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Experimental Estimates of College Coaching on Postsecondary Re-enrollment

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July 2024

School of Education
UNIVERSITY OF COLORADO **BOULDER**

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July 2024

Abstract: College attendance has increased significantly over the last few decades, but dropout rates remain high, with fewer than half of all adults ultimately obtaining a postsecondary credential. This project investigates whether one-on-one college coaching improves college attendance and completion outcomes for former low- and middle-income income state aid recipients who attended college but left prior to earning a degree. We conducted a randomized control trial with approximately 8,000 former students in their early- to mid-20s. Half of participants assigned to the treatment group were offered the opportunity to receive coaching services from InsideTrack, with all communication done remotely via phone or video. Intent-to-treat analyses based on assignment to coaching shows no impacts on college enrollment and we can rule out effects larger than a two-percentage point (5%) increase in subsequent Fall enrollment.

* *Acknowledgments:* We Arnold Ventures for supporting this work and InsideTrack for being a willing research partner, especially Kai Drekmeier, Megan Fillman, and Linda Firth. We are grateful to participants in the 2021 Association for Education Finance and Policy conference for helpful comments and suggestions. Experiment reviewed under the University of Missouri's IRB Determination Notice Project #2017082 Review #253351. Pre-registration plan can be found at https://osf.io/6wfsz. ⓡ indicates random assignment of author order through the AEA Author Randomization Tool. *Disclosures:* This project was funded by a grant from Arnold Ventures. Turner has received compensation from the Strada Education Network as a member of Strada's Research Advisory Group (Inside Track is part of the Strada Network).
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1. Introduction

College enrollment has increased significantly over the last few decades, yet among individuals who enroll in college, fewer than 61% graduate within eight years (Shapiro et al. 2019). A robust literature provides evidence that college completion substantially increases employment and lifetime earnings, and difficulties in repaying student loans are disproportionately experienced by students who leave college before receiving a degree (Bhuller, Mogstad, and Salvanes 2017; Barrow and Malamud 2015; Oreopoulos and Petronijevic 2013; Looney and Yannelis 2015).

Many studies show that providing students additional supports, such as college counseling or financial aid, increases graduation rates.[1] In this project, we examine whether student supports can improve short-term reenrollment outcomes for non-traditional students, in this case, those who have attempted but dropped out of college and wish to return. Many of these former students face challenges and hardships that could be addressed with existing resources and support but are often unaware of or unable to access them. Further, once a student is out of college and in a new routine, they may need encouragement and strategic support in order to return. Nationwide, only 2% of former college students with no degree choose to re-enroll (Ortagus and Perrault 2019; Causey et al. 2023).

We conducted a randomized control trial in which college dropouts who expressed an interest in returning to college were offered coaching and counseling services from InsideTrack, a college counseling service provider. Bettinger and Baker (2014) provide experimental evidence

[1] See, for example, Bettinger and Baker (2014); Bettinger et al. (2019); Denning, Marx, and Turner (2019); Page et al. (2017); and Weiss et al. (2019). Evans et al. (2019) find that emergency assistance grants are only effective at increasing community college student attainment when paired with additional supports.

that InsideTrack counseling offered to traditionally-aged enrolled college students increased degree completion in a cost-effective manner. InsideTrack's stated goal is to have coaches establish a personal connection with the student and their potential postsecondary institution, identify student or institutional barriers to successful re-enrollment, and help students overcome these barriers. Our sample includes low- and middle-income students who received a state aid payment for one to three years in a California community college or California State University but then stopped receiving the award, which we use as a proxy for dropping out before graduating. We sent these students emails and text messages informing them of an opportunity to receive coaching to help them return to college, and students who responded they had not yet earned a degree could opt-in and be randomly assigned to receive services. This process occurred over two years. We first emailed and texted students in January and February 2020 and randomized the 4,042 students who affirmatively opted-in to receive coaching services. In the second year, we began earlier, by emailing and texting students from October 2020 through February 2021, with 3,998 students opting-in. This second round included both students who were eligible but had not opted-in during the prior round, as well as a new group of students who had first enrolled in college in 2018 before leaving.

Roughly half of all students assigned to the treatment group engaged with their college coach at least once, but we find no evidence that treatment assignment increased college enrollment in the following fall semester. Intent-to-treat estimates of effects on immediate college enrollment are small – generally less than one percentage point – and statistically insignificant, and we find no evidence of heterogeneous treatment effects by parental education, gender, or whether a student had initially enrolled in a two- versus four-year institution. We also find no impacts on

Free Application for Federal Student Aid (FAFSA) submission or enrollment and persistence rates into the second academic year after the experiment.

Although our experimental results are internally valid, it is important to note the context that students in the first cohort began coaching right at the start of the COVID-19 pandemic in March 2020, and those in the second cohort began coaching during November 2020. In the conclusion, we discuss potential challenges experienced by the students and coaches during this experiment, and ways to improve this work moving forward.

2. The Causes and Consequences of College Dropout

First-time college enrollees enter with high expectations of earning a degree, although only 64% of four-year students and 34% of students at two-year colleges do so within 150% percent of the expected time-to-degree (National Student Clearinghouse, 2020). Students leave college without earning a degree for many reasons, including financial constraints, academic difficulties, or lack of focus or interest in their degree program, and U.S. adults who belong to the category of having "some college, no degree" face worse employment and earnings outcomes than associate and bachelor's degree recipients (Bird et al. 2020; Torpey 2018). While many students intend to only "stop out" from their studies, even short-term interruptions can have long-term negative consequences for attainment and post-college success, and nationally only 2% of former college attendees re-enroll in a given year (DesJardins, Ahlburg, and McCall 2006; Crosta 2014; Goldrick-Rab 2006; Charles, Hurst, and Notowidigdo 2018; Causey et al. 2023).

Prior research suggests that providing appropriate support can help improve students' postsecondary attendance, completion, and labor market outcomes. Financial aid can help capable but credit-constrained students afford classes and reduce work hours or other stressors that might negatively impact academic performance (Darolia 2014; Broton, Goldrick-Rab, and Benson 2016). However, financial aid alone may be ineffective without more intensive individualized support, especially for community college students (Carruthers and Ozek 2016; Anderson and Goldrick-Rab 2018). A number of college counseling programs have targeted high school students undergoing the challenging transition to college, generally – though not always – finding positive impacts on attendance or enrollment in more selective colleges.[2] However "high-touch" counseling may be difficult to conduct at scale and less intensive interventions that simply provide students additional information or low-touch guidance are generally less effective (Oreopoulos and Petronijevic 2018; Gurantz et al. 2021; Bergman, Denning, and Manoli 2019; Bird et al. 2021; Clotfelter, Hemelt, and Ladd 2018; Bettinger et al. 2022), and may be less likely to change the decisions of older, non-traditional students who have already been unsuccessful in the college environment.

