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ABSTRACT

Going to college has consistently conferred a large wage premium. We show that the relative premium received by lower-income Americans has halved since 1960. We decompose this steady rise in 'collegiate regressivity' using dozens of survey and administrative datasets documenting 1900–2020 wage premiums and the composition and value-added of collegiate institutions and majors. Three factors explain 80 percent of collegiate regressivity's growth. First, the teaching-oriented public universities where lower-income students are concentrated have relatively declined in funding, retention, and economic value since 1960. Second, lower-income students have been disproportionately diverted into community and for-profit colleges since 1980 and 1990, respectively. Third, higher-income students' falling humanities enrollment and rising computer science enrollment since 2000 have increased their degrees' value. Selection into college-going and across four-year universities are second-order. College-going provided equitable returns before 1960, but collegiate regressivity now curtails 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). Universities did so by offering large average and marginal labor market returns to students (Autor, 2014; Zimmerman, 2014), but these large returns have masked striking changes in the relative returns received by students from higher- and lower-income families. Figure 1 visualizes the difference in average wages between high school graduates who complete at least one year of college and those who do not – the "college-going premium" – by parental income at the beginning and the end of the 20th century.[1] The college-going premium was similarly large for lower- and higher-income students born in the 1900s, but by the end of the 20th century it had narrowed for lower-income students and widened for their higher-income peers. We call this new feature of American higher education the rise of "collegiate regressivity": higher-income students now derive greater average observational value from going to college than the lower-income students who make the same choice.

This study documents when and why the labor market returns to college became positively correlated with childhood parental income. We compile dozens of survey and administrative datasets spanning the 20th and 21st centuries that match high school graduates' educational attainment with pre-college cognitive skill, collegiate institutions and majors, and early-30s employment outcomes (see Figure 2). Pooling these data into an nationally-representative database of Americans spanning the 20th century, we pin down the origin of collegiate regressivity to the 1950s. We then decompose why parental income has become a stronger determinant of post-college earnings. Over 80 percent of the steady growth of collegiate regressivity can be explained by three factors: (1) growing stratification in quality between research- and teaching-oriented universities, (2) lower-income students' diversion to community and for-profit colleges, and (3) higher-income students' net exodus out of the humanities and into computer science and other engineering fields. Despite substantial policy efforts to widen lower-income students' access to high-value four-year universities (e.g. Bleemer, 2023), their increasing enrollment at those institutions play only a small countervailing role. As a

[1] Wage ranks are invariant to changes in the wage distribution over time, but Figure A-1 shows a similar trend for log wage returns. We measure mobility among male children in the combined ADD Health and NLSY97 surveys and in the linked 1920–40 US Censuses, where we impute parental income by industry, occupation, state, and demographics.

result of collegiate regressivity, the share of all intergenerational income transmission in the US that can be explained by differential returns to college-going – even without altering the composition of college-goers – has increased from about 0 to 25 percent since the 1950s.

Our study begins by investigating a series of alternative hypotheses that could explain why today's observational college-going premium for students from the bottom parental income tercile is half that received by top-tercile students.[2] While many other well-known changes have occurred in American higher education since the 1950s – e.g. enrollment dramatically expanded in the mid-century, real tuition has been rising since 1980, and universities have implemented widespread curricular updates (though the distribution across disciplines has changed relatively little) – none of these trends align with the rise of collegiate regressivity. We also find that selection into college-going by pre-college cognitive skill, measured using a variety of assessments given to high schoolers over the past century, has not changed across the parental income distribution since the mid-1960s, leaving little scope for differential selection to explain the observed wage patterns.

We therefore focus on the two key sources of college treatment effect heterogeneity between lower- and higher-income students: their enrollment institutions and their declared majors. Each may affect the relative returns to college-going by parental income through two channels: changes in students' enrollment composition or shifts in the relative value of different majors or institutions.

We first establish that institutions' wage value-added has diverged by parental income over the past 60 years. We estimate the value-added of hundreds of colleges and universities in the 1960s and the 1990s by applying the selection-on-observables specification of Chetty et al. (2020) to the mid-century 400,000-respondent Project Talent survey and late-century administrative enrollment and wage records (reported by Bleemer, 2022). We validate prior observational and quasi-experiment evidence that 70-80 percent of variation in institutions' estimated value-added is indeed causal (Chetty et al., 2020; Bleemer, 2021, 2022). We then demonstrate that the correlation between mid- and late-century institutional value-added is only 0.23. The institutions with the largest value-added gains between the 1960s and the 1990s were private and research-oriented public universities, which have also improved along other dimensions of measured quality like student-to-faculty ratios, revenues and expenditures per student, and graduation rates. Those

[2] We focus our main analysis on men, whose consistently high labor force participation avoids selection bias in measuring educational wage returns, but we document similar trends among female college students in Section 10.

institutions also disproportionately enroll higher-income students. The teaching-oriented public universities where lower-income students tend to enroll had similar 1960s measures of quality to research-oriented public universities, but those measures of quality have steadily deteriorated.

Lower-income students' flow out of the traditional four-year sector further exacerbates increasing regressivity in institutional returns. We observe where lower- and higher-income students have gone to college since the 1930s by combining historical longitudinal and retrospective surveys with contemporary federal administrative records from the IRS (Chetty et al., 2020) and IPEDS. Lower-income students enrolling at traditional four-year institutions have benefited from broadening access to high-quality universities since the 1980s (e.g. Bleemer, 2022; Black et al., 2023), but these gains have been more than offset by their simultaneously rising diversion from teaching-oriented public universities into two lower-value segments (Rouse, 1995). The first, community colleges, grew among both bottom- and top-tercile students in the 1960s and 1970s to enroll about 40 percent of all first-time college students in 1980. After 1980, a 20-percentage-point gap opened between bottom- and top-tercile students which has not closed. The second is for-profit colleges, which grew to 20 percent of bottom-tercile student enrollments (but much less among top-tercile students) between 1980 and 2005 before regulatory changes led the segment to shrink considerably (countervailing regressivity since 2010; see Cellini et al., 2020). In sum, growing diversion out of the traditional four-year sector and rising stratification between four-year universities *each* explain about one-quarter of the growth in collegiate regressivity since the 1950s.

Turning to major choice, we construct and validate a series of major-specific wage premiums back to the start of the 20th century. First, we confirm prior observational and quasi-experimental evidence that simple average wage differences between college majors are an effective 1-to-1 proxy for majors' causal wage value (Bleemer and Mehta, 2022b; Dahl et al., 2023): even the highly detailed covariates available in the NLSY97 and Project Talent fail to absorb *any* average between-major selection bias. Leveraging this finding and turning to our host of longitudinal and retrospective surveys, we construct novel measures of the relative returns to different college majors – grouped into 10 high-level disciplines or 66 detailed majors – spanning the past 100 years. The dispersion between majors' wage premiums has increased, but their ranking has remained strikingly stable: e.g. engineering and business majors consistently offer far higher returns than humanities and agriculture.

No matter the year in which returns are measured, lower-income students earned much lower-value majors than their higher-income peers in only two periods: the mid-20th century and since 2000. We show that two recent trends in college major choice – the 'death' of humanities enrollment (Schmidt, 2018) and rising computer science enrollment – have both been driven by higher-income students. Students from the bottom parental income tercile declared similar-value majors to their top-tercile peers in the 1980s and 1990s but declare majors that annually pay about 4 percentage points less today, explaining another quarter of the growth of collegiate regressivity.

This study's primary contribution is to document the rise of collegiate regressivity and identify the channels through which it has (and has not) risen. Our decomposition organizes and extends many prior findings in a single broad narrative. First, we bring together several strands of literature on the four-year university sector: institutional quality varies by parental income (e.g. Winston, 1999) and sector (Bound et al., 2010), in part due to falling state support (Deming and Walters, 2017; Chakrabarti et al., 2020) and growing research funding (Ehrenberg et al., 2003). We trace changes in institutional quality and enrollment backwards in time to show how they contribute to declining economic mobility. Second, (Rouse, 1995) documents that lower-income students are more likely to divert from traditional four-year universities, but the factors identified in prior work – e.g. geographic proximity (Kane and Rouse, 1995), lower tuition (Denning, 2017), and for-profits' high marketing expenditures (Armona and Cao, 2022) – have always been true; they cannot explain why this diversion became notably larger *after* community colleges' 1960–1980 expansion. Third, we document that GPA-based major restriction policy proliferation (Bleemer and Mehta, 2022a) corresponds with recent major stratification and the ensuing reduction in economic mobility. Our framework shows that students' changing enrollment composition across four-year universities, another focus of prior work (e.g. Gurantz et al., 2021; Black et al., 2023; Dynarski et al., 2021; Bleemer, 2022, 2021), partially offset the regressivity created by these three channels.

Our findings link the long literature on the relationship between education, inequality, and economic mobility with the more recent microeconomic literature documenting heterogeneity in the college wage premium. 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 institution and major 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 institutional stratification and collegiate regressivity more generally plays an important role in America's more recent mobility decline (Aaronson and Mazumder, 2008; Chetty et al., 2017).[3] We also contribute to the broader social science literature on the aggregate socioeconomic mobility implications of higher education (Hout, 1988; Torche, 2011; Zhou, 2019) by showing that the causal effects of higher education expansions on economic mobility change over time and have significantly declined since 1960.

In parallel to these macro-level studies, a large recent literature has turned from estimating the average college premium (Card, 1999) toward identifying heterogeneity by institution (Chetty et al., 2020; Mountjoy, 2022) and field of study (Altonji et al., 2016; Kirkeboen et al., 2016). We provide some of the first characterizations of both long-run trends in institution- and major-specific wage premiums and the first longitudinal analysis of differences in the overall premium by parental income, extending 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 income by a half-century (Patnaik et al., 2022; Torche, 2011).[4] These series reveal the macroeconomic implications of heterogeneity in the college premium: changes in major attainment and both institutional returns and composition have increased the intergenerational transmission of income by about one-third since the 1960s.

[3] Secondary school quality was highly stratified even before 1960 (Card and Krueger, 1992; Card et al., 2022). Despite contemporary collegiate regressivity, local university access continues to promote overall economic mobility (Russell and Andrews, 2022; Howard and Weinstein, forthcoming).

[4] Abramitzky et al. (2024) demonstrate that low-income students were consistently under-represented at highly-selective institutions throughout the 20th century. Weinstein (2025) provides suggestive evidence that universities that enrolled higher-income students saw relative increases in value-added in the late 2000s.

2 Data

We compile a comprehensive collection of longitudinal individual-level survey and administrative 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 the datasets we compile and the age-18 cohorts for which each key characteristic is observed; Table A-1 provides sample counts by characteristic.

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).[5] In 1940, we observe reported 1939 wage and salary earnings and impute earlier parental income using occupation, industry, age, sex, race, state, Census region, farm residence status, and self-employment status and their interactions by LASSO in the 1 percent 1950 Census sample ('LIDO', e.g.: Saavedra and Twinam, 2020). We then algorithmically link 1940 teenage boys to their AGCT test scores on World War II Army enlistment records and to years of education on the 1950 Census to measure differential selection into college-going.[6]

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.[7] We bring together a series of well-known federal longitudinal surveys – the three National Longitudinal Surveys of Youth (NLSY), five National Center of Education Statistics longitudinal surveys (NLS72, HS&B, NELS, ELS, and HSLS), 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 Great Aspirations survey of 1961 college graduates, and Project Talent, an extraordinary longitudinal survey of over

[5] We use the NYSIIS standard approach from Abramitzky et al. (2022) in the baseline for men – paralleling our other linkages discussed below – 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.

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

[7] The American Community Survey (ACS) lacks parental income, but it is nevertheless valuable as the latest available measures of average wages by college major. We measure parental income for a Census-linked subsample of our earliest national survey with majors, the 1947 Time Survey of College Graduates. See Appendix A for details.

400,000 mid-century high school students.[8] Parental income is either predicted using detailed parental characteristics (OCG) or observed continuously or in 10–100 bins; Figure A-2 shows that there is no clear relationship between bin size and year after the earliest predicted-income datasets.[9] We construct parental and child income ranks for each cohort within each nationally representative survey or using CPS data for surveys that are only representative of high school completers (e.g. NLS72). Each survey conducts different tests of high school cognitive skill; 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 OCG, Project Talent, and NLS72 – include enrollment institutions.

Although institution-level college enrollments by parental income are not publicly available in recent years, the federal IPEDS database publishes annual first-year Pell enrollments by institution since 2009. 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.[10] IPEDS also contains detailed institutional characteristics like tuition and completion rates since 1980. We supplement IPEDS with the annual number of students who enroll at each institution by parental income quintile between 1998 and 2009 from Chetty et al. (2020), who assign students to their modal (rather than their initial) institution. The federal College Scorecard provides institution-by-major degree counts by Pell status for only the 2015–2016 graduating cohorts.

We augment these nationally representative sources with the Wisconsin Longitudinal Survey, a comprehensive longitudinal survey of that state's 1957 high school graduates, and more detailed administrative student records covering most 1920–1940 and 1975–2014 enrollees at public research universities in California (the University of California system; Bleemer, 2018). We observe family income in the pre-1950 data by linking students to the linked Censuses (following Abramitzky et al., 2022); in post-1950 student records we approximate students' parental income

[8]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.11 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.12.

[9]Project Talent collects 5 parental income bins along with detailed 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. We do the same for the contemporaneous Great Aspirations survey.

[10]Survey data from NPSAS shows that the median 2009–2023 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.14).

by the average household income in their Census tract (1975-1994, from the Census) or Zip code (1995-2014, from IRS SOI).

Finally, we collect a series of auxiliary datasets covering universities' institutional characteristics since 1900, including tuition, student-to-faculty ratios, and revenues per student from 6 Blue Books between 1923 and 1962; mid-century STEM degree availability from HEGIS; and additional tuition and enrollment statistics from a variety of historical surveys. More detail on our data sources can be found in Appendix A.

3 The Increasing Regressivity of Higher Education in the US

College-going and the college wage premium have risen in recent decades (e.g. Autor, 2014; Goldin and Katz, 2009), but surprisingly little is known about differences in the economic value of college-going between 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 pooled longitudinal database by OLS:

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

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

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 annual wage $Wage_{it}$. 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.[11] The level of intergenerational income persistence (α_t) and average returns to higher education (β_t) vary flexibly for each cohort and dataset 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 separate interaction term, and a parametric

[11] When reporting estimates for college attainment, we parameterize $Coll_{it}$ as a matrix of two indicators – enrollment and four-year degree completion – and report the latter coefficient.

version estimating the average linear trend over time. In our baseline specification, we estimate 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 heteroskedasticity-robust.

Figure 3a shows estimates of γ for students from the bottom and top parental income terciles in each dataset. 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 percentage points – relative to the college enrollment premium received by their lower-income peers.[12] The time trend is clearer in Figure 3b, which shows both the non-parametric and parametric estimates of δ_t. 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.[13] 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 income and post-college wages was positive and large.[14]

The linear δ trend of 0.0042 confirms that between 1950 and 2000, the college-going premium for students in the top parental income tercile rose considerably, by about 0.14 log points relative to bottom-tercile students.[15] 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-7 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 data sources.

In short, the relative return to college-going for lower-income students has fallen since the 1950s. The remainder of this study investigates the sources of this new collegiate regressivity.

[12]Figure A-3 shows qualitatively similar patterns when child income is measured in wage ranks, as in Figure 1. Figure A-4 replicates Figure 1 using each of our datasets, reiterating that the observational return to college for lower-income students begins its relative decline around the 1960s.

[13]Feigenbaum and Tan (2020) show that rich and poor twins received similar returns to schooling in 1940.

[14]Figure A-5 shows a slightly flatter δ_t trend for college attainment. Figure A-6 shows the β_t coefficients, which mirror the levels shown in Figure 3a.

[15]If we restrict to only the PSID or only the NLSY's, we obtain very noisy point estimates of δ – 0.0119 (0.0148) and 0.0007 (0.0078) – highlighting the advantage of combining many datasets. The point estimates in rank dollars – 0.24 (0.53) and 0.19 (0.34) – are noisy but similar to our baseline estimate of 0.21 (0.077) shown in Figure A-3.

4 College Choice in the Long Run

Higher education dramatically expanded in the 80-year period spanned by Figure 3, raising the question of whether industry-wide shifts explain the rise in collegiate regressivity. Many features of US higher education, however, have remained largely unchanged since the 1920s, including widespread availability of two- and four-year college enrollment spread across the country (Geiger, 2014). This section quantifies and discusses four high-level 20th century trends in US higher education – growing enrollments, rising tuition, and shifts in degree program and institutional choice – and shows that none can meaningfully explain the documented rise in collegiate regressivity since the 1950s.

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 has been sufficiently common across the parental income distribution to make regressivity meaningful over the entire timeline of our study. We leverage our longitudinal datasets to plot college enrollment by parental income tercile in Figure 4a, with the black line and diamonds indicating overall college enrollment rates and the triangles denoting enrollment among the bottom (△) and top (▽) income terciles.[16] Two insights emerge. First, college attendance by parental income rapidly diverged among age-18 cohorts in the 1940s as higher-income veterans took up GI Bill tuition benefits (Stanley, 2003).[17] Second, this gap continued to expand until the Vietnam War era, after which higher-income sons' college enrollment stagnated.[18] Since 1990, however, the college enrollment gap again widened; about 80 percent of top-tercile men go to college.[19] Interestingly, the proportional rates of college-going by parental income are quite similar at the start and end of the 20th century (see Figure A-9c).

[16]Overall college enrollment – shown in the solid black line – is measured among Census respondents aged 28-42, or older respondents in the earliest years (where the line is dotted). These data closely match those from NCES (2021). Figure A-8 presents college enrollment and attainment by parental income in each of our datasets.

[17]Figure A-9c shows that older top-tercile sons disproportionately enrolled in and attained college degrees after World War II, as shown in Abramitzky et al. (2024) and Collins and Zimran (2024).

[18]Bailey and Dynarski (2011) and Jackson and Holzman (2020) document the post-1960 divergence in men's college enrollment; tracing these trends back to the 1920s reveals they began only after World War II.

[19]We find similar trends when we examine the share of respondents who report at least four years of college education (Figure A-9b), restrict to high school graduates (Figure A-10), or look at graduate school attendance for male students (see Figure A-11). We discuss female enrollment in Section 10.

4.2 College Costs

Credit constraints play a central 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.[20] The annual trend in (sticker or net) tuition is very different from that of collegiate regressivity. 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. This latter trend lagged behind collegiate regressivity by two decades, though – there was already a meaningful new gap between lower- and higher-income students' college-going premiums by 1980 – suggesting that college costs play a second-order role in explaining regressivity's growth over time.

4.3 College Curriculum

The income returns to college also depend on whether colleges' degree programs align with contemporaneous skill premia and preferences (Altonji et al., 2016). Figure 4c shows annual major attainment shares from three retrospective surveys spanning the 20th century. About 20 percent of college graduates had engineering degrees in both of the cohorts shown in Figure 1. Science and humanities degrees contracted and social science, business, and other professional (the residual) degrees expanded, reflecting economic developments between the two periods.[21] Overall, four-year universities' degree composition remained stable as collegiate regressivity increased.

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 lead their alumni to high-status positions (Abramitzky et al., 2024; Chetty et al., 2023; Michelman et al., 2022). However, Figure 4d shows that the share of college degrees granted by these elite institutions has been steadily declining for a

[20] The net cost of college attendance is the enrollment-weighted sticker price minus grant and scholarship aid, in contrast to prior work that approximates tuition using overall revenues and expenditures (Jones and Yang, 2016; Donovan and Herrington, 2019). Our net tuition measures do not include veterans' educational aid, which reduced college costs in post-war periods. See Appendix A.18 for details on data and construction.

[21] Appendix C shows similar trends in faculty and coarse disciplines since 1900 at two colleges with available data.

century, well before university regressivity began.[22] The college-going premium tilts more towards higher-income students today – 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 increasing in cost, there is no obvious correlation between the post-1960 rise in collegiate regressivity 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.

5 The Sources of Rising Collegiate Regressivity

What explains the relative increase in higher-income students' college-going premium in the second half of the 20th century? This section decomposes rising collegiate regressivity into the determinants of collegiate returns: differential selection into college-going (and the return to pre-college cognitive skill), the composition and returns to enrollment institutions, and the composition and returns to college majors, with a residual absorbing any remaining differences.

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 cognitive skill a_i, enrollment institution u_i, major m_i, and parental income PI_i 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:

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

For tractability, we make three simplifications to decompose D_t into component parts. First, we define a residual term ϵ'_t to capture potential interdependence between students' valuations of different collegiate experiences and the experiences of other students – e.g. capturing institutional

[22] See Appendix A.19 for the sources used to construct these historical enrollment trends. The decline was only interrupted by a short-lived enrollment spike during the post-World War II GI Bill period.

or major peer effects – and disaggregate the remaining terms by a, u, and m:

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

Second, we 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. It also includes all interactions between these terms, most problematically (1) the joint relationships between a and each of u and m, which translates into any return to university and major value that varies in pre-college cognitive skill, and (2) the joint relationship between u and m, including the reflection of any effects of enrollment institution on major choice.[23] Finally, 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 stratification of each characteristic decomposes D_t into three enrollment 'composition' components and three relative 'value' components:

$$\begin{aligned} D_t = \epsilon_t + &\overbrace{\int_a v_t^a(a) \Delta_q \big[P_0(a|q) \big] \, da}^{\text{Test Returns}} + \overbrace{\int_a \Delta_q \big[P_t(a|q) - P_0(a|q) \big] v_t^a(a) \, da}^{\text{Test Composition}} + \overbrace{\sum_u v_t^u(u) \Delta_q \big[P_0(u|q) \big]}^{\text{Institutional Returns}} + \\ &\underbrace{\sum_u \Delta_q \big[P_t(u|q) - P_0(u|q) \big] v_t^u(u)}_{\text{Institutional Composition}} + \underbrace{\sum_m v_t^m(m) \Delta_q \big[P_0(m|q) \big]}_{\text{Major Returns}} + \underbrace{\sum_m \Delta_q \big[P_t(m|q) - P_0(m|q) \big] v_t^m(m)}_{\text{Major Composition}} \end{aligned} \tag{6}$$

The following three sections leverage our panoply of data sources to measure each of this de-

[23] We show that lower-income students receive similar average returns to institutions and majors in Tables 2 and 4. Prior evidence on supermodularity across universities is inconclusive (Dillon and Smith, 2020; Bleemer, 2021, 2022).

composition's components, focusing in particular on measurement of $\Delta_q\left[P_t(x|q)\right]$ and $v_t^x(x)$ for $x \in a, u, m$ and $t \in [1920, 2020]$. These six components explain most of the collegiate regressivity trend, suggesting that the other channels captured by ϵ offer only second-order contributions.

6 Selection Into College

The observed regressivity of the college-going premium could combine the treatment effect of college enrollment with bias resulting from differential selection into enrollment. Higher-income college-goers may increasingly earn more than 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 men's college-going between 1960 and 2000 – whether or not enrollment is conditioned on high school graduation – Figure 4a 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 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 5a 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 wages 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 reversion in this period (Deming, 2017; Castex and Dechter, 2014).

Next, we turn to trends in college-goers' test scores by parental income over time. We re-estimate Equations 1 and 2 replacing Y_{it} with student test score outcomes; Panels (b) and (c) of Figure 5 plot the resulting γ and δ coefficients.[24] College-goers have consistently higher test scores than other high school graduates, 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 enlistees.[25] After 1960, a positive gap emerges, but stays stable at around 10 test score ranks

[24] As above, we exclude the pre-1950 period from the linear estimation because it deviates from the long-run trend.

[25] Both high school graduation rates by parental income and relative academic selection into high school graduation by parental income stabilized in the early 1960s; see Figure A-12.

between the highest- and lowest-income students.[26]

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

7 Collegiate Institutions

7.1 Institutional Returns

Lower- and higher-income students enroll at different colleges and universities, and heterogeneity in the economic value of those institutions (or changes in that economic value) may play an important role in explaining the rise of collegiate regressivity. While the average wages of graduates from different universities vary widely, positive selection on student characteristics into more-selective institutions likely biases average wage differences across institutions as proxies for those institutions' wage value to their students. We factor in these issues when we measure the relative value of each US postsecondary institution by using linear value-added models of individuals' early-career wages (e.g. Chetty et al., 2020; Eller, 2023):

$$w_{it} = Inst_{u_i} + \beta_t X_i + \alpha_t + \epsilon_{it} \tag{7}$$

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. Students are assigned to their first post-secondary enrollment institution, treating transfers and dropout as endogenous to initial enrollment. 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 gender 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

[26] Hendricks et al. (2021) also show that the within-income correlation between cognitive skill and college enrollment rose in the 1940s–50s and then remained unchanged in subsequent decades. Figure A-13 shows weak evidence of a relative *rise* in lower-income college *graduates*' pre-college cognitive skill since the 1960s.

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 California campus. Institutions with fewer than 20 (50) assigned enrollees are omitted from the 1963 (1996) value-added estimates; lower- and higher-income enrollment at remaining institutions is reweighted to full enrollment by year and segment.[27] We augment our late-century value-added estimates by assigning for-profit universities 11 percent lower wage value-added than the average community college, following Cellini and Turner (2019).[28]

Two- and four-year institutions' wage value-added changed substantially between the mid- and late- 20th century. Figure 6a shows that the correlation in value-added across time is only 0.23, but that sample is limited to the 46 institutions observed in both datasets.[29] We characterize trends in institutional value-added by dividing institutions into five segments: research- and teaching-oriented four-year public universities, private four-year universities, for-profit institutions, and all other two-year colleges.[30] Table 1 shows that public research universities and private universities have higher and faster-growing value-added than other institutional segments, which also tracks with their relatively higher instructional expenditures per student and lower drop-out rates and student-to-faculty ratios.[31] Two-year institutions' measures of institutional quality were lower than those of four-year institutions in the 1960s and have continued to decline. For-profit universities also fare poorly on our measures of university quality. Like today, the highest collegiate value-added states in 1960 were in the Northeast and West (Figure A-14), but Ivy Plus institutions stand out far less in mid-century value-added estimates than they do decades later (Chetty et al., 2023).

The relationship between parental income and value-added has notably strengthened over time.

[27] The full set of 1963 value-added estimates are available in Appendix D; they are available for institutions that enroll about 50 percent of all contemporaneous college students. Though our 1996 value-added estimates are only observed for workers who earn wages covered by California unemployment insurance, they are nevertheless available for many non-California institutions and account for 15-20 percent of total FTE enrollment nationwide.

[28] Table A-2 compares our value-added estimates with the 2001 estimates of Hoxby (2015), who employs a paired comparison design to estimate value-added statistics in IRS data. While the patterns are similar, Hoxby (2015)'s estimates suggest even greater levels of cross-institution stratification.

[29] US higher education's average enrollment-weighted wage value-added declined in the mid-20th century using either measure, 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 (see Figure A-15).

[30] Universities are defined by their earliest control and degree level in IPEDS. R1 and R2 universities are research-oriented; colleges that are identified as two-year or categorized by Carnegie as primarily awarding Associate's degrees are two-year colleges.

[31] Table A-3 shows that both mid- and late-century institutional value-added statistics are strongly related to other contemporaneous measures of university quality like graduation rates and STEM degree shares, but that the relationship between value-added and university expenditures has strengthened over time.

Panels (b) and (c) of Figure 6 visualize this contrast by plotting the distribution of institutional value-added by parental income tercile in both 1963 and 1996. There was only slight institutional stratification by income in the 1960s, but by the end of the 20th century the value-added distribution of universities in the top income tercile had shifted substantially upwards.[32]

We next benchmark the causal interpretability of these estimates, as there is no scholarly consensus on the validity of $\widehat{Inst}_u$ in Equation 7. Quasi-experimental studies have identified contemporary forecast coefficients in the 0.7-0.8 range for Ivy institutions (Chetty et al., 2023) and about 2 for students on California public university admission margins (Bleemer, 2022, 2021).[33] We provide the first evidence on the causal interpretability of mid-century value-added statistics by estimating versions of:

$$\widehat{Inst}_u^C = \alpha^f + \beta^f \widehat{Inst}_u + \epsilon_i^f \tag{8}$$

which provides observational forecast coefficients for $\widehat{Inst}_u$ (as in Chetty et al., 2020). Holding $\widehat{Inst}_u$ fixed, we add additional covariates to Equation 7, estimate new university fixed effects $\widehat{Inst}_u^C$, and then interpret the second-stage β^f from Equation 8 as the share of variation in $\widehat{Inst}_u$ not explained by this further selection on observables, and thus the share that is plausibly causal.

Table 2 shows that detailed high school test scores, 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 0.78 in Project Talent. This suggests that at least 20 percent of the variation in $\widehat{Inst}_u$ reflects selection bias, mirroring Chetty et al. (2020)'s estimate of $\beta^f = 0.8$ for similarly-constructed contemporary value-added estimates. Restricting estimation of $\widehat{Inst}_u^C$ to lower-socioeconomic status students does not substantially reduce the forecast coefficients (Panel B); lower- and higher-income students appear to derive similar relative wage value from high-value institutions.

