The Effect of Summer Employment on the Educational Attainment of Under-Resourced Youth*

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Abstract

Summer youth employment programs are a popular way for municipalities to provide adolescents with skills and experiences thought to improve labor market outcomes. While research evidence on such programs has grown in recent years, it is still limited. In particular, it is not clear how, if at all, participation influences key educational outcomes. We study the program in Detroit, Michigan using a selection on observables identification strategy. In addition to controlling for a rich set of covariates, including baseline educational measures, we match participants to their classmates of the same race and gender who applied for the program, but did not participate. We find that participation is associated with a modest increase in educational attainment. Specifically, it increased the likelihood of enrolling in public school after the program by 1.5% and of graduating high school by 4%, relative to comparison means of 94.5% and 85%. Youth with the weakest academic skills benefited the most, as participation increased school enrollment by 2.2% and high school graduation by 5.5% for this group. Falsification tests of whether participation predicts pre-program characteristics, as well as robustness checks which account for omitted variable bias, as proposed in Oster (2016), suggest that our results are not driven by unobservable differences between participants and other applicants.

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1 Introduction

Even though the U.S. economy has climbed out of the Great Recession, the labor market opportunities for many people remain a concern. The labor force participation rate among all civilians age 16 or older was 5% lower in 2016 than its pre-recession level of 66.2% in 2006, according to data from the U.S. Bureau of Labor Statistics. The recovery for young adults age 16 to 24, particularly for young men, has been slower than for the population overall; the labor force participation rate for these groups declined by 9% and 11% respectively over the same timeframe. As policymakers seek to improve work opportunities for youth, there has been a growing interest in alternative pathways for them to obtain career skills.

Summer youth employment programs are a popular way for municipalities to provide adolescents with skills and experiences thought to improve labor market outcomes. The first such program began in the United States over 50 years ago in 1964, and as of 2016, at least 42 cities across the country offered summer jobs to over 115,000 youths.[1] New York City and Chicago operate the largest programs, providing jobs to about 50,000 and 24,000 youths respectively, though many smaller cities operate them as well, including Tuscaloosa, AL, Charlottesville, VA and Madison, WI (Dollarwise, 2016).

These programs are arguably more important in today's economy than ever before, as recent work has highlighted the increasing returns to non-cognitive skills in the labor force (Deming, 2017). Working during the summer offers youth the opportunity to interact with professionals who can teach them valuable interpersonal skills. Also, many summer youth employment programs include an explicit curriculum designed to teach a variety of work readiness and life skills, ranging from how to prepare a resume to the benefits of establishing a bank account. Finally, some have a particular focus on career and technical education (CTE), which is receiving increased attention from many cities and states (Alfeld, 2016; Jacob, 2017). In this sense, summer youth employment programs are similar in spirit to

[1] There is no exact count of how many local governments organize summer youth employment programs as these programs are decentralized (Belotti, Rosenberg, Sattar, Esposito, and Ziegler, 2010).

school-based CTE programs, in which schools offer students internships or connections to employers, in that both are part of the education for career readiness.

There are three main purposes of summer youth employment programs. First, they intend to give young adults the opportunity to earn an income during the summer. Second, they aim to reduce crime during the summer by keeping youth 'off the streets.' Finally, they serve to improve labor market outcomes beyond the summer.

While research evidence on such programs has grown in recent years, it is still limited. In particular, it is not clear how, if at all, participation influences key educational outcomes such as high school graduation. This is a critical gap as education is one of the strongest predictors of labor market success. Participation might increase educational attainment by increasing the opportunity cost of dropping out of high school or by increasing academic engagement as a result of improved soft skills. Using detailed administrative data, this paper analyzes a more extensive set of educational outcomes than previous studies, including continued enrollment in school, attendance rates, test scores, high school graduation, and college enrollment.

In this paper, we study how participation in a summer youth employment program is associated with educational outcomes. Specifically, we study the program in Detroit, Michigan using a selection on observables identification strategy, comparing youth who participated in the program to those who applied but did not participate. The main threat to our identification is that participants differ from applicants who did not participate along unobservable dimensions that are related to educational outcomes. To address this, we first match participants to applicants who did not participate that were of the same race and gender and were in the same grade in the same school in the same year. Participants look quite similar to their matched peers along a variety of observable dimensions.

We also control for information related to the selection process in our analysis. While details vary across job placements, program administrators select participants using information from the application, from brief conversations at a career fair, or, if applicable, from an existing relationship with the applicant. In our analysis, we control for all of the information from the application, including gender, race and a second order polynomial of age. We also control for some things that administrators may be able to infer from the application, such as characteristics of the neighborhood where the applicant lives. Lastly, we control for information that was unobservable to program staff, such as prior attendance records and test scores. Specifically, we control for prior school attendance, special education status, limited English proficiency, eligibility for free or reduced price lunch, ever retained in grade as well as prior standardized math and reading test scores, and an interaction of the two.

We perform falsification exercises and do not find that participation predicts pre-program characteristics. For the 99.9% of youth who completed 6th grade before applying, we test whether participation predicted educational performance in 6th grade. These measures could not have been caused by participation in the summer program. We condition on all of the covariates listed above, including baseline attendance and test scores, and do not find that participation predicts 6th grade attendance or test scores.

We find that participation is associated with a modest increase in educational attainment. Specifically, it increased the likelihood of enrolling in public school after the program by 1.5% and of graduating high school by 4%. We perform the robustness checks proposed in Oster (2016) and find that, consistent with the falsification exercises, these results are not driven by selection bias from unobservable characteristics that are correlated with observables. Specifically, we can bound the effect on public school enrollment between 0.8% and 1.5% and on high school graduation between 3.4% and 4%, assuming that selection on unobservables was not larger than selection on observables.

Youth with the weakest academic skills benefited the most, as participation increased school enrollment and high school graduation by 2.2% and 5.5% respectively for this group. Our findings suggest that summer youth employment is an important complement to in-school work-based learning programs, such as career technical education and school-employer partnerships. A youth's first experience in the labor force may have a significant impact on future education and career aspirations.

2 Prior Literature

Until recently, research on summer youth employment programs was limited to descriptive analyses of program implementation or short-run participant outcomes. To the best of our knowledge, there are only seven studies which credibly estimate causal effects of these programs — four that study the same New York City program (Leos-Urbel, 2014; Schwartz, Leos-Urbel, and Wiswall, 2015; Gelber, Isen, and Kessler, 2016; Valentine, Anderson, Hassain, and Unterman, 2017), two that study the same Chicago program (Heller, 2014; Davis and Heller, 2017) and one that focuses on the program in Boston (Modestino, 2017). The NYC and Boston studies utilize a quasi-experimental lottery design that compares youth who were provided an opportunity to participate via a random admissions lottery to those who applied but did not receive an offer to participate. The Chicago study analyzes two randomized control trials, comparing young adults who were randomly assigned to participate in the program to those who were assigned to a control group.

Several consistent findings stand out. First, participation in summer employment programs seems to be associated with a reduction in crime. In New York City, Gelber, Isen, and Kessler (2016) find that participation reduced the likelihood of incarceration up to 9 years after the program by 10% relative to a comparison mean of 1%. In Chicago, the program reduced violent-crime arrests by 35% during the summer and the following school year (Heller, 2014; Davis and Heller, 2017). In Boston, participation reduced the number of arraignments for violent and property crime in the following year by 35% and 57% respectively, with the largest impacts for African-American and Hispanic males (Modestino, 2017).

Second, participation in the programs does not seem to have a meaningful impact on employment or earnings. Researchers followed NYC participants for up to 7 years and do not find that participation had a long run effect on labor market outcomes. They do find a small increase in the likelihood of having a job in the first two follow-up years, accompanied with a small decline in wages, which they associate with a greater likelihood of working in the public sector (Gelber, Isen, and Kessler, 2016; Valentine, Anderson, Hassain, and Unterman, 2017). However, the studies of the Boston and Chicago programs follow youth for one and two years respectively and did not find an effect on employment or earnings over this timeframe. (Davis and Heller, 2017; Modestino, 2017).

It is decidedly less clear how, if at all, participation in a summer youth employment program influences educational outcomes. An early study in NYC of the 2007 cohort found that participation increased attendance in the following school year by 1.7%, driven by youth age 16 and older who had low baseline attendance rates (Leos-Urbel, 2014). However, a study that included a broader sample of five cohorts of the program from 2006 to 2010 found no effect on attendance (Valentine, Anderson, Hassain, and Unterman, 2017). Moreover, studies following NYC participants over a longer time period find that it did not influence high school graduation, college enrollment or degree completion (Gelber, Isen, and Kessler, 2016; Valentine, Anderson, Hassain, and Unterman, 2017).[2]

Modestino (2017) finds that participation in Boston's summer youth employment program increased attendance rates in the following school year by 3% relative to a baseline of 87%. This was driven by larger increases for Hispanic youths, males and those older than 16. Studying the Chicago program, however, Davis and Heller (2017) find that participation did not affect daily attendance or grade point average the following school year. It also did not affect persistence in school, defined as continued enrollment or high school graduation, within 3 years. However, the authors do find that relatively more advantaged youths, those with the highest pre-program school attendance and grades, do benefit from participation. Specifically, participation increased school persistence by 13% for those in the top quartile of predicted employment impact relative to a baseline mean of 60%, which was statistically

[2]Schwartz, Leos-Urbel, and Wiswall (2015) studies the test taking and performance of four cohorts of applicants between 2005 and 2008 and find that participation did not increase either the number of Regents exams or the number of exams passed among first-time applicants. Youth who participated in the program for more than one summer do benefit, though.

significant at the 10% level.

