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

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Abstract: Over the past several decades, many urban high schools have experienced little or no improvement in closing academic achievement gaps along socioeconomic and racial lines. Recently there has been an emphasis on how time spent outside of the classroom can affect student outcomes, including high school graduation. This paper provides experimental evidence regarding a particular type of out-of-school activity—work experience—on high school academic outcomes. 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 high school graduation and dropout rates as measured by administrative school records. We find that SYEP lottery winners are 2.6 percentage points (24.8 percent) less likely to drop out of high school relative to the control group, and 6.1 percentage points more likely to graduate from high school, resulting in a benefit-to-cost ratio of 4-to-1. These improvements appear to be driven by better attendance in the year after being selected for the program, and better course performance if selected for a second summer. Survey data suggest that the Boston SYEP affects academic outcomes by increasing aspirations to attend college, gaining basic work habits, and improving social skills.

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1. Introduction

Over the past several decades, many urban high schools have experienced little or no improvement in closing the academic achievement gap that exists along socioeconomic and racial lines (Musu-Gillette et. al. 2017; Duncan and Murnane 2011; Ladd 2012). As of 2016, 76.4 percent of Black students and 79.3 percent of Hispanic/Latino students graduated compared to 88.3 percent of white students (U.S. Department of Education, 2016). In 2014, only 77.6 percent of low-income students graduated on time compared to 90 percent of non-low-income students (DePaoli et. al 2018). Moreover, both the higher labor market returns to completing a high school degree (Jaeger and Page 1996; Cameron and Heckman 1993) as well as the greater likelihood of poverty, diminished health, and involvement in the criminal justice system associated with dropping out of high school (Bjerk 2012) have been well-documented.

In addition to the many in-school interventions that have been implemented over the past several decades, policymakers and researchers have recently examined how time spent outside of the classroom can affect student outcomes, including high school graduation. Using largely quasi-experimental methods, prior studies have shown that participating in sports boosts graduation rates (Stevenson 2010) and overall participation in extracurricular activities can reduce dropout rates by up 18 percentage points (Crispin 2017).

This paper provides experimental evidence regarding the impact of another type of out-of-school activity—work experience—on high school academic outcomes. Specifically, we study the outcomes of randomized admissions lotteries for the 2015 cohort of students who applied to the Boston Summer Youth Employment Program (SYEP). We match these records to administrative school records to estimate the effect of being selected to participate in the SYEP on high school graduation and dropout rates. We find that lottery winners are 2.6 percentage points (24.8 percent) less likely to drop out of high school relative to the control group. In addition, youth in the treatment group were 6.1 percentage points more likely to graduate from high school on time. Given that high school graduates have better outcomes than dropouts along a number of dimensions, including being more likely to be employed and earn a higher taxable income (Child Trends 2017) as well as being less likely to engage in criminal behavior or require social services (Lochner and Moretti 2001), a back of the envelope calculation suggests that the long-term benefits of the Boston SYEP outweigh the costs by a factor of 4-to-1.

We also examine short-term behavioral changes associated with participating in the program to better understand how these impacts are achieved and for whom the benefits are the greatest. During the school year after participation, youth who were randomly selected into the SYEP treatment group experienced significant improvements in attendance rates of 1.9 percentage points, in part due to reducing their unexcused absences by 1.1 days. Moreover, youth in the treatment group were 7.8 percentage points more likely to achieve an attendance rate of 90 percent or better, reducing chronic absenteeism by 27 percent relative to baseline. Larger improvements are found for youth with initially low attendance rates and youth age 16 and older who are able to legally drop out. We also find small, but significant, impacts on overall GPA in the year after participation, but no meaningful improvements in standardized test-taking or scores. Linking the academic records to self-reported survey data on short-term program impacts, we show that these outcomes are correlated with increasing aspirations to attend college, gaining basic work habits, and improving social skills over the course of the summer.

This paper makes three key contributions to the literature. First, although prior literature on SYEP has found strong positive impacts for reducing crime (Heller 2014; Gelber, Isen, and Kessler 2014; Modestino 2019), the evidence on improving academic outcomes is more mixed.

For example, Leos-Urbel (2014) finds significant increases of one to two percent in school attendance for the treatment group relative to the control group during the year following participation in the New York City (NYC) SYEP, with larger improvements for students aged 16 years and older with prior low baseline attendance. Schwartz, Leos-Urbel, and Wiswall (2015) find small, but significant, increases in the share of NYC SYEP participants taking and passing statewide high school exams relative to the control group. However, other research indicates that the NYC SYCP did not have a positive effect on longer-term academic outcomes, such as graduating from high school (Valentine et al. 2017) or college enrollment (Gelber, Isen, and Kessler 2016).

Second, while the results of the SYEP literature have 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. We build on this literature by linking survey data on changes in self-reported behaviors over the summer to administrative records on subsequent secondary school outcomes to shed light on what works for whom, under what conditions, and why.

Third, prior studies of year-round workforce development programs aimed at youth have often shown negative impacts that when students work too many hours, the likelihood of high school graduation and college attendance decreases (Mortimer 2010; Stasz and Brewer 1999). Instead, the association between hours of work and school performance follows an inverted-U 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). Yet, summer jobs programs differ from year-round programs in several important ways. First, SYEPs occur in summer months when youth are often idle, creating fewer conflicts with their academic studies compared to year-round employment programs. Second, SYEPs may help ameliorate summer learning loss among low-income and at-risk youth when school is out of session by providing the opportunity to practice existing skills or learn new skills on the job (Alexander, Olson, and Entwisle 2007; Cooper et al. 1996). Third, the Boston SYEP incorporates several features—such as a formal career readiness curriculum, greater exposure to private sector employers, and job-skill ladders across summers—that are designed to specifically address skill deficits arising from a lack of opportunities among at-risk youth.

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 the estimates of the program's impact on both the longer-term secondary school outcomes as well as the short-term behavioral changes in skills and attitudes and analyzes the relationship between the two. Finally, Section 5 concludes with a discussion of the policy implications and future research.

2. The Boston SYEP Intervention

The Boston SYEP was introduced in the early 1980s and currently 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. Participants are placed in either a subsidized position (e.g., with a local nonprofit, community-based organization, or city agency) or a job with a private-sector employer and are paid the Massachusetts minimum wage. The program operates for a six-week period starting in early July through mid-August Youth during which youth work a maximum of 25 hours per week and receive 20 hours of job-readiness training that 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 intermediaries under contract with the Boston Mayor's Office of Workforce Development (OWD) and most typically apply to the intermediary in their immediate neighborhood.[1] The intermediaries are responsible for reviewing applications, matching applicants with jobs, supervising placements, and delivering the program's career-readiness curriculum.

How Might SYEPs Improve Academic Outcomes?

Understanding the mechanisms by which SYEPs can lead to better school outcomes down the road can help inform policymakers and practitioners about the types of interventions that might be successful at reducing dropout and raising high school graduation rates. Recently, chronic absenteeism—attending less than 90 percent of the school year—has been highlighted as a serious challenge for policies aimed at improving academic performance among low-income and at-risk youth (Ready 2010, U.S. Department of Education 2016). In high poverty areas, as many as one third of all high-school students are chronically absent (Balfanz and Byrnes 2012, Sheldon and Epstein 2004) with greater rates of absenteeism among non-white students (U.S. DOE 2016). Chronic absenteeism has been linked to poor outcomes including inability to read at grade level and increased risk of drop-out (Mac Iver and Mac Iver 2010, Utah Education Policy Center 2012).

Below we describe four primary channels through which SYEPs have the potential to improve chronic absenteeism, and subsequently improve course performance, reduce dropout, and increase the likelihood of high school graduation. In addition to helping administrators

[1] Administrative data provided by the City of Boston shows that only 6.8 percent of youth apply to more than one agency. Although no individual receives more than one offer of employment., roughly 3.0 percent of the control group obtained a job through one of the three other summer job intermediaries.

improve existing summer jobs programs, these insights may also enable cities to maximize resource allocation by targeting specific groups.

(1) *Improving behaviors correlated with school success.* Some SYEPs, including the Boston program, offer programming aimed at improving non-cognitive skills such as responsibility, positive work habits, motivation, time management, determination, self-confidence, and “grit”—attributes that have been shown to be important for adult success (Heckman 2008, Duckworth et al. 2007) and have the potential to boost attendance and reduce the likelihood of dropout (Jackson 2012). In addition, the types of early work experience provided by SYEPs gives participants the opportunity to by providing the opportunity to practice existing skills or learn new skills on the job (Alexander, Olson, and Entwisle 2007; Cooper et al. 1996), possibly raising subsequent course performance.

(2) *Increasing career and academic aspirations.* One of the stated objectives of the Boston SYEP is to provide youth with meaningful employment experiences that can lead to alternative pathways—whether it be to obtain career training or attend college (Boston Mayor’s Office of Workforce Development 2018). In addition to providing meaningful work experiences, the Boston SYEP also aims to develop the skills needed to access these pathways through its career readiness curriculum which focuses on exploring careers, writing a resume and cover letter, searching for jobs, and interviewing. These program objectives are based on the observation that greater exposure to employment provides 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).

(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. As such, SYEPs may limit opportunities for youth to engage in delinquent activity or disrupt risky behaviors that may occur due to a lack of supervision or guardianship (Cohen and Felson 1979; Heller 2014; Modestino 2019). 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 such as suspension (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.[2] According to our survey data, roughly half of youth participating in the Boston SYEP indicate that they help pay one or more household bills and one in five report that they are saving for college tuition.

3. Experimental Design, Data, and Empirical Methodology

Experimental Design

Previous studies of early work experience have been skeptical of empirical findings, citing positive selection into employment based on the preexisting characteristics of teens who work versus those who do not (Hotz et al. 2002; Bacolod and Hotz 2006). To address this potential bias, 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

[2] 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.

established nonprofit that works in all of Boston's 18 neighborhoods. 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.[3] ABCD uses a computerized system with a 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, creating a control group of youth who apply to the SYEP but are not chosen.

Table 1 provides descriptive statistics for the preexisting characteristics of SYEP lottery applicants collected by ABCD. Of the 4,235 youth who applied to ABCD in 2015, a total of 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. The sample means show that ABCD serves a predominately young, school-aged, and low-income population.[4] Applicants were just under 16 years of age and slightly more likely to be female as well as African American. Approximately 88 percent of applicants were in school at the time they applied and roughly 7 percent identified as having limited English ability. In addition, nearly 7 percent reported being homeless and upwards of 18 percent acknowledged receiving cash public assistance of some form.[5] Comparing

[3] 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. In addition, DYEE chose not to implement the survey during the summer of 2015 so it is not possible to test program mechanisms using their data.

[4] Table A2 in the online appendix shows that ABCD draws applicants from all 18 Boston neighborhoods with greater representation among those with higher shares of youth age 0-17 (see Figure A2). 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 A3).

[5] 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.

these observable characteristics across youth who were selected by the lottery versus not confirms that the lottery is 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. The sample is similarly balanced among the school-aged population.[6]

It is also important to test whether the Boston SYEP delivered a meaningful intervention. Although Boston's overall unemployment rate of 4.4 percent as of July 2015 would suggest a relatively tight labor market, the labor market for youth was still quite depressed. 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. In addition, Figure 1 provides descriptive information about the summer employment experiences among individuals responding to an end-of-summer survey of both the treatment group and control groups. Survey respondents in the control group who found a job worked fewer hours per week than SYEP participants (see panel A), yet participants had less variation in the type of daily work they performed with over half of SYEP participants working at a day care or day camp (see panel B). Yet, SYEP participants were more likely than their counterparts in the control group to report that they would consider a career in the type of work they did, had an adult they considered a mentor and who they could use as a reference in the future, and felt better prepared to enter a new job (see panel C).

Data and Empirical Methodology

The first phase of the analysis uses administrative data during the one to two school years following the intervention (2015-16 and 2016-17) to assess SYEP impacts on longer-term secondary school outcomes. The second phase of the analysis uses survey data on self-reported

[6] We test for balance using separate models estimating the effect of winning the lottery on preexisting applicant characteristics among school-aged youth for gender/race groupings (see Table A1).

behavioral changes in skills and attitudes that occur during the summer to provide insight into program mechanisms that may have enabled participating youth to increase their attendance and/or academic performance.

Using Administrative Data to Assess SYEP Impacts on School Outcomes

Data for the first phase of the analysis come from school records obtained from the Massachusetts Department of Elementary and Secondary Education (DESE), which provide information on all students within the state of Massachusetts, including both private and public schools. This rich data source contains information on secondary school outcomes including attendance, course grades, statewide test scores, dropouts, and high school graduation. The benefit of using administrative data is that one avoids the problems of self-reported data such as social desirability bias, which might be large if individuals in the treatment group feel compelled to embellish their school performance when applying for a summer job.

The drawback to administrative data is that individuals must be matched across two different record keeping systems, often resulting 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 original sample, 79.6 percent were in school and 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 match rate than that of previous summer jobs studies, likely due to having state-level records that capture youth in both regular public as well as charter schools, even if they switch schools within the state.[7] Even though the lottery has been confirmed to be random and the 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 following participation in the program. Of the students in grades 8 to 11 in the school year prior to SYEP, 90.4 percent of those selected by the lottery were enrolled in the following school year compared to 91.1 percent of those not selected.[8] To more rigorously test for selective attrition, Table A5 in the appendix presents estimates of the effect of winning the lottery on the same preexisting demographic characteristics as before, confirming that selective attrition is not a problem.[9] To further test for validity and balance, we also estimate the effect of the lottery indicator on individual baseline outcomes, where possible, and find no significant pre-existing differences between youth in the treatment versus control groups.[10]

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 (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 who want to account for

[7] Leos-Urbel (2014) reports a 77 percent match rate for applicants to 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.
[8] These attrition rates are similar to those of prior studies such as Leos-Urbel (2014) which reports that 93.5 percent of those selected by the NYC lottery were enrolled in the following school year compared to 93.4 percent of those not selected. See Table A.4 in the online appendix for these tabulations.
[9] The SYEP indicator does not significantly predict any individual characteristics—with the exception of the one characteristic (e.g. Asian) that was noted in the earlier balance test for the full sample. We also find no evidence of attrition by grade level (see Table A6).
[10] Note that it is not possible to test for baseline outcomes for taking the MCAS or for high school graduation. See Table A7 in the appendix for these comparisons.

take-up among the applicants, rather than just assessing outcomes for those who also choose to participate. Nonetheless, because not all youth accept the offer, the ITT estimate will understate the effects of the program for those youth who choose to participate. As such, we also provide treatment-on-the-treated (TOT) estimates using a two-stage-least-squares method.

We measure multiple outcomes of interest during the post-intervention period within each domain: attendance, course performance, standardized test taking and scores, dropout, and high school graduation. The construction of these variables is described in detail in the online appendix. Note that 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 improve the precision of our estimates:

$$Y_{it} = SYEP_i\, \pi_1 + X_{i(t-1)}\, \beta_1 + s + \mu_{it1} \qquad (1)$$

where Y_{it} is the school outcome, $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, $X_{i(t-1)}$ is a set of pre-existing demographic characteristics, academic characteristics, and baseline school outcomes[11], s is a vector of school fixed effects to control for the influence of time-invariant school characteristics on educational outcomes, and μ_{it1} 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.[12]

Additionally, we are interested in exploring whether SYEP impacts fade over time as

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

[12] 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. Marginal effects are reported in all tables when using these nonlinear estimation methods.

well as if additional summers (e.g., increased "dosage") enhances outcomes. Given that 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 explore these questions, we make use of an additional year of DESE data for the 2016-17 school year that provides information on school outcomes for the second academic year after participating for the summer 2015 cohort. We then use administrative program data from OWD to identify youth who applied and won the lottery during the summer of 2016 to construct indicators for whether youth had received only one summer (SYEP1) or two summers (SYEP2) of the intervention.[13] About one-quarter (26.8 percent) of youth in the original treatment group applied and were selected by lottery for a second summer, yielding enough variation to assess the importance of both dosage and fade out. To estimate separate impacts by number of summers of treatment, we use equation (2):

$$Y_{it} = SYEP1_i \pi_{10} + SYEP2_i \pi_{11} + X_{i(t-1)} \beta_1 + s + \mu_{it1} \qquad (2)$$

Note that there are some limitations to this analysis. For example, having won the lottery in the first year is likely to increase the likelihood of applying for a second time and the opposite is likely to be true for those who did not win the lottery the first time. Indeed, only 3.7 percent of those in the control group apply and are selected into the program during the summer of 2016. As such, our estimates of the impact of a second summer of treatment (π_{11}) primarily reflect the impact of the program conditional on having won the lottery the first time. Nonetheless, we believe it is still informative to explore program impacts two years post treatment and assess how much can be explained by the number of summers (e.g., dosage).

