

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

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**Abstract:** Policymakers in cities across the U.S. have increasingly turned to summer youth employment programs (SYEPs) to provide early work experiences to inner-city, low-income youth with the goal of improving long-term behavioral, economic, and academic outcomes. The impact of SYEPs on long-term academic outcomes can have far-reaching consequences as greater exposure to employment provides youth with experiences that can both shape their aspirations—to complete high school, obtain career training, or attend college—as well as offer opportunities to apply academic concepts, learn work-related skills, and transition from school to the labor force. An important limitation of prior work has been a lack of information on the mechanisms driving these improved outcomes as well as the heterogeneity in outcomes across groups of at-risk youth. This paper adds to the literature by evaluating the effects of the Boston SYEP on both short-term behavioral impacts as well as longer-term academic outcomes to better understand what factors lead to impacts, for whom the benefits are the greatest, and how these elements can be applied consistently across intermediaries. Using an embedded randomized controlled trial (RCT), we find that the Boston SYEP has a significant impact on improving attendance and test-taking but no detectable impact on course performance, test scores, or disciplinary incidents. Youth who were randomly selected into the SYEP treatment group experienced significant improvements in reducing unexcused absences (-1.9 days) and achieving an attendance rate of greater than 85 percent (+2.9 percentage points) relative to the control group that provide a meaningful improvement in reducing chronic absenteeism relative to baseline (27 percent). Moreover, these medium-term attendance outcomes appear to be linked to improvements in social skills and academic aspirations among participants that occur during the summer, as measured by a pre-/post-program survey, and are greater in magnitude for older and “at-risk” youth. These results give policymakers some insight into the broader set of short-term program effects while also providing a look inside the “black box” as to how SYEPs affect youth in the long-run and which youth benefit the most.

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## 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 (National Center for Education Statistics 2017). Recently, chronic absenteeism among low-income and at-risk youth has been highlighted as a serious challenge for policies aimed at improving academic performance among these groups (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 & 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, increased risk of drop-out (Mac Iver & Mac Iver 2010), and reduced rates of post-secondary enrollment (Balfanz & Byrnes 2012). Chronic absenteeism in even a single year between 8th and 12th grade is associated with a seven-fold increase in the likelihood of dropping out among public school students (Utah Education Policy Center 2012). In turn, high school dropout has been linked to poor outcomes later in life, from poverty and diminished health to involvement in the criminal justice system (Bjerk 2012).

In response, policymakers have been seeking ways to reduce learning loss among low-income and at-risk youth when school is out of session, especially during the summer (Alexander, Olson, & Entwisle, 2007; Cooper et al., 1996; Duncan & Murnane, 2011; Ladd, 2012). Although recent experimental research examining the impact of other policy efforts aimed at boosting school attendance indicates that modest increases in attendance are possible using cash transfers/penalties (Dee, 2011; Riccio et al., 2010) and early warning systems (Faria et al. 2017). Yet, these interventions have not achieved sufficient scale or scope to affect youth across multiple school districts or geographical areas. For example, although a randomized controlled

trial demonstrated that the Early Warning Intervention and Monitoring System (EWIMS) was shown to reduce the percentages of students with chronic absences and course failures, it was challenging for schools to implement with only two schools employing the full model and eight schools stopping the use of EWIMS during the study (Faria et. al. 2017).

This paper provides experimental evidence regarding the impact of large-scale Summer Youth Employment Programs (SYEPs) on high school students' school attendance and academic performance during the school year after participation. Early work experience, such as that provided by SYEPs, can help students to develop non-cognitive skills that are important for success (Heckman 2000, Lillydahl 1990, Mortimer 2003, Duckworth et al, 2007), possibly increasing attendance and reducing the likelihood of dropout (Jackson 2012). In addition, SYEPs may also raise career and academic aspirations that can lead to better schooling outcomes, particularly for low-income and at-risk youth living in neighborhoods with high crime rates and few job opportunities. Indeed, mayors in cities such as Boston and Chicago are seeking to use SYEPs to provide meaningful employment experiences that can lead to alternative pathways for inner-city youth—whether it be a career or some type of postsecondary education. And because SYEPs take place during the summer when youth are often idle, these interventions are less likely to interfere with academic and extracurricular activities compared to employment experiences that occur during the school year.

Moreover, SYEPs continue to be important vehicles for employing youth in high-poverty and high-crime neighborhoods even as the economy has recovered from the Great Recession. With just under one-third of U.S. teens aged 16 to 19 years currently working, youth employment rates remain just shy of their pre-recession levels and are far below the 40 percent threshold that prevailed up until the 2000-01 recession (see Figure 1). Employment rates are

even lower among non-white teens from low-income families living in high-poverty neighborhoods (Sum et al., 2014). In addition, 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—especially for those not enrolling in college (Dennett & Modestino, 2013). Indeed, postsecondary credentials—whether it be a certificate, an associate degree, or a bachelor’s degree—have become a requirement for many jobs that previously required only a high-school degree (Modestino, Shoag & Ballance, 2014). At the same time, employer expectations for work readiness, communication, and other soft skills have risen—qualifications that are difficult for youth to demonstrate without a track record of work experience (Harrington et al., 2013). Together, these hurdles make it hard for many young people, particularly those with weak school and work records, to enter and move up in the labor market.

