Diversifying the STEM Pipeline: Evidence from STEM Summer Programs for Underrepresented Youth *

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September 2022

Abstract

Underrepresentation of Black and Hispanic workers in STEM fields contributes to racial wage gaps and reduces innovation and economic growth. Billions of dollars a year are spent on "pipeline" programs to increase diversity in STEM, but there is little rigorous evidence of their efficacy. We fielded a randomized controlled trial to study a suite of such programs that are targeted to underrepresented high school students hosted at an elite, technical institution. Students offered seats in the STEM summer programs are more likely to enroll in, persist through, and graduate from college. The programs also increase the likelihood that students graduate with a degree in a STEM field, with the most intensive program increasing four-year graduation with a STEM degree by 33 percent. The shift to STEM degrees increases potential earnings by 2 to 6 percent. Program-induced gains in college quality fully account for the gains in graduation, but gains in STEM degree attainment are larger than predicted based on institutional differences.

*We are grateful to staff of and applicants and participants in the summer programs that are the subject of this paper, as well as the host institution and its institutional research office. Because the institution is anonymous we unfortunately cannot individually thank the many people who supported this project by name. Their commitment was instrumental to this experiment, as was feedback and suggestions from Joshua Angrist, Lawrence Katz, and Amanda Pallais. Rebecca Johnson, Stephanie Owen, and seminar and conference participants at APPAM, AEFP, Colby College, the Opportunity and Inclusive Growth Initiative at the Minneapolis Federal Reserve Bank, and the University of Michigan provided helpful comments. Excellent research assistance came from Elizabeth Huffaker, Taylor Myers, Marisa Morin, and Katharine Parham. We thank the Alfred P. Sloan Foundation, the Teachers College Provost's Investment Fund, the National Center for Institutional Diversity at the University of Michigan, the Lab for Economic Applications and Policy at Harvard University, and the National Science Foundation for financial support. The National Bureau of Economic Research; Teachers College, Columbia University; Blueprint Labs; Harvard University; and the University of Michigan all provided institutional support. This study is registered in the AEA RCT Registry and the identifying number is: AEARCTR-0002888.

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1 Introduction

Black and Hispanic workers are underrepresented in the high-wage, college degree-holding science, technology, engineering, and mathematics (STEM) workforce (National Science Board, 2021). The lack of diversity in STEM fields contributes to racial and ethnic wage gaps (Altonji et al., 2016), reduces innovation quality (Parrotta et al., 2014; Hofstra et al., 2020; Yang et al., 2022), and dampens economic growth (Cook et al., 2021; Hsieh et al., 2019).[1] Billions of dollars are spent each year on STEM pipeline programs to correct racial disparities in STEM fields, but we know little about the efficacy of such programs.[2]

Increasing STEM college degree attainment among underrepresented minorities (URM) is necessary to ultimately diversify the STEM workforce. About 9 percent of STEM bachelor's degrees went to Black students and 16 percent to Hispanic students despite these groups representing 14 and 21 percent of the college-age population in the United States, respectively (National Science Board, 2022). The disparity in STEM degrees is not due to differences in interest. Upon entering college, URM students plan to major in STEM fields at similar rates to their White peers, but they are more likely to switch away from a STEM field or leave college (Riegle-Crumb et al., 2019), suggesting that in-college experiences are important factors in STEM degree attainment. In particular, access to well-resourced colleges and STEM preparation could help students persist in college and in a STEM major.

Given that access to college and preparation for in-college STEM experiences are shaped prior to college entrance, STEM-focused enrichment programs for high school students are promising vehicles to reduce disparities in STEM degree attainment and STEM workforce participation. However, the efficacy of such programs on long-term STEM persistence is unknown. The existing evidence primarily relies on survey assessments and on observational studies whose findings can be substantially driven by selection bias and often focus only on short-term outcomes (Kitchen et al., 2018a; Kitchen et al., 2018b; Bradford et al., 2021).[3] An exception is Robles' (2018) prior investigation of one of the most intensive of the three programs we examine here. Using long-term

[1]The STEM wage premium likely reflects selection into STEM fields by individuals with high earning potential, but STEM earnings premiums remain even when controlling for student backgrounds (Altonji et al., 2012, 2016) or estimating returns within a discrete choice model (Arcidiacono, 2004; Kinsler and Pavan, 2015).

[2]In 2011, there were over 250 federal programs and $3.4 billion invested in the STEM pipeline (Granovskiy, 2018), including over $1 billion in funding through the National Science Foundation (NSF) with the specific goals of increasing diversity and representation (see NSF budgets for "Broadening Participation" efforts here: `https://www.nsf.gov/od/broadeningparticipation/bp_investments.jsp`.) About three-quarters of federal investment supports undergraduate and graduate education and training, with less support for K-12 initiatives (Authors' calculations from Granovskiy (2018)).

[3]Some summer programs to increase representation in STEM-adjacent fields have been rigorously analyzed and found to increase representation in their focus areas, including the American Economic Association Summer Program (Price, 2005; Becker et al., 2016) and the Robert Wood Johnson Foundation Summer Medical and Dental Education Program (Cosentino et al., 2015). However, although these evaluations are more rigorous about comparisons to non-program students than many other studies, they are not randomized trials and they focus on STEM-adjacent fields (e.g., economics, health professions). This work generally finds that program participation leads to greater success in the focus field.

administrative data on earlier cohorts and a selection-on-observables design, she found that access to a six-week, residential STEM summer program increases matriculation at the host institution, graduation rates, and likelihood of graduation with a STEM degree. However, this study could not fully account for selection into the program. Thus, to better understand whether STEM programs for high school students can be successful tools to diversify STEM fields, we conducted a randomized controlled trial of a suite of summer programs targeted at enhancing the pipeline of underrepresented students in STEM degrees and careers, following students from their application to the programs through college degree attainment.

This study provides the first evidence from a randomized controlled trial on the impact of a STEM-focused summer program on college matriculation, completion, and graduation with a STEM degree. It is also one of the few RCTs of *any* intervention to diversify the STEM educational pipeline. Three cohorts of high-achieving, STEM-interested students were randomized to three STEM-focused programs and a control group in the summer between their junior and senior years of high school in 2014, 2015, and 2016, prior to college application. The programs were held at the Host Institution (HI), an elite technical university in the Northeast. They differed in their modality and intensity: six weeks full-time on-site, one week full-time on-site, or six months with periodic meetings online. Students were selected into the randomization pool based on their academic preparation as well as a holistic assessment of need that included whether they had backgrounds that were underrepresented in STEM fields. The six-week program was held on the HI campus and offered a shortened version of the HI's freshman curriculum, along with college counseling, field trips, introductions to role models in STEM fields, and a college-like living experience. The one-week version of the program offered a short, intensive course in a STEM field and an abbreviated version of other aspects of the six-week program. Finally, the online version of the program offered a six-month engagement in STEM enrichment activities, with online speakers and interactions and a short "conference" visit to the HI campus over the summer.

The STEM summer programs do not increase first year college enrollment: almost all students in the control group (87 percent) attend a four-year college immediately after high school graduation, and the programs increase this by a small amount (2 to 4 percentage points, and not statistically significant). The programs do induce large, statistically significant shifts toward attending more competitive schools: for the six-week on-site program, this operates entirely through the HI; for the one-week and online programs, enrollment in the most competitive institutions is split between the HI and other elite schools, though some of the effects are not statistically significant. The effects of the STEM summer programs on college enrollment become more pronounced as students progress through university. By the fourth year of college, those offered a seat in any of the three STEM summer programs are 3 to 12 percentage points more likely to still be enrolled in a four-year college. The effect is largely drive by control group reductions in college attendance (to 75 percent) and treatment group students' persistence in Barron's top ranked colleges, including the HI.

The programs' effects on college persistence translate to increased on-time college graduation. Only 53 percent of students in the control group graduate within four years from any four-year school, despite being an academically talented group. The STEM programs increase this by 8 percentage points (six- and one-week on-site programs) and 1.6 percentage points (online program) though not all the differences are not statistically significant. Again, most of the gains for the six-week program operate through increased graduation from the HI; for the one-week and online programs, college graduation increases are shared by the HI and other highly ranked institutions. Graduation impacts are larger and statistically significant with a five-year window for graduation, but this may be due to sample composition as graduation information is only available for two of three cohorts due to a shorter time horizon.[4]

Degree gains are entirely in STEM fields, reflecting both an overall increase in the number of degrees and a shift to STEM fields among graduates. In the control group, 34 percent of students graduate within four years with a STEM degree—64 percent of degree recipients. The six-week program increases the rate at which students graduate with a STEM degree to 50.7 percent, 46.8 percent for the one-week program, and 37.2 percent for the online program (the latter is not significant). Much of the shift to STEM occurs with degrees at the HI, but both the six-week and one-week programs increase receipt of STEM degrees at non-HI institutions as well when looking at five-year graduation, though these differences are not statistically significant. Degree gains for the online program are split between an increase in STEM degrees at the HI and an increase in non-STEM degrees at other institutions. The shift in composition of majors toward STEM induces potential earnings increases of 2 to 6 percent, which is likely an underestimate of the programs' effects on potential earnings because it only accounts for changes in majors, not an increase in graduation or selectivity of institution.

We find evidence that the programs' effect on degree completion is due to the shift in institutional quality that they induce. The increases in overall graduation are the same as what would be predicted by the shifts in institutional quality, as measured by institution-level graduation rates. The programs potentially achieve upgrades in institutional quality by improving information students have about colleges and the college application process. We use survey data to explore other mechanisms, such as improvements in study and independent living skills and high school preparation but find that college quality likely plays the greatest role in explaining graduation effects.

This paper makes three main contributions. First, we add to the evidence on STEM degree attainment as well as diversity among STEM degree holders. Most research on STEM degree production focuses on what happens during college, concentrating on the gender or race match between students and instructors or peers (see, for example, Bettinger and Long (2005); Hoffmann and Oreopoulos (2009); Griffith (2010); Carrell et al. (2010); Bettinger (2010); Price (2010); Fairlie

[4]Table 3 presents positive six-year graduation effects as well, but they reflect only a single cohort, so the discussion does not emphasize them. Online Appendix Table B.3 shows how graduation effects fluctuate across cohorts.

et al. (2014); Fischer (2017); Griffith and Main (2019), student beliefs in their own capability and signals from grades (Astorne-Figari and Speer, 2019; Kaganovich et al., 2021; Owen, 2020; Kugler et al., 2021; Owen, 2021) and institutional effects (Griffith, 2010; Arcidiacono et al., 2016). Less attention has been paid to the preparatory experiences that may shape college attendance and major choices, despite the potential influence of pre-college experiences on STEM degree attainment (Sass, 2015; Green and Sanderson, 2018). The few studies on high school STEM exposure find differing effects on STEM major choices and degree attainment. In the United States, Darolia et al. (2020) found that exposure to more STEM courses in high school does not increase STEM degree attainment in college, while De Philippis (2021) found that in the United Kingdom, such exposure increases the likelihood of male students majoring in STEM, and in Denmark, Joensen and Nielsen (2016) found an increase only for female students. Although the differences in these findings may be due to differences in context, it is also possible that broad programs that do not specifically focus on underrepresented students or do not affect college applications may have little effect.

Second, we contribute to the understanding of access to college, the match between student preparation and institutional quality, and the potential for college education to reduce racial economic inequality in the United States. There are large gaps in college enrollment by family income in the United States (Bailey and Dynarski, 2011; Chetty et al., 2020; Dynarski et al., 2021). And, in addition to whether students enroll in college, there are differences in the type and quality of institution they enroll in (Baker et al., 2018; Gerber and Cheung, 2008). College enrollment and selectivity trail even for high-achieving, underserved students (Hoxby and Avery, 2013; Dillon and Smith, 2017) resulting in "undermatch," that is, when students who could succeed at selective institutions do not apply (and thus cannot enroll). The college a person attends can influence the likelihood that the student graduates (on time), the likelihood the student graduates with certain degrees, and the student's future employment and earnings (Hoekstra, 2009; Cohodes and Goodman, 2014; Zimmerman, 2014; Goodman et al., 2017; Chetty et al., 2020; Bleemer, 2021). Interventions prior to college application can influence enrollment and the specific institution students enroll in (Avery, 2010, 2013; Carrell and Sacerdote, 2017; Castleman and Goodman, 2018; Andrews et al., 2020; Dynarski et al., 2021). Similar to effective college counseling and informational interventions that modify college application and enrollment behavior, the STEM summer programs we examine happen at a crucial time: students are seriously considering college but have not yet applied. However, the STEM summer programs we focus on differ in their intensity and focus on STEM.

Finally, this paper is relevant to a large literature on the impacts of affirmative action (or lack thereof) in college admissions. The STEM summer programs do not introduce group-based preferences in college admissions—policies that are typically the focus of the affirmative action literature in economics. They do, however, focus on populations exhibiting a broad definition of need that includes identifying with historically underrepresented groups, and aim to increase access to STEM fields and elite universities. However, much of the literature on affirmative action is concerned with "mismatch"—the idea that URM students will be unprepared for the academic rigor of campuses with affirmative action preferences and thus might be made worse off by such policies (see Arcidiacono and Lovenheim (2016) for an overview). Although Arcidiacono et al. (2016) found some evidence of mismatch, Bleemer (2022) found college and earnings benefits for URM students induced to attend more selective University of California campuses due to affirmative action. Our work adds to the evidence that when URM students are induced to attend high-quality institutions, they reap the benefits of those institutions and are successful, in contrast to the predictions of mismatch theory, though we note that the study sample has significant academic preparation for college.

The paper proceeds as follows. Section 2 describes program background and context, including more details on the interventions; Section 3 details the data; and Section 4 explains the study design and estimation methods. Results are reported in Section 5, with a discussion of potential mechanisms in Section 6. Section 7 concludes.

2 STEM summer programs at the Host Institution

The HI maintains an office devoted to outreach programs to increase representation of URM students in STEM fields; we refer to this unit as the "outreach office." Programming includes outreach to the local community with initiatives designed for elementary and secondary students, as well as national summer programs for high school juniors. The summer programs are the focus of this study. The aim of the programs is to diversify the STEM workforce and increase access to STEM careers by exposing students to high-achieving peers, STEM mentors, STEM curriculum, tours of a college campus and research facilities, and college admissions information. Recruitment is national. All programs cover student costs except for transportation to and from the HI. The programs are funded by the HI, with some funding due to earmarked charitable gifts. High-achieving students in any geographic region can be recruited, as long as they are U.S. citizens or permanent residents. One source of student information used in direct mailings for recruitment is the PSAT, though test scores are not a prerequisite for admission.

We describe each summer program below as it existed in the summers of 2014-2016, the period over which randomization occurred. All of the outreach office's programs offer similar experiences that are designed to promote persistence in STEM fields, but the intensity and modality of the experiences vary.

1. *Six-week program*: The six-week program is the longest-running summer program of the three studied. It is a residential program that immerses rising high school seniors in rigorous science and engineering classes. Students take courses in math, physics, life sciences, and

humanities, as well as a STEM-related elective course with topics ranging from digital design to genomics. In addition, students take tours of labs and work spaces at the HI; attend workshops with leaders of industry and academics and admissions officers; and interact with teaching assistants who are current college students. Students also visit STEM-focused companies and workplaces. The program encourages social cohesion by bringing students together to live in dorms at the HI and leading team-building exercises. About 80 students are offered a seat in this program each year.

2. *One-week program*: The one-week program encapsulated some aspects of the six-week program in a shorter time frame and was also a residential program. Over one week, students completed a project course in an engineering field; attended admissions and financial aid sessions; toured labs; met with HI faculty, students, and alumni; and participated in social events. The time constraint necessarily reduced the dosage of all aspects of the six-week program, though to what extent outcomes are sensitive to this reduction is an empirical question. Typically, 75 to 120 students participated in this program each year[5].

3. *Online program*: The online treatment draws on communications technology to serve students. The six-month program provides a platform for multimedia interaction between students and instructors, staff at the HI, and industry leaders. HI students are hired to mentor small groups of participants and lead discussions. The online summer program provides top-down content in the form of videos, articles, or webinars. Students must also complete project-based engineering assignments. The forum and discussion groups provide user-generated (and instructor facilitated) content. Finally, students spend five days on campus presenting their final projects, attending workshops, and meeting their classmates in person. The campus visit occurs five weeks into the online experience, which lasts until the end of the calendar year. About 150 to 175 students participate in the online program. The summers we study occurred well before the COVID-19 pandemic, but the technology platform used for the online program facilitated a transition to digital learning for all of the summer programs in COVID-affected years.

4. *Control condition*: Students assigned to the control condition also applied to the HI's summer programs but were not randomly assigned to participate in any programs offered by the outreach office. However, these applicants are generally also accomplished students (typically in the top third of applicants to the summer programs as a whole). Being assigned to the control group does not mean that an applicant has no exposure to STEM focused programming. Many students in this group participate in alternative summer programming, both STEM-focused and otherwise, such as programs administered by other universities or

[5]This program is no longer operating.

organizations like Girls Who Code or Leadership Enterprise for a Diverse America. However, many also work or study over the summer in lieu of specialized programming.

3 Data and descriptive statistics

This study measures the effectiveness of STEM summer programs for high-achieving, underrepresented high school students using data from a randomized experiment of admission to these programs for the summers of 2014, 2015, and 2016. Below, we detail the data sources used, which include records from the outreach office on summer program application and admission, records of college attendance and graduation, and survey data, and also describe the characteristics of program applicants.

3.1 Data

Data for our key analyses come from two main sources: program application and admissions information from the HI and college attendance and enrollment information from the National Student Clearinghouse (NSC) (Cohodes et al., 2022). All applicants in the three cohorts from 2014 to 2016 were admitted via conditional random assignment, and the assignment and randomization process was jointly created by the research team, program staff, and the HI institutional research office to meet both research and operational needs. Background information on the randomized sample comes from program applications which include demographic information, academic qualifications, and essays, as well as a baseline survey. The outreach office provided details on offers of admission and which students ultimately participated in the programs, as well as details on ratings of applicants' files and details about the admissions process.

Outcome data come from records of college enrollment provided by the HI institutional research office and the NSC; the former also provides information on applications and majors at the HI. Almost all of the applicant pool appears in the HI or NSC college enrollment data, and since all students' information was shared with the NSC and HI for matching to enrollment data, there is no differential attrition in the possibility of appearing in the college data (see Online Appendix Table A.13). College outcomes include graduation as of spring 2021, included in both the HI and NSC data, which means we have the potential to observe four-year college graduation for all cohorts, five-year college graduation for the first two cohorts, and six-year graduation for the first cohort. The NSC data also included information on students' majors, which we categorize as either a STEM field or not.[6] Figure 1 shows data availability and progress through college for each of the three

[6] Due to lack of reporting to the NSC, information is missing for 12 to 15 percent of degree recipients depending on the time horizon. To address this, we assume that bachelor of science degrees represent STEM majors for those missing information on majors. We also explicitly display results for rates of missing degree information, as well as show upper and lower bounds from categorizing all missing degrees as non-STEM or STEM, respectively. The HI always reports degree field. See Table 4 for details.

cohorts, assuming on-time progression.

3.2 Surveys

The study also collected periodic survey data from the study sample. Longer surveys were conducted (1) in the fall shortly after the program summer, (2) May of students' senior year of high school, and (3) in the spring of students' sophomore year of college. Shorter, more frequent surveys kept track of college enrollment and students' ultimate or intended college major. Respondents received Amazon gift cards if they participated (not contingent on answering all questions or on treatment assignment), with larger incentives for participating in the longer surveys ($25) and smaller incentives for participating in the shorter surveys ($5).

The survey at the end of the summer program included questions about college plans, knowledge of the application process, intended major, and study and life skills. The survey in May of senior year collected information on college application and admission and fall plans. The final long-format survey at the end of sophomore year in college asked about college experiences, majors, and career intentions. More details on these surveys are in Online Appendix C. When a survey included multiple items on similar topics, we constructed standardized indices of outcome measures from the surveys by "family" of outcomes using the method in Anderson (2008) to minimize concerns about multiple hypothesis testing.

Response rates were relatively high but declined over time and were lower for the control group. Online Appendix Table A.13 shows response rates for each survey. For example, for the first long follow-up survey in the fall after program participation, treatment groups' response rates ranged from 85 to 90 percent; 65 percent of the control group responded to this survey. Differences in response rates are not surprising, given that treatment students were more likely to have positive associations with the program office or be enrolled in the HI, which was the entity sending out the surveys.

To assess the representativeness of the survey response sample, the analysis compares program impacts on college attendance and graduation between the survey sample and the full sample both with and without inverse propensity-to-respond weights (see Online Appendix Table C.1). Impacts on attendance and graduation are generally quite similar when restricted to the sample of survey respondents, perhaps a little larger for survey respondents. Adding inverse propensity weights based on demographic characteristics and program assignment has an inconsistent impact on the treatment effects. In some cases, it makes the estimates of college impacts for survey responders look more similar to those of the full sample than those of the unweighted respondents; in others, it makes the estimates for survey respondents look more divergent from those from the full sample. Thus, it is not clear that propensity to respond predicts program effects in a meaningful way. Following Dutz et al. (2021), we use unweighted survey responses but caution that the sample is not fully representative; thus, we consider findings based on the surveys to be suggestive.

3.3 Descriptive statistics

Table 1 reports demographic and background academic information for the randomized sample. As described below, randomization included design strata, so we do not expect treatment and control groups to be exactly similar on all characteristics, though we show in Columns 6 through 8 that the treatment group has fewer strata-adjusted differences than the control group.[7] Overall, almost all randomized applicants identify as a member of a group underrepresented in higher education and STEM fields, with 35 percent of the sample identifying as Black, 43 percent identifying as Hispanic, and 4 percent identifying as Native American (note that these categories are overlapping as students were able to report more than one race or ethnicity). About one-quarter of the group are first-generation college students, which we define as having no parents who ever attended a four-year college. Program applicants have strong academic backgrounds. The average grade point average (GPA) is 3.86 on a four-point scale, and the average standardized math test score is two standard deviations above the national mean.[8] The largest contrast between treatment and control groups is by gender. The outreach office seeks to host programs equally split between young women and men, but the applicant pool skews male. Thus, the randomization strata include gender.

4 Research design

STEM summer program applicants were randomized to receive an offer to participate in one of the three summer programs or were randomly assigned to a control group. This section describes the process through which applicants were randomized and how the analysis estimates program effects based on that randomization process.

4.1 Selection

Selection into the programs is a multi-step process, described in more detail in Online Appendix A. Each year, after an initial screening, the outreach office sent approximately 600 to 750 highly qualified applicants to a selection committee made up of stakeholders, community members, and affiliates with long-standing ties to the outreach office. The selection committee ranked applicants in terms of suitability for the six-week program and provided detailed scores for academic preparation and personal circumstances, based on grades, test scores, letters of recommendation, and application

[7] For more details on covariate balance, see Online Appendix Tables A.14 through A.16, which show that within strata, there are few differences by background characteristics across treatment and control conditions *within* randomization blocks.

[8] Applicants to the STEM summer programs submit standardized test scores from various exams, the most common being the PSAT. We use information on national test score means and standard deviations to convert each test score to a z-score, which allows us to combine across several standardized exams including the PSAT, SAT, ACT, and PLAN. To compare to the HI, we standardized the HI's 25th percentile of the math SAT in the same time period as the STEM summer program experiment, which was 740, or 1.91 in standard deviation units. Thus average applicant scores of program applicants were in line with scores of incoming students at the HI.

essays. In addition, because the mission of the programs is to promote access to STEM for traditionally underrepresented populations, the selection process included consideration of the following factors on a holistic basis, though no element in isolation guaranteed admission:

1. The applicant would be the first in the family to attend college
2. The applicant's family did not have science and engineering backgrounds
3. The applicant's high school historically sent less than 50 percent of its graduates to four-year colleges
4. The applicant attended a high school that presented challenges for success at an elite, urban university (e.g., rural high school or a high school with a predominantly URM population)
5. The applicant was a member of a group that is underrepresented in the study and fields of science and engineering (African American or Black; Hispanic or Latino/a; or Native American)

Additionally, the outreach office requested regional priorities to increase representation across the country and these entered into the rankings in 2015 and 2016. In 2014, the outreach office exempted several applicants from randomization and offered admission to these applicants to support national representation; we call these cases "certainty spots," and they are excluded from the analytic sample. The HI institutional research office performed the final program assignment by lottery after all students were ranked, creating a ranking variable that was a weighted average of applicant ratings and regional priorities; the rankings were used to allocate students to random assignment blocks as described in Section 4.2 below. Notably, because randomization occurs in a group designated as top applicants from a pool of more than 2,000 applicants, program effects estimated here may not apply to all applicants or to others outside the selective applicant pool. The program, if offered to any high school senior, might have very different (or no) impacts on those less academically prepared.

4.2 Randomization

The selection process above created a pool of 600 to 750 applicants eligible for randomization each year. Because the outreach office wanted to ensure that top ranked students had access to one of the summer programs, the study employed a block randomization design. The HI institutional research office placed students into randomization blocks based on the rating variable that took into account application ratings and regional priorities. An overview of the randomization scheme is in Figures 2a-2c. Details on the randomization process for each cohort are in Online Appendix A.

In general, the highest-ranked students were placed in Block 1 and randomized between the three summer programs. To maintain gender balance in the program, there were different rating score cutoffs for male and female students. Additionally, to ensure students in Block 1 were prepared to take on the rigorous coursework in the six-week program, a math test score floor was imposed for assignment to Block 1. The remaining students were placed in Block 2, and were randomly assigned between the online program and a control group. The size of the blocks and assignments to programs varied by year based on operational considerations. This randomization scheme formed the research design in cohorts 2 and 3 of the experiment. Cohort 1, in 2014, underwent a slightly different design, where students were differentiated to a greater extent and randomly assigned within three blocks. Results are very similar including and excluding the first cohort (see Online Appendix Table A.6). We include the 2014 cohort to increase statistical precision, despite the slightly different underlying randomization structure, which we account for with randomization strata.

The crucial component of our randomization design is the overlap of the online program across the blocks, which we use to extrapolate comparisons between Block 1 programs and the control group in Block 2. The key assumption behind this extrapolation is that we assume that if we control for randomization strata (based on application year, gender, block, and regional preferences), we fully account for differences across the blocks and can compare applicants assigned to Block 1 (which has no control group) to those in Block 2 (which has a control group). By design, we can fully account for membership in block with known variables. We test this assumption in multiple ways, including controlling for the rating variable directly, which we describe below after presenting our main estimation strategy.

4.3 Estimation

We use random assignment to program offers to estimate the causal effect of assignment to one of the three STEM summer programs, as follows:

$$Y_i = \beta_1 6week_i + \beta_2 1week_i + \beta_3 Online_i + \sum_{j=1}^{J} \delta_j R_{ij} + X_i' \gamma + \epsilon_i \tag{1}$$

where Y_i is an outcome of interest for applicant i, such as enrollment in a top ranked college, and $6week_i$, $1week_i$, and $Online_i$ are indicators for random assignment to an offer of treatment for each of the three programs. The parameters of interest are the β coefficients, which reflect the intent-to-treat estimate of assignment to one of the three programs. Most students attend their assigned program if offered a spot, with 87 percent of students accepting a seat at the six-week program, 85 percent at the one-week program, and 77 percent at the online program (Online Appendix Table A.2). Very few students end up participating in a different program, and the outreach office did not offer any spots to students in the control group.[9] A vector of student-level control variables, X_i, including GPA, standardized math scores, race/ethnicity, and free and reduced-price lunch status, increases precision. Gender is accounted for in the randomization strata described below. We use heteroskedasticity robust standard errors.

Key to our estimation strategy is the inclusion of a set of control variables, or risk sets, R_{ij}, which are indicators representing randomization strata. Randomization strata represent gender, regional preferences,[10] and randomization block, and are formed within randomization year. Offers are randomized within these strata. Students are assigned to randomization blocks based on a rating variable and a standardized test score floor. The rating variable includes ratings by a selection committee, assessment by the HI admissions office, and prioritization for certain regions and states of the country (typically to make sure that participants are broadly representative of the United States as a whole). Our estimation method compares students within the same cohort, gender, regional preference, and randomization block. We show in Table 1, Columns 6 through 8, that once we control for randomization strata there are few differences in student characteristics. Online Appendix Tables A.14 through A.16 show that demographic characteristics are balanced within randomization strata.

The fundamental assumption behind our randomization strategy is that once we control for randomization block, we are controlling for all differences across blocks, and can compare students randomly assigned to a treatment group to those in the control group, even when we do not have a direct treatment-control contrast in the same block. Including the online program in both randomization blocks provides the link that makes it possible to estimate this comparison. Because assignment to block is based completely on known, observable variables, controlling for randomization block should control for all differences between the two groups.

Nevertheless, because this differs from complete random assignment, several alternative estimation strategies support the interpretation that the estimates represent causal effects, detailed in Section 5.5. Specifically, we verify this strategy is successful by showing our results controlling for the rating variable in lieu of the block-based randomization strata, which shows that the blocking strategy fully accounts for the factors that determine assignment to randomization group, removing selection bias. Because the blocks are constructed out of known parameters and the analysis fully accounts for them, the modified random assignment structure comes close to estimates under complete random assignment. Additionally, we conduct a bounding exercise to consider the degree of selection on unobservables that would have to be included in our blocks to render our estimates null, using the techniques from Oster (2019). In short, there would need to be a very high degree of selection on unobservables to drive our estimates to zero.

[9] Assignment closely parallels participation, and our estimates will be quite close to treated-on-the-treated impact estimates. Nevertheless, we also instrument program attendance with program assignment and present treatment-on-the-treated estimates, as well (Online Appendix Tables B.4 andB.5).