Attenuation may be small in this setting due to the shared ϵ_{it} terms in the second-stage (forecast) estimation. Since the same respondents are used to estimate $\widehat{Inst}_u$ and $\widehat{Inst}_u^C$, small-sample

[32] Figure A-16 shows that high- and low-*testing* students' distributions of value-added have stratified to an even greater degree since 1960, illustrating the rise of the meritocratic consensus (Hoxby, 2009; Bleemer and Rothstein, 2025).

[33] Forecast coefficients estimate the ratio between the causal treatment effect and the estimated index of institutional value-added, where the two could differ due to either selection bias or treatment effect heterogeneity. An alternative selection-on-observables design following Dale and Krueger (2002) results in 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.

bias could bias β^f toward 1. Panel C in Table 2 shows versions of Equation 8 where $\widehat{Inst}_u$ and $\widehat{Inst}_u^C$ are estimated in a split sample (stratified by institution). Though the smaller sample sizes lower the baseline correlations, adding covariates continues to reduce the forecast coefficient by only about 25 percent.[34]

We conclude that $\widehat{Inst}_u$ largely reflect causal differences across institutions. We maintain Chetty et al. (2020)'s assumption of $\beta^f = 0.8$ for contemporary value-added statistics and conservatively assume $\beta^f = 0.7$ for our noisier mid-century value-added statistics in Section 9's decomposition, though we also discuss the sensitivity of our findings to alternative forecast coefficients.

7.2 Institutional Composition

We observe initial enrollment institutions by parental income tercile in the CPS OCG, Great Aspirations, Project Talent, the NLS72, and HS&B surveys.[35] In order to measure changes in institutional value-added by parental income in more recent years, we compare enrollment by top two and bottom two parental income quintiles between 1998-2009 using IRS data reported by Chetty et al. (2020). After 2009, we proxy lower-income enrollment by the share of first-time first-year students at each institution who receive federal grant aid through the Pell grant program after 2009 in IPEDS.[36]

Figure 7 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.[37] Lower-income students enrolled at slightly lower-value institutions in both the early- and mid-20th century when measured using

[34] Note that we employ the full-sample estimates of $\widehat{Inst}_u$ below, for which these split-sample intraclass correlation coefficients are a lower bound.

[35] Students' transfer and dropout decisions are endogenous to their first enrollment institution, making it a more relevant and actionable determinant of outcomes than their modal or final institution. CPS OCG and Great Aspirations only elicited final (not initial) institution. The Time survey is omitted because it does not include two-year institutions.

[36] We approximate gaps in the average characteristics of initial enrollment institutions of top- and bottom-tercile students by linearly adjusting each observed gap toward the gap between those terciles (which is 66.7 parental income ranks). We measure Pell and non-Pell students' average parental wage rank using the every-three-years NPSAS survey (See Appendix A.14). For example, the median recipient (non-recipient) in 2016 came from a family earning about \$26,500 (\$100,000), the 24nd (69th) percentile of CPS household incomes in that year. As a result, we assume that the top/bottom tercile gap is equal to the Pell/non-Pell gap multiplied by $\frac{0.833-0.167}{0.69-0.24}$. Pell recipiency is unavailable by gender; we assume Pell gender shares correspond to the institution's gender share.

[37] Figure A-17 shows that when universities are valued by average wages (Chetty et al., 2020) – which relaxes the restriction to universities with observed value-added – enrollment gaps between income terciles reveal similar trends, though the gap is much larger because raw wage differences greatly exceed value-added differences.

1960s value-added, but by the 1980s, lower- and higher-income students attended similar-value institutions using their mid-century valuations. The pattern is markedly different when we apply universities' late-century valuations.[38] Lower-income students have always enrolled at far lower-value institutions according to this measure, with gaps ranging from 0.05 to 0.07 log dollars from the 1930s to the 2020s. In other words, had institutions persisted in their mid-century valuations, lower- and higher-income students would be enrolled at similar-value institutions today. Instead, changes in institutional valuations have led lower-income students to enroll at far lower-value institutions today than their higher-income peers.

Table 1 characterizes universities' measured quality and student composition over time in order to illuminate the determinants of this decline. The composition of lower- and higher-income students enrolling at higher-value private and research public universities did not meaningfully change between 1960 and 2010.[39] Changes in these institutions' student bodies therefore cannot explain increases in collegiate regressivity.

However, the teaching-oriented public institutions where lower-income students are concentrated had relative declines in revenue and graduation rates in the 1970s and 1980s (as shown by Bound et al., 2010).[40] Lower-income students have always enrolled at institutions with poorer measured quality than those enrolling higher-income students, but these quality gaps have grown over time (Table 3).[41] This divergence accelerated after 1990; the gap in annual CPI-adjusted per-student revenue grew (from a $4,000 base) by $63 per year between the 1960s and 1990s and then by $105 per year until the 2010s.[42] STEM degree attainment is similar at lower- and higher-income students' enrollment institutions, but lower-income students' universities have offered fewer STEM degrees over time as these schools' educational opportunities have declined (Figure A-19).

The second driver of lower-income students' declining institutional value-added is their dispro-

[38]Part of the difference between these value-added gaps could be explained by higher measurement error in the mid-century value-added estimates. However, those estimates' intraclass correlation coefficient (ICC) of 0.64 (Table 2) implies insufficient measurement error to explain the observed gap, even if the late-century estimates' ICC were 1. See Tables 1, 3, and A-4 for further evidence of quality declines at lower-income students' institutions.

[39]Baker et al. (2018) show that URM college-goers shifted toward less-selective institutions in the 1990s and 2000s.

[40]Figure A-18 shows that the overall dropout rate's stability obscures that the gap in dropout rates between bottom- and top-tercile students doubled between 1960 and 1990, tracking differences in their institutions' completion rates. Bound et al. (2010) and Denning et al. (2022) find graduation rates have changed slightly when measuring completion at a young age (25).

[41]Table A-4 replicates Table 3 but fixes enrollment compositions in the 1960s. We find that changes in institutional characteristics alone fully explain the growth in quality gaps by parental income in the four-year segments.

[42]Bound and Turner (2007) similarly employ per-student expenditures as a credible measure of institutional quality.

portionate diversion into two-year and for-profit colleges (Rouse, 1995). Both of these segments have persistently lower value-added and measured institutional quality than the teaching-oriented public universities from which these lower-income students were diverted. Figure 8 shows that lower-income students were 0–10 percentage points more likely to start at a two-year college than their peers from the start of our sample through the community college boom of the 1970s. Then the two-year college entry rate gap doubled, reaching 20 percentage points after 1990.[43] Increases in lower-income students' enrollment at for-profit institutions exacerbated this trend: for-profit enrollment rose from 0 to nearly 20 percent of lower-income students' college enrollment between 1980 and the 2000s (compared to 5 percent for high-income students), until regulatory changes shrank the sector in the 2010s. Combining these two segments indicates that only half of current college-goers from the bottom parental income tercile enroll at four-year universities, relative to about 70 percent among college-goers from the top tercile or lower-income college-goers in the 1960s. Diversion from four-year colleges diminishes the economic value of lower-income students' education, likely in part due to their receiving fewer years of college education due to failed transfers and dropout (Kane and Rouse, 1995).

We conclude that two factors related to collegiate institutions meaningfully contribute to the declining value of college-going in the US. First, the research-oriented public and private universities where higher-income students disproportionately enroll have seen large relative improvements in quality over the past 50 years. Second, lower-income students have been increasingly diverted toward lower-value two-year and for-profit universities since the 1980s. In contrast, the composition of four-year universities by parental income has been relatively stable over time. Each of these channels will be quantified in the decomposition below.

8 College Majors

8.1 Major Returns

Next we examine the contributions of college majors to collegiate regressivity. This necessitates measuring the relative economic value of each college major over time. We do this by estimating

[43] There were neither large tuition changes (Figure 4b) nor many campus openings (Bleemer and Quincy, 2025) in the 1980s or 1990s, suggesting that other factors explain this change.

the following linear model over high school graduates in each dataset that records both majors and early-30s incomes (Time, Wisconsin, CPS OCG, Project Talent, the NLSY's, and ACS):

$$w_{it} = Major_{m_i} + \zeta SomeCollege_i + \alpha_t + \epsilon_{it} \tag{9}$$

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. Some datasets record all declared college majors while others are restricted to majors declared by four-year college graduates; for consistency, we always set $Major_m$ to 0 and $SomeCollege_i$ to 1 for non-graduates. We do not control for graduate degree attainment, instead treating all subsequent educational outcomes as endogenous to major declaration and potentially part of majors' value.

Figure 9 shows $\widehat{Major}_m$ for each of the six most popular disciplines spanning the 20th century.[44] Though the overall college wage premium declined in the 1950s and rose thereafter, the ordering across disciplines' average wages has stayed remarkably constant over time, with humanities at the bottom and engineering and business majors earning the highest wages.[45] 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 has widened as the observational college wage premium increased.[46]

What share of cross-discipline or cross-major wage variation is the causal treatment effect of declaring that major? 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 between the average wages of graduates with majors A and B, students earn just over 1 dollar more by switching from A to B.[47] Since no such quasi-experimental evidence is available historically, we again employ a selection-on-observables strategy to estimate the forecast coefficient of average wages by major. For both coarse disciplines and detailed majors, we estimate multiple versions of

[44] 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-5 for coefficient estimates and Table A-6 for detailed major coefficient estimates.

[45] Figure A-20 shows that even detailed major categories have highly persistent premiums between the mid-20th century and the early 21st century, with a small number of outliers comprising occupation-specific degrees with changed labor demand (nursing, journalism) and/or degree requirements (law, veterinary medicine) over the period.

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

[47] Dahl et al. (2023) estimate a forecast coefficient of 0.96 in the context of Swedish high school majors. Arcidiacono (2004) presents a three-discipline structural model suggesting a forecast coefficient of about 0.7.

Equation 9 – a baseline version and versions with additional X_{it} covariates – using the NLSY97 and Project Talent (where all requisite respondent characteristics are observed). We then forecast the latter estimates ($\widehat{Major}_m^C$) with the baseline estimates ($\widehat{Major}_m$), weighted by enrollment:

$$\widehat{Major}_m^C = \alpha^f + \beta^f \widehat{Major}_m + \epsilon_i^f \tag{10}$$

Panel A of Table 4 shows evidence of a forecast coefficient (β^f) close to 1: conditioning on parental income, test scores, and race does not meaningfully attenuate the relative wage gaps between disciplines or majors in the NLSY. Restricting $\widehat{Major}_m^C$ to students with below-median parental incomes shows that those students derive marginally lower premiums from high-value majors than their higher-income peers, but that $\widehat{Major}_m$ remains a strong barometer of majors' value for lower-income students. Panel C shows that estimating $\widehat{Major}_m$ and $\widehat{Major}_m^C$ separately using a split NLSY97 sample results in lower baseline correlations due to the smaller sample size, but adding covariates still has no meaningful effect on the relative wages of different majors.[48] Panel D confirms the same findings in Project Talent, which also permits adding additional covariates like high school fixed effects (as in Table 2); the resulting split-sample β^f is 0.97.

We conclude that $\widehat{Major}_m$ approximately captures average *causal* wage differences between college majors. Going forward, we refer to $\widehat{Major}_m$ as a major's 'premium'. We discuss how a true $\widehat{Major}_m$ forecast coefficient below 1 would attenuate our findings in Section 9.

8.2 Major Composition

We observe college major declaration disaggregated by parental income in nationally-representative survey datasets every 5-10 years between the 1925 and 2000 age-18 cohorts, 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 declaration by students in the University of California system grouped by parental income between 1920 and 1945 and again between 1975 and 2015; Figure 10 shows that the California-specific data (denoted as solid lines) tracks national

[48] Our analysis below employs $\widehat{Major}_m$ estimates from Time and the ACS, both of which have much larger samples (and far less sampling error) than the NLSY97.

trends (denoted as symbols) in the years when both sources are available.[49] When available (all datasets other than Time, Wisconsin, and College Scorecard), we assign all college students (including dropouts) to their latest observed declared major, treating dropout as a potentially endogenous ramification of major choice. We measure the annual difference in the average $\widehat{Major}_m$ of majors declared by students from the top and bottom income tercile using three different estimates of $\widehat{Major}_m$, the log income return to each major: the returns to ten aggregated major disciplines in the 1930s, when detailed major declaration is generally unobserved ($Disc_m^{1932}$); ACS disciplinary returns in the 2000s ($Disc_m^{2005}$); and more-detailed ACS major returns in the 2000s ($Major_m^{2005}$).

First consider the light gray squares and lines in Figure 10, which show that 1920–1940 top-tercile students declared degrees in slightly higher-paying disciplines than their bottom-tercile peers when disciplines are valued by $Disc_m^{1932}$. This gap grew in the 1940s but fully disappeared by the 1980s. 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 10 replace $Disc_m^{1932}$ with $Disc_m^{2005}$. While the trends look somewhat similar, these valuations' 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 in the darkest shading and triangles ($Major_m^{2005}$): bottom-tercile students now declare majors worth about 4 percentage points less than those declared by their top-tercile peers, matching the largest gap in 100 years.[50]

Figure 11b 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) in order to pinpoint which disciplines and majors are most responsible for the recent rise in major stratification by parental income. It shows that computer science and economics/finance – the highest-value detailed majors – are also the biggest contributors: about 40 percent of the rise is attributed just to higher-income students' increased declaration of computer science degrees. Another 20 percent is explained by lower-income students' increasing declaration of lower-value humanities degrees. Figure 11a confirms these trends: the well-known national

[49]Figure A-21 shows that California exhibits slightly below-average major stratification by parental income in both the 1960s and 2010. The South and Midwest tend to have higher levels of major stratification.

[50]Bleemer and Mehta (2022a) show a similar trend in the value of college majors earned by underrepresented minority students. Startz (2024) documents contemporary Pell stratification across majors in the College Scorecard.

decline in humanities degree attainment (e.g. Schmidt, 2018) has been driven by higher-income students. The majority of students remaining in those majors are from lower-income backgrounds. The rise in computer science attainment has also been largely driven by higher-income students, likely as a result of severe academic restrictions on access to those high-value majors in recent years (Bleemer and Mehta, 2022a).[51] These two enrollment changes alone explain a 3 percentage point decline in lower-income students' value of college-going since 1995.[52]

9 Decomposing Rising Regressivity in US Higher Education

Figure 12 visualizes the combined contributions of pre-college cognitive skill, enrollment institution, and major to the rise in collegiate regressivity over the past century by presenting direct measurements of D_t and each of the six components named in Equation 6. The gray diamonds are the dataset-specific estimates of D_t. The solid line, formally $D_t \approx (\frac{5}{6} - \frac{1}{6})(t - 1950) \times \delta$, multiplies the best fit line from Figure 3 (estimated in log dollars) after 1950 by the approximate difference in parental income ranks between the top and bottom terciles. Figure 3 suggests that D_t is only slightly positive between 1920 and 1950, but it has grown to be about 0.15 log dollars in recent years. We also summarize the contributions of each component by period in Table 5.

The first two components measure the magnitude of selection in explaining the rise in collegiate regressivity. The first term, Test Returns, holds the standardized test composition of lower- and higher-income college-goers fixed at its earliest measure – 1942, in the World War II draft cards – and then plots the product between the gap between top- and bottom-tercile college-goers' test scores in that year and the measured value of higher test scores as estimated in each of the datasets shown in Figure 5, linearly smoothing between estimates.[53] Because there is no such gap in the 1940s, the resulting component – plotted in orange in Figure 12 – is always zero.

The second term, Test Composition, measures the contribution of changes over time in the skill composition of college-goers by parental income. As presaged by the widening testing

[51] Lower-income students have not kept up with higher-income students' expanding engineering presence in recent years, but this is largely explained by their lower enrollment in computer science (Figure A-22).

[52] Replicating this decomposition in the 2010 College Scorecard similarly shows economics and finance to be the two largest components. It also reflects that California is a leading indicator for computer science, as that major had not yet begun its recent national growth and was still more likely to be earned by lower-income students in 2010.

[53] This abstracts from content differences between tests, which are reflected in the Test Composition component.

gap between lower- and higher-income students during the 1960s in Figure 5b, Figure 12 shows that students' measured pre-college cognitive skill explains part 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 cognitive skill peaked.[54] However, the role of testing composition has shrunk since that time. In all, Table 5 shows that differential selection into college-going explains only 8 percent of the rise in regressivity after 1960.

Next we turn to the contributions of institutions, shown in green. The Institutional Returns component measures the degree to which changes in the relative value of collegiate institutions contributes to the growth in collegiate regressivity, fixing higher- and lower-income students' institutional composition in the 1930s. Figure 7 shows that the institutions where lower-income students have continuously enrolled over the past 100 years have substantially declined in economic value. Differences in the return to institutional choice explained less than 2 percentage points in the gap in the value of college-going between lower- and higher-income students before the 1960s. Since then, their importance has tripled; today they explain over 5 percentage points of overall collegiate regressivity.[55] Almost one-third of the growing disparity in the college premiums by parental income comes from the falling relative value of colleges where lower-income students disproportionately enrollment, especially public teaching-oriented universities.

We split the Institutional Composition component – $\sum_u \Delta_q \big[P_t(u|q) - P_0(u|q)\big] v_t^u(u)$ – into three terms: those where u is a traditional four-year university, a two-year college, or any for-profit university. The four-year university term has almost always been negative. It became even more negative between 1980 and 2010, a period with growing access-oriented admissions policies at selective universities (e.g. race-based affirmative action). However, this trend was more than offset by rising lower-income enrollment at community colleges and for-profit colleges since the 1980s and 1990s. Together, these two components substantially decreased the value of college-going for lower-income students, and explain over 20 percentage points of the rise in collegiate regressivity.

Finally, we turn to the contributions of college majors, shown in blue. The Major Returns component fixes the composition of college majors declared by lower- and higher-income students

[54] These trends could also reflect improvements in the tests' measurement of pre-college cognitive skill.

[55] These estimates assume forecast coefficients of 0.7 and 0.8 for mid- and late-century value-added estimates. This gap would be linearly scaled upward (downward) if the true mid-century (late-century) forecast coefficients were lower. For example, if the true late-century forecast coefficient was 0.7, the share of collegiate regressivity explained by institutional returns would decline from 5.3 to 4.7 percent.

to their 1920s level (in the Census-linked Time survey) and measures the degree to which changes in the relative returns to different college majors by parental income have contributed to rising regressivity.[56] As shown in Figure 10, the relative returns to different college majors have remained largely unchanged over time, so these changes have contributed little to collegiate regressivity.

The Major Composition component shows that between-major shifts explain part of the rise of collegiate regressivity in the 1970s, but have become an important contributor to collegiate regressivity only since 2000. Between 2000 and 2014, higher-income students were more likely to shift out of humanities majors and into computer science and other lucrative fields, likely due to entry restrictions into those majors excluding lower-income students (Bleemer and Mehta, 2022a). As a result, higher-income students' relative value of college-going has risen by over 4 percentage points, explaining another quarter of current collegiate regressivity.[57]

In sum, the relative decline in American higher education's value for lower-income Americans can largely be explained by three factors: the rising relative value of higher-income students' research-oriented universities, lower-income students' increasing diversion to community and for-profit colleges, and lower-income students' redirection out of computer science and engineering into the shrinking humanities disciplines within four-year universities. Differential selection into college, reallocation of students within the four-year sector, and changes in the value of different college majors have played minimal roles, especially in recent years.

10 College-Going Among Women

Measuring changes in the wage 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 A-23 demonstrates that female students' *household* income return to college-going has become substantially more regressive over time (partly reflecting college-educated women's tradeoffs between career and family, e.g. Goldin (1997)), though from a more

[56]Our earliest measure of major composition is at the discipline level, We fix the 1921 composition by using the disciplinary compositions in 1921 and then assume that the gap in value between detailed majors would have grown between 1921 and 1937 – our earliest observed majors, in CPS OCG – by as much as it did between disciplines.

[57]As in the case of institutions, these estimates scale linearly in the forecast coefficient of measured major value. If the true forecast coefficient were only 0.8 – that is, if only 80 percent of wage variation between majors is causal – then the share of collegiate regressivity explained by major composition would remain almost 4 percentage points.

progressive base than male wages. Figure 13 indicates that the determinants of collegiate regressivity have become regressive for women as enrollment patterns have converged across gender.

As with men, Figure 13a 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 or major declaration to collegiate regressivity for women until the latter two factors turned regressive in the 1990s.[59] Figure 13b suggests that test score-based selection does not appear to have changed by parental income over the last half of the 20th century. Lower-income women declared *higher*-premium majors than their higher-income peers for most of the 20th century, though the past 20 years' patterns mirror the trajectory of male regressivity.[60] Relative institutional returns appear more similar for female and male students, and have also declined among lower-income female college students in recent years, as seen in Figure 13d. [61] Together, these results indicate that college has become more regressive for both male and female students in the past 100 years, perhaps beginning from a more progressive base for women.

11 Discussion

American universities have provided college-goers with high average wage premiums for over a century, 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 traditional four-year university sector than their peers, experienced declines in the relative value of the public teaching-oriented universities where they have long enrolled, and become less likely to study high-value fields like computer science than their higher-income peers. These forces – more than changes in high-quality university access or net tuition costs – now lead students from the bottom parental

[58]College-going has risen more among women across income terciles than men (see Bailey and Dynarski, 2011).

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

[60]Women's major declaration trended towards that of men between 1960 and 1980, but women still declare far lower-paying majors than men 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.

income tercile to earn less than 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. Assuming that 10 percent of the rise in regressivity is the result of differential selection (see Table 5), we find that equalizing the college-going wage premium across the parental income 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 very little prior to 1960. For comparison, equalizing collegiate *attainment* by parental income – under the assumption that 80 percent of the return to college is causal – would reduce the rank-rank correlation by only 0.014 points more (to 0.189), but would require eradicating a 100-year-old gap in college dropout rates (see Figure A-18) rather than adjusting existing programs.[62]

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, the phase-in and phase-out of race-based affirmative action, the phase-in of grade-based restrictions on access to lucrative college majors, 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 that closing the value-added gap for male and female students' *modal* enrollment institution would decrease intergenerational income transmission by 25 percent, but this would require changes in transfer and completion likelihoods in addition to changes in initial college-going. Bloome et al. (2018) discuss similar simulations equalizing educational attainment by parental income.

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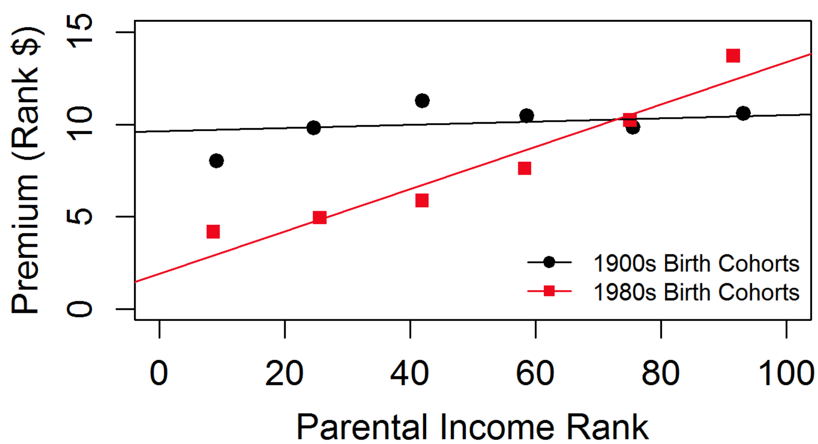
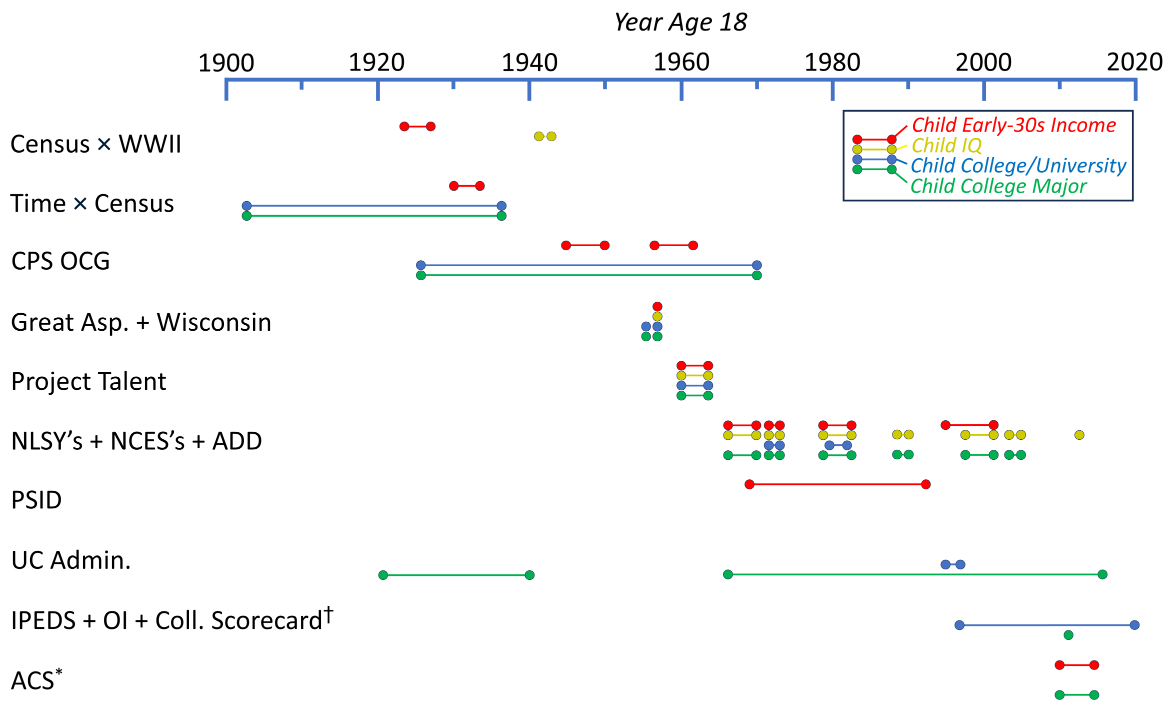
Figure 1: The Observational Wage Premium to Going to College by Parental Income



Note: The difference between the average wage income rank among employed early-30s men who had completed at least one year of college and those who completed high school but not any years of college, by quantile of parental income rank. The 1905–1910 ('1900s') cohorts are measured among father-son pairs matched in the 1920 and 1940 Censuses following Abramitzky et al. (2012) with imputed father's incomes; the '1980s' cohorts are measured among youths in the ADD Health survey (1977–1981) and the NLSY97 (1980–1984) and use survey weights. The best-fit lines are simultaneously estimated without additional covariates in the underlying data; their slopes are 0.009 (s.e. 0.003) and 0.114 (0.030), and the t-statistic of their difference is 3.52. The sample includes 352,995 men in the earlier cohorts and 4,072 men in the later cohorts. Unemployed sons and those with incomes below the contemporaneous half-time federal minimum wage are omitted. See Appendix A for details on data construction. Source: US Census, ADD Health, and NLSY97.

Figure 2: Summary of Data Sources

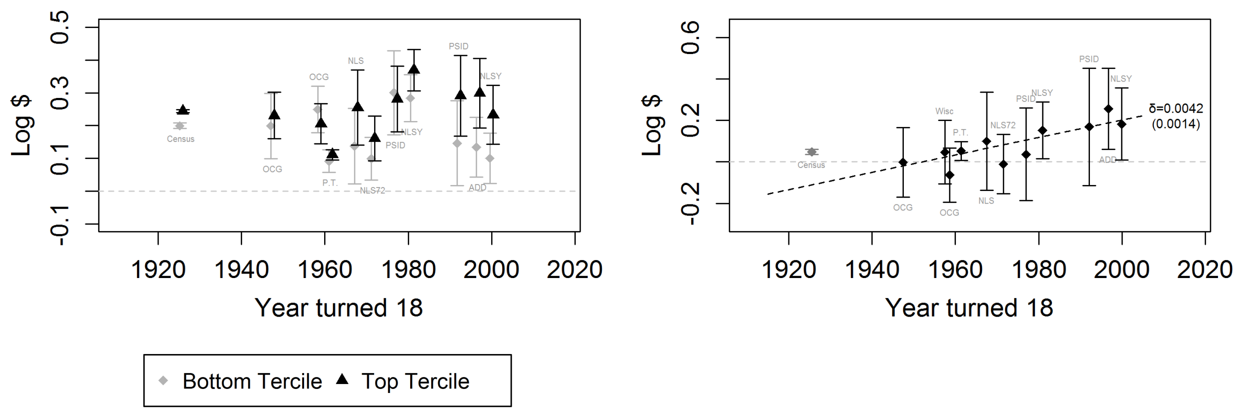


Note: Available cohorts of longitudinal, retrospective, or cross-sectional datasets used to measure changes in the relative value of college-going, test performance, institutions, and majors in the US labor market since 1920. Except where noted, each data source is nationally representative. Parental 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 x WWII' refers to the Helgertz et al. (2023) 1940–50 Census crosswalk linked to 1943 AFQT on enlistment cards (following Abramitzky et al., 2012); 'Time Magazine' to Time's 1947 College Graduate Survey; 'CPS OCG' to the retrospective Occupational Changes in a Generation CPS supplements; 'Great Asp.' to the Career Plans and Experiences of June 1961 College Graduates survey; 'Wisconsin' to the Wisconsin Longitudinal Study (restricted to Wisconsin high school graduates); 'Project Talent' to the AIR study by that name; 'NLSY's + NCES's + ADD' and 'PSID' to the federal longitudinal studies by those acronyms (NLSY: NLS Young Men/Women, NLSY79, and NLSY97; NCES: NLS72, HS&B, NELS, ELS, and HSLS); 'UC Admin' to administrative University of California records (restricted to enrollees of those institutions, and replacing parental income with average local residential income); 'IPEDS + OI + College Scorecard' to institution-level university enrollment characteristics from federal IPEDS data and the IRS (Chetty et al., 2020) and institution-major degree attainment from the College Scorecard; and 'ACS' refers to cross-sectional wage and major data from the American Community Survey. * Parental income is not observed. † Data are observed at the institution (not individual) level.

