NBER WORKING PAPER SERIES

DEGREES OF MOBILITY:
SHIFTING LOW-INCOME STUDENTS TO HIGHER QUALITY COLLEGES

Elizabeth Burland
Nora Delaney
Susan Dynarski
Katherine B. Leu
CJ Libassi
Katherine Michelmore
Stephanie Owen
Mary A. Quiroga
Elizabeth Salinas

Working Paper 35434
http://www.nber.org/papers/w35434

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2026

We thank collaborators at the University of Michigan Office of Enrollment Management, particularly Kedra Ishop, Steve Lonn, Paul Robinson, and Betsy Brown. We are grateful for research assistance from Zia Kinzy. Stefano DellaVigna, John Friedman, Larry Katz, Rich Murphy, Matt Notowidigdo, Sarah Turner, and Chris Walters provided helpful comments on earlier versions of this work, as did many seminar participants. This study is registered as RCT ID AEARCTR-0001831, with 10.1257/rct.1831-4.0. A pre-analysis plan was filed in April 2017. This research used data structured and maintained by the MERI-Michigan Education Data Center (MEDC). MEDC data is modified for analysis purposes using rules governed by MEDC and are not identical to those data collected and maintained by the Michigan Department of Education (MDE) and/or Michigan's Center for Educational Performance and Information (CEPI). The research was supported by IES grant R305B150012 and the Smith Richardson Foundation. This research does not represent the views of IES or the US Department of Education. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Degrees of Mobility: Shifting Low-Income Students to Higher Quality Colleges
Elizabeth Burland, Nora Delaney, Susan Dynarski, Katherine B. Leu, CJ Libassi, Katherine Michelmore, Stephanie Owen, Mary A. Quiroga, and Elizabeth Salinas
NBER Working Paper No. 35434
July 2026
JEL No. D31, I22, I23, I24, I26, I28

ABSTRACT

Earnings inequality among college graduates has grown, and returns to a bachelor's degree (BA) vary substantially across colleges. We experimentally evaluate a program that shifted high-achieving, low-income students into higher-quality colleges. Students randomly offered a guarantee of four years of tuition and fees at the University of Michigan were 21 percentage points more likely to enroll at a UM campus. The offer shifted students across colleges rather than into college: most compliers would otherwise have attended less-selective four-year colleges. HAIL increased four-year BA completion by 4 percentage points, but had no effect on attainment after six years. It substantially increased degree quality: students offered the scholarship were 12 percentage points more likely to earn a bachelor's degree from a highly or most selective college. IV estimates indicate that students induced to attend a University of Michigan campus were 56 percentage points more likely to earn such a degree. The increase in college quality predicts earnings gains of 18 to 25 percent. Faster degree completion adds about another 1 percent to the present discounted value of lifetime earnings.

Elizabeth Burland
University of Connecticut
eburland@umich.edu

Nora Delaney
Harvard University
ndelaney@fas.harvard.edu

Susan Dynarski
Harvard University
Graduate School of Education
and NBER
susan_dynarski@harvard.edu

Katherine B. Leu
University of Michigan
kleu@umich.edu

CJ Libassi
Columbia University
Teachers College
cjl2232@tc.columbia.edu

Katherine Michelmore
University of Michigan
Gerald R. Ford School of Public Policy
and NBER
kmichelm@umich.edu

Stephanie Owen
Colby College
sowen@colby.edu

Mary A. Quiroga
University of Michigan
mquiroga@umich.edu

Elizabeth Salinas
Harvard University
esalinas@g.harvard.edu

A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/1831/history/

I. Introduction

Over the late 20th century, increasing returns to education drove rising earnings inequality. In the 21st century, by contrast, earnings inequality has primarily grown *within* education groups, especially among those with a bachelor's degree (BA) (Autor, Goldin, and Katz, 2020). Growing evidence suggests that the quality of a college is tied to the economic returns it delivers. Low-income students disproportionately attend open-access and less selective schools, while high-income students are far more likely to enroll in highly selective colleges. In the Ivy League, there are more students from the top one percent of the parental income distribution than there are students from the bottom 50 percent (Chetty et al., 2020). These enrollment patterns raise concerns that postsecondary education in the US may act more as a reinforcer of class rigidities than as an engine of economic mobility.

Michigan makes this national pattern concrete. Its public higher-education system includes a high-mobility flagship, less-selective four-year colleges, and community colleges that enroll many high-achieving, low-income students. Half of students from the bottom quintile of parental income who attend the University of Michigan flagship in Ann Arbor (hereafter, UM flagship) reach the top income quintile as adults, compared with just 10 percent at Grand Rapids Community College, 19 percent at Eastern Michigan University, and 34 percent at Michigan State University.[1]

But while the UM flagship has high upward mobility rates among low-income students, it enrolls few of them: as of 2014, just three percent of its undergraduates were from the bottom quintile of parental income. Three times as many (nine percent) were from the top *one percent* of the income distribution (Chetty et al., 2022).

These observed mobility patterns do not identify the causal effect of attending a more selective college. Since students select colleges based on family resources and academic preparation, simple comparisons confound selection with treatment. Quasi-experimental designs using admissions cutoffs and waitlists identify sizable returns to college quality for students at the margin of admission. Less is known about the broader set of low-income students who could plausibly attend highly selective colleges. We provide unusually clean experimental evidence on the effect of attending a higher-quality college for high-achieving, low-income students.

We fielded a randomized intervention designed to encourage high-achieving, low-income students to apply to and enroll at Michigan's highly selective flagship. The High Achieving Involved Leader (HAIL) Scholarship guaranteed to high school students four years of free tuition and fees at the flagship if they were admitted. Low-income students were pre-qualified using data on subsidized lunch eligibility from state administrative records; no financial aid forms, including the FAFSA, were required. This was not an increase in aid, since the flagship already

[1] The colleges we use throughout the paper as examples are frequently attended by students in the control group.

fully met financial need. Rather, it was a retiming of when the aid offer was made, from after admission to before applications.

HAIL more than doubled applications to the UM flagship and shifted enrollment from community colleges and less-selective four-year schools into the UM system (Dynarski et al., 2021). Because the scholarship did not change aid received conditional on enrollment at UM, random assignment of the offer generates clean variation in college choice for high-achieving, low-income students. We use the offer of the scholarship as an instrument for enrollment at any UM campus within one year of expected high school graduation, examining effects on completion and degree quality.

We estimate intent-to-treat effects of the offer on degree attainment, degree quality, and field of study. The offer changed where students enrolled, not whether they enrolled in college. The HAIL offer accelerated completion, raising four-year BA attainment by 4 percentage points (11 percent), with no effect on BA attainment after six years. Increased full-time enrollment and faster credit accumulation are the mechanisms for shorter time to degree. It also upgraded degree quality: treated students were 12 percentage points (81 percent) more likely to earn a BA from a highly selective institution. The treatment did not affect the share of students earning STEM degrees; treated students are just as likely as the control group to earn a STEM degree.

The students whose choices changed because of HAIL were academically strong and largely oriented toward four-year colleges. In the absence of HAIL, most would have attended four-year colleges, but few would have attended a college as selective as the UM flagship. HAIL therefore shifted compliers into colleges with stronger peers, greater resources, higher rates of full-time enrollment, and higher graduation rates.

Using assignment to treatment as an instrument, we find that attending any UM campus increased four-year BA attainment among compliers by 20 percentage points, relative to a control-complier mean of 42 percent. Enrolling at any UM campus as a freshman increases by 56 percentage points the likelihood of earning a BA from a college as selective as the UM flagship.[2] This is extremely large relative to the control-complier mean of five percent.

Based on this increase in BA quality, we project substantial increases in earnings. As the sample is too young to obtain meaningful estimates at the individual level, we project earnings using college-level data from the College Scorecard (2021). Attending a UM campus increases projected earnings eight years after college entry by 26 percent, or $16,300 relative to a control-complier mean of $62,300.[3] Using earnings data for students from families earning less than $30,000, the projected gain is 31 percent. The overall projection is roughly equivalent to

[2] Barron's classifies the UM flagship as a “highly competitive” university, the second highest category.

[3] We caution that these earnings predictions reflect both the causal effect of attending a particular college as well as non-random selection into higher-quality colleges. Recent work suggests that about 70-80% of the variation in earnings across institutions reflects the causal effect of college quality (Bleemer and Quincy, 2025).

moving a graduate from the 52nd to the 66th percentile of the earnings distribution in the Current Population Survey for those with a BA.

On top of this quality return is an increase in lifetime earnings from an additional year in the labor force resulting from reduced time to degree. Among compliers, attending a UM campus increases the likelihood of graduating within four years by 20 percentage points. For these students, the earlier labor force entry adds 4 to 5 percent to the present discounted value of lifetime earnings, contributing roughly 1 percent to expected earnings across all compliers.[4]

II. Prior Literature

II.A. What is college quality?

"College quality" has no formal definition. Researchers, policymakers, and the media use terms such as "high quality," "highly selective," and "elite" to describe colleges that are distinctive along multiple dimensions. Colleges with higher achieving students, greater instructional resources, lower student-faculty ratios, and more selective admissions tend to have higher graduation rates, faster degree completion, and higher earnings (Black and Smith, 2006). Because these inputs are highly correlated, it is challenging to isolate their separate effects. Throughout the paper, we use "college quality" to refer broadly to this bundle of peer, resource, and institutional characteristics.

II.B. Evidence on the Causal Effect of College Quality

Estimating the effect of college quality is difficult because students sort into colleges based on characteristics that also affect later outcomes. We focus in this review on studies that use quasi-experimental or experimental methods to isolate the causal effect of college quality.[5] Existing studies vary in the students they examine, the quality changes they identify, and the extent to which quality is bundled with supports supplied in the context of a particular intervention. We summarize the existing research in Appendix Table A.1.[6]

A small experimental literature studies interventions that shift students toward higher-quality colleges. Angrist, Autor, and Pallais (2022) evaluate a scholarship that shifts students from

---

[4] See Section VIII.D for these calculations.

[5] We do not review the large literature that relies on observational comparisons or structural models rather than a clearly identified source of exogenous variation. Examples include Light and Strayer (2000) and Arcidiacono et al. (2014). We also do not review the extensive literature on affirmative action and college quality, which addresses a distinct set of questions regarding admissions policy and student sorting across colleges (e.g., Bleemer, 2022).

[6] A separate qualitative literature examines the experiences of low-income students in higher education. It documents challenges meeting basic needs such as food, housing, and transportation, as well as difficulties navigating the norms and expectations of highly selective colleges (Goldrick-Rab, 2016; Jack, 2019; Jack and Black, 2024). While this literature provides insight into why low-income students may face challenges in college, it does not provide causal evidence on how college quality affects their educational outcomes.

community colleges into four-year colleges and find no effect on four-year BA attainment, but an eight-percentage-point increase in six-year attainment among students who planned to attend a four-year college. Barr and Castleman (2025) evaluate an advising intervention that shifts students toward higher-quality colleges and increases four-year degree attainment by six percentage points and six-year attainment by 7.6 percentage points. Cohodes, Ho, and Robles (2026) study a STEM summer program that induces students to enroll in highly selective colleges; they find a large increase in STEM degree attainment, an imprecise five-percentage-point increase in four-year attainment, and no effect on six-year attainment. These studies provide important evidence, but each combines movement to a higher-quality college with additional aid, advising, mentoring, or academic support supplied by the intervention itself.

A larger quasi-experimental literature uses admissions cutoffs to estimate the effect of college quality. Hoekstra (2009) estimates a large effect of attending a flagship on earnings; the paper provides no information about counterfactual colleges, so we cannot gauge the quality shock experienced by students. Zimmerman (2014), Goodman, Hurwitz, and Smith (2017), and Kozakowski (2023) estimate the effect of gaining admission to a four-year public college in settings where the counterfactual is often a community college or no college. They find positive effects on bachelor's degree completion, with point estimates summarized in Appendix Table A.1. Mountjoy (2026) studies admissions cutoffs at public four-year colleges in Texas, where the counterfactual includes both community colleges and other four-year colleges. He estimates increases of roughly one percentage point in four-year BA attainment, two percentage points in six-year BA attainment, and one percent in earnings.

These quasi-experimental designs provide credible causal evidence. However, by design, they focus on students at admissions thresholds. The resulting estimates apply to students who are academically marginal relative to their college peers. As a result, they confound the effect of college quality with the effect of low academic rank within a college.

In a closely-related paper, Cohodes and Goodman (2014) examine high-achieving students in Massachusetts induced by a small tuition discount to alter their college choices. The college landscape of Massachusetts is a mirror image of that in Michigan: the highest quality colleges in Massachusetts are private, while in Michigan the top college is the public flagship. Treated students were induced into public colleges of lower average quality than the private colleges they would have otherwise attended. As a result, their likelihood of graduating college was reduced by 2.5 percentage points.

A related literature focuses on elite private colleges. Chetty et al. (2026) estimate the effect of attending an Ivy-plus college and find effects on earnings and access to elite occupations. Dale and Krueger (2002) compare outcomes for students accepted by similarly selective colleges and conclude that college selectivity has little effect on earnings, though an update using administrative data finds positive earnings effects for Black, Hispanic, and first-generation students (Dale and Krueger, 2014). This evidence speaks to a different margin: movement among highly selective colleges, rather than movement from lower-quality colleges into flagship-quality options.

This literature highlights an important challenge: the college-quality “treatment” varies substantially across contexts. A student moving from a community college to a four-year college experiences a different change in peers, resources, and academic opportunities than a student moving from one elite college to another. Sound extrapolation therefore requires attention to both the students studied and the quality changes induced by treatment. Appendix Table A.1 reports, when available, the population and quality shock in each study. Missing information in the underlying papers makes it difficult to determine whether differences in estimated effects arise from differences in populations, differences in quality changes, or both.

II.C. Contribution

We make three contributions to the literature on college quality.

First, we isolate the consequences of attending a higher-quality college more cleanly than previous experimental studies. The HAIL intervention changed college enrollment but provided no additional financial aid, academic support, or advising once students enrolled. By contrast, the limited experimental evidence on college quality comes from interventions that provide treated students with additional grant aid, advising, mentoring, or other supports beyond those ordinarily provided by the colleges they attend.

Second, we provide an unusually detailed description of the quality shock. Most studies treat college quality as a black box, characterizing treatment as movement between broad categories such as community colleges and four-year colleges or across Barron's selectivity categories. Our administrative data follow students from high school into college, allowing us to measure how HAIL alters peer achievement, academic match, instructional resources, selectivity, and the socio-economic composition of peers. This provides a clearer picture of what changes when students move to a higher-quality college.

Third, we provide experimental evidence on the effect of college quality for academically strong students. Much of the existing literature identifies effects at admissions cutoffs, where students are academically marginal relative to their college peers. The students in our sample score higher than 85 percent of SAT-takers nationwide and have academic records typical of students at the highly selective UM flagship.

III. The Intervention and Randomization

III.A. The HAIL Scholarship

We partnered with the UM flagship to increase its enrollment of low-income students. We jointly designed and implemented a randomized intervention,[7] the HAIL Scholarship. HAIL offered selected students an up-front guarantee of four years of tuition and fees at the UM flagship if they were admitted. The offer conferred no admissions advantage. Students in both treatment and control schools applied through the standard admissions process, which reviews transcripts, test scores, essays, letters of recommendation, and extracurricular activities.

We identified sample students using state administrative records. Public high school students qualified if they were eligible for subsidized school meals and performed above test-score and GPA cutoffs chosen by the UM admissions office.[8] Qualifying SAT scores started at 1100 while qualifying GPAs started at 3.3. Students with higher test scores faced a lower GPA threshold (and vice versa).[9] Because Michigan public schools administer the SAT in school to all eleventh graders, we could identify high-achieving, low-income students who, in other states, may not have even attempted the SAT.[10] The distribution of student achievement in Michigan mirrors the national distribution, with students in 2017 averaging around 1000 on the SAT with a standard deviation of 195 (U.S. Department of Education, National Center for Education Statistics, 2017b).

Applicants from treatment and control schools were admitted to the flagship at similar rates. Forty-six percent of HAIL-eligible applicants were admitted, substantially higher than the overall admission rate of 29 percent (Dynarski et al., 2021). The intervention therefore did not simply induce marginally qualified students to apply. If anything, HAIL-eligible students were more academically qualified than the typical applicant.

The intervention consisted of targeted outreach that communicated the guarantee. Students in the treatment group were mailed a packet describing the HAIL offer and encouraging them to apply. Parents received a follow-up letter with the same information, while high school principals received a list of eligible students and information about the scholarship (see Dynarski et al., 2021 for materials and implementation details). Sample members at control schools received the

---

[7] The randomized trial was approved by the IRB at the University of Michigan (HUM00096289). The present analysis is overseen by the University of Michigan (HUM00224676), Harvard University (IRB21-1221), Colby College (2021-093), and the University of Connecticut (X23-0649).

[8] The family income threshold for subsidized meal eligibility is 185 percent of the federal poverty line.

[9] Michigan switched from the ACT to the SAT in 2016, one year into the study; we convert ACT scores to SAT scores using ACT-SAT concordance tables provided by the College Board.

[10] Hyman (2017) shows universal testing led more disadvantaged students to take the college admissions test, with many appearing in the upper end of the score distribution.