Although SYEPs have the potential to enhance youth outcomes along several dimensions, only a handful of studies have evaluated such programs in a rigorous manner. Thus far, the literature has focused on longer-term outcomes captured by administrative data on criminal activity, employment and earnings, and academic outcomes (Sachdev, 2011; Gelber, Isen, & Kessler, 2014; Leos-Urbel, 2014; Heller, 2014; Schwartz, Leos-Urbel, & Wiswall, 2015; Valentine et al. 2017; Davis & Heller 2017a, 2017b). While the results of this research have demonstrated encouraging results in some cities, to date they have shed little light on the mechanisms driving these improved outcomes.

This paper fills some of those gaps by evaluating the impact of the Boston SYEP on both short-term behavioral changes in skills and attitudes as well as medium-term academic outcomes to better understand how these impacts are achieved and for whom the benefits are the greatest. Using an embedded randomized controlled trial (RCT), we find that the Boston SYEP has a

significant impact on improving attendance but little evidence of improvements in academic performance. Youth who were randomly selected into the SYEP treatment group experienced significant improvements in reducing unexcused absences (-1.9 days) and achieving an attendance rate of greater than 85 percent (+2.9 percentage points) relative to the control group that provide a meaningful improvement in reducing chronic absenteeism relative to baseline (27 percent). We also find modest impacts on reducing course failures and increasing standardized test-taking, but no impact on disciplinary incidents. Moreover, these medium-term academic outcomes appear to be linked to improvements in social skills and academic aspirations among participants that occur during the summer, as measured by a pre-/post-program survey, and are greater in magnitude for older and “at-risk” youth. These results give policymakers some insight into the broader set of short-term program effects while also providing a look inside the “black box” as to how SYEPs affect youth in the long-run and which youth benefit the most.

This paper is organized as follows: In the first two sections, we provide an overview of the relevant literature and policy context. Next, we describe the Boston SYEP and the experimental design and explain the data and methodology that we use to evaluate program outcomes. We then estimate the impact of the program on the medium-term academic outcomes as well as the short-term behavioral changes in skills and attitudes and analyze the relationship between the two. Finally, we conclude with a discussion of the policy implications and future research.

## **RELEVANT LITERATURE**

This paper contributes to the existing evidence on the impacts of early work experience both in general and in terms of the specific experience provided by summer jobs programs. Prior studies of year-round workforce development programs aimed at youth and young adults have

provided mixed results. Often these earlier initiatives failed to improve employment without very high levels of investment, suggesting that other interventions could be more effective and efficient at achieving the same goals (Cave et al., 1993; Bloom et al., 1997; Uggen, 2000; Schochet, Burghardt, & McConnell, 2008; Milenky et al., 2011).

Yet summer jobs programs differ from these earlier programs in several important ways. First, SYEPs primarily serve younger youth who are more likely to still be enrolled in school and less likely to have already held a job. As such, SYEP may act as a preventive measure compared to previous youth employment programs that were targeted at “opportunity” youth who had already dropped out of school and were struggling in the labor market. Second, 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.

Finally, SYEPs occur in summer months when youth are often idle, creating fewer conflicts with academic studies compared to year-round employment programs. Indeed, the evidence regarding the impacts of early work experience on academic performance during the school year is mixed. When students work too many hours, this ultimately decreases high school graduation and college attendance rates and inhibits later economic success (Mortimer, 2010; Stasz & 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 & Briggs, 2001). However, by earning steady income over the summer, SYEPs may in fact reduce the number of hours a student needs to work during the school year and in turn increase time devoted to academics.<sup>1</sup>

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<sup>1</sup> On the other hand, SYEPs could negatively affect educational outcomes if the work experience that students gain during the summer leads them to work more during the school year and focus less on school or forego education

## How Might SYEPs Improve Academic Outcomes?

A variety of rationales are often cited in support of summer jobs programs. Many of these stem from the belief that early work experience has the potential to improve future academic, employment, and criminal justice outcomes down the road. In this paper, we examine how SYEPs improve academic outcomes such as attendance, course performance, test scores, and disciplinary incidents. There are four primary channels through which SYEPs are thought to improve academic outcomes:

- (1) *Improving behaviors correlated with academic success.* By placing youth in jobs that are supported by mentors and program staff, SYEPs help develop strong, supportive, and sustained relationships with adults and peers that are critical as youth move from adolescence into adulthood relationships (Nagaoka et al., 2015). In addition, the types of early work experience provided by SYEPs gives participants the opportunity to engage in tasks that help them develop the sense of agency, identity, and competency necessary for adult roles and success. Some SYEPs, including the Boston program, also offer programming aimed at improving non-cognitive skills such as responsibility, positive work habits, motivation, time management, determination, self-confidence, and “grit” that are important for success (Heckman 2000, Lillydahl 1990, Mortimer 2003, Duckworth et al, 2007), possibly increasing attendance and reducing the likelihood of dropout (Jackson, 2012).
- (2) *Increasing career and academic aspirations.* Early work experience can also improve current job readiness skills as well as raise career and academic aspirations—both of which can lead to better long-term academic outcomes, particularly for disadvantaged youth living in neighborhoods with few job opportunities. Labor force attachment at an early stage in one’s

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altogether in favor of employment. However, unlike private sector jobs, jobs obtained through the Boston SYEP are subsidized by the Mayor’s Office of Workforce Development and end when the summer is over.

career typically predicts better labor market outcomes in terms of both employment and earnings later in life (Carr, Wright, & Brody, 1996; Baum & Ruhm, 2014). In addition, greater exposure to employment gives youth experiences that can shape their goals—whether it be to complete high school, obtain career training, or attend college (Duckworth et al., 2007; Heckman, 2008; Lillydahl, 1990; Mortimer, 2010). The Boston SYEP curriculum also focuses on developing work-readiness skills such as exploring careers, writing a resume and cover letter, searching for jobs, completing online applications, and interviewing.

(3) *Reducing opportunities to engage in delinquent behavior.* Summer jobs programs may “incapacitate” youth by limiting their time to engage in delinquent activity or by disrupting “routine activities” that provide likely offenders with suitable targets and a lack of supervision or guardianship (Cohen & Felson, 1979; Heller, 2014; Modestino, 2018). 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 disciplinary actions such as suspension (Wilson, 1996).

(4) *Providing direct income support to youth and their families.* Wages earned from employment in the program can help reduce poverty and provide resources that lead to better outcomes.<sup>2</sup> In addition, by providing youth with a steady source of income, SYEPs may increase the motivation for youth to save for post-secondary education. The income channel may be particularly important for youth as employment rates for this population have been declining relative to that of other age groups.

Understanding the mechanisms by which the summer jobs program can lead to better

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<sup>2</sup> 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.

outcomes down the road is important for both policymakers and practitioners to maximize resource allocation. We will explore these channels by examining how the Boston SYEP affects youth behaviors during the summer and whether these short-term improvements are correlated with medium-term improvements in academic outcomes during the year following participation.

### **Summer Jobs Programs: What Do We Know?**

Although SYEPs have the potential to enhance youth outcomes along several dimensions, researchers have only recently focused on evaluating early work experiences provided by summer jobs programs. These studies typically use an RCT design to compare impacts for youth that were randomly selected into the program to youth that applied but were not selected. In terms of academic outcomes, the results are somewhat mixed but encouraging. For example, the New York City SYEP is associated with modest improvements in test taking and school attendance, but not high school graduation or college matriculation. 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, with larger improvements for students aged 16 years and older with prior low baseline attendance. Schwartz, Leos-Urbel, & Wiswall (2015) find small but significant increases in the share of SYEP participants taking and passing statewide high school exams relative to the control group. However, other research indicates that the program 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, & Kessler, 2014).

More consistent SYEP impacts have been found in terms of criminal justice outcomes. Heller (2014) finds that participating in Chicago's One Summer Plus program decreased violent crime for youth in the treatment group by 43 percent over 16 months relative to the control group, with

much of the decline occurring during the year after participation. Similarly, Modestino (2018) finds that the Boston SYEP reduces the number of arraignments for violent (-35 percent) and property (-29 percent) crimes among youth in the treatment group relative to the control group during the 17 months after participation. Finally, Gelber, Isen, & Kessler (2014) use an embedded RCT to show that participating in the New York City SYEP reduced the probability of incarceration and mortality from “external causes,” including homicides, suicides, and accidents.

Several studies examine the link between summer jobs programs and subsequent employment and earnings but find little evidence of permanent improvement associated with SYEPs. Two studies find that the New York City SYEP increases average earnings and the probability of employment during the program, but also that these effects subsequently faded (Gelber, Isen, & Kessler, 2014, Valentine et al., 2017). Another study using machine-learning to identify sub-group impacts in Chicago finds that employment improved for only a subset of SYEP participants; this group was younger, more likely to be Hispanic, female and enrolled in school, and less likely to have an arrest record (Davis & Heller 2017a).

While the results of this research have demonstrated encouraging results in some cities—particularly for criminal justice outcomes—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 academic outcomes to shed light on what works for whom, under what conditions, and why.

