ. of Connecticut	CT	-0.15	-0.15	99	Towson Univ.	MD	-0.28	-0.29	60
Univ. of Delaware	DE	-0.23	-0.24	39	United States Naval Acad.	MD	0.06	-0.01	48
Florida Ag. and Mech. Univ.	FL	-0.27	-0.09	25	Univ. of Baltimore	MD	0.11	0.07	215
Florida State Univ.	FL	-0.04	-0.08	125	Univ. of MD-Coll. Park	MD	-0.16	-0.13	23
Pensacola State Coll.	FL	0.03	0.02	118	Univ. of MD-Global Campus	MD	-0.09	-0.08	39
Santa Fe Coll.	FL	-0.11	-0.13	31	Univ. of Maine	ME	-0.22	-0.25	56
The Univ. of West Florida	FL	-1.10	-1.15	60	Univ. of Maine at Augusta	ME	-0.17	-0.21	98
Univ. of Florida	FL	0.01	-0.03	183	Central Michigan Univ.	MI	-0.13	-0.14	81
Univ. of South Florida	FL	-0.05	-0.06	31	Eastern Michigan Univ.	MI	-0.02	-0.00	73
Georgia Institute of Tech.	GA	0.16	0.11	65	Ferris State Univ.	MI	0.08	0.01	46
Georgia Southern Univ.	GA	0.12	0.08	28	Henry Ford Coll.	MI	0.11	0.11	27
Georgia State Univ.	GA	-0.03	0.03	51	Michigan State U.	MI	-0.21	-0.24	274
Univ. of Georgia	GA	-0.37	-0.38	98	UMich-Ann Arbor	MI	0.15	0.10	244
Univ. of Hawaii at Hilo	HI	-0.16	-0.08	164	Wayne State Univ.	MI	-0.01	-0.05	309

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each public four-year institution (continued in Table DD-3) with at least 11 employed male enrollees in the Project Talent database, and the number of (weighted) respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured academic aptitude rank and parental income rank and race indicators. University names and states from matched universities in IPEDS.

Source: Project Talent and IPEDS.

Appendix D: Institutional Value-Added in 1963

Tables DD-1 to DD-1 present the relative average wage, average value-added, and (weighted) number of enrolled students observed and estimated in the Project Talent dataset. The estimates and counts are restricted to men and include the 523 last-enrollment institutions with at least 11 employed respondents, with wages measured at age 29. Smaller institutions are aggregated to the state and sector (four- or two-year).

Table DD-3: 1963 Value-Added of Public Universities 2

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Western Michigan Univ.	MI	-0.10	-0.11	111	East Central Univ.	OK	-0.45	-0.43	23
Bemidji State Univ.	MN	-0.47	-0.49	42	Northeastern State Univ.	OK	-0.10	-0.17	59
MN State Univ.-Mankato	MN	-0.15	-0.12	84	OK State Univ.-OK City	OK	-0.07	-0.10	176
MN State Univ. Moorhead	MN	-0.26	-0.26	41	Univ. of OK-Norman	OK	-0.19	-0.22	119
Saint Cloud State Univ.	MN	0.22	0.19	55	Eastern Oregon Univ.	OR	-0.31	-0.30	23
Univ. of MN-Duluth	MN	-0.06	-0.08	299	Oregon State Univ.	OR	-0.02	-0.03	79
Winona State Univ.	MN	-0.27	-0.29	22	Portland State Univ.	OR	-0.21	-0.17	65
Northwest MO State Univ.	MO	-0.03	-0.01	44	Bloomsburg U. of PA	PA	-0.23	-0.25	40
Southeast MO State Univ.	MO	-0.30	-0.28	56	Cal. Univ. of PA	PA	-0.12	-0.13	29
Truman State Univ.	MO	-0.27	-0.24	45	Clarion Univ. of PA	PA	-0.09	-0.11	41
Univ. of Central MO	MO	0.00	0.00	132	East Stroudsburg U. of PA	PA	-0.17	-0.17	34
Univ. of MO-Columbia	MO	0.05	0.03	322	Edinboro Univ. of PA	PA	-0.32	-0.36	25
MS State Univ.	MS	-0.07	-0.11	70	Indiana Univ. of PA	PA	-0.05	-0.04	123
Univ. of MS	MS	0.08	0.03	68	Kutztown U. of PA	PA	-0.70	-0.71	22
Univ. of Southern MS	MS	0.12	0.08	94	Millersville Univ. of PA	PA	-0.56	-0.57	85
Montana State Univ.	MT	0.03	0.04	97	Pennsylvania State Univ.	PA	-0.20	-0.21	239
Montana State Univ. Billings	MT	-0.13	-0.14	120	Temple Univ.	PA	0.02	0.04	98
U. of Montana	MT	-0.39	-0.42	89	U. of Pitt.-Bradford	PA	0.05	0.02	108
Appalachian State Univ.	NC	-0.11	-0.12	27	West Chester U. of PA	PA	-0.19	-0.23	30
East Carolina Univ.	NC	-0.21	-0.22	56	Clemson Univ.	SC	-0.08	-0.09	48
NC A & T State Univ.	NC	-0.04	-0.03	91	Lander Univ.	SC	0.02	-0.03	46
U. of NC-Chapel Hill	NC	-0.01	-0.05	88	U. of SC-Columbia	SC	0.07	0.03	74
U. of NC-Pembroke	NC	-0.28	-0.29	26	Northern State Univ.	SD	-0.19	-0.19	22
Western Carolina Univ.	NC	0.00	-0.06	21	South Dakota State Univ.	SD	-0.06	-0.03	51
North Dakota State Univ.	ND	0.02	0.09	26	East TN State Univ.	TN	-0.21	-0.20	30
Univ. of North Dakota	ND	-0.16	-0.17	46	Middle TN State Univ.	TN	-0.24	-0.27	43
Chadron State Coll.	NE	-0.02	-0.03	26	TN Tech. Univ.	TN	-0.03	-0.05	43
Univ. of Nebraska-Lincoln	NE	-0.10	-0.11	210	U. of TN-Chattanooga	TN	-0.05	-0.03	27
Univ. of Nebraska at Kearney	NE	-0.20	-0.19	23	U. of TN-Knoxville	TN	-0.04	-0.04	157
Univ. of Nebraska at Omaha	NE	-0.18	-0.17	79	U. of TN-Martin	TN	0.14	0.16	26
Wayne State Coll.	NE	-0.29	-0.33	27	Lamar Univ.	TX	-0.02	-0.06	209
Univ. of New Hampshire	NH	-0.19	-0.23	64	Sam Houston State Univ.	TX	-0.15	-0.19	56
New Jersey Institute of Tech.	NJ	0.21	0.17	20	San Jacinto Comm. Coll.	TX	-0.13	-0.08	28
Rutgers Univ.-New Brunswick	NJ	0.04	0.03	97	Stephen F Austin S. U.	TX	-0.19	-0.20	38
The Coll. of New Jersey	NJ	-0.28	-0.28	47	TX A & M U.-Commerce	TX	-0.18	-0.19	133
Eastern New Mexico Univ.	NM	-0.15	-0.16	25	TX A & M U.-Kingsville	TX	0.15	0.18	26
Univ. of New Mexico	NM	-0.43	-0.44	69	TX State Univ.	TX	-0.09	-0.14	37
Western New Mexico Univ.	NM	-0.08	-0.04	36	TX Tech Univ.	TX	-0.01	-0.05	138
Coll. of Southern Nevada	NV	-0.29	-0.32	52	U. of TX-Arlington	TX	0.00	0.02	57
Univ. of Nevada-Reno	NV	0.56	0.46	29	Univ. of Houston	TX	0.15	0.13	96
Binghamton Univ.	NY	-0.21	-0.23	36	West TX A & M Univ.	TX	0.08	0.06	37
CUNY Baruch Coll.	NY	0.08	0.08	61	Southern Utah Univ.	UT	0.07	0.09	119
CUNY Brooklyn Coll.	NY	0.12	0.10	214	Univ. of Utah	UT	0.20	0.16	31
CUNY City Coll.	NY	0.22	0.21	297	Old Dominion Univ.	VA	0.00	-0.03	217
CUNY Hunter Coll.	NY	0.15	0.14	152	Univ. of Virginia	VA	0.15	0.16	85
CUNY John Jay C. of Crim. Just.	NY	0.15	0.14	33	VA Poly. Ins. and State U.	VA	0.05	0.03	95
CUNY NY Coll. of Tech.	NY	0.01	0.03	75	William & Mary	VA	-0.20	-0.21	27
Farmingdale State Coll.	NY	0.32	0.20	25	Univ. of Vermont	VT	-0.43	-0.42	42
SUNY-New Paltz	NY	-0.21	-0.19	24	Central WA Univ.	WA	-0.27	-0.28	37
SUNY at Albany	NY	-0.15	-0.15	59	Eastern WA Univ.	WA	0.05	0.01	48
SUNY at Fredonia	NY	-0.29	-0.27	32	U. of WA-Seattle Campus	WA	0.01	-0.01	154
SUNY Buffalo State	NY	-0.21	-0.24	136	Walla Walla Comm. Coll.	WA	0.02	0.02	39
SUNY Coll. at Oswego	NY	-0.32	-0.34	45	WA State Univ.	WA	0.01	-0.05	68
SUNY Coll. at Plattsburgh	NY	-0.73	-0.74	30	Western WA Univ.	WA	-0.64	-0.67	38
SUNY Coll. at Potsdam	NY	-0.09	-0.11	33	Yakima Valley Coll.	WA	-0.31	-0.31	24
SUNY Cortland	NY	-0.19	-0.22	37	U. of WI-Madison	WI	-0.15	-0.16	423
SUNY Oneonta	NY	-0.12	-0.12	40	U. of WI-Oshkosh	WI	-0.23	-0.24	31
United States Military Acad.	NY	0.13	0.04	34	U. of WI-Platteville	WI	-0.04	-0.05	56
Bowling Green S. Univ.-Firelands	OH	-0.11	-0.14	149	U. of WI-River Falls	WI	-0.27	-0.28	47
Central State Univ.	OH	-0.36	-0.39	94	U. of WI-Stevens Point	WI	-0.09	-0.14	22
Cleveland State Univ.	OH	-0.02	-0.06	131	U. of WI-Stout	WI	-0.11	-0.10	41
Kent State Univ. at Kent	OH	-0.08	-0.09	274	U. of WI-Whitewater	WI	-0.15	-0.21	58
Miami Univ.-Oxford	OH	-0.11	-0.12	67	Concord Univ.	WV	-0.16	-0.12	26
Ohio State Univ.-Marion Campus	OH	-0.09	-0.12	362	Marshall Univ.	WV	-0.07	-0.06	41
Ohio State Univ.-Newark Campus	OH	-0.06	-0.04	26	Shepherd Univ.	WV	-0.05	-0.06	20
Ohio Univ.	OH	-0.11	-0.15	160	West Liberty Univ.	WV	-0.06	-0.07	31
Univ. of Akron	OH	-0.10	-0.12	183	West Virginia Univ.	WV	0.06	0.04	58
Univ. of Cincinnati	OH	-0.04	-0.09	69	WV U. Inst. of Tech.	WV	-0.02	-0.06	26
Univ. of Toledo	OH	-0.13	-0.17	43	Univ. of Wyoming	WY	-0.18	-0.19	26
Youngstown State Univ.	OH	-0.12	-0.15	97					

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each public four-year institution (continued from Table DD-2) with at least 11 employed male enrollees in the Project Talent database, and the number of (weighted) respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured academic aptitude rank and parental income rank and race indicators. University names and states from matched universities in IPEDS.

Source: Project Talent and IPEDS.

