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College Summer School: Educational Benefits and Enrollment Preferences

Andy Brownback
University of Arkansas

Sally Sadoff*
UC San Diego

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Abstract

We experimentally examine whether a policy targeting college summer school enrollment can accelerate degree progress and completion. We randomly assign summer scholarships to community college students and find a large impact on degree acceleration, increasing graduation within one year of the intervention by 32% and transfers to four-year colleges by 58%. We elicit preferences for the scholarships and find substantial treatment effects on enrollment, graduation, and transfer among students with a preference *against* summer school. These results suggest that many more students could benefit from summer school than the minority who currently enroll.

JEL codes: I23, C93
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*Corresponding Author: Sally Sadoff, UC San Diego, Rady School of Management, 9500 Gilman Drive, La Jolla, CA 92093; ssadoff@ucsd.edu. Andy Brownback, University of Arkansas, Department of Economics, 220 N. McIlroy Ave, Fayetteville, AR 72701; abrownback@walton.uark.edu. We gratefully acknowledge Ivy Tech Community College and our invaluable partners there, Ronald Sloan and Neil Anthony. Eurika Bennett, Jaclyn Fisher, Ryan Johnson, Jacklyn Redwine, Tammy Jo Robbins, and Alisa Wells provided truly outstanding assistance with data collection and experimental execution. The project was made possible by the generous financial support of the Laura and John Arnold Foundation and the Yankelovich Center for Social Science Research. This research was conducted with approval from the University of Arkansas and UC San Diego Institutional Review Boards. AEA RCT Registry number: AEARCTR-0005849

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1 Introduction

A two-year degree is increasingly becoming a misnomer. Currently, the average community college student takes three years to earn their two-year associate degree (Shapiro et al., 2016). This delay creates pressing economic concerns for students, administrators, and policymakers. Other success metrics are similarly disappointing. Over eighty percent of community college students intend to transfer to a four-year college, but only about a quarter do so within five years (Jenkins and Fink, 2016). Long delays en route to graduation or transfer are costly because they increase the time paying tuition and accumulating debt, and decrease the time benefiting from the increased earnings that result from a degree. The opportunity cost of foregone earnings is substantial—depending on the field of study, associate degrees increase earnings by an estimated 15 - 47% per year (Stevens et al., 2019).

One potential tool for decreasing time to degree is to expand enrollment in summer courses. However, policymakers have been unsure about how to use this policy tool to improve post-secondary outcomes—Pell Grant funding for summer courses was approved, withdrawn, and subsequently re-approved in recent years.[1] Unlike the traditional K-12 school year, where summer is differentiated from the regular school year, college credit hours earned from summer courses are equivalent to those in the fall and spring. Despite their similarities, summer school courses are dramatically less popular among students. Only about 30 percent of students at two-year colleges and 21 percent at four-year colleges enroll in summer courses (Attewell and Jang, 2013).

It is not clear why summer enrollment rates are so low. College summer school has received relatively little research attention and student enrollment preferences are particularly poorly understood. This is an important gap because correlational data show that students who attend in the summer are significantly more likely to persist into subsequent semesters and graduate on time (Adelman, 2006). Attewell and Jang (2013) estimate that summer enrollment is associated with 26 percent and 10 percent increases in on-time graduation for two-year and four-year college students, respectively. However, there is little causal evidence on the impact of summer school.

In this paper, we seek to understand the causal benefits of college summer school and how the benefits connect to selection into summer enrollment. To do so, we implemented a field experiment with 398 community college students testing a policy that targets summer school. We focus on the effects of this policy on subsequent educational outcomes. Over the summers of 2016 and 2017, we randomly assigned scholarships to

[1] See Liu (2020) for a discussion of this policy change and its impact on student outcomes.

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students for a single summer course (worth \$405). Prior to assignment, we elicited students' preferences for the summer scholarships relative to fall scholarships. We then tracked enrollment, credit accumulation, degree receipt, and transfer to a four-year college both one and two years after the intervention ended.

The summer scholarship offer has large impacts on summer enrollment and degree acceleration. Scholarships increase summer enrollment by 20 percentage points, an almost 60% increase over the no scholarship control group ($p < 0.001$). Treated students are 7.3 percentage points ($p = 0.055$) more likely to graduate with an associate degree in the next year, a 32% increase above the control group. Transfer rates to four-year colleges increase by an estimated 7.6 percentage points ($p = 0.050$), a 58% increase. The impact on transfer rates persists two years after the program ends while the effect on associate degree attainment fades in the second year post-program. Taken together, we estimate that about eight percent of students graduate or transfer a year earlier than they otherwise would due to the one-time scholarship.

Given the large average impacts we estimate, we examine student preferences for summer school in order to better understand why so few students enroll in the absence of our intervention. In particular, preferences against enrolling in summer school could be rationalized if those students face substantially higher costs to enrollment or relatively lower benefits from enrollment. We explore heterogeneity by preferences for summer school, comparing students who prefer a summer scholarship to those who prefer a fall scholarship of the same value. We find that preferences strongly predict differences in baseline summer enrollment. In the absence of the scholarship, students who prefer summer are over three times as likely to attend summer school than those who prefer fall. We then explore the extent to which differences across students in summer enrollment preferences reflect either heterogeneous benefits or heterogeneous costs of summer enrollment.

We first examine the relationship between preferences and costs of summer enrollment. In a survey of barriers to summer enrollment, students who prefer fall are no more likely to cite the direct financial costs of being able to afford summer courses. However, they are significantly more likely to report other costs such as needing to work during the summer, not having time for summer courses, and disliking summer courses. If such costs are high enough, the scholarships could have a limited ability to induce this group into summer enrollment. This is not what we find. The effects of our scholarships on enrollment are as high, if not higher, among students who prefer fall or who report barriers to summer enrollment compared to those who prefer summer or do not report enrollment barriers. This suggests that, at the margin, whatever summer

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enrollment barriers students may face, there is a meaningful share of students for whom the costs are overcome by a $405 scholarship.

We next examine the relationship between preferences and benefits of summer school. We find that students who dislike summer school benefit substantially when induced to enroll. Indeed, the impact of our intervention is driven by students who have a preference *against* the summer scholarships. Among these students, we estimate an increase in one-year graduation and transfer rates of over fifty percent. We also conduct heterogeneity and selection tests, which estimate that the vast majority of our sample would experience positive impacts from attending summer school. These results suggest that preferences against summer school do not reflect low educational benefits to enrollment. Taken together, our results suggest that neither costs nor benefits are fully driving students' decisions about whether to enroll in summer school.

Because our findings leave open the question of why so few students enroll in summer school, we explore summer school behavior more generally and how our intervention affects selection into summer school. To do so, we examine enrollment behavior in our control group and in an observational sample of students who did not participate in our experiment. We find that, in the absence of our intervention, students who select into summer school have higher baseline achievement and that our intervention largely closes these achievement gaps. Despite inducing more marginal students into summer school, we find that treated students perform as well, if not better, than control group students in the summer courses they take. Examining summer course taking also suggests a mechanism for the large impacts of our intervention: summer courses are more likely to be ones that students previously failed. If students retake courses that are barriers to progressing toward their degree, then improving their performance in these courses could accelerate graduation and transfer.

While there is a large literature on general financial aid, it offers little evidence on financial aid targeting summer.[2] Prior work focused on summer financial aid uses policy changes to estimate the impact of expanding the availability of federal Pell Grants in summer terms. These studies find positive effects on summer credit completion (Bannister and Kramer, 2015; Friedmann, 2016) and increases in graduation rates, but decreases in transfer rates for community college students (Liu, 2020). The limited work on interventions targeting summer enrollment focuses on short-term impacts. Franke and Bicknell (2019) examine summer enrollment after the introduction

[2]See e.g., Carlson et al. (2019); Anderson et al. (2020); Anderson and Goldrick-Rab (2018); Angrist et al. (2016); Denning (2019); Denning et al. (2019); Carruthers et al. (2020); Angrist et al. (2020) for a discussion of the broader literature on financial aid and free community college. Nguyen et al. (2019) provide a recent review and meta-analysis.

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of a community college initiative that, like our intervention, funds a single summer course. They estimate increases in summer enrollment as well as persistence into the fall semester. Anzelone et al. (2020) experimentally test informational and financial aid interventions aimed at promoting summer enrollment and find an increase in summer enrollment and summer credits, but no impact on fall enrollment.[3]

We present a simple analysis suggesting that targeting summer school is potentially attractive from a cost-effectiveness perspective. Schools have unused capacity in the summer, so the marginal cost of expanding enrollment is low relative to other terms. Low baseline enrollment also means that fewer inframarginal students would receive subsidies without changing their enrollment behavior, and there is greater potential to influence the extensive margin compared to fall and spring terms. In our study, scholarships increase summer enrollment by 20 percentage points, from about a third in the control group to over half in the treatment group. Such increases are difficult if not impossible in non-summer terms. In our sample, about three-quarters of control group students enroll in the fall, so a 20 percentage point enrollment increase would require nearly full enrollment. Finally, targeting summer is much lower cost than providing full-year financial aid.

Our study provides promising evidence for interventions targeting summer. We demonstrate that a relatively low-cost intervention can help overcome the barriers to summer enrollment and accelerate long-run student success. More broadly, our findings suggest that many more students could benefit from summer school than the minority who currently enroll.

In the section that follows, we describe the details of our experimental design. Section 3 presents the experimental results. Section 4 explores selection and the generalizability of our results. To do so, we leverage a large, observational dataset of community college students that serves as a comparison group for our smaller experimental sample. Section 5 concludes with a discussion of the cost-effectiveness of our intervention and college summer school policies more broadly.

2 Experiment

We implemented our experiment in partnership with Ivy Tech Community College (Ivy Tech) of Indiana, which serves over 170,000 students statewide. Community colleges

[3]Anzelone et al. (2020) find no evidence of impacts on degree receipt but caution that they do not expect to detect effects given the length of the follow-up period. A related literature examines interventions targeting the summer between high school and college with a focus on increasing fall enrollment rates (e.g., Barnett et al., 2012; Castleman et al., 2014; Castleman and Page, 2015).

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like Ivy Tech currently serve almost forty percent of all undergraduates and half of those who will eventually earn a four-year degree (Snyder et al., 2018; McFarland et al., 2018). They also facilitate year-round enrollment by offering a variety of daytime, nighttime, weekend, and online courses to accommodate part-time and non-traditional students, such as those who work or have children.

Like many two-year colleges, Ivy Tech struggles with low retention and graduation rates. At the time of our experiment, Ivy Tech's performance on these outcomes was slightly better than the bottom 10 percent of community colleges. About 40 percent of fall term students were retained through the following fall term and fewer than one-quarter of full-time, first-time students graduated or transferred to a four-year institution within three years (NCCBP, 2014).

Our students were recruited from two of Ivy Tech's fourteen regions: East Central and Richmond. These regions included campuses in Anderson, Connorsville, Marion, Muncie, and New Castle.[4] The Ivy Tech East Central region serves a community in the 4th percentile of national median income, poorer than about 90 percent of community colleges. Over 60 percent of their student body is eligible for need-based federal Pell Grants, a higher rate than about 90 percent of community colleges (NCCBP, 2014).

