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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 surprisingly large impacts on college attendance and persistence. 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 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. 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[1]. 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).[1] 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

[1]See **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.

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 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. 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 degree.

We use three separate interventions and survey and administrative data to ask which interventions matter and for whom. We test five broad and non-exclusive hypotheses as to why college going interventions matter. Under Hypotheses 1-3, students are naïve or behavioral with regard to application decisions. Hypotheses 1-3 are 1) irrational fear of the process, 2) a lack of easily obtained information, or 3) inattentiveness and forgetfulness.

Hypothesis 1 is that students want to apply to and attend college but they are so terrified of beginning the process or of failing at the process that the students never complete and file applications. Under this hypothesis, letters of encouragement from college admissions offices (containing a near promise of admission) or a $100 cash incentive could induce students to apply. Hypothesis 2 is 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. Hypothesis 3 is that students are so disorganized that they forget to apply, miss deadlines or never get the job completed through their own general incompetence.

In the language of economics, hypotheses 1-3 suggest 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. But 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.

Hypothesis 4 is that students at the margin are informed and rational, but fairly indifferent between attending or not attending college. Under this hypothesis, the students actually have a career path which they prefer to college and do not have stated aspirations of attending college. While an intervention might induce some of these students to apply and attend college, the students may be unlikely to persist. And the students would reject offers from colleges (after applying successfully) and would offer career reasons as to why college is not for them. One version of hypothesis 4 is that students already know they lack the skills needed to succeed in college.[2]

Hypothesis 5 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.

To test these 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 student and school each week until all steps in college applications are completed and filed. We also make

[2] Since short term college going interventions are unlikely to build meaningful cognitive skills, we suspect that our interventions would not work via erasing a cognitive deficit.

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 admissions office. 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.

Women assigned to the mentoring treatment group see large (13-15 percentage point) increases in their college going rate and these differences persist through at least the second year of college. Mentoring appears to move some women from attending two year colleges to four year colleges and some from "no college" to a two year or a four year college. Since program take up is only about 50%, our treatment on the treated (instrumental variables) estimates of the programs' impact are roughly twice as large i.e. 30 percentage points as measured against a control complier mean rate of college going of 49 percent.

The average effects for men are smaller, less robust and in most specifications statistically insignificant. There are subsets of men (e.g. those who have not taken SATs or those who do not rely on parental help for applications or those with lower test scores) for whom there is a 7-13 percentage point treatment effect on enrollment in college. The smaller effects for men are consistent with the facts that the men in the sample are a.) more likely to say that they have a job that they prefer to attending college, b.) more likely to forecast higher wages for careers which only require a high school diploma, and c.) actually earn high wages than the high school educated women in the sample.

Perhaps more interesting is our evidence on mechanisms through which the treatment works. One of our preferred initial hypotheses was that the treatment would be helpful for students who describe themselves as forgetful, disorganized or have a tendency to miss deadlines. We do not find any evidence to support this hypothesis.

We also hypothesized that the treatment could be helpful to students with low self-esteem or self-confidence. Measures of self-esteem and self-confidence are correlated with college going behavior in general. And we find suggestive evidence that the mentoring treatment is more effective for students with low self-esteem.

We also had hypothesized that fear of rejection or fear of starting the complicated application process was a key obstacle for students. To combat fear of rejection we introduced the transcript only intervention in which students had to merely sign a short release form to get their transcript into the hands of numerous admissions offices. Take up of that intervention was moderate (at 14 percent) and we can rule out statistically that assignment to the treatment has large effects.

What can explain (statistically at least) much of the mentoring effect is the degree to which students report that they can rely on parents or teachers for help in making educational plans and help with applications.

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 examples 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 dozen of papers that analyze the effects of TRIO programs, but among the most credible and comprehensive is the 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.[3]

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) gives us optimism that targeted programs which intervene at the right time with the right assistance or incentives can have a large impact. For example, Hoxby and Turner (2013) find that high achieving low income students apply to and attend more selective schools when mailed information specifically tailored to that student. 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, Oreopoulos, 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.

[3] 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.

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.[4]

There are several papers by economists that deal directly with college coaching. Avery and Kane (2004) provide evidence that coaching in a set of Boston schools raised interest in college and college attendance. Oreopolous, Brown and Lavecchia (2014) find that a comprehensive mentoring program in a Toronto housing project raises high school graduation and college going rates.[5] 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 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 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.

[4] 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

[5] In addition to the published literature there are ongoing mentoring and college application experiments being conducted by Oreopoulus in Toronto and Reber and Phillips in California. These experiments are designed to raise college going among low income students.

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 have a very different object in that we are focused on expanding college access for the group of students who are at the margin of not applying anywhere. In fact, 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.[6]

Second among the existing papers specifically on college coaching or near peer mentoring, we have among the largest sample, the longest follow up period and cleanest identification since we are relying on a randomized control trial of various related treatment arms.

Third, 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.

We mirror several of the existing papers in that we attempt to say as much as possible about mechanisms. We do this in two ways. First we test a few different versions of the treatment to see which aspects are most effective. Second we use survey data and SAT Questionnaire data to understand what strategies and resources the students in the treatment and control groups used to apply to and enroll in college. And we have personality and behavioral measures for both treatment and control students. By interacting resource measures and personality measures with

[6] 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.

treatment status, we discover for whom the treatment works and try to make inferences about why the treatment works.

Our preliminary 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 us 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 our 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 the 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.[7] Since the stated goal is to study interventions for students at the margin of not attending college, there is little shame in actually focusing on those students. Statistical power is likely enhanced by applying the experimental treatments to students for whom there is likely to be some effect on college going behavior.

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 were enrolled in college. We think that our targeted approach has merit, though we were actually surprised at the high college enrollment rate in our control group and we would actually favor more precise targeting as opposed to the elimination of targeting.

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 say the set of students who have not taken the SAT or ACT. However, in most high schools guidance staff know which students have not filed applications particularly because transcripts and often recommendation letters are routed through the guidance department. There may be some large schools in other states that do not know what progress their seniors have made on college applications. Even though those schools could not duplicate our selection process, it's still possible that these 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

[7] See Katz, Kling and Liebman [2001] for definition and estimation of the control complier mean.

right out of the box, but rather to understand more deeply which students fail to apply to college and why.

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, in fact, the program is already being scaled up by Let's Get Ready as part of their College Choice curriculum. Let's Get Ready uses undergraduates coaches from many different selective undergraduate institutions throughout New England and the Mid Atlantic. We are helping conduct the randomized evaluation of that larger program; unfortunately no initial results from that evaluation are available at the time of writing this paper.

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 compares students in our 20 high schools to the other 59 high schools in the state that did not participate. We use Common Core data to make these comparisons. On paper the participating high schools resemble the nonparticipating high schools a great deal. 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.

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 other 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.[8]

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

[8] 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.

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^{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.

Figure 2 addresses the same point but uses scaled rather than standardized math scores and switches to a frequency (count) histogram. The median scaled score is 1136. Of the 14,000 students in the class of 2010, there are more than 1,000 who have math test scores greater than the median score and who do not apply to college. In 2010, more than 1600 students of 14,000 are within these test score deciles and not attending college.

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 or 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.

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.[9] 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 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

[9] The difficult part of the process is using multiple websites to

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 either 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 thereby 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 gets 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 us in order to allow us to send her transcript to all of 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 Appendix 17 for examples of the Community College letter sent to students.

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 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.)

How the interventions relate to our hypotheses about college going behavior

In the introduction we outlined five hypotheses as to why qualified students might fail to apply to college. The two interventions (plus the cash bonus only treatment) are designed to overcome these obstacles and shed light upon which hypotheses are at work. Because the interventions test simultaneously more than one of the five hypotheses, 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 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.

If the mentoring intervention works, it could be overcoming any of: fear (hypothesis 1), a lack of information about college costs and benefits (hypothesis 2), disorganization (hypothesis 3), or a lack of skilled help from a parent or counselor (hypothesis 5).