Table DD-4: 1963 Value-Added of Private Universities 1

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Samford Univ.	AL	-0.28	-0.29	29	Fairleigh Dick. U.-Florham	NJ	-0.57	-0.58	73
Tuskegee Univ.	AL	-0.31	-0.15	31	Princeton Univ.	NJ	0.04	0.02	47
Chapman Univ.	CA	-0.22	-0.08	20	Rider Univ.	NJ	-0.00	-0.01	46
Stanford Univ.	CA	-0.08	-0.11	38	Seton Hall Univ.	NJ	-0.21	-0.24	22
U. of San Francisco	CA	-0.60	-0.70	22	Adelphi Univ.	NY	-0.01	-0.08	40
U. of Southern California	CA	0.16	0.16	41	Alfred Univ.	NY	0.07	-0.03	26
U. of Denver	CO	-0.09	-0.10	51	Clarkson Univ.	NY	0.25	0.17	22
U. of Bridgeport	CT	0.05	-0.05	27	Columbia University	NY	-0.22	-0.17	70
U. of Hartford	CT	-0.10	-0.10	51	Cornell Univ.	NY	0.01	-0.03	91
Yale Univ.	CT	0.05	-0.00	42	Fordham Univ.	NY	-0.22	-0.23	39
American Univ.	DC	0.09	0.06	38	Hofstra Univ.	NY	0.09	0.04	47
George Washington Univ.	DC	-0.08	-0.11	40	Iona Coll.	NY	0.06	0.07	46
Georgetown Univ.	DC	0.10	0.06	30	Long Island Univ.	NY	0.04	-0.02	140
Trinity Washington Univ.	DC	0.08	0.06	20	Manhattan Coll.	NY	0.21	0.17	35
Univ. of Miami	FL	0.22	0.19	30	Marist Coll.	NY	-0.03	-0.10	21
Emory Univ.	GA	0.07	0.07	27	New York Institute of Tech.	NY	-0.10	-0.14	42
Drake Univ.	IA	-0.18	-0.17	22	New York Univ.	NY	-0.14	-0.12	161
Bradley Univ.	IL	0.14	0.11	31	Pace Univ.	NY	0.06	0.05	84
DePaul Univ.	IL				Pratt Institute-Main	NY	0.02	-0.05	27
Elmhurst Univ.	IL	-0.03	-0.05	37	Rensselaer Poly. Institute	NY	-0.08	-0.05	35
Illinois Institute of Tech.	IL	0.24	0.20	48	Rochester Institute of Tech.	NY	-0.22	-0.28	42
Roosevelt Univ.	IL	0.07	0.07	39	St Bernard's S. of Theology	NY	-0.25	-0.27	27
Univ. of Chicago	IL	0.04	0.05	22	Univ. of Rochester	NY	-0.17	-0.20	43
Butler Univ.	IN	-0.01	-0.03	39	Utica Univ.	NY	-0.16	-0.18	88
DePauw Univ.	IN	0.14	0.07	21	Wagner Coll.	NY	-0.09	-0.12	20
Goshen Coll.	IN	-0.18	-0.19	25	Ashland Univ.	OH	0.20	0.15	30
Trine Univ.	IN	-0.25	-0.27	26	Baldwin Wallace Univ.	OH	-0.23	-0.24	33
Univ. of Evansville	IN	-0.06	-0.03	84	Capital Univ.	OH	-0.22	-0.15	22
Univ. of Indianapolis	IN	-0.10	-0.13	28	Case Western Reserve Univ.	OH	0.15	0.12	73
Univ. of Notre Dame	IN	0.08	-0.00	69	John Carroll Univ.	OH	-0.05	-0.07	48
Valparaiso Univ.	IN	-0.20	-0.23	44	Oberlin Coll.	OH	-0.39	-0.40	24
Southwestern Coll.	KS	-0.17	-0.18	69	Univ. of Dayton	OH	-0.46	-0.53	41
Univ. of Pikeville	KY	-0.28	-0.31	41	Wittenberg Univ.	OH	-0.50	-0.62	22
Univ. of the Cumberlands	KY	-0.14	-0.12	32	Oklahoma City Univ.	OK	-0.23	-0.25	21
Loyola Univ. New Orleans	LA	-0.00	-0.01	61	Southwestern Christian U.	OK	-0.17	-0.14	27
American Int. Coll.	MA	-0.06	-0.08	21	Univ. of Tulsa	OK	0.10	0.08	123
Bentley Univ.	MA	0.27	0.27	28	Bucknell Univ.	PA	0.09	0.00	29
Boston Coll.	MA	-0.01	-0.05	58	Carnegie Mellon Univ.	PA	0.15	0.14	49
Boston Univ.	MA	0.14	0.10	129	Drexel Univ.	PA	-0.02	-0.06	38
Coll. of the Holy Cross	MA	0.09	0.02	30	Duquesne Univ.	PA	-0.03	-0.09	68
Harvard Univ.	MA	0.04	0.00	70	Elizabethtown Coll.	PA	-0.04	-0.07	28
Massachusetts Institute of Tech.	MA	0.23	0.21	41	Franklin and Marshall Coll.	PA	-0.12	-0.13	33
Northeastern Univ.	MA	-0.01	-0.02	239	Gannon Univ.	PA	0.00	-0.00	21
Suffolk Univ.	MA	-0.22	-0.26	20	Geneva Coll.	PA	-0.06	-0.05	75
Tufts Univ.	MA	0.09	0.07	34	Grove City Coll.	PA	0.28	0.28	24
Wentworth Institute of Tech.	MA	0.05	0.03	32	King's Coll.	PA	-0.20	-0.19	29
Wheaton Coll. (Massachusetts)	MA	0.02	0.06	24	Lehigh Univ.	PA	0.11	0.05	38
Johns Hopkins Univ.	MD	0.11	0.10	42	Point Park Univ.	PA	-0.13	-0.17	20
Loyola Univ. Maryland	MD	0.03	0.01	21	Univ. of Pennsylvania	PA	-0.03	-0.06	75
Husson Univ.	ME	-0.20	-0.15	39	Univ. of Scranton	PA	0.08	0.05	33
Alma Coll.	MI	-0.06	-0.10	23	Villanova Univ.	PA	0.05	0.02	21
Kettering Univ.	MI	-0.26	-0.32	27	Widener Univ.	PA	-0.07	-0.08	39
Lawrence Tech. Univ.	MI	0.02	-0.02	45	Wilkes Univ.	PA	-0.17	-0.18	39
Univ. of Detroit Mercy	MI	-0.04	0.07	59	Brown Univ.	RI	0.18	0.15	29
Saint Johns Univ.	MN	-0.03	-0.05	85	Providence Coll.	RI	0.19	0.15	21
St Olaf Coll.	MN	0.19	0.18	26	Vanderbilt Univ.	TN	0.01	-0.09	23
Rockhurst Univ.	MO	-0.24	-0.26	21	Baylor Univ.	TX	-0.08	-0.09	45
Saint Louis Univ.	MO	0.02	0.01	38	Rice Univ.	TX	0.16	0.15	215
Washington Univ. in St Louis	MO	-0.11	-0.10	147	Southern Methodist Univ.	TX	0.33	0.27	50
William Jewell Coll.	MO	0.21	0.15	31	St. Mary's Univ.	TX	-0.12	-0.13	40
Campbell Univ.	NC	-0.41	-0.40	26	Texas Christian Univ.	TX	-0.71	-0.75	27
Duke Univ.	NC	0.03	-0.02	40	Trinity Univ.	TX	0.10	0.09	36
Guilford Coll.	NC	-0.02	-0.05	29	Brigham Young Univ.	UT	-0.20	-0.22	196
Queens Univ. of Charlotte	NC	0.04	0.01	208	Gonzaga Univ.	WA	-0.22	-0.24	62
Wake Forest Univ.	NC	0.02	-0.03	27	Seattle Univ.	WA	-0.22	-0.25	22
Concordia Univ.-Nebraska	NE	-0.21	-0.19	27	Univ. of Puget Sound	WA	-0.30	-0.33	22
Dartmouth Coll.	NH	0.02	-0.01	38	Marquette Univ.	WI	0.15	0.10	67
Saint Anselm Coll.	NH	-0.16	-0.17	20					

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each private four-year institution (continued in Table DD-5) with at least 11 employed male enrollees in the Project Talent database, and the number of (weighted) respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured academic aptitude rank and parental income rank and race indicators. University names and states from matched universities in IPEDS.

Source: Project Talent and IPEDS.

Table DD-5: 1963 Value-Added of Private Universities 2

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Carnegie Mellon Univ.	PA	0.15	0.14	42	Brown Univ.	RI	0.18	0.15	31
Drexel Univ.	PA	-0.02	-0.06	66	Bryant Univ.	RI	-0.07	-0.12	11
Duquesne Univ.	PA	-0.03	-0.09	57	Providence Coll.	RI	0.19	0.15	38
Elizabethtown Coll.	PA	-0.04	-0.07	24	Wofford Coll.	SC	-0.21	-0.25	11
Franklin and Marshall Coll.	PA	-0.12	-0.13	14	Lincoln Memorial Univ.	TN	-0.15	-0.17	25
Gannon Univ.	PA	0.00	-0.00	11	Vanderbilt Univ.	TN	0.01	-0.09	11
Geneva Coll.	PA	-0.06	-0.05	96	Baylor Univ.	TX	-0.08	-0.09	14
Gettysburg Coll.	PA	-0.13	-0.22	18	Rice Univ.	TX	0.16	0.15	161
Grove City Coll.	PA	0.28	0.28	24	Southern Methodist Univ.	TX	0.33	0.27	29
King's Coll.	PA	-0.20	-0.19	12	St. Mary's Univ.	TX	-0.12	-0.13	18
La Salle Univ.	PA	0.28	0.20	14	Texas Christian Univ.	TX	-0.71	-0.75	45
Lafayette Coll.	PA	-0.06	-0.13	16	Texas Wesleyan Univ.	TX	-0.28	-0.27	35
Lehigh Univ.	PA	0.11	0.05	15	Trinity Univ.	TX	0.10	0.09	11
Point Park Univ.	PA	-0.13	-0.17	29	Brigham Young Univ.	UT	-0.20	-0.22	288
Saint Vincent Coll.	PA	-1.08	-1.19	17	Middlebury Coll.	VT	-0.12	-0.18	13
Univ. of Pennsylvania	PA	-0.03	-0.06	80	Gonzaga Univ.	WA	-0.22	-0.24	54
Univ. of Scranton	PA	0.08	0.05	51	Seattle Univ.	WA	-0.22	-0.25	17
Ursinus Coll.	PA	-0.19	-0.26	11	Univ. of Puget Sound	WA	-0.30	-0.33	23
Villanova Univ.	PA	0.05	0.02	13	Marquette Univ.	WI	0.15	0.10	54
Washington & Jefferson C.	PA	0.07	0.10	17	Milwaukee Sch. of Eng.	WI	-0.05	-0.06	93
Widener Univ.	PA	-0.07	-0.08	34	Bethany Coll.	WV	-0.15	-0.16	11
Wilkes Univ.	PA	-0.17	-0.18	56					

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each private four-year institution (continued from Table DD-4) with at least 11 employed male enrollees in the Project Talent database, and the number of (weighted) respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured academic aptitude rank and parental income rank and race indicators. University names and states from matched universities in IPEDS.

Source: Project Talent and IPEDS.

Table DD-6: 1963 Value-Added of Community Colleges

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
C. of San Mateo	CA	-0.06	-0.09	22	Oakland Comm. C.	MI	0.31	0.24	21
Contra Costa Coll.	CA	-0.37	-0.33	31	Metr. Comm. C.-Kansas City	MO	0.00	0.01	55
El Camino Comm. C. District	CA	-0.09	-0.12	24	Saint Louis Comm. C.	MO	-0.14	-0.16	25
Glendale Comm. C.	CA	-0.24	-0.25	43	Jones County Junior C.	MS	-0.01	0.02	24
Long Beach City C.	CA	-0.12	-0.09	29	Cayuga County Comm. C.	NY	-0.24	-0.25	53
San Bernardino Valley C.	CA	0.12	0.15	39	CUNY Bronx Comm. C.	NY	-0.15	-0.16	47
San Diego City C.	CA	-0.25	-0.27	35	CUNY Queensbor. Comm. C.	NY	0.07	0.02	26
Triton C.	IL	-0.00	0.01	40	Erie Comm. C.	NY	0.07	0.05	29
Cowley County Comm. C.	KS	-0.00	-0.07	23	Nassau Comm. C.	NY	-0.01	-0.13	30
Louisiana State U.-Eunice	LA	0.09	0.06	145	SUNY Broome Comm. C.	NY	-0.52	-0.53	50
Comm. C. of Baltimore Co.	MD	-0.23	-0.12	74	Cuyahoga Comm. C. District	OH	-0.13	-0.18	70
Lansing Comm. C.	MI	0.02	0.16	23	San Antonio C.	TX	-0.32	-0.28	88
Macomb Comm. C.	MI	-0.02	-0.06	87	Everett Comm. C.	WA	0.09	0.08	32

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each two-year college with at least 11 employed male enrollees in the Project Talent database, and the number of (weighted) respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured academic aptitude rank and parental income rank and race indicators. University names and states from matched universities in IPEDS.

Source: Project Talent and IPEDS.

Appendix E: Dale-Krueger Value-Added in California

Mountjoy and Hickman (2021) study the value-added of public colleges and universities in Texas. They show that there is substantial variation in late-20s wages by institution even after conditioning on a restricted set of covariates, including proxies for academic preparation and parental income. However, they show that conditional on admission portfolio fixed effects – that is, a separate fixed effect for each complete *set* of Texas institutions that admit the student, a strategy due to Dale and Krueger (2002) – little wage variation remains, resulting in a forecast coefficient on traditional value-added estimates (treating the former statistics as causal) of approximately 0.

The Dale-Krueger strategy has well-known limitations: the source of within-portfolio enrollment variation is unobserved and may generate substantial selection bias, and students who are admitted to a single university (including most of Texas's most-prepared students due to the Texas Top Ten policy and about one-third of students overall) are omitted. Mountjoy and Hickman (2021) also measure wages at a young age (27-29), prior to many students' graduate school completion and before young Americans' wage ranks have generally stabilized. Nevertheless, in this appendix we work toward reconciling the striking difference between this and other available forecast coefficients by replicating the Mountjoy and Hickman (2021) findings in a different setting: California.