Ivy Tech enrollment during the summer term is lower than the fall and spring terms. However, the vast majority of courses are still available.[5] Participants in our study enrolled in summer courses spanning 66 unique departments. No single department represents more than 10 percent of the courses taken.

2.1 Recruitment

Figure A.1 presents a visual summary of the eligibility, enrollment, and random assignment procedures that we used. We conducted the study in two waves: 2016 and 2017. During the Spring 2016 and Spring 2017 terms, our partners identified any currently-enrolled students who were eligible to participate in our study (see Appendix Figure B.1 for our recruitment email). A student was considered eligible if they were (1) currently enrolled at Ivy Tech, (2) not scheduled to graduate at the end of the current

[4]Since the conclusion of our intervention, the regional structure of Ivy Tech has changed. Additionally, retention and completion rates have risen.

[5]Using our broad convenience sample described in Section 4, we estimate that over 92% of the course enrollments from Spring 2017 were also available during Summer 2017. Technical programs, such as nursing, with strictly ordered curricula are an exception—enrollment is more continuous through the summer, and course options are limited. With fewer sections of each course in the summer term, a given section is more likely to be taught by a full-time faculty member, rather than an adjunct instructor. See Brownback and Sadoff (2020) for a discussion of the heterogeneous impact of instructors on student outcomes in the same community college context.

semester, (3) not currently enrolled in the summer semester, and (4) not included in any existing study incentivizing student enrollment behaviors. These selection criteria were developed in partnership with the Ivy Tech leadership in order to avoid confounds while retaining external validity.

Our first and second eligibility criteria addressed practical concerns. First, many students who were not currently enrolled had graduated, moved, or were otherwise inaccessible to our partner institution. Second, Ivy Tech's primary objective is to graduate students, so extending an intervention to students who had already achieved this objective made little sense. Our third eligibility criteria was designed around budget considerations. Subsidizing tuition for students already enrolled in the summer term would limit both the number of students we could afford to include in the study and the impact we could have on the behavior of participating students. Our fourth eligibility criteria helped us avoid confounds associated with other experimental studies running in parallel at the same Ivy Tech campuses. In 2016, there was a summer enrollment incentive given to all Pell-eligible students. To ensure that our incentives had the same dollar value to all participants, we restricted our sample to those not participating in this study—that is, non-Pell-eligible students. In 2017, summer enrollment incentives were assigned as part of Brownback and Sadoff (2020). Thus, participants in that study were ineligible.

Eligible students who were interested in participating enrolled by completing an online survey that was included in the recruitment email.[6] After students completed the enrollment survey and consented to participate, our partners matched the students' survey responses to administrative data containing their academic progress: enrollment, grades, credit accumulation, graduation, transfer, and dropout status. This matching was successful for 121 of 156 students in the 2016 cohort (78%) and 277 of 285 students in the 2017 cohort (97%).[7] Our random assignment occurred after successfully matching student data, so our internal validity is not threatened by this margin of attrition.

To better understand the external validity of our results, we compare demographic and baseline academic characteristics where available for (1) the enrolled participants, (2) the eligible participants, (3) the statewide Ivy Tech undergraduate population at the time of our two recruitment waves, and (4) statistics from all 2-year public colleges nationwide. As shown in Appendix Table A.1, female students selected into our study

[6] The study enrollment period for Spring 2016 began April 22nd, 2016 and ended May 6th, 2016. The study enrollment period in Spring 2017 began April 21st, 2017 and ended May 4th, 2017.

[7] We were more successful at matching the second cohort because of improved procedures for eliciting students' administrative identifiers.

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at slightly elevated rates relative to the eligible population and the broader student body. The proportion of white students in our sample is similar to the Ivy Tech population but higher than the share of white students at community colleges nationwide. Based on our qualification criteria, our first cohort had no Pell-eligible students, which clearly deviates from the overall Ivy Tech population. However, in our second cohort, our study sample is disproportionately likely to be Pell-eligible: 65% of students in the experimental sample are Pell-eligible compared to 40% of the Ivy Tech population and 35% of students at community colleges nationwide. We do not have academic data for the statewide Ivy Tech population nor for the nationwide 2-year public college population but we can compare participants to eligible non-participants with respect to baseline credits accumulated and GPA. Participating students tended to have higher GPAs and to be further along in their academic careers. To the extent that treatment effects are larger (smaller) among these students, our estimated impacts may overestimate (underestimate) the treatment effects in the broader population. We examine treatment effect heterogeneity for these characteristics in Tables A.5 and A.6.

2.2 Preference elicitation

During the online enrollment survey, we explained to students the nature of the scholarships, when and how they could be used, and their exact tuition value. We then elicited cash-equivalents for both summer and fall tuition scholarships, relative preferences between the two scholarships, and the relative value of unconditional cash rewards delivered in the summer versus the fall. The first two elicitations provide revealed preferences for the scholarships. The third elicitation provides a measure of average discounting between the two time periods. To ensure incentive compatibility, we randomly selected one participant from each wave and implemented one of their decisions that we selected at random.

Our primary preference measure captures the *relative* value of summer and fall tuition scholarships. To elicit individual preferences, we conducted a multiple price list in which students chose their preferred option between a free summer course or a free fall course to identify weak preferences for summer. The multiple price list then compared (1) a free summer course to a fall course with a varying price and (2) a free fall course to a summer course with a varying price. This revealed the willingness to pay to receive the scholarship in the preferred term, potentially identifying strict preferences between summer and fall scholarships. We used a similar elicitation to measure the relative value of receiving unconditional cash rewards in the summer versus the fall. See Appendix Figures B.2 and B.5 for screenshots of the preference elicitations.

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We also elicited the cash value of summer and fall tuition scholarships for each student through multiple price lists. Students first chose between a summer scholarship and amounts of money ranging from $50 to $300 and then chose between a fall scholarship and the same money amounts. We estimate a student's cash value for each scholarship as the midpoint between the highest amount for which the student prefers the scholarship and the lowest amount for which the student prefers the cash. See Appendix Figures B.3 and B.4 for screenshots of the preference elicitations.

Along with enrollment preferences, we asked for stated summer enrollment plans, graduation plans, and reasons for non-enrollment in the summer semester. We provided multiple-choice options as reasons for non-enrollment along with a free response option (see Appendix Figure B.6 for the complete list).

2.3 Randomization

Our experimental sample includes 398 enrolled and matched students across the two cohorts. Based on budget availability, we randomly awarded 69 scholarships in the 2016 cohort (57%) and 97 scholarships in the 2017 cohort (35%). The scholarships had a face value of $405 and could be used to pay for tuition for one summer course of up to three credit hours (scholarships did not cover other costs such as books, materials, and lab fees).

We assigned the scholarships using a stratified randomization within each cohort. In the 2016 cohort, the randomization strata were: five Grade Point Average (GPA) groups, above or below the median summer scholarship preferences (elicited through the enrollment survey), above or below the median age, and gender. In the 2017 cohort, the randomization strata were: three GPA groups, above or below the median age, and gender. The assignment ratio was constant across strata within a cohort but varied across cohorts because of different budget constraints. In our analysis, we control for differences in the stratification and assignment ratio by using fixed effects for cohort.[8] Table 1 shows no differences in baseline characteristics between the treatment and control groups that are statistically significant at the 10% level.

[Insert Table 1 here.]

[8]We adjusted the randomization strata between the two cohorts because the added strata did not allow enough variation in the potential randomizations of the 2017 cohort.

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3 Results

Our data include 398 total students across the 2016 and 2017 cohorts. For all students, we have educational outcomes from the Spring 2016 term through the Summer 2019 term. This gives us ten and seven terms of post-assignment outcomes for the 2016 and 2017 cohorts, respectively. To ensure comparability across cohorts, we evaluate the program based on outcomes in the one-year or two-year windows after the intervention.[9] We first estimate the impact of our scholarships on summer enrollment. We then examine key educational outcomes for community college students: graduation with an associate degree and transfer to a four-year school to pursue a bachelor's degree.[10] Finally, we explore heterogeneity by enrollment preferences.

3.1 Enrollment

We begin by examining the impact of our scholarship offer on summer enrollment. Figure 1 presents the distribution of summer credit hours attempted for both the treatment and control students. In the control group, 33 percent of students enroll in the summer term (i.e., attempt more than zero credits), which is similar to rates at community colleges nationally (Attewell and Jang, 2013). These rates are far lower than students' stated enrollment plans: 56 percent state they plan to enroll with an additional 30 percent stating they may enroll. The scholarship offer significantly increases summer enrollment with 52 percent of treatment students enrolling. These results suggest that the scholarships help students better fulfill their enrollment intentions.

Figure 1 shows that the treatment effects are almost entirely on the extensive margin. The scholarship offer decreases the share attempting zero credits and increases the share attempting three credits—the maximum value of the scholarship. We find no evidence of effects on the intensive margin—attempting more than three credits—and therefore focus on the extensive margin in our analysis.

[Insert Figure 1 here.]

[9]We define the one-year (two-year) post-program windows as one year (two years) after the *completion* of the summer term—i.e., end of summer 2017 (2018) for the 2016 cohort, and end of summer 2018 (2019) for the 2017 cohort.

[10]The degree and transfer categories are not mutually exclusive. Around half of the students who start at two-year colleges and eventually earn degrees from four-year institutions do so after completing a two-year degree (Shapiro et al., 2018). Ivy Tech also provides over 100 different certificates. We do not evaluate these because of the vast heterogeneity in requirements for and benefits of these certificates. Further, 78% of students in our sample who receive a certificate go on to receive an associate degree or transfer.

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Panel A of Table 2 presents OLS regression estimates of the treatment effect on different measures of enrollment, which are reported for each row. All regression estimates include covariates for cohort, baseline GPA, baseline credit accumulation, age, race, gender, and stated plans for enrolling in the summer term. We estimate that scholarships increase enrollment rates by 20.3 percentage points, a nearly 60% increase over the control group ($p < 0.001$). This enrollment increase is concentrated in 3-credit courses, translating to an estimated increase of 0.586 credit hours attempted during the summer term ($p = 0.018$). At the end of the summer, treated students have completed 0.489 more credit hours than control group students ($p = 0.040$), a 32% increase above baseline.

We employ an instrumental variables approach to estimate the impact of summer enrollment on credits attempted and credits completed in Column 2 of Panel A. Students who are experimentally induced to enroll in the Summer term attempt an average of 2.885 additional summer credits and complete 2.408 of those credits ($p < 0.01$ for both). For comparison, we present correlational estimates from the control group in Column 3. In the control group, enrolling in the summer has an effect on credits attempted and credits completed that is nearly 50% larger than the instrumental variables estimate from Column 2.

[Insert Table 2 here.]

3.2 Educational outcomes

Panel B of Table 2 presents the one-year and two-year impacts on graduation with an associate degree and transfer to a four-year college. The dependent variable is reported for each row. All regressions estimate a linear probability model. In Column 1, we estimate the Intent to Treat (ITT) effects of offering students a summer scholarship regardless of whether the student uses the scholarship. In Column 2, we use assignment of the scholarship as an instrument for summer enrollment to estimate the causal impacts of experimentally-induced summer enrollment. These can be compared to correlational estimates relating summer enrollment and educational outcomes in the control group, which are presented in Column 3. Column 4 reports control group means.