Figure 3: Regressivity of US Higher Education Over Time

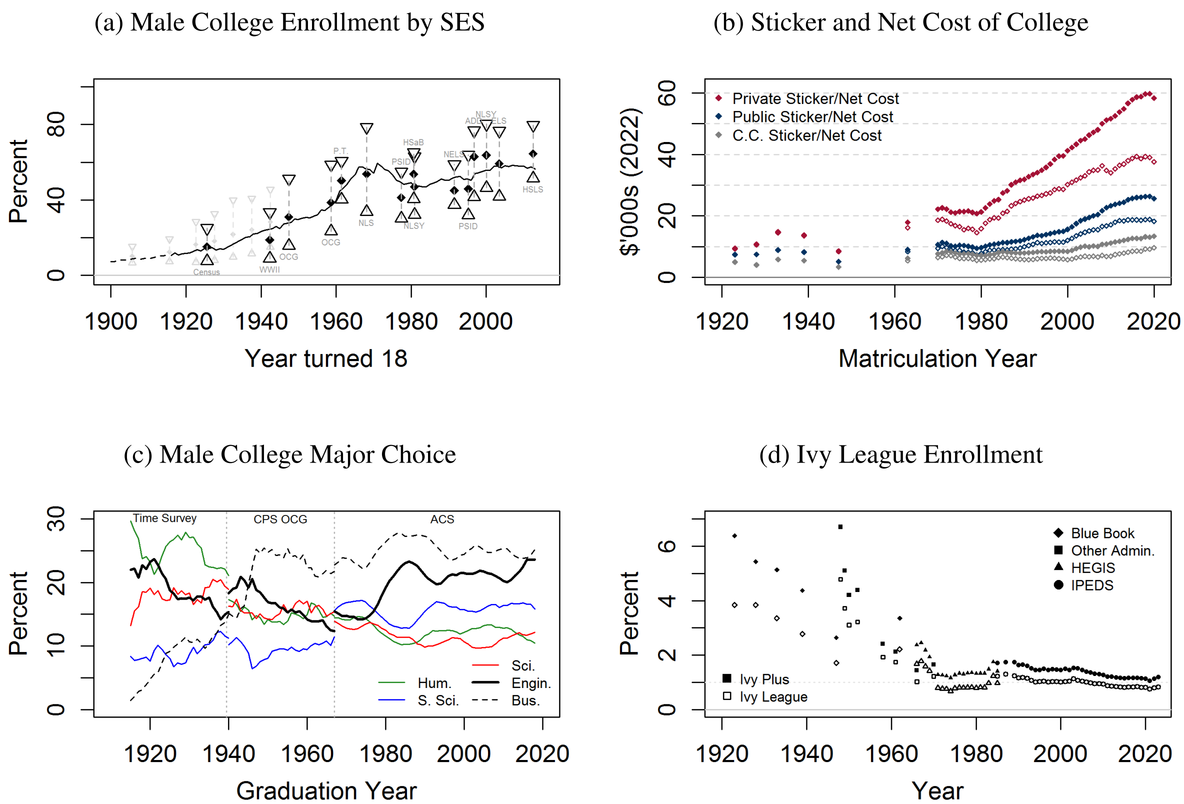
(a) Average Return to Enrollment by Tercile

(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. Estimated following Equation 1 in the pooled dataset. **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.

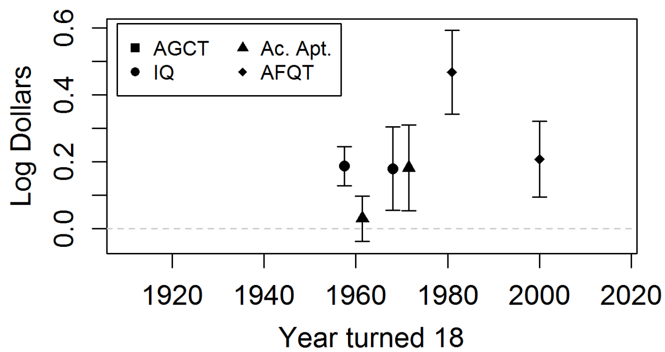
Figure 4: Trends in US Higher Education



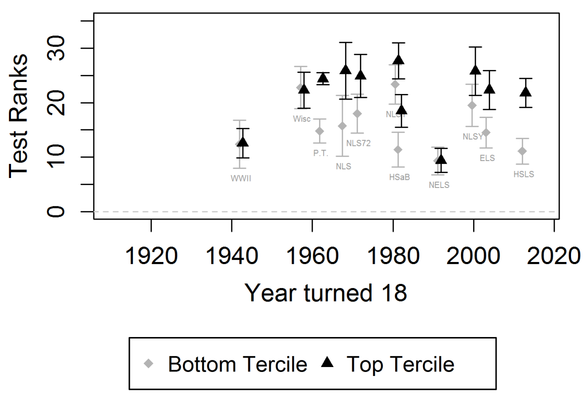
Note: **Panel (a)**: Points in black show the share of men between ages 30 and 35 who had completed at least one year of college overall (black diamond) or among those from the bottom (△) or top (▽) tercile of parental incomes when age 14–17. The solid line reports the same overall average educational outcome for 1940 and 1960–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, 2021, and 2023 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 bin 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.18 for data construction and sources for (b); and Appendix A.19 for data construction and sources for (d).

Figure 5: Pre-College Cognitive Skill Trends

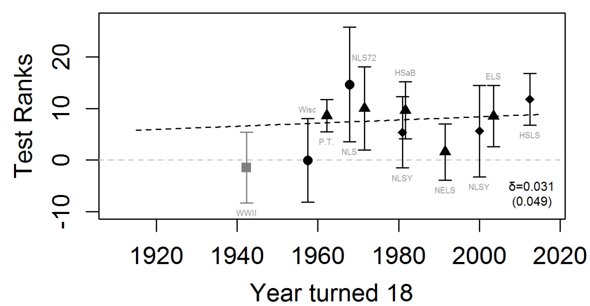
(a) Return to Pre-College Cognitive Skill



(b) Average HS-College IQ Gap by Tercile



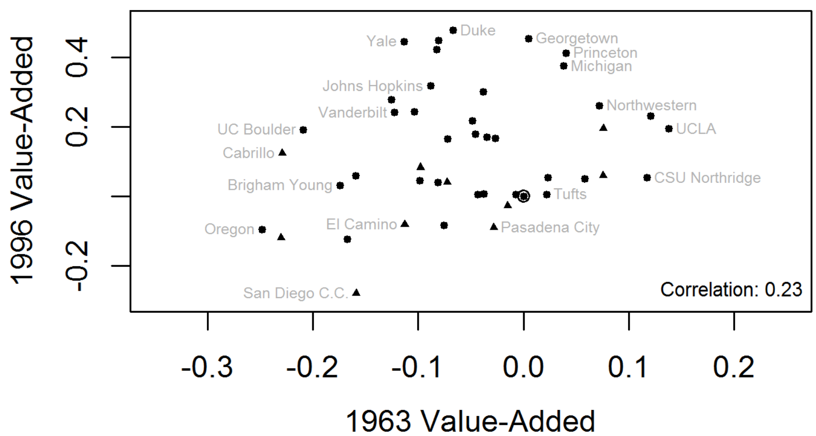
(c) Differential Selection into College Enrollment



Note: **Panel (a)** plots the estimated log income return to measured male pre-college cognitive skill 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 skill. 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. AGCT, IQ, Academic Aptitude, and AFQT exams are transformed into within-sample ranks; see Appendix A for details on variable definition and data construction. Source: US Census, WWII Enlistment Cards, Wisconsin, Project Talent, NLSM, NLS72, HSaB, NLSY79, NELS, NLSY97, ELS, and HSLS.

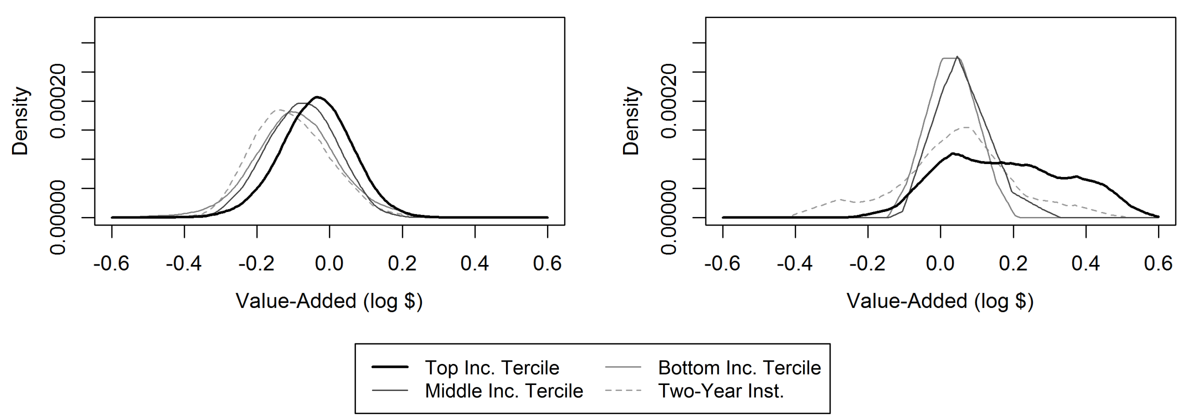
Figure 6: Institutional Wage Value-Added in the 1960s and 1990s

(a) Comparison Between 1963 and 1996 Wage Value-Added



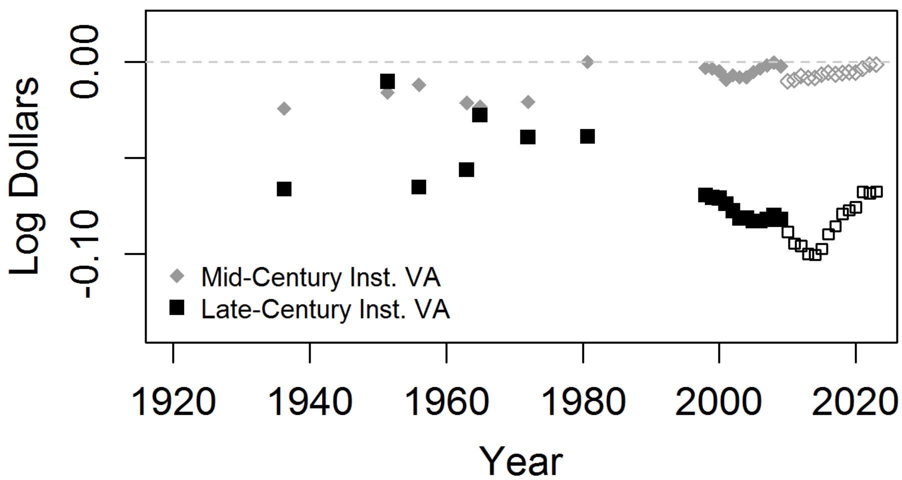
(b) Distribution of 1963 Wage Value-Added

(c) Distribution of 1996 Wage Value-Added



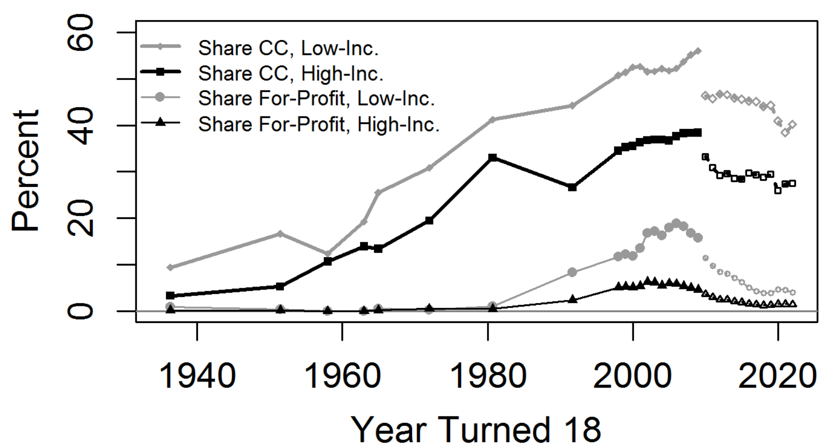
Note: The institutional value-added of US colleges and universities in log dollars estimated from 18-year-olds in 1963 ("mid-century", using Project Talent) and 1996 ("late-century", from University of California applicant records), estimated relative to CSU Long Beach (which is set to 0) and visualized 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); there are no for-profit universities in the sample. 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 7: Institutional Enrollment by Student Socioeconomic Status



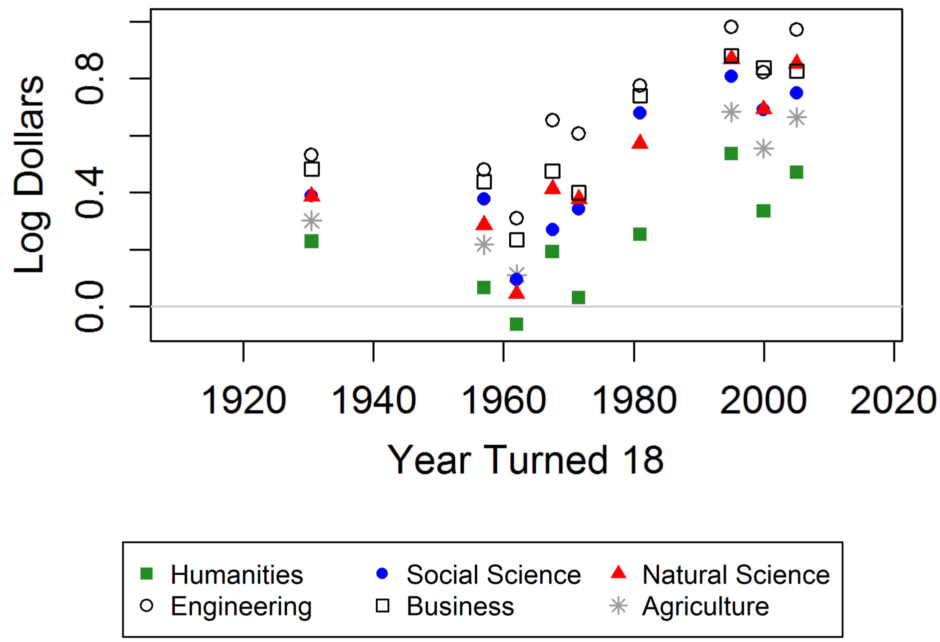
Note: The difference in average institutional value-added of the enrollment institutions between students from the bottom and top parental income tercile. The institutional value-added of US colleges and universities estimated from 18-year-olds in the mid-20th century (using Project Talent) and the late 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.14 for details. Pell and non-Pell enrollments are reweighted to match total enrollments by segment 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, Great Aspirations, Project Talent, NLS72, HSaB, IPEDS, NPSAS, Bleemer (2022) Appendix I (late-century value-added estimates), and Chetty et al. (2020).

Figure 8: Enrollment in Two-Year and For-Profit Institutions by Parental Income Tercile



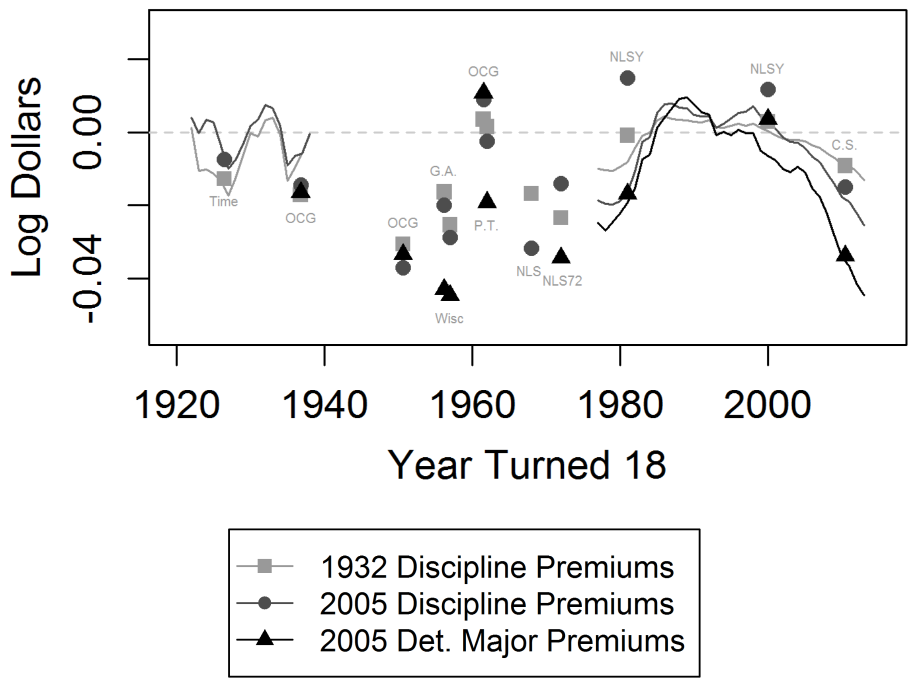
Note: Annual share of first-time first-year enrollees who attend two-year colleges (excluding for-profits) and for-profit institutions among students from the bottom and top parental income tercile. Income terciles are proxied by being in the top two or bottom two parental income terciles in 1998-2009 (Chetty et al., 2020) – and institutions are measured as the modal enrollment institution by age-18 cohort – and by the share of first-time first-year students receiving Pell grants in 2010-2023 (marked by open shapes, since non-Pell students are an imperfect proxy for top-tercile students). Source: CPS OCG, Wisconsin, Project Talent, NLS72, HS&B, NELS, Chetty et al. (2020), and IPEDS.

Figure 9: 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. See Appendix A for details on data construction. Source: Time survey, US Census, Project Talent, Wisconsin, NLS, NLSY79, NLSY97, and ACS.

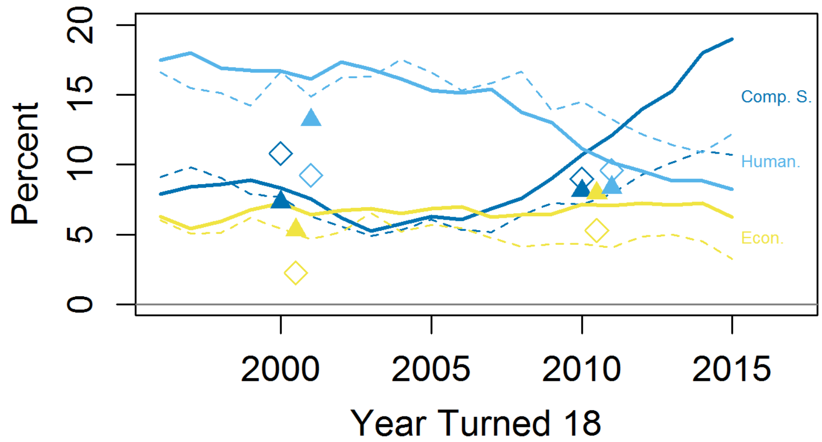
Figure 10: Difference in Average Major Premium by Student Parental Income Tercile



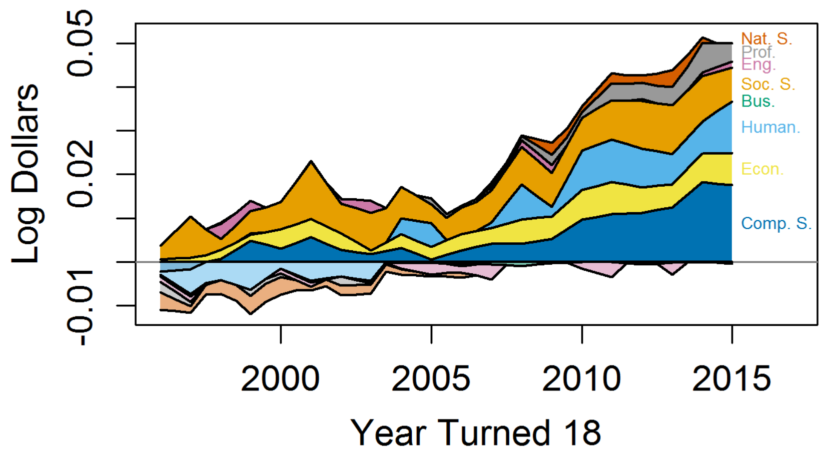
Note: The difference in average major premiums declared by male University of California enrollees (lines) or nationally-representative male respondent graduates (symbols) from the bottom and top parental income tercile in that year, where negative values indicate that lower-income students declare relatively lower-paying majors. Major premiums are measured 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 9). 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.14). 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, Time survey, CPS OCG, Great Aspirations survey, Wisconsin, Project Talent, NLSM, NLS72, NLSY79, NLSY97, College Scorecard, and the ACS (Ruggles et al., 2024).

Figure 11: College Major Attainment by Parental Income Since 1995

(a) Annual Share of Declared College Majors by Parental Income Tercile

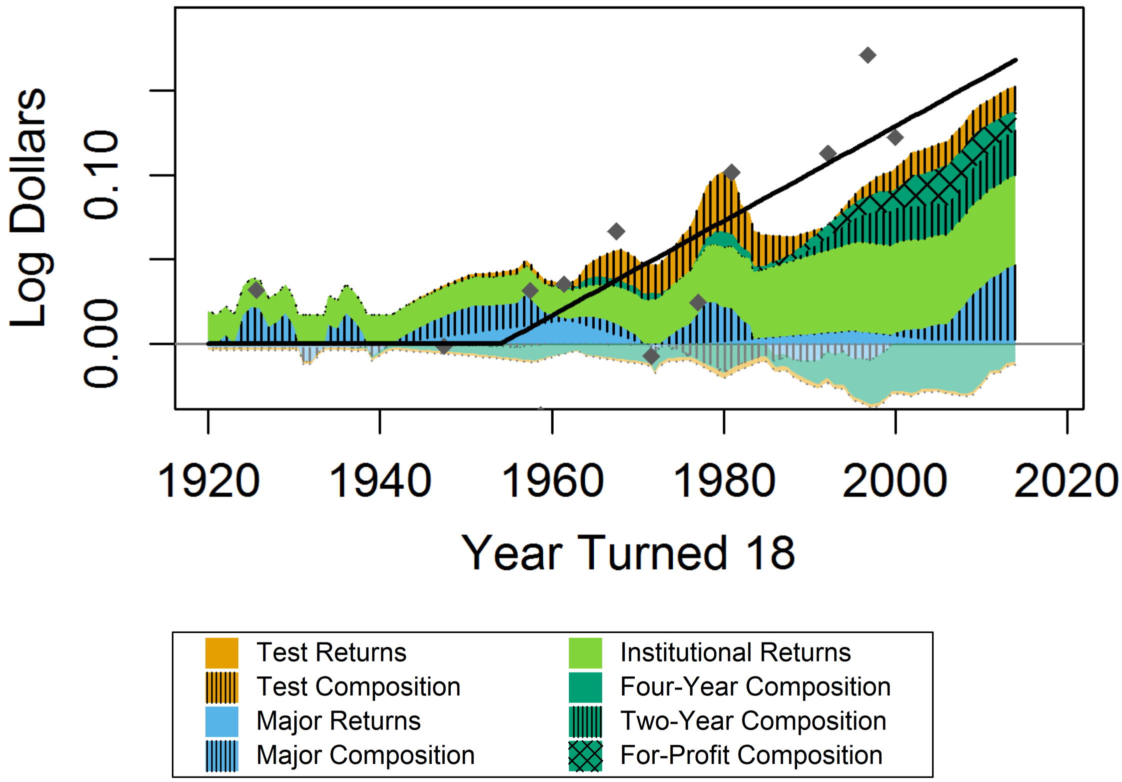


(b) Decomposition of Rising Major Stratification by Discipline



Note: **Panel (a)**: 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 recipients in the College Scorecard; see Appendix A.14). **Panel (b)**: 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). Negativ values are reflected below the x-axis. 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, IRS SOI, NLSY97, the College Scorecard, and the ACS (Ruggles et al., 2024).

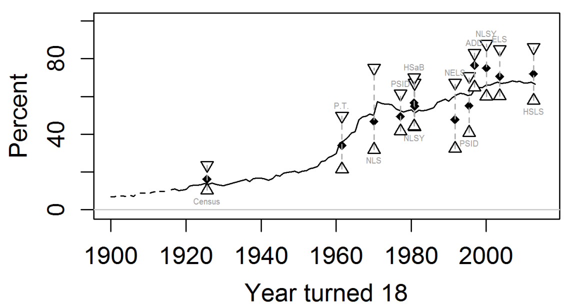
Figure 12: Contributions to the Rise in Collegiate Regressivity in the US



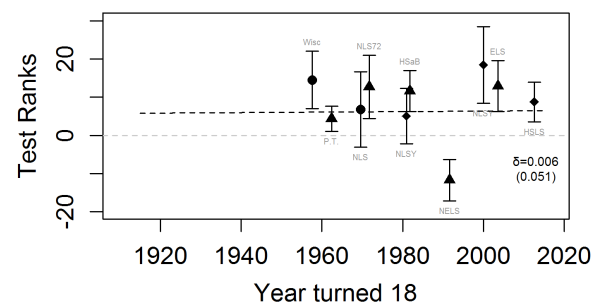
Note: This figure decomposes the rise in regressivity of US college enrollment – shown by scaling the non-parametric (gray dots) and parametric (black lines) estimates from Figure 3 to a comparison between students from top- and bottom-tercile parental incomes – into the components defined in Equation 6. **Test Returns** holds the standardized-test-rank composition of college-going students by parental income fixed in 1932 (WWII) and measures the effect of changes in the wage return to tests over time, measured from 1957 to 2003. **Test Composition** measures the additional effect of changes in the test-rank composition of college-going students by parental income until 2010. **Major Returns** holds the major composition of college-going students by parental income fixed in 1921 (Time, assuming that detailed majors would have trended by about as much as disciplines trended between 1921 and 1936, when detailed majors are first observed in OCG) and measures the effect of changes in the wage return to majors over time, measured from 1931 to 2005. **Major Composition** measures the additional effect of changes in the major composition of college-going students by parental income until 2015. **Institutional Returns** holds the institutional composition of college-going students by parental income fixed in 1936 (OCG) and measures the effect of changes in the wage return to different institutions over time, measured in 1962 and 1996. **Four-Year**, **Two-Year**, and **For-Profit Composition** split the final term of the decomposition into three pieces depending on each institution's segment – assigning all for-profit institutions to the latter category and all remaining two-year institutions or institutions classified by Carnegie as awarding mostly Associates degrees to the middle category – and measure the additional effect of changes in the institutional composition (in that segment) of college-going students by parental income until 2015. See Section 9 for details on construction of this decomposition. See Figure 2 and Appendix A for details on data construction and sources.

Figure 13: Differential Trends in US Women's College Choices by Parental Income

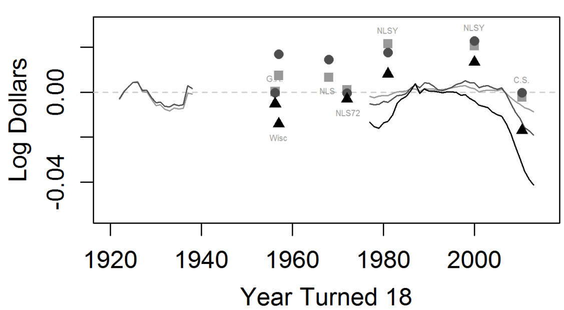
(a) College Enrollment



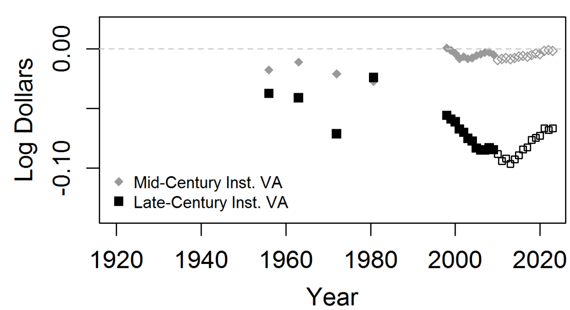
(b) Pre-College Cognitive Skill



(c) Major Enrollment



(d) Institutional Enrollment



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. The solid line reports the same overall average educational outcome for 1940 and 1960–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, 2021, and 2023 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 skill. Further information can be found in Figure 5b. **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 9). Figure 10 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 7 for sources and definitions. See Figure 2 and Appendix A for details on data construction and sources for all panels.

