WHY DO COLLEGE GOING INTERVENTIONS WORK?

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Abstract

We present evidence from a recent field experiment 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. We test several theories as to why a short lived intervention has large impacts on lifetime human capital investments. 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 substitutes for the potentially expensive and often missing ingredient of skilled parental or teacher time and encouragement. The treatment interacts with students’ non-cognitive skills. Our positive effects are concentrated among students who do not rely on parental or teacher support for college applications and who are less extraverted. 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.

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Introduction

The United States ranks 12th in the world in the fraction of 25-65 year olds who have completed four years of college, though as recently as 1990 the US ranked first in this measure. The rate of four year college completion in the US among 25-34 year olds has leveled off at roughly 32-35 percent (OECD 2011). This leveling off has occurred in spite of evidence of strong returns to college education (Goldin and Katz 2009) and educational attainment in general (Gunderson and Oreopoulos 2010).

President 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. There are already a myriad of programs, partnerships and non-profits that seek to raise college going among students in the US. 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 6th, 7th and 8th graders, though not exclusively so.

More recently, economists and education researchers have begun to ask whether there is a payoff to working or communicating directly with high school seniors on college choice, college applications, and financial aid decisions. See for examples Hoxby and Turner (2013), Castleman, Page, and Schooley (2014), Bettinger, Long, Oreopoulos, and Sabonmatsu (2009), and Bettinger et al., (2012). Several non-profit groups including Let’s Get Ready, BottomLine (see Castleman and Goodman (2014)) 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 five percentage points more like to be enrolled in a “peer” institution (i.e. one that is a good matched based on selectivity). And Castleman, Page and

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1See 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.
Schooley (2012) find that 2-3 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 even late in a student's high school career and more importantly, why? 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 applications to the point where the student settles for a high school diploma rather than a preferred two or four year college degree.

We use three separate randomized interventions, along with survey and administrative data to ask which interventions matter and for whom. We test several broad and non-exclusive hypotheses as to why college going interventions matter. First, we ask whether students are naïve or behavioral with regard to application decisions. These behaviors could stem from irrational fear of the process, a lack of easily obtained information, or inattentiveness and forgetfulness. We investigate whether students are so disorganized that they forget to apply, miss deadlines, or fail to get the job completed through their own general incompetence. Or it may be that students vastly overestimate the costs of attendance or the financial or time costs of applying to college and fail to use Google or other resources to investigate these questions.

The above hypotheses imply that students’ lack of specific non-cognitive skills (as in Cunha and Heckman [2008] and Heckman and Rubenstein [2001]) are a serious barrier to their investing in college. College going interventions could substitute for these non-cognitive skills in the application process. However, the same missing non-cognitive skills might prevent students from persisting or succeeding in college, even if interventions can push such students into college. Conversely, college going interventions might actually be complimentary to non-cognitive skills and only be effective for students who already have particular skills such as meeting deadlines and being organized.

A second competing hypothesis is that the students at the margin want to attend college but are missing a key (and non-trivial) ingredient for success, such as sufficient SAT scores, parental
encouragement, cash for application fees, or skilled help navigating the application and financial aid process.

Third we also consider the neoclassical hypothesis that students at the margin are informed and rational, but fairly indifferent between attending and not attending college. Under this hypothesis marginal college going students have a career path, which they prefer to college. While an intervention might induce some of these students to apply and attend college, the students may be unlikely to persist.

To test these various hypotheses, we designed a mentoring program and an informational/transcript transmission program that works with students in the winter of their senior year. We then 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 (and the one of greatest interest), 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 FAFSA form is started and the sections other than the parental income section 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, emails, and phone calls from the admissions office of their local community college. We collect transcripts from these students and provide the transcripts to all of the public institutions in the state plus Southern New Hampshire University (a four year private institution). The admissions offices use the transcripts to identify potential candidates and send a strongly worded letter encouraging an application. In the remainder of the paper, we refer to students in this treatment arm as the “transcript only” group.
Existing Literature

There is a broad literature on the determinants of college going and much of the literature highlights the facts that a.) key college going decisions occur in middle school or even earlier and b) test score gaps (among socioeconomic groups) that open up by fourth grade tend to widen rather than close. See for example Wimberly and Noeth (2005), Levine and Nidiffer (1996), Nettles and Perna (1997) and Swail and Perna (2002). Much of the literature concludes that early interventions are needed both to address the aspirations of students (fact a) 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 b).

This literature has in part motivated the design of the U.S. Department of Education’s TRIO programs, which include Upward Bound and Talent Search. These programs catch students relatively early, i.e. 8th of 9th grade and provide a comprehensive suite of services. There are several dozens of papers that analyze the effects of TRIO programs, but among the most credible and comprehensive is a Mathematica Study of Upward Bound programs, which featured a randomized control trial (Myers et al 2004). This study 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.²

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 (much of it developed in parallel with our project and also in the working paper stage) 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

² 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.
attend more selective schools when mailed information specifically tailored to that student. The information mailed to students includes a guide on application strategies, lists of where other students with similar SAT or ACT scores applied, and a set of application fee waivers. Assignment to the treatment group raises the likelihood that a student enrolls in a peer institution by about 5 percentage points, raises the median SAT score of that institution by 15 points, and raises the graduation rate of the chosen institution by 3 percentage points.

Bettinger, Long, Oreopulos, and Sabonmatsu (2012) find that having HR Block auto fill the FAFSA (Free Application for Federal Student Aid) form for families with high school seniors results in a 8 percentage point increase in college going. This is particularly impressive and surprising given that the intervention only helped with the FAFSA and did not assist students with college choice or filing of college applications.

Castleman and Page (2013) show that targeted text messages increase the fraction of college bound seniors who actually enroll in the fall. In follow on work (Castleman and Page (2014)), they show that reminding first year undergraduates to re-file the FAFSA increases persistence into the second year.3

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. Oreopulous, Brown and Lavecchia (2014) find that a comprehensive mentoring program in a Toronto housing project raises high school graduation and college going rates.4 And Castleman and Goodman (2014) find that the BottomLine counseling program shifts students towards a set of recommended (largely public) colleges and away from a set of private institutions with lower graduation rates. Castleman and Page (2015) 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.