We begin with the university value-added statistics presented in Appendix I of Bleemer (2022). The base data include all 1995–1997 freshman applicants to any University of California campus and includes their standardized test score (SAT or converted ACT), their parental income, and their application and admission portfolio across UC campuses.[91] Applicants are matched by name and birth date to their first enrollment institution in the National Student Clearinghouse; by name, birth date, and address to the portfolio of all institutions to which they sent their SAT scores (a proxy for application) as reported in the College Board California master testing file; and by social security number to quarterly wages from the California Employment Development Department, from which early-30s wages were constructed as the average non-zero annual wage between ages 30 and 35.[92] Four sets of institution-level statistics are available for the 133 colleges and universities (including two-year institutions) with at least 50 in-sample enrollees with observed wages:

1. "Raw wages": Average early-30s wages;
2. "Traditional value-added": Average early-30s wages partialing out (15) ethnicity indicators and fifth-order polynomials in SAT score and parental income, following Chetty et al. (2020);
3. "MH value-added": Average early-30s wages partialing out fixed effects for the full UC application-admission portfolio; and
4. "MH' value-added": Average early-30s wages partialing out fixed effects for the full set of institutions to which the applicant sent their SAT scores.

[91] About 13 percent of applicants do not report parental income on their application, which is distinct from the financial aid application. Reporting is not strongly correlated with parental income (Bleemer, 2023).

[92] Wages are collected for California unemployment insurance purposes and exclude federal employment, self-employment, and employment outside of California. Wages are observed for about two-thirds of applicants.

Table EE-1: Dale-Krueger Forecast Coefficients of Institutional Value-Added

	All Colleges and Universities			Four-Year Uni.		CA Pub.		UC System	
	Trad. VA	MH	MH'	MH	MH'	MH	MH'	MH	MH'
Raw Wages	0.75 (0.02)								
Traditional Value-Added		0.81 (0.02)	0.82 (0.07)	0.79 (0.03)	0.78 (0.07)	0.68 (0.05)	0.74 (0.08)	0.52 (0.05)	0.66 (0.08)
Adj. R^2	0.91	0.91	0.80	0.90	0.86	0.86	0.84	0.94	0.90
Obs.	131	131	38	83	20	27	18	8	8

Note: Each cell in this table displays the coefficient from an OLS regression of average institutional value-added (either traditional, MH, or MH') on average traditional value-added (which control for fifth-order polynomials in family income and SAT score and ethnicity indicators). "All Colleges and Universities" includes the 131 colleges and universities for which value-added estimates are available from Bleemer (2022), which are the institutions where at least 50 1995–1997 University of California applicants first enrolled and subsequently had observable early-30s wages in California's UI wage database; "Four-Year Uni." drops community colleges; "CA Pub." restricts to the University of California and California State University systems; and "UC System" restricts to that system. Regressions are weighted by the observed number of students who enroll at each institution. Standard errors are robust and do not correct for first-stage sampling error.

Source: Bleemer (2022).

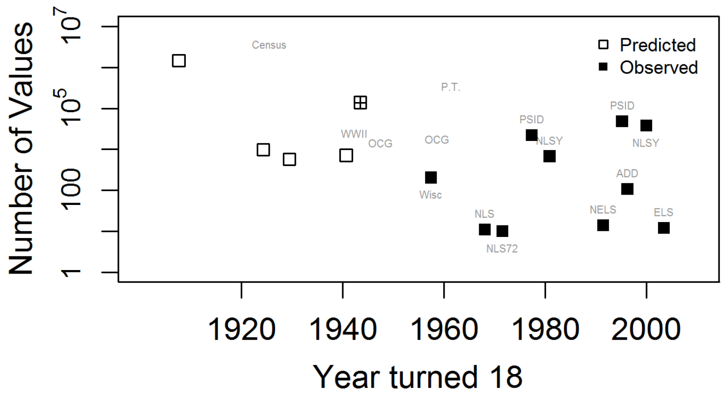
Notice that neither of these "MH" value-added statistics exactly replicates Mountjoy and Hickman (2021), since the first set are restricted to admission across only UC campuses and the second set are restricted to (a proxy for) *application* across all US institutions. We balance between replicating the earlier study and extending that study to a larger and more diverse set of institutions – including both private universities and two-year colleges – by presenting forecast coefficients across different sets of institutions.

Table EE-1 presents a series of forecast coefficients summarizing the degree to which portfolio fixed effects absorb cross-university variation in early-30s wage outcomes. The first column shows that traditional value-added estimates absorb about 25 percent of all cross-institution wage variation. The next pair of columns shows that MH and MH' absorb a further 15-20 percent of cross-institution wage variation, with similar estimates when the sample is restricted to four-year universities. The forecast coefficients decline further when restricted to public California universities (about 0.7) or the UC system; the forecast coefficient closest to Mountjoy and Hickman (2021) is approximately 0.5, though it is only estimated over the eight UC campuses. All of the forecast coefficients are far above 0, suggesting that method alone does not drive our institutions' predictive power for individual earnings to those observed in the Texas context.

In combination with the quasi-experimental and selection-on-observables evidence presented in Section 8.1 and the limitations of the Dale-Krueger methodology discussed above, we believe that the evidence suggest that a forecast coefficient on traditional value-added estimates of 0.7-0.8 is a reasonable posterior, implying that most of the rising gap in average institutional value-added by parental income reflects changes in the causal return of higher education to those students. We employ these forecast coefficients in Figure 12 as described in the text.

Other Appendix Figures and Tables

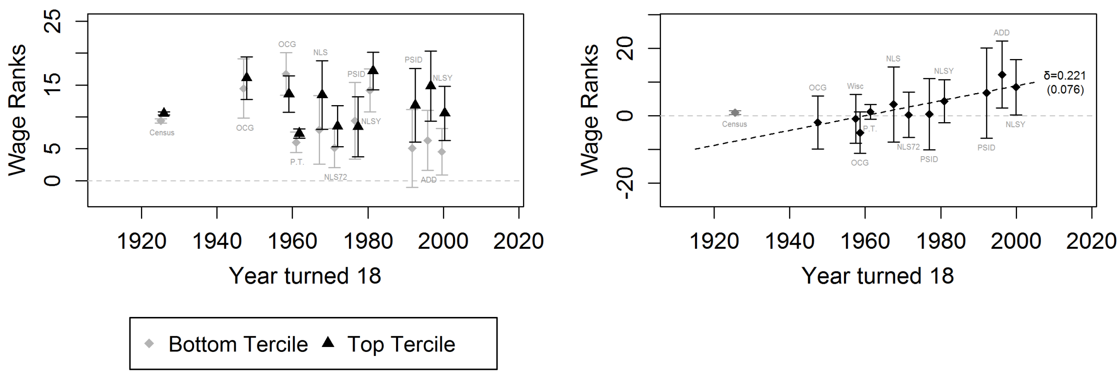
Figure A-1: Number of Parental Income Bins by Dataset



Note: This figure shows that parental income is predicted in the earliest survey datasets (including being partially predicted in Project Talent), but that starting in 1960 there is no meaningful relationship between bin count and year, suggesting that the observed dynamics are unlikely to be explained by steady improvements in data quality. Number of unique parental incomes in each data source by average male birth year, plotted on a log scale. Census (and WWII) parental incomes are predicted by parental occupation, industry, region, and race using the 1950 Census. OCG parental incomes are predicted by occupation, race, region, and gender using the contemporaneous Census. Project Talent parental incomes are observed in six bins; we then predict continuous income by parental income bin, occupation, education, home value or rent, number of children, region, and race using the 1960 Census. Source: US Census, WWII Draft Cards, CPS OCG, Wisconsin, Project Talent, NLSM, NLS72, PSID, NLSY79, ADD Health, and NLSY97.

Figure A-2: Regressivity of US Higher Education Over Time in **Wage Rank**

(a) Average Return to Enrollment by Tercile (b) Increased Return per Parent Income Rank (δ)



Note: This figure shows that the growing regressivity of US higher education is also observable when children's incomes are reported in within-age wage rank. **Panel (a)**: The estimated observational annual wage return to at least one year of college enrollment at age 31-35 among high school graduates by survey dataset and contemporaneous parental income tercile (displaying only the top and bottom tercile), measured in contemporaneous income rank and conditional on dataset-cohort-tercile fixed effects. **Panel (b)**: Estimated regressivity of male college enrollment over time in the United States, where the trend line is the estimated δ and standard error from Equation 2, parameterizing $Coll_{it}$ as indicating at least one year of college. Dataset-specific estimates and 95-percent confidence intervals are from a version of Equation 2 estimated with separate δ_t terms for each dataset; the linear slope (and standard error) is from a version with δ_t permitted only a linear trend over time, excluding Census respondents, and can be interpreted as the annual increased relative rank wage value of college-going per 100 family income wage ranks. Child incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. See Appendix A for details on data construction. Source: US Census, CPS OCG, Wisconsin, Project Talent, NLSM, NLS72, PSID, NLSY79, ADD Health, and NLSY97.

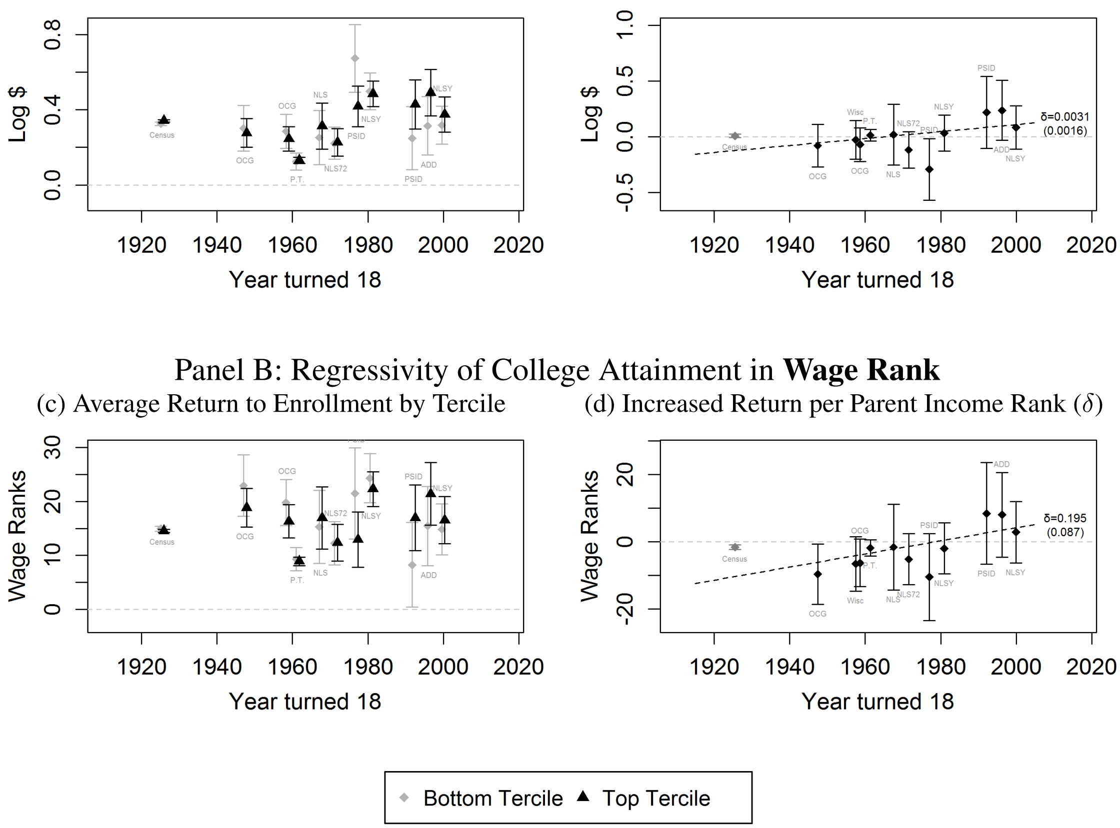
Figure A-3: Regressivity of US Higher Education **Attainment** Over Time

Panel A: Regressivity of College Attainment in Log Dollars

(a) Average Return to Enrollment by Tercile (b) Increased Return per Parent Income Rank (δ)

Panel B: Regressivity of College Attainment in **Wage Rank**

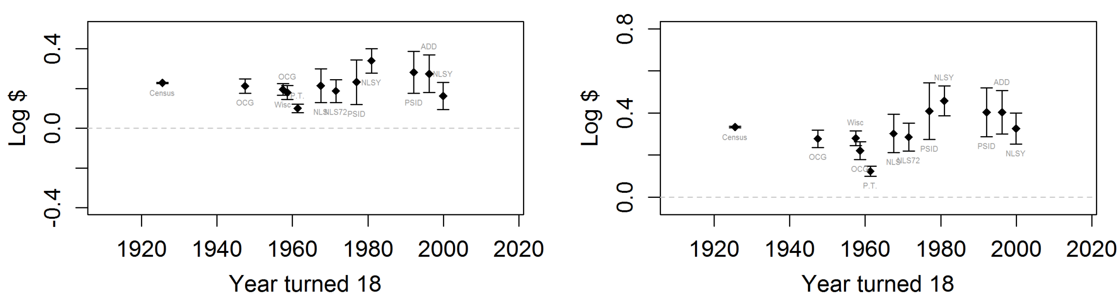
(c) Average Return to Enrollment by Tercile (d) Increased Return per Parent Income Rank (δ)



Note: This figure shows that the growing regressivity of US higher education is also observable when examining college attainment (earning a Bachelor's degree) rather than enrollment, though the attainment slope is statistically noisy when measured in logs. Note: **Panels (a,c)**: The estimated observational annual wage return to at least four years of college enrollment at age 31-35 among high school graduates by survey dataset and contemporaneous parental income tercile (displaying only the top and bottom tercile), measured in contemporaneous income rank (c) or CPI-adjusted 2022 log dollars (a) and conditional on dataset-cohort-tercile fixed effects. **Panels (b,d)**: Estimated regressivity of male college enrollment over time in the United States, where the trend line is the estimated δ and standard error from Equation 2, parameterizing $Coll_{it}$ as indicating at least one and at least four years of college and plotting the latter coefficients. Dataset-specific estimates and 95-percent confidence intervals are from a version of Equation 2 estimated with separate δ_t terms for each dataset; the linear slope (and standard error) is from a version with δ_t permitted only a linear trend over time, excluding Census respondents, and can be interpreted as the annual increased relative rank or log wage value of college-going per 100 family income wage ranks. Child incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. See Appendix A for details on data construction. Source: US Census, CPS OCG, Wisconsin, Project Talent, NLSM, NLS72, PSID, NLSY79, ADD Health, and NLSY97.