[10] These are geographic preference indicators for regions of the country that are preferred, neutral, or down-weighted.

There is another potential threat to the validity of the modified randomization scheme: If there are differential returns to the program for different types of applicants—that is, if there are heterogeneous treatment effects—our results may not be fully representative because the control group only includes relatively lower-ranked students. Online Appendix A.1.6 explores this concern and shows little evidence of heterogeneous treatment effects.

Section 5.5 presents several additional robustness checks. Because randomization possibilities are constrained to only those within the strata discussed above (i.e., complete randomization is not possible), we also present our estimates using randomization inference to account for the possibility that only a subset of potential outcomes are possible given the constraints of our design (see Athey and Imbens (2017) for a discussion of this). Specifically, we rerun our randomization scheme 1,000 times, subject to the same rules and constraints as in the actual randomization. We then compare our point estimate to the distribution of estimates generated by random assignment. As in a Fisher's exact test, if the point estimate exceeds the 97.5th percentile of the distribution of randomized estimates (or 95th percentile with a one-tailed test), we consider that estimate to be different from zero.

5 Results

This section details how the programs increase both college enrollment and college graduation and shift the type of institution that students attend to higher-quality colleges. The programs increase enrollment in and graduation from the HI as they were designed to do. However, it could be the case that the programs are simply shifting enrollment and graduation from similar institutions to the HI. We explore the impact of a STEM summer program offer on enrollment and graduation from four-year colleges more broadly, as well as by college quality and type. Because the NSC, which tracks enrollment and graduation, is the data source for this analysis, we can only examine college attendance and graduation, not application and admission.[11] All of our outcomes are based on time windows since high school graduation. For example, five-year college graduation reflects college completion within five years of high school graduation (six years after the summer of the programs), not five years since college entrance.

5.1 College Enrollment and Persistence

The STEM summer programs induce a small, positive increase in college enrollment, and large shifts in institution type. The programs shift students to the HI and other elite institutions. As time goes on, we observe that students offered a seat in any program are more likely to remain enrolled throughout college and to graduate. These increases in enrollment and graduation are

[11]We discuss application and admission more in Section 6.

concentrated at the HI and other elite institutions, indicating that the programs induce a college upgrade, regardless of whether students attend the HI.

Figure 3 shows attendance at and graduation from four-year institutions, split between the HI (darker bars) and other four-year institutions (lighter bars), with a panel for each program. Table 2 shows the same estimates for college attendance as well as information about attendance at elite institutions, as designated by Barron's college ratings. Table 3 shows the estimates for graduating college in four-, five-, and six-years. Because the time horizon differs for the cohorts, the graduation outcomes have different samples: we observe four-year graduation for all cohorts, five-year graduation for the 2014 and 2015 cohorts, and six-year graduation for the 2014 cohort alone. For four-year colleges and Barron's most competitive colleges, we show those groups both with and without the HI included.

Almost all students in the study sample attend four-year college immediately after high school graduation (one academic year after the program summer), with 87 percent of the control group enrolling in the first year (Column 2 of Table 2). The remaining students either enroll in two-year institutions, join the military, or work. Attending one of the STEM summer programs has positive, but not statistically significant, impacts on attendance (Column 2). Column 3 shows attendance at four-year institutions other than the HI and demonstrates the programs draw students to the HI from other institutions. For the HI in particular (Column 1), 8 percent of the control group enroll. The offer of the six-week program increases enrollment by 17 percentage points, with 25 percent of students in this group enrolling. The one-week and online programs also increase HI enrollment, by 5 and 4 percentage points, respectively. The estimate for the one-week program is not statistically significant, despite being slightly larger than that of the online group.

The increases in immediate enrollment and shifts in institution reflect shifts to high-quality colleges. The HI is an elite institution, and all three programs shift enrollment there, with the largest gains for the six-week program (Column 4). Enrollment at any of Barron's most competitive institution, including the HI, sees a statistically significant gain of 17 percentage points for the six-week program, a 14 percentage point gain for the one-week program, and a 10 percentage point gain for the online program, as shown in Columns 4 and 5. For the six-week program, the institutional upgrade offered by the STEM summer program operates through the HI, as there is no difference in initial enrollment at other highly ranked institutions besides the HI (Column 5). However, the one-week and online programs increase enrollment both at the HI (by 4 to 5 percentage points, Column 1) *and* other non-HI institutions rated as most competitive by Barron's (by 6 to 8 percentage points, Column 5). Thus, for the one-week and online programs, institutional upgrades are split between the HI and other elite institutions. However, these enrollment differences are generally not statistically significant. Effects on attendance in the second and third years of college are generally similar to the initial enrollment effects (Panels B and C of Table 2). However, by the fourth year of college, the STEM summer programs show an even greater edge.

In the fourth year after high school graduation, there are positive, statistically significant impacts on enrollment in any four-year college (Column 2). Students assigned to a summer program maintain enrollment in their fourth year, whereas control group students are less likely to be enrolled. The differences in enrollment reflect a combination of control group students dropping out, taking time off, or delaying. In the first year of college, 87 percent of control group students were enrolled; by the fourth year control group enrollment falls to 75 percent. The drop for control students is primarily coming from non-HI institutions: in the third year, 74 percent of control group students are enrolled in a non-HI, four-year institution, but by the fourth year this drops to 68 percent. Persistence in the fourth year is particularly high at the HI for all summer programs (Column 2). For non-HI institutions, the one-week program induces a 6 percentage point increase in attendance in the fourth year (Column 3) and a 12 percentage point increase at Barron's most competitive colleges besides the HI (Column 5). Additionally, the six-week program also appears to support persistence into the fourth year at non-HI institutions, as the negative impacts on attendance get smaller in the fourth year relative to earlier years (Column 3 and Column 5) but there is not a commensurate increase at the HI (which could indicate transfers across schools as opposed to differences relative to the control group in overall enrollment). By sustaining persistence through the first four years of college, the STEM summer programs set the stage for college graduation.

5.2 College Graduation

Only 53.2 percent of control group students graduate from a four-year college in four years (Table 3, Column 2). This is higher than the national four-year graduation rate observed at all U.S. institutions of 45.3 percent (U.S. Department of Education, 2020), but is low considering the students in this sample have near-perfect GPAs and standardized test scores two standard deviations above the country-wide mean. Additionally, the schoolwide four-year graduation rate at the HI for the same cohort is 87 percent. Both the six-week and one-week programs increase the likelihood that a student graduates from any institution within four years by about 8 percentage points. For the six-week program, much of the increase in four-year graduation comes from the HI, but the gains for the one-week program are split fairly evenly between the HI and other institutions. The online program increases graduation from the HI by 3.3 percentage points, with a drop at other institutions of 1.6 percentage points for an overall small gain. Column 4 of Table 3 shows that there is an overall increase in graduating with a bachelor's degree in four years from any highly-ranked institution including the HI by 5 to 12 percentage points, though this is imprecisely measured.

For five-year graduation, the STEM summer programs maintain or increase their gains in comparison to the control group (however, these estimates reflect one less cohort). Control group graduation rates increase over time, but all three programs make it more likely that a student graduates with a degree within five years; and, in the case of the one-week and online programs, these gains are driven by non-HI institutions, especially highly-ranked institutions (Column 4). We show six-year graduation impacts but note these findings rely on a single cohort. The one-week and online programs continue to have an edge in six-year graduation, again driven by non-HI, highly-ranked institutions, but the six-week program shifts where (and when) students get their degree, not whether they do so. However, the six-year graduation rates are only available for the 2014 cohort, so some of these findings may be due to idiosyncratic differences between cohorts.[12]

Online Appendix Table B.6 further explores where gains in graduation come from, showing four-year graduation from various college types. Most of the graduation gains come from students who would not have otherwise graduated from a four-year college on time (Column 1), while some reflect a shift from graduation at slightly lower ranked institutions (Barron's rankings of highly competitive and very competitive, as shown in Columns 4 and 5). Thus college graduation gains are due both to a greater number of completions, as well as from upgrading institutional quality, either by shifting students to the highly ranked HI or other high-ranking institutions.

As a whole, assignment to any of the three programs increases enrollment in and graduation from a four-year college. The gains are concentrated at elite institutions: the HI for the six-week program, and the HI and other highly-ranked colleges for the one-week and online programs. The largest increases in on-time graduation come from the six-week program. The programs were successful at two of their goals: (1) increasing representation of URM students at the HI, and (2) improving the trajectories of students regardless of institution. In the next section, we consider whether the programs induce an increase in attainment of STEM degrees.

5.3 STEM degree attainment and potential earnings

One of the stated goals of the programs is to increase the proportion of URM students in STEM careers, and a key part of the STEM pipeline is completing a major in a STEM field. We examine STEM degree completion in Table 4. This table shows overall degree attainment and then divides that by major between STEM degrees, non-STEM degrees, and bachelor's degrees with no reported information on majors in the NSC. The table shows completion within four years in Columns 1 through 4, and within five years (one less cohort) in Columns 5 through 8. The STEM degree category includes any degree within the broad categorizations used by IPEDS that denote STEM fields, following the National Center for Education Statistics: Computer and Information Science, Engineering, Engineering Technologies, Biological and Biomedical Sciences, Mathematics and Statistics, Physical Sciences, and Science Technologies.[13] We categorize all other degrees that report a major as non-STEM degrees and also separately report degrees with no major code as

[12]Online Appendix Table B.3 shows graduation from the HI, limited by cohorts, and gives some credence to the idea that the differences in six-year graduation are due to fluctuations across cohorts. Current graduation outcomes include students who graduated in 2020 and 2021, the latter of whom may have been affected by the COVID-19 pandemic. We will continue to update graduation information as time goes on. See Figure 1 for a timeline of the cohorts and their on-time progress through college.

[13]If a student is missing information on major, but their degree is designated a bachelor of science degree (or some variation thereof, like B.S.) rather than a bachelor of arts or B.A. degree, then we count it as a STEM degree.

"missing major."

In 2019, 36 percent of bachelor's degrees in the US were in STEM fields (National Science Board, 2022). The study population is predisposed to STEM fields to a greater extent than other students in the US: 69 percent of degrees earned within four years in the *control group* were in STEM fields. Even so, the STEM summer programs increased the prevalence of STEM degrees.

The six-week program increases overall receipt of four-year degrees by 8.2 percentage points: this reflects both small shifts away from non-STEM degrees (-2.6 percentage points) and unreported majors (-1.9 percentage points), and a large increase in STEM degree attainment (12.7 percent).[14] The shift to STEM induced by the six-week program is entirely driven through the HI. For the one-week program, STEM gains come mostly from increased graduation, with a small amount of switches in major, though the increases are not statistically significant. About 39 percent ($\frac{0.035}{0.090}$) of the 9 percentage point increase in STEM degree attainment from the one-week program comes from non-HI institutions. For the online program, all of the attainment gains are concentrated in STEM fields at the HI.

The shift to STEM is even larger when looking at five-year STEM degree attainment in Columns 5 through 8. With five-year graduation, the six-week program increases STEM degrees by 20 percentage points, the one-week program by 15 percentage points, and the online program by 5 percentage points. HI-driven increases are about the same for four-year and five-year degree outcomes, meaning that the five-year gains are coming from non-HI institutions (possibly because a large share of students at the HI graduate on time). Online Appendix Table B.9 separates the STEM majors into specific fields, showing that engineering dominates the increase in STEM. These programs induce not just college graduation, but college graduation in STEM fields.

The shift in STEM degrees is due to degree completion, not differences in STEM interest for applicants offered a seat in the program. Turning briefly to survey evidence, the programs do not change reported interest in STEM majors immediately after the program (Figure 6), with 93 percent of both treatment and control students finishing the program summer reporting plans to major in a STEM degree. Once in college, both treatment and control students maintain very high levels of interest in STEM degrees, with about 83 percent of students planning to declare a STEM major. There are some differences when it comes to intentions to pursue a STEM career: both the six-week and online programs increase the likelihood that students report wanting to pursue a STEM career in the fall of the senior year of high school, and the six- and one-week programs induce similar gains in STEM career intentions mid-college (though they are not statistically significant). Thus, we conclude that the increase in STEM degree attainment is due to the groups with program offers being more likely to follow through on STEM intentions—perhaps due to an upgrade in

[14] Online Appendix Tables B.7 and B.8 report the same results with missing majors counted as non-STEM and STEM fields, respectively. The coding of missing degrees does not affect the overall finding that the programs increase STEM degree attainment, though the various coding schemes do change the magnitudes and statistical significance of some coefficients. However, the pattern of changes is not systematically smaller or larger.

institutional quality that we discuss more below—rather than the programs inspiring a greater degree of interest in STEM fields.

To understand how the shift to STEM may influence future earnings, we use the concept of "potential earnings" from Sloane et al. (2021). Sloane et al. (2021) define potential earnings as the earnings of "the median middle-aged, US-born, White male" by major, and calculate this using the American Community Survey from 2014 to 2017. This measure represents the major-specific potential earnings subtracting out labor market experiences such as discrimination and penalties for taking time off for family responsibilities. We assign to each student in the sample the potential earnings of their major (in natural log units) and plot the distribution of majors by earnings, for those who graduate with a degree in four years and in five years (Figure 4). The changes in distribution of degrees will thus reflect changes in potential earnings due to the composition of majors, *not* differences due to the increased graduation rate induced by the program. Thus it is perhaps a conservative estimate of impacts on potential earnings. Included in the figure are the program-specific impacts on potential earnings based on graduates. Online Appendix Table B.10 reports these regression estimates, along with estimates that impute earnings for non-graduates.

Figure 4 shows the same shift to engineering shown in Online Appendix Table B.9 but highlights that engineering is the major with the highest potential earnings. The figure also shows a decline in psychology majors for the six- and one-week programs. The average difference in potential earnings is about 4 percent for the six- and one-week programs, and 1.4 percent for the online program (not significant) when using four-year graduation. With five-year graduation, these estimates are even larger, at 6 percent for the six-week program, 5 percent for the one-week program, and 2 percent for the online program. Although these estimates cannot predict exact earnings, they show that those offered seats in the program are set up to enter fields where their earnings are at least 2 to 6 percent higher than they would have been in the absence of the STEM summer programs.

5.4 Heterogeneity

In Online Appendix Tables B.11 through B.14, we present estimates for key outcomes by gender, URM status, self-reported free or reduced-price lunch receipt (a proxy for family income), and first-generation college-going status as these groups include populations of particular interest for increasing representation in STEM fields. Both male and female students appear to benefit equally from the six-week program, but female students have slightly larger gains from the one-week and online programs, especially for graduating from a highly ranked institution (Online Appendix Table B.11). Larger gains for female students are perhaps not surprising because women are less likely to choose STEM majors so there is more room for improvement. Thus, STEM programs that also emphasize serving women (by ensuring gender balance in the program, even while not explicitly focusing on women's representation) can also contribute to closing gender gaps in STEM.

In Online Appendix Table B.12, we show estimates for Black, Hispanic, and Native American individuals separately from students who do not identify as such (White, Asian, multiethnic, and "other race" students are in the non-URM category). Because less than 18 percent of the sample is non-URM, estimates are noisy, but there is some evidence, at least for STEM degree attainment, that the non-URM impacts are larger for the six- and one-week programs. However, it appears that almost all of the benefits of the online program accrue for URM students. Again, the sample of non-URM students is quite small, and there are large and meaningful benefits for both groups.

The clearest difference comes from a comparison of students who report receiving subsidized lunch at their high school with those that do not (Online Appendix Table B.13). Although exposure to the programs is generally beneficial for both groups, students who do *not* receive subsidized lunch reap larger gains, especially for graduating from an elite institution and graduating with a STEM degree. Students with more resources may be better poised for an institutional upgrade, perhaps because of fewer concerns about college costs. Increasing representation by race in STEM fields will not necessarily increase representation by family income, and vice versa.

Examining differences by first-generation college-going status (defined as no parent ever attending a four-year college) shows that both groups benefit from participation in a STEM summer program, but that the six-week program is more effective for first-generation students than non-first generation students, likely due to the large shift to the HI. Gains at elite institutions from the one-week and online programs (which we showed earlier were split between the HI and other elite institutions) are more consistent for the non-first-generation group. The programs may be most effective for first-generation students who are directly influenced to attend the HI, and the non-HI benefits of the programs may accrue more for students who already have social capital.

5.5 Validating the Experiment

This section considers several tests of whether the estimation strategy under constrained random assignment effectively accounts for selection bias and the extent to which heterogeneous treatment effects account for our findings, the role of cohort variability, and the importance of limiting the range of randomization scenarios. We also conduct a bounding exercise to consider the role of unobserved covariates (Oster, 2019). We compare estimates from Equation 1 to alternative specifications on key outcome variables at the HI and across institutions.

5.5.1 Investigating selection bias

We argue that our conditional random assignment specification is as good as random because the conditions of random assignment are based on observable characteristics that we fully account for with randomization blocks. To make this explicit, in Online Appendix Tables A.3 and A.4, we show how controlling for randomization block effectively controls for selection bias. Panel A of these tables first shows the main specification, but *without* controls for randomization block (retaining controls for cohort, gender, and geographic preference). In this case, most of the estimates are biased upward in comparison to the main estimates with the blocks (Panel B) because students with higher rating variables have greater success in college, regardless of program participation. Panel C adds a control for the rating variable to the main estimates in Panel B, which leaves the findings unaffected. This is not surprising because within the block, the rating variable is randomly assigned. Panel D shows an alternative way of controlling for assignment: removing the rank-component of the blocks as in Panel A, but controlling for rating variable as in Panel C. In this case, the estimates are of slightly smaller magnitudes at the HI but tell the same story as the main estimates. Outside of the HI, the reduction in magnitudes is slightly larger but again show positive effects of program offer. Any of the estimates in Panels B through D are causal estimates of the programs' effectiveness, and they all account for the selection bias shown in Panel A. We use the estimates in Panel B as our main specification, as this was the intended design of our experimental estimates. In Online Appendix A.1.6, we also discuss the scope for heterogeneous treatment effects in this modified random assignment to explain the treatment effects. Although the heterogeneous response of highly rated students may contribute to the magnitudes for the one-week program, the evidence that differential response by rating drives program effects is limited (Online Appendix Tables A.11 and A.12). We also show in this appendix that estimates of the online program effect, both within block and across blocks—using the alternative controls from Online Appendix Table A.3—show very similar treatment effects for the online program, demonstrating that our "link" across blocks is sound.

5.5.2 Oster Bounds

We do not find evidence that the constrained randomization design fails to account for selection on observables, but it is possible that unobservable differences between blocks drive the findings. For example, students in Block 1 may have a different distribution of academic interests than those in Block 2, and this is not fully accounted for in our other controls. To address this concern, we conduct a bounding exercise using the techniques described in Oster (2019), which build on earlier work by Altonji et al. (2005). The Oster procedure compares an "uncontrolled" model to an estimate generated with controls to estimate the degree to which selection on observables accounts for bias in the uncontrolled model. We can then estimate δ, which is the degree of selection on unobservables (in terms of proportion of selection on observables) that is needed to negate the treatment estimate, at a given R^2. The R^2 should be set to R_{max}, the variance explained by both the observable and unobservable controls. As this is unknown, we follow Oster (2019) in setting R_{max} to $1.3 \times R^2$ from our preferred model.

We present results from this procedure in Online Appendix Table B.1. The first column of this table repeats estimates from our main specification for five key outcomes. Columns 2, 4, and 6 then present estimates from specifications with a reduced number of controls: Column 2 excludes all controls; Column 4 excludes individual-level controls that were not used in the constrained randomization procedure and block controls, but includes year by gender by geographic preference controls, which we term "unrelated" controls as they are plausibly unrelated to the relevant unobservables; and finally Column 6 excludes only individual-level controls that were not used in the constrained randomization procedure, leaving the experiment's blocking structure intact. These three estimates provide a range of plausible scenarios for our "uncontrolled" model. Columns 3, 5, and 7 then present the δ, given $R_m ax = 1.3 \times R^2$ of our preferred specification, that would render our findings null.

For comparisons to either the fully uncontrolled model or the unrelated control model, the results are similar: for many outcomes and programs, selection on unobservables would have to be as large as selection on observables ($\delta = 1$), even greater than selection on observables ($\delta > 1$), or work in the opposite direction as selection on observables (no value of δ reported). For a few of our outcomes, δ is less than 1, with the smallest estimate being 0.58. For these outcomes and estimates, selection on unobservables would have to be about 60 to 95 percent as large as selection on observables to negate our estimates. This is an unlikely scenario, especially since our observable controls account for the factors that were directly used in the constrained randomization process. Notably, the results for earning a STEM degree are the most robust, with a large degree (or no degree) of selection on unobservables necessary to shift the estimates to null. Finally, if we examine columns 6 and 7, it is clear that blocking structure is effective at controlling for observables. That is, there is little difference between columns 1 and 6, and the necessary δ's to render the estimates null are nonexistent or quite large. This implies, as should be the case for the addition of individual-level covariates in a randomized controlled experiment, that individual-level controls absorb variation but do not alter the conclusions of the analysis, and that our blocking structure fully accounts for observable differences between blocks.

5.5.3 Alternative specifications

Online Appendix Tables A.5 and A.6 report alternative specifications for outcomes across institutions and at the HI, respectively. Panel A shows the main estimates without baseline covariates. As expected, precision decreases a bit, but the magnitude and direction of the estimates remain the same. Panels B through D remove each cohort in turn. We expect there to be idiosyncrasies across cohorts due to sampling variation, but each panel reports estimates that are generally in line with the main findings. In some cases there is a loss of precision due to smaller samples. Panel B is notable in that it removes the 2014 cohort. This cohort had the most modifications to random assignment, with more blocks than in the two subsequent cohorts. If the study design with a greater number of blocks did not remove selection bias as fully as the design with fewer blocks, we would expect that the estimates in Panel B would be *smaller* than the main findings because removing 2014 would remove upwardly biased estimates. Instead, the results limited to the later cohorts with a design closer to complete random assignment show either very similar or larger impacts.

5.5.4 Randomization inference

A different, but related, threat to validity comes from the randomization scheme. By imposing randomization strata and modifying complete random assignment, the study limits the range of potential outcomes that is possible. For example, if a state is preferred in the assignment for representational reasons, it is always going to limit the range of randomization scenarios possible under the assignment scheme. Standard inference methods do not account for these constraints (Athey and Imbens, 2017). As an alternative, we present results using randomization inference. Specifically, we re-randomize applicants to the programs, using the same randomization criteria (blocks, location, gender preferences, etc.) 1,000 times, and compare the estimate from our main specification to the distribution of estimates from the 1,000 randomizations. Each randomization faces the constraints imposed by our research design, so we are limiting possible comparisons to those that might actually occur rather than a hypothetical scenario of full randomization.

We display results from this exercise in Online Appendix Figures B.3 and B.5. Each panel shows the distribution of treatment estimates from the 1,000 hypothetical randomizations (bars), compared to the main specification estimate (dashed line). If the impact estimate from the main specification is at or above the 97.5th percentile (two-tailed test) or 95th percentile (one-tailed test), then that implies statistical significance. For any college attendance, impact estimates are at the 76th to the 83rd percentiles, but at elite institutions, impacts on attendance are at the 97th to 99th percentile—generally similar to the pattern of findings in the main estimates. Impacts on graduation and STEM degree attainment range from the 80th percentile to the 98th percentile, with six-week program effects more likely to be outliers. Again, this pattern aligns with our main estimates.[15] We consider the randomization inference exercise as confirmatory evidence that we can draw inferences from our constrained randomization scheme using traditional statistical methods.

6 Mechanisms

In this section, we explore mechanisms behind the successful STEM summer programs, focusing on graduation from a four-year college and obtaining a STEM degree as the main outcomes of interest. For many of these analyses, we use data from student surveys, which have some differential response by treatment arm. As we note in Section 3, we use unweighted survey responses due to the inconsistent influence of reweighting schemes to address survey response bias, and instead consider findings from the surveys suggestive rather than conclusive.

[15] In the case of application and admission to the HI, the main specification estimate is above the 99th percentile of the distributions of placebo randomization estimates, implying that it is extremely unlikely that the estimate was due to chance. When we turn to the impacts on attendance at and graduation from the HI, the patterns again follow the main estimates closely. The estimates for the six-week program are larger than all of the estimates from the placebo randomizations. The one-week and online programs have the impact estimates at the 80th to the 87th percentiles of the placebo randomizations. However, this, too, is in line with our main estimates, as attendance and graduation impacts for the one-week program tend to be of modest magnitude and not statistically significant.

There are several reasons why the programs might improve college graduation and STEM degree attainment, and we focus on three, human capital-related explanations here.[16] First, the programs may drive impacts by increasing participants' human capital with respect to the college application process, which shifts where students go to college. In turn, this drives differential institutional quality, which causes the differences in outcomes that we observe. We also discuss the extent to which institutional upgrading is due to greater likelihood of admission because of a signaling effect of the programs. Second, the program may improve participants' human capital with respect to subject matter knowledge, helping them get ahead in college and be more likely to graduate. Finally, we consider whether there is an increase in participants' human capital with respect to their soft skills, which better position them to succeed in college. As we detail below, we have the most evidence that the first channel explains our findings, though we cannot fully rule out other explanations.

6.1 College application behavior and college quality shifts

The STEM summer programs have potential to affect the trajectories of young people, but ultimately, even the most intense six-week program is a short period in a young person's life. However, by shifting students to different colleges from those they would have attended otherwise, the programs may set students on a different path. We have shown that program participation increases enrollment in and graduation from the HI and other highly ranked colleges. Here, we show the primary path for shifting students into these institutions is through changing college application strategy. We first show that application and admissions patterns change for program participants, and then discuss how this shift induces institutional upgrading.

We observe increased application to the HI (via administrative data from the HI, displayed in Online Appendix Figure B.1 and Online Appendix Table B.2) and other elite institutions (via survey data, displayed in Table 5). About 31 percent of control group students apply to the HI. This would be a very high rate of application for typical high school students, but program applicants are a selected group who were interested in the outreach office's summer programs. Even so, being assigned to one of the summer programs more than doubles application to the HI for all the programs, with almost all students (78 percent) in the six-week group applying to the HI.

Almost 11 percent of the control group are admitted to the HI, again demonstrating that the study sample is a selected group: the HI typically admits fewer than 10 percent of applicants.

[16]We focus on these explanations both because they are meaningful, plausible mechanisms and because we have the most data to bring to bear on these mechanisms. Another interesting possibility is that participation in the program serves as a signal to college admissions officers. Signaling is probably most powerful at the HI, and many of the one-week and online program gains are at non-HI institutions. Additionally, while this channel may play a role, we note that it would primarily influence *admission* but not necessarily what happens during college. So, it is unlikely to be an explanation for the increase in STEM degree attainment, especially given that students in both the treatment and control groups report an intention to major in STEM at very high rates. Nonetheless, we discuss signaling with respect to college admissions when we discuss college application behavior below.

Given that a little over 30 percent of the control group applied, this is an admission rate of about 34 percent. Admissions rates are generally on par with the control group admission rate among those with program offers. About 31 percent of students assigned to the six-week program are admitted to the HI, with an implied admission rate of 40 percent. For the one-week program, about 21 percent are admitted, with an implied admission rate of about 30 percent; for the online program, 19 percent are admitted, with an implied admission rate of about 29 percent. Thus, while there is a small bump in likelihood of admission for the six-week program, the main difference between the control group and treatment groups is in likelihood of applying. Students offered seats at the STEM summer programs are more likely to be admitted to the HI, but for the most part this seems to be due to greater likelihood of application, rather than greater admission conditional on application. We explore this in more detail below.

Enrollment and graduation changed beyond the HI, with programs shifting students to elite institutions other than the HI (especially the one-week and online programs), so admission behavior to these institutions likely changed as well. We turn to survey responses to examine changes in application behavior more broadly. Survey respondents reported their college applications and admission in a survey conducted in May of their senior year of high school. As we show in Table 5, those offered seats at a STEM summer program are more likely to apply and be admitted to a Barron's most competitive institution, even when excluding the HI from that category (Column 5). The one-week and online programs also induce an increase in the overall number of applications and admissions (Column 2), though only the online program's effect on applications is statistically significant. However, the most dramatic change in application behavior comes from a reduction in application to a single school (Column 4). Attending the six-week program eliminates the (small) possibility that students apply to only one school, and attendance at the one-week or online program halves this likelihood. As shown by enrollment in higher-quality institutions (Table 2), these changes in application behavior translate into more chances to attend a high-quality institution.

These improvements in college application come from increased knowledge about the college admissions process. The programs also improve students' college resources through knowledge of both the landscape of available colleges and the college application process itself, as we show in Online Appendix Table C.3. Survey responses from the fall of the senior year of high school show that the programs—especially the six-week program—increase sources of college application advice (Columns 1 through 4), with students assigned to the treatment group reporting greater likelihood of getting advice from friends and a teacher or counselor. Assignment to a program also increased familiarity with non-HI institutions: students were more likely to report familiarity with a technical institution (by 7 to 10 percentage points), an elite institution (by 1 percentage point), and a liberal arts college (by 7 to 11 percentage points), and were less likely to report familiarity with a highly-ranked public university or a fake institution with a made-up name, though these differences are not significant.

These changes in knowledge of the college application process and application behavior translate into increased enrollment in high-quality institutions, as we show in Section 5. We investigate whether enrollment in these institutions spurs the college graduation and STEM results we observe in Figure 5, which compares the *institution-level* outcome of the university attended immediately after high school graduation to the individual-level outcome. To generate the institution outcomes, we assign the institution-specific graduation rate to those who attend a four-year institution in their first year of college.[17] This outcome then measures the "predicted" graduation rates for each treatment group, if we assume that students in the study group graduate at the graduation rate of the institution they attend. We construct a similar outcome for STEM degrees, which is the institution specific share of degrees that are in STEM fields, based on degrees reported to IPEDS.