Most directly related to our work, Berman, Bos, and Ortiz (2008) study Los Angeles high school students who were mentored (mostly remotely) in the college choice and application

3 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

4 In addition to the published literature there are ongoing mentoring and college application experiments being conducted by Oreopoulous in Toronto and Reber and Phillips in California. These experiments are designed to raise college going among low income students.
process by UCLA and USC students under the SOURCE program. Students receiving the treatment did not experience increased college enrollment but there were increases in the fraction of students attending four year colleges and University of California and Cal State institutions. Interestingly the effects of SOURCE are concentrated among women, which is similar to our result, and we can partially explain the differences in effects by gender. More recently Phillips and Reber (2015) find that an online, email and text based mentoring version of Source raises application rates for high school seniors without raising the overall college going rate.

More broadly, high profile financial aid programs such as California's CalGrant (Kane 2003), Georgia's HOPE Scholarship (Dynarski 2000, Cornwell Mustard and Sridhar 2003 ), and West Virginia's PROMISE scholarship (Scott-Clayton 2011) also have significant impacts on the fraction of high school seniors who attend college.

Our current work is distinct from the existing working papers and published papers in a number of important respects. First, the target population and the intended outcomes from the interventions are quite different from most of the above studies. While Hoxby and Turner (2013) is among the most comprehensive of the studies, they are interested in changing the college choice set of top performing high school seniors. We are focused on expanding college access for the group of students who are at the margin of not applying anywhere. When we tried an intervention involving mailing a letter of information and encouragement to the students in our sample, we did not have any measurable impact on outcomes. We found similar results for non-college bound seniors in Delaware in ongoing work with Castleman and Page. The Castleman and Page (2013, 2014, 2015) interventions work with students who are already college bound, whereas we are trying to expand the set of students who are college bound. 5

Second, the main intervention that we test (in person mentoring) is more intensive and more involved than some of the other interventions discussed above. This intensity may be appropriate and necessary given that we are trying to solve a different problem than the Castleman and Page or Hoxby and Turner papers. 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.

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5 These and several other experiments were all fielded either simultaneously to, or slightly after, ours and our original working papers have been available in a similar time frame.
Our results on the use of financial 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.

Target Audience and the Sample

The program is targeted towards 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 twenty different New Hampshire high schools. There are roughly 60 high schools in the state and we called principals and guidance counselors at 35 of the largest schools. We worked with 20 of those schools who were most interested in the intervention and who were willing to allow a randomized evaluation thereof. Of the fifteen schools not interested, the most common reasons given for a lack of interest was insufficient time in the school day to facilitate mentoring sessions or a belief that students were already receiving sufficient help with applications.

During December or January of each year, guidance counselors in the 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. Most nominated students have not submitted requests for transcripts and recommendations to the counselor, which is of course a strong indicator for progress in the application process.

In the larger high schools, roughly 60 students of a graduating class of 300 seniors are nominated as fitting our suggested guidelines of being on the margin of applying and having made little or no progress in the application process. 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 then send the list of treatment and control students back to the high school. In almost all cases this correspondence takes place between the researchers and the head of guidance at each school.

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 rather than a weakness of the study since we are targeting more
precisely the students who are marginal (with regard to college going) late in the game. Our intent was to avoid providing expensive services to students who were already highly likely to apply to and attend college. 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.\(^6\)

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.

A closely related objection is that it may be hard for high schools outside our experimental set to identify the group of students who have not made progress on applications as of mid-December. Admittedly this set of students may be less quickly identified than, for example, the set of students who have not taken the SAT or ACT. However, in most high schools, guidance staffs know which students have not filed applications because transcripts and often recommendation letters are routed through the guidance department. It also possible that the schools could use SAT taking or SAT score sending as a coarse proxy for filing applications. In either event, our objective is not to design the perfect universally implementable intervention right out of the box, but rather to understand more deeply which students fail to apply to college and why.\(^7\)

A different set of questions arises regarding whether our students and schools look radically different from other large high schools in New Hampshire or the United States in general. Appendix Table 1 (available online and in Carrell and Sacerdote 2015) compares students in our 20 high schools to the other 59 high schools in the state that did not participate using Common Core data. For the most part, the participating high schools resemble the nonparticipating high schools a great deal in terms of demographics. For example 5 percent of experimental high schools are located in a large suburb (Census Bureau/Common Core Definition) versus 5.1 percent of all other high schools. In the experimental high schools, 12.2 percent of students are eligible for free and reduced lunch versus 14.0 percent in all other high schools.

\(^6\) See Katz, Kling and Liebman [2001] for definition and estimation of the control complier mean.

\(^7\) Another potential objection to our study is that the intervention cannot be scaled up since we rely on a high touch model with high achieving college students working as the mentors. However, we note that the program is already being scaled up by Let’s Get Ready as part of their College Choice curriculum. Let’s Get Ready uses undergraduate "coaches" from many different semi-selective undergraduate institutions throughout New England and the Mid Atlantic.
Columns (3) and (5) of Appendix Table 1 compare demographics for students in our experimental high schools to students in the US in general. Relative to the US, students in New Hampshire are more likely to be white and less likely to be eligible for free and reduced lunch.

We now turn to discussing the details of our sample. 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 and 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 that 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, but we were able to treat more cohorts and employ more interventions than expected when we initially designed the program.

We randomize students to treatment arms within school. In randomizing, we do not employ any stratification by gender, test scores, race, free lunch etc. In fact, gender is the only covariate available to us at the time of randomization. Each randomization is run exactly once (using Microsoft Excel's random number generator) and then used.

Mentoring treatment, cash bonus only, and transcript only/ informational students are notified by multiple methods (in person, over email, 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.