There is limited evidence on the effectiveness of interventions and supports for helping former college students return to and complete a postsecondary credential, even though they may experience sizeable increases in their income if they were to do so. In partnership with several Florida community colleges, Ortagus, Tanner, and Isaac McFarlin (2021) implemented a randomized control trial targeting community college dropouts who were previously academically successful. Students assigned to a text messaging campaign who received information about the reenrollment process and a one-course tuition waiver were significantly more likely to reenroll, but effects for the text message only group were small and insignificant. In another experiment, Barr et al. (2022) randomly assigned veterans who were separating from

[2] See, for instance, (Barr and Castleman 2018; Page et al. 2017; Bettinger and Evans 2019; Castleman and Goodman 2018; Carrell and Sacerdote 2017; Phillips and Reber 2019; Gurantz et al. 2020; Oreopoulos and Ford 2019). Similarly, in-college mentoring has been shown to reduce the risk of dropout and, in some cases, increase degree completion (Oreopoulos and Petronijevic 2018; Evans et al. 2020).

the military to receive text messages with personalized information, reminders, and/or advising about their college and university options, but the authors find no effect on subsequent college enrollment or college quality. Although existing evidence points to positive effects of InsideTrack mentoring on attainment among enrolled students (Bettinger and Baker 2014), the challenges faced by students who have left college may be different and more extensive.

3. Experimental Setup

A. Data

We use data provided by the California Student Aid Commission (CSAC) to identify students who likely left college before completing their degrees. CSAC provides financial aid to hundreds of thousands of low- and middle-income students each year through the Cal Grant program. The largest Cal Grant program is the “Entitlement” award, and high school graduates apply by submitting the FAFSA and having their school submit a one-page grade point average (GPA) verification form by March 2nd.[3] The Cal Grant is a generous award that covers up to four years of enrollment, essentially offering students full tuition and fees at any in-state public four-year institution, or an annual subsidy for private colleges of approximately $9,000. Students below the low-income cutoff can also choose to receive a cash subsistence award to support community college attendance, which was $1,648 per year in 2018-19.

[3] Students are offered the Cal Grant Entitlement award if they are from middle- or low-income families and have an unadjusted GPA of at least 3.0 or 2.0, respectively. Income limits that define middle- and low-income families vary slightly by year and family size, but for dependent students from a family of four in 2018-19 they were $98,900 and $52,000, respectively. Students have two years to apply for the Entitlement award, either as a high school senior or one year later, though most apply in their senior year. Once students are offered an award, they can place it on hold for up to two years at any point if they wish to pause their college enrollment, though by construction none of our experimental sample initially put their aid on hold.

The CSAC data include information from a student's initial FAFSA and Cal Grant application. The FAFSA includes student background characteristics (e.g., birthdate, sex, income, degree objective, family size, zip code), and the Cal Grant application provides high school GPA and high school attended. We also observe state financial aid payments, including the last institution a student attended and the years in which they received payments.

Per the pre-registration plan, our primary outcome is re-enrollment in a postsecondary institution within one year of treatment assignment.[4] We measure this outcome by matching our sample to the National Student Clearinghouse (NSC), which provides enrollment and degree receipt information at most colleges nationwide (Dynarski, Hemelt, and Hyman 2015). We also obtained a complementary source of enrollment data from California's public colleges that is provided to CSAC each fall, which we refer to as "CSAC enrollment" data. These data include dummy variables that identify fall enrollment in the California State University (CSU) and University of California (UC) systems and term-level enrollment (Fall and Spring) in California community colleges.[5] Both data sources provide similar results and unless otherwise noted, all results are based on NSC data and focused on enrollment in the Fall term of the subsequent academic year.

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[4] The pre-registration plan can be found at https://osf.io/6wfsz/. We proposed two primary outcomes, with the second being earning a postsecondary degree within three years of treatment assignment. We will be able to observe this second measure for cohorts after the summer of 2024. Given we find no treatment effects on initial attendance and persistence, we do not anticipate finding substantial effects on degree completion.

[5] Appendix C provides additional information comparing NSC and CSAC enrollment data. Public college data is provided to CSAC from each college during September each year, but the exact time at which these data are transferred varies by college and may reflect slightly different enrollment dates. NSC data in this report were submitted for matching in February 2022. The benefit to using the NSC data, in addition to being our pre-registered data source, is that we can observe enrollment in private or out-of-state colleges (which are not available in CSAC in-state, public college enrollment data), though in our sample only 2% of students enroll in these alternate sectors. The benefit to using CSAC enrollment data is that the matching is likely more accurate (as it relies on SSN rather than NSC's name and birthdate approach) and is not subject to "FERPA Blocking".

We also examine effects on intermediate steps that indicate an interest in college enrollment, such FAFSA submission and/or subsequent financial aid receipt. We rely on CSAC data to measure whether students submitted a FAFSA and whether they received a Cal Grant payment for enrollment in the Fall semester after randomization occurred.

B. Experimental sample recruitment

Our experiment focuses on students who received a Cal Grant and attended a CSU or community college (CC) but left before earning a degree. Although we do not observe college completion data for Cal Grant recipients, available data suggest that many struggle to finish their studies. Among students who received a Cal Grant at a CSU, only 60% received aid for a full four years, and among community college entrants, the four-year persistence rate was an even lower 20%. Although some of these students may have earned a community college credential, three-year completion rates at California community colleges are low, averaging only 36% for recent cohorts, similar to the national average.[6]

In the first year of the study, the pool of former students eligible to participate in the intervention was approximately 130,000 former Cal Grant applicants who had an email address and phone number on file, first received a Cal Grant payment at a CC or CSU between 2014 and 2017, and who received aid payments for one to three years. We contacted individuals in this group via email and text messages (shown in Appendix B) in January and February 2020. Recruitment emails were sent from an official CSAC email address to garner trust and included a link to an official CSAC website explaining the project for those who may have had concerns.[7]

[6] Author's calculations using data from the Integrated Postsecondary Education Data System (IPEDS) on 150% completion rates in two-year colleges.
[7] https://www.csac.ca.gov/researchinside-track

Outreach emails and texts invited former students to complete a questionnaire if they were interested in returning to college. The questionnaire could be accessed through a hyperlink in the emails and text messages and asked the following questions: (1) name, (2) updated phone and email contact information, (3) an opportunity to choose from a short list of challenges that the student believed had prevented their degree completion, (4) whether the student had ever used a Cal Grant, and (5) whether they earned a degree. The questionnaire also asked the student to affirmatively opt-in to the experiment, provided they had not earned a degree.

