LATE INTERVENTIONS MATTER TOO: THE CASE OF COLLEGE COACHING IN NEW HAMPSHIRE

Scott Carrell

University of California Davis and NBER

bruce.sacerdote@dartmouth.edu

Bruce Sacerdote*

Dartmouth College and NBER

bruce.sacerdote@dartmouth.edu

March 28, 2013

Abstract

We present evidence from an ongoing field experiment in college coaching/ mentoring. The experiment is designed to ask whether mentoring plus cash incentives provided to high school students late in their senior year have meaningful impacts on college going and persistence. For women, we find large impacts on the decision to enroll in college and to remain in college. Intention to treat estimates are an increase in 15 percentage points in the college going rate (against a base rate of 50 percent) while treatment on the treated estimates are 30 percentage points. Offering cash bonuses alone without mentoring has no effect. There are no effects for men in the sample. The absence of effects for men is not explained by an interaction of the program with academic ability, work habits, or family and guidance support for college applications. However, differential returns to college and/or occupational choice may explain some of the differences in treatment effects for men and women.

*Corresponding author: Department of Economics, Dartmouth College, 6106 Rockefeller, Hanover NH 03755. bruce.sacerdote@dartmouth.edu. We thank Alan Gustman, Caroline Hoxby, Phil Oreopoulos, Sarah Reber, Doug Staiger, Sarah Turner, Hiromi Ono and seminar participants at NBER Summer Institute for helpful suggestions. Sam Farnham and Minal Caron provided outstanding research assistance. Tim Vanderet and Beth Staiger were superb project managers for the field experiment and a dedicated team of 40 Dartmouth students conducted the college coaching/ mentoring. The US Department of Educations' Institute for Education Sciences provided generous funding. Data are provided by the New Hampshire Department of Education and we thank Michael Schwartz, Irene Koffink, and Sudha Sharma for building the state's Data Warehouse and providing support and data. Finally the project could not have succeeded without the help, support and patience of principals and guidance counselors across the state including but certainly not limited to Maureen O'Dea at Nashua North and South and Cindy Bilodeau and Patty Croteau at Manchester West High School.

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 2008) and educational attainment in general (Oreopoulos 2009).

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 thing 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 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.

Our research question is a rather different one, namely can we have a positive impact on college going even late in a student's high school career? Our goal is to provide a road map to college for students who are a) unsure about their future path, b) intimidated by the multi step process of applying, c) or who are perhaps defaulting to a decision of not attending based on their parents' or siblings' behavior rather than on their own personal pecuniary and non-pecuniary returns to college.

Working with high schools around the state of New Hampshire, we designed and implemented a mentoring program that works with students in the winter of their senior year. The high school guidance departments identify students who have expressed interest in college but have taken

[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.

few or no steps to apply. We randomly select half of the students in each school to be members of the treatment group.

For students in the treatment group, we match them with a mentor (a Dartmouth undergraduate) and we visit the student and school each week until all steps in college applications are completed and filed. We also make sure that the FAFSA form is started and the sections other than parental income section are completed. We pay for all application fees and we pay treatments students a $100 bonus in cash for completing the whole program.

Women assigned to the treatment group see large (15 percentage point) increases in their college going rate and these differences persist through at least the second year of college. The treatment 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 base rate of college going of 50 percent in the control group.

There are no effects for men, which suggests that the program interacts with gender in a potentially interesting way. We cannot find evidence that the interaction is related to the students' test scores, work habits (perseverance), or available support from home or the guidance department. The treatment does have modest effects on the career aspirations of women and not men, and this suggests that career choice and/or returns to education may be contributing to the lack of treatment effects for men.

Another possibility is that men respond differentially (or even negatively) to advice or indirect feedback received during the program. Women might infer from the program that they are better prepared or suited for college than their previous personal estimate. Men might get no such positive feedback or may even infer that they are less prepared or less capable relative to peers than their previous belief. More broadly the treatment could be correcting or compensating for some lack of personal confidence or lack of family attention experienced by the women but not the men.

Existing Literature

There is a broad literature on the determinants of college going and most of the literature finds that that key college going decisions occur in middle school or even earlier. See for example Wimberly and Noeth (2005) and Swail and Perna (2002). This literature might suggest that our devised college coaching program for high school seniors is unlikely to have meaningful impacts. Furthermore, one might expect that if we did boost college going for high school seniors, this effect would be short lived and our additional marginal college students would persist in college at a lower than average rate.

However, a recent literature within economics gives us optimism that targeted programs which intervene at the right time with the right assistance or incentives can have a large impact. For example, Hoxby and Turner (2013) find that high achieving low income students apply to and attend more selective schools when mailed information specifically tailored to that student. Bettinger, 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 7 percentage point increase in college going. Castleman and Page (2013) show that targeted text messages increase the fraction of college bound seniors who actually enroll in the fall. Avery and Kane (2004) provide evidence that coaching in a set of Boston schools raised interest in college and college attendance.

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 2008) also have significant impacts on the fraction of high school seniors who attend college.

Our preliminary results on the use of financial incentives confirm results found by Angrist, Lang and Oreopoulos (2009) and Fryer (2010). Specifically we find that financial incentives alone without a support structure or a plan to succeed are not effective but that combining incentives and a plan or support framework can work.

Finally, we designed our intervention with the concept of switching students' default behavior from not attending college to attending. In other words we are hoping to benefit from any human tendency to lean heavily towards the default choice rather than actively resist or undo the default choice. This is in the same spirit as the literature on default choices in savings and retirement plans (Madrian 2000, Choi Laibson, Madrian, Metrick 2004, and Beshears, Choi Laibson Madrian 2009).

Target Audience and the Sample

The program is targeted towards high school seniors who on the verge of failing to apply to college. To identify a group of such seniors, we worked closely with guidance departments at twelve 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 those schools who were most interested in the intervention and who were willing to allow a randomized evaluation thereof.

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. Guidance counselors have a wealth of information about individual students that is not observed to the researchers or in administrative data. For example, the guidance counselors have likely had in the prior twelve months two or three brief (or even extensive) conversations in which the counselor asked the student about college plans. The student may or may not have 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 might be 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 choose half the students to be in the treatment group and 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.

We do not attempt 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.

Treatment 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 coaching program intended to help them complete college applications. Students are told that the program includes in person mentoring, having college application and College Board (or ACT) fees paid, and a $100 cash bonus for completing the process. Students sign a waiver 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.

Each day that we are working with students in a particular high school, the guidance department will notify a 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.

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 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. Formally we defined our target audience as students in deciles 4-8. Appendix Table 7 shows that more than 1600 students of 14,000 are within these test score deciles and not attending college.

In Appendix Table 6, we ask how well test scores plus basic demographics can predict college enrollment for the class of 2010. We find that test scores predict about 14 percent of the variation and that this rises to 17 percent when we include gender, free lunch status, and race.

The Intervention

The intervention has three main 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 the school. Visits are typically 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 was 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.

We ensure that transcript requests are properly filled out and given to each students' 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 a 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. 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.

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.

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, and 2011 cohorts, mentoring was always combined with the cash bonus and application fees. In the 2012 cohort we had a treatment group which received all aspects of the program (mentoring, fees, bonus) and a "control" group that was offered the cash bonus only.

Data Description

Data come from several different sources. First, we have student names and unique ID numbers provided by guidance departments. Second, for the treatment group we have data on colleges applied to, number of visits, name and gender of mentor. Third, for both the treatment and control group we collected post-program survey data on parent's education and intended plans after high school graduation. For some cohorts we also collected 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.

Fourth, we have data from the New Hampshire Department of Education's Data Warehouse. These data include student gender, free lunch status, year of graduation, race, 10th grade math, reading and science scores, high school, and the year that the student first shows up in New Hampshire public schools. We have the Data Warehouse data not just for our treatment and control students, but for every student in New Hampshire in the 2009-2012 graduation cohorts.

The Data Warehouse also provides us with National Student Clearinghouse data on each college enrollment experienced by a student in the 2009-2012 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.[2]

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

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

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 2009-2011, 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.