Students in the mentoring and transcript only treatments sign a waiver/ consent form agreeing to participate in the process. In the case of students who are under 18 years of age, their parent or guardian also signs the waiver. The presence of the consent form may be a barrier to participation (take-up) though we made the form as simple and clear as possible and we had no feedback suggesting that students were refusing to participate because of the form.
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 Datawarehouse items are available for all students in the treatment and control groups.\footnote{The IRB determined that, consistent with standard practice, the pure control (business as usual) i.e. non-participating students did not need to sign a waiver in order for the State of NH to provide de-identified existing administrative data for analysis.}

Certainly we were intellectually curious about potential peer spillovers from treated students to other treated students and to control students. Since some mentored students attended the sessions with one or two friends who had also been randomly assigned to mentoring, we think there is some possibility for peer effects, particularly in take-up. However, we did not take the time to collect data on friendship networks as we are convinced that we would not have enough statistical power to measure these peer effects. If there are positive spillovers to the control students, this will attenuate our measured treatment effects. Since we conclude that hands-on mentoring is the most effective of our interventions, we are not persuaded that the valuable part of the treatment is easily transmitted from one high school student to another.

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 10\textsuperscript{th} grade math scores for the graduating class of 2010. Separate distributions are shown for college goers and non-college 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.

In Carrell and Sacerdote (2013), we examine how well test scores plus basic demographics can predict college enrollment for the class of 2010. We find that test scores predict about 13 percent of the variation and that this rises to 15 percent when we include gender, free lunch status, and race.

**The Interventions**

*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 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 twenty 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 2-3 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 track each high school student's tasks, progress and various login ids and passwords. Essays are often outlined during the mentoring session and then further progress is made on essays at home.

Sessions typically take place in the schools' library, career center, or computer lab in which there are a set of internet enabled (usually hard wired) computers available. Having all or most of the group working in a single area allows the students and mentors to collaborate and exchange information about online applications at various colleges. Guidance counselors usually attend our sessions and stand ready to answer specific questions about various New Hampshire public and private colleges.

The specific steps required to "complete" our program include completing college essays, completing and filing applications, 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 the student sign up for these and provide email 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 high school students do not need to provide the money and ask for reimbursement later.

We ensure that transcript requests are properly filled out and given to each student’s guidance department. In some schools we provide envelopes and stamps to enable paper sending of transcripts.
The mentors always provide their own cell phone and email contact information to the treatment students. Frequently there is email and phone contact between students and mentors to aid in the process.

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

Perhaps surprisingly, the choice of where to apply and how many applications to file is not the most involved or difficult part of the process. Mentors are given lists and websites for all of the major New Hampshire and Vermont public and private colleges. Most of the high school students already have definitive ideas as to where they wanted to apply and attend. Many of these ideas are based on discussions with guidance counselors, friends, and/or family. And at least 85% of students apply to one or more institutions located in New Hampshire. In cases where the high school student needs detailed advising on where to apply, mentors rely on guidance staff, college websites, The College Board website and prior experience.

Almost all of the mentored time is spent completing college applications (often via the Common App), discussing and outlining college essays for the student to complete at home, sending SAT scores, sending transcripts, requesting recommendation letters, and filing the FAFSA.

Most students finish the application process within 3-4 weeks. In many cases mentors provide additional remote help (between sessions) over email and the phone. In a few cases, mentors make individual trips between sessions in order to help a student. Mentors and high school students keep in contact so that the mentors can learn about the high school student's college acceptances and plans for the following year. Whenever possible, we re-visit the treatment students in May to discuss college options and further encourage the student to attend college in the fall.

Most mentoring sessions overlap with lunch and study periods. At some high schools the students miss lunch plus a non-academic class such as woodshop and occasionally (with explicit teacher permission) the student will miss an academic subject. We suspect that the number of

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9 The difficult part of the process is manipulating multiple websites to get all of the required pieces of the application in place.
visits/ missed classes is not enough to impact the high school student’s GPA or probability of graduation.

Each day that we are working with mentoring students in a particular high school, the guidance department notifies the student AND her teacher that the student should be excused from class to participate in the program. Some students decline to participate simply by not showing up for any sessions while a few actively decline by notifying their guidance counselor that college applications are already complete or that they have no interest in filing applications.

Treatment students are told up front that they will receive a $100 cash bonus for completing applications. This is paid in person in the form of five $20 bills. Students sign receipts for cash received. In the 2009, 2010, 2011, and 2012 cohorts, mentoring was always combined with the cash bonus and application fees. In the 2014 cohort we decided to omit the cash bonus aspect from the mentoring treatment, allowing us a test of how the bonus interacts with the other two facets of the program.

In the 2012 cohort we had a mentoring treatment group which received all aspects of the program (mentoring, fees, bonus) and a second group that was offered the cash bonus only. There was no pure control (no intervention) group in 2012.

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. Transcript only students are notified of their selection in the same way as mentoring students, i.e. through all of email, 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. 1) The student fills out an online survey, which asks her to denote which of the participating colleges and universities interest her. 2) The student signs a form, which releases her transcript to allow us to send to the participating colleges. 3.) 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 emails 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.\textsuperscript{10}

Based on transcript data, some fraction (roughly twenty five 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 state that there are additional financial aid funds available if the student should choose to apply. See Online Appendix 18 for example letters.

Again, we recognize the imperfection of having treatment arms coincide with cohorts rather than having all treatment arms running simultaneously within cohorts. We did not originally anticipate having six cohorts and the sample size for multiple treatments. Nor did we think it desirable/ feasible to offer cash bonuses to some mentees but not other mentees within a school and cohort. 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 the associated Appendix Table 15.)

\textit{How the interventions relate to our hypotheses about college going behavior}

In the introduction we outlined three hypotheses (behavioral biases/non-cognitive skills/ lack of information versus missing key ingredients) as to why qualified students might fail to apply to college. Because the interventions test simultaneously more than one hypothesis, we also rely upon subgroup analyses, which interact treatment status with family background measures, personality measures, and behavioral measures to obtain a more detailed picture of how each

\textsuperscript{10} Appendix Tables 3-18 are intended as an on-line appendix to the final (published) version of the paper.
treatment works. We use survey data to understand students’ level of knowledge of college costs, their perceived labor market opportunities and the resources (help) available to them. 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.