UM flagship's standard outreach materials, which emphasized generous scholarships and high academic standards but made no concrete promise of financial aid.

The HAIL offer differed from typical financial aid in several ways. In the traditional system, students learn their net price only after applying and being admitted to a college. HAIL offered an aid guarantee before college applications were due and required no financial aid forms. In the traditional system, students reapply annually for aid, and aid can change from year to year. HAIL provided a four-year commitment in the fall of senior year of high school. By replacing uncertainty and administrative burden with an early, credible guarantee, the offer changed enrollment choices among high-achieving students from low-income families (Dynarski et al., 2021; Burland et al., 2023).

HAIL did not increase aid, but rather shifted when it was promised. Among those who attended the UM flagship, students in the treatment and control groups received the same amount of grant aid both annually and cumulatively (Appendix Table A.2). The two groups also received the same academic and social services at the flagship, with no special resources devoted to HAIL admits. Thus, HAIL altered college choice but did not increase the aid or services students received conditional on enrolling at UM.

We learned of an additional implementation detail after the publication of our first two studies of the intervention's impacts (Dynarski et al., 2021; Burland et al., 2023): the admissions office made an offer of deferred admission to some treatment-group students.[11] Affected students were promised transfer to the UM flagship after one year at a UM satellite campus (Dearborn or Flint), with the tuition guarantee applying to this pathway.[12]

The satellite campuses have lower peer achievement and graduation rates than the flagship. In principle, the deferred-admission offer could therefore reduce college quality for students diverted from other four-year colleges into a satellite campus.[13] Below, we show that the satellite campuses, while below the flagship on most quality dimensions, are similar to the colleges attended by the control group. We also find that many students entering through the satellite pathway ultimately earn highly competitive degrees, suggesting that the deferred-admission pathway served as a bridge into the flagship rather than a lower-quality alternative.

---

[11] The initial admissions review is "need blind," with scoring done by staff who do not know students' financial circumstances (or HAIL status). In the next stage, admissions decisions are made, with students admitted, rejected, deferred, or waitlisted. It is our understanding that at this stage satellite offers were assigned to marginal applicants.

[12] In the first cohort, 147 treatment students were offered deferred admission to the flagship if they first enrolled at Dearborn; half took the offer. In the second cohort, 214 treatment students were offered deferred admission if they first enrolled at Dearborn or Flint; a third took the offer. Across the two cohorts, students who took the offer mostly enrolled at Dearborn (60 percent vs 40 percent at Flint).

[13] The average SAT score for in-state students is 1360 at the flagship, 1160 at Dearborn, and 1061 at Flint. Six year graduation rates are 92 percent at the flagship, 62 percent at Dearborn, and 56 percent at Flint (College Scorecard, 2021).

III.B. Randomization

Random assignment occurred at the high school level, with all eligible students at a given school assigned the same treatment status. School randomization occurred within four strata defined by the number of HAIL-eligible students in a school. High schools with eligible students in both years of the experiment kept their treatment status in the second year.

We study 3,910 students who graduated from high school in 2016 and 2017: 1,932 treated students (from 289 unique schools) and 1,978 control students (from 299 schools). Table 1 shows the experimental balance on school-level characteristics. The treatment and control groups are balanced on these characteristics. In the control group, the average SAT score is 1254 and the average GPA is 3.82. Fifty-three percent of control-group schools are in rural areas, 40 percent are in Southeast Michigan, and average 93 miles from the University of Michigan. All students are eligible for free or reduced-price lunch; 71 percent qualify for free lunch and 29 percent for reduced-price lunch. The sample is primarilyWhite and non-Hispanic and majority female. Forty-six percent of students sent their ACT scores to the UM flagship when they took the test in their junior year (before randomization).

IV. Data and Methods

IV.A. Data

We obtained longitudinal, student-level administrative data from the Michigan Center for Educational Performance and Information (CEPI) that contain the universe of students attending Michigan public schools. These data served as the base population from which we identified students for the original intervention, and also provide information on pre-treatment characteristics such as gender, race, school geography, and test scores. The ACT and SAT data files include indicators for up to four colleges that each student could choose to send their scores to at the time of test-taking (pre-treatment). The score-send data are incomplete for the second cohort; we therefore report statistics on score sending only for the first cohort.

To track postsecondary enrollment and degree completion, we link to National Student Clearinghouse (NSC) data, which provide college attendance and credential records from virtually all U.S. colleges and universities. NSC data include the field of degree. Our primary educational outcomes are degree attainment, measured within four and six years after expected high school graduation, and the selectivity of the degree-granting college. We categorize degrees as "highly selective" if they are from colleges categorized as highly competitive or most competitive by the 2013 Barron's selectivity index. Detailed information on how we define our outcome measures is included in Appendix B.

To characterize the institutions attended by the students in our sample, we use several sources. We match NSC data to institutional data from College Scorecard and IPEDS. These sources allow us to track instructional spending per student, the proportion of Pell-eligible students, student-faculty ratios, and average graduation rates. By necessity, analyses using these data sources are limited to students who enroll in college. As we show below, there is no treatment-induced selection on the extensive margin of college. We also use the student-level CEPI data on SAT scores matched to NSC enrollment records to calculate the average score of all in-state students who attend each college. Due to universal test-taking and the full coverage of the administrative data, we can do this even for schools that do not require test scores.

We supplement NSC records with transcript data from the Student Transcript and Academic Record Repository (STARR), which includes credits attempted and earned at Michigan public colleges.[14] Because the intervention shifts enrollment toward Michigan public colleges, treatment status affects whether students appear in STARR. We therefore impute missing credits using the approach of Angrist, Autor, and Pallais (2022), predicting credits based on the sector and selectivity of the college attended. As a robustness check, we also calculate Lee (2009) bounds. The two approaches (described in more detail in Online Appendix B.7) yield similar conclusions, though the Lee bounds are wide.

Data on adult earnings are not yet available for our cohorts. We therefore project earnings using publicly available College Scorecard data on median earnings eight years after college entry (2021). We assign students earnings based on the modal college they attend. For students who do not attend college, we use earnings estimates from Opportunity Insights (Chetty et al., 2022). We also calculate projections separately by family income at college entry. All dollar amounts are converted to 2025 values.

IV.B. Empirical Strategy

We estimate the effects of the HAIL Scholarship on initial enrollment, degree attainment, degree selectivity, STEM degree completion, credit accumulation, transfers, and projected earnings. Because treatment was randomly assigned at the high school for each cohort, our analytic unit is the high school-by-cohort cell:

$$(1)\ Y_{jt} = \beta_0 + \beta_1 HAIL_j + S_j + u_{jt}$$

Where Y_{jt} is an outcome of interest for cohort t at high school j. $HAIL_j$ indicates assignment to the treatment group, and S_j is a vector of strata indicators. We cluster standard errors at the high school level. As an alternative method of inference, we also report exact p-values from randomization inference (described in Appendix B.8). β_1 measures the causal effect of being offered the HAIL Scholarship.

[14] We do not reliably observe credits transferred into a college, whether from advanced high school classes or from other colleges.

To estimate the causal effect of college quality, we compute local average treatment effects (LATEs) using instrumental variables (IV). The LATE captures the effect of attending a UM campus for students whose enrollment choices were altered by HAIL. We use the randomized HAIL offer as an instrument for enrollment at any UM campus within one year of expected high school graduation. This definition incorporates students induced into either the flagship or the satellite campuses through the deferred-admission pathway.

We estimate the following with two-stage least squares, with UM_{jt} representing the rate of enrollment at any UM campus for cohort *t* from high school *j* and γ_1 representing the LATE.

$$(2)\ UM_{jt} = \alpha_0 + \alpha_1 HAIL_j + S_j + e_{jt}$$

$$(3)\ Y_{jt} = \gamma_0 + \gamma_1 \hat{UM}_{jt} + S_j + u_{jt}$$

The exclusion restriction requires that HAIL affects downstream outcomes only through changes in the college attended. The LATE estimates capture the effect of UM attendance induced by HAIL. These estimates attribute HAIL's effects on downstream outcomes to compliers induced by the offer into a UM campus.

We also estimate control complier means (CCMs), following Imbens and Rubin (1997). The exclusion restriction, together with absence of defiers, allows us to estimate population shares and outcome means for always-takers (students in the control group who attend UM) and never-takers (treated students who do not attend UM). Using these shares and means, we back out averages for control compliers. We use the same method to compare always-takers, never-takers, and compliers in Section VII. Appendix B.9 explains the procedure in more detail.

V. The Landscape of College Quality in Michigan

Michigan has no Ivy League college, but the state has substantial variation in college quality. The UM flagship is the best-resourced college in the state and the only large school classified as highly competitive by Barron's. Michigan State leads a second tier of selective public universities. A large set of broad-access colleges fills the rest of the distribution, including Eastern Michigan, Grand Valley State, regional four-year colleges, and 28 community colleges. Low-income high achievers in the HAIL sample are more likely to be in the lower part of this distribution. Upper-income students with the same academic credentials are concentrated nearer the top.

We compare the HAIL control group to students who have the same academic credentials but are ineligible for subsidized lunch ("upper-income"). Low-income and upper-income high achievers sort into different parts of the college-quality distribution (Figure 1). HAIL students attend colleges where their classmates average 1148 on the SAT, compared to 1182 for upper-income students. Instructional spending per student averages $16,388 at colleges attended by HAIL students compared to $18,062 for upper-income students. Faculty to student ratios are similar for the two groups. HAIL students go to colleges where 78 percent of students enroll full time, compared to 84 percent for upper-income students. There are also large differences in the likelihood of graduation. At colleges attended by HAIL controls, 54 percent of students graduate within four years compared to 62 percent for upper-income students (60 and 69 percent graduate within six years, respectively).

These are not small differences. Students who satisfy the same high-school achievement criteria enter colleges with meaningfully different peers, resources, and enrollment environments. Sorting across colleges by family income compounds the advantages that upper-income students carry into higher education.

The UM flagship anchors the top of this distribution and defines the quality margin in the paper. Enrolled students average 1360 on the SAT. Instructional spending exceeds $30,000 per student. Only 3 percent attend part time. Median parental income exceeds $200,000. Gaining admission to the flagship means entering a very different educational environment for low-income, high achievers.

Values for the satellite campuses locate the deferred-admission pathway in this quality landscape. UM-Dearborn and UM-Flint lie well below the UM flagship but are near the middle of the distribution attended by HAIL controls. Treatment-group students steered toward the satellites through deferred admission were therefore not sent into unusually weak colleges but rather into colleges similar to those attended by the control group. This supports the monotonicity assumption underpinning the IV estimates in the paper.

VI. Intent-to-Treat Effects

VI.A. Enrollment at a University of Michigan Campus

HAIL shifted where students enrolled, not whether they enrolled in college. Overall college enrollment was 90 percent in both the treatment and control groups (Figure 2A). This differs from Dynarski et al. (2021), in which the HAIL offer was estimated to increase overall college attendance by 3.8 percentage points. Using updated NSC records, we no longer find an effect on college attendance.[15] HAIL increased enrollment at any UM campus within one year of expected high school graduation by 21 percentage points (standard error 2.1 pp, F-statistic 100). This first stage combines effects on both flagship and satellite enrollment.

[15] The state of Michigan receives data from National Student Clearinghouse each fall which it uses to update its longitudinal college attendance panel. Dynarski et al. (2021) used the first pull of data available for sample students. We later learned that NSC updates its data when colleges report an error in their previous submissions. Five percent of Michigan high school graduates had a correction to their NSC enrollment record for the year after high school. The schools making corrections are more likely to be open-access colleges. In the HAIL sample, the records of 260 students changed.

The HAIL offer increased enrollment at the flagship as a freshman by 15 percentage points (Figure 2B). This is the enrollment effect we reported in Dynarski et al. (2021), published before we learned of the deferred-admission offer. The deferred-admission pathway increased freshman enrollment at UM-Dearborn or UM-Flint in our sample by 6 percentage points. Including this response raises the estimated effect on UM system enrollment from 15 to 21 percentage points.

Half of those induced into the satellite campuses subsequently transferred to the flagship in their sophomore year (+2.9 pp, Appendix Table A.3). Transfers from the satellites to the flagship are essentially absent in the control group (0.1 percent in year two), indicating that the deferred-admission pathway expanded access to the UM flagship.

The treatment-control contrast sheds light on the college choices that students would have made in the absence of the HAIL offer. Of the 21 percentage-point increase in UM enrollment, 4 percentage points came from students who would otherwise have attended community colleges. The other 17 percentage points came from students who would otherwise have attended four-year colleges less selective than the UM flagship.[16] Later we show that these enrollment shifts generated large changes in peer achievement, institutional resources, and degree quality. These estimates also support the monotonicity assumption that underlies the IV analysis.

VI.B. Time to Degree and BA Selectivity

The HAIL offer accelerated degree completion and increased degree quality. It had no effect on eventual bachelor's degree attainment or STEM completion.

The offer increased four-year BA completion by 4.1 percentage points (standard error 2.2 pp), relative to a control mean of 39 percent (Table 2). By six years after expected high school graduation, however, BA attainment is identical in the treatment and control groups at 63 percent. HAIL therefore changed the timing of degree completion rather than the likelihood of ultimately earning a bachelor's degree. This pattern is consistent with the enrollment results: HAIL shifted college choice among students already very likely to enroll in college. We return to these results in the Discussion.

The effect on degree quality, by contrast, is large and persistent. Within six years, treated students are 11.7 percentage points more likely to earn a BA from a highly selective or most selective college. Relative to a control mean of 14.4 percent, the offer increases by 81 percent the likelihood of earning such a selective degree. Within the treatment group, 43 percent of bachelor's degrees are earned from highly selective colleges, compared with 24 percent in the control group.

[16] Greater detail on the enrollment effects of HAIL is available in Dynarski et al. (2021) and Burland et al. (2023).

One concern is that students induced into more competitive academic environments may avoid demanding, high-return fields of study (Arcidiacono et al., 2016). We find no evidence of this pattern. The HAIL offer has no effect on the likelihood of earning a STEM degree (Table 2). Roughly one quarter of students in both groups earn a STEM BA. This finding is unchanged under alternative STEM definitions, including classifications that incorporate business and economics (Appendix Figure B.1).

The four-year completion estimate is imprecise. As we show in the next subsection, transcript records corroborate the attainment results.

VI.C. Faster Credit Accumulation

Nearly all students at the UM flagship enroll full time. Only 3 percent attend part time, compared with 9 percent at Michigan State University and 68 percent at Grand Rapids Community College (College Scorecard, 2016). These differences in enrollment intensity suggest a mechanism for shortened time-to-degree: full-time students tend to accumulate credits faster than part-time students.

Students in the treatment group accumulate credits at four-year colleges more quickly than those in the control group (Figure 3). This difference emerges in the summer after high school, with the treatment group earning 0.34 more four-year credits than the control group (standard error 0.09). The UM flagship has a pre-college summer program for students from disadvantaged backgrounds, which likely explains this result. The credit advantage grows to 1.3 after the freshman year and 1.98 after the sophomore year, then levels off at about 2.5 credits. The estimates for the first two spells are statistically significant at conventional levels; the confidence bands include zero for the later years.

These differences in credit accumulation may seem small, but they correspond to substantial differences in the share of students who are on track to earn a degree within four years. The typical BA in the United States requires 120 credits. Students therefore need to enroll in 30 credits per year to graduate on time. By the end of the first year of college, treated students are five percentage points more likely to have earned 30 credits at a four-year school (relative to a control mean of 31 percent). Over the first four years of college, treated students are three to five percentage points more likely to reach the 30-credit benchmark. This is in line with the estimated effect on four-year BA completion (4.1 pp, Table 2), pointing to faster credit accumulation as a central mechanism for the shift toward on-time graduation. This pattern aligns with evidence that early credit accumulation strongly predicts degree completion (Angrist et al., 2022).

Our credit information is from the STARR data, which covers only students in Michigan's public colleges. Treatment status affects inclusion in STARR: 73 percent of the control group have transcripts in the first year after high school, compared with 77 percent of the treatment group (Appendix Table B.1). These marginal STARR students would otherwise have attended a private college or a college outside Michigan, but HAIL induced them to attend a Michigan public college.

This creates a risk of selection bias in the credit estimates since presence in STARR depends on an outcome downstream of random assignment. As described in the data section, we impute credits for these students for Figure 3. As a robustness check, we estimate effects in the unimputed sample and calculate Lee bounds (Appendix B.7). The Lee bounds are wide but consistent with the imputed-credit estimates (Appendix Figure B.3).

VI.D. Reduced Attendance at Community Colleges and AA Degree Attainment

Community college tuition is well below that at four-year colleges (College Scorecard, 2016). The community-college-to-BA pathway is often promoted as a lower-cost route to a bachelor's degree. The largest state aid program for low-income students in Michigan (the Tuition Incentive Program, or TIP) paid full tuition only for community college (Burland et al., 2025).

Evidence suggests that this pathway can slow progress through college and lead to credit loss during transfer (Miller, 2025). In the United States, just 18 percent of those starting at a community college have earned an associate degree (AA) after six years (U.S. Department of Education, National Center for Education Statistics, 2017a). Even fewer (13 percent) earn a BA (U.S. Department of Education, National Center for Education Statistics, 2017a).

