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

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Abstract

We present evidence from an ongoing field experiment in college coaching/ mentoring. The experiment is designed to ask whether coaching plus cash incentives provided to high school students late in their senior year have meaningful impacts on college going and persistence. For women and recent immigrants (male or female), we find large impacts on the decision to enroll in college and to remain in college. Intention to treat estimates are an increase in 12 percentage points in the college going rate (against a base rate of 50 percent) while treatment on the treated estimates are 24 percentage points. Offering cash bonuses alone without mentoring has no effect.

There are no effects for non-immigrant 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 can explain some or even all of the differences in treatment effects for men and women.

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 (Cascio, Clark & Gordon 2008). 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 high school students in the winter of their senior year. The high school guidance departments identify students who have expressed interest in college but

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not limited to Maureen O'Dea at Nashua North and South and Cindy Bilodeau and Patty Croteau at Manchester West High School.
[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.

have taken 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 (12 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 students from attending two year colleges to four year colleges and some students from no college to a two year or a four year college. We also have large impacts for students who are recent immigrants to the US. Since program take up is only about 50%, our treatment on the treated (instrumental variables) estimates of the programs' impact are twice as large i.e. 24 percentage points as measured against a base rate of college going of 50 percent in the control group.

There are no effects for native born 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, Bettinger, Long, Oreopoulos, and Sabonmatsu (2009) 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. 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 not applying, having expressed some interest in college, having made little or no progress in the application process and having a 10th grade test score that is at or above the 40th percentile for the state. We do not enforce the test score guideline and in fact at the time of nomination we do not have access to test score data anyway.

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 10th 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 does include 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 info to the high school 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 need 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 could 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 would 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 which was offered the cash bonus and only the cash bonus.

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, 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 career.

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

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. We have also run all of our analysis 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. (We can best look at persistence in the 2009 and 2010 cohorts since the 2011 cohort only has at most one year of college as of the latest data pull.) First we 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 two 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 2009-2011 cohorts. In those three cohorts we have data for 949 experimental students with 448 of those students being in the control group. We have fewer control students for two reasons. First, in the smallest (2009) cohort, two of the high schools asked for a 60-40 treatment versus control split. Second, we are missing data for a few control students in the 2009 and 2010 cohorts.[3]

Forty seven percent of the treatment students participated in the treatment. 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 10th 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 <u>standardized</u> math and reading scores are potentially misleading since the distributions are not normal and have very fat left hand tails. Relative to a normal

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[2] For more information on Clearinghouse data see http://www.studentclearinghouse.org/colleges/studenttracker/.
[3] This occurred because we did not collect the universal unique student id (called SASID) until the 2011 cohort.

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 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.

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.[4]

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 F statistics for the joint significance of all pre-treatment variables are 1.1 and 1.4 for men and women respectively.

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) $\text{Enroll}_i = \alpha + \beta 1 * \text{treat}_i + \gamma * \mathbf{X}_i + \rho * \mathbf{Z}_i + \varepsilon_i$

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[4] Again we do not have perfectly even numbers of treatment and control students in each cell ; including cell fixed effects is needed both in checking the randomization process and in calculating treatment effects.

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 clustering at the school*cohort level. In practice we control for age by including a full set of birth year*cohort dummies. This yields modestly 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 participate. (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 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 results 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. Column (1) show the treatment effect (from assigned to treatment group) for both genders combined and the effect is a statistically insignificant 3.8 percentage point increase in the college going rate.

However the effects look very different when we split the sample for men and women. There is no effect of assignment to the program on college going for men but a statistically significant 12.1 percentage points for women. This is against a control group mean college going rate of 52 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 .51.

The second stage regression for the women is in column (4). The treatment has an effect of 24 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 (52%).

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 now significant at the 6% level.[5] The intention to treat effect for women is again 12 percentage points and the treatment on the treated effect for women is 24 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 20 percent. In other words, for treated women, the program doubles their four year college going rate.

Since the program has the absolute sized effect on both "any college" and "four year college", that implies that the program should not be moving the rate at which treated men and women attend 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 lowers two year college for women by an insignificant .6 percentage points.

The program raises the overall four year college going rate for women but not the two year rate. This is does not imply that the program did not 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.[6]

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

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[5] The t statistic is 2.03 and just misses significance at the 5% level.
[6] 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.

scores with a statistically significant effect of 15 percentage points. For these women there is no impact on four year college going. The reverse is true for women with above median test scores. The latter group shows large impacts for four year college going (16 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.