Our paper makes several contributions to the literature. First, we use more detailed education data than previously available, allowing us to look at a broader set of educational outcomes altogether than any other single study. Second, in an effort to understand contradictory findings in the prior literature, we provide some additional evidence as to which subgroups benefit the most from summer employment programs. Finally, our study is the first to evaluate the summer youth employment program in Detroit, Michigan. While it is similar to those in NYC and Boston in terms of implementation, the program in Detroit serves a less resourced population than these other cities.[3] For example, according to the 2016 American Community Survey, 51% of children in Detroit live in poverty, compared to 27% in NYC, 31% in Boston and 28% in Chicago. Furthermore, 6th graders in Detroit score 2.3 grade levels below the national average on standardized tests, relative to 0.3 grade levels in NYC and Boston and one grade level in Chicago (Reardon, Ho, Shear, Fahle, Kalogrides, and DiSalvo, 2017). Therefore, youth in Detroit may benefit from having a summer job more than those in other cities.

3 Institutional Background

The current iteration of Detroit's summer youth employment program began in 2009 with federal stimulus funding provided at the onset of the Great Recession. It has provided summer jobs to over 15,000 youths between 2015 and 2017. The program, commonly known as Grow Detroit's Young Talent or GDYT, employs young adults for 20 hours per week for six weeks from July through August, at between $8 and $9.50 per hour depending on age and job type. Youth selected for the program receive 24 hours of work readiness training before and during their employment.

[3] The program in Chicago has more of an emphasis on a social-emotional learning curriculum than programs in other cities. Participants in Chicago spent up to 40% of their hours engaged with the curriculum. In contrast, participants in NYC, Boston and Detroit spent about 10-20% of their hours in programming about work readiness.

All Detroit residents between the ages of 14 to 24 are eligible to apply for the program. The application period begins in February and is open for five weeks. It is widely advertised in the city as the Mayor's office holds a kickoff event and works with schools, religious leaders and community organizations to recruit a pool of applicants. About 10% of the eligible population applies for the program.[4] The application is very simple; in it, youth provide basic demographic information, indicate whether they have any past work experience and specify their interest in different industries. The application does not include a resume or personal statement and does not ask for details about past job responsibilities or school achievement.

GDYT consists of four sub-programs, each of which has a distinct selection process and involves a different summer experience. Although there may be meaningful differences in how each influences educational outcomes, we can not explore heterogeneity across sub-programs in our analysis. Except for the Junior Police and Fire Cadets program, we do not have data on which particular sub-program youth participated in.

3.1 Junior Police and Fire Cadets

The Junior Police and Fire Cadets program (JPC) is reserved for 14 to 15 year olds, and is structured to provide a first work experience for youth. Working with JPC includes a variety of community service activities, including providing support and companionship to seniors and cleaning parks and other neighborhood commons. College students serve as day-to-day supervisors for the young adults, and police officers interact with the youth in various capacities throughout the summer. JPC employees make up 20 percent of GDYT participants.

According to our conversations with program staff, youth are not purposely chosen for this program. Instead, staff pick a (somewhat) random set of 14- and 15-year-old applicants to GDYT and invite them to participate. When we compare pre-program characteristics of

[4] According to authors' calculations from the 2015 ACS population estimates.

JPC participants to age-eligible youth who applied but did not participate in GDYT, we find they are quite similar, though not identical. As reported in Table A1, there were no statistically significant differences in academic performance, including attendance and test scores, between these groups, yet JPC participants lived in lower income neighborhoods.

3.2 Community-Based Organizations (CBOs)

The largest group of GDYT youth — roughly 60 percent — work for one of many non-profit community-based organizations (CBO) in Detroit, similar to the employment models in NYC and Chicago. The work covers a wide range of activities, from camp counselor for younger children to clerical office support to community beautification. These organizations recruit applicants on their own, frequently selecting youth with whom they have a long-term relationship (e.g., those who participated in programming during the academic year with the CBO, or were known to staff in the neighborhood).[5] CBO participants typically range in age from 15-18 years old.

3.3 Industry Led Training and Career Pathways Programs

The final two opportunities for employment are through Industry Led Training (ILT) and Career Pathways internships. The ILT program is a new and relatively smaller component for applicants at least 16 years old, with about 10% of GDYT youths participating each summer. It consists of work-based training programs in high-growth sectors (e.g., hospitality, child care, IT, advanced manufacturing, healthcare), which are typically run by non-profit organizations in Detroit. The Career Pathways program provides an internship for older applicants, usually at least 19 years of age, at a variety of major private sector employers in the city, including Detroit Manufacturing Systems, Touchpoint Support Services, and Wayne State University. To be eligible for an ILT program or Career Pathways internship, applicants

[5] Youth who are recruited to participate in a CBO summer program must still submit a GDYT application. Therefore, we have complete data on these applicants.

must have had some prior work experience, and be referred by some other organization. Eligible youths are invited to attend a career fair to meet the employers, and then employers select who they want to hire.[6] Given this selection process, we assume that applicants who participate in the ILT and Career Pathways programs are likely to be quite different than those who do not.

4 Data and Sample

This study uses administrative records from both GDYT, the Michigan Department of Education (MDE) and the Center for Educational Performance and Information (CEPI). From GDYT, we have application and participation data from the program for the summer of 2015. The application data consists of all applications which were started during the submission window between February and March of 2015, regardless of whether they were completed.[7] We define applicants as those who completed the entire application during the submission window or, in some rare cases, as those who did not complete an application during the window yet still participated in the program.[8] In total, there were 12,255 applicants in 2015.[9]

We matched the application data to payroll data using exact matches on first and last name. The payroll data was maintained by multiple organizations that managed records for the youth who worked during the summer. Some of the payroll systems were unreliable and crashed repeatedly over the course of the summer. Therefore, we are missing important information about job placement, hours worked and earnings. We consider any youth who

[6] GDYT staff assigns any remaining spots to other eligible youths.

[7] Only 2.44% of applications were incomplete. 97% of these stalled after the first step of the application, which only asked for first name, last name and email address. These incomplete applications do not contain sufficient information to match youth to the education data, so we drop them from our analysis.

[8] Youth who are recruited to participate in a CBO summer program can apply after the submission window ends and still work with GDYT in the summer. In total, 0.79% of completed applications were submitted after the submission window.

[9] While we do not have information on whether applicants in 2015 applied or participated in prior summers, data from a different summer indicates that about 30% of all applicants, and 42% of participants, participated in the program during the previous summer.

appeared in a payroll system to have participated with GDYT.[10] In total, 2,807 youths worked in 2015.[11]

We matched the GDYT data to administrative records from MDE and CEPI consisting of the universe of K-12 public school students in the state from 2003 to 2017. We used a quasi-probabilistic matching algorithm based on first name, middle initial, last name, suffix, date of birth and gender. We successfully matched 94% of applicants to the education data, and exclude youth who did not match from our main analysis.[12] The education records contain information on school enrollment, attendance, test scores and graduation. They are linked to data from the National Student Clearinghouse, which provides information on college enrollment. We analyze the effect of participation in GDYT on a variety educational outcomes, including K-12 enrollment, attendance, expulsions, taking the SAT, SAT score, high school graduation and college enrollment.

Table 1 shows summary statistics for four groups. Column 1 shows characteristics for all Detroit residents in 8th to 12th grade who were enrolled in a public school during the 2014-2015 school year, while column 2 focuses only on the youth who applied for GDYT. Comparing column 1 to column 2 provides insight about who applied for the summer program.[13] Applicants were disproportionately black, as 95% of those who applied for the program were black compared to 86% of Detroit youths. Females were also more likely to apply. To draw comparisons between student needs in school as well as the neighborhoods

[10] Although we do not have information on hours worked for the 2015 cohort, data from a different summer indicates that 12% of youth in the payroll system did not ever show up to work. Youth who appeared in the payroll data but did not actually work creates classical measurement error in our indicator for participation with GDYT. This would attenuate our estimates of the effects of program participation toward zero.

[11] The number of participants reported in official GDYT reports is greater than what we report here because the GDYT counts include youth who are employed by affiliate organizations, a separate employment model that isn't captured in the available application or payroll records.

[12] Applicants may not have matched to the education data because (1) they only attended private K-12 schools in Michigan, (2) they moved to Michigan after high school, or (3) the combination of their name, birthdate and gender did not provide enough information to identify a match. Of the 6% of applicants who did not match to the education data, about 80% were 18 years old or younger when they applied, suggesting that the non-matches were not driven by (2). We cannot disentangle explanations (1) and (3) with our data.

[13] The distribution of age and educational status between these groups differs by construction, as column 1 is restricted to middle and high school students while column 2 includes all applicants, regardless of their age or school enrollment status.

where they live, we measure applicant characteristics in the most recent pre-program year that they were enrolled in a public K-12 school. Although applicants and Detroit residents overall were similar in terms of eligibility for free or reduced priced lunch, there are notable differences in terms of where they live and how they performed on standardized tests. Applicants lived in relatively more advantaged neighborhoods; 13% of adults had a Bachelors degree and 33% of households earned below the poverty line, compared to 12% and 35% respectively for all Detroit youths. Similarly, applicants were more likely to take standardized tests in 8th grade, and those who took them scored higher. Overall, this comparison shows that applicants were a somewhat positively selected group of Detroit youths.

Since youth who applied for the program were different than those who did not along a variety of observable dimensions, our empirical strategy compares youth who participated in GDYT to those who applied but did not participate. Comparing columns 3 and 4 of Table 1 shows that these two groups look nearly identical in terms of age and academic enrollment during the 2014-2015 school year, and quite similar in terms of race/ethnicity, gender and needs in school. Participants and non-participants lived in very similar neighborhoods, where about one third of households live below the poverty line. While participants were slightly less likely to take standardized tests in 8th grade, those who took the tests were more likely to be proficient, suggesting that they were similar in terms of academic performance overall.

5 Empirical Strategy

The key empirical challenge in our analysis is to control for pre-program differences between participants and other applicants that directly influence educational outcomes, and would bias our impact estimates. We use a combination of exact matching and regression adjustment to control for such differences.

