Achieving Academic Success after School:

A Randomized Evaluation of the Higher Achievement Program[1]

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Leigh L. Linden
(The University of Texas at Austin, BREAD, NBER, J-PAL, IZA)

Carla Herrera
(Independent Consultant)

Jean Baldwin Grossman
(Princeton University and MDRC)

Abstract:
We assess the potential of academic OST programs through a four-year RCT of an intensive, comprehensive program. The program improves reading comprehension and problem solving scores after two years—but only the latter persists four years later. The program increases matriculation at competitive private high schools and reduces it at academically non-competitive magnet and charter schools. These effects may result from the program's intensity, specific services, and interestingly a decline in academic attitudes. Conversely, we can rule out peer effects, stemming the summer learning loss, or general adult support as mechanisms.

JEL Codes: I21, I28, H52
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I. Introduction

The American educational system struggles with inequity. Ethnic minorities and students of low socioeconomic status underachieve relative to their peers (Stern, 1989). These children enter school at a deficit, only to fall further behind (Campbell, Hombo and Mazzeo, 2000). While socioeconomic factors may play a role, most schools serving disadvantaged youths are weak (Johnson and Stafford, 1973), despite significant evidence that better schools could boost performance (e.g. Neal, 1997; Krueger, 1999; Roderick, Jacob, and Bryk, 2004; Dobie and Fryer, 2011; and Tuttle et al., 2013)

Unfortunately, fixing these schools is difficult. Instead, academic out-of-school time (OST) programs seek to sidestep the problem through supplemental instruction. OST programs aim to improve students' academic performance through high-quality programming and educational opportunities that improve students' attitudes about academic achievement and encourage academically oriented behavior. Within this group of programs, researchers have identified several potentially important characteristics. First, programs should have a strong engaging academic curriculum that elicits consistent participation to provide youths with multi-year academic support. Programs should carefully combine both school-year (i.e., after-school) and summer learning opportunities to extend the amount of time youths devote to learning (Black et al., 2008; Mahoney and Zigler, 2006). Targeting students as they start middle school also allows programs to reach youths at a time when even strong students can experience academic challenges (Eccles and Midgley, 1989; Seidman et al., 1994; Blyth et al., 1983). Finally, OST programs that encourage application to and matriculation at competitive high schools may provide students with the promise of continued academic support and opportunity.

However, the strongest programs face limitations. First, unlike school, participation is voluntary. Only the most academically motivated may choose to participate—a fact that may prove particularly problematic for the most intensive programs. Second, while OST programs may try to coordinate with the local schools' curricula, the systems remain distinct. Compared to schools that follow a year-round calendar with extended school days, OST programs have limited ability to ensure continuity of instruction during and outside of school. Additionally, students come to these programs after attending school for a full day—they may simply be too tired to benefit from additional instruction.

Current research has not determined whether OST programs are a viable option for improving academic performance. The most rigorous evaluation of the impact of OST programs on test scores finds no effect after two years (James-Burdumy, Dynarski and Deke, 2007). However, the programs included in this evaluation had few of the characteristics described above. As a result, the study cannot speak to the limitations of the OST model–only the limitations of poorly designed OST programs (Mahoney and Zigler, 2006).

To test the limits of the OST model itself, we evaluate the long-term effects of a "Cadillac" voluntary OST program. If this program fails to improve students' academic performance, then OST programs in general may be incapable of doing so. Specifically, we conduct a four-year randomized controlled trial of the Higher Achievement (HA) program in Washington, DC. The sample includes 952 students recruited in three annual cohorts starting before the summer of 2006. We then conducted follow-up surveys one, two, and four years after baseline to evaluate the direct effect of the overall program, as well as two follow-ups conducted before and after the summer of 2010 to assess the effects over the summer.

We find that the program increases students' standardized test scores and changes high school matriculation patterns. For test scores, we find no impact in the first year, but improvements of 0.10 standard deviations in problem solving and 0.08 standard deviations in reading comprehension in the second. However, only the problem solving effect persists into the fourth year—a change of 0.11 standard deviations. Students attending HA experienced increases in problem solving scores of 0.19 standard deviations in the second year and 0.17 in the fourth.[2]

The program also expands students' high school options. While a significant fraction of the students would have availed themselves of academically competitive magnet and charter schools without HA, the program increases the probability that students apply to, are accepted at, and matriculate at competitive private schools by six percentage points (a 200 percent increase over the control group). Without assistance, these students would have matriculated at uncompetitive schools. Treatment students are 10 percentage points less likely to apply to and 7 percentage points less likely to attend academically weak magnet and charter schools (a 33 percent reduction). For those caused to attend HA, the effects are 9 percentage points for private schools and -10 for uncompetitive magnet and charter schools.

We also evaluate several possible mechanisms for these effects: participation in other OST programs, exposure to academically oriented activities, peer effects, adult relationships, non-cognitive skills related to their academic self-perceptions, and changes in performance over the summer. We find that the program's effects are likely related to the intensity and nature of the program and possibly a change in students' academic attitudes. Treatment students are more likely to have completed many activities related to the process of applying for high schools, including taking the test required to apply to private schools, applying for a scholarship, and

[2] No control students enrolled in HA and students in the treatment and control groups enrolled in academic OST programs other than HA at equal rates. As a result, the standard 2SLS LATE estimate using any involvement in an academic OST program in the first stage identifies the effect on students who attended HA.

receiving a scholarship.[3] We also find that treatment students spent much more time in formal extra-curricular instruction (377 hours more than controls in the first year and 327 hours in the second year), and are more likely to engage in a number of academic activities. Finally, rather than improving students' academic self-perceptions, the program significantly reduces them in the first year. This seems to be related to the students' transition into a more challenging academic environment. The effect disappears over the following year. While we find that the program does change students' friendship networks, we find no effect on students' perceptions of peer academic support—probably because students' new friendships are similar to those they would have made anyway. We also find no change in the level of general support students report receiving from adults. Finally, we find no change in students' test scores over the summer, suggesting that HA does not improve students' scores by stemming the typical decline many students experience over the summer.

Selection into the applicant pool may explain the ineffectiveness of some of these mechanisms. HA targets and attracts students who are motivated—who even without HA engage in many academically beneficial activities. For example, 29 percent of the control group enrolled in another academic OST programs within the first year. They also report frequent academically focused interactions with peers and adults. This could explain the program's lack of effect on peer and adult support, even though they are key components of the HA model. For example, HA is designed to generate peer effects by bringing similar students together. However, if the students in HA are similar to the students with whom treatment students would have become friends anyway, then the experience they would have with those friends would be the same. This would explain the lack of improvement in peer academic support despite the fact that treatment

[3] This may be consistent with evidence that some high achieving students fail to attend competitive colleges for similar reasons (Avery and Hoxby, 2012).

students more often report having met friends through HA. Finally, control students are also very likely to apply to and matriculate at academically competitive magnet and charter schools.

The mechanisms and applicant selection process suggest that voluntary OST programs like HA succeed not by focusing academically unfocused students. Instead, the programs provide disadvantaged but motivated families the experiences and resources necessary to achieve existing goals. This suggests that to improve their outcomes, these programs should recognize the uniqueness of their clients and focus on providing other services or resources that these families' lack. It also suggests that such academically-intensive, voluntary OST programs are unattractive to less motivated families, who, in fact, might benefit most.

Comparing HA with other interventions is difficult. First, very few studies estimate effects on high school matriculation. Many studies estimate effects on test scores, but few track students longer than a year or two; and those that do focus on different grade levels.[4] That said, the estimated effects of the HA program on test scores are larger than those of other rigorously evaluated voluntary OST programs. To date, there have been only two large-scale randomized controlled trials of OST programs. James-Burdumy, Dynarski and Deke (2007) find no academic effects of the 21st Century Community Learning Centers (CCLCs) after two years in a large-scale experimental and quasi-experimental evaluation. Black et al.'s (2008) RCT compared 21st CCLC programs that employed a well-delivered, research-based math and reading curriculum in lieu of homework help with programs that did not. They find that the treatment programs increase math

[4] Cascio and Staiger (2012), for example, demonstrate that widening test score distributions as youth age, complicates inter-grade comparisons both when comparing with other studies that focus on youth in different grades and when examining treatment effects over time.

scores by only 0.05 standard deviations after two years and decreased reading scores by 0.17 standard deviations.[5,6,7]

Using HA as an estimate for the potential of voluntary OST programs, these seem to be a viable option for improving scores in math, but not reading, when compared to other basic educational improvements. For example, the effects on math, particularly the LATE estimates, are similar to the effect of reducing class size after four years (Krueger, 1999). However, even for math scores, OST programs show less promise than programs like KIPP (Tuttle et al., 2013) or the Harlem Children's Zone Promise Academies (Dobie and Fryer, 2011) that provide more significant changes to the structure of schools.

The remainder of the manuscript is organized as follows: Section II provides an overview of the HA program. Section III describes the research design, and Section IV assesses the internal validity of the study. We present the results in Section V. Finally, we conclude in Section VI.

[5] There are a number of other RCT's in the literature. However, they suffer from various methodological problems. The vast majority have very small sample sizes—often under 100 students (Beckett et al., 2009). Chaplin and Capizzano (2006) estimate the effects of the Build Educated Leaders for Life (BELL) summer program on students' reading comprehension scores using a larger sample. However, while this study is built around a randomized controlled trial, the control group experienced more total days of school than the treatment group. Their preferred 0.08 standard deviations treatment effect estimate adjusts for the days of school received by each student. The unadjusted intent-to-treat estimates show no effect.

[6] Several meta-analyses have demonstrated larger effects on students' standardized test scores. While some of these estimates are comparable to the estimates in our study (Lauer et al., 2006, for example), these studies (a.) rely on non-experimental treatment effect estimates or experimental estimates based on very small samples for the larger estimates in their sample and (b.) are subject to the significant analytical problems associated with aggregating results across studies, including for example publication bias.