Figure A-4: Average Observational Return to US Higher Education at Median Income

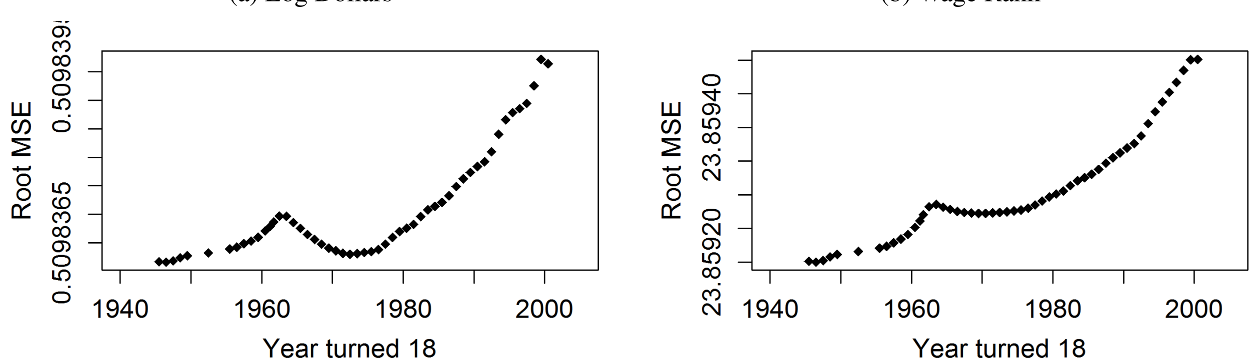
(a) College Enrollment (b) College Attainment



Note: This figure shows that the average observational log wage return to college enrollment for median-income students declined in the mid-century and rose in recent decades, with an even larger rise for college attainment (largely complete by the 1980s). Estimated income returns from male college enrollment and attainment over time in the United States, with dataset-specific coefficients (β) and 95-percent confidence intervals from a version of Equation 2 estimated separately in each dataset without time-varying coefficients (and omitting the δ terms). Child incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. See Appendix A for details on data construction. Source: US Census, WWII draft cards, Wisconsin, Project Talent, NLSM, NLSY79, and NLSY97.

Figure A-5: Root Mean Squared Error from Regressivity Models with Kink Points

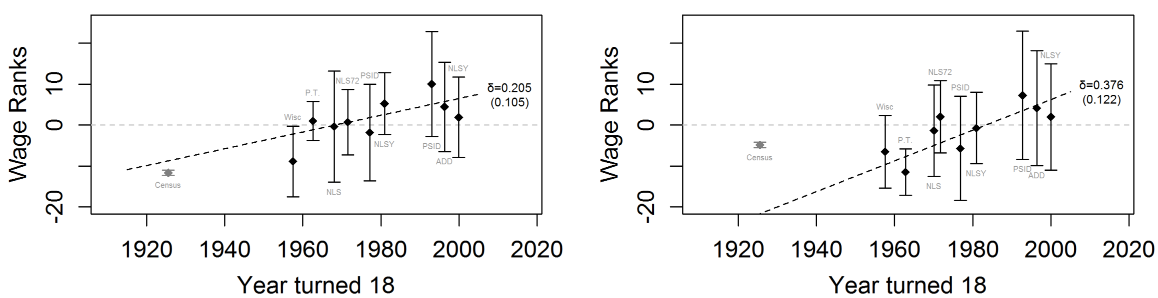
(a) Log Dollars (b) Wage Rank



Note: This figure shows that when re-estimating Equation 2 (including the 1940 US Census and equalizing weight across datasets) for college enrollment with a one-kink linear δ term, the model that places the kink point between the US Census and all other datasets has the lowest RMSE, justifying our modeling choice in Figure 3 of estimating the regressivity trend separately across all post-Census data sources. The root mean squared error of versions of Equation 2 in which $Coll_{it}$ is parameterized as an indicator for college enrollment and δ_t is required to be linear except for a single kink point at the age-18 cohort on the x-axis. Models are estimated at every possible kink point with at least two cohorts on either side of the kink. Unlike in the linear specification shown in Figure 3, 1940 US Census data are *included* in estimation. Child incomes are measured in annual log wages or contemporaneous wage ranks; incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight), normalized to equalize the weight placed on each dataset. See Appendix A for details on data construction. Source: US Census, CPS OCG, Wisconsin, Project Talent, NLSM, NLS72, PSID, NLSY79, ADD Health, and NLSY97.

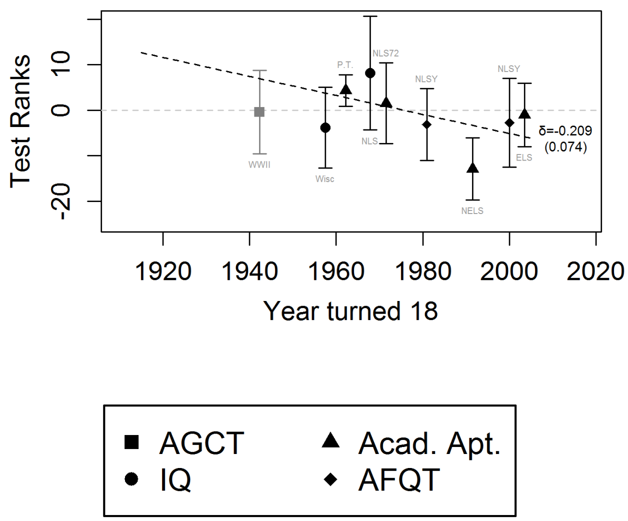
Figure A-6: Regressivity of US College Enrollment in **Household Income Rank** Over Time

(a) Male (b) Female



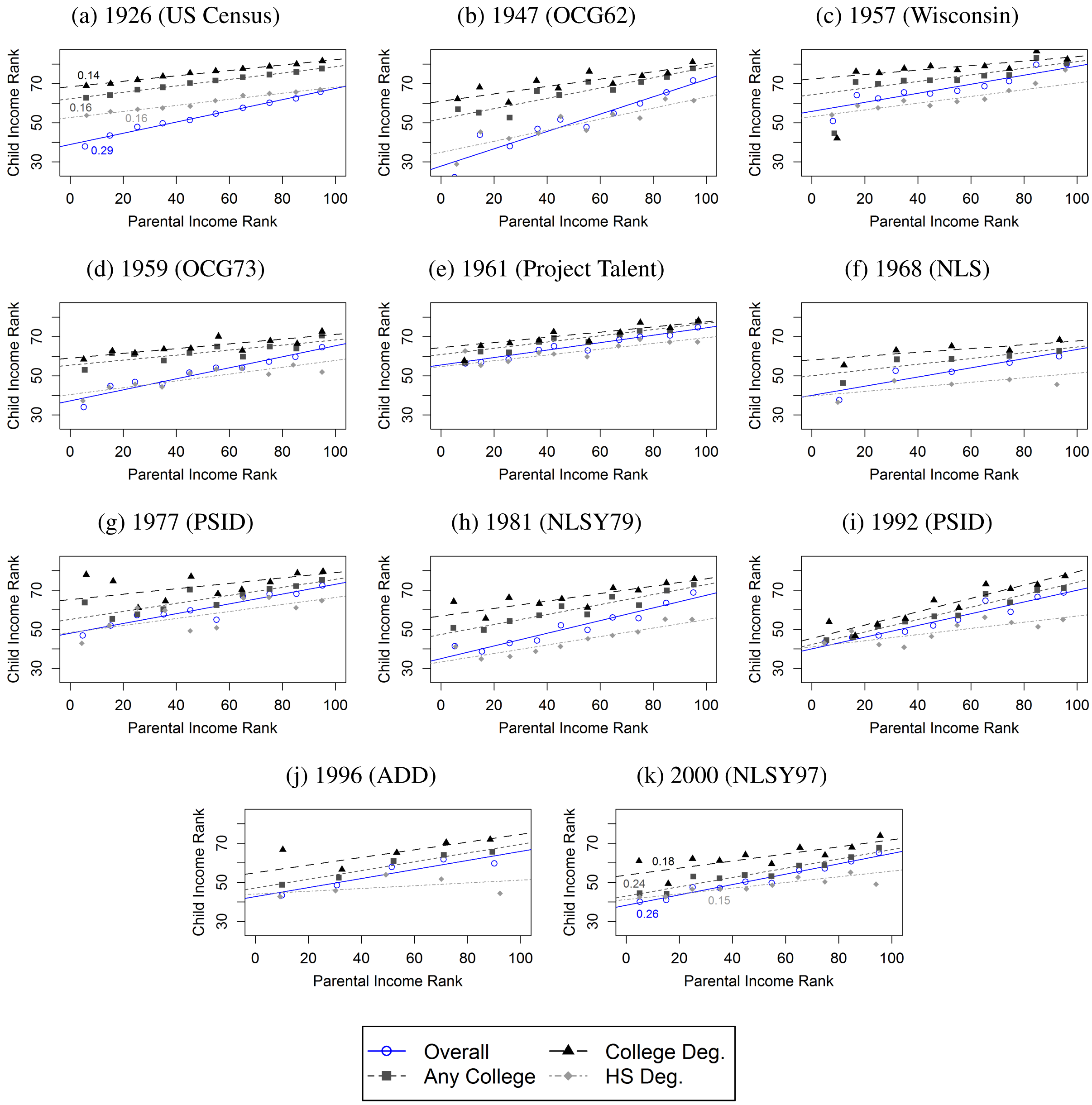
Note: This figure shows that the growing regressivity of US higher education is observable in **household** income ranks for both male and female children, though levels differ for women: college-going was progressive for women (with lower-income women gaining more household income than higher-income women), but is no longer. The estimated regressivity of male (a) or female (b) college enrollment over time in the United States as measured in terms of child's household income rank at age 30-35, where the trend line is the estimated δ and standard error from Equation 2, parameterizing $Coll_{it}$ as indicating at least one year of college. Dataset-specific estimates and 95-percent confidence intervals are from a version of Equation 2 estimated with separate δ_t terms for each dataset; the linear slope (and standard error) is from a version with δ_t permitted only a linear trend over time, excluding Census respondents. Child incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. See Appendix A for details on data construction and definition of household income rank. Source: US Census, CPS OCG, Wisconsin, Project Talent, NLSM, NLS72, PSID, NLSY79, ADD Health, and NLSY97.

Figure A-7: Pre-College Human Capital-Based Selection into College Attainment



Note: This figure shows that college graduates' pre-college academic preparation did was similar by parental status over time, extending the enrollment-based findings in Figure 5(b). Again, there is no upward trend in high-income students' pre-college test scores relative to those of low-income students, which leads us to reject any meaningful role for academic selection in explaining the rise in observational regressivity in college degree attainment over time. The estimated differential selection into male college **attainment** over time in the United States, with dataset-specific coefficients (δ) and 95-percent confidence intervals from a version of Equation 2 estimated with separate β's in each dataset and replacing $Wage_{it}$ with measures of pre-college cognitive skills. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. See Appendix A for details on variable definition and data construction. Source: US Census, WWII draft cards, Wisconsin, Project Talent, NLSM, NLSY79, and NLSY97.

