Keep Me In, Coach: The Short- and Long-Term Effects of Targeted Academic Coaching *

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This Version: June 20, 2023

Abstract

To boost college graduation rates, policymakers often advocate for academic supports such as coaching or mentoring. Proactive and intensive coaching interventions are effective, but are costly and difficult to scale. We evaluate a relatively lower-cost group coaching program targeted at first-year college students placed on academic probation. Participants attend a workshop where coaches aim to normalize failure and improve self-confidence. Coaches also facilitate a process whereby participants reflect on their academic difficulties, devise solutions to address their challenges, and create an action plan. Participants then hold a one-time follow-up meeting with their coach or visit a campus resource. Using a difference-in-discontinuity design, we show that the program raises students' first-year GPA by 14.6% of a standard deviation, and decreases the probability of first-year dropout by 8.5 percentage points. Effects are concentrated among lower-income students who also experience a significant increase in the probability of graduating. Finally, using administrative data we provide the first evidence that coaching/mentoring may have substantial long-run effects as we document significant gains in lower-income students' earnings 7–9 years following entry to the university. Our findings indicate that targeted, group coaching can be an effective way to improve marginal students' academic and early career outcomes.

*This paper has benefited from helpful conversations with Rachel Baker, Kelly Bedard, Laura Giuliano, Mark Hoekstra, Greg Kaplan, Philip Oreopoulos, Uros Petronijevic, David Molitor, Sarah Reber, Heather Royer, Jesse Rothstein, Bruce Sacerdote, Sally Sadoff, Wendy Stock, and seminar participants at UCSB, University of Melbourne, Hitotsubashi University, and UCSD's Rady School of Management. We thank participants at the following conferences for providing useful feedback: Society of Labor Economists Annual Meeting (2021), Association for Education, Finance and Policy Annual Meeting (2021), Montana State University's Applied Micro Workshop (2022), IZA Annual Education Workshop (2022), and Australian Annual Labour Econometrics Workshop (2022). We also thank the W.E. Upjohn Institute for Employment Research for support in conducting this research.

JEL codes: I23, I24, J16

Keywords: College Graduation, Mentoring, College advising, Academic coaching

1 Introduction

The labor market returns to a bachelor's degree are substantial and have been rising over time, with lower-income students realizing the largest economic gains (Hoekstra, 2009; Zimmerman, 2014; Goodman et al., 2020). At the same time, there exists a large and growing socioeconomic gap in postsecondary attainment (Bailey and Dynarski, 2011). By age 24, degree completion rates are about five times higher for students from the highest income quartile relative to the lowest (Cahalan et al., 2021). There are a number of non-financial factors that impede 4-year degree completion, including informational and behavioral barriers (Dynarski et al., 2022). In light of these facts, university-level academic supports in the form of mentoring and coaching have been at the forefront of policy discussions as a way to overcome this type of barrier.

In general, coaching programs provide support to students to help them overcome academic and non-academic obstacles, but evidence of their effectiveness is mixed. At one end of the intensity distribution are low-touch interventions such as virtual coaching delivered via emails and text messages. While these are inexpensive and potentially scalable, they tend to be ineffective (Oreopoulos and Petronijevic, 2018, 2019; Dobronyi et al., 2019). On the other hand, high-touch interventions that include one-on-one and continual support can be effective at producing long-lasting improvements in academic performance, but are expensive and difficult to scale (Bettinger and Baker, 2014; Barr and Castleman, 2018; Canaan et al., 2022).

To address this dilemma, we consider a medium-touch intervention that has the potential to be both effective and scalable. We evaluate a coaching program that strategically incorporates two low-cost features that have proven effective at influencing student behaviors in other academic settings. First, the program is conducted in a group setting and operates much like a group therapy session. Connecting with peers who have encountered similar academic difficulties may allow students to feel supported. Indeed, programs in which group activities are a key component, such as learning communities, have been shown to greatly improve students' academic outcomes (Weiss et al., 2015).

Second, the coaching is targeted. Targeted interventions have been shown to be highly effective, but can be costly to implement as it is often difficult to identify the students who will benefit most (Carrell and Sacerdote, 2017). We focus on retaining a group of students who are particulary discouraged and likely have the most to gain from coaching support, first-year students who are placed on academic probation by the university. Academic probation is a near-universal policy in higher education which labels students who have performed inadequately (i.e., scored below a certain GPA threshold). Capable students may find themselves on probation early on in college as the transition from high school to college can be difficult. Students often struggle with the shock of more demanding course work, higher grading standards, more autonomy, and less-than-ideal living situations all while simultaneously trying to find a supportive community in a new environment. For some, these challenges interfere with academic performance. Consequently, probation is meant to serve as a wake-up call, but this label has been shown to have discouragement effects which manifest in increased first-year dropout and reduced graduation rates (Lindo et al., 2010). Such a policy will achieve its goal and be welfare-improving if it removes students who would benefit from going to a less selective institution. A concern, however, is that probation unintentionally weeds out students, especially those from non-traditional and disadvantaged backgrounds, who view the probation label as a signal that they do not belong, but who would benefit from remaining at the university. The coaching program aims to rectify this belief and correct for the potential inefficient attrition caused by academic probation.

The program is implemented at a large, selective, public 4-year university in California and delivers mandatory in-person group coaching to all first-year students who are placed on academic probation in their first quarter at the university. Participants attend a two-hour workshop, where they are divided into small groups led by faculty and staff who are trained as coaches. The primary aim of the program is for coaches to improve participants' self-confidence by normalizing failure and academic probation. Coaches also facilitate a process whereby students identify the sources of their academic difficulties, find solutions to address their challenges, and create an action plan. The plan includes goal-setting and a time management exercise. Following the workshop, students are required to either use a campus resource once (e.g., office hours or counseling services) or have a follow-up one-on-one meeting with their coach.

We draw on rich administrative data for all first-year students entering the university in fall cohorts from 2007 to 2017. Students' academic outcomes are observed each quarter until they separate from the institution, allowing us to evaluate the impact of coaching on course performance, dropout rates, and degree attainment. We also investigate the program's effect on labor market outcomes, linking student data to administrative files from the State of California's Employment Development Department, which includes quarterly employment and earnings information for all employment covered by unemployment insurance (UI) in California for the years 2000-2020.

To estimate the causal effect of the program, we leverage the fact that first-year students are assigned to the program if their GPA is less than 2.0 during their first quarter. Students who score below this threshold are also placed on academic probation, precluding us from using a standard regression discontinuity design. However, because the coaching program was introduced in 2009, we are able to leverage the fact that cohorts entering after that date were exposed to both academic probation and coaching, while those enrolled before that date were only exposed to academic probation. Consequently, we implement a difference-in-discontinuity (DiRD) design which compares the difference in outcomes for students on either side of the 2.0 GPA eligibility threshold in the 2009 entering cohort and later cohorts (treated cohorts) with this same discontinuity for the cohorts entering prior to 2009 (control cohorts). Intuitively, the DiRD design estimates the discontinuity in outcomes for treated cohorts and differences out any potential discontinuities in control cohorts' outcomes, thereby isolating the impact of the coaching program from that of being placed on academic probation.

We find that students achieve large academic gains from participating in the coaching program.

Overall, the program increases participants' first-year GPA by 16.4% of a standard deviation, increases credits earned in the remainder of the year by 1.2, and decreases their likelihood of dropping out at the end of the first year by 8.6 percentage points (pp). We also find a positive albeit imprecisely estimated impact on 6-year graduation. Effects are more pronounced for groups of students who are expected to benefit most from such programs such as men and students majoring in science, technology, engineering and math (STEM). The coaching program is also particularly beneficial to students from lower-income backgrounds, as it increases their probability of graduating in six years by a significant 13.6 pp. These findings are unsurprising given that lower-income students and men persist in and complete college at much lower rates than their counterparts (Bailey and Dynarski, 2011), and attrition rates for STEM majors are typically high (National Center for Education Statistics, 2013).

We next examine how the program impacts students' earnings 7-9 years following entry to the university.[1] Despite the caveat of reduced sample size and precision, our earnings analysis yields results that are consistent with the documented effects of the program on academic outcomes. In the full sample, we find that the coaching program has positive but imprecise impacts on students' earnings. For the subgroups of students who benefit the most from the program academically, we document large and statistically significant earnings gains. Coaching increases male and lower-income students' earnings by approximately 25-31%. Point estimates among STEM majors are similar but less precisely estimated. These point estimates are consistent with and contribute to a burgeoning body of work showing large earnings gains from attending a relatively more selective 4-year institution for academically marginal students (Zimmerman, 2014; Bleemer, 2018; Goodman et al., 2020; Bleemer, 2022; Black et al., 2022). Taken together, our findings indicate that providing discouraged students with group coaching largely improves their academic and early labor market outcomes.

We further explore the potential mechanisms underlying the main effects to help uncover which aspects of the coaching program explain its effectiveness. To do this, we utilize data from pre- and post-program surveys administered in select years to all students who participated in it. Using an individual fixed-effects model, we find that by the end of the program students feel significantly more supported by a faculty or staff member, are less likely to feel that they are the only ones struggling, are more familiar with the university's student services and, are better at managing their time. On the other hand, we detect no significant changes in their motivation, likelihood of attending class, or feeling that they are connected to a community at the university. Given these findings and the goals of the program, we posit that the main effects are largely operating through improvements in participants' social-emotional state—in particular, improvements in the level of support they feel from the university and improvements in their feelings of isolation in their academic struggles. Related, other recent studies have found support from the university and

[1] We focus on earnings in quarters 25-36 (7-9 years) after initial enrollment, where quarter 1 is the fall quarter of the student's first year. The typical student enrolls at age 18, so this corresponds approximately to ages 24-26.

faculty improves student's experience and improves outcomes (Oreopoulos et al., 2020; Carrell and Kurlaender, 2020).

Our paper is related to a broad literature which examines ways to effectively boost low college completion rates. A large body of work has considered the role of financial aid in increasing college attainment (Dynarski, 2003; Bettinger, 2015; Cohodes and Goodman, 2014; Bulman and Hoxby, 2015; Bettinger et al., 2019; Angrist et al., 2020). Other work has focused on the myriad of non-financial barriers to a college degree (see Dynarski et al. (2022) for a review of this literature). We contribute to the group of studies that considers the role of university-level academic supports in increasing academic attainment, specifically those focused on providing counseling and coaching as a way to change behavior. The most effective programs deliver proactive and individualized coaching (Bettinger and Baker, 2014; Barr and Castleman, 2018; Canaan et al., 2022).[2] Moreover, comprehensive programs which offer an array of structured student supports are particularly successful (Clotfelter et al., 2018; Page et al., 2019; Weiss et al., 2019; Evans et al., 2020). For example, Weiss et al. (2019) show that ASAP, a multifaceted three-year program in which participants are offered a variety of supports such as proactive one-on-one advising, financial aid, weekly tutoring, and incentives to enroll in similar classes, substantially increases community college graduation rates.

Our paper advances this literature in several important ways. First, we focus on a coaching program that is relatively lower touch and is delivered in a shorter time span (one quarter) than other successful interventions. Students in our setting are required to attend only one group coaching workshop, and hold one individualized meeting with their coach or visit an on-campus resource. In contrast, a common feature of all the aforementioned effective programs is that coaches or advisors are proactive, and they regularly initiate contact and schedule one-on-one meetings with their students. Furthermore, most of these interventions are implemented for at least one academic year, with some lasting multiple years.[3] While effective, these programs are expensive and often difficult to scale (Oreopoulos and Petronijevic, 2019). Instead, the nature of the coaching program studied in this paper makes it a lower-cost and potentially more scalable way to effectively improve college outcomes, a finding we document in Section 5.1. Understanding which student supports are most cost-effective is important as per-student resources at U.S. postsecondary institutions have been declining over time (Bound et al., 2010; Denning et al., 2021).

Second, beyond the nature of the program, we add to this literature by providing the first

[2]Coaching seems to be most effective when it is proactive. Previous work finds that non-proactive coaching has limited positive effects on students' academic performance, with effects dissipating once the intervention ends (Angrist et al., 2009; Scrivener and Weiss, 2009; Angrist et al., 2014).

[3]Specifically, Barr and Castleman (2018) report increases in college enrollment and persistence in students enrolled in Bottom Line (BL), a program which offers intensive counseling to students starting their senior year in high school and up to 6 years after high school. BL counselors meet one-on-one with first-year college students around three to four times per semester. Bettinger and Baker (2014) document a rise in persistence from InsideTrack, a for-profit coaching service offered to non-traditional college students where coaches regularly initiate contact via phone and keep in touch with students. Oreopoulos and Petronijevic (2018) implement a one-year coaching intervention at a Canadian University in which they instruct coaches to be proactive and offer personalized regular support. Their coaching intervention substantially increased academic performance.

estimates of the impact of coaching on later earnings. The existing literature focuses solely on academic outcomes, but understanding if treatment effects persist in the labor market provides a more comprehensive view of the benefits of coaching. Our setting is well suited to study labor market outcomes because the coaching program is mandatory and we have obtained ten years of rich administrative earnings files. While we cannot conclusively establish that *all* program participants realize labor market gains, we nonetheless show that our coaching program substantially increases earnings of students who benefit the most from it academically.

Finally, our findings provide new insights into the impacts of academic probation. Specifically, our results validate the concern that an unintended consequence of academic probation is inefficient attrition. Indeed, probation appears to push out some students who would benefit from remaining in the more selective university. We find this is particularly the case for students from lower income backgrounds. They incur the largest penalties from probation and also experience large long-term gains from the coaching program. This is unsurprising given that high achieving low income students are often susceptible to non-financial barriers which impede their completion. While academic probation is likely here to stay as universities need a way to warn students who are performing inadequately, the group coaching program under study is an effective and low-cost way to remedy this inefficient attrition. The intervention comes at a pivotal time, and we find it greatly changes the long-term trajectories of these students.

The rest of this paper is organized as follows: Section 2 details the institutional background. Section 3 describes the data. Section 4 outlines the empirical framework. Section 5 presents results, discusses the mechanisms and provides context for the magnitudes. Section 6 discusses the plausibility of the identifying assumptions. Section 7 presents labor market results. Section 8 concludes.

2 Background

The setting for this study is a large, selective, public 4-year university located on the Central Coast of California. The university serves approximately 21,000 students, with a focus on undergraduate education, particularly in engineering and agriculture fields. To provide context, for the 2019–20 academic year, the undergraduate acceptance rate was 28%, and tuition and fees (excluding books, supplies, room/board, etc.) totaled nearly \$10,000/year for California residents and \$25,000 for out-of-state and international students. The 2019 entering cohort of first-time freshmen had an average high school GPA of 4.1 (on a 5.0 scale), an average SAT score of 1,375 (among the top 20% nationally), and an average ACT score of 29.

2.1 Success Program

Like many institutions, this university is concerned with retention and completion rates. Not only are these factors important inputs in university rankings, but administrators are also aware that it is costly to students both directly and indirectly to begin and not complete a degree, as they are unable to realize the associated wage premium and often accumulate student loans. In an attempt to improve these outcomes, in 2009 the university introduced the Success Program (SP), a mandatory academic coaching program for first-time freshmen who are placed on academic probation at the end of the fall quarter of their first year.[4] At this university, it has always been the case that students are placed on academic probation if their term GPA or cumulative GPA falls below 2.0.[5] While the intent of academic probation is to serve as a warning or motivate students to improve their performance, there is a concern that it is ineffective or may even discourage some students. In light of this concern, SP was intentionally designed to take a different approach from the standard academic probation corrective measure. SP curriculum is designed based on psychologist Dr. Albert Bandura's Theory of Self-Efficacy. The theory posits that individuals with sufficient levels of self-efficacy have confidence in their ability to exert control over their motivation and behavior and, consequently, are able to achieve specific performance benchmarks. As such, SP aims to improve self-efficacy. The program was first implemented as a pilot in the fall of 2009 for four of the university's six colleges, and was extended to the entire university the following year.[6]

This institution operates on a quarter calendar where three eleven-week terms make up the academic year: fall quarter, winter quarter and spring quarter (Q1–Q3). Students who earn below a 2.0 GPA in Q1—their first fall quarter at the institution—are required to complete SP during Q2 (winter quarter). The program consists of two parts: (1) a two-hour workshop led by trained faculty coaches that is held during the first two weeks of Q2, and (2) a mandatory campus engagement assignment to be completed by week 5 of that quarter. For this assignment, students choose between either visiting a campus resource and completing a reflection assignment, or attending a one-on-one follow-up meeting with their workshop coach and completing a reflection assignment. The university typically offers two workshop dates during the first weeks of Q2 to accommodate student and faculty schedules. Roughly 320 students qualify for the program each fall quarter, and almost all of them participate in one of the two workshop sessions. A small share of students are unable to attend the session due to a scheduling conflict and complete the requirement one-on-one with a trained coach. To enforce participation, students are unable to enroll in Q3 (spring quarter) courses until they have completed all parts of the program.

The two-hour workshop is broken into two parts: a thirty-minute meeting with all session participants (typically about 150 students), followed by a ninety-minute breakout session with a smaller group of 6 to 8 students led by one or two trained faculty or staff coaches. The goal of the large-group portion of the workshop is to normalize failure and academic probation, and to show students that they are not the only ones at the university who experienced academic challenges

[4] To preserve institution anonymity, we have modified the name of the program to "Success Program".

[5] Students on probation are subject to dismissal if their cumulative GPA does not exceed the 2.0 threshold by the end of their first year.

[6] One might be concerned that the pilot colleges in 2009 are non-randomly chosen and thus are driving our results. We show in Table B9 that the results are robust to the exclusion of the 2009 cohort.

in their first quarter. During this part of the workshop, students watch a video featuring various high-profile people in Silicon Valley (e.g., Elon Musk) who have overcome challenges and failure. The SP leadership team presents students with information on campus resources, including tutoring services, health and well-being services, counseling services, and cross-cultural services such as the Gender Equity Center, Pride Center and Multicultural Center (see Appendix D). They also outline the rules of academic probation in an attempt to remove some of the anxiety surrounding this term.

The second part of the workshop is meant to be more interactive and discussion based. It involves a breakout session with a smaller group, typically 6–8 students, led by one or two trained coach. Coaches are faculty and staff from across the university who have undergone a two-hour training led by the SP leadership team.[7] In an attempt to reiterate the message that failure is common among successful people, the session opens with coaches sharing a time when they experienced failure. Then the coaches facilitate a reflection and goal-setting exercise. Students are allotted time to reflect on factors that may have contributed to their academic struggles in Q1, such as academic challenges (e.g., problems with time management, study skills, class attendance, or school/life balance); college adjustment difficulties (e.g., roommate issues, homesickness, difficulty finding resources, or difficulty fitting in); and personal hardships (e.g., mental or physical health issues, personal or family crises, or identity-based isolation). To guide this process, participants work through a worksheet, pausing to discuss their responses with the group. The worksheet, presented in Appendix D, is set up in three steps: identifying weaknesses, identifying solutions or resources that can aid in overcoming these weaknesses, and goal-setting for the current quarter. The intent of this exercise is for students to leave the workshop with a tangible plan to change their academic trajectory going forward. During this time, participants are also required to fill in a time management worksheet where they are encouraged to allocate time for classes, studying and social activities (Appendix D).

2.2 Academic Probation

Academic probation is a near universal policy in higher education institutions in the US. Such a policy notifies students who have fallen below a minimum GPA threshold to remain in good academic standing and is meant to serve as a warning. Once on probation, students who do not improve their GPA in a set number of terms are then typically eligible for dismissal from the university. The policy is well-intentioned as it is meant to serve as a wake-up call for capable students who have been underperforming, and to weed out students who are "mismatched" and would be better off going to a lower-ranked university. Indeed, probation has been shown to increase some students' chances of dropping out of college and to reduce graduation rates (Lindo et al., 2010). This is efficient if it only removes students who would be better off going to a lower-ranked institution. The concern is that this label may unintentionally weed out capable students

[7] Faculty and staff who become coaches select into this service role. Faculty receive service credit from their home departments for their participation, and staff's time spent on coaching is considered part of their typical work day.

from lower income backgrounds as they often face barriers to completion that their higher-income counterparts do not.

