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A summer bridge program for first-generation low-income students stretches academic ambitions with no adverse impacts on first-year GPA

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Affiliations are included on p. 8.

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A large body of research documents the barriers faced by first-generation, low-income (FGLI) students as "hidden minorities" on elite college campuses. Although existing studies show brief psychological interventions can help mitigate some of these obstacles, universities are investing in more intensive interventions that try to both shift mindsets and mitigate structural disadvantages in FGLI students' academic preparation. In collaboration with the administrators at a highly selective university, we conducted a randomized controlled trial of a summer bridge program targeted at FGLI students. During summers between 2017 and 2019, we randomly selected 232 out of 418 first-generation or low-income students and invited them to attend an intensive, six-week-long residential summer program featuring courses for academic credit. Students randomized to the control group either interacted with online content offering no academic credit or had no summer intervention. Our preregistered analysis shows that the program encouraged FGLI students to pursue a more ambitious first-year program, increasing the proportion of nonintroductory courses by 7 percentage points. The program also increased the proportion of courses taken for a grade rather than as pass-fail by 6 percentage points. These improvements were accompanied by no discernible impact on first-year grade point averages (GPAs) and academic withdrawal. The findings show the potential to academically integrate FGLI students into selective university communities.

inequality | field experiment | higher education | diversity

Significance

As highly selective universities expand access for first-generation and low-income (FGLI) students, how can they promote student success? We conduct a randomized controlled trial in partnership with a university to evaluate the impact of an intensive summer bridge program targeted at FGLI students. We show that the program encouraged FGLI students to pursue a more ambitious first-year program, increasing the proportion of nonintroductory courses by 7 percentage points. The program also increased the proportion of courses taken for a grade rather than as pass-fail by 6 percentage points. These improvements were accompanied by no discernible impact on first-year grade point averages (GPAs) and academic withdrawal. The findings show the ability to academically integrate FGLI students into selective university communities.

First-generation, low income (FGLI) have historically been severely underrepresented at highly selective U.S. universities. At "Ivy-Plus" institutions,* students from families in the top 1% of the US income distribution are heavily overrepresented, while fewer than 4% of students come from families earning less than $25,000 a year, the bottom quintile of the income distribution (1). There exists a large literature on *undermatching*, which refers to the phenomenon where students with good academic credentials (e.g., high test scores and high school GPAs) either attend less selective institutions than they are qualified for or do not pursue postsecondary education at all. These studies show that low-income students are more likely to be "undermatched" and that this undermatching is correlated with lower rates of college completion (2–7).† Interventions to remedy undermatching have shown that "information alone" may not be sufficient to remedy the issue (8); instead, targeted recruiting and generous scholarships can increase representation of FGLI students (9, 10).

Using targeted recruitment strategies and generous scholarships, highly selective universities have succeeded at increasing the representation of high-performing, low-income students. For instance, 19.4% of Harvard's class of 2026 is composed of first-generation students compared to 16.3% in their class of 2021 (11, 12). Over the same time period, Yale increased their proportion from 16.6% to 18% (13, 14). Looking over a longer time horizon, the increase in access is more pronounced—17% in Princeton's class of 2026 are first generation compared to only 6% in the class of 2005 (15). Since these universities can offer outsize benefits (1), they have an important role to play in increasing social mobility. Framed in terms of the undermatching literature, highly selective universities have, through a combination of proactive outreach and generous

*These include the eight Ivy League colleges and Duke, MIT, Stanford, and University of Chicago.

†Authors hypothesize about the mechanism through which undermatching can affect completion rates. They argue that the undermatched students, who attend less selective institutions, can have less institutional scaffolding and support for them to graduate within six years.

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financial aid, been able to help FGLI students access universities commensurate with their outstanding academic credentials.

Yet FGLI students' *formal access* to elite universities, the focus of the undermatching literature, is only one part of the challenge in promoting social mobility in higher education. Another difficulty is ensuring that, once granted access, FGLI students have the same chances at high academic performance and postgraduation opportunities as their continuing-generation, higher socioeconomic status peers. Existing research shows how, amid this increased access, elite universities still struggle to support enrolled FGLI students (16–21). Specifically, FGLI students experience lower feelings of belonging in social networks where wealth presents hidden barriers to inclusion and can face difficulty seeking help from professors and other campus resources, hindering academic achievement and career milestones (16, 18, 22–25). In addition to the on-campus barriers, research shows how past and ongoing experiences of off-campus family and neighborhood trauma spill over onto students' on-campus experiences (19).