Table 1: Changes in College Quality by Segment Since 1960

	Average 2010s Characteristics					Change Since 1960s				
	Public Research	Public Teaching	Private	For-Profit Inst.	Two-Year College	Public Research	Public Teaching	Private	For-Profit Inst.	Two-Year College
***Panel A**: All Institutions*										
Log Wage Value-Added (log $)	0.09 [0.11]	0.03 [0.08]	0.09 [0.15]	-0.15 [0.00]	0.00 [0.12]	0.15 (0.01)	0.08 (0.01)	0.12 (0.01)		0.07 (0.01)
Students per Faculty	19.8 [5.5]	24.7 [8.2]	22.7 [18.0]	60.0 [30.9]	36.0 [12.4]	3.0 (0.2)	6.9 (0.2)	6.9 (0.4)		14.7 (0.3)
Total Rev. per Student ($'000s)	51.9 [18.3]	27.2 [9.7]	44.5 [21.4]	23.1 [10.8]	20.0 [8.2]	32.4 (0.4)	16.9 (0.2)	30.1 (0.4)		10.9 (0.2)
***Panel B**: Four-Year Institutions*										
Six-Year Grad. Rate (%)	60.6 [14.9]	45.6 [12.4]	63.0 [17.1]	54.6 [18.7]		1.0 (0.5)	-9.7 (0.5)	1.2 (0.7)		
Share of Degrees in STEM (%)	22.1 [9.1]	13.1 [7.5]	14.9 [11.5]	10.5 [12.2]		-8.9 (0.4)	-8.9 (0.3)	-12.9 (0.5)		
Normalized Avg. Test Score	0.76 [0.50]	0.06 [0.44]	0.64 [0.71]	0.29 [0.48]		-0.40 (0.02)	-0.90 (0.02)	-0.63 (0.03)		
***Panel C**: Four-Year Enrollment Shares by Tercile, and Share of All Enrollment in Two-Years*										
Bottom Terc.	21.1	31.8	19.3	27.8	57.1	-15.7	-9.7	-2.4	27.8	33.9
Top Terc.	31.6	32.4	30.9	5.0	37.0	-13.4	9.5	-1.1	5.0	22.4

Note: The mean 2015 enrollment-weighted characteristics of institutions by segment, and changes in the mean since the early 1960s. Standard deviations are in brackets and standard errors are in parentheses. Enrollment shares are among 2009 students at four-year universities from the bottom two and top two parental income quintiles (Chetty et al., 2020), and the share who enroll at two-year colleges. '**Students Per Faculty**' is total enrollment per total faculty in the 1962 Blue Book or FTE enrollment per full-time faculty in 2015 IPEDS; '**Total Revenue Per Student**' is total annual income per enrolled student (1962 Blue Book) or total non-hospital revenue per FTE student (2015 IPEDS), both CPI-adjusted to 2022; '**Six-Year Graduation Rate**' is the share of 1960s (Project Talent) or 2009 (IPEDS) freshman students who graduate from that institution within six years; '**Share STEM Degrees**' is the percent of BAs given in biological sciences, computer science, mathematics, physical sciences or engineering as reported in the 1966 HEGIS (among men) or 2015 IPEDS; '**Normalized Avg. Test Score**' is the institution's average academic score (Project Talent) or 2015 SAT score (the summed mean of the schools' 25th and 75 percentiles of math and reading subscores), both standardized across all test-takers; and '**Log Wage Value-Added**' is estimated following Equation 7 in the 1960s using age 29 wages from Project Talent and in the 2000s using age 31–35 California wages among 1995–1997 UC applicants as reported in Appendix I of Bleemer (2022). 1996 value-added estimates are propensity-score-weighted to represent US higher education by segment interacted with expenditures, average parental income, and first-time enrollment. Value-added estimates are unshrunk and estimates are unadjusted for VA sampling error. All values are winsorized at an enrollment-weighted 5 percent to reduce noise in the historical statistics. Source: 1962 Blue Book, Project Talent, HEGIS, IPEDS, and Chetty et al. (2020).

Table 2: Selection-on-Observables Forecast Coefficients of Institutional Value-Added

Add'l Cov.:	Base	+Tests	+Grades	+HS FE	+Extra.
***Panel A**: Full Sample, Project Talent*					
Inst. FE		0.98	0.96	0.79	0.78
		(0.003)	(0.005)	(0.022)	(0.022)
Obs.				521	
1st Stg. Obs.				36,348	
***Panel B**: Students with Below-Median Parental Income, Project Talent*					
Inst. FE	1.16	1.15	1.13	0.82	0.81
	(0.056)	(0.055)	(0.056)	(0.082)	(0.083)
Obs.			179		
1st Stg. Obs.			8,660		
***Panel C**: Split Sample, Project Talent*					
Inst. FE	0.64	0.63	0.61	0.47	0.47
	(0.054)	(0.054)	(0.053)	(0.054)	(0.054)
Obs.			269		
1st Stg. Obs.			14,409		

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 and academic performance and indicators for Black and other non-white races, all interacted with survey cohort). 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 (for the 95 percent of students from a high school with at least 10 respondents), and self-reported indices of 'Extracurricular' participation, reading, hobbies, sports, leadership, and 'socialness'. Panel A uses the same baseline sample to estimate both sets of institution fixed effects; Panel B restricts the left-hand-side set of institution fixed effects to be estimated in a first-stage regression of only graduates from families with below-median family incomes; and Panel C splits the sample evenly (within institution) and jointly estimates each set of separate fixed effects, restricting to schools with at least 20 respondents in each sample. '1st Stg. Obs.' reports 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 and female 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 3: Changes in College Quality by Parental Income Since 1960

	Average Inst. Characteristics by Parental Inc. Tercile							
	1960s		1990s		2010s		Differences	
Var.	Bottom	Top	Bottom	Top	Bottom	Top	90s-60s	10s-60s
***Panel A**: All Institutions*								
Students Per Faculty	18.2 [9.8]	19.0 [13.2]	27.1 [10.2]	23.5 [9.8]	31.8 [12.4]	26.6 [11.8]	4.4 (0.6)	6.0 (0.6)
Total Rev. per Student ($‘000s)	14.1 [9.4]	18.5 [11.4]	22.2 [13.5]	28.7 [17.4]	26.4 [15.3]	35.0 [21.7]	-2.2 (0.8)	-4.2 (0.7)
***Panel B**: Four-Year Institutions*								
Students Per Faculty	14.7 [11.0]	15.3 [13.7]	19.5 [5.5]	18.3 [5.3]	23.8 [9.1]	20.6 [7.2]	1.9 (0.7)	3.8 (0.7)
Total Rev. per Student ($‘000s)	10.8 [10.5]	13.8 [13.0]	31.6 [16.1]	36.5 [18.2]	38.2 [20.6]	45.4 [23.9]	-1.9 (1.0)	-4.2 (1.1)
Six-Year Grad. Rate (%)	56.0 [10.8]	59.1 [11.7]	52.1 [16.7]	59.6 [17.0]	50.7 [15.9]	59.0 [16.2]	-4.4 (1.0)	-5.1 (0.9)
Endowment per Student ($‘000s)	5.7 [16.6]	11.3 [23.4]	22.6 [41.2]	37.5 [57.1]	25.3 [41.1]	42.1 [56.8]	-9.3 (2.8)	-11.3 (2.6)
Share of Degrees in STEM (%)	25.8 [10.5]	27.3 [11.1]	13.5 [8.1]	15.0 [8.5]	13.3 [9.5]	15.7 [9.7]	-0.0 (0.7)	-1.0 (0.6)
Normalized Avg. Test Score	0.96 [0.27]	1.14 [0.26]	0.35 [0.55]	0.59 [0.57]	0.30 [0.61]	0.57 [0.63]	-0.06 (0.03)	-0.10 (0.03)

Note: The mean institutional characteristics of schools where 1960-1963 students from the bottom and top parental income terciles (1960s, Project Talent) and 1998 or 2009 students from the bottom two and top two parental income quintiles (1990s and 2010s, Chetty et al., 2020) enrolled, and the difference-in-differences for lower-income and higher-income students between each period. Standard deviations are in brackets and standard errors are in parentheses; all values are winsorized at an enrollment-weighted 5 percent to reduce noise in the historical statistics. ‘**Students Per Faculty**’ is measured as total enrollment per total faculty in the 1962 Blue Book or FTE enrollment per full-time faculty in 1997 or 2015 IPEDS; ‘**Total Revenue Per Student**’ is measured as total annual income per enrolled student (1962 Blue Book) or total non-hospital revenue per FTE student (1995 or 2015 IPEDS), both CPI-adjusted to 2022 dollars; ‘**Six-Year Graduation Rate**’ is the share of 1960s (Project Talent) or 2009 (IPEDS) freshman students who graduate from that institution within six years; ‘**Endowment Per Student**’ is measured as endowment per enrolled student (1962 Blue Book) or endowment per FTE student (2007 or 2015 IPEDS), both CPI-adjusted to 2022 dollars; ‘**Share STEM Degrees**’ is the percent of BAs given in biological sciences, computer science, mathematics, physical sciences or engineering as reported in the 1966 HEGIS (among men) or 1995 or 2015 IPEDS; and ‘**Normalized Avg. Test Score**’ is the institution’s average academic score (Project Talent, column 1) or 2001 or 2015 SAT score (the summed mean of the schools’ 25th and 75 percentiles of math and reading subscores), both standardized across all test-takers. Year of observation and sample sizes vary by variable due to data availability in the Blue Book, Project Talent, HEGIS, and IPEDS.

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

Table 4: 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	+More
***Panel A**: Full Sample, NLSY97*									
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**: Students with Below-Median Parental Income, NLSY97*									
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			
***Panel C**: Split Sample, NLSY97*									
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 D**: Split Sample, Project Talent*									
Full-Sample	0.98	0.98	0.97	0.97	1.09	1.07	1.03	1.03	0.97
β	(0.08)	(0.09)	(0.12)	(0.12)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Obs.		9				30			
1st Stg. Obs.		13,281				13,235			

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, indicators for Black and other non-white races, and 'More' covariates in Project Talent: verbal, quantitative, technical, and scientific "Test" score components; high school "Grade" point average, high school fixed effects (for the 95 percent of students from a high school with at least 10 respondents), and self-reported indices of 'Extracurricular' participation, reading, hobbies, sports, leadership, and 'socialness'. **Panel A** uses the same baseline sample to estimate both sets of college major fixed effects; **Panel B** 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; and **Panels C** and **D** split the sample evenly (within major) for each first-stage estimation. '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: Project Talent and NLSY97.

Table 5: Tabular Decomposition of Collegiate Regressivity in the US in Log Dollars

Year:	1960	1980	2000	2014
Obs. Regressivity	0.017	0.073	0.129	0.168
Test Selection	0.002	0.032	0.011	0.013
	[10.2]	[43.8]	[8.5]	[7.5]
Net Regressivity	0.015	0.041	0.118	0.155
Major Returns	0.013	0.002	0.005	0.002
	[76.7]	[2.2]	[3.9]	[1.3]
Major Composition	0.004	0.020	0.002	0.045
	[26.5]	[27.4]	[1.3]	[26.8]
Institutional Returns	0.017	0.036	0.053	0.053
	[101.9]	[49.0]	[41.2]	[31.6]
Four-Year Composition	-0.007	0.010	-0.028	-0.010
	[-41.9]	[13.1]	[-21.7]	[-6.2]
Two-Year Composition	0.000	-0.016	0.020	0.027
	[1.7]	[-21.5]	[15.9]	[15.8]
For-Profit Composition	0.000	-0.001	0.011	0.011
	[0.0]	[-1.8]	[8.7]	[6.8]

Note: This table summarizes the contributions of selection (on pre-college cognitive skill), 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 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. See Section 9 for details on the construction of this decomposition. See Figure 2 and Appendix A for details on data construction and sources for all panels.

Online Appendix

Changes in the College Mobility Pipeline Since 1900

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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 Enlistment Records

Previous studies of pre-1940 higher education in the United States have almost exclusively used data from post-1940 US Censuses.[63] 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"). 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.[64] Using automated record linkage techniques – following 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. Appendix B discusses link quality and robustness to alternative linking strategies.

In our baseline analysis, 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 a positive imputed income in the 1920 Census.[65] We measure children's wage income as any non-zero wage income reported in the 1940 Census and rank incomes within-sample. While income is unobserved in the 1920 Census, we define parental income rank by extending Saavedra and Twinam (2020)'s "LIDO" measure: we predict socio-economic status by occupation, industry, age, sex, race, state, Census region, farm residence status, and self-employment status and their interactions using LASSO coefficients generated among workers ages 21–64 reporting positive income in the 1% sample of the 1950 Census. 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 imputed income in the 1920, 1910, and 1900 Censuses, respectively.

We also link the 1940 US Census to 1943 World War II enlistment cards by name, birth year,

[63] 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.

[64] This measure reflects "years of schooling" rather than completed educational attainment, which may not be strictly equivalent, especially in the South (Margo, 1986). There is evidence that retrospective high school completion rates are higher in the 1940 census than in contemporary regulatory reports (see, e.g. Goldin (1998)).

[65] 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).

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.[66] We then link matched enlistment cards to the 1950 Census using the Helgertz et al. (2023) crosswalk from the 1940 to 1950 censuses. We restrict 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 cognitive skill.[67] We further restrict to men who resided with at least one parent with non-zero wage income in 1940 and use that parent's income as our measure of family income.[68] 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 enlistment card data are available from the National Archives as "Electronic Army Serial Number Merged File, ca. 1938 - 1946" via Access to Archival Databases.

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 be made available on that institution's website (Bureau of Applied Social Research at Columbia University, 1947).

The survey includes respondents' gender, age (in ten-year bins), current annual wage income (8 bins), and coarse college major (15 bins); these data are used to construct the 1930s college major premiums shown in Figure 9. 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.[69] We use childhood location, probable age (year of college graduation - 22 in Time), and race to identify which survey respondent is

[66] The AGCT is the World War II equivalent of the modern AFQT. Additionally, Ferrie et al. (2012) show that the overall AGCT test score distribution looks quite similar to the enlistees in their sample which is linked to the 1930 Census 5% 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.

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

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

[69] We use the Abramitzky et al. (2022) 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.

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 band.[70] We remove these unique links from the pool of potential matches for the next step.

2. In the next step, we add information on "pre-college" state and city size (defined by their childhood census city and state of residence), and repeat the same procedure.[71] We append these links to those from Step 1 and remove them from the pool of potential matches.

3. Finally, we add in 1940 information on marital status, state of residence, and city size to capture post-college characteristics. 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.

This process yields 829 unique matches out of 4,808 male Time magazine respondents, a 17 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 but do not have full names to break ties on other criteria. 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. We use these linked data to measure coarse discipline composition by parental income; we do not measure institutional composition because first enrollment institution is unobserved and two-year institutions are omitted.

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 prior-year household wage income, the male household head's prior-year wage income, 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 college-going, major, and institution among earlier birth cohorts. All analysis uses baseline survey weights.

In order to construct accurate and continuous measures for both parental and (in 1962) respondent income, we follow the approach of Collins and Wanamaker (2022), who non-parametrically

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

[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.

impute incomes as the average earnings of individuals in the same occupation, race, region, and gender cell from the nearest decennial census.[72] 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 the final college that respondents attended, irrespective of degree attainment. Institutions were reported by 71 FICE code; we hand-construct a FICE-IPEDS crosswalk using a historical NCES codebook. 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 and replication files for Collins and Wanamaker (2022) are 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. Majors are only observed for four-year college graduates.

We construct dictionaries matching Wisconsin respondents' enrollment institutions to IPEDS *UnitID*'s – but omit from our analysis due to non-representativeness – 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.

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

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.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. We employ the follow-up survey weights for our baseline analysis – a small number of follow-up non-respondents were insistently surveyed, with multiple in-person visits, and substantially upweighted to account for non-response – but employ the baseline weights for analysis by major and institution, because the upweighted respondents otherwise fully determine some majors' and institutions' characteristics.

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) using LASSO. We restrict the Census to households with at least one member aged 30–64 and at least one member aged 13–20.

Students report their enrollment institutions to Project Talent three times: 1, 5, and 11 years following high school graduation, in each case reporting their most recent undergraduate institution. We default to the earliest institution that students report, only using later years if they did not report any institution in the earlier year. 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. Institutions with fewer than 20 male enrollees with observed age-29 income are assigned to the state-level aggregate for the purpose of value-added estimation. All declared college majors are observed, even absent degree attainment. 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; contemporaneous sample weights are similarly averaged. 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 for all enrollees (not just graduates) 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 in 1979, the first year of observation. AFQT test scores were measured on a 1–99 scale in 1979.[73] Child income is CPI-adjusted and averaged between observed incomes in 1994, 1996, 1998, and 2000 conditional on having non-zero income and being aged 30–35; contemporaneous 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 for all enrollees (not just graduates) 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.[74] Education is measured as the highest completed year of schooling across all survey responses. College majors are reported for all enrollees (not just graduates) 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 five cohorts of National Center for Education Statistics longitudinal studies: the National Longitudinal Survey of 1972 (NLS72), the High School and Beyond Survey of 1980 (HSaB), the National Educational Longitudinal Study of 1988 (NELS) the Educational Longitudinal Study of 2002 (ELS), and the High School Longitudinal Study of 2009 (HSLS). The first four surveys' datasets are available from ICPSR: the NLS72, the HSaB, the NELS and the ELS; HSLS is available directly from NCES.

The final follow-up with NELS and ELS participants occurred at age 26, when respondents

[73]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.

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

were still too young for their annual wages to be representative of their career outcomes; follow-ups ended with HSaB respondents at 22 or 24 (in public-use data) and at 22 in HSLS. As a result, we exclude all four from employment, wage, and major analysis and the latter two from measuring college completion. The final follow-up of the NLS72 was conducted 14 years later, when respondents were about 32; we measure educational attainment and wages in that year. In each sample, we restrict to individuals with observed positive family income and latest-age level of education. 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. 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 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 "cognitive skill" 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 reported FICE code for enrollment 1-4 years following high school graduation that matches a crosswalked FICE code, 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.

HSaB respondents are split between high school sophomores and seniors in 1980, so they are mostly from the 1961-1964 birth cohorts. Family income is measured in 1980. Test scores are the composite vocabulary, reading, and mathematics scores from 1980; scores for seniors are omitted because the full testing distribution is unobserved (since seniors are non-representative of all students due to drop-out). Sample weights for baseline respondents are used. Education is measured as the number of completed years of school as of 1986; college-going is defined as starting at a university and still being enrolled at that university the following year, at least once before 1986. Enrollment institution is defined as the earliest reported FICE code for enrollment 1-4 years following high school graduation that matches a crosswalked FICE code, conditional on completing at least one year of college.

NELS respondents are mostly in the 1974 birth cohort. Family income is measured 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. Family income is measured 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.

HSLS respondents are mostly in the 1994-1995 birth cohorts. Family income is measured in

2009. Test scores are a from a mathematics exam taken in 2009. Baseline sample weights are used. Education is measured at age 24; college-going is defined as starting at a university and still being enrolled at that university the following year, at least once before 2017.

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 Wave IV survey weight.[75] 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)" 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 often 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

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

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 Great Aspirations

The Great Aspirations survey, also called Career Plans and Experiences of June 1961 College Graduates, was a longitudinal survey of probable June 1961 graduates from an array of post-secondary institutions conducted by NORC. We leverage Goldin (2022)'s cleaned version of the data, available from Open ICPSR. The sample clustered on 135 institutions out of 1,039 accredited institutions with at least 500 students in 1958. NORC stratified institutions based on their PhD-sending behavior but provides sample weights (used in all analysis) to make the survey nationally representative of June 1961 college graduates based on PhD-sending and survey response rates.

ICPSR anonymizes respondents' institutions, and the code-to-institution name crosswalk has been lost. We identify each institution by matching each named institution's number of responses and location in NORC archival records to the unique combination of survey responses, location size, and state for each institution in the ICPSR data. This yields data on all 135 institutions.

Parental income is observed in six bins. In order to add nuance to observed parental income, we use the 1960 1% US Census sample to predict parental income using race (three codes), mother's and father's education (six codes each), mother's and father's occupation (nine codes), and six parental income bins (e.g. <5000, 5000–7500,... when observed). We restrict the Census to households with at least one member aged 30–64 and at least one member aged 13–20. College major is observed for all respondents.

The institution recorded in Great Aspirations is the final undergraduate institution where the student enrolled. We construct dictionaries matching respondents' 135 enrollment institutions to IPEDS *UnitID*'s and 95 college majors to our 10-code disciplines and 66-code detailed majors. We employ the survey's sample weights, but also construct institution-level weights to match the Project Talent distribution of institutions by control, level, and state, winsorizing at 1 percent.[76]

[76] Enrollments in control-level-state bins that aren't present in the Great Aspirations sample are proportionally added to other states' control-level bins.

A.11 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.

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.

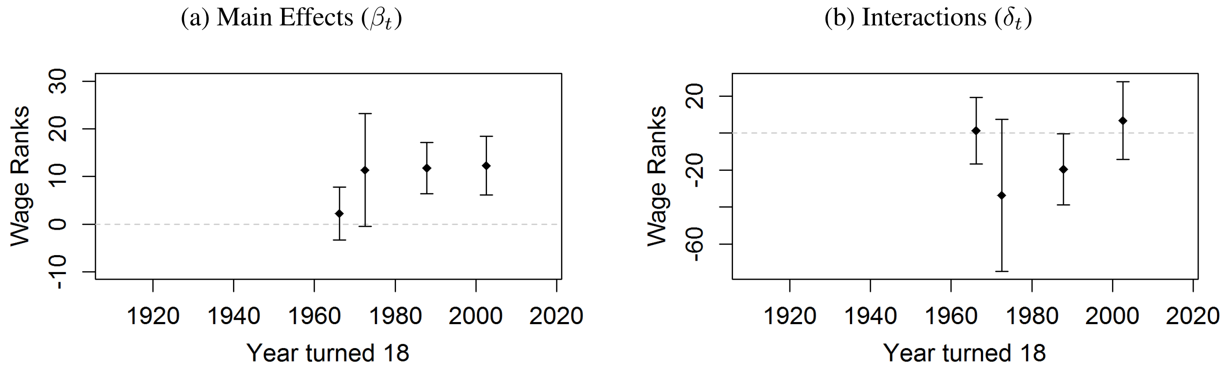
The GSS parental income methodology is comparable (though coarser) to the parental income imputation conducted in the CPS OCG survey described above, but covers a similar time period to longitudinal surveys in which parental income is directly observed. It also employs coarse wage bins with relatively 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

[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.

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



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.

to exclude GSS from our main analysis, though we do not believe that it meaningfully undermines our presented findings.

A.12 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 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.13 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 undergraduates in health science fields), and UC Davis (which enrolled undergraduates in agriculture).[78] The data are available from the UC ClioMetric History Project.

We link the registers to contemporaneous Censuses following Abramitzky et al. (2012), though this setting requires two departures from the standard linking algorithm. First, we assume that students' birth year is approximately 18 years prior to initial matriculation. Second, registers did not 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.[81] For matched students, we assign their family income based on the observed father's imputed income score discussed in Section A.1. Parental income terciles are defined by the national percentile of their matched father's imputed income.

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

[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".

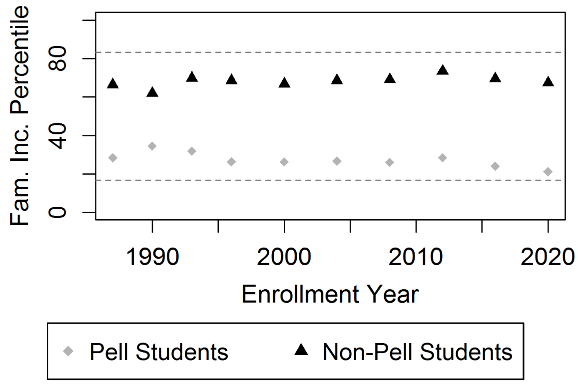
[79] This biases us towards finding students who did not move between a census and their first year of college. Therefore, we only look for students in the closest pre-college census among youths between the ages of 6 and 20.

[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 target populations and a comparison of linkage methods for our main findings. While linking often under-represents Black sons (Ward, 2023), college-goers are largely white.

[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.

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.

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.14 IPEDS and NPSAS

We use data from the Integrated Postsecondary Education Data System (IPEDS) to construct annual first-year and undergraduate enrollment by institution and Pell status since 2009 and institutional characteristics since 1995. These measures are augmented by the percent of 1998-2009 undergraduate students from the bottom and top two parental income quintiles from (Chetty et al., 2020), assigning students to their modal institution by age-18 cohort. We exclude institutions outside the 50 states and schools that only award degrees completed in less than two years.

We infer the gap in institutional value-added between the schools where students from the top and bottom income terciles enroll by linearly extrapolating from the gap between non-Pell and Pell students and between students from the top and bottom two parental income quintiles. The composition of students who receive or do not receive Pell grants changes over time due to changes in 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 income distribution of households with 16- or 17-year-olds in the contemporaneous CPS March supplement (Ruggles et al., 2024). Figure AA-2 shows the resulting triennial median income ranks of Pell and non-Pell students.

We linearly interpolate 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. The parental income of students from the top (bottom) parental income tercile is approximately $\frac{500}{6}$ ($\frac{100}{6}$), 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. We assume that the difference in parental incomes between students from the top two and bottom two parental income quintiles is $80 - 20 = 60$ ranks.

A.15 College Scorecard

The College Scorecard (C.S.) has released large annual data files since 2015. The fields recording field of study by institution and Pell status – which are present in each 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 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 second 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 Pell grants has changed over time due to changes in Pell eligibility criteria, we follow Appendix A.14 and adjust differences in major attainment by parental income using triennial median parental income measures from the NPSAS survey.

[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.

A.16 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.17 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 digitize most fields in these volumes and make them available in the replication package.

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 0.5 relative to undergraduate or doctoral degrees). Income per student is total annual income divided by total enrollment. 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. Junior colleges are classified as two-year institutions; public institutions are those controlled by the federal, state, city, or other local government.

A.18 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 enrollment-weighted 2-year public schools, 4-year public schools, and 4-year non-profit private schools.

We measure sticker and net tuition before 1960 using the 1923–1947 Blue Books (see Appendix A.17), restricting to institutions in the 50 states.

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 Table 330.10 of the same volume, converted to 2022 dollars.

From 1970 onward, four-year sticker tuition, fees, room and board cost estimates by institutional control are from College Board's Trends in College Pricing Table CP-2. The College Board surveys colleges annually for 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 and resident/commuter student tuition within responding school, and then enrollment-weighting across all respondents in each year.[85]

[85] Note that the IPEDS tuition series instead enrollment-weights all institutions' average charges for full-time students

To measure net costs, we difference average grant aid per student from these sticker costs. We measure total annual undergraduate aid as the sum total of all state, institutional and private aid programs reported by the College Board, which is available annually in 1963 and 1970-2022.[86] We then allocate that grant aid across institutions by level and control using the proportional aid received by students at each institution type in the 1987–2020 NPSAS, interpolating the ratios between years in which the NPSAS is not offered and using the 1987 NPSAS ratios in all years from 1970-1987.[87]

Finally, we scale aggregate aid by annual enrollments in each sector to measure average grant aid per student. 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 1985-2022.[88] This is differenced from average sticker price per student to obtain average net price per student.

A.19 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.17) as the product between total Bachelor's degrees awarded in a year and the share of enrolled students who were male or female.

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 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

reported for the entire academic year.

[86] 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, CPI-inflated back to 2022 dollars. 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 by subtracting the 1990 ratio of graduate to total post-secondary aid from each prior year.

[87] In particular, we use TOTGRT for full-time undergraduate students enrolled in only one type of institution, including those receiving zero aid. We adjust the 1987 estimate (which does not distinguish between undergraduate and postgraduate students) to be consistent with later years by subtracting the 1990 ratio of graduate to total post-secondary aid in the College Board data. The resulting per-student aid estimates are 10-15 percent below annual NPSAS per-student aid but follow the same trend over time.

[88] In 1970-1985, 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.

1958, 1961, 1963, 1966, and 1970. These reports include Fall enrollments of all degree-credit students across institutions and enrollments of first-time degree credit students. We extract Ivy degree shares by dividing first-time Fall enrollment at each institution with reported total first-time Fall enrollment. Note that these 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 that the denominator (total first-time enrollment) includes junior colleges, which is not true in some other sources (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.[89] 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.[90] We restrict to institutions in the 50 US states.

Finally, we use IPEDS to measure Ivy Plus enrollments by institution since 1984. See A.14.

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.

Appendix B: Robustness of Historical Record Linking

Automated record linking can bias 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 enrollees, these biases are a major concern. This appendix examines whether the NYSIIS-standard linked, father imputed income rank- son wage/salary 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

[89] We thank Lucas Marron and Joseph Altonji for suggesting this source.

[90] 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.