The results from this exercise are in Figure 5, with more details in Online Appendix Table B.17. Panel A compares the difference in "actual" graduation with the "predicted" graduation rates. For the six-week and one-week programs, the change in actual four-year graduation rates almost exactly parallels the difference in predicted graduation rates (for the online program, the change in graduation is about one-third the size of the difference in expected graduation), which is consistent with many of the program benefits operating through students' upgrading college quality. When we conduct a similar analysis for STEM degree share, we see in Panel B that the six-week and online programs increase attendance at institutions with higher proportions of STEM degrees, but the actual gains in STEM degree receipt are even greater than the predicted difference. Thus, students offered STEM summer programs increase their likelihood of graduation by the amount predicted due to institutional upgrading, but they increase their STEM degree attainment to an even greater extent, providing evidence that program participation helps students achieve their intention to obtain a STEM degree in very competitive institutions.

6.2 Signaling

We examine how much of the admission effect is due to applying as opposed to greater odds of admission upon applying to colleges in Online Appendix Table B.15. In this table, we show application, admission, and admission conditional on applying to the HI (Columns 1 through 3), at any Barron's most competitive institutions (Columns 4 through 6), and at Barron's most competitive institutions excluding the HI. The latter two categories are restricted to the survey respondents who report admissions and enrollment. The conditional admissions estimates show the increased likelihood of being admitted to an institution for the STEM summer programs, which may be due to signaling or program-induced improvements in students' application packages or human capital. At the HI, part of the admissions gains are due simply to increased likelihood of application, but part of the gains are from an additional bump in admissions for the STEM summer programs (15 percentage points for the six-week program, and 5 to 5.5 percentage points for the one-week

[17] For students who attend community colleges or do not attend college, we substitute zeroes instead.

and online programs, though these differences are not statistically significant). At non-HI elite institutions, the online and one-week programs increase the likelihood of conditional admissions by 5 and 6 percentage points, respectively, though only the former is statistically significant. Overall, these findings show that there are some increases in conditional admission but the role of changes in application behavior is large.

Panel B compares the HI STEM summer programs to a restricted control group, where control students are limited to those who report attending any STEM summer experience.[18] This control group is more likely to be female and has slightly higher rating scores than control students who do not participate in STEM summer experiences, but is otherwise quite similar to the control group overall (see Online Appendix Table B.19). If we assume different STEM summer experiences impart similar human capital gains, comparing those assigned to the HI STEM summer programs with those in the control group with STEM summer experiences tests whether the HI programs have signaling value above and beyond that of having any STEM summer programming. Even with this comparison group, there is still a conditional admission gain of 10 percentage points at the HI for the six-week program, but there is no difference for the one-week and online programs. We interpret this to mean there is likely some role for signaling for the six-week program at the HI. However, outside the HI, there appears to be little difference in conditional admissions between the HI STEM summer groups and control group members with other STEM summer experiences, implying that outside of the HI there are no special gains for studied summer programs compared to any STEM summer programs, except those operating through the application channel.[19] These estimates are consistent with some role for signaling, but do not prove its existence. Even when comparing to control students with STEM summer experiences, our assumption about similar human capital gains across programs could be wrong, and the additional boost for the six-week program compared to control students with STEM summer experiences might be due to human capital gains from the program. These findings provide a ceiling on the signaling mechanism and confirm the application channel accounts for many of the program effects.

[18]These include similar programs at other colleges and universities, nonprofit programs like Girls Who Code, internships in STEM or medical fields, and taking STEM courses.

[19]We also compare graduation and STEM degree outcomes between those assigned to HI STEM summer programs and the selected control group with STEM summer experiences in Online Appendix Table B.16). This comparison contrasts randomly assigned students with a group that will both have gains from their STEM summer experiences and a selection effect, as these students sought out additional STEM experiences. Thus, we expect program impacts to be smaller when comparing to this group, and they are, as seen in Panel B. However, we note that the HI STEM programs still maintain an increase above this selected comparison group in five-year graduation (Column 6), due to increased five-year graduation at the HI for the six-week program (Column 2) and increased five-year graduation from other institutions for the one-week and online programs (Column 10), though the magnitudes are smaller and they are not statistically significant with the full control group (Panel A). In terms of STEM degrees, the shift is less stark, which makes sense if we think the control group represents the most STEM-ambitious comparison group.

6.3 Subject matter knowledge, skills, and confidence

Another way the STEM summer programs may lead to college graduation and STEM degrees is by increasing students' STEM subject matter knowledge, helping them get ahead in college. This could occur in two ways: the programs could change high school classes taken prior to college entrance, or they could directly increase knowledge of STEM, better preparing them for future STEM majors. Additionally, the programs may improve more general skills such as study skills, as well as soft skills such as confidence and self-esteem.

Table 6 shows impacts on both high school course plans and on a direct measure of human capital. Most students were already planning to take at least one Advanced Placement (AP) or International Baccalaureate (IB) course in their senior year in high school (control group mean of 89 percent), and program participation had positive impacts on these intentions (only statistically significant for the one-week program), as shown in Panel A. When it came to increasing AP/IB coursework, the programs increased computer science take-up. About 10 percent of the control group planned to enroll in such a course, and program offers increased that by 7 to 10 percentage points. Perhaps because science and math advanced coursework was already extremely popular (control means of 73 to 74 percent for both), the programs made little difference in science and math advanced coursework. During the fall of the senior year of high school, applicants offered seats in the programs were slightly better able to answer a calculus question, though the differences were not statistically significant (Column 7 in Panel B). The minor calculus improvement and coursework changes in non-core subjects indicate that although the programs may impart STEM knowledge and inspire subsequent high school coursework, these changes are likely to be relatively small.

We illustrate program impacts on soft skills in Table 6, which shows student survey responses to questions about life skills, study skills, confidence, interest in learning, and attention span.[20] Life skills include tasks such as setting an alarm to be on time and doing one's laundry. For many program participants, the time on the HI campus might be their first time away from their family, and we observe an increase in the self-reported life skills that one would gain in such a situation. As predicted, gains in life skills are larger for residential programs as opposed to the online program (even though this program had a short campus visit). We also see that the programs increase self-reported study skills, such as asking questions and taking notes. However, there is no statistically significant change in confidence, which included a self-assessment of students' math ability, though the direction is positive. The negative coefficients for attention span (statistically significant for the one-week and online programs) may reflect students engaging with more challenging material over the summer and in the fall of their senior year of high school. The positive, statistically significant increases in life and study skills, and the positive but not statistically significant increases

[20] See Online Appendix C for details on the variables included and how we generated indices from individual variables. Online Appendix C also includes additional survey responses not discussed in the text.

in confidence and enjoyment of intellectual activities are all consistent with the idea that the STEM summer programs better prepare students to succeed in and graduate from college and may contribute to the increases in STEM degree attainment that we cannot account for solely through upgrades in institutional quality.

7 Conclusion

We present evidence from the first randomized controlled trial to investigate the impact of STEM summer programs for underrepresented youth, examining a suite of programs that includes a six-week program, a one-week program, and an online program. Program offers increase student matriculation at the HI and other elite universities. Students exposed to the STEM summer programs are more likely to persist through college, with the most notable difference coming in the fourth year of college, when a larger share of control group students are no longer enrolled. The programs increase overall four-year college graduation, with gains concentrated at the HI and other elite institutions. The programs also induce increases in attainment of STEM degrees. Response to program assignment, especially at the HI, is largest for the most intensive, six-week program, but both the one-week and online programs consistently increase graduation and STEM degree attainment. The shift in major toward STEM degrees may increase potential earnings by 2 to 6 percent. We show evidence consistent with the idea that a change in college application behavior shifts students to higher-quality institutions, which drives the gains in college graduation.

The benefits for generating STEM degrees, in contrast, go beyond what we would expect solely based on institutional quality (as measured by share of STEM degrees on campus). This is a highly STEM-motivated group, with almost all of the study sample intending to major in STEM in college. Like students across the nation, those intentions face a "leaky pipeline" where students leave STEM preparation throughout college. The STEM summer programs make students more resistant to leaks in the pipeline, though there are still STEM-intending students who do not ultimately complete a STEM degree. We cannot definitively say why this is but note that the programs have comprehensive coverage of many hypothesized STEM-supportive pathways, including STEM curricula; URM role models in the form of near-peer teaching and residential assistants, staff and instructors, and guest speakers; and a shared group experience.

The contrast between the three programs provides some basis for a back-of-the-envelope cost effectiveness calculation. The six-week program generates the largest response, but is also the most expensive, in contrast to the one-week and online programs. In 2015, the six-week program cost about $15,000 per student, while the one-week and online programs cost about $2,000 per student. Perspectives on which program is the most cost-effective will differ by the objective. For the HI, the six-week program produces an increase in on-time graduation four times as large as the one-week or online program, using the treatment-on-the-treated estimates from Online Appendix Table B.5. But if a policymaker is interested in overall graduation from the most competitive institutions, the six-week program only outperforms the one-week program by a small amount (an increase of 13 percentage points versus an increase of 12 percentage points, as shown in Online Appendix Table B.4), though it still doubles the effect of the online program. The comparison is similar for on-time STEM degrees. A more detailed cost-effectiveness analysis is left as a future exercise.

This analysis shows that targeted programs to increase representation on college campuses can have wide-ranging benefits for participants. There may be additional spillover benefits for peers at elite institutions who benefit from a more diverse and inclusive STEM classroom. As the U.S. Supreme Court continues to erode affirmative action as a component of higher education admissions, more colleges and universities may turn to programs like the STEM summer programs we study here to provide the benefits of diversity to their campuses through indirect avenues. Indeed, many campuses already have "summer bridge" programs that provide support for matriculating underrepresented students in the summer before their freshman year. Additionally, federal investment in STEM fields is targeted to higher education, not earlier in the pipeline. Our findings show that focusing on higher education after students apply to college may miss a key opportunity to intervene in students' lives *before* they apply to college—the point in time crucial to the institutional choices that may ultimately help students succeed.

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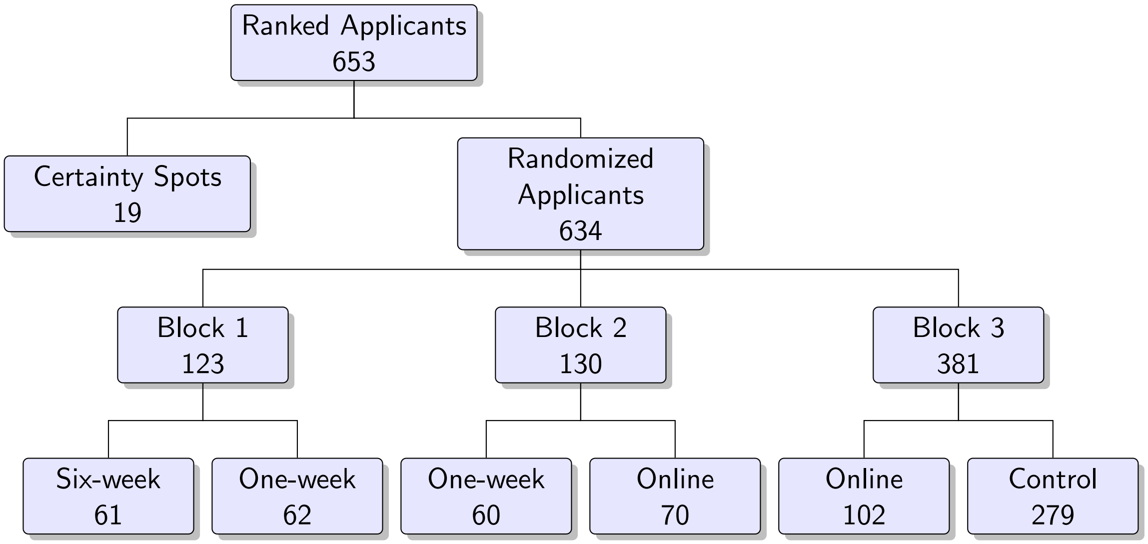
Figure 1: Experiment Timeline and Available Data

Cohort	Program summer	Senior Year of High School	First Year of College	Second Year of College	Third Year of College	Fourth Year of College	Graduate in 4 Years	Graduate in 5 Years	Graduate in 6 Years
2014	2014	2014-15	2015-16	2016-17	2017-18	2018-19	Spring 2019	Spring 2020	Spring 2021
2015	2015	2015-16	2016-17	2017-18	2018-19	2019-20	Spring 2020	Spring 2021	
2016	2016	2016-17	2017-18	2018-19	2019-20	2020-21	Spring 2021		

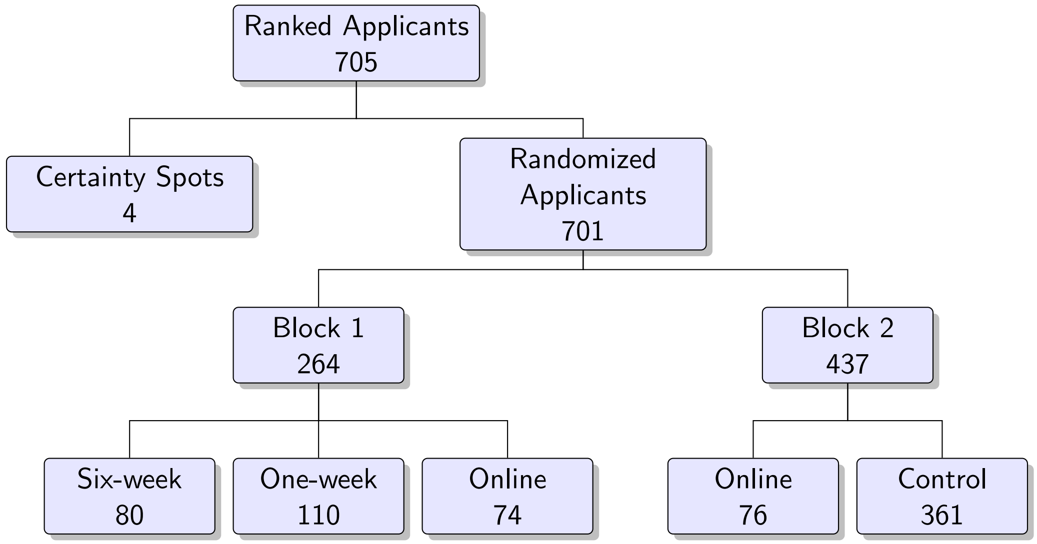
Notes: This figure shows student progress over time by experimental cohort, assuming students maintain on-time progress through college. The most recent available information on college enrollment and graduation reflects the spring 2021 semester.

Figure 2a: Randomization Design: Cohort 1 (2014)



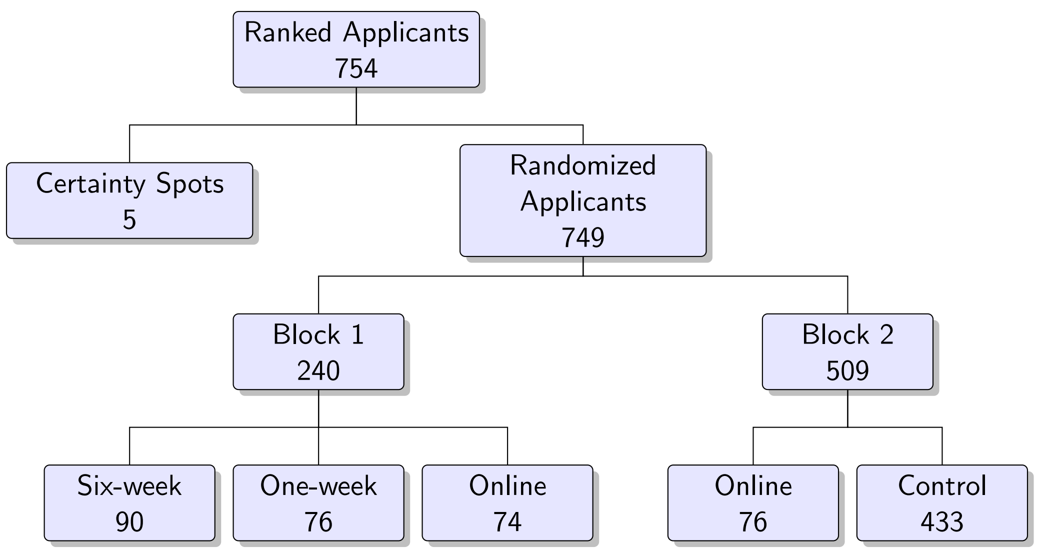
Notes: This figure shows the blocked randomization design in 2014. Certainty spots were applicants offered admission with certainty and excluded from the experimental analysis. All other ranked applicants were subject to random assignment within block. Block assignment reflects applicant ratings, math scores, and regional priorities, with gender as a stratum within block.

Figure 2b: Randomization Design: Cohort 2 (2015)



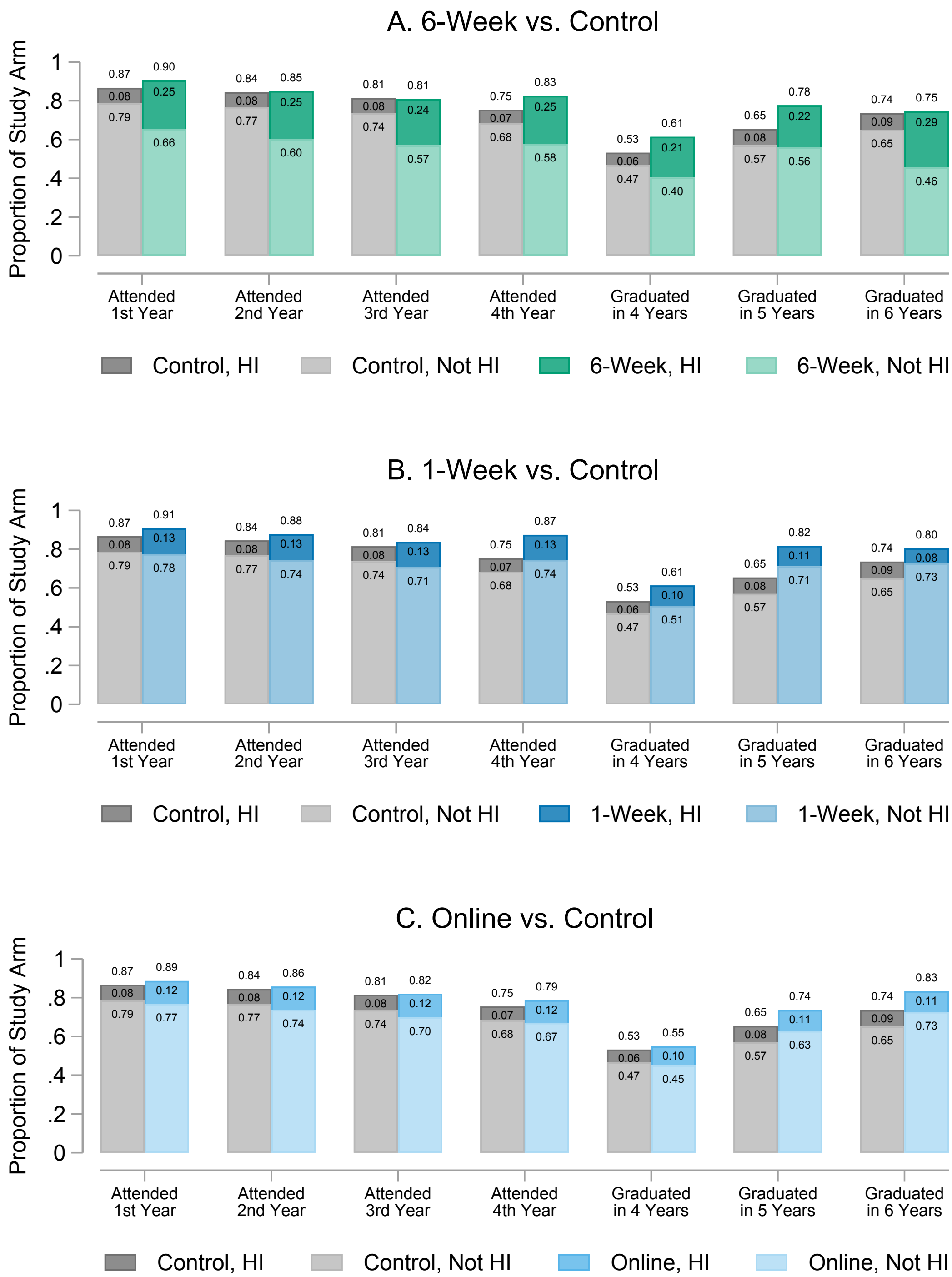
Notes: This figure shows the blocked randomization design in 2015. All other notes are the same as in Figure 2a.

Figure 2c: Randomization Design: Cohort 3 (2016)



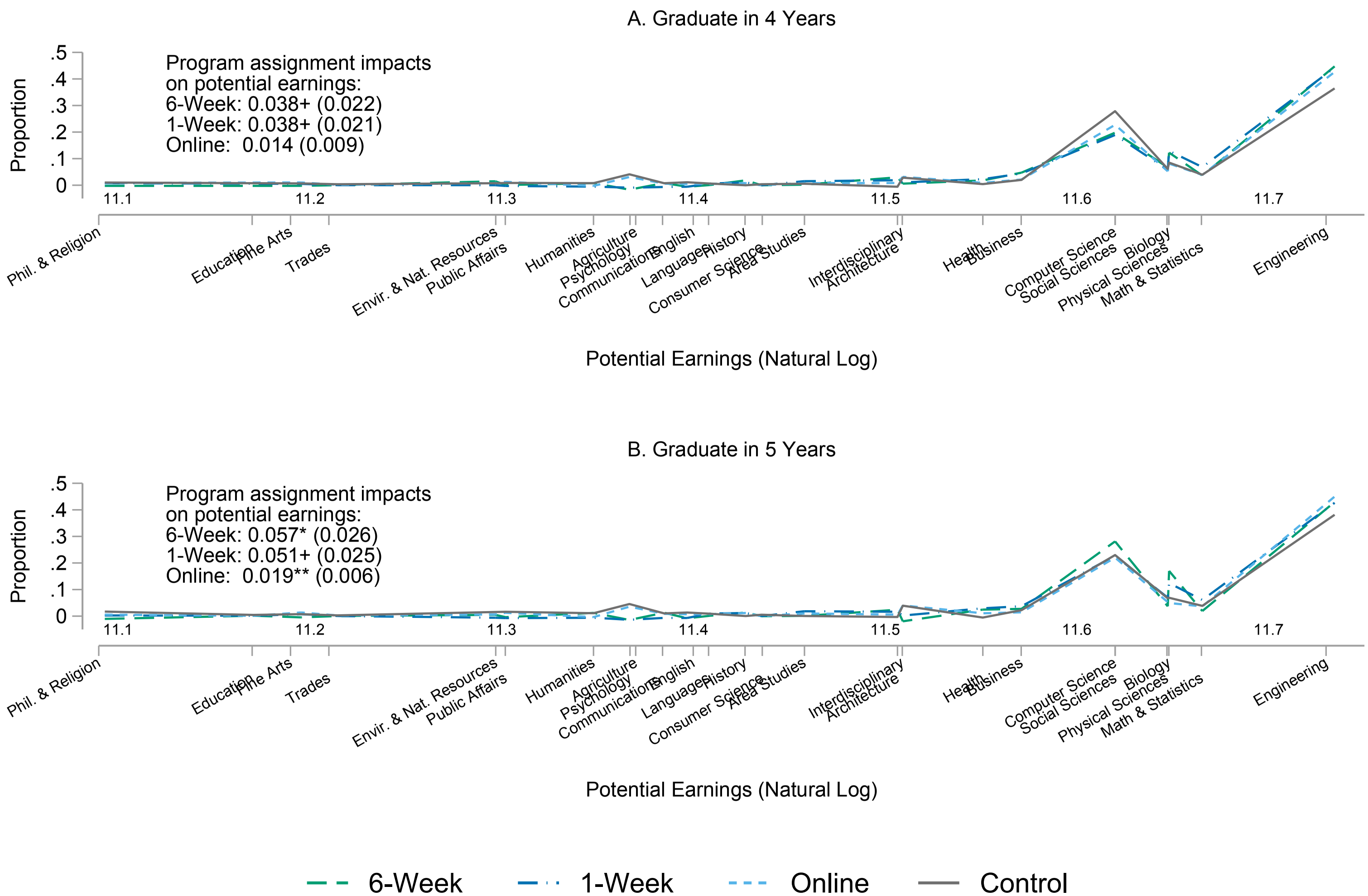
Notes: This figure shows the blocked randomization design in 2016. All other notes are the same as in Figure 2a.

Figure 3: The Impact of STEM Summer Assignment on Four-Year College Attendance and Graduation



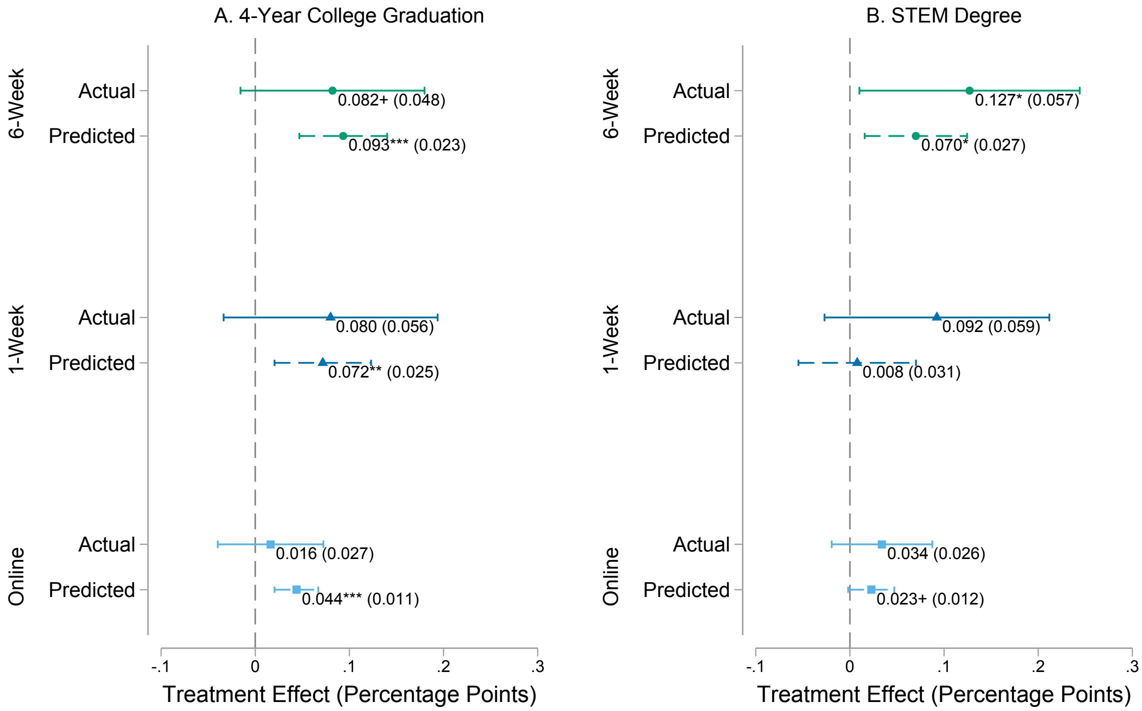
Notes: This figure summarizes impact estimates for four-year institution outcomes, with and without the HI included. For details on the specification and exact point estimates and standard errors, see Tables 2 and 3.

Figure 4: The Impact of STEM Summer Programs on Potential Earnings by Major



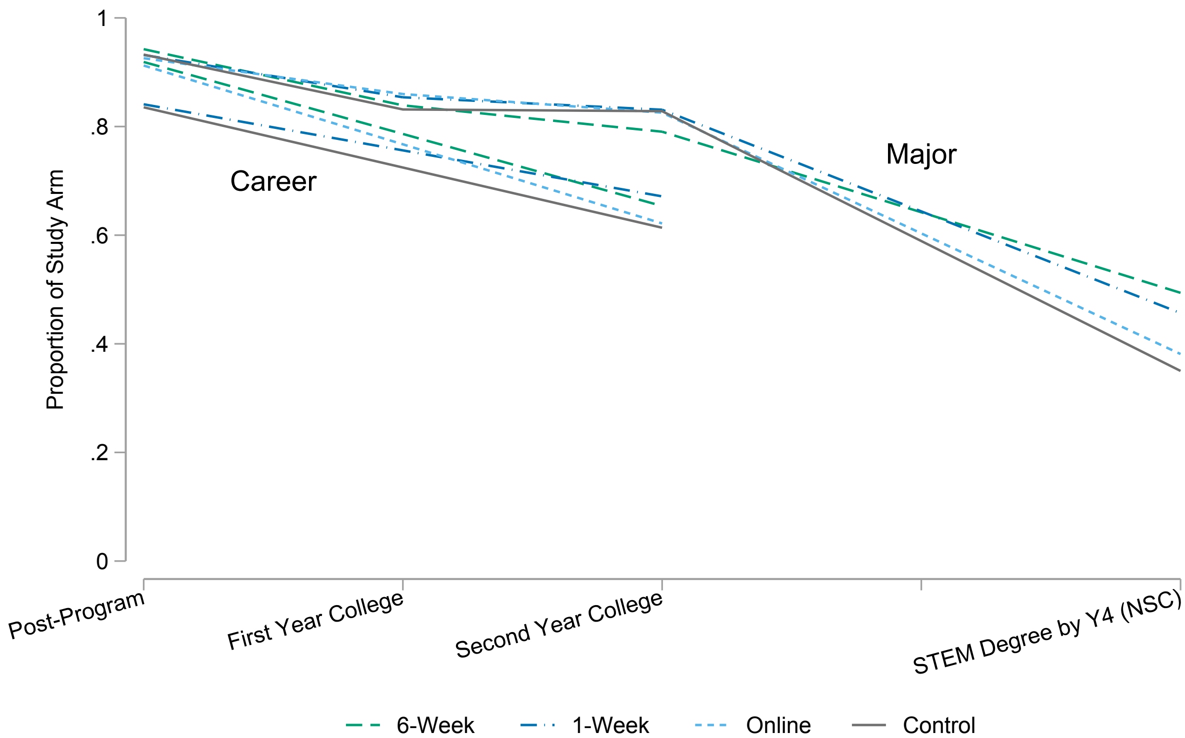
Notes: This figure displays changes in majors by program offer, with majors arrayed by "potential earnings" (Sloan et al. 2021). For detailed impact estimates on potential earnings, see Online Appendix Table B.10.