As shown earlier, the HAIL offer reduced the share of students starting at a community college by four percentage points (Figure 2). Subsequently, HAIL reduced transfers from community colleges into four-year colleges by 1.4 percentage points (Appendix Table A.3). This pattern suggests that some students induced into the UM system would otherwise have started at a community college and then transferred to a four-year college.[17]

Consistent with this enrollment shift, HAIL reduced AA degree attainment by six percentage points relative to a control mean of 15 percent (Figure 4A). The decline reflects reductions in both the probability of earning only an AA and the probability of earning both an AA and BA, each by three percentage points (Figure 4B).

At the same time, the offer increased the share of students earning only a bachelor's degree by two percentage points (Figure 4B). Combined with the earlier evidence on credit accumulation and four-year completion, these patterns suggest that HAIL diverted students away from the community-college-BA pathway and toward direct enrollment in four-year colleges.

[17] We also observe a small increase in transfers away from the flagship. In the second year after high school, transfers from the flagship to colleges outside the UM system increased by one percentage point. Relative to the 15 percentage-point increase in first-year flagship enrollment, this implies that roughly one in fifteen students induced into the flagship subsequently transferred elsewhere.

The decrease in AAs, combined with no change in BAs, has ambiguous welfare implications because we cannot observe which counterfactual pathway each affected student would have followed. Some students diverted from community college to the UM system may otherwise have earned an AA and then gone on to earn a less-selective BA. For these students, earlier enrollment in a four-year college in the UM system saves time and increases BA selectivity, predicting higher lifetime earnings.

Other students diverted from community colleges to the UM system may otherwise have earned an AA but now end up with some four-year credits and no BA. For these students, the welfare implications depend on the return to an AA compared with the return to four-year credits without a degree. We cannot assess these welfare implications until actual earnings are observed.

VI.E Summary of Intent-to-Treat Effects

Taken together, the intent-to-treat results indicate that HAIL substantially altered students' educational trajectories. The offer shifted students away from community colleges and less-selective four-year institutions, accelerated progress toward a bachelor's degree, increased the selectivity of the degrees they earned, and changed their pathways through higher education. We next ask whose choices changed and how large a quality shock HAIL induced.

VII. Whose College Choices Does a Tuition Guarantee Change?

The local average treatment effects we report later in the paper are identified by compliers, the students whose college choices were altered by the HAIL Scholarship. This section profiles that population, and compares them to the students who would have attended UM regardless of HAIL (always takers) and those who do not attend even when offered the scholarship (never takers).

Compliers differ from always-takers less in academic preparation than in college search and enrollment. They have an average SAT score of 1267, compared with 1288 among always-takers and 1246 among never-takers, and an average GPA of 3.84, compared with 3.89 and 3.81, respectively (Table 3).

Score-send data provide a window into the colleges students viewed as plausible options before receiving the HAIL offer. This logic parallels that of Dale and Krueger (2002, 2014), who use application behavior to capture dimensions of student ability and aspiration that are difficult to observe directly.

Before treatment, compliers actively engaged in the college search process. More than half sent scores to the University of Michigan flagship, compared with 36 percent of never-takers and 79 percent of always-takers. Similarly, 65 percent sent scores to a highly or most competitive college, a category that includes the UM flagship, compared with 49 percent of never-takers and 84 percent of always-takers. The average admit rate of colleges receiving score sends is 59 percent for compliers, compared with 66 percent for never-takers and 50 percent for always-takers. Across these measures, compliers fall between the two comparison groups.

Two features of the score-send patterns stand out. First, compliers showed substantial interest in selective colleges: 71 percent sent scores to very competitive colleges and 65 percent sent scores to highly or most competitive colleges. Second, their consideration sets were broader and less centered on the UM flagship than those of always-takers: they were less likely than always-takers to send scores to the UM flagship and sent scores to colleges with higher admit rates. These patterns suggest that compliers considered selective colleges, but with less certainty than always-takers about which selective colleges were within reach.

Geography helps explain whose choices HAIL changed. HAIL had the least effect on those who lived closest and furthest from the flagship. Compliers are more likely than always-takers to attend rural high schools, more likely to attend high schools outside Southeast Michigan, and more likely to live farther from the UM flagship in Ann Arbor. Fifty-eight percent attend rural schools, compared with 32 percent of always-takers. The average distance from their high school to Ann Arbor is 82 miles, compared with 64 miles among always-takers. These students therefore come disproportionately from parts of the state where the UM flagship may be less salient. Compared with compliers, never-takers are similarly rural (57 percent) and even farther from Ann Arbor (106 miles). Research in other settings shows that distance to campus can shape college access (Acton et al., 2025).

The demographic composition of the complier population largely reflects these geographic patterns. Compliers are more likely than always-takers to be White and less likely to be Black or Hispanic. In Michigan, race and geography are closely linked. Black and Hispanic students are concentrated in urban Southeast Michigan, while the rural regions that contribute many compliers are overwhelmingly White.[18]

The complier analysis identifies a population that is academically strong, actively engaged in the college search process, and largely oriented toward four-year colleges. Yet absent HAIL, they rarely enroll in colleges as selective as the UM flagship. HAIL changes the choices of students who were well prepared for highly competitive colleges and considered selective options, but who would have enrolled in less-selective colleges without the offer.

[18] In the control group, Black urban students in Southeast Michigan have considerably higher rates of application to and enrollment in the UM flagship than do White rural students. As shown in Dynarski et al (2021), HAIL reduced heterogeneity by region in these outcomes.

VIII Local Average Treatment Effects

VIII.A. The Quality Shock for Compliers

HAIL induced students to attend colleges with substantially stronger peers and greater resources. We measure this quality shock in Table 4 by estimating treatment effects on the characteristics of colleges attended. To capture transfers, including those induced by deferred admission, we average college characteristics over the first two years after high school. We report both ITT and LATE effects, focusing on LATEs to highlight the quality shock experienced by compliers.

Compliers experienced a 187-point increase in peer achievement, shifting from colleges whose students averaged 1128 on the SAT to colleges whose enrolled students averaged over 1300. This change came with large changes in institutional resources, peer characteristics, and graduation rates. Full-time enrollment rises from 76 percent to 90 percent. Instructional spending per student nearly doubles (a gain of $13,000) while endowment per student increases seven-fold, rising $236,000 from a base of $39,000. Faculty resources also increase, with instructional faculty per 100 students rising from 6.5 to 8.

Peer composition changes markedly. For students induced to attend UM, median parental income rises from $133,573 to $196,527. The share of students with a college-educated parent increases from 70 percent to 78 percent, and the share not receiving a Pell Grant rises from 68 percent to 80 percent. The rejection rate at attended colleges increases from 23 percent to 67 percent.

Large differences also appear in institutional graduation rates. The average six-year graduation rate for Pell recipients rises by 32 percentage points, from 52 percent at the colleges attended by control compliers to 84 percent at colleges attended by treated compliers. This explains our null effects on overall degree attainment: control compliers complete college at rates (77 percent, Table 2) far above the Pell graduation rates of the colleges they would otherwise have attended.

Taken together, changes to enrollment (Figure 2) and institutional characteristics (Table 4) show that HAIL induces large changes in the colleges attended by compliers. Under treatment, compliers attend colleges with academically stronger peers, more resources, higher graduation rates, and more advantaged student bodies. The LATE estimates below therefore capture the consequences of a substantial increase in college quality.

VIII.B. Educational Attainment and Degree Quality

We now estimate the consequences of these quality changes for educational attainment and projected earnings. We estimate these effects using the IV strategy described in Section IV.B, instrumenting enrollment at any UM campus within one year of expected high school graduation with assignment to the HAIL offer.

Attending a UM campus as a freshman increases the likelihood of earning a BA within four years by 20 percentage points (standard error 9.9 percentage points, Table 2). This represents a large increase over the control complier mean of 42 percent. We find no evidence that these gains translate into higher rates of six-year degree attainment.

A longer time spent attaining a given level of education is costly to both students and society. Tuition is charged by the year and not by the degree. The opportunity cost, in the form of forgone wages, accrues annually. And the later in life a degree is granted, the fewer years there are for it to pay off in the labor force. Researchers have therefore focused intently on the determinants of time-to-degree (e.g. Bound, Lovenheim, and Turner 2012). Our results suggest that time-to-degree is heavily influenced by the quality of the college that a student attends.

The effect on degree quality is large. Students induced by HAIL to attend a UM campus are 56.4 percentage points more likely to earn a highly selective BA. The standard error is 6.2 percentage points. Relative to the control complier mean of 4.9 percent, this represents an eleven-fold increase in the likelihood of earning a highly selective or most selective degree.

As in the ITT estimates, HAIL changed degree timing and quality, not eventual BA attainment. In Appendix Table A.4, we further show no effect on graduate school enrollment or attainment.

VIII.C. The Satellite Pathway

To assess the satellite pathway, we construct joint enrollment-attainment outcomes that combine students' first UM campus with the degree they ultimately earn.[19] These outcomes, reported in the middle panel of Table 2, show whether students who begin at a UM satellite later earn any BA and whether they ultimately earn a BA from a highly competitive institution. The joint outcomes suggest that the satellite pathway was not simply a diversion into lower-quality colleges without later access to selective degrees. Among treated compliers, 19 percent start at a UM satellite and earn a BA within six years; 11 percent start at a satellite and ultimately earn a highly competitive BA. Thus, a substantial share of students entering through the satellite pathway ultimately complete high-selectivity degrees.

VIII.D Projected Earnings Effects

Because our cohorts are still young, we do not yet observe their earnings. We therefore project earnings using institution-level earnings data from College Scorecard (2021). Eight years after college entry, students who attended the UM flagship earn an average of $86,000, compared with $69,000 at Michigan State University and $38,000 at Michigan's community colleges.

[19] Joint enrollment-attainment outcomes are unconditional on initial enrollment. They are set to one when both of the underlying outcomes are equal to one.

The ITT effect on projected earnings is $3,400 (last panel of Table 2). Attending any UM campus increases compliers' projected earnings by $16,300 (standard error $4,000). This is 26 percent of the control complier mean of $62,300. In national data, this difference is equivalent to moving from the 52nd to the 66th percentile of the earnings distribution among those with a BA.[20]

These projections are based on average earnings at each college. HAIL students, however, come from lower-income families than the typical college student. We therefore also project earnings using data on students from families with incomes below $30,000. These projections imply larger effects. The ITT effect is $3,600, and attending any UM campus increases projected earnings of compliers by $17,100 (standard error $3,500). This is 31 percent of the control complier mean of $55,402. In national data, this difference is equivalent to moving from the 45th to the 61st percentile of the earnings distribution among those with a BA.[21]

These projected earnings estimates should be interpreted with caution. Earnings differences across colleges reflect both college quality and non-random sorting of students into colleges. Recent estimates suggest that roughly 70 to 80 percent of the variation in institutional earnings can be attributed to the causal effects of college quality (Bleemer and Quincy, 2025). To the extent that these estimates apply in our setting, they imply a causal, annual earnings effect of 18 to 25 percent.

The reduced time to degree induced by the increase in college quality also boosts projected earnings. An additional year in the labor force at the very start of a worker's career is particularly valuable, adding 4-5 percent to the present discounted value of lifetime earnings at age 21.[22] Since attending a UM campus increases the likelihood of graduating within four years by 20 percentage points, this implies that earlier graduation contributes roughly 1 percent to expected earnings across all compliers.

VIII.E. Plausibility of the Exclusion Restriction

The exclusion restriction requires that, among students who enroll at UM, HAIL affects attainment and earnings only through the UM enrollment decision, not through HAIL-specific differences in aid, services, or supports after enrollment. If HAIL changes outcomes for

[20] These calculated percentiles are based on the earnings of those aged 28-32 with at least a bachelor's degree in the March Current Population Survey in the years 2014 through 2017. All values are converted to 2025$.
[21] The corresponding estimate based on earnings of students from families with incomes above $75,000 is a 22 percent increase relative to the complier mean (Appendix Table A.5). This pattern suggests that the payoff to attending a more selective college may be larger for low-income students than for their higher-income peers.
[22] This calculation assumes a discount rate of 3 percent, a career that ends at 67 and starts at either 21 or 22, and no real increase in annual earnings. If real earnings are instead allowed to rise by 2 percent a year, an extra year is worth 5 percent of lifetime discounted earnings (since the annual increase in earnings also starts a year earlier). Since attending a UM campus increases four-year graduation by 20 percentage points, the expected gain across all compliers is approximately 0.20 times 4 to 5 percent, or about 1 percent.

always-takers after they enroll at UM, the LATE estimates will attribute their response to compliers and therefore overstate the causal effect of college quality. The ITT estimates do not rely on the exclusion restriction and remain valid as average effects of the HAIL offer.

A natural concern is that treated students who enrolled at UM received more financial aid than comparable control students who enrolled at UM. They did not. Treated and control students who enrolled at the flagship received nearly identical grant aid in both the first year and over four years (Appendix Table A.2). HAIL students also received no special academic, advising, or social support after enrollment; they had access to the same programs and services as other UM students. The intervention changed when students learned about aid, not how much aid they received, and did not alter the services available to them after enrollment.

The main remaining concern is certainty about college costs. Always-takers in the treatment group received an up-front, four-year guarantee of tuition and fees, while always-takers in the control group learned about comparable aid through the standard admissions and financial-aid process and had to reapply every year. We argue in Burland et al. (2023) that this certainty, rather than greater generosity of aid, changed application and enrollment decisions.

The exclusion restriction assumes that any effect of certainty ends once students enroll. This assumption would hold if students learned about the generosity of UM financial aid either through HAIL or through the standard aid process. The flagship meets the financial need of all admitted students, and aid is not conditioned on academic performance. A low-income student who enrolls and begins interacting with the financial aid office may therefore develop the same understanding of aid generosity regardless of HAIL status.

Alternatively, cost certainty may affect student behavior after enrollment. Students who receive a four-year guarantee may experience less financial stress, work fewer hours while enrolled, or focus more intensely on completing a degree on time. The guarantee may also encourage students to think in terms of a four-year completion horizon.

We think these channels are unlikely to explain the main results. Students who remain enrolled beyond four years are eligible for financial aid that is as generous as the HAIL offer. The guarantee therefore does not create a sharp financial incentive to graduate within four years.

We cannot separately identify effects for compliers and always-takers. If always-takers respond positively to the HAIL guarantee after enrollment, the LATE estimates should be interpreted as upper bounds on the effect of college quality for compliers.

IX. Discussion

In earlier work, we predicted that HAIL would increase bachelor's degree attainment by roughly seven percentage points (Table 8 of Dynarski et al. 2021). Instead, we find large effects on four-year completion and degree selectivity, but no effect on bachelor's degree attainment after six years. This section explains why.

Our earlier prediction was not an impact estimate from HAIL. It used prior cohorts of high-achieving, low-income Michigan students to project how enrollment at more selective colleges might affect completion. The updated results show why that projection was too large: HAIL compliers are a selected subset of an already selected population. They were high-achieving, applied to UM, were admitted, and changed their enrollment decision in response to the offer.

Two new findings help explain why we overpredicted the effect of HAIL on bachelor's degree attainment. First, in Dynarski et al. (2021), HAIL appeared to increase overall college enrollment by 3.8 percentage points; using updated NSC records, we no longer find an effect.[23] Given that more than three-quarters of enrollment compliers earn BAs even absent treatment, this revision alone implies that the earlier data underpredicted BA completion in the control group by roughly three percentage points.

Second, the complier analysis reveals that the students whose enrollment decisions changed because of HAIL completed degrees at much higher rates than we would have predicted based on the institutional graduation rates of their counterfactual schools. Within six years of entering college, 77 percent of control compliers earn a bachelor's degree (Table 2). Yet the colleges they attended had an average Pell-recipient graduation rate of just 52 percent (Table 4). This suggests that the students induced to enroll at a UM campus were highly likely to complete degrees, even in the absence of treatment. This likely reflects both their strong academic preparation and unobserved strengths that matter for college success, including characteristics observed by UM admissions officers.

Peer-achievement data help clarify why this result differs from a simple low-income-student narrative. Because Michigan universally administers the SAT in public high schools, we can calculate peer achievement even at open-access colleges. Figure 5A shows that low-income students statewide typically attend colleges where their SAT scores fall below the peer mean. The HAIL sample is different. These students are low-income but high-achieving. Figure 5B shows that 83 percent of control-group HAIL students scored above the average SAT score of their college peers, compared with 75 percent of treatment-group students. HAIL shifted students into colleges with stronger peers, but it did not push them into academic overmatch. It reduced undermatch while leaving most students academically well prepared relative to their peers.

[23] See section VI.A for a detailed explanation.

For academically strong students, attending a higher-quality college may change the timing and quality of the degree rather than whether they complete a BA. This distinction matters for how we evaluate college quality: degree completion alone misses shifts in time-to-degree, degree selectivity, and projected earnings.

The credit and pathway results point to the mechanism: HAIL moved students away from slower routes through community colleges and less-selective institutions and into colleges where full-time enrollment and faster credit accumulation are more common.

This pattern helps reconcile our estimates with the broader literature. Studies that move students from no college or community college into four-year colleges often find gains in bachelor's degree completion. By contrast, HAIL compliers were already highly likely to attend and complete college. Our results are closer to studies that examine movement among four-year colleges or among highly prepared students: the gains appear less in eventual degree completion than in the quality, timing, and projected labor-market value of the degree earned. This comparison underscores why the population and quality shock matter when interpreting estimates of college quality.