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 and 2010 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 make use as the dependent variable a dummy for the student being enrolled in three or more semesters. The treatment effect is 12 percentage points which is similar to the effects we saw in Table 3 for "ever enrolled" for any length of time. 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 9 percentage points which is modestly lower than the estimate for "ever enrolled" in any college. However, when we examine effects on being enrolled in a four year college for both years post-high school graduation, the treatment effect is again 12 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 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.[7]

Clearinghouse data are not yet available for the 2012 cohort. The guidance departments generously provided their "future plans" lists for their graduating students which enabled us to construct dummies for planning to attend a two year college or a four year college. At the moment, we do not know how correlated the self reported plans are with enrollments as measured by the Clearinghouse. It's possible that the self reports are more accurate than Clearinghouse data as the there is often the possibility of an incorrect or failed match of NH data into Clearinghouse data.[8]

Results are shown in Table 6. Columns (1) and (2) repeat the results from Table 3 (cohorts 2009-2011) showing the program raised "any college" and "four year college" for women by 12 percentage points. Column (3) 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) actually rises to 18 percentage points. In other words the point estimate for the treatment effect is larger not smaller for the 2012 cohort, even though the comparison group 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. The one caveat to this regression is that it is not possible to also include the full set of cohort*high school fixed effects since the cash bonus is collinear with being in the control group for the 2012 cohort. For this regression only we constrained the high school fixed effects for 2011 and 2012 to be the same, i.e. we included a dummy for high

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[7] 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.

[8] The Clearinghouse performs the match and in the case of New Hampshire they are using name and birthdate for matching.

school times cohort>=2011.   In this stacked regression, we can see that the full treatment raises college going for women by 14 percentage points but the cash only treatment raises college going by a statistically insignificant 2.7 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. 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 students 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 students. However, the survey results combined with the statistical results suggest that the bonus at least by itself is not very 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 math or reading scores that are above the 75th percentile. We find little evidence that the program works better (or worse) for high scoring students. Column (1) is for the women in the sample and the dependent variable is enrollment in "any college." The

interaction between high reading score and the treatment is positive and statistically significant. However the coefficient loses significance when we switch to enrolled in a four year college. And the interactions between high math score and treatment are not significant for the women. Furthermore the interactions between treatment and high reading score are negative and not significant for the men.

One potential measure of disadvantage is recent immigrant status. Interestingly Manchester New Hampshire has a nontrivial number of recent immigrants or refugees. This is due in large part to the resettlement program that the US Department of State offers to refugees. In the case of New Hampshire, there is a non-profit agency called US Committee for Refugees and Immigrants which contracts with the Department of State to provide resettlement and support services to refugees. This particular agency places refugees almost exclusively in Manchester.

As a result, about 9 percent of our sample of students at Manchester West High School consists of non-native born students who have typically been in the US for less than four years. The nationalities of birth for these students run the gamut from Nepal to Vietnam to African countries. To examine whether these students experience larger or smaller treatment effects, we coded immigrant status for the students and we interact immigrant status with treatment status. At the moment we only have determined immigrant status for the Manchester West students and so in Table 8 we show results limiting the sample to two cohorts of the Manchester West students.

Immigrant status is a source of disadvantage in college going. For example, among the women in the sample, immigrants are 25 percent less likely to attend a four year college and 67 percent less likely to attend any college. However the treatment effect interacts positively with immigrant status. In column (1), the treatment fully compensates for the disadvantage that immigrant women face in enrollment at any college. Immigrant men also see large positive treatment effects. In column (2), the treatment effect for the native born men at Manchester West is 8 percentage points and the immigrants in the treatment group have an additional 13 percentage point effect.

The strong positive interaction between the treatment and immigrant/ refugee status, makes one wonder whether the treatment is also more effective for students who are potentially disadvantaged in other ways. In Table 9, we interact the treatment dummy with free and reduced lunch dummies, a dummy for mother's education is high school or less, and nonwhite[9]. We limit the sample to women since we only have (reliably) positive treatment effects for the women. There is no evidence that the treatment interacts with any of these other characteristics. The sample for whom we know mother's education is smaller because we rely on our own survey data (not administrative data) to create that measure.

Another 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

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[9] The level effect of nonwhite is actually positive and insignificant in our sample so we are likely not measuring a source of disadvantage with race dummies anyway.

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 that 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.[10]

In Table 10 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

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[10] 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.

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 11. We take the sample of all individuals in the American Community Survey during 2005-2010. We limit the sample to people ages 22-30. 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.

An interesting and potentially relevant fact emerges from these regressions. Measured in this way, women have higher average returns to college. And more importantly, young men in New Hampshire (or New England) see no monetary return to having 1-3 years of college. This is apparent in column (1) in which the coefficients for NH men for the high school dummy and the 1-3 years of college dummy are both 34 percent. In contrast, women in New Hampshire with 1-3 years of college earn 16 percent more per year than women with only a high school degree.

The same pattern holds true for the rest of the country but it is particularly striking in New Hampshire and New England (see Appendix Table 10). The low returns to "some college" for men and high returns for women strike as quite plausible. To give a 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 12 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 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 13 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), women appear to experience equally large treatment effects when assigned a mentor of either gender. And in column (3), men experience the same zero treatment effect with a mentor of either 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 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 plausible explanation is that returns to "some college" completed are much lower for men than for women. Data on average returns to some college for young men in the American Community Survey are zero for men but sixteen percentage points for women.

Overall there is evidence that college mentoring helps the most for students who may have less support available to them or are from schools with fewer resources. For example, we find very strong positive effects for women and men in refugee/ very recent immigrant families.

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. We find in fact 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.

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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.

