School's Out:
How Summer Youth Employment Programs Impact Academic Outcomes

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This version: November 3, 2021

Abstract: Recently there has been an emphasis on how time spent outside of the classroom can affect student outcomes, including high school graduation, with the hope of closing academic achievement gaps along socioeconomic and racial lines. This paper provides experimental evidence regarding a particular type of out-of-school activity—early work experience—on high school academic outcomes for low-income inner-city youth. Using randomized admissions lotteries for students who applied to the Boston Summer Youth Employment Program (SYEP), we estimate the effect of being selected to participate on academic outcomes as measured by administrative school records. We find that SYEP lottery winners are 4.4 percentage points more likely to graduate from high school on time and 2.5 percentage points less likely to drop out of high school during the four years after participating in the program relative to the control group. These improvements appear to be driven by better attendance and course performance in the year after being selected for the program, with the program's impact on attendance persisting into the second year. Survey data suggest that the Boston SYEP may affect academic outcomes by increasing aspirations to attend college, gaining basic work habits, and improving social skills during the summer.

JEL Classifications: J13, J18, J24, I21

Keywords: Youth, human capital, schooling

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ACKNOWLEDGMENTS
Special thanks to Trinh Nguyen and Midori Morikawa of the City of Boston for providing the application data from the Boston SYEP program and to Rashad Cope, Mark Isenberg, Joe McLaughlin, and Mallory Jones for their efforts to implement the survey. Thanks also to Carrie Conaway and Matthew Deninger of the Office of Planning and Research at the Massachusetts Department of Elementary and Secondary Education for providing access to statewide administrative school records. We are also grateful to Third Sector Capital, and the William T. Grant Foundation for their generous support of this work.

1. Introduction

Students of color and students from low-income families graduate from American public high schools at much lower rates than white students and those from upper-income families (Duncan and Murnane 2011; Ladd 2012; Musu-Gillette et al. 2017). For example, in 2018, the percentage of U.S. public high school students who graduated on time, as measured by the adjusted cohort graduation rate (ACGR), was 8-10 percentage points higher on average for white (89 percent) as compared to Black (79 percent) and Hispanic/Latino (81 percent) students (U.S. Department of Education 2020).[1] Similarly, only 79 percent of low-income students graduated on time, as compared to 91 percent of non-low-income students (Atwell et al. 2020). Over the past decade, low-income urban school districts such as Boston have seen graduation gaps of 10-15 percentage points narrow only slightly across racial groups and even widen by socioeconomic status (Boston Public Schools 2021). These gaps matter because, on average, high school graduates have considerably higher labor-market earnings than dropouts (Cameron and Heckman 1993; Jaeger and Page 1996). Moreover, high school dropouts are more likely than graduates to experience health problems and to be involved in the criminal justice system (Bjerk 2012).

In addition to the many in-school interventions that have been implemented over the past several decades to increase high school graduation rates, policymakers and researchers have recently examined how time spent outside of the classroom can affect student outcomes, including high school graduation (Stevenson 2010; Crispin 2017). This paper provides experimental evidence regarding the impact of one type of out-of-school activity—early work

[1] State education agencies calculate the ACGR by identifying the "cohort" of first-time ninth graders in a particular school year. The cohort is then adjusted by adding any students who emigrate from another country or transfer into the cohort after ninth grade, and subtracting any students who transfer out, immigrate to another country, or die. The ACGR is the percentage of students in this adjusted cohort who graduate within four years with a regular high school diploma.

experience—on high school academic outcomes (Loewenberg 2020). To estimate causal impacts, we use experimental variation from randomized admissions lotteries for the 2015 cohort of students who applied to the Boston Summer Youth Employment Program (SYEP). We match these program data to administrative school records and follow students over time to estimate the impact of being offered an SYEP job on both high school graduation rates and more proximal outcomes, such as attendance and course grades, to assess potential mechanisms.

We find that, relative to the control group, SYEP lottery winners are 4.4 percentage points (+7.0 percent) more likely to graduate from high school on time, in large part due to a 2.5 percentage point reduction in the likelihood of dropping out of high school during the four-year post-program observation period. Analyses of more proximal outcomes show that the attendance rates of youth who were randomly selected into the SYEP treatment group significantly improved, by 2.4 percentage points (2.7 percent), during the school year immediately after participation, in part because their unexcused absences were reduced by 2.1 days. Moreover, youth in the treatment group were 5.9 percentage points less likely to experience chronic absenteeism, defined as an attendance rate of less than 90 percent. We also find small but significant improvements in overall GPA (6.8 percent) in the year after participation but no reductions in course failures. While the main program's impacts on days attended and unexcused absences persist into the second year, the GPA improvements appear to fade out after the first year, except among youth who apply for and win a second summer of participation. Self-reported survey responses suggest that these outcomes may be correlated with relative improvements in basic work habits and soft skills as well as increasing aspirations to attend college. A simple back-of-the envelope calculation based on both the higher taxable income (Child Trends 2017) and lower rates of arrest and incarceration (Lochner and Moretti 2004) for high school graduates versus dropouts suggests that the long-term benefits of the Boston SYEP outweigh the costs by more than 2-to-1.

This paper makes three key contributions to the literature. First, although prior research has found that summer jobs programs have strong positive effects on reducing crime (Gelber, Isen, and Kessler 2016; Heller 2014; Modestino 2019; Davis and Heller 2020), the evidence on improving academic outcomes is more mixed, with some studies finding improved attendance rates (Leos-Urbel 2014) and a greater likelihood of passing statewide high school exams (Leos-Urbel et al. 2012; Schwartz et al. 2020) but no positive impacts on graduating from high school (Valentine et al. 2017) or college enrollment (Gelber, Isen, and Kessler 2016). Compared to the previous literature, this study has several advantages enabling us to detect schooling impacts across both short-term (e.g., attendance rates one year post-program) and longer-term (e.g., on-time high school graduation) outcomes. This includes access to state-level administrative data that yield a very high match rate over multiple years to better measure outcomes due to the ability to track students even if they transfer to another school within the state. In addition, the Boston SYEP largely serves a population of younger, school-aged, and low-income youth who may be more likely to benefit academically from early work experiences. Finally, the Boston program's implementation yields a cleaner experimental design with high take-up and little crossover, providing a meaningful contrast between the treatment and control groups.

Second, while the SYEP literature has demonstrated encouraging results in some cities, its utility for policymakers has been limited by the lack of insights into the *mechanisms* driving these improved outcomes and their potential for reducing inequality across groups. We build on this research by examining proximal outcomes, such as attendance and grades, and linking them to more distal outcomes, such as dropout and high school graduation. Combining additional data on subsequent SYEP lotteries with outcomes measured two years post, we also study the duration of the program's effects while exploring the "dosage" (e.g., number of summers of participation) needed to sustain the impact of the program beyond its first year. Supplementing these analyses with self-reported behaviors from survey data, we further shed light on how structured youth experiences outside the classroom can affect school outcomes.

Finally, our results provide some of the most compelling evidence that early work experiences can enhance, rather than diminish, academic progress—likely because SYEPs differ from year-round programs in several important ways. Prior studies of year-round workforce development programs aimed at youth have often shown negative impacts on school outcomes: when students work too many hours, academic achievement suffers (Tyler 2003), and the likelihood of high school graduation and college attendance decreases (Stasz and Brewer 1999; Mortimer 2010). Others find that the association between hours of work and school performance follows an inverted-U shaped pattern, with students who work moderate hours performing at a higher level than students who work more or not at all (Stern and Briggs 2001). By contrast, SYEPs occur during summer break, when youth are often idle (Gershenson 2013), creating fewer conflicts with academic studies and extracurricular activities. SYEPs may also help ameliorate summer learning loss when school is out of session by providing opportunities to practice existing skills or learn new ones on the job (Cooper et al. 1996; Alexander, Olson, and Entwisle 2007; Castleman and Page 2014). Further, the Boston SYEP incorporates several features designed to specifically address skill deficits arising from a lack of opportunities among at-risk youth, including a formal career readiness curriculum, greater exposure to private sector employers, and job-skill ladders across summers.

This paper is organized as follows: Section 2 provides an overview of the policy context and potential mechanisms. Section 3 describes the data and methodology that we use to evaluate program outcomes. Section 4 presents estimates of the program's impact on both high school graduation as well as more proximal outcomes, like attendance and course grades, and analyzes the relationship between the two. Finally, Section 5 concludes with a discussion of how this research fits into the prior literature on summer jobs and the implications for policy.

2. The Boston SYEP Intervention

Introduced in the early 1980s, the Boston SYEP relies on approximately $10 million in city, state, and private funding to connect about 10,000 youth each summer with roughly 900 local employers. All Boston city residents aged 14 to 24 years are eligible for the program, and participants are paid the Massachusetts minimum wage. Youth are placed in either a subsidized position (e.g., with a local nonprofit, community-based organization, or city agency), with upwards of one-third working in a daycare or day camp, or a job with a private-sector employer.

For six weeks, from early July through mid-August, SYEP youth work a maximum of 25 hours per week and receive 20 hours of job-readiness training, which includes evaluating learning strengths, skills, and interests; developing soft skills, such as communication, collaboration, and conflict resolution; and learning how to search for a job, draft a resume and cover letter, and answer typical interview questions. Youth apply through one of the four intermediary organizations under contract with the Boston Mayor's Office of Workforce Development (OWD); most typically apply to the intermediary in their immediate neighborhood.[2] The intermediaries are responsible for reviewing applications, matching applicants with jobs, supervising job placements, and delivering the career-readiness curriculum.

[2] Administrative data provided by the City of Boston shows that only 6.8 percent of youth apply to more than one agency, and no individual receives more than one offer of employment. Moreover, there is little crossover across intermediaries using random assignment, with only 3.0 percent of the control group obtaining a job through one of the three other summer job intermediaries.

How Might SYEPs Improve Academic Outcomes?