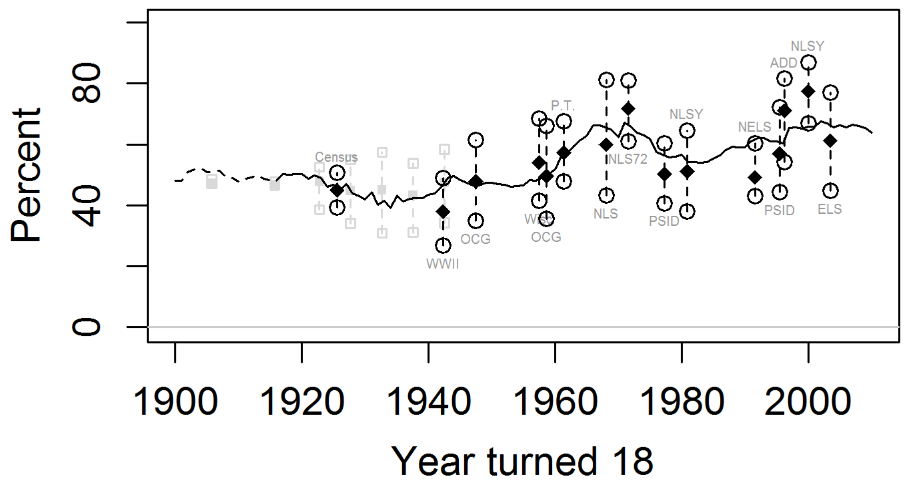
Figure A-8: Rank-Rank Income Correlation for Age 31–35 Children by Survey



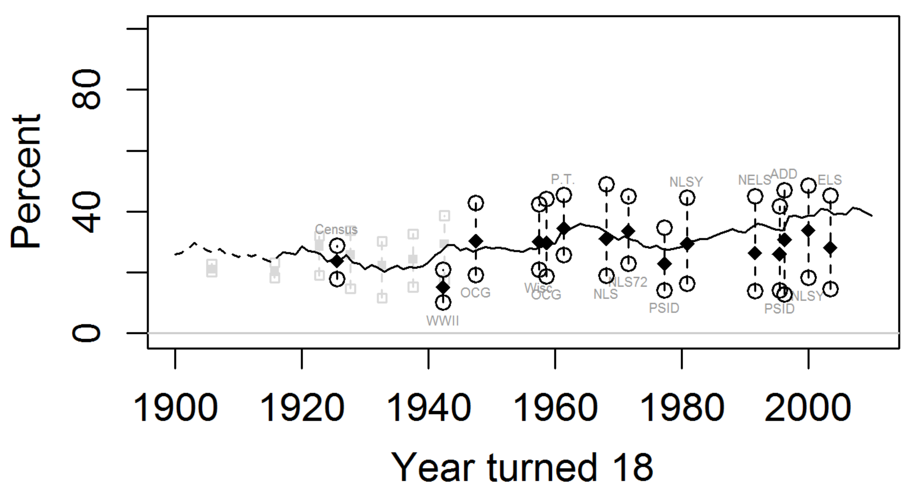
Note: These figures visualize the change over time in the relative observational return to college-going for lower- and higher-income students in the US. The income gains to college attendance fall to zero in the lowest parental income deciles beginning with the 1961 cohort. Binned scatterplots and slopes of income rank among employed age 31–35 men by pre-college parental income rank overall, for college graduates, for people who had completed at least one year of college, and for high school graduates who had not completed any years of college. See Appendix A for details on data construction and variable definition. Source: See Appendix A.

Figure A-9: Male High School Graduates' College Enrollment and Attainment Over Time

(a) College Enrollment

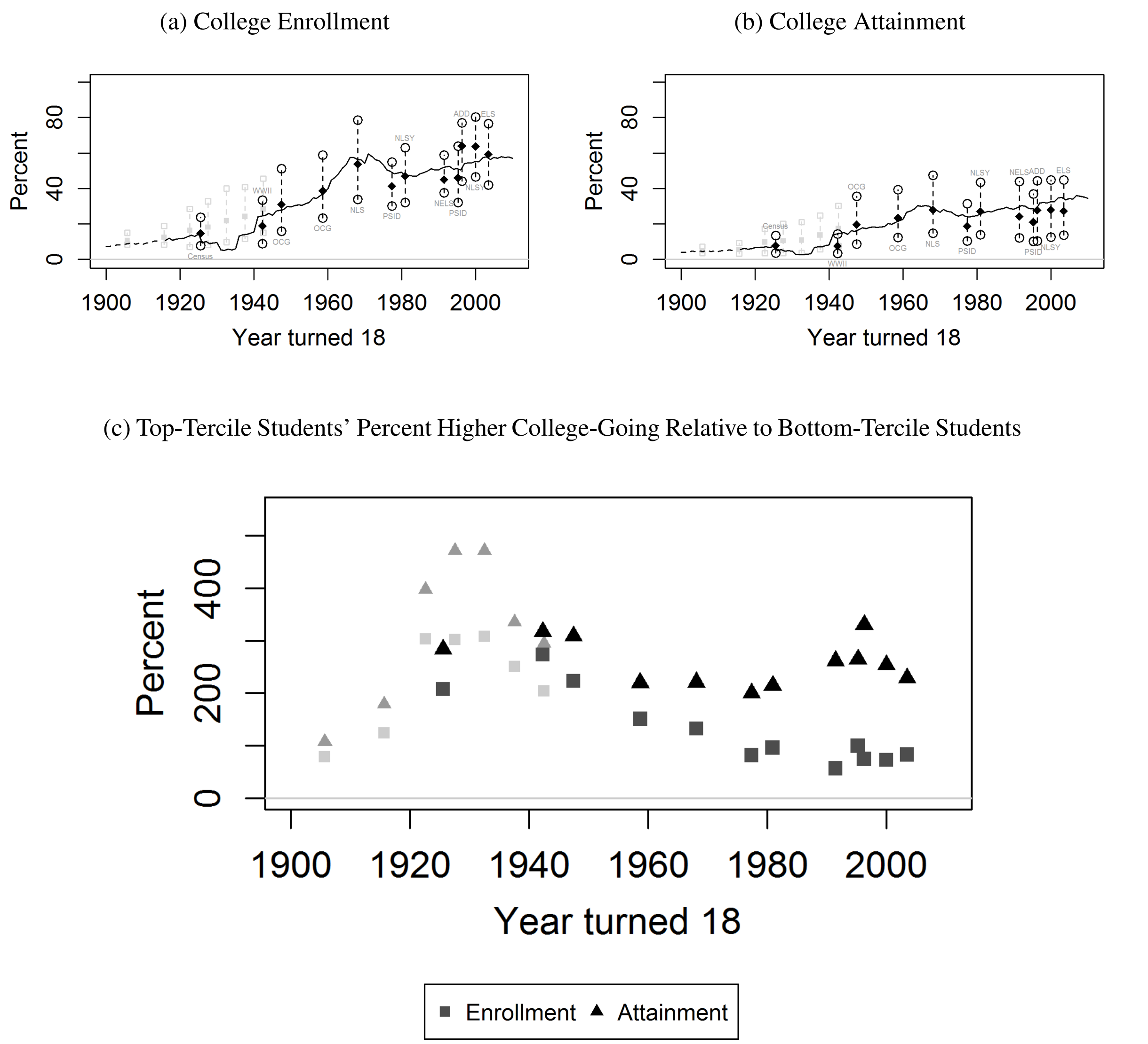


(b) College Attainment



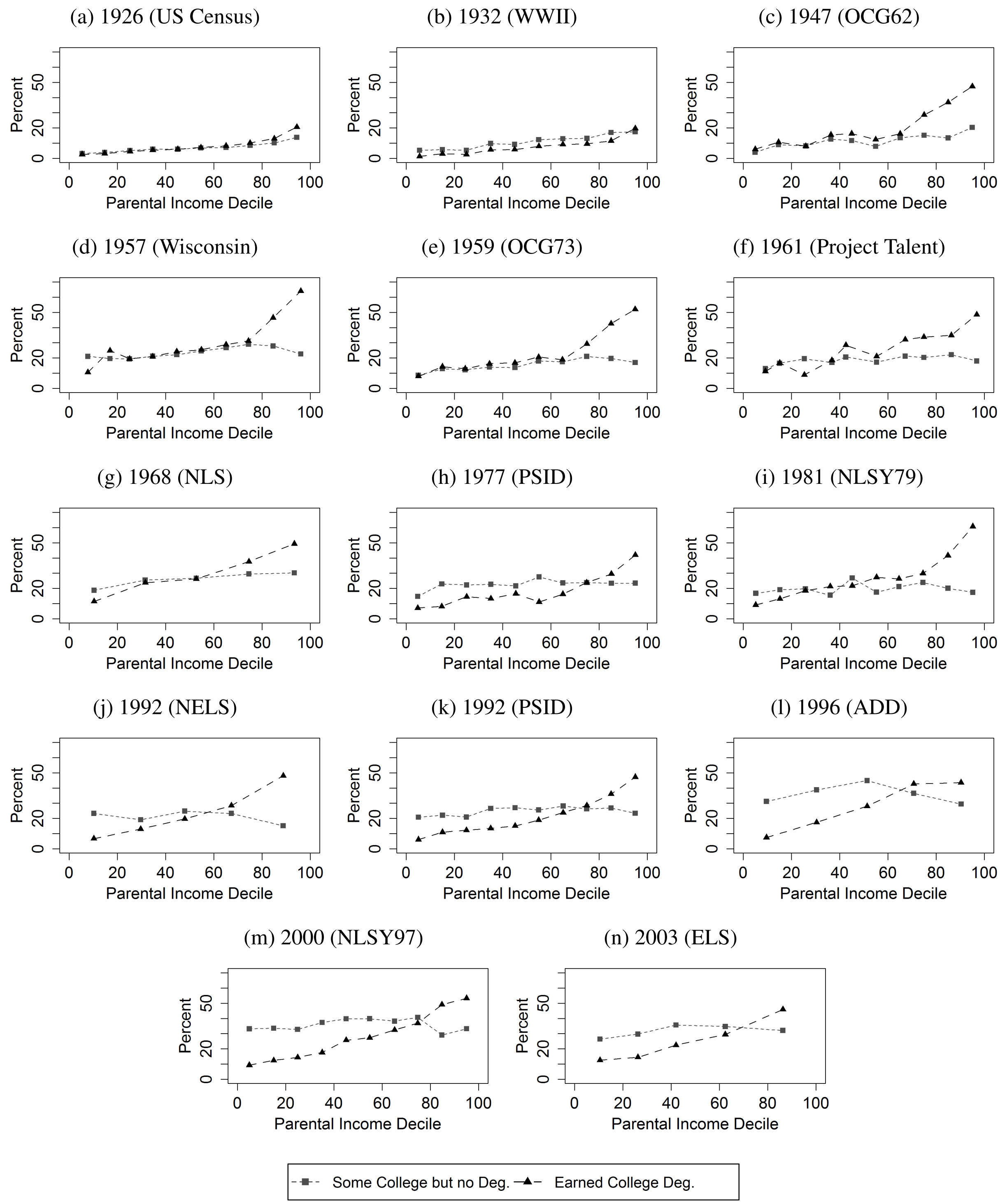
Note: This figure shows that, as in Figure 4a, the difference in college-going between lower- and higher-income students among high school graduates widens in the mid-century, narrows in the 1970s, and widens again at the turn of the 21st century. Share of male **high school graduates** between ages 30 and 35 who had completed at least one (enrollment) or four (attainment) years of college overall (black diamond) or among those from the bottom or top tercile of parental incomes when approximately aged 14–17 (circles). The solid line reports the same overall average educational outcome for 1940–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, and 2021 American Community Survey respondents between ages of 28 and 42. Points in gray show the same for older men when other data are unavailable: linked 1900–1940 Census respondents (age 50–55), 1910–1940 Census respondents (age 40–45), and 1962 CPS OCG respondents for every 5-year age range from 35–40 to 55–60. See Appendix A for details on data construction. Source: US Census, WWII Draft Cards, Wisconsin, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, and ELS.

Figure A-10: College Enrollment and Attainment By Tercile Since 1900



Note: This figure shows that lower-income students became relatively less likely to attend or graduate from college than higher-income peers after the 1920s age-18 cohorts, followed by stagnation in college enrollment and declines in degree attainment since the 1970s. Panels (a) and (b): Points in black show the share of men between ages 30 and 35 who had completed at least four years of college overall (black diamond) or among those from the bottom or top tercile of parental incomes when age 14–17 (circles). The solid line reports the same overall average educational outcome for 1940–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, and 2021 American Community Survey respondents between ages of 28 and 42. Points in gray show the same for older men when other data are unavailable: linked 1900–1940 Census respondents (age 50–55), 1910–1940 Census respondents (age 40–45), and 1962 CPS OCG respondents for every 5-year age range from 35–40 to 55–60. Panel (a) replicates Figure 4a. Panel (c): The percent higher college enrollment (squares) or attainment (triangles) observed among students from families with top-tercile parental incomes relative to those from bottom-tercile parental incomes, as measured in the same datasets as shown in the earlier panels. See Appendix A for details on data construction. Source: US Census, WWII Draft Cards, CPS OCG, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, ELS, and ACS.