At this institution, the academic probation threshold is determined by the minimum GPA of 2.0, which is a "C" average. At the end of each quarter, students who have a cumulative GPA or a term GPA below 2.0 are placed on academic probation and are promptly notified of this status via email. Students remain on probation until their term and cumulative GPA are above the 2.0 threshold. First-year students are granted a probationary period of the first year to improve their GPA above this threshold. If they fail to meet this mark by the end of their first year (Q3), they are subject to dismissal and are not eligible to return for their second year.[8]

3 Data

All data in the main analysis are student level and come from three sources: (1) administrative records from the university office of Institutional Research, (2) SP participation files, and (3) pre- and post-SP survey responses from the Success Program office, which are discussed in detail in Section 5.3. The full sample includes eleven entering cohorts of first-time freshmen who enrolled at the university in a fall quarter between 2007 and 2017 (45,864 students) and tracks them by quarter through graduation or until they separate from the university. In an auxiliary analysis, these data are then augmented by labor market outcomes from the California Employment Development Department and are described in Section 7.

The administrative transcript files provide a detailed view of students' academic progress, allowing for examination of a rich set of outcomes. By student-quarter, we observe enrolled courses, course grades, cumulative GPA, academic major, probation status, and the timing of separation from the university either as a dropout or graduate. As noted above, Q1 is defined as a student's first fall quarter at the university, Q2 and Q3 to their first winter and spring quarters, respectively. Understanding student performance in year one is of primary interest as students qualify for the coaching program in Q1 and complete it in Q2. As such, the main outcome of interest is dropping out at the end of Q3 (i.e., year one retention). Q3 dropout takes the value of 1 if a student does not appear in the data the following academic year (Q5–Q7), and 0 otherwise.[9] Q1 and Q2 dropout are coded in a similar fashion. Other outcomes of interest include graduating: whether a student graduates in 4 years (on-time graduation) or 6 years (a proxy for ever graduating). We construct two additional outcomes, Q2 + Q3 GPA and Q2 + Q3 total credits earned, to capture a student's academic performance in the rest of their first year.[10] These files also contain a rich

[8]Students who are placed on academic probation in any other quarter than Q1, are granted only one quarter to improve their academic standing above the 2.0 threshold before being eligible for dismissal.

[9]This coding of Q3 dropout allows a student to take several quarters away, perhaps to study abroad or for employment reasons, and not be coded as a dropout. The results, however, are robust to alternative ways of defining Q3 dropout including coding Q3 equal to 1 if a student never appears in the data again following Q3 or if they don't appear in the fall quarter of their second year (Q5).

[10]While GPA is a common measure of performance used in the literature, it is only defined for students who are

set of time-invariant background characteristics including a student's high school GPA, gender, race/ethnicity, whether they are required to enroll in remedial math and English courses, eligibility for the Federal Pell Grant program, their expected family contribution (EFC) as determined by the Free Application for Federal Student Aid (FAFSA), and parental education.

Probation status is observed in the administrative files and takes the value of 1 if a student is placed on academic probation in Q1, and 0 otherwise. Based on the probation policy assignment rule, students who score below a 2.0 GPA in a given quarter are placed on probation. For this analysis, Q1 probation status is of primary interest. Figure A1 confirms that the probability of Q1 probation is solely determined by Q1 GPA for both treated and control cohorts. The probability of probation changes sharply at the 2.0 threshold.[11]

The administrative files are merged with SP participation records to identify which students complete the program. Recall that SP assignment is determined by a Q1 GPA of less than 2.0. Figure 1 shows the likelihood of program participation by Q1 GPA for all entering cohorts from fall 2010 to fall 2018 (i.e., since the inception of the coaching program).[12] Indeed, there is a sharp jump at the 2.0 GPA threshold in the likelihood of SP participation. No student with a Q1 GPA above the threshold of 2.0 participates in SP while virtually all students scoring below the 2.0 cutoff participate.[13] Together, Figure A1 and Figure 1 confirm that probation status and SP participation are binding in practice and are solely a function of Q1 GPA.

Summary statistics are presented in Table 1. While column 1 presents means for the full sample to provide context, column 2 reports summary statistics for the 22,225 students who are part of the analysis sample (i.e., the marginal students with GPAs between 1 and 3 in their first quarter at the university). Relative to the full sample, the analysis sample has more men (56% vs. 52%), more non-white students (42% vs. 38%), and is lower income. The share of Pell Grant-eligible students is 19% compared with 16% in the full sample. Moreover, this group experiences relatively worse academic outcomes. More than half of the students in the analysis sample are placed on academic probation at some point during their college career, and 8% dropout after their first year. Consequently, the 4- and 6-year graduation rates for the students around the 2.0 GPA cutoff are 35% and 77%, respectively—much lower than the full sample.

Columns 3 and 4 report summary statistics for the group of students in the control cohorts. This constitutes a sample of 4,294 students enrolled in all colleges in 2007 and 2008 and the 2 colleges in 2009 that did not participate in the pilot program.[14] Column 3 includes the students

enrolled in Q2 and Q3. As a way to circumvent this selection issue, we also analyze total credits earned in Q2 and Q3, as that is defined for all students in the sample.

[11] Unfortunately, the probation variable indicator is missing for the 2010–2016 entering fall cohorts. As such, our first-stage analysis is based on the freshmen 2007, 2008, 2009, and 2017 fall cohorts.

[12] We exclude from this figure students from the few faculties who were selected for the SP pilot in 2009 as we do not have program participation data for them.

[13] Each fall quarter there are a few students (less than 10) who qualify for the SP but who are granted a waiver by the dean of their college and are thus excused from participating. These waivers are typically reserved for extenuating circumstances such as health shocks or family emergencies.

[14] The following colleges participated in the 2009 pilot: College of Agriculture, Food, and Environmental Sciences;

who are placed on probation, GPA $\in [1, 2)$, and column 4 includes students scoring just above the 2.0 GPA cutoff, GPA $\in [2, 3]$, and thus not on probation. Columns 5 and 6 present summary statistics for the 16,241 students in the treated cohorts. We split this sample into those eligible for both programs, GPA $\in [1, 2)$, as presented in column 5, and those who are barely ineligible, GPA $\in [2, 3]$, as presented in column 6.

4 Empirical Methods

4.1 Visual Motivation for DiRD Design

We begin by presenting graphical motivation for the DiRD design. Figure 2 presents regression discontinuity (RD) plots for several outcomes separately for treated and control cohorts, with first quarter GPA as the running variable. All figures take similar forms, in that circles represent local averages over a 0.1 GPA score range. All figures are drawn over a bandwidth of 1 GPA point on either side of the cutoff using a linear fit. Figures in the left panel summarize effects at the 2.0 GPA cutoff for students in control cohorts (exposed to probation-only policy), while those on the right present effects for those in treated cohorts (exposed to probation + SP).

Figure 2 provides visual evidence of meaningful differences between students exposed exclusively to the probation policy (control cohorts) and those exposed to probation and SP (treated cohorts). Figure 2a and Figure 2b highlight significant first-year dropout differences between control and treated cohorts at the cutoff. Students who just qualify for probation are 10.9 pp more likely to drop out after first year compared to those who just avoid probation. This large and meaningful gap at the cutoff is greatly reduced to a statistically insignificant 2 pp for cohorts exposed to probation and SP. Under the assumption that the negative effects of probation are similar for all cohorts, this suggests that students who qualified for the coaching program experienced significantly lower dropout rates compared with those not exposed. Figure A2 shows the results are robust to using a quadratic fit on either side of the cutoff.

While there is not strong visual evidence that SP changes students' probability of graduating on time, comparing Figure 2c and Figure 2d, there is for 6-year graduation as shown in Figure 2e and Figure 2f. We find that students in control cohorts with GPAs just below the threshold are 9.1 pp less likely to graduate in 6 years compared with treated cohorts who are only 4.7 pp less likely. Finally, Figure 2g and Figure 2h show that while students exposed to probation alone were not significantly affected in terms of first year GPA, those exposed to probation and SP experienced a large and significant 16% of a standard deviation increase in performance at the cutoff. That is, the coaching program seems to have positive grade impacts on marginal students. While this exercise provides suggestive evidence of positive impacts of the program, we next turn to an econometric framework using a DiRD design to more rigorously probe this possibility.

College of Business; College of Engineering; and College of Architecture and Environmental Design. The College of Science and Mathematics and College of Liberal Arts did not participate.

4.2 Difference-in-Discontinuity Design

To identify the causal effect of coaching, we draw on variation in exposure to the coaching program within cohorts and across cohorts. First, in the spirit of an RD design, we leverage variation in SP participation that arises from the strictly enforced SP assignment rule which is a function of first quarter GPA at the university. Students who score below a 2.0 GPA in Q1 are required to complete SP and those above the threshold are excluded from the program. In a standard RD framework, if this cutoff is orthogonal to student characteristics, any observed discontinuity in outcomes around the threshold can be attributed to SP. However, because the SP assignment rule is identical to the academic probation assignment rule, the interpretation of the standard RD estimate will capture both the effect of SP and probation. To isolate the effect of SP net of the confounding probation policy, we further leverage the fact that some cohorts were exposed to SP and others were not. The SP pilot was introduced in the 2009 academic year for a subset of colleges at the university, and for all cohorts in all colleges from 2010 to the present. Thus, the 2007 and 2008 cohorts and some students in the 2009 cohort (control cohorts) were exposed to the probation rule but not to the SP assignment rule. All other cohorts were exposed to SP and probation (treated cohorts).

Formally, we implement a DiRD design. Intuitively, this design estimates the discontinuity in outcomes across the 2.0 GPA cutoff for cohorts exposed to both SP and probation and then purges the effects of probation by differencing out any discontinuity in outcomes for cohorts exposed to the probation policy only.

The estimation equation is as follows:

$$\begin{aligned}Y_i = {} & \beta_1 + \beta_2 GPA_i + \beta_3 Treat_i + \beta_4 Below_i + \beta_5 (Treat_i * GPA_i) \\ & + \beta_6 (Below_i * GPA_i) + \beta_7 (Below_i * Treat_i) + \beta_8 (Below_i * Treat_i * GPA_i) \\ & + \rho_c + \delta_k + \gamma X_i + \epsilon_i,\end{aligned} \tag{1}$$

Y_i is the outcome of interest for student i. GPA_i is the running variable and represents student i's normalized first quarter GPA relative to the cutoff of 2.0. $Treat_i$ is a binary variable that takes the value of 1 for treated cohorts, corresponding to all students exposed to both the probation policy and SP, and 0 for control cohorts exposed only to the probation policy. *Below* is a binary variable that takes the value of 1 for students scoring below the GPA cutoff of 2.0, and 0 otherwise. The interactions with GPA_i allow slopes to vary on either side of the GPA cutoff as well as across treated and control cohorts.

The parameter of interest is β_7, which represents the difference between treated and control cohorts in the discontinuous jump in the outcome at the 2.0 GPA cutoff.[15] X_i is a vector of controls composed of students' predetermined characteristics—high school GPA, gender, race, required

[15] The parameter β_3 summarizes the average difference in outcomes for students scoring above the 2.0 cutoff in the treated versus control cohorts. The parameter β_4 represents the average difference in outcomes for students scoring below to those scoring above the cutoff in the control cohorts.

enrollment in remedial math and English courses, Pell Grant eligibility, EFC and parental education—and is included to improve precision by reducing residual variation in the outcome variable. ρ_c is cohort fixed effects which control for any common shocks and overall trends in the outcome. δ_k is a college fixed effects. Finally, ϵ_i represents the error term. We report robust standard errors rather than clustering. Clustering with a discrete running variable yields confidence intervals with worse coverage properties and does not resolve specification bias issues (Kolsar & Rothe, 2018).

Unfortunately, the standard data-driven bandwidth selectors often implemented with a RD design do not extend to a DiRD research design. As such, in the main tables of results, we report estimates using bandwidths for the running variable of 0.75 and 1.0 GPA points on either side of the cutoff. To further probe the sensitivity of the results to bandwidth choice, following Jackson (2021) Figure A3 reports point estimates across a variety of bandwidths ranging from 0.25 to 2 GPA points. Additionally, we compute the optimal bandwidth separately for treated and untreated cohorts using the CCT procedure described in Calonico et al. (2014), and then estimate Equation (1) using the average of the two optimal bandwidths, as is done in Grembi et al. (2016). Figure A3 denotes this bandwidth for each outcome with a vertical line.

As formalized in Grembi et al. (2016), when both policies induce sharp RDs the validity of the DiRD estimate, β_7, requires that the following three identifying assumptions hold.

A1. Potential outcomes are smooth across the threshold (standard RD assumption).

A2. The effect of the confounding policy, probation, is constant over time (akin to the DiD parallel trends assumption).

A3. Local average treatment effects (LATEs) are additively separable.

Section 6 presents several pieces of evidence in strong support of A1 and A2. Because probation is never observed absent SP, it is difficult to assess the plausibility of A3. If A3 does not hold, it does not bias the estimate of β_7 but rather shapes its interpretation. We discuss this in more detail in Section 6.

5 Results

5.1 Academic Results

The main results come from estimating Equation (1). Table 2 reports the point estimates using two different bandwidths, 0.75 and 1 GPA points. All estimates are reported with and without controls to ensure results are robust to the inclusion of predetermined student characteristics. Columns 1 through 3 present DiRD estimates for Quarter 1 (Q1), Quarter 2 (Q2) and Quarter 3 (Q3) dropout, respectively. As shown in columns 1 and 2, we find no significant treatment effects for Q1 or Q2 dropout indicating that SP had no effect on first-year dropout. The null effect on Q1 and Q2 dropout is reassuring given the timing of the program and the probationary period for first-year students; program participation is in Q2 and university policy states that a student is not subject to dismissal for poor academic performance until the end of the first year (Q3). On the other hand, we find large and statistically significant treatment estimates on dropout directly following first year (column 3). SP decreases marginal students' Q3 dropout by 7.3–8.8 pp, an approximate 30% decrease from the baseline dropout rate of 30% for students placed on probation in pre-program years.

Consistent with the improved first-year retention, SP also positively impacts academic performance in the rest of the first year. Column 4 shows standardized Q2 + Q3 GPA improves by a large and significant 14.6–16.8% of a standard deviation, depending on bandwidth choice.[16] Column 5 reveals that the program increases total earned credits in Q2 and Q3 by approximately 1.2, which is 5.5% from a baseline mean of 21.9.

Columns 6 and 7 report treatment effects for graduation outcomes. The program does not appear to improve on-time graduation, as the point estimates for 4-year graduation are small and indistinguishable from zero (column 6). There is suggestive evidence, however, that 6-year graduation is affected (column 7). While the point estimates are imprecise, they are positive and relatively large in magnitude. Given that the program targets students lower down in the grade distribution, it is not surprising that it has no effect on the likelihood of on-time graduation. A more plausible story, one inline with our findings, is that the program reduces dropout at the end of the first year (as shown in column 3), and students who remain because of this, are more likely to eventually graduate than those who just missed out on the program. Finally, all estimates are robust to the inclusion of predetermined student characteristics and bandwidth choice (see Figure A3).[17]

5.2 Heterogeneity Analysis

Certain groups may be more likely to respond to academic coaching. To investigate this, we estimate Equation (1) separately by gender, field of study and socioeconomic status (SES). Results are presented in Table 3.[18] To begin, while we can not reject that the point estimates for the various outcomes are the same for men and women, comparing effects across gender reveals that the baseline effects presented in Table 2 are largely concentrated among men. Not only does SP attendance result in significantly reduced Q3 dropout for men compared with women, it also produces large and statistically significant effects on men's academic performance compared with women's in the remainder of the first year. Q2 + Q3 GPA improves by 30.3% (column 4) and Q2 + Q3 earned credits increases by 1.3. These estimates are larger and more statistically significant

[16]For comparison purposes, we standardize Q2 + Q3 GPA within cohort to mean zero, standard deviation 1.

[17]Our focus is on year 1 dropout because most attrition occurs in year 1. Year 1 dropout is 6%; year 2, 4%; year 3, 1.8% and year 4, 1.7%. Bostwick et al. (2022) find a similar dropout pattern among students attending Ohio public institutions. Moreover, Table B2 shows no detectible effects of the coaching program on dropout in years 2-4 suggesting that the program averts dropout among this discouraged group of students rather immediately.

[18]Table B3 reports results using an alternative bandwidth (0.75 GPA points). Results are similar to those reported in Table 3 which uses a bandwidth of 1.0 GPA points.

than for women.

At this university and most, STEM majors tend to have relatively higher dropout rates.[19] Often interventions such as mentoring, advising, and coaching are proposed as ways to combat STEM attrition. Our analysis supports this hypothesis as we find large effects of the program for STEM majors. SP decreases first-year dropout for STEM majors by a substantial 12.5 pp (column 3) and improves Q2 + Q3 GPA by 25.2% of a standard deviation (column 4). Typically, heterogeneity analyses by major suffer from student sorting that is related to the treatment, but in this setting, major can be viewed as a predetermined characteristic much like high school GPA. At this university, students apply and are admitted to a specific major at a specific college (e.g., an Electrical Engineering major in the College of Engineering) and switching colleges and majors is quite difficult, particularly in the fist year.

The final two rows of Table 3 explore effects by students' SES. Lower SES takes the value of 1 if a student files the FAFSA and is eligible for federal financial aid (i.e., they have an EFC<30,000), and 0 otherwise. Higher SES is the complement. We find that the program effects are largely driven by students from lower-income backgrounds (i.e., those who ex ante may be more likely to benefit from advising or coaching). Students from lower SES backgrounds who are marginally exposed to coaching are 12.5 pp less likely to dropout after first year and are 13.8–15.9 pp more likely to graduate within six years, depending on the bandwidth. In line with the 6-year graduation findings, these students also experience large improvements in their Q2 + Q3 GPA and Q2 + Q3 total earned credits. Overall, the heterogeneity analysis reveals coaching for students on the margin of the 2.0 GPA threshold has substantial impact for men, STEM majors and especially those who come from lower-income backgrounds.

5.3 Mechanism Exploration

Our results indicate that SP improves students' academic performance, retention and probability of graduating. Next we seek to understand the channel underlying these positive findings. While the program is designed as a coaching intervention, it includes a bundle of treatments (i.e., emotional support, information, goal-setting, and time management skills) which all have the potential to individually boost students' academic success. As such, there are several possible channels to consider. First, the program may improve students' social-emotional state. A primary goal of the program is to combat feelings of self-doubt and anxiety that may have been brought about by failure and the academic probation label. Much like a group therapy session, participants spend time reflecting on and discussing their failure in small groups led by their coach. It is possible that this process helps alleviate negative thoughts and replaces them with a growth mindset.[20] The program may also improve students' social-emotional state in other ways. The group aspect likely

[19] We define a STEM major as a student who is in the College of Engineer or the College of Science and Mathematics.

[20] The "growth mindest" framework views intelligence as malleable and encourages students to keep negative events in perspective (Oreopoulos and Petronijevic, 2018; Yeager et al., 2016).

reduces feelings of isolation, and having in-person contact with a compassionate coach may help students feel more supported by the university.

Second, provision of information about university resources may be an important channel in supporting academic success going forward. Moreover, most students visit a campus resource in the follow-up assignment which could initiate a habit. Third, goal-setting may provide motivation and focus. Fourth, improvements in time management skills may play an important role in providing necessary structure.

To aid in our assessment of the plausibility of each of these potential mechanisms, we utilize data from surveys conducted by the university's Success Program Office. These surveys were administered to all SP participants pre- and post-program completion for eight cohorts of students: those qualifying in the fall quarters of 2013 and 2015–2018 and those qualifying in the winter quarters of 2017–2019.[21] All surveys were administered through the online platform SurveyMonkey and are included in Appendix E. Given the structure of the data, this exercise is more descriptive in nature as the analysis relies on within student comparisons of outcomes before and after SP participation. We estimate the following model:

$$Y_{it} = \beta_1 + \beta_2 Post_t + \delta_i + \epsilon_{it}, \tag{2}$$

where Y_{it} is the outcome for individual i in period t, $Post_t$ takes the value of 1 to indicate the post-program period, and 0 otherwise; and δ_i is an individual fixed effect. All standard errors are clustered at the individual level.

Results from this analysis are reported in Table 4. Columns 1 to 5, focus on outcomes addressing students' knowledge of academic resources, time management, class attendance, feelings of connectedness to a community at the university, and academic motivation.[22] Results in Table 4 suggest that SP impacts these goals in meaningful ways. We find that it significantly increases their awareness and possibly usage of various student resources offered by the university (column 1).[23] Estimates from column 2 further indicate that the program improves students' time management skills. On the other hand, we detect no significant changes in their self-reported class attendance (column 3), self-reported level of connectedness to a community at the university (column 4), or

[21]SP was first implemented university-wide in Fall 2010 (though, there was a pilot in Fall 2009 for a subset of faculties) and has been in operation each fall since. For a subset of years, the university also operated a second program in the year for students qualifying in the winter quarter (Winter quarters 2014, 2015 and 2017–2019). While we do not use Winter quarter participation in our main analysis, we do use survey responses from these cohorts of students in the mechanism analysis.