Understanding the channels through which SYEPs can lead to better school outcomes can help inform policymakers and practitioners about the types of interventions that might be successful at raising high school graduation rates. Recently, chronic absenteeism—attending less than 90 percent of school days in an academic year—has been highlighted as a serious challenge for policies aimed at improving academic performance among low-income and at-risk youth (Ready 2010; Gershenson 2016; U.S. Department of Education 2016). In high-poverty areas, as many as one-third of all high school students are chronically absent (Sheldon and Epstein 2004; Balfanz and Byrnes 2012), and rates of absenteeism are higher among non-white students (U.S. Department of Education 2016). High school absences and chronic absenteeism have been linked to poor outcomes, including inability to read at grade level (Mac Iver 2010), grade retention (Nield and Balfanz 2006), drug use (Hallfors et al. 2002), and increased risk of dropout (Rumberger and Thomas 2000; Utah Education Policy Center 2012). Below, we describe four channels through which SYEPs have the potential to reduce chronic absenteeism and improve academic performance, leading to high school graduation.

(1) *Improving behaviors correlated with school success.* Some SYEPs, including the Boston program, offer structured curriculum designed to improve work habits and soft skills, such as time management, punctuality, responsibility, determination, self-confidence, and "grit." These non-cognitive skills have been linked to increases in attendance and high school completion (Jackson 2012) and to more distal measures of adult success (Duckworth et al. 2007; Heckman 2008). In addition, summer jobs provide experiential learning opportunities to practice both cognitive and non-cognitive skills on the job (Cooper et al. 1996; Alexander, Olson, and Entwisle 2007), which could potentially raise subsequent course performance.

(2) *Increasing career and academic aspirations.* Through career exploration and the development of job-readiness skills, the program aims to provide youth with experiences that can shape their goals by raising career and academic aspirations—both of which can lead to better school outcomes, particularly for disadvantaged youth living in neighborhoods with few job opportunities (Lillydahl 1990; Mortimer 2010). In addition, youth are assigned a job supervisor who can act as a mentor and provide a strong, supportive, and sustained relationship with an adult to help youth develop a sense of agency, identity, competency, and self-efficacy.

(3) *Reducing opportunities to engage in delinquent behavior.* Many summer jobs programs were initially established to "keep kids off the street" and reduce violence during the summer months, primarily by limiting opportunities for youth to engage in delinquent activity or disrupting risky behaviors stemming from a lack of supervision or guardianship (Cohen and Felson 1979). By providing youth with a set of socially productive activities, SYEPs may decrease the risk of exposure to, or participation in, delinquent behavior that could lead to truancy or other disciplinary actions affecting absenteeism and dropout (Wilson 1996).

(4) *Providing direct income support to youth and their families.* Wages earned from employment in the program can also help reduce poverty and provide resources that lead to better school outcomes.[3] According to our survey data, roughly half of youth participating in the Boston SYEP indicated that they helped pay one or more household bills, and one in five reported saving for college tuition.

3. Experimental Design, Data, and Empirical Methodology

Experimental Design

[3] Note that it is often not possible to parse out any effect of the income associated with SYEPs from other changes related to the experience itself. Nonetheless, we lay out the main arguments supporting why we might expect SYEPs to improve outcomes independent of the income effect.

We rely on a lottery assignment that effectively controls for selection into the program while also accounting for changes that might occur during the normal course of adolescent development. Our analysis is restricted to youth who applied to the Boston SYEP for summer 2015 through Action for Boston Community Development (ABCD), a large and established nonprofit that works in all of Boston's 18 neighborhoods and serves a predominately young, school-aged, and low-income population.[4] We focus on ABCD because it is one of the two intermediaries that make use of random assignment due to the high number of applications it receives for the limited number of SYEP jobs that are available.[5] ABCD uses a computerized system with a simple random-assignment algorithm to select youth based on their applicant ID numbers and the number of available slots, which is determined by the amount of funding each year. This system effectively assigns the offer to participate in the program at random, without any stratification by geography or other characteristics, thereby creating a control group of youth who apply to the SYEP but are not chosen.[6]

The context in which the Boston SYEP was delivered during the summer of 2015 is noteworthy. Despite the labor market having largely recovered from the 2007-2008 Great Recession, the youth unemployment rate remained elevated at 8.7 percent in Massachusetts.[7] Of

[4] Approximately 80 percent of ABCD applicants are Boston Public School (BPS) students—similar to the proportion of Boston high school-aged residents that are enrolled in BPS (Boston Foundation 2006). ABCD applicants also have similar gender and racial characteristics in comparison to the population of low-income Boston youth (see Table A6 in the online appendix).

[5] The other intermediary that uses random assignment, the Department of Youth Employment and Engagement (DYEE), does so only on a partial basis where 60 percent of the jobs for a given employer are assigned randomly and the other 40 percent are selected by the employer.

[6] See Table A1 in the online appendix, which provides descriptive statistics for the preexisting characteristics of SYEP lottery applicants collected by ABCD. Comparing these observable characteristics across youth who were selected by the lottery versus not confirms that the lottery was indeed random with only one statistically significant difference found across the two groups, as would be expected by random chance when testing 15 different characteristics. An F-test of joint significance further demonstrates that the original lottery assignment was balanced across the treatment and control groups when all preexisting characteristics are controlled for simultaneously.

[7] Governing Magazine. Youth Unemployment Rate, Figures by State. https://www.governing.com/archive/youth-employment-unemployment-rate-data-by-state.html

the 4,235 youth who applied to ABCD in 2015, 1,186 (or 28 percent) were offered a job via random assignment, leaving 3,049 individuals in the control group. Of those selected by the lottery, 83.6 percent accepted a job offer, with only a handful dropping out during the program while it was in progress. According to quarterly wage record data provided by the Massachusetts Division of Unemployment Assistance, only 28.2 percent of youth in the control group had worked during the third quarter (July-September) of 2015.

Administrative School Record Data

Our primary source of data comes from state-level administrative school records provided by the Massachusetts Department of Elementary and Secondary Education (DESE), which includes information on all public-school students within the state of Massachusetts, including those attending charter schools. This rich data source contains information on secondary-school outcomes, including attendance, course grades, statewide test scores, dropouts, and high school graduation for one year prior to and up to four years after participation in the program. Using administrative data avoids problems associated with self-reported data, such as social desirability bias, which might be present if individuals in the treatment group feel compelled to embellish their school performance when applying for a summer job.

A drawback of administrative data is that individuals must be matched across two different record-keeping systems, which often results in a less-than-perfect match. Since the individual-level SYEP and DESE files do not share a unique common student identifier, students were matched based on their name and birth date. Of the full lottery sample, 79.6 percent were in grades 8-11 during the 2014-15 school year before applying to the summer jobs program and would be expected to attend school during the year after participating. Of these, almost all (96.9 percent) were matched to the 2014-15 DESE file—a much higher pre-program match rate than that of previous summer jobs studies, likely due to having state-level records that capture youth even if they transfer out of the Boston Public Schools system.[8]

How do we track youth over time? Figure 1 provides a high-level conceptual timeline of student participation, data collection, and tracking by grade level for the four-year post-observation period through the 2018-19 school year. Note that an additional cohort of students graduates with each successive year of observation after the program ends, which limits our ability to assess impacts on attendance and course grades as more proximal outcomes during the first one to two years after participation in the program.[9] However, we are able to fully measure terminal outcomes, such as on-time high school graduation, during this four-year post-program observation period.

Even though the original lottery was confirmed as random, and the initial match rate with the administrative data is quite high, estimates of the impact of SYEP on student outcomes could be biased if there is selective attrition from enrolling in school during the year(s) following participation in the program. Having access to state-level administrative data helps mitigate this concern, since youth will have a record even if they switch schools, as long as they remain in the state. Of the students in the SYEP lottery who were enrolled in grades 8-11 in the school year

[8] See Table A2. Leos-Urbel (2014) reports a 77 percent match rate for applicants in the New York City summer jobs program. He attributes this lower match rate to unmatched records including an unknown number of students in private or parochial schools or schools outside of New York City, as well as nonstudents. In the balance test provided in Table A1, we include an indicator for whether students were matched into the administrative data used for the analysis to demonstrate that the analysis subsample is balanced across the treatment and control groups.
[9] Of the 3,011 students who can be tracked during our post-program observation period, 32 (1.1 percent) are missing baseline attendance data because they dropped out prior to the start of the 2014-15 school year, with no difference in the proportion missing across the treatment and control groups. Another 130 students (4.3 percent) are missing baseline course grades, almost all of which (96.2 percent) were listed as enrolled for the entire school year. Table A3 performs the same balance check as Table 1 for the sample of youth with non-missing attendance, and Table A7 reports the regressions results on this slightly more restricted sample. Analogous balance checks and results for the sample of youth with non-missing grades can be found in Tables A4 and A8 respectively.

prior to applying, 92 percent were enrolled in the following school year (2015-16) and 89 percent can be tracked for the full post-program observation period with no significant differences between the treatment and control groups.[10]

To more rigorously test for selective attrition, Table 1 provides descriptive statistics for the preexisting baseline characteristics of SYEP lottery applicants who were matched to the administrative data and were able to be tracked during the post-program observation period. Columns (1) and (2) compare these characteristics across the treatment and control groups, while column (3) provides the difference between the two groups and the p-value to indicate whether any of the differences are statistically significant. In terms of demographics, applicants were just under 16 years of age and slightly more likely to be female and African American. Consistent with ABCD serving a more vulnerable population, roughly 7 percent of youth identified as having limited English ability, another 7 percent reported being homeless, and upwards of 18 percent acknowledged receiving cash public assistance of some form.[11] In terms of academics, roughly two-thirds of students were in grades 8 and 9 when applying for the program, and nearly 10 percent had switched schools during the academic year, indicating that this is indeed a somewhat transient population that could be difficult to track across districts without state-level data. About 15 percent attended a charter school, and just over half of the student population in their schools had scored proficient or better on the statewide MCAS standardized test. Few students had dropped out of school prior to applying to SYEP, while nearly 30 percent were chronically absent, with 12 days of unexcused absences on average. Mean GPA was 1.9 and a

[10] See Table A2 in the online appendix for a detailed breakdown of how the matched follow-up sample was constructed. The only students that completely attrit from our sample are those who transfer to private school or out of state and do not re-enroll in public school in Massachusetts at any point, or those who become deceased.
[11] Cash public assistance includes Emergency Assistance to Elderly Disabled and Children, Social Security Income, Social Security Disability Income, Temporary Aid to Families with Dependent Children, Unemployment Insurance, or Worker's Compensation.

little more than one-quarter of students had failed a course. The SYEP indicator does not significantly predict any preexisting baseline characteristics, with the exception of one characteristic (the percentage of students who are Asian) that was also found to be significant in the balance test for the full lottery sample.[12] Separate F-tests for each set of covariates used in the main tables confirm that the baseline covariates are also jointly insignificant (see Table A5).