A total of 4,042 students opted-in to the study in the first year. We initiated this project anticipating a larger sample, and so extended recruitment into a second year. In this second year we conducted outreach to two groups: (1) all students in the first round who had not opted-in to the program, and (2) a new cohort of students who first received a Cal Grant payment in 2018 but stopped receiving payments after one year (i.e., newly eligible students who could not have been identified when we conducted outreach in the first year). Outreach was conducted from late October 2020 through early February 2021, and an additional 3,998 students opted-in to the study, resulting in a total of 8,040 participants across both cohorts.

C. College coaching treatment condition

As we recruited students over a multi-month period, students were assigned to treatment on a rolling basis. Students assigned to treatment were offered the opportunity to work with an InsideTrack coach. InsideTrack has engaged in student re-entry work since 2007, partnering with large state systems and institutions. Students who opted-in but were assigned to the control group received information about the steps required for college re-entry, including websites they could visit such as those provided to the public by CSAC, CSU, and California community colleges.

All InsideTrack coaches have a bachelor's degree and coaches receive close to 100 hours of professional development every year. InsideTrack had two coaches continuously working with students in the first and second years of the experiment and, in the second year, an additional part-time coach. Coaches received the typical training provided by InsideTrack, though there was additional training on helping students on academic probation, given the prevalence of that issue with stopped out students. Coaches also received additional training and acculturation around the outreach portion of the work, given that they were often required to reach out to students compared to other InsideTrack initiatives. Coaches were available from the time of randomization through the following September, at which point students could have returned to college and so the counseling intervention ended.

Interactions between InsideTrack coaches and treatment group members focused on creating and advancing a student's reenrollment plan and identifying and addressing obstacles to reenrollment. InsideTrack first set up a short (5-10 minute) online or phone meeting to confirm the participant's interest in returning to school, gather basic information as to where they are in the schooling process, and update or expand the former student's contact information. After the first meeting, discussions typically focused on the issues that were most significant in the former student's reason for leaving college.

Based on the intake survey (Appendix B), the two most common (not mutually exclusive) reasons provided for a respondent's dropout decision were "work became my main priority" (64%) and "needed to leave temporarily to take care of a family member, or fulfill another short-term commitment" (45%), and 81% of respondents listed at least one of these two categories. In descending order, the remaining responses included college expenses (33%), failing to meet important administrative deadlines (31%), difficulty of coursework (26%), and not feeling like part of the community (22%).[8] Intake survey results were broadly similar between cohorts, with most answers varying by 2 to 8 percentage points (e.g., “work became my main priority” was 60% in the first cohort and 68% in the second cohort). The one exception was “needed to leave temporarily to take care of a family member, or fulfill another short-term commitment”, which was 51% in the first cohort and dropped to 39% in the second cohort.

InsideTrack’s work providing coaching to currently enrolled undergraduate students had previously been evaluated (Bettinger and Baker 2014), so we highlight some potential differences and similarities between this experiment and those prior approaches here. The main thrust of the coaching relationship – to develop trust and help students meet their goals – was similar to prior work. Differences in populations served did end up leading to significantly different types of conversations or activities undertaken by the coach. Sample text message conversations we reviewed discussed things such as: where the student was considering enrollment; reasons why they previously took a break from studying (e.g., external responsibilities, such as caretaking of family members, uncertainty about why they were in college, such as confusion over the link between major choice and post-college jobs); current feelings about returning to college (e.g., whether they were confident they would succeed, whether had they had identified a potential major) and; administrative obstacles to re-enrollment and financial aid receipt, which we discuss in the conclusion. Some of these text-based conversations could be quite long and include tens of comments each from the coach and student.

[8] Students were not required to list a reason for not earning their degree on the intake survey. Overall, 91% listed at least one reason for leaving, with 88% and 93% doing so in the first and second cohorts. Students who did not list a pre-specified reason either left the field blank or listed their own reasons (e.g., mental health, pregnancy, lack of motivation, unable to decide on an area of focus).

In terms of coaching activities in the previous study, even though InsideTrack used online coaching, coaches often helped facilitate in-person meetings between students and staff within the students' institutions, and these meetings often centered around course performance or other educational activities. In contrast, the coaches in this experiment spent significantly more time conducting outreach to working adults who were not always responsive, thus leading to an increase in more "administrative" tasks rather than the preferred individual communication and coaching. In that sense, the actual coaching was relatively "light touch", as participants preferred less contact but around larger issues, and often required a high level of work on the part of the coach. Very roughly, InsideTrack estimates that one-third of coach's time was spent in direct communication with students in the treatment group. One part of the remaining time was devoted to outreach, which included both broad messages sent to all treatment group students encouraging them to reach out and targeted messages towards individuals with whom the coach had prior contact. Another part of coaches' time—discussed further in the conclusion—was spent on differentiated activities focused on resolving administrative burdens and personal crises.[9] In short, the diffuse nature of the experiment, in which coaches worked with students all over the state, required the coaches to assist students in resolving administrative obstacles to re-enrollment that varied across colleges. In addition, the timing of the experiment during COVID led to elevated rates of crises that required coaches to help students resolve non-academic issues

[9] While we do not have direct measures of how many hours each student received in coaching, we do a back-of-the-envelope calculation of the average per-student hours the intervention entailed. The first cohort had two full-time coaches working for nine months, or about 3120 hours for approximately 2000 treatment group students. This is roughly 1.5 hours of effort per student. Although about half of the treatment group students never directly engaged with their coach, there was still effort on the part of the coach to reach out and engage these students. On occasion this outreach might be a generic email "blast", though at other times this could be more personalized, especially for students who might have met once with the coach. In the second cohort there were 2.5 coaches (2 full-time and 1 part-time) and the intervention started slightly earlier. This resulted in closer to 5000 hours of effort for around 2000 treatment group students, or about 2.5 hours per student. Given at least some of the coaches' time was likely taken up with administrative tasks and other similar activities, these likely represent upper bound estimates of one-on-one time coaches spent with students.