We create match groups consisting of all applicants who were the same race and gender, and also were in the same grade in the same school in the most recent pre-program year that they attended a public K-12 school in Michigan.[14] For example, if a student was in 11th grade during the 2014-2015 school year, then his or her match group consists of all of the other 11th grade applicants from their school who were of the same race and gender. Using this example, we define the match year as the 2014-2015 school year and the match school as the school they attended during the match year. Similarly, if a student was in 12th grade in 2012-2013 and graduated high school that year, then their match group consists of all of the other applicants who were also in 12th grade in their school in 2012-2013 of the same race and gender.

Of all participants, 81% are in match groups with at least one participant and one non-participating applicant, and thus provide variation with which we can identify effects of the program. As shown in Table 2, these youth differ in several ways from other applicants, which influences the generalizability, or external validity, of our analysis. Relative to the participants in a degenerate match group, those in a match group with at least one participant and non-participant were substantially younger, more likely to be black and lived in a somewhat higher poverty neighborhood.[15]

Because our identification is driven by within match group differences between participants and other applicants, it is useful to compare these two groups in terms of pre-program characteristics. To do so, we estimate the following model

$$X_{ij} = \alpha_0 + \alpha_1 Participated_{ij} + \gamma_j + \nu_{ij} \tag{1}$$

[14] The most recent pre-program year an applicant attended a public K-12 school in Michigan (match year) was almost always (1) the 2014-2015 school year, or (2) the year that they graduated or dropped out of high school. 81% of applicants have match years of 2014-2015, 11% have match years of 2012-2013 or 2013-2014 and 8% have match years before the 2012-2013 school year. In some rare cases, though, the match year represents when an applicant moved out of state or began attending private school. As a result, 1% of applicants have match groups which consist of their classmates when they were in 6th grade or below, even though they applied for GDYT when they were much older. In addition, there were 10 applicants who first enrolled in a Michigan public school after the 2014-2015 school year. We exclude them from the analysis since we cannot construct match groups for them without pre-program information.

[15] For example, 10% of youths in a non-degenerate match group were older than 18, 98% were black and 34% of households in their neighborhoods lived below the poverty line, compared with 30%, 89% and 30% respectively for youth in a match group with only participants or non-participants.

where X_{ij} represents a baseline characteristic of youth i in match group j, and $Participated_{ij}$ is a binary indicator for participation. Lastly, γ_j is the full set of match group fixed effects. We give participants a weight of one and other applicants a weight equal to the ratio of participants to non-participants in their match group.[16] This weighting scheme accounts for the fact that match groups vary in terms of the proportion of participants to non-participants. We cluster standard errors by match school.[17]

Table 3 shows the results of the balance tests estimated using equation 1. Column 1 shows the comparison mean, the weighted average of the baseline characteristic for non-participants, and column 2 shows α_1, the coefficient on the indicator for participation. Participants look quite similar to non-participants along most observable dimensions.[18] For example, 14.5% of participants and 13.2% of non-participants received special education services in the match year and 82.9% of participants were eligible for free or reduced price lunch compared to 82.7% of non-participants. The differences in these baseline characteristics, as well as in neighborhood poverty rate and those related to 8th grade standardized tests, are not statistically significant. However, there are meaningful differences between participants and non-participants in terms of baseline attendance. Participants had higher attendance rates by 1.5 percentage points and were 4.4 percentage points less likely to be chronically absent.[19] To address these differences, we control for a rich set of baseline controls, including baseline attendance and chronic absenteeism, in our main analysis.

In our context, testing for mean differences in baseline characteristics may not be very informative as to how similar participants and non-participants were before the program. Some CBO's employ youth who are particularly 'at risk' while others target those who are

[16]That is, if a match group contains two participants and six non-participants then we give each participant a weight of one and each non-participant a weight of $\frac{1}{3}$. Similarly, if a match group contains three participants and two non-participants then we give each participant a weight of one and each non-participant a weight of $\frac{3}{2}$.

[17]All of our results are robust to using an unweighted regression model as well as to clustering standard errors by the zipcode where youth resided in the match year or by the school they attended during the 2015-2016 school year.

[18]We do not test for differences along race/ethnicity, gender or age because the match group fixed effect ensures that participants and non-participants are comparable along these dimensions.

[19]We define chronic absenteeism as having an attendance rate less than 90%.

especially high-achieving. Therefore, it is possible that participants and non-participants look similar on average, even though they are actually quite different. To address this concern, Figure 1 shows kernel density plots of the entire distribution of four baseline characteristics, separately for participants and non-participants.[20] They are residualized by match group and are restricted to the sample of youth who are in a match group with at least one participant and one non-participant. The figures show that there exists a common support between participants and non-participants; participants are not drawn from the two extremes of the distribution.

Having established that participants and non-participants within match group are quite similar on most observable dimensions, we estimate the impact of participation in the program with the following regression model

$$Y_{ij} = \beta_0 + \beta_1 Participated_{ij} + \beta_2 \mathbf{X_{ij}} + \gamma_j + \epsilon_{ij} \tag{2}$$

where outcome Y for youth i in match group j is a function of a binary indicator for participation (i.e., worked in the GDYT program during the 2015 summer), and other covariates. We include a vector of match group fixed effects, γ_j, which ensures we are comparing outcomes within $race * gender * school * grade$ cells. In addition, we control for a rich set of covariates, $\mathbf{X_{ij}}$, consisting of all of the information from the application, some information not from the application but likely observable to program administrators, as well as information that was not available to program staff during the selection process. The information from the application that we control for includes an indicator of whether the applicant graduated from high school before the program as well as linear and quadratic terms of age as of July 1, 2015. Program administrators could infer neighborhood characteristics from the application, so we use controls, measured at the census block group level, for the fraction of adults with at least a Bachelor's degree, the fraction of households living

[20] The kernel density plots for the binary characteristics in Table 3 look similar to those in Figure 1 but are much less smooth. They are available upon request.

in poverty, the fraction of households that are owner-occupied and the employment rate.[21] Finally, program staff did not have access to past academic records, yet we use academic controls, including attendance rate, binary indicators for chronic absenteeism, special education status, limited English proficiency, eligibility for free or reduced price lunch, ever retained in grade, as well as standardized math and reading test scores and an interaction of math and reading scores.[22] We include indicators for missing control variables. As before, we give participants a weight of one and other applicants a weight equal to the ratio of participants to non-participants in their match group. We cluster standard errors by match school.

The coefficient of interest, β_1, represents the difference in average outcomes between participants and those who applied but did not participate. β_1 represents the true causal effect of program participation if the conditional independence assumption holds. There cannot be any unobservable differences between participants and non-participants that directly influence educational outcomes. While we cannot explicitly test this assumption, we can probe it. There are two ways in which we examine the robustness of our results to omitted variable bias. First, we implement the tests proposed in Oster (2016) to determine the extent to which selection on unobservables explains our results. We discuss these tests in detail in section 6.1.

Second, if participants differ from non-participants along unobservable dimensions, even after controlling for match group fixed effects and the rich set of covariates listed above, we might expect participation to predict youth outcomes prior to the program. Therefore, we estimate variants of equation 2 where the dependent variable is a pre-program educational outcome. We focus on 6th grade outcomes, as 99.9% of youths applied after completing the sixth grade. Weighting and standard errors are handled as before.

Table 4 shows the results of these falsification tests. Column 1 reports the weighted average of the 6th grade outcome for non-participants and column 2 shows the coefficient

[21] Unless otherwise specified, all of the pre-program characteristics are measured in the match year.

[22] We use 8th grade test scores whenever possible. If the applicant did not reach 8th grade before they applied or did not take the tests in 8th grade, we use 7th grade scores. If neither 7th nor 8th grade scores are available, we use 6th grade scores.

on the indicator for participation. Participation does not predict prior student outcomes, including attendance and performance on standardized tests. This exercise suggests that it is unlikely that there are important omitted variables which confound our impact estimates.

6 Effects of Participation in GDYT

We assess the effect of participation in GDYT on a variety of short-run educational outcomes. We observe outcomes during two follow-up school years, 2015-2016 and 2016-2017. Overall, we find consistently positive effects of program participation on enrollment, test taking rates and high school graduation, with no evidence of subsequent decreases in attendance or test scores.

Table 5 reports the effect of participating in GDYT on educational outcomes, showing the estimates from equation 2. Since applicants range in age from 14-24, each outcome is defined only for a certain set of youth.[23] We describe the sample who were eligible for each outcome in column 1 and the number of youths in the sample in column 2.[24] Column 3 shows the weighted average of the outcome for non-participants and column 4 displays the cumulative effect of participation in GDYT in the two years following the program. While we focus our discussion on the effects after two years, we show estimates of the effect of participation after each follow-up year separately in Table A3.

Participation in GDYT increased the likelihood of being enrolled in a Michigan public school by 1.4 percentage points. In the two years after the program, 95.9% of participants remained enrolled compared to 94.5% of non-participants, a difference which is statistically significant at the 1% level. Although we cannot directly test it, this is likely driven by a reduction in dropping out of school. It is unlikely that this is a result of differential mobility out of state or enrollment in private school given that we find even larger positive effects

[23]For example, we do not analyze the effect of participation on subsequent enrollment in K-12 for a 19 year old participant who already graduated high school.

[24]Table A2 further describes the sample of eligible youths for the outcomes in each of the first two follow-up years and clarifies the formula used to calculate the cumulative measure of the outcomes after the first two follow-up years.

on high school graduation from a Michigan public school, which will be discussed in further detail below.

We interpret our results as the effect of working with GDYT compared to a 'business as usual' control group. We do not observe the employment status of non-participants during the 2015 summer, so we do not know whether they worked outside of GDYT or did not have a job at all.[25] However, all of the prior studies of summer youth employment programs suggest that participation increases the likelihood of having any job during the summer and we have little reason to suspect that this result would not generalize to Detroit (Gelber, Isen, and Kessler, 2016; Davis and Heller, 2017; Modestino, 2017).

