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Experimental Estimates of College Coaching on Postsecondary Re-enrollment

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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.

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Experimental Estimates of College Coaching on Postsecondary Re-enrollment*

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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.

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1. Introduction

College enrollment has increased significantly over the last few decades, yet fewer than half of all adults ultimately obtain a postsecondary degree (Ryan & Bauman, 2016). Among individuals who enroll in college fewer than 61 percent have graduated eight years later (Shapiro et al., 2019). A robust literature provides evidence that college completion substantially increases in employment and lifetime earnings (Barrow & Malamud, 2015; Bhuller, Mogstad, & Salvanes, 2017; Oreopoulos & Petronijevic, 2013). Furthermore, given that student loan repayment difficulties are disproportionately experienced by students who leave college before receiving a degree, helping students cross the college finish line could address concerns around students' growing reliance on federal loans (Looney & Yannelis, 2015). Completion and loan repayment are particularly relevant issues for low-income and first-generation college students, who enter college with fewer personal resources and often attend less selective institutions where available supports are stretched thin. Many studies have shown 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 reenrollment and completion outcomes for non-traditional students, in this case, those who have attempted but dropped out of college. Better interventions to help students re-engage in

[1] See, for example, Bettinger and Baker (2014); Bettinger, Gurantz, Kawano, Sacerdote, and Stevens (2019); Denning, Marx, and Turner (2019); Page, Kehoe, Castleman, and Sahadewo (2017); and Weiss, Ratledge, Sommo, and Gupta (2019).

postsecondary education are needed, as studies on workforce retraining, including aid offered without counseling, have generally found limited impacts (Barnow & Smith, 2015; Gurantz, 2022; McCall, Smith, & Wunsch, 2016).[2] Many of these disconnected students experience challenges and hardships that could be addressed with existing resources and support, but students are often unaware of or unable to access them. Furthermore, once a student is out of college and in a new routine, they may need encouragement and strategic support in order to return (Ortagus & Perrault, 2019).

We conduct 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 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. Emails and text messages were sent to these students that informed them of an opportunity to receive

[2] Similarly, Evans, Kearney, Perry, and Sullivan (2019) find that emergency assistance grants are only effective at increasing community college student attainment when paired with additional supports.

coaching to help them re-assess their interest in returning to college, and students who responded they had not yet earned a degree were able to 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 chose to affirmatively opt 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 the same 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.

We find that roughly half of all students assigned to the treatment group engaged with their college coach at least once, but no evidence that treatment assignment increased college enrollment. Intent to treat estimates of on immediate college enrollment were small – generally less than one percentage point – and statistically insignificant, and we find no strong evidence of heterogeneous treatment effects by student race, parental education, gender, or whether a student initially enrolled in a two- versus four-year institution. We also find no impacts on alternate outcomes, such as FAFSA submission or persistence rates.

Although our experimental results are internally valid, it is important to note the context that students in the first cohort began coaching right as COVID began to impact the nation in March 2020, and those in the second cohort began coaching during the first large wave of COVID during Winter 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 percent and 34 percent of students at four-year and two-year colleges do so within 150 percent of the "normal time" to degree (6 years and 3 years, respectively). Persistence rates are notably lower among community college entrants, older students, and Black and Hispanic students.[3] In 2019, over 36 million U.S. adults belonged to the category of having "some college, no degree" (National Student Clearinghouse, 2019), a group that faces worse employment and earnings outcomes than associate and bachelor's degree recipients (Bird, Castleman, Fischer, & Skinner, 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 (Charles, Hurst, & Notowidigdo, 2018; Crosta, 2014; DesJardins, Ahlburg, & McCall, 2006; Goldrick-Rab, 2006).

[3] According to the National Student Clearinghouse (2020), among degree-seeking students who first entered college in 2018, 76 percent returned to a higher education institution in the following year. Only 47 percent of older (25+ years old) students and 62 percent of community college entrants returned for a second year of higher education. Across all sectors, persistence rates for Black and Hispanic students were 66 and 72 percent, respectively.

Students exit college for a myriad of reasons, including financial constraints, academic difficulties, or lack of focus or interest in their degree program. Enrolling in college provides students with necessary information to help them weigh the costs and benefits of proceeding, and for some dropping out can be an optimal decision if they assess that their individual returns are likely to be too low or if the costs of completing are too high, relative to their initial expectations.[4] Students are also susceptible to various shocks—for instance to their employment, income, or changes in college prices—that can lead students to leave college even when their expected returns are high (e.g., Ortagus, Skinner, and Tanner (2021)). Many dropout decisions may be inconsistent with standard models of educational investment, and an extensive literature suggests that students' decisions are affected by complexity, present bias, framing, and other behavioral factors (Lavecchia, Liu, & Oreopoulos, 2016).

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 (Broton, Goldrick-Rab, & Benson, 2016; Darolia, 2014). However, financial aid alone may be ineffective without more intensive individualized support, especially for community college students (Anderson & Goldrick-Rab, 2018; Carruthers & Ozek, 2016). A number of college

[4] See, for example, Manski (1989); Altonji (1993); and Stinebrickner and Stinebrickner (2012).

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.[5] 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 (Bergman, Denning, & Manoli, 2019; Bettinger, Castleman, Choe, & Mabel, 2022; Bird et al., 2021; Clotfelter, Hemelt, & Ladd, 2018; Gurantz et al., 2021; Oreopoulos & Petronijevic, 2018), and unlikely to change the decisions of older, non-traditional students who have already been unsuccessful in the college environment.