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	0.296***	0.168***	0.180***	0.147***	0.298***	0.168***	0.180***	0.147***
	(0.000905)	(0.00208)	(0.00322)	(0.00288)	(0.000930)	(0.00216)	(0.00333)	(0.00299)
R^2	0.10	0.03	0.04	0.03	0.10	0.03	0.04	0.03
Obs.	967,295	194,592	75,641	83,530	967,295	194,592	75,641	83,530
B: NYSIIS conservative								
Father rank	0.306***	0.177***	0.194***	0.157***	0.313***	0.180***	0.198***	0.160***
	(0.00111)	(0.00254)	(0.00391)	(0.00350)	(0.00113)	(0.00262)	(0.00407)	(0.00371)
R^2	0.10	0.04	0.05	0.04	0.11	0.04	0.05	0.04
Obs.	645,452	132,995	52,213	58,013	645,452	132,995	52,213	58,013
C: Exact standard								
Father rank	0.303***	0.172***	0.183***	0.148***	0.308***	0.175***	0.185***	0.151***
	(0.000959)	(0.00219)	(0.00337)	(0.00300)	(0.000972)	(0.00225)	(0.00348)	(0.00313)
R^2	0.10	0.03	0.04	0.03	0.11	0.04	0.04	0.03
Obs.	864,410	178,098	69,249	77,634	864,410	178,098	69,249	77,634
D: Exact conservative								
Father rank	0.307***	0.180***	0.189***	0.155***	0.321***	0.187***	0.198***	0.164***
	(0.00112)	(0.00253)	(0.00389)	(0.00347)	(0.00113)	(0.00262)	(0.00410)	(0.00374)
R^2	0.10	0.04	0.05	0.04	0.12	0.04	0.05	0.04
Obs.	646,473	135,806	53,212	59,704	646,473	135,806	53,212	59,704
E: MLP panel								
Father rank	0.314***	0.182***	0.201***	0.161***	0.339***	0.199***	0.211***	0.174***
	(0.000871)	(0.00203)	(0.00323)	(0.00299)	(0.000963)	(0.00237)	(0.00378)	(0.00357)
R^2	0.11	0.04	0.05	0.04	0.13	0.05	0.06	0.05
Obs.	1,045,712	215,058	76,625	81,413	1,045,243	214,895	76,571	81,362

Note: This table shows that linking method and/or weighting our links for representativeness does not alter the relative progressivity of the pre-World War II college wage premium relative to the NLSY97. 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 imputed income 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 imputed income 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. Further detail on data sources and methods can be found in Appendix A.1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: Ruggles et al. (2024), Abramitzky et al. (2022), and Helgertz et al. (2023).

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 30 to 35 in 1940, we modify the status variables used to rank fathers. Table BB-2 shows that decreasing the information set used to construct father's standing (as in Panels B and C) does not greatly alter our conclusions.

Table BB-2: Historical status measure mobility robustness

	All	HS	Some coll	Coll grad
A: Inc Rank - Imputed Rank				
Father Imputed Rank	0.296***	0.168***	0.180***	0.147***
	(0.000905)	(0.00208)	(0.00322)	(0.00288)
R^2	0.10	0.03	0.04	0.03
Obs.	967,295	194,592	75,641	83,530
B: Inc Rank - LIDO Rank				
Father LIDO Rank	0.281***	0.155***	0.169***	0.139***
	(0.000914)	(0.00211)	(0.00330)	(0.00297)
R^2	0.09	0.03	0.04	0.03
Obs.	938,407	186,253	71,707	78,414
C: Inc Rank - Occscore Rank				
Father Occscore Rank	0.316***	0.167***	0.182***	0.143***
	(0.00121)	(0.00258)	(0.00396)	(0.00341)
R^2	0.06	0.02	0.03	0.02
Obs.	1,011,260	202,617	78,934	87,143
D: Inc Rank - CW Inc Rank				
Father CW Inc Rank	0.269***	0.138***	0.159***	0.146***
	(0.000882)	(0.00204)	(0.00324)	(0.00294)
R^2	0.08	0.02	0.03	0.03
Obs.	1,011,218	202,605	78,931	87,140

Note: This table shows that using alternative measures of father's socioeconomic standing does not affect the relative progressivity of the college wage premium before World War II compared to in the NLSY97. All men between 30 and 35 years old (inclusive) in 1940 reporting positive wage and salary income who lived with a parent with a positive imputed income 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, imputed using our baseline measure, (Saavedra and Twinam, 2020) LIDO, IPUMS $OCCSCORE$, and Collins and Wanamaker (2022). Income is 1939 wage and salary income only, censored at the contemporary minimum wage working 20 hours a week and 50 hours a year. Ranks constructed within this sample. Standard errors are heteroskedasticity robust only. NYSIIS standard links used to follow all men over time. Further details on data construction are in Appendix A.1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: Ruggles et al. (2024), Abramitzky et al. (2022), Collins and Wanamaker (2022), and Saavedra and Twinam (2020).

We also use Collins and Wanamaker (2022) income scores, which adjust farmer standings to account for the relatively low rank of farmers by mid-century (which underlies all of our other imputed income measures), and again find quite similar results.

Finally, we provide evidence in Table BB-3 that our 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 linking-based results are unlikely to be driven solely by biases induced by linking method.

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: This table displays the positive selection into linking for our 1920–40 baseline sample and the smaller gaps found when weighting for representativeness, which do not alter our college wage premium findings. 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).

Appendix C: Faculty and Courses in US Higher Education

The dramatic growth of US higher education over the 20th century suggests that its nature could have changed over this period. This appendix assesses whether US colleges experienced fundamental changes in this period that would prohibit reasonable estimation of changes over time in the relative value of college-going. We focus on two institutions – the University of California, Berkeley and Stanford University – that have evolved over the past 100 years and collect detailed records on faculty teaching and course availability to complement the degree completion and college costs statistics presented in the main text.

The first period of our dataset spans the two decades before World War II. Students typically financed their studies by working during the school year. [91] Once in college, students could expect to encounter an array of specialties similar to today's (see Figure CC-1). The largest concentrations of faculty were in natural science and humanities between 1900 and 1940, 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. In contrast, humanities courses remained the single largest set of classes at UC Berkeley before 1940, as seen in Figure CC-2.

Faculty composition suggests that agriculture and humanities became less of a university priority between 1940 and 1960 as commerce degrees grew in importance in the postwar boom. Staffing in business-related departments offset the fall in agriculture and humanities at UC Berkeley while Stanford grew its professional coursework (Figure CC-2).[92] Figure CC-1 reveals that science,

[91] Half of college men and one-quarter of college women relied on wages during the school year to pay tuition at both private and land-grant schools, often as clerks, teachers, and waiters (Greenleaf, 1939). Employment included work for room and board, either with a family or the institution (Richtmyer and Willey, 1936).

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

Figure CC-1: University Faculty Distribution in the 20^{th} Century

Panel A: Faculty Distribution at UC Berkeley

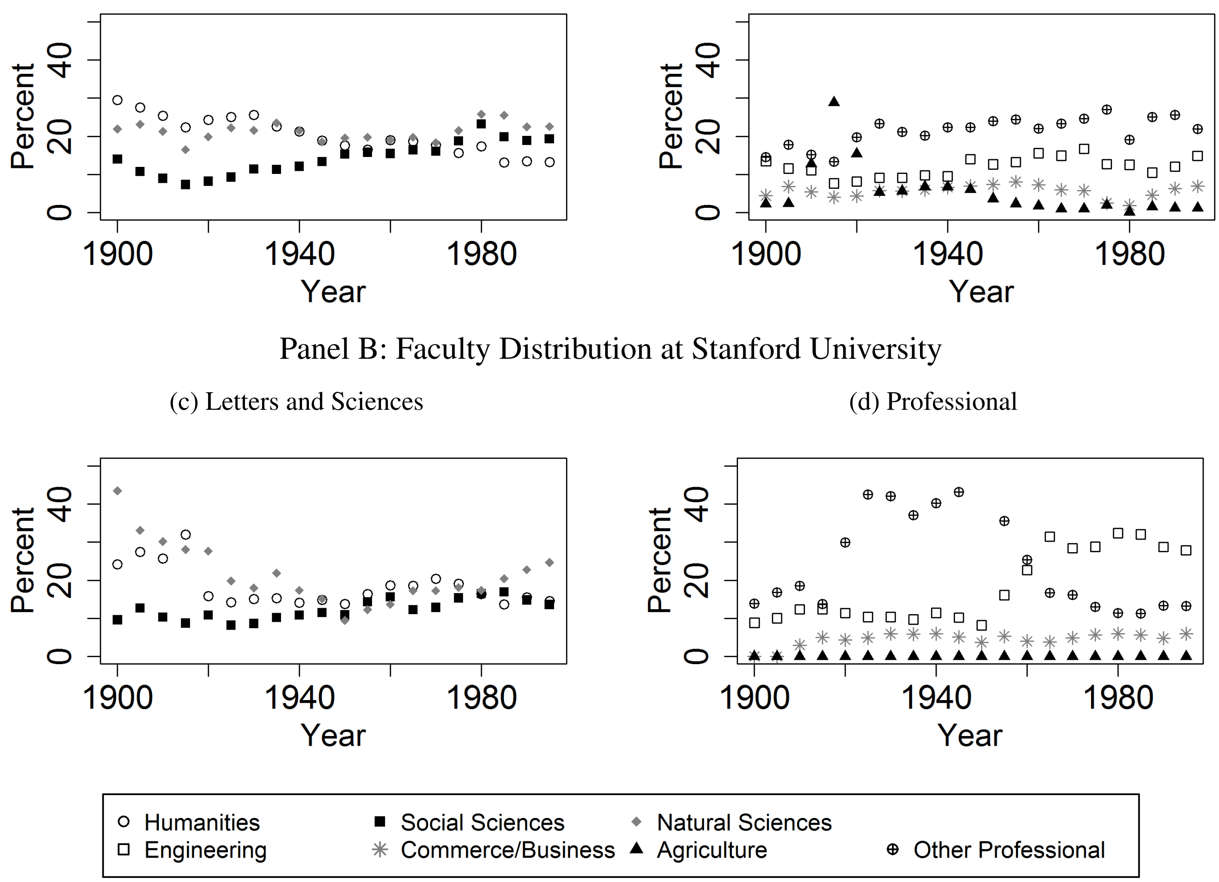
(a) Letters and Sciences

(b) Professional

Panel B: Faculty Distribution at Stanford University

(c) Letters and Sciences

(d) Professional



Note: This figure shows that university faculty composition has shifted towards social science and engineering and out of the humanities over the past century. The annual share of undergraduate UC Berkeley instructors in a partition of university disciplines, averaged over five-year periods. 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).

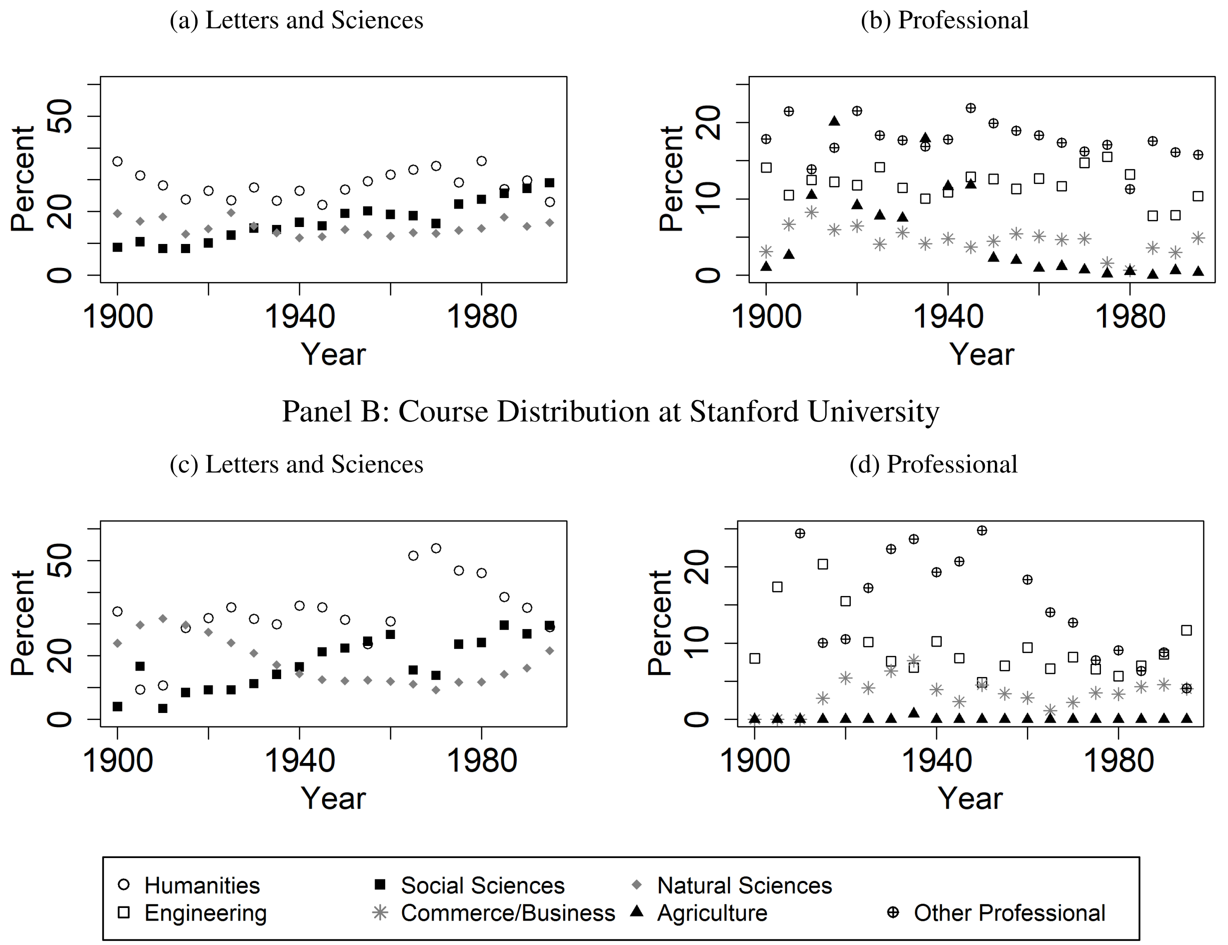
engineering, and professional schools were consistently popular in the 1940 to 1960 window.

There was a marked shift towards social and natural sciences in faculty and course offerings after 1960, paralleling changes in degree choice. 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 at UC Berkeley.

Figure CC-1 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

Figure CC-2: University Course Distribution in the 20^{th} Century

Panel A: Course Distribution at UC Berkeley



Note: This figure shows that university course composition has also shifted towards social science but otherwise has been more stable over time than faculty shares. The annual share of undergraduate UC Berkeley and Stanford courses taught in a partition of university disciplines, averaged over five-year periods. 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).

century-long trajectories, social science faculty shares are higher than they have ever been while humanities staffing is now at its lowest point. At Stanford, for instance, the post-1980 period marks the first time that the humanities were not the largest discipline by coursework over our entire study.

Almost two-thirds of faculty are now in natural or social science, engineering, or business fields, compared to half in 1920. Coursework shifted to a lesser extent. Social science training, measured with either faculty or coursework, is more prevalent now than in the past. Most of these changes occurred in the decades surrounding World War II, suggesting college disciplines have been largely stable over the period of rising collegiate regressivity.

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

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Auburn Univ.	AL	0.11	-0.03	188	Univ. Kansas	KS	0.13	-0.01	102
Jacksonville State Univ.	AL	-0.09	-0.12	64	Washburn Univ. of Topeka	KS	-0.07	-0.18	21
Troy State Univ.	AL	-0.08	-0.03	29	Wichita State Univ.	KS	-0.19	-0.18	20
Univ. Alabama Tuscaloosa	AL	0.11	-0.04	117	East Kentucky Univ.	KY	0.02	0.00	61
Univ. of Motevallo	AL	-0.15	-0.17	33	Morehead State Univ.	KY	-0.15	-0.12	49
Ag. Mech. and Normal Coll.	AR	-0.18	-0.01	23	Murray State Univ.	KY	0.01	-0.04	52
Arkansas State Univ.	AR	-0.14	-0.21	38	Univ. Kentucky	KY	0.09	0.01	118
Arkansas Tech Univ.	AR	-0.20	-0.24	28	Univ. Louisville	KY	-0.17	-0.26	51
Southern State Coll.	AR	-0.17	-0.22	78	West Kentucky Univ.	KY	-0.13	-0.13	97
Univ. Arkansas	AR	0.07	-0.04	42	Francis T. Nicholls State Coll.	LA	-0.12	-0.12	45
Arizona State Univ.	AZ	-0.05	-0.11	47	Louisiana Poly. Institute	LA	0.08	-0.09	28
Northern Arizona Univ.	AZ	0.06	-0.04	41	Louisiana State Univ.-Eunice	LA	0.16	-0.03	188
Univ. Arizona	AZ	0.19	0.06	83	McNeese State Univ.	LA	0.18	-0.13	24
California State Poly. Univ.	CA	0.17	0.02	44	Northeast Louisiana Univ.	LA	-0.18	-0.20	42
CSU San Bernardino	CA	0.16	0.12	66	Southern Univ. and A & M Coll.	LA	-0.32	-0.12	36
CSU Chico	CA	0.00	-0.08	65	Univ. Southwestern Louisiana	LA	-0.09	-0.14	170
CSU Fresno	CA	-0.07	-0.05	20	Boston State Coll.	MA	-0.03	0.06	81
CSU LA	CA	0.08	0.03	67	Bridgewater State Coll.	MA	-0.18	-0.09	36
CSU Long Beach	CA	0.00	0.00	61	Lowell Technological I	MA	0.16	-0.06	22
CSU San Diego	CA	0.00	-0.04	106	Salem State Univ.	MA	-0.09	-0.16	48
CSU San Francisco	CA	-0.11	-0.01	98	Univ. Mass Amherst	MA	0.11	-0.02	152
CSU San Jose	CA	0.01	-0.05	85	Morgan State Coll.	MD	-0.04	-0.00	86
UC Berkeley	CA	0.02	-0.05	75	St. Mary's Coll. of Maryland	MD	-0.22	-0.46	75
UCLA	CA	0.23	0.14	81	Towson State Coll.	MD	-0.02	0.00	74
UCSB	CA	0.33	-0.05	27	United States Naval Acad.	MD	0.20	-0.09	53
Colorado School of Mines	CO	0.40	-0.11	20	Univ. Maryland	MD	-0.02	-0.10	23
CSU Fort Collins	CO	-0.05	-0.16	116	Univ. of Baltimore	MD	0.13	-0.02	243
UC Boulder	CO	-0.06	-0.21	229	Univ. of Maryland Global Campus	MD	0.08	-0.11	42
Univ. North Colorado	CO	-0.24	-0.24	69	Univ. Maine	ME	-0.01	-0.17	144
US Air Force Acad.	CO	0.44	0.17	37	Univ. of Maine at Augusta	ME	-0.19	-0.12	27
Western State Coll. of Colorado	CO	-0.07	-0.10	23	Univ. of Southern Maine	ME	-0.20	-0.34	29
Central Conn State Coll.	CT	-0.01	-0.03	127	Central Michigan Univ.	MI	-0.13	-0.13	110
Univ. Conn Storrs	CT	0.06	-0.05	139	Eastern Michigan Univ.	MI	0.10	0.07	112
Univ. Delaware Newark	DE	0.02	-0.07	43	Ferris State Coll.	MI	0.10	-0.04	70
Florida Ag. and Mech. Univ.	FL	-0.13	-0.15	27	Michigan State Univ.	MI	0.09	-0.04	378
FSU Tallahassee	FL	0.07	-0.00	115	Michigan Tech Univ.	MI	0.30	0.09	25
UF Gainesville	FL	0.06	-0.06	228	Univ. of Michigan-Ann Arbor	MI	0.17	0.04	342
Univ. South FL Tampa	FL	-0.15	-0.18	21	Wayne State Univ.	MI	0.14	0.01	359
Fort Valley State Coll.	GA	-0.49	-0.15	23	Western Mich Univ.	MI	-0.04	-0.13	116
Georgia Southern Coll.	GA	-0.28	-0.15	25	Bemidji State Coll.	MN	-0.10	-0.25	33
Georgia State Coll.	GA	-0.01	-0.01	54	Mankato State Coll.	MN	-0.05	-0.13	108
Georgia Tech	GA	0.24	-0.03	90	Moorhead State Coll.	MN	-0.06	-0.16	27
UGA	GA	0.04	-0.06	103	St. Cloud State Coll.	MN	-0.10	-0.14	57
Univ. of Hawaii at Hilo	HI	0.04	0.01	227	Univ. Minnesota	MN	-0.04	-0.16	310
Iowa State Univ.	IA	0.08	-0.10	135	Winona State Univ.	MN	-0.07	-0.14	31
Univ. Iowa	IA	0.05	-0.05	90	Central Missouri State	MO	-0.11	-0.17	161
Univ. North Iowa	IA	-0.09	-0.17	49	Northeast Missouri State Coll.	MO	-0.10	-0.20	54
Idaho State Univ.	ID	-0.05	-0.09	20	Northwest Missouri State Coll.	MO	-0.28	-0.40	40
Univ. Idaho	ID	0.06	-0.06	36	Southeast Missouri State Coll.	MO	-0.09	-0.15	79
Chicago State Coll.	IL	0.01	0.18	40	Univ. Missouri	MO	0.16	-0.01	374
Eastern Ill Univ.	IL	0.16	0.00	50	Mississippi State Univ.	MS	0.14	-0.02	60
Ill State Univ.	IL	-0.20	-0.13	77	Univ. Missssissippi	MS	-0.08	-0.15	90
Northern Ill Univ.	IL	0.06	-0.01	151	Univ. Southern Mississippi	MS	-0.00	-0.06	90
Northwestern	IL	0.19	0.07	104	Eastern Montana Coll.	MT	-0.06	-0.17	206
South Ill	IL	0.07	-0.03	225	Montana State Univ. Bozeman	MT	0.09	-0.04	115
Univ. Ill	IL	0.13	-0.03	412	Montana State Univ. Missoula	MT	-0.10	-0.22	87
West Ill Univ.	IL	0.05	-0.04	148	Appalachian State Teachers Coll.	NC	-0.12	0.01	36
Ball State	IN	-0.12	-0.15	184	East Carolina Univ.	NC	-0.02	-0.08	81
Ind Univ. Bloomington	IN	0.07	0.01	287	NC Ag and Tech State Coll.	NC	-0.23	-0.08	21
Ind Univ. Reg Campus	IN	0.04	-0.11	71	NC State Univ.	NC	0.20	-0.03	83
Purdue Univ.	IN	0.20	0.02	233	Univ. NC Chapel Hill	NC	0.20	-0.05	103
Purdue Univ. Centers	IN	0.12	-0.04	27	Univ. of NC at Greensboro	NC	-0.20	0.00	66
Fort Hays Kansas State Coll.	KS	-0.04	-0.13	83	Univ. of NC at Pembroke	NC	0.06	-0.08	24
Kansas State Teachers Coll.	KS	-0.09	-0.21	40	Western Carolina Univ.	NC	-0.06	-0.13	26
Kansas State Univ.	KS	0.07	-0.05	139	North Dakota State Univ.	ND	0.21	0.06	26

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each institution with at least 20 employed male enrollees in the Project Talent database, and the number of respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured test score rank and parental income rank and race and gender indicators. University names are from matched universities in IPEDS. Source: Project Talent and IPEDS.

Appendix D: Institutional Value-Added in 1963

Tables DD-1 to DD-6 present the relative average wage, average value-added, and (weighted) number of enrolled students observed and estimated in the Project Talent dataset. The estimates

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

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Univ. North Dakota	ND	-0.17	-0.27	48	Shippensburg State Coll.	PA	0.05	-0.11	24
Chadron State Coll.	NE	-0.05	-0.07	36	Slippery Rock Univ. of PA	PA	-0.08	-0.08	36
Kearney State Coll.	NE	-0.22	-0.38	39	Temple Univ.	PA	0.11	0.05	141
Univ. Nebraska Lincoln	NE	-0.02	-0.12	318	Univ. of Pittsburgh-Bradford	PA	0.07	-0.01	151
Univ. Nebraska Omaha	NE	-0.04	-0.17	81	West Chester State Coll.	PA	-0.09	-0.04	38
Wayne State Coll.	NE	-0.15	-0.21	38	Univ. Rhode Island	RI	0.00	-0.08	98
Univ. New Hampshire	NH	-0.00	-0.16	85	Clemson Univ.	SC	0.14	-0.08	64
Montclair State Coll.	NJ	0.11	0.07	33	Lander Univ.	SC	-0.23	-0.24	51
Newark Coll. of Engineering	NJ	0.42	0.19	26	The Citadel	SC	0.06	-0.18	21
Rutgers	NJ	0.05	-0.07	118	Univ. South Carolina	SC	0.08	-0.07	95
Trenton State Coll.	NJ	-0.13	-0.15	76	Winthrop Univ.	SC	-0.34	-0.12	44
William Paterson Coll.	NJ	0.11	0.07	30	Black Hills State Univ.	SD	-0.45	-0.25	25
Eastern New Mexico Univ.	NM	-0.04	-0.18	36	Northern State Coll.	SD	-0.28	-0.25	25
Univ. of New Mexico	NM	-0.09	-0.19	93	South Dakota State Univ.	SD	-0.06	-0.15	53
Western New Mexico Univ.	NM	-0.00	-0.15	40	State Univ. South Dakota	SD	-0.05	-0.25	31
Coll. of Southern Nevada	NV	-0.01	-0.14	36	Univ. of South Dakota	SD	-0.02	-0.25	24
Univ. of Nevada	NV	0.12	-0.01	93	East Tennessee State Univ.	TN	-0.04	-0.07	30
Brooklyn Coll.	NY	0.15	0.07	394	Middle Tennessee State Univ.	TN	-0.07	-0.08	61
City Coll.	NY	0.26	0.09	371	Tennessee Tech. Univ.	TN	0.03	-0.07	65
Coll. at Geneseo	NY	-0.38	-0.07	24	Univ. of Chattanooga	TN	0.05	-0.10	44
Coll. at New Platz	NY	0.07	-0.07	42	Univ. Tennessee	TN	-0.04	-0.11	256
Coll. at Oneonta	NY	-0.08	-0.01	62	East Texas State Univ.	TX	-0.49	-0.50	33
Coll. at Oswego	NY	0.01	0.07	54	Lamar Univ.	TX	0.02	-0.09	299
Coll. at Plattsburgh	NY	-0.12	-0.13	45	North Texas State Univ.	TX	-0.15	-0.19	159
Hunter Coll.	NY	0.16	0.15	259	Sam Houston State Univ.	TX	-0.06	-0.10	55
John Jay Coll. of Criminal Justice	NY	0.27	0.11	26	Southwest Texas State Univ.	TX	0.02	-0.01	52
State Univ. of New York at Cortland	NY	-0.05	-0.11	65	Stephen Austin State Univ.	TX	-0.05	-0.13	36
SUNY Albany	NY	-0.02	-0.02	43	Texas A & I Univ.	TX	0.00	-0.14	33
SUNY at Fredonia	NY	-0.11	-0.17	57	Texas A & M	TX	0.27	-0.13	23
SUNY Binghamton	NY	0.10	0.01	45	Texas Tech Univ.	TX	-0.02	-0.12	197
SUNY Coll. at Potsdam	NY	0.10	0.08	39	Texas Woman's Univ.	TX	-0.26	-0.13	38
United States Military Acad.	NY	0.31	0.02	37	Univ. Houston	TX	0.06	-0.05	74
Univ. at Buffalo	NY	-0.20	-0.14	124	Univ. Texas Arlington	TX	-0.04	-0.15	27
Bowling Green State Univ.	OH	-0.14	-0.19	235	West Texas State Univ.	TX	-0.03	-0.19	38
Fenn Coll.	OH	0.07	-0.08	128	Southern Utah Univ.	UT	-0.13	-0.22	171
Kent State Univ.	OH	-0.01	-0.09	396	Univ. of Utah	UT	0.07	-0.05	34
Miami Univ. Oxford	OH	0.07	-0.04	122	Coll. of William and Mary	VA	-0.04	-0.11	50
Ohio State Univ.-Marion Campus	OH	0.07	-0.09	503	James Madison Univ.	VA	-0.07	0.15	36
Ohio State Univ.-Newark Campus	OH	0.11	-0.15	21	Longwood Univ.	VA	-0.00	0.01	35
Ohio Univ.	OH	0.03	-0.06	249	Old Dominion Univ.	VA	-0.04	-0.10	226
Univ. Akron	OH	0.03	-0.05	222	Univ. Virginia	VA	0.11	-0.09	121
Univ. Cincinnati	OH	0.09	-0.10	85	Virginia Military Institute	VA	0.22	0.01	20
Univ. Toledo	OH	-0.09	-0.20	50	Virginia Poly. I	VA	0.09	-0.05	137
Youngstown State Univ.	OH	-0.04	-0.11	129	Univ. of Vermont	VT	0.00	-0.14	62
East Central State Coll.	OK	-0.31	-0.34	36	Central washington State Coll.	WA	-0.07	-0.11	32
Northeastern State Coll.	OK	-0.16	-0.20	87	Eastern Washington Univ.	WA	0.06	-0.03	44
Northwestern State Coll.	OK	-0.15	-0.16	21	Univ. Washington	WA	-0.00	-0.12	177
Oklahoma Panhandle State Univ.	OK	-0.20	-0.16	26	Walla Walla Comm. Coll.	WA	-0.17	-0.14	60
OK State Univ.-Oklahoma City	OK	-0.06	-0.16	208	Washington State Univ.	WA	0.03	-0.09	101
Univ. Oklahoma	OK	0.07	0.00	153	Western Washington State Coll.	WA	-0.13	-0.04	44
Eastern Oregon Coll.	OR	-0.02	-0.12	33	Stout State Univ.	WI	-0.06	-0.13	50
Oregon State Univ.	OR	0.11	-0.04	106	UW Madison	WI	0.00	-0.10	579
Portland State Univ.	OR	-0.11	-0.15	92	WI State Univ.	WI	-0.23	-0.09	27
Univ. Oregon	OR	-0.21	-0.25	77	WI State Univ. Eau Claire	WI	-0.23	-0.09	29
Bloomsburg State Coll.	PA	-0.10	-0.17	52	WI State Univ. Oshkosh	WI	-0.02	-0.09	41
California State Coll.	PA	0.07	-0.11	31	WI State Univ. Platteville	WI	0.10	-0.03	71
Clarion Univ. of Pennsylvania	PA	-0.08	-0.06	109	WI State Univ. River Falls	WI	-0.14	-0.29	62
East Stroudsburd State Coll.	PA	-0.15	-0.13	51	WI State Univ. Stevens Point	WI	-0.14	-0.26	34
Edinboro State Coll.	PA	-0.20	-0.25	43	WI State Univ. Whitewater	WI	0.09	-0.03	90
Indiana State Univ.	PA	-0.09	-0.11	115	Concord Coll.	WV	-0.03	-0.09	28
Kutztown State Coll.	PA	-0.28	-0.28	34	Marshall Univ.	WV	-0.01	-0.04	44
Lock Haven State Coll.	PA	-0.09	-0.11	25	Shepherd Coll.	WV	-0.04	-0.09	32
Millersville State Coll.	PA	-0.15	-0.18	103	West Virginia Univ.	WV	0.09	-0.03	70
Penn State Commonwealth Campus	PA	-0.04	-0.16	50	WV Univ. Institute of Tech.	WV	0.07	-0.04	49
Pennsylvania State Univ.	PA	0.13	-0.03	353	Univ. of Wyoming	WY	-0.12	-0.12	39

