

## Why Do College-Going Interventions Work?<sup>†</sup>

By SCOTT CARRELL AND BRUCE SACERDOTE\*

*We present evidence from a series of field experiments in college coaching/mentoring. We find large impacts on college attendance and persistence, but only in the treatments where we use an intensive boots-on-the-ground approach to helping students. Our treatments that provide financial incentives or information alone do not appear to be effective. For women, assignment to our mentoring treatment yields a 15 percentage point increase in the college-going rate while treatment on the treated estimates are 30 percentage points (against a control complier mean rate of 43 percent). We find much smaller treatment effects for men, and the difference in treatment effects across genders is partially explained by the differential in self-reported labor market opportunities. We do not find evidence that the treatment effect derives from simple behavioral mistakes, student disorganization, or a lack of easily obtained information. Instead our mentoring program appears to substitute for the potentially expensive and often missing ingredient of skilled parental or teacher time and encouragement. (JEL I21, I23, I28)*

The United States ranks twelfth in the world in the fraction of 25–65-year-olds who have completed four years of college, though as recently as 1990 the United States ranked first in this measure. The rate of four-year college completion in the United States among 25–34-year-olds has leveled off at roughly 32–35 percent (OECD 2011).<sup>1</sup> This leveling off has occurred in spite of evidence of strong returns to college education (Goldin and Katz 2008) and educational attainment in general (Gunderson and Oreopoulos 2010).

\*Carrell: Department of Economics, University of California, One Shields Avenue, Davis, CA 95616, and NBER (email: [secarrell@ucdavis.edu](mailto:secarrell@ucdavis.edu)); Sacerdote: Department of Economics, Dartmouth College, 6106 Rockefeller, Hanover, NH 03755 (email: [bruce.sacerdote@dartmouth.edu](mailto:bruce.sacerdote@dartmouth.edu)). We thank Alan Gustman, Caroline Hoxby, Phil Oreopoulos, Sarah Reber, Doug Staiger, Sarah Turner, Hiromi Ono, and seminar participants at NBER Summer Institute for helpful suggestions. Sam Farnham, Minal Caron, Jay Graham, and Kevin Xie provided outstanding research assistance. Tim Vanderet, Beth Staiger, and Aaron Goone were superb project managers for the field experiment and a dedicated team of 80 Dartmouth students conducted the college coaching/mentoring. The US Department of Education's Institute for Education Sciences and the National Science Foundation provided generous funding. Data are provided by the New Hampshire Department of Education and we thank Michael Schwartz, Irene Koffink, and Sudha Sharma for building the state's Data Warehouse and providing support and data. Finally, the project could not have succeeded without the help, support, and patience of principals and guidance counselors across the state including but certainly not limited to Maureen O'Dea at Nashua North and South, Jan Delault at Pinkerton Academy, and Cindy Bilodeau and Patty Croteau at Manchester West High School.

<sup>†</sup>Go to <https://doi.org/10.1257/app.20150530> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

<sup>1</sup>See [www.oecd.org/edu/eag2011](http://www.oecd.org/edu/eag2011). The exact college completion rate varies by plus or minus 2 percentage points depending on which year of OECD data is used.

President Barack Obama and the US Department of Education have made increasing college completion rates a national priority. And college-going and completion is a key outcome measure being used in many states' Race to the Top programs.<sup>2</sup> There are already a myriad of programs, partnerships, and nonprofits that seek to raise college-going among students in the United States. One aspect that many of these programs have in common is a desire to "catch students early" in their educational careers and to promote college readiness (through choice of middle and high school courses), and awareness of the value of college. For example, some of the oldest and most well-funded programs fall under the umbrella of the US Department of Education's TRIO programs and include the GEAR Up and Talent Search programs, which are available in most states. These programs target sixth, seventh, and eighth graders, though not exclusively so.

More recently, economists and education researchers have begun to ask whether there is a payoff to communicating directly with high school seniors on college choice, college applications, and financial aid decisions. See, for example, Hoxby and Turner (2013); Castleman, Page, and Schooley (2014); and Bettinger et al. (2012). Several nonprofit groups including Let's Get Ready, BottomLine (see Castleman and Goodman 2015), and OneGoal (see Kautz and Zanoni 2014) offer free SAT prep and college choice counseling to high school juniors and seniors.

Initial results from some of these interventions suggest that low cost and brief interventions can have a meaningful impact on long-term student outcomes. For example, Hoxby and Turner (2013) show that mailing high achieving seniors an information packet and application fee waivers makes those students 5 percentage points more like to be enrolled in a "peer" institution (i.e., one that is a good match based on selectivity). Castleman, Page, and Schooley (2014) find that two to three hours of summer counseling raised college enrollment (among college bound high school graduates) by 5 percentage points.

Our research question is whether we can have a positive impact on college-going and persistence even late in a student's high school career and more importantly, why? We ask whether students' lack of specific noncognitive skills (as in Cunha and Heckman 2008 and Heckman and Rubenstein 2001) are a serious barrier to investing in college. Standard human capital theory suggests that students (and their parents and advisers) are forward looking and engage in careful planning about investments in college. Therefore, how can something as small as a text message, an application fee waiver, or several hours of extra coaching change a student's educational and career trajectory? Even within the set of behavioral economic theories, it may not be plausible to posit that large numbers of students "forget to apply," are inattentive to college options, or procrastinate filing applications to the point where the student settles for a high school diploma rather than a preferred two- or four-year college degree.

<sup>2</sup>Race to the Top is a large federally funded program in which states competed for grants based on implementation of innovative education policies such as use of data to target teaching to individual students or school choice policies.

We use three separate randomized interventions, along with survey and administrative data to ask which interventions matter and for whom. We designed a mentoring program and an informational/transcript transmission program that works with students in the winter of their senior year. We worked with high schools around the state of New Hampshire to implement the treatments. The high school guidance departments identify students who have expressed interest in college but have taken few or no steps to apply. The intent is to capture students who are right at the margin of applying to college or failing to apply. We randomly assign students within each school to one of several different treatment arms.

For our largest treatment group, we match high school seniors with a mentor, specifically a Dartmouth undergraduate. The mentors visit the students in the treatment group at their high school each week until all steps in college applications are completed and filed. We also make sure that the Free Application for Federal Student Aid (FAFSA) form is started and the sections other than the parental income portion are completed. We pay for all application fees (upfront) and in some cohorts we pay treatment students a \$100 bonus in cash for completing the program.

We also have treatment students assigned to receive only the cash bonus for completing applications but no mentoring. And we have a set of students assigned to an information and encouragement treatment. All students in this latter group receive letters, e-mails, and phone calls from the admissions office of their local community college. And we give the students, transcripts to a set of admissions offices at public colleges and universities to enable admissions officers to reach out to qualified prospective students.

## I. Existing Literature

There is a broad literature on the determinants of college-going. Much of the literature highlights the facts that, (i) key college-going decisions occur in middle school or even earlier; and (ii) test score gaps (among socioeconomic groups) that open up by fourth grade tend to widen rather than close.<sup>3</sup> Much of the literature concludes that early interventions are needed both to address the aspirations of students (fact i) and to prevent disadvantaged students from falling behind in their academic achievement and failing to take high school classes that prepare them for college (fact ii).

This literature has in part motivated the design of the US Department of Education's TRIO programs, which include Upward Bound and Talent Search. These programs catch students relatively early, i.e., eighth or ninth grade and provide a comprehensive suite of services. A randomized control trial (Myers et al. 2004) finds that Upward Bound students did not experience increased postsecondary enrollments, though there was a statistically insignificant 5 percentage point increase in the rate of enrollment in four year institutions relative to two year institutions.<sup>4</sup>

<sup>3</sup>See, for example, Wimberly and Noeth (2005); Levine and Nidiffer (1996); Nettles and Perna (1997); and Swail and Perna (2002).

<sup>4</sup>Importantly though, Upward Bound did increase the rate of four-year college-going at the expense of two-year college-going for students who had lower educational aspirations. We also find larger impacts for students with lower aspirations.

The education literature combined with findings on Upward Bound might suggest that because our target students are significantly behind in their college planning and application process (by the second half of senior year), our devised college coaching program is unlikely to have meaningful impacts. Furthermore, one might expect that if we did boost college-going for high school seniors, this effect would be short lived and our additional marginal college students would persist in college at a lower than average rate.

However, a recent literature within economics gives us optimism that targeted programs, which intervene at the right time with the right assistance or incentives can have a large impact. For example, Hoxby and Turner (2013) find that high achieving low income students apply to and attend more selective schools when mailed information specifically tailored to that student. Bettinger et al. (2012) find that having HR Block auto fill the FAFSA form for families with high school seniors results in an 8 percentage point increase in college-going. Likewise, Castleman and Page (2014a) show that targeted text messages increase the fraction of college bound seniors who initially enroll in college and, Castleman and Page (2014b, c) show that reminding first year undergraduates to re-file the FAFSA increases persistence into the second year.<sup>5</sup> And Bulman (2015) finds that increased availability of the SAT (or mandatory SAT taking) increases college-going.

There are several papers by economists that deal directly with college coaching. Avery and Kane (2004) provide evidence that coaching in a set of Boston schools raised interest in college and college attendance. Oreopolous, Brown, and Lavecchia (2014) find that a comprehensive mentoring program in a Toronto housing project raises high school graduation and college-going rates. And Castleman and Goodman (2015) find that the BottomLine counseling program shifts students toward a set of recommended (largely public) colleges and away from a set of private institutions with lower graduation rates.<sup>6</sup>

Most directly related to our work, Oreopolous and Ford (2016) have students attend three workshops in which students complete college applications and financial aid forms. This intervention results in a 5 percentage point increase in college-going, largely from increased community college attendance. Berman, Bos, and Ortiz (2008) study Los Angeles high school students who are mentored (mostly remotely) in the college choice and application process by UCLA and USC students under the SOURCE program. Students receiving the treatment do not experience increased college enrollment but there are increases in the fraction of students attending four-year colleges. Interestingly, similar to our results, the effects of SOURCE are concentrated among women. More recently Phillips and Reber (2015) find that an online, e-mail and text based mentoring version of SOURCE raises application rates for high school seniors without raising the overall college-going rate.<sup>7</sup>

<sup>5</sup>There is also a separate literature within social psychology that demonstrates that academic achievement can be boosted by short interventions that boost a student's sense of belonging or self worth. See Walton and Cohen (2011) for a heavily cited example and Walton and Yeager (2011) for a summary.

<sup>6</sup>Castleman and Page (2014) assign mentors to high school graduates who have been admitted to University of New Mexico. While they find no average effect, they do find that Hispanic students are more likely to enroll on time.

<sup>7</sup>Financial aid programs such as California's CalGrant (Kane 2003), Georgia's HOPE Scholarship (Dynarski 2000, Cornwell, Mustard, and Sridhar 2006), and West Virginia's PROMISE scholarship (Scott-Clayton 2011) also have significant impacts on the fraction of high school seniors who attend college. Our results on the use of financial

Our current work is distinct from the existing literature in a number of respects. First, we are focused on expanding college access for students at the margin of not applying anywhere. Second, we are able to examine college persistence while most existing papers used college enrollment as the main outcome. Third, we test a more intensive and involved intervention (in person mentoring) than many of the other interventions discussed above. This intensity may be appropriate and necessary. While our mentoring intervention is significantly more expensive (i.e., \$300 per student) than the cost of texting or mailing information, the estimated benefits of our mentoring treatment still vastly exceed the costs.