There is relatively 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 return and earn their degree. In partnership with several Florida community colleges, Ortagus, Tanner, and Isaac McFarlin (2020) implemented a randomized control trial targeting community college dropouts who were previously academically successful. Treatment arms included a text messaging campaign providing information about the reenrollment process and text messaging paired with a one-course tuition waiver. Students who received

[5] These studies include (Barr & Castleman, 2018; Bettinger & Evans, 2019; Carrell & Sacerdote, 2017; Castleman & Goodman, 2018; Gurantz, Pender, Mabel, Larson, & Bettinger, 2020; Oreopoulos & Ford, 2019; Page et al., 2017; Phillips & Reber, 2019). Similarly, in-college mentoring has been shown to reduce the risk of dropout and, in some cases, increase degree completion (Evans, Kearney, Perry, & Sullivan, 2020; Oreopoulos & Petronijevic, 2018).

information and the tuition waiver were significantly more likely to reenroll (1.5pp or 21 percent), but effects for the text message only group were small and insignificant. In another recent experiment, Barr, Bird, Castleman, and Skimmyhorn (2022) ran on experiment with 13,000 veterans who were separating from the military in 2016 and 2017, and provided them with text-based personalized information, reminders, and/or advising about their college and university options, but find no results on subsequent college enrollment or quality. Although existing evidence points to positive effects of InsideTrack mentoring on attainment among existing students (Bettinger & Baker, 2014), the challenges faced by students who have left college are likely different and more extensive.

3. Experimental Setup

A. Data

We draw our sample from data provided by the California Student Aid Commission (CSAC), which 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 known as the "Entitlement" award, and high school graduates apply by submitting the Free Application for Federal Student Aid (FAFSA) and having their school submit a one-page GPA verification form by March 2nd.[6] Students are offered the Cal Grant 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. 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.[7]

CSAC data include information from the 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 institution in which they were previously enrolled and the years in which a student received payments.

[6] 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.

[7] Tuition in CSU and UC institutions was $5,742 and $12,570 per year in 2018-19, respectively. An alternate program, the Board of Governor fee waiver, recently renamed as the California Promise Grant, provides full community college tuition to low-income students who receive government assistance or belong to families with income below 150 percent of the federal poverty guideline.

As per the pre-registration plan, our primary outcome measure is re-enrollment in a postsecondary institution within one year of treatment assignment.[8] 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, & Hyman, 2015). We also use 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.[9] Both data sources provide similar results and lead us to the same conclusions, and unless otherwise noted all results are based on NSC data.

We are also interested in understanding intermediate steps that indicate an interest in college enrollment, such as whether treated students were more likely to submit the FAFSA and/or secure financial aid as part of college re-entry. We rely on CSAC data to

[8] 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 after the summer of 2023 and 2024 for the two treated cohorts. Given the null results on initial attendance and persistence, we do not anticipate substantial treatment effects on degree completion.

[9] 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 on February 16, 2022, and received on March 17, 2022, with the files having a timestamp of March 14, 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).

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 California State University (CSU) or community college (CC) but left before earning a degree. Although we do not observe college completion data for Cal Grant recipients, the available data suggest that many struggle to finish their studies. Among those who received a Cal Grant at a CSU, only 60 percent receive aid for four years, and among the community college population the four-year persistence rate was an even lower 20 percent. 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 percent for recent cohorts, similar to the national average.[10]

In the first year, the pool of former students eligible to participate in the intervention was approximately 130,000 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 1) in January and February 2020. Recruitment emails were sent from an official CSAC email address to garner trust and

[10] Author's calculations using IPEDS data on 150% completion rates in two-year colleges.

included a link to an official CSAC website explaining the project for those who may have had concerns.[11]

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.

[11] https://www.csac.ca.gov/researchinside-track

C. College coaching treatment condition

As we recruited students over a multi-month period, students were assigned to treatment on a rolling basis (discussed below). Students assigned to treatment were offered the opportunity to receive coaching and counseling services through a partnership with InsideTrack. InsideTrack has engaged in student re-entry work since 2007, partnering with large state systems and institutions.

Students assigned to the treatment group were invited to work with an InsideTrack coach. 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 to three coaches continuously working with students in the first and second years of the experiment. 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. Only about half of all treatment group members had any interactions with their assigned coach, and about one-third communicated two or more times (see Table 2 and Appendix Table 2). Students' preferred method of communication was text, with about 81 percent of interactions occurring in this format.

Interactions between InsideTrack coaches and treatment group members focused on creating and advancing a student's reenrollment plan, as well as 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 1), the two most common (not mutually exclusive) reasons provided for a respondent's dropout decision were "work became my main priority" (64 percent) and "needed to leave temporarily to take care of a family member, or fulfill another short-term commitment" (45 percent), and 81 percent of students who responded listed at least one of these two categories. In descending order, the remaining responses included college expenses (33 percent), failing to meet important administrative deadlines (31 percent), difficulty of coursework (26 percent), and not feeling like part of the community (22 percent).[12] 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-

[12] Students were not required to list a reason for not earning their degree on the intake survey. Overall, 90.7% listed at least one reason for leaving, with 88.4% and 93.0% doing so in the first and second cohorts. Students who did not list a pre-specified reason either left the field blank or listed a number of their own reasons (e.g., mental health, pregnancy, lack of motivation, unable to decide on an area of focus).

term commitment", which was 51% in the first cohort and dropped to 39% in the second cohort.