Finally, although one might question whether a six-week intervention can provide a

[13] Note that youth who participated for only one summer includes both members of the original treatment group who only participated in summer 2015 as well as members of the control group who participated in summer 2016.

meaningful turning point to affect youth development, such impacts may be greater for at-risk youth (Sampson and Laub 2003). As one researcher concluded, "Having a positive work experience can help to turn you around. For those who have a lot of disadvantages, any positive experience is likely to have a greater impact than on people with a lot of advantages already" (Mortimer 2010, p. 8-11). 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 impact based on a greater likelihood of chronic absenteeism—specifically among older youth, males, those 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).

Using Survey Data to Explore SYEP Program Mechanisms

To explore program mechanisms, we link the secondary school outcomes described above to the short-term behavioral changes in skills and attitudes observed during the summer for the treatment group, as measured by a pre-/post-program survey. Whereas the first part of the analysis using administrative data establishes the causal impacts of the Boston SYEP on school outcomes, the goal here is to provide a glimpse into *how* the program achieves these outcomes. Because we rely on self-reported survey data to assess the short-term behavioral changes in skills and attitudes, this second part of the analysis should be regarded as more exploratory in nature.

Assessing Short-Term Behavioral Impacts

To explore how the Boston SYEP affects youth behavior over the course of the summer, ideally one would want to compare the change over time in the pre/post-program survey results for the treatment versus the control group. However, while the survey was administered to participants at both the beginning and the end of the summer to assess changes over time, program administrators chose to administer the survey to the control group only at the end of the summer to provide a point of comparison. Therefore, we measure program impacts as those outcomes where there was a significant improvement among participants over the summer as well as a significant difference relative to the control group at the end of the summer.

There are several potential sources of bias arising from this analysis. First, it might be the case that the individuals in the treatment group who responded to the survey differ from those who did not. Fortunately, the high response rate among the treatment group (66.9 percent, N=663) was sufficient such that there were no significant differences in observable characteristics for the full treatment group versus those responding to both the pre- and post-survey.[14] Thus, short-term behavioral changes in skills and attitudes measured over the course of the summer for the treatment group are likely to be unbiased.

A second source of bias could arise from the differential response rates of the treatment and control groups. Indeed, while the number of respondents in the control group was similar (N=664), this represented a response rate of only 21.8 percent. Because the two groups were randomly selected, we can use the observable characteristics to determine the direction of bias. Relative to the treatment group, respondents from the control group were more likely to be older, female, identify as white or Asian, and indicate that they live in a two-parent household.[15] We argue that the selection bias goes *against* finding a positive impact for the Boston SYEP, given that the survey respondents in the control group exhibit characteristics that are on average associated with *better* outcomes.[16] To minimize the possibility of selection bias due to survey

[14] Table A8 in the online appendix compares the characteristics of the full treatment group to those participants who responded to the survey.
[15] Table A9 in the online appendix compares the characteristics of the survey respondents across the treatment and control groups.
[16] In terms of academic outcomes, females are more likely than males to graduate high school and attend college (Autor and Wasserman 2013, Hugo-Lopez and Gonzalez-Barrera, 2014). In addition, standardized test scores are lower among African-American children and those living in single parent households (Jencks and Phillips, 1998).

response rates, we will control for observable characteristics using equation (3):

$$M_{it} = SYEP_{it}\,\pi_2 + X_{it}\,\beta_2 + \mu_{it2} \qquad (3)$$

where M_{it} is one of the short-term program outcomes (e.g., work habits), $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, and X_{it} is a set of demographic characteristics. Because the selection among survey respondents in the control group is correlated with better outcomes, the coefficient π_2 is likely to provide downward-biased estimates of the program's impact on short-term behavioral outcomes.[17]

Linking Short-Term Behavioral Impacts to Academic Outcomes

Ideally, a full mediation analysis would be used to generate evidence for how the Boston SYEP program improves academic outcomes (Keele et. al 2015). However, because the post-survey was administered to the control group anonymously rather than confidentially, as was done for the treatment group, we can only link the survey responses to the school record data for youth in the treatment group who responded to the survey, ruling out a full mediation analysis. Nevertheless, it is still possible to explore whether improvements in the short-term behavioral impacts on skills and attitudes are correlated with better school outcomes to shed light on the program's mechanisms. To do this, we modify equation (1) as follows:

$$Y_{it} = SYEP_i\,\pi_3 + X_{i(t-1)}\,\beta_3 + s + \Delta M_i\,\delta + \mu_{it3} \qquad (4)$$

On the left-hand side, the dependent variable is one of the longer-term school outcomes (e.g., attendance rate) while on the right-hand side is a dummy indicating positive improvement

Higher employment rates are observed among females, whites, and older youth (Child Trends, 2017). Age, male gender, and living in a single-parent home have been shown to be significant predictors of re-offending among youth (Cottle et. al., 2001).

[17] We also recognize that self-reported data is subject to measurement error arising from social desirability bias and item non-response (Meyer, Mok, and Sullivan 2015). However, if we assume that measurement error is random across the treatment and control groups, this would reduce efficiency but not cause bias. Indeed, the item non-response rate for the survey questions used in the analysis was less than 5 percent for both the treatment and control groups (see Table A10).

for a specific short-term behavioral impact ΔM_i (e.g., being on-time). A positive and significant coefficient on ΔM_i indicates that improvement in the short-term behavioral impact observed during the summer of participation is positively correlated with the subsequent improvement in school outcomes, such as attendance.[18]

Note that the mediator analysis implicitly assumes that there was no change in the short-term behavioral measures for youth in the control group. We argue that this assumption is plausible if the analysis is restricted to those short-term program impacts for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group at the end of the summer. Moreover, there is abundant evidence that youth typically lose academic and social skills and experience a decrease in college aspirations over the summer, and this tendency is particularly acute among disadvantaged groups (Cooper et al. 1996; Panayiotou et al. 2017; Castleman and Page 2014).

4. Results

Assessing SYEP Impacts on Academic Outcomes Using Administrative Data

High School Dropout and Graduation

While improving attendance rates and course performance are worthy goals in and of themselves, the primary interest in improving school outcomes is to prevent dropout and increase the likelihood of high school graduation. Table 2 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.[19]

[18] Given that this approach could also be driven by unobservable characteristics such as youth motivation (e.g. as reflected in their willingness to answer the survey), we also test whether these same relationships hold when the sample is restricted to participants completing both the pre- and post-survey.
[19] Note that the sample size differs by outcome depending on the time horizon (e.g., one year post or ever) and whether youth would be eligible to drop (e.g., given their age) or graduate (e.g., given their grade). Also note that at this point we do not have sufficient sample size to assess the impact of multiple summers of participation on dropout

The first column of Panel A shows the raw difference with no controls and indicates that the dropout rate in the year following the summer jobs program was 1.5 percentage points lower for students in the treatment versus the control group—a 25 percent improvement. Adding in individual controls for demographic and academic characteristics improves the precision but has little impact on the estimate. The inclusion of school fixed effects reduces the magnitude of the coefficient somewhat, perhaps reflecting different attendance policies or cultures across schools. Yet controlling for baseline outcomes seems to counteract those effects. With the inclusion of all controls we find that dropout rates improved by 1.4 percentage points during the year after winning the lottery and by 2.8 percentage points during the remainder of one's high school career. Correspondingly, being selected into the Boston SYEP raises the likelihood of graduating from high school on time by 5.8 percentage points and of graduating at any point after participating in the program by 6.1 percentage points. To our knowledge, this is the first study to document an improvement in high school dropout and graduation rates associated with an SYEP.[20] In the following sections we determine whether these improvements are driven by better attendance, higher grades, or greater likelihood of passing standard tests for graduation.

Attendance

In terms of attendance, we find that the Boston SYEP had strong positive impacts across all of our measures, including chronic absenteeism, during the first year after participation. Table 3 reports the ITT estimates of the difference between the treatment group and the control group from equation (1) on several attendance outcomes. With the inclusion of all controls we find that

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and high school graduation. However, future work may involve studying multiple cohorts which would provide a larger number of students to observe across multiple states of participation for these outcomes.

[20] Indeed, no such impacts have been found for either the Chicago New York City program, perhaps because the Boston program serves a younger population who is less likely to engage in criminal activity. In addition, the Boston SYEP also has a relatively high share of private sector jobs and a greater focus on career readiness.

attendance rates improved by 1.9 percentage points or 3 school days during the year after participation and 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 have a meaningful impact on chronic absenteeism with the treatment group being 7.8 percentage points more likely than the control group to have attended at least 90 percent of the school year after winning the SYEP lottery—a 27.2 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).[21] 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 operate as a preventive intervention for chronic attendance among school-aged youth.

Indeed, the relative improvement in attendance among the treatment group did not simply reflect fewer days out due to illness or other excused absences, but also a reduction in truancy, suggesting a behavioral shift in the propensity to attend school.[22] Average days attended increased by 3.1 days among the treatment group compared to the control group and this was partly driven by a reduction of 1.2 days of unexcused absence (a 10 percent decrease). This is on par with other interventions aimed at addressing chronic absenteeism, such as notifying parents

[21] A recent RCT of the Early Warning Intervention and Monitoring System (EWIMS) indicate 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.

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

of absences via postcard (10 percent) or text messaging (17 percent).[23]

Looking at the two-year impacts suggests that the program's effect on attendance tended to fade out over time without a second dose of SYEP. Although all of the coefficients reflect continued improvements into the second year, they are by and large not statistically significant for youth who only won the lottery for one summer. In contrast, youth that applied and were randomly selected to participate for a second summer appear to maintain the 1.9 percent improvement in their attendance rate from the first year, due to an additional 4.7 days attended, including a significant reduction of 2.8 days of unexcused absences.

Course Performance

In terms of course performance, we find that the program had a small impact on overall GPA and course failures in year one that grow over time with a second year of participation. In terms of the one-year outcomes, Table 4 shows that when controlling for all individual and school factors, the treatment group had overall GPAs that were 0.08 points higher than the control group, although the impact was only marginally significant. Similarly, we find a small reduction in the likelihood of failing a course during the first year after participation but it is not statistically significant, except when controlling for school fixed effects.

In contrast, the second year impacts on course performance are larger in magnitude and significance—but only for youth who applied and won the lottery for a second summer. Table 4 indicates that the overall GPA of the treatment group was 0.12 points higher (an improvement of 6.1 percent from baseline) and the likelihood of failing a course was reduced by 6.1 percentage

[23] Rogers and Feller (2014) randomly assign parents of high-risk, K-12 students to receiving received one of three yearlong 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.

points. More striking was the 10.2 percentage point reduction in the likelihood of failing an ELA course during the second year. Overall, these results suggest that the impact of the program on academic performance is less immediate than that of attendance and may accumulate over time with continued participation in the program. However, we need to be careful in attributing a causal interpretation to the second-year results for the repeat participants given that youth need to have applied for a second time, possibly indicating greater intrinsic motivation or ability.

Standardized Testing

We also explore whether participating in the Boston SYEP had a measurable impact on student performance on the Massachusetts Comprehensive Assessment System (MCAS), a statewide standardized test. Students must receive a passing grade on both the mathematics and ELA tests to receive a high school diploma.[24] Similar to Leos-Urbel (2014), we find no impact on performance in terms of improving scores or raising the likelihood of proficiency (see Table 5). In contrast, Schwartz et al. (2014) find a small, marginally significant increase in passing any New York State Regents exam, as well as in the number of exams passed, for SYEP lottery winners in New York City. These two prior studies also found an increase in the likelihood of taking standardized tests. Yet, we find little increase in the likelihood of taking the MCAS, possibly because—unlike the Regents exams—the MCAS is a mandatory requirement for high school graduation. Nonetheless, we do find a small increase of 3.6 percentage points in the likelihood of taking the ELA MCAS on time, but the effect is only marginally significant.[25]

Heterogeneity in Outcomes by Subgroup

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[24] Note that because students take the MCAS in the 10th grade, we must observe participants as 9th 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.

[25] Low-performing students may defer taking one or more of the MCAS tests to their junior year to increase the likelihood that they will be able achieve a passing score.

As prior research has shown, it could be the case that the impact of the Boston SYEP on school outcomes is greater for more marginal students (Leos-Urbel 2014). As such, it is natural to ask whether SYEPs might have a disproportionate effect on subgroups. For example, prior research indicates that chronic absenteeism is 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 6 reports the ITT estimate of the differential program impact on the improvement in academic outcomes for the subgroups described above as well as for "marginal" students—defined as those having either chronically high absenteeism or low GPAs (depending on the outcome of interest) during the baseline pre-period (e.g., the 2014-15 school year).[26] Among attendance outcomes, the Boston SYEP had a greater impact on students with prior chronic absenteeism as well as youth of legal drop-out age (e.g., 16 years or older)—both groups experience an additional 4 percentage point boost to their attendance rates compared to the average student in the treatment group. In terms of course performance, the program appears to have a disproportionate impact on improving overall GPA and reducing course failures among marginal students, youth of legal dropout age, and those with limited English ability. The latter finding is consistent with prior research that shows learning English is more effective in a contextualized setting, such as on the job (Burt and Mathews-Aydinli 2007). We found no differential impacts of the Boston SYEP on dropout or high school graduation for any of our

[26] For attendance, dropout, and graduation outcomes, marginal youth are defined as those who previously had an attendance rate below 90 percent in the year prior to SYEP participation (e.g., 2014-15 academic year). For course performance outcomes, marginal students are defined as those previously having an overall GPA that was below a C average in the year prior to SYEP participation.

subgroups, suggesting that this aspect of the program is more universal in nature.

Exploring SYEP Program Mechanisms Using Survey Data

What might be driving the improvements in chronic absenteeism, dropout, and graduation? It could be the case that participating in the SYEP improves behaviors that are correlated with 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 the case that the program's career readiness curriculum, coupled with real-world experience, boosts career and academic aspirations that lead to greater motivation or effort in school during the following year. Finally, prior research has shown that SYEP reduce the propensity to engage in delinquent behavior, including truancy, that would be disruptive to learning. We explore these mechanisms further in the next two sections by assessing the degree to which SYEP participants learn new skills over the summer and how these changes are correlated with improvements in attendance after participating in the program.