(e.g., mental health), often through directing them towards alternative resources or InsideTrack's crisis specialists.

4. Methods

Random assignment of access to coaching allows for identification of causal effects with minimal assumptions, namely successful random assignment and no spillovers from treatment to control group members. We show that baseline characteristics are balanced between treatment and control group members. Spillovers are highly unlikely due to the small number of students in the experimental sample relative to the total population of dropouts, as well as the wide geographic distribution of students across California.

We conduct intent-to-treat (ITT) analyses that compare outcomes for students who were offered InsideTrack re-entry counseling versus outcomes for students who were not. To identify these effects, we estimate ordinary least squares (OLS) models of the following form:

$$Y_{irs} = \beta T_{irs} + \theta_{rs} + \boldsymbol{\gamma}\mathbf{X_i} + \varepsilon_{irs} \qquad (1)$$

Y_{irs} is the outcome of interest for individual *i* in randomization round *r* and strata *s*, and T_{irs} is a binary variable equal to 1 if the individual was assigned to the treatment group. To provide students coaching as soon as possible after signing up, individuals were randomized on a rolling basis in four rounds from mid-January to late February 2020 and in nine rounds from October 2020 to February 2021. Within each round, randomization was stratified such that each student was assigned to a group based on: (1) whether they first attended a community college or a CSU (determined by their first Cal Grant payment); (2) the first year receiving Cal Grant aid, and; (3) the last year receiving Cal Grant aid. This resulted in 222 unique strata, though 80% of the full sample belonged to one of 96 larger strata that had from 27 to 380 individuals. When a stratum had an odd number of students in a given round, we assigned the extra student to the treatment group. Thus equation (1) also includes "strata-by-round" fixed effects (θ_{rs}).[10]

Although not necessary for identification, our main pre-registered specification includes a vector of baseline characteristics ($\mathbf{X_i}$): sex, parental education, median household income within the student's zip code, and two high school characteristics from the Common Core of Data (urbanicity and percent free/reduced-price lunch). Results in this paper include these pre-registered covariates but all analyses produce similar results when using (1) strata fixed effects with no covariates and (2) additional covariates.[11] Standard errors are clustered by randomization strata (Deeb and Chaisemartin 2021; Chaisemartin and Ramirez-Cuellar 2020).

We note three issues that arose in the context of the experiment. The first was that some students had dropped out of college but were considering immediate reenrollment (e.g., we contacted students in early January 2020 who were planning to restart in the Winter term), whereas our primary pre-registered outcome was more traditional enrollment in the subsequent Fall term. As these were college dropouts returning to school, these students continued to receive support from InsideTrack to help them transition back into college. The second issue was that a small number of individuals in the first cohort identified as "dropouts" and who opted into the experiment subsequently told InsideTrack counselors that they had never dropped out of college – even though the survey they filled out asked them explicitly about reasons they had dropped out that prevented them from earning a degree. These students did *not* continue to receive counseling support from InsideTrack, though at this point it was impossible to remove them from

[10] The exact randomization dates are provided in Appendix Table 1. Relatively few opt-in students entered college in 2014 and exited in 2016 or 2017, so we combined these students into the same stratum as those who entered college in 2014 and exited in 2015 (separately for CSU and community college students).
[11] Over the course of the project, we were able to add additional variables including high school GPA from the Cal Grant application, and age, family size, and degree objective (bachelor's, associate, or other/missing) from the FAFSA.

the experimental sample. We perform an additional analysis that uses enrollment data to disaggregate the experimental sample into those who were enrolled in college in 2019-20 versus those who were not; although we think this constitutes the relevant treatment effect based on the experiment's goals, we recognize that this outcome is "exploratory" given our pre-registration plan.

The final and more minor issue affects just the first year of the experiment, when a small group of students who were randomized in the second experimental round were accidentally included again in the third experimental round because they had filled out the opt-in survey multiple times and were not appropriately screened out. We classify these students based on their initial treatment assignment in the second experimental round, though this led some students to have the wrong treatment assignment. Because we classify participants as treatment and control group members based on the initial assignment, this does not cause any issues in identification of treatment effects, but slightly reduces the treatment-control contrast.[12]

5. Effects of access to coaching and reentry support

A. Descriptive statistics and baseline equivalence of treatment and control groups

Table 1 displays tests of the equivalence of baseline characteristics between treatment and control students; control group means are located under these estimates. There were 8,040 students who opted into the study, with 4,076 assigned to the treatment group and 3,964 to the control group. Appendix Table 1 shows treatment assignment by cohort and randomization date. Among students who opted-into the study, 61% were female, 21% reported having a college-

[12] As we show later in the paper, 1.6% of the 2,003 control students in the first cohort engaged in outreach to a coach, and 0.4% engaged two or more times, rather than 0 percent as might be expected if this mistake had not occurred.

educated parent, and average age at the time of opt-in was 23 years old. Participants previously attended high schools in which an average of 67% of the student body received free or reduced-price lunch and 46% of high schools were in urban settings, 37% in suburban settings, and 9% in town or rural settings (7% missing). The average high school GPA was 2.82, consistent with the characteristics of high school students who attend less selective community colleges or the broad-access CSU system. We find relatively small differences in the composition of opt-in students between the two years of the experiment, even though the first cohort had already been recruited and assigned to treatment by February 2020, one month before the onset of the Coronavirus in March 2020. The second cohort opted-in during the following academic year.[13]

B. Effects of treatment assignment on coaching take-up and intensity

We examine contacts between students and coaches to quantify how treatment assignment affected actual coaching receipt. We do not present instrumental variable estimates based on these results, but simply show a few measures of engagement which can be used to scale impacts.

Table 2 shows regression estimates of differences in coach contract rates between treatment and control students. These recorded contacts only include those initiated by the students and marked as incoming in the InsideTrack data. The first row shows estimated effects on total communications, which could take the form of longer phone calls or video meetings or be as

[13] Appendix Table 2 compares our experimental sample of 8,040 individuals to the broader eligible pool of approximately 160,000 students who received outreach. Students who opted into the experiment had a number of characteristics typically associated with disadvantage (e.g., less likely to have college-educated parents, lower high school GPA, attended more urban high schools with higher levels of free and reduced-price lunch participation). They were also slightly younger and more likely to be female. Overall, we find no difference in enrollment rates of students who opted into the experiment compared to those that did not, though those who did not opt into the experiment were more likely to be enrolled in a CSU, perhaps indicating that they did not participate because their postsecondary plans were already in progress.

limited as a single text message from a participant to their coach. Assignment to treatment led to an increase of 2.9 communications, over a baseline of essentially zero (0.03 on average) in the control group. The second row shows that treatment group members were 49 percentage points (pp) more likely to communicate with their coach at least once, over a baseline rate of 0.9pp in the control group.