## II. Target Audience and the Sample

The program is targeted toward high school seniors who are on the verge of failing to apply to college. To identify a group of such seniors, we worked closely with guidance departments at 20 different New Hampshire high schools. There are roughly 60 high schools in the state. We worked with 20 of the larger schools who were most interested in the intervention and who were willing to allow a randomized evaluation thereof. In Carrell and Sacerdote (2013), we discuss how our high schools compare to other NH high schools and US high schools in general.

During December or January of each year, guidance counselors in our experimental high schools identify and nominate a set of seniors who are on the margin of applying or not applying to college. Specifically, we ask for the set of students who have expressed interest in attending college but have made little or no progress on filing an application. In the larger high schools, roughly 60 students of a graduating class of 300 seniors are nominated. Upon receiving the list of nominated students from a given high school, we randomly assign half the students to one of two treatment arms (the choice of which two arms varies by cohort). We randomize students to treatment arms within school. In randomizing, we do not employ any stratification by gender, test scores, race, free lunch, etc.

One objection to our sampling frame may be that we are narrowing our group of interest to students who are deemed to be at risk of failing to apply, as opposed to treating all students. We think this approach is a strength, since we are targeting more precisely the students who are marginal (with regard to college-going) late in the game.<sup>8</sup> Even with our focus on at-risk students, the mean rate of college-going in the control group is 44 percent and the control complier mean is 49 percent.<sup>9</sup>

---

incentives are consistent with results found by Angrist, Lang, and Oreopoulos (2009) and Fryer (2010). Specifically we do not find evidence that financial incentives alone (without a support structure or a plan to succeed) are effective, but we do find that combining incentives and a plan or support framework can work.

<sup>8</sup>In our scale up project with Let's Get Ready (a national nonprofit) we are instead randomizing among all students who volunteer to receive mentoring. In this ongoing work, we are finding similar sized treatment effects from a higher base rate of college-going and in a different set of schools.

<sup>9</sup>See Katz, Kling, and Liebman (2001) for definition and estimation of the control complier mean. The SOURCE program in contrast had 94 percent of the control group applying to college and 77 percent of the control group attending college. In the Upward Bound evaluation, 69 percent of the control group enrolled in college.

Online Appendix Table 2 shows how the sample sizes and treatment arms employed vary by cohort. The majority of the students are randomized between the mentoring treatment versus pure (no intervention) control. However, in 2013, due to expiration of funding, students were randomized between the informational/transcript only treatment and pure control. In 2014, students were randomized between the mentoring treatment and the informational/transcript only treatment. In 2012, students were randomized between the mentoring treatment versus the cash bonus only treatment. While we recognize, from a statistical point of view, having all four treatment arms employed simultaneously within each cohort would have been preferable, this was not possible. Not only did our funding arrive in two separate waves, we were able to treat more cohorts and employ more interventions than expected when we initially designed the program.<sup>10</sup>

Mentoring treatment, cash bonus only, and transcript only/informational students are notified by multiple methods (in person, over e-mail, and via letters) from their guidance counselor that they have been selected for a Dartmouth College program intended to help them complete college applications. Mentoring students are told that the program includes in person mentoring, having college applications and College Board (or ACT) fees paid, and a \$100 cash bonus for completing the process. The mentoring students in 2014 were not offered a cash bonus but were given all other aspects of the program.

Pure control (no intervention) students are not contacted prior to their graduation because we were concerned about changing their behavior or making them upset that they were randomized out of receiving mentoring and a cash bonus. The Clearinghouse data, College Board data, and other NH Data Warehouse items are available for all students in the treatment and control groups.<sup>11</sup>

The study was in part motivated by the fact that within Vermont and New Hampshire, there are large numbers of students who do not attend college but who have test scores above the fortieth percentile and even above the median. Figure 1 shows distributions of tenth grade math scores for the graduating class of 2010. Separate distributions are shown for college goers and noncollege goers. Clearly, the median for the second group lies below the median for the first group, but there is still substantial overlap in the distributions.

### III. The Interventions

#### A. *Mentoring/College Coaching Intervention*

The main intervention consists of three components, which include mentoring, paying application and College Board/ACT fees, and a \$100 cash bonus for

<sup>10</sup>We recognize the imperfection of having treatment arms coincide with cohorts rather than having all treatment arms running simultaneously within cohorts. Our results are robust to splitting the sample into four pieces (2009–2011, 2012, 2013, 2014) and viewing the findings as a set of four related experiments (see online Appendix Table 15).

<sup>11</sup>The IRB determined that, consistent with standard practice, the pure control (business as usual) i.e., nonparticipating students did not need to sign a waiver in order for the State of New Hampshire to provide de-identified existing administrative data for analysis.

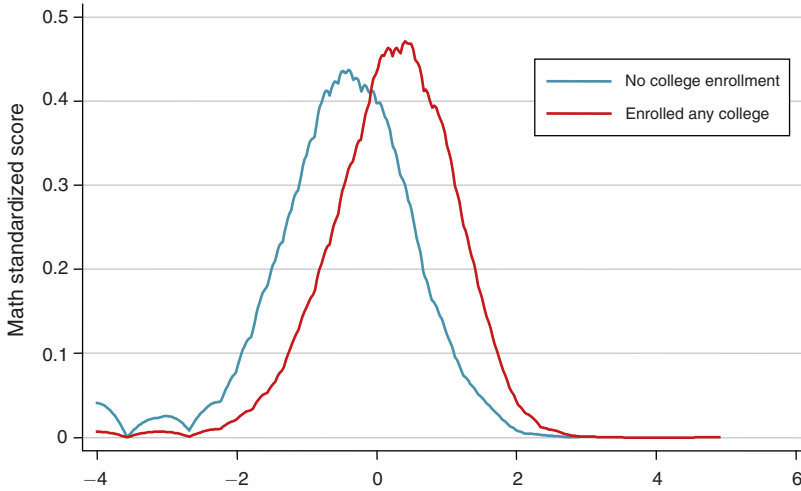


FIGURE 1. 2010 COHORT: STANDARDIZED TENTH GRADE MATH SCORES FOR COLLEGE GOERS AND NONCOLLEGE GOERS

completing the process. The process also includes starting the FAFSA. The most noticeable component (and most costly to implement) is in person mentoring by a Dartmouth College student. We had a team of roughly 20 Dartmouth students each year and most of these students worked full time on the project during January, February, and part of March.

For each high school, we choose a specific time and day of week to visit that school and all of the treatment students in that school. Visits are typically two–three hours in length and we promise up front to keep returning each week until every student has met his or her goals for college applications. The Dartmouth mentors keep track of each high school student’s tasks, progress, and various login IDs and passwords. Essays are often outlined during the mentoring session and further progress is made on essays at home.

The specific steps required to “complete” our program include completing college essays, completing and filing at least one application, requesting transcripts and recommendation letters, sending College Board or ACT scores where appropriate, and starting the student section of the FAFSA and requesting a PIN (personal identification number) for the FAFSA.

If students need to take the SAT or ACT, we help them sign up and provide e-mail and phone reminders before the testing date. We pay for all SAT and ACT fees including additional costs of sending scores to schools. SAT fees and application fees are paid in real time for the high school students using the project’s credit cards.

The program is not limited to applications to four-year colleges. Many students file applications to both two- and four-year colleges, while some (roughly one-third) file applications at two-year colleges only.

Almost all of the mentored time is spent completing college applications (often via the Common App), discussing and outlining college essays, sending SAT scores, sending transcripts, requesting recommendation letters, and filing the FAFSA. Most students finish the application process within three to four weeks.

### *B. Transcript Only/Letter of Encouragement Intervention*

In 2013 and 2014 we introduced another intervention designed to test whether the students in our sampling frame would be induced to attend college if they received a personalized letter of encouragement from one or more college admissions offices. Students in the “transcript only” intervention are nominated by guidance counselors as part of the same sample that is randomized to pure control or to mentoring treatment arms. Like the mentoring intervention, students selected for the transcript only intervention are notified of their selection through e-mail, in person notification by guidance counselors, and a letter/release form, which is mailed to parents.

If a student in the transcript only intervention agrees to participate, several steps occur: (i) The student fills out an online survey, which asks her to denote which of the participating colleges and universities interest her; (ii) The student signs a form, which releases her transcript to allow us to send to the participating colleges; and, (iii) We send all transcripts to all colleges, but we highlight for each admissions office those students that showed a particular interest in that institution.

All students receive a letter from the Community College System, which highlights the financial and non-pecuniary benefits of attending college and provides the URL to enable the student to apply. The Community College admission offices follow up the letter with e-mails and school visits to encourage the transcript only students to file an application. See online Appendix 17 for examples of the Community College letter sent to students.<sup>12</sup>

Based on transcript data, some fraction (roughly 25 percent) of participating transcript only students are selected by one of the selective four year institutions (among UNH, Keene State, Plymouth State, and Southern New Hampshire University) for additional encouragement. Those institutions send each selected student a letter stating that the admissions office has reviewed her transcript, considers her to be a strong applicant, and strongly encourages an application. Furthermore, most of these additional letters from admissions offices mention the possibilities of financial aid and explain that there are additional financial aid funds available if the student should choose to apply. See online Appendix 18 for example letters.

## **IV. Data Description**

The data come from several different sources. First, we have student names and unique student ID numbers provided by guidance departments. Second, for the mentoring treatment group we have data on the number of visits and the name and gender of the assigned mentor. Third, for all students we collected post-program survey data on parent’s education, applications filed, acceptances received, and intended plans after high school graduation. We also collected post-program survey data on intended occupation, the student’s estimate of annual income in that occupation, and their belief as to whether a college degree was needed to succeed in that occupation. The survey also included a host of personality questions designed to elicit

<sup>12</sup>Online Appendix Tables 1–18 are intended as an online Appendix to the final (published) version of the paper.



self-esteem, work ethic, and ability to meet deadlines. We asked a battery of questions about sources of help and advice on careers and college-going.<sup>13</sup> Fourth, we have data from the New Hampshire Department of Education's Data Warehouse. These data include student gender, free lunch status, year of graduation, race, 10th grade math, reading, and science scores, high school, and the year that the student first shows up in New Hampshire public schools. We also have SAT taking status, SAT scores, and the SAT Questionnaire data. We have the Data Warehouse data not just for our experimental sample, but also for every student in New Hampshire in the 2009–2014 graduation cohorts.

The Data Warehouse also provides us with National Student Clearinghouse data on each college enrollment experienced by a student in the 2009–2014 cohorts. Clearinghouse data detail the college attended, dates of enrollment, two-year versus four-year college, and any degrees earned. The Clearinghouse data cover 95 percent or more of enrollments at accredited colleges and universities.<sup>14</sup>

We define several outcome variables using the Clearinghouse data. Our main outcome variable is a dummy variable for a student having any enrollment in college. We also create dummy variables for any enrollment in a four year college, any enrollment in a two year college, and enrollments in two year colleges only. Most of our analysis focuses on outcomes of “ever enrolled” during the sample period as opposed to having separate dummies for enrolled in the first year after college, enrolled in the second year, etc. Naturally “ever enrolled” rises slightly as a cohort ages, and we control for this with the inclusion of cohort dummies. As a robustness check, we also ran all of our analyses with dummies for “ever enrolled in the first year” or “ever enrolled in the first two years” and results are similar.

Persistence in college (not just enrollment) is a major focus of the study and we define two different variables to measure persistence. For the graduating cohorts of 2009–2012, we first create a dummy for enrollment in three or more semesters of college. This is useful but not perfect since some colleges have quarters or mini terms in-between semesters. Second, we create a dummy for having enrolled in college in both the first 365 days following high school graduation and also the second 365 days following graduation.