Assessing Short-Term Behavioral Impacts

The self-reported survey data indicate that 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. Table 7 shows the change over time for the pre-/post-program survey responses of the treatment group as well as the difference between the post-program responses for the treatment versus the control group, estimated using equation (3). Recall that we measure program impacts as those outcomes where there was a significant improvement among participants over the summer as well as a significant difference relative to the control group at the end of the summer. For example, panel A shows that the share of participants reporting that they plan to attend a four-year college or university increased significantly by nearly 5 percentage points during the summer and was 11 percentage points higher than the share of the control group reporting similar academic aspirations at the end of the summer. Coincidentally, the share of SYEP participants who reported saving for college also increased by 5 percentage points and was significantly higher than that of the control group at the end of the summer. In contrast, although the share of participants reporting that they wanted to work in the fall increased by 7 percentage points, this measure was below that reported by the control group at the end of the summer.[27]

SYEP participants also indicated sizeable growth in job readiness skills during the summer, many of which were significantly greater those reported by the control group (see panel B of Table 7). This included large increases in the share of participants reporting that they had prepared a resume and a cover letter, practiced interviewing skills with an adult, and developed answers to typical interview questions. Perhaps more directly relevant to our earlier findings regarding school attendance is the significant increase in the share of participants who reported knowing "how to be on time" and "how to organize my work and keep to my schedule."

Finally, panel C of Table 7 indicates that participants' attitudes toward their communities improved greatly (by 15 percentage points), and these outcomes were significantly better than those reported by the control group at the end of the summer. Given that most SYEP job placements are with community-based organizations in the participants' neighborhoods, it could be that the program provides youth with an opportunity for more positive social engagement within their communities. Although smaller in magnitude, participants also showed significant

[27] If we think that youth might substitute working in the summer for time spent working in the fall, then this finding would be consistent with youth in the treatment group having a higher propensity to do so relative to the control group, most of whom did not work during the summer of 2015.

improvements in social skills and behaviors that have been shown to be correlated with academic success—such as managing emotions, asking for help, and resolving conflict with a peer—measures that were also significantly higher relative to the control group by the end of the summer. These improvements might reflect additional soft-skills development stemming from the program's career readiness curriculum and practiced on the job throughout the summer.

Evaluation of Program Mechanisms

Although participants demonstrated significant gains in a variety of short-term behaviors and skills according to the survey data, only some of those changes appear to be correlated with subsequent improvements in school outcomes. Table 8 reports the results of the mediation analysis specified in equation (4) that estimates the program's impact on school outcomes, while separately controlling for improvements in each of the short-term behavioral skills and attitudes (ΔMi) as well as the full set of controls. For example, Panel A reports the impact of measures related to academic aspirations and reveals that youth who reported that they had started to save for college over the summer experienced greater improvements in attendance and graduating on time. This suggests that the program not only operates through raising academic aspirations but also by providing youth with the knowledge and/or resources to act on those aspirations.

Panel B of Table 8 shows that improvements in work habits such as being on time and organizing one's work / keeping to a schedule were found to have positive impacts across all of our attendance measures, suggesting that the old adage that "80 percent of success is just showing up" might in fact be true.[28] Interestingly, improvements in almost all of the job readiness skills were significantly correlated only with reductions in unexcused absences. Again, this is suggestive of a behavioral shift as absences related to truancy are more likely to reflect

[28] In 1989 Woody Allen was asked about this saying by William Safire, the language columnist for the New York Times, and Allen replied with a letter in which he asserted: "I did say that 80 percent of success is showing up."

choices made by youth, rather than other absences that might be related to illness.

Finally, improvements in social skills—such as managing emotions and asking for help—were almost universally correlated with increasing the likelihood of graduating from high school on time (see panel C of Table 8). In addition, gaining a mentor appears to have an impact on both high school graduation as well as reducing the truancy. These findings are consistent with prior research on summer jobs programs that has linked improvements in social skills to reductions in a wide range of delinquent and criminal behavior among youth (Heller 2014; Modestino 2019).

Although these findings are only suggestive, the results presented here regarding the program's behavioral mechanisms are consistent with prior research on the effects of work-based learning programs in high schools. These programs link classroom instruction to workplace skills through placements in internships, mentoring, workplace simulations, and apprenticeships. Students in work-based learning programs have been shown to have higher attendance and graduation rates than those not enrolled in such programs (Colley and Jamison, 1998). Yet we note that our mediation 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 reducing dropout and increasing graduation rates among youth. Being randomly selected into the Boston SYEP reduces the likelihood of dropout by 2.6 percentage points—or 24.8 percent—relative to the control group. Prior studies have documented that high school graduates have better outcomes than dropouts along a number of dimensions including higher employment rates and incomes (Child Trends 2017) as well as lower rates of criminal activity and take-up of social services (Lochner and Moretti 2001). By some estimates, each new high school graduate confers a net benefit to taxpayers of about $127,000 over the graduate's lifetime.[29] According to the City of Boston, the SYEP costs roughly $2,000 per participant, resulting in a total cost of $2.4 million for the 1,200 youth that participated through ABCD during the summer of 2015.[30] Given that the program appears to increase the likelihood of high school graduation by 6 percentage points, this would yield an additional 72 graduates, who on net would collectively confer a benefit of $9.1 million over their lifetimes, resulting in a benefit-to-cost ratio of 4-to-1.

These improvements in high school dropout and graduation appear to be driven by better attendance among students in the year after being selected for the program, and somewhat better course performance if they applied and were selected for a second summer. Additional work is needed to more precisely estimate the minimum "dosage" (e.g., number of summers) needed to achieve meaningful impacts. This is a priority for currently oversubscribed programs, such as Boston, where participation is assigned by lottery. Currently, about one-third of the Boston SYEP 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 it is indeed the case that multiple summers are needed to obtain lasting impacts.

However, it is not clear how the Boston SYEP compares with other interventions that have been shown to improve attendance but do not involve the program administration costs of soliciting commitments from employers, matching teens to jobs at the start of each summer, and supervising youth at multiple job sites. For example, other studies have found that lower-cost interventions, such as notifying parents of absences via postcard (10 percent) or text messaging

[29] 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

[30] 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.

(-17 percent), 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 also outweigh the program's costs. For example, SYEPs confer job experience, which may yield additional advantages in terms of future employment, career pathways, or post-secondary education. With just under one-third of U.S. teens aged 16 to 19 years currently working, youth employment rates remain just shy of their prerecession levels and are far below the 40 percent threshold that prevailed up until the 2000-01 recession, and are even lower among nonwhite teens from low-income families living in high-poverty neighborhoods (Sum et al., 2014). More than half of unemployed teens report that they are looking for their first job, suggesting that there may be fewer pathways for teens to enter the labor market (Dennett & Modestino, 2013). In addition, SYEPs help families at or near the poverty line by providing income to youth—with upwards of one in five youth contributing directly to their household's expenses, according to our survey data—potentially increasing household resources that can affect a wide range of youth outcomes.

Finally, by linking the academic records to self-reported survey data on short-term changes in behaviors and skills, we are able to shed light on how the program achieves these better outcomes among the youth being served. Our mediation analysis reveals 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" as to how SYEPs affect youth outcomes in the long run.

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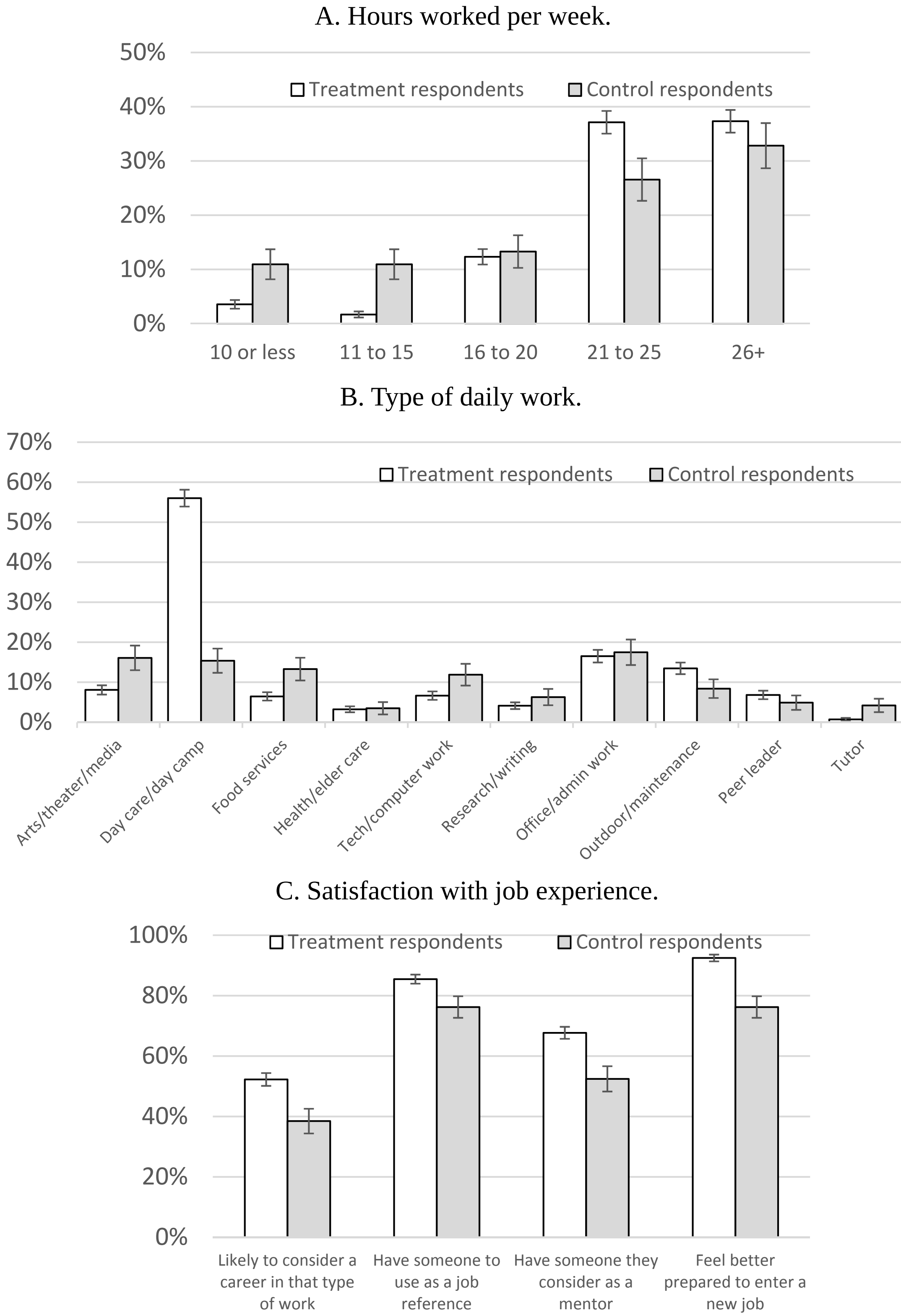
Figure 1. Summer Experiences for SYEP Survey Respondents by Lottery Outcome.



Notes: This figure displays descriptive information about the self-reported summer employment experiences among individuals responding to an end-of-summer survey of both the treatment group and control groups to assess whether the Boston SYEP provides a meaningful intervention. Individuals in the treatment group work more hours per week, are more likely to work in day cares and day camps, and are more satisfied with their job experience.

Source: Survey data provided by the City of Boston, Office of Workforce Development.

Table 1. Lottery Summary Statistics and Randomization Check

	Selected (treatments)		Not Selected (controls)		Treatment-Control	
	Mean	Std. Error	Mean	Std. Error	Difference	*p* -value
Age	15.917	(0.058)	15.845	(0.033)	0.073	(0.258)
Percentage 14-17 years	0.794	(0.008)	0.802	(0.007)	-0.008	(0.292)
Percentage female	0.531	(0.014)	0.539	(0.009)	-0.008	(0.640)
Percentage in school	0.876	(0.010)	0.884	(0.006)	-0.008	(0.497)
Percentage African American	0.513	(0.015)	0.540	(0.009)	-0.027	(0.197)
Percentage Asian	0.065	(0.007)	0.050	(0.004)	0.015	(0.088)
Percentage White	0.096	(0.009)	0.084	(0.005)	0.012	(0.211)
Percentage other/two or more races	0.325	(0.014)	0.326	(0.009)	0.000	(0.983)
Percentage Chinese	0.002	(0.001)	0.001	(0.001)	0.001	(0.557)
Percentage English	0.951	(0.006)	0.955	(0.004)	-0.004	(0.620)
Percentage Spanish	0.033	(0.005)	0.027	(0.003)	0.006	(0.287)
Percentage other language	0.014	(0.003)	0.018	(0.002)	-0.003	(0.465)
Percentage limited English ability	0.071	(0.007)	0.071	(0.005)	0.000	(0.969)
Percentage homeless	0.067	(0.007)	0.069	(0.005)	-0.002	(0.822)
Percentage receiving public assistance	0.187	(0.011)	0.172	(0.007)	0.015	(0.240)
Percentage disabled	0.040	(0.006)	0.033	(0.003)	0.007	(0.276)
Number of youth	1,186		3,049		-1,863	

Notes: The table shows that the treatment variable is uncorrelated with the individual's background variables. Each line of the table provides the mean of the the background variable listed in the first column for the treatment versus the control group as well as the difference between the two groups. The last column provides the p-value from a regression of the background variable on the treatment dummy. The only statistically significant difference is the share of Asian youth being slightly higher (7 percent) in the treatment group versus the control group (5 percent). Having at least one statistically significant difference at the $p<0.10$ level would be expected by random chance when testing 15 different characteristics.

Source: Author's calculations based on application data provided by the City of Boston Office of Workforce Development.

Table 2. ITT Estimates of SYEP Impact on High School Dropout and Graduation.

	Coefficient on Winning the Lottery (Treatment Dummy)				
	(1)	(2)	(4)	(5)	(6)
Panel A. Dropout Rates					
Dropout One Year Post	-0.015 **	-0.016 **	-0.014 **	-0.011 **	-0.014 **
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Dropout Ever Post	-0.028 **	-0.030 **	-0.030 **	-0.026 **	-0.028 **
	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)
Demographic characteristics	No	Yes	Yes	Yes	Yes
Academic characteristics	No	No	Yes	Yes	Yes
School fixed effects	No	No	No	Yes	Yes
Baseline outcomes	No	No	No	No	Yes
Number of youth	2,970	2,970	2,970	2970	2,970
Panel B. Graduation Rates					
Graduated On Time Post	0.053 **	0.059 **	0.064 ***	0.058 ***	-----
	(0.024)	(0.022)	(0.021)	(0.019)	
Graduated Ever Post	0.058 **	0.063 **	0.069 ***	0.061 ***	-----
	(0.024)	(0.022)	(0.022)	(0.021)	
Demographic characteristics	No	Yes	Yes	Yes	-----
Academic characteristics	No	No	Yes	Yes	-----
School fixed effects	No	No	No	Yes	-----
Baseline outcomes	No	No	No	No	-----
Number of youth	1,953	1,953	1,953	1,953	

Notes: The sample includes youth who were matched in 2014-15 and 2015-16. Each coefficient is from a separate regression where the dependent variable is the outcome listed. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

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

Table 3. ITT Estimates of SYEP Impact on School Attendance

	One Year Post					Two Years Post	
						Coefficient on Winning the Lottery:	
	Coefficient on Winning the Lottery (Treatment Dummy)					One Summer	Two Summers
	(1)	(2)	(3)	(4)	(5)	(6)	
Attendance rate	0.025 ***	0.028 ***	0.031 ***	0.018 ***	0.019 ***	0.010	0.019 *
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.011)
Increased attendance rate	0.048 *	0.042	0.039	0.039 *	-----	0.042 *	0.035
	(0.028)	(0.028)	(0.028)	(0.021)		(0.023)	(0.032)
Decreased attendance rate	-0.067 **	-0.060 **	-0.056 **	-0.056 **	-----	-0.045 **	0.024
	(0.028)	(0.029)	(0.029)	(0.021)		(0.023)	(0.033)
Attendance rate >=90%	0.066 **	0.074 **	0.081 **	0.061 ***	0.078 ***	0.005	0.031
	(0.026)	(0.026)	(0.028)	(0.018)	(0.016)	(0.022)	(0.032)
Average days attended	3.604 *	4.464 **	5.208 **	3.204 **	3.089 **	1.583	4.660 **
	(2.044)	(2.046)	(2.003)	(1.335)	(1.305)	(1.507)	(2.282)
Unexcused absences	-2.514 **	-2.594 **	-2.648 **	-1.754 ***	-1.196 **	-1.441	-2.753 *
	(1.168)	(1.064)	(0.980)	(0.580)	(0.576)	(1.223)	(1.481)
Demographic characteristics	No	Yes	Yes	Yes	Yes	Yes	
Academic characteristics	No	No	Yes	Yes	Yes	Yes	
School fixed effects	No	No	No	Yes	Yes	Yes	
Baseline outcomes	No	No	No	No	Yes	Yes	
Number of youth	2,852	2,852	2,852	2,852	2,852	2,439	