In terms of more sustained contact, we find that treatment assignment increased the likelihood of having two or more contacts by 30pp (baseline = 0.2pp). This implies that approximately 19% of treatment group students only had one contact with their coach.[14] Although in prior work, InsideTrack coaches typically set-up a short initial meeting with students, in this case, they found that many of these initial communications were longer conversations between the coach and student that often went into depth about the student's goals.

We find lower levels of student-initiated communications for those in the second cohort. Students assigned to treatment group in the second year of the experiment were 9pp less likely to reach out to their coach (44% versus 53%), and 5pp less likely to have two or more communications (27% versus 32%). Appendix Table 3 shows contact rates by mode of communication, with about 81% of total communications coming from text messages rather than phone or email contacts. Even so, of the students who engaged with their coach about 36% had at least one phone call.

As about half of students assigned to the treatment group ultimately did not engage with their coach, thus, treatment on the treated impacts of any coaching receipt would be twice as large as the reduced form effects discussed below. Among the subsample of students who contacted their

[14] Contact could imply a series of texts or communications in a day (the length of the contact was not recorded).

coach, they reached out roughly 6 distinct times on average. This average masks substantial heterogeneity, with some students contacting coaches a few times and others a more significant amount. A histogram showing total communications (top-coded at 15 communications) is shown in Figure 1.

C. Effects of treatment assignment on college reenrollment

We find that assignment to college coaching in our experiment produces no statistically significant impacts on postsecondary enrollment. Unless otherwise stated, all enrollment outcomes are based on Fall enrollment in the subsequent academic year (i.e., Fall 2020 enrollment for the first cohort who received outreach in early 2020, and Fall 2021 enrollment for the second cohort who received outreach in late 2020 and early 2021). Most point estimates are below 1pp and estimates from our main specification based on NSC enrollment data have 95% confidence intervals that exclude effects of treatment assignment increasing enrollment by greater than 2pp.

Table 3 shows estimated impacts on postsecondary enrollment using NSC data. Focusing on the first row, estimates show that assignment to the treatment group led to no change in enrollment, compared to a baseline of 33%. Disaggregating by sector, enrollment in California community colleges declined by 0.6pp and enrollment in other sectors increased by 0.8pp.[15] Results using the complementary CSAC enrollment data are shown in the first three columns of Appendix Table 4 and produce similar results, with a 0.1pp decline in enrollment over a baseline of 36%. In the CSAC enrollment data, community college enrollment declines by 0.3pp and

[15] Results are the same when we include both Fall and Spring term enrollment (omitted for brevity).

CSU/UC enrollment increases by 0.8pp, which is marginally significant at $p<0.10$.[16] The last columns of Appendix Table 4 show estimated effects on an alternative enrollment measure that combines the NSC or CSAC data and classifies a student as enrolled if either data set indicates this. Results are similar and generally smaller in magnitude.[17]

Treatment effects do differ by cohort, though the magnitude and statistical significance of differences between cohorts vary by outcome data. Table 3 shows the first cohort's Fall enrollment increases by a statistically insignificant 1.3pp and, for the second cohort, declines by an insignificant 1.3pp. The *p*-value from a test of the hypothesis of equivalent effects for the two cohorts is 0.16. The gap in treatment effects between cohorts is larger in the CSAC data (Appendix Table 4)—indicating that treatment resulted in a 2.0pp increase in enrollment for the first cohort and a 2.2pp decrease for the second cohort. We can reject the hypothesis of equal treatment effects in this case ($p = 0.02$).

One question is whether the difference between the two cohorts represents a meaningful difference in treatment effects or just variation in the point estimates due to chance. Although we cannot provide any definitive answers, there are a few reasons why treatment effects may differ. First, 74% of the second cohort experimental sample were students who we contacted in the first

[16] We combine CSU and UC as very few students are enrolling in the UC system. Overall estimates may be different than simply adding these two coefficients as some students attend multiple sectors, and all regressions control for assignment strata, thus producing variance weighted results. Comparing simple averages does not change the results. Estimating impacts on traditional measures of college quality, such as graduation rates, loan default rates, and earnings, also produces small, insignificant results.

[17] One possibility is that our treatment effects are downwardly biased towards zero if students in the experiment were interested in career and technical education or job training programs that might not appear in either the NSC or CSAC enrollment data, such as schools that do not participate in Title IV programs and for-profit colleges (e.g., cosmetology). Although InsideTrack supported students who expressed interest in these alternate or online programs, minimally we can say that the offer of coaching did not induce changes in community college enrollment, which constituted a large portion of the conversations between coaches and students and was the destination of approximately one-quarter of the students in our sample.

year of outreach but initially did not choose to participate. The remaining 26% of students in the second cohort experimental sample had not been previously contacted (i.e., these students first enrolled in college in 2018-19 but stopped receiving state aid after one year). Appendix Table 5 shows that the negative enrollment impacts for the second cohort are driven by students who had been contacted in the first year (-1.8pp), not newly contacted students (0.2pp), though both results are statistically insignificant.[18]

Given this, we are essentially comparing a group of students who differed in when they responded to the outreach and the year in which they were considering enrollment (i.e., during the beginning of COVID versus one year later). Although Table 1 shows that the two cohorts were relatively similar on observable measures, the second cohort has a much lower baseline enrollment rate of 24%, compared to 40% in the first cohort. Differences in control group enrollment also correspond to differences in coaching engagement rates, where students in the second cohort who were assigned to receive college coaching were less likely to later engage with their coach (Table 2). This suggests that the second cohort may have differed along unobservable characteristics that contributed to their delayed response to our initial outreach and lower probability of communicating with their coach or follow-up on their intention to return. This may be a consequence of COVID, as the cumulative effects of the pandemic may have taken a toll on prospective students; year-to-year enrollment declines for older students at two-year colleges were larger nationally in the second year of the pandemic (Berg et al. 2023). Yet another difference between the cohorts is that we slightly changed our intake process in the

[18] Behaviors were relatively similar between students in the second cohort with prior contact and those who were newly contacted. Among prior contact students assigned to the treatment group, 43% reached out to their coach compared to 49% of newly contacted students, and baseline enrollment rates were 24% and 25%, respectively.

second round, to better screen out individuals who were already enrolled in college, which likely lowered our baseline enrollment rates.