The SAT Questionnaire data are useful in that they were mostly gathered administratively prior to the experiment.<sup>15</sup> The downside is that only 42 percent of the experimental sample took the SATs and, hence, completed the questionnaire. These SAT survey questions include (for example) desired level of education, whether the student wants to attend college close to home, involvement in sports and extracurricular activities, and whether the student needs help in forming educational plans.

Our own survey data were gathered 0–24 months after students graduated from high school. Admittedly, typical experimental designs use both pre- and post-surveys of the treatment and control groups to gather demographic information or measures

<sup>13</sup> We are grateful to Sarah Reber and Meredith Phillips who designed a similar survey for their college-going work and shared the survey with us.

<sup>14</sup> For more information on Clearinghouse data see <http://www.studentclearinghouse.org/colleges/studenttracker/>.

<sup>15</sup> The mentoring treatment does raise SAT taking by 7 percentage points, so some portion of the participants did in fact complete the SAT Questionnaire after the experiment had begun.

of attitude or knowledge. We worried that a pre-survey of both groups would alert the control students that they had been *nominated* to receive cash bonuses, payment of application fees, and mentoring but that they were randomly assigned to the control condition. Our fear was that this might affect their behavior or create resentment from not being chosen.

Instead, we engaged in a comprehensive effort to contact students by e-mail and Facebook following their high school graduation. To maximize the response rate we offered a \$75 gift card to any of Amazon, Starbucks, J-Crew, or iTunes. Even with numerous contacts per student, our survey response rate is roughly 25 percent.<sup>16</sup> Means for basic demographic variables and test scores for survey respondents and nonrespondents are shown in online Appendix Table 3. The response rate by treatment condition is shown at the bottom of online Appendix Table 2.

To account for potential non-response bias we used propensity score weighting to weight the data by the inverse probability of responding. Such a weighting method does not appreciably change the means of the survey variables or the empirical results that rely on survey measures. In estimating the propensity to respond, very few observables affect the likelihood of responding other than being enrolled in college, which raises the likelihood of response by 4 percentage points.

A copy of the survey is included as online Appendix 19.<sup>17</sup> We discuss specific survey items in depth in the results section. For the moment, we highlight a couple of the questions that we expected to be the most useful for distinguishing among various theories as to why marginal students fail to apply. In question 31, we ask students how much education their mother and their father want the student to complete. In question 10, we ask the subjects who are not enrolled in college to explain why they are not enrolled (open ended). Question 16 contains 8 subparts that measure self-esteem including “I feel I am a person of worth, equal to others (Strongly Agree, Agree, Disagree, Strongly Disagree).” Question 38 asks 6 different sub-questions about personal organization and ability to meet deadlines including “I often miss important deadlines if no one reminds me (Strongly Agree, Agree, Disagree, Strongly Disagree).”<sup>18</sup>

Table 1 shows summary statistics for the treatment and control groups for the 2009–2014 cohorts. In those 6 cohorts, we have data for a total of 2,624 students in the experiment, with 871 of those students in the mentoring treatment group. Forty-five percent of the students in the mentoring treatment participated in the study. Fourteen percent of students assigned to the transcript only intervention participated. Roughly 20 percent of mentoring treatment students and 17 percent of control students are nonwhite.<sup>19</sup> Twenty-eight percent of control students and 29 percent of mentoring treatment students are free and reduced lunch eligible.

<sup>16</sup>One takeaway from our project is that re-contact of study subjects can be extremely challenging even when incentives are employed.

<sup>17</sup>Several appendices are mentioned out of numerical order due to either length of the online Appendix (e.g., the multipage survey, which is left to the end) or the lower importance of the online Appendix table.

<sup>18</sup>One downside to having a post, but not pretreatment survey is that the treatment itself might affect the responses. For the personality measures, we think that this is unlikely and we test this statistically by regressing survey measures directly on the treatment dummy to test whether the treatment affected the mean response.

<sup>19</sup>“Nonwhite” includes all students who identify as Hispanic, Asian, black, Native American.

TABLE 1—SUMMARY STATISTICS FOR MENTORING TREATMENT AND CONTROL GROUPS

Variable	Control group			Mentoring treatment			Transcript only group		
	Observations	Mean	SD	Observations	Mean	SD	Observations	Mean	SD
Accepted treatment	902	0	0	871	0.454	0.498	851	0	0
Accepted transcript only	902	0	0	871	0	0	851	0.141	0.348
10th grade math score (standardized)	798	-0.480	0.937	778	-0.286	0.943	750	-0.370	0.957
10th grade reading score (standardized)	799	-0.436	0.928	772	-0.278	0.966	751	-0.394	0.940
Math > 50th percentile in state	798	0.312	0.464	778	0.335	0.472	750	0.304	0.460
Reading > 50th percentile in state	799	0.350	0.477	772	0.398	0.490	751	0.381	0.486
Math > 75th percentile	798	0.164	0.371	778	0.185	0.389	750	0.157	0.364
Reading > 75th percentile	799	0.213	0.410	772	0.224	0.417	751	0.221	0.415
Free and reduced lunch eligible	902	0.277	0.448	871	0.286	0.455	851	0.283	0.451
Male	902	0.548	0.498	870	0.575	0.495	851	0.605	0.489
Nonwhite	902	0.173	0.378	871	0.201	0.401	851	0.160	0.367
Graduation year	902	2011.527	1.281	871	2011.658	1.641	851	2013.280	0.449
No SAT data	902	0.708	0.455	871	0.457	0.498	851	0.489	0.500
Any college (clearinghouse)	902	0.438	0.496	871	0.592	0.492	851	0.353	0.478
Four year college (clearinghouse)	902	0.169	0.375	871	0.276	0.447	851	0.108	0.311
<i>For 2009–2012 cohorts</i>									
Persist for first two years post-grad	573	0.307	0.462	616	0.339	0.474			
Persist in a four-year college	573	0.148	0.356	616	0.162	0.369			
Enrolled 3+ semesters	573	0.361	0.481	616	0.412	0.493			

Notes: Students are randomly assigned to treatment within high school. Data include 2009–2014 cohorts. Regressions include high school  $\times$  cohort dummies, which is the level at which randomization occurred. Persistence outcomes (last three rows) are for 2009–2012.

About 35 and 39 percent of control and mentoring treatment students (respectively) have a 10th grade reading score that is above the state median, while 31 and 33 percent have a math score that is above the median. The average *standardized* math and reading scores are potentially misleading since the distributions are not normal and have very fat left hand tails. Carrell and Sacerdote (2013) shows that the mentoring treatment versus control score distributions overlap nearly perfectly. Randomization was performed at the high school times cohort level.<sup>20</sup> While pretreatment means for test scores and “nonwhite” are slightly different between the mentoring treatment and control arms, most of these differences disappear when we control for high school times cohort effects.

In Table 2, we show regressions of a dummy for mentoring treatment status on pretreatment variables controlling for high school  $\times$  cohort fixed effects and birth-year  $\times$  cohort effects. Standard errors are corrected for clustering at this level. We show separate regressions for the men and women in the sample. The pretreatment variables are not significantly correlated with treatment status for either gender. The

<sup>20</sup>We also include high school times cohort fixed effects when calculating our treatment effects as this is the level in which randomization occurs. This procedure is similar to the charter school literature that includes lottery fixed effects. See Hoxby and Murarka (2009) and Abdulkadiroglu et. al. (2011).

TABLE 2—MENTORING TREATMENT STATUS REGRESSED ON PRETREATMENT CHARACTERISTICS

	Treatment status men (1)	Treatment status women (2)
Standardized 10th grade math score	0.001 (0.012)	0.041 (0.025)
Standardized 10th grade reading score	-0.025 (0.014)	-0.006 (0.020)
Free reduced lunch eligible	-0.043 (0.027)	0.073 (0.046)
Student is nonwhite	0.019 (0.032)	-0.038 (0.057)
Observations	1,216	866
$R^2$	0.355	0.321
$F$ pretreatment variables	1.281	2.109
$p$ -value	0.294	0.098

Notes: Students are randomly assigned to treatment within high school. Data include 2009–2014 cohorts. Regressions include high school  $\times$  cohort dummies, which is the level at which randomization occurred. Standard errors are clustered at the high school  $\times$  cohort level. Regressions also include birth year  $\times$  cohort dummies.

$p$ -values on the test for the joint significance of all pretreatment variables are statistically insignificant for both men (0.29) and women (0.10).

## V. Empirical Strategy

We calculate treatment effects from the interventions in a straightforward manner. We regress outcome variables (e.g., Enrolled in Any College) on dummies for treatment arm, high school  $\times$  cohort fixed effects, and demographic characteristics. Specifically, we run regressions of the following form:

$$(1) \quad \text{Enroll}_i = \alpha + \beta_1 \times \text{mentoring treat}_i + \beta_2 \times \text{transcript only}_i \\ + \beta_3 \times \text{cash bonus only}_i + \gamma \times \mathbf{X}_i + \rho \times \mathbf{Z}_i + \varepsilon_i.$$

Here, the outcome is whether or not student  $i$  enrolls in college following graduation, i.e., after the intervention. The dummy variables *mentoring treat* <sub>$i$</sub> , *transcript only* <sub>$i$</sub> , and *cash bonus only* <sub>$i$</sub>  denote whether the student is assigned to one of three treatment groups, while the omitted category is the no intervention control group.<sup>21</sup> The vector  $\mathbf{X}$  is a set of student level background characteristics including gender, nonwhite, age, free and reduced lunch status, and in some specifications 10th grade test scores. The vector  $\mathbf{Z}$  is a set of high school by cohort fixed effects. Standard errors are corrected for clustering at the high school  $\times$  cohort level, which

<sup>21</sup> Our cash bonus only results are so noisy that we do not present those in the main tables but do present results for them in an online Appendix and in the text.

is the level at which the experiment is run.<sup>22</sup> We control for age by including a full set of birth year  $\times$  cohort dummies. This yields slightly greater precision than when we only include age dummies or continuous variables for age and age squared.

We present OLS regressions with robust standard errors. The alternative of running Probits and presenting marginal effects yields quantitatively and qualitatively similar results. See online Appendix Table 6 for baseline specifications using Probits.

Equation (1) describes an intention to treat estimate. As noted above, only about half of the invited mentoring treatment students participate. (None of the control students were allowed to participate.) We also calculate treatment-on-the-treated estimates by instrumenting for participation in each treatment arm with dummy variables for assignment to the various treatment groups. Not surprisingly, the treatment-on-the-treated estimates for mentoring are roughly twice the intention to treat estimates since half the students are taking up the mentoring program.

As discussed above, we are also interested in whether the mentoring treatment is particularly effective for subgroups of students. The hope is that subgroup analysis will shed light on which hypotheses can explain the effectiveness of college-going interventions. To do this we estimate equations of the following form:

$$\begin{aligned}
 (2) \quad \text{Enroll}_i &= \alpha + \beta_1 \times \text{mentoring treat}_i + \beta_4 \times \text{student characteristic}_i \\
 &+ \beta_5 \times \text{mentoring treatment}_i \times \text{student characteristic}_i \\
 &+ \beta_2 \times \text{transcript only}_i + \beta_3 \times \text{cash bonus only}_i \\
 &+ \gamma \times \mathbf{X}_i + \rho \times \mathbf{Z}_i + \varepsilon_i.
 \end{aligned}$$

Here,  $\beta_4$  captures the direct effect of a particular student characteristic (e.g., having a college educated mother or “struggles to meet deadlines”) on college-going, while  $\beta_5$  captures any interaction between that characteristic and the mentoring treatment.