Despite the importance of ensuring that highly selective universities provide FGLI students with appropriate support, existing research nearly exclusively focuses on one tool of support: brief psychological interventions (*SI Appendix*, Tables S1 and S2 in *SI Appendix*, section B summarize past research). Prior studies have evaluated the impact of brief (less than 1 h) psychological interventions and online programs rather than more intensive efforts (16, 21, 26–28).

As a more intensive type of assistance, universities have invested in summer bridge programs (29). These programs, present at most of the Ivy-Plus institutions (see a comprehensive list in *SI Appendix*, section A), bring FGLI students who have matriculated at the university to campus the summer before their freshman year to help them acclimate to campus and prepare academically. Despite the proliferation of these intensive investments, we are only aware of one evaluation study (30). This study does not focus on the highly selective universities that have struggled to improve the on-campus experiences of FGLI students; instead, it analyzes a remedial academic program offered at community colleges and nonselective universities. While these universities educate a large number of FGLI students, FGLI students at highly selective universities may face greater hurdles in academic preparation and more acute issues with feelings of belongingness.

We partnered with a highly selective university (hereafter we use a pseudonym, *SB University*) and conducted a preregistered randomized controlled trial (RCT) to evaluate the impact of their summer bridge program (*SB Program*) on student outcomes in their first year of university (see https://osf.io/qh75m for a preanalysis plan). First, we show that the students eligible for *SB Program* face hardship far beyond their first-generation status, including high rates of food and housing insecurity, family death, and other trauma. Second, we show that *SB Program* significantly improves outcomes despite this high depth of need. The program increases two observable measures of academic ambition during the students' first year of college—enrollment in nonintroductory courses and taking courses for a grade rather than pass-fail—without negatively impacting student GPAs or rates of academic withdrawal.

In sum, our study makes three contributions. First, we expand research on supports for FGLI students beyond brief psychological interventions (16, 21, 26–28), often delivered online, to include intensive in-person experiences aimed at reducing gaps in academic preparation. Second, we focus on an intensive program at a highly selective university, a setting where past research has documented that FGLI students face significant on-campus challenges. Third, our field experiment allows us to identify effects for the target population of all FGLI students flagged by administrators as potentially benefitting from the program, in contrast to some past studies that have opt-in participation and that thus estimate effects on a select subset of students (16, 26).

Overview of Experimental Design and Analysis

Next, we provide a brief overview of our experimental design and analysis while leaving the details to *Materials and Methods*. *SB University* is a highly selective U.S. university that accepts fewer than 10 percent of applicants, has median SAT scores in the 1,500 range on the 1,600 point scale, and over 30% of the incoming class attended a private high school. The university, like many of its peer institutions, has tried to increase the racial and socioeconomic diversity of the student population through targeted recruitment of high-achieving, low-income students, need-blind admissions, and generous financial aid.

Alongside its attempts to increase enrollment of FGLI students are efforts to enable the success of FGLI students who matriculate at the university. Many of these students attended underresourced high schools that risk leaving them underprepared for two important aspects of campus life at *SB University*. First is the rigorous formal academic curriculum. Second is the "hidden curriculum," a set of generally unwritten rules and norms for navigating academic achievement, including the use of office hours and student groups, that often represents an additional barrier to FGLI student success (19).

We randomized three cohorts of students ($N = 418$ total, with 232 students randomized to treatment and 186 students randomized to control during the summers of 2017–2019). These students were selected by administrators from a pool of students who were marked as potentially eligible for *SB Program* by the university admissions office. Administrators assessed need through both quantitative data in the students' application (SAT/ACT scores; high school curriculum) and through hardship flags coded through their application essays (e.g., food and housing insecurity). All students were those flagged by administrators as potentially benefitting from a program targeted at FGLI students, though some students had one status but not the other (e.g., were low income but had one parent attend some college).

Within this analytic sample, administrators grouped students into tiers based on their relative need for the program. We then randomized the treatment assignment within each tier by giving a higher probability of selection to the students whom administrators deemed as having greater need. Students in the treatment group received an invitation to attend a residential form of *SB Program* that offered academic credit and intensive interaction. As discussed in *Materials and Methods* and *SI Appendix*, section C1, this stratified randomization also allowed us to reduce the resulting variance of causal effect estimates in our subsequent analysis.

SB Program is a multiweek residential program where students enroll in credit-bearing, graded courses. Students take a small, seminar-style humanities course as well as a quantitative Science, Technology, Engineering, and Mathematics (STEM) laboratory course. In addition to academic coursework, the program provides training in *SB University*'s hidden curriculum, offering guidance on what to expect socially and how to seek academic help from on-campus tutoring and other resources. Throughout the program, administrators schedule extracurricular community-building activities and provide opportunities for students to meet faculty members across a range of fields. Students

are also invited to various programs after the summer that focus on academics, mentorship, professional development, and social activities. Administrators point to the credit-bearing nature of the courses and in-person, intensive support networks as particularly important ingredients to the success of students in the program. While there is less research studying preparatory credits, other research shows that in-person support networks are an important aspect of FGLI students' feelings of belongingness (31).