X. Conclusion

We analyze a randomized intervention that shifted high-achieving, low-income students into higher-quality colleges. HAIL gave students a simple, early, credible guarantee of affordability before they applied. By moving information about aid to the front of the college-choice process, the intervention changed where students enrolled. Because HAIL left post-enrollment aid and services unchanged, the experiment provides unusually clean evidence on the consequences of attending a higher-quality college.

The students induced to enroll at a UM campus were already likely to attend college and complete a BA. HAIL therefore changed the quality and timing of the degree they earned, rather than whether they earned a degree. The intervention increased four-year BA completion, sharply increased the likelihood of earning a BA from a highly or most selective college, and raised projected earnings. The college quality premium implies causal gains of 18 to 25 percent; earlier graduation adds roughly 1 percent more. The intervention shifted high-achieving, low-income students toward the types of degrees typically earned by their upper-income counterparts (Figure 6).

The results also speak to concerns about academic mismatch. The students induced to enroll at a UM campus were not marginal admits. They were academically strong, scored above the average student at the colleges they would otherwise have attended, and resembled students who enrolled at the UM flagship. HAIL reduced undermatch while moving students into substantially higher-quality colleges. This pattern aligns with evidence that academic match and college quality can improve outcomes for well-prepared students (Dillon and Smith, 2020; Bleemer, 2022).

The lesson has limits. Better information, simpler applications, and clearer aid promises can move students toward higher-quality colleges. But they cannot create more high-quality seats. Abdulkadiroğlu, Pathak, and Walters (2026) make a similar point in K12 school choice: allocation policy can improve who gets access to strong schools, but it works within a fixed supply of those schools. HAIL reduced undermatch by reallocating access to high-quality colleges. Mobility at scale requires expanding that supply.

The constraint matters especially in higher education. Many elite colleges have responded to rising demand by becoming more selective rather than by adding seats (Blair and Smetters, 2021). UM Ann Arbor is a counterexample: its entering first-year class has grown by roughly one-third since HAIL began.[24] Mobility at scale requires more of this kind of capacity growth: not just better access to high-quality colleges, but more seats at institutions that reliably generate degrees, earnings, and upward mobility.

[24] UM Ann Arbor's first-year class grew from 6,071 students in fall 2015 to 8,178 in 2025 (University of Michigan Office of the Registrar, n.d.; University Record, 2025).

References

Abdulkadiroglu, A., Pathak, P. A., & Walters, C. R. (2026). *Who Gets What in Education: Can School Matching Improve Student Achievement?* (No. w34936). National Bureau of Economic Research.

Acton, R., Cortes, K. E., Miller, L., & Morales, C. (2025). Distance to degrees: How college proximity shapes students' enrollment choices and attainment across race-ethnicity and socioeconomic status. *Economics of Education Review*, 102724. https://doi.org/10.1016/j.econedurev.2025.102724

Angrist, J., Autor, D., & Pallais, A. (2022). Marginal effects of merit aid for low-income students. *The Quarterly Journal of Economics*, *137*(2), 1039–1090. https://doi.org/10.1093/qje/qjab050

Arcidiacono, P., Aucejo, E., Coate, P., & Hotz, V. J. (2014). Affirmative action and university fit: Evidence from Proposition 209. *IZA Journal of Labor Economics*, *3*(1), 7. https://doi.org/10.1186/2193-8997-3-7

Arcidiacono, P., Aucejo, E. M., & Hotz, V. J. (2016). University differences in the graduation of minorities in STEM fields: Evidence from California. *American Economic Review*, *106*(3), 525-562. https://doi.org/10.1257/aer.20130626

Autor, D., Goldin, C., & Katz, L. F. (2020). Extending the race between education and technology. *AEA Papers and Proceedings*, 110, 347-351. https://doi.org/10.1257/pandp.20201061

Barr, A., & Castleman, B. (2025). Increasing degree attainment among low-income students: The role of intensive advising and college quality. *American Economic Review*, *115*(11), 4075–4103. https://doi.org/10.1257/aer.20240669

Black, D. A., & Smith, J. A. (2006). Estimating the Returns to College Quality with Multiple Proxies for Quality. *Journal of Labor Economics*, *24*(3), 701–728. https://doi.org/10.1086/505067

Blair, P. Q., & Smetters, K. (2021). *Why Don't Elite Colleges Expand Supply?* (No. w29309). National Bureau of Economic Research.

Bleemer, Z. (2022). Affirmative action, mismatch, and economic mobility after California's Proposition 209. *The Quarterly Journal of Economics*, *137*(1), 115–160. https://doi.org/10.1093/qje/qjab027

Bleemer, Z., & Quincy, S. (2025). *Changes in the college mobility pipeline since 1900* (No. w33797). National Bureau of Economic Research.

Bound, J., Lovenheim, M. F., & Turner, S. (2012). Increasing time to baccalaureate degree in the United States. *Education Finance and Policy*, *7*(4), 375–424. https://doi.org/10.1162/EDFP_a_00074

Burland, E., Dynarski, S., Michelmore, K., Owen, S., & Raghuraman, S. (2023). The power of certainty: Experimental evidence on the effective design of free tuition programs.

American Economic Review: Insights, *5*(3), 293–310. https://doi.org/10.1257/aeri.20220094
Burland, E., Camo-Biogradlija, J., Ferrara-Gerson, K., Fields, X., Michelmore, K., Sotherland, N., Stange, K., Thompson, M., and Tompkins-Stange, M. (2025). Bureaucracy and burden: Understanding take-up of a need-based financial aid program. (EdWorkingPaper: 25-1283). Annenberg Institute at Brown University. https://doi.org/10.26300/0ezs-5g46
Chetty, R., Deming, D. J., & Friedman, J. N. (2026). Diversifying society's leaders? The determinants and causal effects of admission to highly selective private colleges. *The Quarterly Journal of Economics*, 141(1), 51-145. https://doi.org/10.1093/qje/qjaf050
Chetty, R., Friedman, J., Saez, E., Turner, N., & Yagan, D. (2020). Income segregation and intergenerational mobility across colleges in the United States. *The Quarterly Journal of Economics,* 135 (3), 1567–1633. https://doi.org/10.1093/qje/qjaa005
Chetty, R., Friedman, J., Saez, E., Turner, N., & Yagan, D. (2022). *Replication data for: Income segregation and intergenerational mobility across colleges in the United States* (Version 1.0) [Dataset]. Harvard Dataverse. https://doi.org/10.7910/DVN/RYLJKZ
Cohodes, S. R., & Goodman, J. S. (2014). Merit aid, college quality, and college completion: Massachusetts' Adams scholarship as an in-kind subsidy. *American Economic Journal: Applied Economics*, *6*(4), 251–285. https://doi.org/10.1257/app.6.4.251
Cohodes, S. R., Ho, H., & Robles, S. C. (2026). Diversifying the STEM Pipeline: Evidence from STEM Summer Programs for Underrepresented Youth. *Journal of Human Resources*. https://doi.org/10.3368/jhr.1024-13934R2
College Scorecard. (2016). *College Scorecard Institution-Level Data* [Dataset]. https://collegescorecard.ed.gov/data/ (accessed December 17, 2025)
College Scorecard. (2021). *College Scorecard Institution-Level Data* [Dataset]. https://collegescorecard.ed.gov/data/ (accessed December 17, 2025)
Dale, S. B., & Krueger, A. B. (2002). Estimating the payoff to attending a more selective college: An application of selection on observables and unobservables. *The Quarterly Journal of Economics*, *117*(4), 1491-1527. https://doi.org/10.1162/003355302320935089
Dale, S. B. & Krueger, A. B. (2014). Estimating the effects of college characteristics over the career using administrative data. *Journal of Human Resources, 49*(2), 323-358. https://doi.org/10.3368/jhr.49.2.323
Dillon, E. W., & Smith, J. A. (2020). The Consequences of academic match between students and colleges. *Journal of Human Resources*, *55*(3), 767–808. https://doi.org/10.3368/jhr.55.3.0818-9702R1
Dynarski, S., Libassi, C., Michelmore, K., & Owen, S. (2021). Closing the gap: The effect of reducing complexity and uncertainty in college pricing on the choices of low-income students. *American Economic Review*, *111*(6), 1721–1756. https://doi.org/10.1257/aer.20200451
Goldrick-Rab, S. (2016). *Paying the price: College costs, financial aid, and the betrayal of the American dream* (D. M. Anderson & P. Kinsley, Eds.). The University of Chicago Press.

Goodman, J., Hurwitz, M., & Smith, J. (2017). Access to 4-Year public colleges and degree completion. *Journal of Labor Economics*, *35*(3), 829–867. https://doi.org/10.1086/690818

Hoekstra, M. (2009). The effect of attending the flagship state university on earnings: A discontinuity-based approach. *Review of Economics and Statistics*, *91*(4), 717–724. https://doi.org/10.1162/rest.91.4.717

Hyman, J., 2017. ACT for all: The effect of mandatory college entrance exams on postsecondary attainment and choice. *Education Finance and Policy,* 12(3), pp.281-311. https://doi.org/10.1162/EDFP_a_00206

Imbens, G. W., & Rubin, D. B. (1997). Estimating outcome distributions for compliers in instrumental variables models. *The Review of Economic Studies*, *64*(4), 555–574. https://doi.org/10.2307/2971731

Jack, A. A. (2019). *The privileged poor: How elite colleges are failing disadvantaged students*. Harvard University Press. https://doi.org/10.4159/9780674239647

Jack, A. A., & Black, Z. (2024). Belonging and boundaries at an elite university. *Social Problems*, *71*(4), 996–1013. https://doi.org/10.1093/socpro/spac051

Kozakowski, Whitney. (2023). *Are four-year public colleges engines for economic mobility? Evidence from statewide admissions thresholds*. (EdWorkingPaper: 23-727). Annenberg Institute at Brown University. https://doi.org/10.26300/VAPT-4E25

Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. The Review of Economic Studies, 76(3), 1071–1102. https://doi.org/10.1111/j.1467-937X.2009.00536.x

Light, A., & Strayer, W. (2000). Determinants of college completion: School quality or student ability? *The Journal of Human Resources*, *35*(2), 299. https://doi.org/10.2307/146327

Marbach, M., & Hangartner, D. (2020). Profiling compliers and noncompliers for instrumental-variable analysis. *Political Analysis*, *28*(3), 435-444.

Miller, L. (2025). Switching schools: Effects of college transfers. (EdWorkingPaper: 25-1159). Annenberg Institute at Brown University. https://doi.org/10.26300/4khw-c054

Mountjoy, J. (2026). Marginal returns to public universities. *The Quarterly Journal of Economics*, 141(1): 429–497. https://doi.org/10.1093/qje/qjaf055

University of Michigan Office of the Registrar. n.d. Enrollment Summary, Fall 2015–Fall 2019. Ann Arbor, MI: University of Michigan. Accessed June 18, 2026. https://ro.umich.edu/sites/default/files/attachments/19enrollmentsummary.pdf

University Record. 2025. "U-M Breaks Enrollment Record, Welcomes 53,000 Students for Fall Term." September 26, 2025. Accessed June 18, 2026. https://record.umich.edu/articles/u-m-breaks-enrollment-record-welcomes-53000-students-for-fall-term/

U.S. Department of Education, National Center for Education Statistics. (2017a). Table 1.2-A: Among 2011–12 first-time postsecondary students who began at a 2-year public college, percentage distribution of 6-year attainment and retention status at any institution attended, by selected personal characteristics: 2012–17. Data Lab Tables Library. Retrieved from https://nces.ed.gov/datalab/table/library/detail/14326

U.S. Department of Education, National Center for Education Statistics. (2017b). Table 226.40: SAT mean scores, standard deviations, and score ranges for high school seniors, and percentage of the graduating class taking the SAT, by state: 2017. Retrieved from https://nces.ed.gov/programs/digest/d17/tables/dt17_226.40.asp

Zimmerman, S. D. (2014). The Returns to college admission for academically marginal students. *Journal of Labor Economics*, *32*(4), 711–754. https://doi.org/10.1086/676661

Degrees of Mobility:
Shifting Low-Income Students to Higher Quality Colleges
MAIN EXHIBITS

Table 1
High-School-Level Summary Statistics and Treatment-Control Balance

Characteristic	Control mean	Treated mean	P-value
High school in Southeast MI	0.401	0.394	0.864
	(0.029)	(0.030)	
Town/rural school	0.530	0.540	0.838
	(0.030)	(0.030)	
Suburban school	0.340	0.360	0.592
	(0.028)	(0.029)	
City school	0.129	0.100	0.273
	(0.020)	(0.018)	
Distance of high school from UM (miles)	93.2	96.4	0.597
	(4.691)	(4.824)	
Proportion eligible for free lunch	0.709	0.692	0.450
	(0.015)	(0.015)	
Proportion eligible for reduced-price lunch	0.291	0.308	0.450
	(0.015)	(0.015)	
Proportion female	0.571	0.605	0.102
	(0.016)	(0.015)	
Proportion White	0.772	0.787	0.566
	(0.017)	(0.016)	
Proportion Black	0.094	0.087	0.685
	(0.013)	(0.011)	
Proportion Asian American	0.061	0.057	0.718
	(0.008)	(0.009)	
Proportion Hispanic	0.053	0.057	0.681
	(0.008)	(0.009)	
Average SAT	1254	1259	0.200
	(2.805)	(3.017)	
Average GPA	3.82	3.83	0.263
	(0.006)	(0.006)	
Proportion sent ACT scores to UM flagship	0.459	0.453	0.835
(first cohort only)	(0.021)	(0.022)	
F-test p-value		0.449	
Number of school-years	526	500	1,026
Number of students	1,978	1,932	3,910

Notes: Analysis conducted at the high school-by-cohort level. Each p-value is from a t-test of the coefficient on treatment status from a regression of the characteristic on treatment and strata dummies. Race/ethnicity categories are mutually exclusive. The omitted race/ethnicity category includes students who are American Indian, Alaska Native, or Native Hawaiian. The first cohort took the ACT, while the second took the SAT; we convert to the SAT scale for consistency. Score send data is incomplete for the second cohort, so we calculate the share sending scores to UM for the first cohort only. F-test is from a joint significance test predicting treatment based on the characteristics listed here (except sending scores to UM) as well as strata dummies. Standard errors clustered at the high-school-level in parentheses.

Table 2
Estimated Effects on Degree Attainment and Projected Earnings

	ITT		LATE	
	Effect of HAIL offer	Control mean	Effect of Attending Any UM Campus	Control complier mean
First Stage				
Enroll Any UM Campus	0.208	0.149		
within 1 year	(0.021)			
F-statistic	100.141			
ITT and LATE				
BA Attainment				
Any BA within 4 years	0.041	0.391	0.197	0.420
	(0.022)		(0.099)	
Any BA within 6 years	-0.006	0.625	-0.030	0.765
	(0.021)		(0.104)	
Highly competitive+ BA within 6 years	0.117	0.144	0.564	0.049
	(0.019)		(0.062)	
STEM BA within 6 years	-0.010	0.257	-0.050	0.424
	(0.019)		(0.091)	
Joint Enrollment + BA Attainment (Satellite Pathway)				
Start at UM Satellite and earn any BA within 6 years	0.040	0.021	0.192	0.000
	(0.009)		(0.041)	
Start at UM Satellite and earn highly comp.+ BA within 6 years	0.023	0.001	0.112	0.000
	(0.005)		(0.022)	
Projected earnings (2025$)				
Projected using institutional average	3,393	58,523	16,321	62,342
	(985)		(3,957)	
Projected using students with family income <= $30K	3,557	52,569	17,109	55,402
	(912)		(3,524)	
N school-cohorts	1,026			

Notes: Analysis conducted at the high-school-by-cohort-level. Robust standard errors reported in parentheses. ITT coefficients are estimates of β_1 in Equation (1), estimated using OLS. LATEs are estimates of γ_1 in Equation (3), estimated using 2SLS. Degree outcomes measured by the end of the fourth or sixth summers following high school, and are not conditional on initial enrollment. Control complier means are calculated at the student level, inverse weighted by size of high school cohort. Institution earnings are median earnings 8 years post college-entry from College Scorecard (2013 entry cohort). Projected earnings are assigned to students based on their modal college attended. Students with two modal institutions are assigned the average for the two modal institutions. College Scorecard earnings are only available for students who enrolled in college. Students who did not enroll or who attend institutions where earnings are not available are assigned earnings from non-attendees in Opportunity Insights data. Earnings are all measured in 2025 values, adjusting for inflation using the consumer price index (CPI-U).