[22]Specifically, using the pre- and post-program surveys for students who qualified for SP in Fall 2013 (434 observations), we construct five binary outcomes: (i) Resources is a dummy variable that is equal to one if a student reports that they are familiar with the university's student services and how to use them and 0 otherwise, (ii) Time Management is equal 1 if a student indicates that they manage their time well and 0 otherwise, (iii) Attend Class is equal to one if a student reports regularly attending classes and 0 otherwise, (iv) Connected is equal to 1 if a student reports feeling connected to a community at the university and 0 otherwise and (v) Motivated is equal to 1 if a student reports they are motivated to focus on school and 0 otherwise.

[23]These include faculty office hours, on-campus tutoring services, the writing center, student clubs, the recreation center, counseling services, the health center, the disability resource center, and diversity and inclusion services.

their self-reported level of academic motivation (column 5).

Next, we use pre- and post-survey responses for students qualifying for SP in the fall 2015–2018 and winter 2017–2019 quarters (1,882 observations) to explore whether program participation affects students' feelings of isolation in regards to their failure, and perceived faculty support. Estimates presented in columns 6 and 7 indicate that students' likelihood of feeling that they were not the only ones who experienced academic difficulties increased by 22.8 pp following program participation. Additionally, column 7 shows that their ability to identify a faculty or staff member who they felt cares about their academic success increased by 42.5 pp. Note that the outcome in column 4 (connected to a community at the university) is distinct from the outcome in column 6 (feeling like they are the only ones who experienced academic difficulty). The latter addresses self-reported feelings of isolation related to academic failure, while the former outcome is not tied to academics and more likely captures whether a student feels like they belong socially to a group at the university.

We draw several conclusions from this analysis. First, while it is reassuring that the program increased participants' self-reported awareness of campus resources and time management skills, this is somewhat expected. These two survey questions are quite aligned with the intervention, as information about resources and time-management skills are salient components of the program. Moreover, low-touch time management and information interventions on their own generally do not produce large, long-lasting effects (Oreopoulos and Petronijevic, 2018; Oreopoulos et al., 2022). Consequently, we do not view these to be primary channels.

Second, the program seems to improve students' morale or social-emotional state as evidenced by the large documented improvements in students' feelings of isolation related to their academic failure and their perceived support from the institution. Given these results, we posit that the *group* aspect of the program, where group members face the shared challenge of academic failure, plays an important role in producing the main results. Indeed, support groups, whereby individuals who share a common challenge come together to support one anther, have been proven effective in a variety of domains; e.g., Alcoholics Anonymous is an international support group for addicts, and there are numerous group therapy programs aimed at helping veterans manage and overcome Post Traumatic Stress Disorder (PTSD).

Another reason to think the program operates through improvements in morale is that the group of students who participate in the program are likely particulary discouraged. By design, the program is *targeted* to students who have been told by the university that they have performed inadequately, and thus may be more responsive to such an intervention. Indeed, we find that the groups of students who benefit the most from the program (males, STEM majors, students from low-SES households and first generation students), are also the ones who experience the largest penalties from academic probation as shown in Figure A5 and Figure A6. Moreover, Morisano et al. (2010) implement an RCT in a Canadian research institution using a one-time online goal-setting exercise and target academically struggling students based on their GPA, as we do in the current study. They find large and significant effects on GPA in the following months. On the other hand, when Oreopoulos and Petronijevic (2018) implement a similar intervention, but in a less targeted way as is highlighted in their paper, they find no meaningful effects on student outcomes further supporting the notion that targeting discouraged students is important.

Our mechanism conclusions are consistent with several recent studies. Drawing from the social psychology literature, Yeager and Walton (2011) show that one-time, appropriately timed interventions that are geared toward changing one's mindset can induce substantial long-lasting academic improvements. Specifically, these psychological interventions aim to change students' beliefs about their ability to improve their intelligence and their belief that they belonging in school. Related, Oreopoulos et al. (2020) find two light-touch behavioral interventions aimed at enhancing participants' social-emotional state are effective at improving students' sense of belonging and overall satisfaction with the university. Finally, Carrell and Kurlaender (2020) show that a light-touch faculty support intervention improves course performance and degree attainment for underrepresented students.

In summary, though it is difficult to conclusively identify which of the program's components are driving the documented improvements in student outcomes, guided by empirical evidence, recent literature, and the features of the coaching program, we speculate that the results are largely driven by improvements in participants' social-emotional state.

5.4 Magnitude Comparisons

Next, to provide context for the impact of SP, we compare our magnitudes to the point estimates obtained from similar interventions that improved retention and completion rates. For each study, we calculate a program cost-effectiveness index (CEI) in the spirit of Dynarski et al. (2013) for a standardized comparison. This index accounts for both the benefit and cost of a program by dividing the program's costs per student by the proportion of affected students. We summarize this comparison in Appendix C.

The total yearly cost of the SP program is \$64,107.50. This includes a fixed setup cost of \$5,174 to initiate the program and a yearly variable cost of \$58,933 which covers the time cost of the program director, staff, and coaches.[24] In the most recent cohort for which we have data (2018 cohort), 442 students were affected by the program, which yields a \$145.04 total cost per student. The main academic outcome affected is first-year student dropout, for which we find an 8.6 pp decline. This indicates that the cost of inducing an additional student to remain at the university following the first year is \$145.04/0.087 = \$1,667. Repeating this exercise for the imprecisely estimated 6-year graduation outcome, yields a CEI of \$3,626 (\$145.04/0.04) per graduated student.

[24] To get the hourly rate of the coaches, we use the average annual salary and benefits package for California State University professors where average benefits are 58% of the salary: (https://www2.calstate.edu/csu-system/faculty-staff/employee-profile/csu-faculty/Pages/average-salaries-for-full-time-faculty-by-rank-and-appointment-type.aspx). We obtain the hourly rate of the program director and support staff from the university budget office.

Bettinger and Baker (2014) study the most comparable intervention to SP, InsideTrack, a for-profit coaching program aimed at non-traditional students that costs $1,000 per year per student. The authors find it increases students' first-year persistence by 5.2 pp, second-year persistence by 3.4 pp and graduation, for a subsample of students, by 4 pp. Thus, inducing a single student to persist after the first year costs $19,230. These costs are higher for 2-year persistence ($29,411) and graduation ($25,000). Additionally, Barr and Castleman (2018) analyze the Bottom Line advising program, which is far more intensive than SP, beginning in high school and continuing throughout college. They find a 5 pp increase in college graduation overall. The program costs $4,000 per student, resulting in a cost per additional degree completed of $80,000. Our program's cost-effectiveness is closest to the high school coaching program analyzed in Carrell and Sacerdote (2017), who find that inducing an additional high school student to attend college costs $2,400.

To summarize, SP is substantially more cost-effective than other successful coaching/mentoring programs because it costs much less while still remaining effective. We speculate that this is largely because the program is mandatory, targeted to a particularly vulnerable group, and is conducted in a group setting. Previous work posits that students on academic probation, for whom the program is required, may be particularly discouraged (Lindo et al., 2010). As such, this group may be relatively more responsive to a coaching intervention aimed at improving self-confidence. While many of the similar higher-touch programs are also delivered in person (see Appendix C), none are mandatory and none target students who are directly at risk of dropping out. Gordanier et al. (2019) target students mid-semester who are at risk of failing a course with a peer-advising intervention, but they focus on final exam scores as the outcome rather than retention and completion. Indeed, they find students who just qualify for the intervention experience significant improvements in their final exam scores. Lastly, SP likely eases feelings of isolation in a cost-saving way, because it is conducted in a group setting, rather than one-on-one as the other programs do.

6 Validity of the Research Design

Standard RD assumption (A1). The identifying assumption required for a valid RD design is that individuals are not able to manipulate the running variable. If individuals can influence which side of the cutoff they are on, it will call into question the causal interpretation of the point estimates as it will be difficult to distinguish between student sorting and the true effect of the intervention. In our setting, this could occur if instructors or students strategically manipulate grades in such a manner that the distribution of observable and/or unobservable characteristics of students are discontinuous at the 2.0 GPA cutoff. Although this is a fundamentally untestable assumption, we provide several indirect tests that support its plausibility.

First, it is unlikely that instructors would be able to strategically manipulate a student's entire quarterly GPA since they are generally responsible for only one of three or four course grades. Second, we check for manipulation around the 2.0 threshold by plotting the distribution of student

GPAs for all cohorts, as shown in Figure A4a. While there are two large density spikes at the GPA cutoffs of 2.0 and 3.0, these heaping patterns are similar across treatment (SP-eligible) cohorts and control (pre-SP) cohorts, as shown in Figure A4b and Figure A4c. Since overall GPA patterns did not change with the implementation of SP, any heaping will be differenced out with the DiRD research design, mitigating concerns that heaping is biasing the DiRD estimate.

More generally, heaping at round GPA points is not necessarily indicative of manipulation. It is possible that discontinuities in the GPA distribution are linked to other exogenous factors such as grade rounding. Natural, non-strategic, institutional grade bumps are common in many U.S. institutions and have been documented in GPA-based RD settings such as Zimmerman (2014) and Ost et al. (2018). To further alleviate concerns over grade heaping, Table B4 presents results from a donut DiRD design which involves dropping the heaping points at the 2.0 and 3.0 GPA cutoff following Barreca et al. (2016). This exercise yields similar results to those obtained from our main specification. We conclude that the heaping observed in our data is most likely non-strategic and due to natural grade rounding, as observed in previous studies.

Finally, we examine whether observable student characteristics evolve similarly around the 2.0 GPA threshold. If individuals are unable to manipulate the side of the threshold they fall on, we should observe no differences in predetermined characteristics across the cutoff. To implement this, we estimate a series of balance tests using Equation (1). Indeed, for the three different bandwidth windows of 0.5, 0.75, and 1 GPA points on either side of the cutoff, we find no evidence of differences in discontinuities for any of the nine observable predetermined student characteristics. Results are reported in Table 5. To summarize these effects, we construct predicted dropout and predicted first-year GPA outcomes for each student based on these nine baseline covariates and estimate our main specification. If no GPA manipulation is present, the estimates should not be statistically different from zero. The DiRD treatment estimates for these predicted outcomes are presented in Table B5 and, in fact, are statistically insignificant at the cutoff. In summary, the fact that observable student characteristics appear to be smooth across the threshold further alleviates concerns over GPA manipulation. Altogether, the findings from these empirical tests indicate that the DiRD design should purge our estimates of any such unobservable bias—assuming the unobservable differences are also constant across cohorts.

The confounding policy is constant over time (A2). For the DiRD estimate to be valid, it is necessary that the effect of the confounding policy (here, academic probation) has the same effect before and after the introduction of the policy of interest (here, SP). First, the probation policy and its implantation did not change over the sample period. We verified this by consulting each year's Student Handbook. We also spoke with administrators from the University's retention office who further confirmed this information. Second, Figure A1 provides empirical evidence that the assignment rule is the same before and after program implementation. The first stage for the probation policy is sharp around the 2.0 cutoff for treatment and control cohorts.

Another way to empirically assess the plausibility of this assumption is to analyze how the effect of probation evolves over time. If the effect is similar across the different control cohorts (i.e., no preexisting trends), then it suggests that the effect of probation is constant over time. To test for this, we separately estimate the RD coefficient for each cohort for the two main outcomes: Q3 dropout and Q2 + Q3 standardized GPA. We focus on these outcomes as this is where we document significant SP impacts. All estimates rely on a bandwidth of 1 GPA point, with the treatment defined as scoring below a 2.0 GPA.

To most easily assess the dynamics of the effect of probation, Figure 3 plots these RD estimates by cohort. Robust standard errors are reported in bars. The first three estimates, those displayed before the dashed vertical line, are probation effects for students enrolled in our two control cohorts (2007, 2008) and the two colleges that were not exposed to the pilot program in 2009. All estimates after the dashed line represent probation effects for the treated cohorts (i.e., the four pilot colleges in 2009 and the 2010–2017 cohorts).

For both outcomes, the effect of probation is quite similar over time as evident among the control cohorts. In Figure 3a, the first three estimates show positive and mostly significant effects on dropout rates for students just eligible for probation alone, and these probation effects are similar across the three control cohorts. Once SP is introduced, the positive dropout effects dissipate, suggesting that SP likely has a moderating effect on probation. Figure 3b displays a similar pattern. Q2 + Q3 standardized GPA is unaffected for the three control cohorts on probation alone, while the cohorts also exposed to SP are positively affected. Importantly, the "pre-trend" patterns in both figures seem to indicate that probation had a similar effect on outcomes regardless of cohort.

Perhaps most compelling is comparing the two different RD estimates from 2009 (2009-1 and 2009-2). Here the year is held constant, but two colleges were exposed only to probation while the other four were exposed to both probation and SP. It is unlikely that the probation policy would differ within the same year. As such, it is reasonable to interpret the difference in the two 2009 estimates as the impact of SP.

Finally, as a placebo test, we estimate Equation (1) but use Q5 GPA (first quarter of the second year) as the running variable where the outcome is year 2 dropout. Recall that scoring below at 2.0 GPA in Q5 still places a student on probation, but importantly there is no SP in year 2. As such, if something else is driving our main result; e.g., the effect of probation changing at the same time as SP implementation, then we should expect the DiRD estimates to be economically and statistically significant. We do not find this to be the case. The point estimates from this exercise are reported in Table B8 and are indeed small and insignificant. Overall, the weight of the evidence suggests that the confounding probation policy had the same effect before and after the introduction of SP.

LATEs are additively separable (A3). A third assumption is that the two LATEs estimated are additively separable. In other words, A3 will hold if in expectation the effect of the treatment does not interact with the confounding policy. An intuitive way to assess this assumption in our setting is to ask, would the coaching program generate the same LATE if it were mandatory for students around the 2.0 cutoff but who were not also on probation? If A3 holds, our DiRD estimate will capture the effect of coaching. If it does not, then our estimate can be viewed as capturing the impact of coaching in the presence or threat of probation. Unfortunately, we cannot test this empirically as we do not observe SP participation absent of probation. Consequently, we cannot rule out that the estimated effects of the program rely on probation status. In fact, we posit that part of what makes coaching so effective in this setting given the intensity of the treatment, is that it targets students who have low morale from the academic probation label and thus are likely to be more responsive to such an intervention. Given that some form of academic probation is a universal policy in the US, even if A3 fails to hold, our results have broad implications.

7 Labor Market Analysis

7.1 Data

To estimate the impact of the coaching program on labor market outcomes, we link the student-level education files to administrative data from the California Employment Development Department. Specifically, we combine two data sources used to administer the state UI program: quarterly earnings records and the Quarterly Census of Employment and Wages (QCEW).

The quarterly earnings records include total earnings in the relevant quarter for each employer–employee (firm) pair. The QCEW data contain earnings and employment data at the establishment-quarter level, which we aggregate to the firm level (summing across establishments in California) before linking to the earnings data. Both datasets include the universe of UI-covered employment in the state for the years 2000–2019.[25,26] As such, we will not observe labor market outcomes for the small share of students who work outside the state of California or for the Federal government.[27] The labor market data are linked to the education files at the student level via social security number.[28] The linked data allow us to construct several labor market outcomes of interest for each student-quarter: log of total earnings, an indicator for employment, and the cumulative quarters of covered work experience since entering the university. We use the Consumer Price Index for All Urban Consumers to adjust dollar amounts to 2019.

For our main analysis, we limit the data to quarters 25–36 (7–9 years) after initial enrollment (where Q1 is the fall quarter of the student's first year) to ensure that earnings are measured

[25]Per Gurantz (2019), the Employment Development Department has estimated that 92% of employed Californians are included in the data.

[26]While more recent data is available, we exclude observations in 2020 to avoid employment and earnings outcomes affected by the Covid-19 pandemic, since only treated cohorts were exposed to this shock in the relevant age range.

[27]A subset of these data has been used in a series of policy briefs on UI in California during the pandemic (Bell et al., 2022, 2020). Similar and/or related data has also been used in other post-secondary education contexts (e.g., Bleemer and Mehta, 2020; Gurantz, 2019; Hoekstra, 2009; Ost et al., 2018; Zimmerman, 2014).

[28]Of the 45,864 students in our main sample, 43,081 (94%) were employed in at least one quarter following entry to the university.

at similar ages for treated and control cohorts. Despite this restriction, we cannot observe labor outcomes in all years (i.e., in every year of years 7 to 9) for students who enrolled at the university after 2011 because our data ends in 2019 Q4. Specifically, control cohorts are always observed in each year 7–9 relative to entry, while treatment cohorts entering after 2011 are observed in only some of these years (e.g., the 2012 cohort is observed only in years 6–8.). To alleviate concerns over having an unbalanced panel in our main analysis, we present additional estimates in Table B11 using only cohorts that enrolled at the university in 2011 or earlier. This creates a balanced panel across treatment and control groups but reduces the sample size. Overall, results are consistent across balanced and unbalanced samples.

Since the typical student is 18 at enrollment, we observe labor market outcomes at approximately ages 24–26. The final dataset used in our labor market analyses includes 127,626 student-quarters that meet these criteria and have GPAs within 1 point of the 2.0 cutoff. Summary statistics for the analysis sample are reported in Table B10. Average quarterly earnings are $10,857 (approximately $43,428 annually) and 72% of students are employed.

7.2 Results

The program we study has the potential to affect student's earnings in their mid-20s in different ways. Suppose, in general, that earnings increase in tandem with level of education and work experience. On the one hand, the program may increase participants' level of education. Indeed, we find it increases the likelihood of obtaining a bachelor's degree for certain groups. On the other hand, the program likely reduces work experience as remaining in school delays entering the labor force, especially relative to the control group who may have dropped out and started working right away. As these two channels are countervailing, the effect of the program on earnings ex ante is unclear.

Our labor market estimates are taken from a variant of Equation (1), which we run at the student-quarter level. We cluster standard errors at the student level, and each student-quarter is weighted by the inverse of the number of quarters the student is present in the sample. We report DiRD estimates for various labor market outcomes in Table 6. The top panel of Table 6 reveals that for the full sample, the coaching program does not appear to produce significant labor market effects. There is suggestive evidence that treated students experience higher earnings—the program leads to a 9% earnings gain (column 1), but the estimate does not attain statistical significance at conventional levels. These results are broadly similar if we restrict our sample to cohorts entering in or before 2011 (i.e., our balanced data in Table B11.).

To better understand the potential labor market effects of the program, we separately estimate its effects for various subgroups. The bottom panel of Table 6 reports DiRD estimates separately for women, men, STEM and non-STEM majors, and those from lower- and higher-income families. We previously showed that the program's educational benefits are concentrated among men, low-income students and students in STEM majors. As a result, we expect any potential labor market benefits to be concentrated among these particular subgroups. Indeed, we find labor market effects concentrated among the marginal students in these groups. The program increases earnings for men and lower-SES students in years 7–9 after entry by 25.7–31% (column 1). While large, the magnitudes of these point estimates are comparable to earnings estimates among men of attending a 4-year university or a higher-quality university (Hoekstra, 2009; Zimmerman, 2014). We also observe similarly positive but less precisely estimated effects on earnings among STEM majors.[29]

To provide further support for these results, Figure A7 and Figure A8 show regression discontinuity plots of log earnings separately for our control (left panels) and treatment cohorts (right panels). The visual evidence is quite consistent with our DiRD estimates. For males, low-income and STEM students, control cohorts' graphs show visible discontinuities at the academic probation cutoff (Panel (e) of Figure A7 and panels (a) and (e) of Figure A8). These indicate that being below the cutoff (or solely on academic probation) reduces these subgroups' earnings at ages 24 to 26—but the drop in earnings at the cutoff is statistically significant at the 5% level only for males. On the other hand, these subgroups' treatment cohorts show no visible discontinuities in log earnings at the probation cutoff, suggesting that the coaching program eliminates the earnings penalty associated with academic probation (Panel (f) of Figure A7 and panels (b) and (f) of Figure A8). For the full sample, females, high-income and non-STEM students (i.e., the groups for which we do not find positive DiRD effects on earnings), both control and treatment cohorts' RD plots show no visible discontinuities in log earnings at the probation cutoff.