We hypothesized that a student may fall through the cracks in the application process because she is disorganized or prone to procrastination (hypothesis 3). The mentoring intervention is intended to solve this problem (at least in the short term); the Dartmouth mentor and the program give the student a checklist and a weekly meeting to review progress and fix any aspects of the application that are missing. The weekly meeting helps overcome procrastination in that there is a fixed time for accomplishing the needed tasks. We test explicitly whether the mentoring treatment is more effective for students who are prone to miss deadlines, wait until the last minute to complete assignments, miss out on things they want to do for failure to sign up etc.

We also hypothesized that some students might be lacking parental support or help in completing applications (hypothesis 5). The mentoring treatment is also designed to help overcome this problem. The Dartmouth mentors provide the weekly or even daily check ins and advice that an involved parent might. When a high school student gets stuck on a particular website or has trouble deciding on a set of colleges to which to apply, the mentors (backed up by the guidance counselors) help the high school student work through the problem. We interact treatment status with mother and father's levels of education, parental expectations about the students' education trajectory, and whether or not the student can rely on parental help in completing applications.

We are also able to test whether mentoring treatment students have more accurate information on college tuition and fees (hypothesis 3). And we interact mentoring treatment status with numerous measures of self esteem and openness to experience which is related to hypothesis 1, namely fear of the process. We view this as a partial test of hypothesis 1.

The transcript only intervention does not provide constant monitoring and advice. The transcript only intervention provides a simple set of information to the student about available colleges (where the student is likely to be admitted), a letter(s) from college admissions offices saying that the student has a high likelihood of being admitted, and information on returns to college. We view the transcript only intervention as providing information (hypothesis 2) and reducing fear of the process (hypothesis 1).

The transcript only intervention also breaks a scary and complex process into a couple of smaller steps. By signing a transcript release form, the students are getting one step in the process done and then they receive a positive letter (or set of letters) which may reduce their anxiety level spur them to complete the process.

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, 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, 10^{th} 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 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.[10]

We define several outcome variables using the Clearinghouse data. Our main outcome variable is a dummy variable for a student having any enrollment in college. We also create dummy variables for any enrollment in a four year college, any enrollment in a two year college, and enrollments in 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 gather 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, degree of involvement in sports and extracurricular activities and whether the student needs help in forming educational plans.

[10] For more information on Clearinghouse data see http://www.studentclearinghouse.org/colleges/studenttracker/.

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 at 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 have tried using 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.[11] 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)."

[11] 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.

One downside to having a post but not pre-treatment survey is that the treatment itself might affect the responses to the survey. For some personality measures we think that this is unlikely.[12] 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 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. Relative to a normal distribution a fair number of students are recorded as having the minimum score. Multiple students have a standardized score of -4.0 standard deviations.

This is evident in Figure 3 which shows the distributions of standardized reading scores for the mentoring treatment versus control groups. Figure 3 shows that the mentoring treatment versus control score distributions overlap nearly perfectly. Figure 4 shows the distributions for the math scores. Figure 5 shows how math scores in the experimental groups compare with math scores for all non-experimental students (ie all other students in New Hampshire). Clearly the experimental students have test scores which are below the average student. But there is a great deal of overlap (perhaps even 70-80 percent overlap) in the distributions between the students in the experiment and all other students.

[12] And we regress survey measures directly on the treatment dummy to test whether the treatment affected the mean response.

Randomization was performed at the high school times cohort level.[13] 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:

(1) $\text{Enroll}_i = \alpha + \beta 1 * \text{mentoring treat}_i + \beta 2 * \text{transcript only}_i + \beta 3 * \text{cash bonus only}_i + \gamma * \mathbf{X}_i + \rho * \mathbf{Z}_i + \varepsilon_i$

Here the outcome is whether or not student i enrolls in college following graduation, i.e. after the intervention. The dummy variables mentoring treat_i , transcript only_i , cash bonus only_i denote whether the student is assigned to one of three treatment groups while the omitted category is the no intervention control group.[14] 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

[13] 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).

[14] 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.

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 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 group. 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 some light on which hypotheses can explain the effectiveness of college going interventions. To do this we estimate equations of the following form:

(2) $Enroll_i = \alpha + \beta1*mentoring\ treat_i + \beta4*student\ characteristic_i + \beta5*mentoring\ treatment_i * student\ characteristic_i + \beta2*transcript\ only_i + \beta3*cash\ bonus\ only_i + \gamma*\mathbf{X}_i + \rho*\mathbf{Z}_i + \varepsilon_i$

Here β4 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 β5 captures any interaction between that characteristic and the mentoring treatment.

Results

Our baseline estimates are shown in Table 3. The panels differ in that we change the dependent variable from Any College to Four Year College. The top panel shows treatment effects for "Enrollment in Any College" for the cohorts of 2009-2014. Column (1) show 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 women. This is against a control group mean college going rate of 41.1 percent for the women and a control complier mean of 43.9 percent for the women.[15] 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 not much different than the standard errors on the mentoring intervention or the standard errors for key outcomes in the Hoxby Turner intervention.[16]

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 instrument for taking up the transcript only treatment with assignment to that group, we can not get enough

[15] See Katz, Kling, and Liebman (2001) for calculation of the estimated control complier mean.
[16] See for example their Table 5 on Enrollment in a Peer Institution.

precision to say much about 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.

The second panel of Table 3 switches the outcome to enrollment in a four year college. (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 in 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. who had a lower level or 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. (These 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 effects should be relatively small for attending two year colleges. In Appendix Table 5 we see 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.[17]

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).

[17] 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.

Evidence on Persistence

Clearly, there is a difference between convincing high school seniors to attend college *at all* and having them persist and graduate. A natural question is whether the differences in college enrollment between the treatment and control groups persist after the first year. Table 4 addresses this question. We limit the sample to the 2009 through 2012 cohorts since these are the only cohorts for whom we more than one year's worth of Clearinghouse 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.[18]

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 in college. 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.

[18] 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 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 (col 1), the main effect of the treatment (col 2) and the main effect of "who helped" (col 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 people who do anticipate needing help is 12.6 percentage points. But the treatment effect is nearly zero for people who do not anticipate a need for additional (outside) help (adding cols 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 a 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 both 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 interaction 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 parent's 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 the number of 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 first row of Appendix Table 8 is 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.[19]" Again we see this as evidence that high school educated men are being differentially drawn into the labor market.

Table 6 also provides evidence on 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. Using the second row of Table 6, doubling a student's estimate of community college tuition would reduce the impact of the mentoring treatment on college enrollment by 2 percentage points.

Interactions with Personality Measures

[19] 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."

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) Adventuresomeness, and 3) Self-Esteem. As an alternative we tried to proxy for four of psychology's Big Five personality indices, namely Openness to Experience, Conscientiousness, Extraversion and Neuroticism. We created each of the seven indices by simply averaging binary variables representing each underlying question.[20]

Table 7 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 7 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.

[20] 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.

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 7 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.

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 Appendix Table 11 we interact the mentoring treatment dummy with a dummy for students whose mothers have a high school education or less. This describes roughly 50 percent of the students in the experimental sample. 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 (col 2) suggest that women without a college educated mom have modestly smaller treatment effects than women with a college educated mom. 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.

In columns (5) through (8) of Appendix Table 11, we interact the mentoring treatment with whether the student is non-white and whether the student is enrolled in the free or reduced lunch program. None of the regressions suggest that the treatment effects are statistically significantly larger 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 a plausible magnitude 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.[21] 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 motivation 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

[21] See Appendix 5 for examples of letters received by 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. Hoxby and Turner also used a personalized letter mailed to high school seniors containing pertinent information about college opportunities. A disadvantage of our mailing was that we did not include application fee waivers while Hoxby and Turner did. One advantage of our program is that the letters came directly from college admissions offices and stated directly that admission was likely and mentioned financial aid.

We suspect that the main reason why our take-up rates and results differ from the ECO intervention are the different populations. 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.