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 number of visits, name and gender of 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. As noted above, the survey also included a host of personality questions designed to elicit self-esteem, work ethic, and ability to meet deadlines. And we asked a battery of questions about sources of help and advice on careers and college going.

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.11

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

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11 For more information on Clearinghouse data see http://www.studentclearinghouse.org/colleges/studenttracker/.
dummy variables for any enrollment in a four year college, any enrollment in a two year college, and enrollments in and only in two year colleges. 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 2009-2013, 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 gathered administratively prior to the experiment. 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 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 email 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. Means for basic demographic variables and test scores for survey respondents and non-respondents are shown in Appendix Table 3.

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.

A copy of the survey is included as Appendix 19. We discuss specific survey items in depth in the results section. For the moment, we highlight a couple of the questions that we expected would 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). In question 11, the respondents choose from among a menu of reasons as to why they are not enrolled. Question 16 contains eight 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 six 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).”

One downside to having a post, but not pre-treatment survey is that the treatment itself might affect the responses. For some personality measures we think that this is unlikely. For example, when we interact treatment status with tendencies toward procrastination or disorganization, we prefer to think of procrastination and disorganization as long standing characteristics of the student rather than outcomes. But we are open to other possible interpretations as we discuss below.

Table 1 shows summary statistics for the treatment and control groups for the 2009-2014 cohorts. In those six cohorts we have data for a total of 2624 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. Roughly 20 percent of mentoring treatment students and 17 percent of control students are nonwhite. Twenty eight percent of control

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12 Several appendices are mentioned out of numerical order due to either length of the appendix (e.g. the multi page survey which is left to the end) or the low importance of the appendix table.

13 And we regress survey measures directly on the treatment dummy to test whether the treatment affected the mean response.
students and twenty nine percent of mentoring treatment students are free and reduced lunch eligible.

About 35 and 39 percent of control and mentoring treatment students (respectively) have a 10th grade reading score which 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 (2015) shows that the mentoring treatment versus control score distributions overlap nearly perfectly. Randomization was performed at the high school times cohort level.14 While pre-treatment means for test scores and "non-white" 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 pre-treatment variables and the high school*cohort fixed effects. Standard errors are corrected for clustering at this level. We show separate regressions for the men and women in the sample. The pre-treatment variables are not significantly correlated with treatment status for either gender. The p-values on the test for the joint significance of all pre-treatment variables are statistically insignificant for both men (0.29) and women (0.10).

**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* cohort fixed effects, and demographic characteristics. Specifically we run regressions of the following form:

\[
\text{Enroll}_i = \alpha + \beta_1 \text{mentoring treat}_i + \beta_2 \text{transcript only}_i + \beta_3 \text{cash bonus only}_i + \gamma \text{X}_i + \rho \text{Z}_i + \varepsilon_i
\]

Here the outcome is whether or not student i enrolls in college following graduation, i.e. after the intervention. The dummy variables mentoring treat, transcript only, cash bonus only, denote whether the student is assigned to one of three treatment groups while the omitted

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14 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 & Murarka (2009) and Abdulkadiroglu, et. al. (2012).
category is the no intervention control group. The vector 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 Z is a set of high school by cohort fixed effects. Standard errors are corrected for clustering at the high school*cohort level which is the level at which the experiment is run. We control for age by including a full set of birth year*cohort dummies. This yields slightly greater precision then 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:

\[
\text{Enroll}_i = \alpha + \beta_1 \text{mentoring treat}_i + \beta_4 \text{student characteristic}_i + \beta_5 \text{mentoring treatment}_i \times \text{student characteristic}_i + \beta_2 \text{transcript only}_i + \beta_3 \text{cash bonus only}_i + \gamma X_i + \rho Z_i + \varepsilon_i
\]

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.

Results

15 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 appendix and in the text.
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 points for the women. This is against a control group mean college going rate of 41.1 percent and a control complier mean of 43.9 percent. 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%. 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 .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 college going of greater than 3.2 percentage points. Given that the insignificant point estimate is negative, we cannot rule out negative effects from the transcript only treatment as large as -4.2 percentage points. While we don’t 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 Turner intervention.

One reason the transcript only treatment is not effective is that it has a 14 percentage point take up rate. This is despite the fact that students received multiple prompts via email, mailed letters and in person notification from their guidance counselors. Unfortunately when we

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16 See Katz, Kling, and Liebman (2001) for calculation of the estimated control complier mean.
17 See for example their Table 5 on Enrollment in a Peer Institution.
instrument for taking up the transcript only treatment with assignment to that group, we cannot get 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 one applied and zero enrolled. University of New Hampshire sent letters of encouragement to fifteen students, of whom zero filed college applications. White Mountains Community College emailed and called twenty transcript only students, of whom one enrolled. This consistent track record strikes us as 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, though we cannot rule out effects as large as 7 percentage points. 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. (The results splitting by gender and SAT status at the same time are not reported in the Tables.)
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.

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.18

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 10th 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).

Evidence on Persistence

18 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.
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 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 significant at the 5-percent level. This effect (for being persisting in any college) is nearly identical to our Table 3 effect for enrolling in college at all. The similarity between 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.\footnote{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.}

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 (5) 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. The bottom line is that, within the available data, 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.

In column (4) we look at the men in the sample and ask whether there are mentoring treatment effects on the likelihood that they are enrolled in a four year college in both years after graduation. We limit the sample to men who did not take the SAT and the outcome to four year
enrollment since that is one of the subsamples and the outcome for which we find treatment effects for the men. Unfortunately, we do not find a statistically significant effect for persistence for the men. We can’t reject that the men are persisting; the treatment effect on being enrolled in both years in a four year college could be as large as 10 percentage points. Overall this is another indication that any effects for the men are neither as large nor as robust as the effects for the women.

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 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. But 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 pre-treatment. 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. But the mentoring treatment effect is non-existent (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 .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.

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

The results in the first row of 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 much 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.