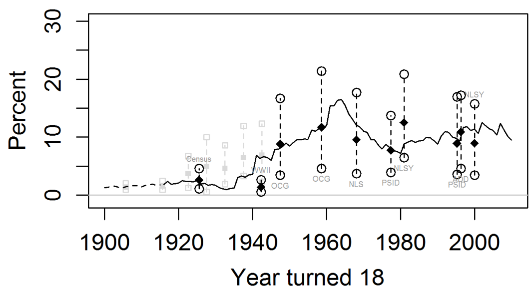
Figure A-11: Selection into College Enrollment and Attainment by Parental Income



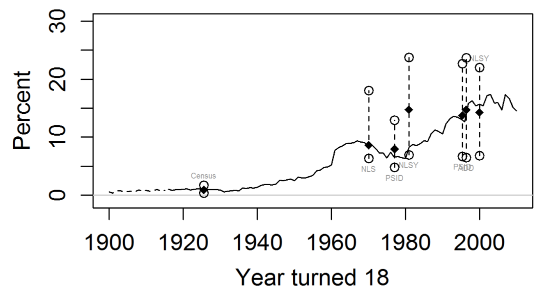
Note: These figures display the rising selection into US college-going and degree-earning overall and by parental income decile over the 20th century with demonstrable overlap between surveys covering similar cohorts. The percent of male youths between 31 and 35 in each survey (by average year at age 18) who have graduated from or attended college by parental income (or estimated parental income) percentile. See Appendix A for details on data construction and variable definition. Source: See Appendix A.

Figure A-12: Graduate School Enrollment Over Time

(a) Male

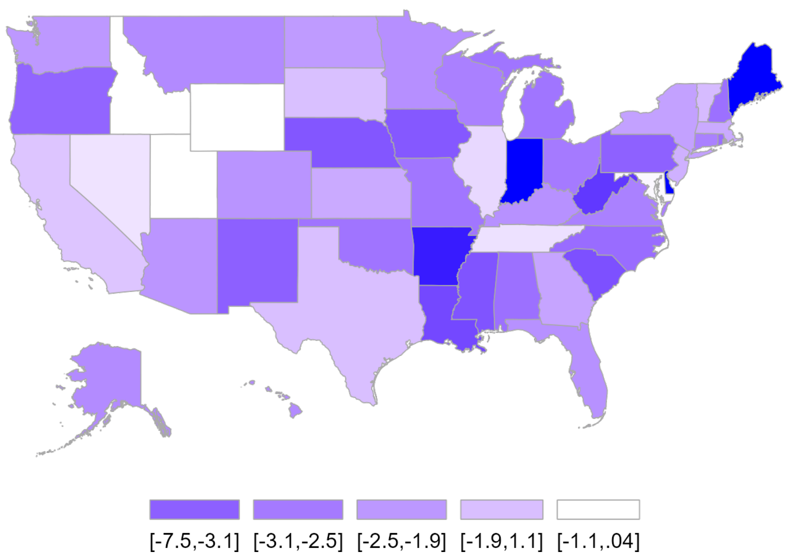


(b) Female



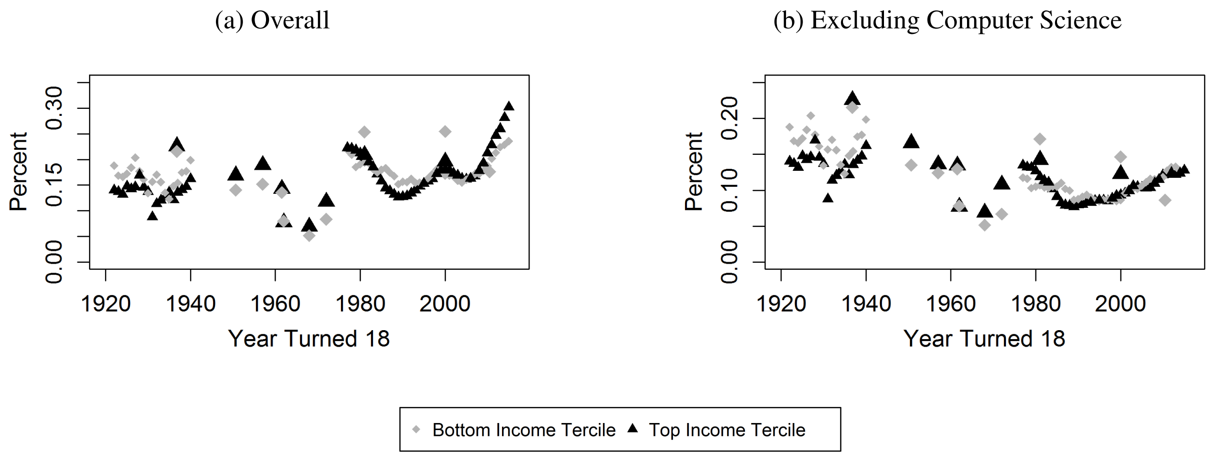
Note: This figure shows parental income-based stratification in graduate school enrollment is higher and more stable than in college enrollment after World War II, though female graduate school enrollment has outpaced that of male students at the bottom of the income distribution since the 1970s. The consistency of these graduate school enrollment gaps suggests that these degrees are unlikely to explain rising regressivity. Share of male and female adults aged 26-35 who had completed at least one year of post-baccalaureate graduate school overall (black diamond) or among those from the bottom or top tercile of parental incomes when age 14–17 (circles). The solid line reports the same overall average educational outcome for 1940–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, and 2021 American Community Survey respondents between ages of 28 and 42. Points in gray show the same for older men when other data are unavailable: linked 1900–1940 Census respondents (age 50–55), 1910–1940 Census respondents (age 40–45), and 1962 CPS OCG respondents for every 5-year age range from 35–40 to 55–60. Survey averages are weighted using respondent weights. See Appendix A for details on data construction. Source: US Census, WWII Draft Cards Wisconsin, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, and ELS.

Figure A-13: Geographic Distribution of 2010 Major Stratification



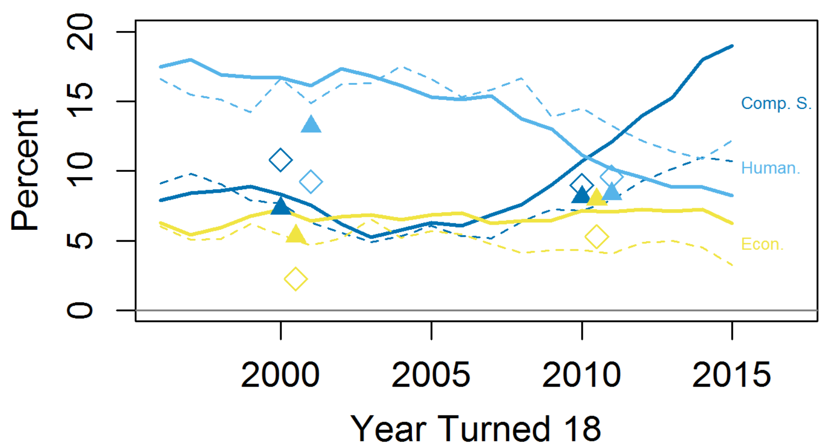
Note: This map shows that the states where the 2010 gap between the value of college majors earned by higher- and lower-income students were generally in the South and Midwest, while major attainment was more equal in the Mountain West and West. The difference in average major premiums declared between male college graduates from the bottom and top parental income tercile in that year, where major premiums are measured in 66 'detailed' categories in the 2019–2021 ACS (Figure 6). Source: College Scorecard and IPEDS. Annual parental income terciles were measured by Pell status among 2015–2016 degree recipients in the College Scorecard; see Appendix A.13. See Appendix A for details on data construction. Source: College Scorecard and the ACS (Ruggles et al., 2024).

Figure A-14: Average Annual Engineering Degree Gap by Income Tercile



Note: This figure shows that engineering – the highest-value discipline in all periods – was at least as commonly declared by lower-income students as their higher-income peers in the early 20th century, but in recent years higher-income students have opened a larger gap in engineering attainment than in any of the the previous 100, with the gap fully explained by increased higher-income students' enrollment in computer science majors. The difference in engineering degree enrollment for University of California enrollees (small diamonds) or survey respondent graduates (large diamonds) from the bottom and top parental income tercile in that year. Historical parental income terciles were measured by Census-linked fathers' estimated income (LIDO) 2–11 years prior to their first year of enrollment (UC 1920–1940), by average income in students' residential Census tract (UC 1975–1995) or Zip code (UC 1996–2016), by reported parental income at ages 14–17 (non-UC surveys), or by Pell status (2015–2016 degree recipents in the College Scorecard; see Appendix A.13). University of California enrollees exclude those from UCLA, UCSD, and UCM. See Appendix A for details on data construction. Source: University registers, US Census, UC-CHP administrative student records, IRS SOI, Wisconsin, NLSM, NLSY79, NLSY97, the College Scorecard, and the ACS (Ruggles et al., 2024).

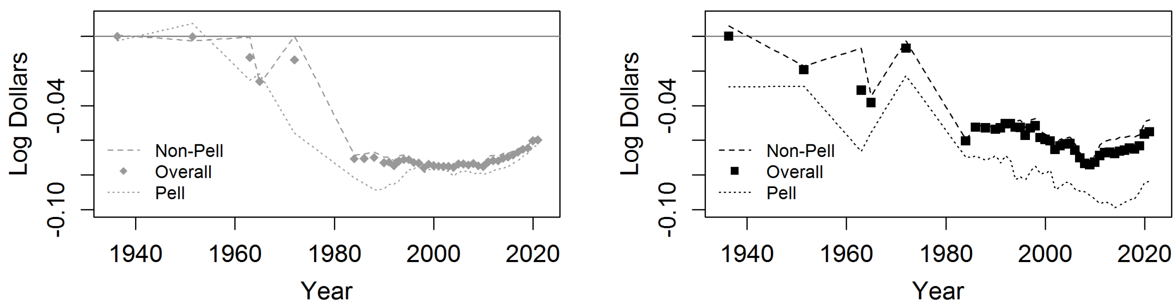
Figure A-15: Annual Share of Declared College Majors by Parental Income Tercile



Note: This figure shows that the decline in humanities enrollment and the rise in computer science enrollment since the 2000s have been disproportionately driven by higher-income students, both at the University of California and nationally. The annual share of University of California enrollees (lines), NLSY97 respondents, or national university enrollees (College Scorecard) who declare (UC) or earn computer science (including computer engineering), economics (including finance), or humanities majors since 1995 by parental income. Solid lines and filled triangles reflect top-tercile or non-Pell (Scorecard) students; dashed lines and open diamonds reflect bottom-tercile or Pell students. NLSY97 does not have a field category for finance, so only includes economics in yellow. Annual parental income terciles were measured by average income in students' residential Zip code (UC), by reported parental income at ages 14–17 (NLSY97), or by Pell status (2015–2016 degree recipents in the College Scorecard; see Appendix A.13). Source: UC-CHP administrative student records, IRS SOI, NLSY97, and the College Scorecard.

Figure A-16: Average Enrollment Value-Added Over Time by Pell Eligibility

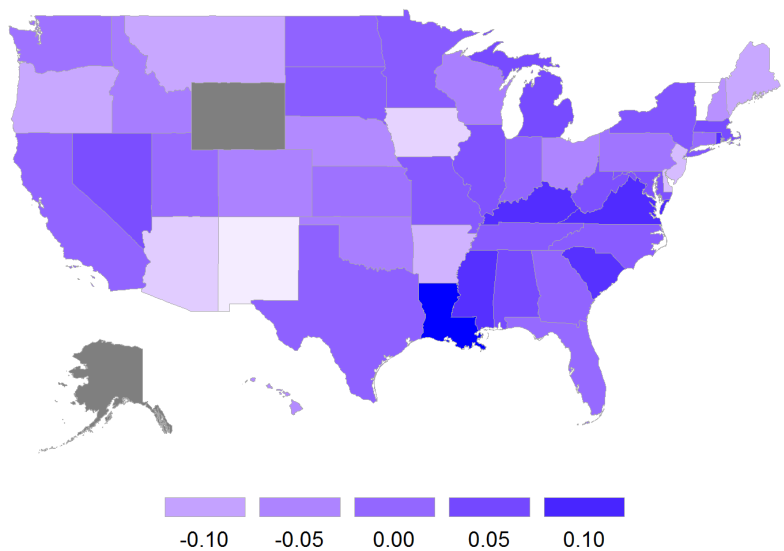
(a) 1960s Value-Added　　　　　　(b) 1990s Value-Added



Note: This figure shows that enrollment growth at lower-value collegiate institutions has outpaced that of higher-value institutions over the past 100 years, driven by the expansion of two-year institutions between 1960 and 1980, but that this trend has reversed in the past decade as community college enrollment stagnates. The male-enrollment-weighted average institutional log wage value-added of US colleges and universities by age-18 cohort, where value-added is estimated in 1963 (using Project Talent) or 1996 (from University of California applicant records) and normalized to be mean-0 in the earliest observation period. Value-added is estimated by OLS with fifth-order polynomials in test scores (measured academic aptitude or SAT), parental income rank, and race indicators as controls (following Chetty et al., 2020). Project Talent value-added estimates are restricted to men and include the 523 last-enrollment institutions with at least 20 employed male respondents, with wages measured at age 29; the 1996 value-added estimates include the 136 first-enrollment institutions where at least 50 1995–1997 University of California applicants enrolled who were employed in California between ages 31 and 35 (using average wages measured at those ages). Enrollments are measured in the CPS OCG (split into birth cohort terciles), Project Talent, and in more recent years by institution-level Pell and non-Pell degree recipients (IPEDS) adjusted for changes over time in the average family income rank of Pell (and non-Pell) recipients; see Appendix A.13 for details. Late 20th century Pell and non-Pell enrollments are reweighted to match total enrollments by degree level, sector, and year due to missing value-added statistics. See Appendix A for details on data construction. Source: Project Talent, IPEDS, Bleemer (2022), and Chetty et al. (2020).