Participation in GDYT did not influence daily attendance rates or chronic absenteeism; we find precise zero point estimates for these outcomes. Students who remained in public school were no more or less likely to show up throughout the school year. If the marginal student were any less qualified, prepared or resourced to remain in school, then they might have had lower attendance rates or higher chronic absenteeism, conditional on enrollment. This is not what we find, however. In addition, we do not find evidence that participation influenced the likelihood of expulsion.

Michigan offers the SAT for free and during school hours, as part of its 11th grade standardized tests. Some students still do not take the test, though, due to exemptions as a result of special education plans or absences on test days. GDYT participants were 3.4 percentage points more likely to take the SAT than non-participants, representing a 4.7% increase from a comparison mean of 72.9%. This estimate is significant at the 10% level. Some, but not all, of the increase in test taking is driven by increased enrollment in school, yet the magnitude of the effect on enrollment is less than half the size of the effect on taking the SAT. Importantly, there was no statistically significant effect on SAT scores among those

[25]Some non-participants were offered a summer job with GDYT but declined, although we also do not observe offers to participate with GDYT. There are a few reasons why someone who received an offer would not participate: 1) they did not complete registration paperwork, 2) they were selected for the ILT or Career Pathways Internship programs but did not show up to the career fairs, 3) they needed to attend summer school, or 4) they did not wish to participate.

who took the SAT.

Participation in GDYT had a large and positive effect on high school graduation. Participants were 3.4 percentage points more likely to graduate than non-participants, representing a 4% increase relative to a comparison mean of 85%. This finding stands in contrast to Valentine et al. (2017) which finds that summer youth employment did not influence high school graduation in NYC. As shown in Table A3, this is mostly driven by an increase in the second follow-up year.[26] Finally, we find meaningfully large, yet statistically insignificant, estimates of participation on college enrollment for applicants who had graduated high school before applying. They suggest that participation increased the likelihood of enrolling in college by 8%. This is driven by an increase in four year college enrollment.

6.1 Robustness to Omitted Variable Bias

Although we use both matching and regression adjustment to control for many observable differences between participants and other applicants, our estimates may still suffer from omitted variable bias. Unobservable differences between these groups could come both from the labor supply side, if the most motivated or committed youth who were offered jobs chose to participate, and from the labor demand side, if employers chose to hire the best applicants based on unobservable features like personality. We expect either to lead us to overstate the positive effects of the program.

In order to test the extent to which omitted variable bias may be driving our results, we perform two exercises proposed by Oster (2016). Building on earlier work by Altonji, Elder, and Taber (2005), Oster proposes that, under some assumptions, one can identify a consistent estimator of the bias. In particular, Oster suggests two complementary methods

[26]This is probably because 11th grade applicants were likely to graduate regardless of participation in the program, whereas the program had a greater influence for 10th grade applicants. However, an alternative explanation could be that there is a cumulative effect of participation, whereby working with GDYT for more than one summer has a larger effect on graduation than working for a single summer. This would be consistent with findings from the NYC program in Schwartz et al. (2015). We do find that 10th grade participants in 2015 were more likely to work with GDYT during the 2016 summer. Descriptively, though, there was not a larger increase in high school graduation among youth who participated in both the 2015 and 2016 summers. Results are available upon request.

to assess the robustness of results to omitted variable bias. The first is to generate a bias-adjusted treatment effect, which represents the value of the treatment effect assuming a given degree of selection on unobservables. Researchers can bound the true treatment effect using the bias-adjusted treatment effects. The second is to examine the amount of selection on unobservables, relative to selection on observables, that would need to exist for the true treatment effect to be equal to zero. If a large amount of selection on unobservables is needed, then the treatment effect can be considered robust to omitted variables bias.

Both of these exercises require a proportional selection assumption, whereby selection on unobservables is proportional to selection on unobservables. The former exercise also assumes that the omitted variable bias does not change the direction of the covariance between the observables and the treatment. This holds as long as the bias not too large. An important caveat with these exercises is that they only address omitted variable bias created by unobservables that are related to observable characteristics. In the context of this study, it seems unlikely that there are unobservables which are orthogonal to prior academic records or socio-demographic characteristics.

Table 6 reports the results of these two exercises.[27] Column 1 shows our estimate of the effect of program participation, as previously reported in Table 5. This is equivalent to the estimate of the treatment effect if there were no omitted variable bias. Column 2 shows the bias-adjusted treatment effect, assuming that the amount of selection on unobservables is equal to the amount of selection on observables.[28] Both Oster (2016) and Altonji, Elder, and Taber (2005) suggest this as an upper bound on the amount of omitted variable bias. Together, columns 1 and column 2 report bounds for the true treatment effect. Finally, column 3 shows the amount of proportional selection needed such that the treatment effect equals zero. Values smaller in magnitude than one mean that selection on unobservables

[27]We used the STATA package *psacalc* for these calculations.

[28]We also assume that the maximum R^2 from a regression of the outcome on the observable characteristics and all unobservables would be equal to one. Assuming a smaller R^2, perhaps due to measurement error in the outcome variable, would only move the bias-adjusted treatment effect away from zero, suggesting this may be a conservative approach.

would not need to be as large as selection on observables, whereas values larger than one mean that selection on unobservables would need to be larger than the amount of selection on observables.[29]

Our impact estimates do not appear to be driven by omitted variable bias. We can bound the effect on public school enrollment between 0.8 and 1.4 percentage points. Selection on unobservables would need to be almost twice as large as selection on unobservables in order for the true effect to equal zero. Consistent with earlier results, the amount of bias needed to generate an effect of zero is much smaller for attendance and expulsion outcomes.

In addition, the estimates for taking the SAT and graduating high school are robust. Selection on unobservables would need to be over twenty times as large as selection on observables for the effect on test-taking to equal zero, and almost four times as large for the effect on graduating high school. We can bound our estimate for graduating high school between 2.9 and 3.4 percentage points, which still represents a large increase of at least 3.4% relative to the comparison mean of 85%. Although estimates for the effect of participation on college enrollment were not statistically significant, these exercises suggest that the substantively large estimate on college enrollment for applicants who graduated high school was unlikely to be driven by omitted variable bias.

6.2 Subgroup Analysis

While we find positive effects of the summer youth employment program overall, we also seek to identify for whom the program was most beneficial. We explore heterogeneity along four dimensions: grade in school, gender, prior academic achievement and prior chronic absenteeism. First, we compare the effect of participation for those in 9th grade or below when they applied to those who were in 10th or 11th grade because previous studies find differential effects by age.[30] Second, we compare males to females because of growing gender

[29]Negative values indicate that selection on unobservables must operate in the opposite direction as selection on observables for the treatment effect to equal zero.

[30]Leos-Urbel (2014) finds that, conditional on low baseline attendance, youth age 16 and older benefited more from the program than younger participants. Modestino (2017) finds that youth older than 16 benefited

gaps in non-cognitive skills and educational attainment (Jacob, 2002; Goldin, Katz, and Kuziemko, 2006). Finally, we examine differences based on both prior academic achievement and prior chronic absenteeism because of past findings of heterogeneity by levels of school engagement.[31] Since test scores and attendance offer different, albeit correlated, measures of engagement, we study these two groups separately.

We tested for balance in pre-program characteristics for each subgroup and show the results in Table A4. Consistent with our main analysis, participants and other applicants look quite similar along most observable dimensions, yet participants had higher attendance rates and were less likely to be chronically absent. As before, we control for a rich set of covariates, including baseline attendance rate and chronic absenteeism, to address these differences. We also conduct the same falsification tests as in our main analysis to check whether participation predicts past academic outcomes. We report the results in Table A5. While we expect to find some statistically significant differences as we are testing many hypotheses, participation is generally not predictive of prior outcomes. Therefore, it is unlikely that there are important unobserved factors which bias our subgroup impact estimates.

Table 7 shows the effect of participation in GDYT on a handful of key educational outcomes for each subgroup in the two years after the program.[32] The program was beneficial for students who were in 9th grade and below when they applied (column 1) as well as for those who were in 10th and 11th grade (column 2). The magnitude of the effect on subsequent enrollment was similar for both groups, although the estimate for younger participants is not

more from participation as well. Davis and Heller (2017) also find that those who benefited most from the program were 16-17 year olds, although their analysis primarily compares this group to participants age 18 and older. In Detroit, 16-17 year olds tend to be in grades 10-12, so our analysis is closer in spirit to Leos-Urbel (2014) and Modestino (2017), in that we compare these youths to younger participants.

[31]While Leos-Urbel (2014) finds that students with low baseline attendance benefit most, Davis and Heller (2017) conclude that the program was most beneficial for youth with higher GPAs and attendance. These studies use very different definitions of attendance, though. Leos-Urbel (2014) focuses on youth with attendance rates less than 95% while Davis and Heller (2017) find that youth who had average pre-program attendance rates of 77% (attended 139 days of school out of 180) benefited more than those who had average attendance rates of 63% (attended 114 days).

[32]Table A6 reports similar results for the full set of outcomes.

statistically significant. Younger participants were 1.0 percentage point more likely to remain enrolled in school, compared to a 1.7 percentage point increase for their older counterparts. Older students who enrolled were more likely to be chronically absent than their younger peers, however. This was likely driven by the enrollment of older students who otherwise would have dropped out; although they remained in school, they were less likely to show up. Despite higher rates of chronic absenteeism, we find that participation still increased the educational attainment of older youths. They were 3.4 percentage points more likely to graduate high school. We conclude that having a summer job with GDYT improved the educational outcomes for middle and high school students.