Table 5 shows no such negative interaction effect between the treatment and receiving help from a school counselor. 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). Thus we are not surprised that students who are more likely to rely on guidance counselors have slightly stronger (at least in the point estimates) treatment effects.

In Table 6 we ask whether the mentoring treatment interacts with student beliefs about wages, future occupations, and college tuition. We asked students to estimate their hourly wage at age 30 if they earned only a high school diploma. Men on average estimated wages of $26.55 per hour while women estimated wages of $17.42 per hour. In row 1 of Table 6 we regress college enrollment on log (estimated high school only wage), the mentoring treatment indicator, and the interaction of the two. Based on the interaction term in column 1, increases in the estimated wage with only a high school diploma significantly decrease the effect of the treatment.

We interpret this as saying 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% 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 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. This fact is not true for women. (In results not 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 NH 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% 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.” Again we see this as evidence that high school educated men are being differentially drawn into the labor market.

In Carrell and Sacerdote (2015) 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 to students to estimate total instate tuition and fees for a typical NH public four year college or university and to estimate total instate tuition and fees for a typical NH public community college. Consistent with prior work (Avery and Kane (2005)), 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(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.

*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 of our own design, namely 1) Not Meet Deadlines/Disorganized, 2) Adventurousomeness, and 3) Self-Esteem. As an alternative we tried to proxy for four of psychology’s Big Five personality indices, namely

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20 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.”
Openness to Experience, Conscientiousness, Extraversion and Neuroticism. We created each of the seven indices by simply averaging binary variables representing each underlying question.\textsuperscript{21}

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 .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 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 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 Appendix Table 10 there are other measures of adventurousness that do not interact with the mentoring treatment effect in a statistically significant way.

\textsuperscript{21} Wording of the questions is shown in the notes to Table 7 and the 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.
How Does the Program Interact with Demographic Sources of Advantage?

One interesting way to cut the data is to ask whether the program interacts with other sources of advantage enjoyed by a subset of the students. In Carrell Sacerdote (2015) 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 suggest 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 did not find that the program was more or less effective for nonwhite or free lunch students.

A final way to ask whether the program is a complement or substitute for advantages enjoyed by students is to examine how the treatment effects vary by high school. Our high schools are located in fairly different communities and the mentoring treatment may work better or worse in high schools with more resources. In 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 did not expect, nor did we have much of an effect is Portsmouth High School, which by any measure 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 which 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.

Why is The Transcript and Letters Intervention Ineffective?
When we conceived of the transcript only intervention we believed that it would be highly effective at encouraging marginal students to apply to college. Our reasoning was that a tailored letter of encouragement from one or more college admissions offices would be a strong positive signal and source of motivation to the students in our experimental sample.\textsuperscript{22} We hoped to design an intervention that was less time and resource intensive than the mentoring intervention, which requires a one to two ratio of college student mentors to high school seniors.

We originally wanted the colleges to send a conditional acceptance letter instead of a “likely” letter. This proved impractical because college admissions offices do not want to reward a small group of students for failing to do the work of actually filing all of the forms and completing essays. Nonetheless, we had hoped that receipt of a likely letter from a public institution would be an exciting and game changing motivator for the students. The letters also emphasized both the financial and non-financial returns to college.

As a practical matter we know that the intervention was ineffective partially because students ignored the repeated offers to participate by signing the release forms. Take-up of the program was 14 percent. But that still leaves a somewhat interesting question as to why do students ignore written offers of help in the application process and why do they not value the chance to get their transcript to a set of colleges of their choice? And as we noted our checks in the field suggested that even students who did receive letters of encouragement did not apply to those specific schools.

Hoxby and Turner (2013) provide a wealth of evidence that the Expanding College Opportunities (ECO) project increased the number and the selectivity of college applications filed by low-income high achieving students. We suspect that the main reason why our take-up rates and results differ from the ECO intervention are the different population of students. ECO is focused on students in the top 10% of the distribution of SAT and ACT takers. In our population, 57 percent of the students have not even taken the SAT and the mean math score among those who did is 440.

\textit{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

\textsuperscript{22} See Appendix 5 for examples of letters received by students.
complete applications. We began to test this intuition formally with the 2012 cohort. We left the treatment condition as is with all three components (bonus, mentoring, application fees). But we offered the $100 bonus to the "control" group. In essence, the 2012 cohort is a different experiment in which we are testing all three components of the mentoring program against a single component.23

Results are shown in Appendix Table 13. Column (1) shows that when we dummy out the cash bonus only sample, the baseline mentoring treatment effect remains the same and the effect of being a cash bonus only students on college enrollment is an insignificant 2 percentage points. The problem is that we do not have any precision and we cannot reject large effects of the cash bonus only arm. Column (2) repeats the results from Table 3 for cohorts 2009-2011 showing the mentoring program (versus no treatment) raised "any college" for women by 15.2 percentage points. Column (3) is the analogous regression for women in the 2012 cohort. The effect for mentoring against pure control (column 2) looks similar to the effects of mentoring against cash bonus only (column 3).

Based on our qualitative and quantitative feedback (and most importantly feedback from guidance counselors) 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, while there was a modest increase in take up of the transcript only program.

Appendix Table 14 shows this formally using take up of the mentoring treatment as the dependent variable. We limit the sample to students who were randomized into the mentoring treatment. 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. We know from Table 1 that average take up of the program is 47-50 percent and this is true for both men and women (not reported). In the 2010-2012 cohorts the mean take-up rate was 57 percentage points. In 2014 this take up rate fell to 20 percent for both the men and the women. In Appendix Table 14 the coefficient on the 2014 dummy is -33 percentage points for the combined sample and -39 percentage points for the women.

23 Our baseline results (Tables 3-5) are robust to omitting the 2012 Cohort or to splitting our regressions into the various sub experiments. See below for the relevant appendix table (Appendix Table 7) and discussion.
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 a) dropping the cash bonus was the only program change made, and b) the cash bonus was a significant part of our advertising the program to selected students in the letters and emails that the students received.