Notes: This table estimates the impact of SYEP participation on attendance related outcomes. The sample for specifications (1)-(5) includes youth who were matched in 2014-15 and 2015-16. Specification (6) includes youth who were matched in both 2014-15 and 2016-17. For specifications (1)-(5), each coefficient is from a separate regression where the dependent variable is the outcome listed. For specification (6), coefficients on indicators for having won the lottery for one summer versus two summers are given from the same regression where the dependent variable is the outcome listed. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes such as increasing or decreasing attendance rate. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

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

Table 4. ITT Estimates of SYEP Impact on Course Performance

	One Year Post					Two Years Post	
	Coefficient on Winning the Lottery (Treatment Dummy)					Coefficient on Winning the Lottery:	
						One Summer	Two Summers
	(1)	(2)	(3)	(4)	(5)	(6)	
Overall GPA	0.085 *	0.085 *	0.080 *	0.113 **	0.080 **	0.037	0.123 *
	(0.050)	(0.049)	(0.046)	(0.042)	(0.036)	(0.049)	(0.074)
Percentage failing any course	0.001	-0.007	-0.007	-0.035 *	-0.023	0.029	-0.061 *
	(0.031)	(0.032)	(0.033)	(0.021)	(0.021)	(0.023)	(0.035)
Percentage failing a math course	-0.011	-0.007	-0.004	-0.028	-0.018	0.041	-0.100
	(0.031)	(0.032)	(0.033)	(0.022)	(0.021)	(0.086)	(0.128)
Percentage failing an ELA course	0.006	0.007	0.012	-0.016	-0.008	-0.020	-0.102 **
	(0.031)	(0.032)	(0.033)	(0.021)	(0.021)	(0.024)	(0.038)
Demographic characteristics	No	Yes	Yes	Yes	Yes	Yes	
Academic characteristics	No	No	Yes	Yes	Yes	Yes	
School fixed effects	No	No	No	Yes	Yes	Yes	
Baseline outcomes	No	No	No	No	Yes	Yes	
Number of youth	2,327	2,327	2,327	2,327	2,327	1,727	

Notes: This table estimates the impact of SYEP participation on course performance. The sample for specifications (1)-(5) includes youth who were matched in 2014-15 and 2015-16. Specification (6) includes youth who were matched in both 2014-15 and 2016-17. For specifications (1)-(5), each coefficient is from a separate regression where the dependent variable is the outcome listed. For specification (6), coefficients on indicators for having won the lottery for one summer versus two summers are given from the same regression where the dependent variable is the outcome listed. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

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

Table 5. ITT Estimates of SYEP Impact on Standardized Test-Taking and Performance

	Coefficient on Winning the Lottery (Treatment)			
	(1)	(2)	(3)	(4)
Panel A. Mathematics				
Took MCAS on time	0.031	0.045	0.039	0.025
	(0.039)	(0.042)	(0.042)	(0.026)
Scaled score	0.583	0.359	0.712	0.764
	(1.321)	(1.234)	(1.220)	(1.104)
Percentage proficient or better	-0.011	0.014	0.024	0.028
	(0.049)	(0.052)	(0.052)	(0.039)
Number of youth	803	803	803	803
Panel B. English Language Arts				
Took MCAS on time	0.053	0.064 *	0.057	0.036
	(0.037)	(0.037)	(0.036)	(0.026)
Scaled score	-0.559	-0.019	0.253	0.553
	(0.844)	(0.794)	(0.780)	(0.765)
Percentage proficient or better	-0.031	-0.012	-0.005	0.003
	(0.035)	(0.033)	(0.032)	(0.039)
Number of youth	815	815	815	815
Demographic characteristics	No	Yes	Yes	Yes
Academic characteristics	No	No	Yes	Yes
School fixed effects	No	No	No	Yes

Notes: This table tests for the impact of SYEP participation on standardized test taking and performance. The sample includes youth who were matched in 2014-15 and 2015-16 and were in grade 9 in the 2014-15 school year. Test-taking is assessed for all youth who were in grade 9 in the 2014-15 school year (N= 1,029 youth). Performance is assessed for youth who took the exam. Each coefficient is from a separate regression where the dependent variable is the outcome listed. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

Table 6. ITT Estimates of SYEP Impact on One-Year Outcomes by Subgroup

	Coefficient on Winning the Lottery* Group Dummy					Total number of youth in each regression
	Marginal Students	Age 16+	Male	Limited English	Public Assistance	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Attendance Outcomes						
Attendance rate	0.037 **	0.042 ***	0.008	-0.004	-0.006	2,852
	(0.016)	(0.013)	(0.012)	(0.018)	(0.016)	
Increased attendance rate	0.050	0.086 *	0.015	-0.016	-0.080	2,852
	(0.053)	(0.050)	(0.047)	(0.080)	(0.056)	
Decreased attendance rate	-0.031	-0.099 **	-0.022	0.127	0.078	2,852
	(0.054)	(0.050)	(0.048)	(0.081)	(0.059)	
Attendance rate >=90%	0.065	0.109 **	0.043	-0.078	-0.052	2,852
	(0.049)	(0.046)	(0.049)	(0.102)	(0.066)	
Average days attended	8.146 *	5.039	0.847	-1.798	-1.878	2,852
	(4.329)	(3.429)	(3.056)	(5.584)	(3.796)	
Unexcused absences	0.567	-1.864	-0.339	0.056	3.135	2,852
	(1.391)	(1.504)	(1.532)	(2.503)	(2.345)	
Panel B. Course Performance						
Overall GPA	0.099	0.146 *	-0.054	0.136	0.048	2,327
	(0.071)	(0.080)	(0.071)	(0.133)	(0.097)	
Percentage failing any course	-0.096 *	-0.048	0.044	-0.224 **	0.001	2,327
	(0.057)	(0.064)	(0.054)	(0.108)	(0.074)	
Percentage failing a math course	-0.058	-0.059	-0.010	0.020	0.033	2,327
	(0.054)	(0.060)	(0.055)	(0.104)	(0.073)	
Percentage failing an ELA course	-0.053	-0.153 **	0.042	-0.130	0.027	2,327
	(0.055)	(0.057)	(0.056)	(0.092)	(0.073)	
Panel C. Dropout						
Dropped out post	-0.005	0.002	-0.001	0.006	0.002	2,970
	(0.009)	(0.013)	(0.012)	(0.027)	(0.016)	
Dropped out ever	-0.004	-0.004	0.004	-0.007	0.018	2,970
	(0.021)	(0.024)	(0.025)	(0.043)	(0.035)	
Panel D. Graduation						
Graduated on time	0.033	0.022	0.013	0.014	-0.047	1,953
	(0.062)	(0.060)	(0.060)	(0.102)	(0.083)	
Graduated ever	-0.005	0.047	0.017	0.037	-0.005	1,953
	(0.060)	(0.064)	(0.056)	(0.090)	(0.077)	
Number of youth in subgroup	852	1,145	1,346	207	514	-----

Notes: This table tests for differential impacts of SYEP participation on education outcomes for various subgroups. The sample includes youth who were matched in 2014-15 and 2015-16. Each coefficient is from a separate regression for the listed outcome and all regressions include the SYEP treatment dummy as well as the interaction of the treatment dummy with the group-level dummy. For attendance, dropout, and graduation outcomes, marginal youth are defined as those who previously had an attendance rate below 90 percent in the year prior to SYEP participation (e.g., 2014-15 academic year). For course performance outcomes, marginal students are defined as those previously having an overall GPA that was below a C average in the year prior to SYEP participation. Each regression includes the full set of covariates from the previous tables including demographic characteristics (age, gender, race, primary lagnuage spoken, limited English, public assistance, homelessness, and disabled status), academic characteristics (e.g., grade level, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years). 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 marginal effects, estimated at means. Robust standard errors are in parentheses.

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

Table 7. Assessing Short-Term Behavioral Changes in Skills and Attitudes

	Treatment Group of Lottery Winners					Treatment-Control		
	(1)	(2)	(3)			(4)		
	Pre-program	Post-program	Post-Pre			Post		
	Mean	Mean	Difference	SE		Difference	SE	
Future work plans and academic aspirations								
I plan to work in the fall	0.406	0.480	0.074	0.009	***	-0.074	0.030	**
I plan to enroll in an eduation or training program after high school	0.674	0.703	0.029	0.014	**	0.003	0.017	
I plan to enroll in a four-year college or university	0.681	0.730	0.049	0.019	**	0.110	0.029	***
I plan to enroll in a two-year college	0.129	0.124	-0.005	0.015		0.062	0.019	***
I am saving for school tuition	0.062	0.114	0.052	0.012	***	0.043	0.021	**
Job readiness skills								
I have all key information to apply for a job	0.810	0.882	0.072	0.021	***	0.094	0.021	***
I have prepared a resume	0.409	0.701	0.293	0.033	***	0.245	0.027	***
I have prepared a cover letter	0.234	0.437	0.204	0.039	***	0.217	0.027	***
I have asked an adult to serve as a reference	0.709	0.745	0.036	0.026		-0.001	0.027	
I have reviewed at least one job application form	0.748	0.824	0.075	0.023	***	0.039	0.028	
I have completed at least one online job application form	0.661	0.709	0.048	0.025	*	-0.033	0.028	
I have searched for jobs online	0.477	0.596	0.119	0.030	***	0.025	0.031	
I have asked an adult for help in finding job opportunities	0.830	0.846	0.017	0.020		0.071	0.024	***
I have developed answers to the usual interview questions	0.679	0.771	0.092	0.027	***	0.069	0.026	**
I have practiced my interviewing skills with an adult	0.548	0.649	0.101	0.021	***	0.064	0.031	**
I know how to be on time	0.431	0.540	0.110	0.018	***	0.081	0.015	***
I know how to organize my work and keep to my schedule	0.418	0.510	0.092	0.014	***	0.086	0.016	***
Community engagement and social skills								
I have a lot to contribute to the groups I belong to	0.319	0.466	0.147	0.023	***	0.156	0.029	***
I feel connected to people in my neighborhood	0.220	0.368	0.148	0.021	***	0.212	0.025	***
I feel safe walking around my neighborhood	0.429	0.467	0.038	0.022	*	0.193	0.028	***
I have a positive role model in my life	0.916	0.926	0.010	0.008		0.005	0.011	
I know how to manage my emotions and my temper	0.442	0.497	0.055	0.023	**	0.065	0.033	**
I know how to ask for help when I need it	0.445	0.487	0.042	0.020	**	0.116	0.030	***
I have a mentor	0.476	0.677	0.201	0.019	***	0.152	0.024	***
I know how to constructively resolve a conflict with a peer	0.366	0.422	0.057	0.018	***	0.136	0.029	***
Number of youth	663	663	663			1,327		

Notes: This tables estimates the changes behaviors and attitudes over the summer for the treatment group as well as the end-of-summer responses for the treatement versus the control groups. Difference over time pre versus post is a simple comparison of means for the same sample of participants completing both surveys. Difference in post-program responses for participants versus controls is the marginal effect showing the difference in the predicted probabilities 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.

Source: S urvey data provided by the City of Boston Office of Workforce Development.

Table 8. Relationship between Short-Term Behavioral Changes and SYEP Imact on Academic Outcomes: ITT Estimates

	(1)			(2)			(3)			(4)			(5)		
	Attendance rate			Attendance rate>=90%			Unexcused absences			Dropped out ever			Graduated on time		
	Coefficient	SE		Coefficient	SE		Coefficient	SE		Coefficient	SE		Coefficient	SE	
Panel A. Academic aspirations															
Planning to attend a four-year college	0.009	(0.012)		0.041	(0.950)		-1.980	(2.367)		0.007	(0.027)		-0.016	(0.051)	
Saving for tuition	0.047	(0.025)	*	0.011	(0.102)		-10.454	(4.742)	**	-----			0.180	(0.100)	*
Panel B. Job readiness skills															
Having key information to apply for a job	-0.002	(0.014)		0.061	(0.038)		-2.144	(1.908)		0.029	(0.023)		-0.025	(0.046)	
Preparing a resume	0.018	(0.010)	*	0.031	(0.032)		-3.515	(1.613)	**	0.000	(0.022)		0.046	(0.040)	
Preparing a cover letter	0.018	(0.012)		0.050	(0.035)		-3.464	(1.820)	*	0.022	(0.022)		0.029	(0.043)	
Developing answers to interview questions	-0.008	(0.014)		0.057	(0.036)		-1.943	(1.791)		0.026	(0.022)		0.000	(0.043)	
Practicing interviewing with an adult	0.009	(0.011)		0.047	(0.035)		-1.806	(1.668)		0.002	(0.023)		0.029	(0.043)	
Being on time	0.020	(0.009)	**	0.070	(0.031)	**	-2.720	(1.383)	**	-0.052	(0.023)	**	0.103	(0.037)	**
Keeping a schedule	0.025	(0.009)	**	0.087	(0.031)	**	-2.287	(1.382)	*	-0.029	(0.022)		0.062	(0.037)	*
Panel C. Community engagement and social skills															
Contributing to the groups they belong to	0.018	(0.011)		0.004	(0.041)		-2.871	(2.097)		-0.055	(0.032)	*	0.137	(0.052)	**
Connecting to people in their neighborhood	0.014	(0.013)		0.059	(0.044)		-3.287	(2.512)		-0.007	(0.030)		0.119	(0.058)	**
Managing emotions	0.020	(0.012)		-0.008	(0.051)		-2.390	(2.107)		-0.080	(0.046)	*	0.150	(0.059)	**
Asking for help	0.015	(0.011)		0.027	(0.049)		-4.342	(2.471)	*	-0.014	(0.032)		0.134	(0.057)	**
Gaining a mentor	0.016	(0.010)		0.015	(0.029)		-3.801	(1.369)	**	-0.026	(0.019)		0.099	(0.035)	**
Resolving conflict with a peer	0.003	(0.010)		-0.018	(0.043)		0.104	(1.769)		-0.004	(0.030)		0.045	(0.022)	**
Demographic characteristics	Yes			Yes			Yes			Yes			Yes		
Academic characteristics	Yes			Yes			Yes			Yes			Yes		
Baseline outcomes	Yes			Yes			Yes			Yes			Yes		
Number of youth	2,852			2,852			2,852			2,970			1,953		

Notes: This table estimates the relationship between improvements in short-term behaviors and skills that occur over the summer and subsequent imporvements in attendance during the year after participating in the program. The sample includes youth who were matched in 2014-15 and 2015-16. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

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

ONLINE APPENDIX

I. Boston SYEP Intervention and Experimental Design

The Boston SYEP is administered by the Boston Mayor's Office of Workforce Development (OWD) and implemented by four non-profit community organizations, known as intermediaries. All Boston city residents aged 14 to 24 years are eligible for the program and youth apply directly to the program through one of the four intermediaries. The intermediaries are responsible for reviewing applications, supervising job placements, and delivering the program's career-readiness curriculum. Youth typically apply to the intermediary in their neighborhood. Administrative records indicate that less than 5 percent of youth apply to more than one agency.