Empirical Methodology

To assess the impact of the Boston SYEP, we compare school outcomes during the period following the intervention for the treatment versus the control group. Because SYEP participation is allocated via lottery, we obtain causal estimates using a simple comparison of means on the outcome of interest. Specifically, we compare outcomes for youth offered an SYEP placement (the treatment group) to those not offered a placement (the control group). This "intent to treat" (ITT) estimate measures the impact of *offering* the program on the outcome. In many cases, this is the policy-relevant estimate for program administrators, as they can offer a program but cannot control who agrees to participate. Of course, because not all youth accept the offer, the ITT estimate understates the effects of the program for those youth who chose to participate. For this reason, we also provide treatment-on-the-treated (TOT) estimates in the online appendix using a two-stage-least-squares method.

We measure multiple outcomes of interest during the four-year post-intervention period across three different domains: primary outcomes of interest (high school graduation and dropout rates), proximal outcomes that serve as potential mediators (attendance, course performance, and standardized test scores), and exploratory mechanisms from our survey data (academic aspirations, work habits, and soft skills). The definition and time period over which each variable

[12] Note that one statistically significant difference found across the two groups would be expected by random chance when testing 32 different characteristics.

is measured is provided in Table 2 and described in greater detail in the online appendix.

ITT Estimates of Program Impacts

Although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned (Bloom 2006), we also use the following regression framework to control for individual characteristics and increase the precision of our estimates:

$$Y_{it} = SYEP_i \pi_1 + X_{i(t-1)} \beta_1 + s_{(t-1)} + \mu_{it} \qquad (1)$$

where Y_{it} is the school outcome for individual i in post-program year t; $SYEP_i$ is a dummy variable indicating the individual received an offer to participate; $X_{i(t-1)}$ is a set of preexisting demographic characteristics, academic characteristics, and baseline school outcomes[13]; $s_{(t-1)}$ is a vector of pre-program school characteristics (e.g., attended a charter school, school-wide proficiency on statewide MCAS exam); and μ_{it} is a stochastic error term. Robust standard errors are clustered at the student level. We use both OLS as well as alternative nonlinear methods to relax the linear functional form assumption.[14]

Mediation Analysis

We theorize that the Boston SYEP could have both a direct as well as an indirect effect on graduation. In terms of direct effects, the program could directly increase career and academic aspirations that motivate students to graduate on time. The literature also suggests two potential indirect effects that could be at work. First, the SYEP is intended to develop good work habits, such as showing up on time, which could help students improve their school attendance and the

[13] Demographic characteristics include age, gender, race, primary language spoken, limited English proficiency, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, high-need special education status, participation in the METCO program, and switching schools within the school year. The inclusion of these controls does little to affect the point estimates but does improve the precision.

[14] For example, to analyze differences in the number of days truant—a count variable—we use a Poisson quasi maximum likelihood estimator (QMLE). The consistency of this estimator only requires the correct specification of the conditional mean, not the entire distribution. To analyze differences in the likelihood of an event, we use a probit estimator. Average marginal effects are reported in all tables when using these nonlinear estimation methods.

likelihood of high school graduation. Second, the SYEP also provides youth with an opportunity to practice existing skills on the job and develop new ones, which may lead to better course performance and, ultimately, increase the probability of graduating. Studying these indirect channels is important for understanding the mechanisms driving the observed improvements in high school graduation among the treatment group. If the program improves attendance, GPA, or both, this might explain why we observe improvements in high school graduation even among younger cohorts of students for whom graduation is a distal outcome.

We explore these potential mediators by relating our terminal outcome (high school graduation) to the more proximal or intermediate outcomes (e.g., attendance rate and GPA) that can be measured in the year immediately after participating in the program. To test this, we modify equation (1) as follows:

$$Y_{iT} = SYEP_i \, \pi_2 + X_{i(t-1)} \, \beta_2 + s_{(t-1)} + M_{it1} \, \delta + \mu_{it} \tag{2}$$

where on the left-hand side, the dependent variable, Y_{iT}, is whether the individual graduated from high school on time during post-program terminal year T. On the right-hand side, M_{it1} represents one of our proximal mediator variables (e.g., attendance rate or GPA) measured one year post-program. A positive and significant coefficient δ indicates that improvement in the mediating variable is positively correlated with a subsequent increase in on-time graduation. If the inclusion of the mediating variable also reduces the magnitude and significance of the coefficient on the SYEP treatment dummy (π_2) compared to the coefficient (π_1) estimated from equation (1), then the direct effect of the program on graduation may be either partially or fully driven by the improvement in the mediator. However, we would caution against interpreting these results as conclusive evidence of mediation, given the possibility of omitted variables (including other plausible mediators) that might correlate with each of the focal mediators (attendance and GPA).

Dosage and Duration of Impacts

Additionally, we are interested in exploring whether the Boston SYEP impacts fade over time, as well as if an additional summer (e.g., increased "dosage") can enhance outcomes. Since the program is oversubscribed, understanding the dynamic nature of program impacts can help policymakers better allocate scarce resources to achieve meaningful outcomes while serving as many youth as possible. To answer whether SYEP program impacts persist, we first re-estimate equation (1) where the left-hand side variable is now measured two years post-treatment:

$$Y_{it2} = SYEP_i \, \pi_3 + X_{i(t-1)} \, \beta_3 + s_{(t-1)} + \mu_{it} \tag{3}$$

To further explore the question of dosage, we then make use of an additional year of program data to construct an indicator (*SYEP2*) for whether youth had also won the lottery during the summer of 2016. Although the vast majority of youth (88 percent) in both the treatment and control groups were eligible to apply for a second time based on their age, the choice of whether to do so varied by treatment status. Not surprisingly, winning the lottery in the first year increases the likelihood of applying for a second time, but the opposite is true for lottery losers.[15] As such, we limit the sample to youth who initially won the SYEP lottery in 2015 to estimate the impact of a second summer of treatment, conditional on having won the lottery the first time using equation (4):

$$Y_{it2} = SYEP2_i \, \pi_4 + X_{i(t-1)} \, \beta_4 + s_{(t-1)} + \mu_{it} \tag{4}$$

Although two-thirds of youth in the treatment group chose to apply for a second summer, one might be concerned that those who did not apply differ in terms of their unobservable

[15] Indeed, only 7.2 percent of those in the control group who are eligible to apply in 2016 based on their age do so and only 3.5 percent win the lottery in that year, making it an uncommon event to study. In contrast, roughly two-thirds (66.5 percent) of youth in the treatment group who are eligible to apply in 2016 do so and about one-third (33.5 percent) win the lottery for a second summer, yielding enough variation to assess the importance of dosage on outcomes measured two years post for this group.

characteristics such as motivation or "grit." As a robustness check, we also estimate equation (4) for just the sub-sample of 2015 lottery winners who also applied to the program in 2016. Given that this is a fundamental policy question of interest to the City of Boston, we believe that exploring whether the program impacts persist beyond the first year and, if so, how much can be explained by the number of summers (e.g., dosage) is informative, especially since the majority of initial lottery winners seek a second summer of participation.

Subgroup Analysis

Finally, although one might question whether a six-week intervention can provide a meaningful turning point for youth development, such impacts may be greater for at-risk youth (Sampson and Laub 2003). This may be especially important for teens growing up in low-income neighborhoods with failing schools (Chetty, Hendren, and Katz 2016). As such, we also test for heterogeneous impacts where one might expect to see a disproportionate effect of the program on those with a greater likelihood of dropping out. This includes older youth, males, students with limited English skills, at-risk youth defined as receiving public assistance, and students with baseline attendance rates that indicate chronic absenteeism (Utah Education Policy Center 2012). To test whether the Boston SYEP had a differential impact on these less advantaged groups of youth, we modify equation (1) to replace the main program effect with a fully specified set of interaction terms between the SYEP treatment dummy and the subgroup of interest. For example, to test whether the program has a differential impact on students with prior chronic absenteeism (attendance rates below 90 percent), we estimate equation (5) as follows:

$$Y_{it} = SYEP_i * ATTEND\_LT90_{i(t-1)}\,\pi_5 + SYEP_i * ATTEND\_GE90_{i(t-1)}\,\pi_6 + X_{i(t-1)}\,\beta_5 + s_{(t-1)} + \mu_{it} \qquad (5)$$

We then test whether the difference between π_5 and π_6 is statistically significant to determine whether the program exhibits heterogeneous treatment effects for students with prior attendance problems. We repeat this exercise separately for each subgroup of interest.

4. Results

ITT Estimates of Program Impacts

High School Dropout and Graduation

Table 3 reports the ITT estimates of the difference between the treatment group and the control group from equation (1) on both high school dropout and graduation rates with each successive column adding an additional set of controls. The first column of Panel A shows the raw difference with no controls and indicates that the probability of graduating from high school on time during the post-program observation period was 4.4 percentage points higher for students in the treatment versus the control group—a 7.0 percent improvement over the control group mean of 63.4 percent. Adding in covariates for demographic, academic, and school characteristics, and controlling for baseline outcomes, has little impact on the unrestricted estimate, although the precision does improve. Over time, youth in the control group do catch up, such that the impact of the program on graduating from high school at all during the four-year observation period is somewhat smaller (4.0 percentage points). Correspondingly, we also find that dropout rates were reduced by 2.5 percentage points during the full observation window, with most of the improvement occurring in the year immediately after winning the lottery (1.9 percentage points).