Variable	Obs	Control Mean	Std. Dev	Obs	Treat Mean	Std. Dev
Accepted Treatment	448	0.000	0.105	501	0.469	0.500
10th Grade Math Score (Standardized)	382	-0.387	0.944	423	-0.301	0.835
10th Grade Reading Score (Standardized)	382	-0.280	0.914	419	-0.317	0.911
Math > 50th Percentile	382	0.552	0.498	423	0.565	0.496
Reading > 50th Percentile	382	0.380	0.486	419	0.396	0.490
Math >75th Percentile	382	0.196	0.398	423	0.184	0.388
Reading > 75th Percentile	382	0.225	0.418	419	0.208	0.406
Free and Reduced Lunch Eligible	448	0.239	0.427	501	0.253	0.435
Male	448	0.525	0.500	501	0.585	0.493
Non-white	448	0.152	0.359	501	0.174	0.379
Low SES	364	0.255	0.437	403	0.246	0.431
Graduation Year	448	2010.422	0.566	501	2010.441	0.558
Any College (Clearinghouse)	448	0.525	0.500	501	0.557	0.497
Four Year College (Clearinghouse)	448	0.225	0.418	501	0.263	0.441

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	0.072
	(0.026)	(0.045)
Standardized 10th Grade Reading Score	-0.046	-0.018
	(0.031)	(0.049)
Free Reduced Lunch Eligible	-0.087	0.099
	(0.070)	(0.109)
Student is Nonwhite	0.047	-0.020
	(0.064)	(0.063)
Constant	0.131	-0.101
	(0.366)	(0.075)
Observations	450	351
R-squared	0.085	0.086
F Pre-Treat Variables	1.118	1.446
p-value	0.387	0.271

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
Treatment	0.038	0.121**	-0.004		0.505**
	(0.031)	(0.036)	(0.042)		(0.056)
Accepted Treatment				0.239*	
				(0.086)	
Constant	0.388+	0.122**	-0.182**	0.358	0.070**
	(0.212)	(0.024)	(0.036)	(0.376)	(0.012)
Observations	949	421	528	421	421
R-squared	0.126	0.129	0.168	0.085	0.414

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
Treatment	0.051+	0.122*	-0.012	
	(0.025)	(0.048)	(0.051)	
Accepted Treatment				0.241*
				(0.092)
Constant	0.339+	0.097**	-0.104*	0.080**
	(0.173)	(0.023)	(0.038)	(0.019)
Observations	949	421	528	421
R-squared	0.101	0.112	0.161	0.103

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	-0.019	-0.006	0.001	
	(0.030)	(0.037)	(0.050)	
Accepted Treatment				-0.012
				(0.073)
Constant	-0.180	0.046*	-0.003	0.047*
	(0.139)	(0.019)	(0.035)	(0.016)
Observations	949	421	528	421
R-squared	0.073	0.095	0.109	0.097

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	0.119*	0.086+	0.123**	0.024
	(0.046)	(0.041)	(0.037)	(0.059)
Constant	-0.106**	0.032**	0.144**	1.318**
	(0.005)	(0.006)	(0.008)	(0.025)
Observations	234	234	234	134
R-squared	0.132	0.152	0.118	0.195

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	0.151+	0.055	0.024	0.161*
	(0.073)	(0.063)	(0.088)	(0.063)
Observations	165	256	165	256
R-squared	0.180	0.198	0.086	0.179

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 (3) are for the 2012 cohort in which the "control" group was offered a $100 bonus for completing applications. 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) **2012** Women Enrollment Any College	(4) **Stacked Data** Women: Enrollment Any College
Treatment	0.121**	0.122*	0.183*	0.141**
	(0.036)	(0.048)	(0.080)	(0.046)
$100 Cash Bonus Only				0.027
				(0.057)
Observations	421	421	113	534
R-squared	0.129	0.112	0.055	0.055

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	(3) Women: Enrolled in Any College	(4) Women: Enrolled in Four Year College	(5) Men: Enrolled in Any College	(6) Men: Enrolled in Four Year College
Treatment	0.053	0.089	0.101**	0.092	0.019	0.007
	(0.040)	(0.058)	(0.034)	(0.059)	(0.047)	(0.055)
Treatment * Reading > 75 Percentile	0.264*	0.177			-0.129	-0.016
	(0.108)	(0.148)			(0.078)	(0.094)
Reading Score Above 75th Percentile	0.142	0.130			0.222**	0.296**
	(0.089)	(0.097)			(0.047)	(0.059)
Treatment * Math > 75 Percentile			0.060	0.237		
			(0.170)	(0.149)		
Math Score Above 75th Percentile			0.129	0.105		
			(0.131)	(0.081)		
Constant	-1.348**	-0.713	-1.057**	-0.705	-0.220**	-0.197*
	(0.088)	(0.473)	(0.161)	(0.500)	(0.055)	(0.074)
Observations	351	351	352	352	450	450
R-squared	0.159	0.125	0.104	0.112	0.146	0.198

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

Table 8: Interaction of Treatment with Immigration Status

Data are from Manchester West 2010,2011 Cohorts. Sample is roughly 9% immigrants.