[7] An additional consideration is cost. HA costs $4,500 per student-year largely due to the program's intensity. Despite the program's high quality, HA costs only $7.45 per student-hour, equivalent to the costs of other similarly-sized OST programs (Grossman et al., 2009).

II. Intervention

The HA program is time-intensive. It includes 650 hours a year of academic instruction, enrichment activities and academically focused mentoring. The program runs year-round, including an after-school program during the academic year and a summer program. HA spans the middle school years, sixth through eighth grade. Students enter either before their fifth or sixth grade year and are expected to participate through the end of their eighth grade year.

During the school year, scholars attend the "After-School Academy" three days a week. This 25-week program runs from 3:30 to 8:00 p.m. and includes homework help, dinner, an elective, a 15-minute group meeting and two hours of academic instruction. Trained teachers and volunteers assist scholars during the homework help sessions, while adult mentors lead the instruction periods. Instruction groups include only two or five scholars.[8] Each day students focus on mathematics, literature, or technology. And mentors work with the same students for the entire year.

The six-week "Summer Academy" operates from 8:00 a.m. to 4:00 p.m., five days a week. Students take four classes from trained instructors a day in mathematics, science, social studies, literature, and two electives. They also take weekly field trips and participate in a three-day university visit. To expose them to college life, they attend classes, sleep in dorms, and eat in dining halls.

Based in DC and Alexandria, VA, HA follows a formal curriculum aligned with those in local public schools. Lessons follow a structured skill sequence, focusing on critical and analytical thinking skills. Much of the content used in class emphasizes social justice to empower students and imbue a sense of personal responsibility. HA also encourages scholars to

[8] At the start of the study, the mentoring groups comprised 4-5 scholars, but by the end of the study, HA had recruited enough mentors to reduce the groups to 2-3 scholars.

express themselves articulately through multiple media, such as poetry, essays, and public speaking.

HA aims to keep students motivated throughout middle school and then to place them into competitive, academically engaging high schools. To do this, families receive high school placement services. HA provides them with information about the quality of schools, and scholars visit high school campuses as part of the program. HA staff also helps with interviewing, selecting schools, and completing admissions and financial aid applications. These efforts peek in the eighth grade when one mentoring session each week in the fall is devoted to high school applications. More generally, by encouraging scholars to value and believe they are capable of academic success, HA hopes to lay an attitudinal foundation that will encourage students to seek out academic opportunities.

Child development theory and research support HA's program structure. HA serves youths during a turbulent time in their development. Many students experience declines in academic motivation, confidence and achievement during the transition from elementary to middle school (Anderman and Maehr, 1994; Seidman et al., 1994; Wigfield et al., 1991; Eccles and Midgley, 1989). Minority youths in particular may struggle with an increasing awareness of racial stereotypes (Simmons and Blythe, 1987).

Eccles and Midgley (1989) argue that middle school adversely affects students by failing to meet their developmental needs (also, Seidman et al., 1994). For example, young adolescents have a strong need for close relationships with adults and desire more autonomy and control over learning (Eccles and Wigfield, 2000; Eccles and Midgley, 1989). A number of studies show a strong correlation between the quality of students' relationships with instructors and academic attitudes and behaviors (e.g., Goodenow, 1993; Hamre and Pianta, 2001) and between active

learning styles and engagement (Stipek, 2002). However, students in most middle schools rotate between teachers rather than spending most of the day with a single teacher, and the pedagogy emphasize passive, rather than student directed, learning. As a result, students have less control over their classroom experiences and fewer opportunities to develop close relationships with teachers. In contrast, HA provides scholars with opportunities to develop close relationships with mentors and to learn in small-group interactive settings.

III. Research Methods

A. Experimental Design

We conducted an oversubscription randomized controlled trial. Each year HA had a fixed number of available positions. They then recruited more eligible youths than necessary, allowing us to randomly assign students to the available positions.

HA recruited students through local media advertisements, contacts at local elementary schools, and referrals from existing scholars. Interested families completed applications and met with HA staff for interviews to determine students' eligibility.[9] They then completed the baseline test and survey, and we randomly assigned them to either a treatment group offered admissions to HA or a control group that could not attend HA. We stratified by the HA center to which the student applied, grade, gender, whether the student applied with a sibling, and baseline problem solving score.[10] We also randomized the 10.3 percent of students that applied with a sibling as a

[9] The main requirement is interest in the program. Both the parents and students must be interested. And HA ensures that families understand the requirements of the summer and after-school programs and agree to participate through the end of the eighth grade.

[10] Given the time required for the publisher to score the standardized tests, our survey firm initially identified the number of correct answers for each test. We stratified by this initial score.

single unit so that both were either accepted or rejected.[11] Families could only apply once—controls could not reapply.

The final sample comprises 952 subjects recruited in three annual cohorts starting in 2006. The cohorts contain 277, 276, and 399 subjects. Given the ratio of the sample to the number of available positions, we assigned two-thirds of the first cohort to the treatment group, and half of the remaining cohorts.

Table 1 summarizes the data collection schedule. For each cohort, it indicates the timing of each survey and the students' grade level. For surveys conducted during the summer, we provide students grade level for the next academic year. All students completed follow-up surveys one, two, and four years after the baseline survey. We also investigate changes in outcomes during the summer of 2010 using data collected in the late spring and early fall of that year. We included all students still eligible to be enrolled in HA during that summer: all students in cohort three and those students entering HA as rising fifth graders in cohort two.

Follow-up and baseline surveys followed the same procedures. Subjects received a mailed invitation three to four weeks in advance of the first testing session. Subjects that failed to attend received additional notices about subsequent testing sessions by mail and phone. We held three to four sessions for each round from April to June.[12] Parents and students completed their surveys in separate rooms with students divided by grade.[13] Families received $120 for each follow-up survey.

[11] Siblings of children already attending HA were automatically admitted. We did not include them in the sample.
[12] A small number of families who did not attend any of the sessions completed the survey and test at home.
[13] None of the students in our sample repeated a grade.

B. Data

We use four sources of data: the application to HA, the standardized test, the parent survey, and the youth survey. The application was completed either online or in person at the HA interview. It included a range of demographic and socioeconomic information including the child's gender, grade, age, race, and whether the child qualified for free or reduced-price lunch at school. Parents also indicated their household income, their educational attainment, household composition, and the language spoken at home.

At each testing session, students completed the abbreviated versions of the Reading Comprehension and Problem Solving sections of the Tenth Edition of the Stanford Achievement Test. The publisher scored the tests and provided the normal curve equivalents, normed relative to the nationally representative test sample. In the analysis below, we rescale these scores to have a mean of zero and a standard deviation of one.

The parent surveys collected information about youth's participation in after-school and summer programs. In the fourth-year follow-up survey, we also asked parents about the high school application process.

Finally, the youth survey included two types of questions. We collected information on a number of non-cognitive skills related to academic self-perceptions and perceived peer and adult support. The individual instruments for these measures are described in Table 2. To contrast the services that students received, we asked students about their participation in a range of activities related to school and OST programs. These included, for example, questions about whether children received homework help outside of school or visited a college campus.

C. Sample Description

The first column of Table 3 provides a summary of key baseline and demographic characteristics for the control students. Just over half of the students entered the study as rising fifth graders. The remainder started before the sixth grade. About 60 percent are girls.

Demographically, the students look like most disadvantaged students in the DC area. Most are African American (76 percent). Thirteen percent are Latino. Two-thirds of students are eligible for free or reduced-price lunches. Thirty percent live with both parents. Twelve percent speak a language other than English at home. About half of the students (55 percent) have parents who either completed high school or some college.

However, our subjects perform well academically—just above the national average on both standardized tests. This is consistent with the findings below that the control students are more academically motivated than typical students with their socio-demographic characteristics. Thus, HA's voluntary admissions process may select an academically motivated subset of disadvantaged students, even without academic admissions requirements.

D. Statistical Models

We conduct the majority of the analysis using the following statistical model, estimated using ordinary least squares:

$$y_{ijk} = \beta + \tau Treat_j + \boldsymbol{\delta}' \boldsymbol{X_{ij}} + \boldsymbol{\theta}' \boldsymbol{Cohort_k} + \varepsilon_{ij}. \quad (1)$$

In this equation, y_{ijk} is the characteristic of interest for student i in family j and cohort k. The variable $Treat_j$ is an indicator variable assigned a value of one if we assigned the children in

family j to the treatment group. The variable τ is the treatment effect. The vector $\boldsymbol{Cohort_k}$ is a set of cohort fixed effects, and $\boldsymbol{X_{ij}}$ is a vector of control variables.[14] We cluster standard errors at the level of randomization–the family. We estimate equation (1) with and without controls. However, in all estimates, we include the cohort fixed effects to account for the differing proportions of students assigned to the treatment group.

We also estimate two other statistics. First, we estimate local average treatment effects (LATE) with two-stage least squares using an indicator for whether a student has ever participated in an academic OST program in the first stage.[15] Second, to account for the numerous outcome variables, we construct a composite index for each set of individual outcomes by estimating the demeaned variance weighted average for each of the individual variables.

IV. Quality of the Experiment

A. Internal Validity

Random assignment should ensure the orthogonality of treatment status and baseline characteristics. In columns one and two of Table 3, we check this by estimating differences in baseline characteristics using equation (1). We include no controls except cohort fixed effects. Column one presents the average characteristics of the control group, and column two presents the estimated differences. We find no statistically significant differences. In the last row, we

[14] Child-level controls include baseline scores for reading comprehension and problem solving, self-perceptions of academic abilities, industry and persistence, creativity, enjoyment of learning, curiosity, and ability to change the future through effort, peer academic support and general adult support, and indicator variables for grade at baseline, age, receipt of free or reduced-price lunch, race, household language, and whether the student applied with a sibling. Family-level controls include fixed effects for the center to which the family applied, parent's education, household income, household composition, and an indicator for whether English is the primary language spoken at home.