To our knowledge, this is the first study to document an improvement in high school dropout and graduation rates associated with any summer jobs program. What might be driving the long-term improvement in on-time high school graduation that we observe for the treatment group? In the following sections, we explore the effects of being offered a job through the SYEP on intermediate outcomes—specifically, better attendance and academic performance—to explore whether these proximal outcomes could serve as potential mediators for the program's impact on high school graduation.

Attendance

During the first year after participation in the program, the Boston SYEP had strong positive impacts across all of our attendance measures (see Table 4). With the inclusion of all controls, we find that attendance rates improved by 2.4 percentage points, or 3.4 school days—effect sizes that are similar in magnitude to those of Leos-Urbel (2014). More importantly, the magnitude of the program's impact on attendance was large enough to reduce chronic absenteeism by 5.9 percentage points during the school year after winning the SYEP lottery—a 21.2 percent improvement. This is similar in magnitude to impacts attributed to other initiatives focused on boosting attendance, such as the Early Warning Intervention and Monitoring System (EWIMS).[16]

Interestingly, the relative difference in attendance rates between the treatment and control groups in the post period is largely driven by the treatment group *not* experiencing a decrease in their attendance rate from the prior year. Given that attendance typically falls as youth age, this suggests that the SYEP might act as a preventive measure to prevent chronic absenteeism among older youth and possibly contribute to the higher graduation rate among the treatment group. Indeed, our mediation analysis from equation (2) confirms that the improvement in attendance is positively correlated with a greater likelihood of graduating from high school (see Table A10).

[16] A recent evaluation of the Early Warning Intervention and Monitoring System (EWIMS) indicates that the program has reduced chronic absenteeism rates from 14 to 10 percent—an improvement of 28.6 percent relative to baseline. EWIMS is primarily a monitoring system, rather than a single intervention, but includes highly detailed and structured guidance for schools, along with a tool to help monitor student attendance and academic performance. Interventions for students found to be off-track are determined and implemented by school or district staff. See https://ies.ed.gov/ncee/edlabs/regions/midwest/pdf/REL_2017272.pdf for more details.

Moreover, the inclusion of attendance as an explanatory variable also reduces both the magnitude and significance of the coefficient on the main SYEP treatment dummy, suggesting that the direct effect of the program on graduation is either partially or fully driven by the program's impact on attendance.

More importantly, the relative improvement in attendance by the treatment group did not simply reflect fewer days missed due to illness or other excused absences, but also a reduction in truancy, suggesting a behavioral shift in the propensity to attend school.[17] While the number of days attended rose by 3.4 days, most of this improvement (2.1 days) came from a reduction in unexcused absences (-17.7 percent). This is on par with other interventions aimed at addressing chronic absenteeism, such as notifying parents of absences via postcard (10 percent) or text messaging (17 percent).[18]

Academic Performance

The program had a small positive impact on overall GPA in year one, but this did not manifest in any reduction in course failures. As shown in Table 5, in models that control for all individual and school factors, the overall GPA for the treatment group was 0.13 points higher than the control group, a 6.8 percent improvement. Surprisingly, our mediation analysis indicates that this small increase in course performance contributes significantly to boosting on-time high school graduation among the treatment group. Even when including both attendance and GPA as explanatory variables as in equation (2), the coefficient on GPA is positive and significant and similar in magnitude to the coefficient on attendance. This suggests that improvements in course

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[17] This is consistent with prior research by Heller (2014) and Modestino (2019) showing that SYEPs reduce delinquent behavior, as captured by criminal arrest and arraignment data.

[18] Rogers and Feller (2014) randomly assign parents of high-risk K-12 students to receive one of three year-long regimes of personalized information. The most effective regime reduced chronic absenteeism by 10 percent across all grade levels, partly by correcting parents' biased beliefs about their students' total absences. Bergman and Chan (2017) find that low-cost text messaging to parents has been shown to improve attendance by 17 percent.

performance during the year after participating in the Boston SYEP are indeed correlated with a greater likelihood of graduating from high school on time, and that this mediating influence operates separately from that of improving attendance (see Table A10). Note that we interpret this as merely suggestive evidence of mediation since there may be other variables that are correlated with either attendance or GPA that could be driving the relationship.

Similarly, when we examine the impact of the Boston SYEP on students taking and passing the Massachusetts Comprehensive Assessment System (MCAS), a statewide standardized test, we find only a small increase in the likelihood of taking the science exam and a marginally significant improvement in students achieving proficiency (see Table A9).[19] However, because students typically take the MCAS during the spring semester of their sophomore year, we are only able to measure impacts on rising ninth and tenth graders—about half of the SYEP participants in our sample. In contrast, prior studies of the New York City SYEP are able to observe whether students take any of the annual statewide Regents exams and find small (1-3 percent) but significant increases in the likelihood of taking and passing both the math and ELA exams (Leos-Urbel 2014; Schwartz et al. 2020).

Dosage and Duration of Impacts

Given that both attendance and course performance appear to have a role to play in improving high school graduation rates, it seems important to test whether these effects endure beyond one year after participation and, if so, whether the longer-term impacts of the program depend on a second summer of SYEP. Table 6 presents our estimates of the program's impact on

[19] Students must receive a passing grade of at least 240 (Proficient) on both the mathematics and ELA tests and a score no lower than 220 (Needs Improvement) to receive a high school diploma. Note that because students take the MCAS in the tenth grade, we must observe participants as ninth graders in the prior summer to assess whether the program has any impact on test-taking or performance, limiting the number of students for whom we can assess MCAS impacts.

outcomes measured two years after participating in the program for those youth who had not yet graduated and could be followed for a second year. Panel A tests whether the program's impacts fade by comparing the coefficient on the SYEP dummy for separate regressions where the dependent variable is the outcome measured one year versus two years post-program. While the main impacts on the attendance rate, days attended, and days unexcused persist, the program's effect on chronic absenteeism and GPA disappears by the second year.

We also explore whether a second summer of participation might be useful in maintaining the effects of the program beyond the first year. In Panel B, we further limit the sample to the treatment group—those who initially won the SYEP lottery in 2015—and include a dummy indicating whether an individual also won the lottery in 2016 to look at the marginal effect of a second summer of treatment on the two-year outcomes. The OLS regressions show that the positive impacts on attendance from the first year endure for youth who apply and are randomly selected to participate for a second summer. Interestingly, the impacts on GPA are also present among youth with a second summer of participation, suggesting that academic performance is perhaps even more dependent on skills being reinforced over time. We find similar results for the sub-sample of youth in the treatment group who also chose to apply to the program in 2016, suggesting that these findings do not simply reflect greater intrinsic motivation.[20]

While these results are suggestive, we acknowledge that we cannot attribute an entirely causal interpretation to the second-year results for the repeat participants because this is conditional on having applied for a second time, which may indicate a greater intrinsic motivation or ability. However, SYEP program data reveal that the group of repeat applicants is

[20] Alternatively, we can also use the full sample and instrument for the number of summers of treatment (0, 1, or 2) and produce effects that are both statistically significant and similar in magnitude (see Table A11).

not as exclusive as one might imagine. The eligible lottery winners from the first summer who choose to apply for a second time are on average younger and more likely to be male, Black, disabled, and living in a household that receives public assistance—characteristics that are often negatively, rather than positively, correlated with academic success. Still, we caution against taking these results as purely causal evidence that a second summer of participation produces program outcomes that are stronger or more persistent.

Heterogeneity in Outcomes by Subgroup

As prior research has shown, the impact of summer jobs programs on school outcomes might be greater for more marginal students (Leos-Urbel 2014). In particular, studies of chronic absenteeism find that improvements are more likely to be observed among older students, those with limited English ability, and at-risk youth, such as those who are homeless or living in households that receive public assistance (Utah Education Policy Center 2012). We note that our subgroup analyses were not pre-specified, but rather are exploratory. Still, exploratory subgroup analyses can be useful for generating new hypotheses and for robustness checking.

Table 7 reports the ITT estimate of the differential program impact on the improvement in academic outcomes for the subgroups described above as well as for students experiencing chronically high absenteeism during the baseline pre-period (e.g., the 2014-15 school year). We run separate regressions for each listed outcome and report the coefficients on the interactions of the main SYEP treatment effect with a set of dummy variables that fully specify the sample according to the subgroup of interest. Each regression also includes the full set of covariates from the previous tables. We then test the coefficients on the subgroup interactions for equivalence to determine whether the program's differential impact on a given subgroup is statistically significant.

The Boston SYEP appears to have a greater impact on certain subgroups, but only for the more proximal intermediary outcomes. Consistent with prior studies, the program's impact on attendance rates is three times greater for males, youth of legal dropout age, and students who were chronically absent prior to participating in the program (Leos-Urbel 2014). For students of legal dropout age, the program's boost in GPA is also three times as large as that for younger youth. Although the program appears to increase the likelihood of high school graduation more for students with limited English proficiency and low socio-economic status, the coefficients are imprecisely estimated due to having far fewer youth that fall into these subgroups.

Insights from Survey Data

What might be driving the reduction in chronic absenteeism and subsequent increase in on-time high school graduation rates? It could be that participating in the Boston SYEP improves behaviors that are important to academic success. For example, focus group participants repeatedly stressed that "being on time" is one of the most important lessons they learned at their summer job. It could also be that the program's career readiness curriculum, coupled with real-world experience and mentoring, boosts career and academic aspirations that lead to greater motivation or effort in school during the following year. Finally, prior research has shown that SYEPs can reduce the propensity to engage in delinquent behavior by developing soft skills such as managing emotions and resolving conflicts with peers (Modestino 2019).