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 are offered InsideTrack re-entry counseling versus outcomes for students who are not. To identify these effects, we estimate ordinary least squares (OLS) models of the following form:

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

Y_{irs} is the outcome of interest for individual i in randomization round r and within strata s, and T_{irs} is a binary variable equal to 1 if the individual was assigned to the treatment group. In order 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 with each student assigned to a group that identified: (1) whether they first attended a community college or a CSU, as 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}).[13]

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 have been duplicated using (1) strata fixed effects with no covariates and (2) including additional covariates that became available, and produce similar results.[14] Standard errors are clustered by randomization strata (Chaisemartin & Ramirez-Cuellar, 2020; Deeb & Chaisemartin, 2021).

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[13] The exact randomization dates are provided in Appendix Table 1. There were relatively few opt-in students who 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, though separately for CSU and community college students.

[14] This covariate list was detailed in our pre-registration report. In the case of missing covariates, the variable was coded as a zero and we include a dummy variable indicating the value is missing as an additional control. In the course of the project, we were able to add additional variables including high school GPA from the Cal Grant application, along with age, family size, and degree objective (bachelor's, associate, or other/missing) from the FAFSA. Finally, CSAC recently engaged in a data match via names and birthdates with the California Department of Education, which allowed us to include student ethnicity

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 enrollment (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. 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 identified as "dropouts" and opted into the experiment but then 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 the experimental sample and they are included in the results. We perform an additional analysis below that uses enrollment data to disaggregate the opt-in sample into those who were enrolled in college in 2019-20 versus those who were not enrolled; 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 (of four total)

as reported in high school; these data begin in 2015 so are missing for the entire 2014 cohort, but only 8 percent of subsequent students did not match to the high school data.

were accidentally included again in the third experimental round. This occurred as they had filled out the opt-in survey multiple times and were not appropriately screened out. After discovering this issue, we continued to assign these students their initial treatment assignment as of the second experimental round, though this led some students to have the wrong treatment assignment (i.e., some control group students in the third round received coaching because they were assigned treatment in round two, some treatment group students in the third round did not receive coaching because they were assigned control in round two). 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. (Table 2 shows that 1.6 percent of the 2,003 control students in the first year engaged in outreach to a coach, and 0.4 percent engaged two or more times, rather than 0 percent as might be expected if this mistake had not occurred).

5. Effects of access to coaching and reentry support

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

Table 1 displays descriptive statistics for our sample and tests of the equivalence of baseline characteristics between treatment and control students; control group means are located under these estimates. There were 8,040 opt-ins with 4,076 students assigned to treatment 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 percent were female, 21 percent reported having a college-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 percent of the student body received free or reduced-price lunch, with 46 percent of high schools in urban settings, 37 percent in suburban settings, and 9 percent in town or rural settings (7 percent missing). The average high school GPA was 2.82, consistent with the characteristics of high school students who would attend less selective community colleges or the broad-access California State University system. Among students with ethnicity data, 68 percent of students were considered Hispanic, with relatively equal proportions of Asian, Black, White, or missing race/ethnicity (~6-8 percent each). We also note relatively small differences in the composition of opt-in students between the two years of the experiment, even though the onset of the Coronavirus occurred in March 2020. The first cohort had already been recruited and assigned treatment by February 2020, though the second cohort opted-in during the following academic year.

B. Effects on 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.[15] 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 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. To measure more sustained contact, we find that treatment assignment increased the likelihood of having two or more contacts by 30pp (baseline = 0.2pp). We find lower levels of incoming communications in the second year of the experiment, as students assigned to treatment were nine percentage points less likely to reach out to their coach (53 percent versus 44 percent), and five percentage points less likely to have two or more communications (32 percent versus 27 percent). Appendix Table 2 shows contact rates by mode of communication, with about 81 percent of total communications coming from text

[15] Noted in the data section above, a small group of participants in just the first year of the experiment were first assigned to the control group and then accidentally assigned to the treatment group in a subsequent round.

messages rather than phone or email contacts. Even so, of the students who engaged with their coach about 36 percent had at least one phone call.[16]

As about half of students assigned to treatment ultimately did not engage with their coach, 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 coach, they reached out roughly six distinct times on average. This average value 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. Most point estimates are below one percentage point and estimates from our main specification based on NSC enrollment data have 95 percent confidence intervals that exclude treatment assignment increasing enrollment by greater than two percentage points.

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 a 0pp

[16] Treatment effects on having at least one contact were 48.6 percent on total and 17.6 percent on having at least one phone call, with 17.6/48.6 = 36 percent.

change in enrollment, over a baseline of 33.1 percent. Disaggregating by sector, enrollment in California community colleges declined by 0.6pp and enrollment in other sectors increased by 0.8pp.[17] Results using the complementary CSAC enrollment data are shown in Appendix Table 3 and produce similar results, with a -0.1pp decline in enrollment over a baseline of 35.7 percent. In the CSAC enrollment data, community college enrollment declines by 0.3pp and CSU/UC enrollment increases by 0.8pp, which is marginally significant at $p<0.10$.[18]

We do find observable differences in treatment effects when comparing the first cohort to the second cohort, though the magnitude and statistical significance of the difference between these cohorts varies 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 3)—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 ($p = 0.02$).