We find large impacts of the scholarship offer on graduation and transfer rates within one year of the intervention (Column 1). We estimate that one-year graduation rates increase by 7.3 percentage points ($p = 0.055$), a 32% increase over the control group in which fewer than a quarter of students receive a degree. Our intervention increases transfer rates by an estimated 7.6 percentage points ($p = 0.049$), a 58% increase. Combined, we estimate a 7.7 percentage point ($p = 0.082$) increase in graduation or transfer within one year, a 25% increase.

When we expand the evaluation window to two years after the intervention, the impact of the scholarship offer on combined graduation or transfer falls to a statistically insignificant 1.8 percentage point increase ($p = 0.707$). Similarly, the treatment effect on associate degree attainment is small and not significant. However, the impact on transfer rates remains large: an estimated 8.5 percentage point increase ($p = 0.034$), which is a 58% increase.

Our instrumental variables approach is presented in Column 2 and shows that scholarship-induced summer enrollment increases the one-year rates of graduation and transfer by an estimated 36 percentage points ($p = 0.066$) and 37 percentage points ($p = 0.061$), respectively. The impact of summer enrollment on the combined graduation or transfer measure is an estimated 38 percentage points ($p = 0.088$).

The causal estimates we find are larger than the correlations observed in the control group. As shown in Column 3, the association between summer enrollment and one-year rates of combined graduation or transfer is about 15 percentage points ($p = 0.033$), less than half of the size of the IV estimate. This difference appears across educational outcomes and evaluation windows. We note that the difference between correlational and IV estimates could reflect selection bias in the correlational data or could be due to differences in the average treatment effect (ATE) compared to the treatment effect on the compliers (i.e., the local average treatment effect, LATE). We examine selection into summer school in Section 4.2 and the extent to which there may be larger treatment effects among compliers in Section 4.4.

Using National Student Clearinghouse (NSC) data, we can consider a longer time horizon as well. Appendix Table A.3 presents the impact of the scholarships on post-transfer outcomes up to five years after the intervention. Given the small fraction of students who transfer, this test has limited statistical power. For this reason, despite finding that our scholarships increase bachelor's degree attainment by 68%, our effects on these margins are not statistically significant.

Taken together, our results show that scholarship-induced summer enrollment substantially accelerates time to degree (i.e., graduating within one year) and has a persistent impact on rates of transfer to four-year colleges both one and two years after the intervention. As noted above, improving transfer rates is critical for community colleges: over eighty percent of students intend to transfer to a four-year college, but only about a quarter achieve that goal (Jenkins and Fink, 2016).

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3.3 Mechanisms

We explore potential mechanisms for the effects on graduation and transfer by examining the impact of scholarship-induced summer enrollment on enrollment in subsequent terms, credit accumulation, and GPA. Panel A of Table 3 presents the causal (IV) estimates for each outcome by term and Panel B presents the corresponding correlational estimates.

[Insert Table 3 here.]

Columns 1–3 of Panel A evaluate the causal effect of summer enrollment on enrollment in subsequent terms. Our estimates are small and vary in sign. This casts doubt on mechanisms such as "momentum" or habit formation where summer enrollment encourages subsequent enrollment—indeed scholarship-induced summer enrollment has a directionally negative impact on the likelihood of enrolling in the fall term.

We find suggestive evidence that the impact on degree acceleration is due at least in part to credits accumulated through summer enrollment. Columns 4–6 of Panel A report the total credits accumulated as of the end of the indicated semester. The consistent sign and magnitude suggest that the boost of credits experienced during the Summer term (2.408, as reported in Panel A of Table 2) is largely carried forward into subsequent terms. Treated students retain their advantage of about three additional accumulated credits through the end of the first year post-intervention, though the estimates are not statistically significant. The effect of summer enrollment on credit accumulation is equivalent to $40-52\%$ of an entire fall or spring semester.[11] This large credit accumulation relative to the average semester offers one potential reason why we find such large estimates for the impact of summer enrollment on degree acceleration.

Additionally, Columns 7–9 of Panel A provide suggestive evidence that summer enrollment improves grades in subsequent courses. We find that the impact of summer enrollment on GPAs is directionally positive in the three subsequent terms. This improvement in GPA could come from the reduced course loads required as a result of the accumulated credits over the summer or it could reflect downstream effects of improved learning during summer courses. However, we note that these data are imperfect, as we can only estimate GPA for students who enroll in a given term and our estimates could be driven by selection out of the sample.

Unlike our causal (IV) estimates, Panel B shows that our correlational estimates are directionally consistent with prior observational studies, which find evidence of an

[11] Control students complete an average of 5.97 and 4.64 credits during the Fall and Spring semesters after the intervention, respectively.

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association between summer enrollment and retention into the next school year (often called "momentum") (Attewell and Jang, 2013; Franke and Bicknell, 2019). Column 3 of Panel B shows that our correlations suggest a role for habit formation as well, since students who enroll in the Summer term are more likely to enroll in the subsequent Summer term. Columns 4–6 of Panel B present the correlations between summer enrollment and credit accumulation, which are almost twice the size of our causal estimates. Columns 7–9 of Panel B replicate our analysis of the impact of summer on student GPA in subsequent terms. The correlations are universally small and statistically insignificant. Our results demonstrate clear differences between correlational and causal estimates of the relationships between summer enrollment and subsequent outcomes.

3.4 Enrollment preferences

As discussed in Section 2, we measure preferences for summer enrollment using incentivized multiple price lists that elicit the value of summer scholarships both relative to cash and relative to fall scholarships. Students value the summer scholarship at about \$238 (60 percent of its face value), and their value of a fall scholarship is $6-7\%$ higher on average (see Appendix Figure A.2 for the distributions).

Average preferences mask important heterogeneity. Using preferences between summer and fall scholarships, we find that 54 percent of students hold at least a weak preference for the fall scholarship (i.e. they prefer a free fall course to a free summer one); and 46 percent hold at least a weak preference for the summer scholarships (i.e., they prefer a free summer course to a free fall one). We can further classify 37 percent of students as strictly preferring fall and 28 percent as strictly preferring summer. These students are willing to sacrifice scholarship value to receive the scholarship in their preferred term. In the analysis below, we split the sample by weak preferences: "Prefer Fall" and "Prefer Summer."[12]

We first explore heterogeneous costs of summer enrollment, which may drive enrollment preferences and behaviors. We focus on students' stated barriers to summer enrollment from our baseline survey (about one-fourth of our participants report at least one barrier to enrollment). Table 4 estimates the association between the most

[12]We exclude from the preference analysis one participant whose responses to the elicitation did not meet any of the classifications. For 98 percent of participants, their preference for Fall vs. Summer scholarships is weakly consistent with their revealed cash value of the scholarships—those who weakly prefer fall to summer also have a cash value for a fall scholarship that is at least as high as their cash value for a summer scholarship and vice versa.

commonly reported barriers and preferring summer to fall scholarships.[13] Students are significantly less likely to prefer the summer scholarship to a fall scholarship if they report needing to work during the summer ($p = 0.003$), having no time for summer courses ($p = 0.004$), or disliking summer courses ($p < 0.001$). Interestingly, we do not find a strong relationship between students who report that they cannot afford summer courses and those who Prefer Summer ($p = 0.789$). Pooling all stated barriers, those who report any barrier to summer enrollment are 20 percentage points less likely to Prefer Summer ($p = 0.007$).

[Insert Table 4 here.]

We next link enrollment preferences to summer enrollment and educational outcomes. Figure 2 shows summer enrollment rates by treatment group and enrollment preferences. Figure 3 then shows the rate of combined graduation or transfer one year post-program. We present regression-adjusted estimates for the same outcomes in Table 5, interacting treatment with enrollment preferences (Column 1) and with reporting any barrier to summer enrollment (Column 2).[14]

[Insert Figure 2 here.]

As shown in Figure 2, our elicited preference measures hold strong predictive validity over actual summer enrollment behaviors at baseline. In the control group, students who Prefer Summer are over three times more likely to enroll in summer school than students who Prefer Fall (50.0% vs. 16.5%). Despite large baseline differences in enrollment, scholarships significantly increase enrollment rates among both students who Prefer Fall and those who Prefer Summer, by an estimated 18-26 percentage points. Column 1 of Table 5 shows that the treatment effects on summer enrollment are directionally larger for students who Prefer Fall but not significantly different across the two groups ($p = 0.453$). Column 2 of Table 5 reveals similar results focusing on students who report barriers to summer enrollment: they are less likely to enroll at baseline ($p < 0.001$), the scholarship significantly increases their enrollment rates ($p < 0.001$), but there is no difference in treatment effects compared to those who do not report

[13] The "Other" category pools barriers to summer enrollment with fewer than 10 positive responses. See Appendix Figure B.6 for a complete list.

[14] We explore additional drivers of heterogeneity in Appendix Tables A.5 - A.6 using survey measures (prefer to receive unconditional cash in summer vs. fall, plan to enroll in summer school, semesters until planned graduation), baseline academic measures (completed semesters at Ivy Tech, baseline GPA, baseline credits), and demographics (age, gender, race). Many characteristics predict baseline summer enrollment and baseline graduation or transfer, but the only characteristic that predicts heterogeneous treatment effects with marginal statistical significance is baseline credits.

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barriers to enrollment ($p = 0.970$). Students who Prefer Fall may face significant barriers that dampen their summer enrollment at baseline, including disliking summer courses and needing to work. Critically, however, our results demonstrate that whatever costs or constraints these students face, the summer scholarships are highly effective at encouraging their summer enrollment.

[Insert Table 5 here.]

Finally, we examine the extent to which enrollment preferences may reflect heterogeneity in how summer school benefits educational outcomes. Similar to summer enrollment behaviors, we find a positive association at baseline between elicited preferences for the summer scholarship and educational outcomes. As shown in Figure 3, one-year graduation or transfer rates in the control group are almost two times higher for students who Prefer Summer compared to those who Prefer Fall (39.7% vs. 21.7%). However, we find no evidence that enrollment preferences are positively related to the *causal* impact of our intervention. In contrast, the treatment effects are concentrated among students who have a preference *against* the summer scholarships. For these students who Prefer Fall, summer scholarships increase one-year graduation and transfer rates by 11-17 percentage points—more than 50% above baseline ($p = 0.068$). Column 3 of Table 5 reveals little impact of the scholarship offer on students who Prefer Summer, though the effects are not statistically distinguishable between the subgroups ($p = 0.256$). Column 4 corroborates this, showing that the impact of our treatment is directionally larger for students who face barriers to summer enrollment, but this heterogeneity is also not statistically significant ($p = 0.385$). Note also that Column 4 shows the baseline associations between enrollment barriers and educational outcomes are weaker than those for enrollment preferences.

We note that, while the enrollment preferences we elicited are predictive of baseline academic behaviors, educational outcomes, and treatment effect heterogeneity, they are not exogenously determined. For this reason, we explore correlates of summer enrollment preferences in Appendix Table A.4. We find that other survey measures (discount rates, summer plans, and proximity of planned graduation) correlate with preferences for summer enrollment. Preferences for summer enrollment are also correlated with baseline credits accumulated and gender. Finally, we find that summer enrollment preferences correlate with the number of credits accumulated during the prior summer term. This could reflect either that summer preferences are stable—inducing enrollment in both the prior summer and the current summer—or that experience in the prior summer shifts preferences towards future summer enrollment (or some combination of the two).