We explore these mechanisms further by assessing the degree to which SYEP participants learn new skills over the summer and how these changes are correlated with improvements in attendance and high school graduation after participating in the program. To do this, we link the administrative data on secondary school outcomes described above to the short-term behavioral changes in skills and attitudes, as measured by a survey that was completed at the end of the summer by 1,327 youth (663 treatment youth and 664 control youth). Because we rely on self-reported survey data to assess these short-term behavioral changes in skills and attitudes, this analysis should be regarded as more exploratory in nature. In particular, there are large differences in the response rates across youth in the treatment and control groups that likely give rise to selection on both observable and unobservable characteristics.[21] Nevertheless, given the lack of data and evidence on potential mechanisms, we feel that there are still some key insights to be gained. Whereas the first part of the analysis using administrative data established the causal impacts of the Boston SYEP on school outcomes, the goal here is to provide a glimpse into *how* the program achieves those outcomes.

Youth participating in the Boston SYEP experienced significant improvements across a variety of short-term behaviors and skills that could plausibly be correlated with the subsequent improvements in school outcomes that were observed in the administrative data. Figure 2 reports the coefficients from separate probit regressions estimating the difference between the treatment and control groups responding to key questions about academic aspirations, work habits, and soft skills.[22] For example, among youth in the treatment group responding to the survey, the share reporting that had gained a mentor over the summer was 15.2 percentage points higher than the control group. This likely reflects SYEP participants being assigned to a job supervisor who can act as a mentor to provide strong, supportive, and sustained relationships with adults and peers, all of which are critical for adulthood (Nagaoka et al. 2015).

In addition, the types of early work experience provided by SYEPs give participants the

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[21] Specifically, survey respondents in the control group appear to be positively selected based on observable characteristics such as being white, female, and living in a two-parent household. Moreover, because the survey was administered via email to the control group at the end of the summer, it's likely that youth who responded are more highly motivated than their peers. This sets a high bar for comparison with the treatment group. Please see the online appendix for details about the survey construction, deployment, and data collection.
[22] See Table A12 in the appendix for the full set of survey measures.

opportunity to engage in tasks that help them develop a sense of agency, identity, and competency necessary for adult roles and success. Youth in the treatment group were significantly more likely to report having developed good work habits, such as being on time and keeping a schedule, as well as essential soft skills, such as managing emotions and asking for help. Notably, youth participating in the Boston SYEP were also 4.3 percentage points higher than the control group respondents to report that they were saving for school tuition—an indication that the participants are not only exposed to experiences that might boost academic aspirations but are also motivated to act on those ambitions.

We then condition the sample on program participation to explore how the short-term behaviors and skills that occur over the summer might be correlated with subsequent improvements in longer-term academic outcomes. Figure 3 reports estimates from separate regressions where we insert the survey-based measures as independent variables along with the prior set of controls and the dependent variable is either on-time high school graduation or the attendance rate one year post-program.[23] Panel A shows that on-time high school graduation is strongly correlated with gaining a mentor, learning to be on time, and managing one's emotions. This is consistent with prior research on summer jobs programs that has linked improvements in social-emotional learning to reductions in delinquent behavior among youth (Heller 2014; Modestino 2019). It also highlights the importance of mentorship and the role it can play in mediating longer-term outcomes, particularly for teens who are still developing into adults and especially for youth from less-advantaged backgrounds who might lack strong adult mentors in their lives. Panel B shows that attendance rates are also strongly correlated with these same factors, particularly work habits such as being on time and keeping a schedule. This is consistent

[23] See Table A13 for a full set of regressions showing the relationship between the full set of survey measures and each of the academic outcomes of interest.

with prior research on the effects of work-based learning programs that link classroom instruction to workplace skills through internships, mentoring, workplace simulations, and apprenticeships (Colley and Jamison 1998). Yet we note that this exploratory analysis cannot fully disentangle the SYEP program effects from other factors, such as the benefits of simply providing youth and their families with additional income.

5. Conclusion

Overall, we find that the Boston SYEP had a significant and meaningful impact on high school graduation rates among youth. Being randomly selected into the Boston SYEP increased the probability of graduating from high school on time by 4.4 percentage points over the control group mean of 63.4 percent—a 7.0 percent improvement relative to the control group. To our knowledge this is the first study to find any effect of summer jobs programs on high school graduation. The magnitude of this impact is similar to the gap in on-time graduation rates that currently exists for economically disadvantaged students in the Boston Public School system.

Our mediation analysis indicates that the higher probability of on-time high school graduation appears to be driven by better attendance (2.7 percent) among students in the year after being selected for the program, particularly those who had experienced baseline chronic absenteeism or were age 16 years and older. This improvement in attendance is similar to the effect size found by Leos-Urbel (2014) once we account for the different underlying sources of variation in that study. We also find evidence of small but statistically significant increases in GPA that have previously not been detected in the literature.

Why do our findings differ from those found in prior research? Although we cannot entirely rule out differences in program design and labor market context, we believe that having access to state-level administrative data is a key factor. Our ability to track students across both public and charter schools throughout the state yields both a higher match rate over multiple years and better measurement of outcomes over time. Both of these strengths overcome the potential for significant measurement error that would attenuate the effect sizes of earlier studies.

Secondarily, there are contextual differences between Boston and other summer jobs programs that may also explain why our findings differ from prior studies. In Chicago, participants were less engaged in school and more likely to be court-involved at baseline, perhaps explaining why the program shows strong benefits in terms of criminal justice outcomes but less so in terms of academic outcomes. In New York City, the application process and scale of the program result in a high degree of crossover from the control group over time, producing a weaker contrast between the treatment and control groups that reduces the estimate of the program's impacts. Despite these differences, when comparing effect sizes in terms of improvements over baseline, our estimates are actually more moderate than they first appear.[24]

More exploratory analyses of the duration of program impacts reveal that while the main program's impacts on days attended and unexcused absences persist into the second year, the GPA improvements appear to fade out after the first year, except among youth who apply for and win a second summer of participation. Due to potential selection issues when applying for the lottery a second time, more work is needed to cleanly identify the minimum "dosage" (e.g., number of summers) needed to achieve meaningful impacts—a high priority for oversubscribed programs, such as the Boston SYEP, where participation is assigned by lottery. Currently, about one-third of the Boston SYEP's funding comes from state sources, which stipulate that only 20 percent of the youth served in any given year can be repeat participants. Such participation constraints might not be efficient if multiple summers are needed to obtain lasting impacts.

[24] See Table A14 in the online appendix.

Though exploratory, our analysis of potential mechanisms associated with the Boston SYEP is useful for thinking about *how* summer jobs programs achieve better outcomes among the youth being served. Our survey data reveal that the program develops basic work habits, increases aspirations to attend college, and improves social skills—and that these behavioral changes are correlated with subsequent improvements in attendance as well as the likelihood of graduating from high school on time. These findings give researchers some insights into the behavioral changes that occur during the program while also providing a look inside the "black box" of summer job programs to identify how they affect youth outcomes in the long run. This is an area where future research is sorely needed, particularly around the role of mentorship.

When assessing the value of any program, effect sizes should also be considered relative to their costs (Kraft 2020). The broader education literature has documented that high school graduates have better outcomes than dropouts along a number of dimensions, including higher employment rates and incomes (Child Trends 2017), and lower rates of criminal activity and take-up of social services (Lochner and Moretti 2004). By some estimates, each new high school graduate confers a net benefit to taxpayers of roughly $127,000 over the graduate's lifetime.[25] The Boston SYEP costs roughly $2,000 per participant, resulting in a total cost of $2.4 million for the 1,200 youth who participated through ABCD during the summer of 2015.[26] Given that the program appears to increase the likelihood of any high school graduation by 4 percentage points, this would yield an additional 48 graduates, who on net would collectively confer a benefit of $6 million over their lifetimes, resulting in a benefit-to-cost ratio of more than 2-to-1.

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[25] Levin, Henry and Cecelia Rouse. 2012. The true cost of high school dropouts. *The New York Times*, January 25, 2012. https://www.nytimes.com/2012/01/26/opinion/the-true-cost-of-high-school-dropouts.html

[26] This includes an average of just over $1,400 in wages. From a societal perspective, the wage cost is simply a transfer from the government to the youth and so is not generally counted as a net change in overall resources. This leaves an administrative program cost of $600, although if one wanted to separate the costs and benefits that accrue to the government, participants, and society, then wages would appear as a cost to the government and a benefit to participants.

Finally, how do summer jobs programs compare with other interventions that have been shown to improve attendance without the administrative costs of soliciting commitments from employers, matching teens to jobs, and supervising youth at multiple job sites? Other studies have found that lower-cost interventions, such as notifying parents of absences via postcard or text messaging, produce improvements in attendance rates that are similar in magnitude (Rogers and Feller 2014) to those we found for the Boston SYEP.

Yet, SYEPs also provide additional benefits to individuals and their families that may further outweigh the program's costs. For example, SYEPs confer job experience that may yield additional advantages in terms of future employment, career pathways, or post-secondary education. During the summer after the COVID-19 pandemic, there were even fewer opportunities for youth to develop work-related skills, with the unemployment rate for U.S. teens rising to 19.3 percent in July 2020, and even higher for Black (22.5 percent) and Hispanic/Latino (21.0 percent) youth (Bureau of Labor Statistics 2020). Even before the pandemic, the long-term decline in youth employment since 2000 has meant that teens are less likely to work compared to two decades ago, leading to fewer opportunities to develop the work habits and soft skills demanded by employers (Loewenberg 2020). Finally, SYEPs also help families at or near the poverty line by providing income to youth—with upwards of one in five Boston SYEP participants contributing directly to their household's expenses. As such, summer jobs programs will continue to be an important vehicle for youth employment, family income support, and skills development. If they also confer benefits in terms of academic achievement, as this study suggests, then such programs also have an important role to play in the landscape of out-of-school-time activities.