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

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. Baseline enrollment differs substantially between the two cohorts—the first cohort's Fall semester enrollment was a bit over 40 percent for the control group compared to only 24 percent for the second cohort. This suggests that even within our analysis sample of former students who expressed interest in returning to college, fewer students in the second cohort ended up following through on their intention to return. The differences in control group enrollment also correspond to differences in coaching engagement rates for the two cohorts noted above, where students in the second cohort who were assigned to receiving college coaching were less likely to later engage with their coach. This may be a consequence of COVID, as the cumulative effects of the pandemic may have taken a toll on prospective students. Yet another difference between the cohorts is that we slightly changed our intake process in the 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 4 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.[19] We examine two persistence outcomes – enrollment in the second year or enrollment 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 the first cohort and marginally significant decreases in the second cohort, and the difference between cohorts is statistically significant ($p < 0.05$) in both cases.

Coaches may have also helped students prepare for college by getting them to submit financial aid forms or other documents, but the students ultimately did not follow through on their intentions to enroll. We do not find evidence that this is the case, as Table 4 shows null impacts of treatment assignment on FAFSA submissions. 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 percent. 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

[19] Given the strong similarities between NSC and CSAC enrollment data, we do not expect second year persistence results to change once the NSC data is made available.

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.[20]

E. Heterogeneous effects of treatment assignment

Table 5 examines heterogeneous impacts of treatment assignment by key subgroups in our experiment.[21] For both substantive interest and due to an issue with the experimental design, a primary focus is on differential effects by whether a student attended college the prior year, which is shown in the first two columns. The experiment's initial focus was on Fall enrollment but early on we realized that our outreach coincided with the time period where 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 attendance but on preparation and solving short-term administrative barriers. Thus, we disaggregate students into two groups based on whether they were enrolled in the year that outreach occurred, to examine which type of students was more likely to be impacted. Overall, we find little difference 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.

[20] Similar analysis found no impacts on Cal Grant receipt, but we omit these results for brevity.
[21] Table 5 uses NSC data but results using CSAC enrollment data are similar.

The rest of Table 5 shows heterogeneous effects by whether a student initially received their Cal Grant at a CC or a CSU, by gender, and by high school identified ethnicity. Although there are some differences in point estimates, no results are statistically significant at the 5 percent level. Though not shown, we find similar null results based on other characteristics, such as parental education or age at the time of the experiment.

F. Description of treatment students who interacted with their coach

Table 6 provides some additional descriptive information about our treatment sample to understand which types of students participated with their coach. We restrict to only students assigned to treatment and estimate OLS regressions of an indicator for whether a student ever communicated with a coach on a variety of baseline characteristics. As shown in Table 2, only half of treatment students ever engaged with their coach after being informed of their treatment status. We find a few key differences between students who engaged with their coach and those that didn't, namely that engaged students were younger (each additional year was associated with a 12 percentage point decline in engagement), were more likely to have listed wanting a bachelor's degree on their original FAFSA (5 percentage point increase), and initially attended a CSU over a CC (5 percentage point increase). Asian students and those who attended lower poverty schools were also somewhat more likely to engage their coach. Somewhat surprisingly, being enrolled in college in the year we engaged in outreach was associated with a 1.9 percentage point increase in engagement, but this result was not statistically significant. Finally, most of the observable differences between treated students who engaged and those who did not are driven by the first cohort, where we had higher levels of overall engagement (results omitted for brevity). The only strong predictor of engagement in the second cohort suggests that older students were less likely to engage with their coach.

6. Conclusions and future work

We randomly assigned approximately 8,000 college dropouts from low- and middle-income families to receive one-on-one coaching. We find small, statistically insignificant treatment effects on college enrollment and FAFSA submission in the following academic year. Even among students who affirmatively opted-in to participate and were assigned to treatment, 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.

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.[22]

[22] 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

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.

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 where 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 the 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.,

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.

losing a 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 percent 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 percent receiving referrals in prior years. This change in focus, although valuable, led to less emphasis on college enrollment as students worked through a variety of issues. Even though these randomized control trial estimates are internally valid, the effect of coaching on college reentry in an unprecedented pandemic may be very different from 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. That being said, InsideTrack coaching was always intended to be provided 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.

Table 1: Sample characteristics and covariate balance

	All students	First cohort	Second cohort
Number of students	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 reduced price lunch	0.011*	0.010	0.011+
	(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
High school ethnicity			
African-American	-0.003	0.000	-0.006
	(0.006)	(0.006)	(0.009)
	0.065	0.051	0.079
Asian	0.008	0.016+	0.001
	(0.006)	(0.009)	(0.006)
	0.061	0.071	0.051
Hispanic	-0.001	-0.012	0.010
	(0.013)	(0.019)	(0.017)
	0.678	0.679	0.677
White	-0.011+	-0.003	-0.020*
	(0.007)	(0.009)	(0.009)
	0.077	0.073	0.082

Notes: Point estimates and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses from regression of characteristic on an indicator for assignment to the treatment group; + $p < 0.1$, * $p < 0.05$. 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. Ethnicity values come from a match between CSAC and the California Department of Education that identified student ethnicity only for 2015 and beyond (students whose application was in 2014 had missing 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 and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses from a regression of the level of communication outcome on assignment to treatment; ** $p < 0.01$. 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 and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses from a regression of enrollment in the specified sector on assignment to treatment; + $p < 0.1$. 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 and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses 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. 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