Table 1 shows summary statistics for the treatment and control groups for the 2009-2012 cohorts. In those four cohorts we have data for a total of 1149 students in the experiment, with 603 of those students in the control group. Forty seven percent of the students in the treatment participated in the study. Roughly 15 percent of treatment students and 17 percent of control students are nonwhite. Twenty four percent of control students and twenty five percent of treatment students are free and reduced lunch eligible.

About 38 and 40 percent of control and treatment students (respectively) have a 10^{th} grade reading score which is above the state median, while 55 and 56 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 treatment and control groups. Figure 3 shows that the treatment and control group test score distributions overlap nearly perfectly. Figure 4 shows the distributions for the math scores. Figure 5 shows how math scores in the treatment group compare with math scores for all nonexperimental 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.

While pre-treatment means for test scores and "non-white" are slightly different between the treatment and control groups, most of these differences disappear when we control for high school time cohort effects. Randomization was performed at the high school times cohort level.[3]

In Table 2 we show regressions of a dummy for 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 for the joint significance of all pre-treatment variables are statistically insignificant for both men (0.387) and women (0.292).

Empirical Strategy

We calculate treatment effects from the program in a straightforward manner. We regress outcome variables (e.g. Enrolled in Any College) on a dummy for treatment status, high school* cohort fixed effects, and demographic characteristics. Specifically we run regressions of the following form:

(1) $Enroll_i = \alpha + \beta 1 * treat_i + \gamma * \mathbf{X}_i + \rho * \mathbf{Z}_i + \varepsilon_i$

Here the outcome is whether or not student i enrolls in college following graduation, i.e. after the intervention. The dummy variables $treat_i$ captures whether or not the student is assigned to the treatment group. The vector X is a set of student level background characteristics including gender, nonwhite, age, free and reduced lunch status, and in some specifications 10th grade test scores. The vector Z is a set of high school fixed effects. Standard errors are corrected for

[3] 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).

clustering at the school*cohort level. In practice 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.

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

Results

Our baseline estimates are shown in Table 3, Panels A, B, and C. The panels differ only in that we change the dependent variable in each panel. Panel A shows treatment effects for "Enrollment in Any College" for the cohorts of 2009, 2010, 2011, and 2012. Column (1) show the treatment effect (from assigned to treatment group) for both genders combined and the effect is a statistically significant 5.4 percentage point increase in the college going rate.

However the effects look very different when we split the sample by gender. There is no effect of assignment to the program on college going for men but a highly significant 15.1 percentage points for women. This is against a control group mean college going rate of 50.4 percent for the women. (See Appendix Table 3 for sample means for just the women.) In column (5) 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 0.50.

The second stage regression for the women is in column (4). The treatment has an effect of 30.1 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 baseline rate (50%).

Panels B and C split the dummy for enrollment in any college into enrollment in a four year college and enrollment in (and only in) a two year college. In panel B, considering enrollment in a four year college, the effect for the combined men and women sample is significant at the 0.05-level. The intention to treat effect for women is 9.9 percentage points and the treatment on the treated effect for women is nearly 20 percentage points. In a *relative* sense, these effects are substantially bigger than in the effects for "any college" since the control mean for women enrolling in a four year college is 22 percent. In other words, for treated women, the program nearly doubles the four year college going rate.

Since the program has large absolute sized effects on both "any college" and "four year college", that implies that the program's effects should be relatively smaller for attending two year colleges. In Table 3, Panel C 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 for women by an insignificant 5.3 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.[4]

Table 5 provides evidence which is consistent with this hypothesis. We split the sample by women who score above and below the state median on 10th grade reading (NECAP) test. Interestingly, the treatment raises two year college going for women with below median test scores with a marginally significant effect of 13.8 percentage points. For these women there is no impact on four year college going. Likewise, the reverse is true for women with above median test scores. The latter group shows large and significant impacts for four year college going (16.5 percentage points) and small and insignificant effects on two year college going. In other words, the treatment affects two year college going for some women and four year college going for others.

[4] It's not possible to observe directly what each woman would have done in the absence of the program so it is not possible to state definitively how the program moved numbers of people between outcome categories.

Clearly, there is a difference between convincing high seniors to attend college *at all* and having them persist and graduate. A natural question to ask 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 2011 cohorts since these are the only cohorts for whom we more than one year's worth of Clearinghouse data. We further limit the sample to women since there are no effects for men at any tenure or in any of the cohorts.

In column (1) we use as the dependent variable a dummy for the student being enrolled in three or more semesters of college. The treatment effect is 11.5 percentage points and significant at the 10-percent level. In 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 12.8 percentage points and significant at the 5-percent level. The effects in columns (1) and (2) are modestly lower than our 15.1 percentage point estimate for "ever enrolled" in any college, though these effects not statistically different from one another. Finally, when we examine effects on being enrolled in a four year college for both years post-high school graduation, the treatment effect is 12.2 percentage points. This effect is slightly larger than our "ever enrolled" estimate of 9.9 percentage points.

Finally in column (4) we limit the sample to women who were enrolled in the first year and ask whether the program affects their likelihood of being enrolled in the second year. The question being asked is whether treatment students in college persist at higher or lower rate than control students 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.

Does the Cash Bonus Alone Generate the Treatment Effect?

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 program against a single component.[5]

Results are shown in Table 6. Columns (1) repeats the results from Table 3 (cohorts 2009-2011) showing the program raised "any college" and "four year college" for women by 14.2 percentage points. Column (2) is the analogous regression for women in the 2012 cohort. The dependent variable is "any college." The point estimate for the treatment effect (against the bonus only) is 14.5 percentage points. In other words the point estimate for the treatment effect for the 2012 cohort is nearly identical to the estimate for the 2009-2011 cohorts, even though the comparison group in 2012 is being offered a $100 bonus if they complete college applications.

In column (4) we stack the 2009-2012 data to formally test whether a treatment of cash bonus alone is correlated with college going. In this stacked regression, we can see that the full treatment raises college going for women by 11.1 percentage points but the cash only treatment raises college going by a statistically insignificant 0.9 percentage points.

To test this notion more qualitatively, we surveyed (post treatment) as many of the 2012 treatment students as possible. We asked them which aspects of the program were most helpful to them. Though the sample size is very small, 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.

[5] Our baseline results (Tables 3-5) are entirely robust to omitting the 2012 Cohort. An ideal situation would of course be to have enough sample to test all possible individual and interacted components of the program. We knew up front that we would not have a large enough sample to attempt this. Our baseline results (Tables 3-5) are entirely robust to omitting the 2012 Cohort.

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.

How Does the Program Interact with Sources of Advantage?

Unfortunately, the experiment was designed only to test whether or not simple steps can boost college going among high school students who are at the margin of attending or not attending. Our ability to parse *why* the program works and why it only works for women is limited as is our available number of covariates.

One interesting way to cut the data is to ask whether the program interacts positively with other sources of advantage enjoyed by a subset of the students. In Table 7 we (again) ask whether the program is more effective for students with higher test scores. We interact treatment status with dummy variables for having reading scores that are above the 75th percentile. We find little evidence that the program works better (or worse) for high scoring students. The interaction between high reading score and the treatment is positive and statistically insignificant for both men and women across both enrollment outcomes.

In Table 8, we interact the treatment with whether the student is non-white and whether the student is enrolled in the free or reduced lunch program. Although the positive point estimates suggest that the treatment effects are larger for these disadvantaged groups, the coefficients are not statistically different from zero.

A final way to ask whether the program is a complement or substitute for advantages faced by students is to examine how the treatment effects vary by high school. Our high schools are located in fairly different communities and the treatment may work better or worse in high schools with more resources. In Appendix Table 1 we report effects separately by high school for any school with more than 20 experimental students total (men and women). We limit the analysis sample to women since again it is only 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.

The one high school in which we did not expect to have much effect and where we did not 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 Figure 6 we graph the measured treatment effects against the average college going rate among (non-experimental) high school seniors in those high schools. Portsmouth High School has the smallest treatment effect and the highest college going rate. Kearsage is a bit of an outlier in that it has a large estimated treatment effect even though it has a high baseline college going rate. The large estimated treatment effect could be a fluke of the smaller sample, or it could be that the Kearsarge guidance department was very successful in nominating a small group of students who really needed the program to succeed.