Columns 3 and 4 of Table 6 assess the degree to which the program impacts employment and work experience. If students are in school longer, they likely have less experience. For the full sample, estimates on employment status and cumulative quarters of experience are negative but imprecisely measured, which precludes us from making definitive conclusions for these two outcomes. For the subgroups that experience increases in their earnings (lower SES, males and STEM majors), we do not see any statistically significant effects. On the other hand, women are less likely to be employed and have fewer quarters of experience. One possible explanation for this somewhat surprising result is that women who complete the coaching program are potentially more likely to enroll in graduate studies thereby reducing their work experience. Unfortunately, we do not observe graduate course work in either of the administrative data sources. Instead, to investigate this possibility, we obtain 6 years of survey data from the Graduate Status Report administered by the university's Career Services office. The survey asks questions about a student's employment and graduate studies status in their first year following graduation. Although self-reported and responses are conditional on graduating and completing the survey, these data show that in the first year after graduating, women who completed the program were twice as likely to be enrolled in a graduate program compared to men. As such, it is very likely that the negative labor market estimates are a function of attending graduate school rather than an adverse response to the

[29] Results are similar if we restrict the analysis to cohorts in or before 2011, as shown in the bottom panel of Table B11

program.[30]

Since we do not observe earnings outside of California, our results are potentially subject to biases induced by out of state migration. We follow several recommendations from Foote and Stange (2022) to deal with this issue. First, our preferred outcome (log earnings) limits the sample to student-quarters with positive earnings. While estimates for this outcome could be affected by differential sample selection, Foote and Stange (2022) note that this is often preferable to incorrectly assuming that the portion of zero-earning observations who have migrated are not working. Second, we test for an effect of the coaching program on in-state employment (nonzero earnings). While the program has a meaningful negative effect on this outcome for females, this is not the case within the subgroups where positive earnings effects of the coaching program are concentrated.

It is worth noting that there are two caveats with our labor market results. First, we observe wages when students are in their mid-twenties, which implies that some students are still completing their bachelor's degree or pursuing graduate studies. Second, we acknowledge that the confidence intervals on labor market estimates are often large due to our reduced sample size. Nonetheless, there are several reasons why we view our analysis as providing compelling evidence that the program positively impacts affected students' earnings. Our DiRD analysis clearly shows that the subgroups who incur earnings gains are the same subgroups who experience improvements in their academic outcomes as a result of the program. Additionally, results from our RD earnings plots are consistent with our DiRD estimates and provide striking visual evidence that the program raised earnings of males, low-income and STEM students.

In summary, while reduced precision precludes us from making definitive conclusions for the overall sample, our results nonetheless highlight that the coaching program substantially increases earnings for certain groups of students 7–9 years following entry to the university. Importantly, the labor market benefits are concentrated among those subgroups that experienced the largest academic gains: male students and lower-income students. Despite a few caveats, we view our analysis as an important step in understanding the full benefits of academic coaching, given the dearth of evidence on the effects of such interventions on labor market outcomes.

8 Conclusion

In an effort to boost college graduation rates, policymakers often propose providing students with coaching or mentoring. However, evidence for the success of these interventions is mixed. This paper evaluates the effectiveness of a mandatory coaching program targeting students placed on academic probation in the first quarter of their first year at a 4-year US university. Program participants attend a workshop in which they are provided with group coaching focused on improving self-confidence. They also reflect on their weaknesses, find solutions to address their challenges, and create an action plan. Several weeks later, students are required to meet one-on-one with their

[30] Across the six years, 25% of graduates responded to this post-graduation employment survey.

coach or use an academic support service.

We find that the coaching program largely boosts targeted students' academic outcomes. Overall, program participants experience substantial improvements in their academic performance and retention. Effects are concentrated among lower-income students who also experience an increase in the probability of graduating. Further heterogeneity analyses reveal that effects are also concentrated among men and students enrolled in STEM majors. For these most affected groups, we find that the program increases earnings 7–9 years following initial enrollment at the university.

Guided by empirical evidence, we speculate that the positive effects of the program are largely driven by improvements in students' morale and social-emotional state. Post-program, participants report feeling relatively more supported by faculty and less likely to feel that they are the only ones struggling. An interpretation of our results is that the program is highly effective at undoing the documented negative effects of academic probation, particularly for students from more disadvantaged backgrounds. While notifying students of their poor academic performance serves an important purpose and is likely here to stay, our findings suggest that institutions should consider adding a positively toned behavioral intervention alongside the standard academic probation message.

Our findings are timely and relevant as policymakers and researchers aim to address the college "completion crisis" in the US. We show that a relatively lighter-touch, short-term, but targeted coaching program can be an effective way to increase marginal students' college retention and long-run success. While the degree to which our findings can be replicated at scale remains an open question, results from this coaching program remain quite promising. From a policy perspective, our program's lower-cost and less complex structure makes it potentially easy to implement and scale.

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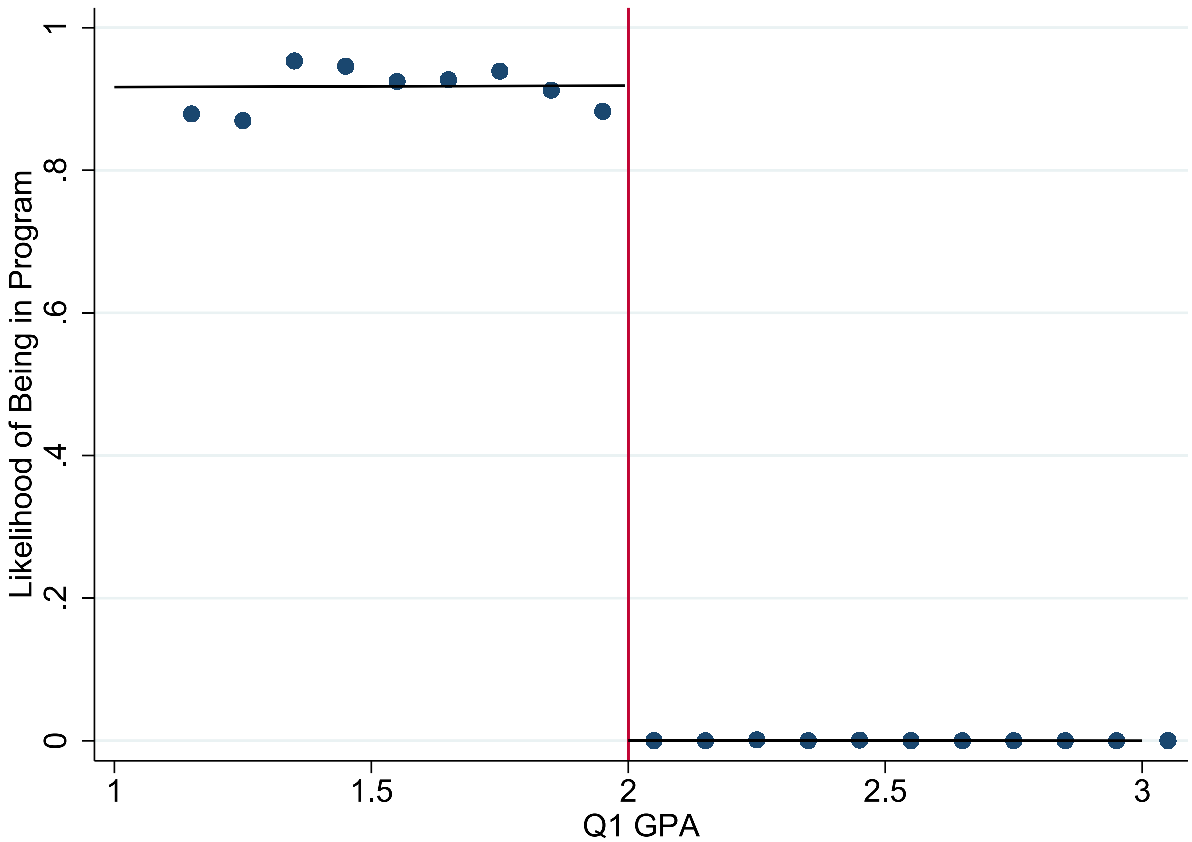
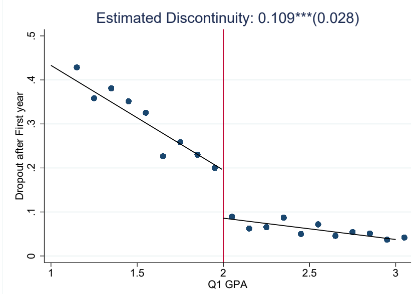
Figure 1: First Stage–Likelihood of Attending Coaching Program



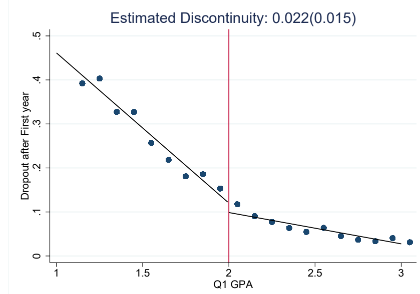
Notes: The sample includes students enrolled at the university after the implementation of the coaching "Success Program" (SP). This includes all first-year students entering the university in the fall cohorts 2010-2017. Circles represent local averages over a 0.1 GPA range. The figure is drawn using a linear fit on either side of the cutoff.

Figure 2: RD Figures for Academic Outcomes by Control and Treated Cohorts

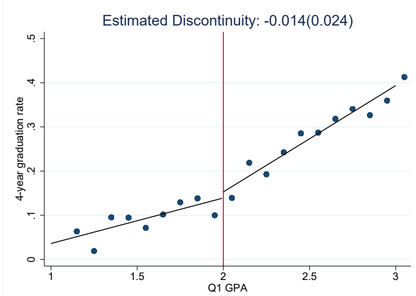
(a) Dropout After First Year (Control Cohorts)



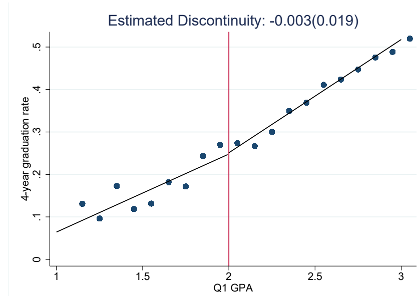
(b) Dropout After First Year (Treatment Cohorts)



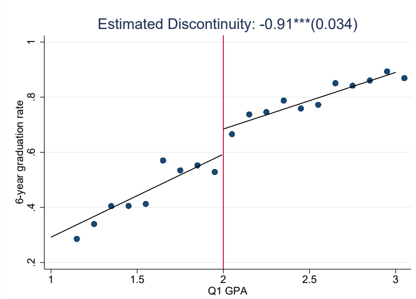
(c) 4-Year Graduation (Control Cohorts)



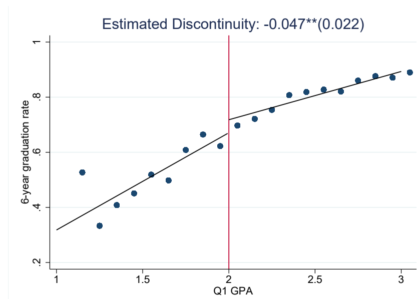
(d) 4-Year Graduation(Treatment Cohorts)



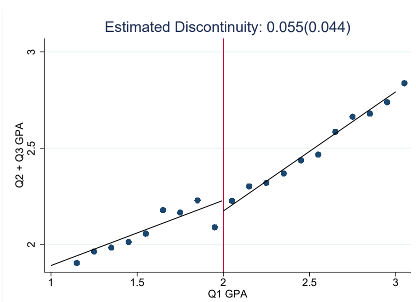
(e) 6-Year Graduation (Control Cohorts)



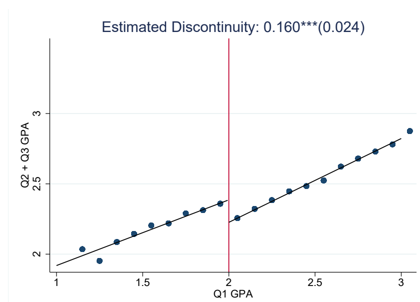
(f) 6-Year Graduation (Treatment Cohorts)



(g) Q2 + Q3 GPA (Control Cohorts)



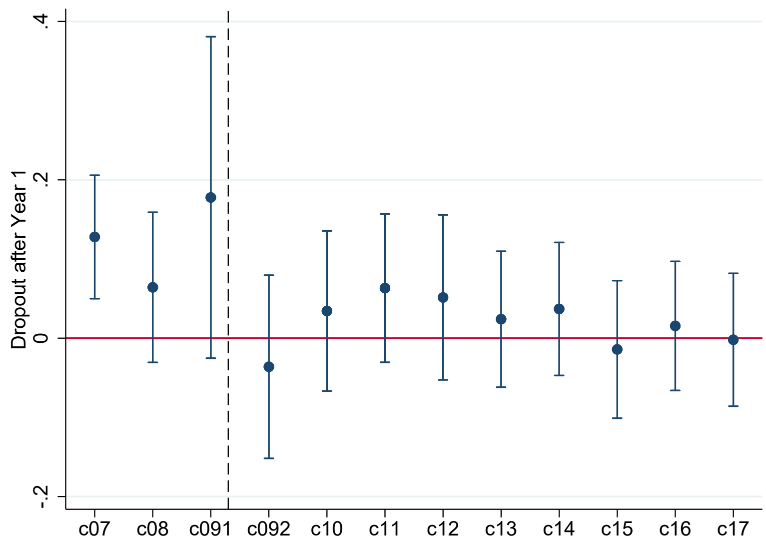
(h) Q2 + Q3 GPA (Treatment Cohorts)



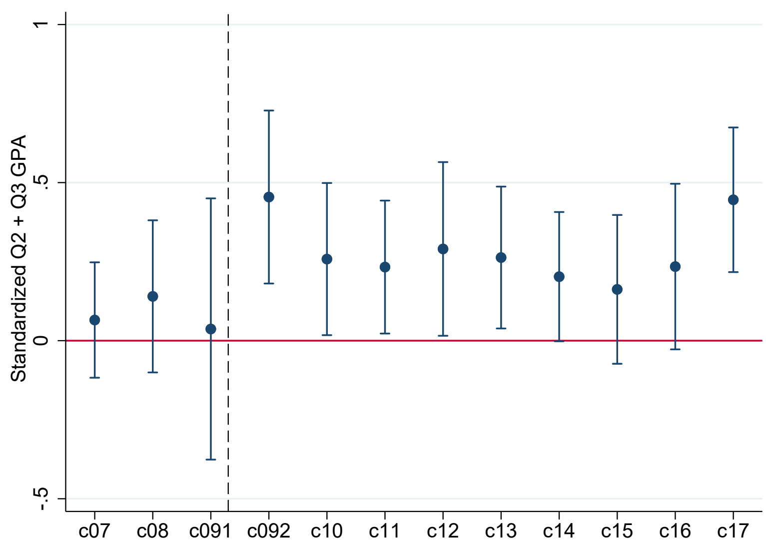
Notes: Control cohort figures include the sample of students enrolled at the university in 2007, 2008 and a subset of colleges in 2009. Treated cohort figures include the sample of students enrolled at the university in select colleges in 2009 and all cohorts from 2010 to 2017 (except for graduation outcomes which include up to the 2014 cohort). Circles represent local averages over a 0.1 GPA range. Figures are drawn using a linear fit on either side of the cutoff. Figure A2 shows these figures are robust to a parametric specification. Estimates and standard errors (in parentheses) are reported above each figure.

Figure 3: RD Estimates by Cohort

(a) Likelihood of Dropout After First Year (Q3)



(b) Standardized Q2 + Q3 GPA



Notes: Figures include all first-year students enrolled at the university in entering fall cohorts 2007-2017. Each point estimate is derived from a separate RD regression, using a bandwidth of 1 GPA point, for each cohort where treatment is defined as scoring below a 2.0 GPA. All point estimates to the left of the dotted line represent control cohorts (first-time freshman students enrolled in 2007, 2008 and two colleges in 2009, i.e. prior to the introduction of the SP). All point estimates to the right of the dotted line represent treated cohorts (first time freshman students enrolled in four colleges in 2009 and the 2010 to 2017 cohorts, i.e. after the introduction of SP). Bars represent upper and lower 95% confidence intervals for each point estimate.

Table 1: Summary Statistics

	(1) Full Sample	(2) Bandwidth=1 Q1 GPA$\in [1,3]$	(3) Pre-program yrs. Q1 GPA $\in [1,2)$ Probation	(4) Pre-program yrs. Q1 GPA $\in [2,3]$ No Probation	(5) Program yrs. Q1 GPA $\in [1,2)$ Probation + SP	(6) Program yrs. Q1 GPA $\in [2,3]$ Neither
Covariates						
HS GPA	3.84	3.73	3.50	3.67	3.61	3.78
	[0.45]	[0.44]	[0.38]	[0.42]	[0.47]	[0.43]
Female	0.48	0.44	0.30	0.46	0.37	0.46
	[0.50]	[0.50]	[0.46]	[0.50]	[0.48]	[0.50]
Non-White	0.38	0.42	0.47	0.35	0.50	0.43
	[0.49]	[0.49]	[0.50]	[0.48]	[0.50]	[0.49]
Remedial Math	0.02	0.03	0.05	0.05	0.04	0.02
	[0.14]	[0.17]	[0.22]	[0.23]	[0.19]	[0.15]
Remedial English	0.04	0.07	0.18	0.12	0.09	0.04
	[0.21]	[0.25]	[0.39]	[0.33]	[0.28]	[0.20]
Pell Grant Eligible	0.16	0.19	0.21	0.13	0.26	0.19
	[0.37]	[0.39]	[0.41]	[0.34]	[0.44]	[0.39]
EFC $< \$30,000$	0.40	0.43	0.43	0.34	0.49	0.44
	[0.49]	[0.49]	[0.50]	[0.47]	[0.50]	[0.50]
Father College +	0.80	0.77	0.70	0.81	0.71	0.77
	[0.40]	[0.42]	[0.46]	[0.39]	[0.45]	[0.42]
Mother College +	0.83	0.80	0.74	0.82	0.74	0.81
	[0.38]	[0.40]	[0.44]	[0.39]	[0.44]	[0.40]
Obs.	45,864	22,225	920	3,708	2,430	15,167
Outcomes						
Dropout Q1	0.01	0.01	0.04	0.01	0.03	0.01
	[0.10]	[0.11]	[0.20]	[0.09]	[0.18]	[0.10]
Dropout Q2	0.02	0.02	0.07	0.02	0.08	0.02
	[0.14]	[0.16]	[0.25]	[0.13]	[0.27]	[0.13]
Dropout Year 1	0.06	0.09	0.30	0.06	0.25	0.05
	[0.24]	[0.28]	[0.46]	[0.23]	[0.43]	[0.23]
Obs.	45,864	22,225	920	3,708	2,430	15,167
4-Yr Grad Rate	0.44	0.35	0.09	0.30	0.18	0.41
	[0.50]	[0.48]	[0.29]	[0.46]	[0.38]	[0.49]
Obs.	36,523	18,281	920	3,708	1,940	11,713
6-Yr Grad Rate	0.83	0.77	0.46	0.81	0.54	0.83
	[0.37]	[0.42]	[0.50]	[0.40]	[0.50]	[0.38]
Obs.	34,438	17,149	920	3,708	1,725	10,796
Q2 + Q3 GPA	2.87	2.53	2.09	2.54	2.21	2.59
	[0.64]	[0.57]	[0.63]	[0.55]	[0.61]	[0.53]
Obs.	44,523	21,421	842	3,619	2,178	14,782
Q2 + Q3 Total Credits	27.77	26.20	21.90	26.43	22.68	26.97
	[5.97]	[6.22]	[7.86]	[5.45]	[8.00]	[5.68]
Obs.	45,864	22,225	920	3,708	2,430	15,167
Treatment						
Probation Ever*	0.38	0.57	1.00	0.53	0.99	0.43
	[0.48]	[0.50]	[0.05]	[0.50]	[0.08]	[0.50]
Probation Yr 1*	0.25	0.41	1.00	0.31	0.99	0.28
	[0.43]	[0.49]	[0.06]	[0.46]	[0.09]	[0.45]
Probation Q1*	0.11	0.17	0.99	–	0.98	–
	[0.31]	[0.38]	[0.09]		[0.12]	
Obs.	45,864	22,225	920	3,708	2,430	15,167
SP Participant Fall**	0.06	0.12	–	–	0.84	–
	[0.24]	[0.32]			[0.37]	
Obs.	37,244	17,597			2,430	

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Standard deviations are in brackets. *The summary statistics reported for the three probation variables (Probation Ever, Probation Yr 1 and Probation Q1) are based only on the 2007-2009 and 2017 entering fall cohorts. The probation variable is not available for the other years. **The reported means for the SP Participant Fall variable are based only on the program years (entering cohorts 2010-2017 and the subset of colleges that participated in the pilot in 2009) as this variable is undefined for the other years.