If the cash bonus is motivating students to take up the program, one might expect that some of those students who are motivated mainly by cash would see small effects for the program. Students showing up for reasons other than the cash may have larger treatment effects. Thus the treatment on the treated estimated may rise in 2014. This is indeed what we find (at least in the point estimates) when we break up the sample by cohort years.

Splitting the Sample into the Component Experiments

One concern with our analysis in Table 3 is that we are combining cohorts in which different pairings of interventions were tested. We are constraining those interventions to have the same effect sizes regardless of the comparison being made between two treatment arms. Table 3 combines experiments of a) mentoring versus pure control, b) transcript only versus pure control, and c) mentoring versus transcript. Table 3 requires the estimate from b) to equal the implied estimate from a) and c).

In Appendix Table 15 we relax this assumption and split the sample into its more natural sub-experiments. Since the identical mentoring experiment was run for cohorts 2009-2011, we show estimates of the effects of mentoring on four year college going for men and women, just women and just men. The effects on four year college going are 8 percentage points in the combined sample which is generated by a 14 percentage point effect for the women and a 4 percentage point effect for the men.

In columns (4) and (5) we consider the 2013 experiment and show results for the transcript only intervention for men and women versus pure control. The effects for men and women are both small in the point estimates, and mildly negative for the men. In the case of the men we have more precision and can reject positive effects of 1 percentage point or more.

In columns 7 and 8 we show effects from the 2014 experiment, which compares mentoring (without the cash bonus) to the transcript only intervention. In the point estimates we see a large (8.3 percentage point) effect from mentoring for the women. This effect is almost significant at
the 10 percent level (p value = 13 percent). This intention to treatment effect occurs despite a much lower level (23 percent) of mentoring take up. The implied IV (treatment on treated) mentoring effect for the women is large since a mere 23 percent in take up moves the college going rate for women assigned to the mentoring treatment group by 8 percentage points.

This finding is consistent with our view that the cash bonus greatly increases take up, but at least a part of that additional take up could be from students who are not terribly interested in enrolling in college after graduation and who see lower benefits from mentoring. One problem with this interpretation is that the point estimates for the men do not increase despite the altered composition of who takes up the program.

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 show that the average woman gains an additional .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 .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 Gunder 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 right 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 $5000 per year and a net present value of $100,000.24 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.

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24 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.
A different approach is to follow Dynarski Hyman and Schazenbach (2013) and calculate the cost per additional student induced into college. Our intervention again looks favorable in this comparison. For example, Dynarski et. al. (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/.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 et. al., find that Head Start costs about $133,000 per additional college enrollee. They also calculate that the Bettinger et al H.R. Block FAFSA intervention costs $1100 per student induced into college.

If we target only women for our mentoring intervention, we spend roughly $300/.25 per woman induced into college or $1200 per additional enrollee. In other words, the mentoring intervention is vastly more cost effective at promoting college enrollment than class size reductions or Head Start. The mentoring intervention is actually 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 H.R. Block intervention is a super low cost automated program so it’s intriguing that our high touch model can come close to replicating the cost per additional enrollee of that work.

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.

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

We found little direct evidence to support this 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 lower of take-up. But even within the students who took up the transcript only treatment and received letters (and in some cases phone calls and emails), admissions offices received very few applications from the students they contacted.25

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

25 We know this from communications with several institutions including Southern NH University, University of NH, and White Mountains Community College.
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.

A different, but potentially interesting hypothesis is that the treatment interacts with perceived non-college 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 significant evidence that the treatment is not 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 (using the coefficient in column 1 row 1 in Table 6).

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, less high 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.

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 for financial aid. A lot of students receive this help from a parent or
college counselor, but a great deal of progress can be made by helping those students who lack
such support. We hope that our work will provide a foundation for other researchers who wish to
investigate cost effective way to boost college going in the US.

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.
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Castleman, B. L., Page, L. C. & Schooley, K. The forgotten summer: Mitigating summer attrition among college-intending low-income high school graduates. Unpublished manuscript.


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Gunderson, M. & Oreopoulos, P. 2010, "Returns to Education in Developed Countries" Chapter in Economics of Education, University of Toronto, Toronto, ON, Canada pp. 37-43.


Hoxby, Caroline and Sarah Turner, 2013, "Expanding College Opportunities for High-Achieving, Low Income Students," SIEPR Discussion Paper No. 12-014


Marzano, R. J., D. Pickering, and J. E. Pollock. 2001. *Classroom instruction that works: Research-based strategies for increasing student achievement* ACSD.


