The Effects of Student Coaching: An Evaluation of a Randomized Experiment in Student Advising

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Abstract:
College success often lags behind college attendance. One theory as to why students do not complete college is that they lack key information about how to be successful or fail to act on the information that they have. We present evidence from a randomized experiment which tests the effectiveness of individualized student coaching. Over the course of two separate school years, InsideTrack, a student coaching service, provided coaching to students attending public, private, and proprietary universities. Most of the participating students were non-traditional college students enrolled in degree programs. The participating universities and InsideTrack randomly assigned students to be coached. The coach contacted students regularly to develop a clear vision of their goals, to guide them in connecting their daily activities to their long term goals, and to support them in building skills, including time management, self advocacy, and study skills. Students who were randomly assigned to a coach were more likely to persist during the treatment period and were more likely to be attending the university one year after the coaching had ended. Coaching also proved a more cost-effective method of achieving retention and completion gains when compared to previously studied interventions such as increased financial aid.

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Introduction

While college attendance rates have risen dramatically over the past four decades, college completion has not kept pace. For example, while the percentage of 23-year old high school graduates with some college experience increased by 31 percent between 1970 and 1999, degree attainment by this age increased by only 4 percent. Over this time period, completion rates among college participants fell by more than 25 percent (Turner, 2004). Whereas the U.S. previously led the world in the percentage of the population having bachelor's degrees, it has now lost that leadership. Over the last three decades, cohort-based bachelor's attainment rates have increased by 2-3 percentage points across cohorts in the US while other OECD countries such as the UK and France have seen 10-15 percentage point increases in degree attainment. (OECD, 2007).

These concerns about educational attainment have led to increased scrutiny of college completion and movements to hold universities accountable for graduation rates. Foundations and policymakers have increased their focus on improving persistence and graduation rates. For example, President Obama's 2009, 2010 and 2011 State of the Union addresses have all touched on college completion, most notably in 2009 when he said, "This country needs and values the talents of every American. That is why we will provide the support necessary for you to complete college and meet a new goal: by 2020, America will once again have the highest proportion of college graduates in the world" (Obama, 2009). This focus on completion rates is not new; universities have long been concerned with low completion rates and have actively searched for strategies to increase college persistence and completion. One such effort which is the focus of our paper has been the use of mentors and coaches to facilitate student persistence and completion.

The use of college counselors is a well established practice in higher education. Work on the social and academic factors used to predict dropout (e.g. Tinto, 1975 and 1998; Bean & Metzner, 1985; Pascarella & Terenzini, 1980) and recent studies focusing on institutional factors that can increase retention (e.g. Goldrick-Rab, 2010; Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2009) highlight how personalized support and advising might bridge students' informational gaps and help students complete tasks they might not otherwise finish.

Our paper focuses on coaching, a form of college mentoring. InsideTrack is an independent provider of coaching services that incorporates a combination of methodologies, curricula, and technologies. InsideTrack matches students to potential coaches, and these coaches regularly contact their students to provide help and support as they are starting their college career and as they continue through their first year in school. In coaches' interactions with students, they work to help students prioritize their studies, plan how they can be successful, and identify and overcome barriers to students' academic success. Specifically, the coaches focus significant time assessing the student's life outside of school, which InsideTrack has found to be the leading influencer on student persistence and completion. Topics such as personal time commitments (work scheduling), primary care-giving responsibilities, and financial obligations are common during a student-coach interaction.

Over the past decade, InsideTrack has provided student coaching at a variety of public, private, and proprietary colleges. The company's model focuses on partnering with universities to deliver its mentoring program. InsideTrack provides the required people, processes and technologies. InsideTrack claims that the economies of scale the company realizes from serving multiple institutions enables it to make investments that are typically out of reach for individual colleges and universities.

Our data come from InsideTrack. We requested data from InsideTrack for the 2003-2004 school years and the 2007-2008 school years.[1] During these two years, InsideTrack conducted a total of 17 different randomized studies in cooperation with participating universities. InsideTrack wanted to convince the participating universities of its effectiveness, so to eliminate bias, InsideTrack used randomization in each of these cohorts to determine with which students they worked. Within institutions, InsideTrack randomly divided eligible students into two balanced groups. These pseudo-lotteries enable us to compare the set of students who received coaching to those who did not and to create unbiased estimates of the impact of the services.

We find that retention and completion rates were greater in the coached group. This held true for every length of time following enrollment. After six months, students in the coached group were 5.2 percentage points more likely to still be enrolled than students in the non-coached group (63.2 percent vs. 58.0 percent). At the end of 12 months, the effect was 5.3 percentage points. The effects persisted for at least one more year after the coaching had concluded. After 18 months, there was a 4.3 percentage point increase in college retention and after 24 months, there was still a 3.4 percentage point treatment effect from the coaching. These differences are all statistically significant over a 99 percent confidence interval. Moreover, these results do not change when we control for a variety of student characteristics. For the three cohorts for which we have degree completion data, we find that graduation rates increased by four percentage points. All of these estimated effects represent the intention to treat, and given that not all

[1] InsideTrack worked with more than just these 17 cohorts during these two years. In the additional cohorts, the universities or colleges wanted InsideTrack to serve all the entering students at their campus rather than a subset. In order to identify the effects of the program, we focus on the 17 cohorts that represent all of the cases where lotteries were used in these two years. The research team selected the two years used in the research. We chose the 2004 cohorts so that we could make some comparisons to the 2003/2004 Beginning Postsecondary Study. We chose the 2007 cohorts as they are the most recent cohort for whom we observe 24-month retention rates.

students selected for the treatment actually participated in the treatment, estimates of the effect of the treatment on the treated are likely much higher.