Dimension of heterogeneity:	(1) A) Enrolled in prior academic year	(2)	(3) B) Initial college where student received Cal Grant	(4)	(5) C) Gender	(6)	(7) D) Ethnicity	(8)
	No	Yes	CC	CSU	Female	Male	Hispanic	Non-hispanic
All students	0.008	-0.003	0.005	-0.012	-0.019+	0.026+	-0.013	0.015
	(0.009)	(0.017)	(0.010)	(0.018)	(0.011)	(0.015)	(0.012)	(0.021)
Control group mean	0.115	0.619	0.337	0.315	0.354	0.294	0.33	0.345
N	4654	3386	5632	2408	4929	3111	4682	2231
First cohort	0.030+	-0.001	0.020	-0.007	-0.008	0.035	-0.017	0.064+
	(0.016)	(0.021)	(0.016)	(0.023)	(0.016)	(0.025)	(0.016)	(0.035)
Control group mean	0.121	0.671	0.421	0.412	0.442	0.379	0.418	0.45
N	1856	2186	2876	1166	2491	1551	2252	1097
Second cohort	-0.007	-0.005	-0.011	-0.018	-0.029+	0.017	-0.010	-0.031
	(0.011)	(0.031)	(0.013)	(0.027)	(0.016)	(0.016)	(0.017)	(0.026)
Control group mean	0.111	0.528	0.249	0.223	0.261	0.211	0.246	0.246
N	2798	1200	2756	1242	2438	1560	2430	1134
Test of equality (p-value)	0.056	0.915	0.148	0.766	0.341	0.559	0.766	0.029

Notes: Point estimates and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses 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; + $p < 0.1$. 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. Ethnicity is derived from a match between CSAC and the California Department of Education and excludes students whose first Cal Grant year was 2014.

Table 6. Differences in background characteristics of participants assigned to the treatment group by coaching participation

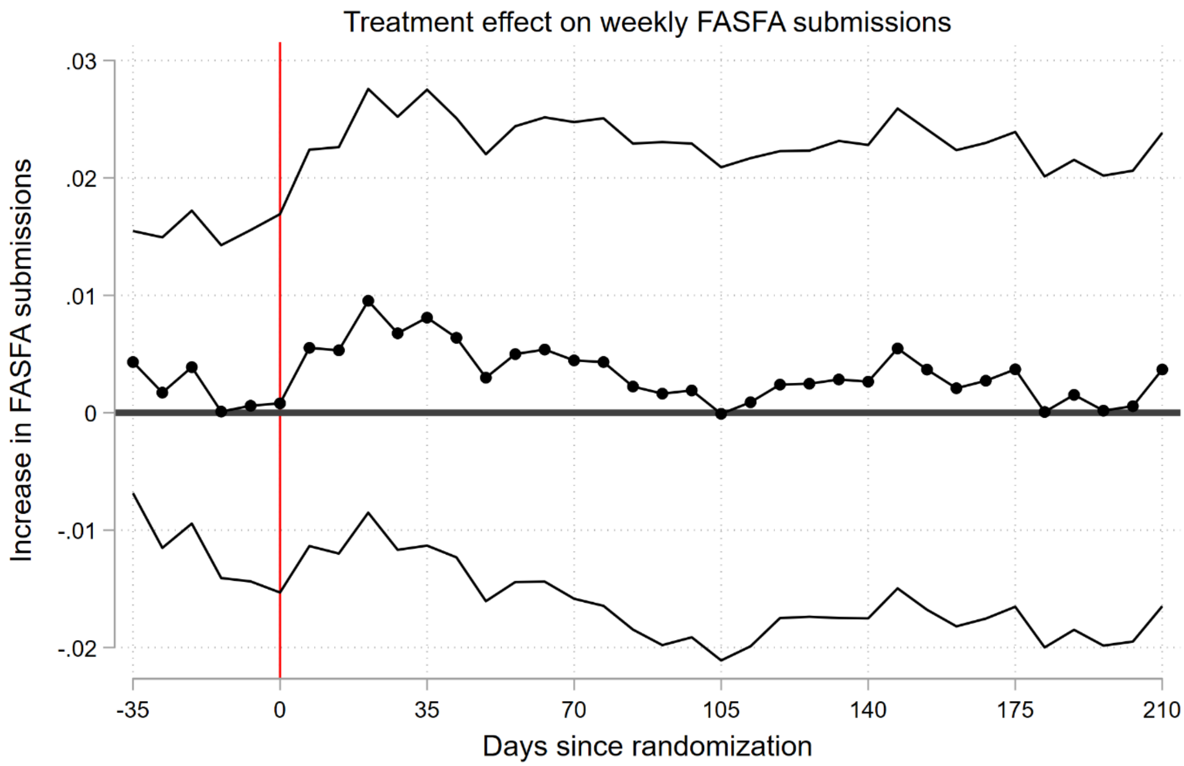
						High school urbanicity			
Pre-registered covariates	Female	College-educated parent	Zip code median HH income	Percent FRPL at high school	Missing FRPL	Urban	Suburban	Town	Rural
Had communication	-0.024	-0.015	298.779	-0.020**	0.013	-0.011	0.001	-0.008	0.005
	(0.015)	(0.013)	(666.905)	(0.007)	(0.009)	(0.016)	(0.015)	(0.006)	(0.006)
Control group mean	0.625	0.236	60,329	0.693	0.091	0.443	0.381	0.047	0.038