Table 3
Pre-Treatment Characteristics by Compliance Sub-Populations

	Full sample	Always-takers	Never-takers	Compliers
Population share	1.000	0.149	0.644	0.207
Geography				
High school in Southeast	0.398	0.640	0.331	0.430
Distance of high school from UM (miles)	94.7	63.5	106.0	82.4
Urbanicity				
Town/rural school	0.535	0.317	0.571	0.580
Suburban school	0.350	0.459	0.344	0.291
City school	0.115	0.224	0.085	0.130
Demographics				
Female	0.588	0.612	0.600	0.532
White	0.779	0.597	0.802	0.840
Black	0.091	0.203	0.082	0.039
Asian American	0.059	0.105	0.044	0.072
Hispanic	0.055	0.078	0.059	0.025
Academic Preparation				
SAT composite	1257	1288	1246	1267
GPA	3.83	3.89	3.81	3.84
N students		3,910		
N school-cohorts		1,026		
Pre-treatment college signaling (first cohort only)				
Sent scores to UM flagship	0.456	0.786	0.361	0.544
Sent scores to very competitive school	0.614	0.587	0.589	0.706
Sent scores to highly competitive+ school	0.567	0.839	0.487	0.646
Mean admit rate of score sends	0.625	0.497	0.663	0.587
N students, first cohort		2,108		
N schools, first cohort		529		

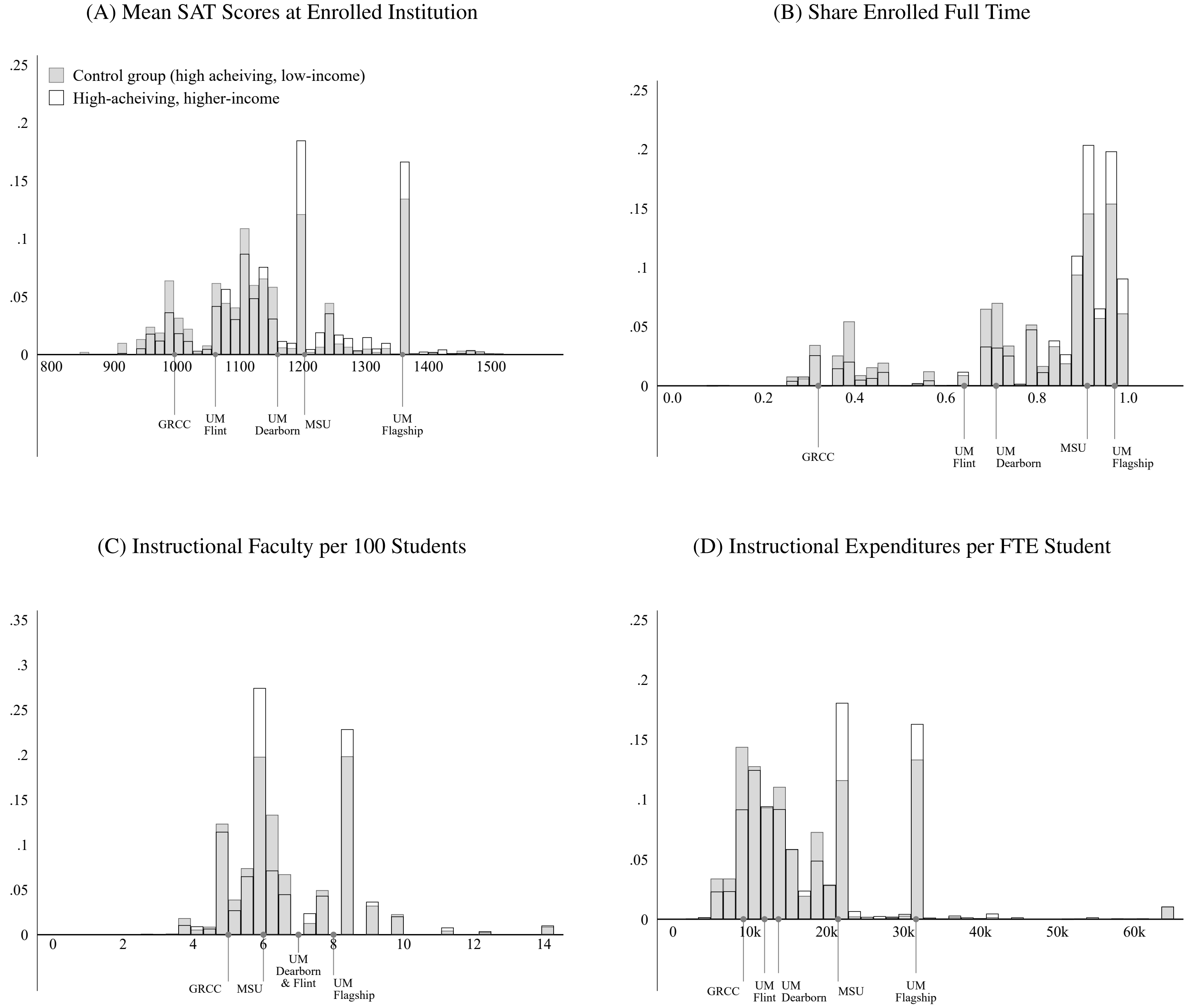
Notes: This analysis is done at the student level and inverse weighted by size of high school cohort. Shares and means are calculated following Imbens and Rubin (1997) and Marbach and Hangartner (2020), described in Online Appendix B.9. Always takers are defined as students in the control group who attend a UM campus within one year of high school graduation. Never takers are students assigned to treatment who do not attend UM. Score send data is incomplete for the second cohort; analysis using score send data uses the first cohort only (including the calculation of population shares).

Table 4
Estimated Effects on Characteristics of Colleges Attended (College-Attending Students Only)

	ITT		LATE	
	Effect of HAIL offer	Control mean	Effect of Attending Any UM Campus	Control complier mean
First Stage				
Enroll Any UM Campus within 1 year	0.208 (0.021)	0.149		
F-statistic	101.043			
ITT and LATE				
Characteristics of the Institutions Attended				
Average SAT of enrolled Michigan students	40 (6)	1143	187 (21)	1128
Share enrolled full time	0.031 (0.009)	0.775	0.147 (0.041)	0.756
Instructional expenditures per FTE student	2,794 (443)	16,170	13,152 (1,399)	15,045
Endowment per FTE student	50,136 (6,432)	71,215	236,031 (18,612)	38,692
Instructional faculty per 100 students	0.324 (0.072)	6.658	1.528 (0.293)	6.479
Median parental income	13,372 (2,018)	140,125	62,954 (6,470)	133,573
Share with parent who graduated college	0.019 (0.005)	0.702	0.087 (0.020)	0.695
Share not receiving a Pell Grant	0.025 (0.006)	0.674	0.116 (0.023)	0.679
Rejection rate (1-acceptance rate)	0.094 (0.012)	0.284	0.441 (0.034)	0.226
Average 6-year BA rate, Pell recipients	0.068 (0.013)	0.559	0.317 (0.047)	0.519
N students	3,652			
N school-cohorts	999			

Notes: Analysis conducted at the high-school-by-cohort-level. Robust standard errors reported in parentheses. This analysis excludes students who do not enroll in college in at least one of the two years following high school. ITT coefficients are estimates of β_1 in Equation (1), estimated using OLS. LATEs are estimates of γ_1 in Equation (3), estimated using 2SLS. Control complier means are calculated at the student level following Imbens and Rubin (1997), described in Online Appendix B.9, and inverse weighted by size of high school cohort. Students are assigned an average of the characteristics for their first attended institution in years 1 and 2. Dollar amounts are adjusted with the CPI-U to 2025 dollars. Variable construction is described in more detail in Appendix B.6.

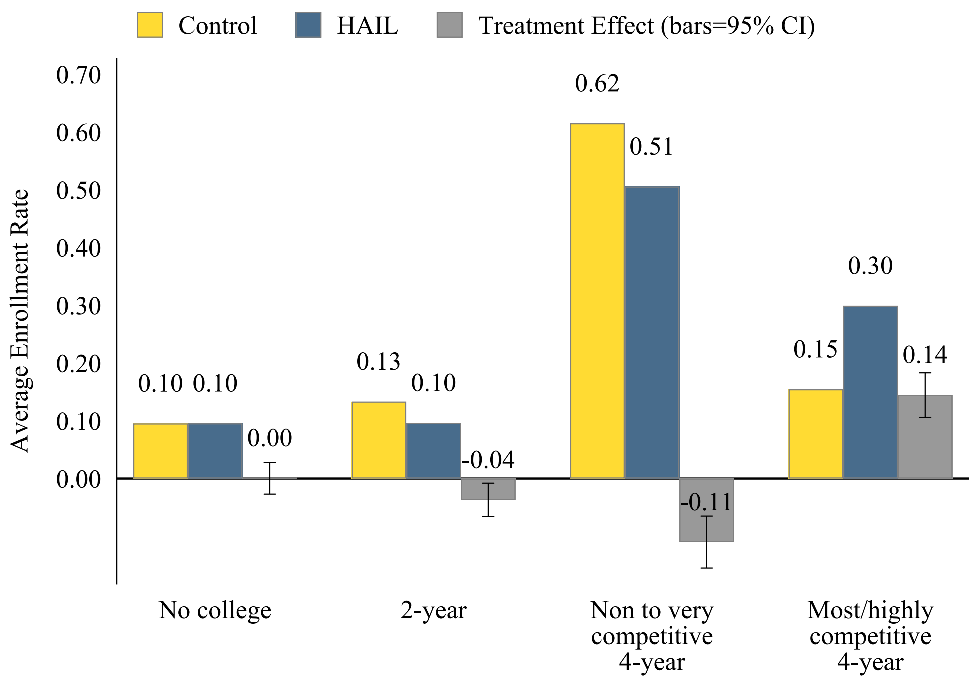
Figure 1
Distribution of Characteristics of Colleges Attended by High-Achieving Students in Michigan, by Family Income



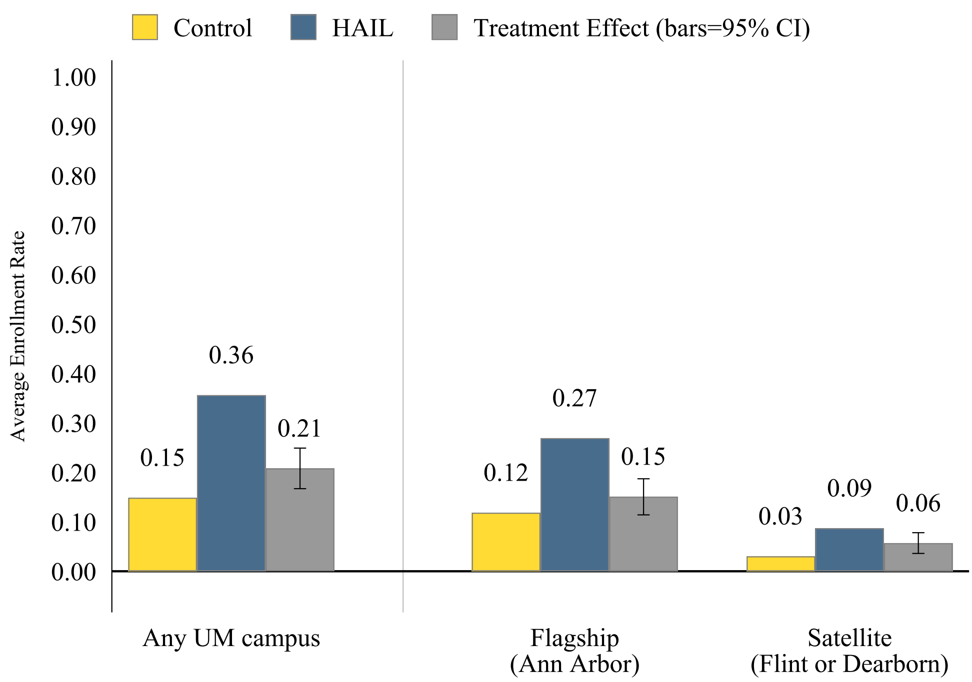
Notes: Higher-income, high achieving students are in the same cohorts and meet the same academic criteria as HAIL students. Distributions are at the student-level. Students are assigned the characteristics of the first college attended after high school. Institution SAT is the mean for pre-HAIL Michigan public high school students. All other institutional characteristics are from IPEDS and College Scorecard (2015-16), described in more detail in Appendix B.6. Dollar amounts are adjusted with the CPI-U to 2025 dollars.

Figure 2
Estimated Effect of HAIL on First-Year College Enrollment

(A) First-year College Enrollment by Selectivity Category



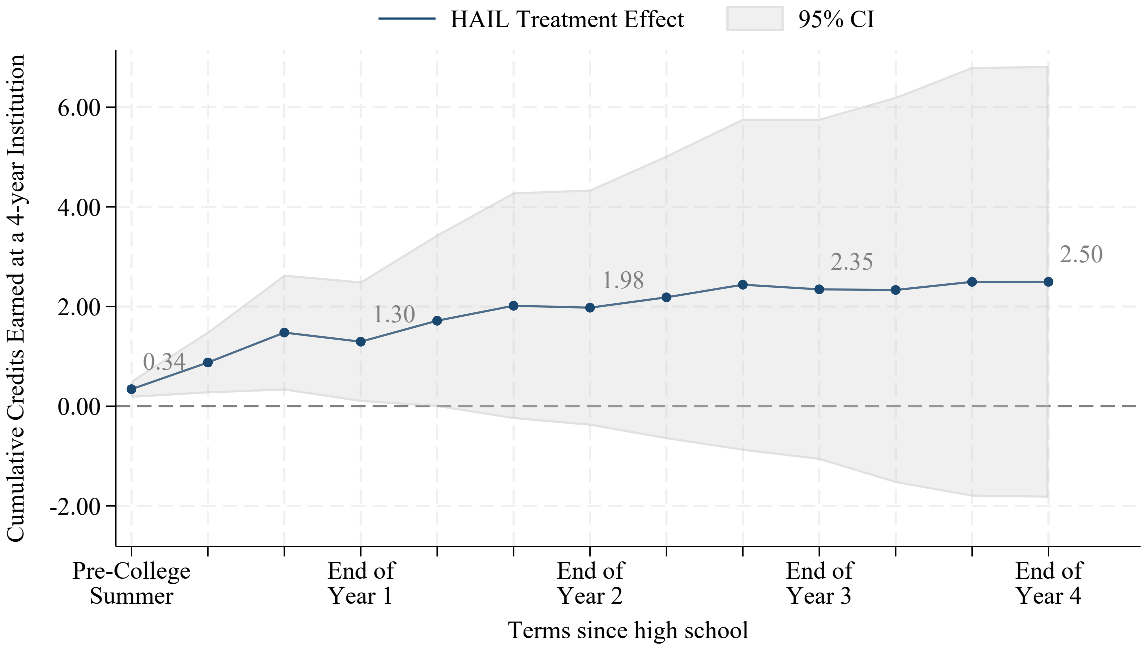
(B) First-year College Enrollment at University of Michigan, by Campus



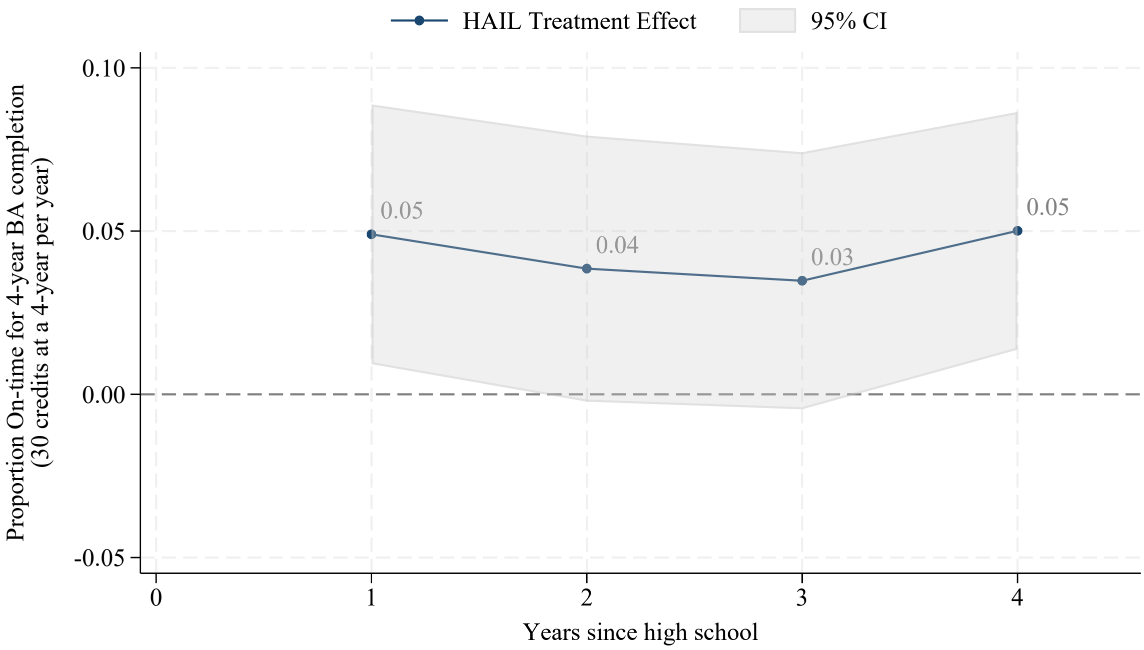
Notes: Figure plots the rate of enrollment in the first year following high school by Barron's institution competitiveness, and by UM flagship (Ann Arbor) and UM Satellites (Flint or Dearborn). This analysis is conducted at the high-school-by-cohort-level. Treatment effect coefficients are estimates of β_1 in Equation 1. Ninety-five percent confidence intervals on treatment effects are based on robust standard errors.