The program is free for all participating students and provides them with meals, housing, coverage of travel costs, and a stipend. More concretely, the stipend covers miscellaneous expenses beyond food and lodging and is calibrated to *SB University*'s financial aid package, giving students approximately $75 per week or approximately $600 over the course of the summer. While the stipend is less than students would earn in part- or full-time summer employment, from conversations with university administrators, the main reason that students decline their invitations to the in-person version of *SB Program* is not related to competing employment opportunities. Instead, the majority of students who decline their offers either cite family responsibilities like caregiving for younger siblings or are already participating in similar opportunities or enrichment programs. While some students are initially drawn to paid employment, discussions with administrators often highlight the longer-term value of the *SB University* degree, and preparation for that degree. Many students thus forgo paid summer employment for the free enrichment opportunity.

Table 1 describes the contrast between the treatment arm—an invitation to the in-person SB program—and the control group who do not receive an initial invitation. Students who did not receive an invitation to the residential program largely either engaged in no preparatory programming over the summer or viewed online programming that lacked course credit and in-person interactions with peers and staff, core features of the residential programming.

A small percentage of students randomized to the control group were removed from the waitlist and attended the in-person version. *SI Appendix*, section F shows the summer activities of students randomized to each group. Importantly, our intent-to-treat (ITT) estimates are based on each student's initial invitation status, even if that student later made it off the waitlist.

Table 1. Features of the randomized treatment intervention (attendance at in-person summer bridge program) compared to control

Feature	Treatment: Invited to in-person SB Program	Control: not invited to in-person SB Program
For-credit coursework	All take scholarly reading/writing + All take choice of quantitative/STEM course	None
Other coursework	All for-credit	Can view non-credit-bearing online content
Interactions with FGLI peers	In-person through residential experience	Can interact online with small groups
Introduction to university faculty and staff	In-person	Can meet online

Given that not every invited student attended the residential program, we examine two causal estimands:

1. ITT effect: this measures the impact of being *invited* to the in-person version of SB Program, regardless of whether the student accepted the invitation and attended. This estimand is relevant because despite the fact that the program is free and provides a stipend for attendance, as we show later, a nontrivial portion of invited students choose not to attend *SB Program* for the reasons we discussed previously. Conversely, because administrators wanted to ensure that no spots in the program went empty, some students randomized to the control group (no initial invitation) were later moved off the waitlist and invited. The ITT effect, therefore, measures the impact of the invitation to the *SB Program* rather than the program itself.
2. Complier average causal effect (CACE): We also examine the impact of the program among "compliers" or those students who would attend the *SB Program* only when invited. Note that there are two types of students who are not included in this group of compliers: those who would never attend the program even when invited and those who would attend the program regardless of whether they receive an invitation. We discuss details of the estimation later in this section, but broadly use two-stage least squares for estimation.

We examine two types of preregistered outcomes over the student's first year. First are measures of *academic difficulty*: 1) the registered levels of enrolled courses, and 2) whether courses are taken for a grade or pass-fail. Second are *academic outcomes*, with a focus on first-year GPAs, credit deficiencies, and leaves of absence for academic reasons. *SI Appendix*, section D discusses secondary outcomes, which include grades in specific subject areas such as STEM and writing coursework. Academic outcomes occurred both during normal academic programming (summer 2017 and 2018 cohorts; SY 2017–2018 and SY 2018–2019, respectively) and with academic programming impacted by the COVID-19 shift to online learning (summer 2019 cohort). For comparability with past studies, we focus on first-year outcomes and pool the three cohorts to gain statistical power.

Once these three cohorts reach graduation, we plan to conduct a separate long-term follow-up analysis with four-year graduation rates and end-of-college GPAs. This long-term follow-up is important because even at selective colleges, first-generation students have significantly lower rates of on-time graduation (32). The null impacts on GPA and rates of withdrawal we show later may change as students progress through their college career.