Note: See note to Table DD-1. Source: Project Talent and IPEDS.

and counts are restricted to men and include the 523 last-enrollment institutions with at least 20 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 Private Universities 1

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Birmingham Southern Coll.	AL	-0.20	-0.28	33	St. Olaf Coll.	MN	0.05	-0.02	41
Samford Univ.	AL	-0.14	-0.20	47	Central Methodist Univ.	MO	0.10	-0.05	29
Tuskegee I	AL	-0.15	-0.04	40	Rockhurst Coll.	MO	0.03	-0.17	26
Lyon Coll.	AR	-0.18	-0.24	22	St. Louis Univ.	MO	-0.12	-0.17	60
Stanford	CA	0.15	-0.08	56	Washington Univ.	MO	0.08	-0.08	198
UC Davis	CA	0.09	-0.05	28	William Jewell Coll.	MO	0.22	0.07	48
Univ. of Redlands	CA	0.35	-0.05	21	MS State Coll. for Women	MS	-0.28	-0.17	32
Univ. of SF	CA	0.19	-0.05	44	Rocky Mountain Coll.	MT	-0.08	-0.24	23
Univ. Santa Clara	CA	0.19	-0.05	31	Campbell Coll.	NC	-0.05	-0.14	39
USC	CA	0.23	0.12	25	Duke	NC	0.10	-0.07	66
Univ. Denver	CO	0.10	-0.03	59	Guilford Coll.	NC	0.09	0.02	32
Connecticut Coll.	CT	-0.53	-0.17	22	Wake Forest	NC	0.17	-0.05	32
Univ. Bridgeport	CT	0.18	0.09	33	Creighton Univ.	NE	0.26	-0.22	28
Univ. Hartford	CT	-0.02	-0.11	66	Nebraska Wesleyan Univ.	NE	-0.05	-0.11	28
Wesleyan Univ.	CT	0.05	-0.17	22	Dartmouth Coll.	NH	0.22	-0.04	45
Yale	CT	0.20	-0.11	52	St. Anselm's Coll.	NH	0.20	-0.02	23
American Univ.	DC	-0.01	-0.13	45	Fairleigh Dickinson Univ.	NJ	0.02	-0.09	102
Columbian Coll.	DC	0.17	0.06	38	Princeton Univ.	NJ	0.36	0.04	59
Georgetown Univ.	DC	0.26	0.00	45	Rider Coll.	NJ	-0.03	-0.06	74
Howard Univ.	DC	0.06	-0.09	22	St. Peter's Coll.	NJ	0.17	0.07	20
Edward Waters Univ.	FL	-0.24	-0.17	30	Adelphi Univ.	NY	0.15	-0.01	47
Univ. Miami	FL	0.25	0.08	44	Alfred Univ.	NY	0.19	0.02	32
Emory	GA	0.24	0.05	38	Barnard Coll.	NY	0.05	-0.07	20
Coe Coll.	IA	0.11	-0.12	22	Clarkson Coll. of Tech.	NY	0.37	0.10	33
Drake Univ.	IA	-0.01	-0.05	36	Columbia	NY	0.21	-0.04	44
Loras Coll.	IA	0.25	-0.12	22	Columbia U–Columbia Coll.	NY	0.30	-0.01	34
Luther Coll.	IA	-0.04	-0.12	22	Cornell Univ.	NY	0.13	-0.10	123
Bradley Univ.	IL	0.05	-0.09	51	D'Youville Univ.	NY	-0.25	-0.07	25
DePaul Univ.	IL	-0.01	-0.17	47	Daemen Univ.	NY	-0.32	-0.07	26
Elmhurst Coll.	IL	-0.05	-0.15	34	Fordham Coll.	NY	0.24	0.02	45
Illinois Institute of Tech.	IL	0.19	-0.02	34	Hartwick Coll.	NY	0.05	-0.07	26
Knox Coll.	IL	-0.00	-0.11	22	Hobart William Smith Coll.s	NY	0.19	-0.07	21
Monmouth Univ.	IL	0.03	-0.14	24	Hofstra	NY	0.30	0.18	57
Roosevelt	IL	0.14	-0.11	22	Iona Coll.	NY	0.29	0.07	62
Univ. Chicago	IL	0.02	-0.06	27	Ithaca Coll.	NY	0.19	-0.07	22
Butler Univ.	IN	-0.03	-0.03	59	Long Island Univ.	NY	0.19	0.04	159
DePauw Univ.	IN	0.12	-0.03	46	Manhattan Coll.	NY	0.35	0.09	33
Evansville Coll.	IN	-0.06	-0.08	115	Marist Coll.	NY	0.11	-0.07	23
Goshen Coll.	IN	-0.10	-0.16	39	New York Institute of Tech.	NY	0.27	0.08	24
Hanover Coll.	IN	0.06	-0.12	20	New York Univ.	NY	0.23	0.09	153
Indiana Central Coll.	IN	-0.03	-0.03	39	Niagara Univ.	NY	-0.08	0.00	43
Manchester Coll.	IN	-0.01	-0.12	25	Pace Coll.	NY	0.27	0.11	72
Tri-State Coll.	IN	0.19	-0.02	24	Poly. I of Brooklyn	NY	0.30	0.04	48
Univ. Notre Dame	IN	0.32	0.02	93	Pratt Institute	NY	0.08	0.00	48
Valparaiso Univ.	IN	0.07	-0.04	57	Rensselaer Poly. I	NY	0.24	-0.06	49
Berea Coll.	KY	-0.34	-0.18	23	Rochester I of Tech.	NY	0.13	-0.02	32
Cumberland Coll.	KY	-0.24	-0.23	42	Russell Sage Coll.	NY	0.02	-0.07	24
Univ. of Pikeville	KY	-0.19	-0.19	38	St Bernard's School	NY	-0.26	-0.07	22
American International Coll.	MA	0.18	-0.09	22	St. Bonaventure Univ.	NY	0.16	-0.07	21
Bentley Coll.	MA	0.16	-0.09	22	St. Francis Coll.	NY	0.26	-0.07	21
Boston Coll.	MA	0.02	-0.03	94	St. Lawrence Univ.	NY	0.12	-0.07	24
Boston Univ.	MA	0.02	-0.04	163	The Cooper Union	NY	0.35	-0.07	23
Coll. of the Holy Cross	MA	0.20	-0.06	37	Univ. Rochester	NY	0.23	0.07	45
Emmanuel Coll.	MA	-0.28	-0.09	46	Utica Univ.	NY	0.11	0.01	111
MIT	MA	0.39	0.10	51	Vassar Coll.	NY	-0.23	-0.07	26
Northeastern	MA	0.12	-0.04	273	Wagner Coll.	NY	0.01	-0.07	30
Smith Coll.	MA	-0.30	-0.17	36	Ashland Coll.	OH	-0.17	-0.24	33
Springfield Coll.	MA	0.17	-0.09	21	Baldwin-Wallace Coll.	OH	-0.03	-0.11	43
Suffolk Univ.	MA	-0.20	-0.09	22	Capital Univ.	OH	-0.10	-0.15	24
Tufts Univ.	MA	0.21	0.02	53	Case I of Tech.	OH	0.31	0.03	44
Worcester Poly. I	MA	0.25	0.04	31	Denison Univ.	OH	-0.12	-0.15	29
Johns Hopkins	MD	0.13	-0.09	41	Heidelberg Coll.	OH	-0.16	-0.15	21
Loyola Coll.	MD	0.27	-0.10	20	Hiram Coll.	OH	-0.13	-0.15	25
Colby Coll.	ME	0.06	-0.12	25	John Carrol Univ.	OH	0.18	-0.05	64
Husson Coll.	ME	-0.13	-0.21	54	Lake Erie Coll.	OH	-0.21	-0.15	24
Albion Coll.	MI	0.10	-0.04	22	Malone Univ.	OH	-0.24	-0.26	29
Alma Coll.	MI	-0.04	-0.15	34	Marietta Coll.	OH	0.21	-0.15	26
Kettering Univ.	MI	0.33	0.07	29	Mount Union Coll.	OH	-0.16	-0.16	31
Lawrence I of Tech.	MI	0.29	0.06	47	Muskingum Coll.	OH	0.01	-0.14	36
Univ. of Detroit	MI	0.15	-0.03	105	Oberlin Coll.	OH	-0.24	-0.32	31
Augsburg Coll.	MN	-0.19	-0.31	31	Ohio Wesleyan Univ.	OH	0.15	0.06	40
Bethel Univ.	MN	0.05	-0.14	21	Otterbein Coll.	OH	0.02	-0.15	26
Carleton Coll.	MN	-0.08	-0.14	20	Univ. Daytona	OH	0.23	-0.01	60
Concordia Coll. at Moorhead	MN	-0.24	-0.14	27	Westerm Reserve Univ.	OH	0.01	-0.04	60
Gustavus Adolphus Coll.	MN	-0.06	-0.15	31	Wittenburg Univ.	OH	0.01	-0.05	39
Hamline Univ.	MN	-0.01	-0.14	22	Oklahoma Baptist Univ.	OK	-0.00	-0.16	26
Macalester Coll.	MN	0.05	-0.14	23	Oklahoma City Univ.	OK	-0.03	-0.08	33

Note: See note to Table DD-1. Source: Project Talent and IPEDS.

Table DD-4: 1963 Value-Added of Private Universities 2

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Phillips Theological Seminary	OK	-0.21	-0.28	88	Brown Univ.	RI	0.00	-0.29	30
Southwestern Christian Univ.	OK	-0.06	-0.13	32	Providence Coll.	RI	0.24	-0.19	21
Univ. Tulsa	OK	-0.03	-0.17	157	Bob Jones Univ.	SC	-0.41	-0.18	29
Univ. of Portland	OR	0.02	-0.24	22	David Lipscomb Coll.	TN	-0.06	-0.16	34
Allegheny Coll.	PA	0.02	-0.10	34	Lincoln Memorial Univ.	TN	-0.07	-0.16	22
Bucknell Univ.	PA	0.19	-0.02	45	Vanderbilt Univ.	TN	0.14	-0.12	35
Carnegie Mellon Univ.	PA	0.21	0.02	67	Abilene Christian Coll.	TX	-0.09	-0.13	33
Drexel Univ.	PA	0.19	0.01	47	Austin Coll.	TX	0.04	-0.13	27
Duquesne Univ.	PA	0.11	-0.02	82	Baylor Univ.	TX	-0.19	-0.21	69
Elizabethtown Coll.	PA	0.12	0.05	33	Main Univ.	TX	0.17	0.01	256
Franklin and Marshall Coll.	PA	0.01	-0.20	50	Rice Univ.	TX	0.23	-0.10	30
Geneva Coll.	PA	0.01	-0.12	105	Southern Methodist Univ.	TX	0.12	0.10	74
Gennon Coll.	PA	0.23	-0.11	20	St. Mary's Univ.	TX	-0.03	-0.20	30
Gettysburg Coll.	PA	-0.01	-0.11	22	Texas Christian Univ.	TX	-0.20	-0.21	40
Grove City Coll.	PA	0.17	-0.03	42	Texas Lutheran Univ.	TX	0.07	-0.13	26
Juniata Coll.	PA	-0.08	-0.11	22	Trinity Univ.	TX	0.08	-0.00	40
King's Coll.	PA	0.12	-0.08	30	Brigham Young Univ.	UT	-0.07	-0.17	288
Lehigh Univ.	PA	0.34	0.03	49	Univ. Richmond	VA	0.18	0.01	25
Marywood Univ.	PA	-0.06	0.16	40	Middlebury Coll.	VT	-0.16	-0.24	21
Penn Morton Coll.	PA	0.23	-0.01	42	Gonzaga Univ.	WA	0.13	-0.13	99
Univ. Penn	PA	0.15	-0.08	92	Pacific Lutheran Univ.	WA	-0.07	-0.07	29
Univ. Scranton	PA	0.18	-0.02	37	Seattle Univ.	WA	0.22	0.05	32
Ursinus Coll.	PA	-0.02	-0.11	24	Univ. Puget Sound	WA	0.04	-0.08	29
Villanova	PA	0.23	-0.03	24	Alverno Coll.	WI	-0.20	0.02	39
Washington and Jefferson Coll.	PA	0.33	-0.11	20	Marquette Univ.	WI	0.10	0.02	105
Wilkes Coll.	PA	0.08	-0.01	54	Milwaukee School of Engineering	WI	0.18	0.00	23

Note: See note to Table DD-1. Source: Project Talent and IPEDS.

Table DD-5: 1963 Value-Added of Community Colleges

Institution	State	$	$ VA	N	Institution	State	$	$ VA	N
Eastern Arizona Coll.	AZ	-0.31	-0.28	21	Lansing CC	MI	0.08	0.00	37
Maricopa County Jr. Coll.	AZ	-0.04	-0.14	27	Macomb County CC	MI	0.14	0.05	183
Cabrillo Coll.	CA	-0.13	-0.23	33	MN State Comm. and Tech. Coll.	MN	-0.23	-0.35	31
City Coll. of San Francisco	CA	-0.07	0.08	74	Metropolitan CC-Kansas City	MO	0.00	-0.08	140
Coll. of San Mateo	CA	0.15	0.08	93	Missouri Western State Univ.	MO	0.01	-0.20	43
El Camino Coll.	CA	-0.00	-0.11	27	Saint Louis CC	MO	-0.06	-0.13	30
Fullerton Jr. Coll.	CA	-0.23	-0.23	39	Jones County Jr. Coll.	MS	-0.06	-0.10	82
Glendale Coll.	CA	0.03	-0.07	176	Chowan Coll.	NC	-0.05	-0.17	25
Long Beach City Coll.	CA	0.08	-0.02	40	North Dakota State School of Science	ND	-0.09	-0.19	87
Los Angeles City Coll.	CA	-0.01	-0.03	441	Trenton J Coll.	NJ	0.08	-0.04	32
Monterey Peninsula Coll.	CA	-0.12	-0.13	47	Acad. of Aeuronautics	NY	0.17	-0.02	24
Napa Valley Coll.	CA	-0.12	-0.17	34	Bronx CC	NY	0.08	0.04	41
Orange Coast Coll.	CA	-0.01	-0.09	24	Broome CC	NY	-0.05	-0.16	101
Pasadena City Coll.	CA	0.01	-0.03	30	Cayuga County CC	NY	-0.11	-0.16	95
San Bernadino Valley Coll.	CA	-0.03	-0.10	87	Erie CC	NY	-0.15	-0.20	50
San Diego City Coll.	CA	-0.07	-0.16	49	Fashion I of Tech.	NY	0.05	0.23	40
San Jose City Coll.	CA	-0.07	-0.09	29	Hudson Valley CC	NY	0.00	-0.17	25
Shasta Coll.	CA	0.05	-0.03	77	Nassau CC	NY	-0.05	-0.04	47
Vallejo Jr. Coll.	CA	0.03	-0.09	22	NY City CC of Appl. Arts and Sci.	NY	0.12	0.01	44
Mitchell Coll.	CT	0.05	-0.05	30	SUNY Ag and Tech at Farmingdale	NY	-0.07	-0.17	135
Goldey Beacom Coll.	DE	-0.16	-0.12	21	Cuyahoga CC	OH	-0.08	-0.15	75
Manatee Tech. Coll.	FL	0.06	-0.07	32	Northeasten OK Ag. and Mech. Coll.	OK	-0.00	-0.07	37
Miami-Dade J Coll.	FL	-0.09	-0.16	22	CC of Allegheny County	PA	-0.24	-0.14	23
Pensacola J Coll.	FL	-0.05	-0.15	388	Point Park Coll.	PA	-0.08	-0.14	27
Santa Fe J Coll.	FL	-0.12	-0.20	23	Robert Morris J Coll.	PA	-0.12	-0.11	38
North Iowa Area CC	IA	0.03	-0.14	24	York Coll. of Pennsylvania	PA	0.03	-0.08	42
City Coll. of Chicago	IL	0.04	-0.04	265	Kilgore Coll.	TX	-0.09	-0.16	43
Triton Coll.	IL	0.18	-0.09	23	San Antonio Coll.	TX	-0.15	-0.19	221
Wabash Valley Coll.	IL	-0.30	-0.32	28	San Jacinto Coll.	TX	-0.10	-0.12	38
Cowley County CC	KS	-0.17	-0.20	79	Texarkana Coll.	TX	-0.33	-0.19	21
Quincy Coll.	MA	-0.19	-0.14	24	Tyler J Coll.	TX	-0.49	-0.19	21
Wentworth I	MA	0.17	-0.05	70	Wharton County J Coll.	TX	-0.04	-0.13	66
Catonsville CC	MD	-0.04	-0.18	27	Everett J Coll.	WA	-0.01	-0.14	105
CC of Baltimore	MD	-0.23	-0.25	52	Olympic Coll.	WA	-0.01	-0.17	21
Essex CC	MD	-0.04	-0.18	42	Yakima Valley Coll.	WA	-0.12	-0.24	99
Henry Ford CC	MI	0.33	0.20	36	Milwaukee School of Engineering	WI	-0.11	-0.17	125

Note: See note to Table DD-1. Source: Project Talent and IPEDS.

Table DD-6: 1963 Value-Added of Small Institutions, Aggregated to States

Institution	$	$ VA	N	Institution	$	$ VA	N
Other AL Univ.	0.02	-0.03	38	Other WV Univ.	-0.04	-0.14	22
Other CA Univ.	-0.04	-0.05	153	Other AL Comm. Coll.	-0.17	-0.22	34
Other CO Univ.	-0.14	-0.11	24	Other CA Comm. Coll.	-0.02	-0.09	123
Other CT Univ.	0.01	-0.17	38	Other CO Comm. Coll.	-0.06	-0.21	53
Other DC Univ.	-0.02	-0.09	25	Other CT Comm. Coll.	-0.01	-0.10	51
Other FL Univ.	-0.23	-0.18	46	Other FL Comm. Coll.	-0.10	-0.16	86
Other GA Univ.	-0.18	-0.15	53	Other GA Comm. Coll.	-0.06	-0.14	67
Other IA Univ.	-0.04	-0.12	46	Other IA Comm. Coll.	-0.12	-0.25	73
Other IL Univ.	-0.02	-0.11	67	Other IL Comm. Coll.	0.01	-0.09	249
Other IN Univ.	-0.11	-0.12	44	Other IN Comm. Coll.	0.06	-0.12	55
Other KS Univ.	-0.19	-0.18	22	Other KS Comm. Coll.	-0.20	-0.23	40
Other KY Univ.	-0.22	-0.18	34	Other KY Comm. Coll.	-0.22	-0.24	30
Other LA Univ.	-0.14	-0.13	51	Other MA Comm. Coll.	-0.11	-0.14	133
Other MA Univ.	0.03	-0.09	163	Other MD Comm. Coll.	-0.03	-0.08	31
Other MD Univ.	-0.13	-0.10	45	Other MI Comm. Coll.	-0.10	-0.16	87
Other ME Univ.	0.09	-0.12	31	Other MN Comm. Coll.	-0.03	-0.16	84
Other MI Univ.	-0.07	-0.04	96	Other MO Comm. Coll.	-0.24	-0.25	71
Other MN Univ.	-0.15	-0.14	52	Other MS Comm. Coll.	-0.26	-0.21	22
Other MO Univ.	-0.05	-0.17	98	Other NC Comm. Coll.	-0.15	-0.17	90
Other NC Univ.	-0.05	-0.08	66	Other NJ Comm. Coll.	-0.09	-0.17	48
Other NE Univ.	-0.31	-0.22	39	Other NY Comm. Coll.	0.03	-0.07	345
Other NY Univ.	-0.06	-0.07	224	Other OH Comm. Coll.	-0.01	-0.08	59
Other OH Univ.	-0.13	-0.15	91	Other OK Comm. Coll.	-0.08	-0.21	50
Other OR Univ.	-0.26	-0.24	31	Other OR Comm. Coll.	-0.08	-0.15	40
Other PA Univ.	-0.12	-0.11	108	Other PA Comm. Coll.	-0.05	-0.14	144
Other SC Univ.	-0.25	-0.18	27	Other SC Comm. Coll.	-0.36	-0.25	25
Other TN Univ.	-0.17	-0.16	55	Other TN Comm. Coll.	-0.33	-0.30	24
Other TX Univ.	-0.03	-0.13	144	Other TX Comm. Coll.	-0.09	-0.19	177
Other VA Univ.	0.03	0.01	36	Other VA Comm. Coll.	-0.09	-0.08	47
Other VT Univ.	-0.20	-0.24	24	Other WA Comm. Coll.	-0.01	-0.17	46
Other WI Univ.	-0.04	-0.09	63	Other WI Comm. Coll.	-0.04	-0.09	58

Note: The average male age-29 log wage and average log wage value-added (relative to CSU Long Beach) of each set of two- and four-year institutions, aggregated by state across institutions with fewer than 20 employed enrollees and restricted to those with at least 11 employed male enrollees in the Project Talent database, and the number of respondents whose wages were used in estimation. Value-added is estimated following Chetty et al. (2020), conditioning on fifth-order polynomials in measured test score rank and parental income rank and race and gender indicators. University names are 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.[93] 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.[94] 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.

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 7.1 and the limitations of the Dale-Krueger methodology discussed above, we believe

[93] 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).

[94] 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

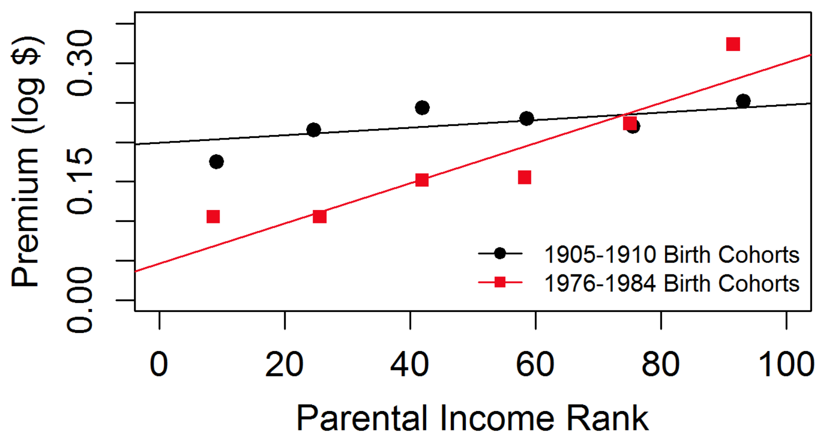
Note: This table shows that Dale-Krueger portfolio fixed effects – either UC application-admissions (MH) or all College Board score-sends (MH') – absorb considerably less than half of between-institution variation in average wages among universities that are frequented by applicants to UC campuses. Each 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).

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.

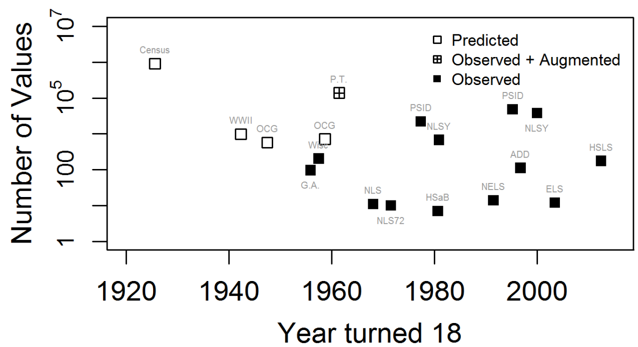
Other Appendix Figures and Tables

Figure A-1: The Observational **Log-Dollar** Wage Premium to Going to College by Parental Income



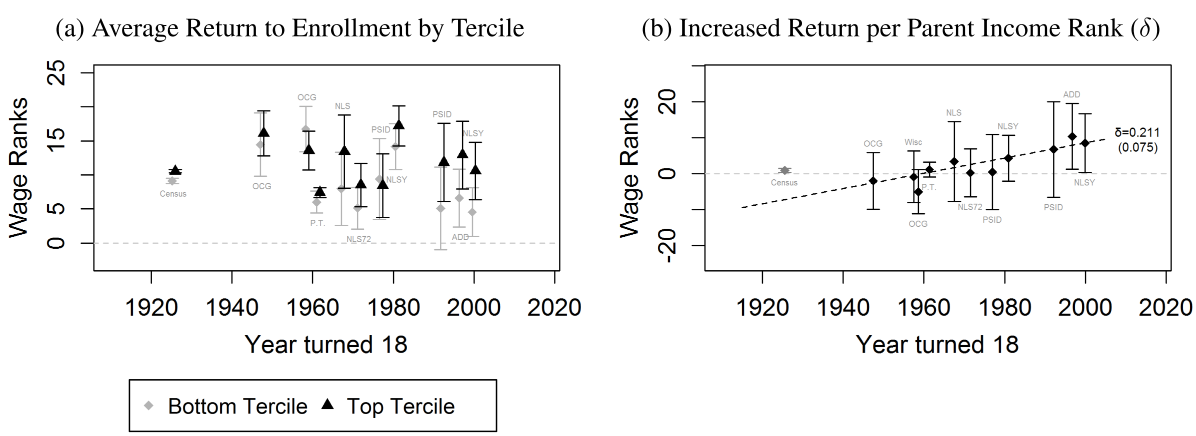
Note: This figure complements Figure 1 by showing a similar pattern of novel collegiate regressivity at the end of the 20th century. The difference between the average log wage income among employed early-30s men who had completed at least one year of college and those who completed high school but not any years of college, by quantile of parental income rank. The 1905–1910 ('1900s') cohorts are measured among father-son pairs matched in the 1920 and 1940 Censuses following Abramitzky et al. (2012) with imputed father's incomes; the '1980s' cohorts are measured among youths in the ADD Health survey (1977–1981) and the NLSY97 (1980–1984) and use survey weights. The best-fit lines are simultaneously estimated without additional covariates in the underlying data; their slopes are 0.0005 (s.e. 0.0001) and 0.0025 (0.0007), and the t-statistic of their difference is 3.12. The sample includes 352,995 men in the earlier cohorts and 4,072 men in the later cohorts. Unemployed sons and those with incomes below the contemporaneous half-time federal minimum wage are omitted. See Appendix A for details on data construction and 1920s parental income imputation. Source: US Census, ADD Health, and NLSY97.