[15] We condition the measure on participation in an academic OST program during the period in which the student is eligible to attend HA. For students starting HA before their fifth-grade year, this includes any observed participation. For older students, this only includes participation before the fourth year of observation because students starting HA before the sixth grade cannot attend HA in their fourth year in the study.

present the results of a joint hypothesis test for all of the characteristics using seemingly unrelated regressions. The resulting p-value, 0.778, is not statistically significant.

Even if similar after the randomization, differential attrition could create differences at follow-up. To test this possibility, we compare the overall rates of attrition for each follow-up survey in Panel A. Overall, the fraction of students at follow-up in each round is consistently high–84, 81, and 76 percent in years one, two, and four respectively—though declining over time. For each year, however, the difference in the completion rates between the treatment and control groups is small, ranging from -0.01 to 0.03 percentage points. None are statistically significant.

Finally, despite similar rates of attrition, different types of students could attrit from the two research groups. In the remaining panels, we test for similarity in characteristics by replicating the estimates in columns 1 and 2 for the full sample and using only the students present at follow-up. The magnitudes of the estimated differences are remarkably similar to those at baseline. Of the fifteen differences, only one is statistically significant–whether the students qualify for free or reduced-price lunch. And the joint tests of all of the variables are also statistically insignificant.

B. Treatment Differential

We could not force students to attend HA. We could only offer them admission. Treatment students declining admission or dropping out early could significantly affect the interpretation of the estimated treatment effects. To assess participation, we estimate the effect of treatment assignment on program participation using equation (1), including only the cohort fixed effects. We present the estimates in Table 4.

Starting with Panel A, we estimate the difference in HA participation at the first-year follow-up survey.[16] We find in column one that while none of the control students attended HA, treatment students were 74.7 percentage points more likely to attend. This decreases to 69.7 percentage points after two years (Panel B) and for student entering HA in the fifth grade, 46.8 percentage points after four year (Panel C). These rates are significantly higher than those of typical OST program. For example, in a survey of 198 OST programs, Deschenes et al. (2010) find that average programs only retain 22 percent of middle-school-aged participants for a year.[17]

In column two we estimate the effect of treatment assignment on participation in other academic OST programs. Panel A shows that in the first year 29 percent of control students attended these other programs. However, treatment assignment did not change students' participation. While some initially try out HA, those treatment students who would have attended other programs if assigned to the control group seem to have enrolled in these programs anyway. The estimates for the second and fourth year are similar. Overall, assignment to the treatment group increases the probability of attending any academic OST program (including HA) by 53.9 percentage points in the first year and 46.8 percentage points after four years.

This pattern of enrollment in academic OST programs determines the interpretation of the Local Average Treatment Effect (LATE) estimates in which we use treatment assignment as an instrument for any participation in an academic OST program. Because treatment students are

[16] These participation rates are self-reported in the parent surveys. We were, however, able to obtain records from Higher Achievement for the summer of 2010 for cross validation (records were not retained for other periods). The records matched closely. Of the 444 subjects who completed our survey in fall 2010, 97.3 percent reported their summer participation consistently with the administrative records. Seven claimed to have attended HA without HA having a record of their participation, and five claimed not to have attended HA but were recorded as having attended by the program.

[17] While surveyed students participated at very high rates, these overestimate HA's participation rates because attriting students are less likely to participate in HA than those who complete the follow-up surveys. In the summer of 2010, we estimate the two-year retention rate for cohort 3 to be 47.7 percent using the administrative data from HA, and the three-year retention rate for cohort 2 to be 44.1 percent. However, while lower than the rate among non-attriting students, these rates are still high relative to typical OST programs.

equally likely to participate in these other programs and none of the control youths have access to HA, the students participating in other academic OST programs are "always-takers" within the Angrist, Imbens, and Rubin (1996) typology, those who would enroll in an academic OST program regardless of treatment assignment. Those students in the treatment group who fail to participate in any academic OST program constitute the "never-takers" who never attend an academic OST program regardless of treatment assignment. The LATE estimates then provide an estimate of the treatment effect for the "compliers" who are the students who attend HA. Thus, our LATE estimates, provide an estimate of the effect of attending HA relative to not participating in any academic OST program for those students who choose to attend HA.[18]

V. Outcomes

A. Test Scores

We estimate the effects of HA on students' scores in Table 5. Panels A, B and C contain the estimates for the first-, second-, and fourth-year tests respectively. Columns one through three present the intent-to-treat estimates (ITT) using equation (1) with no controls, student-level controls, and all controls. Starting with Panel A, we find no effects on problem solving or reading comprehension in the first year. The estimated effects with all controls are 0.03 and 0.02 standard deviations. Neither is statistically significant. In the second year, we do find effects of 0.10 and 0.08 (Panel B). These are statistically significant at the five- and ten-percent levels. Only the effect on problem solving, however, persists into the fourth year with an effect of 0.11,

[18] Since 87.1 percent of students in the treatment group enroll in HA in the first year, this interpretation also requires an additional assumption—that the time spent in HA before changing programs did not benefit students more than they would have benefitted had they enrolled in the other programs without ever having attended HA. If students do benefit more, the LATE estimate will over-estimate the effect of attending HA on those students who attend. HA is, in fact, more intensive than these other programs (results available upon request). However, switching students spent very little time in HA. For example, the 44 students who switched in the first year only spent an average of 2.5 months in HA.

statistically significant at the five-percent level. Although, the overall change in test scores is still statistically significant at the ten-percent level.

As we note in Section IV.B, the LATE estimate identifies the effect on the students enrolled in HA given the observed HA and alternate OST participation patterns. Column four provides these estimates. We find that attending HA increases problem solving and reading comprehension scores by 0.19 and 0.15 standard deviations respectively in the second year, but causes no increase in the first year. We also find that again, only the effect on problem solving persists into the fourth year with an effect of 0.17 standard deviations.

We also estimate the difference in the treatment effects for individual subgroups.[19] We estimated effects by baseline test scores, race, income, gender, entering grade level, ward, free or reduced-price lunch status, and HA center of application. We find no statistically significant differences in the impacts on any of these subgroups for any follow-up period.

B. High School Application Process

To assess the effect of HA on students' matriculation at competitive high schools we included detailed questions on high school application activities in the fourth-year parent follow-up survey. We present these results in Table 6. In the DC area, the options for academically competitive high schools include private, magnet, and charter schools. The first four columns of Table 6 provide information on students' applications to private schools while those in columns five through eight provide information on magnet and charter schools. We estimate effects of the probability that children apply to, are admitted to, and matriculate at each type of school as well as the LATE estimate for matriculation. The average for the control group is provided below the standard errors for reference.

[19] Results are available upon request.

Starting with column one, we find that HA has a 6 percentage point effect on the probability that students apply to private schools. This effect remains consistent through acceptance and matriculation. Students attending HA experience a 9 percentage point effect. In columns two, three and four, we subdivide this effect by type of private school. Elite schools are the extremely competitive schools such as Georgetown Day and Sidwell Friends. Competitive schools are those that are not as competitive as the elite schools but with strong academic reputations. The remaining schools are classified as non-competitive schools.[20] HA does not affect the probability that students attend elite schools, but the effect on private school matriculation is concentrated in the competitive, rather than non-competitive, schools.

Turning to the public high school options, the first thing to note is that while students in the control group are unlikely to apply to private schools, a large percentage of them do apply to charter and magnet schools. Overall, 68 percent of control students apply, and 56 percent eventually attend a magnet or charter school. Treatment students, however, are less likely to apply to them. They are 5 percent less likely to apply and 11 percent less likely to matriculate at magnet and charter schools. In columns six through eight, we further divide these schools into competitive magnet, competitive charter, and uncompetitive schools. Application and enrollment to the competitive public schools remains unchanged. Rather, HA significantly reduces the probability that students apply to and matriculate at uncompetitive schools. Treatment students overall are 7 percentage points (over fifty percent) less likely to matriculate at one of these schools, and students attending HA are 10 percentage points less likely. These estimates suggest

[20] We provide the list and classification of schools in Appendix Table A1. We classify magnet and charter schools using information provided by the DC public and charter school systems. Unfortunately, private schools do not provide similar information. We classified these schools based on our knowledge and with the assistance of HA staff. However, no information was available on four schools. These are counted as private schools, but they are not included in the tabulation of effects by type of private school.

that HA expands the types of schools available to its students and successfully steers students away from weak options.

While all of the possible mechanisms discussed in section V could explain these effects, there are mechanisms specific to the application process that could also contribute to these differences. We assess two possible antecedents—students' preferences and whether they take the required steps of the application process. Table 7 presents the control averages and estimated treatment effects on students' preferences grouped into characteristics of the school (Panel A) and influences from other students and adults (Panel B). Students responded to each question using a four-point Likert scale; higher scores indicate greater importance. For all characteristics, we find no statistically significant effect on students' preferences. Even the joint test is not statistically significant. It is important to note, however, that even without exposure to HA, control students register a strong preference for competitive schools. The average score for the importance of a schools' academic strength generally and in the students' area of interest are 3.6 and 3.4 respectively.

Table 8 provides the estimated treatment effects on activities related to the high school application process. For almost all activities, we find statistically significant differences. In Panel A, we find that students visited schools more often and were more likely to speak with students or teachers from a high school of interest. And in Panel B, we find that HA increased the probability that students took steps in the private school application process. Students were more likely to have attended a mock interview, prepared for and taken the admissions test, applied for a scholarship and received a scholarship. The magnitudes are consistent with the increase in private school matriculation.

These estimates suggest that HA increases matriculation at competitive schools, not by changing families' preferences, but by helping them achieve goals they already have. HA does not foment a desire for competitive schools. HA students want academically competitive schools, even without being exposed to HA–a fact evidenced by the high applications rates to magnet and charter schools in the control group. Instead, HA helps families apply to competitive private schools to which they would not otherwise apply and steers them away from weak public schools.