Figure 3
Estimated Effect of HAIL on Credit Accumulation at 4-year Institutions

(A) Cumulative Credits Earned



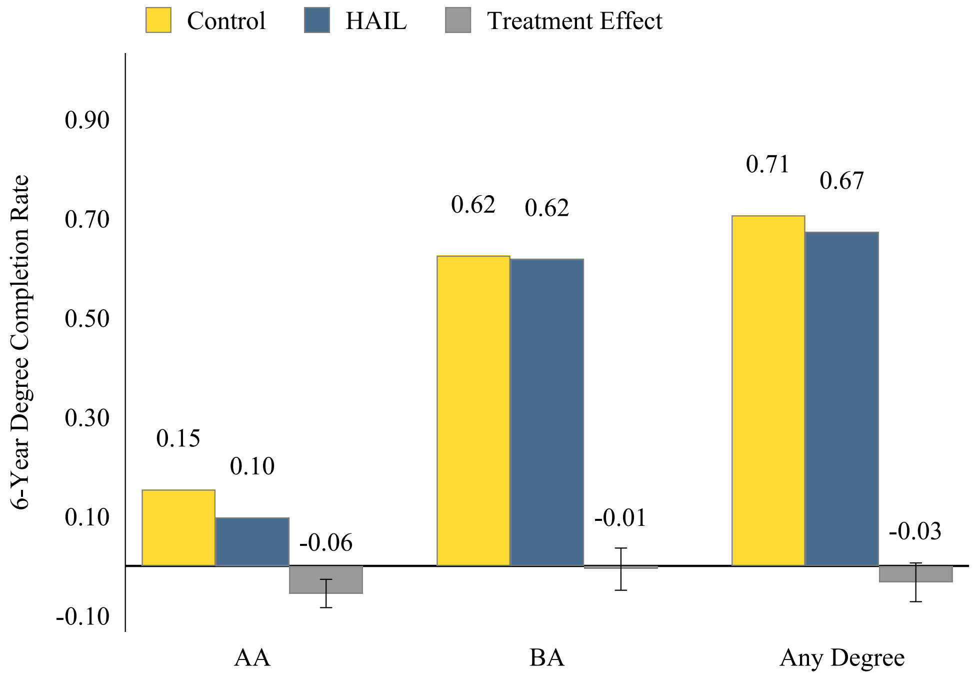
(B) On-track for 4-year BA Completion



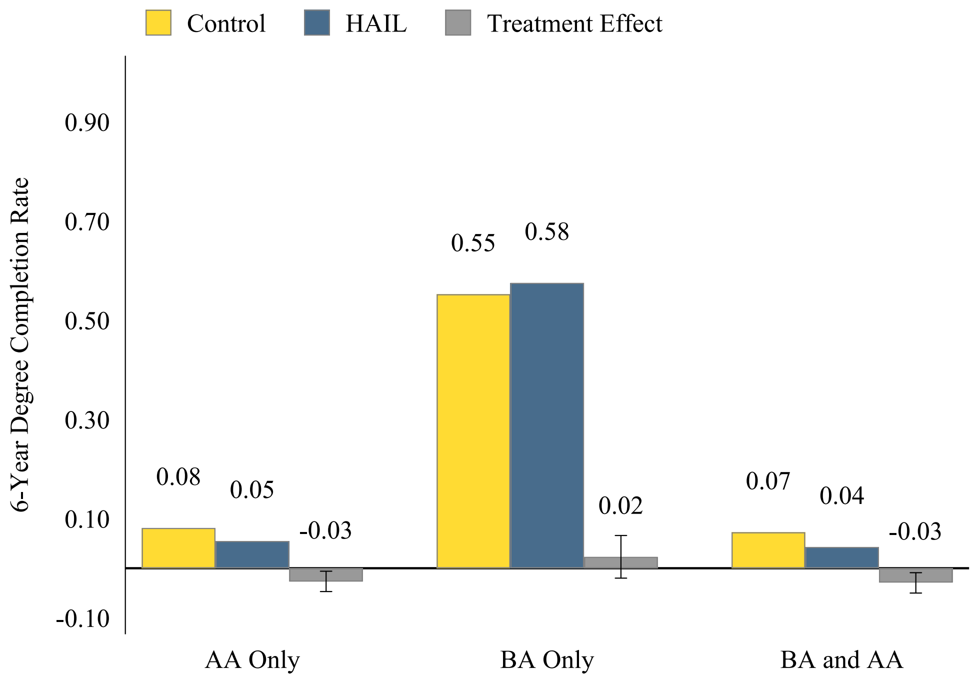
Notes: This analysis is conducted at the high-school-by-cohort-level. Robust standard errors are reported in parentheses. Treatment effect coefficients are estimates of β_1 in Equation 1. Credits are calculated at the term-level then aggregated to the student-year level. Measured credits include undergraduate coursework taken at a given institution. Annual credits are measured at the end of the summer term and include fall, spring, and summer credits. Year 0 includes the summer between high school and fall 1. Credits are imputed for students who have an undergraduate enrollment record in NSC but are missing credit information in STARR using a regression of the credit outcome on selectivity, enrollment-status, years since high school, and semester-type (fall, spring, summer) for control students with non-missing credits, then summed by year.

Figure 4
Estimated Effect of HAIL on Degree Attainment

(A) Any Degree Attainment



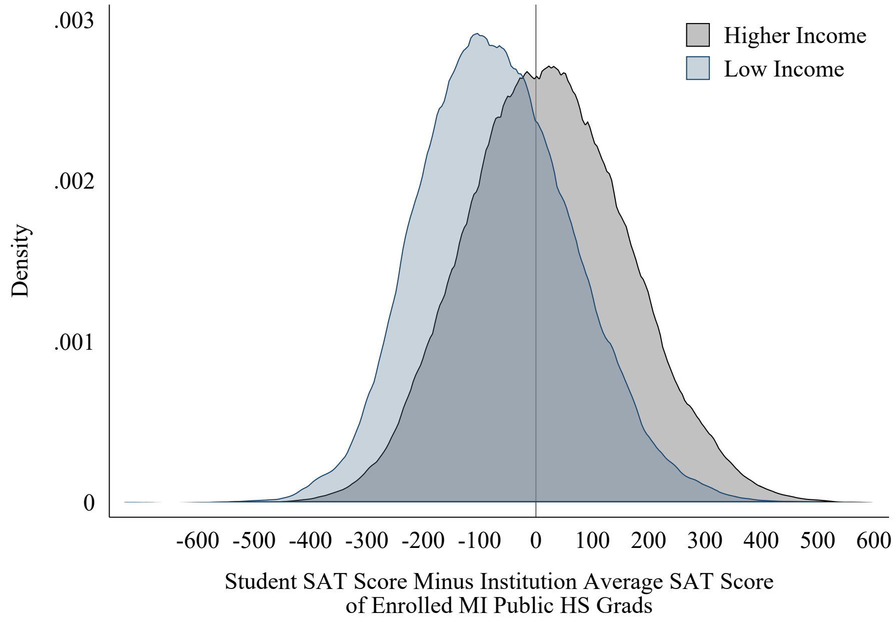
(B) Degree Combinations



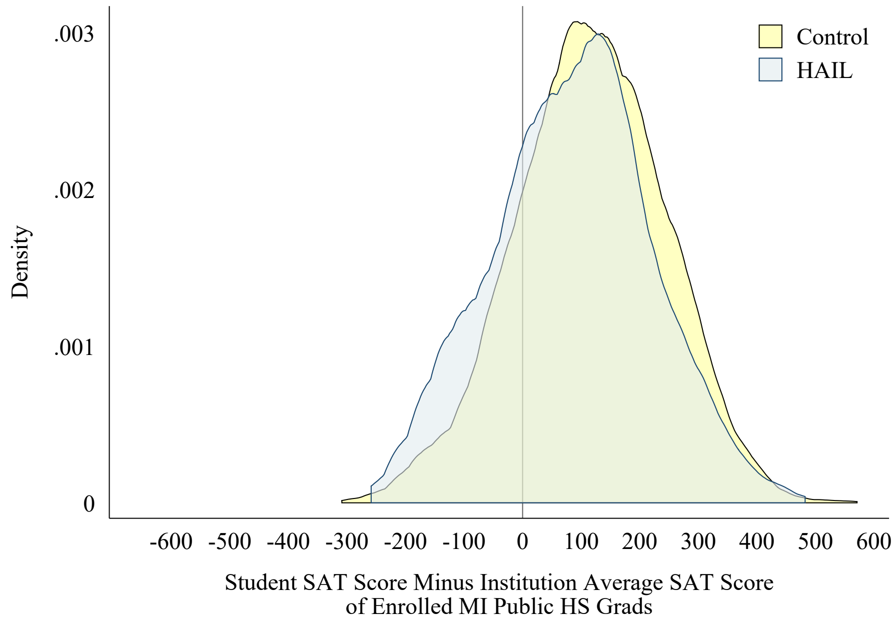
Notes: Figure plots the rate of degree attainment within 6 years of high school. This analysis is conducted at the high-school-by-cohort-level. Treatment effect coefficients are estimates of β_1 in Equation 1. Ninety-five percent confidence intervals on treatment effects are based on robust standard errors.

Figure 5
Average SAT Scores Relative to the Average SAT Score of Institutional Peers

(A) All Michigan Students

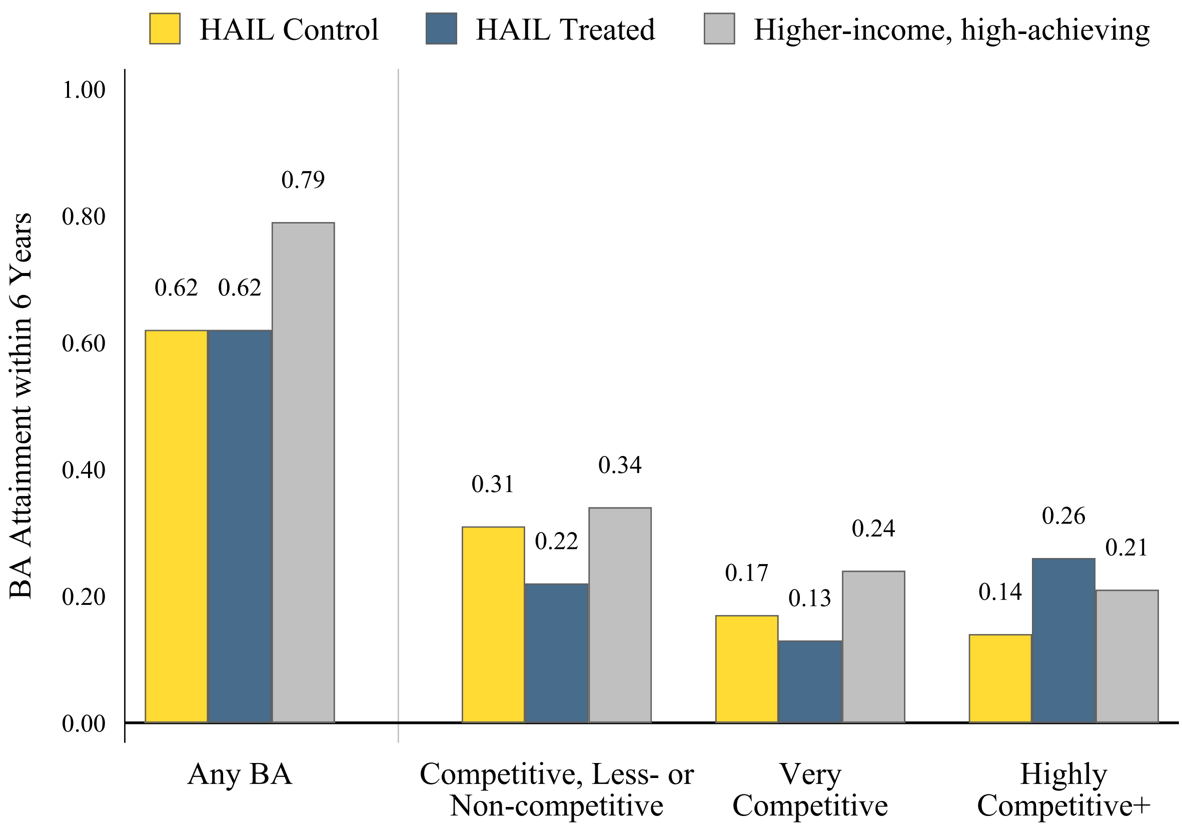


(B) HAIL Treated and Control Students



Notes: Figures plot the difference between a student's SAT score and the average SAT at the institution they attend. Distributions are at the student-level. Institution mean SAT is calculated using the test scores of Michigan public school students finishing high school in 2013-2014 who attend a given institution in the first year following high school. All ACT scores converted to SAT scale using concordance tables provided by the College Board. For institutions with fewer than 20 Michigan public high school students enrolled, the average score for institutions grouped by selectivity and public/private status. Students not enrolled during the first year are excluded. Panel A includes all Michigan public high school students expected to graduate in the same cohorts as the HAIL sample (2016-2017), excluding those who received the HAIL treatment, not restricted by academic achievement.

Figure 6
Selectivity of Bachelor's Degrees Earned by High-Achieving Michigan Students,
by Income and HAIL Treatment Status



Notes: Sample is eleventh grade students in Michigan public schools in 2015 and 2016 who meet HAIL GPA and ACT/SAT criteria. Higher-income students are those not eligible for free- or reduced-price lunch. Degree selectivity is based on Barron's competitiveness index. Degree attainment rates for HAIL treatment and control groups use our high-school-by-cohort-level measures. Average degree attainment for higher-income students is calculated at the student level.

APPENDIX FOR:
Degrees of Mobility:
Shifting Low-Income Students to Higher Quality Colleges

Elizabeth Burland, Nora Delaney, Susan Dynarski, Katherine Leu, CJ Libassi,
Katherine Michelmore, Stephanie Owen, Mary Quiroga, and Elizabeth Salinas

A Supplementary Exhibits

Appendix Table A. 1
Summary of Literature on Effects of College Quality

Citation	Average SAT score of sample	T vs C (test scores, SDs, ITT)	4-year BA attainment: Control mean	4-year BA attainment: Treatment Effect (ITT)	6-year BA attainment: Control mean	6-year BA attainment: Treatment Effect (ITT)	Earnings: Control mean	Earnings: Treatment effect (%, ITT)	Context
Present Analysis	**1254**	**0.270**	**0.390**	**0.040**	**0.620**	**-0.010**	**37,907**	**0.072**	**MI**
Chetty et al. (2026)	1374							0.040	U.S.
Cohodes et al. (2026)	1436		0.560	0.053	0.770	0.025	104,213	0.118	U.S.
Mountjoy (2026)	916	0.075	0.100	0.008	0.300	0.018	40,815	0.013	TX
Barr & Castleman (2025)			0.268	0.062	0.528	0.076			MA, NY, IL
Bleemer (2024)	1043	0.220			0.600	0.022	70,000	0.039	CA
Kozakowski (2023)	878	0.060			0.390	0.036	30,577	0.064	MA
Angrist et al. (2022)	1100		0.320	-0.028	0.636	0.081			NE
Goodman et al. (2017)	906	0.020			0.370	0.009			GA
Dale & Krueger (2014)									
Overall	1210						139,698	-0.008	U.S.
Black and Hispanic	1024						98,472	0.076	U.S.
Cohodes & Goodman (2014)+	1279	0.018	0.462	0.019	0.631	0.025			MA
Zimmerman (2014)	943		0.050	-0.007	0.200	0.057	28,963	0.051	FL
Hoekstra (2009)	1000							0.135	?

Notes: + signifies the effects are reverse coded to align with the direction of the other results for easier comparison. Notes on average SAT score of the sample: We convert Angrist et al. (2022) reported ACT scores to SAT scores. For estimates from Cohodes and Goodman (2014) and Cohodes et al. (2026), we convert reported standardized test scores (in SD units) to SAT scores using NCES-reported SAT means and SDs. Cohodes and Goodman (2014) report that those in the RD sample score about 0.90 standard deviation above average MCAS scores relative to all high school grads in Massachusetts, we use Massachusetts specific estimates to convert this to an equivalent SAT score. Cohodes et al. (2026) report a standardized math score (a mix of PSAT, SAT, and ACT scores), an average of 1.929 sd which we convert to an equivalent SAT score using the national mean and standard deviation from 2017. For the paper from Hoekstra (2009), the SAT score we report is based on this description in the text: "the average SAT scores of the entering classes at the flagship over the period of time studied were between 1000 and 1100" (p. 717). We use the lower number since that range is the average, and the RD is for the marginal student.The T vs C contrast column represents the average test scores of students attending institutions in the treated group relative to the comparison group. Unless otherwise noted, these reflect differences in SAT scores between treatment and comparison groups. Goodman et al. (2017) report PSAT scores rather than SAT scores, Mountjoy (2026) reports 11th grade standardized test scores, and Kozakowski, Whitney (2023) reports MCAS standardized test scores from Massachusetts. Cohodes and Goodman (2014) only report SAT math scores, not composite. All other papers use SAT composite test scores (Zimmerman 2014; Dale and Krueger 2014; Chetty et al. 2026; Bleemer 2024; Barr and Castleman 2025). SAT score treatment effects converted into standard deviation units using our institution distribution, where a standard deviation unit is approximately 133 points.