Additional covariates	GPA	Missing GPA	Current age	Family size	Goal: Bachelor's degree	Hispanic	Asian	Black	White
Had communication	0.028+	-0.006	-0.121*	0.023	0.049**	-0.011	0.022*	-0.011	0.005
	(0.015)	(0.007)	(0.047)	(0.045)	(0.016)	(0.016)	(0.009)	(0.008)	(0.008)
Control group mean	2.814	0.053	23.204	3.912	0.508	0.682	0.058	0.068	0.064

	Sector of initial Cal Grant payment		Enrolled during outreach		
	CC	CSU	Any college	CC	CSU
Had communication	-0.048**	0.048**	0.019	0.010	0.013
	(0.014)	(0.014)	(0.015)	(0.015)	(0.009)
Control group mean	0.721	0.279	0.405	0.335	0.087

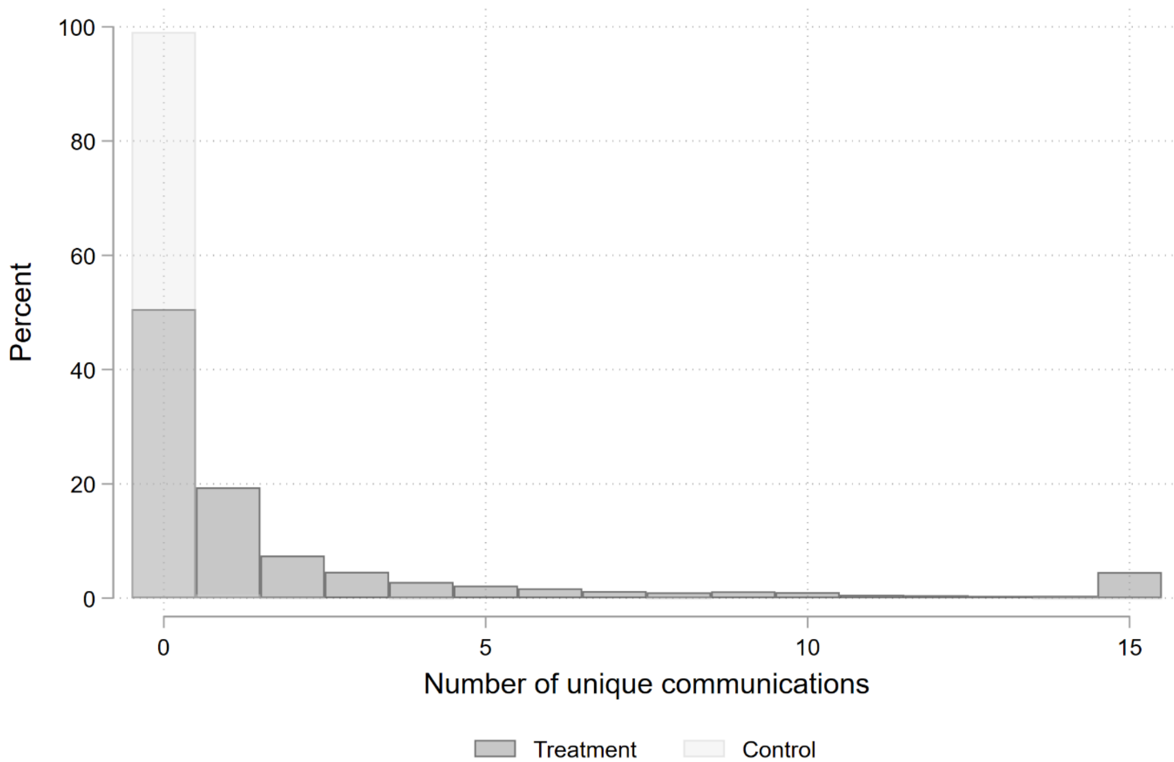
Notes: Point estimates and robust standard errors in parentheses from a regression of the listed characteristic on an indicator for having any contact with a coach; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Regression sample is limited to participants assigned to the treatment group (N = 4,076). Unless otherwise stated all values are from the student's first FAFSA submission. High school free and reduced-price lunch (FRPL) percentage and urbanicity are from the Common Core of Data linked to each student's high school identifier from their original Cal Grant application. Ethnicity is derived from a match between CSAC and the California Department of Education but excludes students whose first Cal Grant year was 2014. "Enrolled during outreach" refers to whether NSC data shows the student was enrolled in the year in which outreach was occurring.

Figure 1. Regression estimates of FAFSA submission, by week



Notes. Point estimates and 95% confidence intervals from a separate regressions of treatment status on the likelihood of completing a FAFSA by the specific date. See text for additional control variables. Regressions measure weekly treatment effects that span the period from five weeks before treatment assignment to 30 weeks after treatment assignment. Standard errors clustered by randomization strata.

Figure 2. Histogram of total incoming communications



Notes. 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. 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. A handful of control group students were mistakenly re-assigned to the treatment sample (as described in the text), which accounts for why control group means are not completely null. Students with more than 15 distinct communications were top coded at 15.