Male and female participants benefited from the program in different ways. While both males (column 3) and females (column 4) enrolled in school at similar rates, participation increased chronic absenteeism for males and reduced it for females. Although neither is statistically different from zero, they are close to being statistically different from each other, with a p-value of 0.119. Surprisingly, participation may have increased the gender gap in high school graduation by a modest amount, yet reduced the gap in college enrollment by even more.[33] The main effect of GDYT on high school graduation was driven by an increase for females, who were already more likely to graduate. Although our estimates on college enrollment are not statistically significant, they are consequential. If taken literally, they suggest that participation reduced the gender gap in college enrollment by 60%, from 11.5 to 4.6 percentage points.

To examine differences based on prior academic achievement, we compare youth who scored above the median on their 8th grade math test (column 5) to those who scored below it (column 6).[34] The benefits of participation were largest for those who entered high school

[33]This appears to be because these outcomes are defined for different samples. That is, the high school graduation outcome is defined for 10th and 11th grade applicants while college enrollment is defined for 11th and 12th grade applicants. When we limit the analysis to 11th grade applicants in order to hold the sample constant, we find that the point estimate for high school graduation is slightly larger in magnitude for males than females, while the estimates for college enrollment for each group are similar to those reported in Table 7.

[34]This analysis only includes youth who reached 8th grade by the 2014-2015 school year. We define the median here as the median among all 8th grade math scores in our sample. Test scores were normalized

with the weakest academic skills. Participation increased school enrollment by 2.1 percentage points and high school graduation by 4.4 percentage points among lower-achieving youths. In contrast, it did not have a meaningful effect on educational outcomes for higher-achieving participants. The differences between these groups in terms of school enrollment and high school graduation are statistically significant at the 10% level. Consistent with these results, we also find that those who were not chronically absent before the program (column 7) benefited less from participation than those who were (column 8). Our estimates of the effects on school enrollment and high school graduation were larger for youth who were chronically absent. Similar to our previous findings, though, despite positive effects on other outcomes, participation did increase chronic absenteeism among youth who were chronically absent before the program. Overall, we conclude that the program was particularly beneficial for those with the weakest academic achievement and lowest attendance.

7 Conclusion

The labor market continues to present challenges for young, low-income, and less educated workers. This has spurred a growing interest in finding new ways for young adults to obtain career skills, both inside and outside of school. Summer youth employment programs represent one such intervention. In Detroit, the Grow Detroit's Young Talent program has provided over 15,000 summer work opportunities to youth between 2015 and 2017.

This study is complementary to prior evaluations of similar programs. Despite the prevalence of summer youth employment programs across the United States, there is surprisingly limited evidence of their effectiveness, particularly in terms of whether participation influences educational outcomes. Despite findings from New York City, Chicago and Boston which suggest that there were small, if any, impacts on education, we find that the program in Detroit increased education attainment. While the implementation of GDYT is similar

within each year so they are comparable across years. Our results are robust to using reading scores instead of math scores.

to the programs in NYC and Boston, the cities themselves are very different. Detroit is a less resourced city than these other sites. The poverty rate is higher, the schools are of lower quality in terms of standardized test scores and the city has uniquely struggled with population decline over the past several decades (Infoplease, 2017). Our analysis suggests that in this context, providing jobs during the summer can change the educational trajectories of young adults.

We find that youth who participate in GDYT experience consistently better educational outcomes than their classmates who applied but did not participate. GDYT increased subsequent enrollment in a Michigan public school by 1.5%, likely driven by a reduction in dropping out. Even with increased enrollment, participants were no more or less likely to be chronically absent. Most importantly, participation increased high school graduation by 4% relative to a comparison mean of 85%. Following two exercises proposed in Oster (2016), these estimates are not driven by selection bias from unobservables characteristics that are related to observables. In addition, those who entered high school with the weakest academic skills benefited the most, as participation increased school enrollment and high school graduation by 2.2% and 5.5% respectively for this group. Overall, the benefits of participation in GDYT continued long after youth received their last paycheck.

Our study has several limitations. First, we focus on only a single cohort of applicants and follow them for only two years after the program. As we continue our partnership with GDYT, we plan to study more cohorts for a longer follow-up period in future work. Second, our study relies on a selection on observables identification strategy, which is considered a less credible design than those used in evaluations of other summer youth employment programs. We are currently working with GDYT to randomly assign a subset of summer jobs to applicants, which will allow for a more refined evaluation in the future. However, after controlling for a rich set of covariates and using matching methods, our analysis shows that it is unlikely that there are important omitted variables which bias our estimates. Finally, we do not address whether participation influenced outcomes other than education. In order to conduct a cost-benefit analysis of the program, it will be important to examine its effect on employment, crime, and health in future work. We are working to establish partnerships to bring in data about these other indicators of wellbeing for future work.

As policymakers from all levels of governance look for ways to improve the job prospects for many people, and particularly for young people, it is important to understand whether summer youth employment programs offer a pathway to success in the labor market.

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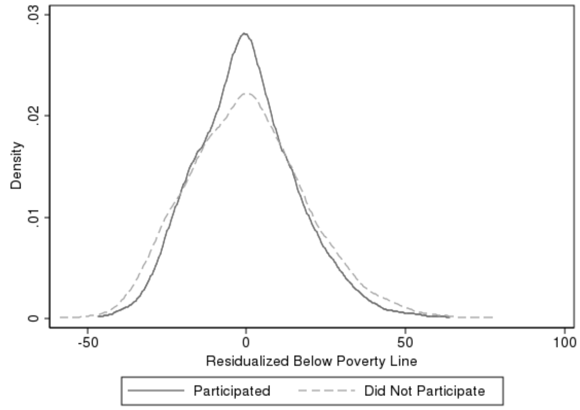
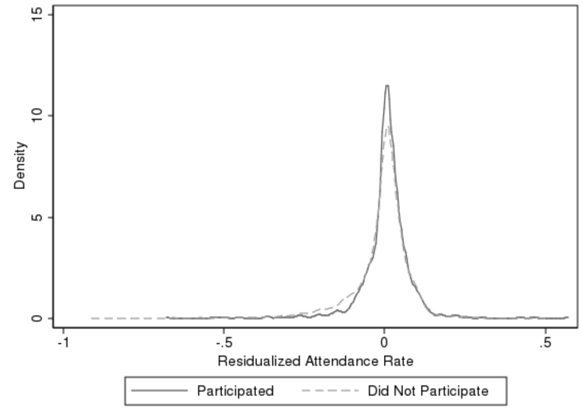
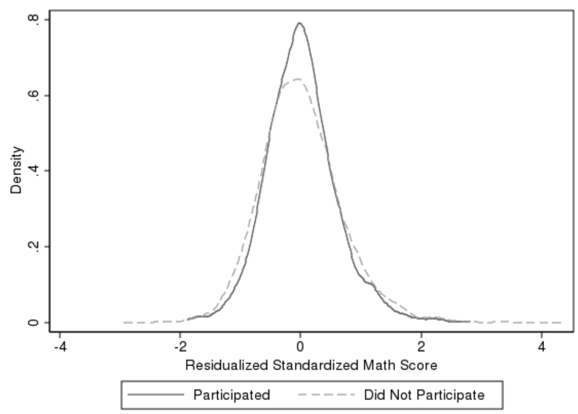
Figure 1: Distribution of Baseline Characteristics for Participants and Non-Participants



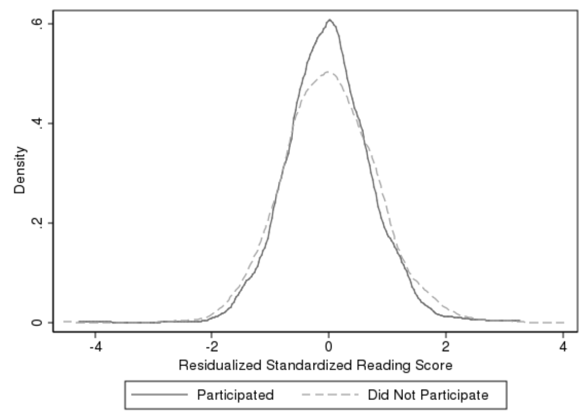
(a) Below Poverty Line in Neighborhood



(b) Attendance Rate



(c) Standardized 8th Grade Math Score



(d) Standardized 8th Grade Reading Score

Notes. Each figure shows a kernel density plot of the distribution of a baseline characteristic for participants and non-participants, residualized by match group. The sample is restricted to youth in match groups with at least one participant and non-participant. All baseline characteristics are measured in the match year, except for 8th grade test scores. Youth who did not reach 8th grade before the program are not included in the analysis of 8th grade test scores.

Table 1: Summary Statistics

	(1) Detroit Grades 8-12	(2) All Applicants	(3) Participated	(4) Did Not Participate
Total	44394	12255	2807	9448
Matched to Education Data	1.00	0.94	0.90	0.95
Age as of July 1, 2015				
15 and Younger	0.41	0.38	0.38	0.37
16-18	0.52	0.46	0.45	0.46
19 and Older	0.07	0.16	0.16	0.16
Educational Status				
Grade 9 and Below	0.43	0.34	0.33	0.34
Grades 10-11	0.39	0.35	0.36	0.35
Grade 12	0.18	0.12	0.12	0.12
Enrolled In College	0.00	0.08	0.09	0.08
Not in School	0.00	0.10	0.10	0.11
Demographics				
Black	0.86	0.95	0.90	0.96
Hispanic	0.08	0.03	0.07	0.02
White	0.04	0.02	0.03	0.01
Asian	0.01	0.01	0.00	0.01
Female	0.51	0.58	0.55	0.60
Needs in School				
Limited English Proficient	0.08	0.03	0.05	0.02
Special Education	0.15	0.12	0.15	0.12
Free or Reduced Priced Lunch	0.82	0.81	0.82	0.81
Neighborhood Characteristics				
BA Degree or Higher	11.94	13.30	12.69	13.47
Below Poverty Line	35.15	32.82	33.02	32.77
Owner Occupied Housing	40.05	41.31	40.84	41.45
Employed (16 and over)	75.10	75.10	75.15	75.09
8th Grade Test Scores				
Took Math Test	0.84	0.88	0.86	0.89
Proficient Math	0.09	0.10	0.11	0.09
Math Score	-0.67	-0.62	-0.60	-0.62
Took Reading Test	0.84	0.87	0.86	0.88
Proficient Reading	0.37	0.41	0.42	0.41
Reading Score	-0.55	-0.46	-0.46	-0.46

Notes. Column 1 shows summary statistics for all 8th to 12th graders who lived in Detroit and attended a Michigan public school during the 2014-2015 school year, regardless of whether they applied to GDYT. Column 2 describes GDYT applicants for the 2015 summer while columns 3 and 4 describes GDYT participants and GDYT applicants who did not participate, respectively. We report educational status in the 2014-2015 school, where not enrolled indicates that the youth was neither enrolled in public school in Michigan nor in college. Needs in school and neighborhood characteristics are measured in the most recent pre-program that a youth was enrolled in a public K-12 school in Michigan. Youth who did not reach 8th grade before the program are not included in the 8th grade test scores summary statistics.