VARIABLES	(1) Women: Enrolled in Any College	(2) Men: Enrolled in Any Year College	(3) Women: Enrolled in Four Year College	(4) Men: Enrolled in Four Year College
Treatment	0.159	0.079+	0.264*	-0.009*
	(0.096)	(0.035)	(0.008)	(0.001)
Immigrant/ Refugee	-0.668**	0.010	-0.247**	-0.158**
	(0.071)	(0.350)	(0.040)	(0.050)
Immigrant*Treatment	0.649**	0.129*	-0.113	0.050**
	(0.218)	(0.057)	(0.138)	(0.012)
Observations	97	123	97	123
R-squared	0.206	0.198	0.183	0.044

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

Table 9

Does Treatment Interact With Other Sources of Disadvantage?

	(1) Enrolled in Four Year College	(2) Enrolled in Any College	(3) Enrolled in Four Year College	(4) Enrolled in Any College	(5) Enrolled in Four Year College
Treatment	0.259**	0.135**	0.137**	0.121**	0.119*
	(0.063)	(0.036)	(0.043)	(0.041)	(0.047)
Mother's Education Is High School Or Less	0.028				
	(0.106)				
Treatment * Mother's Education Is High School Or Less	-0.111				
	(0.137)				
Student is Nonwhite		0.039	0.062		
		(0.097)	(0.065)		
Treatment * Nonwhite		-0.090	-0.092		
		(0.152)	(0.109)		
Treatment * Free Lunch				-0.002	0.009
				(0.107)	(0.117)
Free Reduced Lunch Eligible				-0.076	-0.116
				(0.058)	(0.073)
Observations	170	421	421	421	421
R-squared	0.167	0.130	0.114	0.129	0.112

Table 10: 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 11: 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 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

Table 12: 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 13: 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	0.146*	0.109+	-0.007	0.096
	(0.065)	(0.061)	(0.084)	(0.067)
assigned_male_mentor	0.104	0.143	0.004	0.039
	(0.071)	(0.102)	(0.075)	(0.075)
assigned_treat_dont_show	0.145+	0.014	-0.058	-0.072+
	(0.073)	(0.050)	(0.056)	(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 14: Could We Estimate the Effects for Women Without a Randomized Control Group?

We take the cohorts of 2009, 2010, 2011. 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, 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	-0.126**	-0.079+		
	(0.035)	(0.046)		
Treated			-0.096**	-0.053
			(0.034)	(0.045)
Constant	-26.794**	-17.533	-27.482	-17.674**
	(0.744)	(146.363)	(92.985)	(0.667)
Observations	22407	22408	22345	22346
R-squared	0.216	0.159	0.216	0.159

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

Table 15: 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	0.072	-0.074	0.091	0.084
	(0.091)	(0.091)	(0.060)	(0.074)
Constant	0.299**	0.368*	-0.286**	0.878**
	(0.057)	(0.138)	(0.042)	(0.110)
Observations	267	267	293	334
R-squared	0.162	0.125	0.172	0.189

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	0.188	0.346	0.154	0.167	0.175*	0.029	0.107	0.203	-0.042
	(0.208)	(0.260)	(0.194)	(0.393)	(0.086)	(0.106)	(0.081)	(0.126)	(0.235)
Constant	0.430**	0.423+	0.584**	0.333	0.517**	0.671**	0.680**	0.296**	0.625**
	(0.152)	(0.220)	(0.139)	(0.321)	(0.093)	(0.103)	(0.079)	(0.106)	(0.182)
Observations	27	16	24	9	136	89	144	62	20
R-squared	0.035	0.128	0.072	0.025	0.037	0.038	0.045	0.054	0.002

Standard errors in parentheses

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

Appendix Table 2: Persistence in Treatment and Control Groups

Notes : This includes men and women (and does not control for highschool and cohort) which is why the overall effect on college going is only 3 percentage points)

Enrolled First 365 Days After Graduation	Enrolled Second 365 Days After Graduation		
	0	1	Total
0	103	6	109
1	62	93	155
Total	165	99	264

Persistence in Control Group

Enrolled First 365 Days After Graduation	Enrolled Second 365 Days After Graduation		
	0	1	Total
0	97	9	106
1	52	84	136
Total	149	93	242

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	213	0.014	0.118	208	0.510	0.501
10th Grade Math Score (Standardized)	176	-0.577	0.938	176	-0.382	0.828
10th Grade Reading Score (Standardized)	176	-0.220	0.936	175	-0.160	0.840
Math > 50th Percentile	176	0.477	0.501	176	0.523	0.501
Reading > 50th Percentile	176	0.364	0.482	175	0.434	0.497
Math >75th Percentile	176	0.131	0.338	176	0.148	0.356
Reading > 75th Percentile	176	0.239	0.427	175	0.229	0.421
Free and Reduced Lunch Eligible	213	0.244	0.431	208	0.293	0.456
Male	213	0.000	0.000	208	0.000	0.000
Non-white	213	0.164	0.371	208	0.168	0.375
Low SES	169	0.278	0.449	167	0.281	0.451
Graduation Year	213	2010.413	0.556	208	2010.399	0.573
Any College (Clearinghouse)	213	0.521	0.501	208	0.615	0.488
Four Year College (Clearinghouse)	213	0.202	0.402	208	0.308	0.463

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 22-30.