Our focus is on students who have proven themselves to be at risk of not filing a single college application. This is a population that guidance counselors and likely parents and teachers have tried to assist with the college choice process without full success. Our interpretation of our results is that for students at the margin of failing to apply, a "boots on the ground" and personal touch intervention is needed. This is consistent with our results on the intervention substituting for parent help. Even though most or all our students have internet access and smart phone technology at their fingertips, simply sending them information and web addresses (such as a college's online application site) does not spur them to action.

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 was not the primary motivation for treatment students to 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.[22]

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 of course that we do not have any precision and we cannot reject large effects of the cash bonus only arm. Columns (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 effects for mentoring against pure controls (col 2) looks similar to the effects of mentoring against cash bonus only (col 3).

To test the effects of the cash bonus more qualitatively, we surveyed (immediately post treatment) as many of the 2012 mentoring treatment students as possible. We asked them which aspects of the program were most helpful to them. Though the sample size is tiny, results indicate that the $100 bonus played at most a minor role in the program. Only 5 of 19 students mentioned the bonus whereas 19 of 19 students cited in person mentoring and 12 of 19 mentioned having application fees paid for. We also asked students explicitly about the bonus and asked them to choose one of four categories to describe how much the bonus mattered to them. Eleven of 19 students said they were aware of the bonus but it had no effect. Another two students said the bonus was initially a motivator but that it had no long run impact while four students were not even expecting the bonus. Only two students said that it was an important factor in their motivation and decision to complete applications.

Certainly students may not be fully cognizant of the factors motivating them and they may not report accurately. Specifically, students might think it unseemly or ungrateful to report that the cash mattered more than the time and effort of the Dartmouth mentors. However, the survey results combined with the statistical results suggest that the bonus by itself is not likely effective.

[22] 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.

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 takeup 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.

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 (23 percent level) 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.

Does Same Gender Mentoring Make a Difference?

Given that the estimated effects vary greatly by gender, it seems natural to ask whether there is an interaction between mentor gender and student gender. Mentors were not explicitly randomly assigned. However, there are two factors which make mentor assignment largely uncorrelated

with a student's interests and ability. First, we assigned students to mentors on a first come, first served basis. In other words, as high school students walked in the door, they were generally assigned to the closest available mentor who was not already working with a student.

Second, at the point of assignment we had no knowledge about the students other than their gender. In Appendix Table 16 we present regressions of outcomes on interactions between treatment status and being assigned a male versus female mentor. We also include a third interaction of treatment status and "not assigned a mentor of either gender" which occurs only when a student chose to not show up for the program. We run separate regressions for male versus female subjects.

In columns (1) and (2), results show a larger treatment effect on any college enrollment for women assigned a same gender mentor, though the effects are not statistically different. And this pattern of coefficients reverses when we examine enrollment in four year colleges. (Women have larger effects with cross gender mentoring in column (2). Men experience a similar sized and insignificant treatment effects, regardless of mentor gender.

Overall we do not have any evidence that same gender interactions are important for the effects from the mentoring treatment and we don't have evidence that gender interactions could explain why the program works more robustly for women.

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[23]. 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 over head or the true value of a college student's time.

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 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 (surprisingly) cost competitive with the H.R. Block intervention which was among the more ingenious, creative and unexpected 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

[23] 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.

spend $120 per student with a closer college match. However as mentioned above, the target population and the goals of the Hoxby and Turner intervention are quite different than ours; Hoxby and Turner target high achieving college bound seniors and they aim to improve match rather than enrollment. By improving match they may raise graduation and earnings significantly.

Discussion and Conclusion

Our study is motivated by the desire to test five 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.[24]

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. So the treatment could be raising awareness of the importance of college for earnings or career choice. Or the treatment could be helping people 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 or teacher in navigating through each piece of the application process. We find several pieces of evidence to support the hypotheses that the treatment substitutes for skilled help from a parent. The treatment effects are concentrated among students who did not rely on a parent to complete applications. This is despite the fact that the treatment did not lower the fraction of students using parents for help.

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

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

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

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 col 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. Though in the case of the cash bonus only treatment we can not say that with any precision. 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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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.

	Control Group			Mentoring Treatment			Transcript Only Group		
Variable	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev
Accepted Treatment	902	0	0	871	0.454	0.498	851	0	0
10th Grade Math Score (Standardized)	798	-0.480	0.937	778	-0.286	0.943	750	-0.370	0.957
10th Grade Reading Score (Standardized)	799	-0.436	0.928	772	-0.278	0.966	751	-0.394	0.940
Math > 50th Percentile in State	798	0.312	0.464	778	0.335	0.472	750	0.304	0.460
Reading > 50th Percentile in State	799	0.350	0.477	772	0.398	0.490	751	0.381	0.486
Math >75th Percentile	798	0.164	0.371	778	0.185	0.389	750	0.157	0.364
Reading > 75th Percentile	799	0.213	0.410	772	0.224	0.417	751	0.221	0.415
Free and Reduced Lunch Eligible	902	0.277	0.448	871	0.286	0.455	851	0.283	0.451
Male	902	0.548	0.498	870	0.575	0.495	851	0.605	0.489
Non-white	902	0.173	0.378	871	0.201	0.401	851	0.160	0.367
Graduation Year	902	2011.527	1.281	871	2011.658	1.641	851	2013.280	0.449
Any College (Clearinghouse)	902	0.438	0.496	871	0.592	0.492	851	0.353	0.478
Four Year College (Clearinghouse)	902	0.169	0.375	871	0.276	0.447	851	0.108	0.311
Persist for First Two Years Post Grad	902	0.195	0.397	871	0.240	0.427	851		
Persist in a Four Year College	902	0.094	0.292	871	0.115	0.319	851		
Enrolled 3+ Semesters	902	0.237	0.426	871	0.292	0.455	851	0.021	0.144
No SAT Data	902	0.708	0.455	871	0.457	0.498	851	0.489	0.500
Accepted Transcript Only	902	0.000	0.000	871	0.000	0.000	851	0.141	0.348

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.

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

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.

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

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.

	(1) Women Enrolled in 3+ Semesters	(2) Women Enrolled Any College Both School Years Post Graduation	(3) Women Enrolled Four Year College Both School Years Post Graduation	(4) Men No SAT Data Enrolled Four Year College Both School Years Post Graduation	(5) Women Enrolled Second Year Conditional on Enrolled First Year
Mentoring Treatment	0.129*	0.105*	0.097**	0.014	-0.040
	(0.053)	(0.042)	(0.030)	(0.041)	(0.066)
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

Table 5: Interaction of Mentoring Treatment with Sources of Assistance on Applications Dependent Variable is Enrolment in Any College

SAT Questionnaire Measure	(1) Coefficients on Treatment*SAT Measure	(2) Coefficient on Treatment Indicator	(3) Coefficient on SAT Measure	(4) N	(5) Mean	(6) SAT indicator regressed on Male Dummy
Do Not Need Help With Educational Planning	-0.116*	0.126**	0.049	1302	0.829	0.015***
	(0.059)	(0.058)	(0.039)			(0.004)

Survey Measure	(1) Coefficients on Treatment*Survey Measure	(2) Coefficient on Treatment Indicator	(3) Coefficient on Survey Measure	(4) N	(5) Mean	(6) Survey indicator regressed on Male Dummy
Parents Help With College Applications	-0.131*	0.118**	0.133***	724	0.468	0.014
	(0.067)	(0.045)	(0.041)			(0.037)
Teacher Helps With College Applications	-0.165*	0.112***	0.089	646	0.172	-0.023
	(0.091)	(0.030)	(0.062)			(0.030)
Guidance Counselor Helps with College Application	-0.009	0.0541	0.037	724	0.312	-0.0982***
	(0.069)	(0.037)	(0.057)			(0.034)

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1 In each row Columns (1)-(5) are from a single regression of "Any College" on the treatment dummy, the survey measure and the interaction of the two. Regressions also include controls for male, free lunch status, and high school*cohort dummies. Column (6) is from an OLS regression of the survey measure on a dummy for male. Numbers are rounded to three decimal places.Survey questions are as follows: "Thinking of the people in your life, which helped you with college applications… Check all that apply."