Two of the intermediaries make use of random assignment to assign youth to jobs because of the high number of applications they receive for the limited number of SYEP jobs available. The analysis in this paper is restricted to youth who applied for a job for summer 2015 through Action for Boston Community Development (ABCD), one of the two intermediaries that make use of random assignment. 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. DYEE also chose not to implement the survey during the summer of 2015.

The enrollment period typically spans February through June, and applicants are notified of their lottery status and job assignment in late June. See Figure A1 for a timeline of the program and data collection. ABCD uses a computerized system with a random-assignment algorithm to select applicants based on their applicant ID numbers and the number of available slots as determined by the amount of funding ABCD receives each year. This system effectively assigns the offer to participate in the program at random, creating a control group of youth who apply to the SYEP but are not chosen. Of the 4,235 youth who applied to ABCD in 2015, a total of 1,186 were offered a job via random assignment (28 percent), 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 of youth dropping out of the program during the summer. As discussed in the main text, randomization successfully balanced all observable characteristics across treatment and control groups. We test for balance using separate models estimating the effect of winning the lottery on preexisting applicant characteristics among school-aged youth for gender/race groupings (see Table A1).

However, are the applicants served by ABCD representative of all youth age 14-24 years in the city of Boston? This question is important for demonstrating internal validity for the city of Boston and for city leaders seeking to bring the summer jobs program to scale. Figure A2 shows the distribution of youth age 0-17 based on data from the 2010 Census, with greater representation among Dorchester, Roxbury, and Mattapan. Table A2 shows that ABCD draws applicants from all 18 of the city's neighborhoods with a similar distribution of applicants showing greater representation among Dorchester (about 33 percent), Roxbury (about 10 percent), and Mattapan (about 9 percent). Applicants from other neighborhoods are also similarly represented in proportion to the distribution of youth as shown by the Census data: Hyde Park (about 6.6 percent), South Boston (about 6.4 percent), South End (about 6 percent), Roslindale (about 5.7 percent), Allston-Brighton (about 5 percent) and Jamaica Plain (about 4.5 percent).

Moreover, data from the 2011-15 5-Year American Community Survey indicate that ABCD applicants have similar gender and racial characteristics in comparison to the population of low-income Boston youth. Table A3 shows that although ABCD applicants are more likely to be younger, within that younger age group (age 14-17 years) the breakdown by gender and race is very similar. In general, it is reasonable to expect that youth applying to summer jobs programs would be younger given the greater difficulty that less experienced youth have in finding a job on their own.

II. Data Sources

A. Administrative Data on Academic Outcomes

The main outcome data consist of student records from the Massachusetts Department of Elementary and Secondary Education (DESE). This rich data source includes information on each student in Massachusetts from 2010 through 2017. This includes data on attendance, course grades, MCAS test scores, dropout, and high school graduation.

Data were matched using name and date of birth where name was that listed as of the end of the school year. The school record data include all public school records, including charter schools, for an individual in the state of Massachusetts, even if they move across school districts. There is little reason to believe that a summer jobs program would affect how names are recorded in the data, meaning that the matching error should be uncorrelated with treatment status. This is particularly true for youth applying through ABCD in Boston. There is a rigorous application process which requires verification of household income and receipt of public assistance for the purposes of being able to match youth to the appropriate funding streams that the organization must braid together each year across both government and charitable sources. As a result, the application process involves the signature of a parent to verify that the information is correct and to give consent for obtaining information from administrative schooling, employment, and criminal justice records.

To match youth who applied to participate through ABCD to DESE data files, a fuzzy match was performed using first name, last name, and date of birth[1]. Youth are matched to DESE data using SIMS data files (discussed in greater detail below). Additional DESE data is then merged in using the unique student level identifier SASID in the SIMS data files.

Table A4 describes the details of the matching process at each stage. The full participant sample for the 2015 summer consisted of 4,235 youth, 1,186 in the treatment group and 3,049 in the control group. Of the original sample, 3,372 were in grades 8-11 during the 2014-15 school year before applying to the summer jobs program. A total of 3,269 youth were matched to the 2014-15 DESE files, yielding a match rate of 96.9 percent. Of these individuals, 2,970 youth (90.9 percent) were matched to the 2015-16 DESE files that contain information for the year immediately after participating in the program. Of the sample that matched in both years, similar proportions were matched in the treatment group (90.4 percent) and control group (91.1 percent).

To more rigorously test for selective attrition, Table A5 presents estimates of the effect of winning the lottery on the same preexisting demographic characteristics as before. The first column limits the sample to youth who were matched in the 2014-15 school year and the second further constrains the sample to those who were also enrolled in the 2015-16 school year. The SYEP indicator does not significantly predict any individual characteristics—with the exception of the one characteristic (e.g. Asian) that was noted in the earlier balance test for the full sample—suggesting that overall SYEP lottery winners and losers did not differentially attrit.

Table A6 provides additional support of similar rates of attrition across treatment and control groups. Using the sample of youth matching in the 2014-15 school year, an indicator for

[1] The Stata user-written command reclink was used to perform the fuzzy match. Following the fuzzy match, all identified fuzzy matches were hand-checked to ensure accuracy. Of the 2,970 youth that were matched in both the 2014-15 and 2015-16 academic years, 2,332 were perfect matches and 638 were fuzzy matches. For more information on the reclink command, see http://fmwww.bc.edu/repec/bocode/r/reclink.html. Our results also hold using the perfectly matched sample.

matching in both 2014-15 and 2015-16 is regressed on a dummy variable indicating treatment, as well as additional covariates such as grade in the year prior to participating and demographic characteristics. Across all specifications, the treatment indicator is not significant predictive of matching in both years, providing further support of similar rates of attrition.

To further test for validity and balance, we also estimate the effect of the lottery indicator on individual baseline outcomes where possible.[2] To do this, we impose the minimal limitations on our sample based on the outcome that we are assessing, although the balance also holds when using the most restricted sample. For example, the dropout outcomes are assessed for the entire sample whereas the attendance outcomes are assessed only for those who did not have a dropout indicator in their school record. Similarly GPA and course failure can only be calculated for students who did not drop or withdraw from a class. Table A7 shows that there were no significant pre-existing differences in the baseline school outcomes between youth in the treatment versus control groups as would be expected under random assignment. During the school year prior to applying to the Boston SYEP, the two groups had similar attendance rates, unexcused days of absence, overall GPA, and course failures—overall or separately for math and English classes. They also had a similar propensity for the likelihood of having dropped out of school prior to participating in the SYEP.

1 Attendance and Related Outcomes Data

Data on attendance, dropout, and graduation comes from the Student Information Management System (SIMS) data files from the Massachusetts DESE. Each entry in this data file is a unique student/school/year observation. A unique identifier, SASID, is given for each student. A student will appear in this file multiple times if that student attends multiple schools in

[2] Note that it is not possible to test for baseline outcomes for taking the MCAS or for high school graduation.

the state of Massachusetts during the school year. For those youth that attend multiple schools during a school year, days in membership, days attended, and unexcused absences are calculated as sums of those variables across all schools attended. One observation is then kept per student, corresponding to the school where the student spent the greatest number of days in membership. The length of the school year in Massachusetts varies slightly across school districts between 180 and 190 days. Those youth for whom days in membership is a given year is above 190 are excluded from estimating attendance outcomes. In the 2015-16 school year, less than 5 percent of youth fall into this category.

Dropout and graduation outcomes are measured using SIMS data files also. A student is classified as a dropout if the variable enrollment status takes on values between 30 and 36 in the SIMS file for a given year. These values comprise all enrollment dropout statuses, where these statuses may include a reported reason why the student chose to drop out if known. A student is classified as a graduate if enrollment status takes on values of 04 or 10. Value 04 corresponds to graduate with a competency determination. Value 10 corresponds to receipt of a certificate of attainment. Only 3 youth in the sample received a certificate of attainment, while 1,408 graduated with a competency determination.

2. Course Performance Outcomes Data

Data on course performance comes from the Student Course Schedule (SCS) data files from the Massachusetts DESE. Each entry in this data file is a unique student/class/year observation. A unique identifier, SASID, is given for each student. The number of entries per student in this data file corresponds to the number of courses a student took in a given academic year. For each course, a student may receive a letter grade, a numeric grade, both letter and numeric grades, or neither. We convert reported grades to a common scale according to the following schedule:

Numeric Grade	Letter Grade	Course Grade
97 – 100	A+	4.0
93 – 96	A	3.7
90 – 92	A-	3.3
87 – 89	B+	3.0
83 – 86	B	2.7
80 – 82	B-	2.3
77 – 79	C+	2.0
73 – 76	C	1.7
70 – 72	C-	1.3
67 – 69	D+	1.0
63 – 66	D	0.7
60 – 62	D-	0.3
Below 60	F	0

Where both letter and numeric grades are given, we use the letter grade. GPA is calculated based on weighted course grades. Classes designated as Basic or Remedial by the State are included in averages as is. An additional 0.3, 0.8, and 1.3 points are added to the course grades of classes designated as general, advanced, and post-secondary credit respectively. The weighted GPA is found by taking the simple average of the weighted course grades.

When measuring course failures, we define a course failure as a numeric grade below 65 or a letter grade of F. If a student took a course as pass/fail, failure of that course is counted as a course failure, although those courses are not included in the calculation of GPA.

3. MCAS Outcomes Data

Data on the Massachusetts Comprehensive Assessment System (MCAS) comes from the MCAS data files from the Massachusetts DESE. Each entry in these files corresponds to a unique student/year observation for all MCAS exams completed in a given year. Students are expected to take MCAS exams in math and English in grades 3-8 and 10. Youth who should take the MCAS in the 2015-16 year are those in grade 9 in the 2014-15 school year. Proficiency or better for the ELA and Math MCAS is defined as having a score that was classified as "proficient" or "advanced" by DESE. Proficiency or better and scores are defined for those youth in grade 9 in the 2014-15 year who take MCAS math or ELA exams in the 2015-16 school year.

B. Survey Data on Pre-/Post-Program Behavioral Outcomes

The survey was originally developed and implemented by the Youth Violence Prevention Collaborative, an initiative that began funding summer employment opportunities in Boston neighborhoods that had been identified by the Boston Police Department as having a high number of fatal and non-fatal shootings. Starting in the summer of 2012, the goal was to measure personal and social behaviors that correlate with youth violence and exposure to violence to determine whether summer employment could reduce the exposure of economically disadvantaged teens to risky, violent, and delinquent behaviors. This original survey was typically administered at the end of the summer to program participants and covered basic demographic information as well as questions on risky and delinquent behavior, community engagement, and general satisfaction with SYEP jobs and programming.

With the help of the Office of Workforce Development (OWD), we expanded the survey's content and scope during the summer of 2015. In terms of content, we added questions related to job readiness, post-secondary aspirations, and financial capability. In terms of scope, OWD engaged ABCD to conduct both a pre- and post-survey to measure changes over time for participants. The pre-survey was administered to participants during orientation in early July and the post-survey was administered in mid-August when participants pick up their last paycheck. Surveys were administered to participants on-site using a paper-based collection method. Although nearly the same number of individuals answered the pre- and post-surveys, these were not necessarily the same individuals as only 66.9 percent of individuals could be matched. However, testing for differential attrition between the pre- survey sample and the matched sample for both ABCD yields no statistically significant differences (see Table A8).

In addition, OWD also worked with ABCD to administer the post-survey to the control group to compare the experiences of participants to the counterfactual experiences of those who had applied but not been selected by the SYEP. The post-survey was administered to the control group on-line via email with a link to the survey web site using SurveyGizmo. The control group was offered the chance to win a free iPad mini for completing the survey. Yet despite several reminders and extensions, the response rates differed significantly across the treatment versus the control group. Indeed, although the number of respondents among the control group was similar (N=664), this represented a response rate of only 21.8 percent.

Moreover, although the control group was randomly selected, those who chose to respond to the post-survey were not. Unlike other household surveys, we know that the characteristics of the control group should be indistinguishable from those of the treatment group because the random assignment was shown to be balanced. This means that we can explore the sign of the bias by exploring how the observable characteristics differ between the two groups. Table A9 shows that relative to the treatment group, survey respondents from the control group exhibited characteristics that are on average associated with better economic, academic, and criminal justice outcomes. They were more likely to be older, female, identify as white or Asian, and indicate that they live in a two-parent household.

We argue that this bias goes *against* finding an impact for the Boston SYEP, given that the survey respondents in the control group exhibit demographic characteristics that would suggest a high bar for comparison. In the literature, each of the observable characteristics that differ for the control group relative to the treatment group has been shown to be associated with better long-term outcomes. In terms of academic outcomes, females are now more likely than males to attend college.[3] There is also a large literature explaining test score gaps that finds lower scores among African-American children and those living in single parent households.[4] In terms of employment, higher employment rates are observed among females, whites, and older youth.[5] In terms of criminal justice outcomes, age, male gender, and living in a single-parent home are significant predictors of re-offending among youth.[6]

Moreover, youth in the control group who responded to the survey are likely to be more intrinsically motivated than those who did not. In general, surveying youth is difficult but particularly so when relying on email for deployment since youth are less likely than adults to use email for personal communication (e.g., texting friends is more common), especially during the summer when school is out. The control group was surveyed about their summer experiences

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[3] Hugo Lopez, M. and Gonzalez-Barrera, A. 2014. "Women's college enrollment gains leave men behind." Pew Research Center. http://www.pewresearch.org/fact-tank/2014/03/06/womenscollege-enrollment-gains-leave-men-behind/

[4] Jencks, C. and Phillips, M. 1998. "The Black-White Test Score Gap: Why It Persists and What Can Be Done." The Brookings Institution. https://www.brookings.edu/articles/the-blackwhite-test-score-gap-why-it-persists-and-what-can-be-done/

[5] Child Trends. 2017. Key Facts about Youth Employment. Retrieved from: https://www.childtrends.org/indicators/youth-employment

[6] See Autor, D. & M. Wasserman. 2013. Wayward Sons: The Emerging Gender Gap in Labor Markets and Education. Third Way, March. See also Cottle, C., Lee, R. & Heilburn, K. 2001. The Prediction of Criminal Recidivism in Juveniles
A Meta-Analysis. Criminal Justice and Behavior. 28(3), 367-94.

via an email that came from the Boston Office of Workforce Development about a program for which they were not selected. As such, taking the time to open the email, read it, and complete the survey suggests a relatively high degree of motivation. One of the survey questions confirms this hypothesis: youth were asked why they wanted to work this summer. Among the respondents, youth in the control group were more likely than those in the treatment group to report wanting a summer job to learn more about college and less likely to report wanting to make money, have something to do, or stay out of trouble.

It is important to acknowledge the other limitations of self-reported survey data such as those raised in Meyer, Mok, and Sullivan (2015). In that paper, the authors measure the degree to which nationally representative surveys suffer not just from unit non-response but also from item non-response and measurement error by comparing survey results to administrative data. In terms of item non-response, this can be a problem, particularly when asking sensitive questions about behavior among developing youth. For example, one of the other intermediaries that works with court-involved youth (Youth Options Unlimited) chose to include a series of questions based on the Youth Behavioral Risk Survey that asked about risky behavior such as drug and alcohol use and physical violence. However, the non-response rate was too high (roughly 20 percent) so that these responses were not informative. In contrast, Table A10 shows that the item non-response rates for the survey questions used in the mediator analysis were less than 5 percent for both the ABCD treatment and control groups with no significant differences across the two groups.