## **THE BOSTON SYEP INTERVENTION**

Introduced in the early 1980s, the Boston SYEP 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. Participants work a maximum of 25 hours per week for a six-week period starting in early July through mid-August and are paid the Massachusetts minimum wage. Youth may be 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. In addition, the Boston SYEP provides 20 hours of job-readiness training using a hands-on, competency-based work-readiness curriculum. Modules include 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, complete an online application, and answer typical interview questions.<sup>3</sup>

All Boston city residents aged 14 to 24 years are eligible for the program and youth apply through one of the four intermediaries under contract with the Boston Mayor's Office of Workforce Development (OWD). Youth typically apply to the intermediary in their immediate neighborhood, and administrative data indicate that less than 5 percent apply to more than one agency. The intermediaries are responsible for reviewing applications, matching applicants with jobs, supervising placements, and delivering the program's career-readiness curriculum.

This analysis is restricted to youth who applied to the program for summer 2015 through Action for Boston Community Development (ABCD), a large and established nonprofit that works in all of Boston's 18 neighborhoods. ABCD is one of the two intermediaries that make use of random assignment because of the high number of applications it receives for the limited number of SYEP jobs available.<sup>4</sup> The enrollment period typically spans February through June,

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<sup>3</sup> The curriculum, Signal Success, was developed by the Commonwealth Corporation, a state agency, and is currently being piloted as part of the regular high school course offerings in both Lowell and Malden.

<sup>4</sup> 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.

and applicants are notified of their lottery status and job assignment in late June. 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 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. 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 of the program.

Table 1 provides descriptive statistics for the preexisting characteristics of SYEP lottery applicants collected by ABCD, which reflects a predominately low-income school-aged population.<sup>5</sup> On average, approximately 88 percent of applicants were in school at the time they applied, with a mean age just shy of 16 years. A slightly higher percentage of applicants were female, and just over 50 percent were African American. Although over 95 percent indicated that their preferred language was English, 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.<sup>6</sup> Less than 5 percent listed themselves as having a disability.

Based on these observable characteristics, the youth selected by the ABCD lottery appear to be almost identical to those not selected, confirming that the lottery is indeed random. The one

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<sup>5</sup> Table A1 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). Finally, ABCD applicants have similar gender and racial characteristics in comparison to the population of low-income Boston youth (see Table A2).

<sup>6</sup> 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.

statistically significant difference is the share of Asian youth being slightly higher (7 percent) in the treatment group versus the control group (5 percent) (see Table 1). We note that having at least one statistically significant difference at the  $p < 0.10$  level would be expected by random chance when testing 15 different characteristics. The sample is similarly balanced among the school-aged population.<sup>7</sup>

To provide some indication as to whether the Boston SYEP provides a meaningful intervention in terms of employment, Figure 2 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. Note that only 26.4 percent of those in the control group responding to the survey had worked during the summer, indicating their comparative inability to secure jobs even with Boston's relatively low unemployment rate of 4.4 percent in July 2015.<sup>8</sup> Survey respondents indicated that youth in the control group who found a job worked fewer hours per week than SYEP participants (panel A), but had more variation in the types of daily work they did; in comparison, over half of SYEP participants worked at a day care or day camp (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 (panel C). Although self-reported, these experiences suggest that the Boston SYEP provided a meaningful intervention in terms of the likelihood, intensity, and type of employment obtained. The next question is whether the Boston SYEP had any meaningful

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<sup>7</sup> We test for baseline equivalence using separate models estimating the effect of winning the lottery on preexisting applicant characteristics among school-aged youth for gender/race groupings (see Table A3).

<sup>8</sup> Quarterly wage record data provided by the Massachusetts Division of Unemployment Assistance recorded a similar proportion of youth in the control group (28.2 percent) as having worked during the third quarter (July-September) of 2015.

positive impacts on youth academic performance.

## **DATA AND EMPIRICAL METHDOLOGY**

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; Bacalod & Hotz, 2006). To address this potential bias, we rely on an embedded RCT that effectively controls for selection while also accounting for changes that might occur during the normal course of adolescent development. The first phase of the analysis uses administrative data during the school year following the intervention (2015-16) to assess SYEP impacts on medium-term academic outcomes. The second phase of the analysis uses survey data on self-reported 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 academic performance.

While some observers question whether a six-week intervention can provide a meaningful turning point to affect youth life-course development, such impacts may be greater for at-risk youth (Sampson & 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, & Katz, 2016). As such, we 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, those with limited English skills, and at-risk youth defined as homeless or receiving public assistance (Utah Education Policy Center 2012).

## **Using Administrative Data to Assess SYEP Impacts on Academic 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 academic outcomes including attendance, course grades, statewide test scores, disciplinary actions, high school graduation, and college matriculation. The benefit of using administrative data, even with their limitations, 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 less than a perfect match. This can be particularly problematic when using school record data since individuals may drop out of school at age 16. As such, the sample for this study is limited to SYEP applicants who were students in both the 2014-15 and 2015-16 school years, the population represented in the education data used for this analysis. Therefore, this analysis excludes SYEP applicants who had dropped out of school, graduated from high school or completed a general equivalency diploma, or attended college at the time of application to SYEP. Unlike previous studies, our sample does include students in both public as well as private and parochial schools, which we will control for in the analysis.