Table 6: Interaction of Mentoring Treatment with Beliefs About High School Graduate Wages, Need for a College Degree, and College Tuition

Dependent Variable is Enrolment in Any College

Survey Measure	(1) Coefficients on Treatment*Survey Measure	(2) Coefficient on Treatment Indicator	(3) Coefficient on Survey Measure	(4) N	(5) Mean	(6) Survey indicator regressed on Male Dummy
Log (Hourly wage at Age 30 w. only HS Diploma)	-0.159*	0.545**	-0.052	354	2.931	0.307***
	(0.078)	(0.233)	(0.099)			(0.052)
Log (Tuition+Fees Community College)	-0.019	0.246	-0.023	506	9.052	0.032
	(0.036)	(0.310)	(0.031)			(0.091)
Log (Tuition Fees NH Public University)	-0.028	0.343	0.015	502	10.076	-0.040
	(0.030)	(0.295)	(0.023)			(0.085)
Need College Degree for Stated Career Goal	-0.010	0.027	0.273***	663	0.777	-0.089***
	(0.112)	(0.094)	(0.055)			(0.032)
SAT Questionnaire Measure	(1) Coefficients on Treatment*Survey Measure	(2) Coefficient on Treatment Indicator	(3) Coefficient on Survey Measure	(4) N	(5) Mean	(6) Survey indicator regressed on Male Dummy
Plan on Four Year Degree	0.065	-0.023	-0.071	1302	0.783	-0.020***
	(0.070)	(0.064)	(0.045)			(0.003)

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

In each row Columns (1)-(5) are from a single regression of "Any College" on the treatment dummy, the survey measure and the interaction of the two. Regressions also include controls for male, free lunch status, and high school*cohort dummies. Column (6) is from an OLS regression of the survey measure on a dummy for male. Numbers are rounded to three decimal places.

Survey questions are as follows:

“Imagine that you graduate from high school and do not go any further in school. Think about how much you might earn in a job when you are 30. What do you think is the MOST you could earn in a job at age 30 with a HIGH SCHOOL degree?”
“What is your best estimate of the cost of one year's tuition and required fees at a public 2-year community college and a New Hampshire State College or University?”
“How much education do you think you need to get a job working in this occupation?”

Table 7: Interaction of Mentoring Treatment with Personality Measures (Including the Big 5)
Dependent Variable is Enrolment in Any College

Survey Measure	(1) Coefficients on Treatment*Survey Measure	(2) Coefficient on Treatment Indicator	(3) Coefficient on Survey Measure	(4) N	(5) Mean	(6) Survey indicator regressed on Male Dummy
Individual Measures						
Likes to meet new people	-0.305***	0.280***	0.150**	530	0.723	-0.096**
	(0.086)	(0.085)	(0.055)			(0.039)
Enjoy Amusement Rides	-0.287**	0.259**	0.097	530	0.696	0.031
	(0.136)	(0.103)	(0.087)			(0.040)
Composite Measures						
Meets Deadlines/	0.083	0.030	0.096	530	0.343	0.011
Organized	(0.189)	(0.082)	(0.133)			(0.022)
Adventuresome	-0.275	0.239	0.144	530	0.657	0.017
	(0.179)	(0.143)	(0.146)			(0.021)
Self-Esteem	-0.097	0.136	0.143	552	0.672	0.007
	(0.128)	(0.096)	(0.092)			(0.028)
4 of Big 5 Measures						
Openness to Experience	-0.136	0.138	0.008	646	0.408	0.032
	(0.171)	(0.085)	(0.140)			(0.020)
Conscientiousness	0.083	0.030	0.096	530	0.343	0.011
	(0.189)	(0.082)	(0.133)			(0.022)
Extraversion	-0.111	0.143	0.104	646	0.560	-0.023
	(0.148)	(0.097)	(0.077)			(0.027)
Neuroticism	0.077	0.047	-0.103	552	0.305	-0.018
	(0.099)	(0.054)	(0.077)			(0.030)

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

In each row Columns (1)-(5) are from a single regression of "Any College" on the treatment dummy, the survey measure and the interaction of the two. Regressions also include controls for male, free lunch status, and high school*cohort dummies. Column (6) is from an OLS regression of the survey measure on a dummy for male. Numbers are rounded to three decimal places.

Survey questions are as follows:

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

"How much do you agree or disagree with the following statements: I have little control over the things that happen to me; There is really no way I can solve some of the problems I have; What happens to me in the future mostly depends on me; There is little I can do to change many of the important things in my life; I often feel helpless in dealing with the problems of life; I can do just about anything I really set my mind to do; Sometimes I feel that I'm being pushed around in life; Becoming a success is a matter of hard work; luck has little or noting to do with it."

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

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

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

Appendix Table 1:

Students in Participating High Schools Versus Other Large NH High Schools And Versus Rest of US Public High Schools

	(1) Means for Experimental Schools	(2) Means for Other NH Schools	(3) Means for US High Schools (excluding NH)	(4) T-test between Experimental High Schools and Other NH High Schools	(5) T-test between NH High Schools and US High Schools
School is in a Large or Medium Sized City	0.050	0.034	0.142	-0.322	2.636
School is in a Small City	0.100	0.000	0.053	-2.528	1.229
School in a Large Suburb	0.050	0.051	0.179	0.015	3.033
Percentage Eligible for Free Lunch Status	0.122	0.140	0.327	0.756	7.522
Senior Class Size	262.550	170.712	178.926	-2.363	-1.444
Native American Percentage of the Student Body	0.003	0.003	0.026	0.093	1.974
Asian Percentage of the Student Body	0.019	0.014	0.029	-1.868	1.743
Hispanic Percentage of the Student Body	0.029	0.016	0.156	-1.922	5.180
Black Percentage of the Student Body	0.014	0.012	0.145	-0.492	5.020

20 observations recorded for experimental schools, 59 observations recorded for other NH high schools, and 16614 observations recorded for nationwide high schools excluding NH. Numbers are rounded to three decimal places.

Appendix Table 2:

Frequency Table for Experimental Sample

	Control	Treat	Transcript Only	Cash Bonus Only	Total
2009	16	16	0	0	32
2010	250	260	0	0	510
2011	208	240	0	0	448
2012	0	100	0	99	199
2013	329	0	613	0	942
2014	0	255	238	0	493
Total	803	871	851	99	2624

Appendix Table 3:

Means for Survey Responders and Non Responders

	(1) Mean Students Who Responded	(2) Mean Students Who Did Not Respond	(3) T–test of Difference
Accepted Treatment	0.401	0.313	-4.099
Transcript Only Group	0.274	0.345	3.345
10th Grade Math Score (Standardized)	-0.182	-0.451	-6.069
10th Grade Reading Score (Standardized)	-0.169	-0.440	-6.125
Math >75th Percentile	0.200	0.158	-2.375
Reading > 75th Percentile	0.261	0.206	-2.808
Free and Reduced Lunch Eligible	0.294	0.274	-0.964
Male	0.546	0.585	1.721
Non-white	0.156	0.186	1.699
Graduation Year	2012.144	2012.126	-0.270
No SAT data group	0.455	0.584	5.732
Any College (Clearinghouse)	0.585	0.422	-7.283
Four Year College (Clearinghouse)	0.280	0.154	-7.215
Persist for First Two Years Post Grad	0.200	0.131	-4.269
Persist in a Four Year College	0.107	0.059	-4.074
Enrolled 3+ Semesters	0.240	0.169	-4.024

For students who did not respond, there were 1953 observations recorded for all variables except for the standardized test scores for Math and Reading (1685 and 1681 respectively), the 75th percentile for math and reading (1685 and 1681 respectively), and the male group (1952). For students who did respond, there were 646 observations recorded for all variables except for the standardized test scores for math and reading (619 and 618 respectively), and the 75th percentile for math and reading (619 and 618 respectively). Numbers are rounded to three decimal places.