Finally, in terms of measurement error, there is little room to assess the magnitude of this bias without access to administrative data that covers the same items as the survey. The only test for measurement error that we can perform is to compare the employment rate for the control group to what is found in the state quarterly wage and employment administrative data. Only 26.4 percent of those responding to the survey in the control group reported that they had worked during the summer. This rate is consistent with the quarterly wage record data provided by the Massachusetts Division of Unemployment Assistance, which shows that a similar proportion of youth in the control group (28.2 percent) reported working during the third quarter (July-September) of 2015. In addition, because we measure impact for the treatment group relative to control group, if we assume that the measurement error is random, then this would reduce efficiency but not cause bias. we do not have any reason to believe that measurement error would differ across the treatment and control groups.

III. Analysis Methods

To assess the impact of the Boston SYEP on academic outcomes, we compare attendance, course performance, MCAS test taking and scores, dropout, and high school graduation during the period following the intervention for youth offered an SYEP placement (the treatment group) to those for youth not offered a placement (control group). Because SYEP participation is allocated via lottery, we obtain causal estimates using a simple comparison of means on the outcome of interest. 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 because program administrators want to account for program take-up to assess the degree to which SYEP could improve academic outcomes among all the applicants, not just the participants. Nonetheless, because not all youth end up participating, the ITT will understate the effects of the program for those youth who choose to participate. To address this, we also provide estimates of treatment-on-the-treated (TOT).

A. *Intent-to-Treat Analysis*

Let Y_{it} denote a post-program outcome for individual i during post-randomization period t. We model this outcome as:

$$Y_{it} = SYEP_i\pi_1 + X_{i(t-1)}\beta_1 + s + \mu_{it1} \quad (1)$$

where Y_{it} is the academic outcome, $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, $X_{i(t-1)}$ is a set of pre-existing baseline academic outcomes and demographic characteristics, s is a vector of school fixed effects to control for the influence of time-invariant school characteristics on educational outcomes, and μ_{it1} is a stochastic error term.

Although baseline characteristics are not necessary for identification, we include them in the regression to improve the precision of estimates by accounting for residual variation in the outcomes. Demographic characteristics collected by OWD during the application process include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics from the school administrative data include indicators for grade, enrollment in the Boston Public School district, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. We also include baseline academic outcomes captured by the administrative data during the pre-program period (e.g., 2014-15 school year). The inclusion of these controls does little to affect the point estimates but does improve the precision. None of the substantive conclusions are different if these variables are excluded from the regressions.

Additionally, we are interested in exploring whether SYEP impacts fade over time as well as if additional summers (e.g., increased "dosage") enhances outcomes. Given that 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 explore these questions, we make use of an additional year of DESE data for the 2016-17 school year that provides information on school outcomes for the second academic year after participating for the summer 2015 cohort. We then use administrative program data from OWD to identify youth who participated in the program during the summer of 2016 to construct indicators for whether youth had participated for only one summer (SYEP1) or two summers (SYEP2).[7] About one-quarter (26.8 percent) of youth in the original treatment group participated for a second summer, yielding enough variation to assess the importance of both dosage and fade out. To estimate separate impacts by number of summers of treatment, we use equation (2):

$$Y_{it} = SYEP1_i\,\pi_{10} + SYEP2_i\,\pi_{11} + X_{i(t\text{-}1)}\,\beta_1 + s + \mu_{it1} \qquad (2)$$

Note that there are some limitations to this analysis. For example, having won the lottery in the first year is likely to increase the likelihood of applying for a second time and the opposite is likely to be true for those who did not win the lottery the first time. Indeed, only 3.7 percent of those in the control group end up participating in the program during the summer of 2016. As such, our estimates of the impact of a second summer of participation (π_{11}) primarily reflect the impact of the program conditional on having won the lottery the first time. Nonetheless, we believe it is still informative to explore program impacts two years post participation and assess how much can be explained by the number of summers of participation.

B. Treatment-on-the-Treated Analysis

Nonetheless, because not all youth end up participating, the ITT will understate the effects of the program for those youth who choose to participate. Under the usual relevance and exogeneity assumptions for instrumental variables, this latter set of effects can be recovered from

[7] Note that youth who participated for only one summer includes both members of the original treatment group who only participated in summer 2015 as well as members of the control group who participated in summer 2016.

the experimental data.[8] We perform this estimation through a two-stage least squares strategy, in which random assignment ($SYEP_i$) is an instrument for actual participation (P_{it}), and P'_{it} is the predicted probability of participation from equation (3):

$$P_{it} = SYEP_i\, \pi_2 + X_{i(t\text{-}1)}\, \beta_2 + s + \mu_{it2} \tag{3}$$

$$Y_{it} = P'_i\, \pi_3 + X_{i(t\text{-}1)}\, \beta_3 + s + \mu_{it3} \tag{4}$$

If all youth respond the same way to the program (i.e., if treatment effects are constant across youth), then equations (3) and (4) also yield an estimate of the average treatment effect (ATE) across this population of disadvantaged youth. Given that treatment effects are likely to be heterogeneous across youth, then the coefficient π3 estimates a local average treatment effect—the effect of participation on those who comply with random assignment.[9] Because there is no control crossover (no always-takers) in this setting, π3 provides an estimate of the treatment-on-the-treated.

C. Functional Form

While ordinary least squares provides the best linear unbiased estimate of the treatment effect under the Gauss-Markov assumptions, we also explore the robustness of the results to alternative assumptions. Specifically, we relax the linear functional form assumption by using non-linear specifications. For example, to analyze treatment-control differences in the number of days attended – a count variable – we use a Poisson quasi-maximum likelihood estimator (QMLE). The consistency of this estimator only requires the correct specification of the

[8] For the random assignment variable, $SYEP_i$, to be a valid instrument, it must be correlated with program participation, P_{it}, and uncorrelated with μ_{it3}.
[9] When treatment effects are heterogeneous, $SYEP_i$, must also satisfy a monotonicity condition to be a valid instrument. In particular, random assignment must make everyone weakly more likely to participate and no one less likely.

conditional mean, not the entire distribution.[10] We also use Huber-White robust standard errors to allow for over-dispersion, relaxing the Poisson distributional constraint that the mean equals the variance. To analyze differences in the likelihood of an outcome such as dropout, a 0/1 dependent variable, we use a probit estimator.

D. Exploration of Program Mechanisms

Ideally, a full mediation analysis would be used to generate evidence for how the SYEP program achieved its effects using measures of the mediating variable as well as the dependent and independent variable.[11] The first step is to estimate a significant relationship between the dependent variable of interest (Y_{it}) and the independent variable ($SYEP_i$) using equation (1) above.

Second, a significant relationship is estimated between the hypothesized mediating variable (M_{it}) and the independent variable (SYEP_i) using the following equation:

$$M_{it} = SYEP_{it}\, \pi_4 + X_{it}\, \beta_4 + \mu_{it4} \qquad (5)$$

where M_{it} is one of the short-term program outcomes (e.g., social skills), SYEP_i is a dummy variable indicating the individual received an offer to participate, and X_{it} is a set of demographic characteristics collected at the time of the survey.

Third, the mediating variable (M_{it}) is shown to be significantly related to the dependent variable (Y_{it}) when both the independent variable and mediating variable are included as predictors:

$$Y_{it} = SYEP_i\, \pi_5 + X_{i(t-1)}\, \beta_5 + s + M_{it}\, \gamma + \mu_{it5} \qquad (6)$$

[10] Wooldridge JM., in Handbook of Applied Econometrics Volume II: Microeconomics, M. H. Pesaran, P. Schmidt, Eds. (Blackwell, Oxford, UK, 1997), pp. 352–406.).
[11] See Baron RM & Kenny DA. 1986. The moderator-mediator variable distinction in social psychological research: conceptual, strategic, and statistical considerations. J Personal Social Psychology, 51, 1173–82. See also Keele, L., Tingley, D. & Yamamoto, T. (2015). Identifying Mechanisms Behind Policy Interventions Via Causal Medication Analysis. Journal of Policy Analysis and Management, 34, 937–963.

To be a valid mediator, the coefficient π_3 relating the independent variable to the dependent variable in equation (6) must be smaller (in absolute value) than the coefficient π_1 relating the independent variable to the dependent variable in the equation (1) without the mediating variable. Researchers often test whether there is complete or partial mediation by testing whether $\pi 5$ is statistically significant, which is a test of whether the association between the independent and dependent variable is completely accounted for by the mediator.

Due to data limitations, we are unable to undertake the typical mediation analysis described above. This is because the post-survey was administered to the control group anonymously, rather than confidentially as was done for the treatment group. As such, we can only link the self-reported survey responses to the medium-term academic outcomes for youth in the treatment group who responded to the survey. Nevertheless, it is still possible to explore whether improvement in the short-term behavioral impacts are correlated with subsequent improvement in the academic outcomes to shed light on the program's mechanisms. We do this in three ways.

First, we modify equation (6) as follows:

$$Y_{it} = SYEP_i\, \pi_6 + X_{i(t-1)}\, \beta_6 + s + \Delta M_i\, \delta + \mu_{it6} \qquad (7)$$

On the left-hand side, the dependent variable is one of the medium-term academic outcomes (e.g., number of unexcused absences) while on the right-hand side is a dummy indicating positive improvement for a specific short-term program impact ΔM_i (e.g., ability to be on-time). A negative and significant coefficient on ΔM_i indicates that improvement in the short-term program impact observed during the summer of participation is negatively correlated with longer-term academic outcome. Moreover, if the coefficient on the $SYEP_i$ dummy in equation (6) is smaller in magnitude than that in equation (1), this would suggest that ΔM_i plays a role in achieving the medium-term impact separate from simply being assigned to treatment.

However, only youth in the treatment group who participated will have responded to the survey. As such, it is still possible that the observed changes in the short-term program measures from the survey are correlated with other unobserved factors (e.g. motivation to participate) that are driving the longer-term improvements in school outcomes We address this in two ways. First, we use a two-stage least squares to estimate the impact of the short-term behavioral impacts on the longer-term academic outcomes using the SYEP treatment dummy as an instrument for participation and include ΔM_i as a control:

$$P_{it} = SYEP_i \, \pi_6 + X_{i(t-1)} \, \beta_6 + s + \Delta M_i \, \zeta + \mu_{it6} \tag{8}$$

$$Y_{it} = P'_i \, \pi_7 + X_{i(t-1)} \, \beta_7 + s + \Delta M_i \, \zeta + \mu_{it7} \tag{9}$$

Again, if the coefficient on ΔM_i is negative and significant and the coefficient on the $SYEP_i$ dummy is smaller in magnitude than that in equation (1), this would suggest that ΔM_i is a potential mediator.

Second, we use an alternative specification for the mediator analysis to test whether these same short-term program measures are driving the improvement in attendance among only program participants completing both surveys using the equation (10):

$$Y_{it} = X_{i(t-1)} \, \beta_4 + s + \Delta M_i \, \zeta + \mu_{it} \tag{10}$$

Note that the mediator analysis implicitly assumes that there was no change in the short-term program measures for youth in the control group. We argue that this assumption is plausible if the analysis is restricted to those short-term program impacts for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group by the end of the summer.

In fact, there is an entire literature on the **loss** of skills among youth over the summer, particularly among disadvantaged groups. A meta-analysis summarizing the findings from the literature regarding academic skills concluded that concluded that: (1) on average, students' achievement scores declined over summer vacation by one month's worth of school-year learning, (2) declines were sharper for math than for reading, and (3) the extent of loss was larger at higher grade levels.[12]

Moreover, summer learning loss is not limited to academic skills but is likely to also affect ***social*** skills. This is because although summer has the potential for more social interaction, youth report being lonelier. A recent survey of 2,000 youth found that more than half of teenagers feel isolated during their time off from school, with a quarter saying the summer is their loneliest time of the entire year, rising to 29 per cent among girls.[13] While changes in technology have made staying in touch with friends easier than ever, an over-reliance on smartphones and social media apps appears to have left teenagers feeling more isolated as they substitute quality time with friends for texting. The study found that although almost two thirds of teens will talk to their friends every day on social media during the summer, only 14 percent will see them face-to-face.

In addition, there is a newer literature regarding "summer melt"—a surprisingly common scenario in which high-school graduates apply, are accepted, and say they plan to enroll in college—but don't. This literature supports the assumption that academic aspirations to attend college do not typically increase over the summer, even among those who have been accepted. For example, 22 percent of the lowest income, college-intending students in Boston uAspire failed to matriculate into college in the fall after high school graduation, compared to an 18

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[12] Cooper H., Nye B., Charlton K., Lindsay J., Greathouse S. (1996). The effects of summer vacation on achievement test scores: A narrative and meta-analytic review. Review of Educational Research, 66(3), 227–268.
[13] Panayiotou S., Newton S., Matthews P., Webster H., Andersson D. 2017. National Citizen Service 2016: Evaluation Technical report. Kantar Public, December.

percent among all other students.[14] A study reporting on two randomized trials finds that offering college-intending graduates two to three hours of summer support and coaching increased college enrollment by 3 percentage points overall, and by 8 to 12 percentage points among low-income students.[15]

IV. Robustness Checks

A. Treatment-on-the-Treated results

As discussed above in the analysis methods section, because not all youth end up participating, the ITT estimates will understate the effects of participating in the program for those youth who choose to participate. Table A11 reports the Treatment-on-the-Treated (TOT) estimates that show the effect of the Boston SYEP for those who chose to participate. For that group, the attendance rate improves by 2.2 percentage points and unexcused days of absences fall by 2.1 days. Improvements in high school dropout and graduation rates only slightly larger than the ITT estimates, likely because the take-up rate is so high (about 85 percent).

B. Alternative Specifications for Mediation Analysis

As discussed above in section 2, due to data limitations, we are unable to undertake the typical mediation analysis because the post-survey was administered to the control group anonymously, rather than confidentially as was done for the treatment group. As such, we can only link the survey responses to the longer-term criminal justice outcomes for youth in the treatment group who responded to the survey.

For most of the mediator variables, improvement can only be measured simply as a 0/1

[14] Castleman, B. and Page, L. A Trickle or a Torrent? Understanding the Extent of Summer 'Melt' among College-Intending High School Graduates. Social Science Quarterly, 95(1).

[15] Castleman, B., Page, L., Schooley, K. 2014. "The Forgotten Summer: DESEs the Offer of College Counseling after High School Mitigate Summer Melt among College-Intending, Low-Income High School Graduates?" Journal of Policy Analysis and Management, 33(2), 320-44.

change for improvement. For example, in terms of academic aspirations an improvement is measured as switching from not wanting to attend college to wanting to attend college. Similarly, in terms of jobs readiness skills an improvement is measured as switching from not being able to write a resume to being able to write a resume. However, for the social skill and community engagement questions it is possible to construct multiple measures. This is because these questions are measured using a Likert scale (e.g., Strongly Agree, Agree, Neutral, Disagree, Strongly Disagree). We measure this as any positive change (e.g. switching from Strongly Disagree to Disagree or any upward shift).

We explore whether improvement in the short-term behavioral impacts are correlated with subsequent improvement in the criminal justice outcomes to shed light on the program's mechanisms using the following equation:

$$Y_{it} = SYEP_i\, \pi_6 + X_{i(t-1)}\, \beta_6 + \Delta M_i\, \delta + \mu_{it6} \qquad (6)$$

On the left-hand side, the dependent variable is one of the longer-term criminal justice outcomes (e.g., number of crimes per youth) while on the right-hand side is a dummy indicating positive improvement for a specific short-term program impact ΔMi (e.g., ability to resolve conflicts with a peer). A negative and significant coefficient on ΔMi indicates that improvement in the short-term program impact observed during the summer of participation is negatively correlated with longer-term criminal behavior. Moreover, if the coefficient on the SYEPi dummy in equation (6) is smaller in magnitude than that in equation (1), this would suggest that ΔMi plays a role in achieving the longer-term impact separate from simply being assigned to treatment.