Most importantly (ignoring Kearsarge) the three large schools of Dover, Pinkerton, and Manchester West have the largest treatment effects and the lowest baseline college going rates of schools in our sample. These three schools are among the larger and most resource challenged of all high schools in New Hampshire.

On balance the evidence suggests that the program may compensate for, rather than reinforce disadvantages that students face in the process of applying for and attending college.

Why is the Effect only for the Women?

Here we provide a bit of evidence on two theories as to why the program works for women and not men. One theory is that the women in the sample are inherently more organized and persistent and the program interacts positively with certain unobserved skills and advantages, rather than compensating for disadvantages faced by students. For example, the women in the 2012 cohort are much more likely to report they typically "complete assignments immediately" instead of "at the last possible moment." Nine percent of the men report that they complete assignments immediately versus 25 percent of the women.[6]

In Table 9 we interact treatment status with a dummy variable for completing assignments immediately. Interestingly, the treatment effect is much lower, not higher for students who complete assignments immediately. A reasonable interpretation of this result is that the program is helpful for students who are pre-disposed to procrastinate and far less beneficial for students who complete tasks without extra prodding. That conclusion is precisely what we envisioned in designing the treatment. However, the empirical finding suggests that the program should have *larger* not smaller treatment effects for men.

A second theory is that returns to education for women in the sample are higher than for the men and that the program interacts positively with the long terms gains from college attendance. A crude way to look at this question is to use observational data to ask whether returns to college for New Hampshire natives differ substantially for men and women.

We turn to this question in Table 10. We take the sample of all individuals in the American Community Survey during 2005-2010. We limit the sample to people ages 30-39. We run our own simple returns to college regressions separating by men and women and looking at New Hampshire (or New England) versus the rest of the country. Our regression is log of total income regressed on dummies for having exactly a high school education, 1-3 years of college, or 4 plus years of college. The omitted category are individuals with less than high school and all coefficients are the change in log points relative to this baseline category.

Results show that women and men have roughly the same returns to education in NH. However, in Appendix Table 8, we re-run the regression when limiting the sample to those employed. An interesting and potentially relevant fact emerges from these regressions. Measured in this way, women in NH have roughly a ten-percentage point higher average return to both one to three years and four years of college compared to men. Additionally, compared to the rest of the country, returns to education for both men and women appreciable smaller. The lower returns to "some college" for men and high returns for women strike us as quite plausible. To give a

[6] Unfortunately we only collected this measure beginning in 2012 because our desperation to understand the treatment effects by gender only began after seeing results for the 2010 and 2011 cohorts.

couple examples, some of the most common occupations targeted by women in our sample who are attending community colleges are dental hygienist, nurse, and medical technician. No men mention these as possible occupations and they frequently list occupations such as contractor or electrician which could be pursued with or without a college degree.

In Table 11 we present a small amount of evidence related to this point. Based on our survey data, the treatment appears to affect women's career choices (or possibly their beliefs about their preferred career). Specifically the treatment raises women's estimate of their annual income and it also raises women's belief that a college degree is needed to succeed in their chosen career. Women in the treatment group estimate annual income in their preferred field at $8400 per year more than women in the control group. Furthermore women in the treatment group are 17 percentage points more likely to believe that a college degree is needed for their field. There are no such effects for the men.

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. We did have a modest bias towards creating same gender pairs and that is evident in the cross tabulation in Appendix Table 5. However, in the analysis that follows we stratify by high school student gender.

In Table 12 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 modestly larger treatment effect for women assigned a same gender mentor, though the effects are not statistically different. Likewise, in columns (3) and (4), results show that men experience a small and statistically insignificant treatment effect, regardless of mentor gender.

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

Discussion and Conclusion

We set out to ask whether encouragement, incentives and mentoring can have a meaningful impact on college going for students who are in the final months of their senior year of high school. Even with the small samples available to us, the answer is clearly yes. However, the program we devised appears to only be effective for female students and not male students. The latter finding remains a bit of a mystery because the men in our sample appear to have a greater tendency to procrastinate, and our program is generally more (not less) effective for students who describe themselves as struggling to meet deadlines.

One possible explanation is that returns to college for employed workers in New Hampshire are appreciably lower for men than for women. While returns to education look similar for men and women in New Hampshire by ages to 30-39, this is not true at younger ages. When we re-run Table 10 for men and women ages 22-30, we find that young adult males have no earnings return to 1-3 years of college while young adult women have a 20 percent increase in annual earnings from 1-3 years of college. Similarly, at ages 22-30 women in NH have a significantly higher return to four years of college than do men. Results are shown in Appendix Table 9. Our hypothesis is that the men in our treatment group may be less attracted to college opportunities than the women in our treatment group because the labor market for high school educated men in NH is relatively stronger.

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 other New Hampshire students do.

One significant next step will be to further develop and test hypotheses as to why the program is only effective for women. 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. For the women at least, 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.

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 and to further understand why programmatic impacts differ so much by gender.

Bibliography

References

Abdulkadiroglu, Atila, Joshua Angrist, Susan Dynarski, Thomas J. Kane, and Parag Pathak. 2010. "ACCOUNTABILITY AND FLEXIBILITY IN PUBLIC SCHOOLS: EVIDENCE FROM BOSTON'S CHARTERS AND PILOTS." NBER Working Paper 15549

Avery, C. & Hoxby, C.M. 2004, "Do and should financial aid packages affect students' college choices?", NBER Chapters, in: Caroline Hoxby ed, *College Choices: The Economics of Where to Go, When to Go, and How to Pay For It*, Chicago: University of Chicago Press.

Avery, Christopher & Thomas J. Kane, 2004. "Student Perceptions of College Opportunities. The Boston COACH Program," NBER Chapters, in: Caroline Hoxby ed, *College Choices: The Economics of Where to Go, When to Go, and How to Pay For It,* Chicago: University of Chicago Press, pages 355-394.

Beshears, J., Choi, J.J., Laibson, D. & Madrian, B.C. 2009, *The importance of default options for retirement saving outcomes: Evidence from the United States, NBER Working Paper* .

Bettinger, E., Long, B.T., Oreopoulos, P. & Sanbonmatsu, L. 2009, *The role of simplification and information in college decisions: Results from the H&R block FAFSA experiment*, .

Borus, M. E., and S. A. Carpenter. 1984. Factors associated with college attendance of high-school seniors* 1. Economics of Education Review 3 (3): 169-76.

Cabrera, A. F., and S. M. La Nasa. 2000. Understanding the College-Choice process. New Directions for Institutional Research 2000 (107): 5-22.

Cascio, E., Clark, D. & Gordon, N. 2008, "Education and the age profile of literacy into adulthood", *The Journal of Economic Perspectives*, vol. 22, no. 3, pp. 47-70.

Castleman, B. L. & Page. L. C. (forthcoming). A trickle or a torrent? Understanding the extent of summer “melt” among college-intending high school graduates. Social Science Quarterly.

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

Castleman, B.L., Arnold, K.C., & Wartman, K.L. (2012). Stemming the tide of summer melt: An experimental study of the effects of post-high school summer intervention on low-income students’ college enrollment. The Journal of Research on Educational Effectiveness 5(1): 1 – 18.

Card David, "The Causal Effect of Education on Earnings," in Orley Ashenfelter and David eds, *Handbook of Labor Economics*, Volume 3, Part 1, 1999, Pages 1801-1863.

Choi, J.J., Laibson, D., Madrian, B.C. & Metrick, A. 2004, *For better or for worse: Default effects and 401 (k) savings behavior, NBER Working Paper* .

Christensen, S., J. Melder, and B. A. Weisbrod. 1975. Factors affecting college attendance. The Journal of Human Resources 10 (2): 174-88.

Cornwell, C., Mustard, D.B. & Sridhar, D.J. "The enrollment effects of merit-based financial aid: Evidence from Georgia's HOPE scholarship", .

Cullen, J.B., Jacob, B.A. & Levitt, S. 2006, "The effect of school choice on participants: Evidence from randomized lotteries", *Econometrica,* vol. 74, no. 5, pp. 1191-1230.