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In total, our results show that students who dislike summer school or face barriers to summer enrollment still benefit substantially when induced to enroll.

[Insert Figure 3 here.]

4 Generalizability

In this section, we explore summer school behavior more generally and how our intervention affects selection into summer school. We also address potential concerns about the representativeness of our sample and the generalizability of our results.

In order to understand summer school behavior in the absence of our intervention, we incorporate a convenience sample of 1,372 unique students from the East Central region of Ivy Tech who began their Ivy Tech studies in the fall of 2016.[15] To best parallel our experimental sample, we study the period between Fall 2016 and Summer 2019. We observe every course in which these students choose to enroll, constituting 17,599 student-course observations.

4.1 Heterogeneity by financial aid status

As we discussed in Section 2.1, our 2016 cohort was limited to students who were not Pell-eligible because of coinciding studies on this population of students. Because of this selection, there may be a concern that our treatment effects are driven by students with unusually favorable financial circumstances.

Our 2017 sample, which faced no selection based on Pell-eligibility, offers us the opportunity to demonstrate the broad impacts of our treatment across students with varying financial circumstances. In Table 6, we restrict our sample to the 2017 cohort and conduct a test of heterogeneous treatment effects based on financial aid status. We include two measures of financial aid status: 1) whether or not a student is Pell-eligible, and 2) the total amount of need-based grants a student receives. The two measures allow us to examine heterogeneity in treatment effects at the extensive and intensive margins of financial aid.[16] In addition, because need-based financial aid increases as greater need is determined, the measure provides an intensive margin of financial resources.

[15] We restrict our sample to students with zero credits and no prior degrees as of the beginning of the Fall 2016 term.

[16] In our sample, 85% of Pell-eligible students receive need-based aid and 6% of Pell-ineligible students receive need-based aid.

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Table 6 estimates treatment effects on enrollment and one-year graduation or transfer interacted with financial aid status, as measured by Pell eligibility and need-based grants received. As shown in Columns 1 and 2, greater financial need is associated with lower summer enrollment in the control group. Interacting financial need with treatment, we find that treatment effects on summer enrollment are directionally larger for Pell-eligible students ($p = 0.093$) and as need-based grants increase ($p = 0.096$). Column 3 shows that the impact of our scholarships on one-year graduation or transfer rates is also potentially higher among Pell-eligible students than Pell-ineligible students ($p = 0.072$) with no evidence of treatment effect heterogeneity by need-based grants (Column 4).

[Insert Table 6 here.]

The results of Table 6 suggest that, if anything, our intervention is more beneficial for students with financial needs. We find that students with greater financial need are marginally more responsive to our scholarships with their summer enrollment decisions and that the impact of our scholarships on graduation and transfer rates may be higher among these students as well. These results suggest that our treatment effects could generalize to Pell-eligible students in the broader community college population.

4.2 Selection into summer school

In the section above, we showed that our intervention may induce students with greater financial need to enroll in summer school, helping to close baseline gaps in enrollment. In this section, we examine the extent of positive selection into summer school based on academic performance and how summer scholarships may affect who enrolls.

In the first two rows of Table 7, we examine the baseline selection of students into summer enrollment in the absence of our intervention. All estimates are presented alongside p-values from t-tests of differences. Estimates in the first row are derived from our convenience sample where we conduct a student-level comparison of academic characteristics (baseline credits and GPA) for students who enrolled in the first available summer term (the Summer 2017 term) and students who did not. Less than a third of the students enroll in summer school, similar to enrollment rates nationally (Attewell and Jang, 2013).

Academically, we find strong positive selection into summer school. Summer students have accumulated 81% more credits by the beginning of the summer term and have average GPAs that are over two times higher than students who do not attend summer school ($p < 0.001$ for both). In the second row, we show that students enrolled

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in summer from our control group have significantly higher GPAs and marginally significantly more credits. This pattern of positive selection on academics is similar to our observational sample, suggesting that the summer school behavior in our experimental sample is in line with the broader population.

[Insert Table 7 here.]

In the bottom row of Table 7, we replicate this selection analysis for the students enrolling in the summer term from our treatment group. We find that the differences in academic characteristics disappear: students enrolled in the summer term no longer have significantly higher baseline credits nor do they have significantly higher GPAs. These results suggest that summer scholarships could help close baseline achievement gaps in summer school enrollment. This pattern of selection also suggests that the positive impacts of the scholarships are not driven by a positive selection of students into summer enrollment.

4.3 Summer school course selection and performance

We return to our convenience sample of observational data to complement our student-level analysis with a more detailed, student-course-level analysis of the courses students select during summer terms as well as their performance in those courses. We use our data on every course that our sample enrolls in to compare the courses taken during the summer term to those taken during the fall and spring terms. Importantly, we compare behavior in this larger sample where summer enrollment is unincentivized to the behavior of our students who are experimentally induced into summer enrollment. Columns 1–2 of Table 8 present the results of regressions with student-level and course-level fixed effects to first identify a key margin of course selection—whether the student previously failed the selected course. Columns 3–6 then explore student performance within the selected courses—their grade points and their completion rates.[17]

In our observational data (Columns 1, 3, 5), we find that students select more challenging courses but tend to perform worse in those courses. Column 1 shows that a given summer course is 8.5 percentage points more likely to have been previously failed by the student than a given non-summer course ($p < 0.001$). Column 3 then shows that students' grades are 0.122 grade points lower in their summer classes relative to their fall and spring grades ($p = 0.002$). Finally, Column 5 shows that, directionally, students are less likely to complete summer courses, but this result is not statistically

[17] With both student fixed-effects and course fixed-effects, all variation is identified within an individual student-course combination.

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significant ($p = 0.338$). Thus, despite the positive selection of students we identified in Table 7, these students tend to underperform during the summer term relative to their performance in the fall and spring terms.

[Insert Table 8 here.]

We replicate this analysis using our experimental data (Columns 2, 4, 6) to help uncover potential mechanisms through which our scholarships may influence student outcomes: 1) the types of courses selected during the treated summer term and 2) the performance in the selected courses. To characterize these mechanisms, we leverage data on all courses that students in our study enrolled in between Spring 2016 and Summer 2019—a total of 5,704 student-course observations. As with the observational data, we examine the likelihood of retaking previously failed courses during the intervention summer compared to the other terms and include an interaction term to identify the differential impact of our scholarships on this behavior.

Column 2 of Table 8 presents our findings on previously-failed courses. We find that, on average, all students are 8.0 percentage points more likely to retake a previously-failed course during the treated summer term than during other terms ($p = 0.056$). Note that this pattern of behavior is almost identical to our findings from Column 1. For treated students, this enrollment behavior increases slightly but not significantly. Columns 4 and 6 corroborate our findings from Columns 3 and 5, showing that, on average, student grades and completion rates are lower during the treated summer relative to other terms ($p = 0.014$ and $p = 0.085$, respectively). If students who will perform relatively better in summer school select into summer enrollment at baseline, then the marginal students that our scholarships induce into summer enrollment should diminish the average performance for the treatment group relative to the control group. However, and in contrast to the predictions of a selection model, our point estimates suggest that our scholarships do not further reduce performance. Indeed, relative to control students, treatment students receive higher grades and complete courses at a higher rate during the treated summer term relative to their fall and spring performance, though neither effect is statistically significant ($p = 0.136$ and $p = 0.651$, respectively).

The mechanisms presented in Table 8 suggest an additional pathway that may contribute to the substantial impact our scholarships had on the long-run outcomes of students. Inducing students into summer may increase the likelihood that students retake courses that are barriers to progressing toward their degree. Improving performance in these courses may partly drive subsequent credit accumulation and performance discussed in Table 3 and accelerate progress toward their degree.

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4.4 Selection Based on Treatment Effects

We can further explore the population of students with potential benefits from summer enrollment by comparing outcomes among those who enroll from the treatment and control groups (i.e. those who enroll with and without the summer scholarships). Appendix Table A.7 follows the approach of Kowalski (2016) to identify marginal treatment effects (MTE). We find that the educational benefits for treatment group students induced into summer enrollment are statistically indistinguishable from control group students who enroll without scholarships. We note that this analysis is likely underpowered. Directionally, summer enrollment has a smaller impact on one-year graduation rates but a larger impact on one-year transfer rates for treatment group students (i.e., "compliers" and "always takers"), compared to control group students (i.e., "always takers"). Combining graduation and transfer, the point estimates of the regression suggest that over 90 percent of students would see positive impacts from summer enrollment.[18]

Taken together, we find strong support for the external validity of our estimated impacts of the summer scholarships. First, the local average treatment effect itself is estimated based on a large population—20 percent of treatment group students are induced to enroll despite a baseline summer enrollment of only 33 percent. Second, the effects hold among a population of students who dislike summer and enroll at very low rates at baseline. Third, our MTE analysis suggests that there are positive treatment effects for the vast majority of students regardless of their propensity to enroll. These results suggest that the population of students who would benefit from summer enrollment is much larger than the minority who enroll in the absence of the scholarships.

5 Discussion and Conclusion

We find a large causal impact of summer tuition scholarships on educational outcomes, suggesting that targeting summer school is a promising avenue for students and schools. This evidence is also critical for financial aid policy, which has been inconsistent in

[18]The coefficient on "Treatment $\times$ Summer Enrollment" estimates the impact of summer enrollment in the treatment group relative to Summer Enrollment in the Control group. The interaction effect is positive for transfer rates and negative for associate degree receipt and is never statistically significant. To extrapolate to the full population with Summer Enrollment propensities measured continuously from 0 to 1, the sum of "Treatment" plus "Treatment x Summer Enrollment" is positive for 94% of the sample (i.e., $0.095 + (-0.101) \times 0.94 \approx 0$). Appendix Table A.8 compares survey measures and demographics across compliance categories.

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providing aid for summer terms.

Our scholarships demonstrate that summer-focused interventions can be highly cost-effective. Consider the following back-of-the-envelope cost-benefit analysis. For students who took up the scholarship, it had a face value of \$405 worth of tuition. Given take-up rates of 51.8%, the intervention has a direct cost of approximately \$210 per student. Suppose that the 20.3% of students who were induced into summer school faced costs that exceeded their benefits by up to \$405—the maximum differential that the scholarships could overcome. Under these assumptions, the aggregate social cost of the intervention would not exceed $\$210 + \$405 \times 0.203 = \$292$ per student. Within one year of the intervention, the scholarships increased the rate of graduation and the rate of transfer by an estimated 7.3 and 7.6 percentage points, respectively. Thus, each additional student induced to graduate within one year of the intervention costs no more than $\$292/0.073 = \$4,000$.