Appendix Table 4:

Mentoring Treatment Effects on Application Rate

	(1) Apply to Any College (Survey) (OLS)	(2) Apply to Any College (Survey) IV Estimate	(3) Women Apply to Any College (Survey) OLS	(4) Men Apply to Any College (Survey) OLS
Mentoring Treatment	0.274**	0.375**	0.294**	0.243**
	(0.050)	(0.068)	(0.049)	(0.078)
Observations	859	859	391	468
R-squared	0.234	0.309	0.293	0.231

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

Appendix Table 5:

Baseline Mentoring Treatment Effects on Enrollment in A Two Year College

Outcome variable is a dummy equal to 1 if the student has an enrollment in ONLY IN a two year college. 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.

	(1) Enrollment Two Year College	(2) Enrollment Two Year College Women	(3) Enrollment Two Year College Men
Mentoring Treatment	0.026	0.048	0.030
	(0.024)	(0.046)	(0.035)
Transcript Only Group	0.001	0.007	0.010
	(0.028)	(0.036)	(0.032)
Observations	2,624	1,114	1,509
R-squared	0.097	0.140	0.113

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

Appendix Table 6:
Probit Regression

	(1) Enrollment Four Year College	(2) Enrollment Four Year College Women	(3) Enrollment Four Year College Men
Mentoring Treatment	0.046**	0.104**	0.016
	(0.017)	(0.032)	(0.027)
Transcript Only Group	-0.002	0.012	-0.002
	(0.022)	(0.036)	(0.039)
Observations	2,393	987	1,322

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

Appendix Table 7:

Split Sample By Test Score

	(1) Enrollment Two Year College Women	(2) Enrollment Four Year College Women	(3) Enrollment Two Year College Men	(4) Enrollment Four Year College Men
Mentoring Treatment	0.127*	0.060	0.045	0.042
	(0.061)	(0.042)	(0.045)	(0.030)
Reading Score > 50th Percentile in Treatment Group	-0.208*	0.118*	-0.034	-0.012
	(0.078)	(0.044)	(0.057)	(0.036)
Transcript Only Group	-0.003	0.024	0.006	0.012
	(0.046)	(0.026)	(0.033)	(0.030)
Observations	967	967	1,331	1,331
R-squared	0.128	0.168	0.092	0.205

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0

Appendix Table 8: Interaction of Mentoring Treatment with Own and Adult Expectations and Adult Sources of Support

Dependent Variable is Enrolment in Any College

Survey Measure	(1) Coefficients on Treatment*Survey Measure	(2) Coefficient on Treatment Indicator	(3) Coefficient on Survey Measure	(4) N	(5) Mean	(6) Survey Measure regressed on Male Dummy
Expects me to attend College						
Parents	-0.079	0.130**	0.310***	646	0.466	-0.034
	(0.075)	(0.052)	(0.049)			(0.040)
Myself	-0.045	0.110	0.232***	623	0.734	-0.038
	(0.081)	(0.072)	(0.047)			(0.036)
Teachers	-0.023	0.101	0.117**	571	0.651	-0.138***
	(0.087)	(0.074)	(0.056)			(0.040)
Guidance Counselors	-0.021	0.078	0.138**	516	0.583	-0.103**
	(0.087)	(0.073)	(0.062)			(0.043)
Talked to About Future Plans						
Parents	-0.072	0.139**	0.158**	646	0.777	0.086***
	(0.068)	(0.059)	(0.064)			(0.033)
Teachers	-0.072	0.104***	0.119**	646	0.347	0.017
	(0.051)	(0.033)	(0.055)			(0.038)
Guidance Counselors	0.079	0.055	0.066	646	0.294	-0.005
	(0.078)	(0.040)	(0.064)			(0.036)
Talked to About College Choice						
Parents	-0.130	0.177*	0.222***	646	0.718	0.072**
	(0.120)	(0.088)	(0.060)			(0.036)
Teachers	-0.064	0.100**	0.138**	646	0.327	0.061
	(0.072)	(0.039)	(0.053)			(0.037)
Guidance Counselors	0.023	0.068	0.108*	646	0.353	-0.022
	(0.074)	(0.045)	(0.063)			(0.038)

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In each row Columns (1)-(5) are from a single regression of "Any College" on the treatment dummy, the survey measure and the interaction of the two. Regressions also include controls for male, free lunch status, and high school*cohort dummies. Column (6) is from an OLS regression of the survey measure on a dummy for male. Numbers are rounded to three decimal places.
Survey questions are as follows:

"How true are the following statements about the adults at your High school?"
"Thinking of the people in your life, which of the following people... Please check all that apply."

Appendix Table 9:
How Do Returns to College Differ for Men Versus Women in NH At Young Ages (22-30)?

We use American Community Survey data from 2005-2010. We limit the sample to individuals ages 22-30. Income is measured as log of total personal income. Sample is not limited by labor force status, but results for just the employed (and also results for all of New England) are in an appendix. State (New Hampshire) is measured as current state of residence. Results by state of birth are in an appendix. Education categories are non-overlapping and hence are each relative to individuals with an education of less than high school.

	(1) Log Total Income Men NH	(2) Log Total Income Women NH	(3) Log Total Income Men All Other States	(4) Log Total Income Women All Other States
High School	0.343**	0.403**	0.345**	0.484**
	(0.075)	(0.100)	(0.004)	(0.005)
One to Three Years of College	0.339**	0.593**	0.405**	0.673**
	(0.078)	(0.101)	(0.004)	(0.005)
Four Plus Years of College	0.663**	0.848**	0.839**	1.193**
	(0.077)	(0.099)	(0.004)	(0.005)
Observations	2925	2898	828,881	794,172
R-squared	0.033	0.046	0.055	0.095
F Test HS=Some College	0.00493	14.49	414.6	3331
p-value	0.944	0.000144	0	0

Standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 10: Personality Measures Interacted with Treatment
Dependent Variable is Enrolment in Any College

Survey Measure	(1) Coefficients on Treatment*Survey Measure	(2) Coefficient on Treatment Indicator	(3) Coefficient on Survey Measure	(4) N	(5) Mean	(6) Survey Measure regressed on Male Dummy
Self Esteem						
Believes In Self	-0.025	0.083	0.096*	552	0.663	0.003
	(0.088)	(0.076)	(0.053)			(0.041)
Deals Well With	-0.063	0.110	0.101	552	0.601	-0.044
Problems	(0.092)	(0.075)	(0.064)			(0.042)
Change Important	-0.057	0.108	0.080	552	0.672	0.027
Things	(0.111)	(0.088)	(0.076)			(0.040)
Solves Problems	-0.151*	0.186***	0.097	552	0.739	0.048
	(0.081)	(0.064)	(0.070)			(0.038)
Not Easily Pushed	0.022	0.055	-0.001	552	0.683	0.003
Around	(0.088)	(0.069)	(0.052)			(0.040)
Adventurous						
Tries Anything Once	-0.040	0.084	0.029	530	0.672	-0.021
	(0.094)	(0.094)	(0.061)			(0.041)
Enjoys Scary Movies	-0.087	0.103	-0.034	530	0.591	-0.040
	(0.083)	(0.061)	(0.051)			(0.043)
Likes to Meet New	-0.305***	0.280***	0.150**	530	0.723	-0.096**
People	(0.086)	(0.085)	(0.055)			(0.039)
Do Crazy Things	0.068	0.014	-0.101	530	0.553	0.135***
	(0.095)	(0.082)	(0.085)			(0.043)
Likes Adventure	0.068	-0.0005	0.095	530	0.813	-0.022
	(0.137)	(0.136)	(0.064)			(0.034)
Enjoy Amusement	-0.287**	0.259**	0.097	530	0.696	0.031
Rides	(0.136)	(0.103)	(0.087)			(0.040)
Move Away	0.072	0.013	0.076	530	0.553	0.135***
	(0.094)	(0.075)	(0.061)			(0.043)
Organization						
Forgets Deadlines	0.002	0.056	0.013	530	0.168	0.025
	(0.128)	(0.044)	(0.071)			(0.033)
Skips Homework	-0.047	0.076	0.059	530	0.408	0.075*
	(0.088)	(0.060)	(0.056)			(0.043)
Lose Papers Easily	-0.049	0.063	-0.080	530	0.157	0.012
	(0.128)	(0.046)	(0.086)			(0.032)
Not Organized	0.119	0.022	0.022	530	0.306	0.048
	(0.087)	(0.057)	(0.055)			(0.040)
Wastes Time	0.055	0.042	-0.039	516	0.479	-0.117***
	(0.058)	(0.056)	(0.046)			(0.044)
Waits Until Last	-0.012	0.073	0.103	516	0.411	0.057
Minute	(0.093)	(0.065)	(0.080)			(0.044)
Surprised By Deadlines	0.071	0.041	0.106*	516	0.481	-0.027
	(0.078)	(0.060)	(0.056)			(0.044)