Table 2: DiRD Estimates for Academic Outcomes

	(1) Dropout Q1	(2) Dropout Q2	(3) Dropout Q3	(4) GPA (Q2 + Q3)	(5) Earned Credits (Q2 + Q3)	(6) Grad. 4 yr.	(7) Grad. 6 yr.
Bandwidth= 0.75	-0.011	0.000	-0.073**	0.168*	1.305*	0.022	0.078
	(0.016)	(0.021)	(0.037)	(0.089)	(0.682)	(0.036)	(0.048)
With Controls	-0.011	0.000	-0.069*	0.149*	1.266*	0.004	0.068
	(0.016)	(0.021)	(0.037)	(0.085)	(0.679)	(0.034)	(0.047)
Observations	14,407	14,407	14,407	13,821	14,407	11,895	11,109
Bandwidth= 1	-0.008	0.001	-0.088***	0.165**	1.241**	0.011	0.044
	(0.014)	(0.018)	(0.032)	(0.076)	(0.589)	(0.031)	(0.041)
With Controls	-0.008	0.001	-0.085***	0.146**	1.189**	-0.012	0.037
	(0.014)	(0.018)	(0.032)	(0.073)	(0.584)	(0.029)	(0.040)
Observations	22,225	22,225	22,225	21,421	22,225	18,281	17,149

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. Figure A3 reports the DiRD estimate for each outcome for a range of bandwidths. Robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: DiRD Estimates for Academic Outcomes by Gender, Field of Study and SES

	(1) Dropout Q1	(2) Dropout Q2	(3) Dropout Q3	(4) GPA (Q2 + Q3)	(5) Earned Credits (Q2 + Q3)	(6) Grad. 4 yr.	(7) Grad. 6 yr.
Female	-0.020	0.002	-0.077	-0.152	0.837	-0.000	0.033
	(0.025)	(0.031)	(0.052)	(0.124)	(1.033)	(0.059)	(0.066)
Male	-0.001	0.001	-0.084**	0.303***	1.316*	-0.012	0.027
	(0.016)	(0.022)	(0.041)	(0.091)	(0.715)	(0.031)	(0.051)
STEM	-0.003	0.010	-0.125***	0.236**	0.962	-0.018	0.029
	(0.018)	(0.024)	(0.043)	(0.097)	(0.756)	(0.033)	(0.054)
Non-STEM	-0.018	-0.016	-0.022	-0.010	1.450	0.010	0.029
	(0.022)	(0.027)	(0.045)	(0.113)	(0.952)	(0.052)	(0.061)
Lower SES	-0.012	-0.015	-0.125**	0.200*	1.912**	-0.064	0.133**
	(0.020)	(0.026)	(0.050)	(0.113)	(0.879)	(0.043)	(0.060)
Higher SES	-0.006	0.013	-0.057	0.111	0.586	0.032	-0.023
	(0.019)	(0.025)	(0.042)	(0.096)	(0.791)	(0.040)	(0.053)
Obs. (Female)	9,798	9,798	9,798	9,465	9,798	7,926	7,536
Obs. (Male)	12,427	12,427	12,427	11,956	12,427	10,355	9,613
Obs. (STEM)	10,735	10,735	10,735	10,339	10,735	8,971	8,348
Obs. (Non-STEM)	11,490	11,490	11,490	11,082	11,490	9,310	8,801
Obs. (Lower SES)	9,491	9,491	9,491	9,158	9,491	7,772	7,254
Obs. (Higher SES)	12,734	12,734	12,734	12,263	12,734	10,509	9,895

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, math and English remedial status, Pell Grant eligibility status, whether EFC is less than $\$30,000$, indicators for whether parents attended college, and cohort and college fixed effects. All regressions use a bandwidth of 1 grade point on either side of the cutoff. The STEM subsample includes students in the college of engineering, architecture and sciences. The non-STEM subsample includes students who are in the college of agriculture, business and liberal arts. Lower SES is defined as students with an expected family contribution (EFC) less than $30,000. Higher SES are those with an EFC greater than or equal to $30,000, and includes those who have a missing EFC. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. Table B9 shows that these results are robust to dropping the 2009 cohort, the year of the pilot program. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Mechanism Exploration

	(1) Resources Awareness	(2) Time Management	(3) Attend Class	(4) Connected Socially	(5) Motivated Academically	(6) Not Along in Failure	(7) Faculty Cares
Post	0.753***	0.256***	0.037	-0.005	0.009	0.228***	0.425***
	(0.029)	(0.033)	(0.034)	(0.026)	(0.017)	(0.015)	(0.017)
Obs.	430	430	430	430	430	1,842	1,842
R-squared	0.742	0.219	0.005	0.000	0.001	0.215	0.402
Pre-program mean	0.19	0.65	0.74	0.87	0.95	0.75	0.48

Notes: The sample uses data taken from pre- and post-SP program surveys. All regressions include a post program indicator "Post" and individual fixed effects. Columns 1-5 include estimates for only Fall 2013 participants because the survey was changed after that year to include different questions. Columns 6 and 7 include estimates for Fall 2015, 2016, 2017 and 2018 as well as Winter 2017, 2018 and 2019 participants. Standard errors are reported in parentheses and are clustered at the individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Baseline Covariates Balance Check for DiRD Research Design

	(1) HS GPA	(2) Female	(3) Non-White	(4) Remedial Math	(5) Remedial English	(6) Pell elig.	(7) EFC < $30K	(8) Father college	(9) Mother college
Bandwidth= 0.5	-0.070	-0.010	0.047	-0.006	0.022	-0.002	0.001	0.017	0.028
	(0.045)	(0.051)	(0.056)	(0.027)	(0.041)	(0.046)	(0.057)	(0.052)	(0.049)
Bandwidth= 0.75	-0.030	-0.022	0.029	-0.029	-0.032	-0.044	-0.055	0.031	0.019
	(0.037)	(0.043)	(0.047)	(0.022)	(0.034)	(0.038)	(0.047)	(0.043)	(0.041)
Bandwidth= 1	0.005	0.010	0.050	-0.020	-0.008	-0.015	-0.020	0.013	-0.028
	(0.032)	(0.037)	(0.040)	(0.018)	(0.029)	(0.033)	(0.040)	(0.037)	(0.035)
Observations (BW=0.5)	8,973	8,973	8,973	8,973	8,973	8,973	8,973	8,973	8,973
Observations (BW=0.75)	14,407	14,407	14,407	14,407	14,407	14,407	14,407	14,407	14,407
Observations (BW=1)	22,225	22,225	22,225	22,225	22,225	22,225	22,225	22,225	22,225

Notes: The sample includes all first-time freshmen enrolled at the university in entering fall cohorts 2007-2017. Each point estimate is from a separate regression. The estimation equation is presented in Equation (1). DiRD estimates are equivalent to differencing two local linear RD regressions. Robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: DiRD Employment Results

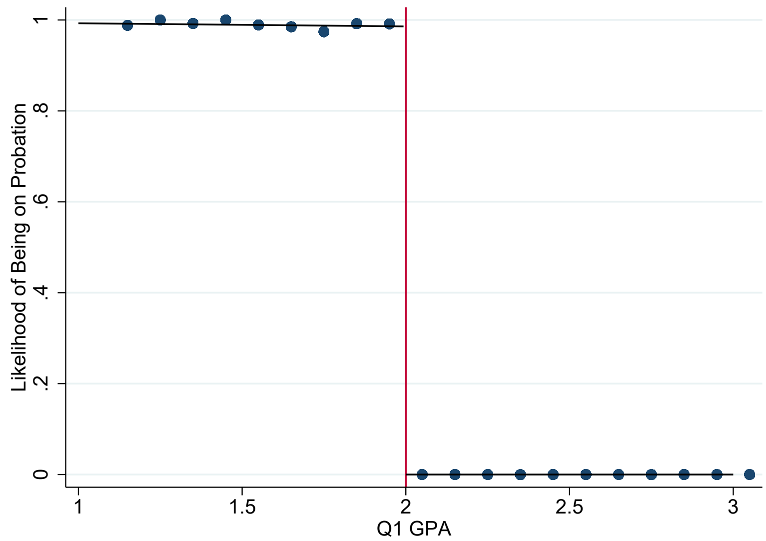
	(1) Log Earnings	(2) Employed	(3) Cum. Exper. (Qtrs)
All	0.0951	–0.0203	–1.28*
	(0.0961)	(0.0342)	(0.699)
Control Mean	12,288	0.752	18.1
Observations	92,200	127,626	127,626
Female	–0.182	–0.0931*	–2.40**
	(0.150)	(0.0549)	(1.08)
Male	0.257**	0.0126	–0.773
	(0.123)	(0.0437)	(0.901)
STEM	0.148	–0.0311	–1.05
	(0.133)	(0.0476)	(0.953)
Non-STEM	–0.00221	–0.00829	–1.67
	(0.144)	(0.0511)	(1.05)
Lower SES	0.311**	0.0195	0.0602
	(0.154)	(0.0516)	(1.08)
Higher SES	–0.0740	–0.0489	–2.22**
	(0.121)	(0.0458)	(0.914)
Ctrl Mean (Female)	10,954	0.740	18.5
Obs. (Female)	39,148	53,480	53,480
Ctrl Mean (Male)	13,442	0.769	17.7
Obs. (Male)	53,052	74,146	74,146
Ctrl Mean (STEM)	14,421	0.734	15.3
Obs. (STEM)	46,379	66,038	66,038
Ctrl Mean (Non-STEM)	12,622	0.772	17.9
Obs. (Non-STEM)	45,821	61,588	61,588
Ctrl Mean (Lower SES)	11,090	0.794	19.4
Obs. (Lower SES)	37,292	51,566	51,566
Ctrl Mean (Higher SES)	13,023	0.730	17.3
Obs. (Higher SES)	54,908	76,060	76,060

Notes: The sample is limited to quarters 7-9 years since students enrolled at university where the unit of observation is the student-quarter. The last calendar quarter included in the sample is 2019, Quarter 4. Table B11 reports result using cohort that enrolled in or before 2011. Dollar amounts have been adjusted to 2019 dollars using the Consumer Price Index for All Urban Consumers. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. All regressions use a bandwidth of 1 grade point on either side of the cutoff. Standard errors are cluster-robust at the student level, and regressions are weighted by one over the number of quarters in which a given student is present in the regression sample. *** p<0.01, ** p<0.05, * p<0.1.

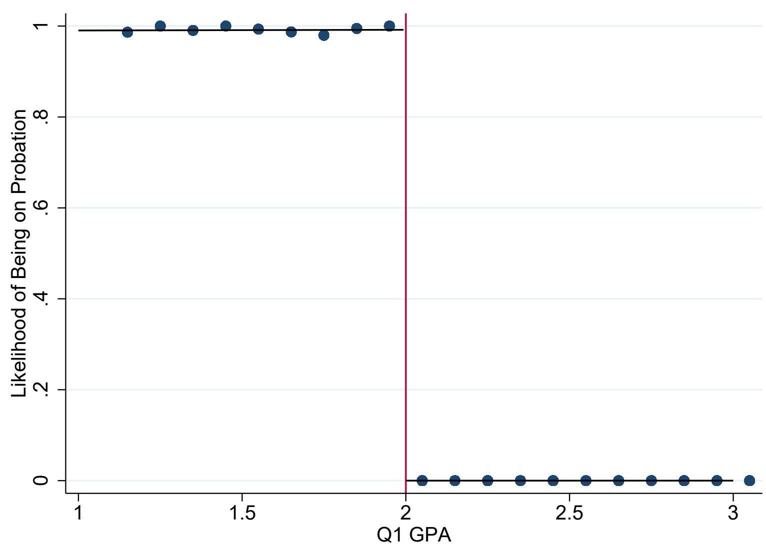
A Appendix Figures

Figure A1: Likelihood of Probation Following Q1

(a) Pre-Program (Control) Cohorts



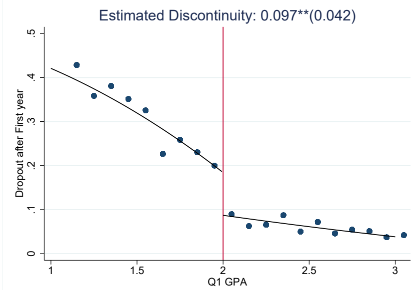
(b) Program (Treated) Cohorts



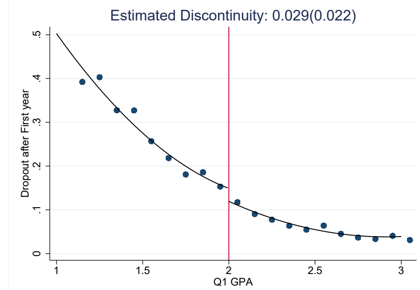
Notes: The sample includes all first-year students enrolled at the university in entering fall cohorts 2007, 2008, 2009 and 2017. 2010-2016 cohorts are excluded because the probation variable is missing. The running variable is first quarter GPA in both figures. Circles represent local averages over a 0.1 GPA range. Figures are drawn using a linear fit on either side of the cutoff.

Figure A2: RD Figures for Academic Outcomes by Control and Treated Cohorts—Parametric Estimation

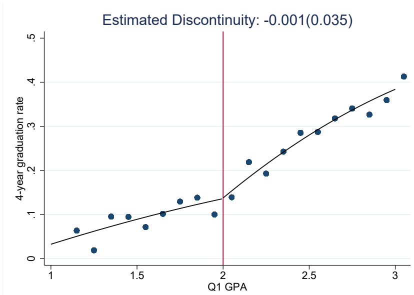
(a) Dropout After First Year (Control Cohorts)



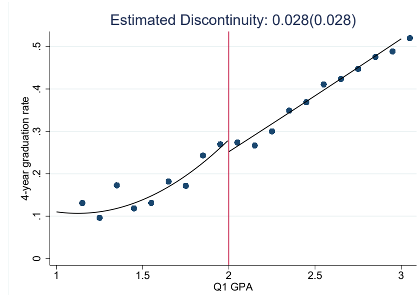
(b) Dropout After First Year (Treatment Cohorts)



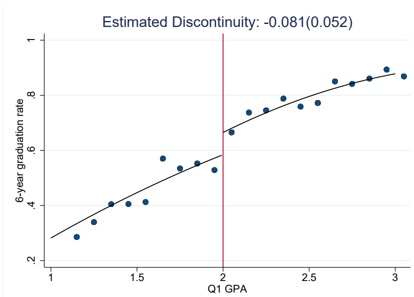
(c) 4-Year Graduation (Control Cohorts)



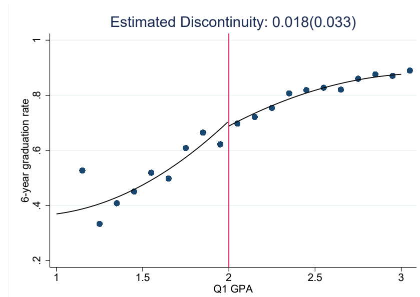
(d) 4-Year Graduation(Treatment Cohorts)



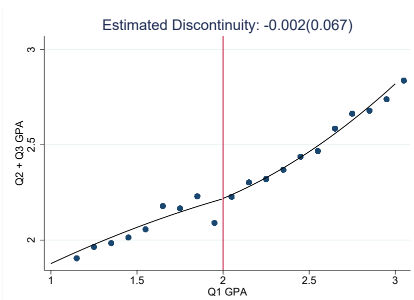
(e) 6-Year Graduation (Control Cohorts)



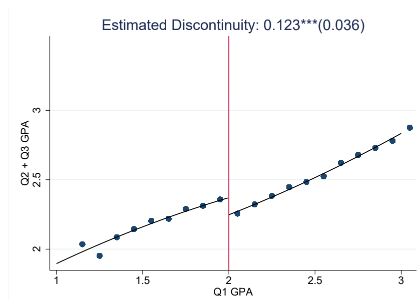
(f) 6-Year Graduation (Treatment Cohorts)



(g) Q2 + Q3 GPA (Control Cohorts)



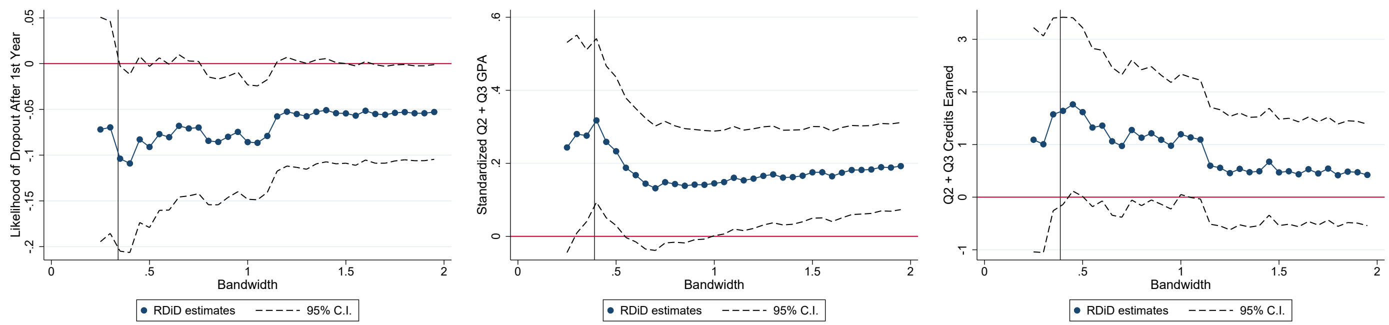
(h) Q2 + Q3 GPA (Treatment Cohorts)



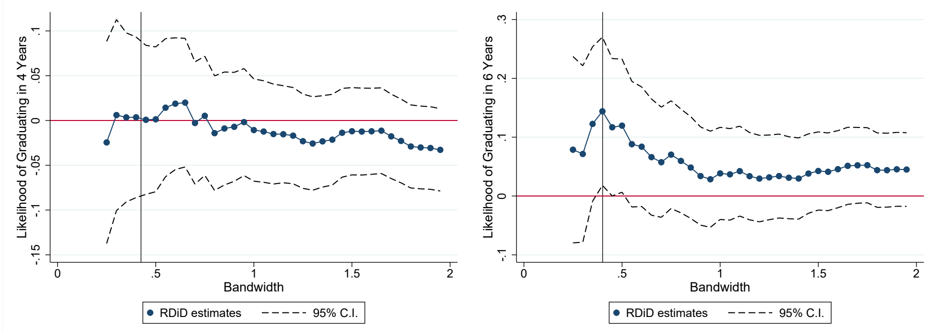
Notes: Control cohort figures include the sample of students enrolled at the university in 2007, 2008 and a subset of colleges in 2009. Treated cohort figures include the sample of students enrolled at the university in select colleges in 2009 and all cohorts from 2010 to 2017 (except for graduation outcomes which include up to the 2014 cohort). Circles represent local averages over a 0.1 GPA range. Figures are drawn using a quadratic fit on either side of the cutoff. Estimates and standard errors (in parentheses) are reported above each figure.

Figure A3: DiRD Estimates for Academic Outcomes by Bandwidth

(a) Dropout End of Yr 1 (b) Standardized Q2 + Q3 GPA (c) Q2 + Q3 Earned Credits



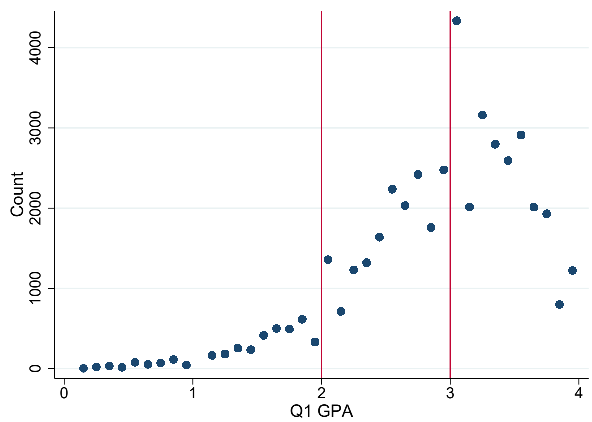
(d) Graduate in 4 Years (e) Graduate in 6 Years



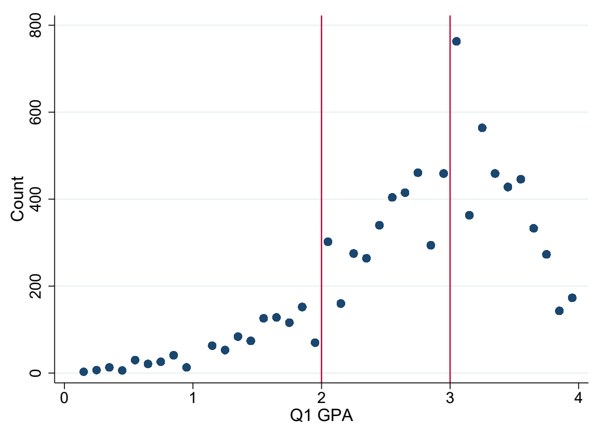
Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. Dashed lines represent 95% confidence intervals. DiRD estimates are equivalent to differencing two local linear RD regressions. The vertical black lines indicate the optimal bandwidth calculated using the CCT procedure as described in Calonico et al. (2014).