Figure 5: Actual vs. Predicted Graduation and STEM Rates



Notes: This figure compares program effects on four-year college graduation and STEM degree attainment, marked as "actual," with "predicted" graduation and STEM degree attainment based on institution-level characteristics. The institutional-level outcomes are college-level characteristics calculated from IPEDS data in 2013. Values for community colleges and non-college-going respondents are set to 0 for both institutional-level bachelor's four-year graduation rates and STEM degrees. For additional details, see Online Appendix Table B.17.

Figure 6: The Impact of STEM Summer Programs on STEM Major and Career Intentions



Notes: This figure shows the proportion of students reporting the intention to major or have a career in a STEM field, by program assignment. All responses come from surveys except for STEM degree completion which uses NSC data. For detailed impact estimates on STEM intentions, see Online Appendix Table B.18.

Table 1: Baseline Characteristics by Program Assignment

	Full Sample	6-Week	1-Week	Online	Control	Strata-Adjusted Differences: 6-Week vs. Control	1-Week vs. Control	Online vs. Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black	0.349	0.403	0.351	0.343	0.339	0.069+	-0.033	-0.019
						(0.041)	(0.035)	(0.026)
Hispanic	0.430	0.407	0.412	0.432	0.440	-0.013	0.008	-0.001
						(0.040)	(0.035)	(0.026)
Native American	0.045	0.056	0.052	0.042	0.041	-0.001	0.005	0.001
						(0.020)	(0.016)	(0.012)
Asian	0.136	0.100	0.140	0.140	0.141	-0.036	0.012	0.014
						(0.027)	(0.025)	(0.019)
White	0.039	0.035	0.045	0.040	0.037	-0.020	0.008	0.002
						(0.017)	(0.015)	(0.011)
Multiethnic	0.358	0.377	0.341	0.354	0.361	0.027	0.002	-0.002
						(0.040)	(0.035)	(0.026)
GPA	3.860	3.911	3.896	3.881	3.830	0.004	-0.003	0.019+
						(0.014)	(0.019)	(0.011)
Free/reduced-price lunch	0.391	0.485	0.455	0.373	0.360	-0.003	0.006	-0.023
						(0.042)	(0.037)	(0.026)
Standardized math score	1.929	2.133	2.135	1.939	1.822	-0.021	-0.030	0.001
						(0.073)	(0.067)	(0.059)
Female	0.404	0.502	0.500	0.502	0.312	0.000	0.000	0.000
						(0.000)	(0.000)	(0.000)
First-generation college	0.242	0.303	0.302	0.225	0.219	-0.007	0.046	-0.025
						(0.037)	(0.033)	(0.023)
p-value						0.830	0.971	0.864
N	2084	231	308	472	1073			

Notes: This table summarizes demographic characteristics, test scores, and GPA for program applicants. Column 1 shows averages taken across the entire sample. Columns 2 through 5 display means of these traits at baseline by program assignment. Race/ethnicity categories are not exclusive. First-generation college is defined as no parental college attendance. Students missing parental college information (N=21) were coded as not first-generation. Columns 6 through 8 report coefficients from regressions of the student characteristic on program offer dummies, including controls for randomization strata (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). The p-values are from tests of the hypothesis that all coefficients on each program offer are zero.

Table 2: The Impact of Assignment to STEM Summer Programs on Four-Year College Attendance

	Host Institution (1)	Any 4-Year College (2)	4-Year Excluding HI (3)	Barron's Most Competitive (4)	Barron's Most Comp. Excluding HI (5)
(A) Attended in Year 1					
6-Week	0.169***	0.038	-0.131***	0.172*	0.003
	(0.041)	(0.025)	(0.046)	(0.065)	(0.084)
1-Week	0.053	0.042	-0.011	0.136*	0.083
	(0.038)	(0.031)	(0.046)	(0.060)	(0.072)
Online	0.038+	0.020	-0.018	0.095*	0.057
	(0.020)	(0.015)	(0.030)	(0.035)	(0.049)
Control Mean	0.080	0.867	0.786	0.494	0.414
(B) Attended in Year 2					
6-Week	0.171***	0.006	-0.166***	0.126*	-0.045
	(0.038)	(0.035)	(0.055)	(0.059)	(0.079)
1-Week	0.058	0.033	-0.025	0.118*	0.060
	(0.038)	(0.034)	(0.050)	(0.047)	(0.062)
Online	0.043+	0.012	-0.031	0.097***	0.054
	(0.022)	(0.023)	(0.038)	(0.027)	(0.043)
Control Mean	0.076	0.845	0.768	0.492	0.415
(C) Attended in Year 3					
6-Week	0.161***	-0.005	-0.166***	0.123*	-0.038
	(0.038)	(0.035)	(0.060)	(0.058)	(0.080)
1-Week	0.051	0.023	-0.029	0.128*	0.077
	(0.038)	(0.039)	(0.046)	(0.051)	(0.064)
Online	0.043+	0.005	-0.038	0.100***	0.057
	(0.022)	(0.021)	(0.036)	(0.032)	(0.048)
Control Mean	0.078	0.815	0.737	0.469	0.391
(D) Attended in Year 4					
6-Week	0.178***	0.072+	-0.107+	0.160***	-0.018
	(0.044)	(0.041)	(0.054)	(0.053)	(0.068)
1-Week	0.059	0.120*	0.061	0.177***	0.118+
	(0.039)	(0.044)	(0.043)	(0.048)	(0.060)
Online	0.047*	0.033+	-0.014	0.112***	0.065
	(0.022)	(0.017)	(0.031)	(0.031)	(0.046)
Control Mean	0.070	0.754	0.684	0.437	0.368

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). N = 2,084 for outcomes during the fourth year and prior, N = 1,335 for fifth year graduation, and N = 634 for sixth year graduation.

Table 3: The Impact of Assignment to STEM Summer Programs on Four-Year College Graduation

	Host Institution (1)	Any 4-Year College (2)	4-Year Excluding HI (3)	Barron's Most Competitive (4)	Barron's Most Comp. Excluding HI (5)
(A) Graduated by Year 4					
6-Week	0.146^{***}	0.082^{+}	-0.064	0.115^{+}	-0.031
	(0.035)	(0.048)	(0.054)	(0.061)	(0.069)
1-Week	0.040	0.080	0.040	0.099	0.059
	(0.036)	(0.056)	(0.050)	(0.066)	(0.064)
Online	0.033^{+}	0.016	-0.016	0.046	0.014
	(0.019)	(0.027)	(0.038)	(0.039)	(0.048)
Control Mean	0.065	0.532	0.468	0.368	0.303
(B) Graduated by Year 5					
6-Week	0.133^{*}	0.122^{+}	-0.010	0.160^{+}	0.028
	(0.051)	(0.061)	(0.064)	(0.083)	(0.085)
1-Week	0.022	0.163^{*}	0.141^{*}	0.176^{*}	0.154^{*}
	(0.049)	(0.072)	(0.059)	(0.080)	(0.071)
Online	0.024	0.082^{+}	0.058	0.117^{*}	0.093^{+}
	(0.027)	(0.047)	(0.048)	(0.049)	(0.050)
Control Mean	0.084	0.654	0.570	0.408	0.324
(C) Graduated by Year 6					
6-Week	0.204^{***}	0.009	-0.194^{*}	0.046	-0.158
	(0.060)	(0.062)	(0.068)	(0.124)	(0.105)
1-Week	-0.009	0.068^{+}	0.077	0.102	0.111^{+}
	(0.053)	(0.031)	(0.051)	(0.076)	(0.058)
Online	0.022^{*}	0.098^{***}	0.076^{*}	0.171^{***}	0.149^{*}
	(0.007)	(0.027)	(0.032)	(0.045)	(0.051)
Control Mean	0.086	0.736	0.651	0.431	0.346

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). N = 2,084 for outcomes during the fourth year and prior, N = 1,335 for fifth year graduation, and N = 634 for sixth year graduation.

Table 4: The Impact of Assignment to STEM Summer Programs on STEM and non-STEM Degrees

	Degree Within 4 Years				Degree Within 5 Years			
	Any Bachelors (1)	STEM (2)	Non-STEM (3)	Missing Major (4)	Any Bachelors (5)	STEM (6)	Non-STEM (7)	Missing Major (8)
(A) Any Four-Year Institution								
6-Week	0.082^{+}	0.127^{*}	-0.026	-0.019	0.122^{+}	0.202^{*}	-0.089	0.009
	(0.048)	(0.057)	(0.034)	(0.022)	(0.061)	(0.075)	(0.056)	(0.045)
1-Week	0.080	0.092	-0.005	-0.007	0.163^{*}	0.145^{+}	-0.011	0.029
	(0.056)	(0.059)	(0.027)	(0.024)	(0.072)	(0.083)	(0.050)	(0.048)
Online	0.016	0.034	-0.001	-0.017	0.082^{+}	0.045	0.014	0.022
	(0.027)	(0.026)	(0.018)	(0.018)	(0.047)	(0.045)	(0.028)	(0.040)
Control Mean	0.532	0.368	0.109	0.055	0.654	0.452	0.149	0.053
(B) Host Institution								
6-Week	0.146^{***}	0.145^{***}	0.001	-	0.133^{*}	0.141^{***}	-0.009	-
	(0.035)	(0.029)	(0.019)	-	(0.051)	(0.043)	(0.024)	-
1-Week	0.040	0.056	-0.016	-	0.022	0.056	-0.034^{+}	-
	(0.036)	(0.035)	(0.014)	-	(0.049)	(0.049)	(0.017)	-
Online	0.033^{+}	0.033^{+}	-0.000	-	0.024	0.031	-0.007^{+}	-
	(0.019)	(0.018)	(0.006)	-	(0.027)	(0.026)	(0.004)	-
Control Mean	0.065	0.051	0.014	0.000	0.084	0.064	0.020	0.000
(C) Other Institutions								
6-Week	-0.064	-0.018	-0.027	-0.019	-0.010	0.061	-0.080	0.009
	(0.054)	(0.055)	(0.031)	(0.022)	(0.064)	(0.071)	(0.050)	(0.045)
1-Week	0.040	0.036	0.011	-0.007	0.141^{*}	0.088	0.023	0.029
	(0.050)	(0.045)	(0.025)	(0.024)	(0.059)	(0.056)	(0.046)	(0.048)
Online	-0.016	0.001	-0.001	-0.017	0.058	0.015	0.021	0.022
	(0.038)	(0.029)	(0.017)	(0.018)	(0.048)	(0.041)	(0.028)	(0.040)
Control Mean	0.468	0.317	0.095	0.055	0.570	0.388	0.129	0.053

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$). N = 2,084 for outcomes during the fourth year and prior and N = 1,335 for fifth year graduation. Students are categorized as STEM if any of their degree majors are STEM. Degrees are categorized using CIP and, if CIP is unavailable, whether the degree is a Bachelor of Science. Students are categorized as STEM if at least one major is STEM. Students are categorized as non-STEM if none of their majors is STEM. Students who attained degrees, but have no degree major codes and did not attain Bachelors of Science, are categorized as Missing Major.

Table 5: The Impact of Assignment to STEM Summer Programs on Self-Reported College Applications and Admissions

	Any 4-Year (1)	Number of Institutions (2)	Number of Institutions Excluding HI (3)	One Institution College (4)	Barron's Most Competitive Except HI (5)	Barron's Less Competitive (6)	Technical School (7)	Technical School Except HI (8)	State Flagship (9)
(A) Applications									
6-Week	0.006	0.148	-0.179	-0.094***	0.113*	-0.029	0.247***	0.039	0.026
	(0.009)	(1.184)	(1.164)	(0.030)	(0.041)	(0.025)	(0.054)	(0.082)	(0.060)
1-Week	0.006	0.342	0.141	-0.041	0.062	-0.029	0.181***	0.101	0.027
	(0.010)	(0.791)	(0.766)	(0.029)	(0.049)	(0.020)	(0.041)	(0.066)	(0.060)
Online	-0.003	0.822+	0.571	-0.032*	0.043+	-0.015	0.182***	0.080*	0.026
	(0.007)	(0.406)	(0.397)	(0.013)	(0.023)	(0.012)	(0.029)	(0.038)	(0.036)
Control Mean	0.992	8.480	8.078	0.077	0.841	0.059	0.600	0.458	0.473
Observations	1418	1354	1354	1354	1407	1407	1407	1407	1407
(B) Admissions									
6-Week	0.006	0.002	-0.154	-0.069	0.103+	-0.018	0.145*	0.011	0.063
	(0.009)	(0.792)	(0.780)	(0.043)	(0.054)	(0.025)	(0.066)	(0.072)	(0.065)
1-Week	0.006	0.322	0.267	-0.012	0.096	-0.020	0.106+	0.084	0.036
	(0.010)	(0.702)	(0.688)	(0.031)	(0.060)	(0.019)	(0.062)	(0.062)	(0.067)
Online	-0.003	0.276	0.209	-0.015	0.081*	-0.014	0.093*	0.035	0.026
	(0.007)	(0.281)	(0.268)	(0.018)	(0.030)	(0.013)	(0.044)	(0.043)	(0.044)
Control Mean	0.992	5.382	5.226	0.124	0.688	0.055	0.475	0.398	0.418
Observations	1418	1400	1400	1400	1402	1402	1402	1402	1402

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). Data are from surveys conducted at the end of the senior year of high school. The any application and any admission outcomes were created from yes or no questions and available for all survey respondents. The number of applications and admissions outcomes in Columns 2 through 4 were calculated for respondents who provided their full list of applications and admissions, respectively. The applications and admissions to types of institutions outcomes in Columns 5 through 9 are populated for respondents who provided at least a partial list of applications and admissions, respectively.

Table 6: The Impact of Assignment to STEM Summer Programs on AP/IB Courses and Skills

	Panel A: Plans to Take AP or IB Courses					
	Any Course (1)	Number of courses (2)	STEM (3)	Science (4)	Computer Science (5)	Math (6)
6-Week	0.063	0.140	0.087^{+}	0.026	0.097^{+}	-0.050
	(0.043)	(0.322)	(0.045)	(0.071)	(0.050)	(0.071)
1-Week	0.112*	0.713^{+}	0.149***	0.058	0.078	0.060
	(0.041)	(0.351)	(0.045)	(0.067)	(0.057)	(0.067)
Online	0.027^{+}	0.284	0.050*	0.008	0.073	0.025
	(0.015)	(0.187)	(0.021)	(0.042)	(0.047)	(0.042)
Control Mean	0.891	4.281	0.842	0.736	0.104	0.734
Observations	1057	1057	1057	1057	1057	1057
	Panel B: Skills and Confidence					
	Calculus Question (7)	Life Skills Index (8)	Study Skills Index (9)	Confidence Index (10)	Likes Intellectual Activities (11)	Attention Span (12)
6-Week	0.029	0.378*	0.441***	0.028	0.171	-0.208
	(0.066)	(0.141)	(0.125)	(0.165)	(0.139)	(0.152)
1-Week	0.036	0.267^{+}	0.369***	0.134	0.065	-0.341*
	(0.051)	(0.143)	(0.088)	(0.143)	(0.117)	(0.128)
Online	0.039	0.116	0.268***	-0.017	0.096	-0.224*
	(0.038)	(0.100)	(0.058)	(0.105)	(0.077)	(0.090)
Control Mean	0.213	-0.029	-0.034	-0.044	-0.029	0.085
Observations	1018	1388	1380	1031	1033	1379

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). Data are from surveys conducted post program, in the fall of the senior year of high school. Data are only available for the 2015 and 2016 cohorts for AP/IB and confidence and likes intellectual activities outcomes.

Online Appendix

STEM Summer Programs for Underrepresented Youth Increase STEM Degrees

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September 2022

Appendix A: Randomization and data details

This appendix describes the application and randomization process in more detail. It also includes more information on the surveys and other outcomes, as well as additional tables and figures.

A.1 Randomization details

Below, we describe the details of the randomization process for each cohort. See Figures 2a through 2c for a general overview of the randomization design and the number of applicants allocated to program spots. Randomization processes were slightly different across years, reflecting different operational preferences and leadership over time. Key participants in the selection process are staff in the HI admissions offices. Typically, these employees work to recruit and select the freshman class at the HI; each year they also help winnow the large pool of applications to the summer program from about 2,000 to about 700. Program selection committees also evaluate applications. They consist of program affiliates—alumni, program staff, community members, and professors who participate in the selection process after the admissions office conducts its initial sort. Prior to randomization years, the applicants who scored highest on selection committee ratings were generally admitted to the program, alongside operational criteria (for example, gender balance or the need to admit students from certain locations to maintain regional representation). During randomization, selection committee ratings, alongside admissions office ratings, as well as regional priority criteria and gender, were used to allocate students to blocks for random assignment.

The number of students admitted to each of the programs varies over time. This reflects different operational constraints each year, as well as an increasing willingness on the part of the program staff to offer a few extra seats in the six-week program to account for the small number of students who declined offers each year. Each year a few applicants received "certainty spots" where admission to a program was guaranteed for program operational reasons. These students are excluded from the impact analysis.

A.1.1 Cohort 1 (2014) randomization details

The research team randomized admission to the summer programs with a block randomization design, with applicants assigned to three blocks and then randomized within blocks. The assignment process proceeded in the following steps during winter and spring of 2014:

1. The HI admissions office selected 674 applicants to move on to the program selection committee.

2. The program leadership separated the remaining applicants into regional groupings with about 30 applicants in each group. Each regional group was reviewed by a selection committee of two or three people, and the applicants were rank ordered within their group.

3. The HI admissions office reread the applications and assessed each applicant for their ability to complete the six-week program. Each applicant was tagged with a numeric variable representing a rating of yes, no, or maybe.

4. The research team combined the selection committee ranking and the HI admissions office vote into a weighted rank that program staff approved of because it supported the regional balance they wished to maintain in their programs.

5. Students were randomized to programs within randomization blocks defined by these rankings, for a total of three blocks. Top-ranked students were randomized between the six-week program and the one-week program. The group with the next highest rankings was randomized between the one-week program and the online program, and the final group was randomized between the online program and a control group. Block cutoffs were chosen to ensure appropriate program size.

    - Because there were fewer female applicants than male applicants and the program office wanted gender balance in its programs, we used gender as a stratum within the block randomization. Thus, there were different rankings cutoffs for male and female applicants.

6. Program staff reserved 19 spots in the six-week programs as "certainty" spots, which program staff chose to use to ensure representation from urban areas. The certainty spots were filled by taking the highest-ranked candidates from priority urban areas.

A.1.2 Cohort 2 (2015) randomization details

A staff member of the institutional research office of the HI randomized admission to the summer programs with a block randomization design, with applicants assigned to two blocks and then randomized within blocks. We highlight a few major differences from the 2014 randomization here, which were applied to the 2015 and 2016 randomization processes. The research team did not directly conduct the randomization; instead, a member of the HI institutional research office did. This was at the request of the Institutional Review Board. Additionally, the process with the admissions office and selection committee was streamlined, so that the admissions office scored applications before they were passed to the selection committees rather than the iterative process used in 2014. The admissions office and the selection committees offered more detailed ranking variables than in 2014. There were fewer certainty spots. Most importantly, the number of randomization blocks was reduced from three to two, making comparisons across blocks more plausible. This was to simplify operations and strengthen the research design. While full randomization would have been ideal, the outreach office was concerned that the most qualified candidates might have received no program and that relatively less qualified candidates might have received more intensive interventions.

The assignment process proceeded in the following steps during spring 2015:

1. The HI admissions office selected 701 applicants to move on to the program selection committee. At this time, they gave a yes/no recommendation for admission to the six-week program, and supplied a personal and academic rating score.

2. The program leadership separated the remaining applicants into regional groupings with about 30 applicants in each group. Each regional group was reviewed by a selection committee of two people, and the applications received several scores, including a yes/maybe/no recommendation for the six-week program, an academic score, a personal score, and a "top 5" indicator (for applicants considered one of the top 5 reviewed by each reviewer). Each selection committee also selected a top 5 jointly.

3. The HI institutional research staff member, in consultation with the research team, combined the selection committee rankings and the HI admissions office rankings into a weighted rank.

The weighted rank also included priorities for students from certain states or territories; this was to ensure representation from across the United States in the program.

4. Students were randomized to programs within randomization blocks defined by these rankings, for a total of two blocks. Top-ranked students were randomized between the six-week program, the one-week program, and the online program. The next group was randomized between the online program and a control group. Block cutoffs were chosen to ensure appropriate program size.
    - Because there were fewer female applicants than male applicants and the program office wanted gender balance in its programs, we used gender as a stratum within the block randomization. Thus, there were different rankings cutoffs for male and female applicants.
    - Program staff imposed a math standardized test score for eligibility for Block 1. An applicant needed to score above one of the following cutoffs to be eligible for Block 1:
        - SAT: 550
        - PSAT: 55
        - ACT: 24
        - PLAN: 24
        - A small number of applicants were shifted from Block 1 to Block 2 due to not meeting the test score criteria. Applicants who were missing scores were allowed to be placed in Block 1.
5. Four students were offered seats in the six-week program in certainty spots.
6. The HI institutional research staff member ran many randomization scenarios: about 50 scenarios that met the program staff geographical preferences and demonstrated covariate balance were offered to the program staff as potential final randomization scenarios. The research team suggested a scenario that demonstrated covariate balance and the program staff agreed to that scenario.

A.1.3 Cohort 3 (2016) randomization details

A staff member of the institutional research office of the HI randomized admission to the summer programs with a block randomization design, with applicants assigned to two blocks and then randomized within blocks. The changes from the 2014 to the 2015 randomization process remained in place, with minor alterations noted below. The assignment process proceeded in the following steps during spring 2016:

1. The HI admissions office selected 749 applicants to move on to the program selection committee. At this time, they gave a yes/no recommendation for admission to the six-week program and supplied a personal and academic rating score.
2. The program leadership separated the remaining applicants into regional groupings with about 30 applicants in each group. Each regional group was reviewed by a selection committee of two people, and the applications received several scores, including a yes/no recommendation for the six-week program, an academic score, a personal score, and a top 5 indicator (for

applicants considered one of the top 5 reviewed by each reviewer). Each selection committee also selected a top 5 jointly.

3. The HI institutional research staff member, in consultation with the research team, combined the selection committee rankings and the HI admissions office rankings into a weighted rank. The weighted rank also included priorities for students from certain states or territories; this was to ensure representation from across the United States in the program.

4. Students were randomized to programs within randomization blocks defined by these rankings, for a total of two blocks. Top ranked students were randomized between the six-week program, the one-week program, and the online program. The next group was randomized between the online program and a control group. Block cutoffs were chosen to ensure appropriate program size.

    - Because there were fewer female applicants than male applicants and the program office wanted gender balance in its programs, we used gender as a stratum within the block randomization. Thus, there were different rankings cutoffs for male and female applicants.
    - Program staff imposed a math standardized test score for eligibility for Block 1. An applicant needed to score above one of the following cutoffs to be eligible for Block 1:
        - SAT: 550
        - Old PSAT: 55
        - New PSAT: 525
        - ACT: 24
        - PLAN: 24
        - ASPIRE: 432
        - All applicants who submitted test scores met the cutoffs. Applicants who were missing scores were allowed to be placed in Block 1.

5. Program staff reserved one spot in Block 1, which program staff chose to use to ensure an applicant who previously participated in programs for middle and high school students sponsored by the program office received a spot in one of the programs. This student was randomly assigned to the online program. (Other students also participated in the prior program; however, the rest of them received rating scores high enough that they were all in Block 1 without intervention.) Two other students received a certainty spot in the six-week program and three in the one-week program due to programmatic considerations.

6. The HI institutional research staff member ran many randomization scenarios: about 50 scenarios that met the program staff geographical preferences and demonstrated covariate balance were offered to the program staff as potential final randomization scenarios. The research team suggested a scenario that demonstrated covariate balance and the program staff agreed to that scenario.

A.1.4 Covariate balance

Table A.1 summarizes Tables A.14 through A.16 (shown later in this appendix) and reports the p-values from joint hypothesis tests of equality of coefficients within randomization blocks, for each randomization block by cohort. The generally high p-values show that randomization produced treatment and control groups that were very similar in terms of demographic characteristics. This is not surprising, of course, because the randomization process included criteria for covariate balance. We do not expect student characteristics to be similar across blocks, as by definition blocks are defined by applicant characteristics.

Table A.1: Covariate Balance: Summary of P-Values for Joint Hypothesis Tests of Strata-Adjusted Mean Differences

	6-Week vs. 1-Week (1)	6-Week vs. Online (2)	1-Week vs. Online (3)	Online vs. Control (4)
2014 Cohort	0.980	-	0.943	0.421
2015 Cohort	0.844	0.934	0.987	0.498
2016 Cohort	0.924	0.218	0.563	0.891

Notes: This table shows p-values for test of joint-significance of strata-adjusted within-block mean comparisons for baseline covariates: race/ethnicity, free and reduced-price lunch status, and standardized math score and GPA. See Online Tables A.14 through A.16 for details and sample sizes.

A.1.5 Take-up

Most students assigned to a program ultimately participated in the program. Program staff generally did not permit students to switch programs, but in 4 cases (2 in 2015 and 2 in 2016), students who were assigned to the 6-week program were switched to the online program (2015) or one-week program (2016). These students are included in their original assignment in the intent-to-treat analysis. Across program years, 87 percent of students assigned to the six-week program ultimately participated; 85 percent of students assigned to the one-week program, and 77 percent of student assigned to the online program participated (Online Appendix Table A.2). No students in the control group were permitted to attend the program.

Table A.2: Program Attendance by Program Assignment

	6-Week (1)	1-Week (2)	Online (3)	Control (4)	All (5)
Attended 6-Week	0.87	0.00	0.00	0.00	0.13
Attended 1-Week	0.01	0.85	0.00	0.00	0.15
Attended online	0.01	0.00	0.77	0.00	0.18
Control	0.00	0.00	0.00	1.00	0.44
N	231	308	472	1073	2084

Notes: This table displays program-takeup rates. Columns 1 through 4 show the share of applicants who attended a program, according to the program office, by the program they were assigned to. Column 5 shows takeup across the entire sample.

A.1.6 Validating the Experiment

In Section 5.5, we detail several exercises that we conduct to show that our modified random assignment structure generates valid causal estimates of an offer to each of the STEM summer programs. Below, we also discuss whether heterogeneous treatment effects in the context of modified random assignment present a threat to validity. The figures and tables associated with both these analyses are displayed here.

A.1.6.1 Heterogeneous treatment effects

Having shown in Section 5.5 that the blocking strategy accounts for selection bias, the second major threat to the validity of the evidence here is that program effects are driven by heterogeneous treatment effects. If our findings are driven by, for example, the highest rated students, this might imply that our estimates based on a linear functional form are not a good estimate of program effectiveness, as we do not have similarly rated students in the control group to compare to. Below, we present several ways to consider this possibility.

Our first strategy to determine if differential response by highly-rated students accounts for our findings comes from cross-block comparisons. We take advantage of the fact that the online program is assigned in both the higher-rated Block 1 and the lower-rated Block 2. We then compare each of the two online groups to the control group separately, controlling for the rating variable and removing the block-specific randomization strata and substituting alternative strata that include cohort, gender, and location. If the block-inclusive strata fully capture the differences between the two groups (as we show above)—and there are not heterogeneous treatment effects—we would expect estimates of each online group to be identical to each other, and to the main estimates. Alternatively, if heterogeneous treatment effects are driving our findings, we would expect the estimates for the online group across blocks to differ, for example, if higher-rated applicants benefited more from the program, then the estimates for Block 1 online vs. control should be larger than those for Block 2 online vs. control.

Online Appendix Tables A.7 and A.8 show the estimates for outcomes at all institutions and the HI from this strategy. Panel A reproduces the main estimation results for the online group for reference. Panel B compares the online group in Block 1 (the higher-rated group) to the control group in Block 2 (the lower-rated group), controlling for rating variable, and Panel C compares the within Block 2 (the lower-rated group) difference between online and control. Note that the two estimates in Panels B and C will not average out to the exact estimate in Panel A, as we control for a slightly different set of covariates intentionally. For program effects at all and elite colleges (Online Appendix Table A.8), estimates from Panels B and C are broadly similar to each other, though there are some differences. However, those differences, if anything, point *against* highly rated students benefiting more from the program. For example, the online impact in Block 1 indicates a *lower* likelihood of attending an elite institution, whereas the online program effect in Block 2 is a positive 9 percentage points for attending elite institutions. Estimates of treatment effects at the HI from Panels B and C are very similar to each other, and to the main estimate (Online Appendix Table A.8). Panel D in both tables shows the difference between the two estimates—none of which is statistically significant. We take this as evidence that—for the one program where we observe students in both rating variable groups—there is no evidence that our positive impacts are driven by higher-rated students responding to programs to a greater extent.