Figure A-2: Number of Parental Income Bins by Dataset



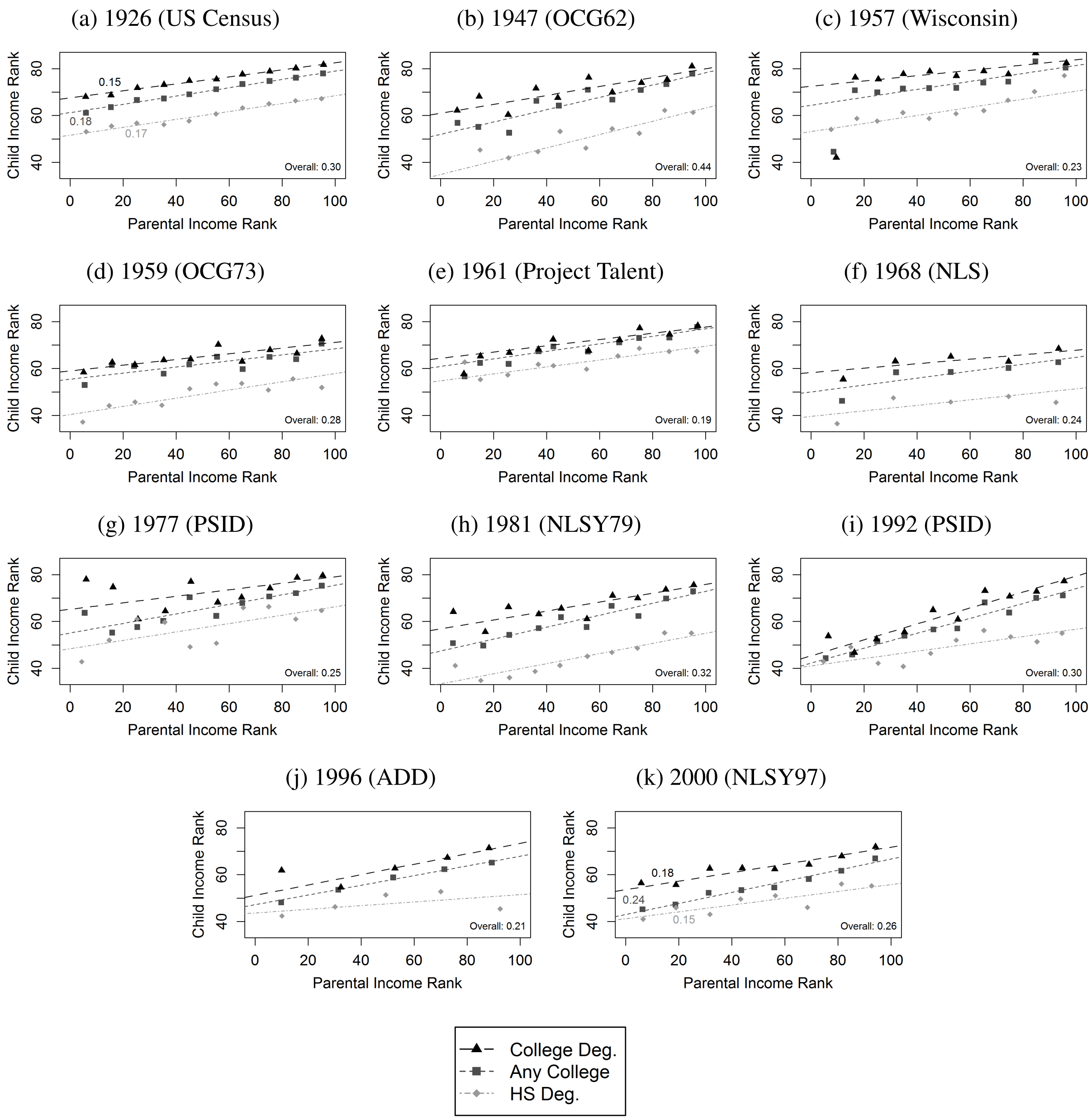
Note: This figure shows that parental income is predicted in the earliest survey datasets (and partially predicted in Project Talent), but that starting in 1960 there is no meaningful relationship between bin count and year, suggesting that the observed dynamics are unlikely to be explained by steady 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 Enlistment Cards, CPS OCG, Wisconsin, Project Talent, NLSM, NLS72, PSID, NLSY79, ADD Health, and NLSY97.

Figure A-3: Regressivity of US Higher Education Over Time in **Wage Rank**



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 and $Coll_{it}$ indicates at least one year of college. Dataset-specific estimates and 95-percent confidence intervals are from Equation 2 estimated with separate δ_t terms for each dataset; the linear slope excludes Census respondents and can be interpreted as the annual increased relative rank wage value of college-going per 100 family income ranks. Child incomes below the contemporaneous half-time federal minimum wage are omitted. All regressions are weighted using standardized survey weights; 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: Rank-Rank Income Correlation for Age 31–35 Children by Survey

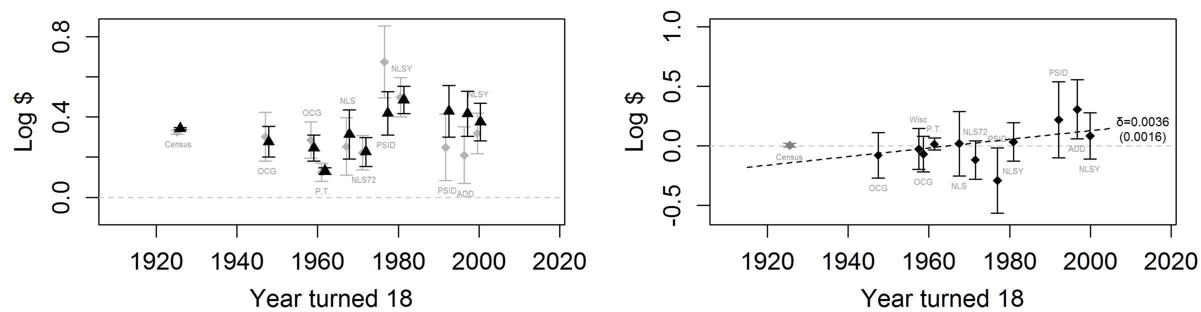


Note: These figures visualize the change over time in the relative observational return to college-going for lower- and higher-income students in the US. The income gains to college attendance fall to zero in the lowest parental income deciles beginning with the 1961 cohort. Binned scatterplots and slopes of income rank among employed age 31–35 men by 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-5: Regressivity of US Higher Education **Attainment** Over Time

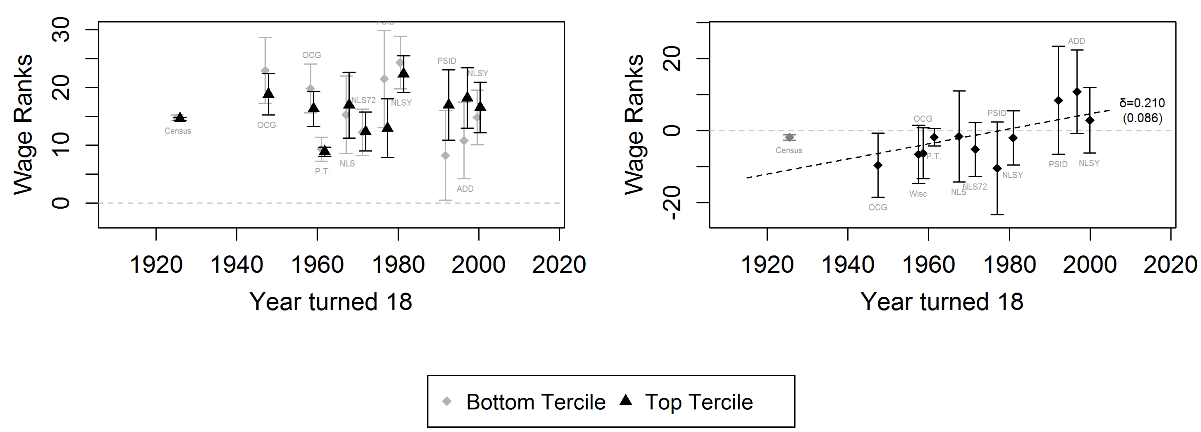
Panel A: Regressivity of College Attainment in Log Dollars

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



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

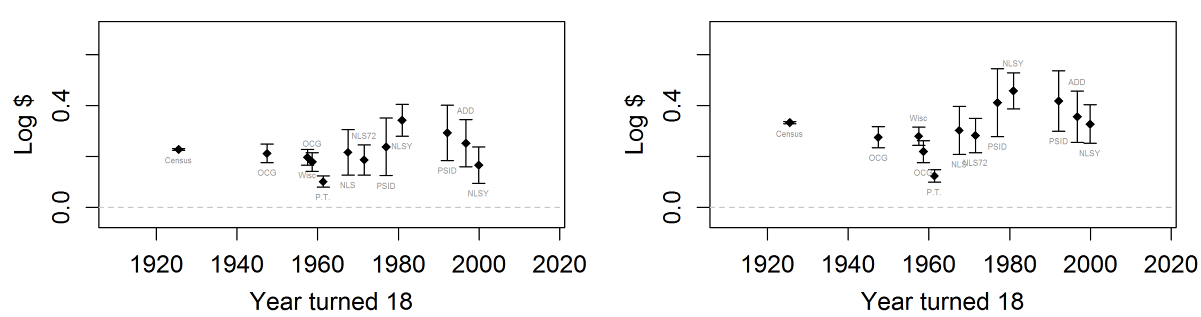
(c) Average Return to Enrollment by Tercile (d) Increased Return per Parent Income Rank (δ)



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-6: Average Observational Return to US Higher Education at Median Income

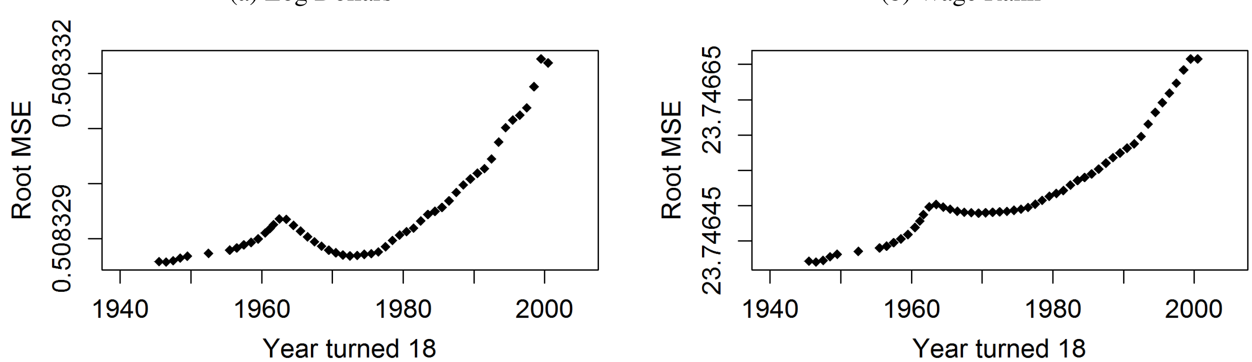
(a) College Enrollment (b) College Attainment



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 Enlistment Cards, Wisconsin, Project Talent, NLSM, NLSY79, and NLSY97.

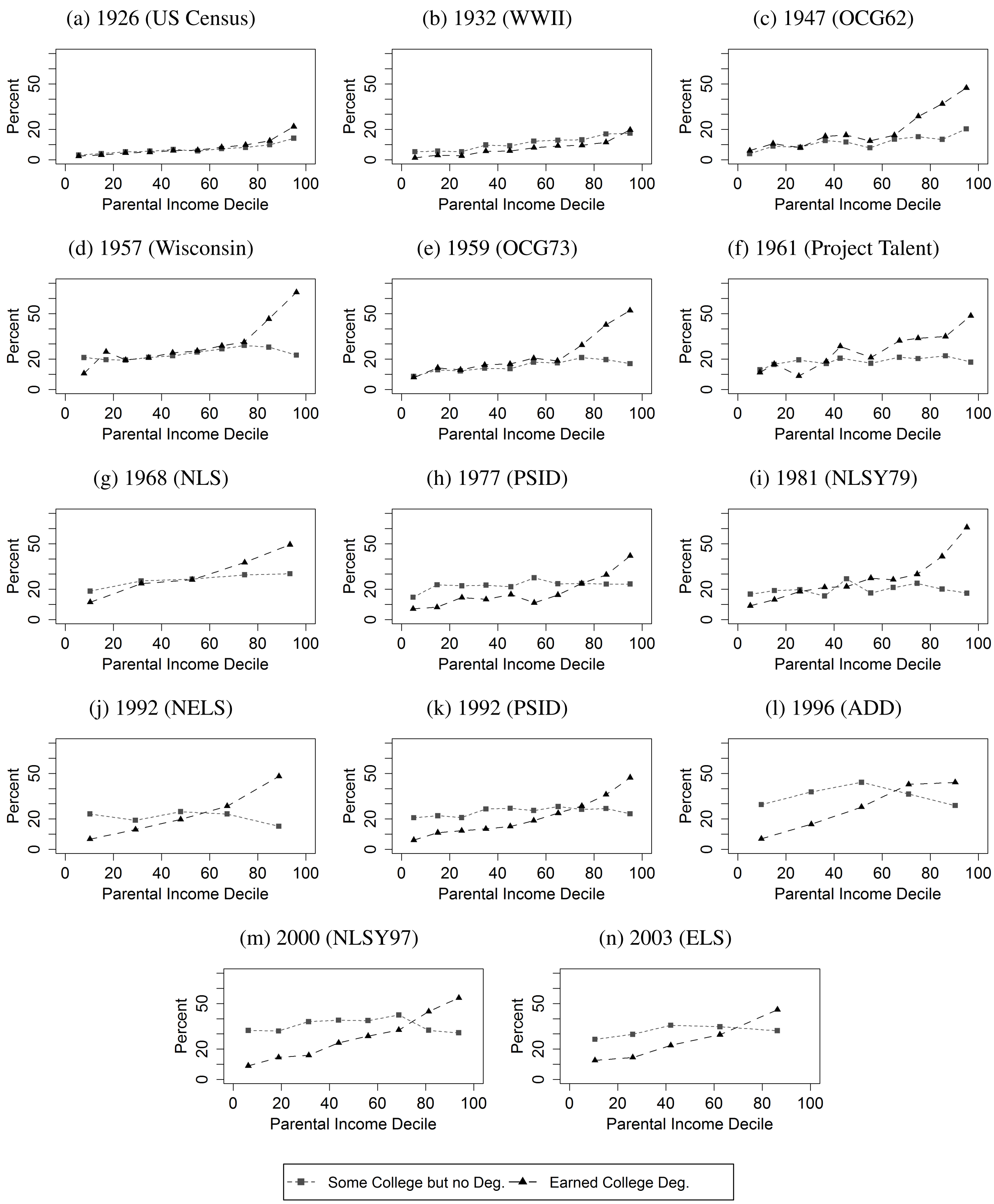
Figure A-7: Root Mean Squared Error from Regressivity Models with Kink Points

(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.

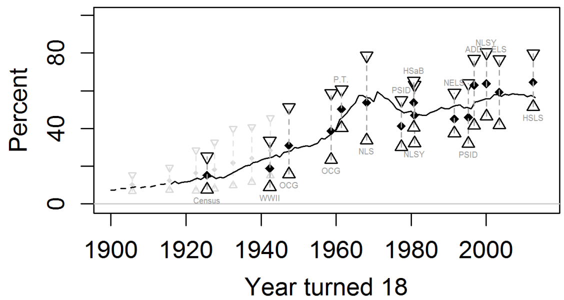
Figure A-8: Selection into College Enrollment and Attainment by Parental Income



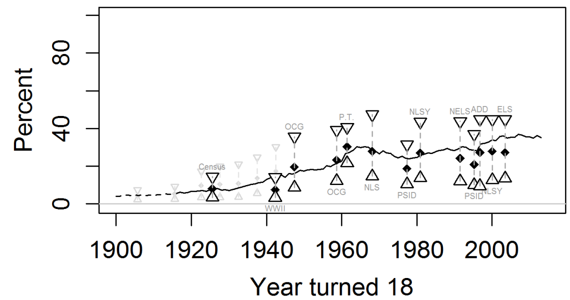
Note: These figures show increasing shares of youths who enroll in college without graduating and who attain a college degree overall and by parental income decile over the 20th century, with changing patterns of non-completion by parental income over time and demonstrable overlap between surveys covering 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-9: Unconditional College Enrollment and Attainment By Tercile Since 1900

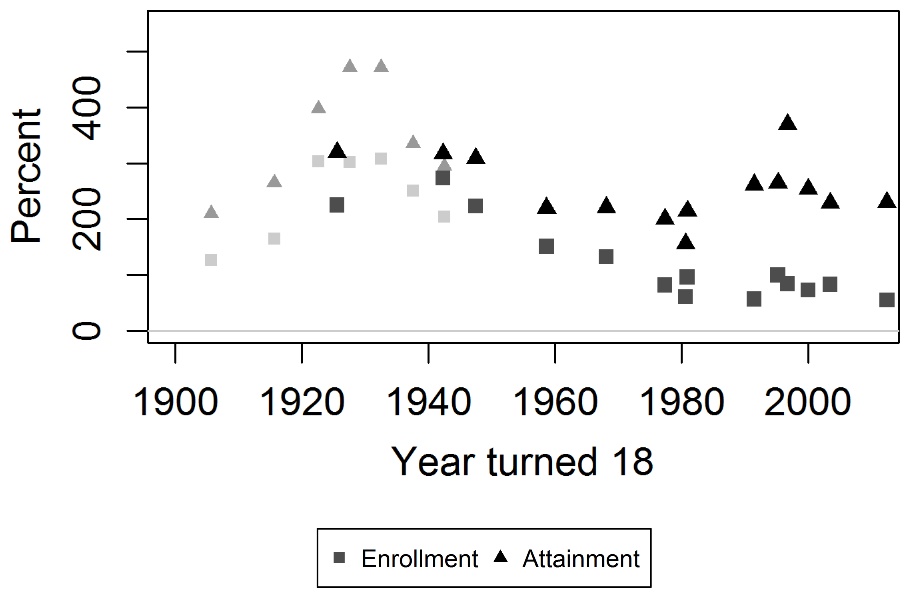
(a) College Enrollment



(b) College Attainment



(c) Top-Tercile Students' Percent Higher College-Going Relative to Bottom-Tercile Students

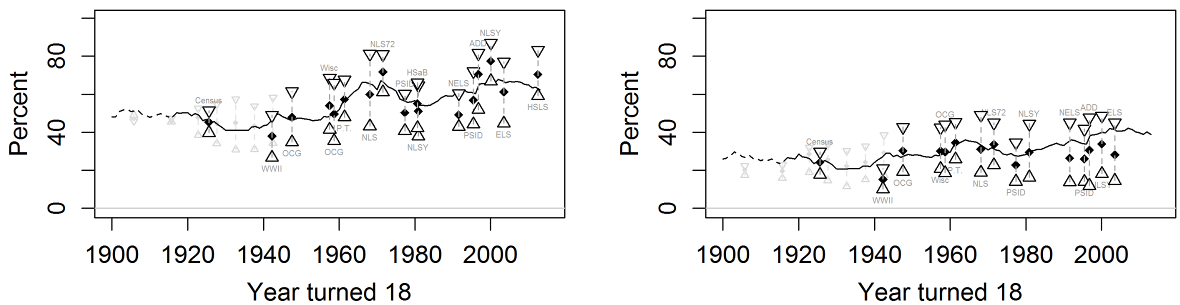


Note: This figure shows that lower-income students became relatively less likely to attend or graduate from college than higher-income peers after the 1920s age-18 cohorts, followed by stagnation in college enrollment and declines in degree attainment since the 1970s. Panels (a) and (b): Points in black 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. The solid line reports the same overall average educational outcome for 1940 and 1960–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, 2021, and 2023 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 bin 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 Enlistment Cards, CPS OCG, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, ELS, and ACS.

Figure A-10: Male High School Graduates' College Enrollment and Attainment Over Time

(a) College Enrollment

(b) College Attainment

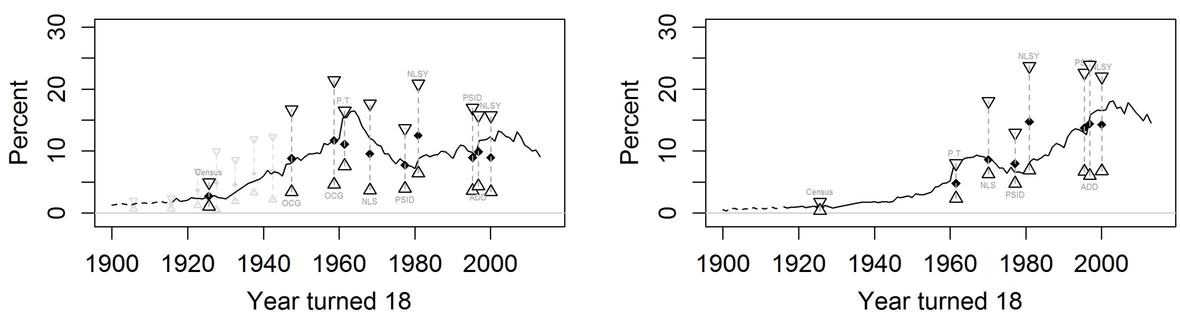


Note: This figure restricts the sample to high school graduates and still replicates the U-shaped gap in college enrollment between high- and low-income students found in Figure 4a. Share of male **high school graduates** between ages 30 and 35 who had completed at least one (enrollment) or four (attainment) years of college overall (black diamond) or among those from the bottom (△) or top (▽) tercile of parental incomes when approximately aged 14–17. The solid line reports the same overall average educational outcome for 1940 and 1960–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, 2021, and 2023American 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 bin from 35–40 to 55–60. See Appendix A for details on data construction. Source: US Census, WWII Enlistment Cards, Wisconsin, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, and ELS.

Figure A-11: Graduate School Enrollment Over Time

(a) Male

(b) Female

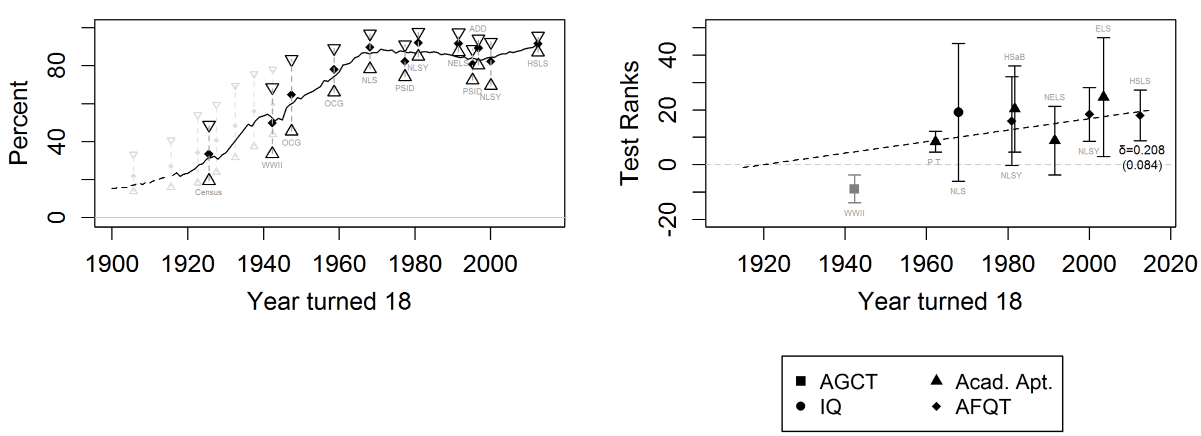


Note: This figure shows that parental income gaps in graduate school enrollment are proportionally larger and more stable than college enrollment gaps after World War II, with female graduate school enrollment outpacing male enrollment at the bottom of the income distribution since the 1970s. The consistency 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. The solid line reports the same overall average educational outcome for 1940 and 1960–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, 2021, and 2023 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 bin 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 Enlistment Cards Wisconsin, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, and ELS.

Figure A-12: Selection into High School Degree Attainment

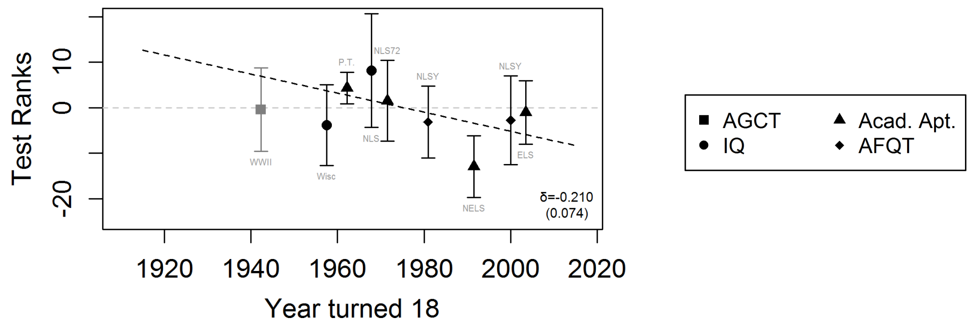
(a) High School Graduation Rate

(b) Academic Selection into HS Graduation



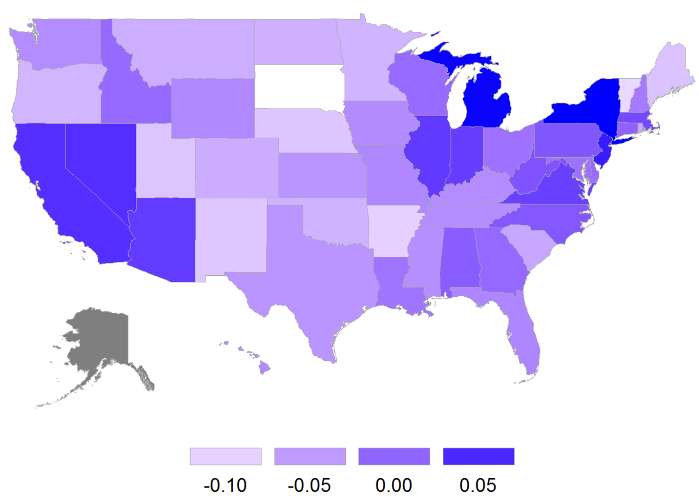
Note: This figure shows that the level and selection by adolescent test scores into high school graduation stabilized by the 1960s both overall and by parental income tercile. **Panel A:** Share of males between ages 30 and 35 who had completed a high school degree overall (black diamond) or among those from the bottom (△) or top (▽) tercile of parental incomes when approximately aged 14–17. The solid line reports the same overall average educational outcome for 1940 and 1960–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, 2021, and 2023 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 bin from 35–40 to 55–60. **Panel B:** The estimated differential selection into male high school graduation 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 skill. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. All regressions are weighted using standardized survey weights; standard errors are robust. AGCT, IQ, Academic Aptitude, and AFQT exams are transformed into within-sample ranks; see Appendix A for details on variable definition and data construction. Source: US Census, WWII Enlistment Cards, CPS OCG, Wisconsin, Project Talent, NLS, NLSY79, PSID, NELS, ADD, NLSY97, and ELS.

Figure A-13: Selection into College **Attainment** by Pre-College Cognitive Skill



Note: This figure shows that college graduates' pre-college cognitive skill was similar by parental income over time (relative to same-income high school graduates), extending the enrollment-based findings in Figure 5b, though there is some evidence of a *downward* trend. The absence of an upward trend in high-income students' relative pre-college test scores rejects a 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 skill. All regressions are weighted using standardized survey weights (where Census respondents each have unit weight); standard errors are robust. All regressions are weighted using standardized survey weights; standard errors are robust. AGCT, IQ, Academic Aptitude, and AFQT exams are transformed into within-sample ranks; see Appendix A for details on variable definition and data construction. Source: US Census, WWII Enlistment Cards, Wisconsin, Project Talent, NLSM, NLSY79, and NLSY97.

Figure A-14: Geographic Distribution of Mid-Century Institutional Value-Added

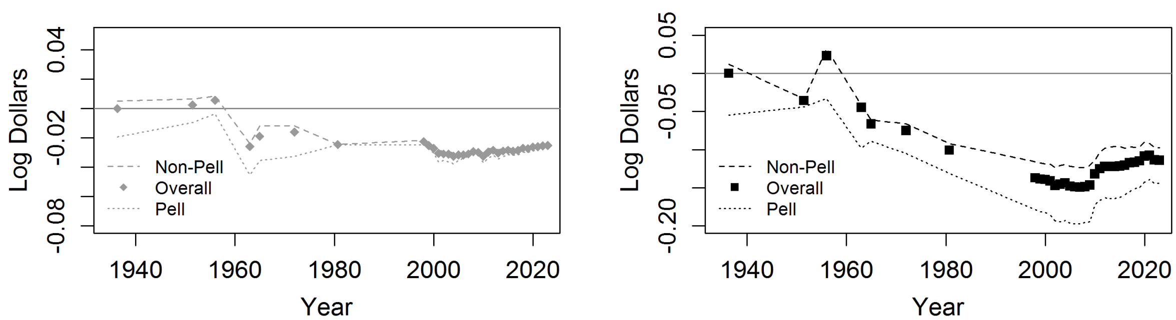


Note: This figure shows that the states offering the highest-value college enrollment in the mid-20th century were in the West and Northeast, while the lowest-value enrollments were in the Plains and Mountain West. The enrollment-weighted average estimated mid-century value-added of institutions where 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-15: Average Enrollment Value-Added Over Time by Pell Eligibility

(a) 1960s Value-Added

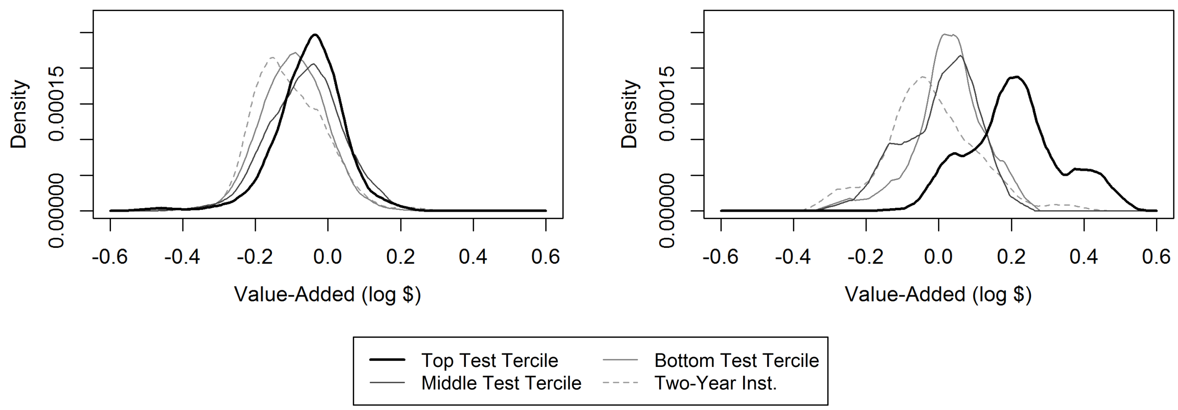
(b) 1990s Value-Added



Note: This figure shows that average enrollment-weighted collegiate value-added declined over the past 80 years, falling sharply during the 1960s to 1980s expansion of two-year institutions and rising somewhat in the last decade as community college and for-profit enrollment has fallen. The male-enrollment-weighted average institutional log wage value-added of US colleges and universities by age-18 cohort, where value-added is estimated for age-18 cohorts 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, the NLS72 and in more recent years by two and bottom two parental income quintiles (1998-2009, Chetty et al., 2020) or 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.14. Pell and non-Pell enrollments are reweighted to match total enrollments by segment 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: CPS OCG, Project Talent, NLS72, IPEDS, Chetty et al. (2020) (average wages by institution), and Bleemer (2022).