D. Effects of treatment assignment on additional outcomes

One possibility is that coaching might not alter initial enrollment but could help students feel prepared or confident to make progress towards a degree, and thus increase persistence. Appendix Table 6 shows estimated effects on second-year enrollment using only the CSAC data, as persistence outcomes using NSC data are not yet available for both cohorts. We examine two persistence outcomes – if a student enrolled in the second year after the experiment (i.e., Fall 2021 or Fall 2022 for the first and second cohorts, respectively), or if a student was enrolled in both the first and second years – and find null results in both cases. When disaggregating by cohort, we again find marginally significant increases in persistence for the first cohort and marginally significant decreases for the second cohort; the difference between cohorts is statistically significant ($p < 0.05$).

Coaches may have also helped students prepare for college by getting them to submit financial aid forms or other documents, even if students ultimately did not follow through on their intentions to enroll. We do not find evidence that this is the case. Table 4 shows null impacts of treatment assignment on FAFSA submissions for the academic year following random assignment. Students in the treatment group were a statistically insignificant 0.4pp more likely to submit the FAFSA over a baseline submission rate in the control group of 46%. Applicants were able to submit a FAFSA beginning on October 1 of their respective application year, so that FAFSA submission could have occurred prior to randomization assignment, but focusing on submissions that occurred after randomization produces a similar result. There is little difference in treatment effects between cohorts, although the baseline FAFSA submission rate was much higher in the first cohort relative to the second cohort (56% versus 37%), thus providing more evidence of weaker attachment to college for this group. Appendix Figure 1 displays estimated treatment effects on FAFSA submissions by weeks since randomization, which shows a small and statistically insignificant 1pp spike in submissions around three weeks after randomization before the control group submissions caught up over time.

E. Heterogeneous effects of treatment assignment

Table 5 examines heterogeneous impacts of treatment assignment by key subgroups in our experiment. For both substantive interest and due to an issue with the experimental design, we first test for differential effects by whether a student attended college in the prior year. The experiment's initial focus was on Fall enrollment, but early on we realized that our outreach coincided with the time period during which a number of students were planning to immediately enroll in college in the Spring term, and thus some students' initial conversations with the coach were less focused on motivating return to college than on preparation and solving short-term administrative barriers. We disaggregate students into two groups based on whether they were enrolled in the academic year during which outreach occurred. Overall, we find little difference in treatment effects on enrollment in the subsequent Fall between groups, though students in the first cohort who were not enrolled in college the prior year were a marginally significant 3.0 percentage points more likely to enroll, over a baseline of 12 percent.

The rest of Table 5 shows heterogeneous effects by whether a student initially received their Cal Grant at a CC or a CSU and by gender. Although there are some differences in point estimates, none are statistically significant at the 5 percent level. We find similar null results based on other characteristics, such as parental education or age at the time of the experiment. We also examined whether treatment effects differed based on a student's self-reported reason(s) for initial drop-out on the intake survey; estimates were quite noisy and not statistically distinguishable.

Appendix Table 7 shows the correlation between treatment group students' characteristics and engagement with their coach to provide insight into which students were most likely to interact with their coach. Students who engaged with their coach attended lower poverty high schools, initially enrolled in a CSU (versus a CC), and were more likely to have listed wanting a bachelor's degree on their original FAFSA. Most of the observable differences in engagement were driven by the first cohort, as the only strong predictor in the second cohort was student age, with older students being less likely to engage (results omitted for brevity).

6. Conclusion

We randomly assigned approximately 8,000 college dropouts from low- and middle-income families who indicated a desire to reenroll to receive one-on-one coaching. We find small, statistically insignificant treatment effects on college enrollment and FAFSA submission in the following academic year. We find similarly null results across most subgroups with one notable exception: students in the first cohort (who were randomly assigned shortly before the start of the COVID pandemic) who were not enrolled in the academic year in which random assignment occurred, who experience a marginally significant 3pp (25%, $p<0.05$) increase in enrollment in the subsequent Fall semester. While this specification was not part of our pre-analysis plan, it may provide suggestive evidence that the largest benefit may go to students whose attachment to higher education is more marginal who were able to engage with a coach before the cumulative negative effects of the pandemic took hold. (Control group students in the first cohort who were not enrolled the prior year only re-enrolled 12% of the time, compared to 67% for other students in that cohort). That said, this group of students represents less than 2% of the initial outreach sample, suggesting that unless very targeted, coaching reenrollment efforts are unlikely to be cost-effective.

Even among students who affirmatively opted-in to participate and were assigned to the treatment group, only half ever contacted or responded to outreach from their coach and fewer sustained continuous engagement. Although coaching may increase college re-enrollment when former students exhibit significant levels of engagement, we are unable to identify this in the context of our experiment given the relatively limited interactions between participants and coaches. Our findings suggest that better screening to identify individuals not currently enrolled may lead to more efficacious treatments, as these students experienced marginally positive enrollment increases. Yet our pre-screening approach seemed to identify students with relatively high rates of college re-enrollment relative to national populations (Causey et al. 2023). Having more engaged prospective students would lower the amount of time the InsideTrack coaches spent on administrative processes such as reaching out to students, which constituted a substantial portion of their time and effort, but in exchange they may be working with more motivated individuals who may need the coaching treatment less than their peers.

At the same time, conversations between coaches and prospective students identified a long list of challenges to transitioning back to college. Many of these students initially dropped out by simply stopping their class attendance, resulting in failing grades that often trigger processes that would limit their ability to access federal financial aid when they attempted to re-enroll.[19] As a result, they were often ineligible for federal financial aid, and many had financial holds remaining on their accounts that needed to be paid before re-enrolling. Anecdotally, re-establishing eligibility for federal aid was a common discussion topic for coaches.

Additionally, coaches reported that many students either did not know where they could go to their former institution to get guidance on issues of financial aid eligibility or other barriers to reentry, or had trouble receiving support from these administrative offices on campus. One responsibility of the coach was to be a constant presence reminding them of the need to follow-up with the college and figure out who there can help them address these concerns, as students frequently grew frustrated with this process or chose to avoid the issue. InsideTrack also noted that this project was a significant departure from their prior work in which they would develop a close connection working with an individual college along with that college's staff who were committed to helping students reintegrate. In this experiment, coaches worked with students who had attended community colleges and CSUs across the entire state, which involved a significant time commitment to investigate and help students understand procedures for each specific institution, with little personal connection between coach and a given college's administrative staff.