Since the individual-level SYEP and DOE files do not share a unique common student identification number, they were matched based on student name and birth date. Of the original sample, Table 2 shows that 76.6 percent were in school and in grades 8-11 during the 2014-15

school year before applying to the summer jobs program. Of these, 78.6 percent were matched to the 2014-15 DOE file and 76.4 percent were matched to the 2015-16 DOE file.<sup>9</sup> However, only 69.3 percent could be matched to the DOE data in both years. The match rate was similar for students selected by the SYEP lottery (67.7 percent) and those not selected (70.0 percent), and the sample is again balanced across both groups with only one significant difference (the percentage Asian) as would be expected by chance.

Table 3 shows that there was no significant pre-existing difference in the baseline academic outcomes between youth in the treatment versus control groups, with one exception. In the 2014-15 school year, youth in the treatment group had a slightly higher incidence of having a disciplinary incident (0.18) compared to the control group (0.15). Otherwise, during the school year prior to applying to the Boston SYEP, the two groups had similar numbers of disciplinary incidents and days suspended. In addition, the attendance rate was nearly identical across the treatment (92.3) and control group (91.6).<sup>10</sup> Moreover, there were no significant differences in course performance as measured by GPA or course failures, overall or separately for math and English classes.

To assess the impact of the Boston SYEP on academic outcomes, we compare attendance, course performance, disciplinary incidents, and MCAS test score 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

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<sup>9</sup> This is similar to the match rate (77 percent) found by Leos-Urbel (2014) for the New York City summer jobs program. We hope to improve the match in the future by using a combination of fuzzy matching and hand matching.

<sup>10</sup> This is significantly higher than what Leos-Urbel (2014) finds for the New York SYEP where youth had an average attendance rate of 82 percent for both the treatment and control groups. However, the administrative data in that paper cover only public school records and likely reflects a school population with low attendance rates.

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 take-up to assess the degree to which SYEP could improve academic outcomes among the pool of applicants, not just the participants. 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 in the online appendix.

We measure multiple outcomes of interest during the post-intervention period within each domain: attendance, course performance, disciplinary incidents, and MCAS test scores. 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 a regression framework to control for individual characteristics and improve the precision of my estimates using equation (1):

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

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, and  $\mu_{it}$  is a stochastic error term. We use both OLS as well as alternative nonlinear methods to relax the linear functional form assumption.<sup>11</sup>

### **Using Survey Data to Explore SYEP Program Mechanisms**

To explore program mechanisms, we link the academic outcomes described above to the short-term behavioral changes in skills and attitudes observed during the summer for the

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<sup>11</sup> To analyze differences in the number of incidents—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.

treatment group, as measured by a pre-/post-program survey. The survey was originally developed by the Youth Violence Prevention Collaborative to measure individual behaviors correlated with youth violence. We built on this original framework to expand the survey's content, adding questions related to job readiness as well as postsecondary aspirations. Whereas the first part of the analysis using administrative data establishes the causal impacts of the Boston SYEP on academic 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 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 entire treatment group versus those responding to both the pre- and post-

survey (see Table A4). 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. Even so, the two groups were randomly selected, so 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 (see Table A5). We argue that the selection bias goes *against* finding an impact for the Boston SYEP, given that the survey respondents in the control group exhibit characteristics that are on average associated with *better* economic, academic, and criminal justice outcomes.<sup>12</sup>

Nonetheless, to minimize selection bias due to survey response rates, we also control for observable characteristics using equation (2):

$$M_{it} = SYEP_{it} \pi_2 + X_{it} \beta_2 + \mu_{it2} \quad (2)$$

where  $M_{it}$  is one of the short-term program outcomes (e.g., social skills),  $SYEP_{it}$  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. Because the selection among survey respondents in the control group is correlated with better outcomes, the coefficient  $\pi_2$  is likely to provide downward-biased estimates of the program's impact on short-term behavioral outcomes.

Finally, it is important to acknowledge the other limitations of self-reported survey data

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<sup>12</sup> Higher employment rates are observed among females, whites, and older youth (Child Trends, 2017). Females are also more likely than males to attend college (Hugo Lopez and Gonzalez-Barrera, 2014) and standardized test scores are lower among African-American children and those living in single parent households, (Jencks and Phillips, 1998). 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).

such as those raised in Meyer, Mok, and Sullivan (2015). For example, self-reported data is subject to measurement error. However, if we assume that the measurement error is random across the treatment and control groups, then this would reduce efficiency but not cause bias. In addition, the degree of item non-response for the survey questions used in the analysis was less than 5 percent for both the treatment and control groups.