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In each row Columns (1)-(5) are from a single regression of "Any College" on the treatment dummy, the survey measure and the interaction of the two. Regressions also include controls for male, free lunch status, and high school*cohort dummies. Column (6) is from an OLS regression of the survey measure on a dummy for male. Numbers are rounded to three decimal places (1 sig. digit if number is too small).
Survey questions are as follows:

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

"How much do you agree or disagree with the following statements: I have little control over the things that happen to me; There is really no way I can solve some of the problems I have; What happens to me in the future mostly depends on me; There is little I can do to change many of the important things in my life; I often feel helpless in dealing with the problems of life; I can do just about anything I really set my mind to do; Sometimes I feel that I'm being pushed around in life; Becoming a success is a matter of hard work; luck has little or noting to do with it."

"How true are the following statements about you: I have a good system for remembering deadlines and important dates; I would like to travel to other countries; I miss out on things I want to do because I forget to sign up; I enjoy spending time in places I'm used to, like at home; I'll try anything once; I often miss important deadlines if no one reminds me about them; I like scary movies; I like to meet people who are different from me; Sometimes when my life is really busy, I don't get all of my homework done; I sometimes do 'crazy' things just for fun; I often lose important papers; I enjoy going places I've never been before; I need a better way to remind myself about important deadlines and due dates; In an amusement park, I prefer fast rides; When I move out of my parents' house, I would still like to live close by."

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

Appendix Table 11:

Does Mentoring Treatment Interact With Other Sources of Disadvantage?

	(1) Enrolled in Any College (Women)	(2) Enrolled in Four Year College (Women)	(3) Enrolled in Any College (Men)	(4) Enrolled in Four Year College (Men)	(5) Women Enrolled in Any College	(6) Women Enrolled in Four Year College	(7) Women Enrolled in Any College	(8) Women Enrolled in Four Year College
Treatment	0.151+	0.225**	0.074	0.191+	0.131**	0.115**	0.130**	0.129**
	(0.086)	(0.059)	(0.090)	(0.097)	(0.045)	(0.030)	(0.044)	(0.032)
Mother's Education Is High School Or Less	-0.192*	-0.054	-0.139	-0.032				
	(0.083)	(0.072)	(0.097)	(0.110)				
Treatment * Mother's Education Is High School Or Less	0.102	-0.057	0.002	-0.115				
	(0.094)	(0.096)	(0.107)	(0.127)				
Student is Nonwhite					0.061	0.066*		
					(0.061)	(0.032)		
nonwhite_treat					0.085	-0.043		
					(0.085)	(0.065)		
Treatment * Free Lunch							0.048	-0.075
							(0.069)	(0.072)
Free Reduced Lunch Eligible							-0.032	-0.015
							(0.044)	(0.043)
Observations	214	214	251	251	1,103	1,103	1,103	1,103
R-squared	0.235	0.203	0.245	0.290	0.245	0.172	0.241	0.171

Robust standard errors in parentheses

** p<0.01, * p<0.05, + p<0.1

Appendix Table 12:

Mentoring Treatment Effects by High School

	(1) Dover Women Any College	(2) Kearsarge Women Any College	(3) Lebanon Women Any College	(4) Londonderry Women Any College	(5) Manchester West Any College	(6) Nashua North Women Any College	(7) Nashua South Women Any College	(8) Pinkerton Women Any College	(9) Portsmouth Women Any College
Treatment	0.123	0.182	0.130	0.333	0.072	0.083	0.188**	0.180*	0.083
	(0.177)	(0.206)	(0.162)	(0.378)	(0.079)	(0.098)	(0.070)	(0.079)	(0.191)
Observations	28	23	32	9	179	170	253	249	20
R-squared	0.046	0.173	0.263	0.100	0.035	0.121	0.154	0.133	0.010

Standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 13:
Evidence From 2012 Cohort (Coaching Plus $100 Bonus Versus Bonus Alone)

Data in columns (1) and (2) include 2009, 2010, 2011 cohorts. Data in column (2) are for the 2012 cohort in which the "control" group was offered a $100 bonus for completing applications. Regression 1 includes high school*cohort dummies which is the level at which randomization occurred. Regression 3 includes high school dummies and cohort dummies (since the cash bonus only treatment is constant within highschool*cohort). Standard errors are clustered at the high school*cohort level. Regressions include birthyear*cohort dummies to control for students' age within grade.

	(1) Men and Women Enrollment Any College	(2) Women Enrollment Any College 2009-2011	(3) Women Enrollment Any College 2012
Mentoring Treatment	0.065**	0.152**	0.234
	(0.016)	(0.046)	(0.218)
Transcript Only Group	0.002		
	(0.018)		
$100 Cash Bonus Only	0.022		
	(0.093)		
Observations	2,598	440	95
R-squared	0.200	0.233	0.167

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

Appendix Table 14:
Take Up Rates Within Mentoring Treatment Group

	(1) Take up Within Treatment Group	(2) Women Take up Within Treatment Group	(3) Men Take up Within Treatment Group
2014 Cohort	-0.328**	-0.394**	-0.283**
	(0.080)	(0.087)	(0.080)
Free Reduced Lunch Eligible	0.065	0.100	0.029
	(0.049)	(0.061)	(0.078)
Student is Male	-0.032		
	(0.026)		
Observations	854	361	493
R-squared	0.168	0.233	0.144

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

Appendix Table 15:

Mentoring Treatment Split by Cohorts

	(1) Men and Women Enrollment Four Year College 2009-2011 Cohorts	(2) Women Enrollment Four Year College 2009-2011 Cohorts	(3) Men Enrollment Four Year College 2009-2011 Cohorts	(4) Men and Women Enrollment Four Year College 2013 Cohort	(5) Women Enrollment Four Year College 2013 Cohort	(6) Men Enrollment Four Year College 2013 Cohort	(7) Women Enrollment Four Year College 2014 Cohort	(8) Men Enrollment Four Year College 2014 Cohort
Mentoring Treatment	0.081**	0.139**	0.037				0.083	-0.038
	(0.025)	(0.037)	(0.044)				(0.062)	(0.037)
Transcript Only Group				-0.020+	0.012	-0.037+		
				(0.011)	(0.024)	(0.020)		
Observations	989	439	550	950	381	569	162	240
R-squared	0.135	0.154	0.199	0.022	0.038	0.031	0.053	0.032

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.

Appendix Table 17:

Is Same Gender Mentoring More Effective?

Mentors were assigned on a first come first served basis, but when multiple arrivals occurred at the same time, we had a bias towards same gender pairings. Regressions include a dummy for being assigned to treatment but not showing up to be assigned a mentor. 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.