Finally, in results presented in Table A2, we find evidence that HA encourages students to take advantage of other sources of assistance. Across a range of items, treatment students report being more likely to receive assistance from an OST program but also from their teachers and other adults. For parents, the evidence is less definitive, but suggests more involvement as well. Treatment students more often report help from their parents to prepare for a test or to apply for financial aid, but the overall index is not statistically significant. However, when reporting whether the parents or child spent more time on the application, treatment parents are 8 percentage points more likely than controls to report that they spent the most time on the application. This result is statistically significant at the ten percent level.[21]

VI. Mechanisms

A. Educational Programming

Academic OST programs aim to improve student performance by providing supplementary educational experiences. In Tables 9 and 10, we compare the experiences of students, regardless of OST participation, to identify which may have been responsible for the observed treatment effects. In both tables, we present estimates for all students using data from the first- and second-

[21] This last estimate is available upon request.

year follow-up survey. However, for the fourth-year follow-up, we include only students entering the study prior to the fifth grade to restrict the sample to those still eligible to attend HA.

Starting with Table 9, HA significantly increased the amount of instructional time for students. We provide estimates for the number of additional days and hours per week spent in any academic OST program during the academic year (Panel A) and summer (Panel B). The estimates in the first and second year are close. We find an overall increase of 1.5 (year one) and 1.4 (year two) days of instruction per week during the academic year and 2.2 and 2.3 additional days during the summer. These are increases of 1.7 days and 1.3 days during the academic year and 0.8 and 1 days during the summer over the control group's average. This results in 10.3 and 8.5 additional hours of instruction each week during the academic year and 19.8 and 19.1 hours a week during the summer. The effects remain four years after baseline, but with slightly diminished magnitudes.

These estimates suggest that HA dramatically increased the amount of instruction received by students. Assuming the 25 weeks during the academic year and 6 weeks during the summer when HA operates, HA causes students to experience 376.5 additional hours in the first year and 326.8 in the second. Even in the fourth year when participation is the lowest, the number of additional hours is still significant–225.5 hours.

In Table 10, we show that HA also exposed students to a set of activities that they would not have otherwise experienced. Each variable indicates whether the student has ever completed the indicated activity. And with the exception of rows seven and eight which are specific to experiences in an OST program, students respond if they have experienced the activity in any context. Each year we find that treatment students participated more frequently than the control group in many of these activities. This is despite the fact that control students also participate at a

high rate in some activities. The aggregate measure (last row) is statistically significant at the one-percent level for each year and consistently indicates an overall increase in activities of about 0.18 to 0.26 standard deviations. Thus, in addition to increasing the total amount of academic instruction, HA also seems to expose students to a range of academic activities.

B. Peer Effects

Peer effects may also contribute to the observed treatment effects. Putting treatment students in closer contact with similarly motivated students may provide a more academically supportive peer group. We assess this in Table 11. Overall, HA did change students' peer groups—they reported being more likely to be friends with other students attending their OST program. These friends, however, seem to be similar to the friends students would have made anyway. Treatment students reported no differences in the types of academic interactions they have with them, and using a scale directly measuring how academically supportive students perceive their peers to be, we find no differences.

Panel A presents the data on students' friendship networks to assess whether HA changes the peers with whom students interact. We asked students to list up to five "closest" friends and to indicate where they met each friend. Students report an average of about 4.5 friends, and except in the first year, students in both research groups report the same number of friends. The sources, however, differ. Treatment students are less likely to report meeting friends through school and more likely to report having meeting them at their OST programs or through their family. Treatment students are also more likely to list HA specifically.[22]

[22] We provided students with three options to indicate where they met their friend. Students could select school, family (e.g., cousin or sibling), or another place. In the latter case, students were asked to specify the location. An OST program or HA were self-reported answers tabulated from the last category. For HA, we checked students'

We assess the academic nature of these relationships in Panel B. We asked students whether they had ever engaged in the indicated academic activities with their friends. Consistent with previous results, the control students frequently engaged in many activities. The rates for all activities are over 80 percent, except for talking about math or science outside of school. Thus, the youths with whom treatment students would have made friends without HA are also academically engaged, and in fact, students in the treatment group are no more likely to engage in these activities with their friends than students in the control group. Only one of the treatment effects is statistically significant (i.e., talked with other kids about a math or science problem outside of school in year four), and the effect on the overall index of activities is small and statistically insignificant except for year four.

Finally, in Panel C, we report the effects on a scale of students' perceptions of peer academic support using the scale describe in Table 2. As in Panel B, we find that HA has no effect. HA changed the peers with whom students made friends, but these new peers end up being just as academically motivated as those with whom students would have chosen without HA.

C. Adult Support

Like peers, HA may cause more academically supportive interactions between students and adults that result in students feeling more general support from adults. We test this hypothesis in Table 12. Panel A reports estimates on whether students ever engaged in the indicated academic activity with an adult. Like Panel B of Table 11, we find that control students have had a number of these experiences. More than half of the controls had experienced each of these interactions

responses by matching the names of listed friends to the students in our sample. And we find similar treatment effects on an indicator of whether a listed friend is in the treatment group.

and for seven of the twelve items more than 70 percent had. However, we still find that treatment students are about 10 to 20 percentage points more likely to engage in each of them. Almost all differences are significant at the five- or one-percent levels, and the composite index for each year is significant at the one-percent level. However, this increased engagement with adults does not translate into more general perceptions of adult support—such as paying attention to the student, caring about what happens to him or her, or being available to help with problems. In Panel B, we test for differences on the scale of adult support in Table 2. For all years, the differences are small and statistically insignificant. Given the significant number of academic interactions students have with adults absent HA, students may already feel supported. The additional interactions may help by providing additional academic inputs, but given the level of interactions they have anyway, students may not perceive the adults in their lives as more generally supportive.

D. Academic Self-Perceptions

Recent attention has focused on non-cognitive skills. While these are themselves important outcomes, some may also be mechanisms for improved test scores. For example, additional confidence in their abilities may make students more willing to grapple with academic material. As described in Table 2, we measure students' self-perceptions along six dimensions: industry and persistence, creativity, academic abilities, enjoyment of learning, curiosity, and ability to change the future through effort. We present the effects of HA on these outcomes in Table 13. We estimate the effects for each outcome and the composite index for each survey round using equation (1) with (first column) and without (second column) controls.

Rather than improving students' perceptions, the program seems to have instead made students feel less confident in the first year. Except for the ability to change the future through effort, all of the point estimates are negative and greater than a tenth of a standard deviation. On the aggregate index, students' perceptions fall by a tenth of a standard deviation both with and without controls—a difference that is statistically significant at the five-percent level in the preferred specification. After the first year, however, the effect disappears. Except for self-perceptions of academic abilities in the second year, all of the estimates are small and statistically insignificant. The effects on the index are similarly small and insignificant.

The negative treatment effect in the first year is driven by the students entering the lottery as rising fifth graders.[23] To examine the dynamics, Figure 1 plots the overall index for each survey period by grade and research group. To measure changes over time, we normalize relative to the baseline control distribution, rather than the contemporaneous control distribution. The short dashed lines depict rising fifth graders and the long-dashed lines depict rising sixth graders. Dots indicate the treatment group. The experiences of the treatment fifth graders are consistent with those of the treatment sixth graders—both fell over time. The counter-factual experiences, however, differ dramatically by grade in the first year of the study. Control sixth graders experienced the same decline in outcomes that most students experience upon entering middle school (Eccles and Midgley, 1989). Fifth-grade control students, however, experienced improvements in their perceptions during the last year of elementary school. They then decline sharply in their first year of middle school. By placing fifth-grade students in a more competitive environment, the HA program may have caused the middle-school decline a year before these

[23] These results are presented in Table A3 in the appendix.

students would have otherwise experienced it. The negative treatment effects may simply have been the consequence of entering a rigorous academic program.[24]

E. Summer Learning

As described in Section II, preventing the summer decline in test scores is a common goal of OST programs, particularly those with summer programming. To evaluate the effects of HA over a summer, we surveyed those students who were eligible for the HA summer program in the summer of 2010 at the end of the spring and the beginning of the fall. This included all students in cohort 3 and those in cohort 2 who applied as rising fifth graders.[25] The results are presented in Table 14. We evaluate the effects of the program on students' test scores (Panel A) and high school preferences (Panel B). We present treatment-control differences measured in the spring in columns one and two. Columns three and four present differences measured in the fall. And columns five through six present the relative changes in scores from spring to fall. Specifically, column six presents the impact estimates on the change in scores over the summer.[26]

Starting with test scores in Panel A, we find that as of the spring, treatment students (who had had the opportunity to participate in HA for two to three years) were already experiencing statistically significant treatment effects of 0.13 standard deviations on the problem solving test and 0.16 standard deviations on the reading comprehension test. By the fall, treatment students were still outscoring controls, on average, but the impact estimates were no longer significant.

[24] We also assess the effects of the program on students' self-reported behavior. At each follow-up, we asked students if they had engaged in a range of negative behaviors including, for example, stealing, breaking something on purpose, being tardy, going to the principal's office, etc. Similar to the changes in academic attitudes we find that students in the treatment group report worse behavior in the first year, and then increasingly similar levels in the second and fourth years. Similar to the changes in attitudes, students may have "acted out" due the stress of a more competitive environment. However, because HA encourages personal responsibility and honesty, these differences could also reflect a greater willingness to report negative information.

[25] Students are eligible until they start the eighth grade.

[26] We have also conducted estimates like those in column four that include the fall score as a control. The estimates are similar to those presented in column six.

These patterns result in no significant difference in how skill levels changed over the summer (columns five and six). Reading comprehension in both groups improved, while their math problem solving skills remained constant. Interestingly, neither group experienced summer learning losses.[27]

Youths' high school preferences in Panel B, however, do show a marked difference between the two groups. In the spring, 11 percentage points more students in the treatment group express a desire to attend a competitive area high school compared to the control group. In the fall, this increases to 16 percentage points; and ten percentage points fewer students express a desire to attend their local public school. In the differences-in-differences specifications, we find that the treatment effect on the changes in preferences are of similar magnitudes and statistically significant at the one-percent level.