For the ITT effects, our main specification is a regression model that includes the binary treatment assignment variable as well as each student's randomization block indicator variable. This block variable represents a nonoverlapping subgroup of students defined by the student's summer cohort (2017, 2018, or 2019), their priority tier within the cohort (high priority, medium priority, and low priority, with one summer having a fourth category for highest priority), and, within a tier, any further strata to group similar students (see *SI Appendix*, section C.1 for more details on the randomization approach).[‡] In sum, the `block` variable adjusts for the student's summer of invitation, their priority tier within that summer, and their block within a

[‡]Within each tier, we created either small blocks or matched pairs for students with similar SAT scores and similar linear predictors from an ordered logit that regressed the "tier of priority" the administrators placed the student in on observed, pretreatment attributes. In summer 2018, blocking did not substantially improve balance so the tiers were the only stratifying factor.

priority tier where applicable (see *SI Appendix*, section E.1 for technical details).

We also examine the robustness of our empirical results to three other preregistered specifications discussed in *SI Appendix*, section E.2, including ones that further adjust for additional, pretreatment covariates. Finally, *SI Appendix*, section E.3 discusses our approach to estimating the CACE using a two-stage least squares approach to adjust for the fact that not every invited student attended the *SB Program* while some others in the control group attended either the online or in-person program.

Results

In this section, we discuss our main results. We begin by describing the characteristics of our analytic sample, and then present our empirical findings.

Students Eligible for SB Program Have a High Degree of Need and Challenges. Table 2 describes our analytic sample (see also *SI Appendix*, Table S5 which shows the number of students by cohorts). The table highlights the degree of both material disadvantage and trauma that students in our analytic sample have experienced before they enter university.[§] The majority are "doubly disadvantaged" (19) in that they attended an underresourced high school and have not yet benefited from participation in a college preparatory, pipeline program (i.e., QuestBridge; Prep for Prep; and other initiatives whose goal is to increase diversity at highly selective universities). While a third have parents who attended some college, students in that category had other forms of need; for instance, many have experienced trauma from family deaths and food and housing insecurity. Overall, the high degree of need shows the importance of the intensive summer program. *SI Appendix*, Table S6, which compares these attributes between treatment and control group students, shows that the randomization balanced these attributes.

SB Program Makes Students Pursue a More Challenging First-Year Program. We first examine whether *SB Program* helps students embark on a more challenging first-year academic program. This is an important goal of *SB Program* as it seeks to provide students with the confidence to tackle challenging coursework and the academic tools to compensate for underresourced high school curricula. Table 3 shows the two estimates of the program's impact: the estimated ITT effect of invitation to attend the in-person version of *SB Program* and the estimated CACE of attending the program (see *SI Appendix*, section E for details of estimation).

We find that the program significantly improves three measures of academic difficulty. First is the percentage of a student's units that are more advanced, nonintroductory courses.[¶] Second is the student's total units, though this effect is no longer statistically significant when we remove units earned during *SB Program*. Third is the student's proportion of courses taken for a grade.

These improvements are not only statistically significant but also substantively meaningful. On average, attendance at the in-person version of *SB Program* causes a 7 percentage point increase in the student's proportion of nonintroductory classes (an 18% increase over the baseline rate), more than a one unit increase in credits taken (a 14% increase over the baseline rate), and a 6 percentage point increase in the proportion of courses students take for a grade rather than pass-fail (a 7% increase over the baseline rate).

Table 3 also shows the weighted control group mean in each group. For instance, the control group typically takes fewer than 9 units in their first academic year, which is above university requirements for remaining on track but is not an especially heavy course load at a semester-system school. The participation in *SB Program* increases this by approximately 1.2 units. *SI Appendix*, section G.1 shows the full regression results for this main specification as well as the robustness of the results to the three other preregistered specifications.

In sum, we show that the program improves three measures of the difficulty of the student's first-year program: courses taken for a grade, the number of nonintroductory courses, and total units attained, though the lattermost impact operates through units attained during the summer program. These outcomes may be interrelated. For instance, the additional course credits earned during *SB Program* might help students take a higher proportion of courses for a grade rather than pass-fail since they can afford to drop a course. Other students may need to remain in a course but switch it to pass-fail since they need the credit hours; the credit buffer from *SB Program* might enable the more ambitious course taking since the students have enough credits even if they ultimately drop the course.

Little Evidence for Adverse Impacts on GPA, Issues Attaining Credits, or Rates of Academic Withdrawal. Amid these positive impacts on measures of academic difficulty, Table 4 shows little evidence that the invitation to and participation in *SB Program* affects first-year GPAs. This is true regardless of whether one accounts for grades earned during the summer program. There is also little evidence of differences in rates of taking too-few credits, with rates low across each group.