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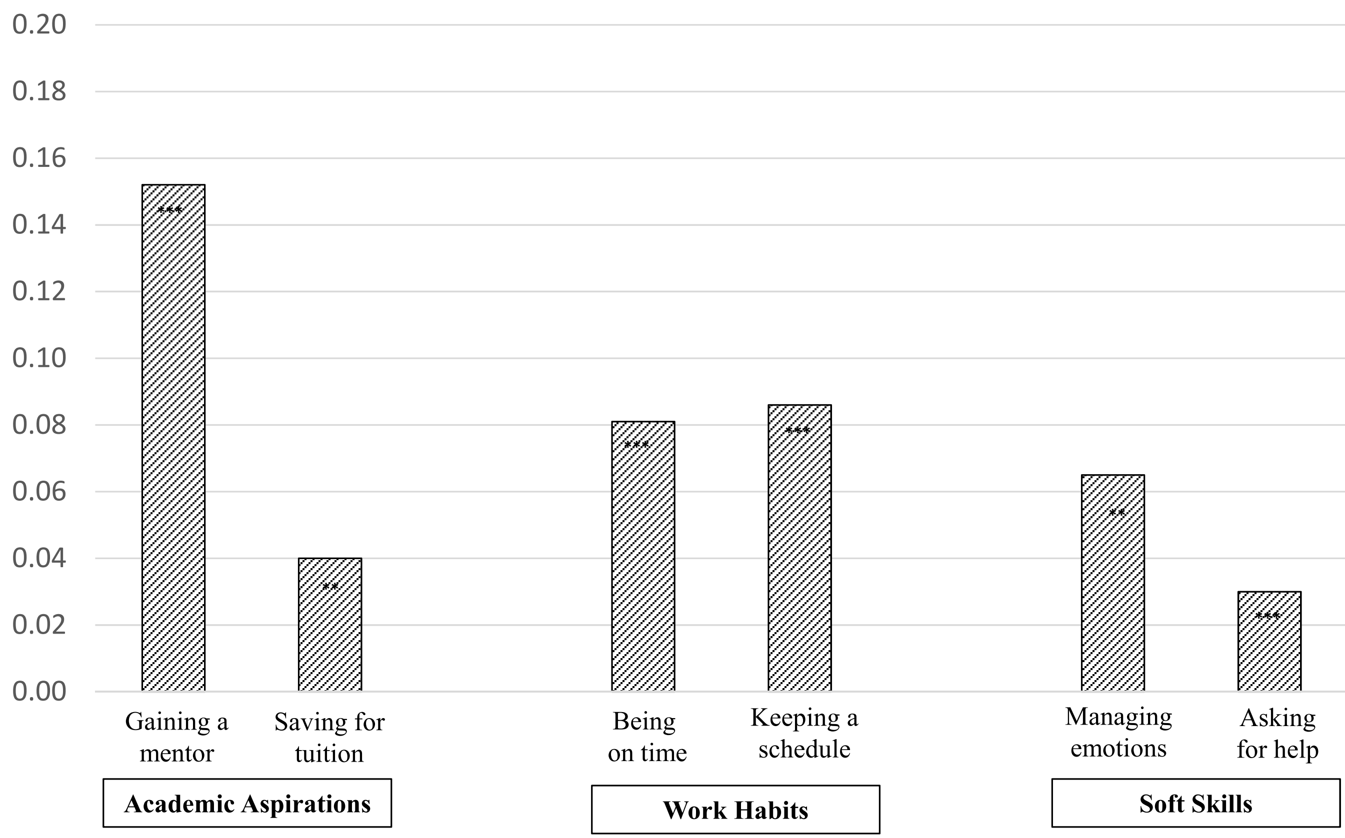
Figure 1. Program Participation and Data Collection, Timeline, and Tracking

Pre-Program Period	SYEP Program Period	Post-Program Observation Period			
AY 2014-15 Baseline Data Collection Application Random Assignment	**July/Aug 2015** Pre-Survey Participation Post-Survey	**AY 2015-16** 1-Year Post Data Collection	**AY 2016-17** 2-Year Post Data Collection	**AY 2017-18** 3-Year Post Data Collection	**AY 2018-19** 4-Year Post Data Collection
	Tracking School Outcomes Using Administrative Data by Grade Over Time				
Grade 8 Enrollment Status Attendance Course Grades		Enrollment Status Attendance Course Grades	Enrollment Status Attendance Course Grades MCAS Test Score	Enrollment Status Attendance Course Grades	Enrollment Status Attendance Course Grades
Grade 9 Enrollment Status Attendance Course Grades		Enrollment Status Attendance Course Grades MCAS Test Scores	Enrollment Status Attendance Course Grades	Enrollment Status Attendance Course Grades	Graduated: No Further Data Available
Grade 10 Enrollment Status Attendance Course Grades		Enrollment Status Attendance Course Grades	Enrollment Status Attendance Course Grades	Graduated: No Further Data Available	
Grade 11 Enrollment Status Attendance Course Grades		Enrollment Status Attendance Course Grades	Graduated: No Further Data Available		
Grade 12 Enrollment Status Attendance Course Grades		Graduated: No Further Data Available			

Figure 2. Comparison of Survey Responses: Treatment versus Control Group
Marginal Effect from Probit Regression

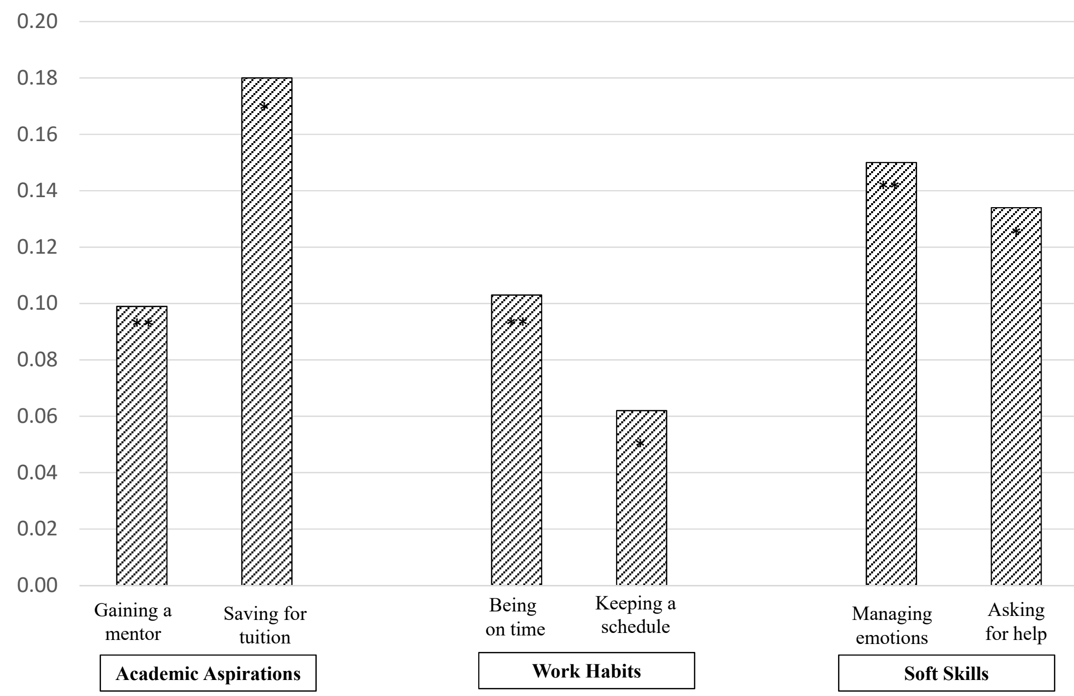


Source: Authors' calculations based on survey data collected by the City of Boston Mayor's Office of Workforce Development.

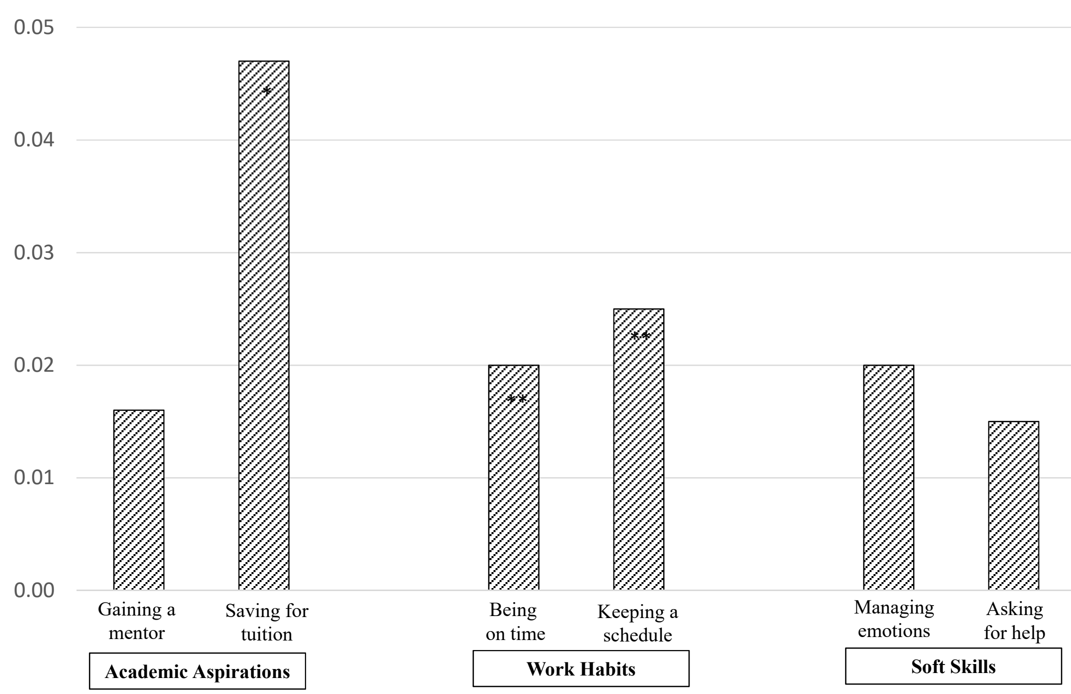
Note: Each coefficient is the marginal effect from a separate probit regression of the outcome on a dummy variable for treatment controlling for age, gender, race, two-parent family, and English as the primary language. *Indicates that the difference is statistically significant at the 10 percent level; ** at the 5 percent level; and *** at the 1 percent level.