VII. Conclusion

There are serious doubts about whether OST programs can improve the academic performance of disadvantaged students. This study answers this question by examining the effect of an academically rigorous, year-round, voluntary OST program using a randomized controlled trial. We demonstrate that such programs can provide long-term improvements in students' math scores. We find no improvement one year into the study, but treatment students perform 0.10 standard deviations better after two years and 0.11 standard deviations better after four. For those students attending HA, we find two- and four-year effects of 0.19 and 0.17 standard deviations respectively. We find no long-term effects on reading scores, but treatment students do perform 0.08 standard deviations better after two years before losing the gains.

[27] In Table A4 in the appendix, we estimate the same statistics for academic attitudes and peer and adult support. We also observe no change in any of these measures during the summer.

We also find that these programs can expand and inform students' choice of high schools. Treatment students are 6 percentage points more likely to attend competitive private schools and 7 percentage points less likely to attend academically weak charter and magnet schools. For students who actually attended HA, the estimates are 9 and 10 percentage points respectively.

These improvements appear to be due to the significant additional instruction time and unique experiences the program affords students, rather than peer effects, perceptions of general adult support, or altered preferences and beliefs. For example, we find that control students value the same types of high schools as treatment students. However, treatment students report engaging more frequently in high school application activities, including activities required to attend a private school such as taking the entrance exam and applying for scholarships. While HA does change students' friendship networks, the networks are not more academically supportive than they would otherwise be, and while HA increases the number of academic interactions students have with adults, students do not perceive adults to be more supportive in general. We also find no long-term improvements in students' academic self-perceptions, although the program worsens self-perceptions in the first year.

While the treatment effects on test scores are comparable to some other interventions and higher than other rigorously evaluated OST programs, they are much lower than those observed from more intensive programs such as the Harlem Children's Zone Promise Academies (Dobie and Fryer, 2011) and KIPP schools (Tuttle et al., 2013) whose longer school day effectively combines the regular school day and after-school instruction. When one includes both time at school and at HA, the total amount of instructional time received by students in HA is similar to that of these other programs. However, there are two important differences. First, these two programs are much more integrated than HA. The overall curriculum, pedagogical strategy, and

other services that students receive during the school day are much more closely coordinated with out-of-school activities. This suggests that such coordination may be an important component of the production function for test scores.

Second, programs like KIPP and the Promise Academies provide services to a much wider variety of students. For HA, the lack of effects observed for many of the mediating outcomes may be due to the types of students attracted by such intensive voluntary OST programs. They are much more academically engaged than typical disadvantaged students. It may be that there are important differences in the education production process for different types of students—intensive educational experiences may have a significantly larger effect on less academically motivated students.[28]

Finally, many researchers predict that improved self-perceptions are an important antecedent to changes in test scores. However, we observe improvements in test scores following an initial deterioration in the student's academic self-perceptions. This raises the question of whether improved self-perceptions are, in fact, a good predictor of later academic gains, and whether initial declines in these perceptions might contribute to high test scores. Students may, for example, be initially shocked upon entering such an academically intensive environment, but later improve as they realize that they are more capable than they had initially thought.

[28] This would be consistent, for example, with Roderick, Jacob, and Bryk (2004) who find that Chicago's Summer Bridge program increased the math and reading scores of students required to attend at the end of the sixth grade by 0.30 to 0.44 standard deviations.

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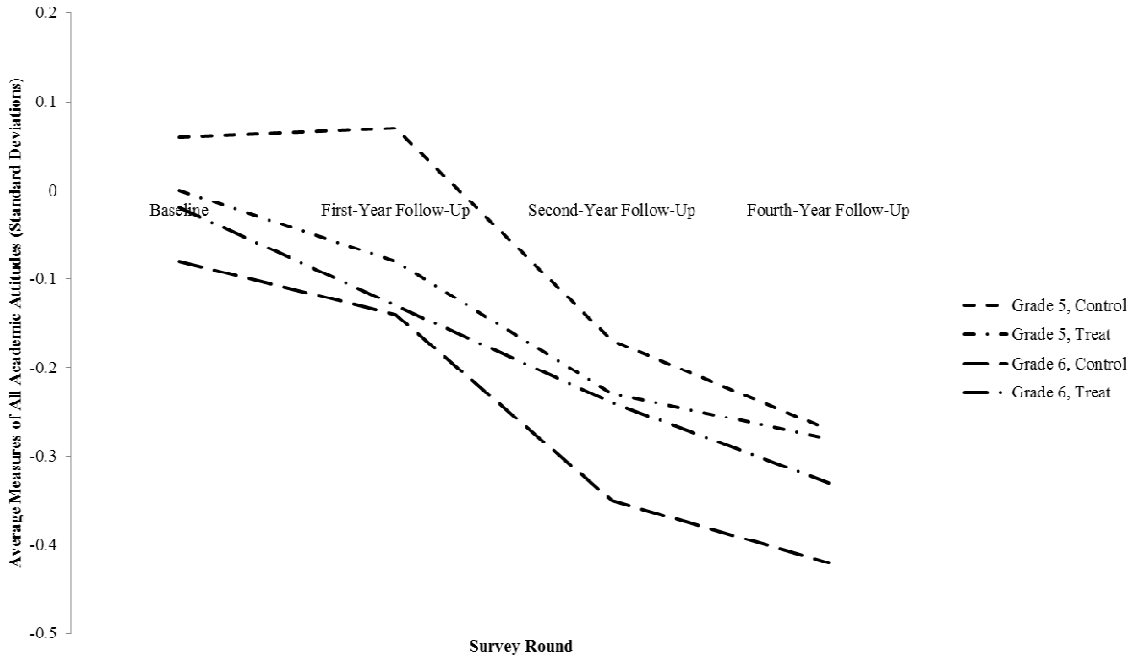
Figure 1: Average Academic Attitudes by Survey Round



Note: This figure presents the overall academic attitudes index by entering grade and treatment assignment for each survey period.

Table 1: Schedule of Survey Activities

	2006 Spring	2007 Spring	2008 Spring	2009 Spring	2010 Spring	2010 Fall	2011 Spring	2012 Spring
Cohort 1 (N=277)								
Survey Round	Baseline	FU1	FU2		FU4			
Grade Entering	5th/6th	6th/7th	7th/8th		9th/10th			
Cohort 2 (N=276)								
Survey Round		Baseline	FU1	FU2	FUSp	FUFa	FU4	
Grade Entering		5th/6th	6th/7th	7th/8th	8th	8th	9th/10th	
Cohort 3 (N=399)								
Survey Round			Baseline	FU1	Fu2/FuSp	FUFa		FU4
Grade Entering			5th/6th	6th/7th	7th/8th	7th/8th		9th/10th

Note: This table provides an overview of the schedule of surveys for each cohort. Students' grade level at the time of the survey is provided below the survey name.

Table 2: Non-Cognitive Skills

Outcome Measures	Source	Description
Panel A: Perceived Support		
General Adult Support	Adapted from Gambone and Arbreton, 1997	Number of adults who assist with personal problems, care about student, listen, etc.
Academically Supportive Friends	Midgley et al., 2000	Whether student's friends try to do well in school and support his or her academic efforts
Panel B: Academic Self-Perceptions		
Industry and Persistence	Park and Peterson, 2006	Student's dilligence, dependability, and willingness to work hard
Creativity	Park and Peterson, 2006	Student's ability to generate ideas and solutions
Enjoyment of Learning	Park and Peterson, 2006	Extent to which student likes to learn new things
Curiosity	Park and Peterson, 2006	Extent to which student wants to know more about things and is willing to ask questions
Ability to Change the Future through Effort	Institute for Research and Reform in Education, 1998	Extent to which the student believes his/her own effort can improve his/her academic success
Self-Perceptions of Academic Abilities	Adapted from Harter, 1985	Student's perceptions of how well he or she is doing academically

Note: This table presents a description of the non-cognitive skill measures.

Table 3: Internal Validity

	Baseline		Year 1		Year 2		Year 4	
	Control Average (1)	Treat-Control (2)	Control Average (3)	Treat-Control (4)	Control Average (5)	Treat-Control (6)	Control Average (7)	Treat-Control (8)
Panel A: Attrition Rates								
Completed Survey			0.84	0.03	0.81	0.01	0.76	-0.01
				(0.02)		(0.03)		(0.03)
Panel B: Demographic Characteristics								
Female	0.59	<0.01	0.58	0.02	0.59	0.02	0.60	0.01
		(0.03)		(0.04)		(0.04)		(0.04)
Grade	5.42	<0.01	5.41	0.01	5.41	0.01	5.40	0.01
		(0.03)		(0.03)		(0.04)		(0.04)
Age	9.83	0.07	9.82	0.10*	9.84	0.08	9.83	0.07
		(0.05)		(0.06)		(0.06)		(0.06)
African American	0.76	-0.03	0.76	-0.03	0.77	-0.03	0.78	-0.03
		(0.03)		(0.03)		(0.03)		(0.03)
Latino	0.13	<0.01	0.13	< 0.01	0.14	-0.01	0.13	-0.01
		(0.02)		(0.02)		(0.03)		(0.03)
Free or Reduced-Price Lunch	0.66	-0.05	0.68	-0.07**	0.70	-0.09**	0.69	-0.08**
		(0.03)		(0.04)		(0.04)		(0.04)
Panel C: Normalized Test Scores								
Reading	0.10	<0.01	0.09	0.05	0.09	0.04	0.07	0.03
		(0.05)		(0.06)		(0.06)		(0.06)
Problem Solving	0.10	<0.01	0.10	0.04	0.09	0.03	0.09	<0.01
		(0.06)		(0.07)		(0.07)		(0.07)
Panel D: Non-Cognitive Skills								
Academic Self-Perceptions Index	0.00	-0.01	-0.01	0.01	-0.02	0.02	-0.02	0.02
		(0.04)		(0.05)		(0.05)		(0.05)
Peer and Adult Support Index	0.00	0.07	0.00	0.07	-0.01	0.09	0.02	0.06
		(0.05)		(0.06)		(0.06)		(0.06)
Panel E: Household								
Parent Education:								
High School or GED	0.24	-0.01	0.25	-0.02	0.25	-0.02	0.24	-0.01
		(0.03)		(0.03)		(0.03)		(0.03)
Some College	0.31	<0.01	0.30	0.01	0.30	0.01	0.31	0.01
		(0.03)		(0.04)		(0.04)		(0.04)
Bachelor's Degree	0.15	<0.01	0.14	0.02	0.14	0.02	0.15	-0.01
		(0.03)		(0.03)		(0.03)		(0.03)
Both Parents in the Home	0.30	-0.03	0.28	<0.01	0.29	0.00	0.29	<0.01
		(0.03)		(0.03)		(0.03)		(0.04)
Non-English Language Spoken at Home	0.12	0.01	0.12	0.01	0.12	0.02	0.11	0.02
		(0.02)		(0.02)		(0.02)		(0.03)
Joint Test								
Chi2		10.63		14.79		17.57		14.17
p-value		0.78		0.47		0.29		0.51