Table 2: Summary Statistics, by Match Group Characteristics

	(1) Match Group with at Least 1 Participant and 1 Non-Participant	(2) Match Group with Only Participants or Non-Participants
Total	7960	3587
Age as of July 1, 2015		
15 and Younger	0.40	0.33
16-18	0.50	0.37
19 and Older	0.10	0.30
Educational Status		
Grade 9 and Below	0.36	0.29
Grades 10-11	0.41	0.21
Grade 12	0.13	0.09
Enrolled In College	0.06	0.15
Not in School	0.04	0.25
Demographics		
Black	0.98	0.89
Hispanic	0.02	0.05
White	0.00	0.04
Asian American	0.00	0.02
Female	0.59	0.56
Needs in School		
Limited English Proficient	0.02	0.04
Special Education	0.12	0.13
Free or Reduced Price Lunch	0.82	0.80
Neighborhood Characteristics		
BA Degree or Higher	12.69	14.65
Below Poverty Line	34.00	30.21
Owner Occupied Housing	39.98	44.28
Employed (16 and over)	74.39	76.67
8th Grade Test Scores		
Non-missing math score	0.89	0.86
Proficient Math	0.10	0.09
Math Score	-0.61	-0.63
Non-missing reading score	0.89	0.84
Proficient Reading	0.43	0.35
Reading Score	-0.45	-0.49

Notes. Column 1 shows summary statistics for youth who are in match groups with at least one participant and one non-participating applicant while column 2 describes youth in match groups with only participants or non-participating applicants. The sample is restricted to applicants who matched to the education data. We report educational status in the 2014-2015 school, where not enrolled indicates that the youth was neither enrolled in public school in Michigan nor in college. Needs in school and neighborhood characteristics are measured in the most recent pre-program that a youth was enrolled in a public K-12 school in Michigan. We define a neighborhood as a census block group. Youth who did not reach 8th grade before the program are not included in the 8th grade test scores summary statistics.

Table 3: Balance Tests of Differences Between Participants and Non-Participants

	(1) Comparison Mean	(2) Participated
Special Education	0.132	0.013
		(0.011)
Low Income	0.827	0.002
		(0.010)
% Below Poverty Line in Neighborhood	34.258	-0.053
		(0.556)
Attendance Rate	0.895	0.015***
		(0.004)
Chronically Absent	0.351	-0.044***
		(0.017)
Took 8th Grade Math Test	0.873	-0.006
		(0.010)
Proficient on 8th Grade Math Test	0.091	0.013
		(0.011)
Standardized 8th Grade Math Score	-0.631	0.019
		(0.025)
Took 8th Grade Reading Test	0.875	-0.007
		(0.009)
Proficient on 8th Grade Reading Test	0.406	0.016
		(0.017)
Standardized 8th Grade Reading Score	-0.476	-0.010
		(0.034)
N	11,547	

Notes. This table reports the results from a weighted regression of a baseline characteristic on an indicator for participated and match group fixed effects, where participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. Column 1 shows the weighted mean of the baseline characteristic for non-participants and column 2 shows the coefficient on the indicator for participation. All baseline characteristics are measured in the match year, except for 8th grade test scores. Youth who did not reach 8th grade before the program are not included in the analysis of 8th grade test scores. Standard errors are clustered by match school. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table 4: Falsification Tests of the Effect of Participation in GDYT on 6th Grade Educational Outcomes

	(1) Comparison Mean	(2) Participated
Attendance Rate	0.909	0.005
		(0.003)
Chronically Absent	0.337	-0.013
		(0.013)
Took Math Exam	0.937	0.003
		(0.006)
Proficient on Math	0.144	-0.011
		(0.011)
Std Math Score	-0.543	-0.039
		(0.024)
Took Reading Exam	0.941	-0.002
		(0.006)
Proficient on Reading	0.405	-0.002
		(0.014)
Std Reading Score	-0.525	-0.013
		(0.027)
N	11,531	

Notes. This table reports the results from a weighted regression of a 6th grade educational outcome on an indicator for participated, a vector of control variables as listed in the text and match group fixed effects, where participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. Column 1 shows the weighted mean of the baseline characteristic for non-participants and column 2 shows the coefficient on the indicator for participation. The sample is restricted to the 99.9% of youths who reached 7th grade before the program so that the control variables, most of which are measured in the match year, are not measured in 6th grade. Standard errors are clustered by match school. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table 5: The Effect of Participation in GDYT on Educational Outcomes

	(1) Description of Sample	(2) N	(3) Comparison Mean	(4) Participated
Enrolled in K12	11th grade or below in 2015	6,340	0.945	0.014***
				(0.006)
Attendance Rate	Enrolled in K-12 in 2016 or 2017	5,637	0.876	0.002
				(0.004)
Chronically Absent	Enrolled in K-12 in 2016 or 2017	5,637	0.409	-0.000
				(0.013)
Expelled	Enrolled in K-12 in 2016 or 2017	6,173	0.002	-0.001
				(0.001)
Took SAT	9th or 10th graders in 2015	4,080	0.729	0.034*
				(0.018)
SAT Composite Score	Took the SAT in 2016 or 2017	3,012	860.307	-0.164
				(3.791)
Graduated HS	10th or 11th grade in 2015	3,131	0.850	0.034**
				(0.014)
College from HS	11th or 12th grade in 2015	2,484	0.435	0.006
				(0.027)
2 Year College from HS	11th or 12th grade in 2015	2,484	0.210	0.012
				(0.026)
4 Year College from HS	11th or 12th grade in 2015	2,484	0.230	-0.008
				(0.018)
College as a HS Grad	Graduated HS before 2015	1,701	0.449	0.038
				(0.040)
2 Year College as a HS Grad	Graduated HS before 2015	1,701	0.216	0.015
				(0.032)
4 Year College as a HS Grad	Graduated HS before 2015	1,701	0.244	0.024
				(0.029)

Notes. This table reports the results from a weighted regression of an educational outcome on an indicator for participated, a vector of control variables as listed in the text and match group fixed effects, where participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. Column 1 describes the sample for whom the outcome is defined, column 2 shows the number of observations in the sample, column 3 shows the weighted mean of the baseline characteristic for non-participants and column 4 shows the coefficient on the indicator for participation. Standard errors are clustered by match school. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table 6: Robustness of the Effects of Participation in GDYT to Omitted Variable Bias

	(1) Observed Effect	(2) Bias-Adjusted Effect	(3) Proportional Degree of Selection for Effect of Zero
Enrolled in K12	0.014	0.008	1.892
Attendance Rate	0.002	-0.006	0.265
Chronically Absent	-0.000	0.028	0.012
Expelled	-0.001	0.002	0.536
Took SAT	0.034	0.036	22.633
SAT Composite Score	-0.164	0.754	0.183
Graduated HS	0.034	0.029	3.809
College from HS	0.006	-0.038	0.152
2 Year College from HS	0.012	0.059	-0.495
4 Year College from HS	-0.008	-0.053	-0.200
College as a HS Grad	0.038	0.15	1.404
2 Year College as a HS Grad	0.015	0.088	-0.490
4 Year College as a HS Grad	0.024	-0.012	0.720

Notes. This table reports the results of robustness checks proposed in Oster (2016). Column 1 reports the observed estimate of the effect of program participation on the outcome, without accounting for omitted variable bias. Column 2 reports the bias-adjusted treatment effect, assuming that selection on unobservables is as large as selection on observables. Column 3 shows the amount of the selection on unobservables, relative to selection on observables, necessary for the treatment effect to equal zero. Columns 2 and 3 were estimated using the STATA package *psacalc*. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table 7: The Effect of Participation in GDYT on Educational Outcomes, by Subgroup

	(1) 9th Grade and Below	(2) 10th and 11 Grade	(3) Male	(4) Female	(5) Above Median Math Score	(6) Below Median Math Score	(7) Not Chronically Absent	(8) Chronically Absent
Enrolled in K12	0.010	0.017**	0.016*	0.011*	0.004	0.021**	0.006	0.024
	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.008)	(0.005)	(0.016)
	[0.957]	[0.934]	[0.939]	[0.949]	[0.965]	[0.940]	[0.975]	[0.907]
		{0.438}		{0.634}		{0.077}		{0.172}
Chronically Absent	-0.030	0.027*	0.026	-0.023	0.003	-0.002	0.004	0.051*
	(0.021)	(0.015)	(0.023)	(0.021)	(0.020)	(0.024)	(0.020)	(0.029)
	[0.387]	[0.428]	[0.377]	[0.435]	[0.333]	[0.455]	[0.243]	[0.743]
		{0.016}		{0.119}		{0.876}		{0.103}
Graduated HS		0.034**	0.008	0.051**	-0.006	0.044*	0.018	0.035
		(0.014)	(0.025)	(0.021)	(0.020)	(0.024)	(0.014)	(0.047)
		[0.850]	[0.833]	[0.862]	[0.928]	[0.791]	[0.933]	[0.787]
				{0.210}		{0.082}		{0.693}
College from HS		0.004	0.046	-0.023	0.016	0.006	-0.031	0.008
		(0.035)	(0.033)	(0.036)	(0.041)	(0.054)	(0.041)	(0.053)
		[0.453]	[0.372]	[0.487]	[0.547]	[0.347]	[0.547]	[0.329]
				{0.114}		{0.861}		{0.422}