Figure 3. Correlation between Short-Term Behavioral Changes and SYEP Impact on School Outcomes

Panel A. Dependent Variable: On-Time High School Graduation



Panel B. Dependent Variable: Attendance One Year Post-Program



Source: Authors' calculations based on survey data collected by the City of Boston Mayor's Office of Workforce Development and administrative school records provided by the Massachusetts Department of Elementary and Secondary Education.

Note: Estimates are from separate regressions for program participants where we insert the survey-based measures as independent variables. All regressions include a set of controls for pre-program demographic, academic, and school characteristics as well as the baseline (pre-program) outcomes listed in Table 1. Probit is used to estimate on-time high school graduation and the coefficients reported in the table are the average marginal effects. * Indicates difference is statistically significant at the 10 percent level and ** at the 5 percent level.

Table 1. Mean Pre-program Characteristics for Treatment and Control Groups Matched to Administrative Data

	Selected (treatments) (1)		Not Selected (controls) (2)		Treatment-Control (3)		
	Mean	Std. Error	Mean	Std. Error	Difference	*p* -value	
Demographic characteristics							
Age	15.345	(0.048)	15.407	(0.029)	-0.061	(0.274)	
Percent female	0.546	(0.017)	0.555	(0.011)	-0.010	(0.309)	
Percent African American/Black	0.528	(0.017)	0.539	(0.011)	-0.010	(0.189)	
Percent Asian	0.075	(0.009)	0.052	(0.005)	0.023	(0.024)	**
Percent White	0.065	(0.009)	0.079	(0.005)	-0.014	(0.190)	
Percent other/two or more races	0.331	(0.016)	0.330	(0.010)	0.001	(0.946)	
Percent Chinese	0.002	(0.002)	0.001	(0.001)	0.001	(0.427)	
Percent English	0.953	(0.007)	0.958	(0.004)	-0.005	(0.581)	
Percent Spanish	0.030	(0.006)	0.023	(0.003)	0.007	(0.279)	
Percent other language	0.014	(0.003)	0.018	(0.002)	-0.003	(0.415)	
Percent limited English ability	0.067	(0.009)	0.070	(0.005)	-0.003	(0.748)	
Percent homeless	0.048	(0.048)	0.057	(0.005)	-0.009	(0.564)	
Percent receiving public assistance	0.183	(0.013)	0.169	(0.008)	0.013	(0.385)	
Percent disabled	0.027	(0.006)	0.030	(0.004)	-0.003	(0.630)	
Academic characteristics							
Percent high-need special education	0.064	(0.008)	0.057	(0.005)	0.008	(0.422)	
Percent in METCO (bussing) program	0.066	(0.008)	0.065	(0.005)	0.001	(0.947)	
Percent switched schools during academic year	0.103	(0.010)	0.108	(0.007)	-0.005	(0.688)	
Percent in grade 8	0.347	(0.016)	0.350	(0.010)	-0.003	(0.885)	
Percent in grade 9	0.320	(0.016)	0.312	(0.010)	0.008	(0.529)	
Percent in grade 10	0.195	(0.013)	0.197	(0.009)	-0.002	(0.429)	
Percent in grade 11	0.138	(0.011)	0.141	(0.007)	-0.003	(0.689)	
School characteristics							
Percent attending a charter school	0.149	(0.012)	0.162	(0.008)	-0.013	(0.368)	
Percent of school population scoring proficient or better on MCAS	53.687	(0.832)	54.605	(0.554)	-0.918	(0.359)	
Baseline (pre-program) outcomes							
Percent dropped out of school	0.012	(0.003)	0.008	(0.002)	0.003	(0.378)	
Attendance rate	0.899	(0.004)	0.903	(0.003)	-0.004	(0.292)	
Percent chronically absent	0.282	(0.015)	0.274	(0.010)	0.008	(0.332)	
Total days attended	160.195	(0.838)	160.928	(0.655)	-0.733	(0.315)	
Total days of unexcused absences	12.030	(0.517)	11.614	(0.394)	0.416	(0.310)	
Grade Point Average (GPA)	1.878	(0.039)	1.890	(0.024)	-0.012	(0.370)	
Percent failing any course	0.281	(0.017)	0.285	(0.011)	-0.004	(0.473)	
Percent failing a math course	0.164	(0.013)	0.166	(0.008)	-0.002	(0.651)	
Percent failing an ELA course	0.194	(0.013)	0.198	(0.009)	-0.004	(0.371)	
Number of SYEP applicants in grades 8-11 matched to administrative data	854		2,157		3,011		
Number of SYEP applicants in grades 8-11 in full sample	951		2,421		3,372		
Match rate of SYEP applicants across full observation period	89.8%		89.1%		0.70	(0.871)	

Note: This table provides mean values of preexisting demographic, academic, and school characteristics as well as pre-program outcomes for the sample of youth who were matched to the administrative data for both the pre- and post-program observation periods. To test whether the treatment variable is correlated with any of the individual's pre-program characteristics we compare the effect of winning the SYEP lottery on pre-program demographic, academic, and school characteristics as well as baseline (pre-program) outcomes. Each row provides the coefficient and p-value from a regression where the dependent variable takes the value of 1 if the individual received an offer to participate in SYEP and the independent variable is the characteristic that is listed. Standard errors are in parentheses. See Table A5 in the appendix for separate F-tests of joint significance for each grouping of covariates used in the analysis.

Source: Application data on program participation were provided by the Boston Mayor's Office of Workforce Development (OWD). Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table 2. Definition of Outcome Measures

	Definition (1)	Time Period (2)	Source of Data (3)
Panel A. Primary Outcomes			
High School Graduation			
Graduated on time during the post observation period	Graduated as expected by 12th grade given their pre-period grade level	Full post-program observation period 2015-16 through 2018-19 school years	Massachusetts adminsitrative school records
Graduated at any point during the post observation period	Graduated by 12th grade or later		
Dropout			
Dropped out at any point during the observation period	Enrollment status listed as dropped out for any reason	Full post-program observation period 2015-16 through 2018-19 school years	
Dropped out one year post-program	Enrollment status listed as dropped out for any reason	One year post-program at end of 2015-16 school year	
Panel B. Potential Mediators			
Attendance			
Attendance rate	Number of days attended / Number of days in membership at all schools	Separately for one year (2015-16) and two years (2016-17) post-program	Massachusetts administrative school records
Increased attendance rate	Positive change in attendance rate post- versus pre-program		
Decreased attendance rate	Negative change in attendance rate post- versus pre-program		
Chronically absent	Attendance rate is <90%		
Total days attended	Number of days attend in a school year		
Total days of unexcused absences	Number of days of unexcused absences in a school year		
Course grades			
Overall GPA	One and two years post-program	Separately for one year (2015-16) and two years (2016-17) post-program	Massachusetts administrative school records
Failed any course	Letter grade of "F" for the year		
Failed a math course	Letter grade of "F" in a designated math course for the school year		
Failed an ELA course	Letter grade of "f" in a designated ELA course for the school year		
Standardized test scores			
Took MCAS on time	Taking the MCAS in the spring of the 10th grade year	Spring of 10th grade year post-program for 8th and 9th graders	Massachusetts administrative school records
Normalized scaled score	Raw scores converted to standardized units (mean 0, variance 1)		
Proficient or better	Score was classified as "proficient" or "advanced" by DESE in the exam year		
Panel C. Exploratory Mechanisms			
Academic aspirations			
Gaining a mentor	Responded "Yes" to "Do you have an adult that you consider a mentor?"	Pre/post-program July/August 2015	SYEP Survey Data
Saving for tuition	Responded "School Tuition" to "Is there something in particular that you are saving your money for?"		
Work habits			
Being on time	Responded "Agree" or "Strongly Agree" on Likert scale		
Keeping a schedule	Responded "Agree" or "Strongly Agree" on Likert scale		
Soft skills			
Managing emotions	Responded "Agree" or "Strongly Agree" on Likert scale		
Asking for help	Responded "Agree" or "Strongly Agree" on Likert scale		

Table 3. ITT Estimates of SYEP Impact on High School Graduation and Dropout during Post-Program Observation Period

	Coefficient on Winning the Lottery (Treatment Dummy)				
	(1)	(2)	(3)	(4)	(5)
Panel A. High school graduation					
Graduated on time during the post-program observation period	0.044 **	0.044 **	0.043 **	0.044 **	-----
	(0.019)	(0.019)	(0.018)	(0.018)	
Graduated at any point during the post-program observation period	0.040 **	0.039 **	0.039 **	0.040 **	-----
	(0.018)	(0.018)	(0.017)	(0.017)	
Panel B. Dropout					
Dropped out one year post-program	-0.019 **	-0.018 **	-0.019 ***	-0.019 ***	-0.019 ***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Dropped out at any point during the post-program observation period	-0.026 **	-0.026 **	-0.025 **	-0.025 **	-0.025 **
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Demographic characteristics	No	Yes	Yes	Yes	Yes
Academic characteristics	No	No	Yes	Yes	Yes
School characteristics	No	No	No	Yes	Yes
Baseline outcomes	No	No	No	No	Yes
Number of youth	3,011	3,011	3,011	3,011	3,011

Note: The sample includes youth who were matched in both 2014-15 and 2015-16 and able to be tracked throughout the post-period (see Table A2). Each coefficient is from a separate regression where the dependent variable is the outcome listed. Each column successively adds in a set of controls for pre-program demographic, academic, and school characteristics as well as the baseline (pre-program) outcomes listed in Table 1. Probit is used to estimate results for binary outcomes. For these non-linear specifications, the coefficients reported in the table are the average marginal effects. Robust standard errors in parentheses.