	(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	0.146*	0.263*	0.270**	0.369**
	(0.074)	(0.108)	(0.004)	(0.006)
One to Three Years of College	0.076	0.384**	0.301**	0.487**
	(0.077)	(0.108)	(0.004)	(0.006)
Four Plus Years of College	0.377**	0.648**	0.698**	0.959**
	(0.075)	(0.106)	(0.004)	(0.006)
Constant	9.974**	9.468**	9.721**	9.243**
	(0.070)	(0.103)	(0.003)	(0.005)
Observations	2525	2458	694309	643542
R-squared	0.025	0.045	0.060	0.094

Standard errors in parentheses

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

Appendix Table 9: Returns Table for NH Born Rather Than NH Residents

American Community Survey Data 2005-2010. Sample includes individuals ages 22-30.

	(1) Log Total Income Men New England	(2) Log Total Income Women New England
High School	0.265**	0.343**
	(0.085)	(0.104)
One to Three Years of College	0.248**	0.531**
	(0.088)	(0.104)
Four Plus Years of College	0.652**	0.950**
	(0.086)	(0.102)
Constant	9.645**	9.131**
	(0.079)	(0.097)
Observations	2699	2691
R-squared	0.039	0.077

Standard errors in parentheses

Appendix Table 10: Returns Table for All of New England Not Just NH

American Community Survey Data 2005-2010. Sample includes individuals ages 22-30.

	(1) Log Total Income Men New England	(2) Log Total Income Women New England
High School	0.412**	0.419**
	(0.024)	(0.029)
One to Three Years of College	0.390**	0.557**
	(0.025)	(0.029)
Four Plus Years of College	0.847**	1.043**
	(0.024)	(0.028)
Constant	9.432**	9.046**
	(0.022)	(0.027)
Observations	35245	36472
R-squared	0.053	0.081