Notes. This table reports the results from a weighted regression of an educational outcome on an indicator for participated, a vector of control variables as listed in the text and match group fixed effects, separately for each subgroup. Participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. Standard errors, which are clustered by match school, are reported in parenthese and the weighted mean of the baseline characteristic for non-participants is shown in brackets. In the even columns, we report the p-value from a test of whether the point estimates are equal across subgroups in curly braces. Columns 5 and 6 are restricted to youth who reached 8th grade before the program since we define prior achievement by 8th grade test scores. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table A1: Balance Tests of Differences Between Junior Police and Fire Cadet Participants and Age-Eligible Non-Participants

	(1) Comparison Mean	(2) Participated in JPC
Special Education	0.107	0.012
		(0.027)
Low Income	0.863	0.025
		(0.030)
% Below Poverty Line in Neighborhood	34.678	3.414**
		(1.513)
Attendance Rate	0.919	0.005
		(0.007)
Chronically Absent	0.283	-0.015
		(0.045)
Took 8th Grade Math Test	0.911	-0.006
		(0.021)
Proficient on 8th Grade Math Test	0.115	0.029
		(0.024)
Standardized 8th Grade Math Score	-0.580	0.051
		(0.067)
Took 8th Grade Reading Test	0.913	-0.001
		(0.022)
Proficient on 8th Grade Reading Test	0.450	-0.018
		(0.036)
Standardized 8th Grade Reading Score	-0.454	-0.089
		(0.070)
N	5,321	

Notes. This table reports the results from a weighted regression of a baseline characteristic on an indicator for participated in Junior Police and Fire Cadets (JPC) and match group fixed effects, where participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. The age-eligible non-participants are applicants who were born between 1/1/1999 and 7/1/2001 and did not participate in JPC. Column 1 shows the weighted mean of the baseline characteristic for non-participants and column 2 shows the coefficient on the indicator for participation in JPC . All baseline characteristics are measured in the match year, except for 8th grade test scores. Youth who did not reach 8th grade before the program are not included in the analysis of 8th grade test scores. Standard errors are clustered by match school. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table A2: Description of the Sample for Each Outcome and the Formula for Outcomes in Follow-up Years One and Two

	Sample Description		
	Follow-up Year One	Follow-up Year Two	Formula for FY1 and FY2
Enrolled in K12	Grade 11 or below in 2014-2015 SY	Grade 10 or below in 2014-2015 SY	Avg of FY1 and FY2
Attendance Rate	Grade 11 or below in 2014-2015 SY and enrolled during 2015-2016 SY	Grade 10 or below in 2014-2015 SY and enrolled during 2016-2017 SY	Avg of FY1 and FY2
Chronically Absent	Grade 11 or below in 2014-2015 SY and enrolled during 2015-2016 SY	Grade 10 or below in 2014-2015 SY and enrolled during 2016-2017 SY	Avg of FY1 and FY2
Expelled	Grade 11 or below in 2014-2015 SY and enrolled during 2015-2016 SY	Grade 10 or below in 2014-2015 SY and enrolled during 2016-2017 SY	Avg of FY1 and FY2
Took SAT	Grade 10 in 2014-2015 SY	Grade 9 in 2014-2015 SY or Grade 10 in 2014-2015 but did not take SAT in 2015-2016	Max of FY1 and FY2
SAT Composite Score	Took SAT in 2015-2016 SY and Grade 10 in 2014-2015 SY	Took SAT in 2016-2017 and Grade 9 in 2014-2015 SY or Grade 10 in 2014-2015 and did not take SAT in 2015-2016	Max of FY1 and FY2
Graduated HS	Grade 11 in 2014-2015 SY and did not Graduate HS in 2014-2015 SY	Grade 10 during 2014-2015 SY or Grade 11 in 2014-2015 SY but did not Graduate HS in 2015-2016	Max of FY1 and FY2

Notes. This table describes the samples for each outcome variable in follow-up year one and follow-up year two, as well as the formula used to calculate the outcome in follow-up years one and two. FY1 indicates follow-up year one and FY2 indicates follow-up year two.

Table A2: Continued- Description of the Sample for Each Outcome and the Formula for Outcomes in Follow-up Years One and Two

	Sample Description		
	Follow-up Year One	Follow-up Year Two	Formula for FY1 and FY2
College from HS	Grade 12 in 2014-2015 SY	Grades 11 or 12 in 2014-2015 SY	Avg of FY1 and FY2
2 Year College from HS	Grade 12 in 2014-2015 SY	Grades 11 or 12 in 2014-2015 SY	Avg of FY1 and FY2
4 Year College from HS	Grade 12 in 2014-2015 SY	Grades 11 or 12 in 2014-2015 SY	Avg of FY1 and FY2
College as a HS Grad	Graduated HS before 2014-2015 SY	Graduated HS before 2014-2015 SY	Avg of FY1 and FY2
2 Year College as a HS Grad	Graduated HS before 2014-2015 SY	Graduated HS before 2014-2015 SY	Avg of FY1 and FY2
4 Year College as a HS Grad	Graduated HS before 2014-2015 SY	Graduated HS before 2014-2015 SY	Avg of FY1 and FY2

Notes. This table describes the samples for each outcome variable in follow-up year one and follow-up year two, as well as the formula used to calculate the outcome in follow-up years one and two. FY1 indicates follow-up year one and FY2 indicates follow-up year two.

Table A3: The Effect of Participation in GDYT on Educational Outcomes in Follow-up Year One and Follow-up Year Two

	Follow-up Year One			Follow-up Year Two		
	(1) N	(2) Comparison Mean	(3) Participated	(4) N	(5) Comparison Mean	(6) Participated
Enrolled in K12	6,340	0.965	0.013*** (0.005)	4,929	0.914	0.014 (0.010)
Attendance Rate	5,034	0.889	0.002 (0.004)	3,684	0.878	0.004 (0.006)
Chronically Absent	5,034	0.375	0.007 (0.015)	3,684	0.398	-0.013 (0.022)
Expelled	6,138	0.002	-0.000 (0.001)	4,547	0.004	-0.002 (0.002)
Took SAT	1,956	0.729	0.023 (0.026)	2,593	0.568	0.038* (0.021)
SAT Composite Score	1,487	862.427	0.423 (6.603)	1,525	858.083	-0.530 (5.196)
Graduated HS	1,375	0.861	0.006 (0.019)	1,871	0.797	0.038* (0.022)
College from HS	1,073	0.481	0.022 (0.052)	2,484	0.404	-0.001 (0.028)
2 Year College from HS	1,073	0.247	0.035 (0.047)	2,484	0.189	0.004 (0.024)
4 Year College from HS	1,073	0.234	-0.013 (0.030)	2,484	0.215	-0.005 (0.020)
College as a HS Grad	1,701	0.516	0.042 (0.041)	1,701	0.381	0.035 (0.045)
2 Year College as a HS Grad	1,701	0.246	0.014 (0.037)	1,701	0.164	0.015 (0.033)
4 Year College as a HS Grad	1,701	0.270	0.028 (0.027)	1,701	0.217	0.020 (0.033)

Notes. This table reports the results from a weighted regression of an educational outcome on an indicator for participated, a vector of control variables as listed in the text and match group fixed effects, separately for follow-up year one and follow-up year two. Participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. For follow-up year one, column 1 reports the number of observations in the sample, column 2 reports the weighted mean of the baseline characteristic for non-participants and column 3 shows the coefficient on the indicator for participation. Columns 4 through 6 show similar results for follow-up year two. Standard errors are clustered by match school. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table A4: Balance Tests of Differences Between Participants and Non-Participants, by Subgroup

	(1) 9th Grade and Below	(2) 10th and 11 Grade	(3) Male	(4) Female	(5) Above Median Math Score	(6) Below Median Math Score	(7) Not Chronically Absent	(8) Chronically Absent
Special Education	0.010	0.003	0.010	0.016	0.009	-0.000	0.012	0.016
	(0.021)	(0.016)	(0.018)	(0.011)	(0.009)	(0.021)	(0.013)	(0.032)
Low Income	-0.022	-0.000	0.006	-0.001	-0.021	0.012	0.007	-0.011
	(0.014)	(0.017)	(0.016)	(0.012)	(0.019)	(0.012)	(0.017)	(0.026)
% Below Poverty Line in Neighborhood	-0.495	0.187	0.449	-0.395	0.719	-1.835*	0.059	0.334
	(0.887)	(0.883)	(0.879)	(0.665)	(0.860)	(1.013)	(0.948)	(1.584)
Attendance Rate	0.005	0.022***	0.013***	0.016***	0.006**	0.021***	0.002*	0.027***
	(0.006)	(0.005)	(0.004)	(0.005)	(0.003)	(0.007)	(0.001)	(0.009)
Chronically Absent	0.021	-0.077***	-0.067**	-0.027	-0.023	-0.091***		
	(0.029)	(0.024)	(0.026)	(0.020)	(0.021)	(0.034)		
Took G8 Math Test	0.001	-0.007	-0.004	-0.009			-0.006	-0.000
	(0.016)	(0.016)	(0.016)	(0.012)			(0.011)	(0.024)
Proficient on G8 Math Test	0.035**	-0.003	0.035**	-0.002	0.027	0.000	0.001	0.021
	(0.015)	(0.019)	(0.015)	(0.012)	(0.021)	(0.000)	(0.015)	(0.020)
Standardized G8 Math Score	0.043	-0.024	0.035	0.008	0.030	-0.006	-0.017	0.035
	(0.047)	(0.041)	(0.036)	(0.032)	(0.032)	(0.016)	(0.029)	(0.059)