Note: This table presents the differences in baseline characteristics between the treatment and control group to assess the internal validity of the study. Columns one and two present results using all subjects participating in the experiment. The remaining columns include only those subjects completing the respective follow-up survey. We estimate all differences using equation (1), including only cohort fixed effects. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 4: Out-of-School Time Program Participation

	Higher Achievement (1)	Non-HA Academic (2)	Any Academic (3)
Panel A: First-Year Follow-Up			
Treatment Effect	0.747***	-0.036	0.539***
	(0.022)	(0.033)	(0.031)
Constant	-0.008	0.225***	0.245***
	(0.022)	(0.036)	(0.035)
R-Squared	0.75	0.02	0.31
Prob > F	<0.01	0.27	<0.01
Sample Size	819	819	819
Control Average	0	0.29	0.29
Panel B: Second-Year Follow-Up			
Treatment Effect	0.697***	-0.038	0.498***
	(0.023)	(0.033)	(0.033)
Constant	-0.124***	0.284***	0.200***
	(0.027)	(0.039)	(0.041)
R-squared	0.5	<0.01	0.24
Prob>F	<0.01	0.26	<0.01
Sample Size	775	775	775
Control Average	0	0.26	0.26
Panel C: Fourth-Year Follow-Up			
Treatment Effect	0.468***	0.012	0.381***
	(0.034)	(0.040)	(0.044)
Constant	0.021	0.151***	0.173***
	(0.036)	(0.042)	(0.048)
R-squared	0.03	0.01	0.15
Prob>F	<0.01	0.76	<0.01
Sample Size	430	430	430
Control Average	0	0.21	0.21

Note: This table presents estimates of the effect of assignment to the treatment group on participation in HA and other academic OST programs. Column one contains estimates of the effect on participation in HA. Column two includes the effects on participation in academic OST programs other than HA. And column three presents estimates of the effect on participation in any academic OST program. Because students can only attend HA through the eight grade, we only use students who applied to HA before their fifth grade year to estimate the effects on the fourth-year follow-up survey in Panel C. We estimate all differences using equation (1), including only cohort fixed effects. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 5: Effects of HA on Standardized Test Scores

	ITT (1)	ITT (2)	ITT (3)	LATE (4)
Panel A: First-Year Follow-Up				
Problem Solving	0.05	0.02	0.03	0.05
	(0.07)	(0.05)	(0.05)	(0.09)
Reading Comprehension	0.05	0.02	0.02	0.04
	(0.06)	(0.05)	(0.05)	(0.09)
Panel B: Second-Year Follow-Up				
Problem Solving	0.12*	0.10**	0.10**	0.19**
	(0.07)	(0.05)	(0.05)	(0.09)
Reading Comprehension	0.11*	0.10**	0.08*	0.15*
	(0.07)	(0.05)	(0.05)	(0.09)
Panel C: Fourth-Year Follow-Up				
Problem Solving	0.08	0.10*	0.11**	0.17**
	(0.07)	(0.05)	(0.05)	(0.08)
Reading Comprehension	0.03	0.04	0.04	0.05
	(0.06)	(0.05)	(0.05)	(0.08)
Cohort Fixed Effects	Yes	Yes	Yes	Yes
Child-Level Controls		Yes	Yes	Yes
Household-Level Controls			Yes	Yes

Note: This table presents estimates of the effect of HA on students' standardized test scores. Columns one through three provide estimates of the intent-to-treat effect using equation (1) with the indicated control variables. Column four presents the Local Average Treatment Effects (LATE) estimated by instrumenting an indicator variable for whether a child has ever participated in an OST program with treatment assignment using Two-Stage Least Squares. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 6: High School Application and Matriculation

	Private				Public Magnet and Charter			
	Any	Elite	Competitive	Non-Competitive	Any	Competitive Magnet	Competitive Charter	Any Non-Competitive
Applied	0.06*	0.03	0.07***	< 0.01	-0.05*	0.01	< 0.01	-0.08***
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)
	0.21	0.1	0.11	0.07	0.68	0.39	0.3	0.22
Admitted	0.07**	-0.01	0.06**	0.01	-0.09***	-0.04	0.02	-0.06**
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)
	0.14	0.04	0.08	0.05	0.63	0.29	0.26	0.16
Matriculated	0.06**	-0.01	0.06***	< 0.01	-0.11***	-0.04	< 0.01	-0.07***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.02)
	0.09	0.03	0.03	0.03	0.56	0.24	0.19	0.13
Matriculated LATE	0.09**	-0.01	0.09***	< 0.01	-0.16***	-0.05	-0.01	-0.10***
	(0.04)	(0.02)	(0.03)	(0.02)	(0.05)	(0.05)	(0.05)	(0.04)

Note: This table presents estimates of the effect of HA on the types of high school at which students matriculate. Rows one through three provide Intent-to-Treat estimates of the effect of HA on students application to, admission to, and matriculation at the indicated type of school using equation (1) with the full set of control variables. Row four provides estimates of the Local Average Treatment Effects (LATE) estimated by instrumenting an indicator variable for whether a student has ever attended an OST program with treatment assignment using Two-Stage Least Squares. Standard errors are clustered at the family level and provided in parentheses. Control group averages are provided below the standard errors. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *. A full list of all schools falling into each category is provided in Table A1 of the Appendix.

Table 7: Importance of High School Characteristics

	Raw Control Mean	Raw Treat-Control	Normalized Treat-Control
Panel A: School Characteristics			
How strong the school is academically	3.56	0.07	0.09
		(0.05)	(0.07)
How strong the school is in the arts, sports or another area that I am interested in	3.44	-0.04	-0.04
		(0.06)	(0.07)
How close the school is to my house	2.75	< 0.01	< 0.01
		(0.08)	(0.08)
How much it would cost to attend	2.68	-0.01	-0.01
		(0.09)	(0.08)
The school's philosophy or topic focus	2.99	0.1	0.1
		(0.08)	(0.08)
The school's size	2.87	-0.03	-0.03
		(0.08)	(0.08)
Whether I thought I had a good chance of getting in	3.24	0.01	0.01
		(0.08)	(0.08)
Panel B: Influences of Others			
Whether my friends were applying to or already attending the school	2.83	-0.06	-0.06
		(0.08)	(0.08)
Whether my brother'(s) or sister'(s) were attending	2.12	-0.03	-0.03
		(0.09)	(0.08)
Whether students from my after-school program had gone there	1.98	< 0.01	< 0.01
		(0.08)	(0.08)
Whether adults from my middle school thought I should apply	2.53	-0.01	-0.01
		(0.08)	(0.08)
Whether adults from my after-school program thought I should apply	2.13	0.08	0.07
		(0.09)	(0.08)
Whether my parents thought I should apply	3.19	0.02	0.02
		(0.07)	(0.07)
High School Preference Index			0.01
			(0.04)

Note: This table presents the estimated treatment effects of HA on students' high school preferences. Panel A includes preference based on characteristics of schools. Panel B includes preferences determined by the actions or preferences of others. For each characteristic, students reply using a four-point Likert scale with higher numbers indicating greater importance. We provide the raw control group mean, the raw treatment effect, and the treatment effect based on the normalized measure of each variable. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 8: High School Application Activities

	Control Average	Treat-Control		Control Average	Treat-Control
Panel A: All High Schools			**Panel B: Private Schools**		
Attended a mock interview	0.15	0.12***	Attended a test preparation class for the SSAT or HSPT	0.13	0.11***
		(0.03)			(0.03)
Visited any of the high schools student was interested in	0.6	0.07*	Practiced for the SSAT or HSPT, but not as part of a class	0.12	0.17***
		(0.04)			(0.03)
Spoke with teachers or other staff at any of schools student was interested in	0.52	0.10**	Took the SSAT or HSPT	0.18	0.07**
		(0.04)			(0.03)
Spoke with students who attended these schools about how they liked it there	0.54	0.12***	Applied for a Scholarship	0.134	0.056*
		(0.04)			(0.031)
Got information about specific high schools	0.6	0.04	Received a Scholarship	0.097	0.049*
		(0.04)			(0.028)
Attended a "shadow" day at a high school	0.26	0.05			
		(0.04)			
			Application Activities Index		0.24***
					(0.05)

Note: This table presents the estimated treatment effects of HA on the probability that students participated in activities related to the high school application process. Panel A includes activities that could be related to applying to private, magnet, or charter schools. Panel B includes activities specific to private school applications. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 9: Academic OST Intensity