Furthermore, there is little evidence of differences in rates of academic withdrawal, which is low across both groups (about eight percent of students). While the estimates are noisy, both the impact on all students invited and invited students who participated have CIs that cross zero. *SI Appendix*, section G.2 shows the full regression results for the main specification as well

Table 2. Attributes of students in the analytic sample

Attribute	Percentage or value
Family hardship	41
Food insecurity	12
Housing insecurity	10
Family death	25
Neither parent attended college	66
Did not attend pipeline	56
SAT 25th percentile	1,400
SAT 50th percentile	1,460
SAT 75th percentile	1,500

Students' highest test scores were recoded to the 1,600-point SAT scale. The table shows both high rates of hardship prior to matriculation at SB university and potential academic challenges, with the median SAT score (1,460) is 50 to 80 points below the university's average. The supplement shows that the two groups are well balanced along these dimensions.

[§]Unfortunately, because many of these characteristics were coded through detailed reading by *SB Program* administrators of admissions documents rather than existing as structured fields in the university's application, we do not have corresponding estimates for the entire undergraduate population at the university. While we are not able to measure these attributes in the entire incoming class, *SB Program* administrators note the levels are much higher in the analytic sample than in the broader undergraduate population.

[¶]This is operationalized using the university registrar's tiering system, and calculated as the percentage of courses a student takes in their first year program that are >100 level.

Table 3. SB Program causes students to pursue a more challenging first-year academic program

Outcome	Estimand	Estimate	Control mean	N
Prop. nonintroductory classes	ITT	0.04 [0, 0.07]	0.38 [0.35, 0.41]	418
	CACE	0.07 [0, 0.14]		418
Prop. units for grade	ITT	0.03 [0.01, 0.06]	0.87 [0.85, 0.89]	418
	CACE	0.06 [0.02, 0.1]		418
Total units (includes SB)	ITT	0.64 [0.4, 0.89]	8.46 [8.3, 8.62]	418
	CACE	1.18 [0.82, 1.53]		418
Total units (excludes SB)	ITT	−0.14 [−0.31, 0.03]	8.19 [8.07, 8.32]	418
	CACE	−0.24 [−0.57, 0.08]		418

The first set of columns show the regression estimates from two specifications, examining outcomes that reflect a more challenging first-year academic program. The smaller point estimates with tighter CIs correspond to the ITT effects on students invited regardless of attendance. The larger point estimates with wider CIs correspond to the effects on students who "complied," or accepted the offer to attend the residential program. The second set of columns show the raw mean for the control group reweighted by the inverse probability weights discussed in *SI Appendix*, section E.1 and the lower and upper bounds on those means (the SE around the mean ± 1.96). We see significant impacts on three of four outcomes, with the differences in total credits going away when we do not include credits earned over the summer.

as the robustness of the results to the three other preregistered specifications.

Focusing on GPA, Fig. 1 shows how the program provides a GPA boost; students earn higher grades during the summer courses than they earn during the school year, with summer course grades clustered in the A- range (median GPA: approximately 3.65) compared to the B+ range for school year courses (median GPA: approximately 3.34). A Kolmogorov–Smirnov test confirms that the distribution of summer grades differs significantly from the distribution of school year grades ($P < 0.001$).[#]

This boost, however, puts treatment group students on parity with rather than ahead of control group students, as Table 4 shows that the program had little estimated effect on raising GPAs. The figure also illustrates the wide variance in GPAs and high academic achievers (near 4.0 GPAs) in both the treatment and control groups.

SI Appendix, section G.3 shows similar null results on the secondary outcomes of grades in STEM courses and in a required expository writing course. *SI Appendix*, section H shows similar null results when we construct a measure of GPA adjusted to the mean university-wide grades in different levels of courses. In sum, our analysis shows that the increased program difficulty neither harmed GPA overall or students' grades in specific, time-intensive coursework.

Does Increased Use of Academic Help Centers Mediate the Program's Impact? The residential form of *SB Program* is a complex intervention that delivers many small interventions simultaneously, including academic programming, peer support, and interactions with supportive adult mentors. Without randomizing students to different versions of the residential programming—for instance, some students take credit-bearing classes only, other students take those classes and receive more adult mentorship—it is impossible to isolate the specific causal mechanisms behind the program's effect (33).

However, one could examine whether the program's impact on the difficulty of students' first-year programs is mediated by a factor that past research has identified as a barrier to FGLI success: willingness to seek out academic help (19). Is the program's positive impact on the difficulty of the student's first-year program mediated by a mechanism where students in the treatment group are more willing to utilize university-based, academic support resources?