Currie, Janet and Enrico Moretti "Mother's Education and the Intergenerational Transmission of Human Capital: Evidence From College Openings," *Quarterly Journal of Economics*, November 2003, Vol. 118, No. 4, Pages 1495-1532.

Dynarski, S. 1999, *Does aid matter? Measuring the effect of student aid on college attendance and completion, NBER Working Paper* .

Dynarski, S.M. 2003, "Does aid matter? Measuring the effect of student aid on college attendance and completion", *American Economic Review,* vol. 93, no. 1, pp. 279-288.

Dynarski, S.M. 2000, "Hope for whom? Financial aid for the middle class and its impact on college attendance", *NBER Working Paper No. 7756*.

Fryer Jr, R.G. 2010, *Financial incentives and student achievement: Evidence from randomized trials, NBER Working Paper* .

Goldin, C. & Katz, L.F. 1998, "The origins of state-level differences in the public provision of higher education: 1890-1940", *The American Economic Review,* vol. 88, no. 2, pp. 303-308.

Goldin, C., Katz, L.F. & Kuziemko, I. 2006, *The homecoming of American college women: The reversal of the college gender gap, NBER Working Paper* .

Goldin, C.D. & Katz, L.F. 2008, *The race between education and technology,* Harvard University Press.

Hoxby, C.M. & Murarka, S. 2009, "Charter Schools in New York City: Who Enrolls and How They Affect Their Students' Achievement", *NBER Working Paper*.

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

Imbens, G. W., and J. D. Angrist. 1994. Identification and estimation of local average treatment effects. *Econometrica: Journal of the Econometric Society*: 467-75.

Kane, T.J. 2006, "Public intervention in post-secondary education", *Handbook of the Economics of Education,* vol. 2, pp. 1369-1401.

Kane, T.J. 2003, "A quasi-experimental estimate of the impact of financial aid on college-going", *NBER Working Paper, NBER Working Paper 9703*.

Lay, C. H. 1986. At last, my research article on procrastination* 1. Journal of Research in Personality 20 (4): 474-95.

Levine, A., and J. Nidiffer. 1996. Beating the odds: How the poor get to college. The Jossey Bass Higher and Adult Education Series.

Madrian, B.C. & Shea, D.F. 2000, *The power of suggestion: Inertia in 401 (k) participation and savings behavior, NBER Working Paper* .

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

Oreopoulos, P. 2006, "Estimating average and local average treatment effects of education when compulsory schooling laws really matter", *The American Economic Review,* , pp. 152-175.

Organisation For Economic Co-Operation And Development, *Education at a Glance*

2011.

Ruggles, Steven, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]. Minneapolis: University of Minnesota, 2010.

Scott-Clayton, J. 2011, "On Money and Motivation: A Quasi-Experimental Analysis of Financial Incentives for College Achievement", Journal of Human Resources, vol. 46 no. 3 614-646.

Senécal, C., R. Koestner, and R. J. Vallerand. 1995. Self-regulation and academic procrastination. The Journal of Social Psychology 135 (5): 607-19.

Swail, W. S., and L. W. Perna. 2002. Pre-college outreach programs. Increasing Access to College: Extending Possibilities for all Students: 15–34.

Watson, D. C. 2001. Procrastination and the five-factor model: A facet level analysis. Personality and Individual Differences 30 (1): 149-58.

Wimberly, G.L.; Noeth, R.J. 2005 College Readiness Begins in Middle School. ACT Policy Report. American College Testing ACT Inc, 33, ERIC.

Table 1: Summary Statistics for 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			Treat		
Variable	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev
Accepted Treatment	546	0.009	0.095	603	0.496	0.500
10th Grade Math Score (Standardized)	464	-0.455	0.958	505	-0.397	0.929
10th Grade Reading Score (Standardized)	464	-0.362	0.939	500	-0.370	0.916
Math > 50th Percentile	464	0.517	0.500	505	0.545	0.499
Reading > 50th Percentile	464	0.356	0.479	500	0.370	0.483
Math >75th Percentile	464	0.179	0.384	505	0.170	0.376
Reading > 75th Percentile	464	0.216	0.412	500	0.200	0.400
Free and Reduced Lunch Eligible	546	0.269	0.444	603	0.292	0.455
Male	546	0.527	0.500	600	0.568	0.496
Non-white	447	0.152	0.360	500	0.174	0.379
Low SES	498	0.295	0.457	555	0.317	0.466
Graduation Year	546	2010.7	0.796	603	2010.7	0.772
Any College (Clearinghouse)	546	0.518	0.500	603	0.572	0.495
Four Year College (Clearinghouse)	546	0.227	0.419	603	0.270	0.444
Persist for First Two Years Post Grad	447	0.340	0.474	503	0.360	0.480
Persist in a Four Year College	447	0.157	0.364	503	0.179	0.384
Enrolled 3+ Semesters	447	0.365	0.482	503	0.408	0.492

Table 2: Treatment Status Regressed on Pre-Treatment Characteristics

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 also include birthyear*cohort dummies.

VARIABLES	(1) Treatment Status Men	(2) Treatment Status Women
Standardized 10th Grade Math Score	0.013	0.070
	(0.026)	(0.045)
Standardized 10th Grade Reading Score	-0.046	-0.018
	(0.031)	(0.050)
Free Reduced Lunch Eligible	-0.087	0.105
	(0.070)	(0.111)
Student is Nonwhite	0.047	-0.022
	(0.064)	(0.065)
Constant	1.198**	-0.115
	(0.024)	(0.076)
Observations	450	349
R-squared	0.085	0.087
F Pre-Treat Variables	1.118	1.377
p-value	0.387	0.292

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

Table 3 Panel A:

Baseline Treatment Effects on Enrollment in Any College

Outcome variable is a dummy equal to 1 if the student has any enrollment in college including 2 year or four 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 Any College	(2) Enrollment Any College Women	(3) Enrollment Any College Men	(4) IV (Treatment on Treated) Enrollment Any College Women	(5) First Stage Women	(6) Test Gender Difference
Treatment	0.054*	0.151**	-0.002		0.500**	0.108**
	(0.020)	(0.040)	(0.032)		(0.046)	(0.034)
Accepted Treatment				0.301**		
				(0.091)		
Male*Treatment Group						-0.098*
						(0.038)
Constant	0.341	0.278**	0.067	-0.012	0.078**	0.328
	(0.270)	(0.016)	(0.136)	(0.405)	(0.012)	(0.240)
Observations	1,146	517	629	517	517	1,146
R-squared	0.186	0.201	0.218	0.160	0.453	0.190

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

Table 3 Panel B:

Baseline Treatment Effects on Enrollment in A Four Year College

Outcome variable is a dummy equal to 1 if the student has any enrollment in a four 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) Enrollment Four Year College	(2) Enrollment Four Year College Women	(3) Enrollment Four Year College Men	(4) IV (Treatment on Treated) Enrollment Four Year College Women	(5) Test Gender Difference
Treatment	0.056*	0.099*	0.026		0.082*
	(0.024)	(0.038)	(0.043)		(0.039)
Accepted Treatment				0.198*	
				(0.077)	
Male*Treatment Group					-0.045
					(0.042)
Constant	-0.049	0.154**	-0.659**	0.810**	-0.040
	(0.057)	(0.018)	(0.183)	(0.177)	(0.058)
Observations	1,146	517	629	517	1,146
R-squared	0.115	0.130	0.167	0.130	0.116

Robust standard errors in parentheses

Table 3 Panel C:

Baseline 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	(4) IV (Treatment on Treated) Enrollment Four Year College Women
Treatment	-0.003	0.053	-0.032	
	(0.025)	(0.040)	(0.040)	
Accepted Treatment				0.102
				(0.080)
Constant	0.356	-0.627**	0.697**	-0.811*
	(0.287)	(0.062)	(0.092)	(0.386)
Observations	1,146	532	614	532
R-squared	0.090	0.129	0.111	0.113

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

Table 4:

Treatment Effects on Persistence in College (Women)

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) Enrolled in 3+ Semesters	(2) Enrolled Any College Both School Years Post Graduation	(3) Enrolled Four Year College Both School Years Post Graduation	(4) Enrolled Second Year Conditional on Enrolled First Year
Treatment	0.115+	0.128**	0.122**	0.033
	(0.055)	(0.039)	(0.032)	(0.043)
Constant	0.002	0.220	0.781*	1.400**
	(0.007)	(0.669)	(0.281)	(0.119)
Observations	419	419	419	222
R-squared	0.148	0.133	0.098	0.174

Table 5:

Split Sample By Test Score

	(1) Enrollment Two Year College Women Below Median Reading Score	(2) Enrollment Two Year College Women Above Median Reading Score	(3) Enrollment Four Year College Women Below Median Reading Score	(4) Enrollment Four Year College Women Above Median Reading Score
Treatment	0.138+	0.037	0.035	0.165*
	(0.066)	(0.079)	(0.061)	(0.061)
Observations	210	218	210	218
R-squared	0.133	0.156	0.084	0.155

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

Table 6

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.