Appendix Table 1. Randomization timing and contact rates

Randomization date	Total	Treatment	Control	Contact rate
First cohort				
January 16, 2020	1735	871	864	56%
January 24, 2020	641	325	316	56%
February 11, 2020	739	374	365	56%
February 21, 2020	927	469	458	49%
Second cohort				
November 2, 2020	1025	519	506	53%
November 6, 2020	915	462	453	42%
November 13, 2020	213	111	102	40%
November 20, 2020	519	264	255	48%
December 6, 2020	176	91	85	43%
December 15, 2020	492	250	242	39%
January 13, 2021	168	88	80	39%
January 21, 2021	264	137	127	39%
February 5, 2021	226	115	111	36%

Notes. Each row identifies the number of students who opted in to the experiment and the date of their randomization by the authors. Contact rates identify the percent of treated students who were recorded by InsideTrack software as contacting the coach via text, email, or phone.

Appendix Table 2: Intent-to-treat estimates of the offer of coaching on communications with coaches

	(1)	(2)	(3)	(4)
	Any format	Text	Email	Phone
Total communications	2.889**	2.344**	0.171**	0.336**
	(0.170)	(0.157)	(0.015)	(0.021)
Control group mean	0.025	0.015	0.002	0.007
Had at least one communication	0.486**	0.389**	0.086**	0.176**
	(0.009)	(0.009)	(0.006)	(0.007)
Control group mean	0.009	0.004	0.001	0.006
Had at least two communications	0.298**	0.234**	0.036**	0.068**
	(0.009)	(0.008)	(0.003)	(0.005)
Control group mean	0.002	0.001	0.000	0.001

Notes: Point estimates and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses from a regression of communications between participants and coaches on assignment to treatment; ** $p < 0.01$. InsideTrack records all incoming communications between coach and students according to one of three formats: text/SMS; emails; and phone calls. 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 alternative. A small number of control group students were mistakenly re-assigned to the treatment sample (as described in the text), which accounts for why control group means are not completely null. 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).

Appendix Table 3. Intent-to-treat estimates of the offer of coaching on postsecondary enrollment (CSAC data)

		(1)	(2)	(3)
		Fall enrollment in:		
	N	Any CA public institution	CC	CSU/UC
All students	8040	-0.001	-0.003	0.008+
		(0.009)	(0.010)	(0.005)
Control group mean		0.357	0.297	0.067
First cohort	4042	0.020	0.015	0.014+
		(0.013)	(0.016)	(0.008)
Control group mean		0.439	0.350	0.102
Second cohort	3998	-0.022+	-0.020+	0.001
		(0.011)	(0.011)	(0.005)
Control group mean		0.273	0.242	0.032
Test of equality (p-value)		0.018	0.067	0.184

Notes: Point estimates and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses from a regression of enrollment in the specified sector on assignment to treatment; + 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 CSAC data.

Appendix Table 4. Intent-to-treat estimates of the offer of coaching on longer-run postsecondary enrollment (CSAC data)

		(1)	(2)	(3)	(4)	(5)	(6)
		Enrollment in Fall of year 2			Enrollment in Fall of year 1 and year 2		
	N	Any CA public	CC	CSU/UC	Any CA public	CC	Other
All students	8040	-0.001	-0.002	0.004	0.005	0.002	0.009*
		(0.009)	(0.009)	(0.006)	(0.008)	(0.007)	(0.004)
Control group mean		0.295	0.231	0.072	0.209	0.153	0.04
First cohort	4042	0.017	0.015	0.006	0.024+	0.017	0.016**
		(0.014)	(0.013)	(0.010)	(0.013)	(0.012)	(0.006)
Control group mean		0.357	0.261	0.111	0.272	0.188	0.059
Second cohort	3998	-0.020+	-0.020+	0.003	-0.013	-0.013	0.002
		(0.012)	(0.012)	(0.006)	(0.010)	(0.008)	(0.005)
Control group mean		0.230	0.201	0.032	0.145	0.118	0.020
Test of equality (p-value)		0.043	0.049	0.763	0.026	0.041	0.063

Notes: Point estimates and robust standard errors clustered by randomization strata (de Chaisemartin & Ramirez-Cuellar, 2020) in parentheses from a regression of enrollment in the specified sector on assignment to treatment; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. 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 CSAC data.

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Appendix 1. Sample emails and text messages

First outreach email:

California Student Aid Commission

Hi Jill,

Now's a great time to give college another chance.

A lot of students leave school before they graduate. Maybe you left for personal reasons. Maybe you were worried about the costs. Maybe the classes weren't what you expected.

No matter why you left college, you can come back and succeed — because this time, you can get one-on-one college counseling. With a personal college counselor, you'll get the support you didn't have before. You'll have someone in your corner — who knows how college works — helping you meet your goals.

Want to learn more? Take the first step **here**. It's a survey where you can tell us about your college journey and sign up to have a counselor contact you. Depending on availability, we may not be able to pair every student who's interested with a counselor. But even if you don't get one-on-one counseling, you can still sign up to get reminders about important deadlines.