To operationalize concepts of willingness to seek academic help, we use deidentified metadata on students' visits to on-campus academic resource centers during their first year. The treatment group has a slightly higher mean of total help center visits—a mean of 0.89 [0.51, 1.28] visits in the first year in the treatment group compared to 0.77 [0.38, 1.15] in the control group—but the high variance in total visits means the

Table 4. SB Program has no adverse impact on GPA, rates of too-few credits, or rates of academic leave

Outcome	Estimand	Estimate	Control mean	N
First-year GPA (includes summer grades)	ITT	0.02 [−0.07, 0.1]	3.3 [3.24, 3.36]	418
	CACE	0.03 [−0.11, 0.18]		418
First-year GPA (excludes summer grades)	ITT	−0.02 [−0.1, 0.07]	3.29 [3.23, 3.35]	418
	CACE	−0.02 [−0.18, 0.14]		418
Too-few credits	ITT	−0.04 [−0.1, 0.02]	0.12 [0.07, 0.16]	418
	CACE	−0.08 [−0.19, 0.03]		418
Takes academic leave	ITT	0.02 [−0.07, 0.1]	0.08 [0.04, 0.13]	418
	CACE	−0.02 [−0.11, 0.07]		418

Both the Intent-to-Treat (ITT) effect and complier average causal effect (CACE) estimates are presented, along with the 95% CIs. The ITT effect represents the average impact of invitation to attend the SB program while the CACE is the effect of attending the program among those who would accept the invitation. We find little evidence for the impact of invitation to or participation in SB program on these outcomes.

[#]This difference is statistically significant when we estimate the difference in the full sample and the difference among treatment group students, who are more likely than control group students to have an observed summer GPA. This higher distribution of grades could occur due to several mechanisms. First, the summer coursework has a significantly higher amount of embedded academic support than regular-year courses. Second, the summer courses are smaller on average—closer to 20 to 30 students—than typical school year courses, which may also help with student achievement. Third, grading may vary between the summer and school-year coursework.

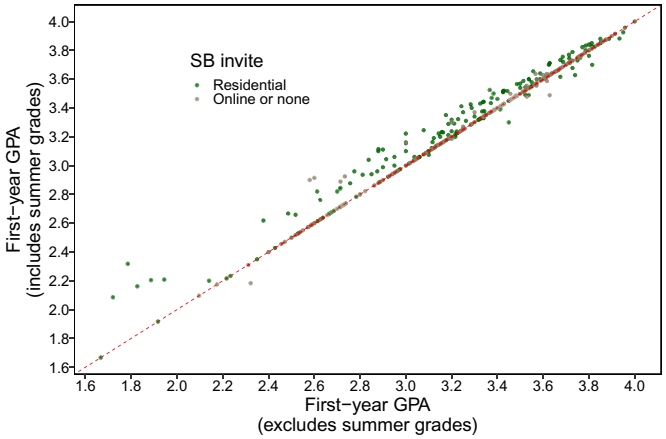

Fig. 1. SB program had no statistically distinguishable effect on GPA. Notes: The scatterplot shows both the heterogeneity in GPAs across the analytic sample—with some students earning a close to 4.0 freshman GPA and others struggling to earn above a 2.5 GPA—and the GPA boost that the grades earned during the summer program provides.

difference in groups is not statistically distinguishable from zero.[‖] *SI Appendix*, section J presents additional details of examining this mediator, with *SI Appendix*, Fig. S4 showing the full distribution of visit counts across each group. Due to the treatment's lack of impact on levels of the mediator, we did not conduct a full mediation analysis.

Did the Program Benefit Some Students More Than Others? We also examine whether or not the program benefits some students more than others. Although FGLI students are highly disadvantaged (Table 2), there exists heterogeneity across students in the degree of this disadvantage with some FGLI students facing more acute need than others. We investigate whether the program benefits all students similarly or provide greater benefits to some students.

Existing research offers competing predictions. On one hand, students exposed to pre-SB University pipeline programs—the "privileged poor" in Jack's framework (19)—might benefit less from *SB Program* because of redundancies between *SB Program*'s content and preuniversity experiences. On the other hand, these students have already gained some cultural capital from past enrichment efforts, so may benefit more from *SB Program* than their "doubly disadvantaged" counterparts.

In addition to examining the first preregistered moderator of past pipeline program, we also estimate heterogeneous effects for two other, preregistered moderators: 1) prematriculation standardized test scores (highest score coded to the SAT scale) and 2) whether neither of the student's parents attended college or whether one parent had some college exposure.

SI Appendix, section I presents the full regression results. We find few consistent heterogeneous effects, either for the academic difficulty outcomes or the outcomes with null main effects (GPA). For instance, while standardized test scores have a strong positive correlation with first-year GPA, we find no statistically significant differential impacts of the program by prematriculation test scores.