A growing literature estimates average earnings returns to associate degrees of between $17-40\%$ (Stevens et al., 2019; Bettinger and Soliz, 2016; Kane and Rouse, 1995; Marcotte, 2019; Marcotte et al., 2005). This translates to an estimated salary increase of \$6,579 – \$15,480 per year from an associate degree compared to some college but no degree.[19] Belfield and Bailey (2017) conduct a review of the literature and provide a more conservative estimate of average returns between \$4,640 – \$7,160 in yearly salary. We match our detailed information on students' fields of study to the field-specific returns from Stevens et al. (2019) to estimate the earnings gains for our graduating students. Assuming the expected earnings gains for our students match these field-specific averages, our treatment group graduates should experience an average yearly return of \$8,516 from their associate degrees. This means that the one-year earnings gain more than doubles our conservative estimate of the cost of the intervention. Even if earnings in the first year after graduation are below these average estimated returns, the intervention is likely to be cost-effective based only on accelerating graduation by one year.[20]

These rough estimates do not include the benefits *to schools* from increasing summer enrollment. For postsecondary institutions, accelerating time to degree is increasingly critical to maintain their funding.[21] Finally, the cost-effectiveness of summer schol-

[19]The 2017 BLS estimate of median earnings for some college with no degree is \$38,700.

[20]Levin and García (2018) provide a comprehensive cost-benefit analysis of accelerating community college degrees in the context of the Accelerated Study in Associate Programs (ASAP). In addition to the earnings returns, they document substantial increases in tax revenues and reduced costs of public services for health, public assistance, and crime.

[21]See, Callahan et al. (2017) for information about performance-based funding for Indiana's community colleges. Additionally, the California Governor's 2018-2019 Budget assigned part of a com-

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arships compares very favorably to traditional financial aid, which has been studied more comprehensively. For example, Denning et al. (2019) and Park and Scott-Clayton (2018) find no impact on community college students' credit accumulation or degree progress of Pell Grants—which provide about twice as much funding per student as our scholarships. Prior estimates from four-year schools find that it costs $53,125 in grants for one student to graduate one year earlier, over ten times our estimated cost of $4,000 to induce one student to graduate within a year of the intervention (Denning, 2019).[22] The differences in cost may be due in part to differences between four-year schools and community colleges. But they may also reflect that aid targeting summer has a greater impact. Among community college students, Liu (2020) examines the impact of year-round Pell (YRP), which does not specifically target summer but does expand the availability of Pell Grants to the summer. The paper estimates that every $1,000 of YRP funding increases associate degree completion among Pell-eligible students by 2.2 percentage points compared to Pell Grants that are restricted to the fall and spring. By comparison, we estimate that eligibility for a $405 scholarship that is *only* available in the summer increases one-year graduation rates by 7.3 percentage points. Our results suggest that specifically targeting summer could have a large impact on student outcomes.

Given the large returns to summer enrollment that we estimate, it is puzzling that enrollment is so low. We examined student preferences for summer school in order to understand whether student preferences against summer school could be explained by either substantially higher costs to attendance or relatively lower benefits to attendance. While preferences are strongly associated with enrollment, we find no evidence that these preferences reflect the educational benefits of summer school. Additionally, on the cost side, many of the barriers to enrollment can be addressed with a relatively low-cost intervention. This creates an opportunity for schools to unlock achievement gains if they can expand summer enrollment. Summer scholarships represent one policy in this direction—they are scalable, cost-effective, and take advantage of the underutilized resources during the summer term. Our finding that students who prefer fall scholarships experience as large if not larger treatment effects than those who prefer

munity college's funding based on 3-year completion rates `http://www.ebudget.ca.gov/2018-19/pdf/BudgetSummary/HigherEducation.pdf`. Similarly, Arkansas' higher education funding is partially contingent on on-time degree completion `https://static.ark.org/eeuploads/adhe/ADHE_Policy_-_8-14-18_for_WEB.pdf`.

[22] Related work focused on four-year colleges estimates that every $1,000 in grant eligibility increases six-year graduation rates by 3.5 percentage points (Castleman and Long, 2016) and every $1,000 in grant aid received increases five-year graduation rates by 5 percentage points with subsequent increases in labor market earnings (Denning et al., 2019).

summer scholarships suggests that students who are less likely to seek out summer school opportunities may benefit most from them. More generally, these findings suggest that students may not be fully aware of or may not be fully considering the benefits of enrolling in summer school.

Future work could examine the extent to which our results replicate at scale among a larger population. And, at the same time, could further explore the mechanisms driving student preferences, such as inattention, the habit of having summers off that may carry over from the K-12 context, beliefs about the expected benefits of summer school, (perceived) costs of attendance, anticipated course selection, and how students trade off the short-run costs and long-run benefits. This could help inform the design of interventions that identify and target students who experience the largest benefits from summer enrollment.

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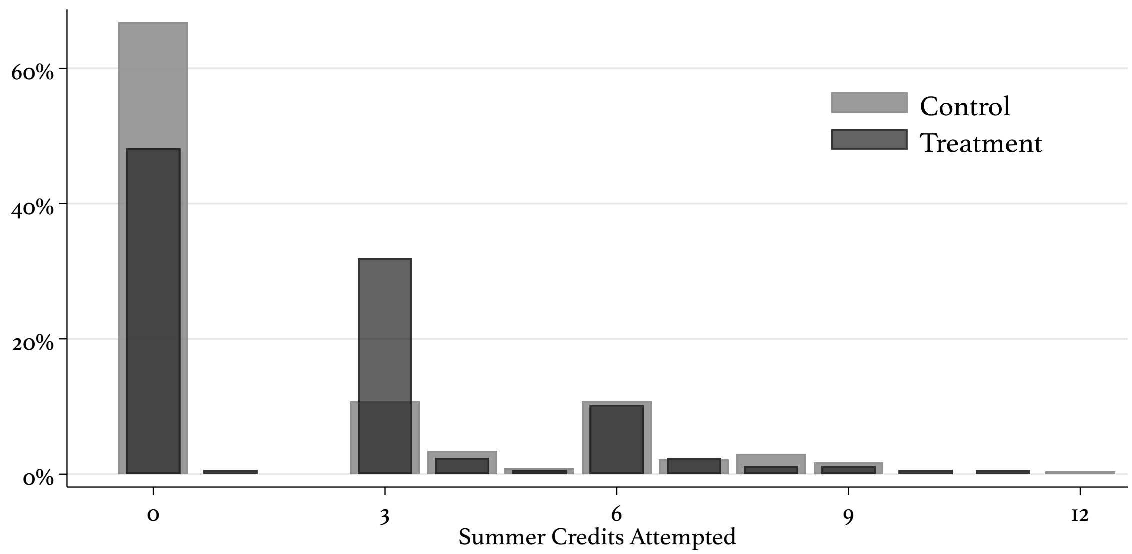
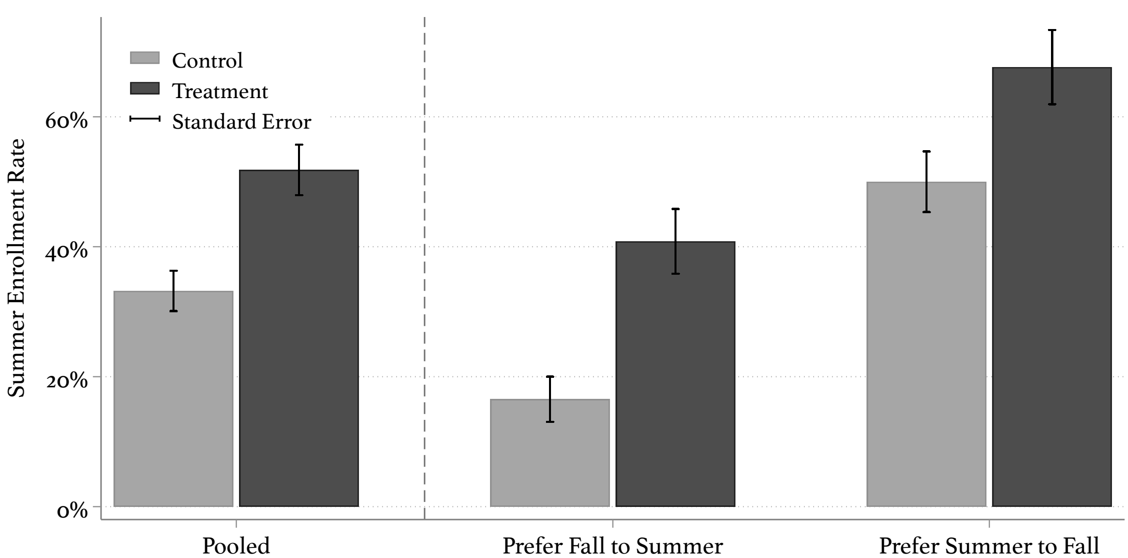
Figure 1: Credit Hours Enrolled in during the Program Summer



Notes: Enrollment reflects all credits attempted during the program summer including failed and withdrawn courses.

Figure 2: Enrollment Preferences and Summer Enrollment



Notes: Students who "Prefer Fall to Summer" prefer a scholarship for a free fall course over a scholarship for a free summer course. Students who "Prefer Summer to Fall" prefer a scholarship for a free summer course over a scholarship for a free fall course.

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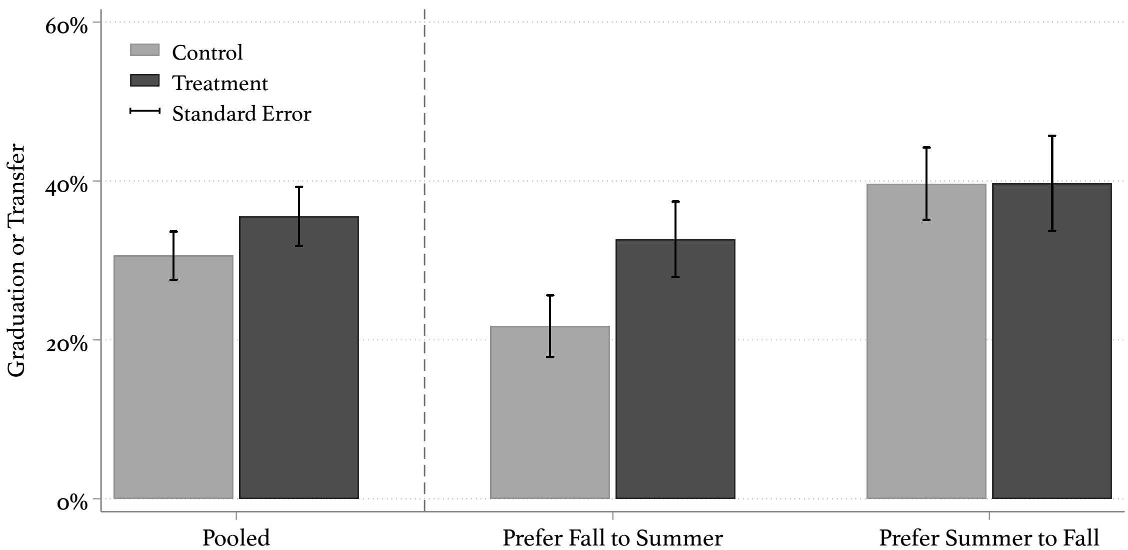
Figure 3: Enrollment Preferences and One-Year Graduation or Transfer Rates



Notes: Students who "Prefer Fall to Summer" prefer a scholarship for a free fall course over a scholarship for a free summer course. Students who "Prefer Summer to Fall" prefer a scholarship for a free summer course over a scholarship for a free fall course.