	(1) Women: Enrollment Any College	(2) Women: Enrollment Four Year College	(3) Men: Enrollment Any College	(4) Men: Enrollment Four Year College
Assigned Mentoring but Did Not Show	0.123*	0.051	-0.038	-0.026
	(0.056)	(0.037)	(0.030)	(0.029)
Assigned Female Mentor	0.220**	0.127*	0.037	0.075+
	(0.064)	(0.054)	(0.065)	(0.043)
Assigned Male Mentor	0.142*	0.207*	0.065	0.079
	(0.053)	(0.090)	(0.048)	(0.051)
Observations	713	713	917	917
R-squared	0.090	0.091	0.081	0.109

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

Appendix 18: Community College System Letter to Transcript Only Group

«First_Name» «Last_Name»
«Address»
«City», NH «Zip»

Dear «First_Name»:

Thank you for participating in the New Hampshire College Going Initiative, and we encourage you to consider attending a Community College System of New Hampshire (CCSNH) institution in the Fall of 2014. The CCSNH offers a wide variety of Associate Degree and Certificate programs preparing students for exciting career opportunities, as well as transfer pathways to four-year colleges and universities.

If you have not already applied, please visit www.ccsnh.edu/admissions to learn more about the seven community colleges in New Hampshire. We recommend you review the college programs and course requirements, and complete an application for admission to the college you wish to attend as soon as possible.

CCSNH programs have affordable tuition, and significant financial aid is available from the federal government. In order to be eligible, please complete the Free Application for Federal Student Aid (FAFSA) form as soon as possible by visiting the website www.fafsa.ed.gov.

Obtaining a college degree is not only personally rewarding, it is also a great way to improve your chances of success in a competitive labor market. Associate Degree holders in New Hampshire earn an average of $43,000.00 annually, and 25% of Bachelor Degree holders earn less than average Associate Degree recipients.

If you have questions or would like to visit one of our colleges, please do not hesitate to contact any of our admission offices. All of the CCSNH institutions provide campus tours and the opportunity to meet with admissions staff to assist with any questions or concerns you or your family might have about attending college.

Thank you,

Admission Directors Committee, Community College System of New Hampshire
Carey Walker, **Great Bay Community College**, http://www.greatbay.edu
Wayne Fraser, **Lakes Region Community College**, http://www.lrcc.edu/admissions
Miho Bean, **Manchester Community College**, http://www.mccnh.edu/admissions
Shelley Duquette, **Nashua Community College**, http://www.nashuacc.edu/admissions
Frank Meyer / Denine Garnett, **N.H.T.I. - Concord's Community College**, http://www.nhti.edu/admissions
Chuck Kusselow, **River Valley Community College**, http://www.rivervalley.edu/admissions.html
Martha Laflamme, **White Mountains Community College**, http://www.wmcc.edu/admissions

Appendix 19: UNH Letter to Transcript Only Group

Dear Name,

Thank you for letting your high school share your transcript with the University of New Hampshire through the New Hampshire College Going Initiative. We appreciated the opportunity to review your high school work and to share our opinion regarding your admissibility to UNH for the fall term 2013. **Based on our review, we would encourage you to apply to UNH because we think you could be a successful student at UNH**.

Applying to UNH is a simple process. You can apply by downloading a paper copy of UNH's first year student application at the following website: http://admissions.unh.edu/apply/ . The instructions for applying are available on the website. You should not hesitate to ask for assistance in completing these application forms. Two good resources would be either the UNH admissions office staff or your high school guidance counselor.

I would ask that if you are interested in applying for admission to UNH, that you do so by June 10. Please contact my office if you have any questions or concerns about this deadline. You can speak with either of the following two individuals about this process:

Chelsea Warner
Assistant Director of Admissions
862-2881

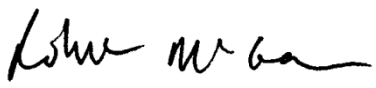
Beth Williams
Assistant Director of Admissions
862-2875

The other task that will be important for you to complete, if you have not already done so, is to complete the Free Application for Federal Student Aid (FAFSA). This is the form you and your family must complete in order to be considered for institutional, state or federal financial aid. A few helpful pieces of information about financial aid at UNH:

- UNH Federal School Code (also called the Title IV Code): 002589
- To complete the FAFSA form, go to the following web site: http://www.fafsa.ed.gov/
- To learn more about financial aid at UNH, go to the following web site: http://financialaid.unh.edu/

Our goal with this process is to encourage you to attend college. You will find that the range of possibilities available to you upon completion of your college degree is enormous. Although you may not know what you want from a college education or what you might do with a college degree, there are many people at UNH (and other institutions) who can help guide you through this process. The first step, however, is for you to apply for admission so that you can begin this journey. Call us if you have any questions or concerns. We look forward to working with you.

Sincerely,



Robert McGann
Assistant Vice President for Student and Academic Services and Director of Admissions

Figure 1

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

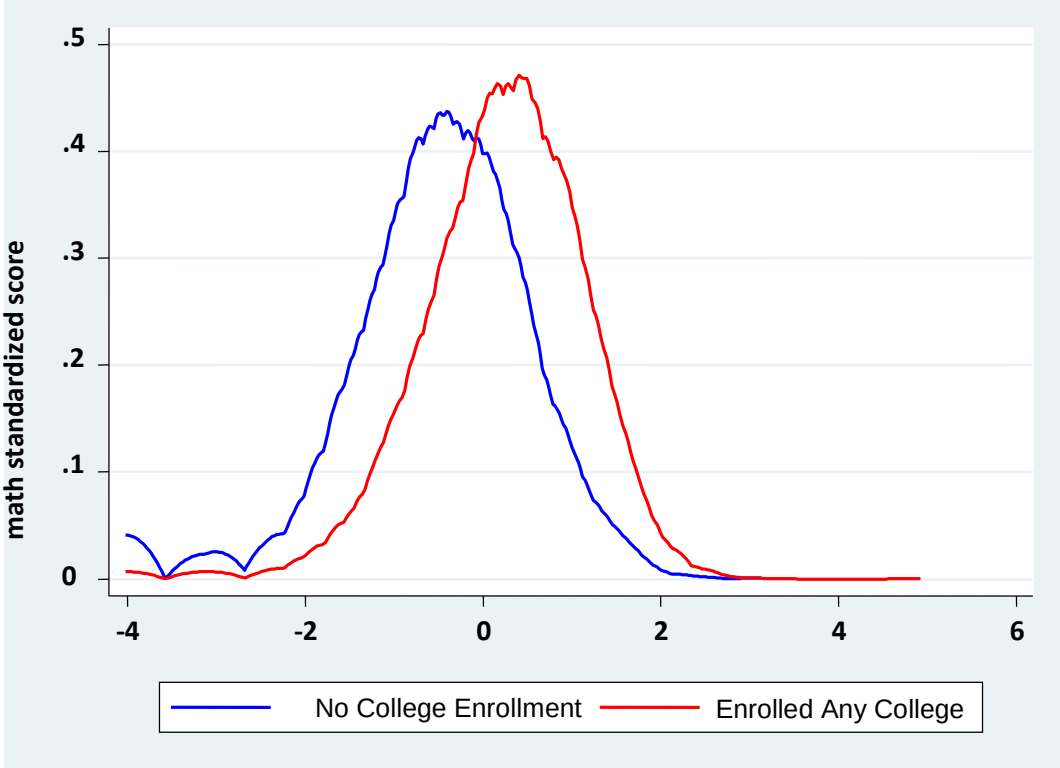


Figure 2

Frequency (count) Historgram. 2010 Cohort: 10th Grade Math Scores for College Goers and Non College Goers