	First-Year Follow-Up		Second-Year Follow-Up		Fourth-Year Follow-up	
	Control Average	Treatment-Control	Control Average	Treatment-Control	Control Average	Treatment-Control
Panel A: Academic Year						
Days per week	1.66	1.52***	1.32	1.39***	0.41	1.57***
		(0.15)		(0.15)		(0.16)
Hours per week	3.94	10.32***	2.98	8.50***	1.08	5.56***
		(0.66)		(0.67)		(0.58)
Panel B: Summer						
Days per week	0.83	2.20***	0.98	2.27***	0.75	1.57***
		(0.14)		(0.17)		(0.22)
Hours per week	4.15	19.75***	5.06	19.05***	3.53	14.42***
		(1.18)		(1.27)		(1.88)

Note: This table presents the estimated treatment effects of HA on the the amount of time students spent in an OST program. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 10: OST-Related Activities

	First-Year Follow-Up		Second-Year Follow-Up		Fourth-Year Follow-Up	
	Control Average	Treatment-Control	Control Average	Treatment-Control	Control Average	Treatment-Control
Done some type of community service or volunteer work	0.53	< 0.01 (0.04)	0.56	0.04 (0.04)	0.7	0.04 (0.05)
Spoken to a group outside of school about your ideas or your work	0.57	0.06* (0.03)	0.55	0.09** (0.04)	0.58	0.11** (0.05)
Visited a college campus to see what it would be like to be a college student	0.44	0.28*** (0.04)	0.45	0.28*** (0.04)	0.53	0.25*** (0.05)
Read books that are not for school	0.71	0.03 (0.03)	0.75	0.04 (0.03)	0.77	0.08* (0.04)
Written things (like poems, letters, or essays) not assigned at school	0.66	0.07** (0.03)	0.71	0.03 (0.03)	0.68	0.14*** (0.04)
Visited a business or organization to see what it would be like to work there	0.52	0.04 (0.04)	0.53	0.08** (0.04)	0.56	0.14*** (0.05)
Gone to events outside of your neighbor-hood with your after-school program	0.67	0.10*** (0.03)	0.74	0.06* (0.03)	0.75	0.09** (0.04)
Participated in academic contests at your after-school program	0.55	0.13*** (0.03)	0.57	0.11*** (0.04)	0.55	0.16*** (0.05)
OST Related Activities Index	0	0.18*** (0.04)	0	0.19*** (0.04)	0	0.26*** (0.05)

Note: This table presents the estimated treatment effects of HA on students' experiences outside of school. For each activity, we present the probability that a student indicates she has ever participated in the indicated activity. Only the quesitons presented in rows seven and eight are specific to experiences in an OST program. For all of the other questions, we ask students about experiences in any context. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 11: Peer Effects

	First-Year Follow-Up		Second-Year Follow-Up		Fourth-Year Follow-Up	
	Control Average	Treatment-Control	Control Average	Treatment-Control	Control Average	Treatment-Control
Panel A: Number of Friends						
Overall	4.5	0.18**	4.64	0.06	4.53	0.04
		(0.07)		(0.07)		(0.08)
School	3.22	-0.26**	2.77	-0.23**	3.16	-0.29***
		(0.10)		(0.09)		(0.10)
Family	0.33	0.12**	0.25	0.11**	0.37	0.07
		(0.06)		(0.06)		(0.06)
Academic OST	0.01	0.28***	0.01	0.34***	0.01	0.18***
		(0.03)		(0.04)		(0.03)
Higher Achievement	0.01	0.27***	0	0.32***	0	0.17***
		(0.03)		(0.04)		(0.03)
Panel B: Academic Interactions with Peers						
Talked with other kids about a math or science problem outside of school	0.65	0.04	0.69	0.02	0.72	0.13***
		(0.03)		(0.03)		(0.03)
Gotten praise for your achievements from your peers	0.82	0.02	0.83	0.02	0.91	0.01
		(0.03)		(0.03)		(0.02)
Helped other kids with their school work	0.88	-0.02	0.87	0	0.89	0.01
		(0.02)		(0.02)		(0.02)
Stood up in front of a group of children to present your ideas	0.82	< 0.01	0.82	0.02	0.86	< 0.01
		(0.03)		(0.03)		(0.03)
Peer Interaction Index		0.02		0.04		0.09*
		(0.04)		(0.05)		(0.05)
Panel C: Peer Support						
Academically Supportive Peers		-0.03		0.05		-0.05
		(0.07)		(0.07)		(0.08)

Note: This table estimates the effect of HA on students' peer networks. Panel A provides students responses to a question that asks them to list and indicate where they met up to five of their closest friends. Panel B provids estimates of the effect of whether students indicate having ever participated in the indicated activity. In Panel C, we estimate the treatment effects on the scale of students' perceptions of peer academic support described in Table 2. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 12: Adult Support

	First-Year Follow-Up		Second-Year Follow-Up		Fourth-Year Follow-Up	
	Control Average	Treatment-Control	Control Average	Treatment-Control	Control Average	Treatment-Control
Panel A: Interactions and Activities						
Spoken with an adult (not your parent) about what you need to do to get into a good high school	0.61	0.11***	0.71	0.09***	0.87	0.01
		(0.03)		(0.03)		(0.03)
Spoken with an adult (not your parent) about going to college or college applications	0.53	0.12***	0.62	0.10***	0.78	0.10***
		(0.04)		(0.03)		(0.03)
Spoken with an adult (not your parent) about what you need to do to get a good job	0.65	0.09***	0.68	0.10***	0.82	0.08***
		(0.03)		(0.03)		(0.03)
Spoken with an adult (not your parent) about what job you might want to have in the future	0.77	0.07**	0.81	0.06**	0.91	0.04
		(0.03)		(0.03)		(0.02)
Adult Interaction Index		0.21***		0.19***		0.15***
		(0.05)		(0.06)		(0.06)
Panel B: Adult Support						
Adult Support		0.04		0.06		0.04
		(0.07)		(0.07)		(0.08)

Note: This table estimates the effect of HA on students' experience of adult support. Panel A provides estimates on whether or not a student has ever experienced the indicated activity. In Panel B, we estimate the treatment effects on the scale of students' perceptions of adult support described in Table 2. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 13: Effects on Academic Self-Perceptions

	First-Year Follow-Up		Second-Year Follow-Up		Fourth-Year Follow-Up	
	(1)	(2)	(3)	(4)	(5)	(6)
Industry and Persistence	-0.14**	-0.15**	-0.02	-0.04	0.03	< 0.01
	(0.07)	(0.06)	(0.07)	(0.06)	(0.08)	(0.07)
Creativity	-0.13*	-0.13*	0.02	0.02	0.02	0.02
	(0.07)	(0.06)	(0.07)	(0.07)	(0.08)	(0.08)
Self-Perceptions of Academic Abilities	-0.1	-0.09	0.1	0.10*	-0.05	-0.04
	(0.07)	(0.06)	(0.07)	(0.06)	(0.08)	(0.07)
Enjoyment of Learning	-0.14**	-0.14**	-0.04	-0.08	0.04	0.02
	(0.07)	(0.06)	(0.07)	(0.07)	(0.08)	(0.07)
Curiosity	-0.11	-0.13*	-0.01	-0.02	0.1	0.1
	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)
Ability to Change the Future through Effort	0.05	0.03	0.02	< 0.01	0.06	0.05
	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Academic Self-Perceptions Index	-0.09*	-0.10**	0.01	< 0.01	0.03	0.03
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)

Note: This table estimates the effect of HA on students' academic self-perceptions using the non-cognitive skill scales described in Table 2. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table 14: Changes in Outcomes, Summer 2010

	Spring Outcomes		Fall Outcomes		Summer Differences	
	Control Average	Treatment-Control	Control Average	Treatment-Control	Control Average	Treatment-Control (Differences)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Standardized Test Scores						
Problem Solving	0.16	0.13**	0.18	0.09	0.02	-0.02
	(0.92)	(0.06)	(0.92)	(0.07)	(0.65)	(0.07)
Reading Comprehension	-0.03	0.16**	0.12	0.07	0.14	-0.04
	(0.92)	(0.06)	(0.94)	(0.07)	(0.61)	(0.07)
Average Reading Comprehension and Problem Solving	0.06	0.14***	0.15	0.08	0.08	-0.03
	(0.84)	(0.05)	(0.83)	(0.05)	(0.46)	(0.05)
Panel B: High School Preferences						
Competitive HS	0.42	0.11**	0.46	0.16***	< 0.01	0.12**
	(0.49)	(0.05)	(0.50)	(0.05)	(0.52)	(0.06)
Local Neighborhood HS	0.31	-0.04	0.33	-0.10**	0.06	-0.12**
	(0.47)	(0.04)	(0.47)	(0.04)	(0.48)	(0.05)

Note: This table presents estimates of the effect of HA on changes in outcomes during the summer of 2010. Panel A presents estimates of the effects on students' test scores. Panel B provides estimates of the effects on students' high school preferences. Columns one and two (three and four) provide estimates from the survey administered just before (after) the summer. Columns five and six provide estimates on the change in scores (fall scores less spring scores) during the summer. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *. Estimates of the effect on students' perceptions of peer and adult support and academic self-perceptions are provided in Table A4 of the Appendix.