### Table 1: Summary Statistics for Mentoring Treatment and Control Groups

Students are randomly assigned to treatment within high school. Data include 2009, 2010, 2011 cohorts. Regressions include high school*cohort dummies which is the level at which randomization occurred.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Control Group</th>
<th>Mentoring Treatment</th>
<th>Transcript Only Group</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Obs Mean Std. Dev</td>
<td>Obs Mean Std. Dev</td>
<td>Obs Mean Std. Dev</td>
</tr>
<tr>
<td>Accepted Treatment</td>
<td>902 0 0</td>
<td>871 0.454 0.498</td>
<td>851 0 0</td>
</tr>
<tr>
<td>10th Grade Math Score (Standardized)</td>
<td>798 -0.480 0.937</td>
<td>778 -0.286 0.943</td>
<td>750 -0.370 0.957</td>
</tr>
<tr>
<td>10th Grade Reading Score (Standardized)</td>
<td>799 -0.436 0.928</td>
<td>772 -0.278 0.966</td>
<td>751 -0.394 0.940</td>
</tr>
<tr>
<td>Math &gt; 50th Percentile in State</td>
<td>798 0.312 0.464</td>
<td>778 0.335 0.472</td>
<td>750 0.304 0.460</td>
</tr>
<tr>
<td>Reading &gt; 50th Percentile in State</td>
<td>799 0.350 0.477</td>
<td>772 0.398 0.490</td>
<td>751 0.381 0.486</td>
</tr>
<tr>
<td>Math &gt; 75th Percentile</td>
<td>798 0.164 0.371</td>
<td>778 0.185 0.389</td>
<td>750 0.157 0.364</td>
</tr>
<tr>
<td>Reading &gt; 75th Percentile</td>
<td>799 0.213 0.410</td>
<td>772 0.224 0.417</td>
<td>751 0.221 0.415</td>
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<tr>
<td>Free and Reduced Lunch Eligible</td>
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<td>871 0.286 0.455</td>
<td>851 0.283 0.451</td>
</tr>
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<td>Male</td>
<td>902 0.548 0.498</td>
<td>870 0.575 0.495</td>
<td>851 0.605 0.489</td>
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<tr>
<td>Graduation Year</td>
<td>902 2011.527 1.281</td>
<td>871 2011.658 1.641</td>
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<tr>
<td>Any College (Clearinghouse)</td>
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<td>871 0.592 0.492</td>
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<tr>
<td>Four Year College (Clearinghouse)</td>
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</tr>
<tr>
<td>Persist for First Two Years Post Grad</td>
<td>902 0.195 0.397</td>
<td>871 0.240 0.427</td>
<td>851</td>
</tr>
<tr>
<td>Persist in a Four Year College</td>
<td>902 0.094 0.292</td>
<td>871 0.115 0.319</td>
<td>851</td>
</tr>
<tr>
<td>Enrolled 3+ Semesters</td>
<td>902 0.237 0.426</td>
<td>871 0.292 0.455</td>
<td>851 0.021 0.144</td>
</tr>
<tr>
<td>No SAT Data</td>
<td>902 0.708 0.455</td>
<td>871 0.457 0.498</td>
<td>851 0.489 0.500</td>
</tr>
<tr>
<td>Accepted Transcript Only</td>
<td>902 0.000 0.000</td>
<td>871 0.000 0.000</td>
<td>851 0.141 0.348</td>
</tr>
</tbody>
</table>
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<tbody>
<tr>
<td></td>
<td>Obs</td>
<td>Mean</td>
<td>Std. Dev</td>
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<tr>
<td>Accepted Treatment</td>
<td>902</td>
<td>0</td>
<td>0</td>
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<tr>
<td>10th Grade Math Score (Standardized)</td>
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<td>-0.480</td>
<td>0.937</td>
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<td>0.928</td>
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<td>0.464</td>
</tr>
<tr>
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<td>799</td>
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<td>0.371</td>
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<td>799</td>
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<td>0.410</td>
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<td>Free and Reduced Lunch Eligible</td>
<td>902</td>
<td>0.548</td>
<td>0.498</td>
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<tr>
<td>Male</td>
<td>902</td>
<td>0.173</td>
<td>0.378</td>
</tr>
<tr>
<td>Graduation Year</td>
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<td>2011.527</td>
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<td>Any College (Clearinghouse)</td>
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<td>Four Year College (Clearinghouse)</td>
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<td>0.094</td>
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<td>Enrolled 3+ Semesters</td>
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<tr>
<td>No SAT Data</td>
<td>902</td>
<td>0.708</td>
<td>0.455</td>
</tr>
<tr>
<td>Accepted Transcript Only</td>
<td>902</td>
<td>0.000</td>
<td>0.000</td>
</tr>
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</table>
Table 2: Mentoring Treatment Status Regressed on Pre-Treatment Characteristics

Students are randomly assigned to treatment within high school. Data include 2009-2014 cohorts. Regressions include high school*cohort dummies which is the level at which randomization occurred. Standard errors are clustered at the high school*cohort level. Regressions also include birth year*cohort dummies.

<table>
<thead>
<tr>
<th></th>
<th>(1) Treatment Status Men</th>
<th>(2) Treatment Status Women</th>
</tr>
</thead>
<tbody>
<tr>
<td>Standardized 10th Grade Math Score</td>
<td>0.001</td>
<td>0.041</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>Standardized 10th Grade Reading Score</td>
<td>-0.025+</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Free Reduced Lunch Eligible</td>
<td>-0.043</td>
<td>0.073</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.046)</td>
</tr>
<tr>
<td>Student is Nonwhite</td>
<td>0.019</td>
<td>-0.038</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.057)</td>
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<tr>
<td>Observations</td>
<td>1216</td>
<td>866</td>
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<tr>
<td>R-squared</td>
<td>0.355</td>
<td>0.321</td>
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<tr>
<td>F Pre-Treat Variables</td>
<td>1.281</td>
<td>2.109</td>
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<tr>
<td>p-value</td>
<td>0.294</td>
<td>0.098</td>
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Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1
Table 3:
Baseline Mentoring Treatment And Transcript Only Treatment Effects on Enrollment in College

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*cohort dummies which is the level at which randomization occurred. Standard errors are clustered at the high school*cohort level. Regressions also include birth year*cohort dummies and controls for race, gender and free lunch.

<table>
<thead>
<tr>
<th></th>
<th>(1) Whole Sample</th>
<th>(2) Women</th>
<th>(3) Men</th>
<th>(4) Did Not Take SAT</th>
<th>(5) Took SAT</th>
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</thead>
<tbody>
<tr>
<td><strong>Effects on Enrollment Any College</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mentoring Treatment (OLS)</td>
<td>0.060**</td>
<td>0.146**</td>
<td>0.007</td>
<td>0.083**</td>
<td>0.035</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.042)</td>
<td>(0.025)</td>
<td>(0.026)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>Transcript Only (OLS)</td>
<td>-0.005</td>
<td>0.005</td>
<td>0.000</td>
<td>0.035</td>
<td>-0.049</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.034)</td>
<td>(0.021)</td>
<td>(0.034)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>Mentoring Treatment (IV)</td>
<td>0.133**</td>
<td>0.299**</td>
<td>0.017</td>
<td>0.160**</td>
<td>0.086</td>
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<tr>
<td></td>
<td>(0.041)</td>
<td>(0.087)</td>
<td>(0.061)</td>
<td>(0.047)</td>
<td>(0.085)</td>
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<tr>
<td><strong>Effects on Enrollment Four Year College</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mentoring Treatment (OLS)</td>
<td>0.057**</td>
<td>0.107**</td>
<td>0.020</td>
<td>0.103**</td>
<td>-0.005</td>
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<td></td>
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<td>(0.031)</td>
<td>(0.028)</td>
<td>(0.026)</td>
<td>(0.033)</td>
</tr>
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<td>(0.030)</td>
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<td>-0.018</td>
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<td></td>
<td>(0.037)</td>
<td>(0.062)</td>
<td>(0.068)</td>
<td>(0.048)</td>
<td>(0.083)</td>
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<td><strong>First Stage for IV</strong></td>
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<tr>
<td>Mentoring Treatment</td>
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<td>(0.044)</td>
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<td>(0.033)</td>
<td>(0.070)</td>
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<td>1,114</td>
<td>1,509</td>
<td>1,453</td>
<td>1,170</td>
</tr>
</tbody>
</table>
Table 2:  
Mentoring Treatment Status Regressed on Pre-Treatment Characteristics