Although participants demonstrated significant gains in a variety of short-term program outcomes according to the survey data, only some of those behavioral changes appear to be correlated with subsequent improvements in school outcomes as shows in Table 8 of the paper. These include saving for college over the summer experienced, gaining a mentor, improvements in work habits such as being on time and organizing one's work / keeping to a schedule, and improvements in social skills—such as managing emotions and asking for help.

Given that this approach could also be driven by unobservable characteristics such as youth motivation (e.g. as reflected in their willingness to answer the survey), we also test whether these same relationships hold when the sample is restricted to participants completing both the pre- and post-survey. Table A12 shows that these results are strongly similar in both magnitude and significance to those using the ITT estimation. Finally, Table A13 confirms that the relationship between improvements in these short-term measures and academic outcomes is even stronger when restricting the analysis to just the participants.

V. Cost Benefit Calculation

A key question from a policy perspective is whether the benefits to society from the program outweigh the program's costs. Although it is somewhat premature to perform a full cost benefit analysis until other key outcomes related to schooling and employment have been measured, I provide some back-of-the-envelope calculations comparing the short-term benefits from the increase in graduation rates to the program's costs.

The cost of administering the program for the City of Boston was about $2,000 per participant, which 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. Note that this is the budgetary cost to the City for funding the program. It may understate the costs from a broader perspective, as it does not include the opportunity cost of city staff, time donated by program providers, or the deadweight loss involved in raising the tax dollars.

Our analysis finds that participating in the summer jobs program significantly reduces the likelihood of dropping out of high school and correspondingly raises the likelihood of graduating. Specifically, being randomly selected into the Boston SYEP reduces the likelihood of dropout by 2.6 percentage points—or 24.8 percent—relative to the control group. High school graduates have better outcomes than dropouts along a number of dimensions including being more likely to be employed and earn a higher taxable income (Child Trends 2017) as well as being less likely to engage in criminal behavior or require social services (Lochner and Moretti 2001).

By some estimates, each new graduate confers a net benefit to taxpayers of about $127,000 over the graduate's lifetime. According to the City of Boston, the program costs roughly $2,000 per participant, resulting in a total cost of $2.4 million for the 1,200 youth that participated through ABCD during the summer of 2015. Given that the program increases the likelihood of high school graduation by 6 percentage points, this would yield an additional 72 graduates, who on net would collectively confer a benefit of $9.1 million over their lifetimes. On an annual basis they would be expected to collectively contribute $130,000 per year, implying that the City would recoup its investment roughly 18 years post-graduation.

Figure A1. Timeline of Boston SYEP Program and Data Collection

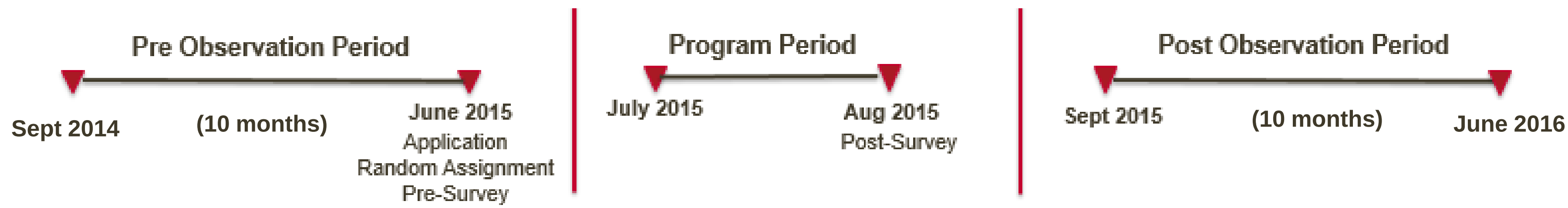
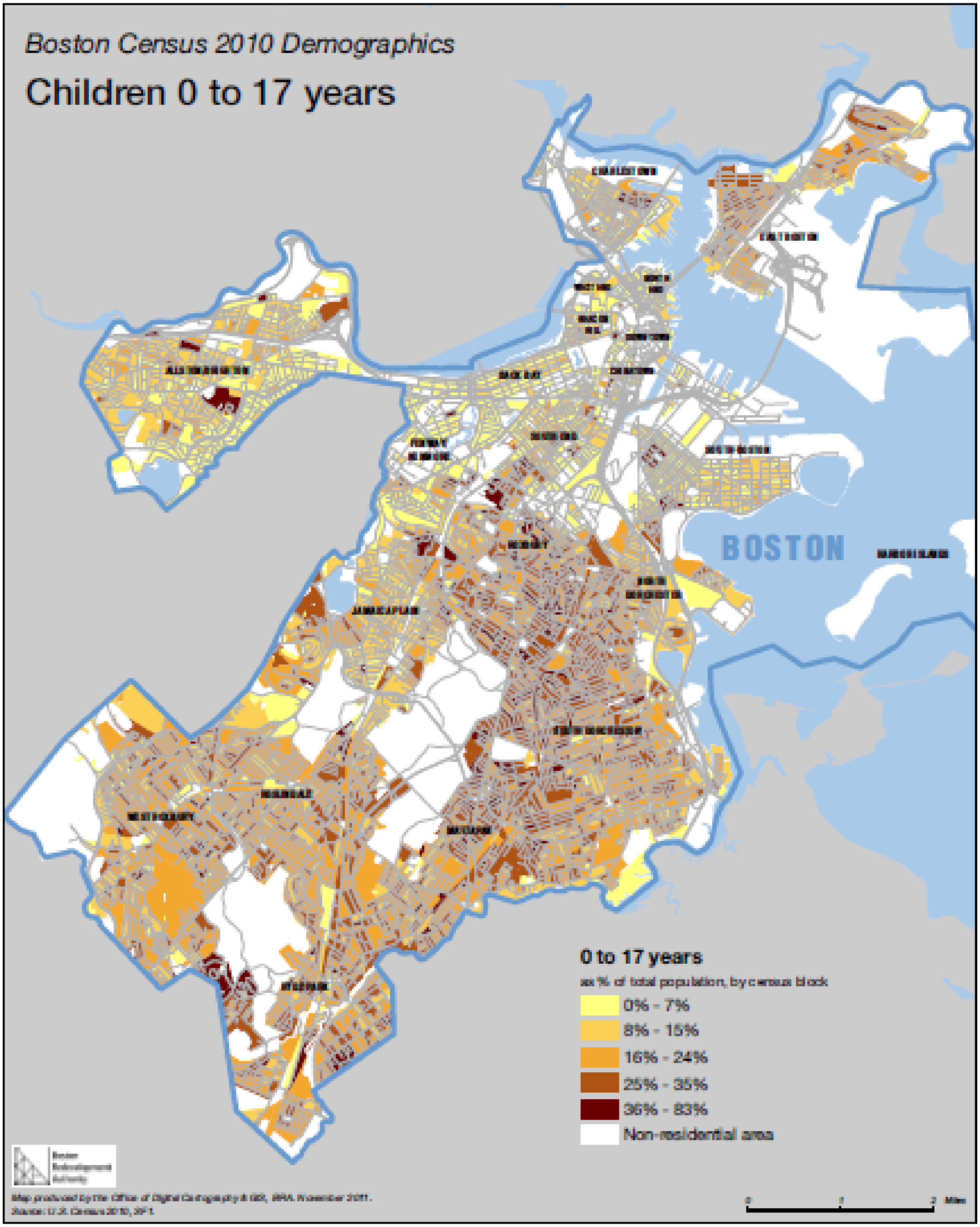


Figure A2. Distribution of Boston Youth Population by Neighborhood.



Source: Boston Planning and Development Agency. http://www.bostonplans.org/3d-data-maps/gis-maps/census-and-demographic-maps

Table A1. Testing the Validity of the ABCD Lottery within Demographic Groups

	All groups combined	Youth: Age 14-18 years					
		African American		White		Other/two or more races	
		Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	0.012	-0.025	0.018	0.005	-0.059	-0.017	0.014
	(0.011)	(0.035)	(0.032)	(0.072)	(0.073)	(0.044)	(0.038)
Male	0.019	NA	NA	NA	NA	NA	NA
	(0.041)						
African American	-0.064	NA	NA	NA	NA	NA	NA
	(0.041)						
Asian	0.167	NA	NA	NA	NA	NA	NA
	(0.089)						
Other/two or more races	-0.001	NA	NA	NA	NA	NA	NA
	(0.044)						
English as preferred language	(0.048)	-0.130	0.412	-0.212	0.222	0.064	0.130
	(0.097)	(0.282)	(0.341)	(0.461)	(0.689)	(0.310)	(0.199)
Limited English ability	-0.003	-0.113	-0.269	0.461	-0.233	-0.097	-0.005
	(0.080)	(0.192)	(0.204)	(0.361)	(0.406)	(0.208)	(0.168)
In school	-0.043	0.114	0.147	0.083	-0.116	0.272	0.061
	(0.063)	(0.241)	(0.233)	(0.415)	(0.399)	(0.295)	(0.262)
Public assistance	0.063	0.004	-0.093	0.205	0.288	0.193	0.167
	(0.053)	(0.114)	(0.108)	(0.230)	(0.279)	(0.148)	(0.123)
Homeless	-0.018	-0.125	-0.308	0.199	0.028	-0.130	-0.082
	(0.082)	(0.216)	(0.198)	(0.290)	(0.388)	(0.264)	(0.210)
N	4235	891	1080	207	176	564	732

Note: Robust standard errors are in parentheses.
Source: Author's calculations based on application data provided by the City of Boston Office of Workforce Development.

Table A2. ABCD Applicant Neighborhood by Lottery Outcome.

	Selected (Treatments)	Not Selected (Controls)		Census
Total selected by random assignment	1,186	3,049		
PERCENT IN EACH CATEGORY:				
Neighborhood				
Allston/Brighton	4.9%	5.1%		7.6%
Beacon Hill/Back Bay	0.4%	0.4%		2.0%
Charlestown	2.2%	2.4%		2.2%
Chinatown	0.8%	0.5%		1.7%
Dorchester	33.4%	32.8%		24.8%
East Boston	6.7%	6.6%		7.6%
Fenway	0.0%	0.0%		6.3%
Hyde Park	6.6%	6.5%		6.5%
Jamaica Plain	4.5%	4.7%		5.2%
Mattapan	9.1%	8.9%		4.7%
Mission Hill	1.8%	2.0%		2.6%
North End	0.1%	0.1%		0.4%
Roslindale	5.7%	5.7%		4.8%
Roxbury	10.3%	10.4%		11.4%
South Boston	6.4%	6.6%		3.7%
South End	6.1%	6.3%		3.1%
West End	0.1%	0.1%		0.4%
West Roxbury	0.8%	0.9%		5.1%

Source: Based on application data provided by the City of Boston Office of Workforce Development.

Table A3. ABCD Applicant Characteristics by Lottery Outcome versus Demographics from 5-Year ACS

	Selected (Treatments)	**Not Selected (Controls)**		**5-Year ACS**
Total selected by random assignment	1,186	3,049		
PERCENT IN EACH CATEGORY:				
Age				
14-17 years	79.4%	80.2%		28.3%
18-24 years	20.6%	19.8%		71.7%
Among those Age 14-17 years:				
Gender				
Female	53.1%	53.9%		51.5%
Male	46.9%	46.1%		48.5%
Race				
African American	51.3%	54.0%		50.1%
Asian*	6.5%	5.0%		6.6%
White	9.6%	8.4%		9.5%
Other / Mixed-Race	32.5%	32.6%		33.8%

Note: Sample for 5-Year ACS are low-income households with Income in the past 12 months below poverty level.

Source: SYEP based on application data provided by the City of Boston Office of Workforce Development.
5-Year ACS is from U.S. Census Bureau, 2011-2015 American Community Survey 5-Year Estimates.

Table A4. Match and Attrition Rates for SYEP Youth in Adminsitrative Data by Treatment Status

	Treatment	Control	Total
Total number of youth applicants	1,186	3,049	4,235
In grades 8-11 at time of application	951	2,421	3,372
Youth matched in the 2014-15 school year (pre-SYEP)	933	2,336	3,269
Youth matched in both 2014-15 and 2015-16 (post-SYEP) school years	843	2,127	2,970
As a percentage of youth matched in the 2014-15 school year	90.4%	91.1%	90.9%
As a percentage of youth in grades 8-11 at time of application	88.6%	87.9%	88.1%

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

Table A5. Testing for Differential Attrition from Administrative Data by Lottery Outcome

	Applicants matched to DESE records during school year prior to participation (2014-15)		Applicants matched to DESE records for both pre (2014-15) and post (2015-16) school years	
	Effect of winning the lottery	*p* -vlaue	Effect of winning the lottery	*p* -vlaue
Age	-0.000	0.998	-0.007	0.298
Male	0.026	0.180	0.026	0.126
Black	-0.011	0.541	-0.011	0.538
White	0.039	0.224	0.033	0.327
Asian	0.071	0.064	0.082	0.039
Language Chinese	0.069	0.765	0.207	0.424
Language English	0.017	0.762	0.042	0.475
Language Spanish	0.058	0.438	0.114	0.146
Limited English Ability	-0.006	0.855	-0.016	0.645
Homeless	-0.016	0.635	-0.024	0.510
Public Assistance	0.029	0.180	0.030	0.178
Disability	-0.007	0.875	-0.021	0.670
Number of youth	3,269		2,970	
F-value, test of joint significance	1.08		1.40	

Notes: This table tests for differential attrition across the treatment and control groups by comparing estimates of the effect of winning the lottery on preexisting demographic characteristics for the sample to youth who were matched in the 2014-15 school year versus the sample to those who were also enrolled in the 2015-16 school year. The dependent variable is a binary variable which takes on a value of 1 if the individual participated in the SYEP. The SYEP indicator does not significantly predict any individual characteristics—with the exception of the one characteristic (e.g. Asian) that was noted in the earlier balance test—suggesting that overall SYEP lottery winners and losers did not differentially attrit.

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

Table A6. Testing the Effect of SYEP Participation on Attrition from Administrative Data, by Grade Level

	Coefficient on Treatment Dummy		
	(1)	(2)	(3)
Matched in both years	-0.007	0.011	0.011
	(0.011)	(0.007)	(0.007)
Grade level pre	No	Yes	Yes
Demographic characteristics	No	No	Yes
N	3,269	3,269	3,269

Note: The sample includes all youth who applied to ABCD and matched in the 2014-15 DESE files. Each coefficient is from a separate regression where the dependent variable is binary and takes on a value of 1 if youth were in the treatment group. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Robust standard errors are in parentheses.

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

Table A7. Testing for balance across baseline outcomes

	Effect of Winning the Lottery	*p* -value	N
Dropout in prior year	-0.066	0.412	2,970
Attendance rate in prior year	0.247	0.229	2,956
Attendance at or above 90% in prior year	-0.028	0.277	2,956
Days attended in prior year	0.000	0.958	2,956
Unexcused absences in prior year	0.000	0.938	2,956
Overall GPA in prior year	-0.011	0.938	2,777
Failed a course during prior year	-0.011	0.374	2,777
Failed a math course during prior year	-0.017	0.655	2,777
Failed an ELA course during prior year	-0.005	0.569	2,777
F-value, test of joint significance	0.94		

Notes: This table tests for balance by estimating the effect of the lottery indicator on individual baseline outcomes. The dependent variable is a binary variable which takes on a value of 1 if the individual participated in the SYEP. Notes that for each set of outcomes we restrict the sample to all youth with data on a given outcomes. The results confirm that there were no significant pre-existing differences in the baseline academic outcomes between youth in the treatment versus control groups as would be expected under random assignment.

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

Table A8. ABCD applicant characteristics by survey response.