Source: IPEDS, Chetty et al. (2020) (average wages by institution), and Bleemer (2022) Appendix I (institutional value-added estimates).

Figure A-17: Geographic Distribution of Mid-Century Institutional Value-Added

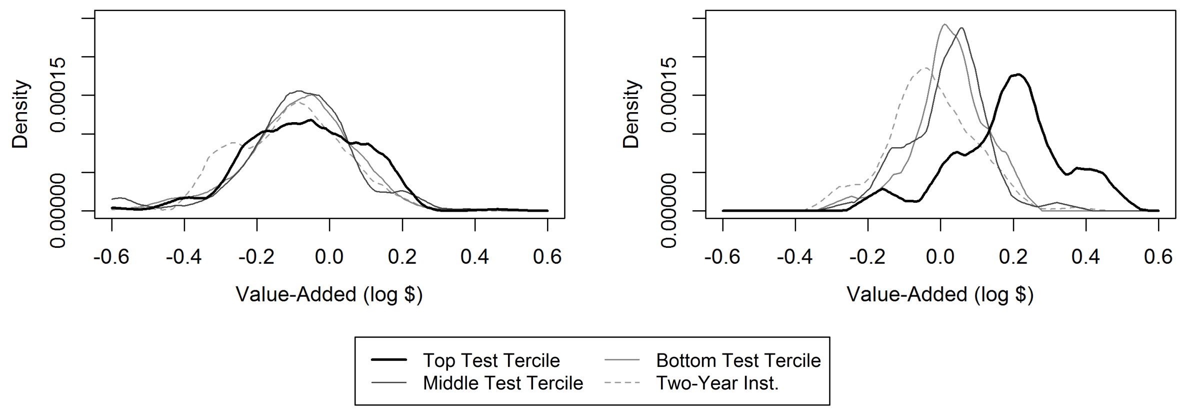


Note: This figure shows that the states offering the highest-value college enrollment in the mid-20th century were in the South, led by the flagship public universities of states like Louisiana, Virginia, and Kentucky, and the Midwest, while the lowest-value enrollments were in the Plains and Mountain West. The enrollment-weighted average estimated mid-century value-added of institutions where students enrolled in the early 1960s, by state of institution. Value-added is estimated by OLS with fifth-order polynomials in test scores (measured academic aptitude or SAT), parental income rank, and race indicators as controls (following Chetty et al., 2020) and using sample weights. Estimation is restricted to men and includes the 523 last-enrollment institutions with at least 20 employed male respondents, with wages measured at age 29; estimates are demeaned across the sample by weighted enrollment. See Appendix A for details on data construction and Appendix D for value-added estimates. Source: Project Talent.

Figure A-18: Institutional Value-Added **by testing tercile** in the 1960s and 1990s

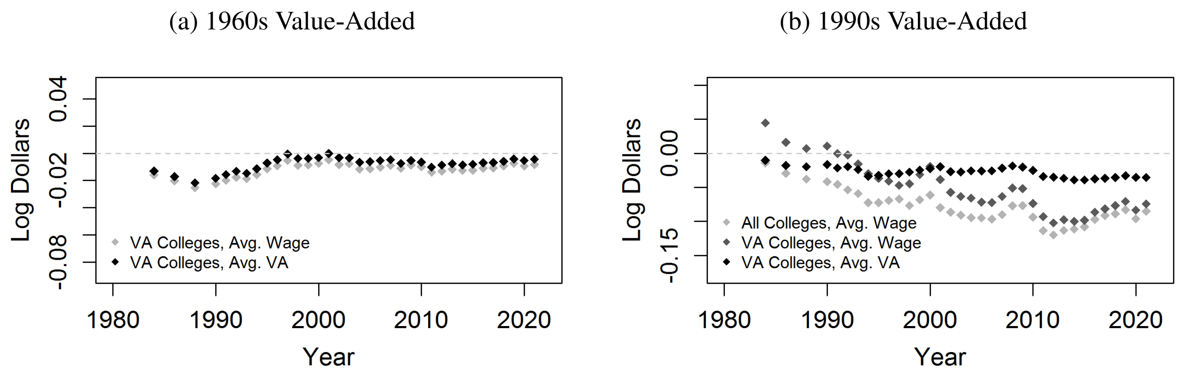
(a) Distribution of 1963 Value-Added

(b) Distribution of 1996 Value-Added



Note: This figure shows that standardized test scores stratify universities by contemporary value-added to an even greater degree than parental income: while high- and low-testing students attended similar-value universities in the mid-20th century, rising meritocracy in selective university admissions has led contemporary higher-testing students to enroll at much higher-value universities than lower-testing students. The institutional value-added of US colleges and universities in log annual wages estimated from 18-year-olds in 1963 (using Project Talent) and 1996 (from University of California applicant records), estimated relative to CSU Long Beach (which is set to 0) and visualized as a scatterplot and as a kernel density plot by four- or two-year institution type and (for the former) tercile of contemporaneous average **test scores**, whereas Figure 9 separates institutions by median parental income. Value-added is estimated by OLS with fifth-order polynomials in test scores (measured academic aptitude or SAT), parental income rank, and race indicators as controls (following Chetty et al., 2020). Project Talent value-added estimates are restricted to men and include the 523 last-enrollment institutions with at least 20 employed male respondents, with wages measured at age 29; the 1996 value-added estimates include the 136 first-enrollment institutions where at least 50 1995–1997 University of California applicants enrolled who were employed in California between ages 31 and 35 (using average wages measured at those ages). The 1996 value-added estimates are propensity-weighted to 2015 (freshman-enrollment-weighted) institutions by interactions between control and two/four-year status and 2021 freshman enrollment; 2021 instructional, research, and student service expenditures per student; and average 2000 parental incomes of students. The triangular kernel bandwidth is 0.1. See Appendix A for details on data construction. Source: Project Talent, IPEDS, Bleemer (2022), and Chetty et al. (2020).

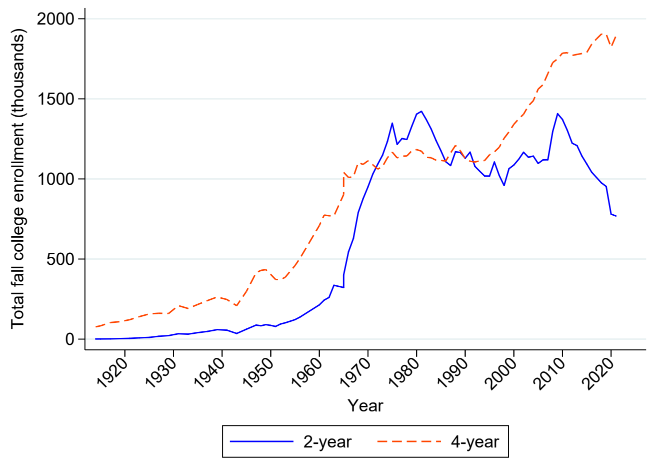
Figure A-19: Institutional Enrollment by Pell Eligibility Indexed by Average Wage or Value-Added



Note: This figure shows that (1) average university wages and value-added were very similar in the 1960s; (2) when university value is indexed by average 2000 early-30s income, restricting to the sample of institutions where late-century value-added statistics are available suggests that such universities exhibit a somewhat steeper 1984–2021 decline in Pell-eligible students' access to high-'value' institutions, though enrollment patterns are similar; but that (3) differences in average incomes sharply overstate (and are a poor proxy for) differences in institutional value-added, suggesting that the covariates employed in producing value-added statistics absorb first-order selection bias across institutions for our purposes. The difference in average enrollee early-30s annual wages or estimated value-added of the institutions where degrees were earned by Pell and non-Pell students. Wages and value-added are measured in Project Talent in the 1960s; average annual wages by institution are measured in 2014 IRS records for all modal enrollees born 1980–1982 and institutional value-added estimates are estimated from average annual California wage records at age 31–35 for 1995–1997 UC applicants who enroll at those schools. "All Colleges" refers to all two- and four-year colleges and universities in the US, and "VA Colleges" refers to the subset of colleges with at least 20 (50) enrollees from the 1960s (1990s) value-added estimation sample. Project Talent value-added estimates are restricted to men and include the 523 last-enrollment institutions with at least 20 employed male respondents, with wages measured at age 29; the 1996 value-added estimates include the 136 first-enrollment institutions where at least 50 1995–1997 University of California applicants enrolled who were employed in California between ages 31 and 35 (using average wages measured at those ages). Pell and non-Pell enrollments are reweighted to match total enrollments by degree level, sector, and year due to missing value-added statistics. Pell student counts are predicted based on the total number of Pell dollars received by the institution in that year and the maximum size of the Pell grant in that year; see Appendix A for those and other details on data construction. Source: IPEDS, Chetty et al. (2020) (average wages by institution), and Bleemer (2022) Appendix I (institutional value-added estimates).

Figure A-20: Enrollment in Two-Year Institutions Overall and by Gender and Parental Income

(a) Total Overall Enrollment



(b) Male Two-Year Enrollment Shares

(c) Female Two-Year Enrollment Shares

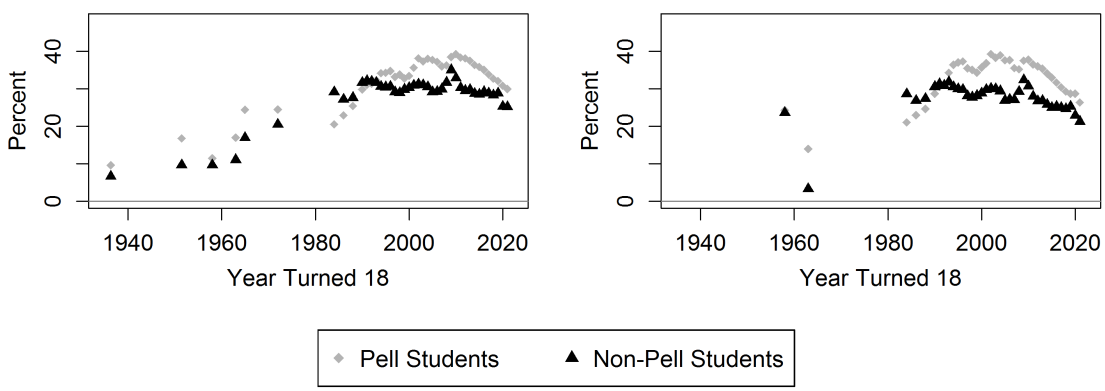
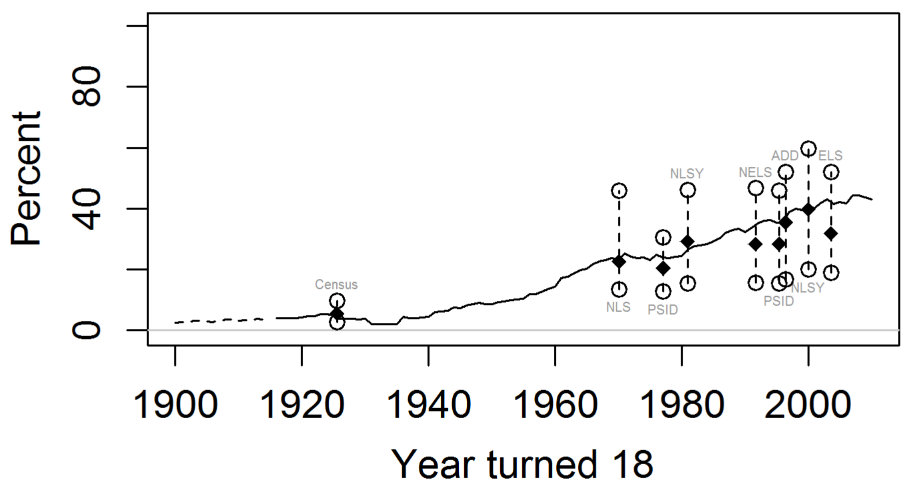


Figure A-21: Female College Attainment Over Time



Note: This figure shows that, as with enrollment (Figure 13a), the share of women earning college degrees has continued to rise over the past 50 years, overall and among both lower- and higher-income women. Share of women between ages 31 and 35 who had completed at least four years of college overall (black diamond) or among those from the bottom or top tercile of parental incomes when age 14–17 (circles). The solid line reports the same variable by birth cohort from the largest possible IPUMS cross-section for women that age. See Appendix A for details on data construction. Source: US Census, Wisconsin, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, and ELS.