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Table 1: Baseline Characteristics by Treatment and Semester

	Summer 2016			Summer 2017		
	Control	Treatment	**t-test**	Control	Treatment	**t-test**
Demographics						
Age	28.250	29.449	**0.594**	28.678	29.155	**0.701**
	(1.661)	(1.495)		(0.725)	(1.027)	
Male	0.346	0.435	**0.328**	0.267	0.278	**0.835**
	(0.067)	(0.060)		(0.033)	(0.046)	
White	0.750	0.841	**0.219**	0.789	0.773	**0.763**
	(0.061)	(0.044)		(0.031)	(0.043)	
Baseline Credits	29.548	31.949	**0.497**	35.903	33.639	**0.425**
	(2.441)	(2.447)		(1.775)	(2.003)	
Baseline GPA	3.069	2.963	**0.468**	2.947	2.896	**0.576**
	(0.113)	(0.094)		(0.053)	(0.078)	
Survey measures						
Value of Summer Scholarship	152.206	143.284	**0.740**	281.301	273.537	**0.657**
	(20.733)	(17.311)		(10.144)	(14.564)	
Value of Fall Scholarship	245.750	235.606	**0.702**	268.032	246.447	**0.264**
	(20.043)	(17.297)		(11.130)	(16.327)	
Prefer Summer Course	0.192	0.203	**0.886**	0.592	0.557	**0.570**
	(0.055)	(0.049)		(0.037)	(0.051)	
Prefer Summer Cash	0.750	0.696	**0.514**	0.844	0.814	**0.537**
	(0.061)	(0.056)		(0.027)	(0.040)	
Plans to Enroll in Summer	0.500	0.540	**0.556**	0.818	0.851	**0.271**
	(0.049)	(0.045)		(0.020)	(0.024)	
Students	52	69		180	97	

Notes: Table reports means/proportions for each group with standard errors in parentheses. Scholarship values are calculated as the midpoint between the highest amount for which the student prefers the scholarship (over cash) and the lowest amount for which the student prefers the cash (over the scholarship). Students who always prefer cash are assigned a value of $25 for the course, and students who always prefer the course are assigned a value of $400 for the course. "Prefer Summer Course" is a binary measure of preference for summer courses over fall. "Prefer Summer Cash" is a binary measure of preference for cash payments in the summer over fall. Plans to enroll in summer are coded as 0 (No), 0.5 (Maybe) or 1 (Yes).

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Table 2: Summer Enrollment and Educational Outcomes

		ITT Estimate	IV Estimate	Corr. Estimate	Control Mean
Panel A: Summer Enrollment					
Summer	Enrollment	0.203			0.332
		(0.046)			
	Credits Attempted	0.586	2.885	5.210	1.750
		(0.247)	(0.762)	(0.230)	
	Credits Completed	0.489	2.408	4.460	1.517
		(0.237)	(0.849)	(0.264)	
Panel B: Educational Outcomes					
One-Year	Associate	0.073	0.362	0.176	0.228
		(0.038)	(0.196)	(0.065)	
	Transfer	0.076	0.373	0.013	0.129
		(0.038)	(0.198)	(0.050)	
	Combined	0.077	0.379	0.154	0.306
		(0.044)	(0.222)	(0.072)	
Two-Year	Associate	0.010	0.052	0.172	0.405
		(0.045)	(0.220)	(0.072)	
	Transfer	0.085	0.428	-0.007	0.147
		(0.040)	(0.212)	(0.051)	
	Combined	0.018	0.097	0.150	0.483
		(0.048)	(0.236)	(0.073)	
	Students	398	398	232	

Notes: The dependent variable is reported for each row. In Panel B, "Combined" is an indicator variable equal to 1 if the student has either graduated or transferred. Column 1 in Panel A estimates the intent to treat (ITT) using ordinary least squares regression. Columns 1 & 3 in Panel B report marginal effects from a linear probability model. Column 2 in both panels instruments for "Summer Enrollment" with the treatment assignment—using the estimates from the first row of the top panel as the first stage. Column 4 in both panels reports means from the control group. All regressions report heteroskedasticity-robust standard errors and include covariates for cohort, baseline GPA, baseline credit accumulation, age, race, gender, and stated plans for enrolling in the summer term, coded as 0 (No), 0.5 (Maybe), or 1 (Yes). Table A.2 replicates this analysis 1) without covariates and 2) clustering standard errors at the level of the randomization strata. Without covariates, we find consistent results, though with larger standard errors and lower statistical significance. Clustering at the level of the randomization strata, we find nearly identical results but with smaller standard errors and increased statistical significance.

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Table 3: Subsequent Enrollment, Credit Accumulation, & GPA

	Enrollment			Credits (Cumulative)			GPA		
	Fall	Spring	Summer	Fall	Spring	Summer	Fall	Spring	Summer
Panel A: Instrumental Variables									
Summer Enrollment	-0.014	0.053	-0.126	1.723	3.021	3.056	0.539	1.396	0.887
	(0.222)	(0.251)	(0.219)	(2.664)	(4.610)	(5.382)	(0.631)	(0.731)	(1.227)
Students	398	398	398	398	398	398	282	220	98
Panel B: Correlations									
Summer Enrollment	0.094	0.021	0.119	5.254	6.124	6.974	0.095	0.142	0.138
	(0.063)	(0.079)	(0.071)	(0.777)	(1.396)	(1.623)	(0.145)	(0.226)	(0.352)
Students:	232	232	232	232	232	232	165	128	60
Control group mean	0.737	0.565	0.267	7.491	12.131	13.450	2.801	2.700	2.836

Notes: Each column presents the coefficient from a separate regression. The dependent variables are: enrollment in each term (Columns 1–3), total cumulative credits at the end of each term (Columns 4–6), and GPA from courses taken during each term (Columns 7–9). Panel A presents results from instrumental variables regressions of the dependent variable on summer enrollment (using the first stage reported in Table 2 Panel A). Panel B presents correlational results from our control group obtained using a linear probability model (OLS) that regresses the dependent variable on an indicator variable for summer enrollment. Heteroskedasticity-robust standard errors. All regressions include covariates for cohort, baseline GPA, baseline credit accumulation, age, race, gender, and stated plans for enrolling in the summer term, coded as 0 (No), 0.5 (Maybe), or 1 (Yes).

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Table 4: Stated Barriers to Summer Enrollment

	DV: Prefer Summer to Fall						
Need to work	-0.197					-0.132	
	(0.066)					(0.075)	
No time for summer courses		-0.193				-0.090	
		(0.067)				(0.081)	
Can't afford summer courses			-0.022			-0.003	
			(0.084)			(0.079)	
Dislike summer courses				-0.233		-0.219	
				(0.042)		(0.050)	
Other					-0.081	-0.110	
					(0.086)	(0.086)	
Any barrier reported							-0.196
							(0.072)
Constant	0.242	0.240	0.203	0.233	0.216	0.304	0.320
	(0.043)	(0.043)	(0.041)	(0.042)	(0.041)	(0.056)	(0.064)
Students	397	397	397	397	397	397	397

Notes: Dependent variable is a binary variable equal to 1 if the student prefers summer scholarships to fall scholarships. Independent variable is a binary variable equal to 1 if the student reports this barrier to summer enrollment on the baseline survey. The elicitation of these barriers is shown in Figure B.6. All barriers with fewer than 10 positive responses were aggregated and categorized with "Other." "Any Barrier Reported" is a binary variable equal to 1 if the student reports any of the 9 possible barriers. 21% of students report any barriers. Of those, 35% report a need to work, 34% report not having time for summer courses, 33% report not being able to afford summer courses, 22% report disliking summer courses, 35% report other barriers. All estimates are obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. All regressions only include covariates for cohort.

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Table 5: Summer Enrollment & Graduation or Transfer by Summer Preferences or Enrollment Barriers

	Summer Enrollment		One-Year Graduation or Transfer	
Treatment	0.256	0.205	0.114	0.043
	(0.061)	(0.057)	(0.062)	(0.055)
Prefer Summer Course	0.316		0.173	
	(0.061)		(0.062)	
Treatment x Prefer Summer Course	-0.072		-0.110	
	(0.095)		(0.097)	
Any Barrier Reported		-0.441		0.010
		(0.066)		(0.087)
Treatment x Any Barrier Reported		-0.003		0.101
		(0.090)		(0.116)
Students	397	398	397	398

Notes: Dependent variable in Columns 1 & 2 is a binary variable equal to 1 if the student enrolls in summer courses. Dependent variable in Columns 3 & 4 is a binary variable equal to 1 if the student graduates or transfers within one year of the program. "Prefer Summer Course" is a binary variable equal to 1 if the student prefers summer scholarships to fall scholarships. "Any Barrier Reported" is a binary variable equal to 1 if the student reports any of the 9 possible barriers from Appendix Figure B.6. All estimates are obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. All regressions only include covariates for cohort. The sample sizes change by one student because of missing preference measures (see footnote 12 for details).

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Table 6: Summer Enrollment & Graduation or Transfer by Pell-Eligibility

	Summer Enrollment		One-Year Graduation or Transfer	
Treatment	0.026	0.100	-0.094	0.057
	(0.111)	(0.083)	(0.097)	(0.074)
Pell-Eligible	-0.116		-0.002	
	(0.072)		(0.071)	
Treatment x Pell-Eligible	0.225		0.222	
	(0.133)		(0.123)	
Need-Based Grants ($1000s)		-0.049		0.025
		(0.019)		(0.021)
Treatment x Need-Based Grants ($1000s)		0.053		-0.001
		(0.032)		(0.033)
Students	270	270	270	270

Notes: Dependent variable in Columns 1 & 2 is a binary variable equal to 1 if the student enrolls in summer courses. Dependent variable in Columns 3 & 4 is a binary variable equal to 1 if the student graduates or transfers within one year of the program. "Pell-Eligible" is a binary variable equal to 1 if the student qualifies for Pell-grants during the baseline term (Spring 2017). "Need-Based Grants" is a variable equal to the amount of need-based financial aid received by a student (in 1000s of USD) during the baseline term. All estimates are obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. All regressions include covariates for baseline GPA, baseline credit accumulation, age, race, gender, and stated plans for enrolling in the summer term, coded as 0 (No), 0.5 (Maybe), or 1 (Yes).

Table 7: Academic Characteristics by Summer Enrollment

	Baseline Credits				Baseline GPA			
	No Summer	Summer	Difference	**p-value**	No Summer	Summer	Difference	**p-value**
Observational	8.200	14.823	-6.623	**0.000**	1.662	2.750	-1.088	**0.000**
Sample	(0.275)	(0.408)	(0.497)		(0.047)	(0.049)	(0.077)	
	[n=960]	[n=412]	[n=1372]		[n=960]	[n=412]	[n=1372]	
Experiment	32.252	38.961	-5.806	**0.071**	2.893	3.139	-0.280	**0.007**
(Control)	(1.599)	(3.084)	(3.201)		(0.064)	(0.062)	(0.103)	
	[n=155]	[n=77]	[n=232]		[n=155]	[n=77]	[n=232]	
Experiment	32.150	33.669	-1.343	**0.669**	2.856	2.987	-0.140	**0.246**
(Treatment)	(2.220)	(2.167)	(3.132)		(0.091)	(0.078)	(0.120)	
	[n=80]	[n=86]	[n=166]		[n=80]	[n=86]	[n=166]	

Notes: Table reports means/proportions for each group with standard errors in parentheses. P-values in the observational sample are calculated based on a t-test of differences. P-values in the experimental samples are calculated based on a t-test of differences after controlling for the student's cohort. "Baseline" academic variables in the observational sample are calculated based on the student's last term of enrollment preceding the Summer 2017 term. "Baseline" academic variables in the experimental sample are calculated based on the term of enrollment in the study.