## VI. Results

Our baseline estimates are shown in Table 3. The panels differ in that we change the dependent variable from any college to four-year college. The top panel shows treatment effects for enrollment in any college” for the cohorts of 2009–2014. Column 1 shows the treatment effects for both genders combined. The mentoring treatment raises college-going by 6.0 percentage points and the effect is significant at the 1 percent level.

However, the effects look very different when we split the sample by gender. There is no average effect of assignment to the mentoring program on college-going for men but a highly significant 14.6 percentage point effect for women. This is

<sup>22</sup>We also estimated our standard errors for our main treatment effects clustering at the high school level, and we use the Wild Bootstrap procedure as suggested by Cameron, Gelback, and Miller (2008) to deal with the small number of clusters. Our confidence are slightly smaller when we use this procedure compared to those presented in Table 3.

TABLE 3—BASELINE MENTORING TREATMENT AND TRANSCRIPT ONLY TREATMENT EFFECTS ON ENROLLMENT IN COLLEGE

	Whole sample (1)	Women (2)	Men (3)	Did not take SAT (4)	Took SAT (5)
<i>Effects on enrollment any college</i>					
Mentoring treatment (OLS)	0.060 (0.018)	0.146 (0.042)	0.007 (0.025)	0.083 (0.026)	0.035 (0.035)
Transcript only (OLS)	-0.005 (0.019)	0.005 (0.034)	0.000 (0.021)	0.035 (0.034)	-0.049 (0.035)
Mentoring treatment (IV)	0.133 (0.041)	0.299 (0.087)	0.017 (0.061)	0.160 (0.047)	0.086 (0.085)
<i>Control mean dependent variable</i>	<i>0.438</i>	<i>0.446</i>	<i>0.431</i>	<i>0.415</i>	<i>0.494</i>
<i>Effects on enrollment four-year college</i>					
Mentoring treatment (OLS)	0.057 (0.018)	0.107 (0.031)	0.020 (0.028)	0.103 (0.026)	-0.005 (0.033)
Transcript only (OLS)	0.001 (0.015)	0.007 (0.022)	0.003 (0.028)	0.002 (0.012)	-0.038 (0.030)
Mentoring treatment (IV)	0.125 (0.037)	0.222 (0.062)	0.047 (0.068)	0.202 (0.048)	-0.018 (0.083)
<i>Control mean dependent variable</i>	<i>0.169</i>	<i>0.164</i>	<i>0.172</i>	<i>0.136</i>	<i>0.247</i>
<i>First stage for IV</i>					
Mentoring treatment	0.463 (0.039)	0.500 (0.044)	0.429 (0.042)	0.511 (0.033)	0.444 (0.070)
Observations	2,623	1,114	1,509	1,453	1,170

Notes: Each estimated effect is from a separate regression with the exception that OLS for mentoring and transcript only treatment effects (rows 1 + 2 and rows 4 + 5) are estimated in the same regression as in equation (2). Regressions include high school  $\times$  cohort dummies, which is the level at which randomization occurred. Standard errors are clustered at the high school  $\times$  cohort level. Regressions also include birth year  $\times$  cohort dummies and controls for race, gender, and free lunch.

against a control group mean college-going rate of 41.1 percent and a control complier mean of 43.9 percent.<sup>23</sup> In the third panel, we show the first-stage regression for the women of participating in the program on assignment to the treatment group. The first-stage coefficient is 0.46.

The second-stage regression for the women is in the third row of column 2. The mentoring treatment has an effect of 29.9 percentage points on college-going for women who take up the treatment (relative to the unidentified set of control women who would have taken up the treatment had they been randomly selected). Again, this is a large effect when measured against the control complier mean of 43.9 percent. Column 3 shows that the mentoring effects for the men, and these are indeed statistically significantly different. The  $p$ -value for the difference in treatment effects between men and women is 0.002 (not reported in the table).

The second row in Table 3 shows effects for the transcript only treatment. The point estimates are small, negative and not statistically significant. For example, for the combined samples of men and women, we can rule out positive effects on

<sup>23</sup> See Katz, Kling, and Liebman (2001) for calculation of the estimated control complier mean.

college-going of greater than 3.2 percentage points and negative effects as large as  $-4.2$  percentage points. While we do not have as much power as we would like, the standard errors on the transcript-only intervention are similar to the standard errors on the mentoring intervention or the standard errors for key outcomes in the Hoxby and Turner (2013) intervention.

One reason the transcript-only treatment may not be effective is that it has a 14 percent take up rate. This is despite the fact that students received multiple prompts via e-mail, mailed letters, and in-person notification from their guidance counselors. Unfortunately, when we instrument for take-up with treatment assignment, we do not have enough precision for that IV analysis to tell us whether “transcript-only” is effective for the students who do take it up.

However, we also have some separate evidence that *even among students who accepted the treatment, they did not apply to the schools that were reaching out to them*. Specifically, University of Southern New Hampshire contacted 24 students of whom only 1 applied and 0 enrolled. University of New Hampshire sent letters of encouragement to 15 students, of whom 0 filed college applications. White Mountains Community College emailed and called 20 transcript-only students, of whom 1 enrolled. This consistent finding provides a strong indication that students taking up the transcript-only intervention are not being induced into additional enrollments.

The second panel of Table 3 switches the outcome to enrollment in a four-year college. (Online Appendix Table 5 contains analogous results for enrollment in a two-year college.) The mentoring effect for the combined men and women sample on four-year college-going is 5.7 percentage points and is significant at the 0.01 level. The intention to treat effect for women is 10.7 percentage points and the treatment on the treated effect for women is 22 percentage points. In a *relative* sense, these effects are substantially larger than the effects for “any college” since the control mean for women enrolling in a four-year college is 13.6 percent and the control complier mean is 14.0 percent. In other words, for treated women, assignment to the mentoring treatment nearly doubles the four-year college-going rate.

Average intention-to-treat effects of mentoring for the men are again small and have large standard errors. In columns 4 and 5, we split the sample by whether or not the student took the SAT. The point estimates are clearly larger for students who did not take the SAT, i.e., those students who had a lower level of preparation going into the process. Among men and women who did not take the SAT, assignment to the mentoring treatment raises four-year college-going by 10.3 percentage points. For men who did not take the SAT, the treatment effect on four-year college-going is a statistically significant 12 percentage points.<sup>24</sup>

We again find that the transcript-only treatment does not promote four-year college enrollment. In all columns, the estimated effects from the transcript-only intervention are small and statistically insignificant. Combining the women and men in the experimental sample, we can rule out effects on four-year college-going of greater than 2.9 percentage points.

<sup>24</sup>For brevity, results splitting by gender and SAT status at the same time are not reported in the tables.

### A. *Effects on Enrolling in Two-Year versus Four-Year Colleges*

The program has similar sized effects on both “any college” and “four-year college.” This result implies that the program’s overall effects should be relatively small for attending two-year colleges. In online Appendix Table 5, we show that this is indeed the case. For example, in column 2, we see that assignment to the treatment group increases two-year college enrollment by an insignificant 2.6 percentage points.

The program significantly increases the overall four-year college-going rate for women but not the two-year rate. This does not necessarily imply that the program failed to shift some women from “no college” status to “two-year college” status. In fact, the most likely (but not observable) mechanism is that the program moved some women from two-year status to four-year status and some women from no college to two-year college and possibly even a few from no college to four-year college status.<sup>25</sup>

Online Appendix Table 7 provides evidence, which is consistent with this hypothesis. We interact the treatment dummies with dummies for above and below the sample median on tenth grade reading (NECAP) test. In column 1, we show the treatment raises two-year college-going for women with below median test scores and decreases two-year college-going among women with above median reading scores. In column 2, we see that four-year college enrollment is boosted by 6.0 percent for below median score women but 18 percent (adding the two coefficients together) for women with above median reading scores.

The mentoring program has different effects for students in different parts of the test score distribution. And the pattern of these heterogeneous effects is consistent with our expectations (i.e., larger effects on four year enrollment for higher scoring students).

### B. *Evidence on Persistence*

Clearly, there is a difference between convincing high school seniors to attend college *at all* and having them persist and graduate. A natural question is whether the differences in college enrollment between the treatment and control groups persist after the first year. Table 4 addresses this question. We limit the sample to the 2009 through 2012 cohorts since these are the only mentoring cohorts for whom we have more than one year’s worth of college-going data. This sample limitation means that we do not include a dummy for the transcript-only treatment since that intervention only exists in the 2013 and 2014 cohorts.

The first three columns are for the women in the sample. In column 1, we use as the dependent variable a dummy for the student being enrolled in three or more semesters of college. The mentoring treatment effect is 12.9 percentage points and is significant at the 5 percent level. This effect (for persisting in any college) is nearly identical to our Table 3 effect for enrolling in college at all. The similarity between

<sup>25</sup> It’s not possible to observe directly what each woman would have done in the absence of the program, so it is not possible to state definitively how the program moved numbers of people between outcome categories.



TABLE 4—MENTORING TREATMENT EFFECTS ON PERSISTENCE IN COLLEGE

	Women enrolled in 3+ semesters (1)	Women enrolled any college both school years postgraduation (2)	Women enrolled four year college both school years postgraduation (3)	Women enrolled second year conditional on enrolled first year (4)
Mentoring treatment	0.129 (0.053)	0.105 (0.042)	0.097 (0.030)	-0.040 (0.066)
<i>Control mean dependent variable</i>	0.365	0.295	0.140	0.696
Observations	535	535	535	263
$R^2$	0.172	0.123	0.105	0.165

*Notes:* Outcome variables are four different ways to measure persistence into the second year of college. Sample is limited to women in the 2009 and 2010 cohorts. Column 4 is dummy for persisting into year two, and the sample is conditioned on having enrolled in the first year. Outcome variables are based on the Nation Student Clearinghouse data. Students are randomly assigned to treatment within high school. Data include 2009, 2010, and 2011 cohorts. Regressions include high school  $\times$  cohort dummies, which is the level at which randomization occurred. Standard errors are clustered at the high school  $\times$  cohort level. Regressions include birthyear  $\times$  cohort dummies to control for students' age within grade.

the effects for enrollment and persistence suggests that the students induced to enroll by the mentoring treatment are persisting in college at the same rate as the students in the control group.<sup>26</sup>

In Table 4, column 2, the dependent variable is a dummy for being enrolled in any college for *both* the first year and the second year after high school graduation. The point estimate is 10.5 percentage points and significant at the 5 percent level. Finally, when we examine effects on being enrolled in a four-year college for both years post-high school graduation, the treatment effect is 9.7 percentage points.

Finally, in column 4, we limit the sample to women who were enrolled in the first year and ask whether the program affects their likelihood of being enrolled in the second year. The question being asked is whether treatment students in college persist at higher or lower rate than control students. Interestingly, the treatment students have persistence that is in line with that of the control students. That is, the treatment has encouraged an extra set of women to attend college and these women persist at a rate that is no more or less than the control average.

### C. Evidence on Mechanisms

We turn now to several related questions: how does the mentoring treatment work?, why does it work particularly well for women?, and why does the transcript only intervention not work? We first confront these questions in part by interacting treatment status with student characteristics and student answers to survey questions.