Table A-1: Sample Counts by Dataset

		HS Grad. Male Respondents				HS Grad. Female Respondents			
Not Missing:	Birth Years	Test	Income	Major	Inst.	Test	Income	Major	Inst.
Panel A: Survey Respondents to Surveys Used to Measure Regressivity									
1940 Census	1905–1910	0	328,570	0	0	0	219,138	0	0
WWII Draft	1923–1926	2,804	0	0	0	0	0	0	0
CPS OCG 62	1927–1932	0	1,711	0	0	0	0	0	0
CPS OCG 73	1938–1943	0	2,778	1,327	1,414	0	0	0	0
Wisc. L.S.	1938–1940	3,239	3,297	1,041	0	3,638	2,117	701	0
Project Talent	1941–1946	18,437	37,751	27,518	23,508	18,637	20,657	13,170	10,564
NLS M/W	1948–1954	1,401	1,171	1,138	0	1,237	1,273	515	0
NLS 72	1952–1954	0	3,865	†	†	0	3,475	†	†
NLSY 79	1961–1965	2,418	1,938	1,314	0	2,467	1,853	1,484	0
NELS	1972–1974	4,570	0	0	0	5,152	0	0	0
ADD Health	1976–1980	0	1,279	0	0	0	1,389	0	0
NLSY 97	1980–1984	2,702	2,690	3,269	0	2,840	2,724	3,445	0
ELS	1984–1986	4,212	0	0	0	4,537	0	0	0
PSID	1952–1988	0	1,991	0	0	0	2,076	0	0
Panel B: Survey Respondents to Other Surveys									
Time Survey	1908–1917	0	1,818	1,809	†	0	532	531	†
ACS	1984–1990	0	156,279	69,168	0	0	144,360	78,813	0
Panel C: Students in University Administrative Data									
UC Reg.	1902–1922	0	0	21,921	21,921	0	0	15,454	15,454
UC Admin.	1957–1997	0	0	439,719	439,719	0	0	491,941	491,941
Panel D: University-Years in Institutional Surveys									
IPEDS	1966–2003	0	0	0	159,741	0	0	0	159,741
Coll. Sc.	1992	0	0	18,135	18,135	0	0	18,135	18,135

Note: This table shows sample counts and survey availability for the various datasets used in our study. The number of observations in each dataset with non-missing pre-college academic preparation, non-missing early-30s respondent income, non-missing college major, and non-missing college institution among males and females with at least a high school degree. Birth years are winsorized at 2 percent in surveys and report enrollment years minus 18 for university-level records. Major and institution counts for Project Talent, Time, and the ACS are conditional on observing early-30s income. "UC Reg." refers to annual UC registers linked to the 1910–1930 US Census; "UC Admin." refers to combined administrative transcript records from six UC campuses; see Appendix A. † These data are available but have not yet been cleaned.
Source: See Appendix A.

Table A-2: Changes in College Major Premiums Over Time

Survey:	Time[1]	Wisc.	P.T.	NLS	NLS72	NLSY79	NLSY97		ACS	
Year 18:	1932	1957	1962	1968	1972	1981	2000		1995	2005
Human.	0.228	0.065	-0.064	0.192	-0.034	0.253	0.334	-0.016	0.536	0.470
	(0.047)	(0.047)	(0.013)	(0.102)	(0.070)	(0.099)	(0.080)	(0.087)	(0.010)	(0.011)
Social Sci.	0.388	0.378	0.093	0.268	0.334	0.680	0.690	0.370	0.807	0.749
	(0.053)	(0.037)	(0.015)	(0.076)	(0.055)	(0.099)	(0.070)	(0.077)	(0.010)	(0.011)
Natural Sci.	0.386	0.285	0.043	0.412	0.285	0.601	0.692	0.342	0.869	0.853
	(0.047)	(0.040)	(0.016)	(0.091)	(0.064)	(0.116)	(0.095)	(0.103)	(0.011)	(0.011)
Agr.	0.301	0.217	0.111				0.555	0.398	0.683	0.664
	(0.075)	(0.092)	(0.037)				(0.234)	(0.242)	(0.030)	(0.038)
Bus.	0.483	0.438	0.234	0.475	0.325	0.740	0.838	0.522	0.880	0.826
	(0.051)	(0.041)	(0.017)	(0.080)	(0.049)	(0.064)	(0.060)	(0.067)	(0.008)	(0.008)
Eng.	0.532	0.481	0.308	0.654	0.505	0.774	0.821	0.521	0.980	0.972
	(0.048)	(0.040)	(0.020)	(0.122)	(0.064)	(0.068)	(0.064)	(0.072)	(0.008)	(0.008)
Health Prof.	0.685	0.938	0.337	0.552	0.289	0.597	0.822	0.603	0.879	0.745
	(0.069)	(0.132)	(0.040)	(0.287)	(0.069)	(0.180)	(0.147)	(0.156)	(0.020)	(0.019)
Other Prof.	0.304	0.189		0.321	0.123	0.566	0.501	0.252	0.627	0.584
		(0.035)		(0.077)	(0.046)	(0.072)	(0.057)	(0.063)	(0.008)	(0.009)
Some College		0.118	0.040	0.166	0.068	0.269	0.218	0.078	0.276	0.236
		(0.021)	(0.009)	(0.047)	(0.029)	(0.043)	(0.035)	(0.039)	(0.005)	(0.006)
Fam. Inc. Rank								0.389		
								(0.058)		
Test Score Rank								0.004		
								(0.001)		
R^2	0.12	0.10	0.03	0.08	0.04	0.13	0.12	0.15	0.15	0.14
$\bar{Y}$	11.1	11.4	10.7	10.9	11.0	10.7	10.7	10.7	10.8	10.8
Obs.	1,812	3,299	17,817	1,178	3,891	1,953	3,060	2,522	194,848	156,279

Note: This table shows the point estimates visualized in Figure 6, revealing surprising stability in relative returns over time and (in the NLSY97) conditional on parental income and high school test score. Linear regression coefficients from models of the relationship between annual age-31-to-35 log wages and college major among male employed respondents with at least a high school degree to several surveys, with the majors' effects estimated relative to non-college enrollment. Some models include covariates for parental income rank and pre-college test score rank. Robust standard errors in parentheses. See Appendix A for details on data construction and college major categorization.
Source: Time Magazine Survey, Wisconsin, NLSM, NLS72, NLSY79, NLSY97, and the ACS (Ruggles et al., 2024).

Table A-3: Estimated Detailed Major Premiums

Det. Major	%	β	σ	Det. Major	%	β	σ
Computer Engineering	1.6	0.75	(0.03)	Agriculture	1.0	0.23	(0.04)
Cognitive Science	0.1	0.61	(0.14)	Criminology	3.2	0.19	(0.03)
Finance	3.5	0.60	(0.03)	Geography	0.3	0.19	(0.06)
Economics	2.7	0.58	(0.03)	Sociology	1.0	0.19	(0.04)
Electrical Engineering	3.2	0.58	(0.03)	History	2.6	0.18	(0.03)
Materials Science	0.2	0.55	(0.08)	Art History	0.1	0.17	(0.11)
Statistics	0.1	0.55	(0.09)	Communications	2.5	0.16	(0.03)
Bioengineering	0.4	0.54	(0.05)	Environmental Studies	0.8	0.16	(0.04)
Chemical Engineering	0.7	0.53	(0.04)	Ethnic Studies	0.3	0.16	(0.07)
Civil Engineering	1.4	0.53	(0.03)	Psychology	3.1	0.14	(0.03)
Computer Science	6.5	0.52	(0.02)	Design	1.1	0.12	(0.04)
Information	0.5	0.52	(0.05)	Nutrition	0.1	0.12	(0.10)
Mechanical Engineering	3.2	0.52	(0.03)	Philosophy	1.0	0.12	(0.04)
Biochemistry	0.6	0.50	(0.05)	Geology	0.4	0.11	(0.05)
Neuroscience	0.2	0.50	(0.08)	Journalism	0.7	0.11	(0.04)
Industrial Engineering	0.7	0.45	(0.04)	Common Languages	0.4	0.10	(0.05)
Accounting	3.4	0.44	(0.03)	Social Welfare	0.3	0.07	(0.06)
Chemistry	1.0	0.44	(0.04)	Interdisciplinary	0.2	0.05	(0.08)
Biology	5.0	0.43	(0.02)	English	1.8	0.04	(0.03)
Mathematics	1.5	0.43	(0.03)	Film	1.5	0.04	(0.03)
Other Engineering	3.1	0.43	(0.03)	Public Health	0.2	0.04	(0.07)
Political Science	2.8	0.39	(0.03)	Linguistics	0.1	0.02	(0.09)
International Studies	0.6	0.38	(0.04)	Other Social Sciences	0.7	0.02	(0.04)
Physics	0.8	0.38	(0.04)	Creative Writing	0.2	-0.00	(0.07)
Law	0.1	0.35	(0.09)	Other Humanities	0.9	-0.00	(0.04)
Other Health Sciences	1.8	0.35	(0.03)	Education	2.4	0	-
Other Natural Sciences	1.0	0.34	(0.04)	Anthropology	0.4	-0.01	(0.05)
Other Professional	1.3	0.34	(0.03)	Other Languages	0.2	-0.03	(0.08)
Nursing	1.3	0.33	(0.03)	Art	1.5	-0.07	(0.03)
Marketing	2.5	0.32	(0.03)	Religion	0.7	-0.08	(0.04)
Business	15.5	0.30	(0.02)	Music	1.1	-0.20	(0.04)
Public Policy	0.2	0.30	(0.07)	Theater	0.7	-0.29	(0.04)
Architecture	0.8	0.27	(0.04)	Speech Pathology	0.1	-0.41	(0.13)

Note: This table shows the set of 66 detailed majors employed in our analysis ranked in descending order by their observational returns estimated in the 2019–2021 ACS. Linear regression coefficients from a model of the relationship between annual age-31-to-35 log wages and detailed college major among the 69,168 male employed 2019 and 2021 ACS respondents with a college degree, holding out the education major as the comparison group. Wages are CPI-adjusted to 2022 and model include birth cohort fixed effects. Detailed majors are defined by the authors; shares report enrollment shares in each major and standard errors are in parentheses. The model R^2 is 0.06. See Appendix A for details on data construction and college major categorization.
Source: 2019 and 2021 American Community Survey (Ruggles et al., 2024).

Table A-4: Summary of Historical and Contemporary Institutional Value-Added

		1963 Ranking		2014 Ranking							
		1963 VA[1]		1963 VA		1996 VA[2]		2001 VA[3]		2014 Inst. Exp.[4]	
		Mean	90-10	Mean	90-10	Mean	90-10	Mean	90-10	Mean	90-10
Top Test Quartile	Public	-756	28,643	4,808	24,710	13,987	18,337	39,004	2,576	8,712	15,650
	Non-Profit	1,150	25,192	7,657	28,924	13,288	38,210	53,900	40,516	21,979	38,872
Second Test Quartile	Public	-839	24,820	4,134	26,072	2,580	22,148	26,609	9,499	3,477	9,120
	Non-Profit	-568	26,162	2,145	21,615	2,446	33,892	27,346	16,779	4,497	7,541
Third Test Quartile	Public	-2,091	18,424	4,181	26,339	4,048	9,702	13,846	10,586	1,145	7,900
	Non-Profit	72	24,953	972	30,886	9,453	0	12,710	15,514	355	9,021
Bottom Test Quartile	Public	0	34,457	0	32,313	0	18,907	0	25,353	0	5,053
	Non-Profit	-3,042	30,956	1,519	24,885	1,609	9,687	-1,129	21,910	-99	7,327
No Reported SAT Scores	Public			1,942	21,542	2,662	18,358			-1,041	6,293
	Non-Profit			2,414	23,285	-8,885	8,188			904	15,533
Community Colleges	Public	-1,241	27,583	1,661	19,578	-1,627	23,053			-2,000	4,000
	Non-Profit	-7,641	19,814	-3,066	15,068					1,808	11,177

Note: This table summarizes the value-added statistics analyzed in this study and compares them with those presented in Hoxby (2015) and with contemporary per-student instructional expenditures. It shows that value-added stratification by test score has increased over time and that, while comparable, high-test universities' value-added appears to grow substantially between 1996 and Hoxby's 2001 estimates. The average and 90-10 difference in student-weighted estimated value-added of US four-year institutions by quartile of student academic aptitude (1963 ranking) or student 25th percentile summed verbal and math SAT score (2014 ranking) or of institutions without reported average SAT scores or of two-year institutions, reported separately for public and private non-profit institutions (but using the same student-weighted rankings). Value-added estimates are unshrunk; standard errors are omitted. [1] Value-added estimated following Equation 9 using age 29 wages from Project Talent. [2] Value-added estimated following Equation 9 using age 31–35 California wages among University of California applicants as reported in Appendix I of Bleemer (2022). Value-added estimates are only available for 136 institutions, which are propensity-score-weighted to represent US higher education; see Appendix A. [3] Value-added estimated using the selection corrections proposed by Hoxby (2015), and reported from Table 2 of that paper (as aggregates; institution-level value-added estimates are not available in that study). [4] These columns replace value-added with the average 2014 instructional expenditures per student as reported to IPEDS.

Source: Project Talent, IPEDS, Bleemer (2022), and Hoxby (2015).