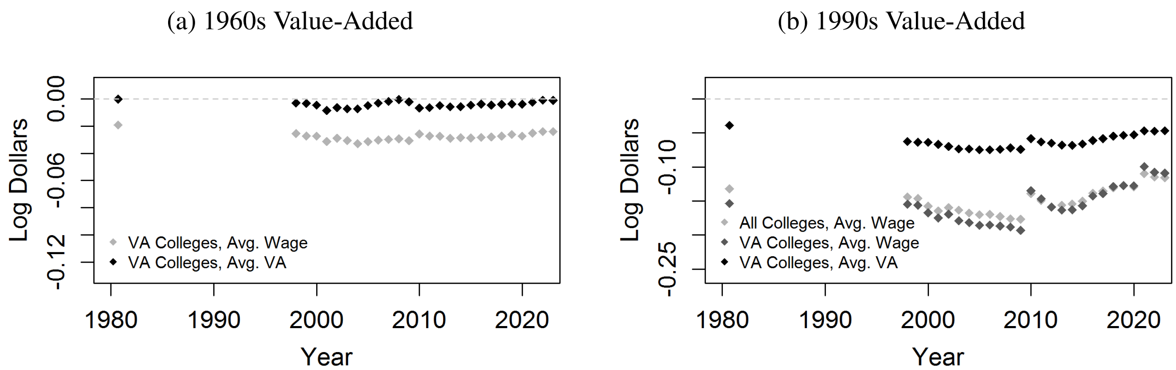
Figure A-16: Institutional Value-Added **by testing tercile** in the 1960s and 1990s

(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 6 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).

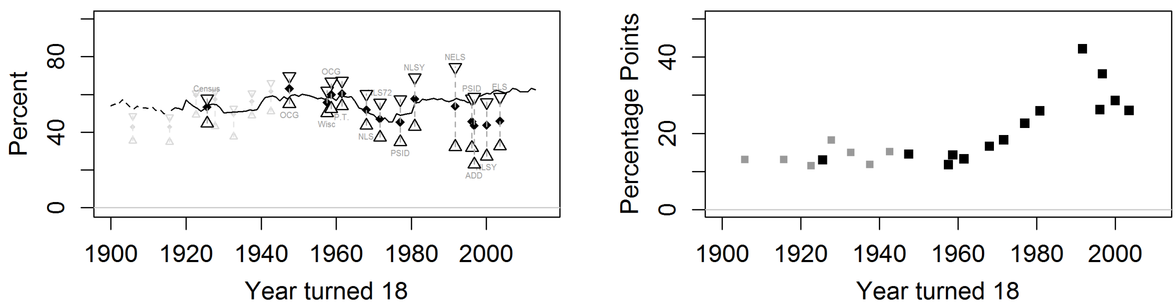
Figure A-17: Institutional Enrollment by Pell Eligibility Indexed by Average Wage or Value-Added



Note: This figure shows that the difference in collegiate returns between Pell- and non-Pell students is similar when using either value-added or average wages in the 1960s but is much larger now. The choice of sample does not affect the latter result, suggesting that the covariates employed in producing value-added statistics absorb first-order selection bias across institutions. 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 segment 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-18: College Completion and Dropout Rates By Tercile Since 1900

(a) Attainment Conditional on Enrollment (b) Bottom-Top Tercile Gap in Dropout Rate

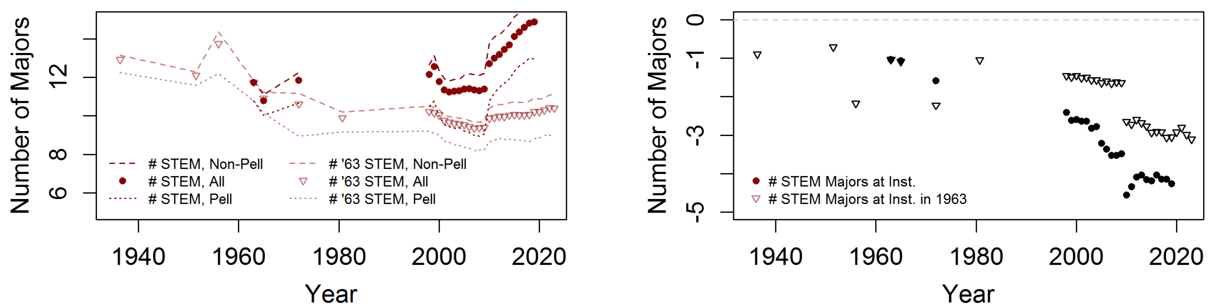


Note: This figure demonstrates that the overall stability of college dropout rates – contrary to Bound et al. (2010) and Denning et al. (2022), who measure college completion at 25 (a younger age) – during the past century masks a substantive decline in four-year degree attainment rates after 1960 among college students from the bottom parental income tercile. Panel (a): 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, conditional on enrolling in college (and reporting completion of one year). The solid line reports the same overall average educational outcome for 1940 and 1960–2000 Census respondents (in the IPUMS 1% sample) and the 2006, 2011, 2016, 2021, and 2023 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 bin from 35–40 to 55–60. Panel (b): The percentage point gap between the college dropout rate (that is, the share of college-goers who do not attain a four-year degree) between students from families with bottom- and top-tercile parental incomes, as measured in the same datasets as shown in Panel (a). See Appendix A for details on data construction. Source: US Census, WWII Enlistment Cards, CPS OCG, WLS, Project Talent, NLS, NLS72, NLSY79, PSID, NELS, ADD, NLSY97, ELS, and ACS.

Figure A-19: Number of Available STEM Major Programs by Income Tercile

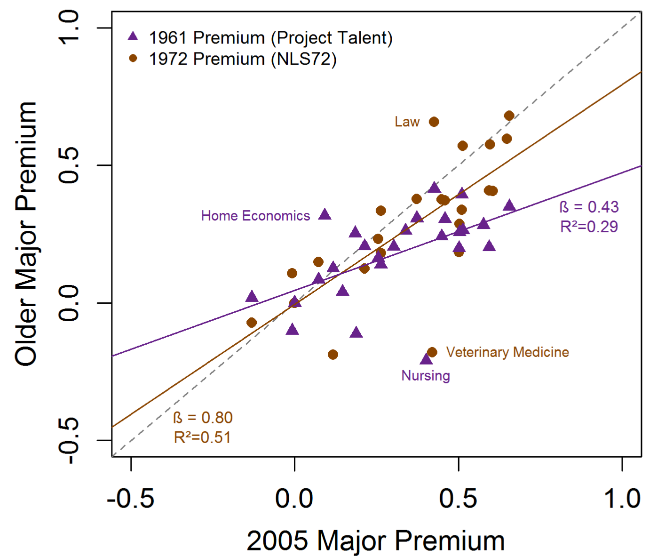
(a) Levels

(b) Difference Between Income Terciles



Note: This figure shows that while lower-income students have not shifted into universities that traditionally offered fewer STEM degree programs, the four-year universities where lower-income students' enroll have seen declining numbers of available STEM majors. Given that lower-income students had high STEM major attainment in the 1990s, however, these declines are more likely indicative of these schools' broader reductions in educational opportunity than of a first-order mechanism of declining educational value. The average number of STEM majors offered at four-year institutions where students enroll, overall and by income tercile, in the contemporaneous year (dark circles) or fixing the institution's available majors in 1963 (open triangles). Number of STEM majors is defined as the number of unique CIP codes in which BAs are earned between 3 and 5 years after students' matriculation, as observed in HEGIS until 1972 and IPEDS thereafter and winsorized at 1 percent. Enrollments are measured in the CPS OCG, Project Talent, the NLS72, and 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.14 for details. STEM is defined as degrees in biological sciences, computer science, mathematics, physical sciences, or engineering. See Appendix A for details on data construction. Source: CPS OCG, Project Talent, NLS72, HEGIS, and IPEDS.

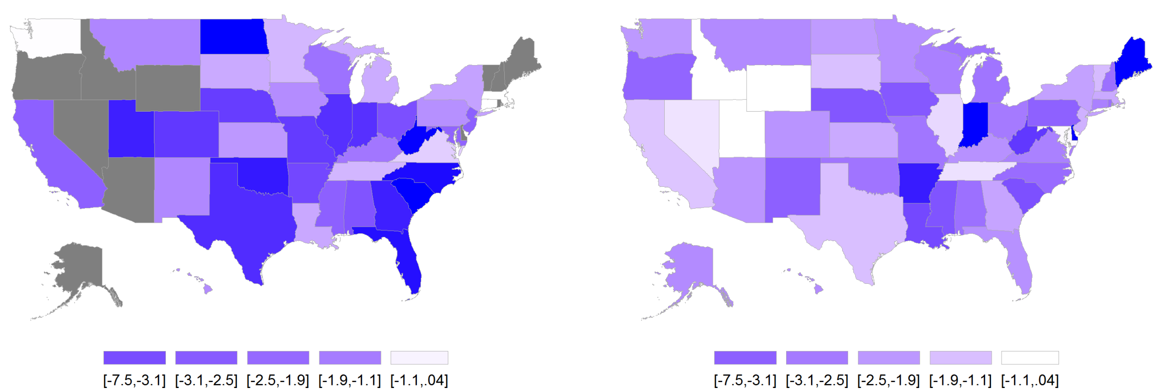
Figure A-20: Correlation Between Detailed College Major Premiums Over Time



Note: This figure shows that detailed college major premiums are highly correlated over time, with exceptions primarily among vocationally-oriented degrees with changing labor demand and/or degree requirements. The relationship between average log wages by 66 detailed college major categories in the 2019-2021 ACS and average wages in either Project Talent or the NLS72, restricting each to specific majors reported by at least 20 male respondents and measuring CPI-adjusted annual log wages at age 31-35 (ACS), 29 (PT), or 32 (NLS72) partialing out cohort fixed effects. The premium of Art majors is normalized to 0. Child incomes below the contemporaneous half-time federal minimum wage are omitted. Source: Project Talent, NLS72, and the ACS (Ruggles et al., 2024).

Figure A-21: Geographic Distribution of Major Stratification

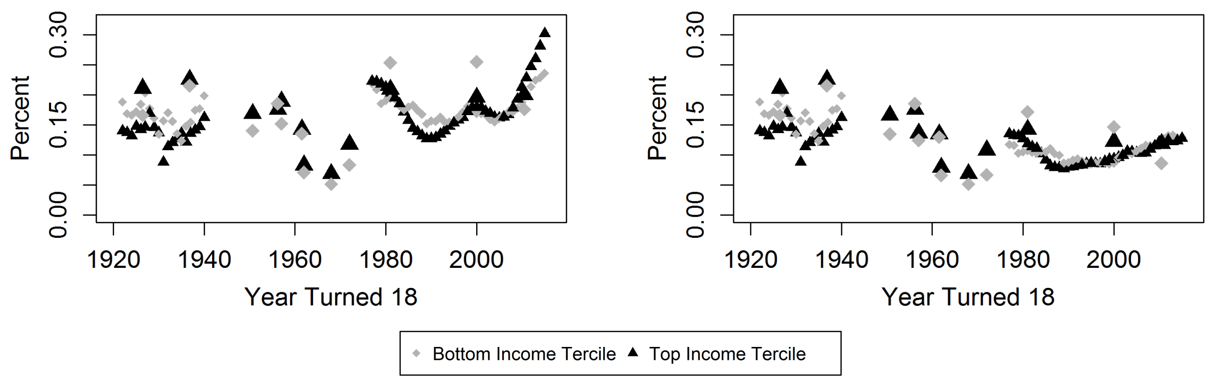
(a) 1960s (b) 2010



Note: This map shows that higher- and lower-income students at universities in the Northeast and West have tended to earn majors with similar economic value in both the 1960s and in recent years, while higher-income students in the South and Midwest tend to declare higher-paying majors than lower-income students in those regions. 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 44 (66) 'detailed' categories in Project Talent (the 2019–2021 ACS; see Figure 9). Annual 2010 parental income terciles were measured by Pell status among 2015–2016 degree recipients in the College Scorecard; see Appendix A.14. See Appendix A for details on data construction. Gray-shaded states had insufficient data to report mean differences (fewer than 11 lower- or higher-income students). Source: Project Talent, College Scorecard, and the ACS (Ruggles et al., 2024).

Figure A-22: Average Annual Engineering Degree Gap by Income Tercile

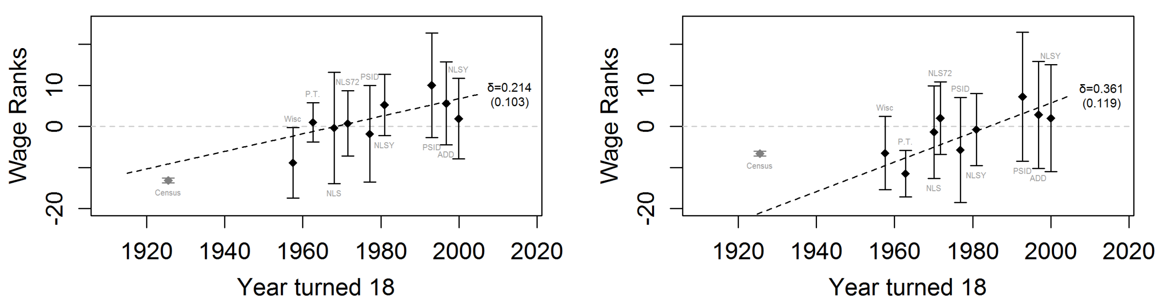
(a) Overall (b) Excluding Computer Science



Note: This figure shows that engineering – the highest-value discipline in all periods – was not stratified by income in the early 20th century but is at now its most stratified point in the last 100 years, which can be largely explained – at least in California – 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 recipients in the College Scorecard; see Appendix A.14). 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-23: 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.

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	352,995	0	0	0	108,422	0	0
WWII Cards	1923–1926	2,923	0	0	0	0	0	0	0
CPS OCG 62	1927–1932	0	1,710	0	0	0	0	0	0
CPS OCG 73	1938–1943	0	2,727	1,327	1,414	0	0	0	0
Wisc. L.S.	1938–1940	3,239	3,283	1,041	0	3,638	1,556	701	0
Project Talent	1941–1946	18,437	37,504	26,495	47,869	18,637	18,854	19,455	34,432
NLS M/W	1948–1954	1,401	1,144	1,138	0	1,237	1,114	515	0
NLS 72	1952–1954	3,536	3,586	2,687	2,648	3,469	3,158	2,353	2,254
HSaB	1961–1964	4,258	0	0	4,479	4,749	0	0	4,901
NLSY 79	1961–1965	2,418	1,866	1,314	0	2,467	1,667	1,484	0
NELS	1972–1974	4,570	0	0	0	5,152	0	0	0
ADD Health	1977–1981	0	1,450	0	0	0	1,536	0	0
NLSY 97	1980–1984	2,702	2,622	3,269	0	2,840	2,563	3,445	0
ELS	1984–1986	4,212	0	0	0	4,537	0	0	0
PSID	1952–1988	0	1,954	0	0	0	1,933	0	0
HSLS	1993–1995	6,958	0	0	0	7,513	0	0	0
Panel B: Survey Respondents to Other Surveys									
Time Survey	1908–1917	0	1,818	1,809	†	0	532	531	†
Time-Census Link	1884-1923	0	††	742	742	0	0	0	0
Great Asp.	1930-1941	0	0	18,570	18,863	0	0	9,039	9,229
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	1966–2005	0	0	0	136,240	0	0	0	136,240
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 datasets used in our study. The number of observations in each dataset with non-missing pre-college cognitive skill, early-30s respondent income, college major, and/or 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. Birth Year is school year minus 18 in Panel D. † These data are only available for four-year institutions, and are omitted from most analysis. †† These data are available but unused. Source: See Appendix A.

Table A-2: 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	5,947	20,669	6,468	17,429	15,390	23,040	39,004	2,576	8,630	15,987
	Non-Profit	8,760	20,036	9,289	26,327	13,798	40,268	53,900	40,516	22,575	40,579
Second Test Quartile	Public	4,320	22,565	3,536	19,090	3,535	21,725	26,609	9,499	3,459	10,297
	Non-Profit	7,465	24,107	6,373	25,359	5,038	24,755	27,346	16,779	4,419	7,180
Third Test Quartile	Public	1,641	21,958	1,193	20,354	2,802	7,213	13,846	10,586	1,305	8,391
	Non-Profit	6,266	22,997	6,023	28,224	-5,639	19,332	12,710	15,514	642	10,066
Bottom Test Quartile	Public	0	17,074	0	23,210	0	16,853	0	25,353	0	4,845
	Non-Profit	-993	24,946	880	23,654	-1,178	8,329	-1,129	21,910	-225	7,334
No Reported SAT Scores	Public			2,682	24,578	3,577	4,146			738	6,888
	Non-Profit			3,061	19,269	1,211	31,262			178	14,707
Community Colleges	Public	-1,164	17,038	-730	20,705	-590	19,219			-2,249	3,935

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-testing 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 7 using age 29 wages from Project Talent. [2] Value-added estimated following Equation 7 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).

Table A-3: Pairwise Correlations Between University Characteristics and Value-Added

Dep. Var.:	Income Per Stud. ($'000s)			Wage Value-Added (log $)		
Year of Dep. Var.:	1963	1996	'96 - '63	1963	1996	'96 - '63
Year of Indep. Vars:	'60s	'90s	'90s	'60s	'90s	'90s
Average Parent Income Rank	0.8669 (0.1247)	1.0138 (0.0356)	0.6010 (0.0612)	0.0035 (0.0006)	0.0059 (0.0005)	0.0069 (0.0017)
Students per Faculty	-0.4776 (0.0700)	-0.0332 (0.0060)	-0.7782 (0.0671)	0.0001 (0.0005)	-0.0018 (0.0002)	-0.0043 (0.0020)
Total Rev. per Student ($'000s)				0.0003 (0.0002)	0.0034 (0.0003)	0.0022 (0.0006)
Six-Year Grad. Rate (%)	0.2600 (0.0562)	0.6740 (0.0259)	0.3955 (0.0406)	0.0008 (0.0002)	0.0006 (0.0005)	0.0052 (0.0013)
Share of Degrees in STEM (%)	33.8970 (7.3999)	59.3322 (4.6305)	35.4902 (5.9971)	0.1260 (0.0322)	0.6210 (0.0682)	0.6891 (0.2967)
Normalized Avg. Test Score	6.2016 (0.8693)	15.1223 (0.4936)	7.1841 (0.8118)	0.0143 (0.0042)	0.0435 (0.0117)	0.0809 (0.0171)
Public Institution	-0.1075 (1.5243)	-10.6752 (1.2790)	-10.9724 (1.5640)	-0.0329 (0.0110)	0.1696 (0.0071)	-0.1490 (0.0536)
Research University	12.5554 (1.8020)	20.7521 (1.4660)	7.8454 (1.8448)	0.0283 (0.0108)	0.2644 (0.0095)	0.2109 (0.0470)
Two-Year Institution	-7.6419 (1.4947)	-18.5436 (0.8351)	-9.8935 (1.7915)	-0.0356 (0.0178)	0.0199 (0.0040)	-0.0479 (0.0502)
Max Obs.	1,223	1,854	819	429	136	46

Note: This table shows that college value-added is persistently and increasingly predicted by measures like student-faculty ratios, completion rates, STEM attainment, and peer academic performance, and is strongly and increasingly correlated with average parental income. 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 OLS correlation *measured separately* (not conditional on the other characteristics). Mid-century value-added is estimated following Equation 7 using age 29 wages from Project Talent; late 20th century value-added estimated following Equation 7 using age 31–35 California wages among 1995–1997 University of California applicants as reported in Appendix I of Bleemer (2022). '**Total Revenue Per Stud.**' is measured as total annual income per enrolled student (Blue Book) or total non-hospital revenue per FTE student (1995 IPEDS) CPI-adjusted to 2022; '**Average Parent Income Rank**' is measured across family incomes in Project Talent or reported for the 1980–1982 birth cohorts from IRS data by Chetty et al. (2020); **Two-year institutions** are measured in the 1962 Blue Book (column 1) or 1996 IPEDS (columns 2–4); '**Normalized Avg. Test Score**' is academic score (Project Talent, column 1) or SAT (averaged across 1995–1997 UC applicants who first enroll at that institution) score standardized across observed enrollment-weighted institutions; '**Share STEM Degrees**' is the percent of BAs given in biological sciences, computer science, mathematics, physical sciences or engineering as reported in the 1966 HEGIS (among men) or 1996 IPEDS, omitting two-year institutions; '**Log Cost of Attend.**' is total posted tuition, required fees, room, board (Blue Book or IPEDS); and '**Six-Year Graduation Rate**' is the share of 1960s (Project Talent) or 2009 (IPEDS) freshman students who graduate from that institution within six years. Combined R^2 is measured from a regression of each dependent variable on all covariates.1996 value-added estimates are propensity-score-weighted to represent US higher education by segment interacted with expenditures, average parental income, and first-time enrollment. Value-added estimates are unshrunk and estimates are unadjusted for VA sampling error. See Appendix A for details on data construction.

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

Table A-4: Changes in College Quality by Parental Income Since 1960, **Fixing Enrollments in the 1960s**

	Average Inst. Characteristics by Parental Inc. Tercile							
	1960s		1990s		2010s		Differences	
Var.	Bottom	Top	Bottom	Top	Bottom	Top	90s-60s	10s-60s
***Panel A**: All Institutions*								
Students Per Faculty	18.2 [9.8]	19.0 [13.2]	19.9 [7.3]	18.4 [10.5]	21.8 [7.5]	19.9 [7.4]	2.2 (1.1)	2.8 (1.0)
Total Rev. per Student ($'000s)	14.1 [9.4]	18.5 [11.4]	31.6 [15.5]	39.5 [17.9]	40.4 [21.1]	50.7 [24.4]	-3.6 (1.5)	-5.9 (1.9)
***Panel B**: Four-Year Institutions*								
Students Per Faculty	14.7 [11.0]	15.3 [13.7]	18.2 [4.7]	17.4 [8.9]	19.7 [5.1]	18.6 [5.5]	1.4 (1.0)	1.8 (0.9)
Total Rev. per Student ($'000s)	10.8 [10.5]	13.8 [13.0]	32.7 [15.4]	39.9 [18.6]	42.5 [21.3]	51.9 [25.8]	-4.2 (1.5)	-6.4 (1.8)
Six-Year Grad. Rate (%)	56.0 [10.8]	59.1 [11.7]	55.6 [16.0]	62.8 [16.2]	55.7 [15.8]	62.7 [16.0]	-4.0 (1.5)	-3.9 (1.5)
Endowment per Student ($ '000s)	5.7 [16.6]	11.3 [23.4]	26.8 [61.1]	45.2 [61.7]	33.1 [62.6]	53.3 [62.5]	-12.9 (4.5)	-14.7 (4.6)
Share of Degrees in STEM (%)	25.8 [10.5]	27.3 [11.1]	15.6 [8.0]	16.8 [8.7]	17.4 [8.5]	19.6 [9.6]	0.3 (1.0)	-0.7 (1.0)
Normalized Avg. Test Score	0.96 [0.27]	1.14 [0.26]	0.39 [0.54]	0.66 [0.56]	0.41 [0.62]	0.73 [0.65]	-0.10 (0.04)	-0.14 (0.05)

Note: This table replicates Table 3 but fixes the enrollment distribution across institutions by parental income in the 1960s across all columns. It shows that the rising quality gap between lower- and higher-income students' 1960s four-year universities is very similar to that of their contemporaneous institutions. It is likely that that institutional change (not compositional change) largely explains lower-income students' declining institutional quality, especially within the traditional four-year sector. The mean 1960s, 1990s, and 2010s institutional characteristics of schools where 1960-1963 students from the bottom and top parental income terciles (Project Talent) enrolled, and the difference-in-differences for lower-income and higher-income students between each period. Standard deviations are in brackets and standard errors are in parentheses; all values are winsorized at an enrollment-weighted 5 percent to reduce noise in the historical statistics. '**Students Per Faculty**' is measured as total enrollment per total faculty in the 1962 Blue Book or FTE enrollment per full-time faculty in 1997 or 2015 IPEDS; '**Total Revenue Per Student**' is measured as total annual income per enrolled student (1962 Blue Book) or total non-hospital revenue per FTE student (1995 or 2015 IPEDS), both CPI-adjusted to 2022 dollars; '**Six-Year Graduation Rate**' is the share of 1960s (Project Talent) or 2009 (IPEDS) freshman students who graduate from that institution within six years; '**Endowment Per Student**' is measured as endowment per enrolled student (1962 Blue Book) or endowment per FTE student (2007 or 2015 IPEDS), both CPI-adjusted to 2022 dollars; '**Share STEM Degrees**' is the percent of BAs given in biological sciences, computer science, mathematics, physical sciences or engineering as reported in the 1966 HEGIS (among men) or 1995 or 2015 IPEDS; and '**Normalized Avg. Test Score**' is the institution's average academic score (Project Talent, column 1) or 2001 or 2015 SAT score (the summed mean of the schools' 25th and 75 percentiles of math and reading subscores), both standardized across all test-takers. Year of observation and sample sizes vary by variable due to data availability in the Blue Book, Project Talent, HEGIS, and IPEDS.

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

Table A-5: Changes in College Major Premiums Over Time

Survey:	Time	Wisc.	OCG73	P.T.	NLS	NLS72	NLSY79	NLSY97	ACS	
Year 18:	1932	1957	1959	1962	1968	1972	1981	2000	1995	2005
Human.	0.228	0.065	0.319	0.004	0.192	0.030	0.253	0.334	0.536	0.470
	(0.047)	(0.047)	(0.094)	(0.008)	(0.102)	(0.060)	(0.099)	(0.080)	(0.010)	(0.011)
Social Sci.	0.388	0.378	0.404	0.158	0.268	0.342	0.680	0.690	0.807	0.749
	(0.053)	(0.037)	(0.098)	0.008	(0.076)	(0.048)	(0.099)	(0.070)	(0.010)	(0.011)
Natural Sci.	0.386	0.285	0.377	0.145	0.412	0.377	0.571	0.692	0.869	0.853
	(0.047)	(0.040)	(0.092)	0.008	(0.091)	(0.054)	(0.107)	(0.095)	(0.011)	(0.011)
Agr.	0.301	0.217	0.274					0.555	0.683	0.664
	(0.075)	(0.092)	(0.169)					(0.234)	(0.030)	(0.038)
Bus.	0.483	0.438	0.412	0.250	0.475	0.399	0.740	0.838	0.880	0.826
	(0.051)	(0.041)	(0.050)	(0.008)	(0.080)	(0.041)	(0.064)	(0.060)	(0.008)	(0.008)
Eng.	0.532	0.481	0.412	0.340	0.654	0.606	0.774	0.821	0.980	0.972
	(0.048)	(0.040)	(0.074)	(0.009)	(0.122)	(0.054)	(0.068)	(0.064)	(0.008)	(0.008)
Health Prof.	0.685	0.938	0.200	0.359	0.552	0.343	0.597	0.822	0.879	0.745
	(0.069)	(0.132)	(0.166)	(0.025)	(0.287)	(0.059)	(0.180)	(0.147)	(0.020)	(0.019)
Other Prof.	0.304	0.189	0.128	0.147	0.321	0.217	0.577	0.501	0.627	0.584
		(0.035)	(0.053)	(0.012)	(0.077)	(0.039)	(0.074)	(0.057)	(0.008)	(0.009)
Some College		0.118	0.129	0.051	0.166	0.121	0.269	0.218	0.276	0.236
		(0.021)	(0.027)	(0.006)	(0.047)	(0.025)	(0.043)	(0.035)	(0.005)	(0.006)
R^2	0.12	0.10	0.06	0.06	0.08	0.08	0.13	0.12	0.15	0.14
$\bar{Y}$	11.1	11.4	11.1	11.2	10.9	11.0	10.7	10.7	10.8	10.8
Obs.	1,812	3,299	2,378	37,495	1,178	3,613	1,953	3,060	194,848	156,279

Note: This table shows the point estimates visualized in Figure 9, 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, CPS OCG73, NLSM, NLS72, NLSY79, NLSY97, and the ACS (Ruggles et al., 2024).

Table A-6: 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).