Table 5 interacts the dummy for the mentoring treatment with the student's reports of need for help in educational planning or which people helped with college applications. Each *row* in Table 5 represents a separate regression and reports coefficients

<sup>26</sup>Table 3 uses a larger sample for six cohorts, so we double checked that the enrollment effect is similar using just the first four cohorts.

TABLE 5—INTERACTION OF MENTORING TREATMENT WITH SOURCES OF ASSISTANCE ON APPLICATIONS  
(dependent variable is enrollment in any college)

SAT questionnaire measure	Coefficients on treatment × SAT measure (1)	Coefficient on treatment indicator (2)	Coefficient on SAT measure (3)	Observations (4)	Mean (5)	SAT indicator regressed on male dummy (6)
Do not need help with educational planning	-0.116 (0.059)	0.126 (0.058)	0.049 (0.039)	1,302	0.829	0.015 (0.004)
Survey measure	Coefficients on treatment × survey measure (1)	Coefficient on treatment indicator (2)	Coefficient on survey measure (3)	Observations (4)	Mean (5)	Survey indicator regressed on male dummy (6)
Parents help with college applications	-0.131 (0.067)	0.118 (0.045)	0.133 (0.041)	724	0.468	0.014 (0.037)
Teacher helps with college applications	-0.165 (0.091)	0.112 (0.030)	0.089 (0.062)	646	0.172	-0.023 (0.030)
Guidance counselor helps with college application	-0.009 (0.069)	0.0541 (0.037)	0.037 (0.057)	724	0.312	-0.0982 (0.034)

Notes: Standard errors are in parentheses. In each row, columns 1–5 are from a single regression of “Any College” on the treatment dummy, the survey measure, and the interaction of the two. Regressions also include controls for male, free lunch status, and high school × cohort dummies. Column 6 is from an OLS regression of the survey measure on a dummy for male. Numbers are rounded to three decimal places. Survey questions are as follows: “Thinking of the people in your life, which helped you with college applications ... Check all that apply.”

on the interactions of mentoring treatment status with a dummy for sources of help with applications (column 1), the main effect of the treatment (column 2), and the main effect of “who helped” (column 3). The outcome variable is enrollment in any college.

The first row uses data from the SAT Questionnaire. The students are asked whether they anticipate needing outside or additional help forming educational plans. We interact a dummy for not needing help with the treatment. In column 2, the baseline treatment effect for students who do anticipate needing help is 12.6 percentage points. On the flip side, the treatment effect is nearly zero for students who do not anticipate a need for additional (outside) help (adding columns 1 and 2 together).

The SAT Questionnaire data are pretreatment, at least for the vast majority of students. We now turn to the post treatment survey and measures of who helped with college applications. The wording of the survey question is “Thinking of the people in your life, which of the following people helped you with college applications?” There are checkboxes for parent, sister or brother, friend, other relative, family friend, teacher, school counselor, mentor coach, or employer.

In the second row, we see that the main effect of the treatment is 11.8 percentage points and the main effect of having parent help with applications is 13.3 percentage points. Importantly, the mentoring treatment effect is nonexistent (point estimate of insignificant -1.3 percent) for students who have parents who help with applications.

This finding resonates with us because of the project design and our conversations with high school students in the field. The mentoring project was designed in part to provide support to students who had lower levels of support from home

or other sources. The effect of the treatment interacted with help from a teacher is similar in magnitude, but not statistically significant. The point estimates suggest that the treatment effect on college-going is large, but only for students who are not relying on help from a teacher.

One problem with the above interpretation is that the mentoring treatment could impact directly whether or not a student receives application help from a parent. In practice this does not appear to be a major concern, as being assigned to the mentoring treatment has an insignificant and negative effect of 0.03 on whether parents help with applications. Another approach to dealing with the endogeneity of parental help is to back up a step and look at the questions of whether the student talked to parents about future plans or talked to parents about college choices. The mentoring treatment is not designed to reduce the amount that students talk to parents about college, if anything the treatment might increase those discussions.

Online Appendix Table 8 shows these interaction results. Students who talk to their parents or teachers about future plans all have a meaningful (but statistically insignificant) reduction in the estimated treatment effect.

The results in the first row of online Appendix Table 8 are interesting and consistent with our story. We interact the treatment with whether parents (either mother or father or both) “expect me to attend college.” The treatment effect is smaller and loses statistical significance for students who report that their parents expect them to attend college. Our interpretation is that the treatment is not useful in cases where parents are already pushing the student to attend college and are involved in the application process.

In contrast, receiving help from a guidance counselor does not reduce the size of the estimated treatment (Table 5 row 4). This finding also has a natural interpretation. The mentoring treatment is offered through guidance departments. There is a strong positive connection between complying with assignment to treatment and using guidance counselors as a source of advice (coefficients not reported here).

In results not reported, we also find that the treatment is less effective among students with high earnings forecasts for their high school-only wage. Men forecast “high school-only” wages that are 52 percent greater than the same forecast for women. This higher forecast is supported by reported actual wages by the students post-graduation. On average, the men in the sample report wages that are 19 percent higher than the reported wages for women. This finding can explain in part why the treatment is less effective for men. High school educated men are receiving signals from the labor market that they will have strong earnings even without a college degree.

We explore this hypothesis further in online Appendix Table 9. We use the American Community Survey to estimate returns to college for men and women in New Hampshire at ages 22–30. We regress log earnings on dummies for education levels. Less than high school is the omitted category. In the ACS data in New Hampshire, young high school-educated men have the same earnings as men with one to three years of college.<sup>27</sup> This fact is not true for women. (In results not

<sup>27</sup>For young men in the United States, the returns to “some college” are 6 percent per year relative to high school grad only. This is not a large number but still much greater than the 0 percent return to “some college” for young men in New Hampshire.

reported here, we find that men age 31 and above do have strong returns to “some college” and “college”). This finding is consistent with the idea that high school-educated young men in New Hampshire are forecasting high wages without a college degree, and this may explain why they are less affected by the mentoring treatment.

As another check of the differential labor market opportunities hypothesis, we asked students who were not enrolled in college, “why not?” We offered one question with an open-ended response and a second question with a series of checkboxes. The possible checkboxes included “I have a job I prefer to college” and “I have a long run career plan I prefer to college” and “I don’t think college would advance my career plans and earnings.” Men were 50 percent more likely than women to respond that they “have a job they prefer to college” and twice as likely to report that “college won’t advance my career plans and earnings.”<sup>28</sup> Again, we see this as evidence that high school-educated men are differentially drawn into the labor market.

In Carrell and Sacerdote (2013), we explored whether students have accurate information about the cost of college, whether this information differs by gender, and whether such information interacts with the treatment effect. We asked students to estimate total in-state tuition and fees for a typical New Hampshire public four-year college or university, and to estimate total in-state tuition and fees for a typical New Hampshire public community college. Consistent with prior work (Avery and Kane 2004), students tend to overestimate the costs of attendance. The median estimate for community college tuition and fees is \$10,000, while the actual number is \$7,000. And the median estimate for a four-year public institution is \$25,000, while the actual number is \$12,500 for Plymouth State and \$16,422 for the flagship public University of New Hampshire at Durham. Despite the upward bias in student estimates,  $\log(\text{estimated tuition})$  does not appear to interact with the effectiveness of the mentoring treatment nor is it correlated with gender. We calculate that doubling a student’s estimate of community college tuition would reduce the impact of the mentoring treatment on college enrollment by only 2 percentage points.

#### *D. Interactions with Personality Measures*

A major focus of the survey was to ask whether the mentoring treatment interacts with certain behavioral characteristics or personality traits. Number one on our list was whether the treatment is particularly helpful to students who are disorganized, forgetful, or have trouble meeting deadlines.

We used a subset of our personality questions to create three indices: (i) Does Not Meet Deadlines/Disorganized, (ii) Adventurousomeness, and (iii) Self-Esteem. As an alternative, we tried to proxy for four of psychology’s Big Five personality indices, namely, Openness to Experience, Conscientiousness, Extraversion, and Neuroticism. We created each of the seven indices by simply averaging binary variables representing each underlying question.<sup>29</sup>

<sup>28</sup>Men and women were equally likely to report that they “know they won’t be successful in college” or that they “haven’t given much thought to college.”

<sup>29</sup>Wording of the questions is shown in the notes to Table 7 and the online Appendix with the survey. Since the responses are categorical, we coded “Agree” and “Strongly Agree” as a 1 and “Disagree” and “Strongly Disagree” as a 0. Our survey didn’t ask questions that would proxy for the other big five measure, namely agreeableness.

Table 6 asks whether the treatment interacts with personality traits. Contrary to our initial hypothesis, we do not find evidence that the treatment is particularly effective or ineffective for students who are disorganized or struggle to meet deadlines. The point estimate on the meet deadlines index interacted with treatment status is 0.083 with a large standard error. This alone is not particularly informative, but we find the same insignificant point estimates with seven individual measures of organization. Results are in online Appendix Table 10. For example, students who “forget deadlines,” “skip homework,” or who are not organized do not have significantly different treatment effects on college enrollment than other students.

We had also hypothesized that the treatment might provide a boost of encouragement to students with low self-esteem. We find, at most, only weak evidence that this is the case. Row 5 of Table 6 interacts the treatment dummy with the self-esteem index. And online Appendix Table 10 interacts treatment status with our specific measures of self-esteem including “I am a person of worth equal to others” and “I can change important things.” In nearly all cases there is neither a large nor statistically significant interaction between treatment status and self esteem. One of the five self-esteem measures interacts statistically significantly with the treatment. Students who do not believe they are good at solving problems have a large treatment effect, while students who do solve problems have no mentoring treatment effect.

The one area in which personality may interact with the effectiveness of the treatment is Openness to Experience. In the top two rows of Table 6, we show that the treatment is ineffective for students who like to meet new people or who enjoy amusement rides. One plausible interpretation of these findings is that outgoing or more adventurous students may be able to find their own sources of help on college applications. Or similarly these students are willing to experiment on their own with the college choice and application process and figure out that the process is manageable after all. We don’t want to push this finding too heavily given that in online Appendix Table 10 there are other measures of adventurousness that do not interact with the mentoring treatment effect in a statistically significant way.

#### *E. How Does the Program Interact with Demographic Sources of Advantage?*

One important question is whether the program interacts with other sources of advantage. In Carrell and Sacerdote (2013) and online Appendix Table 11, we find little evidence that the program works better (or worse) for students with a high school-educated mother. The point estimates for the women suggests that women without a college-educated mother have modestly smaller treatment effects than women with a college-educated mother. This result is distinct from our results on parents helping with applications or parents’ expectations about college, where we find statistically significant and robust results. We suspect that mother’s college status or a student’s “first-generation” status is not by itself a good screen for discerning whether a student needs help navigating the college application process. Similarly, we do not find that the program is more or less effective for nonwhite or free lunch students.