II. Background on Student Coaching

College Retention Studies

College retention has long been the focus of research in education, sociology, and economics, and the relationship between student and institutional characteristics and college graduation rates has been a frequent topic in the academic literature (e.g. Tinto 1975, 1998; Gansemer-Topf and Schuh, 2006). In the past few decades, numerous empirical and theoretical studies have attempted to accurately isolate the most influential obstacles and identify potential interventions. The literature has identified several barriers which could potentially reduce graduation rates.[2]

One identified barrier to post-secondary success is lack of access to appropriate information. The need for student guidance in college has been well documented.[3] Research has found that many community college students have little knowledge of course requirements and are unsure if their courses will meet requirement needs (Goldrick-Rab, 2010). Deil-Amen and Rosenbaum (2003) noted that explicitly structured advising is advantageous to students with less social know-how (first generation college students and those from lower socio-economic

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2 In this paper we will focus on the barriers that are most germane to our study of college mentorship. Financial barriers and liquidity constraints, obstacles to college completion that have received much recent research attention (e.g. Deming and Dynarski, 2008; Bettinger, 2004), will not be addressed. For a thorough overview of recent research on financial barriers and interventions, see Long (2008).

3 For a comprehensive view of the complexity of the decision processes that college students face and the lack of structured support for making these decisions, see Scott-Clayton (2011).

backgrounds). They found that such students often do not know that they need help, don’t take the initiative to seek it out, or don’t know what questions to ask.

A related line of study comes from the emerging research in behavioral economics. Recent studies have focused on the complexity of processes that students face and the information they use to make decisions (e.g. Bettinger, Long, Oreopoulos, and Sanbanmatsu, 2010). Students often need a “nudge” (Thaler and Sunstein, 2008) to complete complex tasks. In higher education, it is often assumed that course requirements provide that nudge or that students are sufficiently self-motivated and do not need external stimuli. College graduation rates indicate that that assumption might not be true; students may benefit from structured “nudges” to complete necessary tasks.

A second barrier to post-secondary success is students’ academic preparation and performance. Academic preparation has long been acknowledged as a contributing factor to college retention (e.g. Adelman & Gonzalez, 2006). Studies of college remediation (e.g. Calcagno and Long, 2008; Bettinger and Long, 2008) have attempted to identify whether academic remediation can improve students’ college outcomes.

Similarly, other interventions have focused on improving the efficacy of students’ non-academic school skills, such as time management and study skills. For example, Zeidenberg, Jenkins and Calcagno (2007) found that enrollment in a student success course (classes that focus on time management, note taking, learning styles and long term planning) at Florida community colleges corresponded to an increase in persistence rates of eight percentage points. Other studies (e.g. Kern, Fagley, & Miller, 1998; Robbins et al., 2004) have shown a positive link between productive study habits and cumulative GPA and college persistence.

The final obstacle to graduation that is related to college mentoring is students' lack of integration into the university community. Tinto (1975) articulated a theory of retention which suggests that feelings of academic or social separation lead to students dropping out. Researchers have attempted to identify ways to increase students' feelings of integration (e.g. Bloom and Sommo, 2005) in an attempt to increase college retention rates.

In addition to interventions that aim to address one particular obstacle, there are a number of interventions which attempt to address several barriers and influence students in multiple dimensions. Learning communities, comprehensive programs that enroll a cohort of undergraduate students in a common set of courses and provide academic and advising support, are a well-researched example of such an intervention.[4]

College mentorship, the focus of this study, is another intervention that addresses the problem of college attrition through multiple dimensions; it has elements of academic preparation, information gathering, and social integration. College mentors can have multiple goals: to help a student academically prepare for their courses, to counsel students on how to acquire better study skills or to provide advice on how to identify additional academic resources at their respective institutions.

Such support may be increasingly necessary, as traditional college counseling programs may be overextended in their efforts to provide support for all students. For example, a study of

[4] There is a great deal of rigorous empirical evidence that suggests that learning communities can support student success. For example, Bloom and Sommo (2005) find that learning communities lead to improved academic performance, although they do not increase college persistence. Scrivener et al. (2008) also examined freshman learning communities. They find that students randomly assigned to the treatment group move through remedial courses more quickly, take and pass more courses, and earn more credits in their first semester than students in the control group. Two years after enrollment in the learning community, they are also more likely to be enrolled in college.

counselors at community colleges conducted by the American College Counseling Association found that counselors report high student-to-counselor ratios. Fifty-five percent of schools have counselor to student ratios between 1 per 1500 and 1 per 3500 (Gallagher, 2010).

Studies of educational interventions that have attempted to use college counseling as a means for improving college outcomes provide an important context for the current investigation. There have been several such studies in the past decade. However, there are two complications that make evaluating these interventions difficult. First, treatments identified as "counseling" or "advising" vary greatly. Some are strictly academic, while others focus on study skills and social needs. Second, the most rigorous evaluations of counseling interventions to date have generally introduced multiple treatments, such as financial awards and social supports. The counseling component has typically been ancillary to the mechanism of interest.

Scrivener & Weiss (2009), for example, looked at the effect of such a combined intervention at two community colleges in Ohio. The intervention consisted of increased counseling (meeting with a program counselor twice a term for two terms) and a small stipend (to incentivize students' attendance in this more frequent, intensive advising). They found that students randomly assigned to the intervention registered for classes at a higher rate than students in the control group; however the effects dissipate after the intervention is over.

Brock and Richburg-Hayes (2006) investigated Opening Doors, an intervention which provided financial incentives and individual college counseling. Students could receive as much as $1000 per semester for adequate academic performance. College counselors followed up with students and reminded them of the incentive. Students in the treatment group signed up for more credits than those in the control group, they were more successful in passing courses and they persisted in school in greater numbers.