VARIABLES	(1) **2009-2011** Women: Enrollment Any College	(2) **2012** Women Enrollment Any College	(3) **Stacked Data** Women: Enrollment Any College
Treatment	0.142**	0.145	0.111*
	(0.040)	(0.108)	(0.042)
$100 Cash Bonus Only			0.009
			(0.113)
Observations	421	96	517
R-squared	0.186	0.207	0.166

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

Table 7:

Does Treatment Interact With High Test Scores?

VARIABLES	(1) Women: Enrolled in Any College	(2) Women: Enrolled in Four Year College	(5) Men: Enrolled in Any College	(6) Men: Enrolled in Four Year College
Treatment	0.115*	0.111+	0.015	0.060
	(0.047)	(0.053)	(0.044)	(0.056)
Treatment * Reading > 75 Percentile	0.096	0.119	-0.083	-0.041
	(0.108)	(0.153)	(0.054)	(0.088)
Reading Score Above 75th Percentile	0.165+	0.143	0.213**	0.279**
	(0.084)	(0.103)	(0.049)	(0.064)
Constant	-1.113**	-0.659	-0.207**	-0.227**
	(0.070)	(0.471)	(0.055)	(0.070)
Observations	349	349	450	450
R-squared	0.127	0.124	0.157	0.192

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

Table 8

Does Treatment Interact With Other Sources of Disadvantage?

	(1)	(2)	(3)	(4)	(5)
	Enrolled in Four Year College	Enrolled in Any College	Enrolled in Four Year College	Enrolled in Any College	Enrolled in Four Year College
Treatment	0.188**	0.141**	0.119**	0.125*	0.107**
	(0.058)	(0.047)	(0.039)	(0.044)	(0.035)
Mother's Education Is High School Or Less	0.019				
	(0.079)				
Treatment * Mother's Education Is High School Or Less	-0.117				
	(0.127)				
Student is Nonwhite		0.019	0.022		
		(0.093)	(0.068)		
Treatment * Nonwhite		0.030	0.035		
		(0.159)	(0.091)		
Treatment * Free Lunch				0.077	0.066
				(0.098)	(0.113)
Free Reduced Lunch Eligible				-0.104+	-0.141
				(0.053)	(0.087)
	0.487*	0.488	0.826**	0.515	0.854**
	(0.195)	(0.502)	(0.281)	(0.445)	(0.243)
Observations	228	419	419	419	419
R-squared	0.187	0.187	0.129	0.187	0.129

Table 9: Does Treatment Interact with Timeliness of Assignment Completion?

Data are from 2012 Cohort. We surveyed students and asked them "Which of the following best describes your work style?" The possible answers are "complete assignments immediately," "complete before deadline," and "last possible moment."

	(1) Men and Women: Enrolled in Any College
Completes Assignments Immediately	0.205
	(0.096)
Complete Immediately* Treatment	-0.457+
	(0.158)
Treatment	0.124
	(0.109)
Constant	0.680**
	(0.061)
Observations	108
R-squared	0.100

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

Table 10: How Do Returns to College Differ for Men Versus Women in NH?

We use American Community Survey data from 2005-2010. We limit the sample to individuals ages 30 to 40. 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	0.399**	0.409**	0.429**	0.477**
	(0.055)	(0.103)	(0.003)	(0.005)
One to Three Years of College	0.686**	0.667**	0.726**	0.761**
	(0.058)	(0.104)	(0.003)	(0.005)
Four Plus Years of College	1.081**	0.951**	1.251**	1.252**
	(0.055)	(0.102)	(0.003)	(0.005)
Constant	9.955**	9.325**	9.748**	9.146**
	(0.051)	(0.099)	(0.003)	(0.004)
Observations	4,674	4,570	1,129,025	1,050,794
R-squared	0.140	0.049	0.168	0.106
F Test HS=Some College	77.69	33.52	15889	9056
p-value	0	7.53e-09	0	0

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

Table 11: Does Treatment Effect Preferred Occupation and Whether a Degree is Needed in Career?

	(1) Women: Estimated Income from Preferred Occupation	(2) Men: Estimated Income from Preferred Occupation	(3) Women: College Degree Needed for This Career?	(4) Men: College Degree Needed for This Career?
Treatment	8,389.95*	-2,997.66	0.167*	-0.035
	(4002.17)	(12697.67)	(0.071)	(0.116)
Constant	59,207.72**	141,812.44**	0.808**	0.875**
	(376.74)	(11925.76)	(0.055)	(0.090)
Observations	46	32	65	41
R-squared	0.143	0.435	0.080	0.002

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

Table 12: 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.

VARIABLES	(1) Women: Enrollment Any College	(2) Women: Enrollment Four Year College	(3) Men: Enrollment Any College	(4) Men: Enrollment Four Year College
assigned_female_mentor	0.170**	0.087	-0.017	0.098
	(0.060)	(0.063)	(0.068)	(0.068)
assigned_male_mentor	0.128*	0.165	0.041	0.070
	(0.058)	(0.098)	(0.059)	(0.067)
assigned_treat_dont_show	0.171*	0.036	-0.027	-0.031
	(0.072)	(0.038)	(0.048)	(0.035)
Observations	534	534	614	614
R-squared	0.059	0.065	0.103	0.138

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

Table 13: Could We Estimate the Effects for Women Without a Randomized Control Group?

We take the cohorts of 2009, 2010, 2011, 2012. We drop the randomized controls. The assigned to treatment group and treated group are as in the experiment. For the control group, we use all other (non-experimental) students in New Hampshire in the same cohort year. Regressions include high school fixed effects, test scores, birthyear*cohort fixed effects, dummies for free lunch eligible and non-white.

	(1) Women Four Year College: Intended Treatment Versus All Nonexperimental	(2) Women Any College: Intended Treatment Versus All Nonexperimental	(3) Women Four Year College: Intended Treatment Versus All Nonexperimental	(4) Women Any College: Intended Treatment Versus All Nonexperimental
Assigned to Treatment Group	-0.162**	-0.004		
	(0.030)	(0.034)		
Treated			-0.138**	-0.041
			(0.040)	(0.040)
Observations	21,042	21,044	20,934	20,936
R-squared	0.273	0.170	0.272	0.171

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

Table 14: Could We Estimate the Effects By Comparing Those That Accept Treatment to Those That Don't?

We take the cohorts of 2009, 2010, 2011. Sample is limited to treatment group and effect is calculated as difference in outcome between those that accept treatment and those that don't. Regressions include high school* cohort fixed effects, birthyear*cohort effects, dummies for free lunch status, nonwhite.