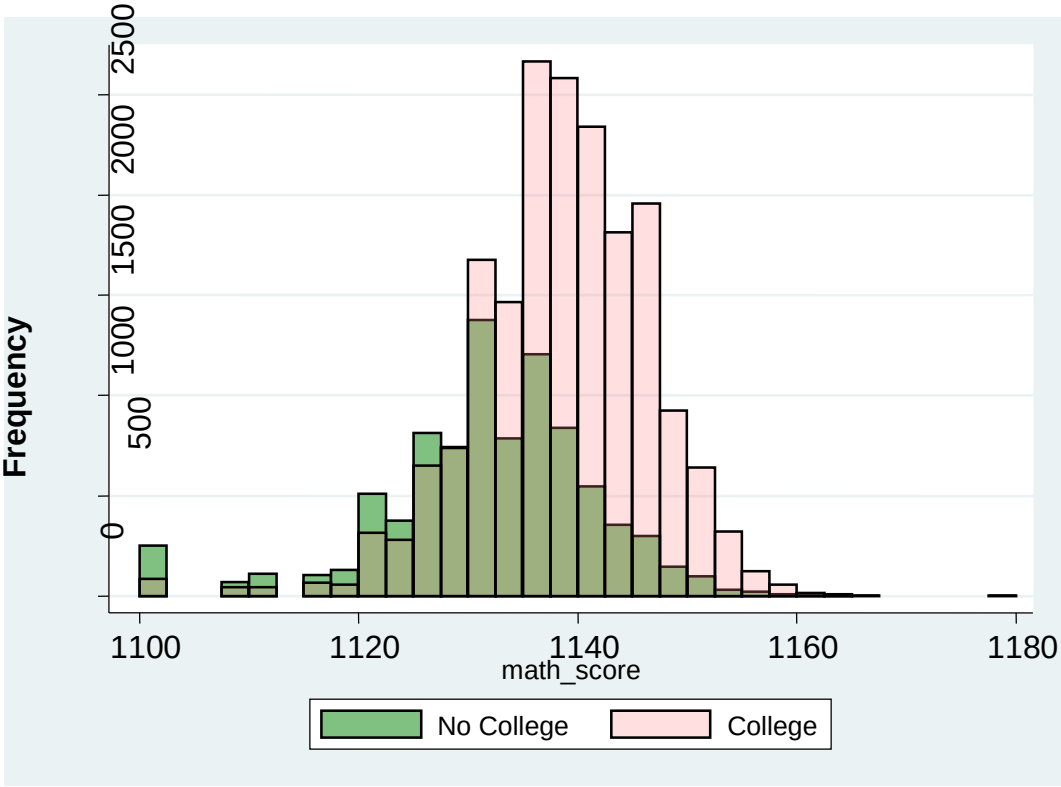


Figure 3:

Treatment and Control Standardized Reading Scores

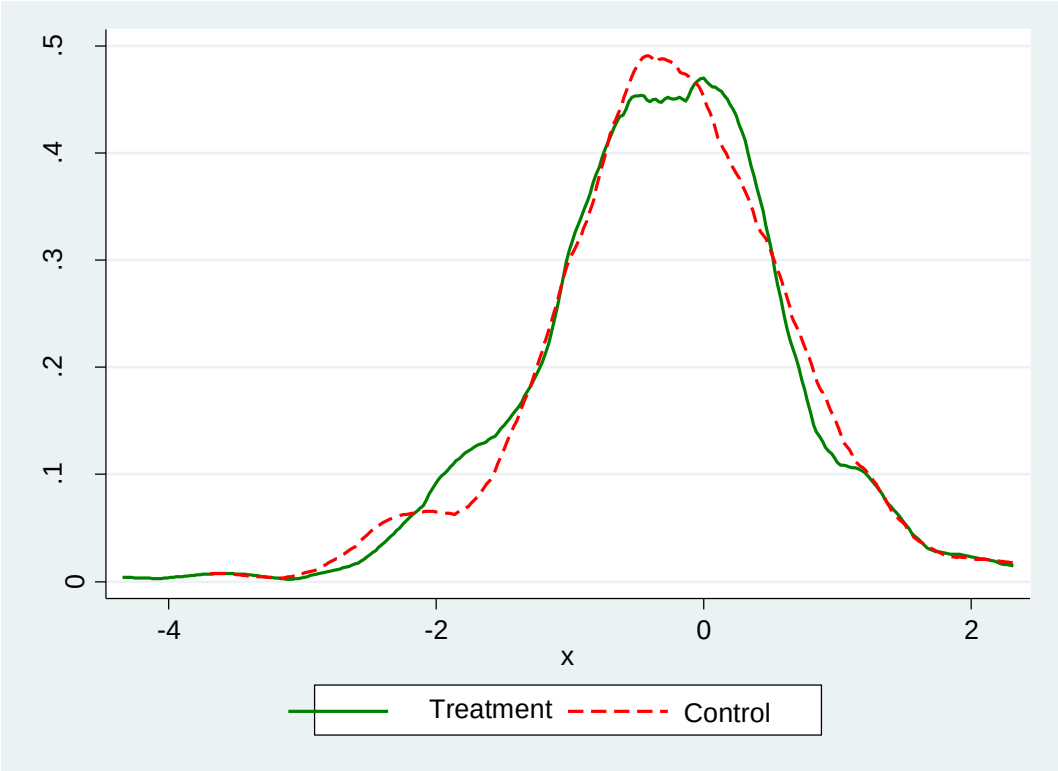


Figure 4: Treatment and Control Standardized Math Scores

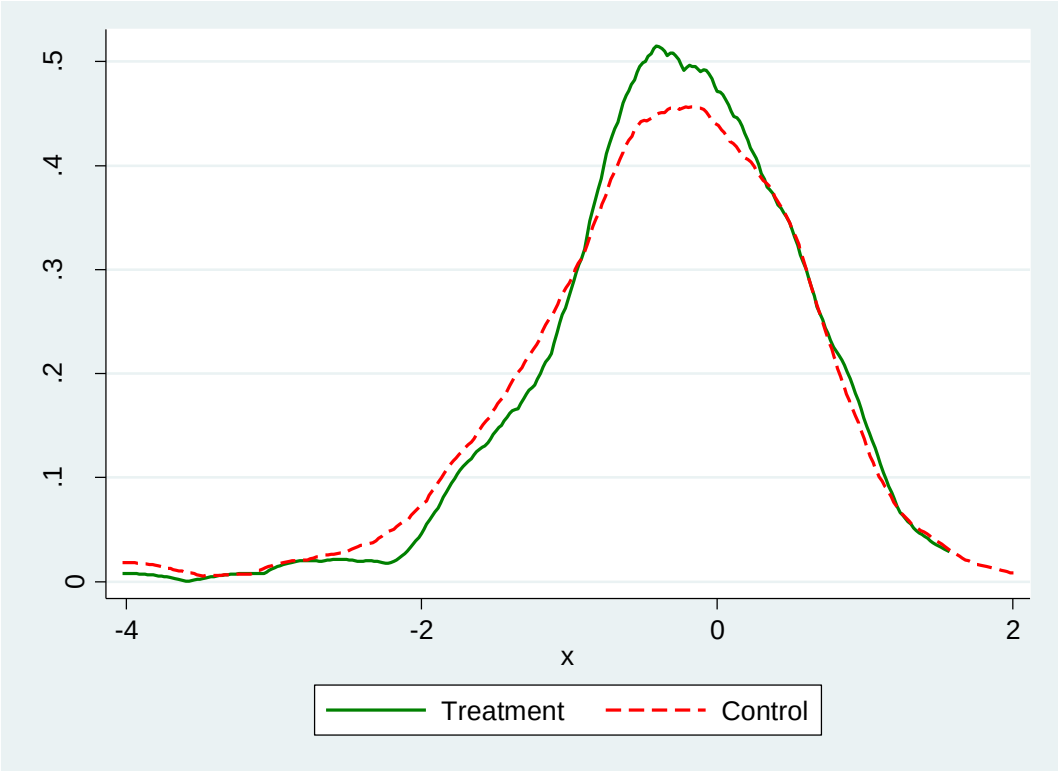


Figure 5

Standardized Math Scores Treatment Versus All Non Experimental

Make this an appendix figure