Angrist, Lang and Oreopoulos (2009) examined the effects of financial incentives and support services on academic achievement and persistence. Students were randomized into three treatment groups and a control group. The first treatment group was offered a range of support services including access to mentoring by older students and additional academic support. The second group was eligible to receive a substantial financial fellowship. The third treatment group was offered a combination of services and financial incentives. The authors found that students who were in the group receiving the combination of financial incentives and support services benefited the most. That group earned more credits, had higher GPAs and had lower levels of academic probation over the course of the year. The effect on grades persisted into the second year, after the program had finished. There was no impact on grades for the services only group and the students who received the fellowship only showed a small increase in grades. Importantly, these results were driven only by significant effects on female students; male students showed no increases in retention or academic success.

These studies suggest that advising can be an effective strategy for improving college retention by addressing common barriers to success. However, the effect of trained one-on-one counselors on retention has not been studied by itself; most rigorous studies have included other interventions in addition to enhanced counseling.

Background on InsideTrack

The motivating principle at InsideTrack is that student coaching in a student's educational career can lead to engagement, learning, retention and an increased probability of completing a degree. InsideTrack began offering services in the 2000-2001 school year and has coached more than 250,000 students nationally. The company first tested its coaching program by offering "free academic strategy sessions" to students at Stanford and the University of California, Berkeley. Building on the success of these initial coaching curricula, the company partnered with universities to provide coaching to their incoming students. InsideTrack is now the largest provider of student coaching in the country, employing hundreds of coaches who work with thousands of students nationwide.

As part of InsideTrack's services, InsideTrack wanted to demonstrate its success to its partner universities. The universities gave a list of potential students to InsideTrack. Each school determined the criteria for inclusion and the size of the sample and selected students according to their own priorities. While most schools assigned a representative sample of new entrants, there was some heterogeneity in the assignment systems. Some schools focused on full-time students; others assigned part-time students. Some assigned upperclassmen; others assigned new entrants. One school assigned athletes.

Part of the agreement between the school and InsideTrack included a procedure for random assignment. To demonstrate the effectiveness of its program, InsideTrack randomly divided the students into two groups while monitoring the randomization to make sure that the two groups were balanced across observable characteristics. After balancing the groups, the partner organization chose which of the two groups would receive counseling and coaching services with a coin flip.[5] These groupings allowed universities to monitor and evaluate ex-post the efficacy of InsideTrack.[6]

5 In some cases, the partner organization wanted a smaller control group. For the most part, these were schools who had used InsideTrack before and had previously had a 50/50 split. In these cases, InsideTrack showed the balance of the two groups and had the respective institutions certify that they were balanced. In Appendix Table 1, we report the results only for those schools that had a 50/50 split balance of students in the treatment and control groups. The degree completion results already relied on three of the lotteries with 50/50 splits in treatment and control, so these results do not change. The results remain the same in the other retention variables and are even stronger in the 18-

Students were then randomly assigned by InsideTrack to a "coach." The goal of the college coach was to encourage persistence and completion by helping students find ways to overcome both academic and "real-life" barriers and to identify strategies for success. InsideTrack is very particular in which coaches they hire and trains them to work with proprietary methodologies and programs to help students navigating college decisions. Each coachcontacts his or her students via phone, email, text messages and social networking sites and initially presents him or herself as a representative of both InsideTrack and the partner institution. Coaches generally work with students over two semesters although some students are part-time students enrolled in a single course.

The coaches contact their students regularly and in some cases have access to course syllabi, transcripts, and additional information on students' performance and participation in specific courses. InsideTrack uses this additional information in a set of predictive algorithms that assess each student's status for the purpose of reaching out to them on the right issues at the right times. Because of this background knowledge, conversations between coaches and students are both individualized and focused on success in school.

Students have the option to participate or not when contacted by the coach. All of the students, regardless of whether they opted to participate in the coaching, are included in our analysis. Because InsideTrack has worked with a variety of private, public, and proprietary institutions, lessons from InsideTrack may be more generalizeable than studies of a particular institution.

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month retention. At 24 months, the estimate is similar to that for all lotteries, but the reduced sample increases the standard errors so that it is no longer significant

6 The partnership contract also stipulated that both the school and InsideTrack needed to independently verify student retention rates.

III. Data and Empirical Methodology

Data

To evaluate InsideTrack's program, we requested the academic records for all of the students who were invited to work with InsideTrack during the 2003-2004 and 2007-2008 school years. During those two years, InsideTrack measured the performance of 13,555 students across eight different higher education institutions, including two- and four-year schools and public, private not-for-profit, and proprietary colleges.[7] The students were randomly assigned in 17 lotteries – five occurring in the 2003-2004 school year and 12 in the 2007-2008 school year. Across these 17 cohorts, Inside Track randomly assigned 8,049 students to receive services. The other 5,506 did not receive InsideTrack coaching services. All other services to the students (i.e. support from academic counselors, access to tutoring on campus) remained the same for both groups of students.

In Table 1, we report basic descriptive statistics for the control group and the differences (with their standard errors) for the treatment group. In terms of descriptive characteristics, the profile of students is weighted more toward non-traditional college students. For example, the average age of students is about 31. Only about 25 percent of students are under the age of 23. Unlike higher education throughout the United States, the sample of students is slightly more male (51 percent) than female.

[7] To protect the respective institutions and their strategies for retention and recruitment, Inside Track did not reveal the names of these colleges to the research team.

As the fourth column of Table 1 illustrates, the data are somewhat uneven across sites. The most commonly reported variable across sites was gender, which we observed in 15 of the lotteries. Age (8 lotteries), SAT score (4 lotteries), and on-campus living status (4 lotteries) are the next most commonly reported variables.