*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 (Baron & Kenny 1986, 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 academic outcomes to shed light on the program’s mechanisms. To do this, we modify equation (1):

$$Y_{it} = SYEP_i \pi_3 + X_{i(t-1)} \beta_3 + \Delta M_i \delta + \mu_{it3} \tag{3}$$

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 behavioral impact  $\Delta M_i$  (e.g., ability to resolve conflicts with a peer). A positive and significant coefficient on  $\Delta M_i$  indicates that improvement in the short-term behavioral impact observed during the summer of participation is positively correlated with the subsequent improvement in academic outcomes, such as attendance. Moreover, if the coefficient on the SYEP<sub>i</sub> dummy in equation (3) is smaller in magnitude than that in equation (1), this would suggest that  $\Delta M_i$  plays a role in improving

attendance separate from simply being assigned to treatment. We also test whether these same short-term program measures are driving the improvements in attendance when the sample is restricted to only participants completing both surveys.

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).

## **RESULTS**

### **Assessing SYEP Impacts on Academic Outcomes Using Administrative Data**

We find that the Boston SYEP has a significant impact on improving attendance among high school students, but little detectable impact on course performance, disciplinary incidents, or MCAS test scores. We test the program's impact on each of these outcomes separately in Table 4 which presents the ITT estimates of the difference between the treatment and control group for various specifications. The first column of Panel A shows the raw difference with no controls and indicates that the attendance rate in the year following the summer jobs program was 1.3 percentage points higher for students in the treatment versus the control group. The impact resulted in a meaningful improvement in attendance with the treatment group being 2.9 percentage points more likely than the control group to have attended at least 85 percent of the school year—below which attendance is considered marginal. This reduction in chronic

absenteeism (-27 percent relative to baseline) is similar to recent efforts such as the Early Warning Intervention and Monitoring System (EWIMS).<sup>13</sup> Average days attended increased by 2 days among the treatment group compared to the control group and was largely driven by a reduction of 1.9 days of unexcused absence, representing a 17 percent reduction in absences relative to baseline that is on par with other interventions aimed at addressing chronic absenteeism such as notifying parents of absences via postcard (10 percent) or text messaging (-17 percent).<sup>14</sup> Columns (2) and (3) demonstrate that controlling for baseline attendance outcomes during the 2014-15 school year and demographic characteristics does little to change the magnitude or the significance of the results

There were no significant differences in the post-period for the other academic outcomes. Although the GPA of the treatment group is higher than that of the control group, the greater heterogeneity in course grades yields a rather noisy sample from which we do not detect any statistical significance (see Panel B of Table 4). The same is true of the relative reduction in course failures among the treatment group relative to the control group where we observe an improvement, but it is not statistically significant. Finally, Panel C of Table 4 shows no evidence that the Boston SYEP reduces school disciplinary incidents—a finding that runs counter to the well-documented impacts of SYEPs on criminal justice outcomes. However, it should be noted that prior research on the program’s criminal justice impacts were largely driven by violent

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<sup>13</sup> 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. 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](https://ies.ed.gov/ncee/edlabs/regions/midwest/pdf/REL_2017272.pdf) for more details.

<sup>14</sup> Rogers & 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% across all grade-levels, partly by correcting parents’ biased beliefs about their students’ total absences. Bergman & Chan (2017) find that low-cost text messaging to parents has been shown to improve attendance by 17 percent.

crime, which may be less likely to occur in a school setting.

We also explore whether participating in the Boston SYEP has 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 English tests to receive a high school diploma. Note that because students take the MCAS in the 10<sup>th</sup> grade, we must observe participants as 9<sup>th</sup> graders in the prior summer to assess whether the program has any impact on test-taking or performance. Similar to Leos-Urbel (2014), we find that participating in the summer jobs program increases the likelihood that students will participate in this statewide standardized assessment but does not improve their performance. Table 5 indicates that 9<sup>th</sup> graders in the treatment group were 4.3 percentage points more likely to take the MCAS exam but there were no detectable improvements in the scaled score or the percent achieving a level of proficient on the test.

Interestingly, when we restrict the sample to public school students, we find even stronger results for some academic outcomes. Figure 3 shows that Boston Public School (BPS) students in the treatment group had attendance rates that were 2.5 percentage points higher than their control group peers, making them 8 percentage points more likely than the control group to have attended at least 85 percent of the school year, in part due to 3.5 fewer days of unexcused absences. Moreover, the share of students in the treatment group failing a course in the post period during the 2015-16 school year was 15 percentage points lower than that of the control group. Thus, it could be the case that the program's impact is greater for more marginal students as prior research has shown (Leos-Urbel 2014).