Additionally, coaching for the first cohort coincided with the beginning of the COVID-19 pandemic and initial lockdowns across California. Many treatment group members in both cohorts experienced major disruptions to their circumstances due to the pandemic (e.g., losing a

[19] Students who withdraw during the semester may be subject to Return of Title IV Fund requirements that require financial aid to be repaid to the federal government, and students with low GPA, potentially exacerbated by failing courses when they withdrew, may be subject to Satisfactory Academic Progress requirements that restrict federal student aid and some sources of state/institutional aid.

job or having to provide for family members that lost a job). InsideTrack coaches noted that compared to their prior experiences, during the pandemic, there was a general shift in their work toward supporting students' well-being and basic needs; approximately 38% of the actively engaged students were referred to InsideTrack's own internal Crisis Support Services due to issues such as food and housing insecurity or mental health concerns, compared to only 13% receiving referrals in prior years. This change in focus, although valuable, led to less emphasis on college reenrollment. Even though our experimental estimates are internally valid, the effect of coaching on college reentry in an unprecedented pandemic may be very different than effects under different conditions. Students in the second cohort were less likely to contact their coach, submit the FAFSA, and re-enroll, patterns consistent with the cumulative effects of the pandemic continuing to wear on prospective students even after vaccines were made available. Further, most students in the second cohort of the experimental sample had been contacted in the first year of outreach, had initially declined to participate, and thus may be different along unobservable dimensions compared to the first cohort of experimental sample members. Regardless, InsideTrack coaching had a long track record of providing coaching remotely (e.g., through texting, phone calls, email, and video chat), and thus coaches did not need to make the adjustment from in-person to remote service provision.

Although these initial estimates suggest that during a time characterized by economic and public health uncertainty, access to coaching did not increase college re-enrollment among former students, it is possible that longer-run outcomes such as degree completion could be affected. Additionally, it may still be the case that during less challenging circumstances, coaching would have been effective. We leave these important open questions to future work.

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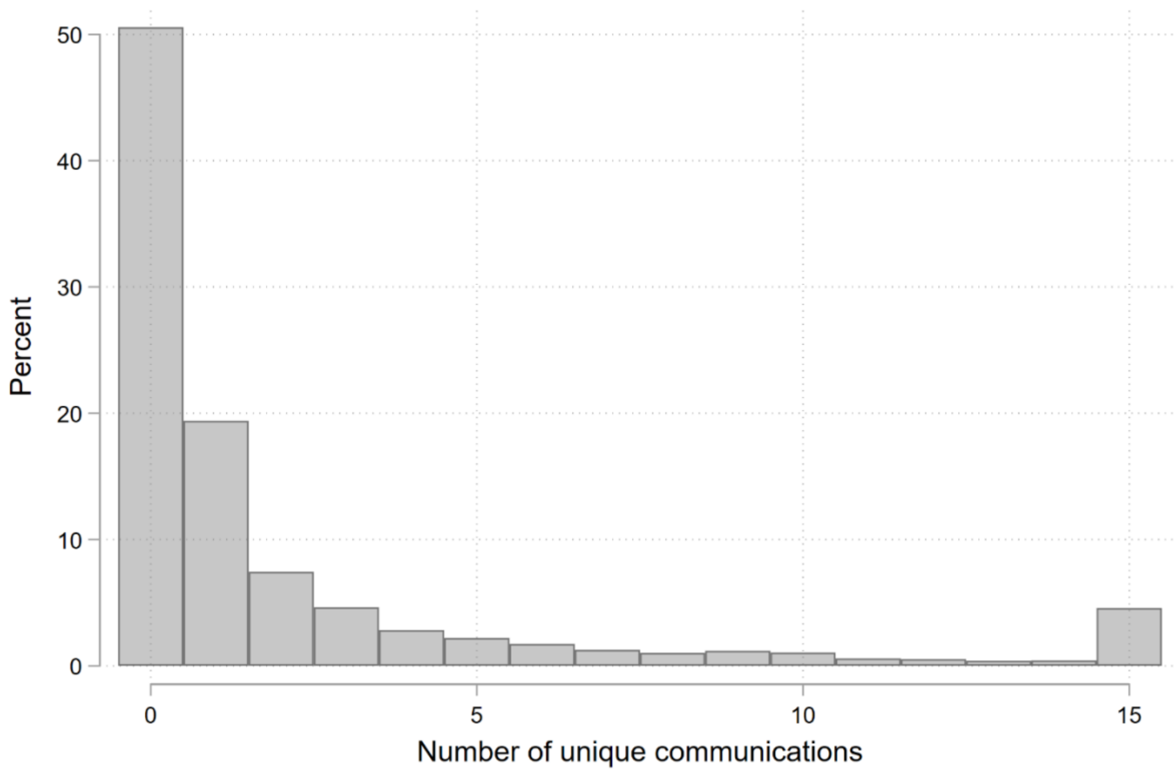
Figures and Tables

Figure 1. Histogram of total incoming communications



Notes. Histogram includes communications for all 4,076 treatment students in the experimental sample. InsideTrack's internal communications system identifies all incoming communications as one of three channels (text/SMS, email, or phone) but does not record the length of the communication, which could include multiple texts or emails. Coaches may agree to student requests for alternate formats, such as video calls, but coaches do not offer this format and must wait for students to initiate any alternate choice. Students with more than 15 distinct communications were top coded at 15.

Table 1: Sample characteristics and covariate balance

	All	First cohort	Second cohort
N	8040	4042	3998
Current age	-0.032	-0.032	-0.018
	(0.022)	(0.031)	(0.033)
Control group mean	23.168	23.000	23.339
Female	0.000	-0.009	0.008
	(0.012)	(0.017)	(0.016)
	0.613	0.621	0.606
College-educated parent	0.015	0.020+	0.010
	(0.009)	(0.012)	(0.014)
	0.213	0.218	0.209
GPA	0.001	-0.003	0.011
	(0.010)	(0.016)	(0.013)
	2.824	2.843	2.803
High school free and	0.011*	0.010	0.011+
reduced price lunch	(0.005)	(0.007)	(0.007)
	0.673	0.668	0.677
High school location			
Urban	-0.022*	-0.015	-0.027+
	(0.011)	(0.015)	(0.015)
	0.459	0.466	0.452
Suburban	0.014	0.008	0.019
	(0.010)	(0.013)	(0.015)
	0.368	0.385	0.349
Town/rural	-0.001	-0.001	-0.001
	(0.007)	(0.009)	(0.009)
	0.085	0.077	0.092

Notes: Point estimates from regression of characteristic on an indicator for assignment to the treatment group. Robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) are in parentheses; + p <0.1, * p <0.05. Control group means are below point estimates of treatment effects. Unless otherwise stated all values come from students' original FAFSA. High school values and GPA come from the Cal Grant one-page GPA verification form which was linked to the 2013-14 Common Core of Data. Regressions also include randomization block fixed effects (mutually exclusive groups defined by cohort, round of randomization, and year of first and last Cal Grant receipt).