Random assignment should ensure that our treatment groups are balanced and comparable. As we explained, InsideTrack randomly divided lists of students provided by the partner schools into two groups. InsideTrack had the same data we have when they did the lottery, so in many cases, the balancing occurred on just one or two student characteristics. Once the lists were divided, the schools then chose which group received coaching and which group received the control (no additional services) treatment. While one might expect some small discrepancies, we should largely observe that there are no significant differences between the control and treatment groups. As shown in Table 1, this is the case. In the sample taken as a whole, there were no significant differences between the coached group and the non-coached group on any of the observable characteristics (gender, age, SAT scores or on- or off-campus residence). Similarly, these variables were missing in comparable proportions of the coached and non-coached groups; there were no significant differences in the information available for the two groups. Because of our sample sizes, we have sufficient power to identify even small differences in the groups. Hence our failure to find differences is an affirmation of the randomization.

To further demonstrate the balance of the treatment and control groups, we can also examine the balance of student characteristics by lottery. Table 2 does exactly this. In most cases, we know little about the overall sample; the lotteries differed on the number of observable characteristics recorded (ranging from one to 14). For each lottery, we tested the difference between the control and treatment groups. The effectiveness of the randomization holds when examining each lottery individually; of the 73 characteristics compared over the 17 lotteries, only one revealed a significant difference between the coached and non-coached groups at the 90 percent confidence level. Had we used a 95 percent confidence interval, we would have found no differences in any of the lotteries.

Finally, Figures 1-3 graph kernel density estimates of the age distributions, SAT scores, and high school grade point averages of both the treatment and control groups. For each variable, the distributions for control and treatment groups are similar. These similarities validate the randomization making it possible to identify the effects solely through comparing coached and non-coached groups within each lottery.

Partner universities also provided data on student persistence after six, twelve, eighteen, and twenty-four months. In some cases, partner institutions provided additional information on students' degree completion. We only track persistence at the partner colleges, but given that public policies are focused on retention at the institutional level, tracking persistence at this level is important for public policies and institutional success.

Empirical Strategy

Because the proposed treatment was administered using randomization, simple comparisons of participants in the treatment and control groups can identify the relative effects of the interventions. We estimate the "intent-to-treat" (ITT) effect using equation 1:

$$(1) \qquad y_{ij} = \delta + \beta * COACH_i + \alpha_j * Lottery_j + bX_i + \varepsilon_{ij}$$

where y is an outcome for individual i who participated in lottery j. *COACH* represents whether the individual was randomized into the treatment coaching group. We also include fixed effects for the student participation in a specific lottery, and X is additional controls for variables such as gender, age, high school GPA, and school type. The outcome of interest is college persistence, measured in six month increments from the start of the treatment. Our standard errors control for heteroskedasticity. As we mentioned above, many of our variables are available for one cohort, but not another. In these cases, we include a dummy variable for each variable indicating whether it is missing or not (e.g. a variable for gender missing, a variable for age missing) while substituting either the mean (for continuous variables) or a value of zero (for binary variables) for the variable itself.

IV. Empirical Results

In Table 3, we report our baseline results. Each column focuses on retention, as reported to InsideTrack by the colleges. We look at retention in six month increments. In Panel A, we report the baseline differences between coached and uncoached students without any controls except for the lottery fixed effects. In Panel B, we add controls for gender, age, ACT score, high school GPA, degree program, living on campus, Pell grant receipt, prior remediation experience, SAT score, and controls for missing values of covariates. The sample size changes across because of data availability from the individual schools.

The baseline persistence rate after six months is 58 percent. This persistence rate is lower than that of the overall college population, possibly due to the fact that many of these students are part-time students or older non-traditional students. In contrast to the uncoached persistence rate of 58 percent, the retention rate among coached students was 63 percent. The difference is significant over a 99 percent confidence interval. The relative effect is about a 9 percent increase in retention. When we control for covariates, the treatment effect is constant at about 5 percentage points.

In Column 2, we examine 12 month retention. Here the persistence rates for coached and non-coached students were 48.8 percent and 43.5 percent respectively. The treatment effect does not change as we include covariates in Panel B. The estimated effect represents a 12 percent increase in college retention.

The results after 6 and 12 months occur at a time when, in most cases, the treatment is still active. Coached students during this period are receiving phone calls from their coaches. Columns 3 and 4 show the results after 18 and 24 months. By this point, the coaches are no longer contacting the students. The treatment is over, yet we still find effects. After 18 months, the treatment effect was 4.3 percentage points representing a 15 percent increase in retention in this sample, and after 24 months, the treatment effect was 3.4 percentage points representing a 14 percent increase in persistence. These differences are all statistically significant over a 99 percent confidence interval. Moreover, these results do not change when we control for age, gender, ACT score, high school GPA, SAT score, on- or off-campus residence, receipt of a merit scholarship, Pell Grant awards, math and English remediation.

For a subsample of students (3 lottery cohorts), we observe whether the student completed college after the start of the treatment. InsideTrack worked with a variety of students, and degree completion could mean the completion of a certificate, an associate's degree, or a bachelor's degree. Across the three lottery cohorts, the average completion rate among the control group is 31 percent. The treatment effect is 4 percentage points and is statistically significant over a 90 percent confidence interval.

These graduation results only strengthen our results on retention. In our analysis in Table 3, we have only included students who are were attending the university after six, 12, 18, or 24 months. Some students may have completed a degree within the first six to twelve months, and these students would not appear to be attending. Our enrollment data did not include these individuals who might have already graduated. If we were to amend our results in Table 3 by redefining persistence as being persistence at time X or eventual graduation, then the estimated effects become slightly stronger.[8]

These effects on persistence (and completion) are large when compared to other interventions. Goldrick-Rab, Harris, Benson, and Kelchen (2011) examined a randomized experiment where students were given money for attending college without seeing any impact on persistence. Other studies of persistence found that need-based financial aid can modestly improve college persistence (e.g. Bettinger, 2004; Dynarski, 2003). These papers note that retention rates increase by 3 percentage points per $1000 of aid. In her study of merit-based aid, Dynarski (2005) found that state scholarships led to 5-11 percentage point increases in college persistence. In the case of the Georgia scholarships, the average expenditure was roughly $2500 per year. There is no evidence that the effects disappear or persist once students are no longer eligible for aid. Over this period of time, InsideTrack charged roughly $500 per semester. The effects are stronger for the InsideTrack study and show persistence at least one year following the end of the treatment.