Online Appendix Tables A.9 and A.10 display two more ways we consider the possibility of heterogeneous response for highly rated students both at all institutions and the HI. We re-weight our estimates by the inverse of the rating variable (adjusted so there are no negative values) in Panel B of these tables. This effectively increases the contribution of lower-ranked students and decreases the contribution of higher-ranked students in comparison to the main estimates (Panel A). If our findings were driven by heterogeneous treatment effects by rating, this re-weighting scheme would de-emphasize those differences, changing our results. However, the estimates in Panel B are quite similar showing little evidence that heterogeneity by rating variable is driving the results. In Panel C, we truncate the control group, removing the relatively lower-rated half of the control group. If treatment effects differ by rating, we would expect this comparison group to yield different findings.[21] The results are of smaller magnitude, but overall, the findings are generally similar to the main results.

In Online Appendix Tables A.11 and A.12, we also consider heterogeneous treatment effects within Block 1 in the 2015 and 2016 cohorts at all institutions and at the HI, respectively.[22] Within Block 1, all students are relatively highly rated, and there is no control group. Thus, having shown above that treatment effects are constant for the online program, we use this as a comparison group and split the sample between the highest-rated students and the lower-rated students.

First, for comparison purposes, Panel A shows the treatment estimates limited to the 2015 and 2016 cohorts, and Panel B shows these estimates limited only to Block 1, where the comparison group is the online treatment. Panel C shows a version of the estimates where there are separate treatment indicators for the top-rated students in the six- and one-week programs and the bottom-rated students in the six- and one-week program. The strata are adjusted to include an indicator variable for being a higher-rated student (within this highly rated group). The program variables can then be interpreted as the treatment effect for students of each type.

Impacts are very similar for relatively higher and relatively lower-rated students for the six-week program at the HI (online Appendix Table A.12). The only variable that looks different is application to the HI, likely because the comparison means differ by rating variable. Thus the larger impact for the lower-rated group essentially brings both groups to the same level of application. When it comes to impacts on enrollment and graduation, treatment effects are consistently similar for both groups. However, when looking at graduation outcomes at larger groups of institutions, or STEM degree impacts, treatment effects are larger for the higher-rated group in the six-week program, though only the difference in overall graduation is statistically significant.

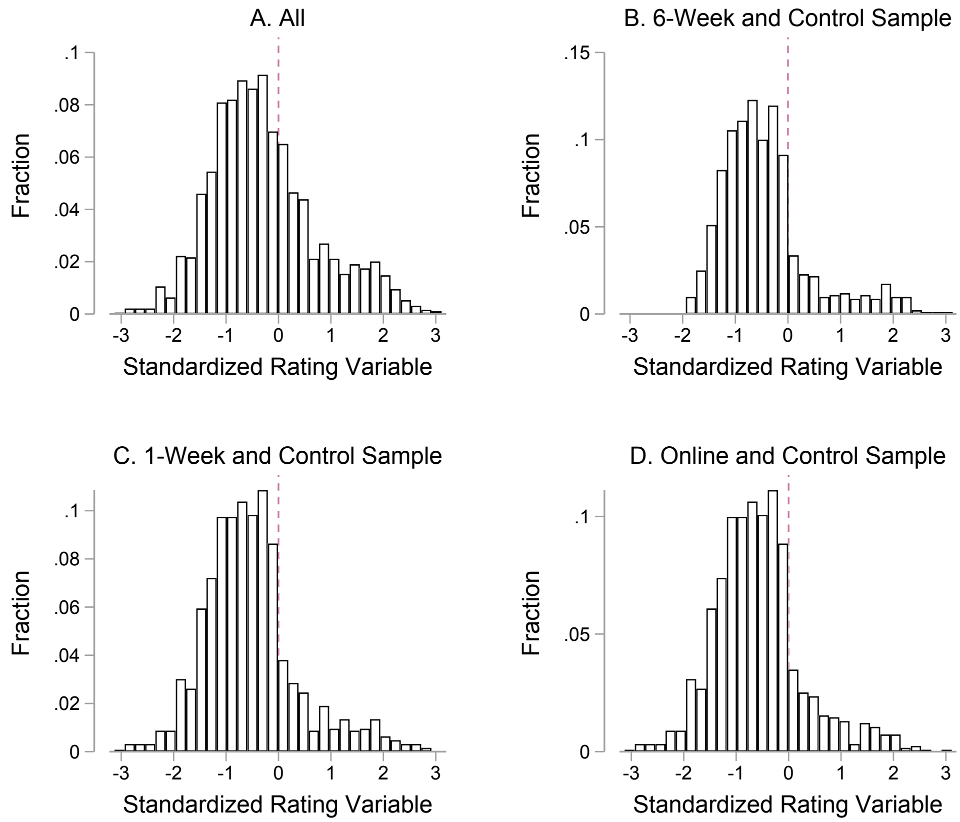
This pattern is reversed for the one-week program. At all institutions, the one-week program has similar impacts for both higher- and lower-rated students, with perhaps larger impacts at elite institutions for lower rated students. But at the HI, relatively higher-rated students admitted to the one-week program are more likely to be accepted at, attend, and graduate from the HI, compared to lower-rated students. The difference is only statistically significant for the attendance in year one, and, given small sample sizes and relatively smaller treatment effects for the one-week

[21] This strategy is reminiscent of a regression discontinuity. Given that we have the rating variable that fully determines whether a student is assigned to Block 1 (and guaranteed offer of a program) and Block 2 (with some probability of a control group), a natural extension would be to re-estimate program effects using a regression discontinuity approach. We do not do so, however, because our program assignment structure necessarily means that the conditional expectation function does not move smoothly across the assignment threshold, violating the assumptions that underlie regression discontinuity estimation. Online Appendix Figure A.1 shows the distribution of the rating variable over the threshold for the cases that would be relevant to regression discontinuity estimation for each program (Panels B through D).

[22] These are the years during which treatment assignment for higher-rated applicants occurs within one large block.

program, few estimates are statistically significant. Overall, we take this group of findings to mean that while there may be heterogeneous treatment effects for some programs for some outcomes, there is no consistent pattern where only higher-rated or only lower-rated students benefit from the program. Thus our exploration of selection bias and heterogeneous treatment effects supports our use of estimates from Equation 1 throughout our analysis.

Figure A.1: Distribution of Rating Scores



Notes: This figure displays the standardized rating score. The score is centered at the break between Blocks 1 and 2, and is standardized within cohort and gender. Panel A includes all students in the sample who meet the test score cutoff. Panels B through D show the relevant samples for a proposed regression discontinuity analysis. Panel B shows the six-week program (to the right of 0) and the control group (to the left of 0), but omits the 2014 cohort (which had a different blocking structure). Panel C shows the one-week program (to the right of 0) and the control group (to the left of 0). Panel D shows the online program (to the right of 0) and the control group (to the left of 0), omitting students assigned to the online program with rating scores below zero.

Table A.3: Alternative Design Controls

	Attended 4-Year in Y1 (1)	Attended Barron's Most Comp. in Y1 (2)	Graduated 4-Year by Y4 (3)	Graduated Barron's Most Comp. by Y4 (4)	STEM Degree by Y4 (5)
(A) Biased Estimate (No Blocks)					
6-Week	0.023	0.194***	0.045	0.132***	0.126***
	(0.024)	(0.034)	(0.036)	(0.036)	(0.036)
1-Week	0.034	0.167***	0.062+	0.118***	0.101***
	(0.021)	(0.033)	(0.033)	(0.033)	(0.033)
Online	0.014	0.097***	0.001	0.043	0.021
	(0.018)	(0.028)	(0.028)	(0.027)	(0.027)
(B) Main Specification					
6-Week	0.038	0.172***	0.082	0.115+	0.144*
	(0.041)	(0.059)	(0.061)	(0.060)	(0.060)
1-Week	0.042	0.136*	0.080	0.099+	0.107+
	(0.037)	(0.056)	(0.057)	(0.056)	(0.056)
Online	0.020	0.095***	0.016	0.046	0.031
	(0.024)	(0.036)	(0.036)	(0.035)	(0.035)
(C)Main Spec + Control for Rating					
6-Week	0.038	0.167***	0.082	0.112+	0.143*
	(0.041)	(0.059)	(0.061)	(0.060)	(0.061)
1-Week	0.042	0.128*	0.080	0.095+	0.105+
	(0.037)	(0.056)	(0.057)	(0.056)	(0.057)
Online	0.020	0.089*	0.017	0.044	0.030
	(0.024)	(0.035)	(0.036)	(0.035)	(0.035)
(D) Control for Rating, No Blocks					
6-Week	0.037	0.074+	0.059	0.077+	0.101*
	(0.030)	(0.043)	(0.045)	(0.045)	(0.045)
1-Week	0.047+	0.052	0.076+	0.065	0.077+
	(0.027)	(0.041)	(0.042)	(0.042)	(0.042)
Online	0.020	0.046	0.007	0.020	0.011
	(0.020)	(0.030)	(0.030)	(0.029)	(0.029)

Notes: The notes for this table are the same as the notes for Table B.2 for Panel B. Panel A removes blocks from the main estimate, reflecting a biased estimate of program effects. Panel C add a control for the rating variable to Panel B. Panel D removes blocks but retains the control for rating variable. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). N = 2,084.

Table A.4: Alternative Design Controls, HI Outcomes

	Applied to HI (1)	Accepted to HI (2)	Attended HI First Year (3)	Attended HI Fourth Year (4)	Graduated HI Within 4 Years (5)
(A) Biased Estimate (No Blocks)					
6-Week	0.450***	0.316***	0.218***	0.217***	0.201***
	(0.031)	(0.032)	(0.031)	(0.031)	(0.029)
1-Week	0.381***	0.207***	0.096***	0.092***	0.091***
	(0.030)	(0.027)	(0.023)	(0.022)	(0.021)
Online	0.334***	0.128***	0.052***	0.054***	0.049***
	(0.026)	(0.020)	(0.017)	(0.017)	(0.016)
(B) Main Specification					
6-Week	0.464***	0.207***	0.169***	0.178***	0.146***
	(0.057)	(0.051)	(0.046)	(0.045)	(0.043)
1-Week	0.398***	0.105*	0.053	0.059	0.040
	(0.054)	(0.046)	(0.039)	(0.039)	(0.036)
Online	0.352***	0.088***	0.038+	0.047*	0.033+
	(0.034)	(0.024)	(0.021)	(0.020)	(0.018)
(C)Main Spec + Control for Rating					
6-Week	0.462***	0.203***	0.167***	0.176***	0.144***
	(0.057)	(0.051)	(0.046)	(0.045)	(0.042)
1-Week	0.395***	0.099*	0.049	0.056	0.037
	(0.054)	(0.045)	(0.039)	(0.038)	(0.036)
Online	0.350***	0.084***	0.035+	0.045*	0.030+
	(0.034)	(0.024)	(0.020)	(0.020)	(0.018)
(D) Control for Rating, No Blocks					
6-Week	0.416***	0.198***	0.142***	0.154***	0.133***
	(0.041)	(0.038)	(0.036)	(0.035)	(0.033)
1-Week	0.348***	0.093***	0.023	0.031	0.025
	(0.039)	(0.033)	(0.028)	(0.028)	(0.026)
Online	0.320***	0.078***	0.019	0.027	0.020
	(0.029)	(0.020)	(0.018)	(0.018)	(0.016)

Notes: The notes for this table are the same as the notes for Table B.2 for Panel B. Panel A removes blocks from the main estimate, reflecting a biased estimate of program effects. Panel C add a control for the rating variable to Panel B. Panel D removes blocks but retains the control for rating variable. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). N = 2,084.

Table A.5: The Impact of Assignment to STEM Summer Programs on Key Outcomes, Alternative Specifications

	Attended 4-Year in Y1 (1)	Attended Barron's Most Comp. in Y1 (2)	Graduated 4-Year by Y4 (3)	Graduated Barron's Most Comp. by Y4 (4)	STEM Degree by Y4 (5)
(A) No baseline controls					
6-Week	0.038	0.177*	0.082	0.114	0.142*
	(0.025)	(0.073)	(0.056)	(0.068)	(0.061)
1-Week	0.042	0.133*	0.080	0.095	0.107
	(0.031)	(0.063)	(0.061)	(0.069)	(0.064)
Online	0.021	0.098*	0.021	0.049	0.037
	(0.015)	(0.037)	(0.031)	(0.039)	(0.031)
Control Mean	0.867	0.494	0.532	0.368	0.350
(B) Excluding 2014					
6-Week	0.031	0.140+	0.070	0.114+	0.131*
	(0.023)	(0.069)	(0.054)	(0.062)	(0.057)
1-Week	0.066+	0.128+	0.052	0.094	0.070
	(0.036)	(0.063)	(0.071)	(0.063)	(0.061)
Online	0.010	0.054	-0.020	0.004	-0.010
	(0.020)	(0.037)	(0.034)	(0.044)	(0.031)
Control Mean	0.872	0.517	0.532	0.367	0.349
(C) Excluding 2015					
6-Week	0.004	0.139	0.062	0.080	0.110
	(0.032)	(0.088)	(0.069)	(0.082)	(0.069)
1-Week	-0.012	0.066	0.059	0.053	0.115
	(0.038)	(0.079)	(0.058)	(0.083)	(0.077)
Online	0.010	0.087+	0.018	0.050	0.053*
	(0.020)	(0.049)	(0.026)	(0.045)	(0.022)
Control Mean	0.878	0.502	0.525	0.364	0.352
(D) Excluding 2016					
6-Week	0.075+	0.233***	0.094+	0.127	0.180*
	(0.037)	(0.081)	(0.050)	(0.084)	(0.075)
1-Week	0.064	0.201***	0.105	0.126	0.122
	(0.040)	(0.069)	(0.071)	(0.087)	(0.078)
Online	0.037+	0.139***	0.046	0.078	0.049
	(0.021)	(0.032)	(0.034)	(0.048)	(0.035)
Control Mean	0.850	0.460	0.542	0.375	0.350

Notes: This table shows alternative specifications to main specification. Panel A (N = 2,084) omits control variables but retains randomization strata. Panels B (N = 1,450), C (N = 1,383), and D (N = 1,335) omit each cohort in turn. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

Table A.6: The Impact of Assignment to STEM Summer Programs on Key HI Outcomes, Alternative Specifications

	Applied to HI (1)	Accepted to HI (2)	Attended HI First Year (3)	Attended HI Fourth Year (4)	Graduated HI Within 4 Years (5)
(A) No baseline controls					
6-Week	0.457***	0.212***	0.171***	0.179***	0.145***
	(0.053)	(0.048)	(0.045)	(0.047)	(0.038)
1-Week	0.396***	0.103*	0.051	0.057	0.038
	(0.055)	(0.044)	(0.038)	(0.040)	(0.037)
Online	0.354***	0.092***	0.039+	0.048*	0.034
	(0.025)	(0.029)	(0.021)	(0.021)	(0.020)
Control Mean	0.312	0.106	0.080	0.070	0.065
(B) Excluding 2014					
6-Week	0.505***	0.217***	0.167***	0.182***	0.153***
	(0.056)	(0.052)	(0.048)	(0.053)	(0.043)
1-Week	0.461***	0.137*	0.100+	0.100+	0.071
	(0.063)	(0.057)	(0.049)	(0.052)	(0.048)
Online	0.372***	0.099*	0.052+	0.062*	0.039
	(0.029)	(0.039)	(0.027)	(0.029)	(0.028)
Control Mean	0.282	0.094	0.072	0.064	0.054
(C) Excluding 2015					
6-Week	0.438***	0.257***	0.219***	0.231***	0.166***
	(0.060)	(0.038)	(0.051)	(0.057)	(0.036)
1-Week	0.381***	0.123*	0.030	0.054	0.024
	(0.073)	(0.044)	(0.034)	(0.038)	(0.034)
Online	0.331***	0.104***	0.041*	0.053*	0.031*
	(0.018)	(0.023)	(0.017)	(0.019)	(0.011)
Control Mean	0.327	0.095	0.077	0.063	0.060
(D) Excluding 2016					
6-Week	0.435***	0.142*	0.127*	0.123*	0.117*
	(0.066)	(0.052)	(0.048)	(0.044)	(0.049)
1-Week	0.351***	0.047	0.027	0.022	0.021
	(0.059)	(0.050)	(0.048)	(0.049)	(0.048)
Online	0.353***	0.059+	0.019	0.025	0.026
	(0.033)	(0.034)	(0.026)	(0.027)	(0.026)
Control Mean	0.331	0.131	0.093	0.084	0.081

Notes: This table shows alternative specifications to main specification. Panel A (N = 2,084) omits control variables but retains randomization strata. Panels B (N = 1,450), C (N = 1,383), and D (N = 1,335) omit each cohort in turn. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

Table A.7: Comparison of Key Outcomes Between Online and Control Conditions

	Attended 4-Year in Y1 (1)	Attended Barron's Most Comp. in Y1 (2)	Graduated 4-Year by Y4 (3)	Graduated Barron's Most Comp. by Y4 (4)	STEM Degree by Y4 (5)
(A) Main Specification (N = 2,084)					
Online	0.020	0.095*	0.016	0.046	0.031
	(0.015)	(0.035)	(0.027)	(0.039)	(0.027)
(B) Block 1 Online vs. Block 2 Control (N = 1,327)					
Online	0.051	-0.024	0.029	0.000	-0.008
	(0.032)	(0.051)	(0.053)	(0.063)	(0.059)
(C) Block 2 Online vs. Block 2 Control (N = 1,291)					
Online	0.021	0.091*	0.016	0.044	0.031
	(0.016)	(0.037)	(0.029)	(0.041)	(0.028)
(D) Block 1 Online vs. Block 2 Online (N = 472)					
Block 1 Online	0.062	-0.109	0.105	0.014	0.073
	(0.047)	(0.091)	(0.069)	(0.067)	(0.055)

Notes: Panel A repeats the main specification as a reference. Panels B, C, and D show alternative comparisons for the online program by block to assess the efficacy of the estimation strategy. All regressions in B, C and D control for alternative strata constructed using gender, geographic location, meeting a minimum test score threshold and cohort, as well as a vector of characteristics including program admissions committee rating variable, GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. These alternative strata do not include the block variables to permit comparisons across blocks. The sample includes STEM summer program applicants who applied in 2014, 2015 and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$).

Table A.8: Comparison of Key HI Outcomes Between Online and Control Conditions

	Applied to HI (1)	Accepted to HI (2)	Attended HI First Year (3)	Attended HI Fourth Year (4)	Graduated HI Within 4 Years (5)
(A) Main Specification (N = 2,084)					
Online	0.352^{***}	0.088^{***}	0.038^{+}	0.047^{*}	0.033^{+}
	(0.024)	(0.028)	(0.020)	(0.022)	(0.019)
(B) Block 1 Online vs. Block 2 Control (N = 1,327)					
Online	0.235^{***}	0.096^{***}	0.020	0.027	0.031
	(0.065)	(0.025)	(0.023)	(0.027)	(0.018)
(C) Block 2 Online vs. Block 2 Control (N = 1,291)					
Online	0.350^{***}	0.088^{***}	0.037^{+}	0.046^{*}	0.032
	(0.024)	(0.027)	(0.020)	(0.021)	(0.019)
(D) Block 1 Online vs. Block 2 Online (N = 472)					
Block 1 Online	-0.053	-0.035	-0.012	-0.010	-0.000
	(0.077)	(0.060)	(0.050)	(0.057)	(0.050)

Notes: Panel A repeats the main specification as a reference. Panels B, C, and D show alternative comparisons for the online program by block to assess the efficacy of the estimation strategy. All regressions in B, C and D control for alternative strata constructed using gender, geographic location, meeting a minimum test score threshold and cohort, as well as a vector of characteristics including program admissions committee rating variable, GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. These alternative strata do not include the block variables to permit comparisons across blocks. The sample includes STEM summer program applicants who applied in 2014, 2015 and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

Table A.9: The Impact of Assignment to STEM Summer Programs on Key Outcomes, Alternative Estimates

	Attended 4-Year in Y1 (1)	Attended Barron's Most Comp. in Y1 (2)	Graduated 4-Year by Y4 (3)	Graduated Barron's Most Comp. by Y4 (4)	STEM Degree by Y4 (5)
(A) Main Specification (Unweighted)					
6-Week	0.038	0.172*	0.082+	0.115+	0.144*
	(0.025)	(0.065)	(0.048)	(0.061)	(0.056)
1-Week	0.042	0.136*	0.080	0.099	0.107+
	(0.031)	(0.060)	(0.056)	(0.066)	(0.059)
Online	0.020	0.095*	0.016	0.046	0.031
	(0.015)	(0.035)	(0.027)	(0.039)	(0.027)
Control Mean	0.867	0.494	0.532	0.368	0.350
(B) Inverse Rating Weighted					
6-Week	0.034	0.176***	0.077	0.108+	0.148*
	(0.041)	(0.061)	(0.062)	(0.061)	(0.061)
1-Week	0.040	0.140*	0.077	0.095+	0.109+
	(0.037)	(0.057)	(0.058)	(0.057)	(0.058)
Online	0.021	0.096***	0.020	0.046	0.038
	(0.024)	(0.037)	(0.037)	(0.035)	(0.036)
Control Mean	0.870	0.474	0.528	0.354	0.346
(C) Vs. Highest Rated Controls					
6-Week	0.021	0.125*	0.044	0.084	0.089
	(0.043)	(0.062)	(0.064)	(0.062)	(0.063)
1-Week	0.024	0.092	0.044	0.070	0.053
	(0.040)	(0.058)	(0.060)	(0.058)	(0.059)
Online	0.002	0.050	-0.020	0.017	-0.023
	(0.027)	(0.039)	(0.040)	(0.039)	(0.039)
Control Mean	0.880	0.561	0.581	0.425	0.409

Notes: The notes for this table are the same as the notes for Table B.2 for Panel A. Panel B modifies the main specification to weight the regression with weights inverse to the rating variable used to assign applicants to blocks (N = 2,084). Panel C limits control group members to those in the top half of control group ratings (N = 1,544). Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

Table A.10: The Impact of Assignment to STEM Summer Programs on Key HI Outcomes, Alternative Estimates

	Applied to HI (1)	Accepted to HI (2)	Attended HI First Year (3)	Attended HI Fourth Year (4)	Graduated HI Within 4 Years (5)
(A) Main Specification (Unweighted)					
6-Week	0.038	0.172*	0.082+	0.115+	0.144*
	(0.025)	(0.065)	(0.048)	(0.061)	(0.056)
1-Week	0.042	0.136*	0.080	0.099	0.107+
	(0.031)	(0.060)	(0.056)	(0.066)	(0.059)
Online	0.020	0.095*	0.016	0.046	0.031
	(0.015)	(0.035)	(0.027)	(0.039)	(0.027)
Control Mean	0.867	0.494	0.532	0.368	0.350
(B) Inverse Rating Weighted					
6-Week	0.473***	0.196***	0.164***	0.172***	0.141***
	(0.058)	(0.050)	(0.045)	(0.044)	(0.041)
1-Week	0.408***	0.095*	0.044	0.051	0.033
	(0.055)	(0.045)	(0.038)	(0.037)	(0.035)
Online	0.355***	0.082***	0.034+	0.043*	0.029+
	(0.034)	(0.023)	(0.019)	(0.019)	(0.017)
Control Mean	0.307	0.088	0.069	0.061	0.055
(C) Vs. Highest Rated Controls					
6-Week	0.448***	0.193***	0.157***	0.168***	0.137***
	(0.059)	(0.052)	(0.047)	(0.047)	(0.044)
1-Week	0.380***	0.092+	0.041	0.050	0.032
	(0.057)	(0.047)	(0.041)	(0.040)	(0.038)
Online	0.333***	0.075***	0.026	0.037	0.024
	(0.038)	(0.027)	(0.023)	(0.023)	(0.020)
Control Mean	0.333	0.151	0.113	0.098	0.092

Notes: The notes for this table are the same as the notes for Table B.2 for Panel A. Panel B modifies the main specification to weight the regression with weights inverse to the rating variable used to assign applicants to blocks (N = 2,084). Panel C limits control group members to those in the top half of control group ratings (N = 1,544). Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

Table A.11: Heterogeneous Treatment Effects

	Attended 4-Year in Y1 (1)	Attended Barron's Most Comp. in Y1 (2)	Graduated 4-Year by Y4 (3)	Graduated Barron's Most Comp. by Y4 (4)	STEM Degree by Y4 (5)
(A) Cohorts 2015 and 2016					
6-Week	0.031	0.140^{+}	0.070	0.114^{+}	0.131^{*}
	(0.023)	(0.069)	(0.054)	(0.062)	(0.057)
1-Week	0.066^{+}	0.128^{+}	0.052	0.094	0.070
	(0.036)	(0.063)	(0.071)	(0.063)	(0.061)
Online	0.010	0.054	-0.020	0.004	-0.010
	(0.020)	(0.037)	(0.034)	(0.044)	(0.031)
Control Mean	0.872	0.517	0.532	0.367	0.349
(B) Within Block 1					
6-Week	0.018	0.079	0.082	0.102^{+}	0.141^{*}
	(0.015)	(0.058)	(0.045)	(0.046)	(0.051)
1-Week	0.055	0.071	0.069	0.090^{+}	0.080
	(0.032)	(0.052)	(0.066)	(0.047)	(0.052)
Online Mean	0.870	0.604	0.509	0.399	0.340
(C) Within Block 1, by Rating					
6-Week * Higher Rated	0.035	0.062	0.184^{*}	0.176^{*}	0.226^{***}
	(0.048)	(0.053)	(0.067)	(0.066)	(0.077)
6-Week * Lower Rated	-0.000	0.092	-0.006	0.033	0.078
	(0.034)	(0.073)	(0.065)	(0.063)	(0.082)
p (6-Week, Higher = Lower)	0.559	0.761	0.050	0.142	0.186
1-Week * Higher Rated	0.063	-0.001	0.103	0.066	0.110
	(0.064)	(0.077)	(0.108)	(0.092)	(0.075)
1-Week * Lower Rated	0.053^{+}	0.128^{+}	0.037	0.102^{***}	0.042
	(0.028)	(0.073)	(0.057)	(0.013)	(0.060)
p (1-Week, Higher = Lower)	0.884	0.259	0.593	0.711	0.482
Online Mean, Higher Rated	0.838	0.702	0.470	0.432	0.340
Online Mean, Lower Rated	0.895	0.517	0.541	0.373	0.338

Notes: Panel A reports estimates with the same specification as those in Table B.2, limited to the 2015 and 2016 cohorts, which are the cohorts with all three programs in Block 1 (N = 1,450). Panel B limits the sample to Block 1, and applies the same specification, which means that program impacts are estimated in comparison to the online program (N = 504). Panel C splits the program assignment between higher-rated and lower-rated individuals, and modifies the strata to fully interact rating status with the previous strata (N = 504). Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$).

Table A.12: Heterogeneous Treatment Effects, HI Outcomes

	Applied to HI (1)	Accepted to HI (2)	Attended HI First Year (3)	Attended HI Fourth Year (4)	Graduated HI Within 4 Years (5)
(A) Cohorts 2015 and 2016					
6-Week	0.505***	0.217***	0.167***	0.182***	0.153***
	(0.056)	(0.052)	(0.048)	(0.053)	(0.043)
1-Week	0.461***	0.137*	0.100+	0.100+	0.071
	(0.063)	(0.057)	(0.049)	(0.052)	(0.048)
Online	0.372***	0.099*	0.052+	0.062*	0.039
	(0.029)	(0.039)	(0.027)	(0.029)	(0.028)
Control Mean	0.282	0.094	0.072	0.064	0.054
(B) Within Block 1					
6-Week	0.144*	0.116***	0.119*	0.124*	0.118***
	(0.050)	(0.031)	(0.040)	(0.044)	(0.033)
1-Week	0.093	0.045	0.052	0.042	0.037
	(0.057)	(0.045)	(0.042)	(0.045)	(0.040)
Online Mean	0.624	0.261	0.154	0.147	0.125
(C) Within Block 1, by Rating					
6-Week * Higher Rated	0.076	0.108+	0.119*	0.125+	0.117***
	(0.052)	(0.054)	(0.056)	(0.063)	(0.041)
6-Week * Lower Rated	0.200*	0.136***	0.118*	0.119*	0.115***
	(0.079)	(0.046)	(0.046)	(0.045)	(0.037)
p (6-Week, Higher = Lower)	0.198	0.700	0.981	0.939	0.971
1-Week * Higher Rated	0.026	0.089	0.122*	0.102	0.083
	(0.048)	(0.059)	(0.054)	(0.062)	(0.059)
1-Week * Lower Rated	0.141	-0.003	-0.033	-0.032	-0.026
	(0.084)	(0.053)	(0.056)	(0.055)	(0.037)
p (1-Week, Higher = Lower)	0.256	0.259	0.058	0.116	0.125
Online Mean, Higher Rated	0.697	0.307	0.166	0.153	0.153
Online Mean, Lower Rated	0.562	0.212	0.147	0.147	0.105

Notes: Panel A reports estimates with the same specification as those in Table B.2, limited to the 2015 and 2016 cohorts, which are the cohorts with all three programs in Block 1 (N = 1,450). Panel B limits the sample to Block 1, and applies the same specification, which means that program impacts are estimated in comparison to the online program (N = 504). Panel C splits the program assignment between higher-rated and lower-rated individuals, and modifies the strata to fully interact rating status with the previous strata (N = 504). Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

A.2 Data details

Data for this analysis come from four main sources: the program application, the HI institutional research office, the National Student Clearinghouse (NSC), and surveys fielded by the HI institutional research office. We describe each data source in detail below, as well as attrition rates for outcomes.