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
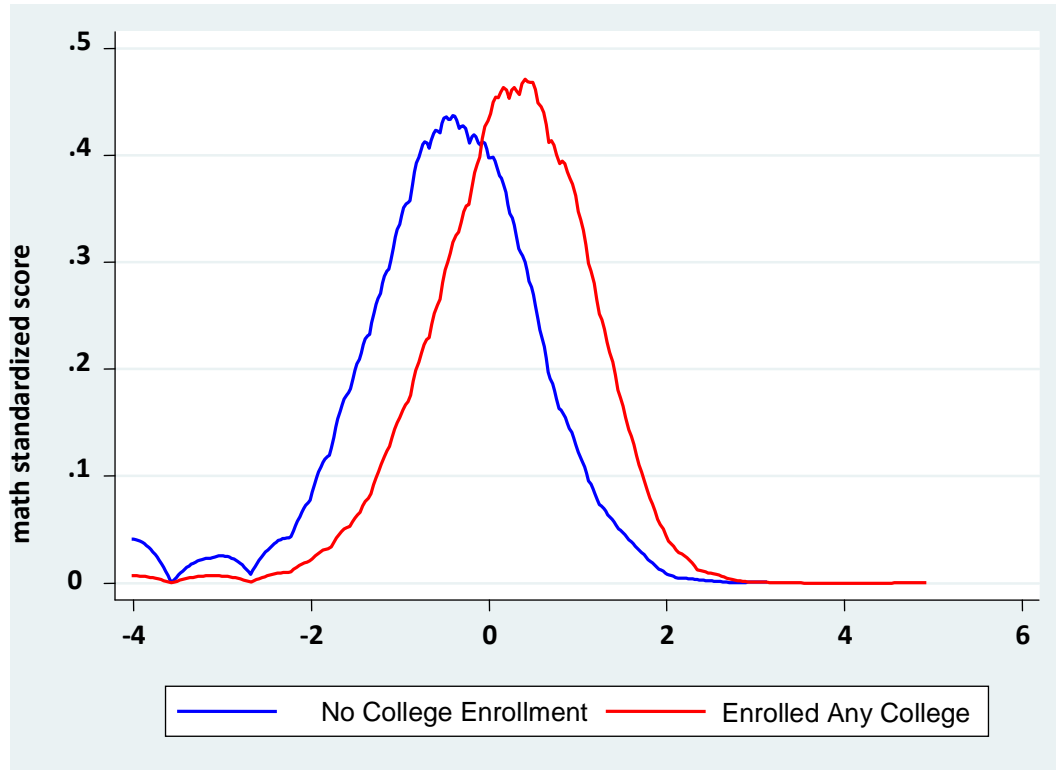
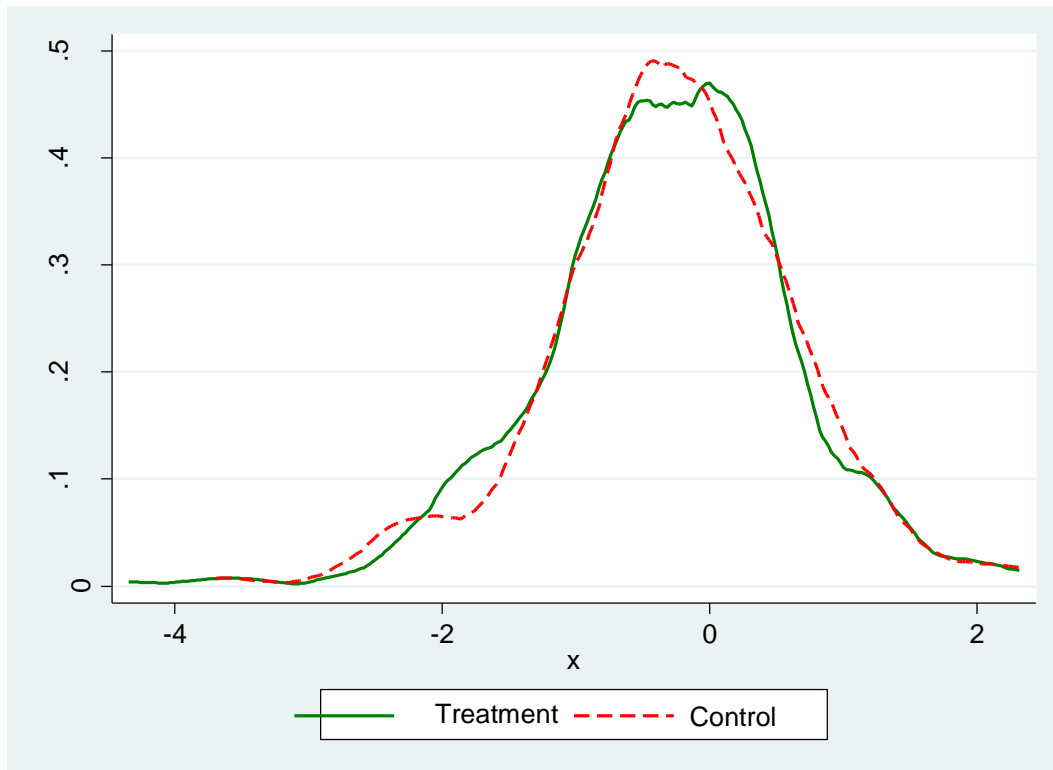**2010 Cohort: 10th Grade Math Scores for Non College Goers**
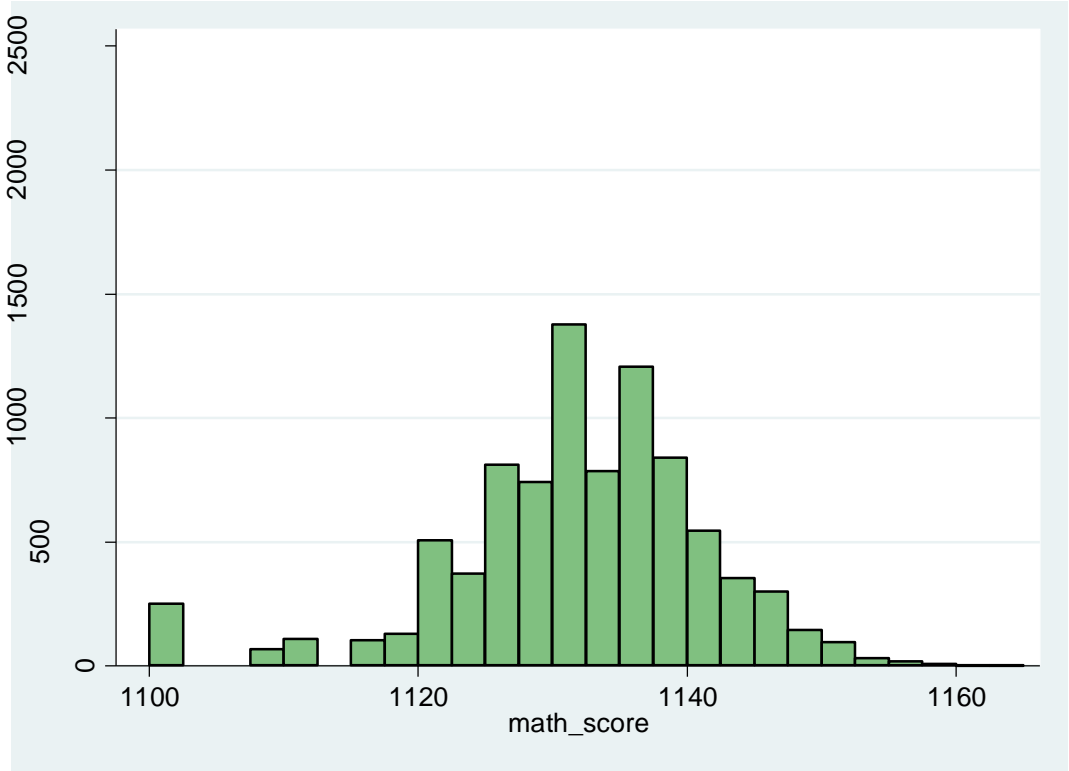
Figure 2