Figure A4: Bunching at Whole GPA Cutoffs

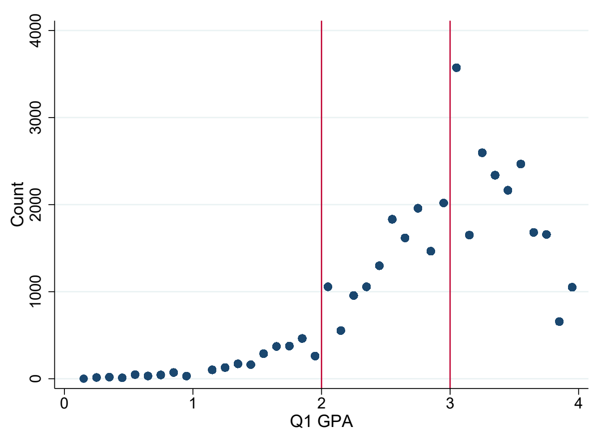
(a) Distribution of Q1 GPA (All Cohorts)



(b) Distribution of Q1 GPA (Control Cohorts)



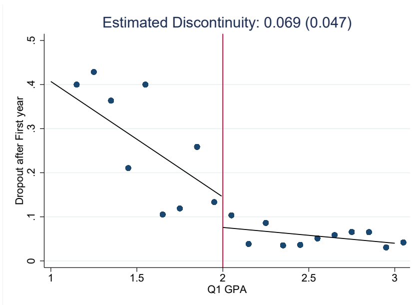
(c) Distribution of Q1 GPA (Treated Cohorts)



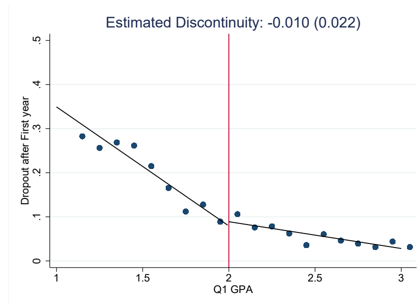
Notes: The sample used in Panel A includes all first-year students entering the university in fall cohorts 2007-2017. The sample used in Panel B includes cohorts never exposed to SP (2007, 2008 and part of 2009 cohort). The sample in Panel C includes all cohorts exposed to SP (the four colleges of the 2009 cohort and 2010-2017 cohorts).

Figure A5: RD Figures for Yr 1 Dropout by Control and Treated Cohorts for Subgroups

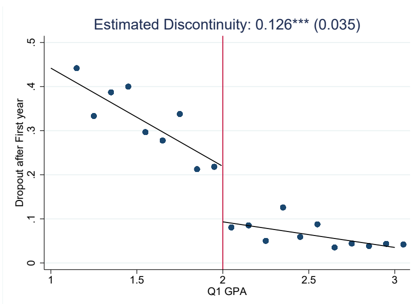
(a) Females (Control Cohorts)



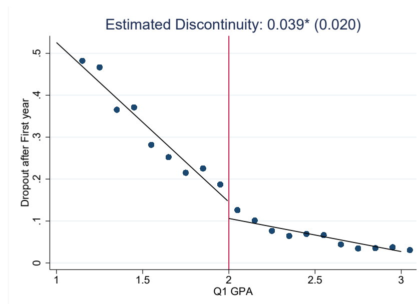
(b) Females (Treatment Cohorts)



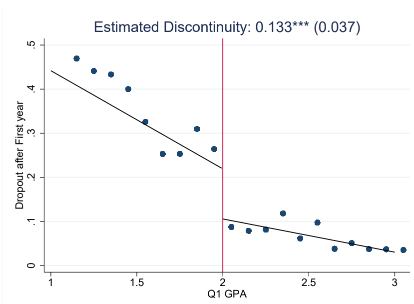
(c) Males (Control Cohorts)



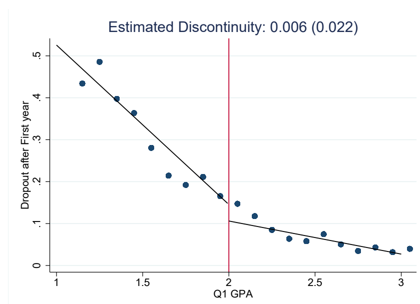
(d) Males (Treatment Cohorts)



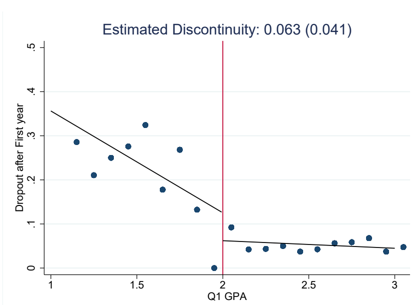
(e) STEM (Control Cohorts)



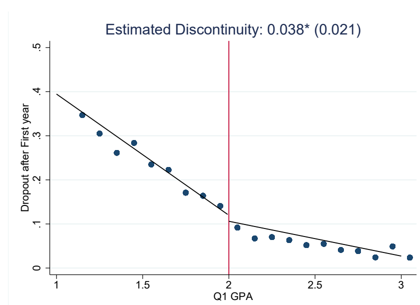
(f) STEM (Treatment Cohorts)



(g) Non-STEM (Control Cohorts)



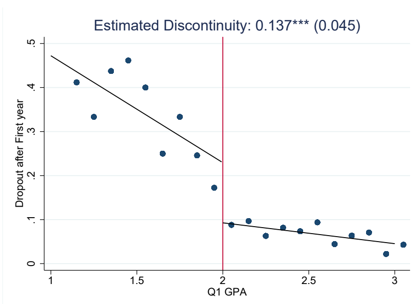
(h) Non-STEM (Treatment Cohorts)



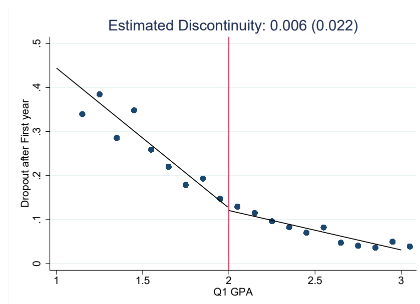
Notes: Control cohort figures include the sample of students enrolled at the university in 2007, 2008 and a subset of colleges in 2009. Treated cohort figures include the sample of students enrolled at the university in select colleges in 2009 and all cohorts from 2010 to 2017 (except for graduation outcomes which include up to the 2014 cohort). The STEM subsample includes students in the college of engineering, architecture and sciences. The non-STEM subsample includes students who are in the college of agriculture, business and liberal arts. Circles represent local averages over a 0.1 GPA range. Figures are drawn using a linear fit on either side of the cutoff. Estimates and standard errors (in parentheses) are reported above each figure.

Figure A6: RD Figures for Yr 1 Dropout by Control and Treated Cohorts for Subgroups

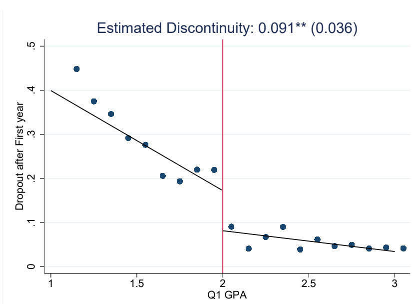
(a) Lower SES (Control Cohorts)



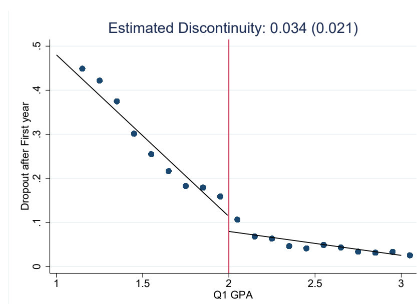
(b) Lower SES (Treatment Cohorts)



(c) Higher SES (Control Cohorts)



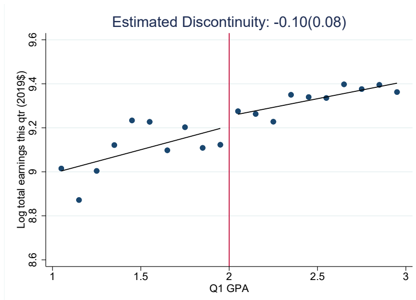
(d) Higher SES (Treatment Cohorts)



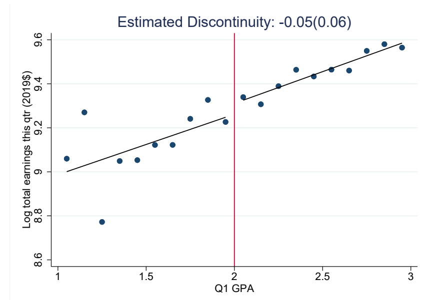
Notes: Control cohort figures include the sample of students enrolled at the university in 2007, 2008 and a subset of colleges in 2009. Treated cohort figures include the sample of students enrolled at the university in select colleges in 2009 and all cohorts from 2010 to 2017 (except for graduation outcomes which include up to the 2014 cohort). Lower SES is defined as students with an expected family contribution (EFC) less than $30,000. Higher SES are those with an EFC greater than or equal to $30,000, and includes those who have a missing EFC. Circles represent local averages over a 0.1 GPA range. Figures are drawn using a linear fit on either side of the cutoff. Estimates and standard errors (in parentheses) are reported above each figure.

Figure A7: RD Figures for Log Earnings by Control and Treated Cohorts for Subgroups

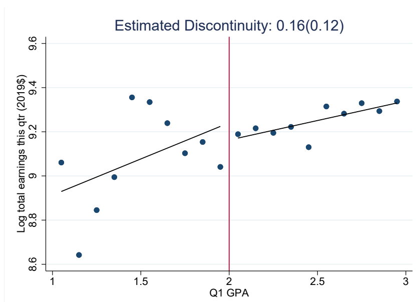
(a) All (Control Cohorts)



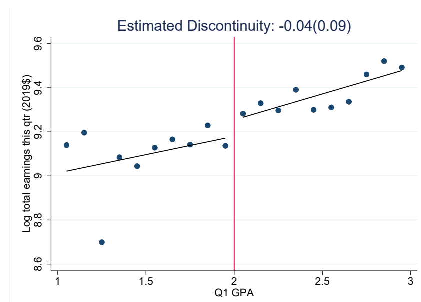
(b) All (Treatment Cohorts)



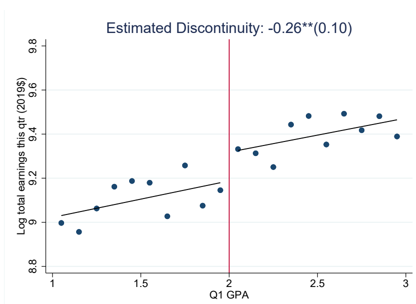
(c) Females (Control Cohorts)



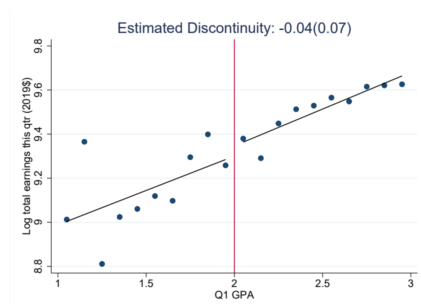
(d) Females (Treatment Cohorts)



(e) Males (Control Cohorts)



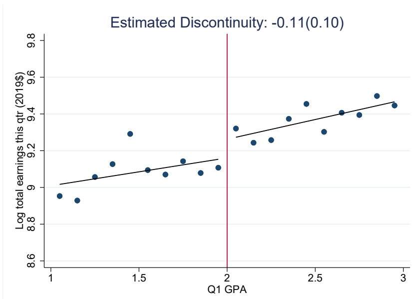
(f) Males (Treatment Cohorts)



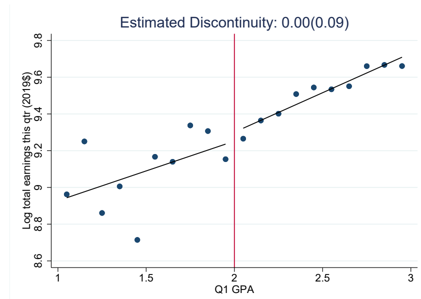
Notes: Control cohort figures include the sample of students enrolled at the university in 2007, 2008 and a subset of colleges in 2009. Treated cohort figures include the sample of students enrolled at the university in select colleges in 2009 and all cohorts from 2010 to 2017 (except for graduation outcomes which include up to the 2014 cohort). The STEM subsample includes students in the college of engineering, architecture and sciences. The non-STEM subsample includes students who are in the college of agriculture, business and liberal arts. Circles represent local averages over a 0.1 GPA range. Figures are drawn using a linear fit on either side of the cutoff. Estimates and standard errors (in parentheses) are reported above each figure.

Figure A8: RD Figures for Log Earnings by Control and Treated Cohorts for Subgroups

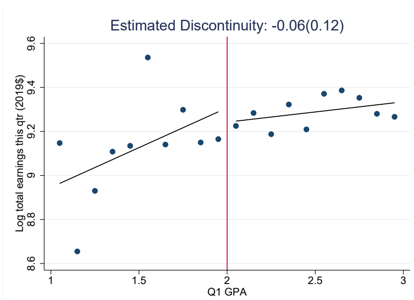
(a) STEM (Control Cohorts)



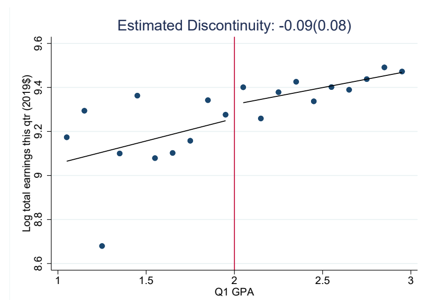
(b) STEM (Treatment Cohorts)



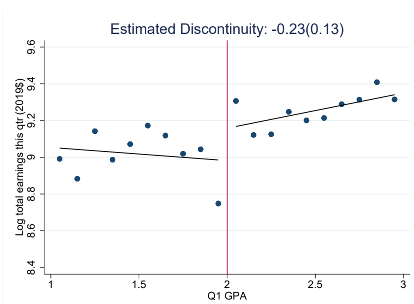
(c) Non-STEM (Control Cohorts)



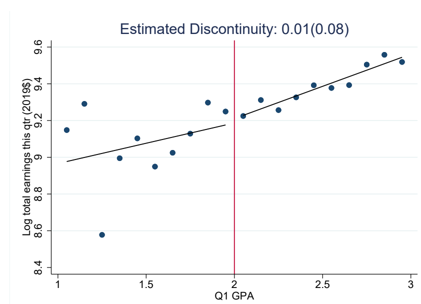
(d) Non-STEM (Treatment Cohorts)



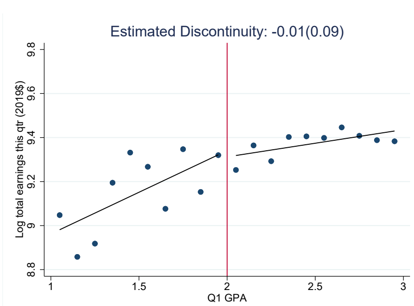
(e) Lower SES (Control Cohorts)



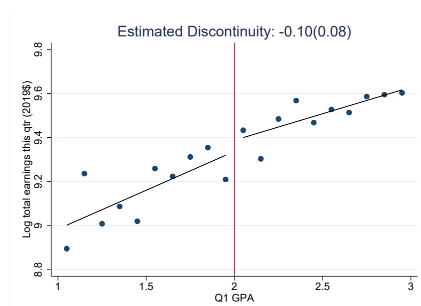
(f) Lower SES (Treatment Cohorts)



(g) Higher SES (Control Cohorts)



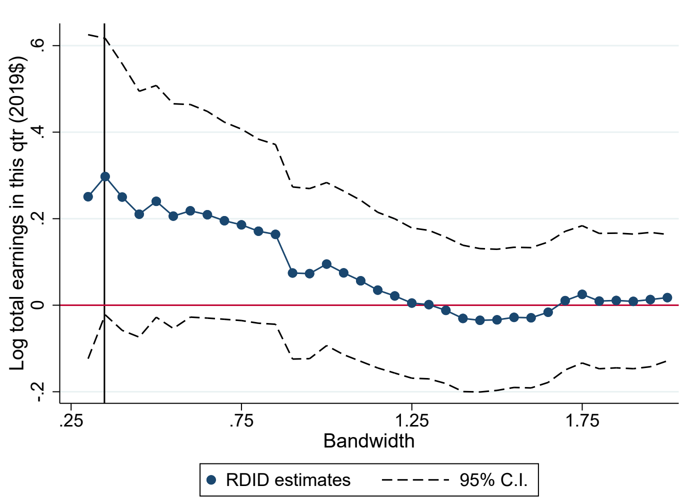
(h) Higher SES (Treatment Cohorts)



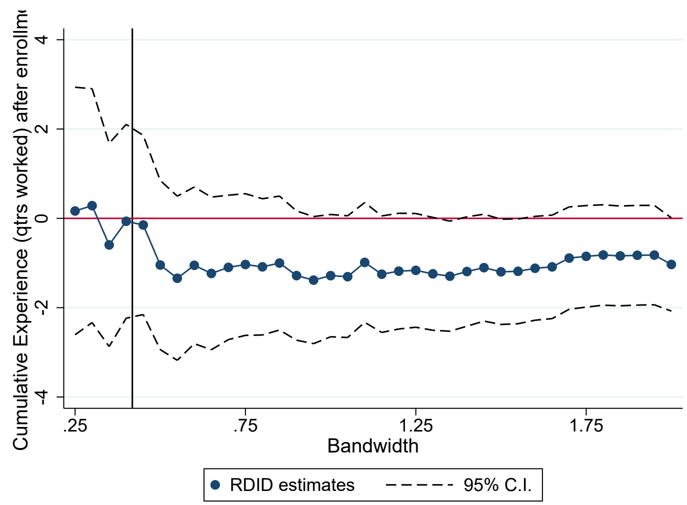
Notes: Control cohort figures include the sample of students enrolled at the university in 2007, 2008 and a subset of colleges in 2009. Treated cohort figures include the sample of students enrolled at the university in select colleges in 2009 and all cohorts from 2010 to 2017 (except for graduation outcomes which include up to the 2014 cohort). Lower SES is defined as students with an expected family contribution (EFC) less than $30,000. Higher SES are those with an EFC greater than or equal to $30,000, and includes those who have a missing EFC. Circles represent local averages over a 0.1 GPA range. Figures are drawn using a linear fit on either side of the cutoff. Estimates and standard errors (in parentheses) are reported above each figure.

Figure A9: DiRD Estimates for Earnings Outcomes by Bandwidth

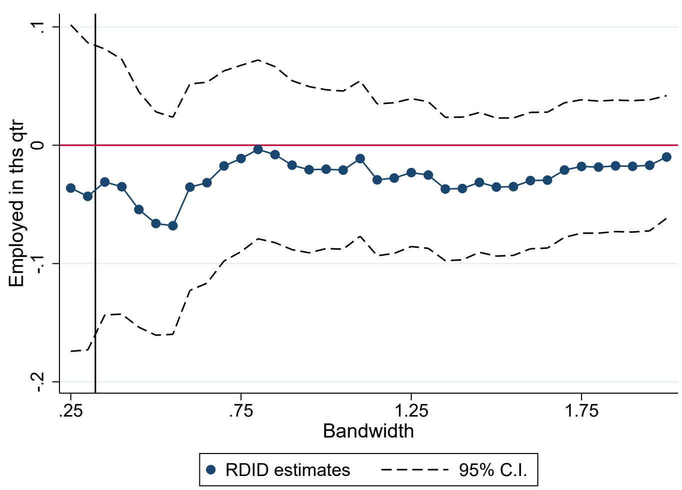
(a) Log Earnings



(b) Cum.Experience (Qts Worked)



(c) Employed



Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. Dashed lines represent 95% confidence intervals. DiRD estimates are equivalent to differencing two local linear RD regressions. The vertical black lines indicate the optimal bandwidth calculated using the CCT procedure as described in Calonico et al. (2014).