A.2.1 Applications and baseline survey

Background information on applicants comes from the program application and a baseline survey. For Cohort 1, the baseline survey was a separate data collection; for subsequent cohorts, baseline survey measures were part of the application itself. Information about applicants from these sources includes demographic and academic information. Family background variables include parental education and demographics, and indicators for immediate family who are summer program or HI alumni. Applicants report income information and an indicator for whether they are eligible for the federal free or reduced price lunch program. High school performance measures such as GPA, standardized test scores, extracurricular activities, awards, essays, and letters of recommendation are also provided. All measures are self-reported, though students needed to submit to the program high school transcripts and official records of standardized test scores. Applicants also consented to participate in research surveys at this point; students who declined to participate were not included in follow-up outreach for additional surveys but are included in randomization. The program office also supplied information on who was offered each program and whether applicants accepted that offer.

A.2.2 HI internal records

The HI institutional research office provided information on program applicants' interactions with the HI, including application (early application), admission, enrollment, declared major (if enrolled), and graduation, including degree and graduation date. All applicants were sent to be matched to HI records; if an applicant does not match to HI data systems, we assume a zero value on indicator variables for each of the outcomes described. These data were last updated in June 2021.

A.2.3 NSC

The HI institutional research office sent applicants' personal information from the application (excluding students known to be enrolled in HI) to the NSC for matching. The NSC returns records that include information on enrolled college and dates of enrollment. The NSC also reports graduation and degree fields; we observe four-year graduation for all cohorts, five-year graduation for the first two cohorts (2014 and 2015), and six-year graduation for the first cohort (2014). We match the college information to the federal Integrated Post-secondary Education Data System as well as other sources for information on college characteristics. All applicants were sent to be matched to the NSC or included in the HI records; if an applicant does not match to the HI or any NSC college, we assume a zero value on indicator variables for enrollment. The NSC has almost complete coverage of colleges and universities in the relevant time period, especially the highly ranked institutions that the applicant sample tends to enroll in. These data were last updated in June 2021.

A.2.4 Surveys

The HI surveyed applicants in the fall shortly after program completion (or equivalent for the control group), in May of their senior year in high school, and in the spring of sophomore year in college. Periodic shorter surveys collected information on college enrollment and choice of major. The shorter surveys were not fielded to students attending HI, as HI provided data on attendance and major. Students received $25 Amazon gift cards if they responded to longer-length surveys and $10 gift cards for short surveys, regardless of their treatment status. We discuss the surveys in more detail in Online Appendix C.

A.2.5 Attrition and response rates

Follow-up information on college enrollment exists from either the HI or the NSC for almost all applicants; those without such information we assume did not enroll in college and instead worked or joined the military. Almost all of the high-achieving students in this experiment immediately enrolled in college after on-time college graduation. Table A.13 shows a follow-up rate of 100 percent for college information, because all students' information was sent to the NSC and the HI for matching. However, survey responses were not as universal and declined over time. Unsurprisingly, those offered seats in the programs were more likely to respond to surveys than control group members. We describe the differential attrition in more detail below. Given large levels of differential attrition, we consider results using the survey data suggestive rather than conclusive. However, note that if those who complete surveys tend to be more motivated and have higher follow-through than those who do not complete surveys, if survey measures are biased, they are likely to underestimate program effects.

Table A.13: Survey Response and Data Availability Rates by Program Assignment

	6-Week (1)	1-Week (2)	Online (3)	Control (4)	All (5)
Pre-program survey	0.96	0.95	0.93	0.85	0.89
Senior year HS fall (post-program) survey	0.90	0.88	0.85	0.65	0.76
Senior year HS spring survey	0.81	0.81	0.78	0.57	0.68
First year college survey	0.49	0.55	0.62	0.49	0.53
Second year college spring survey	0.66	0.61	0.67	0.53	0.59
Included in HI/NSC data request	1.00	1.00	1.00	1.00	1.00
N	231	308	472	1073	2084

Notes: This table displays the response rates for follow-up surveys and for whether an applicant was included in the request for National Student Clearinghouse post-secondary data. Columns 1 through 4 show response rates by treatment assignment and column 5 shows response rates across the entire sample.

A.2.6 Covariate balance by cohort

Tables A.14 through A.16 show detailed covariate information and p-values for joint hypothesis tests, separately for each cohort. Because the block structure differs slightly by cohort, not all comparisons are possible in every case.

Table A.14: Covariate Balance: 2014 Cohort

	Covariate Means						Strata-adjusted Mean Differences		
	6-Week (Block 1) (1)	1-Week (Block 1) (2)	1-Week (Block 2) (3)	Online (Block 2) (4)	Online (Block 3) (5)	Control (Block 3) (6)	6-Week vs. 1-Week (7)	1-Week vs. Online (8)	Online vs. Control (9)
Black	0.377	0.355	0.383	0.329	0.324	0.362	0.026	0.042	-0.065
							(0.088)	(0.087)	(0.058)
Hispanic	0.410	0.419	0.383	0.457	0.510	0.477	-0.004	-0.055	0.050
							(0.091)	(0.087)	(0.062)
Native American	0.066	0.065	0.033	0.014	0.059	0.039	-0.002	0.019	0.024
							(0.044)	(0.028)	(0.031)
Asian	0.131	0.113	0.183	0.157	0.098	0.097	0.015	0.007	0.015
							(0.059)	(0.064)	(0.037)
White	0.016	0.048	0.017	0.043	0.010	0.025	-0.035	-0.013	-0.024
							(0.033)	(0.027)	(0.021)
Multiethnic	0.426	0.371	0.283	0.357	0.480	0.427	0.050	-0.071	0.079
							(0.090)	(0.084)	(0.062)
GPA	3.921	3.915	3.905	3.849	3.880	3.811	0.003	0.053	0.053*
							(0.027)	(0.062)	(0.023)
Free/reduced-price lunch	0.492	0.484	0.367	0.329	0.314	0.301	0.003	0.037	-0.004
							(0.091)	(0.086)	(0.058)
Standardized math score	2.200	2.326	2.044	2.167	1.934	1.870	-0.113	-0.098	0.195*
							(0.157)	(0.137)	(0.096)
Female	0.508	0.484	0.517	0.486	0.520	0.154	0.000	0.000	0.000
							(0.000)	(0.000)	(0.000)
First-generation college	0.213	0.258	0.250	0.200	0.147	0.136	-0.046	0.036	0.023
							(0.077)	(0.076)	(0.045)
						p-value	0.980	0.943	0.421
Observations	61	62	60	70	102	279	123	130	381

Notes: This table shows means for baseline characteristics and outcomes. Columns 1 through 6 shows the proportion of students assigned to each program with a given characteristic. Columns 7 through 9 report coefficients from regressions of the student characteristics on assignment to each program, including controls for randomization strata (+ p<0.10). N=634.

Table A.15: Covariate Balance: 2015 Cohort

	Covariate Means					Strata-adjusted Mean Differences			
	6-Week (Block 1) (1)	1-Week (Block 1) (2)	Online (Block 1) (3)	Online (Block 2) (4)	Control (Block 2) (5)	6-Week vs. 1-Week (6)	6-Week vs. Online (7)	1-Week vs. Online (8)	Online vs. Control (9)
Black	0.425	0.364	0.419	0.408	0.335	0.067	0.053	-0.027	0.076
						(0.073)	(0.081)	(0.070)	(0.058)
Hispanic	0.388	0.391	0.365	0.368	0.418	-0.026	-0.004	-0.006	-0.055
						(0.068)	(0.077)	(0.064)	(0.058)
Native American	0.075	0.055	0.054	0.039	0.042	0.024	0.015	-0.002	-0.008
						(0.038)	(0.046)	(0.033)	(0.023)
Asian	0.087	0.145	0.122	0.132	0.163	-0.046	-0.049	0.024	-0.022
						(0.048)	(0.053)	(0.051)	(0.044)
White	0.025	0.045	0.041	0.053	0.042	-0.018	-0.015	0.010	0.010
						(0.027)	(0.030)	(0.029)	(0.028)
Multiethnic	0.300	0.318	0.284	0.250	0.274	-0.039	-0.009	0.015	-0.039
						(0.069)	(0.077)	(0.067)	(0.052)
GPA	3.896	3.862	3.912	3.851	3.807	0.033	-0.025	-0.050	0.030
						(0.042)	(0.024)	(0.040)	(0.027)
Free/reduced-price lunch	0.487	0.455	0.419	0.303	0.391	0.045	0.069	0.025	-0.076
						(0.077)	(0.084)	(0.074)	(0.058)
Standardized math score	2.215	2.304	2.251	1.799	1.939	-0.123	-0.071	0.055	-0.131
						(0.104)	(0.131)	(0.127)	(0.103)
Female	0.500	0.500	0.500	0.500	0.343	0.000	0.000	0.000	0.000
						(0.000)	(0.000)	(0.000)	(0.000)
First-generation college	0.188	0.209	0.162	0.118	0.205	-0.002	0.035	0.050	-0.078+
						(0.062)	(0.065)	(0.058)	(0.044)
					p-value	0.844	0.934	0.987	0.498
Observations	80	110	74	76	361	190	154	184	437

Notes: This table shows means for baseline characteristics and outcomes. Columns 1 through 5 shows the proportion of students assigned to each program with a given characteristic. Columns 6 through 9 report coefficients from regressions of the student characteristic on assignment to each program, including controls for randomization strata (+ $p<0.10$). N=701.

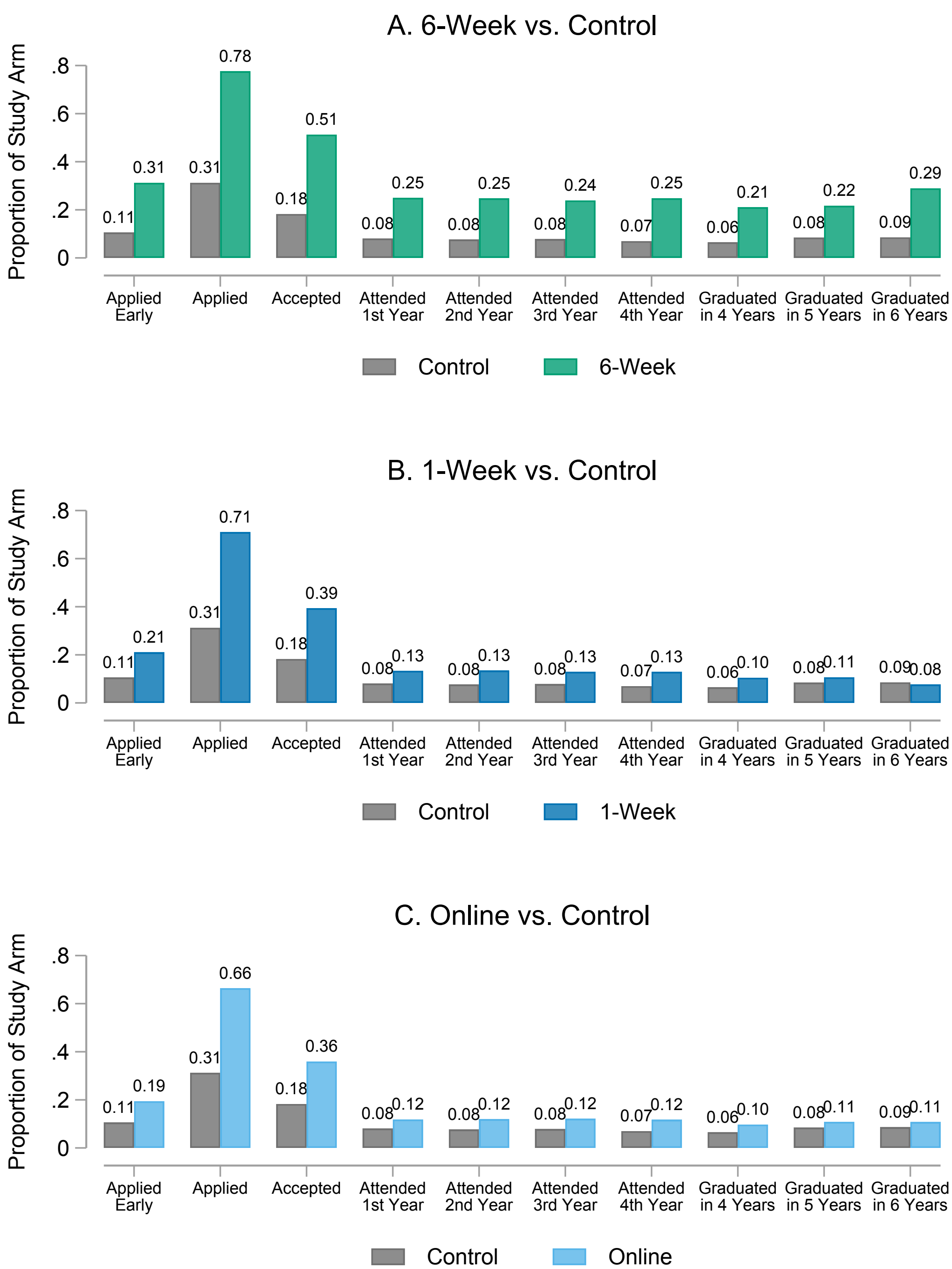
Table A.16: Covariate Balance: 2016 Cohort

	Covariate Means					Strata-adjusted Mean Differences			
	6-Week (Block 1) (1)	1-Week (Block 1) (2)	Online (Block 1) (3)	Online (Block 2) (4)	Control (Block 2) (5)	6-Week vs. 1-Week (6)	6-Week vs. Online (7)	1-Week vs. Online (8)	Online vs. Control (9)
Black	0.400	0.303	0.297	0.289	0.328	0.099	0.108	-0.000	-0.042
						(0.073)	(0.073)	(0.074)	(0.056)
Hispanic	0.422	0.461	0.432	0.434	0.434	-0.046	-0.001	0.056	-0.005
						(0.073)	(0.076)	(0.078)	(0.062)
Native American	0.033	0.053	0.041	0.039	0.042	-0.018	-0.013	0.005	0.001
						(0.031)	(0.029)	(0.033)	(0.024)
Asian	0.089	0.118	0.162	0.184	0.150	-0.030	-0.069	-0.044	0.033
						(0.049)	(0.052)	(0.057)	(0.048)
White	0.056	0.066	0.068	0.039	0.042	-0.005	-0.025	-0.017	0.005
						(0.036)	(0.039)	(0.042)	(0.024)
Multiethnic	0.411	0.395	0.297	0.408	0.390	0.010	0.115	0.115	0.011
						(0.077)	(0.074)	(0.078)	(0.062)
GPA	3.918	3.924	3.918	3.876	3.861	-0.005	0.001	0.009	0.013
						(0.019)	(0.020)	(0.021)	(0.018)
Free/reduced-price lunch	0.478	0.500	0.554	0.342	0.372	-0.040	-0.061	-0.039	-0.029
						(0.075)	(0.076)	(0.079)	(0.059)
Standardized math score	2.016	1.808	2.045	1.467	1.694	0.217	-0.024	-0.221	-0.190
						(0.173)	(0.128)	(0.161)	(0.218)
Female	0.500	0.500	0.500	0.500	0.388	0.000	0.000	0.000	0.000
						(0.000)	(0.000)	(0.000)	(0.000)
First-generation college	0.467	0.513	0.419	0.329	0.284	-0.048	0.057	0.103	0.038
						(0.078)	(0.075)	(0.080)	(0.059)
					p-value	0.924	0.218	0.563	0.891
Observations	90	76	74	76	432	166	164	150	508

Notes: This table shows means for baseline characteristics and outcomes. Columns 1 through 5 shows the proportion of students assigned to each program with a given characteristic. Columns 6 through 9 report coefficients from regressions of the student characteristic on assignment to each program, including controls for randomization strata (+ $p<0.10$). N=748.

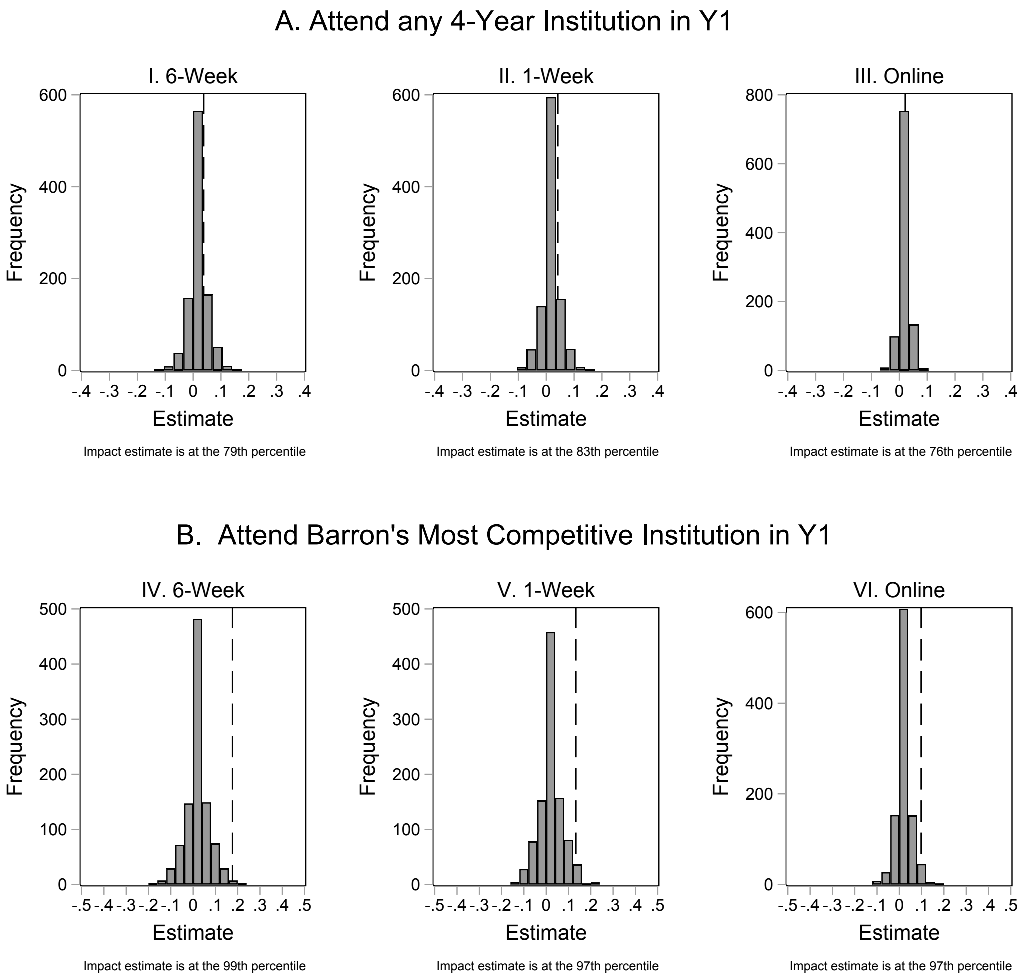
Appendix B: Additional results and robustness checks

Figure B.1: The Impact of STEM Summer Assignment on Key HI Outcomes



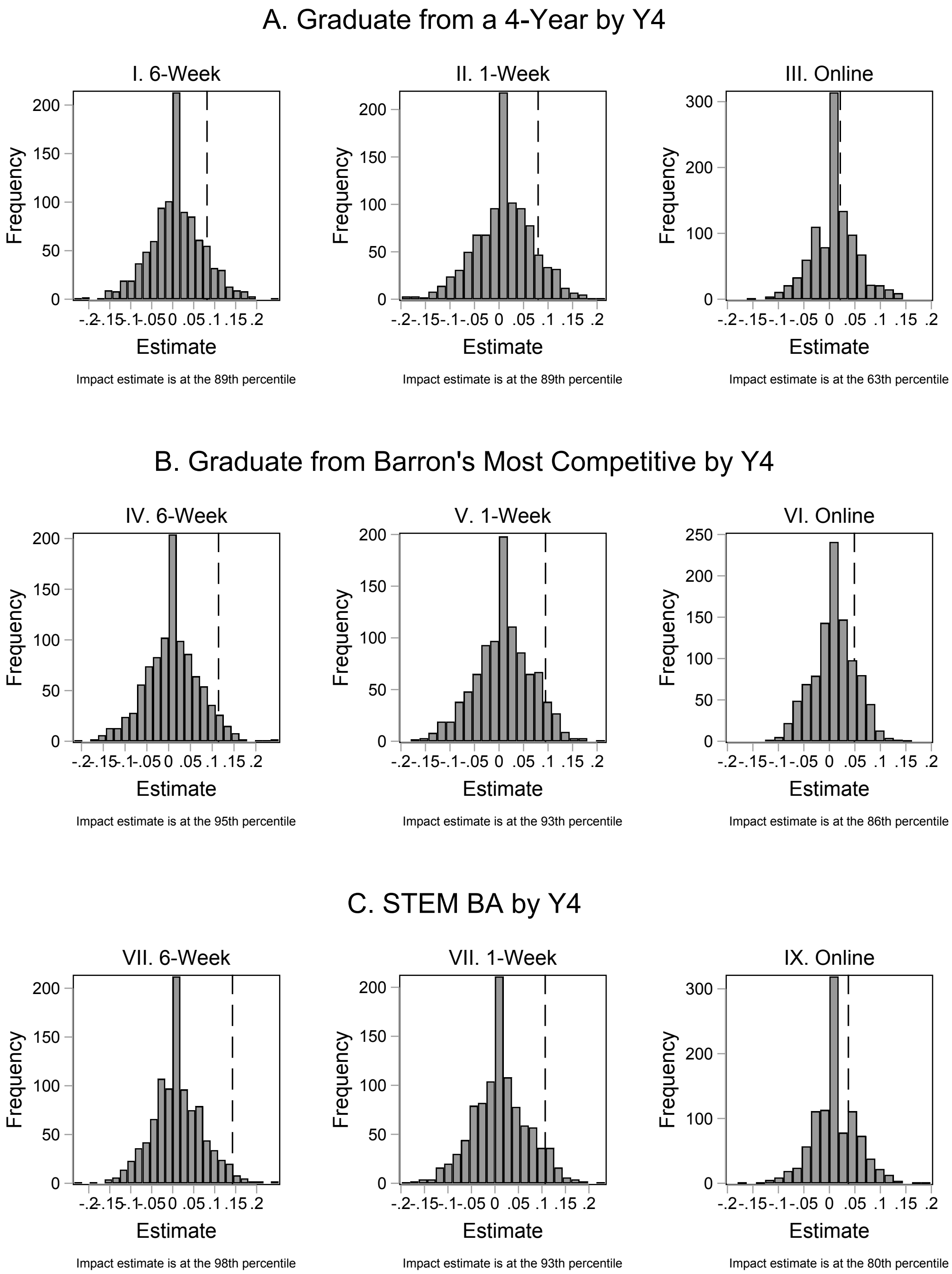
Notes: This figure summarizes impact estimates for HI outcomes. For details on the specification and exact point estimates and standard errors, see Table B.2.

Figure B.2: Randomization Inference: 4-Year Institution Attendance



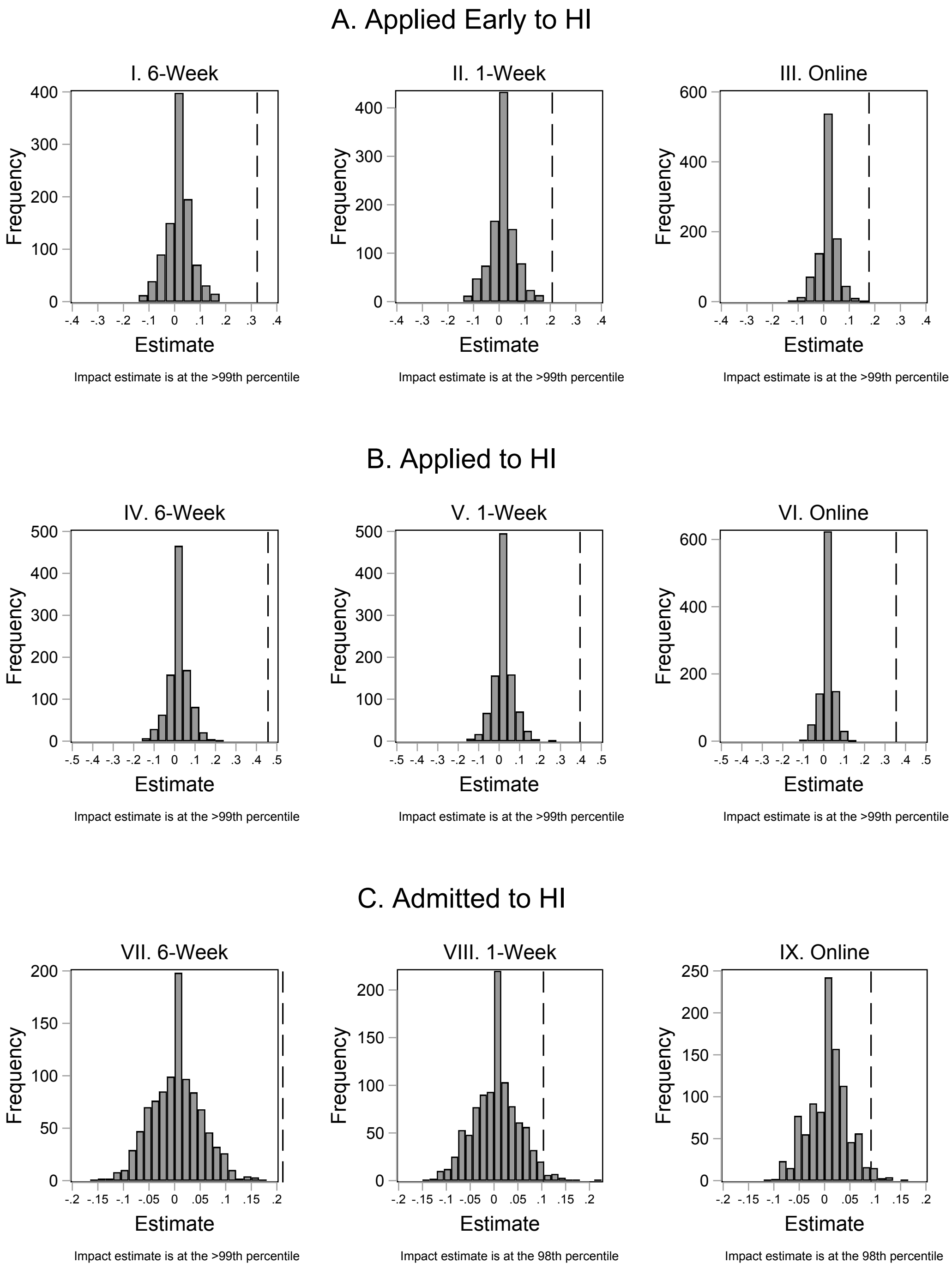
Notes: Each panel in the above figure shows the distribution of treatment impacts from 1,000 randomizations subject to the same criteria as the main randomization design but with a new random number. This generates placebo estimates of impacts on outcomes, to which the actual outcome, indicated by a dashed line, is compared.

Figure B.3: Randomization Inference: Graduation



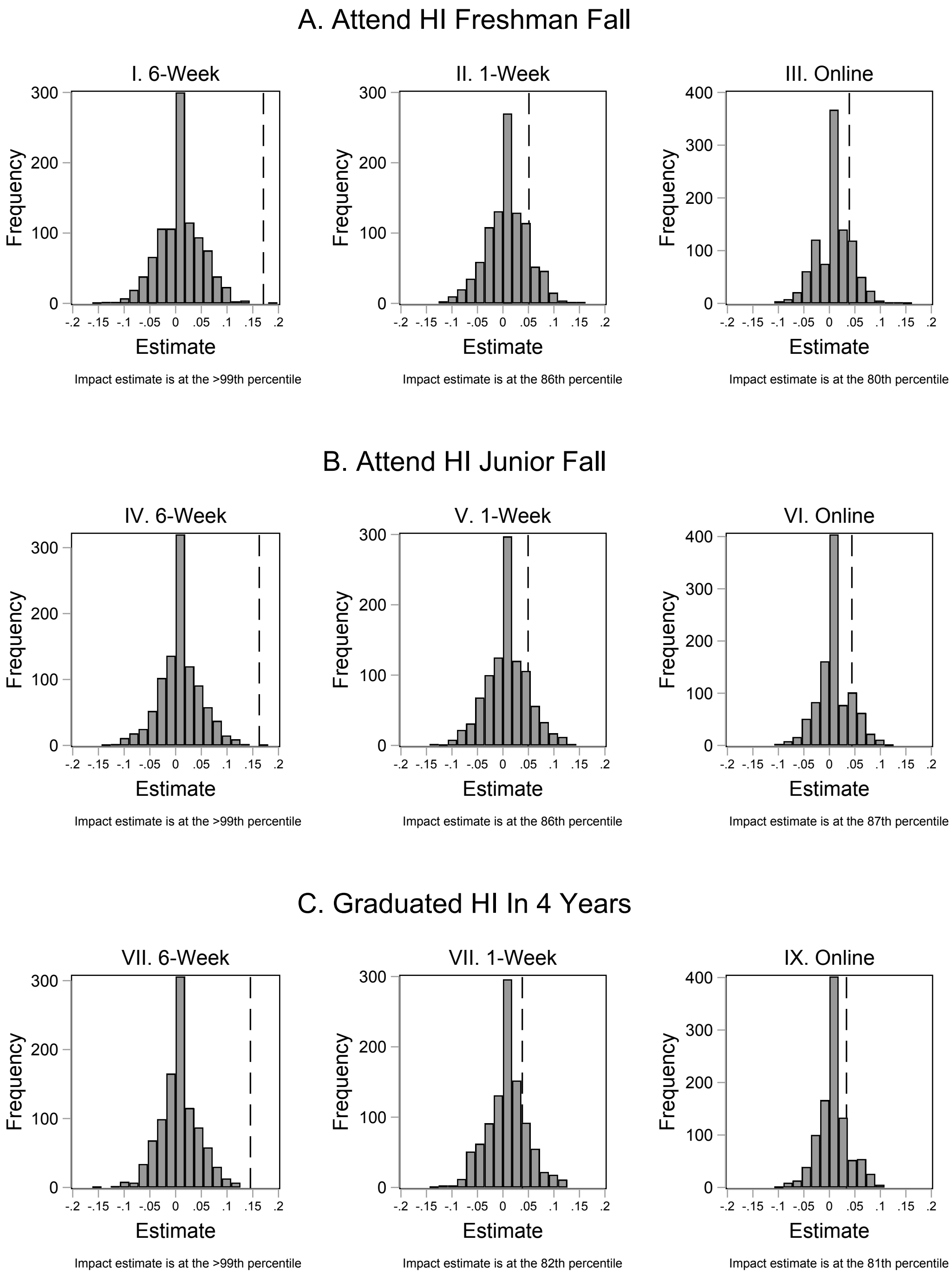
Notes: Each panel in the above figure shows the distribution of treatment impacts from 1,000 randomizations subject to the same criteria as the main randomization design but with a new random number. This generates placebo estimates of impacts on outcomes, to which the actual outcome, indicated by a dashed line, is compared.