[8] In Appendix Table 2, we report the same group of findings with the assumption that all missing data reflect attrition from college.

Robustness

The balance in the randomization and the failure of covariates to reduce the treatment effect suggest that the results are somewhat robust. One worry might be that a single lottery or single year could somehow account for the treatment effects. In Table 4, we estimate treatment effects separately for each lottery. We focus on the 12-month retention rate and the 24-month retention rate.

All of the lotteries show positive treatment effects after 12 months except for two (lottery 12 and lottery 17). The positive treatment effects are somewhat uniform around the average treatment effect of 5 percentage points. Two lotteries show effects in excess of 10 percentage points. Nine of the observed effects are statistically significant within the lotteries.

After 24 months, we only observe treatment effects in 11 of the 17 lotteries. Among the treatment effects after 24 months that we observe, four are positive and statistically significant with the maximum observed effect around 6.6 percentage points. Five are positive but not statistically significant with three of these five being larger in magnitude than the average treatment effect across all sites. Two are negative with the lowest observed effect at -1.7 percentage points.

The lesson from Table 4 is that the treatment effects are not arising because of one specific lottery. The observed effects are quite similar across sites. Broadly speaking the results suggest that the program is having a consistent effect across sites.

Another possibility is to check whether there are differences in treatment effects across years. If, for example, InsideTrack were to have different levels of effectiveness in different types of schools, we might expect some differences in treatment effects depending on whether InsideTrack's client base is similar across years. If these differences are large enough, then one year's impacts might explain the overall effects, but as we show in Table 5, the effects are balanced across years. Except in one case (2004 cohorts after 24 months), the treatment effects are all positive and significant for both samples across the different time horizons. The effects appear somewhat smaller in the case of the 2007 cohort although the differences are not statistically different except in the estimates of retention after six months. The effects seem to be somewhat balanced over time suggesting that the program's effects are not being driven by one year.

Heterogeneity in Treatment Effects

In Table 6, we investigate whether the effects differ for males and females. In Panel A, we report the effects for females, and in Panel B, we report the effects for males. After six months, the treatment effects were 2.5 percentage points for females and 6.1 percentage points for males. The difference is statistically significant. After 12 months, the treatment effects are 4.5 and 5.4 percentage points for females and males respectively. After 18 months, the treatment effects are 3.3 and 4.7 percentage points for females and males respectively. The impacts of coaching on persistence are not significantly different across genders after 12 or 18 months. The impacts after 24 months are 2.2 and 4.7 percentage points for females and males respectively. These differences are statistically significant.

The difference between the non-coached and coached groups was always greater for males than for females. While males persisted at rates lower than their female peers, student coaching had larger effects for males. Two of the four differences in treatment effects were statistically significant. Male completion rates typically lag behind females and have been somewhat insensitive to interventions. There appears to be some evidence that the effect is larger for males suggesting that this type of student coaching could reduce gender gaps in completion.

In Table 7, we examine the effects of the program for different age groups. We find that the estimated treatment effects have similar magnitudes across different age groups. The treatment effects are about 3.7 percentage points for students 30 and under after six months and about 6.2 percentage points for students older than 30. The treatment effects are 5.2 and 4.4 percentage points respectively after 12 months. After 18 months, the treatment effects are 4.0 and 3.4 percentage points for students 30 and under and over 30 respectively. After 24 months, the treatment effects are 4.1 and 2.4 percentage points respectively. All of the estimates are positive and only the treatment effect on older students after 24 months is statistically insignificant.

V. Conclusion

In the typical economic model of higher education, we assume that students know how to behave. We assume that they know how to study, how to prioritize, and how to plan. However, given what we know about rates of college persistence, this is an assumption that should be called into question. Across all sectors of higher education, more needs to be known about how to increase college persistence. Literature in economics, education, and sociology suggests that student coaching may be one way to help students succeed in college.

We find exactly this. While coaching was taking place during the first year, coached students were about 5 percentage points more likely to persist in college. This represents a 9 to 12 percent increase in retention. We also find that the effect of coaching on persistence does not disappear after the treatment. Coached students were 3-4 percentage points more likely to persist after 18 months and 24 months. These represented roughly a 15 percent increase in college retention among our sample. All of these effects were statistically significant. For the three campuses for which we have degree completion data, we find that coached students had graduation rates four percentage points higher than uncoached students after four years.

These results are highly supportive of the potential of student coaching. When we compared the costs and benefits of student coaching to programs that target financial aid, we find that student coaching leads to larger effects than financial aid and are much less costly to implement. The persistence of the effects after the treatment period and impact on completion only increases the cost effectiveness.

The results also shed light on recent interventions which included a counseling component. For example, in the Opening Doors initiative, students were provided financial incentives and counseling. While economists have stressed the incentives as being important in the observed effects, the regular contact from a college counselor may have been the operative mechanism by which effects occurred.

Additionally, Angrist, Oreopoulos and Lang (2009) found that students who had access to incentives and counseling had higher academic performance in college. They, however, did not find any effect of counseling by itself. There are two key differences between InsideTrack and the intervention studied by Angrist et al. One is that the counseling was voluntary in the treatment studied by Angrist et al. Students had to find the counselors. In the case of InsideTrack, the coaching remains voluntary but the counselors attempt to find the students and provide both proactive and continuing outreach to the students. The outreach by counselors was also present in the Opening Doors experiment. Another key difference is that the advisers in the

Angrist et al. study were trained upper class students, not full-time coaches and were not supported by the process and technology infrastructure that InsideTrack utilizes.