B Appendix Tables

Table B1: DiRD Estimates: Persistence by Year and Longer-Term Credits Earned

	(1) Complete Yr 2 (or Graduate)	(2) Complete Yr 3 (or Graduate)	(3) Complete Yr 4 (or Graduate)	(4) Complete Yr 5 (or Graduate)	(5) Complete Yr 6 (or Graduate)	(6) Total Credits Earned at 6 yrs (or at Separation)	(7) Grad in 6 yrs (or Still Enrolled)
DiRD Estimates							
All Students	0.076**	0.067*	0.052	0.049	0.053	1.063	0.059
	(0.036)	(0.037)	(0.037)	(0.037)	(0.037)	(2.191)	(0.038)
Observations	22,225	22,225	22,225	22,225	22,225	22,225	22,225
Low-SES	0.122**	0.157***	0.182***	0.159***	0.159***	0.851	0.162***
	(0.056)	(0.057)	(0.057)	(0.057)	(0.057)	(2.966)	(0.058)
Observations	9,491	9,491	9,491	9,491	9,491	9,491	9,491
Men	0.071	0.060	0.035	0.039	0.040	0.738	0.045
	(0.045)	(0.047)	(0.047)	(0.047)	(0.047)	(2.653)	(0.048)
Observations	12,427	12,427	12,427	12,427	12,427	12,427	12,427
STEM	0.082*	0.064	0.055	0.043	0.049	1.144	0.057
	(0.048)	(0.050)	(0.050)	(0.050)	(0.050)	(2.817)	(0.051)
Observations	10,735	10,735	10,735	10,735	10,735	10,735	10,735

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. Robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: DiRD Estimates for Dropout in Yrs 2-4

	(2) Dropout Yr 2	(3) Dropout Yr 3	(3) Dropout Yr 4
DiRD Estimates			
Bandwidth= 0.75	0.017	-0.000	-0.022
	(0.032)	(0.020)	(0.014)
Bandwidth= 1	0.036	0.012	-0.008
	(0.027)	(0.017)	(0.013)
Observations (BW=0.75)	11,109	11,098	10,571
Observations (BW=1)	17,149	17,122	16,162

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2016 for column 1, cohorts 2007-2015 for column 2, and cohorts 2007-2014 for column 3. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. Robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B3: DiRD Estimates for Academic Outcomes by Gender, Field of Study and SES (Bandwidth=0.75)

	(1) Dropout Q1	(2) Dropout Q2	(3) Dropout Q3	(4) GPA (Q2 + Q3)	(5) Earned Credits (Q2 + Q3)	(6) Grad. 4 yr.	(7) Grad. 6 yr.
Female	-0.012	0.011	-0.077	-0.118	0.805	0.013	0.103
	(0.031)	(0.037)	(0.061)	(0.150)	(1.234)	(0.071)	(0.079)
Male	-0.012	-0.008	-0.062	0.297***	1.561*	0.002	0.0511
	(0.018)	(0.025)	(0.046)	(0.105)	(0.824)	(0.037)	(0.059)
STEM	-0.004	0.005	-0.120**	0.290**	1.414	-0.009	0.103
	(0.021)	(0.028)	(0.051)	(0.113)	(0.888)	(0.038)	(0.064)
Non-STEM	-0.028	-0.010	-0.006	-0.080	0.969	0.022	0.014
	(0.024)	(0.031)	(0.050)	(0.130)	(1.083)	(0.061)	(0.070)
Lower SES	-0.012	-0.006	-0.091	0.267**	1.694	-0.076	0.154**
	(0.025)	(0.031)	(0.058)	(0.131)	(1.037)	(0.050)	(0.071)
Higher SES	-0.011	-0.002	-0.048	0.050	0.942	0.069	0.011
	(0.021)	(0.028)	(0.048)	(0.112)	(0.909)	(0.046)	(0.061)
Obs. (Female)	6,004	6,004	6,004	5,780	6,004	4,893	4,625
Obs. (Male)	8,403	8,403	8,403	8,041	8,403	7,002	6,484
Obs. (STEM)	7,032	7,032	7,032	6,743	7,032	5,898	5,476
Obs. (Non-STEM)	7,375	7,375	7,375	7,078	7,375	5,997	5,633
Obs. (Lower SES)	6,255	6,255	6,255	6,014	6,255	5,121	4,748
Obs. (Higher SES)	8,152	8,152	8,152	7,807	8,152	6,774	6,361

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC scores are missing, indicators for whether parents attended college, and cohort and college fixed effects. All regressions use a bandwidth of 0.75 grade points on either side of the cutoff. The STEM subsample includes students in the college of engineering, architecture and sciences. The non-STEM subsample includes students who are in the college of agriculture, business and liberal arts. Lower SES is defined as students with an expected family contribution (EFC) less than \$30,000. Higher SES are those with an EFC greater than or equal to \$30,000, and includes those who have a missing EFC. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. Robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B4: 'Donut' DiRD Estimates

	(1) Dropout Q1	(2) Dropout Q2	(3) Dropout Q3	(4) GPA (Q2 + Q3)	(5) Earned Credits (Q2 + Q3)	(6) Grad. 4 yr.	(7) Grad. 6 yr.
Bandwidth= 0.5	-0.017	-0.013	-0.094**	0.192*	1.435*	0.034	0.092
	(0.018)	(0.024)	(0.046)	(0.114)	(0.850)	(0.048)	(0.062)
With Controls	-0.016	-0.011	-0.094**	0.212*	1.403*	0.035	0.089
	(0.018)	(0.024)	(0.045)	(0.110)	(0.849)	(0.045)	(0.060)
Observations	8,369	8,369	8,369	7,989	8,369	6,930	6,427
Bandwidth= 0.75	-0.012	-0.005	-0.080**	0.174*	1.452**	0.041	0.074
	(0.016)	(0.021)	(0.038)	(0.093)	(0.706)	(0.039)	(0.050)
With Controls	-0.012	-0.005	-0.075**	0.149*	1.391**	0.026	0.060
	(0.016)	(0.021)	(0.037)	(0.089)	(0.703)	(0.036)	(0.048)
Observations	13,803	13,803	13,803	13,238	13,803	11,358	10,601
Bandwidth= 1	-0.009	-0.002	-0.090***	0.165**	1.304**	0.023	0.041
	(0.014)	(0.018)	(0.032)	(0.078)	(0.599)	(0.032)	(0.042)
With Controls	-0.009	-0.002	-0.087***	0.141*	1.238**	0.002	0.032
	(0.014)	(0.018)	(0.032)	(0.075)	(0.595)	(0.030)	(0.041)
Observations	21,621	21,621	21,621	20,838	21,621	17,744	16,641

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $\$30,000$, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. 'Donut' DiRD estimates are equivalent to differencing two local linear RD regressions after excluding the heaping point at GPA = 2.0. Robust standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B5: Predicted Outcomes Based on Baseline Characteristics (Test of RD Assumption)

	(1) Predicted Dropout	(2) Predicted Q2 + Q3 GPA
DiRD Estimates		
Bandwidth= 0.5	0.004	-0.050
	(0.004)	(0.042)
Bandwidth= 0.75	-0.002	0.004
	(0.003)	(0.035)
Bandwidth= 1	-0.001	0.009
	(0.003)	(0.030)
Observations (BW=0.5)	8,973	8,973
Observations (BW=0.75)	14,407	14,407
Observations (BW=1)	22,225	22,225

Notes: The sample includes all first-year students entering the university in the fall cohorts 2007-2017. All outcomes are predicted based on the following control variables: high school GPA, whether a student is non-white, gender, Math and English remedial status, Pell eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B6: Placebo Test: Running Variable=Yr 2, Q1 GPA

	(1) Dropout Yr 2
DiRD Estimates	
Bandwidth= 0.75	-0.015
	(0.032)
Bandwidth= 1	-0.020
	(0.029)
Observations (BW=0.75)	13,309
Observations (BW=1)	18,958

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2015. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. There is no SP for students who are placed on probation in their second year. As such, the DiRD estimates reported in this table act as a placebo test. Note that the sample is smaller than in Table 2 because some students dropped out during the first year and we cannot include the 2018 cohort when studying year 2 outcomes. *** p<0.01, ** p<0.05, * p<0.1.

Table B7: Placebo Test STEM only: Running Variable=Yr 2, Q1 GPA

	(1) Dropout Yr 2
DiRD Estimates	
Bandwidth= 0.75	0.019
	(0.043)
Bandwidth= 1	0.004
	(0.038)
Observations (BW=0.75)	6,934
Observations (BW=1)	9,629

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2015 who enter as a STEM major. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $\$30,000$, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. There is no SP for students who are placed on probation in their second year. As such, the DiRD estimates reported in this table act as a placebo test. Note that the sample is smaller than in Table 2 because some students dropped out during the first year and we cannot include the 2018 cohort when studying year 2 outcomes. *** p<0.01, ** p<0.05, * p<0.1.

Table B8: Placebo Test Low-SES only: Running Variable=Yr 2, Q1 GPA

	(1) Dropout Yr 2
DiRD Estimates	
Bandwidth= 0.75	0.033
	(0.052)
Bandwidth= 1	0.018
	(0.047)
Observations (BW=0.75)	5,634
Observations (BW=1)	7,897

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2015 who are from low income families. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $\$30,000$, indicators for whether parents attended college, and cohort and college fixed effects. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. There is no SP for students who are placed on probation in their second year. As such, the DiRD estimates reported in this table act as a placebo test. Note that the sample is smaller than in Table 2 because some students dropped out during the first year and we cannot include the 2018 cohort when studying year 2 outcomes. *** p<0.01, ** p<0.05, * p<0.1.

Table B9: DiRD Estimates for Academic Outcomes by Subgroup Excluding 2009 Cohort

	(1) Dropout Q1	(2) Dropout Q2	(3) Dropout Q3	(4) GPA (Q2 + Q3)	(5) Earned Credits (Q2 + Q3)	(6) Grad. 4 yr.	(7) Grad. 6 yr.
Female	-0.022	0.013	-0.054	-0.108	0.451	-0.024	0.052
	(0.027)	(0.035)	(0.055)	(0.136)	(1.146)	(0.063)	(0.071)
Male	-0.001	-0.004	-0.086**	0.283***	1.314*	-0.010	0.013
	(0.017)	(0.022)	(0.042)	(0.094)	(0.729)	(0.033)	(0.053)
STEM	0.002	0.013	-0.115**	0.202**	0.637	-0.032	0.018
	(0.018)	(0.024)	(0.045)	(0.101)	(0.785)	(0.034)	(0.057)
Non-STEM	-0.026	-0.027	-0.029	0.039	1.586	0.020	0.035
	(0.024)	(0.030)	(0.048)	(0.121)	(1.003)	(0.054)	(0.064)
Lower SES	-0.002	-0.004	-0.119**	0.222*	1.542*	-0.081*	0.128**
	(0.020)	(0.027)	(0.052)	(0.120)	(0.923)	(0.044)	(0.064)
Higher SES	-0.013	0.003	-0.056	0.109	0.599	0.029	-0.026
	(0.020)	(0.026)	(0.044)	(0.100)	(0.824)	(0.042)	(0.055)
Obs. (Female)	8,953	8,953	8,953	8,646	8,953	7,081	6,691
Obs. (Male)	11,338	11,338	11,338	10,929	11,338	9,266	8,524
Obs. (STEM)	9,770	9,770	9,770	9,432	9,770	8,006	7,383
Obs. (Non-STEM)	10,521	10,521	10,521	10,143	10,521	8,341	7,832
Obs. (Lower SES)	8,715	8,715	8,715	8,424	8,715	6,996	6,478
Obs. (Higher SES)	11,576	11,576	11,576	11,151	11,576	9,351	8,737

Notes: The sample includes all first-time freshmen enrolled at the university in the entering fall cohorts 2007-2017. Control variables include high school GPA, gender, non-white, math and English remedial status, Pell Grant eligibility status, whether EFC is less than $30,000, indicators for whether parents attended college, and cohort and college fixed effects. All regressions use a bandwidth of 1 grade point on either side of the cutoff. The STEM subsample includes students in the college of engineering, architecture and sciences. The non-STEM subsample includes students who are in the college of agriculture, business and liberal arts. Lower SES is defined as students with an expected family contribution (EFC) less than $30,000. Higher SES are those with an EFC greater than or equal to $30,000, and includes those who have a missing EFC. Each point estimate is from a separate regression. DiRD estimates are equivalent to differencing two local linear RD regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B10: Summary Statistics for Labor Market Outcomes

	Full Sample mean/sd	Bandwidth=1 Q1 GPA$\in[1-3]$ mean/sd	Pre-program yrs. Q1 GPA$\in[1-2)$ Probation mean/sd	Pre-program yrs. Q1 GPA$\in[2-3]$ No Probation mean/sd	Program yrs. Q1 GPA$\in[1-2)$ Probation + FYSP mean/sd	Program yrs. Q1 GPA$\in[2-3]$ Neither mean/sd
Total earnings this qtr	12077.63	11323.86	9235.91	10962.19	9118.51	12336.44
	(14498.36)	(12214.44)	(11563.87)	(11526.23)	(10780.56)	(12781.32)
Log total earnings this qtr	9.46	9.38	9.12	9.34	9.16	9.48
	(0.93)	(0.94)	(1.04)	(0.93)	(1.01)	(0.89)
Employed in this qtr	0.71	0.72	0.70	0.73	0.68	0.73
	(0.45)	(0.45)	(0.46)	(0.44)	(0.47)	(0.44)
Cumulative qtrs worked	15.87	16.06	16.34	16.77	15.07	15.69
	(8.00)	(8.10)	(8.70)	(8.20)	(8.36)	(7.83)
Age	24.79	24.80	24.93	24.92	24.73	24.71
	(0.91)	(0.92)	(0.92)	(0.92)	(0.92)	(0.91)
Student-Qtrs	247,558	127,626	11,016	40,392	11,006	55,642
Students	27,097	13,858	918	3,366	1,363	7,166

Notes: The sample is limited to years 7–9 after first enrollment at the university. The table reports means and standard deviations in brackets. The last calendar quarter included in the sample is 2019 Q4.

Table B11: DiRD Employment Results (cohorts $\leq$ 2011)

	(1) Log Earnings	(2) Employed	(3) Cum. Exper. (Qtrs)
All	0.0963	–0.0642*	–1.86**
	(0.108)	(0.0387)	(0.801)
Control Mean	12,242	0.751	17.9
Observations	81,213	112,038	112,038
Female	–0.233	–0.159**	–3.66***
	(0.172)	(0.0629)	(1.28)
Male	0.297**	–0.0117	–0.754
	(0.137)	(0.0490)	(1.02)
STEM	0.134	–0.0625	–1.26
	(0.150)	(0.0547)	(1.11)
Non-STEM	0.0227	–0.0693	–2.67**
	(0.160)	(0.0563)	(1.18)
Lower SES	0.337*	–0.00239	–0.0968
	(0.173)	(0.0588)	(1.24)
Higher SES	–0.0955	–0.111**	–3.04***
	(0.135)	(0.0516)	(1.05)
Ctrl Mean (Female)	10,924	0.741	18.4
Obs. (Female)	34,252	46,630	46,630
Ctrl Mean (Male)	13,393	0.766	17.6
Obs. (Male)	46,961	65,408	65,408
Ctrl Mean (STEM)	14,691	0.713	15.1
Obs. (STEM)	41,140	58,346	58,346
Ctrl Mean (Non-STEM)	12,528	0.773	17.9
Obs. (Non-STEM)	40,073	53,692	53,692
Ctrl Mean (Lower SES)	10,939	0.785	19.3
Obs. (Lower SES)	31,765	44,030	44,030
Ctrl Mean (Higher SES)	13,021	0.733	17.1
Obs. (Higher SES)	49,448	68,008	68,008

Notes: The sample is limited to quarters 7-9 years since students enrolled at university and to students who enrolled in or before 2011. The unit of observation is the student-quarter. The last calendar quarter included in the sample is 2019, Quarter 4. Dollar amounts have been adjusted to 2019 dollars using the Consumer Price Index for All Urban Consumers. Control variables include high school GPA, gender, non-white, Math and English remedial status, Pell Grant eligibility status, whether EFC is less than $\$30,000$, indicators for whether parents attended college, and cohort and college fixed effects. All regressions use a bandwidth of 1 grade point on either side of the cutoff Standard errors are cluster-robust at the student level, and regressions are weighted by one over the number of quarters in which a given student is present in the regression sample. *** p<0.01, ** p<0.05, * p<0.1.

C Comparison to other Coaching Programs

Findings from recent college access or success coaching studies

College Coaching/Mentoring Study	**College Program Name and Characteristics**	**Assignment method and Program Length**	**Program Participation**	**Method of Contact**	**Coaching Goals**	**Effect Sizes**
Bettinger & Baker (2014)	"Inside Track" coaching program. Students assigned to college coaches who call their students regularly and, in some cases, have access to course syllabi, transcripts, and additional information on students' performance and participation in specific courses.	Students randomly assigned to college coach for **two Semesters or less.**	**Nonmandatory:** Students have the option to participate or not when contacted by the coach.	**Virtual:** Coaches contact students via phone, email, text messages and social networking sites.	Help students find ways to overcome both academic and "real-life" barriers and to identify strategies for success.	- 5 percentage point increase in college persistence rates after 6 and 12 months. -4.3 and 3.4 percentage point increase in college persistence rates after 18 and 24 months. - 4 percentage point increase in 4-year completion rates for a subsample of students.
Barr & Castleman (2021)	*"Bottom Line"* advising program. Divided into two stages. College access advisors provide individualized advising to students to help get them into college. Advisors have an average caseload of 50-60 students. College success advisors then continue to provide individualized, campus-based support to enrolled students for up to six years following high school.	Students randomly assigned to advisors and meet with each student for an hour every three or four weeks during senior year. Campus-based advisors at each target institution are also available to meet with students.	**Nonmandatory:** Students need to apply to the program. Once enrolled in program, meeting with advisor is also nonmandatory.	**In-Person and virtual:** At *Bottom Line*'s office in each community or over the phone.	Advisors provide comprehensive college and financial aid support for students and selecting a college or university that aligns with a student's goals and circumstances.	-9.1 percentage point increase in 4-year college enrollment rates. -9.6 percentage points more likely to earn a bachelor's degree within six years of high school.

Findings from recent college access or success coaching studies (Continued)

College Coaching/Mentoring Study	**College Program Name and Characteristics**	**Assignment method and Program Length**	**Program Participation**	**Method of Contact**	**Coaching Goals**	**Effect Sizes**
Carrell & Sacerdote (2017)	A series of field experiments in college coaching/mentoring. The program is targeted toward high school seniors who are on the verge of failing to apply to college.	Students randomly assigned to mentor during final year of high school. Visits are typically two–three hours in length with a promise up front to keep returning each week until every student has met his or her goals for college applications.	**Nonmandatory:** Treated Students are told they are enrolled in program by email, letters, phone call and encouraged to participate.	**In-Person:** in person mentoring by a Dartmouth College student.	Coach makes sure that the Free Application for Federal Student Aid (FAFSA) form is started and the sections other than the parental income portion are completed. Program pays for all application fees (upfront) and in some cohorts they pay treatment students a $100 bonus in cash for completing the program	-30 percentage point increase in likelihood of attending any college for women. -22 percentage point increase in likelihood of attending a 4-year college for women. -12.9 percentage point increase in college persistence rates for women.

D SP Workshop Materials

Campus Resource Guide

Campus Resources (Scan QR codes to learn more!)

1-3: Study Skills: Find resources and videos on popular topics such as:

- Study strategies, text anxiety tips and study guides
- Flashcards, video tutorials, interactive exercises
- Lecture note taking
- Memorization
- Learning style (Vark Questionnaire)

Study Skills

4-5: Tutoring on Campus (Location Varies)

- Free 1-1 or group tutoring for a variety of classes
- Supplemental Workshops: 1 unit workshop to go alongside certain Science and Math classes. Recap information from class, get help with study skills, test prep, and group studying.
- Study sessions: Weekly sessions made of 8-15 students for multiple subjects. Submit a request through the portal

Tutoring

6-7: Office hours can sometimes be intimidating and confusing on what you should ask. Here are some helpful tips:

- If office hours conflict with your schedule, contact professors for an alternate time to meet. They are more than happy to help out!
- Show problems on homework or tests that you were confused about, ask the professor to walk you through each steps.
- Bring in class notes that you would like more explanations on
- Explain your study strategies and ask about additional tips or tricks

Professors are very knowledgeable in their field and know of many outside resources and sometimes even internship or research opportunities, get to know them!