Table A1: High School List and Classification

Academic Magnet
- Potomac High School
- Benjamin Manneker High School
- Columbia Heights Educational Campus (Bell Multicultural HS)
- H. B. Woodlawn High School
- McKinley Technology High School
- North Point High School
- Oxon Hill High School
- School Without Walls
- Thomas Jefferson Science and Technology High School
- Woodrow Wilson High School Academies

Non-Academic Magnet
- Duke Ellington School of the Arts
- Phelps Architecture, Construction, and Engineering High School

Competitive Charter
- Capital City PCS - Upper School
- Cesar Chavez Public Policy Charter High School
- E.L. Haynes PCS
- KIPP DC
- Paul PCS
- Thurgood Marshall PCS
- Washington Latin PCS
- Washington Math, Science & Technology PCS

Uncompetitive Charter
- Booker T. Washington PCS
- Carlos Rosario International PCS
- Friendship Collegiate Academy PCS
- Hospitality PCS
- IDEA PCS
- Ideal Academy PCS
- Kamit Institute for Magnificent Achievers PCS
- LAYC Career Academy PCS
- Maya Angelou PCS
- National Collegiate Preparatory PCS
- Next Step/El Proximo Paso PCS
- Options PCS
- Perry Street Prep Upper PCS
- Richard Wright PCS
- St. Coletta Special Educaiton PCS
- The SEED School of Washington, DC PCS
- Tree of Life PCS
- William E. Doar Junior PCS
- Young America Works PCS
- Youth Build LAYC PCS

Elite Private
- Georgetown Visitation Preparatory School
- Gonzaga College High School
- National Cathedral School
- St. Anselm's Abbey School
- St. Albans School
- Sidwell Friends School
- Georgetown Preparatory School
- Phillips Academy
- Holton-Arms School
- Foxcroft Boarding School
- Madeira Boarding School for Girls
- Washington International School
- Georgetown Day School
- Maret School

Non-Elite Competitive Private
- Baylor School
- Bishop Ireton High School
- Bishop McNamara
- Bishop O'Connell High School
- Bullis School
- Darlington School
- DeMatha Catholic High School
- Don Bosco Cristo Rey High School
- Edmund Burke School
- Elizabeth Seton High School
- Emerson Preparatory School
- Episcopal High School
- Linden Hall
- McCallie School
- McDonogh School
- Mercersburg Academy
- Montrose Christian School
- Oak Hill Academy
- Parkmont School
- Paul VI
- Riverdale Baptist School
- Sandy Springs Friends School
- St. Andrew's Episcopal School
- St. James Boarding School
- St. John's College High School
- St. Margaret School
- St. Stephen and St. Agnes School
- St. Timothy
- Stone Ridge School of the Sacred Heart
- Takoma Academy
- The Academy of the Holy Cross
- The Barre School
- The Field School
- The Landon School
- Woodberry Forest
- Woodstream Academy

Non-Competitive Private
- Academy for Ideal Education
- Archbishop Carroll High School
- Kingsbury Day School
- Model Secondary School for the Deaf
- Preparatory School of the District of Columbia
- Rhema Christian Center School
- San Miguel School
- Sankofa Fie

Private Unable to Rank -
(Included in Totals Only)
- Baltimore School for the Arts
- Chelsea School
- Coeus International School
- Fort Union Military Academy
- Maplebrook School

Note: This table provides the classification of high schools in the Arlington and Washington, DC area used in Table 6. We classified public schools using information provided by the DC public school systems, while we used a classification provided by HA for private schools. We were unable to find test scores or third party rankings of private schools.

Table A2: Sources of Help in Applying to High Schools

Type of Help	OST Program (1)	Teachers (2)	Parents (3)	Other Adults (4)	Friends (5)
Preparing for admissions tests	0.18***	0.06	0.10**	0.09**	0.11**
	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)
	0.22	0.59	0.62	0.3	0.45
Learning about specific high schools	0.24***	0.09**	0.05	0.02	-0.03
	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)
	0.27	0.55	0.76	0.45	0.54
Filling out the application forms	0.19***	0.09**	0	0.07*	-0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
	0.19	0.45	0.72	0.23	0.33
Choosing schools to apply to	0.18***	0.13***	-0.03	0.06	0.06
	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
	0.24	0.47	0.76	0.33	0.44
Understanding the application process	0.27***	0.08**	0.01	0.07	0.07*
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
	0.23	0.52	0.63	0.33	0.31
Applying for scholarships or financial aid	0.11***	0.06	0.07*	0.05	0.04
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
	0.17	0.32	0.49	0.21	0.17
Understanding what a private or specialize high school would be like	0.17***	0.08**	0.06	0.07	0.03
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
	0.23	0.43	0.55	0.3	0.32
Taking you to an interview, audition, or school visit	0.14***	0.10**	0.01	0.06	0.01
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
	0.17	0.33	0.75	0.25	0.24
Writing essays	0.15***	0.09**	-0.01	0.06	0.03
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
	0.23	0.49	0.63	0.26	0.33
Practicing for interviews	0.19***	0.09**	-0.01	0.09*	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
	0.19	0.36	0.62	0.27	0.37
Sources of Help Index	0.42***	0.15***	0.02	0.13*	0.05
	(0.08)	(0.06)	(0.05)	(0.07)	(0.06)

Note: This table estimates the effect of HA on whether or not students report having received help from the indicated types of individuals for each activity. Standard errors are clustered at the family level and provided in parentheses. Control group averages are provided below the standard errors. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table A3: Academic Self-Perceptions by Grade

	Academic Self-Perceptions Index	Industry and Persistence	Ability to be Creative	Perceptions of Acad Abilities	Enjoyment of Learning	Curiosity	Change Future through Effort
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: First-Year Follow-Up							
Treat*Grade Five	-0.15***	-0.30***	-0.12	-0.22**	-0.18**	-0.10	0.02
	(0.05)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.08)
Treat*Grade Six	-0.04	0.02	-0.11	0.09	-0.09	-0.16	0.03
	(0.07)	(0.11)	(0.09)	(0.10)	(0.11)	(0.11)	(0.10)
Grade Five	0.09	0.22*	-0.03	0.18*	0.12	-0.04	0.09
	(0.07)	(0.11)	(0.10)	(0.10)	(0.11)	(0.11)	(0.11)
R-squared	0.314	0.288	0.288	0.300	0.265	0.241	0.164
Observations	819	818	817	817	819	818	818
H0: Treat*Grade Five = Treat*Grade Six							
t-statistic	1.690	5.594**	0.004	5.985**	0.521	0.185	0.006
P-value	0.194	0.018	0.949	0.015	0.471	0.667	0.938
Panel B: Second-Year Follow-Up							
Treat*Grade Five	-0.06	-0.18*	<0.01	-0.05	-0.15	0.01	0.02
	(0.06)	(0.10)	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)
Treat*Grade Six	0.07	0.14	0.06	0.33***	-0.01	-0.07	-0.04
	(0.07)	(0.11)	(0.10)	(0.10)	(0.11)	(0.11)	(0.11)
Grade Five	0.03	0.24**	-0.05	0.22*	0.06	-0.21*	-0.07
	(0.08)	(0.12)	(0.11)	(0.12)	(0.12)	(0.12)	(0.11)
R-squared	0.281	0.264	0.250	0.268	0.205	0.227	0.131
Observations	777	775	776	776	777	776	775
H0: Treat*Grade Five = Treat*Grade Six							
t-statistic	1.788	4.722**	0.262	7.708***	0.943	0.332	0.166
P-value	0.182	0.030	0.609	0.006	0.332	0.565	0.683
Panel C: Fourth-Year Follow-Up							
Treat*Grade Five	-0.01	-0.10	-0.01	-0.11	-0.04	0.16	0.03
	(0.07)	(0.10)	(0.10)	(0.09)	(0.10)	(0.10)	(0.10)
Treat*Grade Six	0.08	0.15	0.07	0.07	0.11	< 0.01	0.08
	(0.08)	(0.13)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Grade Five	0.02	0.20	-0.06	0.22*	-0.01	-0.18	-0.06
	(0.08)	(0.13)	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)
R-squared	0.220	0.203	0.188	0.223	0.173	0.197	0.095
Observations	724	724	724	724	724	722	723
H0: Treat*Grade Five = Treat*Grade Six							
t-statistic	0.772	2.428	0.358	1.452	0.993	1.214	0.115
P-value	0.380	0.120	0.550	0.229	0.319	0.271	0.735

Note: This table provides estimates of the effects of HA on students academic self-perceptions by grade and survey round with outcomes normalized relative to the baseline control distribution. These estimates are similar to those depicted in Figure 1. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *.

Table A4: Changes in Outcomes, Summer 2010

	Spring	Fall	Summer Differences		
	Treatment-Control	Treatment-Control	Control Average	Treatment-Control (Differences)	Treatment-Control (Spr Controls)
Panel A: Perceived Support					
Academically Supportive Friends	-0.03	0.02	0.01	0.05	0.03
	(0.09)	(0.09)	(0.95)	(0.10)	(0.09)
Adult Support	0.18**	0.09	-0.02	-0.08	< 0.01
	(0.09)	(0.09)	(0.94)	(0.10)	(0.08)
Panel B: Academic Self-Perceptions					
Industry and Persistence	-0.16*	-0.02	-0.02	0.13*	0.09
	(0.08)	(0.08)	(0.72)	(0.07)	(0.06)
Ability to be Creative	0.01	-0.02	< 0.01	-0.02	-0.01
	(0.09)	(0.09)	(0.77)	(0.08)	(0.07)
Self-Perceptions of Academic Abilities	0.07	-0.01	-0.02	-0.09	-0.04
	(0.08)	(0.08)	(0.80)	(0.09)	(0.08)
Enjoyment of Learning	-0.12	0.03	-0.01	0.15**	0.11*
	(0.09)	(0.09)	(0.69)	(0.07)	(0.06)
Curiosity	-0.07	0.10	< 0.01	0.13	0.10
	(0.09)	(0.09)	(0.85)	(0.08)	(0.07)
Ability to Change the Future through Effort	0.07	0.11	< 0.01	0.06	0.08
	(0.10)	(0.09)	(0.95)	(0.10)	(0.09)
Academic Self-Perceptions Index	-0.04	0.03	< 0.01	0.06	0.05
	(0.06)	(0.06)	(0.47)	(0.05)	(0.05)

Note: This table presents estimates of the effect of HA on changes in outcomes during the summer of 2010. Panel A presents estimates of the effects on students' perceptions of peer and adult support. Panel B provides estimates of the effects on students'academic self-perceptions. Columns one and two (three and four) provide estimates from the survey administered just before (after) the summer. Columns five and six provide estimates on the change in scores (fall scores less spring) during the summer. We estimate all treatment effects using equation (1), including the full set of control variables. Standard errors are clustered at the family level. Significance at the one-, five-, and ten-percent levels is indicated by ***, **, and *. Estimates of the effect on students academic self-perceptions are provided in Table A4 of the Appendix.