Our study is one of the first studies to use random assignment to evaluate the effects of student coaching and additional study is warranted. Research in other educational evaluations (e.g. Dee, 2004; Bettinger and Long, 2009) suggests that the traits of high school and college instructors influence student outcomes. It would be interesting to know if there are specific characteristics of the college coaches which increase their efficacy. We also do not know the specific types of coaching services and the specific actions of coaches which are most effective in motivating students.

Further study can also shed light on how student coaching might affect other student populations. Our study includes public, private, and proprietary institutions, and it includes a broad range of students including students who are pursuing associate's degrees, and bachelor's degrees. While the sample with whom InsideTrack works represents the broad range of college students, we cannot observe all of the unique characteristics of students in our samples, and even if we could, we do not have enough power to identify the effects on important subgroups. We do have power to identify the effects on males and females and younger and older students. We find that the effects do not vary by age. The effects on older students and younger students are similar. While the effects are positive for both males and females, we do find some evidence that the effect is larger for males. As such, it could reduce some of the disparities in college completion that exist by gender.

In an era when college retention is receiving increased attention in public policy and the media, our paper provides strong evidence that college coaching is one strategy that can improve retention andgraduation rates.

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Table 1. Descriptive Statistics and Balance Across Lotteries

	Control Group Mean	Difference for Treatment Group	Sample Size	Number of Lotteries With this Variable
Female	.488	.009 (.009)	12,525	15
Missing Gender	.091	.001 (.002)	13,555	17
Age	30.5	.123 (.209)	9,569	8
Missing Age	.294	.0001 (.0010)	13,555	17
SAT	886.3	-11.01 (16.19)	1,857	4
Missing SAT	.827	.001 (.002)	13,555	17
Living on Campus	.581	-.005 (.017)	1,955	4

Notes: Standard errors appear in parentheses.

Table 2: Significant Differences in Covariates By Lottery

Lottery	# of Characteristics	# with Significant Difference (90%)	% Receiving Treatment	N in Treatment (Control)
1 (n=1583)	2	0	62.8	994 (589)
2 (n=1629)	2	0	67.5	1,099 (530)
3 (n=1546)	2	0	54.1	836 (710)
4 (n=1552)	2	0	51.4	797 (755)
5 (n=1588)	2	0	59.4	944 (644)
6 (n=552)	3	0	79.9	441 (111)
7 (n=586)	3	0	84.3	494 (92)
8 (n=593)	3	0	79.8	473 (120)
9 (n=974)	9	0	49.8	485 (489)
10 (n=326)	6	0	49.7	162 (164)
11 (n=479)	6	0	49.9	239 (240)
12 (n=400)	2	0	50.0	200 (200)
13 (n=300)	1	0	50.0	150 (150)
14 (n=600)	1	0	50.0	300 (300)
15 (n=221)	3	1	63.3	140 (81)
16 (n=176)	14	0	39.8	70 (106)
17 (n=450)	12	0	50.0	225 (225)

Table 3. OLS Estimates of Baseline Treatment Effects on Persistence over Time

	6-month retention	12-month retention	18-month retention	24-month retention	Completed Degree
Control Mean	.580	.435	.286	.242	.312
Baseline Model					
Treatment Effect	.052***	.053***	.043***	.034**	.040*
	(.008)	(.008)	(.009)	(.008)	(.024)
Lottery Controls	Yes	Yes	Yes	Yes	Yes
N	13,552	13,553	11,149	11,153	1,346
Baseline w/ Covariates					
Treatment Effect	.051***	.052***	.042***	.033**	.040*
	(.008)	(.008)	(.009)	(.008)	(.024)
Lottery Controls	Yes	Yes	Yes	Yes	Yes
N	13,552	13,553	11,149	11,153	1,346

* significant over 90 percent CI, ** 95 percent CI, *** 99 percent CI
Notes: When included, covariates include age, gender, ACT score, high school GPA, SAT score, on- or off-campus residence, receipt of a merit scholarship, Pell Grant awards, math and English remediation, and controls for missing values. Standard errors appear in parentheses.

Table 4: Treatment Effects on Persistence Over Time by Lottery

Lottery	12-month Persistence	24-month Persistence	Lottery	12-month Persistence	24-month Persistence
1	.078***	.020	10	.052	--
2	.057**	.039**	11	.091**	--
3	.043*	.050**	12	-.055	--
4	.050**	.050**	13	.162***	.054
5	.040	.029	14	.054	-.010
6	.072*	--	15	.136**	--
7	.018	.066**	16	.062	.047
8	.023	-.017	17	.000	.058
9	.058**	--			

* significant over 90 percent CI, ** 95 percent CI, *** 99 percent CI
Notes: When included, covariates include age, gender, ACT score, high school GPA, SAT score, on- or off-campus residence, receipt of a merit scholarship, Pell Grant awards, math and English remediation, and controls for missing values.

Table 5. Treatment Effect by Year

	6-month retention	12-month retention	18-month retention	24-month retention
Control Mean	.617	.479	.381	.356
2004 Lotteries				
Treatment Effect	.088***	.070***	.068***	.030
	(.020)	(.020)	(.021)	(.020)
Covariates	Yes	Yes	Yes	Yes
N	1,774	1,745	1,520	1,524
2007 Lotteries				
Control Mean	.573	.426	.265	.217
Treatment Effect	.044***	.049***	.037***	.034***
	(.008)	(.009)	(.010)	(.009)
Covariates	Yes	Yes	Yes	Yes
N	11,808	11,808	9,629	9,629

* significant over 90 percent CI, ** 95 percent CI, *** 99 percent CI
Notes: When included, covariates include age, gender, ACT score, high school GPA, SAT score, on- or off-campus residence, receipt of a merit scholarship, Pell Grant awards, math and English remediation, and controls for missing values. Regressions include fixed effects for lottery. Standard errors appear in parentheses.