As such, it is natural to ask whether SYEPs might have a disproportionate effect on subgroups that are more likely to experience chronic absenteeism, poor course performance, or

disciplinary incidents. It should be noted that these 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. 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 and that this can lead to poor course performance (Utah Education Policy Center 2012). In addition, Boston Public Schools have engaged in an initiative to dismantle the “school to prison pipeline”—a set of disciplinary policies and practices that have been identified as having a disproportionate impact on both at-risk students and students of color becoming involved with the juvenile and criminal justice systems.<sup>15</sup>

Table 6 reports the ITT estimate of the differential program impact on the reduction in academic outcomes for these subgroups as well as for “marginal” students—defined as those having chronically high absenteeism, low GPAs, or a prior disciplinary incident during the pre-period (e.g., the 2014-15 school year). Among attendance outcomes, the Boston SYEP has a greater impact on students with prior chronic absenteeism, youth of legal drop out age (age 16 years or older), and those living in households that receive public assistance. While the first two groups suggest the program may increase attendance by affecting youth behavior, the latter suggests some role for an income effect where perhaps the added earnings from summer employment help to stabilize the household.<sup>16</sup> In terms of course performance, the program appears to have a disproportionate impact on reducing course failures among students with

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<sup>15</sup> Nationally, students of color are suspended or expelled at three times the rate of white students. In 2012, students of color in Massachusetts were suspended at disproportionately higher rates compared to white students, with some subgroups outpacing national averages. See <https://www.bostonpublicschools.org/domain/2117> for more detail.

<sup>16</sup> Indeed, based on our survey data, roughly half of youth participating in the Boston SYEP indicate that they help pay one or more household bills.

limited English ability. Prior research indicates that 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 disciplinary incidents for students of color or at-risk students.

What might be driving these results? 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 during the summer at their job. It could also be the case the program’s career readiness curriculum, coupled with real-world experience, boosts career and academic aspirations. Finally, the program has been shown in prior work to reduce the propensity to engage in delinquent behavior that would be disruptive to learning, although we find no evidence of this from the disciplinary data. We explore these mechanisms further in the next section by assessing the degree to which SYEP participants learn new skills over the summer and how these changes are correlated with the improvement in attendance after participating in the program.

### **Exploring SYEP Program Mechanisms Using Survey Data**

#### *Assessing Short-Term Behavioral Impacts*

The self-reported survey data indicate that youth participating in the Boston SYEP made significant improvements across a variety of short-term program outcomes that could plausibly be correlated with the subsequent reduction in criminal arraignments. 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, participants' attitudes toward their communities improved greatly (+15 percentage points) and these outcomes were significantly better than those reported by the control group at the end of the summer. Although smaller in magnitude, participants also showed measurable 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—outcomes that were also significantly improved relative to the control group. In retrospect, these large improvements are perhaps not so surprising, given that most SYEP job placements are with community-based organizations in the participants' neighborhoods, providing youth with an opportunity for greater and more positive social engagement within their communities. The Boston SYEP also provides additional soft-skills development through the career readiness curriculum which can be practiced on the job throughout the summer.

SYEP participants also indicated sizeable growth in job readiness skills and academic aspirations during the summer, many of which were significantly greater those reported by the control group (see Table 7). In terms of jobs readiness, this included large increases in the percent of participants reporting that they had prepared a resume and a cover letter, practiced interviewing skills with an adult, developed answers to typical interview questions, and learned how to be on time. And while there was no significant change among participants with regards to their plans to attend an education or training program after high school, there was a significant shift towards wanting to pursue a four-year college degree.

The one area where participants clearly demonstrated a deficit relative to the control group was in terms of future work plans. Although the percentage of participants indicating that they planned to work in the fall increased by 7.4 percentage points over the summer, this was lower

than the share reported by the control group at the end of the summer (see Table 7). This lower likelihood of future labor force participation among SYEP participants may reflect less need to work in the fall compared with those in the control group, who were far less likely to report being employed during the summer.

### *Evaluation of Program Mechanisms*

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 reductions in criminal arraignments. Table 8 reports the results of the mediation analysis specified in equation (3) that provides the ITT estimate of the program's impact on attendance while separately controlling for each of the short-term behavioral outcomes described in the previous section ( $\Delta M_i$ ) as well as pre-existing baseline academic outcomes and demographic characteristics. Improvements in social skills such as asking for help and resolving conflict with a peer were found to be positively correlated with improvements in attendance. Moreover, the SYEP<sub>i</sub> dummy was no longer statistically significant when these social skill measures were included, suggesting that improvements in social skills play a mediating role in improving attendance. Other factors such as having gained a mentor over the summer and having increased aspirations to attend college also play a mediating role in improving attendance outcomes.

Finally, although these findings are suggestive, the results presented here regarding the program's behavioral mechanisms are consistent with prior research. For example, Hill and Wang (2014) find that parenting practices (i.e., monitoring, warmth, and autonomy support) at 7th grade had significant indirect effects on college enrollment 3 years post high school, through their effects on aspirations, school engagement, and grade point average (GPA). All 3 parenting

practices were related to aspirations and behavioral engagement at 8th grade, with 2 of the 3 parenting practices related to the emotional (monitoring and warmth) and cognitive (autonomy support and warmth) engagement. Yet the analysis in this paper can fully disentangle the SYEP program effects from the benefits of simply providing youth and their families with additional income and as such, should be interpreted with caution.