Table 2. Impact on treatment assignment on communications between students and counselors

Level of communication	All students	First cohort	Second cohort	Test of equality (p-value)
N	8040	4042	3998	
Total communications	2.889**	3.298**	2.478**	0.015
	(0.170)	(0.278)	(0.186)	
Control group mean	0.025	0.047	0.002	
At least one communication	0.486**	0.530**	0.442**	<0.001
	(0.009)	(0.012)	(0.013)	
Control group mean	0.009	0.016	0.002	
At least two communications	0.298**	0.323**	0.274**	0.006
	(0.009)	(0.013)	(0.012)	
Control group mean	0.002	0.004	0.000	

Notes: Point estimates from a regression of the level of communication outcome on assignment to treatment. Robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) are parentheses; ** p <0.01. Sample includes all students in the experiment (N = 8,040). Regressions also include randomization block fixed effects (mutually exclusive groups defined by cohort, round of randomization, and year of first and last Cal Grant receipt) and pre-registered covariates (indicators for female and having a college-educated parent, zip code level median household income, high school percent free & reduced-price lunch, high school urbanicity dummies (urban, suburban, town, rural), and dummies for students with missing values). The last column of the table shows p-values from a test of hypothesis of equal treatment effects for cohorts 1 and 2.

Table 3. Intent-to-treat estimates of the offer of coaching on postsecondary enrollment

		(1)	(2)	(3)
		Fall enrollment in:		
	N	Any sector	CC	Non-CC
All students	8040	0	-0.006	0.008
		(0.009)	(0.009)	(0.006)
Control group mean		0.331	0.234	0.101
First cohort	4042	0.013	0.007	0.011
		(0.013)	(0.014)	(0.011)
Control group mean		0.418	0.278	0.147
Second cohort	3998	-0.013	-0.019+	0.005
		(0.012)	(0.010)	(0.007)
Control group mean		0.241	0.190	0.053
Test of equality (*p*-value)		0.156	0.148	0.646

Notes: Point estimates from a regression of enrollment in the specified sector on assignment to treatment. Robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) are in parentheses; + p <0.1. Regressions also include randomization block fixed effects (mutually exclusive groups defined by cohort, round of randomization, and year of first and last Cal Grant receipt) and pre-registered covariates (indicators for female and having a college-educated parent, zip code level median household income, high school percent free & reduced-price lunch, high school urbanicity dummies (urban, suburban, town, rural), and dummies for students with missing values). The bottom row of the table shows p-values from a test of hypothesis of equal treatment effects for cohorts 1 and 2. Enrollment outcomes are measured using NSC data.

Table 4. Intent-to-test estimates of the offer of coaching on FAFSA submissions

FAFSA submission timing	All students	First cohort	Second cohort	Test of equality (*p*-value)
N	8040	4042	3998	
Any time during award year	0.004	0.013	-0.005	0.387
	(0.010)	(0.016)	(0.014)	
Control group mean	0.461	0.556	0.365	
Post-randomization	0.003	0.001	0.006	0.769
	(0.010)	(0.015)	(0.013)	
Control group mean	0.301	0.328	0.273	

Notes: Point estimates from a regression of the probability of submitting a FAFSA any time during the academic year or any time after random assignment on assignment to treatment. Robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) are in parentheses. Regressions also include randomization block fixed effects (mutually exclusive groups defined by cohort, round of randomization, and year of first and last Cal Grant receipt) and pre-registered covariates (indicators for female and having a college-educated parent, zip code level median household income, high school percent free & reduced-price lunch, high school urbanicity dummies (urban, suburban, town, rural), and dummies for students with missing values). The last column of the table shows *p*-values from a test of hypothesis of equal treatment effects for cohorts 1 and 2.

Table 5. Heterogeneity in intent-to-test estimates of the offer of coaching on postsecondary enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
Dimension of heterogeneity:	A) Enrolled in prior academic year		B) Initial college where student received Cal Grant		C) Gender	
	No	Yes	CC	CSU	Female	Male
All students	0.008	-0.003	0.005	-0.012	-0.019+	0.026+
	(0.009)	(0.017)	(0.010)	(0.018)	(0.011)	(0.015)
Control group mean	0.115	0.619	0.337	0.315	0.354	0.294
N	4654	3386	5632	2408	4929	3111
First cohort	0.030+	-0.001	0.020	-0.007	-0.008	0.035
	(0.016)	(0.021)	(0.016)	(0.023)	(0.016)	(0.025)
Control group mean	0.121	0.671	0.421	0.412	0.442	0.379
N	1856	2186	2876	1166	2491	1551
Second cohort	-0.007	-0.005	-0.011	-0.018	-0.029+	0.017
	(0.011)	(0.031)	(0.013)	(0.027)	(0.016)	(0.016)
Control group mean	0.111	0.528	0.249	0.223	0.261	0.211
N	2798	1200	2756	1242	2438	1560
Test of equality (*p*-value)	0.056	0.915	0.148	0.766	0.341	0.559

Notes: Point estimates from a regression of the probability of submitting a FAFSA any time during the academic year or any time after random assignment on assignment to treatment. Robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) are in parentheses; + p <0.1. Regressions also include randomization block fixed effects (mutually exclusive groups defined by cohort, round of randomization, and year of first and last Cal Grant receipt) and pre-registered covariates (indicators for female and having a college-educated parent, zip code level median household income, high school percent free & reduced-price lunch, high school urbanicity dummies (urban, suburban, town, rural), and dummies for students with missing values). The last column of the table shows p-values from a test of hypothesis of equal treatment effects for cohorts 1 and 2. Enrollment is measured using NSC data. Initial college of Cal Grant receipt is measured using CSAC data.