	Treatment group All individuals		**Treatment group Responding to pre-/post-program survey**	
Number of youth	1,186		663	
Percent in each category:				
Age				
Mean	15.9	(0.058)	15.6	(0.083)
14-17 years	80.2%	(0.323)	80.7%	(0.013)
18-24 years	19.8%	(0.012)	19.3%	(0.013)
Gender				
Female	53.1%	(0.014)	54.7%	(0.016)
Male	46.9%	(0.014)	45.3%	(0.020)
Current education status				
In-school	87.6%	(0.010)	89.8%	(0.012)
Race				
African American	51.3%	(0.015)	54.6%	(0.016)
Asian	6.5%	(0.007)	6.5%	(0.010)
White	9.6%	(0.009)	9.9%	(0.012)
Other/Two or more races	32.5%	(0.014)	28.9%	(0.018)
Preferred language				
Chinese	0.2%	(0.001)	0.0%	(0.000)
English	95.1%	(0.006)	97.3%	(0.006)
Spanish	3.3%	(0.005)	1.7%	(0.005)
Other	1.4%	(0.003)	0.8%	(0.004)
Limited english ability				
Yes	7.1%	(0.007)	6.7%	(0.009)
Housing status				
Homeless	6.7%	(0.007)	6.4%	(0.006)
Household income type				
Public assistance	18.7%	(0.011)	17.6%	(0.015)
Disabled				
Yes	4.0%	(0.006)	3.7%	(0.037)

Source: Author's calculations based on survey data provided by the City of Boston, Office of Workforce Development.
Note: Standard errors are in parentheses. None of the differences are statistically significant.

Table A9. ABCD survey respondent characteristics by lottery outcome.

	Treatment group		Control group		Difference	
Total selected by random assignment	1,186		3,049			
Number of youth responding to survey	663		664			
Response rate	66.9%		21.8%		45.1	
Age	Mean	SE	Mean	SE	Difference	p-value
Mean	15.7	(0.078)	16.4	(0.081)	0.7	0.000
14-17 years	80.2%	(0.014)	80.6%	(0.014)	-0.40	0.492
18-24 years	19.8%	(0.012)	19.4%	(0.012)	0.40	
Gender						
Female	53.9%	(0.021)	65.2%	(0.021)	-11.38	0.000
Male	46.1%	(0.021)	34.8%	(0.021)	11.38	0.000
Race/ethnic group						
African American	51.5%	(0.021)	48.9%	(0.021)	2.63	0.808
Asian	6.5%	(0.010)	12.0%	(0.014)	-5.53	0.001
White	3.2%	(0.007)	9.2%	(0.012)	-5.99	0.000
Other/Two or more races	36.1%	(0.020)	26.8%	(0.019)	9.33	0.001
Living situation						
Single parent family	63.7%	(0.020)	57.6%	(0.021)	6.17	0.036
Two parent family	29.4%	(0.019)	37.8%	(0.021)	-8.38	0.003
Other relative	8.1%	(0.012)	10.7%	(0.013)	-2.62	0.136
Other	6.3%	(0.010)	4.4%	(0.009)	1.86	0.173
Language spoken at home						
Chinese	3.9%	(0.008)	5.5%	(0.010)	-1.59	0.216
English	74.0%	(0.019)	70.3%	(0.020)	3.67	0.175
Spanish	18.5%	(0.016)	10.7%	(0.013)	7.79	0.000
Other	3.6%	(0.008)	13.5%	(0.015)	-9.88	0.000

Source: Survey data provided by the City of Boston, Office of Workforce Development.

Table A10. Item non-response rates for post-program survey: SYEP treatment group versus control group

CATEGORY	Treatment group (N=663)	Control group (N=664)	Difference (Percentage point)
Community engagement and social skills			
I have a lot to contribute to the groups I belong to	1.2%	0.9%	0.30
I feel connected to people in my neighborhood	1.2%	0.9%	0.30
I know how to manage my emotions and my temper	1.1%	1.5%	-0.45
I know how to ask for help when I need it	0.8%	0.6%	0.15
I know how to constructively resolve a conflict with a peer	0.6%	0.6%	0.00
I need to improve my conflict resolution skills	1.8%	1.8%	0.00
Job readiness skills			
I have all key information to apply for a job	2.3%	2.9%	-0.60
I have prepared a resume	2.6%	2.6%	0.00
I have prepared a cover letter	2.9%	3.0%	-0.15
I have developed answers to the usual interview questions	2.9%	2.7%	0.15
I have practiced my interviewing skills with an adult	3.9%	3.5%	0.46
I need to improve my job readiness skills	1.8%	1.8%	0.00
Future work plans and academic aspirations			
Plan to enroll in any eduation or training program after high school	2.4%	3.0%	-0.60

Source: Author's calculations based on survey data provided by the City of Boston Office of Workforce Development.

Note: None of the differences were statistically significant.

Table A11. Treatment-on-the Treated Estimates from Two Stage Least Squares Regressions

	First stage	Second stage				
	Depvar=Participation dummy	Attendance rate	Attendance>=90%	Uexcused absences	Dropped out ever	Graduated on time
Participation dummy	-----	0.022	0.071	-2.110	-0.037	0.066
	-----	(0.007)	(0.020)	(0.891)	-0.013	-0.026
SYEP treatment dummy	0.847	-----	-----	-----	-----	-----
	(0.008)	-----	-----	-----	-----	-----
Other SYEP participation	0.007	0.007	0.020	-0.004	-0.020	0.062
	(0.011)	(0.008)	(0.023)	(1.013)	(0.015)	(0.025)
Baseline outcome (e.g. attendance rate in prior year)	-0.008	0.568	0.533	0.633	0.089	-----
	(0.029)	(0.022)	(0.017)	(0.021)	(0.048)	-----
Age	0.001	-0.028	-0.046	1.408	0.084	-0.155
	(0.004)	(0.003)	(0.009)	(0.389)	(0.006)	(0.011)
Male	0.010	-0.011	-0.012	1.213	0.032	-0.045
	(0.007)	(0.006)	(0.015)	(0.671)	(0.010)	(0.019)
Black	0.005	-0.006	-0.002	2.369	0.012	-0.005
	(0.014)	(0.011)	(0.028)	(1.275)	(0.019)	(0.035)
Asian	-0.030	0.023	0.105	-1.665	-0.024	0.121
	(0.020)	(0.015)	(0.040)	(1.799)	(0.027)	(0.048)
Other/mixed race	-0.018	-0.026	-0.042	4.209	0.031	-0.028
	(0.014)	(0.011)	(0.029)	(1.312)	(0.020)	(0.037)
Limited English	-0.013	0.032	0.028	-0.865	-0.044	-0.008
	(0.015)	(0.011)	(0.029)	(1.318)	(0.020)	(0.037)
Homeless	0.020	-0.010	-0.019	0.576	0.038	-0.088
	(0.017)	(0.013)	(0.034)	(1.521)	(0.023)	(0.042)
Public assistance	-0.007	0.001	-0.037	0.518	0.014	-0.033
	(0.010)	(0.007)	(0.020)	(0.891)	(0.013)	(0.025)
Disabled	0.035	0.014	0.026	-2.062	-0.044	-0.002
	(0.022)	(0.017)	(0.045)	(2.013)	(0.030)	(0.057)
High need special education status	-0.016	-0.043	-0.062	5.613	0.054	-0.122
	(0.016)	(0.012)	(0.033)	(1.477)	(0.022)	(0.041)
METCO program	-0.022	0.018	0.049	-1.476	-0.022	0.187
	(0.016)	(0.012)	(0.033)	(1.478)	(0.022)	(0.039)
BPS student	-0.013	-0.001	-0.018	2.927	0.013	0.045
	(0.010)	(0.007)	(0.019)	(0.877)	(0.013)	(0.024)
Switched schools across years	0.017	0.015	0.049	0.305	0.044	-0.211
	(0.010)	(0.008)	(0.021)	(0.935)	(0.014)	(0.029)
Switched schools within the school year	-0.014	-0.049	-0.070	-2.302	-0.011	-0.040
	(0.014)	(0.011)	(0.028)	(1.257)	(0.019)	(0.049)
Grade 9	0.013	-0.089	-0.185	7.548	0.211	-0.708
	(0.017)	(0.013)	(0.034)	(1.552)	(0.023)	(0.045)
Grade 10	0.004	(0.050)	(0.106)	5.309	0.160	-0.371
	(0.014)	(0.011)	(0.029)	(1.309)	(0.020)	(0.034)
Grade 11	-0.006	-0.027	-0.060	4.518	0.051	-0.130
	(0.013)	(0.010)	(0.027)	(1.214)	(0.018)	(0.030)
Constant	-0.001	0.858	1.096	-24.457	-1.394	3.406
	(0.085)	(0.064)	(0.153)	(6.726)	(0.099)	(0.186)
F-statistic	561.260					
Number of Observations	2,852	2,852	2,852	2,852	2,970	1,953

Notes: The sample includes youth who were matched in 2014-15 and 2015-16 who were members of schools in Massachusetts for between 0 and 190 days in both years. Each coefficient is from a separate regression where the dependent variable is the outcome listed and includes demographic characteristics (age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status) and academic characteristics (grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years). Probit is used to estimate results for binary outcomes. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

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

Table A12. Relationship Between Short-Term Behavioral Changes and SYEP Impact on Academic Outcomes: TOT Estimates

	(1)		(2)		(3)		(4)		(5)	
	Attendance rate		Attendance rate>=90%		Unexcused absences		Dropped out ever		Graduated on time	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Academic aspirations										
Planning to attend a four-year college	0.005	(0.016)	0.032	(0.041)	-0.619	(1.858)	0.014	(0.028)	-0.045	(0.055)
Saving for tuition	0.044	(0.021)	0.016	(0.083)	-6.410	(3.735)	-0.090	(0.054)	0.121	(0.105)
Job readiness skills										
Having key information to apply for a job	-0.007	(0.014)	0.047	(0.038)	-1.264	(1.686)	0.045	(0.025)	-0.056	(0.052)
Preparing a resume	0.014	(0.013)	0.013	(0.390)	-2.600	(1.495)	0.006	(0.023)	0.036	(0.044)
Preparing a cover letter	0.014	(0.014)	0.034	(0.036)	-2.965	(1.632)	0.030	(0.025)	0.012	(0.047)
Developing answers to interview questions	-0.013	(0.013)	0.045	(0.036)	-0.819	(1.610)	0.034	(0.024)	-0.018	(0.048)
Practicing interviewing with an adult	0.005	(0.014)	0.030	(0.036)	-1.447	(1.627)	0.010	(0.025)	0.010	(0.048)
Being on time	0.018	(0.009)	0.047	(0.022)	-2.642	(1.351)	-0.036	(0.022)	0.086	(0.043)
Keeping a schedule	0.022	(0.010)	0.069	(0.032)	-2.290	(1.349)	-0.020	(0.022)	0.078	(0.043)
Community engagement and social skills										
Contributing to the groups they belong to	0.015	(0.015)	-0.013	(0.041)	-1.881	(1.823)	0.015	(0.017)	0.110	(0.053)
Connecting to people in their neighborhood	0.010	(0.016)	0.038	(0.042)	-2.745	(1.889)	0.004	(0.018)	0.094	(0.056)
Managing emotions	0.016	(0.018)	-0.018	(0.047)	-0.931	(2.113)	0.019	(0.020)	0.109	(0.066)
Asking for help	0.011	(0.017)	0.011	(0.044)	-2.293	(1.970)	0.007	(0.018)	0.098	(0.059)
Gaining a mentor	0.011	(0.008)	-0.008	(0.033)	-2.122	(1.493)	0.012	(0.014)	0.080	(0.045)
Resolving conflict with a peer	-0.001	(0.016)	-0.032	(0.043)	0.730	(1.931)	0.002	(0.018)	0.030	(0.057)
Other SYEP participation	Yes		Yes		Yes		Yes		Yes	
Demographic characteristics	Yes		Yes		Yes		Yes		Yes	
Academic characteristics	Yes		Yes		Yes		Yes		Yes	
Baseline outcomes	Yes		Yes		Yes		Yes		Yes	
Number of youth	2,852		2,852		2,852		2,970		1,953	

Notes: The sample includes youth who were matched in 2014-15 and 2015-16 who were members of schools in Massachusetts for between 0 and 190 days in both years. Each coefficient is from a separate regression where the dependent variable is the outcome listed and includes demographic characteristics (age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status) and academic characteristics (grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years). Probit is used to estimate results for binary outcomes. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

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

Table A13. Relationship between short-term behavioral changes and SYEP impact on academic outcomes Participants Only

	(1)		(2)		(3)		(4)		(5)	
	Attendance rate		Attendance rate>=90%		Unexcused absences		Dropped out ever		Graduated on time	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Academic aspirations										
Planning to attend a four-year college	0.003	(0.013)	0.029	(0.042)	-1.188	(2.095)	0.006	(0.024)	-0.028	(0.052)
Saving for tuition	0.046	(0.021)	-0.003	(0.094)	-7.909	(3.651)	-----		0.189	(0.100)
Job readiness skills										
Having key information to apply for a job	-0.004	(0.014)	0.057	(0.037)	-1.287	(1.683)	0.032	(0.020)	-0.023	(0.046)
Preparing a resume	0.018	(0.012)	0.025	(0.032)	-2.420	(1.421)	0.001	(0.020)	0.055	(0.041)
Preparing a cover letter	0.012	(0.013)	0.041	(0.035)	-2.726	(1.626)	0.024	(0.020)	0.038	(0.044)
Developing answers to interview questions	-0.007	(0.014)	0.053	(0.036)	-1.123	(1.594)	0.027	(0.019)	0.004	(0.044)
Practicing interviewing with an adult	0.007	(0.012)	0.047	(0.035)	-0.891	(1.474)	0.006	(0.021)	0.033	(0.044)
Being on time	0.027	(0.010)	0.078	(0.031)	-2.227	(1.334)	-0.052	(0.020)	0.129	(0.037)
Keeping a schedule	0.029	(0.009)	0.089	(0.030)	-1.644	(1.299)	-0.031	(0.019)	0.081	(0.038)
Community engagement and social skills										
Contributing to the groups they belong to	0.016	(0.012)	-0.001	(0.040)	-2.234	(1.827)	-0.043	(0.028)	0.126	(0.052)
Connecting to people in their neighborhood	0.014	(0.013)	0.051	(0.041)	-2.783	(2.135)	-0.011	(0.027)	0.113	(0.055)
Managing emotions	0.017	(0.012)	-0.013	(0.048)	-1.130	(1.790)	-0.074	(0.039)	0.155	(0.060)
Asking for help	0.020	(0.012)	0.027	(0.045)	-3.875	(2.142)	-0.017	(0.028)	0.138	(0.058)
Gaining a mentor	0.017	(0.011)	0.006	(0.030)	-2.858	(1.365)	-0.029	(0.018)	0.116	(0.036)
Resolving conflict with a peer	0.004	(0.011)	-0.016	(0.042)	0.244	(1.654)	0.001	(0.026)	0.057	(0.054)
Other SYEP participation	Yes		Yes		Yes		Yes		Yes	
Demographic characteristics	Yes		Yes		Yes		Yes		Yes	
Academic characteristics	Yes		Yes		Yes		Yes		Yes	
Baseline outcomes	Yes		Yes		Yes		Yes		Yes	
Number of youth	663		663		663		663		663	

Notes: The sample includes youth who were matched in 2014-15 and 2015-16 who were members of schools in Massachusetts for between 0 and 190 days in both years. Each coefficient is from a separate regression where the dependent variable is the outcome listed and includes demographic characteristics (age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status) and academic characteristics (grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years). Probit is used to estimate results for binary outcomes. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

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