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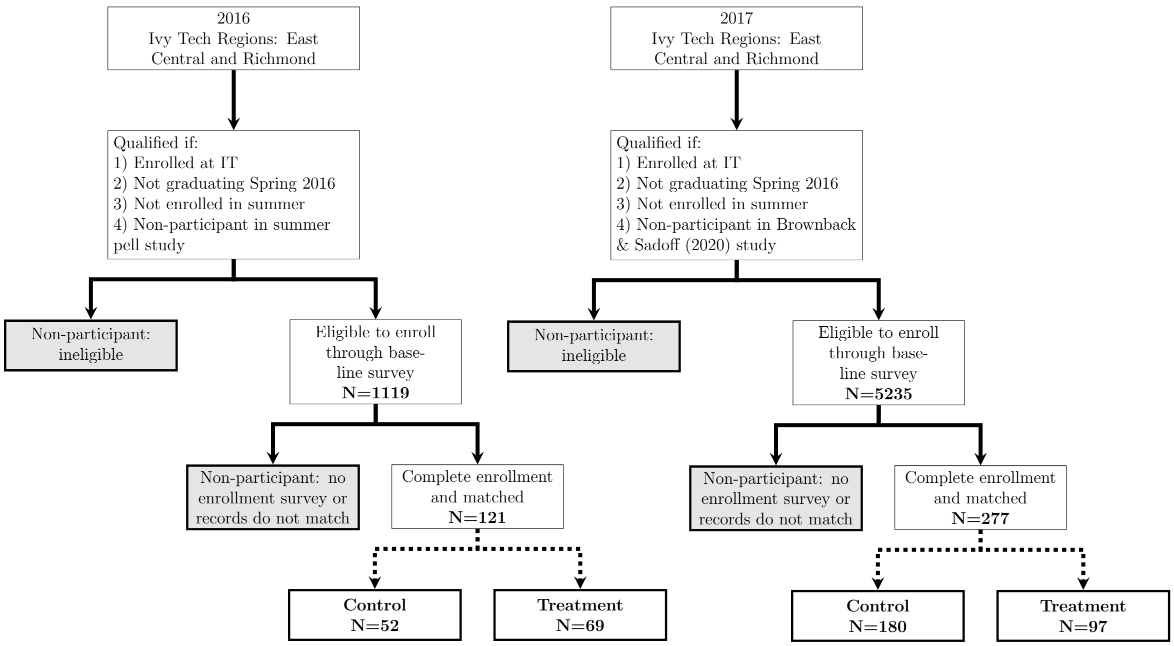
Table 8: Summer Course Selection and Performance

	Prior Performance		Current Performance			
	Previously Failed		Grade Points		Completion Rate	
Summer Course	0.085	0.080	-0.122	-0.318	-0.013	-0.059
	(0.012)	(0.042)	(0.040)	(0.129)	(0.014)	(0.033)
Treatment × Summer Course		0.007		0.268		0.024
		(0.057)		(0.180)		(0.053)
Constant	0.186	0.173	1.597	2.148	0.515	0.676
	(0.024)	(0.046)	(0.116)	(0.163)	(0.037)	(0.059)
Sample:	Obs.	Expt.	Obs.	Expt.	Obs.	Expt.
Course Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Student Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Student-Course Obs.	17599	5704	12924	5348	14097	5631
Students	1366	398	1329	397	1365	398

Notes: All regressions include student-level and course-level fixed effects, so all variation is identified within-student and within-course. Dependent variables in Columns 1 & 2 are binary variables equal to 1 if the student has previously failed the course in question. Dependent variables in Columns 3 & 4 are students' grade points from the selected course. Dependent variables in Columns 5 & 6 are binary variables equal to 1 if the student completes the selected course. For the observational data (Columns 1, 3, 5), "Summer Course" is a binary variable equal to 1 if the selected course is during a summer term. For the experimental data (Columns 2, 4, 6), "Summer Course" is a binary variable equal to 1 if the selected course is during the summer term in which the student's cohort received scholarships. All standard errors are clustered at the student level and reported in parentheses. Observations are limited in Columns 3 & 4 because of assigned grades that do not fit on a grade-point scale (e.g. "Withdrawal," "Satisfactory," or "Incomplete"). Observations are limited in Columns 5 & 6 because of assigned grades that do not fit on a completion scale (e.g. "Satisfactory" or "Incomplete").

A Appendix: Figures and Tables

Figure A.1: Eligibility and Randomization for Each Recruitment Wave



Notes: Dashed arrows indicate random assignment.

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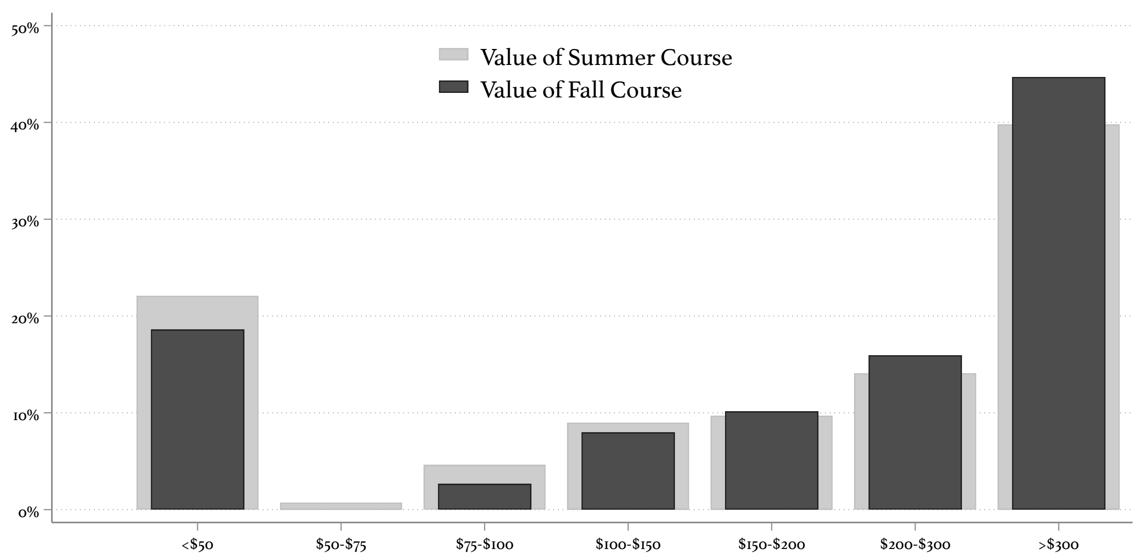
Figure A.2: Baseline value for free summer course and free fall course



Notes: Based on tuition rates at the time, the tuition voucher had a face value of just over $400. Values are given in terms of the interval between the highest amount for which the student prefers the scholarship (over cash) and the lowest amount for which the student prefers the cash (over the scholarship).

Table A.1: Demographics for Participants, Eligible Students, and Statewide Ivy Tech Population

	Summer 2016				Summer 2017			
	Study Sample	Eligible Students	Ivy Tech Population	All 2-Year Public	Study Sample	Eligible Students	Ivy Tech Population	All 2-Year Public
Demographics								
Male	0.397	0.504	0.429	0.439	0.271	0.397	0.432	0.439
	(0.045)	(0.015)			(0.027)	(0.007)		
White	0.802	0.815	0.753	0.574	0.783	0.847	0.761	0.558
	(0.036)	(0.01)			(0.025)	(0.005)		
Pell-Eligibility	0	0	0.45	0.346	0.648	N/A	0.40	0.347
	(—)	(—)			(0.478)			
Baseline Academic Progress								
Baseline Credits	30.917	26.250	N/A	N/A	35.110	24.922	N/A	N/A
	(1.742)	(0.577)			(1.350)	(0.338)		
Baseline GPA	3.008	2.691	N/A	N/A	2.929	2.340	N/A	N/A
	(0.072)	(0.030)			(0.044)	(0.016)		
Students	121	1,119	78,910	6,283,390	277	5,235	75,486	5,902,040

Notes: Table reports means/proportions and standard errors for each group. Statistics for the statewide Ivy Tech population and 2-year public institutions nationwide were retrieved from Institute of Education Sciences (2017). Average academic progress at the time of the recruitment is not available through Institute of Education Sciences (2017) (neither Ivy Tech statewide nor 2-year public institutions nationwide). "N/A" indicates that data were not available for the characteristic in question for the indicated population.

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Table A.2: Impact of Summer on Future Enrollment, Credit Accumulation, & GPA

	Summer		One Year Post-Program					
	Enrollment		Associate		Transfer		Combined	
Treatment	0.219	0.203	0.058	0.073	0.068	0.076	0.062	0.077
	(0.049)	(0.050)	(0.045)	(0.038)	(0.039)	(0.032)	(0.049)	(0.039)
Constant	0.197	-0.493	0.165	-0.373	0.110	0.212	0.254	-0.119
	(0.046)	(0.107)	(0.043)	(0.102)	(0.038)	(0.085)	(0.048)	(0.118)
Clustered SEs	Student	Strata	Student	Strata	Student	Strata	Student	Strata
Covariates?	N	Y	N	Y	N	Y	N	Y
Students	398	398	398	398	398	398	398	398

Notes: Results from a linear probability model (OLS) that regresses the dependent variable on an indicator variable for assignment to the treatment group. The dependent variables are: enrollment in the Summer term (Columns 1–2), graduation with an associate degree (Columns 3–4), transfer to a 4-year college (Columns 5–6), and the combination of either graduation with an associate degree or transfer to a 4-year college (Columns 7–8). Standard errors clustered at the level specified in the table. Odd columns only include covariates for cohort, while even columns include covariates for cohort, baseline GPA, baseline credit accumulation, age, race, gender, and stated plans for enrolling in the summer term, coded as 0 (No), 0.5 (Maybe), or 1 (Yes).

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Table A.3: Treatment Effects on Transfer Outcomes and Bachelor's Degree Attainment

	Bachelor's Degree	Any Degree	Enrollment Length
Treatment	0.017	0.020	24.459
	(0.019)	(0.020)	(24.459)
Constant	0.026	0.027	98.336
	(0.050)	(0.050)	(59.048)
Students	398	398	398
Control Group mean	0.025	0.029	110.795

Notes: All estimates obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. Dependent variables are based on transfer outcomes from the National Student Clearinghouse records as of Fall 2021 for the 2016 cohort and Fall 2022 for the 2017 cohort. "Bachelor's Degree" is a binary dependent variable equal to one if the student has obtained a bachelor's degree from the transfer institution. "Any Degree" is a binary dependent variable equal to one if the student has obtained a bachelor's degree, associate degree, diploma, or certificate from the transfer institution. "Enrollment Length" is equal to the number of total days the student is enrolled at the transfer institution (coded as 0 for students that do not transfer). All regressions include covariates for cohort, GPA and credit accumulation at baseline, age, race, gender, and stated plans for the summer semester.