8-9: Associated Students, Inc. (ASI):

- Student Government
- Clubs and Organization
- Craft Center Classes
- The Recreation Center
- Activities and events

ASI

Dean of Students

- Club Sports
- Center For Service in Action
- Center for Leadership
- Fraternity & Sorority Life

Dean of Students

10: Cross Cultural Centers(Location varies)

- **Gender Equity Center-** Educating and empowering feminist, womxnist, mujerista moments though an intersectional lens and striving for social justice.
- **Men & Masculinity-** Creates spaces to express and evaluate masculinity and intersections with other identities through programs, dialogs and trainings.
- **Multicultural Center-** Provides space and events for people across all races, ethnicities, gender, sexual orientation, disability, economic class, religion, citizenship and their intersections
- **Pride Center-** Provides brave spaces and events to all sexualities, gender identities and expressions. Check out their peer mentoring program!

CCCs

Student Academic Services (Location Varies)

- **Dream Center-** Offers an inclusive space and a multitude of events for all undocumented students, those in mixed-status families and their allies. Stop by for a space to study, or to hang out with friends!
- **Black Academic Excellence Center (BAEC)-** Offers a supportive and enriching environment to promote excellence among Black students on campus. Stop by their center to say hello or attend one of their events!

SAS

11-14: Campus Health and Wellbeing (Bldg. 27):
Health Services:

- Mostly free services
- Walk in or make an appointment for medical attention or advice
- Educational programs about drugs, alcohol, sexuality and other topics
- On site lab testing, X-rays, and Shots (e.g Flu, TB tests)
- Discounted Pharmacy
- After-hours nurse advice
- PULSE peer mentoring health education program, covering topics such as, drugs and alcohol use and/or recovery, physical and sexual health and mental well being.

Health & Wellbeing

Counseling Services:

- Individual, couples, group therapy sessions
- Emotional Well Being Workshops
- End of Quarter Survival Kit Workshops
- Clinicians specializing in: Anxiety, Eating Disorders, Multicultural issues, Trauma, Alcohol and Drug Abuse, Suicide Prevention and many more.

Counseling Services

15-18: Basic Needs & Crisis Services (Location Varies)

Food Insecurity:

- CalFresh- Provides monthly payments to eligible students that can be used where food is sold like grocery stores, and farmers markets.
- Meal Vouchers- Students experiencing short-term financial need, can dine at 805 Kitchen during the school year and The Avenue for the summer.
- Food Pantry (Bldg 27, Lower Level)- Students can access free, packed and canned foods, frozen meals and personal hygiene products.
- Food Bank Distribution- Once a week on Mott Lawn, bags of fresh produce and food for free.

Financial Hardships:

- Cares Grant- One-time grant for unexpected emergencies like, paying for tuition, academic supplies, medical expenses, emergency housing and other temporary hardships.
- Professional Clothing Closet- Free, high-quality work clothes for interviews and future internships and jobs
- Financial Aid Office: Offers daily drop in hours where students can meet with a counselor to discuss ways to cover the cost of college.

Basic Needs

15-18 (Cont.): Crisis Services

- Safer (Bldg 65, Rm 217)- Provides confidential crisis counseling, advocacy and education and support resources by state-certified advocates. Learn about your options, rights, and other resources about sexual assault or misconduct, dating or domestic violence and stalking.

Safer

Reporting Hate Crimes

- Bias Incident Report- If you believe you have witnessed an act of discrimination or harassment on or off campus, you may file a report online though the Dean of Students.

Bias Incident Report

19-20: Career Services (Bldg. 124): Drop in or make an appointment with the Freshman Focus Team or any other Career Counselor to talk about:

- Career exploration
- Major Exploration
- Interviewing skills
- Resume and Cover letter

Career Services

21-22: Advising Centers: Have a question and don't know where to start? Visit an advisor!

College Advising Center: CAED, CAFES, CENG, CLA, CSM, OCOB,

- Course planning
- Navigating your curriculum
- Major and support related classes
- Tracking progress to degree
- Concentration

Specialty Advising

- Pre-Health Career Advising
- International/Study Abroad

Success Center (Bldg 52-D37):

- Referring you to academic and/or on-campus resources
- Understanding university and college-specific policies and procedures
- Navigating tools such as PASS and Student Center
- Change of Major process
- GE Classes
- Minors
- Transfer courses

Advising

23-24: Conflict resolution Ombuds (Library 35-113):

- If you feel that you got an unfair grade in a class
- If you feel that you got treated unfairly by someone in the University Community
- If you want to discuss a sensitive question or issue

Ombuds

25: Disability Resource Center (Bldg. 124): Provides services to those with long-term <u>or</u> short-term disabilities:

- How to request services
- Eligibility
- Information on testing for Learning Disabilities
- Possible accommodations - in and outside the classroom
- Peer Mentor Program

DRC

xx

My Success Plan

I decided to attend University X because:

____

____

A positive experience I have had at University X:

____

____

My favorite part of University X:

____

____

During my time at University X, I am most looking forward to:

____

____

Creating S.M.A.R.T. Goals

S	M	A	R	T
Specific Make your goal detailed and specific to know what you are working towards	**Measurable** Set parameters so that you can identify tangible evidence towards achieving your goal	**Attainable** Draft realistic goals that challenge you but you are confident to achieve	**Relevant** Make sure each goal is consistent with other goals you have established and fits with your immediate and long-term plans	**Time-Bound** Set a time that you would like to achieve your goal by

Original Goal:		**SMART Goal:**
I want to read more	**S**: 12 books a year **M**: 1 book a month **A**: 1 hour at lunch, 1 hour before bed **R**: More than currently reading **T**: 1 book a month, 12 a year	I am going to read 1 book a month by reading for an hour during lunch and an hour before bed for a total of 12 books a year.

xx

WINTER QUARTER SMART GOAL:

Action steps:

I will complete the following action steps:

Personal	Academic	Social
(ex: *I will create a calendar/schedule to keep me on track with attending classes & completing assignments)*	(ex: *I will use time in between classes to study, read, and review notes)*	(ex: *I will refrain from social outings, TV, parties, social media, video games, etc. until all my homework is complete for that day)*

Resources:

I will utilize the following resources to help me achieve my goal:

(Example: I will visit the Success Center, , by Week 3 to discuss Change of Major)

1. I will visit ________________________________located in__________________________by____________

   to discuss____

2. I will visit ________________________________located in__________________________by____________

   to discuss____

Challenges: (What could stop/de-motivate me along the way?)

1. ____
2. ____

Ways to overcome my challenges:

1. ____
2. ____

Who's got your back? (The person(s) in my life that I will share my action plan with and ask to help keep me accountable for accomplishing my goal & action steps)

____

I am fully committed to completing this success plan this quarter.

SIGN: ____DATE:___________

You will receive an email from your Coach in Week 5 to follow up on your goals and action steps. Once you have communicated with your coach and completed a post-survey, your requirements with the First Year Success Program will be fulfilled.

ACADEMIC SKILLS CENTER

Weekly Schedule

	Sun	Mon	Tues	Wed	Thurs	Fri	Sat
6:00 AM							
7:00 AM							
8:00 AM							
9:00 AM							
10:00 AM							
11:00 AM							
12:00 PM							
1:00 PM							
2:00 PM							
3:00 PM							
4:00 PM							
5:00 PM							
6:00 PM							
7:00 PM							
8:00 PM							
9:00 PM							
10:00 PM							
11:00 PM							
12:00 PM							
1:00 AM							
2:00 AM							

Quarter Schedule

	Sun	Mon	Tues	Wed	Thurs	Fri	Sat
Week 1							
Week 2							
Week 3							
Week 4							
Week 5							
Week 6							
Week 7							
Week 8							
Week 9							
Week 10							
Week 11							
Finals							

E SP Pre- and Post-Surveys

All surveys were administered via the online platform SurveyMonkey.

Pre-Survey for Students Qualifying for SP in Fall 2013

Q1: What is your first name?

Q2: What is your last name?

Q3: What is your university email address?

Q4: Which college are you from?

Q5: In the Freshman Success Workshop, you will participate in a small group that will be led by an academic coach. An academic coach will lead a discussion and help group members develop an action plan to achieve success after being put on academic probation. Please check all the ways you wish to work with an academic coach.

- ☐ Identify resources to improve my study skills
- ☐ Generally improve my academic performance
- ☐ Identify ways to achieve my goal GPA
- ☐ Identify why my grades do not reflect my effort
- ☐ Stay motivated and on track to achieve my academic goals
- ☐ Learn about relevant policies
- ☐ Reduce anxiety and stress about my academic performance
- ☐ Complete the Freshman Success Workshop
- ☐ Other, please explain

Q6: On average, how many hours per week do you study?

Q7: On average, how many hours do you sleep each night?

Q8: How many times did you attend faculty office hours last quarter?

Q9: How many hours each day do you spend socializing or doing extracurricular activities?

Q10: How many hours each day do you spend watching TV, going on Facebook, gaming, etc.?

Q11: Please read the below prompts and respond to each with one of the following options: "Always, Sometimes or Rarely".

- ☐ I feel motivated to focus on school.
- ☐ I complete the assigned reading for all my classes.
- ☐ My class notes help me prepare adequately for a test.
- ☐ I retain the information I read for homework assignments.
- ☐ I feel confident about my writing ability.
- ☐ I take the time to revise my writing to make it clear, correct, and consistent.
- ☐ I easily and effectively communicate my thoughts.

☐ When I do not understand my professor, I ask the right questions to clarify.

☐ I easily remember things I learn in class.

☐ At the end of a lecture, I can summarize what was presented.

☐ I feel confident when taking an exam.

☐ When I think I did poorly on a test I just finished, I go back to my notes and review all the information I had forgotten.

☐ I prepare in advance for a test rather than "cramming" the night before.

☐ I manage my time well.

☐ I change my other priorities to have enough time for studying and completing course assignments.

☐ I can successfully balance many aspects of my life (such as friends, family, school, work, extracurricular, etc.).

☐ I study even when less important things distract me.

☐ When I have to take a course that doesn't interest me, I find a way to motivate myself to earn a good grade.

☐ I attend my classes regularly.

☐ I ask for help from family members, friends, or other appropriate individuals when needed.

☐ I know about the student services offered by Cal Poly and know how to use them.

☐ I easily adjust my learning style to my instructors' teaching styles.

☐ I feel connected to a community at Cal Poly.

Post-Survey for Students Qualifying for SP in Fall 2013

Q1: What is your first name?

Q2: What is your last name?

Q3: What is your university email address?

Q4: Which college are you from?

Q5: What day and time did you attend a workshop?

Q6: Please rate your academic coach in the following areas by selecting "Excellent, Average, Below Average, or Not Applicable" for each of the following:

- ☐ Approachability
- ☐ Knowledge
- ☐ Preparation

Q7: Which part of the Freshman Success Program was most effective? (select one)

- ☐ The presentation at the beginning
- ☐ The breakout session
- ☐ Both were equally effective

Q8: What did you find most beneficial from the big session? (please select one)

- ☐ Identified resources to improve my study skills
- ☐ Identified ways to achieve my goal GPA
- ☐ Identified why my grades do not reflect my effort
- ☐ Learned about relevant policies
- ☐ Learned how to improve my academic performance
- ☐ More motivated and on track to achieve my academic goals
- ☐ Reduced anxiety and stress about my academic performance
- ☐ I didn't find anything beneficial from this session
- ☐ Other (please explain)

Q9: What did you find most beneficial from the small group breakout session? (please select one)

- ☐ Discussion with other students
- ☐ Learning about resources
- ☐ SMART goals/goal setting
- ☐ The Self-Evaluation
- ☐ I didn't find anything beneficial from this session
- ☐ Other (please explain)

Q10: As a result of attending the Freshman Success Program, I am more likely to...(check all that apply)

- ☐ Attend class
- ☐ Do the assigned reading
- ☐ Manage my time more efficiently
- ☐ Seek out resources I need
- ☐ None of the above
- ☐ Other (please explain)

Q11: Next year, if we were to incorporate a student panel (video) segment into the big presentation of previous students on academic probation, would you be interested in participating?

Q12: In what area do you think your behavior has changed the most this quarter? (please select one)

- ☐ Increased the number of hours of sleep per night
- ☐ Increased the number of hours spent studying per day
- ☐ Increased the number of visits to office hours
- ☐ Managing my time better
- ☐ Utilizing campus resources

Q13: So far this quarter, how many hours per week do you study?

Q14: So far this quarter, on average, how many hours do you sleep each night?

Q15: So far this quarter, how many times have you been to faculty office hours?

Q16: So far this quarter, how many hours each week do you spend socializing or doing extracurricular activities?

Q17: So far this quarter, how many hours each week do you spend watching TV, going on Facebook, gaming, etc.?

Q18: Please read the below prompts and respond to each with one of the following options: Always, Sometimes or Rarely.

- ☐ I feel motivated to focus on school.
- ☐ I complete the assigned reading for all of my classes.
- ☐ My class notes help me prepare adequately for a test.
- ☐ I retain the information I read for homework assignments.
- ☐ I feel confident about my writing ability.
- ☐ I take the time to revise my writing to make it clear, correct, and consistent.
- ☐ I easily and effectively communicate my thoughts.
- ☐ When I don't understand my professor, I ask the right questions to clarify.
- ☐ I easily remember things I learn in class.
- ☐ At the end of a lecture, I am able to summarize what was presented.
- ☐ I feel confident when taking an exam.

- ☐ When I think I did poorly on a test I just finished, I go back to my notes and locate all the information I had forgotten.
- ☐ I prepare in advance for a test rather than ”cramming” the night before.
- ☐ I manage my time well.
- ☐ I change my other priorities to have enough time for studying and completing course assignments.
- ☐ I can successfully balance many aspects of my life (such as friends, family, school, work, extracurricular, etc.).
- ☐ I study even when less important things distract me.
- ☐ When I have to take a course that doesn't interest me, I can find a way to motivate myself to earn a good grade.
- ☐ I attend my classes regularly.
- ☐ I ask for help from family members, friends, or other appropriate individuals when needed.
- ☐ I know about the student services offered by Cal Poly and know how to use them.
- ☐ I easily adjust my learning style to my instructors' teaching styles.
- ☐ Although I exert great effort, my grades are lower than I expect them to be.
- ☐ I feel connected to a community at Cal Poly.

Q19: Do you have any additional comments you would like to add?

Pre-Survey for Students Qualifying for SP in Fall or Winter 2015-2018

Q1: What is your first name?

Q2: What is your last name?

Q3: What is your university email address?

Q4: Which college are you from?

Q5: What is your major?

Q6: After taking the StrengthsFinder assessment, what are your top five strengths?

Q7: Which statement best applies to you?

- ☐ I know I am not the only one on academic probation at Cal Poly.
- ☐ I feel as though I am the only one on academic probation at Cal Poly.

Q8: Which statement best applies to you?

- ☐ I can identify a staff or faculty member at Cal Poly who cares about my success.
- ☐ I am looking for a staff or faculty member at Cal Poly who cares about my success.

Q9: Looking back at Fall Quarter, were there internal factors affecting your academic performance? Mark up to three that apply to you regarding your Fall Quarter academic difficulties.

- ☐ I could not find motivation to focus on academics.
- ☐ I felt like I did not have the appropriate study skills to succeed.
- ☐ I managed my time poorly.
- ☐ I did not attend all my classes.
- ☐ I recognized that I was having difficulty, but I was not comfortable seeking campus resources.
- ☐ I focused on extracurricular activities more than I should have.
- ☐ None of the above (no internal factors affected my academic performance).
- ☐ Other (please explain).

Q10: Looking back at Fall Quarter, were there external factors affecting your academic performance? Mark up to three that apply to you regarding your Fall Quarter academic difficulties.

- ☐ I had roommate issues that kept me from studying.
- ☐ I do not like my major and, therefore, did not do well in my classes.
- ☐ I had mostly General Education classes and was not interested in my classes.
- ☐ I got sick and missed too many classes.
- ☐ I had a personal crisis and had to focus my energy in other areas besides school.
- ☐ I had a bad professor(s) during Fall Quarter, which led to me being on Academic Probation.
- ☐ I did not have a choice in my block enrolled schedule, so I didn't like the times I had classes.

☐ None of the above (no external factors affected my academic performance).

☐ Other (please explain).

Q11: Which statement best applies to you?

☐ I know of at least one campus resource that will help me get back on track.

☐ I do not know of at least one campus resource that will help me get back on track.

Q12: List your involvement in campus clubs, organizations, or activities.

Q13: List your interests outside of your academic life.

Q14: Which statement best applies to you?

☐ I am motivated to focus on my academics at Cal Poly.

☐ I am not motivated to focus on my academics at Cal Poly.

Q15: Which statement best applies to you?

☐ I feel connected to Cal Poly.

☐ I do not feel connected to Cal Poly.

Q16: Which statement best applies to you?

☐ I am confident in my time management skills.

☐ I am not confident in my time management skills.

Q17: How confident are you in your decision to attend Cal Poly?

Q18: How confident are you that you will be able to get your grades up enough to be taken off academic probation by the end of Winter Quarter?

Q19: How confident are you that you will graduate from Cal Poly?

Post-Survey for Students Qualifying for SP in Fall or Winter 2015-2018

Q1: What is your first name?

Q2: What is your last name?

Q3: What is your university email address?

Q4: Which college are you from?

Q5: After the First Year Success Program, which statement best applies to you?

- ☐ I know I am not the only one on academic probation at Cal Poly.
- ☐ I feel as though I am the only one on academic probation at Cal Poly.

Q6: After the First Year Success Program, which statement best applies to you?

- ☐ I can identify a staff or faculty member at Cal Poly who cares about my success.
- ☐ I still have not yet found a staff or faculty member at Cal Poly who cares about my success.

Q7: Looking back at Fall Quarter, were there internal factors affecting your academic performance? Mark up to three that apply to you regarding your Fall Quarter academic difficulties.

- ☐ I could not find motivation to focus on academics.
- ☐ I felt like I did not have the appropriate study skills to succeed.
- ☐ I managed my time poorly.
- ☐ I did not attend all my classes.
- ☐ I recognized that I was having difficulty, but I was not comfortable seeking campus resources.
- ☐ I focused on extracurricular activities more than I should have.
- ☐ None of the above (no internal factors affected my academic performance).
- ☐ Other (please explain).

Q8: Looking back at Fall Quarter, were there external factors affecting your academic performance? Mark up to three that apply to you regarding your Fall Quarter academic difficulties.

- ☐ I had roommate issues that kept me from studying.
- ☐ I do not like my major and, therefore, did not do well in my classes.
- ☐ I had mostly General Education classes and was not interested in my classes.
- ☐ I got sick and missed too many classes.
- ☐ I had a personal crisis and had to focus my energy in other areas besides school.
- ☐ I had a bad professor(s) during Fall Quarter, which led to me being on Academic Probation.
- ☐ I did not have a choice in my block enrolled schedule, so I didn't like the times I had classes.
- ☐ None of the above (no external factors affected my academic performance).

☐ Other (please explain).

Q9: Do you feel that incorporating your top five strengths helped you come up with a relevant and productive Winter Quarter goal?

☐ Yes

☐ No

☐ Other (please explain)

Q10: After the First Year Success Program, which statement best applies to you?

☐ I know of at least one campus resource that will help me get back on track.

☐ I do not know of at least one campus resource that will help me get back on track.

Q11: Which statement best applies to you?

☐ After identifying a resource (academic advising, Career Services, professor's office hours, etc.) in the First Year Success Program, I have not utilized this resource by the time of completing this survey.

☐ After identifying a resource (academic advising, Career Services, professor's office hours, etc.) in the First Year Success Program, I have utilized this resource by the time of completing this survey.

Q12: After the First Year Success Program, which statement best applies to you?

☐ I am more motivated to focus on my academics at Cal Poly.

☐ I am equally as motivated to focus on my academics at Cal Poly as before the program.

Q13: After the First Year Success Program, which statement best applies to you?

☐ I feel more connected to Cal Poly.

☐ I feel equally as connected to Cal Poly as before the program.

Q14: After the First Year Success Program, which statement best applies to you?

☐ I am more confident in my time management skills.

☐ I am equally as confident in my time management skills as before the program.

Q15: After the First Year Success Program, how confident are you in your decision to attend Cal Poly?

Q16: After the First Year Success Program, how confident are you that you will be able to get your grades up enough to be taken off academic probation by the end of Winter Quarter?

☐ Very confident

☐ Confident

☐ Somewhat confident

☐ Not confident

Q17: After the First Year Success Program, how confident are you that you will graduate from Cal Poly?