A final way to ask whether the program is a complement or substitute to socioeconomic advantage is to examine how the treatment effects vary by high school. Our

TABLE 6—INTERACTION OF MENTORING TREATMENT WITH PERSONALITY MEASURES (including the Big 5)  
(dependent variable is enrollment in any college)

Survey measure	Coefficients on treatment × survey measure (1)	Coefficient on treatment indicator (2)	Coefficient on survey measure (3)	Observations (4)	Mean (5)	Survey indicator regressed on male dummy (6)
<i>Individual measures</i>						
Likes to meet new people	-0.305 (0.086)	0.280 (0.085)	0.150 (0.055)	530	0.723	-0.096 (0.039)
Enjoys amusement rides	-0.287 (0.136)	0.259 (0.103)	0.097 (0.087)	530	0.696	0.031 (0.040)
<i>Composite measures</i>						
Meets deadlines/organized	0.083 (0.189)	0.030 (0.082)	0.096 (0.133)	530	0.343	0.011 (0.022)
Adventuresome	-0.275 (0.179)	0.239 (0.143)	0.144 (0.146)	530	0.657	0.017 (0.021)
Self-esteem	-0.097 (0.128)	0.136 (0.096)	0.143 (0.092)	552	0.672	0.007 (0.028)
<i>4 of big 5 measures</i>						
Openness to experience	-0.136 (0.171)	0.138 (0.085)	0.008 (0.140)	646	0.408	0.032 (0.020)
Conscientiousness	0.083 (0.189)	0.030 (0.082)	0.096 (0.133)	530	0.343	0.011 (0.022)
Extraversion	-0.111 (0.148)	0.143 (0.097)	0.104 (0.077)	646	0.560	-0.023 (0.027)
Neuroticism	0.077 (0.099)	0.047 (0.054)	-0.103 (0.077)	552	0.305	-0.018 (0.030)

Notes: Standard errors are in parentheses. In each row, columns 1–5 are from a single regression of “Any College” on the treatment dummy, the survey measure, and the interaction of the two. Regressions also include controls for male, free lunch status, and high school × cohort dummies. Column 6 is from an OLS regression of the survey measure on a dummy for male. Numbers are rounded to three decimal places.

Survey questions are as follows:

*Self Esteem*: “How much do you agree or disagree with the following statements: I feel that I’m a person of worth, equal to others; I feel useless at times; I feel that I have a number of good qualities; I often feel that I am a failure; I am able to do things as well as most people; I feel I do not have much to be proud of; I take a positive attitude toward myself; On the whole, I am satisfied with myself.”

“How much do you agree or disagree with the following statements: I have little control over the things that happen to me; There is really no way I can solve some of the problems I have; What happens to me in the future mostly depends on me; There is little I can do to change many of the important things in my life; I often feel helpless in dealing with the problems of life; I can do just about anything I really set my mind to do; Sometimes I feel that I’m being pushed around in life; Becoming a success is a matter of hard work, luck has little or nothing to do with it.”

*Organization*: “How true are the following statements about you: I have a good system for remembering deadlines and important dates; I would like to travel to other countries; I miss out on things I want to do because I forget to sign up; I enjoy spending time in places I’m used to, like at home; I’ll try anything once; I often miss important deadlines if no one reminds me about them; I like scary movies; I like to meet people who are different from me; Sometimes when my life is really busy, I don’t get all of my homework done; I often lose important papers; Deadlines always seem to come faster than I expect them to.

*Adventuresome*: “I sometimes do ‘crazy’ things just for fun; I enjoy going places I’ve never been before; I need a better way to remind myself about important deadlines and due dates; In an amusement park, I prefer fast rides; When I move out of my parents’ house, I would still like to live close by (reversed).”

“How true are the following statements: I make sure I get my work done before I have fun; You can learn new things, but you can’t really change your basic intelligence; I use my time wisely; Intelligence is something about you that you can’t change very much; I often spend time playing around with my phone or computer, even when I know I should be doing homework; I wait until the last minute to do things; I often buy things I wasn’t planning to buy; I am good at saving up money when I want to buy something special; I put off starting things I don’t like to do; It is important to me to get better grades than my classmates; I often spend money I was planning to save for something else; I feel angry when I get worse grades than other students; I have a hard time NOT answering the phone or texts when I’m supposed to be doing homework.”

high schools are located in fairly different communities and the mentoring treatment may work better or worse in high schools with more resources. In online Appendix Table 12, we report effects separately by high school. We limit the analysis sample to women since again it is the women who show reliably positive treatment effects. Reassuringly, even in these small samples, the estimated effects are positive and of plausible magnitudes for most of the high schools.

One high school in which we do not have much of an effect is Portsmouth High School, which is located in an affluent community with a highly educated population. Portsmouth has more resources per pupil than the other high schools and specific college counselors whose primary jobs already incorporate the mentoring and hours of individual attention that is offered by our program. In contrast, Pinkerton Academy has among the largest estimated treatment effects. Pinkerton is a large high school in an economically diverse community and has the fewest guidance counselors per student among our high schools.

#### *F. Does the Cash Bonus Alone Generate the Treatment Effect? Does It Affect Participation?*

Our experiences with the high school students suggested that the \$100 cash bonus itself was fun and created some buzz, but it was not the primary motivation for treatment students to complete applications. We began to test this intuition formally with the 2012 cohort. In online Appendix Table 13, we find that offering the cash bonus only to students (with no mentoring) has an insignificant 2 percentage point effect on college-going. However, our estimates are very imprecise.

Based on our qualitative and quantitative feedback about the cash bonus, we tried removing the cash bonus from the mentoring treatment in 2014. Interestingly, we saw very significant reductions in take up of the mentoring treatment in 2014. Online Appendix Table 14 shows this formally. We regress a dummy for mentoring take-up on high school dummies, individual demographics, and a dummy variable for whether the student was in the 2014 cohort. The coefficient on the 2014 dummy is  $-33$  percentage points for the combined sample and  $-39$  percentage points for the women.

We cannot be certain that the lack of a cash bonus was the only reason for reduced take-up in 2014. However, we suspect that this was an important factor because (i) dropping the cash bonus was the only program change made, and (ii) the cash bonus was a significant part of our advertising the program to selected students in the letters and emails that the students received.

#### *G. Cost-Benefit Calculations*

The average student in our mentoring treatment required two application fees at a total cost of \$80. Plus, we paid a cash bonus of \$100 and provided an average of 8–10 hours of mentoring at \$12 per hour. The marginal cost of treating an additional student is about \$300.

The treatment on the treated estimates shows that the average woman gains an additional 0.3 years of college for at least each of the first two to three years of

college. This suggests that on average treated women receive at least 0.9 to 1.0 additional years of college. Using some of the more widely cited surveys of estimates of the returns to college (Card 1999 or Gunderson and Oreopoulos 2010), this increase in education would raise annual earnings by 10 percent and this benefit would be enjoyed every year of a woman's working career. Zimmerman (2014) uses a regression discontinuity design to find that students at the margin of acceptance to a public four-year institution versus a community college experience earning returns of 8.7 percent for each additional year of college completed.

Conservatively, we estimate the earnings benefits at perhaps \$5,000 per year and a net present value of \$100,000.<sup>30</sup> In other words, if there is a positive return to college for the experimental women, the earnings benefits alone will absolutely swamp the modest costs of \$300. The same conclusion goes through even if we double the treatment costs to cover program overhead or the true value of a college student's time, or to assume that we treat both men and women.

A different approach is to follow Dynarski, Hyman, and Schanzenbach (2013) and calculate the cost per additional student induced into college. Our intervention again looks favorable in this comparison. For example, Dynarski, Hyman, and Schanzenbach (2013) calculate that the class size reductions in STAR cost \$12,000 per student and induce a 3 percentage point increase in college attendance, so this equals  $\$12,000/0.03$  or \$400,000 per additional student enrolled in college. Upward Bound spends roughly \$5,620 per student and about \$93,667 per additional college enrollee. Dynarski, Hyman, and Schanzenbach (2013) find that Head Start costs about \$133,000 per additional college enrollee. They also calculate that the Bettinger et al. 2012 H.R. Block FAFSA intervention costs \$1,100 per student induced into college.

If we target only women for our mentoring intervention, we spend roughly  $\$300/0.25$  per woman induced into college or \$1,200 per additional enrollee. If we target men and women, the cost per additional enrollee doubles to \$2,400. In other words, the mentoring intervention is vastly more cost effective at promoting college enrollment than class size reductions or Head Start. For women, the mentoring intervention is cost competitive with the H.R. Block intervention, which was among the more ingenious, creative, and cost effective interventions that social scientists have designed.

The Hoxby and Turner (2013) intervention is the least expensive to implement per student, costing only \$6 per student. Since they alter the college choice for 5 percent of the students, they spend \$120 per student with a closer college match without impacting (or intending to impact) college attendance.

## VII. Discussion and Conclusion

Our study is motivated by the desire to test hypotheses as to why qualified high school seniors fail to apply to and enroll in college. One of our initial hypotheses was that students' lack of organizational skills or procrastination prevents them

<sup>30</sup>This is an additional year of college for the average treated woman. We take average earnings of roughly \$50,000 from the following census table: <http://www.census.gov/compendia/statab/2012/tables/12s0232.pdf>.



from doing something important that they really want to do and could easily do, namely attend college.

We found little direct evidence to support our hypothesis. Our index of disorganization and our individual measures of disorganization, losing papers, and forgetting deadlines are uncorrelated with the treatment effect. Furthermore, when we advertised a \$100 cash bonus for getting the job done (i.e., completing applications), we had no measurable impact on college-going.

In a sense, we are relieved that the problem does not appear to be about simple deadline meeting skills. If it were, we would worry that we are pushing these students into college only to have the students immediately fail in college due to the same lack of basic skills and attentiveness.

A related hypothesis is that students are so terrified of the process and afraid of failure that the students never get started down the path of applying. We had hoped that the transcript only intervention would address this fear since we help students begin the process with a simple one-page form and in response they receive one or more letters of strong encouragement from admissions offices that have reviewed their transcript. This program was unsuccessful in part due to low take-up. But even within the students who took up the transcript-only treatment and received letters (and in some cases phone calls and e-mails), admissions offices received very few applications from the students they contacted.<sup>31</sup>

The missing information hypothesis is that students lack basic information about how to apply, the benefits of college, or the costs of attendance. Consistent with other authors (e.g., Avery and Kane 2004), we find that students tend to overestimate the costs of tuition and fees. However, the mentoring treatment doesn't have any effect on students' biased estimates of tuition and fees. And the bias is not correlated with the size of the treatment effect.

Interestingly, the treatment is associated with a 6 percentage point increase in the fraction of students who say they need a college degree to meet their career goals. This increase is roughly similar for men and women. Thus, the treatment could be raising awareness of the importance of college for earnings or career choice. Or the treatment could be helping students get into college, which then changes their response as to their career goal and whether a degree is necessary.

The mentoring treatment was designed in part around the hypotheses that some students lack sustained help from a parent, counselor, and/or teacher in navigating through each piece of the application process. We find several pieces of evidence to support the hypotheses that the treatment substitutes for skilled help from a parent. The treatment effects are concentrated among students who did not rely on a parent to complete applications. This is despite the fact that the treatment did not lower the fraction of students using parents for help.

We find a similar result when we examine the interaction between the treatment and SAT Questionnaire measures of needing help making educational plans. The treatment is highly effective for students who anticipated needing help.

<sup>31</sup> We know this from communications with several institutions including Southern New Hampshire University, University of New Hampshire, and White Mountains Community College.

A different, but potentially interesting hypothesis is that the treatment interacts with perceived noncollege opportunities in the labor market. It's plausible that many of the qualified students who fail to apply do so because of attractive short run or long run labor market opportunities. We find evidence that the treatment is less effective for students who forecast high wages for themselves with only a high school diploma. In particular, men have smaller and less robust average treatment effects from the mentoring treatment. And men forecast their "high school only wages" to be 50 percent higher than the comparable forecast for women. And when we interact mentoring treatment status with high school-only wage forecasts, we estimate that a 50 percent increase in the expected high school only wages lowers the treatment effect on college enrollment by 8 percentage points.