Appendix Table A. 2

Estimated Effect of HAIL on Financial Aid Receipt at the UM Flagship

Adapted from Burland et al. (2023, p.304)

	Control Mean	HAIL mean	P-value
Grants at UM Flagship, First Year	$24,729	$24,207	0.360
	(497)	(235)	
Grants at UM Flagship, Years 1-4	$106,643	$105,735	0.720
	(2230)	(1011)	
N (Enrolled at UM Flagship)	237	465	

Notes: Adapted from Table 3 in "The power of certainty: Experimental evidence on the effective design of free tuition programs," by Burland, E., Dynarski, S., Michelmore, K., Owen, S. and Raghuraman, S., 2023. American Economic Review: Insights, 5(3): 293–310. (https://doi.org/10.1257/aeri.20220094). Analysis done at the student-level. Includes only students enrolled at the UM Flagship full time and who have financial aid data reported. Standard errors are clustered at the high-school-by-cohort-level. Includes zeros for students who receive no aid. "Grants" includes all institutional and departmental scholarships and grants, federal grants, state grants and scholarships, private scholarships, and other departmental aid.

Appendix Table A. 3
Estimated Effect of HAIL on Transfer Pathways

	Transfer in Fall 2		Transfer in Fall 3	
Outcome	ITT: Effect of HAIL Offer	Control mean	ITT: Effect of HAIL Offer	Control mean
Any Transfer	0.023	0.080	-0.021	0.070
	(0.012)		(0.011)	
Institution type				
2-year to 4-year	-0.007	0.020	-0.014	0.031
	(0.005)		(0.008)	
4-year to 2-year	-0.002	0.027	-0.004	0.011
	(0.007)		(0.004)	
2-year to 2-year	0.005	0.002	-0.002	0.003
	(0.003)		(0.002)	
4-year to 4-year	0.028	0.031	-0.001	0.023
	(0.009)		(0.006)	
Transfer into UM Flagship				
From 2-year or non-UM 4-year	-0.002	0.004	-0.003	0.004
	(0.002)		(0.002)	
From UM Satellite	0.029	0.001	0.001	0.001
	(0.005)		(0.001)	
Transfer from UM Flagship				
To a 2-year or non-UM 4-year	0.010	0.000	0.003	0.002
	(0.004)		(0.003)	
To a UM Satellite	0.003	0.000	0.001	0.000
	(0.002)		(0.001)	
N school-cohorts		1026		

Notes: Analyses are conducted at the high-school-by-cohort-level. Robust standard errors reported in parentheses. Treatment effect coefficients are estimates of β_1 from Equation (1). Transfers are defined as having different enrollment from one fall to the following fall (fall 1 to fall 2, fall 2 to fall 3). Transfer outcomes are unconditional on initial enrollment. For example, a student who started at a four-year school would be coded as a zero for transfer from two- to four-year, as would a student who started at a two-year and did not transfer.

Appendix Table A. 4

Estimated Effect of HAIL on Graduate School Outcomes

	ITT		LATE	
	Effect of HAIL offer	Control mean	Effect of Attending Any UM Campus	Control complier mean
First Stage				
Enroll Any UM Campus within 1 year	0.208 (0.021)	0.149		
F-statistic	100.141			
ITT and LATE				
Graduate School, within 6 Years				
Enrolled in Graduate School	-0.004 (0.016)	0.180	-0.021 (0.077)	0.225
Earned Graduate Degree	-0.004 (0.009)	0.053	-0.020 (0.044)	0.066
N school-cohorts	1,026			

Notes: Analyses are conducted at the high-school-by-cohort-level. Robust standard errors reported in parentheses. ITT coefficients are estimates of β_1 in Equation (1). LATEs are estimates of γ_1 in Equation (3), estimated using 2SLS. Control complier means are calculated at the student level, inverse weighted by size of high school cohort. Graduate school enrollment reflects terms that a student is enrolled after they have earned a bachelor's degree, or in terms flagged in the transcript data as graduate enrollments. Graduate degree completion is measured in the summer following the 6th spring and includes any graduate degrees awarded.

Appendix Table A. 5

Estimated Effect of HAIL on Projected Earnings 8 years Following College Entry, using Alternate Populations to Project Earnings

	ITT		LATE		
	Effect of HAIL offer	Control mean	Effect of Attending Any UM Campus	Control complier mean	Percent change
First Stage					
Enroll Any UM campus within 1 year	0.208 (0.021)	0.149			
F-statistic	100.141				
ITT and LATE					
Projected earnings (2025$), Overall and by Family Income					
Projected using institutional average	3,393 (985)	58,523	16,321 (3,957)	62,342	26%
Projected using students with family income <= $30K	3,557 (912)	52,569	17,109 (3,524)	55,402	31%
Projected using students with family income $30K-75K	3,020 (902)	58,801	14,528 (3,639)	62,629	23%
Projected using students with family income > $75K	3,191 (989)	64,437	15,350 (4,047)	68,825	22%
N (School-cohorts)	1,026				

Notes: Analyses are conducted at the high-school-by-cohort-level. Robust standard errors reported in parentheses. ITT coefficients are estimates of β_1 in Equation (1). LATEs are estimates of γ_1 in Equation (3), estimated using 2SLS. Control complier means are calculated at the student level, inverse weighted by size of high school cohort. Institution earnings (both overall and by family income) reflect the median earnings of enrolled students 8 years post college-entry from College Scorecard (2013 entry cohort). Projected earnings are assigned to students based on their modal college attended. Students with two modal institutions are assigned the average for the two modal institutions. Scorecard earnings are only available for students who enrolled in college. Students who did not enroll or attend institutions where earnings are not available are assigned earnings from Opportunity Insights. Earnings are all measured in 2025 values, adjusting for inflation using the consumer price index (CPI-U).

B Outcomes and Measurement

B.1 Enrollment

We use enrollment from the National Student Clearinghouse (NSC). We use NSC enrollment dates to structure enrollment into three terms per academic year: fall, spring, and summer. We measure enrollment from expected high school graduation. The first HAIL cohort was expected to graduate high school in the spring of 2016, therefore "fall 1" is fall 2016, "spring 1" is spring 2017, and "summer 1" is summer 2017. In the transcript data, we enforce the same three term structure that aligns with the NSC enrollment outcomes, which allows us to merge these data at the student-by-term level. For each year, we create mutually-exclusive enrollment outcomes based on the first institution enrolled in a given year.

Our primary measure of enrollment only captures undergraduate enrollment, which we define as any enrollment terms that occur prior to a student earning their first bachelor's degree in NSC. We also exclude terms defined as "Graduate" in the student transcript data. We also exclude terms where a student was enrolled in a non-degree program according to the student transcript data, or terms where a student was enrolled for fewer than 15 days, which corresponds with the timing to withdraw and still receive a refund on tuition paid.

B.2 Degree Attainment

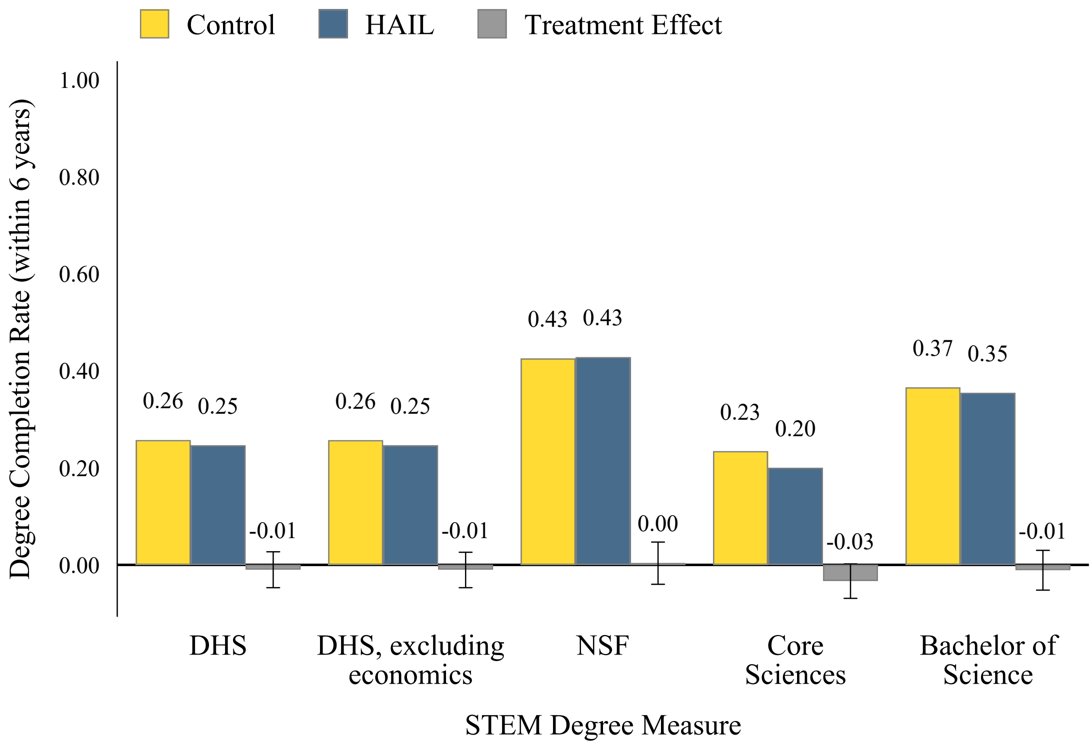
We use degree attainment from NSC, which also we structure into three terms per academic year using dates provided by NSC for degree attainment to align with the enrollment terms. We supplement this with degree attainment from the STARR transcript data. We measure time to degree from expected high school graduation, similar to how we approach enrollment. Degrees "by" a given term include degrees earned in that term, or any prior term. In cases where a student earns multiple bachelor's degrees, we report their first bachelor's degree earned. We measure 4- and 6-year degree attainment in the summer following the 4th and 6th spring. For example, for a student in the first HAIL cohort (expected to graduate high school in 2016), 4-year BA attainment is measured in the summer of 2020 and 6-year BA attainment in the summer of 2022.

B.3 STEM Degrees

Our primary measure of STEM uses the Department of Homeland Security's list of STEM CIP codes (United States Department of Homeland Security 2022). This includes all 2-digit CIP codes classified under the broad categories of biological and biomedical sciences, mathematics and statistics, physical sciences, and engineering, as well as certain 6-digit CIP codes under other categories. We test the robustness of our findings to different definitions of STEM:

- DHS excluding economics: excludes economics from the DHS definition due to the subjectivity of defining only certain economics degrees as "STEM."
- NSF: National Science Foundation's "science and engineering (S&E) fields" (National Science Board 2024). Using a crosswalk provided by NCES, we identify all of the CIP codes which crosswalk with these broader categories of STEM, at the 4-digit CIP level, and supplement with "degree major" information from NSC when CIP code is not available.
- Core Sciences: Includes any degree in biological sciences, atmospheric sciences, computer sciences, mathematics, chemistry, physics, and engineering (e.g., Griffith 2010; Riegle-Crumb et al. 2012; Hatfield et al. 2022).
- Bachelor of Science: National Student Clearinghouse degree type is Bachelor of Science.

Appendix Figure B.1 shows the results using each of these definitions of STEM. Regardless of which definition we use, we find no effect of HAIL on STEM degree attainment.



Appendix Figure B. 1

Effect of HAIL on STEM BA degree attainment using alternative STEM definitions

Notes: The first column, "STEM BA (DHS)" is the original definition we use in our current analyses. With the exception of "Bachelor of Science" (which is exclusively from NSC), we start with STEM degree information from the STARR dataset and then merge on NSC data. We count a student as having a STEM degree if they are coded as having a STEM degree in either dataset.

B.4 Projected Earnings

To project earnings, we use data College Scorecard, which reports median adult earnings for all Title IV funded students. We use data for the cohort who entered college in 2013 measured eight years post college entry (College Scorecard 2021). We calculate each student's modal institution attended between year 1 and year 6 following high school, and merge on the institutional earnings from College Scorecard using OPEID code. Students with multiple modal institutions are assigned the average earnings for their two modal institutions. We measure median earnings in 2025 values, adjusted for inflation using the consumer price index (CPI-U). These data are only available for students who ever enroll. While HAIL does not affect college enrollment overall, we still assign these students to the projected earnings from Opportunity Insights data, measured at age 30 (Chetty et al. 2022), for students who do not enroll in college. We also use these data for students who enroll in institutions with missing earnings data.

B.5 Graduate School Enrollment and Degree Attainment

Graduate school enrollment is measured using NSC, supplemented with transcript data, structured the same way as our undergraduate enrollment measures. Students are considered enrolled in graduate school during terms they are enrolled in NSC after they have already earned a bachelor's degree or if their enrollment is flagged in the transcript data as enrollment in a graduate degree program. Graduate degree completion is measured using NSC, and supplemented with transcript data, and includes any degrees earned that are classified as master's degrees or doctoral degrees (including an M.D., J.D., or Ph.D.). We measure 6-year graduate degree attainment in the summer following the 6th spring. For example, for a student in the first HAIL cohort (expected to graduate high school in 2016), 6-year graduate degree attainment is counted if a student earned a graduate degree during the summer of 2022 or a prior term.

B.6 Institution Characteristics

We calculate institution SAT scores by taking the average SAT score (converted from ACT scores using ACT-SAT concordance tables provided by the College Board) of Michigan public high school students who finished high school between 2013-2015 and who enrolled at each institution in their first year. Enrollment for these cohorts is measured the same as for our sample, described above. Institutions with fewer than 20 students enrolled in previous cohorts are assigned the average SAT score for institutions with their same Barron's selectivity and public/private designation. We assign HAIL students the average institution quality for the first school they attend in each of their first and second years following expected high school graduation. Figure 1a uses just the first year. Table 4 uses an average of the institutions attended in years 1 and 2.

Median parental income comes from Opportunity Insights (Chetty et al. 2022). Endowment data comes from the IPEDS finance survey, which we download using Urban Institute's Education Data Portal (U.S. Department of Education's National Center for Education Statistics 2023). The remaining characteristics use College Scorecard data (College Scorecard 2016). Dollar amounts are measured in 2025 values, adjusted for inflation using the consumer price index (CPI-U). We merge on the characteristics for the institutions students enrolled in for their first year and for their second year (measured as described above). For Figure 1b-d, we just use the characteristics of the first-year enrollment. For Table 4, individuals are assigned an average of the institution characteristics for their year 1 institution and their year 2 institution. For students who only enrolled in year 1 following high school, they are assigned the values for their year 1 institution. The same logic is applied for students who only enroll in year 2. Students who do not enroll in years 1 or 2 receive missing values.

B.7 Credits

We construct measures of cumulative credits earned while enrolled at four-year institutions and credits earned while enrolled at community colleges. The transcript data does not allow us to distinguish between transferred credits, AP credits, and dual enrollment credits. Counting these credits would result in counting the same credits multiple times, first at the initial institution and again at the institution students transfer to. Therefore, our measures exclusively focus on credits attempted and earned at a given school.[1] We also focus exclusively on undergraduate credits, using our enrollment measure to exclude any graduate credits. Students not enrolled anywhere in a given term are assigned zero credits. For those who do not attend public colleges within Michigan (i.e. are enrolled, but we do not have transcript data for them), we impute credits attempted and credits earned (described below).

Selection into the Transcript Sample For those students who attend public colleges within Michigan, we have access to student transcript data. While these transcripts are far richer than the NSC data, we only have them for students attending public colleges in Michigan. Students attending private colleges, and those attending college outside Michigan, are missing from these data. This could induce selection bias: the HAIL offer plausibly could have affected the likelihood that a student appears in the STARR data by increasing the share of students attending a public college in Michigan. We therefore use non-missing STARR data combined with universal NSC enrollment data to impute missing credit information for those students who do not attend public colleges within Michigan.

Appendix Table B.1 shows the effect of HAIL on selection into the transcript sample by comparing NSC enrollment (with universal coverage) to enrollment calculated from the transcript data. The estimated effect of the HAIL offer on attending a public college in Michigan, and therefore having any credits reported in the transcript data, is 4.1 percentage points, with a standard error of 2.0 percentage points. Since HAIL induced students into the STARR sample, a simple comparison of credits attempted and earned between the

[1] After this cleaning process, 99 percent of student-term observations have 18 or fewer credits attempted or earned, the standard upper-bound for full-time enrollment. However, the cleaning process is imperfect, and one percent of student-terms have credits attempted or earned that exceed 18. We top-code credits in any given semester (regardless of the type of institution) at 22 credits (changing the credits attempted/earned for 0.1 percent of cases). This does not meaningfully change the average credits attempted or earned.

treatment and control groups is likely biased in favor of finding a positive treatment effect, since we are more likely to observe credits for students in the treatment group.

Credit Imputation To address this, we impute credits attempted and earned for students who are enrolled in either a private institution within Michigan, or an institution outside the state of Michigan. We follow a similar procedure to that of Angrist et al. (2022).