Figure B.4: Randomization Inference: HI Application and Admission



Notes: Each panel in the above figure shows the distribution of treatment impacts from 1,000 randomizations subject to the same criteria as the main randomization design but with a new random number. This generates placebo estimates of impacts on outcomes, to which the actual outcome, indicated by a dashed line, is compared.

Figure B.5: Randomization Inference: HI Attendance and Graduation



Notes: Each panel in the above figure shows the distribution of treatment impacts from 1,000 randomizations subject to the same criteria as the main randomization design but with a new random number. This generates placebo estimates of impacts on outcomes, to which the actual outcome, indicated by a dashed line, is compared. Attendance at the HI in the second and fourth years is omitted for space.

Table B.1: Bounds Analysis Accounting for Selection on Unobservable Characteristics

		No Controls		Unrelated Controls		Design Controls	
	Main Specification (1)	Treatment Estimates (2)	δ Required to Reach $\beta = 0$ (3)	Treatment Estimates (4)	δ Required to Reach $\beta = 0$ (5)	Treatment Estimates (6)	δ Required to Reach $\beta = 0$ (7)
Panel A: Attend 4-year in Y1							
6-week	0.038	0.019	none	0.024	none	0.038	none
1-week	0.042	0.030	none	0.037	none	0.042	none
Online	0.020	0.013	none	0.018	none	0.021	12.146
R^2	[0.030]	[0.001]		[0.010]		[0.021]	
Panel B: Attend Barrons Most Competitive in Y1							
6-week	0.172	0.237	0.775	0.250	0.717	0.177	52.681
1-week	0.136	0.202	0.648	0.214	0.616	0.133	none
Online	0.095	0.116	1.930	0.123	1.764	0.098	47.213
R^2	[0.102]	[0.034]		[0.040]		[0.048]	
Panel C: Graduate from 4-year by Y4							
6-week	0.082	0.077	1.541	0.068	1.447	0.082	none
1-week	0.080	0.108	0.876	0.086	0.936	0.080	none
Online	0.016	0.039	0.945	0.018	2.666	0.021	5.656
R^2	[0.056]	[0.007]		[0.020]		[0.026]	
Panel D: Graduate from Barron's Most Competitive by Y4							
6-week	0.115	0.177	0.731	0.178	0.651	0.114	none
1-week	0.099	0.174	0.588	0.159	0.585	0.095	none
Online	0.046	0.082	1.234	0.067	1.507	0.049	26.713
R^2	[0.090]	[0.022]		[0.034]		[0.041]	
Panel E: Graduate with STEM degree by Y4							
6-week	0.144	0.130	1.491	0.146	1.055	0.142	none
1-week	0.107	0.117	1.062	0.123	0.850	0.107	321.961
Online	0.031	0.035	2.857	0.038	2.110	0.037	8.729
R^2	[0.066]	[0.011]		[0.024]		[0.031]	

Notes: This table reports results of Oster's (2019) procedure for determining the degree of proportional selection on unobservables, δ, that would result in a 0 coefficient. We assume that R_{max}, the R^2 of the hypothetical regression that includes all relevant selection factors is 1.3 times the R^2 of our main specification. Each panel focuses on one of our main non-HI outcomes. Column 1 reports the treatment estimates and R^2s for our main specification. Columns 2 and 3 report results using coefficient movements between a specification with no controls and our main specification. Columns 4 and 5 report results using coefficient movements between a specification with year-gender-geography strata and our main specification. Columns 6 and 7 report results using coefficient movements between a specification with year-gender-geography-block strata and our main specification. Columns 2, 4, and 6 report treatment coefficients and Column 3,5, and 7 report δs. We report "none" when treatment estimates in Columns 2, 4, or 6 are smaller than the estimates in our main specification, indicating negative selection into treatment. R^2s for each specification is reported in square brackets.

Table B.2: The Impact of Assignment to STEM Summer Programs on Key HI Outcomes

	Applied Early to HI (1)	Applied to HI (2)	Accepted to HI (3)	Attended HI First Year (4)	Attended HI Second Year (5)	Attended HI Third Year (6)	Attended HI Fourth Year (7)	Graduated HI Within 4 Years (8)
6-Week	0.330^{***}	0.464^{***}	0.207^{***}	0.169^{***}	0.171^{***}	0.161^{***}	0.178^{***}	0.146^{***}
	(0.058)	(0.050)	(0.041)	(0.041)	(0.038)	(0.038)	(0.044)	(0.035)
1-Week	0.211^{***}	0.398^{***}	0.105^{*}	0.053	0.058	0.051	0.059	0.040
	(0.054)	(0.055)	(0.042)	(0.038)	(0.038)	(0.038)	(0.039)	(0.036)
Online	0.176^{***}	0.352^{***}	0.088^{***}	0.038^{+}	0.043^{+}	0.043^{+}	0.047^{*}	0.033^{+}
	(0.034)	(0.024)	(0.028)	(0.020)	(0.022)	(0.022)	(0.022)	(0.019)
Control Mean	0.183	0.312	0.106	0.080	0.076	0.078	0.070	0.065

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). N = 2,084.

Table B.3: The Impact of Assignment to STEM Summer Programs on HI Graduation

	4-Year Graduation (1)	5-Year Graduation (2)	6-Year Graduation (3)
(A) All cohorts			
6-Week	0.146***	0.133*	0.204***
	(0.035)	(0.051)	(0.060)
1-Week	0.040	0.022	-0.009
	(0.036)	(0.049)	(0.053)
Online	0.033+	0.024	0.022*
	(0.019)	(0.027)	(0.007)
Control Mean	0.065	0.084	0.086
N	2,084	1,335	634
(B) Cohorts 2014 and 2015			
6-Week	0.117*	0.133*	0.204***
	(0.049)	(0.051)	(0.060)
1-Week	0.021	0.022	-0.009
	(0.048)	(0.049)	(0.053)
Online	0.026	0.024	0.022*
	(0.026)	(0.027)	(0.007)
Control Mean	0.081	0.084	0.086
N	1,335	1,335	634
(C) Cohort 2014			
6-Week	0.137*	0.190***	0.204***
	(0.056)	(0.060)	(0.060)
1-Week	-0.018	-0.022	-0.009
	(0.054)	(0.053)	(0.053)
Online	0.013*	0.009+	0.022*
	(0.005)	(0.005)	(0.007)
Control Mean	0.089	0.091	0.086
N	634	634	634

Notes: The notes for this table are the same as in Table B.2 except the outcomes are limited to college graduation from the HI in the fourth, fifth, and sixth year. Because some graduation outcomes are limited in availability by time, Panel A shows the results for all outcomes regardless of cohort with the sample changing by outcome, and Panels B and C restrict this sample to older cohorts. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001.)

Table B.4: The Impact of STEM Summer Program Attendance on Key Outcomes (2SLS)

	Attended 4-Year in Y1 (1)	Attended Barron's Most Comp. in Y1 (2)	Graduated 4-Year by Y4 (3)	Graduated Barron's Most Comp. by Y4 (4)	STEM Degree by Y4 (5)	(6)
6-Week	0.863***	0.042	0.190***	0.092	0.128+	0.162*
		(0.046)	(0.066)	(0.068)	(0.067)	(0.068)
1-Week	0.856***	0.048	0.157*	0.094	0.115+	0.125+
		(0.043)	(0.065)	(0.066)	(0.065)	(0.065)
Online	0.785***	0.025	0.122***	0.021	0.059	0.040
		(0.031)	(0.045)	(0.046)	(0.044)	(0.044)
Control Mean		0.872	0.473	0.525	0.342	0.350

Notes: Each coefficient in columns 2 through 9 is the instrumental variables estimate of the effect of attending the indicated summer program. An indicator variable for assignment to a particular program is the instrument for program attendance. Column 1 presents first stage estimates of the impact of assignment to a program on attendance. All regressions control for randomization strata, as well as a vector of characteristics including GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015 and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). N = 2,084.

Table B.5: The Impact of STEM Summer Program Attendance on Key HI Outcomes (2SLS)

	First Stage (1)	Applied Early to HI (2)	Applied to HI (3)	Accepted to HI (4)	Attended HI First Year (5)	Attended HI Second Year (6)	Attended HI Third Year (7)	Attended HI Fourth Year (8)	Graduated HI Within 4 Years (9)
6-Week	0.863***	0.367***	0.508***	0.232***	0.193***	0.194***	0.183***	0.202***	0.166***
		(0.065)	(0.060)	(0.058)	(0.052)	(0.051)	(0.052)	(0.051)	(0.048)
1-Week	0.856***	0.243***	0.455***	0.121*	0.062	0.068	0.060	0.069	0.047
		(0.062)	(0.058)	(0.053)	(0.045)	(0.045)	(0.045)	(0.045)	(0.042)
Online	0.785***	0.226***	0.451***	0.113***	0.049+	0.055*	0.055*	0.060*	0.042+
		(0.041)	(0.040)	(0.031)	(0.026)	(0.026)	(0.026)	(0.026)	(0.023)
Control Mean		0.184	0.317	0.061	0.057	0.056	0.056	0.051	0.040

Notes: Each coefficient in columns 2 through 9 is the instrumental variables estimate of the effect of attending the indicated summer program. An indicator variable for assignment to a particular program is the instrument for program attendance. Column 1 presents first stage estimates of the impact of assignment to a program on attendance. All regressions control for randomization strata, as well as a vector of characteristics including GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015 and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$). N = 2,084.

Table B.6: The Impact of Assignment to STEM Summer Programs on 4-Year College Graduation by College Type

	Any 4-Year Institution (1)	Technical Institutions (2)	Barron's Most Competitive (3)	Barron's Highly Competitive (4)	Barron's Very Competitive (5)	Barron's Competitive (6)
(A) All Institutions						
6-Week	0.082^{+}	0.104^{*}	0.115^{+}	-0.013	-0.018	0.007
	(0.048)	(0.041)	(0.061)	(0.039)	(0.022)	(0.019)
1-Week	0.080	-0.006	0.099	-0.027	-0.023	0.020
	(0.056)	(0.047)	(0.066)	(0.033)	(0.020)	(0.019)
Online	0.016	0.021	0.046	-0.010	-0.006	-0.003
	(0.027)	(0.020)	(0.039)	(0.021)	(0.010)	(0.015)
Control Mean	0.532	0.130	0.368	0.099	0.032	0.022
(B) Host Institution						
6-Week	0.146^{***}	0.146^{***}	0.146^{***}			
	(0.035)	(0.035)	(0.035)			
1-Week	0.040	0.040	0.040			
	(0.036)	(0.036)	(0.036)			
Online	0.033^{+}	0.033^{+}	0.033^{+}			
	(0.019)	(0.019)	(0.019)			
Control Mean	0.065	0.065	0.065			
(C) Institutions Except HI						
6-Week	-0.064	-0.042^{*}	-0.031			
	(0.054)	(0.019)	(0.069)			
1-Week	0.040	-0.046^{*}	0.059			
	(0.050)	(0.020)	(0.064)			
Online	-0.016	-0.011	0.014			
	(0.038)	(0.011)	(0.048)			
Control Mean	0.468	0.065	0.303			

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$). N = 2,084.

Table B.7: The Impact of Assignment to STEM Summer Programs on STEM and non-STEM Degrees, Counting Missing as Non-STEM

	Degree Within 4 Years			Degree Within 5 Years		
	Any (1)	STEM (2)	Non-STEM (3)	Any (4)	STEM (5)	Non-STEM (6)
(A) Any 4-Year Institution						
6-Week	0.082^{+}	0.127^{*}	-0.045	0.122^{+}	0.202^{*}	-0.080
	(0.048)	(0.057)	(0.037)	(0.061)	(0.075)	(0.067)
1-Week	0.080	0.092	-0.012	0.163^{*}	0.145^{+}	0.018
	(0.056)	(0.059)	(0.033)	(0.072)	(0.083)	(0.070)
Online	0.016	0.034	-0.018	0.082^{+}	0.045	0.037
	(0.027)	(0.026)	(0.016)	(0.047)	(0.045)	(0.030)
Control Mean	0.532	0.368	0.164	0.654	0.452	0.202
(B) Host Institution						
6-Week	0.146^{***}	0.145^{***}	0.001	0.133^{*}	0.141^{***}	-0.009
	(0.035)	(0.029)	(0.019)	(0.051)	(0.043)	(0.024)
1-Week	0.040	0.056	-0.016	0.022	0.056	-0.034^{+}
	(0.036)	(0.035)	(0.014)	(0.049)	(0.049)	(0.017)
Online	0.033^{+}	0.033^{+}	-0.000	0.024	0.031	-0.007^{+}
	(0.019)	(0.018)	(0.006)	(0.027)	(0.026)	(0.004)
Control Mean	0.065	0.051	0.014	0.084	0.064	0.020
(C) Other Institutions						
6-Week	-0.064	-0.018	-0.046	-0.010	0.061	-0.071
	(0.054)	(0.055)	(0.035)	(0.064)	(0.071)	(0.057)
1-Week	0.040	0.036	0.004	0.141^{*}	0.088	0.052
	(0.050)	(0.045)	(0.029)	(0.059)	(0.056)	(0.062)
Online	-0.016	0.001	-0.018	0.058	0.015	0.043
	(0.038)	(0.029)	(0.017)	(0.048)	(0.041)	(0.029)
Control Mean	0.468	0.317	0.151	0.570	0.388	0.182

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). N = 2,084 for outcomes during the fourth year and prior and N = 1,335 for fifth year graduation.

Table B.8: The Impact of Assignment to STEM Summer Programs on STEM and non-STEM Degrees, Counting Missing as STEM

	Degree Within 4 Years			Degree Within 5 Years		
	Any	STEM	Non-STEM	Any	STEM	Non-STEM
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Any 4-Year Institution						
6-Week	0.082^{+}	0.108^{+}	-0.026	0.122^{+}	0.211^{***}	-0.089
	(0.048)	(0.055)	(0.034)	(0.061)	(0.057)	(0.056)
1-Week	0.080	0.085	-0.005	0.163^{*}	0.174^{*}	-0.011
	(0.056)	(0.059)	(0.027)	(0.072)	(0.068)	(0.050)
Online	0.016	0.017	-0.001	0.082^{+}	0.068^{+}	0.014
	(0.027)	(0.026)	(0.018)	(0.047)	(0.035)	(0.028)
Control Mean	0.532	0.423	0.109	0.654	0.506	0.149
(B) Host Institution						
6-Week	0.146^{***}	0.145^{***}	0.001	0.133^{*}	0.141^{***}	-0.009
	(0.035)	(0.029)	(0.019)	(0.051)	(0.043)	(0.024)
1-Week	0.040	0.056	-0.016	0.022	0.056	-0.034^{+}
	(0.036)	(0.035)	(0.014)	(0.049)	(0.049)	(0.017)
Online	0.033^{+}	0.033^{+}	-0.000	0.024	0.031	-0.007^{+}
	(0.019)	(0.018)	(0.006)	(0.027)	(0.026)	(0.004)
Control Mean	0.065	0.051	0.014	0.084	0.064	0.020
(C) Other Institutions						
6-Week	-0.064	-0.037	-0.027	-0.010	0.070	-0.080
	(0.054)	(0.055)	(0.031)	(0.064)	(0.051)	(0.050)
1-Week	0.040	0.029	0.011	0.141^{*}	0.118^{*}	0.023
	(0.050)	(0.050)	(0.025)	(0.059)	(0.042)	(0.046)
Online	-0.016	-0.015	-0.001	0.058	0.037	0.021
	(0.038)	(0.035)	(0.017)	(0.048)	(0.032)	(0.028)
Control Mean	0.468	0.372	0.095	0.570	0.442	0.129

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$). N = 2,084 for outcomes during the fourth year and prior and N = 1,335 for fifth year graduation.

Table B.9: The Impact of Assignment to STEM Summer Programs on Detailed STEM Majors

	Computer Science (1)	Engineering (2)	Engineering Tech (3)	Bio and Biomed Sci (4)	Math and Stats (5)	Phys Sci (6)	No STEM Major (7)	Missing Major Code (8)
(A) By Year 4								
6-Week	-0.007	0.121*	-0.009	0.035	0.013	0.009	-0.026	-0.036
	(0.036)	(0.045)	(0.007)	(0.033)	(0.022)	(0.022)	(0.034)	(0.026)
1-Week	-0.032	0.098*	-0.002	0.034	0.021	0.006	-0.005	-0.021
	(0.026)	(0.046)	(0.009)	(0.021)	(0.016)	(0.018)	(0.027)	(0.030)
Online	-0.018	0.053*	0.000	-0.004	-0.005	-0.003	-0.001	-0.014
	(0.014)	(0.022)	(0.005)	(0.015)	(0.008)	(0.006)	(0.018)	(0.021)
Control Mean	0.104	0.145	0.006	0.046	0.026	0.033	0.109	0.073
(B) By Year 5								
6-Week	0.011	0.109*	-0.009	0.036	0.005	0.014	-0.049	0.014
	(0.040)	(0.051)	(0.007)	(0.035)	(0.023)	(0.022)	(0.036)	(0.030)
1-Week	-0.022	0.093^{+}	-0.002	0.036	0.019	0.007	-0.001	0.027
	(0.028)	(0.053)	(0.009)	(0.025)	(0.017)	(0.018)	(0.033)	(0.035)
Online	-0.015	0.046	0.000	-0.009	-0.007	-0.005	0.010	0.022
	(0.015)	(0.028)	(0.005)	(0.018)	(0.009)	(0.007)	(0.019)	(0.027)
Control Mean	0.110	0.187	0.006	0.050	0.030	0.034	0.094	0.042

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$). N = 2,084 for outcomes during the fourth year and prior, N = 1,335 for fifth year graduation, and N = 634 for sixth year graduation. Students who attained degrees, had at least one major code, and had no STEM major codes were categorized as non-STEM. Students who attained degrees, but had no major codes, were categorized as missing major. Coefficients may not add up to the coefficients on degree attainment, since students can have multiple major.

Table B.10: The Impact of Assignment to STEM Summer Programs on Potential Earnings (Natural Log)

	Potential Earnings Based on Degree Within 4 Years				Potential Earnings Based on Degree Within 5 Years			
	Only	Non-Degrees Imputed With:			Only	Non-Degrees Imputed With:		
	Degree Holders	5th Percentile	Median	95th Percentile	Degree Holders	5th Percentile	Median	95th Percentile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
6-Week	0.038^{+}	0.082^{*}	0.046^{*}	0.015	0.057^{*}	0.088^{+}	0.056^{*}	0.028^{+}
	(0.022)	(0.031)	(0.017)	(0.010)	(0.026)	(0.043)	(0.024)	(0.015)
1-Week	0.038^{+}	0.073^{*}	0.042^{*}	0.015	0.051^{+}	0.093^{+}	0.055^{*}	0.023
	(0.021)	(0.033)	(0.018)	(0.010)	(0.025)	(0.047)	(0.026)	(0.015)
Online	0.014	0.020	0.012	0.005	0.019^{***}	0.032	0.018	0.007^{*}
	(0.009)	(0.016)	(0.007)	(0.005)	(0.006)	(0.029)	(0.013)	(0.003)
Control Mean	11.625	11.376	11.521	11.647	11.617	11.432	11.543	11.639

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). N = 2,084 for outcomes during the fourth year and prior and N = 1,335 for fifth year graduation. Columns labeled Only Degree Holders are limited to degree holders with a major listed (N = 1,024 for 4 year, = 841 for 5 year). The other columns impute potential earnings with the 5th, 50th, and 95th percentile of earnings in the distribution of earnings at the major level. Potential earnings measures come from Sloan et al. (2021) and are reported in natural log units.

Table B.11: The Impact of Assignment to STEM Summer Programs on Key Outcomes by Gender

	Attended Any 4-Year in Y1 (1)	Attended HI in Y1 (2)	Attended Barron's Most Competitive in Y1 (3)	Graduated from 4-Year by Y4 (4)	Graduated from HI by Y4 (5)	Graduated from Barron's Most Competitive by Y4 (6)	STEM Degree by Y4 (7)
(A) Full Sample							
6-Week	0.038	0.169^{***}	0.172^{*}	0.082^{+}	0.146^{***}	0.115^{+}	0.144^{*}
	(0.025)	(0.041)	(0.065)	(0.048)	(0.035)	(0.061)	(0.056)
1-Week	0.042	0.053	0.136^{*}	0.080	0.040	0.099	0.107^{+}
	(0.031)	(0.038)	(0.060)	(0.056)	(0.036)	(0.066)	(0.059)
Online	0.020	0.038^{+}	0.095^{*}	0.016	0.033^{+}	0.046	0.031
	(0.015)	(0.020)	(0.035)	(0.027)	(0.019)	(0.039)	(0.027)
Control Mean	0.867	0.080	0.494	0.532	0.065	0.368	0.350
(B) Male							
6-Week	0.034	0.191^{*}	0.185^{+}	0.081	0.155^{*}	0.114	0.125^{+}
	(0.033)	(0.065)	(0.092)	(0.072)	(0.057)	(0.076)	(0.068)
1-Week	-0.003	0.020	0.055	0.067	0.001	0.027	0.048
	(0.044)	(0.054)	(0.092)	(0.076)	(0.050)	(0.097)	(0.091)
Online	0.013	0.047	0.058	0.002	0.042	-0.011	0.021
	(0.014)	(0.032)	(0.041)	(0.032)	(0.033)	(0.033)	(0.039)
Control Mean	0.876	0.081	0.487	0.499	0.069	0.349	0.365
(C) Female							
6-Week	0.024	0.137^{*}	0.137^{+}	0.065	0.132^{***}	0.113	0.157^{+}
	(0.032)	(0.050)	(0.077)	(0.066)	(0.040)	(0.070)	(0.086)
1-Week	0.074^{+}	0.070	0.197^{*}	0.094	0.070	0.178^{*}	0.172^{*}
	(0.042)	(0.045)	(0.072)	(0.085)	(0.042)	(0.082)	(0.070)
Online	0.024	0.022	0.121^{*}	0.026	0.019^{*}	0.110^{+}	0.046
	(0.029)	(0.021)	(0.051)	(0.048)	(0.009)	(0.061)	(0.039)
Control Mean	0.854	0.084	0.505	0.583	0.063	0.392	0.330

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$).N = 2,084 for the full sample, N = 1,242 for the male sample, N = 842 for the female sample

Table B.12: The Impact of Assignment to STEM Summer Programs on Key Outcomes by Underrepresented Race/Ethnicity

	Attended Any 4-Year in Y1 (1)	Attended HI in Y1 (2)	Attended Barron's Most Competitive in Y1 (3)	Graduated from 4-Year by Y4 (4)	Graduated from HI by Y4 (5)	Graduated from Barron's Most Competitive by Y4 (6)	STEM Degree by Y4 (7)
(A) Full Sample							
6-Week	0.038	0.169***	0.172*	0.082+	0.146***	0.115+	0.144*
	(0.025)	(0.041)	(0.065)	(0.048)	(0.035)	(0.061)	(0.056)
1-Week	0.042	0.053	0.136*	0.080	0.040	0.099	0.107+
	(0.031)	(0.038)	(0.060)	(0.056)	(0.036)	(0.066)	(0.059)
Online	0.020	0.038+	0.095*	0.016	0.033+	0.046	0.031
	(0.015)	(0.020)	(0.035)	(0.027)	(0.019)	(0.039)	(0.027)
Control Mean	0.867	0.080	0.494	0.532	0.065	0.368	0.350
(B) Underrepresented Minority							
6-Week	0.051	0.155***	0.161*	0.073	0.135***	0.105	0.095
	(0.031)	(0.044)	(0.067)	(0.062)	(0.039)	(0.072)	(0.060)
1-Week	0.060	0.026	0.143*	0.093	0.007	0.102	0.062
	(0.039)	(0.044)	(0.064)	(0.070)	(0.039)	(0.076)	(0.061)
Online	0.031	0.037+	0.121***	0.017	0.030	0.066	0.035
	(0.019)	(0.021)	(0.039)	(0.045)	(0.021)	(0.044)	(0.035)
Control Mean	0.854	0.091	0.491	0.523	0.073	0.361	0.352
(C) Not Underrepresented Minority							
6-Week	-0.012	0.229*	0.300*	0.142	0.176*	0.206	0.345*
	(0.094)	(0.089)	(0.139)	(0.147)	(0.086)	(0.131)	(0.143)
1-Week	-0.015	0.176+	0.136	0.045	0.192+	0.122	0.257+
	(0.073)	(0.094)	(0.128)	(0.140)	(0.095)	(0.140)	(0.143)
Online	-0.025	0.018	-0.024	-0.028	0.017	-0.078	-0.043
	(0.058)	(0.036)	(0.070)	(0.092)	(0.035)	(0.082)	(0.060)
Control Mean	0.917	0.035	0.501	0.572	0.028	0.400	0.366

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$).N = 2,084 for the full sample, N = 1,717 for the underrepresented minority sample, N = 367 for the non-underrepresented minority sample. Underrepresented minority is defined as Black, Native American, or Hispanic. Multiethnic and other race are not included in underrepresented minority.

Table B.13: The Impact of Assignment to STEM Summer Programs on Key Outcomes by Free or Reduced-Price Lunch Status

	Attended Any 4-Year in Y1 (1)	Attended HI in Y1 (2)	Attended Barron's Most Competitive in Y1 (3)	Graduated from 4-Year by Y4 (4)	Graduated from HI by Y4 (5)	Graduated from Barron's Most Competitive by Y4 (6)	STEM Degree by Y4 (7)
(A) Full Sample							
6-Week	0.038	0.169^{***}	0.172^{*}	0.082^{+}	0.146^{***}	0.115^{+}	0.144^{*}
	(0.025)	(0.041)	(0.065)	(0.048)	(0.035)	(0.061)	(0.056)
1-Week	0.042	0.053	0.136^{*}	0.080	0.040	0.099	0.107^{+}
	(0.031)	(0.038)	(0.060)	(0.056)	(0.036)	(0.066)	(0.059)
Online	0.020	0.038^{+}	0.095^{*}	0.016	0.033^{+}	0.046	0.031
	(0.015)	(0.020)	(0.035)	(0.027)	(0.019)	(0.039)	(0.027)
Control Mean	0.867	0.080	0.494	0.532	0.065	0.368	0.350
(B) Free or Reduced-Price Lunch							
6-Week	-0.019	0.147^{*}	0.079	0.036	0.135^{*}	-0.014	0.052
	(0.062)	(0.062)	(0.094)	(0.101)	(0.064)	(0.111)	(0.109)
1-Week	-0.040	0.087	0.034	0.014	0.085^{+}	-0.042	0.024
	(0.063)	(0.055)	(0.097)	(0.095)	(0.049)	(0.101)	(0.111)
Online	-0.001	0.028	0.096^{+}	-0.063	0.008	-0.029	-0.023
	(0.045)	(0.030)	(0.054)	(0.045)	(0.027)	(0.061)	(0.043)
Control Mean	0.882	0.068	0.507	0.515	0.052	0.393	0.340
(C) Not Free or reduced-price Lunch							
6-Week	0.076	0.189^{***}	0.238^{***}	0.072	0.151^{***}	0.189^{***}	0.180^{***}
	(0.046)	(0.053)	(0.075)	(0.062)	(0.053)	(0.050)	(0.057)
1-Week	0.107^{*}	0.021	0.224^{***}	0.102	-0.001	0.190^{***}	0.144^{***}
	(0.040)	(0.052)	(0.068)	(0.068)	(0.050)	(0.058)	(0.047)
Online	0.033	0.038	0.098^{*}	0.049	0.040	0.085^{*}	0.053
	(0.025)	(0.026)	(0.039)	(0.038)	(0.029)	(0.035)	(0.033)
Control Mean	0.858	0.090	0.487	0.552	0.075	0.361	0.366

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$).N = 2,084 for the full sample, N = 814 for the free or reduced-price lunch sample, N = 1,270 for the non-free or reduced-price lunch sample.