Notes. This table reports the results from a weighted regression of a baseline characteristic on an indicator for participated and match group fixed effects, separately for each subgroup. Participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. All baseline characteristics are measured in the match year, except for 8th grade test scores. Youth who did not reach 8th grade before the program are not included in the analysis of 8th grade test scores. Columns 5 and 6 are restricted to youth who reached 8th grade before the program since we define prior achievement by 8th grade test scores. Standard errors are clustered by match school. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table A4: Continued- Balance Tests of Differences Between Participants and Non-Participants, by Subgroup

	(1) 9th Grade and Below	(2) 10th and 11 Grade	(3) Male	(4) Female	(5) Above Median Math Score	(6) Below Median Math Score	(7) Not Chronically Absent	(8) Chronically Absent
Took G8 Reading Test	0.001	-0.006	-0.006	-0.007	-0.001	0.001	-0.002	-0.011
	(0.017)	(0.017)	(0.015)	(0.011)	(0.003)	(0.003)	(0.011)	(0.022)
Proficient on G8 Reading Test	0.014	0.019	0.015	0.017	0.050*	0.006	0.023	0.025
	(0.022)	(0.029)	(0.028)	(0.018)	(0.027)	(0.022)	(0.026)	(0.023)
Standardized G8 Reading Score	-0.003	-0.013	0.003	-0.020	0.029	0.007	-0.023	-0.024
	(0.045)	(0.052)	(0.049)	(0.036)	(0.053)	(0.049)	(0.044)	(0.050)
N	3900	4036	4789	6748	4949	4901	5842	3204

Notes. This table reports the results from a weighted regression of a baseline characteristic on an indicator for participated and match group fixed effects, separately for each subgroup. Participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. All baseline characteristics are measured in the match year, except for 8th grade test scores. Youth who did not reach 8th grade before the program are not included in the analysis of 8th grade test scores. Columns 5 and 6 are restricted to youth who reached 8th grade before the program since we define prior achievement by 8th grade test scores. Standard errors are clustered by match school. Stars indicate: *$p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A5: Falsification Tests of the Effect of Participation in GDYT on 6th Grade Educational Outcomes, by Subgroup

	(1) 9th Grade and Below	(2) 10th and 11 Grade	(3) Male	(4) Female	(5) Above Median Math Score	(6) Below Median Math Score	(7) Not Chronically Absent	(8) Chronically Absent
Attendance Rate	-0.002	0.015***	0.008	0.001	0.001	0.009	0.004	-0.004
	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)	(0.003)	(0.010)
Chronically Absent	0.011	-0.051**	-0.023	-0.003	0.005	-0.053**	-0.019	-0.026
	(0.023)	(0.023)	(0.024)	(0.014)	(0.018)	(0.026)	(0.018)	(0.031)
Took Math Exam	-0.006	0.007	0.003	0.005	-0.005	0.002	-0.005	-0.009
	(0.014)	(0.006)	(0.009)	(0.008)	(0.008)	(0.013)	(0.006)	(0.014)
Proficient on Math	-0.023	-0.001	-0.025	-0.000	-0.018	-0.007	-0.008	-0.007
	(0.020)	(0.021)	(0.018)	(0.013)	(0.019)	(0.016)	(0.018)	(0.027)
Std Math Score	-0.073	-0.036	-0.038	-0.042	-0.032	-0.067	-0.035	-0.028
	(0.049)	(0.030)	(0.040)	(0.026)	(0.029)	(0.050)	(0.037)	(0.044)
Took Reading Exam	-0.012	0.005	-0.004	0.001	-0.010	-0.002	-0.003	-0.019
	(0.014)	(0.006)	(0.008)	(0.009)	(0.008)	(0.011)	(0.007)	(0.014)
Proficient on Reading	-0.027	-0.004	-0.007	-0.001	-0.002	-0.006	0.007	0.026
	(0.024)	(0.020)	(0.021)	(0.020)	(0.021)	(0.023)	(0.023)	(0.038)
Std Reading Score	-0.083**	0.012	0.001	-0.034	-0.030	-0.037	-0.005	-0.001
	(0.038)	(0.043)	(0.045)	(0.031)	(0.030)	(0.046)	(0.039)	(0.069)
N	3900	4036	4789	6748	4949	4901	5842	3204

Notes. This table reports the results from a weighted regression of a 6th grade educational outcome on an indicator for participated, a vector of control variables as listed in the text and match group fixed effects, separately for each subgroup. Participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. Columns 5 and 6 are restricted to youth who reached 8th grade before the program since we define prior achievement by 8th grade test scores. Standard errors are clustered by match school. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table A6: The Effect of Participation in GDYT on Additional Educational Outcomes, by Subgroup

	(1) 9th Grade and Below	(2) 10th and 11 Grade	(3) Male	(4) Female	(5) Above Median Math Score	(6) Below Median Math Score	(7) Not Chronically Absent	(8) Chronically Absent
Attendance Rate	0.002	0.001	0.002	0.002	0.001	0.003	-0.002	-0.002
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.009)	(0.004)	(0.016)
	[0.885]	[0.867]	[0.879]	[0.873]	[0.900]	[0.865]	[0.925]	[0.781]
		{0.771}		{0.981}		{0.894}		{0.965}
Expelled	-0.002	-0.000	-0.001	-0.001	-0.002	0.001	-0.000	0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
	[0.004]	[0.001]	[0.002]	[0.003]	[0.002]	[0.003]	[0.001]	[0.002]
		{0.545}		{0.782}		{0.149}		{0.565}
Took SAT	0.038	0.029	0.033	0.033	-0.003	0.049*	0.011	0.011
	(0.025)	(0.024)	(0.024)	(0.023)	(0.025)	(0.029)	(0.022)	(0.051)
	[0.713]	[0.745]	[0.710]	[0.743]	[0.831]	[0.707]	[0.870]	[0.577]
		{0.781}		{0.995}		{0.183}		{0.993}
SAT Composite Score	-1.045	0.921	6.529	-5.610	7.539	2.957	3.097	-6.487
	(5.054)	(6.404)	(5.683)	(5.234)	(6.954)	(6.007)	(6.043)	(15.635)
	[859.284]	[861.239]	[853.922]	[865.057]	[937.142]	[793.735]	[884.968]	[821.039]
		{0.817}		{0.123}		{0.603}		{0.476}
2 Year College from HS		0.016	0.010	0.017	0.035	-0.005	-0.031	0.053
		(0.029)	(0.037)	(0.031)	(0.044)	(0.039)	(0.038)	(0.044)
		[0.197]	[0.199]	[0.219]	[0.213]	[0.219]	[0.235]	[0.196]
				{0.883}		{0.420}		{0.068}
4 Year College from HS		-0.012	0.036	-0.041	-0.024	0.014	-0.004	-0.045
		(0.024)	(0.034)	(0.025)	(0.025)	(0.037)	(0.029)	(0.034)
		[0.256]	[0.176]	[0.275]	[0.344]	[0.128]	[0.319]	[0.139]
				{0.095}		{0.338}		{0.307}

Notes. This table reports the results from a weighted regression of an educational outcome on an indicator for participated, a vector of control variables as listed in the text and match group fixed effects, separately for each subgroup. Participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. Standard errors, which are clustered by match school, are reported in parenthese and the weighted mean of the baseline characteristic for non-participants is shown in brackets below. In the even columns, we report the p-value from a test of whether the point estimates are equal across subgroups in curly braces. Columns 5 and 6 are restricted to youth who reached 8th grade before the program since we define prior achievement by 8th grade test scores. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.

Table A6: Continued- The Effect of Participation in GDYT on Additional Educational Outcomes, by Subgroup

	(1) 9th Grade and Below	(2) 10th and 11 Grade	(3) Male	(4) Female	(5) Above Median Math Score	(6) Below Median Math Score	(7) Not Chronically Absent	(8) Chronically Absent
College as a HS Grad			-0.013	0.052	0.030	0.042	0.007	0.083
			(0.056)	(0.051)	(0.046)	(0.089)	(0.049)	(0.059)
			[0.389]	[0.487]	[0.550]	[0.356]	[0.532]	[0.352]
				{0.323}		{0.879}		{0.162}
2 Year College as a HS Grad			-0.057	0.038	0.022	-0.003	-0.040	0.077
			(0.056)	(0.032)	(0.045)	(0.069)	(0.040)	(0.053)
			[0.232]	[0.206]	[0.231]	[0.215]	[0.215]	[0.223]
				{0.091}		{0.664}		{0.024}
4 Year College as a HS Grad			0.044	0.014	0.001	0.051	0.043	0.006
			(0.050)	(0.036)	(0.039)	(0.057)	(0.047)	(0.039)
			[0.163]	[0.295]	[0.336]	[0.144]	[0.329]	[0.138]
				{0.614}		{0.375}		{0.423}

Notes. This table reports the results from a weighted regression of an educational outcome on an indicator for participated, a vector of control variables as listed in the text and match group fixed effects, separately for each subgroup. Participants have a weight of one and non-participants have a weight equal to the ratio of participants to non-participants in their match group. Standard errors, which are clustered by match school, are reported in parenthese and the weighted mean of the baseline characteristic for non-participants is shown in brackets below. In the even columns, we report the p-value from a test of whether the point estimates are equal across subgroups in curly braces. Columns 5 and 6 are restricted to youth who reached 8th grade before the program since we define prior achievement by 8th grade test scores. Stars indicate: *p<0.1, ** p<0.05, *** p<0.01.