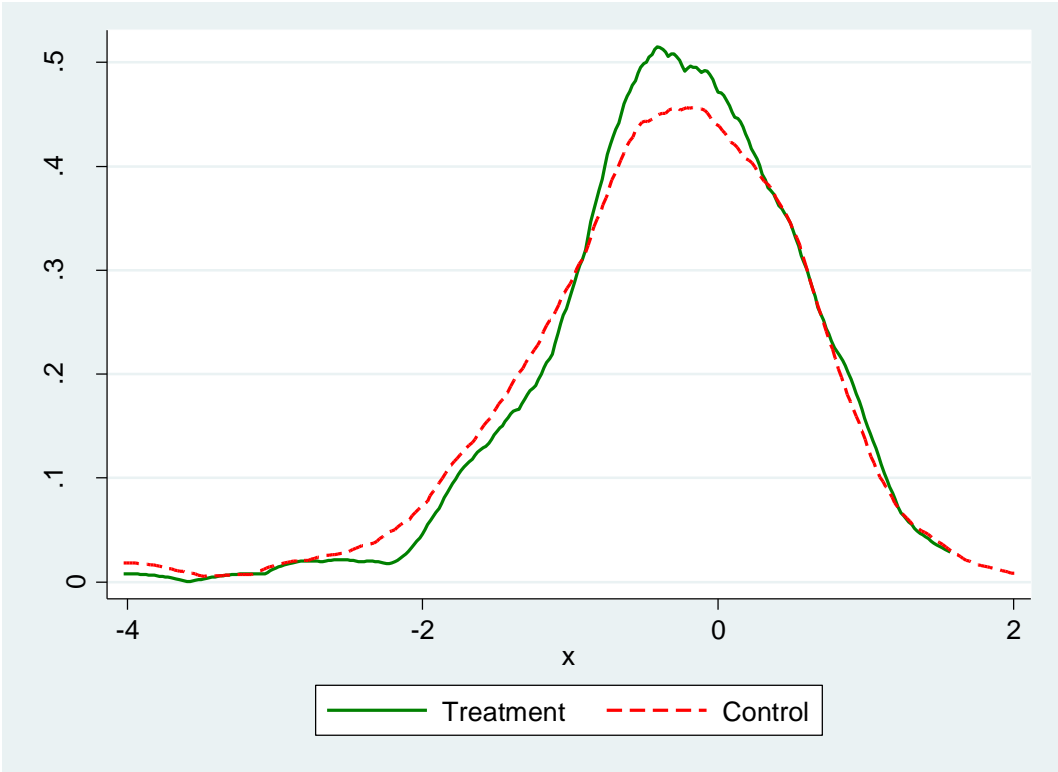**Frequency (count) Historgram. 2010 Cohort: 10th Grade Math Scores for College Goers and Non College Goers**
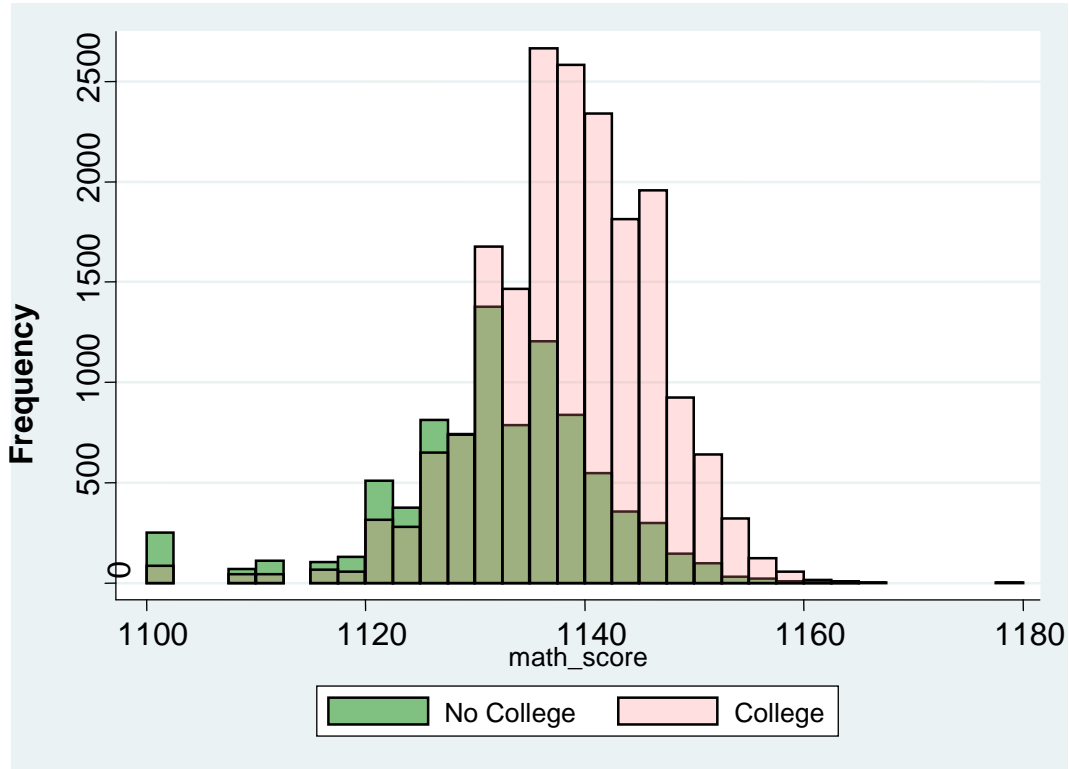
Figure 3:

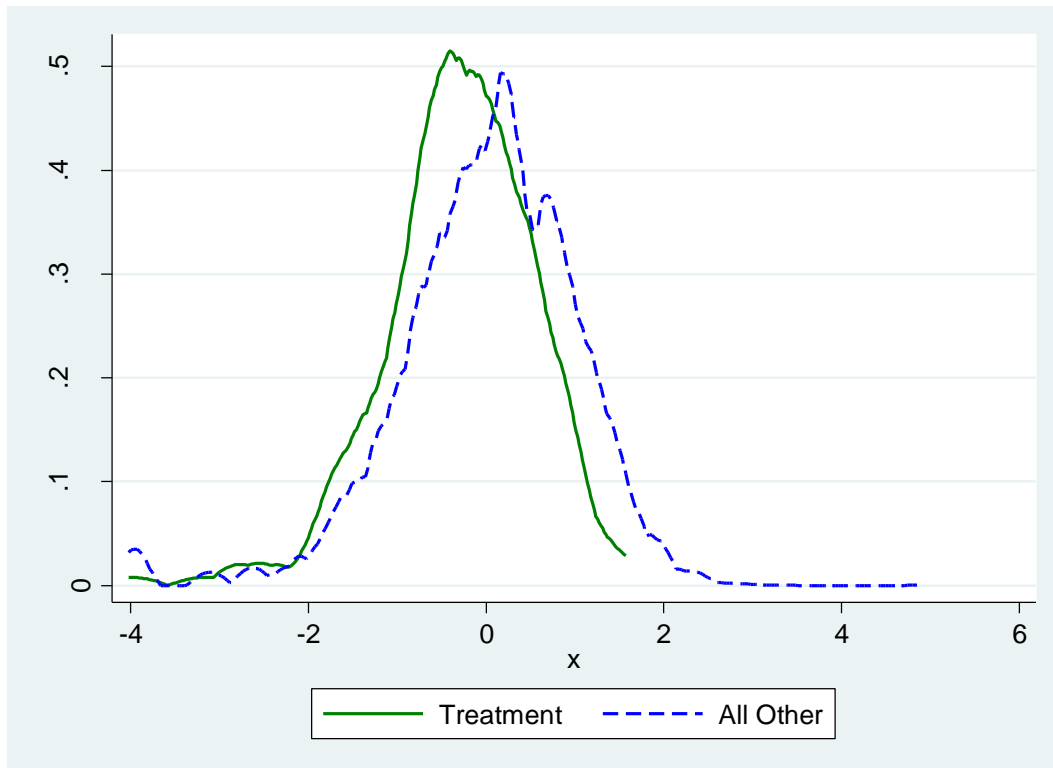**Treatment and Control Standardized Reading Scores**

**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.
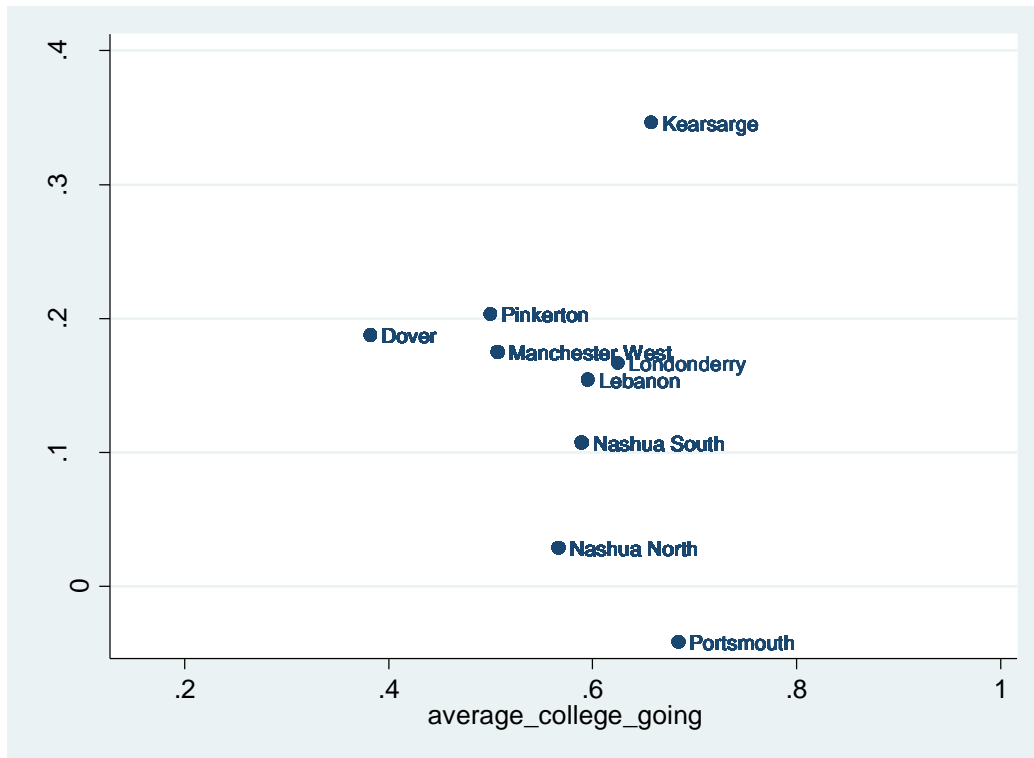
Figure 7

Percent Free and Reduced Lunch Versus Effect Size For Women