For terms where students attend Michigan public institutions, we have their actual credits attempted and earned. For terms where students attend out-of-state institutions or in-state private institutions, we impute credits (attempted and earned) at the student-by-term level, separately for 2- and 4-year enrollments. To do so, we regress credits on enrollment term (fall, spring, summer), year (1-6 post high school graduation), and enrollment status (full-time, half-time, less than half-time, withdrawn/deceased). For four-year enrollments, we also include indicators for Barron's competitive index (highly or most competitive, very competitive or competitive, less-competitive or non-competitive four-year school) and the interaction between Barron's category and enrollment status. We then assign student-terms with missing credits their predicted values from the regression. Credits are imputed for 8.7 percent of student-terms (7.3 percent of student-terms for treated students, 9.2 percent of student-terms for control-group students).

Appendix Table B. 1

Effect of HAIL on selection into the transcript data sample

Outcome	Effect of HAIL offer (ITT)	Control mean
Enrolled at any college, Year 1 (NSC)	-0.001 (0.014)	0.905
Attempted any credits, Year 1 (transcript data)	0.041 (0.020)	0.730
N school-cohorts	1026	

Notes: All analyses done at the high-school-by-cohort-level. Robust standard errors reported in parentheses. Treatment effect coefficients are from estimating Equation (1), the coefficient reports estimates of β_1. Credits calculated at the term-level and include undergraduate coursework taken at a given institution and collapsed to the student-year level. Difference between treatment effect on enrollment in NSC and treatment effect on credits attempted in the transcript data represents selection into the transcript sample.

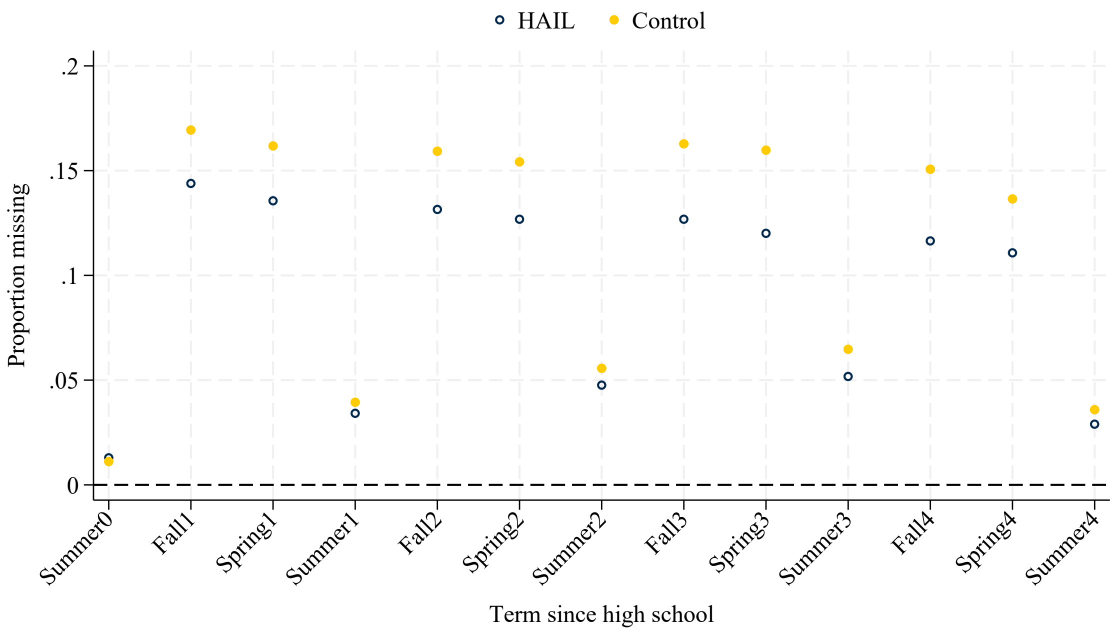
Lee Bounds In addition to our primary specification, we also estimate Lee Bounds (Lee 2009). Lee Bounds allow us to bound our treatment effects on credits by modeling the likelihood of missingness from the sample as a latent feature that is predicted by treatment assignment. The lower bound assumes all additional treated students who select into the sample are the top performers, and the upper bound assumes they are the bottom performers. In practice, this means trimming the highest values of earned credits for a portion of the sample equivalent to the differential attrition rate, then estimating treatment effects on the trimmed sample (lower bound); the upper bound is estimated on a sample with the equivalent proportion of the lowest credits trimmed. Lee Bounds do not bound the average treatment effect on all students. Rather, the approach bounds the effect for individuals for whom we would have observed credit outcomes regardless of treatment.

For each semester, we calculate the treatment-control difference in missingness for each credit outcome, at the student-level. We define missing as a term where a student is observed as enrolled in NSC, and missing in the transcript data. In each semester, we drop the top and bottom N_{drop} observations, where N_{drop} is equal to the differential missingness rate times the number of treated students. For example, in Fall 1, the missing rate for control is 16.94 percent, and the rate for treated is 14.39 percent, for a difference of 2.55 percent. There are 1,932 treated students. Thus in Fall 1 N_{drop} = 0.0255 * 1932 = 49 students. Appendix Figure B.2 below shows the missingness by treatment status and academic term. The difference between these two is the differential missingness rate.

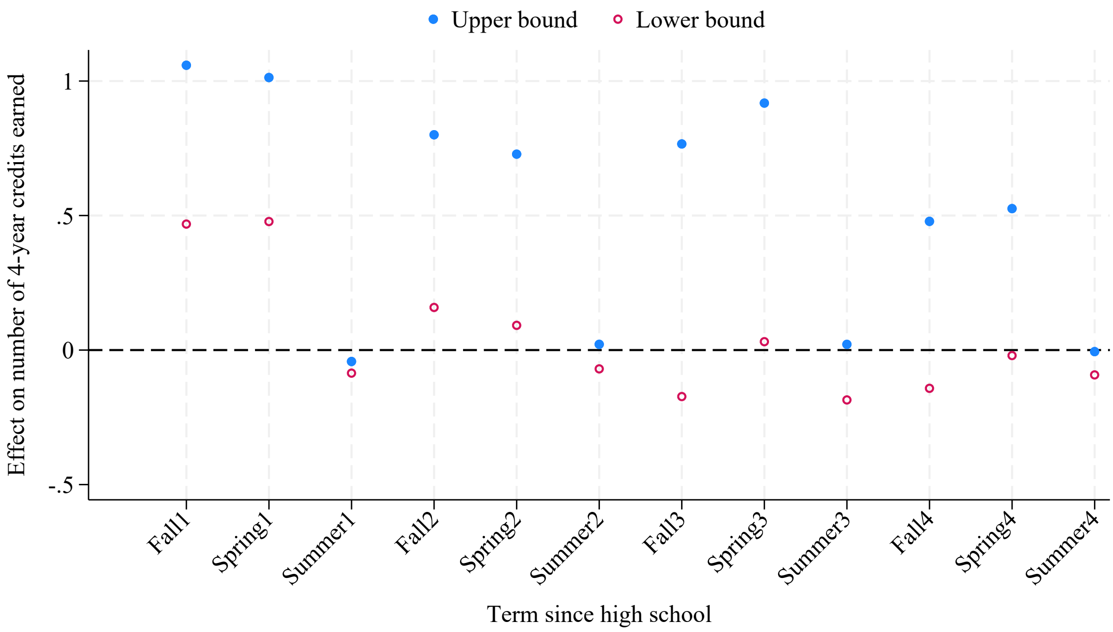
We estimate treatment effects with the trimmed variables at the school-by-cohort level, using Equation 1 (i.e., controlling for strata and clustering at the high-school-level). The lower bound estimates the effect of

HAIL on the credit outcome, dropping the top Ndrop observations from the credit outcome. The upper bound does the same, dropping the lower Ndrop observations.

Appendix Figure B.3 shows the bounds on earned credits from four-year institutions in each academic term. The treatment effects using Lee Bounds are largely consistent with the results of our primary approach using imputation: effects on credits earned in fall and spring of years 1 through 4 are likely positive, effects on credits earned in years 5 and 6 are likely negative, and credits effects in summer terms are close to zero.



Appendix Figure B. 2
Missingness by Treatment Status and Academic Term



Appendix Figure B. 3
Lee Bounds on Earned Four-Year Credits, by Academic Term: ITTs

B.8 Randomization Inference

Along with traditional sampling-based inference, for our primary intent-to-treat effects in Table 2 we also report exact p-values from randomization-based inference. This approach calculates the likelihood of obtaining the observed treatment effects by random chance, where the randomness comes from assignment of a fixed number of units (in our case, high schools) to treatment, rather than from random sampling from a population. Randomization inference tests the sharp null hypothesis of no treatment effect for any unit (Athey and Imbens, 2017).

Using the same procedure used in the original randomization, we randomly reassign treatment status to the 1,026 school-cohorts in our sample. We then estimate "treatment effects" based on this reassignment, controlling for randomization strata as in Equation 1. We repeat this procedure 1,000 times to generate a distribution of potential treatment effects that could be due to baseline differences between schools assigned to treatment and control. For each outcome, we calculate the share of the 1,000 simulated treatment-control differences that are larger in absolute value than the difference observed in the actual random assignment discussed throughout the paper. This proportion represents the randomization-based p-value.

Appendix Table B.2 compares sampling-based p-values to randomization-inference based p-values. None of the p-values for a given outcome differ by more than 0.02, and the conclusions are unchanged by the inference approach.

Appendix Table B. 2

Comparison of Sampling- and Randomization-Based Inference

	Effect of HAIL Offer (ITT)	Sampling-based p-value	Randomiz. Inference-based p-value
First Stage			
Enroll any UM campus within 1 year	0.208	0.000	0.000
Outcomes			
Any BA within 4 years	0.041	0.060	0.070
Any BA within 6 years	-0.006	0.768	0.758
Highly competitive+ BA within 6 years	0.117	0.000	0.000
STEM BA within 6 years	-0.010	0.583	0.596
Start at UM satellite and earn any BA within 6 years	0.040	0.000	0.000
Start at UM Satellite and earn highly comp.+ BA within 6 years	0.023	0.000	0.000
Projected earnings, using institution average	3,393	0.001	0.002
Projected earnings, using family income <= $30K	3,557	0.000	0.000

Notes: Analysis done at the school-cohort level. Table reports standard sampling-based p-values, which correspond to the estimates and robust standard errors in Table 2, compared to randomization-inference-based p-values. Randomization inference p-values are from 1000 treatment assignment iterations, and test a sharp null hypothesis of no effect for any unit.

B.9 Complier Analysis

To calculate control complier outcome means (Tables 2 and 4), as well as average pre-treatment characteristics for the latent sub-populations defined by the LATE framework (Table 3), we use the methods from Imbens and Rubin (1997) and formally extended by Marbach and Hangartner (2020). By relying on the same assumptions required to estimate LATEs–namely, the exclusion restriction and the absence of defiers–we can directly estimate means for always takers, never takers, and compliers.

First, we estimate the population shares using the sample analogues. For this to work, we must assume there are no "defiers"—students who are less likely to enroll at UM when they receive a HAIL offer relative to no offer. We calculate the proportion of always takers as the share of the control group who enroll at UM:

(1) $$p_{at} = E[EnrollUM|Treat = 0]$$

Similarly, the proportion of never takers is the share of the treatment group who do NOT enroll:

(2) $$p_{nt} = 1 - E[EnrollUM|Treat = 1]$$

Then, the proportion of compliers is simply the remainder (assuming no defiers):

(3) $$p_{cm} = 1 - p_{at} - p_{nt}$$

We can also directly calculate pre-treatment characteristic and outcome means for always- and never-takers. By assuming their behavior is not affected by treatment (exclusion restriction), we are assuming control and treated always (and never) takers have the same outcomes in expectation. The always-taker outcome mean is the mean among control students who enroll at UM:

(4) $$\bar{Y}_{at} = E[Y|Treat = 0, EnrollUM = 1]$$

And the never-taker mean is the outcome mean among treated students who do not enroll:

(5) $$\bar{Y}_{nt} = E[Y|Treat = 1, EnrollUM = 0]$$

We consider the overall control mean for any outcome of interest to be a weighted average across three types of students: always-takers, never-takers, and compliers. We directly observe outcomes for the always-takers—those students in the control group who enroll at UM. Assuming control and treated always-takers (and never-takers) have the same outcomes in expectation, we also observe the outcomes for the never-takers—those students in the treatment group who do not enroll at UM.

We can then use the fact that the overall control mean is a weighted average across the three types of students to solve for the control complier mean (CCM) as a function of observable quantities:

(6) $$\begin{aligned} \bar{Y}_{control} &= p_{at}\bar{Y}_{at} + p_{nt}\bar{Y}_{nt} + p_{cm}\bar{Y}_{cm} \\ \Rightarrow \bar{Y}_{cm} &= \frac{\bar{Y}_{control} - p_{at}\bar{Y}_{at} - p_{nt}\bar{Y}_{nt}}{p_{cm}} \end{aligned}$$

We estimate each of these at the student-level, since compliance is an individual-level behavior. However, we weight all averages by the inverse of the school size to be comparable with our high-school-by-cohort-level treatment effects.

Estimated CCMs for outcomes that should be bounded at zero can be negative, and are in some instances. Treated and control students (and all sub-populations, including compliers, which are defined by pre-treatment characteristics) are by assumption identical in expectation but may not be in our finite sample. Kling et al. (2007) describe this in detail:

> For binary outcomes, sampling variation can produce negative estimates of the CCM. Our method assumes that there would have been the same fraction of noncompliers in the control group as were observed in the treatment group and that these noncompliers had exactly the same outcome prevalence as treatment noncompliers. In any one sample, this method may produce a negative estimate of the CCM if a particular realization of the treatment noncomplier mean is higher than the realization of the control noncomplier mean, even though the method is unbiased in repeated sampling. (p. 94)

REFERENCES

Angrist, J., Autor, D., and Pallais, A. (2022). Marginal effects of merit aid for low-income students. *The Quarterly Journal of Economics*, 137(2):1039–1090.

Athey, S. and Imbens, G. W. (2017). The econometrics of randomized experiments. In *Handbook of Economic Field Experiments*, volume 1, pages 73–140. Elsevier.

Barr, A. and Castleman, B. (2025). Increasing degree attainment among low-income students: The role of intensive advising and college quality. *American Economic Review*, 115(11):4075–4103.

Bleemer, Z. (2024). Top percent policies and the return to postsecondary selectivity. Unpublished manuscript.

Chetty, R., Deming, D. J., and Friedman, J. N. (2026). Diversifying society's leaders? The determinants and causal effects of admission to highly selective private colleges. *The Quarterly Journal of Economics*, page qjaf050.

Chetty, R., Friedman, J., Saez, E., Turner, N., and Yagan, D. (2022). Replication data for: Income segregation and intergenerational mobility across colleges in the united states.

Cohodes, S. R. and Goodman, J. S. (2014). Merit aid, college quality, and college completion: Massachusetts' Adams scholarship as an in-kind subsidy. *American Economic Journal: Applied Economics*, 6(4):251–285.

Cohodes, S. R., Ho, H., and Robles, S. C. (2026). Diversifying the STEM pipeline: Evidence from STEM summer programs for underrepresented youth. *Journal of Human Resources*.

College Scorecard (2016). College Scorecard Institution-Level Data.

College Scorecard (2021). College Scorecard Institution-Level Data.

Dale, S. B. and Krueger, A. B. (2014). Estimating the Effects of college characteristics over the career using administrative earnings data. *The Journal of Human Resources*, 49(2):323–358.

Goodman, J., Hurwitz, M., and Smith, J. (2017). Access to 4-Year public colleges and degree completion. *Journal of Labor Economics*, 35(3):829–867.

Griffith, A. L. (2010). Persistence of women and minorities in STEM field majors: Is it the school that matters? *Economics of Education Review*, 29(6):911–922.

Hatfield, N., Brown, N., and Topaz, C. M. (2022). Do introductory courses disproportionately drive minoritized students out of STEM pathways? *PNAS Nexus*, 1(4):pgac167.

Hoekstra, M. (2009). The Effect of attending the flagship state university on earnings: A discontinuity-based approach. *Review of Economics and Statistics*, 91(4):717–724.

Imbens, G. W. and Rubin, D. B. (1997). Estimating outcome distributions for compliers in instrumental variables models. *The Review of Economic Studies*, 64(4):555–574.

Kling, J. R., Liebman, J. B., and Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, 75(1):83–119. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.1468-0262.2007.00733.x.

Kozakowski, Whitney (2023). Are four-year public colleges engines for economic mobility? Evidence from statewide admissions thresholds. Technical Report 23-727, Annenberg Institute at Brown University.

Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies*, 76(3):1071–1102.

Marbach, M. and Hangartner, D. (2020). Profiling compliers and noncompliers for instrumental-variable analysis. *Political Analysis*, 28(3):435–444.

Mountjoy, J. (2026). Marginal returns to public universities. *The Quarterly Journal of Economics*, 141(1):429–497.

National Science Board (2024). The State of U.S. Science and Engineering 2024. Technical report, National Science Foundation.

Riegle-Crumb, C., King, B., Grodsky, E., and Muller, C. (2012). The more things change, the more they stay the same? Examining gender equality in prior achievement and entry into STEM college majors over time. *American Educational Research Journal*, 49(6).

United States Department of Homeland Security (2022). DHS STEM Designated Degree Program List. Technical report.

U.S. Department of Education's National Center for Education Statistics (2023). Integrated Postsecondary Education Data System (2010).

Zimmerman, S. D. (2014). The Returns to college admission for academically marginal students. *Journal of Labor Economics*, 32(4):711–754.