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Table A.4: Heterogeneity: Preferences for Summer Enrollment

	Likelihood of Preferring Summer to Fall										
Survey Measures											
Prefer Summer Cash	0.166***										0.105*
	(0.055)										(0.060)
Summer Plans		0.433***									0.375***
		(0.072)									(0.085)
Semesters until Planned Graduation			-0.038***								-0.018
			(0.011)								(0.012)
Academics											
Completed Semesters at Ivy Tech				0.002							-0.002
				(0.003)							(0.004)
Baseline GPA					0.018						0.016
					(0.031)						(0.033)
Baseline Credits						0.002**					0.001
						(0.001)					(0.001)
Credits Completed in Prior Summer							0.025**				0.008
							(0.010)				(0.011)
Demographics											
Age								0.000			0.002
								(0.002)			(0.002)
Male									0.168***		0.127**
									(0.050)		(0.052)
White										0.008	0.044
										(0.060)	(0.060)
Constant	0.079	-0.028	0.379***	0.187***	0.144	0.131***	0.181***	0.187**	0.132***	0.192***	-0.194
	(0.049)	(0.044)	(0.072)	(0.044)	(0.101)	(0.048)	(0.037)	(0.073)	(0.037)	(0.062)	(0.159)
Students	397	397	347	397	397	397	397	397	397	397	347

Notes: Dependent variable is a binary variable equal to 1 if the student prefers summer vouchers to fall vouchers. "Prefer Summer Cash" is a binary variable equal to 1 if the student prefers cash payments in the summer over fall. "Summer Plans:" is measured from $0-1$ based on the student's stated plans to enroll in summer courses (1 means the student plans to enroll, 0 means the student does not). All estimates obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. Columns 1–9 only include controls for cohort. Column 10 includes all covariates listed: cohort; GPA, credit accumulation, and completed semesters at baseline; age; race; gender; stated plans for the summer semester and for graduation; and preferences for payment in the summer. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table A.5: Heterogeneity: Summer Enrollment

	Summer Enrollment Rate								
Treatment	0.251**	0.237***	0.051	0.225***	0.370**	0.280***	0.270*	0.231***	0.114
	(0.100)	(0.080)	(0.117)	(0.074)	(0.185)	(0.092)	(0.142)	(0.059)	(0.107)
Prefer Summer Cash	0.150**								
	(0.070)								
Treatment x Prefer Summer Cash	-0.035								
	(0.115)								
Summer Plans		0.637***							
		(0.078)							
Treatment x Summer Plans		-0.055							
		(0.112)							
Semesters until Planned Graduation			-0.042***						
			(0.015)						
Treatment x Semesters until Planned Grad.			0.037						
			(0.023)						
Completed Semesters at Ivy Tech				0.000					
				(0.004)					
Treatment x Completed Semesters at Ivy Tech				-0.001					
				(0.007)					
Baseline GPA					0.110***				
					(0.033)				
Treatment x Baseline GPA					-0.049				
					(0.061)				
Baseline Credits						0.003*			
						(0.001)			
Treatment x Baseline Credits						-0.002			
						(0.002)			
Age							-0.001		
							(0.003)		
Treatment x Age							-0.002		
							(0.005)		
Male								0.056	
								(0.069)	
Treatment x Male								-0.005	
								(0.101)	
White									0.041
									(0.072)
Treatment x White									0.132
									(0.120)
Students	397	398	347	398	398	398	398	398	398

Notes: Dependent variable is a binary variable equal to 1 if the student enrolls in summer courses. "Prefer Summer Cash" is a binary variable equal to 1 if the student prefers cash payments in the summer over fall. "Summer Plans:" is measured from $0 - 1$ based on the student's stated plans to enroll in summer courses (1 means the student plans to enroll, 0 means the student does not). All estimates obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. All regressions include controls for cohort. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table A.6: Heterogeneity: Degree Acceleration

	Graduation or Transfer One Year Post-Program					
Treatment	0.002	0.157	-0.064	0.032	0.052	0.022
	(0.070)	(0.150)	(0.071)	(0.137)	(0.077)	(0.091)
Completed Semesters at Ivy Tech	-0.002					
	(0.004)					
Treatment x Completed Terms at IT	0.008					
	(0.007)					
Baseline GPA		0.120***				
		(0.032)				
Treatment x Baseline GPA		-0.030				
		(0.052)				
Baseline Credits			0.006***			
			(0.001)			
Treatment x Baseline Credits			0.004*			
			(0.002)			
Age				-0.004		
				(0.003)		
Treatment x Age				0.001		
				(0.004)		
Male					0.002	
					(0.066)	
Treatment x Male					0.016	
					(0.099)	
White						0.138**
						(0.066)
Treatment x White						0.048
						(0.106)
Students	398	398	398	398	398	398

Notes: Dependent variable is a binary variable equal to 1 if the student graduates or transfers within one year of the program. “Prefer Summer Cash” is a binary variable equal to 1 if the student prefers cash payments in the summer over fall. “Summer Plans:” is measured from $0 - 1$ based on the student’s stated plans to enroll in summer courses (1 means the student plans to enroll, 0 means the student does not). All estimates obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. All columns include controls for cohort. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table A.7: Selection on Levels and Selection on Treatment Effects

	Combined	Associate	Transfer
Treatment	0.095	0.104	0.039
	(0.055)	(0.046)	(0.047)
Summer Enrollment	0.174	0.177	0.023
	(0.067)	(0.060)	(0.050)
Treatment × Summer Enrollment	-0.101	-0.127	0.061
	(0.090)	(0.079)	(0.078)
Students	398	398	398

Notes: Dependent variable is a binary variable equal to 1 if the student graduates or transfers (Column 1), graduates (Column 2), transfers (Column 3) within one year of the intervention. "Summer Enrollment" is a binary variable for enrollment in the summer term. All estimates obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. All regressions include covariates for cohort, GPA and credit accumulation at baseline, age, race, gender, and stated plans for the summer semester.

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Table A.8: Demographics that Predict Take-Up of Summer Scholarships

	Prefer Summer Course	Prefer Summer Cash	Summer Plans	Semesters until Planned Grad.	Completed Semesters	Baseline GPA	Baseline Credits	Age	Male	White
Never-Takers	-0.357***	-0.150**	-0.253***	0.537	0.047	-0.333***	-5.604	1.750	-0.008	-0.064
	(0.073)	(0.064)	(0.042)	(0.381)	(1.140)	(0.116)	(3.792)	(1.942)	(0.077)	(0.070)
Never-Takers + Compliers	-0.317***	-0.094*	-0.243***	0.825***	-0.221	-0.270***	-6.124*	0.440	-0.063	-0.042
	(0.063)	(0.048)	(0.033)	(0.307)	(0.967)	(0.090)	(3.455)	(1.493)	(0.064)	(0.058)
Always-Takers + Compliers	-0.134*	-0.059	-0.020	0.402	-0.312	-0.187*	-4.458	0.356	-0.007	0.037
	(0.072)	(0.054)	(0.034)	(0.384)	(1.050)	(0.101)	(3.744)	(1.622)	(0.074)	(0.061)
Constant: Always Takers	0.460***	0.816***	0.696***	4.614***	7.485***	3.259***	36.046***	28.133***	0.424***	0.828***
	(0.066)	(0.054)	(0.038)	(0.337)	(0.993)	(0.102)	(3.440)	(1.788)	(0.072)	(0.062)
Students	397	397	398	347	398	398	398	398	398	398

Notes: Dependent variable is a binary variable equal to 1 if the student enrolls in summer courses. "Prefer Summer Course" is a binary variable equal to 1 if the student prefers summer vouchers to fall vouchers. "Prefer Summer Cash" is a binary variable equal to 1 if the student prefers cash payments in the summer over fall. "Summer Plans:" is measured from $0-1$ based on the student's stated plans to enroll in summer courses (1 means the student plans to enroll, 0 means the student does not). All estimates obtained using OLS regressions with heteroskedasticity-robust standard errors in parentheses. Columns 1–9 only include controls for cohort. Column 10 includes all covariates listed: cohort; GPA, credit accumulation, and completed semesters at baseline; age; race; gender; stated plans for the summer semester and for graduation; and preferences for payment in the summer. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Appendix: Materials

Figure B.1: 2017 Recruitment Email Text

Dear [NAME],

The East Central and Richmond Regions of Ivy Tech have just been awarded funds as part of a research study to help additional students attend summer classes. We will be distributing vouchers to cover the cost of tuition for one (1) three-credit hour course for Summer 2017 at Ivy Tech (over a $400 value). The voucher will not cover books or fees.

We have a limited number of vouchers, so we ask that interested students enroll in the program by May 3, 2017. After May 5, 2017 we will draw names randomly to assign the free tuition vouchers. You can enroll at the following link: http://tinyurl.com/IvyTechSummer17

These vouchers are intended for students who plan to continue through Fall 2017 or will graduate with a credential at the end of Summer 2017.

Please contact your campus Bursar Office for any questions:

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Figure B.2: Incentive-Compatible Elicitation of Scholarship Preferences

For each of the following, which do you prefer?

A Free Summer Course	A Full-Priced Fall Course
A Free Summer Course	$100 discount on a Fall Course
A Free Summer Course	$200 discount on a Fall Course
A Free Summer Course	$300 discount on a Fall Course
A Free Summer Course	A Free Fall Course
$300 discount on a Summer Course	A Free Fall Course
$200 discount on a Summer Course	A Free Fall Course
$100 discount on a summer course	A Free Fall Course
A Full-Priced Summer Course	A Free Fall Course

Figure B.3: Incentive-Compatible Elicitation of Summer Scholarship Value

For each of the following, which do you prefer?

A Free Summer Course	$300 gift card on June 5
A Free Summer Course	$200 gift card on June 5
A Free Summer Course	$150 gift card on June 5
A Free Summer Course	$100 gift card on June 5
A Free Summer Course	$75 gift card on June 5
A Free Summer Course	$50 gift card on June 5

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Figure B.4: Incentive-Compatible Elicitation of Fall Scholarship Value

For each of the following, which do you prefer?

A Free Fall Course	○	○	$300 gift card on August 21
A Free Fall Course	○	○	$200 gift card on August 21
A Free Fall Course	○	○	$150 gift card on August 21
A Free Fall Course	○	○	$100 gift card on August 21
A Free Fall Course	○	○	$75 gift card on August 21
A Free Fall Course	○	○	$50 gift card on August 21

Figure B.5: Incentive-Compatible Elicitation of Preferences for Cash

For each of the following, which do you prefer?

$50 gift card on June 5	○	○	$100 gift card on August 21
$50 gift card on June 5	○	○	$75 gift card on August 21
$50 gift card on June 5	○	○	$50 gift card on August 21
$75 gift card on June 5	○	○	$50 gift card on August 21
$100 gift card on June 5	○	○	$50 gift card on August 21

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Figure B.6: Elicitation of Barriers to Summer Enrollment

If you don't enroll in summer courses at Ivy Tech, what would be the reason(s)? Please check any responses that apply or provide your own:

- I already received my degree from Ivy Tech
- I'm transferring to another school
- I don't want to take any more courses at Ivy Tech
- I don't like to take courses in the summer
- I can't afford to take summer courses
- I don't have time to take summer course
- I have to work
- I have to take care of children who are out of school for the summer
- Other