Students are randomly assigned to treatment within high school. Data include 2009-2014 cohorts. Regressions include high school*cohort dummies which is the level at which randomization occurred. Standard errors are clustered at the high school*cohort level. Regressions also include birth year*cohort dummies.

<table>
<thead>
<tr>
<th></th>
<th>Treatment Status Men</th>
<th>Treatment Status Women</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Standardized 10th Grade Math Score</strong></td>
<td>0.001</td>
<td>0.041</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.025)</td>
</tr>
<tr>
<td><strong>Standardized 10th Grade Reading Score</strong></td>
<td>-0.025+</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.020)</td>
</tr>
<tr>
<td><strong>Free Reduced Lunch Eligible</strong></td>
<td>-0.043</td>
<td>0.073</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.046)</td>
</tr>
<tr>
<td><strong>Student is Nonwhite</strong></td>
<td>0.019</td>
<td>-0.038</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.057)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1216</td>
<td>866</td>
</tr>
<tr>
<td><strong>R-squared</strong></td>
<td>0.355</td>
<td>0.321</td>
</tr>
<tr>
<td><strong>F Pre-Treat Variables</strong></td>
<td>1.281</td>
<td>2.109</td>
</tr>
<tr>
<td><strong>p-value</strong></td>
<td>0.294</td>
<td>0.098</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1
Table 3:
Baseline Mentoring Treatment And Transcript Only Treatment Effects on Enrollment in College

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*cohort dummies which is the level at which randomization occurred. Standard errors are clustered at the high school*cohort level. Regressions also include birth year*cohort dummies and controls for race, gender and free lunch.

<table>
<thead>
<tr>
<th></th>
<th>(1) Whole Sample</th>
<th>(2) Women</th>
<th>(3) Men</th>
<th>(4) Did Not Take SAT</th>
<th>(5) Took SAT</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Effects on Enrollment Any College</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mentoring Treatment (OLS)</td>
<td>0.060**</td>
<td>0.146**</td>
<td>0.007</td>
<td>0.083**</td>
<td>0.035</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.042)</td>
<td>(0.025)</td>
<td>(0.026)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>Transcript Only (OLS)</td>
<td>-0.005</td>
<td>0.005</td>
<td>0.000</td>
<td>0.035</td>
<td>-0.049</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.034)</td>
<td>(0.021)</td>
<td>(0.034)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>Mentoring Treatment (IV)</td>
<td>0.133**</td>
<td>0.299**</td>
<td>0.017</td>
<td>0.160**</td>
<td>0.086</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.087)</td>
<td>(0.061)</td>
<td>(0.047)</td>
<td>(0.085)</td>
</tr>
<tr>
<td><strong>Effects on Enrollment Four Year College</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mentoring Treatment (OLS)</td>
<td>0.057**</td>
<td>0.107**</td>
<td>0.020</td>
<td>0.103**</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.031)</td>
<td>(0.028)</td>
<td>(0.026)</td>
<td>(0.033)</td>
</tr>
<tr>
<td>Transcript Only (OLS)</td>
<td>0.001</td>
<td>0.007</td>
<td>0.003</td>
<td>0.002</td>
<td>-0.038</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.022)</td>
<td>(0.028)</td>
<td>(0.012)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Mentoring Treatment (IV)</td>
<td>0.125**</td>
<td>0.222**</td>
<td>0.047</td>
<td>0.202**</td>
<td>-0.018</td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
<td>(0.062)</td>
<td>(0.068)</td>
<td>(0.048)</td>
<td>(0.083)</td>
</tr>
<tr>
<td><strong>First Stage for IV</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mentoring Treatment</td>
<td>0.463**</td>
<td>0.500**</td>
<td>0.429**</td>
<td>0.511**</td>
<td>0.444**</td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
<td>(0.044)</td>
<td>(0.042)</td>
<td>(0.033)</td>
<td>(0.070)</td>
</tr>
<tr>
<td>Observations</td>
<td>2,623</td>
<td>1,114</td>
<td>1,509</td>
<td>1,453</td>
<td>1,170</td>
</tr>
</tbody>
</table>
Table 4:

Mentoring Treatment Effects on Persistence in College

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 2 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, 2011 cohorts. Regressions include high school*cohort dummies which is the level at which randomization occurred. Standard errors are clustered at the high school*cohort level. Regressions include birthyear*cohort dummies to control for students' age within grade.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Enrolled in 3+ Semesters</td>
<td>Enrolled Any College Both School Years Post Graduation</td>
<td>Enrolled Four Year College Both School Years Post Graduation</td>
<td>No SAT Data Enrolled Four Year College Both School Years Post Graduation</td>
<td>Enrolled Second Year Conditional on Enrolled First Year</td>
</tr>
<tr>
<td>Mentoring Treatment</td>
<td>0.129* (0.053)</td>
<td>0.105* (0.042)</td>
<td>0.097** (0.030)</td>
<td>0.014 (0.041)</td>
</tr>
</tbody>
</table>

| Observations | 535 | 535 | 535 | 445 | 263 |
| R-squared | 0.172 | 0.123 | 0.105 | 0.220 | 0.165 |

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1
Figure 1

2010 Cohort: Standardized 10th Grade Math Scores for College Goers and Non College Goers