Table 6. Treatment Effects on Retention Over Time by Gender

	6-month retention	12-month retention	18-month retention	24-month retention
Females				
Control Mean	.661	.497	.346	.299
Treatment Effect	.025**	.045***	.033**	.022*
(std error)	(.012)	(.013)	(.014)	(.013)
N	6,045	6,045	4,740	4,744
Males				
Control Mean	.536	.403	.260	.215
Treatment Effect	.061***	.054***	.047***	.047***
	(.012)	(.012)	(.012)	(.011)
N	6,479	6,480	5,457	5,457

* significant over 90 percent CI, ** 95 percent CI, *** 99 percent CI
Notes: When included, covariates include age, gender, ACT score, high school GPA, SAT score, on- or off-campus residence, receipt of a merit scholarship, Pell Grant awards, math and English remediation, and controls for missing values. Regressions include fixed effects for lottery. Standard errors appear in parentheses.

Table 7. Treatment Effects on Retention Over Time by Age

	6-month retention	12-month retention	18-month retention	24-month retention
Students 30 or under				
Control Mean	.600	.438	.234	.184
Treatment Effect	.037***	.052***	.040***	.041***
(std error)	(.010)	(.011)	(.012)	(.011)
N	7,850	7,850	5,671	5,671
Students over 30				
Control Mean	.513	.400	.311	.266
Treatment Effect	.062***	.044***	.034**	.024
	(.017)	(.017)	(.016)	(.015)
N	3,958	3,958	3,958	3,958

* significant over 90 percent CI, ** 95 percent CI, *** 99 percent CI
Notes: When included, covariates include age, gender, ACT score, high school GPA, SAT score, on- or off-campus residence, receipt of a merit scholarship, Pell Grant awards, math and English remediation, and controls for missing values. Regressions include fixed effects for lottery.Standard errors appear in parentheses.

Figure 1. Age Distribution for Treatment and Control Groups.