Importantly, the fact that our mentoring treatment is more effective with females than with males tends to increase, rather than decrease, the growing female-male gap in college-going and graduation. This finding is consistent with other interventions that have been shown to be more effective with women than men (see, for example: Angrist, Lang, and Oreopoulos 2009; Angrist and Lavy 2009; and Lindo, Sanders, and Oreopoulos 2010)

Overall we find that the mentoring treatment is largely acting as a substitute for the potentially scarce resource of parental help or skill. This in person help could be in part offsetting problems of procrastination, disorganization, or fear of failure. However, despite lots of looking, we cannot find much direct evidence that lack of organization or lack of self esteem play a direct role in explaining why mentoring works.

In contrast, lighter touch approaches including simply offering cash bonuses or letters of encouragement from college admissions offices (as in the transcript only treatment) are not effective. This again suggests to us that, in our population, the failure to apply and enroll is not based on a small behavioral cost, which can easily be overcome by low cost nudges. Our results contrast somewhat with Castleman and Page who find that text messages both reduce summer melt and promote FAFSA filing. Because of the differing populations any number of explanations may be at work. However, one sensible interpretation is that small touches (text messages) can help keep students on track once they are in college. But the process of getting non-applying students to apply is more demanding and cannot necessarily be done with low touch, more easily scaled interventions.

Most models of human capital formation might suggest that students at the margin of not attending college would be the most likely to drop out after one or two years. However, we find that our "marginal" students persist in college to the same degree that as other New Hampshire students with similar test scores.

We conclude that many students at the margin of failing to apply and attend need direct in-person help and hand holding in order to navigate the United States' convoluted process for applying to colleges and financial aid. A lot of students receive this help from a parent or college counselor, but a great deal of progress can be made in helping those students who lack such support. As such, hope that our work will provide a foundation for other researchers who wish to investigate cost effective ways to boost college-going in the United States.

In the long run, we hope to gather average earnings measures for both the treatment and control groups and test whether returns to college differ for men and

women in this sample. The program serves as an instrument for college attendance, which will provide a useful measure of the returns to college for a particular group of students.

## REFERENCES

- Abdulkadiroğlu, Atila, Joshua D. Angrist, Susan M. Dynarski, Thomas J. Kane, and Parag A. Pathak.** 2011. "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots." *Quarterly Journal of Economics* 126 (2): 699–748.
- Angrist, Joshua, Daniel Lang, and Philip Oreopoulos.** 2009. "Incentives and Services for College Achievement: Evidence from a Randomized Trial." *American Economic Journal: Applied Economics* 1 (1): 136–63.
- Angrist, Joshua, and Victor Lavy.** 2009. "The Effects of High Stakes High School Achievement Awards: Evidence from a Randomized Trial." *American Economic Review* 99 (4): 1384–1414.
- Avery, Christopher, and Thomas J. Kane.** 2004. "Student Perceptions of College Opportunities: The Boston COACH Program." In *College Choices: The Economics of Where to Go, When to Go, and How to Pay For It*, edited by Caroline M. Hoxby, 355–94. Chicago: University of Chicago Press.
- Bettinger, Eric P., Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu.** 2012. "The role of Application Assistance and Information in College Decisions: Results from the H&R Block FAFSA Experiment." *Quarterly Journal of Economics* 127 (3): 1205–42.
- Bulman, George.** 2015. "The Effect of Access to College Assessments on Enrollment and Attainment." *American Economic Journal: Applied Economics* 7 (4): 1–36.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller.** 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90 (3): 414–27.
- Card, David.** 1999. "The Causal Effect of Education on Earnings." In *Handbook of Labor Economics*, Vol. 3A, edited by Orley C. Ashenfelter and David Card, 1801–63. Amsterdam: North-Holland.
- Carrell, Scott E., and Bruce Sacerdote.** 2013. "Why Do College-Going Interventions Work?" National Bureau of Economic Research (NBER) Working Paper 19031.
- Carrell, Scott, and Bruce Sacerdote.** 2017. "Why Do College-Going Interventions Work?: Dataset" *American Economic Journal: Applied Economics*. <https://doi.org/10.1257/app.20150530>.
- Castleman, Benjamin, and Joshua Goodman.** 2015. "Intensive College Counseling and the College Enrollment Choices of Low Income Students." <https://scholar.harvard.edu/files/joshuagoodman/files/counseling.pdf>.
- Castleman, Benjamin L., and Lindsay C. Page.** 2014a. "A Trickle or a Torrent? Understanding the Extent of Summer 'Melt' Among College-Intending High School Graduates." *Social Science Quarterly* 95 (1): 202–20.
- Castleman, Benjamin L., and Lindsay C. Page.** 2014b. "Freshman Year Financial Aid Nudges: An Experiment to Increase FAFSA Renewal and College Persistence." EdPolicyWorks Working Paper 29.
- Castleman, Benjamin L., and Lindsay C. Page.** 2014c. "Summer Nudging: Can Personalized Text Messages and Peer Mentor Outreach Increase College Going Among Low-Income High School Graduates?" EdPolicyWorks Working Paper 9.
- Castleman, Benjamin L., Lindsay C. Page, and Korynn Schooley.** 2014. "The Forgotten Summer: Does the Offer of College Counseling After High School Mitigate Summer Melt Among College-Intending, Low-Income High School Graduates?" *Journal of Policy Analysis and Management* 33 (2): 320–44.
- Cornwell, Christopher, David B. Mustard, and Deepa J. Sridhar.** 2006. "The Enrollment Effects of Merit-Based Financial Aid: Evidence from Georgia's HOPE Program." *Journal of Labor Economics* 24 (4): 761–86.
- Cunha, Flavio, and James Heckman.** 2008. "Formulating, Identifying, and Estimating the Technology of Cognitive and Noncognitive Skill Formation." *Journal of Human Resources* 43 (4): 738–82.
- Dynarski, Susan.** 2000. "Hope for Whom? Financial Aid for the Middle Class and Its Impact on College Attendance." National Bureau of Economic Research (NBER) Working Paper 7756.
- Dynarski, Susan, Joshua Hyman, and Diane Whitmore Schanzenbach.** 2013. "Experimental Evidence on the Effect of Childhood Investments on Postsecondary Attainment and Degree Completion." *Journal of Policy Analysis and Management* 32 (4): 692–717.
- Fryer, Roland G., Jr.** 2010. "Financial Incentives and Student Achievement: Evidence from Randomized Trials." National Bureau of Economic Research (NBER) Working Paper 15898.
- Goldin, Claudia, and Lawrence F. Katz.** 2008. *The Race between Education and Technology*. Cambridge, MA: Belknap Press.

- Gunderson, M., and P. Oreopoulos.** 2010. "Returns to Education in Developed Countries." In *Economics of Education*, edited by Dominic J. Brewer and Patrick J. McEwan, 37–43. San Diego, CA: Academic Press.
- Heckman, James J., and Yona Rubinstein.** 2001. "The Importance of Noncognitive Skills: Lessons from the GED Testing Program." *American Economic Review* 91 (2): 145–49.
- Hoxby, Caroline M., and Sonali Murarka.** 2009. "Charter Schools in New York City: Who Enrolls and How They Affect Their Students' Achievement." National Bureau of Economic Research (NBER) Working Paper 14852.
- Hoxby, Caroline, and Sarah Turner.** 2013. "Expanding College Opportunities for High-Achieving, Low Income Students." Stanford Institute for Economic Policy Research (SIEPR) Working Paper 12–014.
- Kane, Thomas J.** 2003. "A Quasi-Experimental Estimate of the Impact of Financial Aid on College-Going." National Bureau of Economic Research (NBER) Working Paper 9703.
- Kautz, Tim, and Wladimir Zanoni.** 2014. "Measuring and Fostering Non-Cognitive Skills in Adolescence: Evidence from Chicago Public Schools and the One Goal Program." Unpublished.
- Levine, Arthur, and Jana Nidiffer.** 1996. *Beating the Odds: How the Poor Get to College*. The Jossey-Bass Higher and Adult Education Series. San Francisco: Jossey-Bass Publishers.
- Lindo, Jason M., Nicholas J. Sanders, and Philip Oreopoulos.** 2010. "Ability, Gender, and Performance Standards: Evidence from Academic Probation." *American Economic Journal: Applied Economics* 2 (2): 95–117.
- Myers, David, Rob Olsen, Neil Seftor, Julie Young, and Christina Tuttle.** 2004. "The Impacts of Regular Upward Bound: Results from the Third Follow-Up Data Collection. Mathematica Policy Research Paper 8464-600.
- Oreopoulos, Philip, Robert S. Brown, and Adam M. Lavecchia.** 2014. "Pathways to Education: An Integrated Approach to Helping At-Risk High School Students." National Bureau of Economic Research (NBER) Working Paper 20430.
- Oreopoulos, Philip, and Rueben Ford.** 2016. "Keeping College Options Open: A Field Experiment to Help All High School Seniors Through the College Application Process." National Bureau of Economic Research (NBER) Working Paper 22320.
- Organisation for Economic Co-operation and Development (OECD).** 2011. *Education at a Glance 2011: OECD Indicators*. Paris: OECD Publishing.
- Scott-Clayton, Judith.** 2011. "On Money and Motivation: A Quasi-Experimental Analysis of Financial Incentives for College Achievement." *Journal of Human Resources* 46 (3): 614–46.
- Swail, Watson Scott, and Laura W. Perna.** 2002. "Pre-College Outreach Programs: A National Perspective." In *Increasing Access to College: Extending Possibilities for All Students*, edited by William G. Tiernet and Linda Serra Hagedorn, 15–34. Albany, NY: State University of New York (SUNY) Press.
- Wimberly, George L., and Richard J. Noeth.** 2005. *College Readiness Begins in Middle School: ACT Policy Report*. American College Testing (ACT). Iowa City, IA: American College Testing (ACT).