Table B.14: The Impact of Assignment to STEM Summer Programs on Key Outcomes by Parental College Experience

	Attended Any 4-Year in Y1 (1)	Attended HI in Y1 (2)	Attended Barron's Most Competitive in Y1 (3)	Graduated from 4-Year by Y4 (4)	Graduated from HI by Y4 (5)	Graduated from Barron's Most Competitive by Y4 (6)	STEM Degree by Y4 (7)
(A) Full Sample							
6-Week	0.038	0.169^{***}	0.172^{*}	0.082^{+}	0.146^{***}	0.115^{+}	0.144^{*}
	(0.025)	(0.041)	(0.065)	(0.048)	(0.035)	(0.061)	(0.056)
1-Week	0.042	0.053	0.136^{*}	0.080	0.040	0.099	0.107^{+}
	(0.031)	(0.038)	(0.060)	(0.056)	(0.036)	(0.066)	(0.059)
Online	0.020	0.038^{+}	0.095^{*}	0.016	0.033^{+}	0.046	0.031
	(0.015)	(0.020)	(0.035)	(0.027)	(0.019)	(0.039)	(0.027)
Control Mean	0.867	0.080	0.494	0.532	0.065	0.368	0.350
(B) Parents Did Not Attend College							
6-Week	-0.023	0.178^{*}	0.064	0.180	0.173^{+}	0.132	0.197^{+}
	(0.058)	(0.080)	(0.119)	(0.119)	(0.090)	(0.096)	(0.108)
1-Week	-0.090	0.000	-0.078	0.035	0.017	-0.017	0.131
	(0.067)	(0.071)	(0.117)	(0.116)	(0.068)	(0.118)	(0.105)
Online	0.009	-0.010	-0.011	-0.103^{+}	-0.025	-0.106	-0.020
	(0.036)	(0.018)	(0.047)	(0.053)	(0.022)	(0.064)	(0.041)
Control Mean	0.890	0.068	0.539	0.488	0.050	0.358	0.299
(C) At Least One Parent Attended College							
6-Week	0.055^{+}	0.164^{***}	0.209^{***}	0.032	0.136^{***}	0.101^{+}	0.116^{+}
	(0.030)	(0.036)	(0.068)	(0.051)	(0.040)	(0.059)	(0.061)
1-Week	0.078^{***}	0.071	0.210^{***}	0.102	0.051	0.148^{*}	0.109^{+}
	(0.028)	(0.044)	(0.057)	(0.063)	(0.044)	(0.061)	(0.059)
Online	0.023	0.050^{+}	0.126^{***}	0.049	0.047^{+}	0.091^{*}	0.043
	(0.020)	(0.028)	(0.036)	(0.040)	(0.028)	(0.036)	(0.037)
Control Mean	0.864	0.086	0.484	0.548	0.070	0.373	0.366

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$).N = 2,084 for the full sample, N = 504 for the no parental college attendance sample, N = 1,580 for the parental college attendance sample. Students missing parent education data (N = 21) are counted as having a parent with college experience.

Table B.15: The Impact of Assignment to STEM Summer Programs on College Applications and Admissions for Summer Experience Attenders

	HI			Barron's Most Competitive			Most Competitive, Excluding HI		
	Applied	Unconditional Admission	Admitted if Applied	Applied	Unconditional Admission	Admitted if Applied	Applied	Uncond. Admission	Admitted if Applied
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(A) Full Control Group									
6-Week	0.464***	0.207***	0.142*	0.102***	0.095	0.021	0.113*	0.103+	0.020
	(0.050)	(0.041)	(0.055)	(0.032)	(0.056)	(0.047)	(0.041)	(0.054)	(0.044)
1-Week	0.398***	0.105*	0.049	0.066+	0.066	0.022	0.061	0.096	0.062
	(0.055)	(0.042)	(0.064)	(0.034)	(0.060)	(0.044)	(0.049)	(0.060)	(0.040)
Online	0.352***	0.088***	0.055	0.040+	0.085*	0.062*	0.043+	0.081*	0.054+
	(0.024)	(0.028)	(0.039)	(0.022)	(0.031)	(0.028)	(0.023)	(0.030)	(0.027)
Control Mean	0.312	0.106	0.282	0.874	0.733	0.837	0.840	0.688	0.818
(B) Control Group Members with a STEM Summer Experience									
6-Week	0.422***	0.204***	0.107	0.071	0.034	-0.015	0.086	0.041	-0.020
	(0.062)	(0.053)	(0.074)	(0.046)	(0.086)	(0.069)	(0.054)	(0.070)	(0.055)
1-Week	0.353***	0.104+	0.013	0.035	0.008	-0.011	0.033	0.037	0.026
	(0.064)	(0.054)	(0.082)	(0.046)	(0.088)	(0.067)	(0.059)	(0.074)	(0.053)
Online	0.307***	0.086+	0.019	0.010	0.028	0.029	0.017	0.024	0.017
	(0.042)	(0.044)	(0.064)	(0.038)	(0.072)	(0.059)	(0.041)	(0.054)	(0.045)
Control Mean	0.358	0.150	0.324	0.903	0.804	0.887	0.862	0.749	0.866

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). Data on summer experiences come from the post-program follow-up survey. For Panel B, we excluded control students who did not respond to the follow-up survey or did not attend a STEM summer experience. For HI outcomes, N = 2,084 for the full sample and N = 1,177 for Panel B. Data on non-HI applications and admissions come from the follow-up survey at the end of the senior year of HS. The samples for these outcomes (Columns 4-9) are restricted to those with data on admissions to specific colleges. The sample sizes are N = 1,402 for Panel A and N = 928 for Panel B. Samples for columns 3, 6, and 9 are conditional on applying to the institution type.

Table B.16: The Impact of Assignment to STEM Summer Programs on College Graduation and STEM Degrees for Summer Experience Attenders

	Host Institution				Any 4-Year				Any 4-Year, Excluding HI			
	Grad in Y4 (1)	Grad in Y5 (2)	STEM Degree in Y4 (3)	STEM Degree in Y5 (4)	Grad in Y4 (5)	Grad in Y5 (6)	STEM Degree in Y4 (7)	STEM Degree in Y5 (8)	Grad in Y4 (9)	Grad in Y5 (10)	STEM Degree in Y4 (11)	STEM Degree in Y5 (12)
(A) Full Control Group												
6-Week	0.146***	0.133*	0.145***	0.141***	0.082+	0.122+	0.127*	0.202*	-0.064	-0.010	-0.018	0.061
	(0.035)	(0.051)	(0.029)	(0.043)	(0.048)	(0.061)	(0.057)	(0.075)	(0.054)	(0.064)	(0.055)	(0.071)
1-Week	0.040	0.022	0.056	0.056	0.080	0.163*	0.092	0.145+	0.040	0.141*	0.036	0.088
	(0.036)	(0.049)	(0.035)	(0.049)	(0.056)	(0.072)	(0.059)	(0.083)	(0.050)	(0.059)	(0.045)	(0.056)
Online	0.033+	0.024	0.033+	0.031	0.016	0.082+	0.034	0.045	-0.016	0.058	0.001	0.015
	(0.019)	(0.027)	(0.018)	(0.026)	(0.027)	(0.047)	(0.026)	(0.045)	(0.038)	(0.048)	(0.029)	(0.041)
Control Mean	0.065	0.084	0.051	0.064	0.532	0.654	0.368	0.452	0.468	0.570	0.317	0.388
(B) Control Group Members with a STEM Summer Experience												
6-Week	0.135***	0.119	0.126***	0.122	0.006	0.074	0.006	0.076	-0.129	-0.045	-0.120+	-0.046
	(0.044)	(0.081)	(0.039)	(0.078)	(0.068)	(0.125)	(0.059)	(0.089)	(0.083)	(0.142)	(0.061)	(0.085)
1-Week	0.029	0.006	0.037	0.035	0.007	0.121	-0.025	0.025	-0.022	0.114	-0.062	-0.009
	(0.046)	(0.082)	(0.044)	(0.082)	(0.073)	(0.130)	(0.059)	(0.094)	(0.081)	(0.142)	(0.053)	(0.077)
Online	0.021	0.007	0.013	0.008	-0.054	0.039	-0.081***	-0.075	-0.075	0.032	-0.094*	-0.083
	(0.033)	(0.071)	(0.032)	(0.071)	(0.055)	(0.120)	(0.028)	(0.063)	(0.074)	(0.138)	(0.040)	(0.067)
Control Mean	0.100	0.134	0.085	0.107	0.609	0.690	0.485	0.555	0.510	0.556	0.400	0.448

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). Data on summer experiences come from the post-program follow-up survey. For Panel B, we excluded control students who did not respond to the follow-up survey or did not attend a STEM summer experience. N = 2,084 for the full sample and N = 1,177 for Panel B.

Table B.17: The Impact of Assignment to STEM Summer Programs on Institution-Level Graduation Rates and STEM Degree

	IPEDS Bachelor's 4-Year Grad Rate (1)	4-Year Degree by Y4 (2)	IPEDS STEM as Pct of Bachelor's Degrees (3)	STEM Degree by Y4 (4)
(A) Full Sample				
6-Week	0.093***	0.082+	0.070*	0.127*
	(0.023)	(0.048)	(0.027)	(0.057)
1-Week	0.072***	0.080	0.008	0.092
	(0.025)	(0.056)	(0.031)	(0.059)
Online	0.044***	0.016	0.023+	0.034
	(0.011)	(0.027)	(0.012)	(0.026)
Control Mean	0.603	0.532	0.346	0.368
Observations	2084	2084	2084	2084
(B) Non-HI Attenders				
6-Week	0.072*	0.021	-0.020	0.049
	(0.029)	(0.054)	(0.016)	(0.061)
1-Week	0.066*	0.066	-0.028	0.047
	(0.029)	(0.054)	(0.021)	(0.052)
Online	0.039***	0.006	0.003	0.019
	(0.013)	(0.034)	(0.011)	(0.028)
Control Mean	0.585	0.510	0.302	0.352
Observations	1843	1843	1843	1843

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). The institutional-level outcomes in Columns 1 and 3 are college-level characteristics calculated from IPEDS data in 2013. Values for community colleges and non-college-going respondents are set to 0 for both institutional-level bachelor's four-year graduation rates and STEM degrees.

Table B.18: The Impact of Assignment to STEM Summer Programs on STEM Intentions

	STEM Major				STEM Career	
	Post-Program Survey (1)	First Year College Survey (2)	Second Year College Spring Survey (3)	STEM Degree by Y4 NSC (4)	Post-Program Survey (5)	Second Year College Spring Survey (6)
6-Week	0.010	0.008	-0.038	0.144^{*}	0.083^{+}	0.040
	(0.042)	(0.055)	(0.058)	(0.056)	(0.047)	(0.079)
1-Week	-0.001	0.022	0.003	0.107^{+}	0.006	0.058
	(0.041)	(0.054)	(0.044)	(0.059)	(0.050)	(0.058)
Online	-0.007	0.028	-0.003	0.031	0.077^{*}	0.008
	(0.035)	(0.035)	(0.026)	(0.027)	(0.037)	(0.036)
Control Mean	0.932	0.832	0.828	0.350	0.835	0.614
N	1410	1255	1191	2084	1382	1144

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$). The survey variables use declared major, if available, and intended major if the respondent is undeclared. The first-year survey was only fielded to non-HI attenders. The survey was fielded at the end of the spring semester for cohorts 2014 and 2016 and at the end of the fall semester for cohort 2015. Column 2 combines the first-year survey data for non-HI attenders and NSC data on declared majors for the fall of sophomore year for HI attenders. Column 4 uses degree data from the NSC and HI.

Table B.19: Baseline Characteristics by Program Assignment

	Control Group With STEM Summer (1)	Control Group Without STEM Summer (2)
Black	0.32	0.34
Hispanic	0.45	0.44
Native American	0.04	0.04
Asian	0.16	0.14
White	0.03	0.04
Multiethnic	0.37	0.36
GPA	3.84	3.83
Free/reduced-price lunch	0.40	0.35
Standardized math score	1.83	1.82
Female	0.42	0.29
First-generation college	0.26	0.21
First-generation college	0.26	0.21
Standardized Rating Variable	-0.72	-0.87
N	166	907

Notes: This table summarizes demographic characteristics, test scores, and GPA for program applicants. Column 1 shows averages taken across the entire sample. Columns 2 through 5 display means of these traits at baseline by program assignment. Race/ethnicity categories are not exclusive. First-generation college is defined as no parental college attendance. Students missing parental college information (N=21) were coded as not first-generation.

Appendix C: Additional survey results and weighting exercises

We detail the contents of the three longer-form surveys below. This appendix also shows different survey weighting schemes and additional survey results.

C.1 Survey Details

C.1.1 Post-program survey

The first long-form outcome survey was offered to the randomized applicants in the fall after the programs. It asked students about:

- Summer programs attended (in addition to HI programs for treatment groups, any for control group)
- College application plans
- Preferences for various college offerings (location, academics, extracurriculars, etc.)
- College major plans
- Familiarity with various colleges
- Career plans
- Sources of advice on college and careers
- AP, IB, and mathematics high school course taking plans
- Study skills
- Life skills
- Self-confidence
- Math problems and a brain teaser

C.1.2 End of high school survey

The second long-form outcome survey was offered to the randomized applicants at the end of their senior year in high school (about eight months after the first long-form survey). It asked students about:

- College enrollment plans
- College application and admissions offers
- SAT and/or ACT scores
- High school GPA

C.1.3 Second-year college spring survey

The third long-form outcome survey was offered to the randomized applicants in the spring of their sophomore year of college (about 2.5 years after the first long-form survey). It asked students about:

- College enrollment
- College major

- College math courses
- College study skills
- Educational experiences outside of class
- Social life
- Summer plans
- Graduate school plans
- Career plans

C.2 Creating indices from survey responses

To avoid emphasizing spurious results due to multiple hypothesis testing, outcomes are grouped into related "families." Following Anderson (2008), each family is converted into an index according to the following procedure:

- For each individual outcome in the family, we define each variable such that higher values are "better."
- We then normalize each outcome into a z-score relative to the control group for that cohort. That is, subtract the cohort-specific control group mean and divide by the standard deviation.
- Construct the weighted average of all the outcomes in the family by cohort. The weight on each outcome is the inverse of the covariance matrix of the outcomes.
- Normalize the index again by subtracting the cohort's control group mean and dividing by the standard deviation.
- If a respondent is missing the answer to some, but not all, items in a family, construct the index based on non-missing items.

We report our findings using survey data with such indices.

The indices used in Table 6 use the following outcomes:

- Life skills
  - I set my alarm each night before I go to bed when I need to wake up early.
  - I return phone calls and emails in a timely manner.
  - I can do my own laundry.
  - I can plan meals for myself.
  - I can balance my checking account. (2014 only)
- Study skills
  - I ask myself questions to make sure I know the material I have been studying.
  - Before I begin studying, I think about the things I will need to do to learn.
  - When I'm reading, I stop once in a while and go over what I have read. (2015 and 2016 only)

- When I get stuck on a problem, I ask a classmate or friend for help. (2015 only)
- I always persist to the end of a project, even when the work is hard. (2014 only)
- I work hard to get a good grade even when I don't like a class. (2014 only)
- When I get stuck on a problem, I ask a teacher for help. (2015 and 2016 only)

- Confidence
  - I am confident that I will succeed in my courses this semester. (2015 only)
  - I am good at math. (2015 and 2016 only)
- Likes intellectual activities
  - I like to tinker (take things apart, fix things, etc.). (2015 and 2016 only)
  - I like brain teasers and puzzles. (2015 and 2016 only)
- Attention
  - I often find that I have been reading for class but don't know what it is all about. (2015 and 2016 only)
  - I find that when the teacher is talking, I think of other things and don't really listen to what is being said.

The indices used in Appendix Table C.6 use the following outcomes:

- Community and belonging
  - I feel a sense of belonging to my college community
  - I feel that I am a member of my college's community
  - I see myself as part of my college's community
  - My friends are taking the same classes as me
- Use of school academic supports
  - I have attended professors' office hours (hours per semester)
  - I have attended teaching assistants' office hours (hours per semester)
  - I have used my university's tutoring resources
- Use of peer academic supports
  - I have a study group for at least one of my classes
  - My friends help me with coursework (e.g., study groups, doing problem sets together).
- Professional development
  - I have worked with a professor as a research assistant
  - I have had an internship while enrolled at my university
  - I know a professor who would be willing to write me a recommendation letter

Table C.1: Main Estimates Restricted to Survey Responders and Inverse Propensity Weights, with Assignment Variables

	Full Sample	Post-Program: Responders Unweighted	Post-Program: Responders IPW	End of High School: Responders Unweighted	End of High School: Responders IPW	Sophomore Year: Responders Unweighted	Sophomore Year: Responders IPW
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(A) Attended Any Four-Year Institution in Year 1							
6-Week	0.038	0.029	0.022	0.008	0.006	-0.008	0.025
	(0.041)	(0.044)	(0.057)	(0.045)	(0.051)	(0.050)	(0.055)
1-Week	0.042	0.047	0.045	0.003	0.011	0.022	0.040
	(0.037)	(0.041)	(0.050)	(0.041)	(0.045)	(0.045)	(0.048)
Online	0.020	-0.004	-0.001	-0.004	0.003	0.023	0.032
	(0.024)	(0.027)	(0.036)	(0.028)	(0.029)	(0.029)	(0.032)
(B) Attended Barron's Most Competitive Institution							
6-Week	0.172***	0.123+	0.152+	0.116+	0.169*	0.095	0.192*
	(0.059)	(0.063)	(0.082)	(0.066)	(0.076)	(0.071)	(0.078)
1-Week	0.136*	0.097	0.121	0.084	0.145*	0.114+	0.186*
	(0.055)	(0.061)	(0.078)	(0.063)	(0.071)	(0.067)	(0.074)
Online	0.095***	0.036	0.062	0.058	0.076+	0.067	0.088+
	(0.035)	(0.040)	(0.052)	(0.041)	(0.045)	(0.044)	(0.048)
(C) Degree from Any Four-Year Institution by Year							
6-Week	0.082	0.069	0.111	0.062	0.085	0.093	0.135
	(0.061)	(0.065)	(0.083)	(0.069)	(0.080)	(0.075)	(0.084)
1-Week	0.080	0.068	0.131+	0.070	0.113	0.103	0.136+
	(0.057)	(0.062)	(0.076)	(0.064)	(0.073)	(0.071)	(0.077)
Online	0.016	-0.023	0.026	-0.006	0.023	0.020	0.049
	(0.036)	(0.040)	(0.052)	(0.042)	(0.045)	(0.045)	(0.050)
(D) Degree from Barron's Most Competitive Institution							
6-Week	0.115+	0.087	0.122	0.073	0.111	0.100	0.168*
	(0.059)	(0.064)	(0.082)	(0.068)	(0.076)	(0.072)	(0.080)
1-Week	0.099+	0.074	0.115	0.071	0.127+	0.147*	0.200***
	(0.056)	(0.061)	(0.078)	(0.064)	(0.071)	(0.068)	(0.074)
Online	0.046	0.011	0.058	0.015	0.038	0.029	0.046
	(0.034)	(0.039)	(0.051)	(0.040)	(0.043)	(0.043)	(0.048)
(E) STEM Degree by Year 4							
6-Week	0.144*	0.116+	0.169*	0.105	0.139+	0.143+	0.201*
	(0.060)	(0.064)	(0.084)	(0.069)	(0.075)	(0.075)	(0.082)
1-Week	0.107+	0.093	0.157*	0.102	0.121+	0.136+	0.152*
	(0.056)	(0.061)	(0.079)	(0.064)	(0.069)	(0.070)	(0.077)
Online	0.031	-0.006	0.052	0.019	0.040	0.025	0.035
	(0.034)	(0.038)	(0.051)	(0.040)	(0.043)	(0.043)	(0.049)

Notes: Each panel uses a different attendance or graduation outcome. Column 1 is the main specification. Columns 2, 4, and 6 restrict the sample to survey responders. Columns 3, 5, and 7 use inverse propensity weighting with survey responders. The response prediction regression includes assignment to programs, randomization strata, rating variable, GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status.

Table C.2: Predictors of Survey Response

	Post-Program (1)	End of High School (2)	Sophomore Year (3)
main			
6-Week	0.745***	0.546***	0.176
	(0.224)	(0.181)	(0.167)
1-Week	0.668***	0.518***	0.046
	(0.193)	(0.168)	(0.154)
Online	0.680***	0.526***	0.245***
	(0.110)	(0.099)	(0.094)
Rating Variable	0.201***	0.179***	0.035
	(0.065)	(0.057)	(0.054)
Free/Reduced Lunch	0.092	0.054	0.151*
	(0.072)	(0.065)	(0.062)
GPA	0.097	0.092	0.272*
	(0.124)	(0.118)	(0.122)
Standardized Math Score	0.039	0.086***	0.010
	(0.037)	(0.033)	(0.031)
Black	0.165	-0.052	0.217
	(0.171)	(0.158)	(0.150)
Hispanic	0.240	-0.029	0.394*
	(0.175)	(0.162)	(0.153)
Native American	-0.089	-0.104	-0.001
	(0.223)	(0.212)	(0.200)
Asian	0.296	0.141	0.562***
	(0.185)	(0.171)	(0.162)
Multiethnic	-0.010	-0.066	-0.025
	(0.079)	(0.073)	(0.070)

Notes: Each column displays probit regression coefficients for the predictors of survey response. Regression coefficents for randomization strata are not displayed.

Table C.3: The Impact of Assignment to STEM Summer Programs on College Application Knowledge

	Sources of Application Advice				Have Heard of Specific Colleges				
	Teacher or Counselor (1)	Family Members (2)	Friends (3)	Internet or Other Written (4)	Non-HI Technical Institute (5)	Ivy League (6)	Liberal Arts College (7)	Top Public (8)	Fake College (9)
6-Week	0.102^{+}	0.065	0.133^{+}	-0.018	0.071^{+}	0.014^{+}	0.108^{*}	-0.080	-0.052
	(0.052)	(0.052)	(0.074)	(0.078)	(0.037)	(0.007)	(0.044)	(0.082)	(0.034)
1-Week	0.035	0.019	0.016	0.000	0.068	0.013^{+}	0.102^{+}	-0.076	-0.039
	(0.050)	(0.050)	(0.066)	(0.075)	(0.044)	(0.007)	(0.056)	(0.070)	(0.030)
Online	0.023	0.023	0.069^{*}	-0.030	0.101^{***}	0.007^{+}	0.069^{***}	-0.036	-0.007
	(0.034)	(0.039)	(0.030)	(0.060)	(0.022)	(0.004)	(0.016)	(0.051)	(0.015)
Control Mean	0.777	0.625	0.489	0.746	0.704	0.991	0.607	0.637	0.113
Observations	1401	1401	1401	1401	1411	1411	1410	1403	1412

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). Data are from surveys conducted post program, in the fall of the senior year of high school.

Table C.4: The Impact of Assignment to STEM Summer Programs on Perceptions of College Costs and Financial Aid

	Estimated Minus IPEDS		Minus IPEDS Windsorized		Estimate with IPEDS Controls	
	Cost of Attendance (1)	Financial Aid Coverage (2)	Cost of Attendance (3)	Financial Aid Coverage (4)	Cost of Attendance (5)	Financial Aid Coverage (6)
6-Week	-1490.782	0.012	-580.834	0.013	-745.096	0.040
	(2915.910)	(0.028)	(2771.935)	(0.028)	(3129.454)	(0.024)
1-Week	-424.287	0.013	-623.320	0.013	-278.095	0.021
	(2408.864)	(0.025)	(2336.924)	(0.025)	(2382.127)	(0.023)
Online	-461.791	0.016	-757.030	0.017	-351.982	0.029
	(1964.294)	(0.022)	(1910.422)	(0.022)	(1978.260)	(0.019)
Control Mean	9895.721	-0.060	9496.540	-0.061	51090.488	0.630
Observations	1342	1277	1342	1277	1341	1276

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ ***$p<0.001$). Data are from surveys conducted at the end of the summer of the programs. Columns 1 and 2 take the average of respondents' estimates of their first three college choices and subtract the averaged IPEDS figures for those colleges.

Table C.5: The Impact of Assignment to STEM Summer Programs on College-Level Courses By Fall of Sophomore Year

	Completed in HS (1)	Completed by Year 2 (2)	Started and Dropped (3)	Required by Major (4)
(A) Single Variable Calculus				
6-Week	0.052	0.028	0.020	0.159^{+}
	(0.059)	(0.043)	(0.047)	(0.084)
1-Week	-0.029	0.020	0.023	0.161*
	(0.050)	(0.039)	(0.041)	(0.076)
Online	0.002	-0.006	0.000	0.064
	(0.032)	(0.032)	(0.026)	(0.062)
Control Mean	0.834	0.942	0.052	0.369
(B) Multivariable Calculus				
6-Week	0.020	0.164*	0.051	0.138^{+}
	(0.046)	(0.076)	(0.039)	(0.081)
1-Week	0.015	0.167*	0.055	0.127
	(0.046)	(0.073)	(0.043)	(0.080)
Online	-0.016	0.099*	0.043	0.056
	(0.032)	(0.037)	(0.033)	(0.062)
Control Mean	0.181	0.689	0.072	0.318
(C) Linear Algebra				
6-Week	0.020	-0.116*	-0.058	-0.063
	(0.047)	(0.054)	(0.051)	(0.075)
1-Week	0.040	0.037	-0.033	0.042
	(0.063)	(0.079)	(0.045)	(0.084)
Online	0.017	0.022	-0.019	0.017
	(0.037)	(0.035)	(0.026)	(0.043)
Control Mean	0.129	0.597	0.090	0.384
(D) Probability and Statistics				
6-Week	-0.016	-0.162^{+}	-0.003	-0.067
	(0.079)	(0.087)	(0.012)	(0.064)
1-Week	-0.021	-0.056	-0.009	-0.028
	(0.070)	(0.087)	(0.008)	(0.061)
Online	-0.073	-0.074*	-0.002	-0.013
	(0.047)	(0.035)	(0.002)	(0.035)
Control Mean	0.238	0.531	0.005	0.346
Observations	1225	1225	1225	1225

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). Data are from follow-up surveys administered during the projected second year of college in the fall.

Table C.6: The Impact of Assignment to STEM Summer Programs on College Experiences

	Community and Belonging (1)	Use of School Academic Supports (2)	Use of Peer Academic Supports (3)	Profofessional Development (4)	Percentage URM (5)
(A) Full Sample					
6-Week	0.248^{+}	-0.156	0.124	0.006	-0.015
	(0.128)	(0.153)	(0.124)	(0.130)	(0.014)
1-Week	0.225	-0.081	0.147	-0.001	-0.019
	(0.134)	(0.134)	(0.135)	(0.132)	(0.013)
Online	0.033	-0.039	0.080	-0.099	-0.017^{+}
	(0.074)	(0.101)	(0.076)	(0.064)	(0.009)
N	1178	1225	1225	1225	1934
(B) Attended HI in Year 2					
6-Week	-0.519^{+}	-0.862^{+}	0.332	0.638	0.000
	(0.292)	(0.462)	(0.553)	(0.444)	(.)
1-Week	-0.808^{***}	-1.044^{*}	0.141	0.681	0.000
	(0.265)	(0.431)	(0.573)	(0.416)	(.)
Online	-0.571^{*}	-0.479^{+}	-0.061	-0.168	0.000
	(0.207)	(0.264)	(0.447)	(0.304)	(.)
N	137	140	140	140	237
(C) Did Not Attend HI in Year 2					
6-Week	0.378^{*}	-0.096	0.059	-0.054	-0.027
	(0.145)	(0.163)	(0.130)	(0.153)	(0.017)
1-Week	0.332^{*}	-0.014	0.073	-0.110	-0.022
	(0.130)	(0.137)	(0.136)	(0.161)	(0.015)
Online	0.093	-0.034	0.091	-0.107	-0.020^{+}
	(0.079)	(0.109)	(0.071)	(0.093)	(0.011)
N	1041	1085	1085	1085	1697

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). Columns 1 through 4 use data from surveys fielded in the spring semester ofthe second year of college. The outcomes are indices constructed from multiple survey questionsas described in Section C.2. The last column uses IPEDS characteristics merged to NSC attendance data.

Table C.7: The Impact of Assignment to STEM Summer Programs on College Clubs and Societies

	Any Club or Society (1)	Race/Ethnicity Affinity (2)	Gender Affinity (3)	Major-Related Club/Society (4)
6-Week	0.032	0.007	-0.003	0.014
	(0.067)	(0.073)	(0.069)	(0.069)
1-Week	0.032	-0.075	-0.018	0.062
	(0.058)	(0.063)	(0.056)	(0.058)
Online	0.005	-0.008	0.010	0.032
	(0.047)	(0.032)	(0.046)	(0.034)
Control Mean	0.786	0.332	0.222	0.334
N	1225	1225	1225	1225

Notes: Each coefficient labeled by program is the estimate of the impact of assignment to one of the three STEM summer programs on the outcome indicated in the heading. All regressions control for randomization strata and a vector of characteristics including indicators for GPA, standardized math score, race/ethnicity, and free and reduced-price lunch status. The sample includes STEM summer program applicants who applied in 2014, 2015, and 2016 and passed an initial screen, who were then subject to random assignment as described in Section 4.2. The control mean is adjusted for randomization strata. Robust standard errors are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001). Data are from follow-up surveys administered during the spring semester of the second year of college.