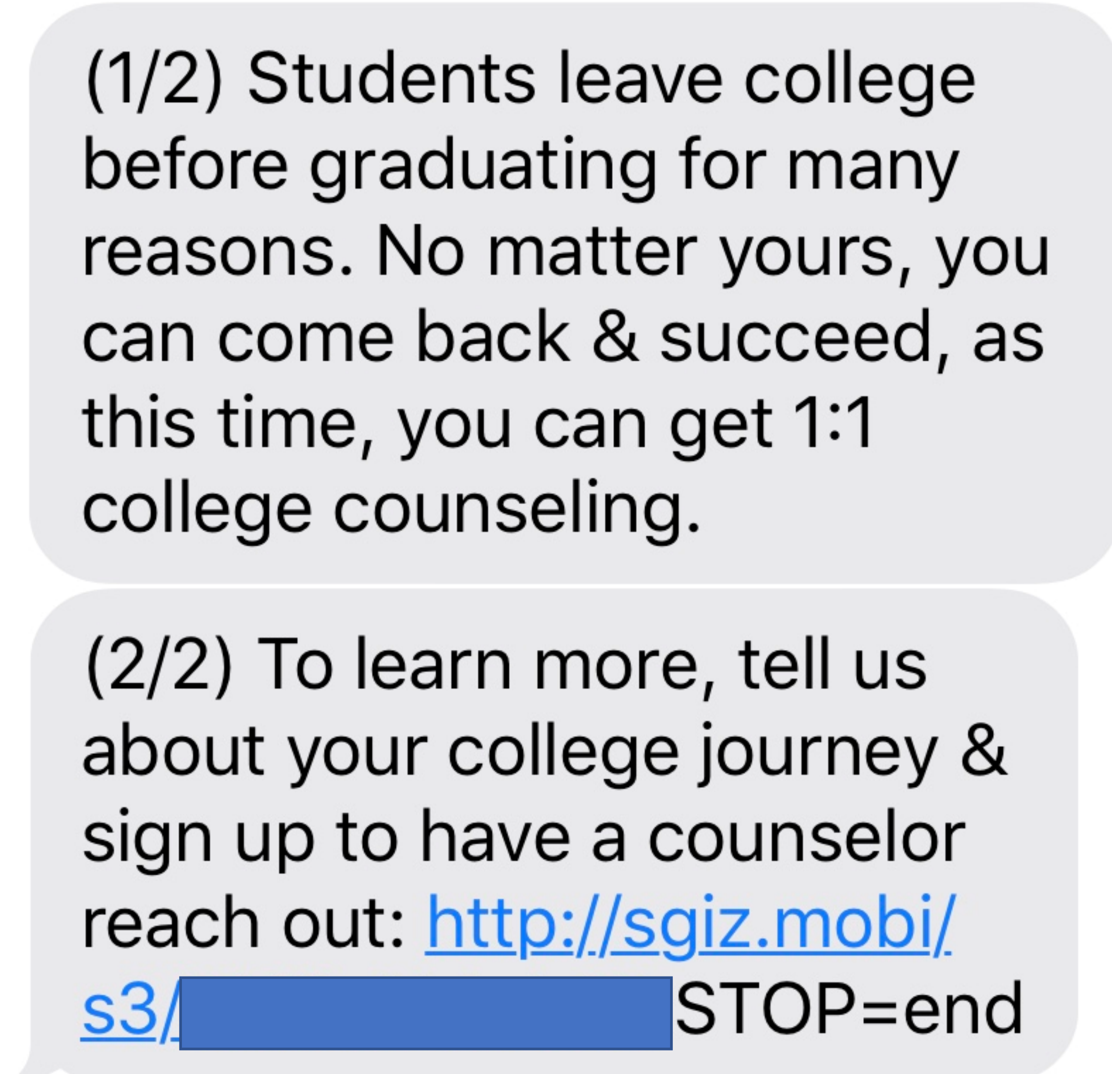
Questions? Click here for more details.

I wish you all the best with your college and career plans.

Sincerely,

Marlene Garcia
Executive Director
California Student Aid Commission

First outreach text:



Initial opt-in survey:

California Student Aid Commission

Now's a great time to give college another chance. No matter why you left college, you can come back and succeed — because this time, you can get one-on-one college counseling. With a personal college counselor, you'll get support you may not have had before. You'll have someone in your corner — who knows how college works — helping you meet your goals.

CSAC is partnering with InsideTrack, a higher education organization that provides coaching and support to college students. Eligible Cal Grant recipients may be selected to receive one-on-one coaching from a CSAC Counselor as they re-enter the world of higher education.

Eligibility is based on your survey responses, so please take a minute to answer the questions below and learn more about coaching.

Our records say that you previously used a Cal Grant to attend a college. Is this correct?

- ○ Yes, I have used a Cal Grant
- ○ No, I have never used a Cal Grant

Have you earned either an associate's or a bachelor's degree?

- ○ I have not yet earned a college degree
- ○ I have earned an associate's degree
- ○ I have earned a bachelor's degree

If you have not earned a degree, can you give a sense of the biggest challenge(s) you faced? Choose all that apply.

- ☐ Work became my main priority
- ☐ College was too expensive
- ☐ Missed a deadline for financial aid or enrollment, or other similar administrative problems
- ☐ Courses were too difficult
- ☐ Did not feel like part of the community
- ☐ Needed to leave temporarily to take care of a family member, or fulfill another short-term commitment

By participating, I am willing to have a counselor contact me using the information I have provided. I also understand that researchers will use anonymous, deidentified data (meaning the researchers will NOT have access to names, phone numbers, or any information that could be used to identify a participant) in order to analyze the success of the project.

- ○ Yes, I agree to participate in this project and be contacted by a counselor
- ○ No, I am not interested in participating

Please share your preferred contact information so that we can best connect you with a college counselor.

Name

Email address

Phone number

Submit