Source: Application data on program participation were provided by the Boston Mayor's Office of Workforce Development (OWD). Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table 4. ITT Estimates of SYEP Impact on School Attendance One Year Post-Program

	Coefficient on Winning the Lottery (Treatment Dummy)				
	(1)	(2)	(3)	(4)	(5)
Attendance rate	0.024 ***	0.024 ***	0.025 ***	0.025 ***	0.024 ***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Chronic absenteeism indicator	-0.059 ***	-0.059 ***	-0.060 ***	-0.060 ***	-0.059 ***
	(0.019)	(0.019)	(0.018)	(0.018)	(0.016)
Total days attended	3.211 **	3.161 **	3.389 **	3.441 **	3.351 **
	(1.348)	(1.310)	(1.294)	(1.286)	(1.239)
Total days of unexcused absences	-1.989 **	-1.996 **	-2.152 **	-2.157 **	-2.073 **
	(0.855)	(0.847)	(0.835)	(0.831)	(0.821)
Increased attendance rate	0.036 *	0.039 *	0.039 *	0.038 *	-----
	(0.021)	(0.021)	(0.021)	(0.021)	
Decreased attendance rate	-0.055 **	-0.057 **	-0.056 **	-0.056 **	-----
	(0.021)	(0.021)	(0.021)	(0.021)	
Demographic characteristics	No	Yes	Yes	Yes	Yes
Academic characteristics	No	No	Yes	Yes	Yes
School characteristics	No	No	No	Yes	Yes
Baseline outcomes	No	No	No	No	Yes
Number of youth	3,011	3,011	3,011	3,011	3,011

Note: The sample includes youth who were matched in both 2014-15 and 2015-16 and able to be tracked throughout the post-period (see Table A2). Each coefficient is from a separate regression where the dependent variable is the outcome listed. Each column successively adds in a set of controls for pre-program demographic, academic, and school characteristics as well as the baseline (pre-program) outcomes listed in Table 1. Probit is used to estimate results for binary outcomes. For these non-linear specifications, the coefficients reported in the table are the average marginal effects. Robust standard errors in parentheses.

Source: Application data on program participation were provided by the Boston Mayor's Office of Workforce Development (OWD). Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table 5. ITT Estimates of SYEP Impact on Course Performance One Year Post-Program

	Coefficient on Winning the Lottery (Treatment Dummy)				
	(1)	(2)	(3)	(4)	(5)
Overall GPA	0.127 **	0.117 **	0.119 **	0.124 ***	0.129 ***
	(0.050)	(0.044)	(0.040)	(0.040)	(0.036)
Failed any course indicator	-0.004	-0.002	-0.004	-0.004	-0.005
	(0.021)	(0.020)	(0.019)	(0.019)	(0.019)
Failed a math course indicator	-0.009	-0.007	-0.008	-0.009	-0.007
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Failed an ELA course indicator	-0.005	-0.005	-0.006	-0.007	-0.003
	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
Demographic characteristics	No	Yes	Yes	Yes	Yes
Academic characteristics	No	No	Yes	Yes	Yes
School characteristics	No	No	No	Yes	Yes
Baseline outcomes	No	No	No	No	Yes
Number of youth	3,011	3,011	3,011	3,011	3,011

Note: The sample includes youth who were matched in both 2014-15 and 2015-16 and able to be tracked throughout the post-period (see Table A2). Each coefficient is from a separate regression where the dependent variable is the outcome listed. Each column successively adds in a set of controls for pre-program demographic, academic, and school characteristics as well as the baseline (pre-program) outcomes listed in Table 1. For these non-linear specifications, the coefficients reported in the table are the average marginal effects. Robust standard errors in parentheses.

Source: Application data on program participation were provided by the Boston Mayor's Office of Workforce Development (OWD). Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table 6. ITT Estimates of SYEP Impacts by Duration and Dosage Intensity

	Attendance Rate	Chronic Absenteeism	Days Attended	Days Unexcused	Overall GPA
	(1)	(2)	(3)	(4)	(5)
Panel A. Duration: One- versus Two-Year Outcomes					
Among All Applicants Followed for Two Years Post-Program					
Dependent variable = Outcome one year post					
Coefficient on winning the SYEP lottery in 2015	0.027 **	-0.055 ***	3.328 **	-2.232 **	0.106 **
	(0.008)	(0.018)	(1.546)	(0.839)	(0.039)
Dependent variable = Outcome two years post					
Coefficient on winning the SYEP lottery in 2015	0.025 **	-0.027	4.027 **	-1.962 **	0.025
	(0.010)	(0.020)	(1.818)	(0.911)	(0.041)
Number of youth followed two-years post-program	2,636	2,636	2,636	2,636	2,636
Panel B. Dosage: Two Year Outcomes for One versus Two Summers of Treatment					
Among All 2015 SYEP Lottery Winners (Treatment Group)					
Dependent variable = Outcome two years post					
Coefficient on also winning the SYEP lottery in 2016	0.035 **	-0.031	8.363 **	-1.486	0.158 **
	(0.017)	(0.038)	(3.241)	(1.447)	(0.075)
Number of 2015 lottery winners followed two years post-program	748	748	748	748	748
Among 2015 SYEP Lottery Winners (Treatment Group) who Applied for a Second Summer					
Dependent variable = Outcome two years post					
Coefficient on also winning the SYEP lottery in 2016	0.037 **	-0.052	8.882 **	-3.498 *	0.207 **
	(0.018)	(0.048)	(4.039)	(1.992)	(0.094)
Number of 2015 lottery winners followed two years post-program who also applied in 2016	497	497	497	497	497

Note: In panel A, the sample includes youth who were matched in both 2014-15 and 2015-16 and able to be tracked throughout the post-period, excluding those who had graduated before the second year of follow-up (see Table A2). In panel B, the sample is further restricted to youth who initially won the lottery in 2015. Each coefficient is from a separate regression where the dependent variable is the outcome listed. All regressions include a set of controls for pre-program demographic, academic, and school characteristics as well as the baseline (pre-program) outcomes listed in Table 1. Probit is used to estimate results for binary outcomes. For these non-linear specifications, the coefficients reported in the table are the average marginal effects. Robust standard errors in parentheses.

Source: Application data on program participation were provided by the Boston Mayor's Office of Workforce Development (OWD). Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table 7. ITT Estimates of SYEP Impact on Outcomes Post-Program by Subgroup

		Outcome of Interest				Number of Youth
		Graduated on time	Dropped out at any point	Attendance rate	Overall GPA	by Subgroup
		(1)	(2)	(3)	(4)	(5)
Chronic absenteeism pre-program						
Attendance rate <90%		0.053	-0.055 *	0.059 ***	0.192 **	840
	std. err.	(0.035)	(0.032)	(0.017)	(0.072)	
Attendance rate >=90%		0.037 *	-0.019 *	0.015 *	0.103 **	2171
	std. err.	(0.020)	(0.011)	(0.009)	(0.044)	
Difference		0.015	-0.035	0.043 **	0.089	3011
	p-value	*0.700*	*0.294*	*0.024*	*0.287*	
Age						
16 years or older		0.049 *	-0.048 **	0.048 ***	0.233 ***	1159
	std. err.	(0.029)	(0.023)	(0.015)	(0.060)	
Under 16 years		0.048 **	-0.025 *	0.014	0.080 *	1852
	std. err.	(0.023)	(0.014)	(0.010)	(0.047)	
Difference		0.000	-0.023	0.034 **	0.154 **	3011
	p-value	*0.995*	*0.378*	*0.049*	*0.042*	
Gender						
Male		0.059 **	-0.043 **	0.046 ***	0.119 **	1347
	std. err.	(0.027)	(0.019)	(0.011)	(0.053)	
Female		0.039	-0.025	0.010	0.149 **	1664
	std. err.	(0.024)	(0.015)	(0.012)	(0.053)	
Difference		0.020	-0.018	0.036 **	-0.031	3011
	p-value	*0.579*	*0.455*	*0.022*	*0.678*	
English proficiency						
Limited English proficiency		0.090	-0.034	0.028	0.202 *	208
	std. err.	(0.063)	(0.048)	(0.029)	(0.123)	
English proficient		0.045 **	-0.033 **	0.027 ***	0.130 ***	2803
	std. err.	(0.019)	(0.013)	(0.009)	(0.040)	
Difference		0.044	-0.001	0.001	0.073	3011
	p-value	*0.499*	*0.990*	*0.961*	*0.573*	
Socioeconomic status						
Household receives public assistance		0.082 *	-0.047	0.006	0.155 *	521
	std. err.	(0.044)	(0.031)	(0.020)	(0.086)	
Household does not receive public assistance		0.041 **	-0.031 **	0.031 ***	0.130 ***	2490
	std. err.	(0.020)	(0.013)	(0.009)	(0.042)	
Difference		0.041	-0.016	-0.025	0.025	3011
	p-value	*0.395*	*0.631*	*0.243*	*0.795*	

Note: The sample includes youth who were matched in both 2014-15 and 2015-16 and able to be tracked throughout the post-period (see Table A2). We run separate regressions for each listed outcome and report the coefficients on the interactions of the main SYEP treatment effect with a set of dummy variables that fully specify the sample according to the subgroup of interest. Each regression also includes the main subgroup effect as well as the full set of controls for the pre-program demographic, academic, and school characteristics as well as the baseline (pre-program) outcomes listed in Table 1. Probit is used to estimate results for binary outcomes. Poisson regressions are used to estimate results for count outcomes. Coefficients reported in the table from non-linear estimation are the average marginal effects. Robust standard errors in parentheses.

Source: Application data on program participation were provided by the Boston Mayor's Office of Workforce Development (OWD). Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).