The lack of detectable heterogeneous effects may not be surprising for two reasons. First, at our given sample size, heterogeneous effects would have to be sufficiently large to be detected given the small sample sizes of certain subgroups.[**] Second, the heterogeneous effects are estimated within an analytic sample comprised students who are all deemed highly disadvantaged by administrators. If *SB Program* were expanded to the full pool of incoming students, we might expect it to have larger effects on disadvantaged students than on students without these markers of disadvantage.

Discussion

Summer bridge programs are a promising policy for universities to address some of the challenges faced by first-generation, low-income students. Intensive, residential programs provide an extended opportunity for students and program administrators to work together to help address student needs, which may be multifaceted and difficult to address with brief online interventions. We experimentally evaluate the impacts of a multiweek residential summer bridge program on outcomes for FGLI students.

We find that students randomly assigned to receive an invitation to a summer bridge program pursued a more challenging first-year academic program. This increase in academic ambition came with no discernible negative impact on GPAs. Thus, students who attended the program took more advanced courses than those who did not while still performing just as well academically. Subsequent analyses suggest these benefits may apply to all FGLI students similarly, and not just those with additional, precollege privileges and opportunities. The estimated cost of *SB Program* is $15,000 per student, which includes food, lodging, their stipends, and payment to the teaching faculty and course assistants. While this is a costly intervention, the *SB University* administrators view the program as an important commitment that aligns with the university's values in welcoming FGLI students to its campus. They plan to continue the in-person version given the benefits and alignment with these values. Other universities, however, may judge the cost–benefit ratio differently and consider alternatives like online programming, shorter-duration summer programming, or scaffolding that is restricted to the academic year.

Future work should track longer-term outcomes as students graduate from university and enter postgraduation workforces, beyond the first-year outcomes we focus on here. College transition and campus adjustment are a core emphasis of summer bridge programs, and our results here show that the program we study encouraged students to pursue more difficult college courses with no impact on academic performance. Nonetheless, positive impacts of any summer bridge program could compound over time, if (for example) student academic paths diverge in later college years or students benefit from bridge program communities in later years when seeking postgraduation opportunities.

Future research should also examine nonacademic outcomes. For example, an intensive summer bridge program could be an ideal opportunity to address other challenges FGLI students face, such as feelings of belonging or comfort when interacting with university authority figures (16, 18, 22, 24, 25). Analyzing these social and psychological outcomes would complement existing studies on mindset-focused interventions (16, 21, 26, 27). In addition, for student privacy reasons, we examined broad categories of courses (e.g., STEM; writing), rather than specific

[‖] We also preregistered examining an alternative visits measure, which is an indicator variable representing whether or not a student has *any* visit. The treatment group has slightly higher visit rates (17% of treatment group students visit an academic help center at least once compared to 14% of control group students). But this difference too is not statistically distinguishable from zero.

[**] We did not preregister conducting a formal power analysis for these conditional average treatment effects. A non-preregistered, post hoc power analysis now that we know the estimated effect sizes is inadvisable.

course-taking patterns that might have produced the course difficulty results. Future research could examine more granular course-taking trajectories.

Our study contributes to the growing body of evidence on how to address the unique challenges that FGLI students face in university environments. Nevertheless, it remains difficult to make generalizable claims about the effectiveness of these programs because of multiple sources of heterogeneity across studies, including student populations, university environments, and program content. Future work should further explore these differences to help us further understand how summer bridge programs best support students.

Materials and Methods

Analytic Sample and Randomization. Our analytic sample comprises students from three summer cohorts who attended the program (see *SI Appendix*, Table S5 for the breakdown of sample size across these three cohorts). In April of each year, after high school seniors had accepted *SB University's* admissions offer, we worked with university administrators on the evaluation design. The study was approved by *SB University*'s Institutional Review Board (IRB) prior to the randomization.[††] The IRB provided a waiver of informed consent because 1) this research involved no more than minimal risk; 2) the waiver would not adversely affect the rights and welfare of the potential participants in *SB Program*; and 3) it was determined that the research could not be carried out without the waiver or alteration.

For the first part, we mitigated the risk by both shielding the highest-priority students identified by administrators from randomization so that administrators could automatically issue them an invitation, thus excluding them from the RCT, and, for the students who remained, using higher probabilities to invite certain students who the administrators thought should be given a priority. For the second part, we offered the control group the online version of programming (as opposed to no programming) so that the control students are also given an opportunity to participate in a summer program. Finally, for the third part, since the RCT did not involve any additional interventions beyond the normal summer programming, and just involved use of existing administrative data, it was determined that opt-out biases, which would arise if students consent to the use of their deidentified administrative records, are likely to threaten the validity of the RCT.