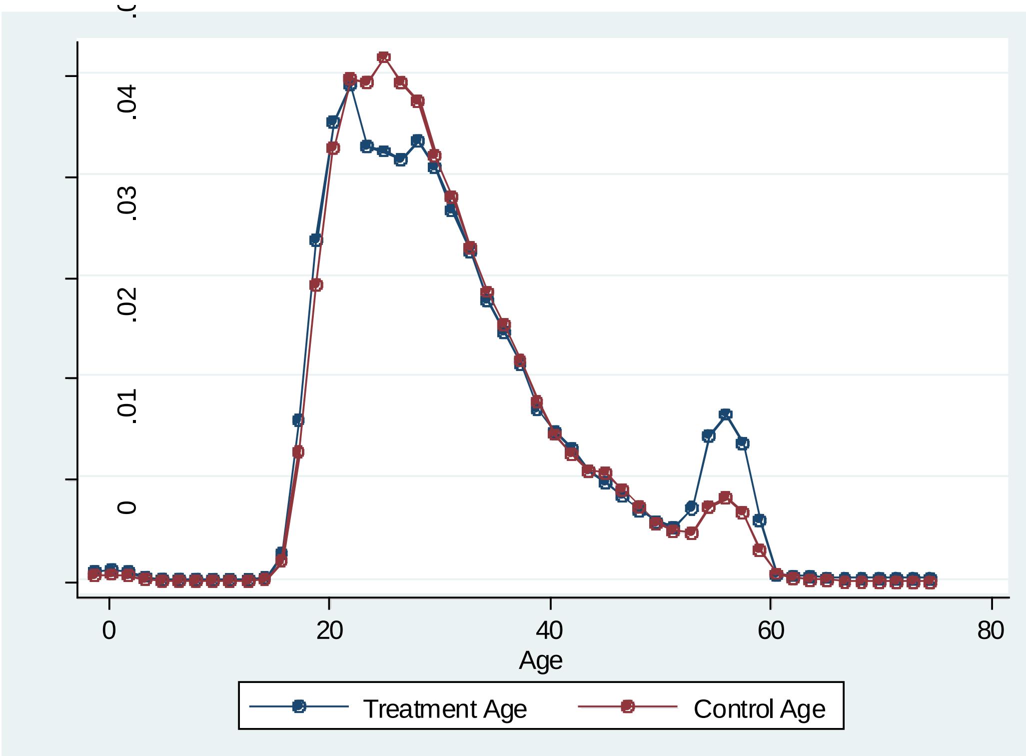


Figure 2. Distribution of SAT Scores for Treatment and Control Groups

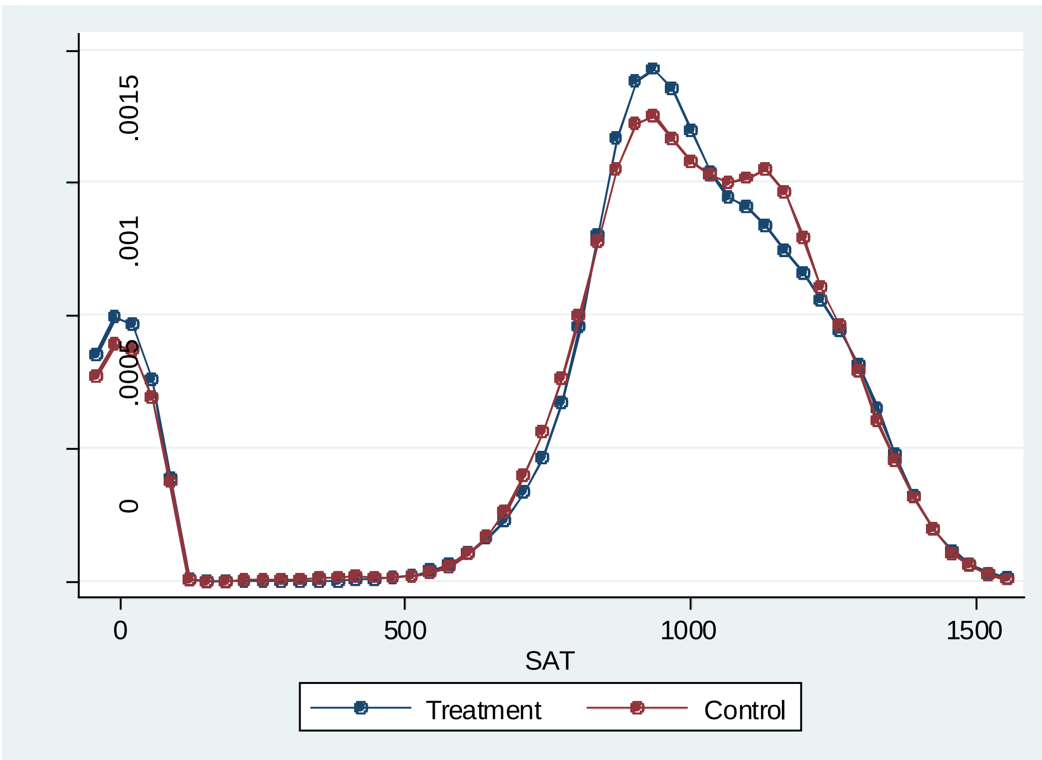
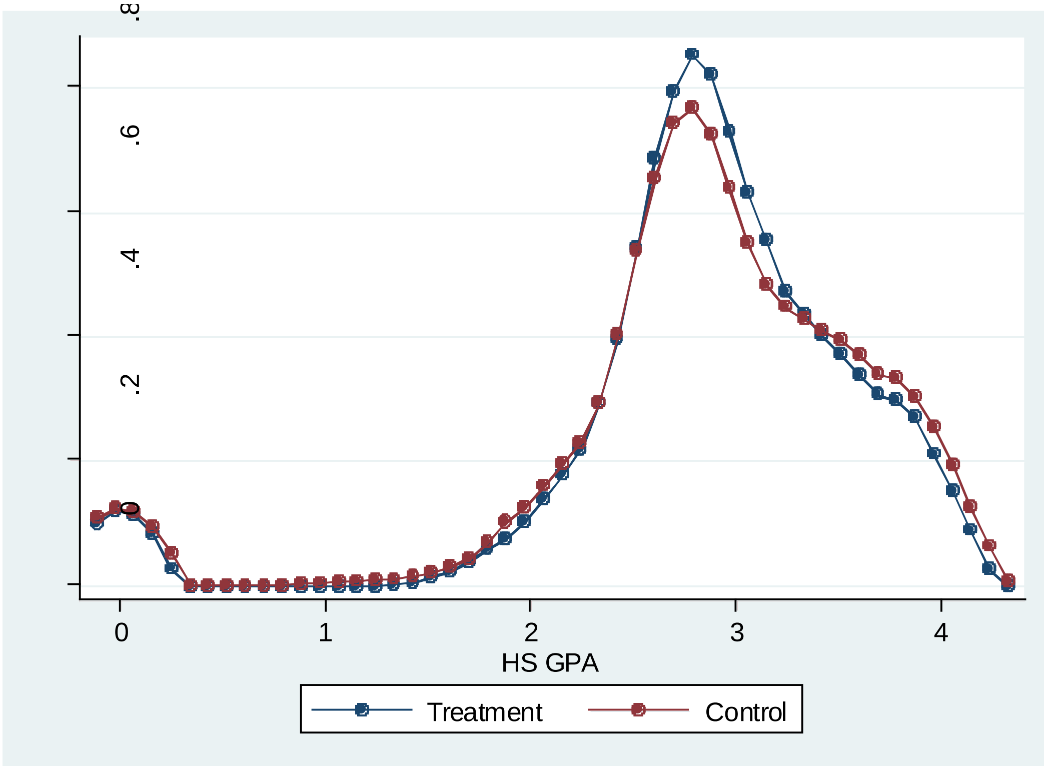


Figure 3. Distributions of High School GPA for Treatment and Control Groups



Appendix Table 1. OLS Estimates of Baseline Treatment Effects on Persistence over Time using only 50/50 split samples

	6-month retention	12-month retention	18-month retention	24-month retention	Completed Degree
Control Mean	.769	.614	.366	.350	.312
Baseline Model					
Treatment Effect	.037***	.050***	.070***	.027	.040*
	(.012)	(.014)	(.021)	(.020)	(.024)
Lottery Controls	Yes	Yes	Yes	Yes	Yes
N	3,527	3,527	1,344	1,348	1,346
Baseline w/ Covariates					
Treatment Effect	.037***	.050***	.070***	.027	.040*
	(.012)	(.014)	(.021)	(.020)	(.024)
Lottery Controls	Yes	Yes	Yes	Yes	Yes
N	3,527	3,527	1,344	1,348	1,346

Appendix Table 2. OLS Estimates of Baseline Treatment Effects on Persistence over Time Assuming Attriters Did Not Succeed

	6-month retention	12-month retention	18-month retention	24-month retention	Completed Degree
Control Mean	.580	.435	.286	.242	.311
<u>*Baseline Model*</u>					
Treatment Effect	.051***	.052***	.043***	.034**	.040*
	(.008)	(.008)	(.009)	(.008)	(.024)
Lottery Controls	Yes	Yes	Yes	Yes	Yes
N	13,555	13,555	11,155	11,155	1,350
<u>*Baseline w/ Covariates*</u>					
Treatment Effect	.050***	.052***	.042***	.033**	.040*
	(.008)	(.008)	(.009)	(.008)	(.024)
Lottery Controls	Yes	Yes	Yes	Yes	Yes
N	13,555	13,555	11,155	11,155	1,350