	(1) Women 4 Year College: Accepted Treatment Versus Didn't	(2) Women Any College: Accepted Treatment Versus Didn't	(3) Men Four Year College: Accepted Treatment Versus Didn't	(4) Men Four Year College: Accepted Treatment Versus Didn't
Accepted Treatment	0.076	-0.035	0.070+	0.017
	(0.091)	(0.092)	(0.035)	(0.067)
Constant	0.613*	0.228	-0.477**	-0.601**
	(0.244)	(0.466)	(0.138)	(0.040)
Observations	259	259	341	341
R-squared	0.167	0.213	0.176	0.231

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

Appendix Table 1

Treatment Effects by High School

VARIABLES	(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	0.078	0.346	0.124	0.333	0.156+	0.116	0.137+	0.284*	-0.000
	(0.193)	(0.260)	(0.202)	(0.378)	(0.088)	(0.111)	(0.081)	(0.124)	(0.208)
Constant	0.448*	0.423+	0.478**	0.333	0.264*	0.518**	0.483**	0.387**	0.750**
	(0.191)	(0.220)	(0.145)	(0.309)	(0.102)	(0.106)	(0.091)	(0.104)	(0.161)
Observations	28	16	24	9	129	82	140	62	20
R-squared	0.073	0.128	0.111	0.100	0.058	0.077	0.079	0.085	0.000

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

Appendix Table 3: Summary Statistics for Treatment and Control Women

Variable	Obs	Control Women Mean	Std. Dev	Obs	Treatment Women Mean	Std. Dev
Accepted Treatment	258	0.000	0.000	259	0.529	0.500
10th Grade Math Score (Standardized)	175	-0.570	0.936	177	-0.382	0.825
10th Grade Reading Score (Standardized)	175	-0.209	0.928	176	-0.168	0.853
Math > 50th Percentile	258	0.326	0.470	259	0.355	0.480
Reading > 50th Percentile	258	0.248	0.433	259	0.297	0.458
Math >75th Percentile	258	0.089	0.286	259	0.100	0.301
Reading > 75th Percentile	258	0.163	0.370	259	0.154	0.362
Free and Reduced Lunch Eligible	258	0.275	0.447	259	0.347	0.477
Male	258	0.000	0.000	259	0.000	0.000
Non-white	212	0.165	0.372	209	0.167	0.374
Low SES	230	0.309	0.463	241	0.373	0.485
Graduation Year	258	2010.694	0.791	259	2010.707	0.816
Any College (Clearinghouse)	258	0.504	0.501	259	0.629	0.484
Four Year College (Clearinghouse)	258	0.217	0.413	259	0.293	0.456
Persist for First Two Years Post Grad	258	0.240	0.428	259	0.320	0.468
Persist in a Four Year College	258	0.109	0.312	259	0.185	0.389
Enrolled 3+ Semesters	258	0.279	0.449	259	0.367	0.483

Appendix Table 4: Did $100 Bonus Matter

Post-Survey of 2012 Treatment Group

$100 bonus affect Decision to Complete Program?		
		Freq.
Aware, no effect		**11**
Initially motivating not long run factor		**2**
Not expecting		**4**
Important		**2**
	Total	**19**

Appendix Table 5: Cross Tab of Student Male and Assigned Male Mentor

Student is Male	assigned_male_mentor 0	1	Total
0	76	73	149
1	61	95	156
Total	137	168	305

Appendix Table 6 Overall Prediction of College Going
Uses the 2010 Non experimental Kids

	(1) Enrolled Any College 2010 Cohort	(2) Enrolled Any College 2010 Cohort
Standardized 10th Grade Math Score	0.081** (0.005)	0.082** (0.005)
Standardized 10th Grade Reading Score	0.102** (0.005)	0.084** (0.005)
Student is Male		-0.075** (0.007)
Free Reduced Lunch Eligible		-0.119** (0.011)
Student is Nonwhite		0.023 (0.016)
Constant	0.715** (0.004)	0.767** (0.005)
Observations	13712	13712
R-squared	0.136	0.173

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

Appendix Table 7: College Going And Persistence By Math Score Quantile

Sample is students in the 2010 Cohort who were not in the Treatment Group

Quantile of 10th Grade Math Score	N	Any College	Four Year College	Persist	N Target Audience
1	1,394	0.41	0.16	0.24	
2	1,395	0.54	0.24	0.35	
3	1,394	0.59	0.30	0.38	
4	1,395	0.70	0.41	0.50	425
5	1,394	0.71	0.49	0.55	404
6	1,395	0.77	0.57	0.62	315
7	1,395	0.80	0.62	0.67	275
8	1,394	0.85	0.72	0.72	215
9	1,395	0.89	0.79	0.79	
10	1,394	0.91	0.84	0.82	
Total	13,945	0.72	0.51	0.56	1,634

Appendix Table 8: Returns To College Table: Limit Sample to Employed

American Community Survey Data 2005-2010. Sample includes individuals ages 30-40.

	(1)	(2)	(3)	(4)
	Log Total Income Men NH	Log Total Income Women NH	Log Total Income Men All Other States	Log Total Income Women All Other States
High School	0.216**	0.346**	0.368**	0.400**
	(0.053)	(0.108)	(0.003)	(0.004)
One to Three Years of College	0.442**	0.554**	0.602**	0.641**
	(0.054)	(0.108)	(0.003)	(0.005)
Four Plus Years of College	0.807**	0.891**	1.073**	1.115**
	(0.052)	(0.106)	(0.003)	(0.004)
Constant	10.278**	9.563**	9.988**	9.467**
	(0.050)	(0.104)	(0.003)	(0.004)
Observations	4,277	3,946	998,784	872,593
R-squared	0.137	0.067	0.179	0.132

Standard errors in parentheses

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

Appendix Table 9:

How Do Returns to College Differ for

Men Versus Women in NH At Young Ages (22-30)?

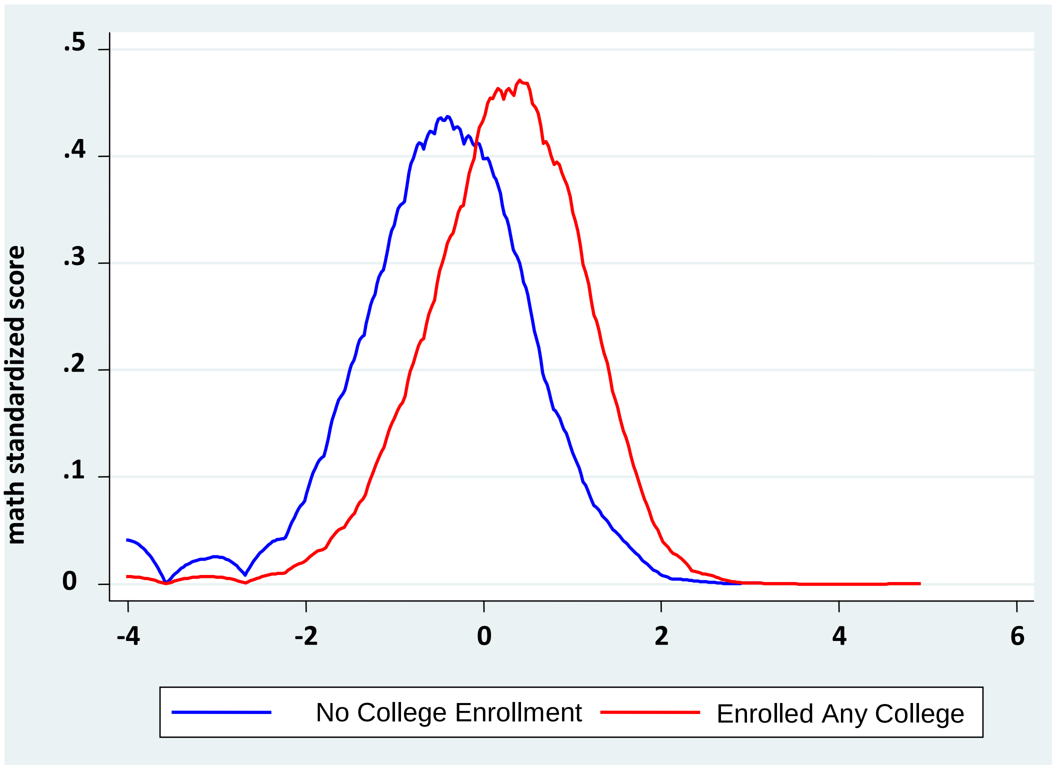
We use American Community Survey data from 2005-2010. We limit the sample to individuals ages 22-30. Income is measured as log of total personal income. Sample is not limited by labor force status, but results for just the employed (and also results for all of New England) are in an appendix. State (New 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)
Constant	9.580**	9.113**	9.438**	8.852**
	(0.069)	(0.094)	(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