Focusing on the design, we provide a summary of randomization procedure while leaving the details to *SI Appendix*, section C.1:

1. *SB University* forwarded a pool of *potentially eligible* students to the administrators of *SB Program*. This pool was based on broad factors like Pell Grant receipt, high school curriculum, expected family contributions, and parent educational attainment.
2. *SB Program administrators* further narrowed down this potentially eligible pool based on a range of family, academic, and high school characteristics, measured both quantitatively and through the admissions essays. They then placed students into two groups:
   (a) *Students who are in high need and ineligible for randomization*. These high-need students had zero probability of being randomized to the control group, because administrators removed them from the randomization pool for ethical reasons. These students, who number fewer than 20 each summer, fall outside our analytic sample.
   (b) *Students who are in lower need and eligible for randomization*. The remaining students were randomized and consist of our analytic sample.
3. *SB program administrators grouped students who are eligible for randomization into priority tiers*. The goal is to ensure that students flagged as high priority by administrators within our analytic sample receives higher odds of selection for *SB In person* (see *SI Appendix*, section C.1 for details).
4. *Research team randomized the students.* Finally, based on the above information, we conducted randomization. First, prior to randomization, we stratified on observed pretreatment covariates (SAT/ACT score; pipeline program participation; presence of different forms of hardship). Second, we gave higher priority to students whom *SB Program* administrators felt were more in need of *SB Program*.

   The exact method we used varied slightly between summer cohorts (see *SI Appendix*, section C.1 for details). In summer 2017, we used a mix of 3-student blocks and 2-student blocks (matched pairs) within each tier to achieve covariate balance. In summer 2018, within-tier blocking did not significantly improve balance, so the priority tiers were the only stratifying factor. In summer 2019, we again used 2-student blocks within tiers. Across all summers, students in higher administrator-designated tiers received higher odds of an invitation to in-person SB Program.

This randomization process resulted in a high degree of covariate balance between the treatment and control students. While Table 2 highlights the degree of disadvantage of the students in the analytic sample, the randomization procedure meant that the treatment and control groups were well balanced along important characteristics. *SI Appendix*, Table S6 shows balance in student attributes between these two groups.

Research Questions and Measurement of Outcomes. We measure outcomes over the student's first year, which occurred both during normal academic programming (summer 2017 and 2018 cohorts) and with academic programming impacted by the COVID-19 shift to online learning. For reasons of statistical power, we pool students across cohorts but adjust for cohort-specific effects by including fixed effects for randomization blocks. Specifically, we focus on the following, preregistered research questions:

- GPA: Did *SB Program* lead to *higher* academic performance in the first year of college, defined in terms of GPA?
  - One goal of *SB Program* is to help students from less well-resourced high school environments succeed academically at the selective private university, both through imparting "hard skills" in the form of academic programming, and through improving "soft skills" such as encouraging students to seek out academic support and help.
- Academic difficulty: Did *SB Program* lead to *lower* rates of academic difficulty in the first year of college, defined in terms of helping a student *avoid* indicators of academic distress?
  - This question is concerned about the effect of *SB Program* on low academic performance. Past work has found that similar interventions may lower the probability of a student experiencing adverse academic outcomes like dropout (21, 27). Since the selective private university has a low dropout rate, we focus on a temporary withdrawal from the university or academic probation. Specifically, we analyze two binary outcomes that are indicators for two forms of academic difficulty: nonvoluntary withdrawals and having too few passing credit units for a year.
- Academic ambition: Did *SB Program* improve academic ambition and course-taking difficulty?
  - We use the following three measures. First is total units taken, which is measured in two ways–including or excluding the units from *SB Program*. Second is the proportion of nonintroductory courses, which reflects taking a more difficult set of classes. Third is the proportion of units that are taken for a grade, measuring attempts to take courses for a grade rather than pass-fail.

For the outcomes involving credits and grades, we constructed two versions, each of which has different strengths and weaknesses for measuring academic inequality. First are versions *with* the grades and credits from *SB Program*; this is the version of a student's GPA that the student him or herself, advisers, graduate schools, and future employers observe. Second are versions *without* the grades and credits from SB Program; if the "with SB GPA" or other outcomes show between-group differences, this helps us understand the extent to which these

[††]We are not able to disclose the name of the approving IRB since this IRB is located at the institution that hosts *SB Program*.

differences derived from the grades and credits earned during *SB Program* or from grades and credits after *SB Program*. We also added a non-preregistered "difficulty-adjusted GPA measure" that we describe further in *SI Appendix*, section H.

Data, Materials, and Software Availability. Data and replication code is available within the Harvard Dataverse repository and is accessible at the following url: https://doi.org/10.7910/DVN/DLBFNS (34).

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