**This article has been cited by:**

1. Tom Swiderski. 2024. Testing the Way Forward: The Impact of Statewide ACT or SAT Testing on Postsecondary Outcomes. *Educational Evaluation and Policy Analysis* 5. . [[Crossref](#)]
2. Brian Heseung Kim, Kelli A. Bird, Benjamin L. Castleman. 2024. Crossing the Finish Line but Losing the Race? Socioeconomic Inequalities in the Labor Market Trajectories of Community College Graduates. *Education Finance and Policy* 19:2, 283-307. [[Crossref](#)]
3. Xiaoyang Ye. 2024. Improving College Choice in Centralized Admissions: Experimental Evidence on the Importance of Precise Predictions. *Education Finance and Policy* 19:2, 308-340. [[Crossref](#)]
4. Darren Page. 2024. College applications and enrollment effects of the Common Application. *Contemporary Economic Policy* 42:2, 237-258. [[Crossref](#)]
5. Stacey L. Brockman, Jasmina Camo-Biogradlija, Alyssa Ratledge, Rebekah O'Donoghue, Micah Y. Baum, Brian Jacob. 2024. Forging a Path to College Persistence: An Experimental Evaluation of the Detroit Promise Path Program. *Educational Evaluation and Policy Analysis* 6. . [[Crossref](#)]
6. Vivian Yuen Ting Liu, Alexandra Haralampoudis, Isabel Polon. 2024. Combating Summer Melt: The Impact of Near-Peer Mentor Matriculation Program in New York City. *Research in Higher Education* 29. . [[Crossref](#)]
7. Michael L. Tidwell, Ellis S. Logan. 2024. Differences in support within the social science graduate admissions pipeline. *Studies in Graduate and Postdoctoral Education* 15:1, 65-81. [[Crossref](#)]
8. Sabina Tomkins, Joshua Grossman, Lindsay Page, Sharad Goel. 2023. Showing high-achieving college applicants past admissions outcomes increases undermatching. *Proceedings of the National Academy of Sciences* 120:45. . [[Crossref](#)]
9. Sarah R. Cohodes, Sean P. Corcoran, Jennifer L. Jennings, Carolyn Sattin-Bajaj. 2023. When Do Informational Interventions Work? Experimental Evidence From New York City High School Choice. *Educational Evaluation and Policy Analysis* 21. . [[Crossref](#)]
10. Christine Mulhern. 2023. Beyond Teachers: Estimating Individual School Counselors' Effects on Educational Attainment. *American Economic Review* 113:11, 2846-2893. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
11. Scott E. Carrell, Michal Kurlaender. 2023. My Professor Cares: Experimental Evidence on the Role of Faculty Engagement. *American Economic Journal: Economic Policy* 15:4, 113-141. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
12. Heike M. Freese, Lola Giner-Pérez, Aharon Oren, Markus Göker, David R. Arahal. 2023. The gender gap in names of prokaryotes honouring persons. *International Journal of Systematic and Evolutionary Microbiology* 73:11. . [[Crossref](#)]
13. Christa Deneault. 2023. College Enrollment and Mandatory FAFSA Applications: Evidence from Louisiana. *American Economic Journal: Economic Policy* 15:3, 465-494. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
14. Yao Luo, Yu Wang. 2023. Dynamic Decision-Making under Rolling Admissions: Evidence from US Law School Applications. *The Journal of Law and Economics* 66:3, 433-463. [[Crossref](#)]
15. Susan Dynarski, Aizat Nurshatayeva, Lindsay C. Page, Judith Scott-Clayton. Addressing nonfinancial barriers to college access and success: Evidence and policy implications 319-403. [[Crossref](#)]
16. Michael Lovenheim, Jonathan Smith. Returns to different postsecondary investments: Institution type, academic programs, and credentials 187-318. [[Crossref](#)]

17. Serena Canaan, Antoine Deeb, Pierre Mouganie. 2022. Adviser Value Added and Student Outcomes: Evidence from Randomly Assigned College Advisers. *American Economic Journal: Economic Policy* 14:4, 151-191. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
18. Sarah Komisarow. 2022. Comprehensive Support and Student Success: Can Out of School Time Make a Difference?. *Education Finance and Policy* 17:4, 579-607. [[Crossref](#)]
19. Sonia Ilie, Konstantina Maragkou, Ashton Brown, Eliza Kozman. 2022. No Budge for any Nudge: Information Provision and Higher Education Application Outcomes. *Education Sciences* 12:10, 701. [[Crossref](#)]
20. Ursula Mello. 2022. Centralized Admissions, Affirmative Action, and Access of Low-Income Students to Higher Education. *American Economic Journal: Economic Policy* 14:3, 166-197. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
21. Meredith Phillips, Sarah Reber. 2022. Does Virtual Advising Increase College Enrollment? Evidence from a Random-Assignment College Access Field Experiment. *American Economic Journal: Economic Policy* 14:3, 198-234. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
22. Andrés Barrios-Fernández. 2022. Neighbors' Effects on University Enrollment. *American Economic Journal: Applied Economics* 14:3, 30-60. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
23. Jan Berkes, Frauke Peter, C. Katharina Spiess, Felix Weinhardt. 2022. Information Provision and Postgraduate Studies. *Economica* 89:355, 627-646. [[Crossref](#)]
24. Cynthia Miller, Michael J. Weiss. 2022. Increasing Community College Graduation Rates: A Synthesis of Findings on the ASAP Model From Six Colleges Across Two States. *Educational Evaluation and Policy Analysis* 44:2, 210-233. [[Crossref](#)]
25. Cathleen D. Zick, Megan Tippetts, Bobbi Davis. 2022. Money Well Spent? The Cost-Effectiveness of a Texting Intervention Targeting College Persistence. *Journal of Research on Educational Effectiveness* 15:2, 394-412. [[Crossref](#)]
26. Michel Grosz, Annie Hines. 2022. In-State Tuition Policies and the College Decisions of Undocumented Students: Evidence from Colorado. *Education Finance and Policy* 17:2, 232-254. [[Crossref](#)]
27. Michael J. Weiss, Susan Scrivener, Austin Slaughter, Benjamin Cohen. 2021. An On-Ramp to Student Success: A Randomized Controlled Trial Evaluation of a Developmental Education Reform at the City University of New York. *Educational Evaluation and Policy Analysis* 43:4, 555-586. [[Crossref](#)]
28. Frauke Peter, C. Katharina Spiess, Vaishali Zambre. 2021. Informing students about college: Increasing enrollment using a behavioral intervention?. *Journal of Economic Behavior & Organization* 190, 524-549. [[Crossref](#)]
29. Susan Dynarski, CJ Libassi, Katherine Micheltmore, Stephanie Owen. 2021. Closing the Gap: The Effect of Reducing Complexity and Uncertainty in College Pricing on the Choices of Low-Income Students. *American Economic Review* 111:6, 1721-1756. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
30. Albert Cheng, Michael B. Henderson, Paul E. Peterson, Martin R. West. 2021. Cost-benefit information closes aspiration gaps – if parents think their child is ready for college. *Education Economics* 29:3, 233-251. [[Crossref](#)]
31. Kelli A. Bird, Benjamin L. Castleman, Jeffrey T. Denning, Joshua Goodman, Cait Lambertson, Kelly Ochs Rosinger. 2021. Nudging at scale: Experimental evidence from FAFSA completion campaigns. *Journal of Economic Behavior & Organization* 183, 105-128. [[Crossref](#)]

32. Barbara Cavalletti, Matteo Corsi, Luca Persico, Enrico di Bella. 2021. Public university orientation for high-school students. A quasi-experimental assessment of the efficiency gains from nudging better career choices. *Socio-Economic Planning Sciences* 73, 100945. [[Crossref](#)]
33. Denisa Gándara, Amy Li. 2020. Promise for Whom? “Free-College” Programs and Enrollments by Race and Gender Classifications at Public, 2-Year Colleges. *Educational Evaluation and Policy Analysis* 42:4, 603-627. [[Crossref](#)]
34. Alfredo Alvarado, Belén Conde, Rafael Novella, Andrea Repetto. 2020. NEETs in Latin America and the Caribbean: Skills, Aspirations, and Information. *Journal of International Development* 32:8, 1273-1307. [[Crossref](#)]
35. Drew M. Anderson. 2020. When financial aid is scarce: The challenge of allocating college aid where it is needed most. *Journal of Public Economics* 190, 104253. [[Crossref](#)]
36. Binzhen Wu, Xiaohan Zhong. 2020. Matching inequality and strategic behavior under the Boston mechanism: Evidence from China's college admissions. *Games and Economic Behavior* 123, 1-21. [[Crossref](#)]
37. William N. Evans, Melissa S. Kearney, Brendan Perry, James X. Sullivan. 2020. Increasing Community College Completion Rates Among Low-Income Students: Evidence from a Randomized Controlled Trial Evaluation of a Case-Management Intervention. *Journal of Policy Analysis and Management* 39:4, 930-965. [[Crossref](#)]
38. Natassia Rodriguez Ott, Sandra Staklis, Jonathan Boyette. 2020. The Effectiveness of Student Coaching in Community Colleges. *Community College Journal of Research and Practice* 44:8, 549-562. [[Crossref](#)]
39. Matthew S. Johnson. 2020. Regulation by Shaming: Deterrence Effects of Publicizing Violations of Workplace Safety and Health Laws. *American Economic Review* 110:6, 1866-1904. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
40. Bernd Fitzenberger, Annette Hillerich-Sigg, Maresa Sprietsma. 2020. Different counselors, many options: Career guidance and career plans in secondary schools. *German Economic Review* 21:1, 65-106. [[Crossref](#)]
41. Lucia Rizzica. 2020. Raising Aspirations and Higher Education: Evidence from the United Kingdom's Widening Participation Policy. *Journal of Labor Economics* 38:1, 183-214. [[Crossref](#)]
42. Joshua Hyman. 2020. Can Light-Touch College-Going Interventions Make a Difference? Evidence from a Statewide Experiment in Michigan. *Journal of Policy Analysis and Management* 39:1, 159-190. [[Crossref](#)]
43. Michael J. Weiss, Alyssa Ratledge, Colleen Sommo, Himani Gupta. 2019. Supporting Community College Students from Start to Degree Completion: Long-Term Evidence from a Randomized Trial of CUNY's ASAP. *American Economic Journal: Applied Economics* 11:3, 253-297. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
44. Albert Cheng, Paul E. Peterson. 2019. Experimental Estimates of Impacts of Cost-Earnings Information on Adult Aspirations for Children's Postsecondary Education. *The Journal of Higher Education* 90:3, 486-511. [[Crossref](#)]
45. Andrew Barr, Ben Castleman. 2019. Exploring Variation in College Counselor Effectiveness. *AEA Papers and Proceedings* 109, 227-231. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
46. Philip Oreopoulos, Reuben Ford. 2019. Keeping College Options Open: A Field Experiment to Help all High School Seniors Through the College Application Process. *Journal of Policy Analysis and Management* 38:2, 426-454. [[Crossref](#)]

47. Elise Swanson, Katherine Kopotic, Gema Zamarro, Jonathan Mills, Jay Greene, Gary Ritter. 2019. An Evaluation of the Educational Impact of College Campus Visits: A Randomized Experiment. *SSRN Electronic Journal* . [[Crossref](#)]
48. Oded Gurantz. 2018. A Little Can Go a Long Way: The Impact of Advertising Services on Program Take-Up. *Educational Evaluation and Policy Analysis* **40**:3, 382-398. [[Crossref](#)]
49. Jonathan Smith. 2018. The Sequential College Application Process. *Education Finance and Policy* **13**:4, 545-575. [[Crossref](#)]
50. Timothy N. Bond, George Bulman, Xiaoxiao Li, Jonathan Smith. 2018. Updating Human Capital Decisions: Evidence from SAT Score Shocks and College Applications. *Journal of Labor Economics* **36**:3, 807-839. [[Crossref](#)]
51. Brian J. Miller, William L. Skimmyhorn. 2018. I Want You! Expanding College Access through Targeted Recruiting Efforts. *Education Finance and Policy* **13**:3, 395-418. [[Crossref](#)]
52. Jesse M. Cunha, Trey Miller, Emily Weisburst. 2018. Information and College Decisions: Evidence From the Texas GO Center Project. *Educational Evaluation and Policy Analysis* **40**:1, 151-170. [[Crossref](#)]
53. Joshua Hyman. 2018. Nudges, College Enrollment, and College Persistence: Evidence from a Statewide Experiment in Michigan. *SSRN Electronic Journal* . [[Crossref](#)]
54. Andrés Barrios Fernández. 2018. Should I Stay or Should I Go? Neighbors' Effects on University Enrollment. *SSRN Electronic Journal* . [[Crossref](#)]
55. Luca Facchinello, Juanna Schrrter Joensen. 2017. Rethinking Education Choices: The Effect of Surveys. *SSRN Electronic Journal* . [[Crossref](#)]
56. Binzhen Wu, Xiaohan Zhong. 2016. Fairness of the Boston Matching Mechanism in China's College Admissions. *SSRN Electronic Journal* **101**. . [[Crossref](#)]