Figure 1

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



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

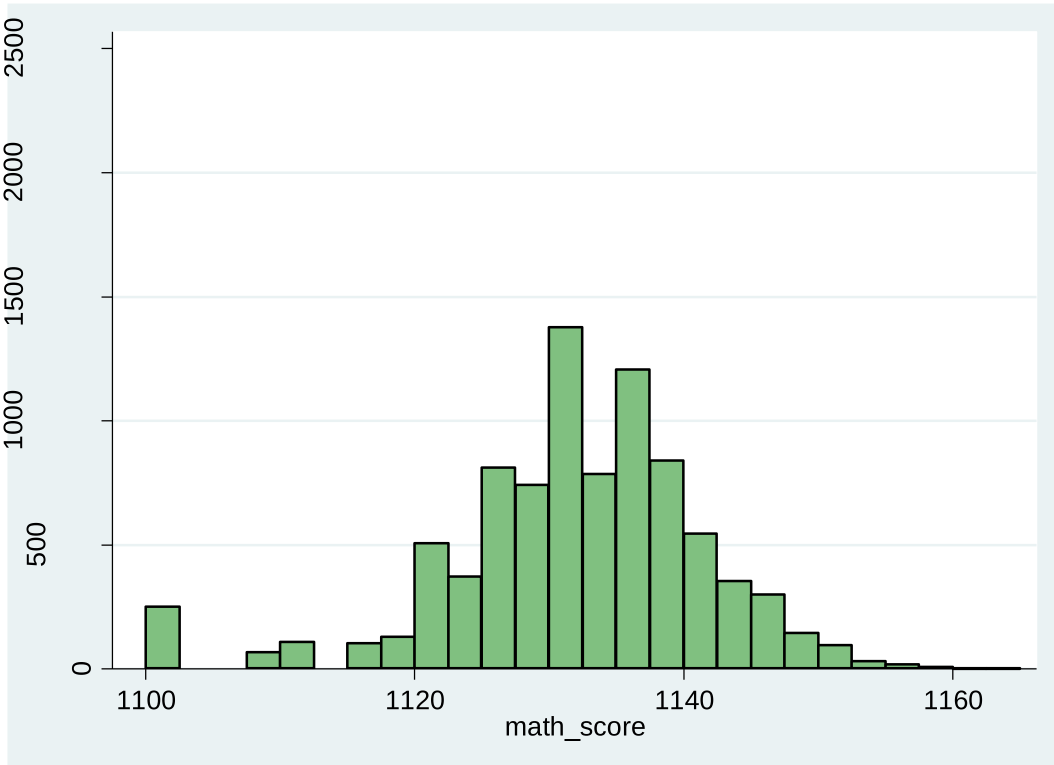


Figure 2

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

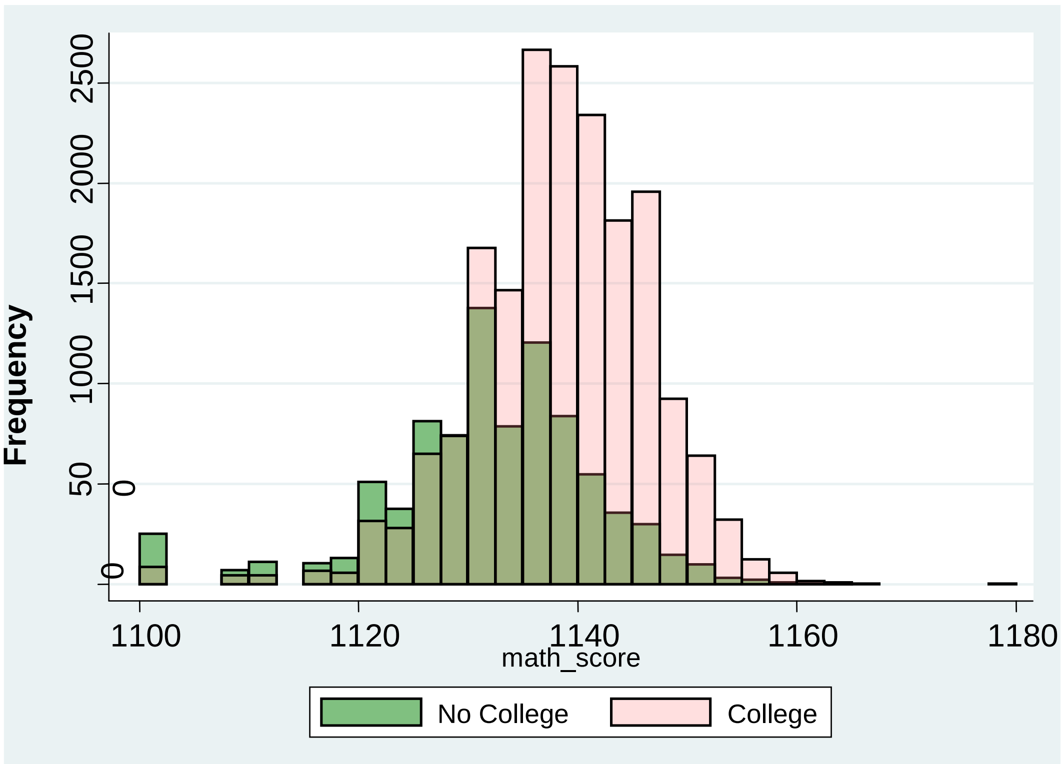


Figure 3:

Treatment and Control Standardized Reading Scores

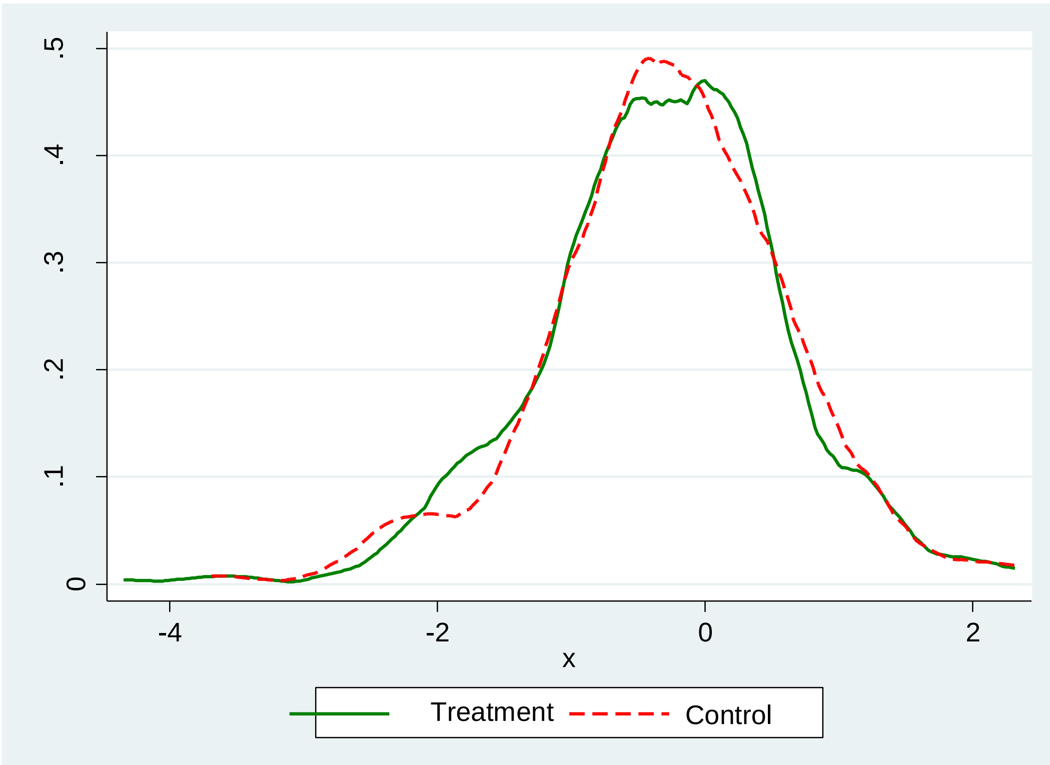


Figure 4: Treatment and Control Standardized Math Scores

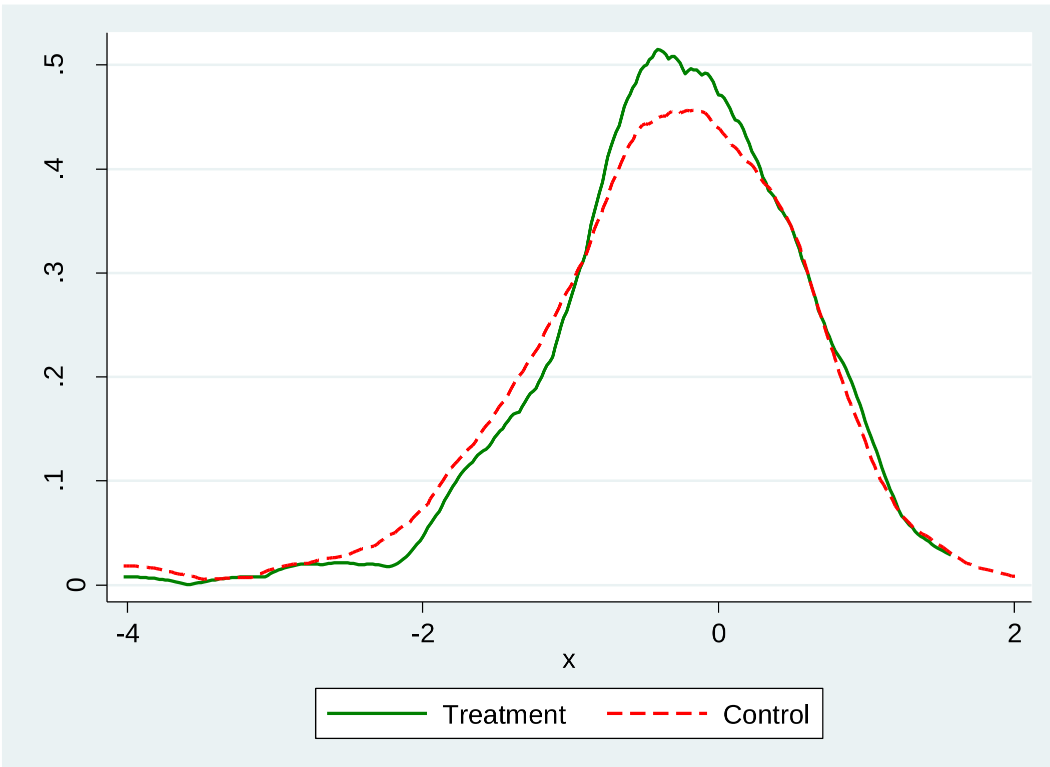


Figure 5

Standardized Math Scores Treatment Versus All Non Experimental

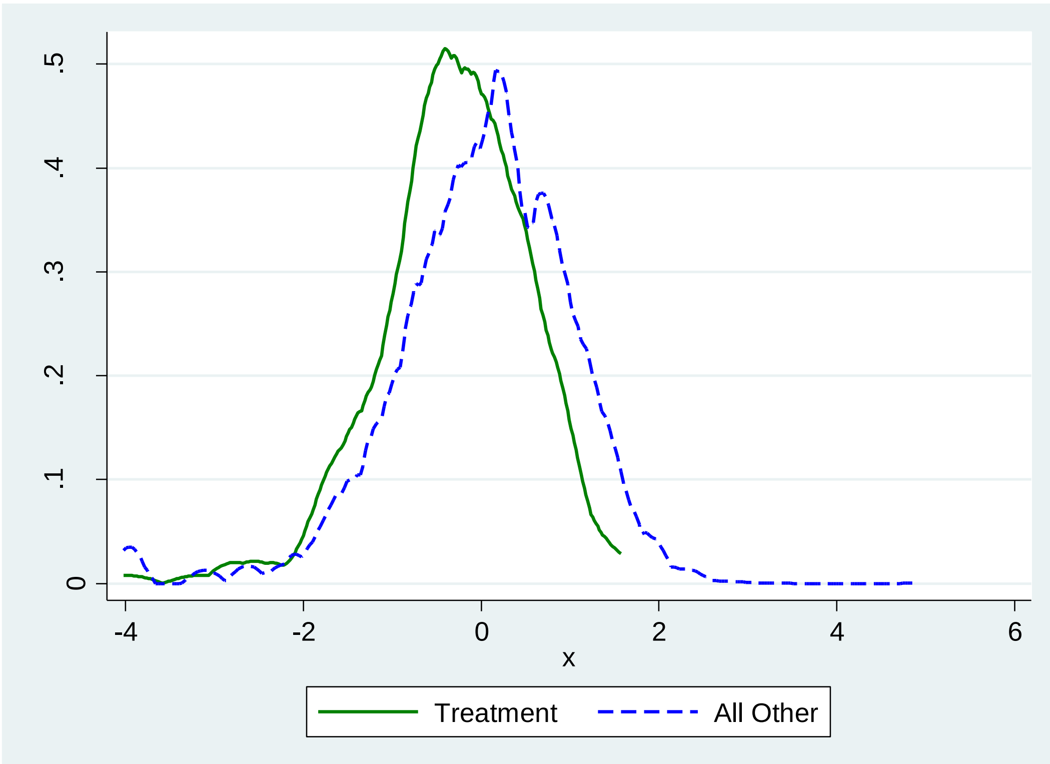


Figure 6

Average College Going Versus Effect Size For Women

We plot treatment effect size against the average college going rate in the high school cohort. The goal is to ask whether the treatment has a smaller impact in schools that already have a high college going rate.

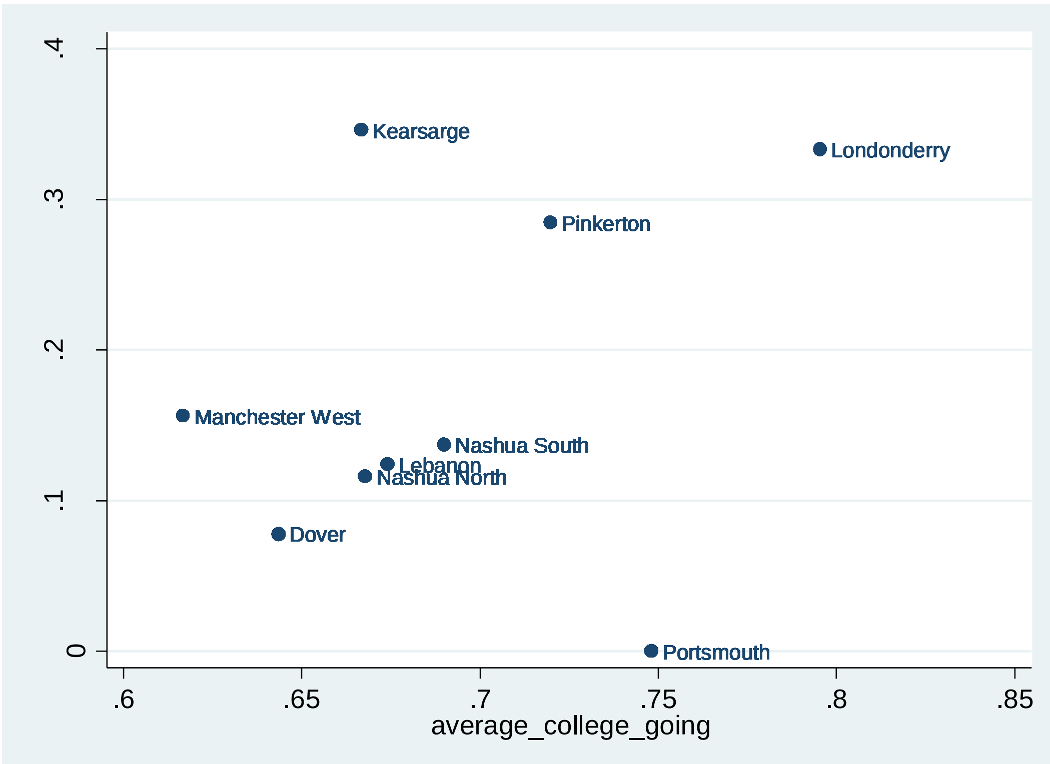


Figure 7

Percent Free and Reduced Lunch Versus Effect Size For Women