## **CONCLUSION AND FUTURE WORK**

This paper seeks to assess the impact of summer job programs on low-income inner-city youth in terms of both short-term behavioral skills and attitudes as well as medium-term academic outcomes. Similar to previous studies, we find that the Boston SYEP has a significant impact on improving attendance and test-taking but no detectable impact on course performance, test scores, or disciplinary incidents. Youth who were randomly selected into the SYEP treatment group experienced significant improvements in reducing unexcused absences (-1.9 days) and achieving an attendance rate of greater than 85 percent (+2.9 percentage points) relative to the control group that provide a meaningful improvement in reducing chronic absenteeism relative to baseline (27 percent). In addition, self-reported improvements in social skills during the summer such as managing emotions, asking for help, and resolving conflict with a peer are correlated with subsequent improvements in attendance. Other short-term improvements in job readiness skills and academic aspirations also play a role. By linking the academic records to self-reported survey data on short-term program impacts, we are able to shed light on how the program achieves better outcomes among the youth being served.

Although some might conclude that because the program does not improve course performance or test scores that SYEPs are not effective, most education researchers would disagree. First, the improvement in attendance provides a meaningful impact on the likelihood of

not being chronically absent which has been linked to poor academic outcomes down the road. Of course, SYEPs do not change the household, neighborhood, or school environment of participating youth—*contextual* factors that also are important in explaining school performance. It is likely that for a subset of youth, such contextual factors would present barriers to improving academic performance beyond increasing attendance. In future work, we will explore the program’s impact on longer-term outcomes such as high-school graduation and college matriculation. We will also be able to include multiple cohorts across both intermediaries and years to potentially provide additional power to detect more subtle impacts on course grades and disciplinary incidents.

In addition, there are several remaining questions that pertain to different features of the program that are important to answer as practitioners seek to improve summer job programs. For example, it is difficult to tell whether the program’s impact on attendance stems from learning new skills on the job or through the career-readiness curriculum (or both)—an important distinction for cities such as Los Angeles and Philadelphia that are considering adding similar curricula as a program feature. Future work using alternative sources of random variation within the other Boston SYEP intermediaries to determine which participants receive the career-readiness curriculum may help answer this question. In addition, understanding the intensity needed to produce better outcomes would help cities seeking to utilize their limited funding more effectively to serve the greatest number of youth. For example, a portion 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. Additional analyses using historical participation records may be useful for determining the minimum “dosage” (e.g., number of summers) needed to achieve meaningful impacts while also helping to alleviate oversubscribed programs.

Moreover, it is not clear how the Boston SYEP compares with other interventions that do not involve the added direct costs of subsidized wages as well as indirect 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 improvements in attendance for other low-cost interventions such as notifying parents of absences via postcard (10 percent) or text messaging (-17 percent) that are similar in magnitude to those found for the Boston SYEP (Rogers and Feller 2014).

Yet SYEPs also have other advantages over alternative programs, providing benefits to individuals, families, and even communities that may also outweigh the costs. First, unlike year-round programs, SYEPs occur when youth tend to be most idle, and are less likely to interfere with academic studies or extracurricular activities. Second, unlike more targeted behavioral programs, SYEPs confer job experience, which may yield additional advantages in terms of future employment, career pathways, or post-secondary education. Third, SYEPs help families at or near the poverty line by providing income to youth with upwards of one in five contributing directly to their household's expenses, according to our survey data. Fourth, SYEPs supply a low-cost source of labor for many community-based programs serving cities, particularly summer camps that provide inexpensive daycare for low-income working parents.

Finally, it appears that the program's impacts on attendance are greater for students with prior chronic absenteeism, youth of legal age to drop out of school, and those living in households receiving public assistance such that targeting SYEPs may help level the playing field for these groups. This is particularly important because the consequences of chronic absenteeism are more likely to result in dropping out of high school for inner-city youth, limiting opportunities that otherwise remain open to teens attending schools with higher graduation rates.

Given that the Workforce Innovation and Opportunity Act of 2014 specifically requires youth workforce-development programs to increase the share of at-risk youth that they serve, understanding for whom the program provides the most benefits can guide cities in using their limited resources more effectively.

Taken together, the outcomes measured by the administrative data analysis and the insights provided by the self-reported survey data can help inform both practitioners and policymakers. For practitioners, understanding what teens learn in the short-term over the summer through their participation in the SYEP can help establish best practices and improve program efficacy *and* efficiency. For policymakers, being able to articulate which short-term behavioral outcomes translate into improvements in subsequent academic outcomes may lead to a more effective intervention that can be scaled up to produce better outcomes at a reduced cost. As such, the findings from this paper, as well as the larger Boston SYEP evaluation, can help guide program development aimed at employing youth in cities across the nation.

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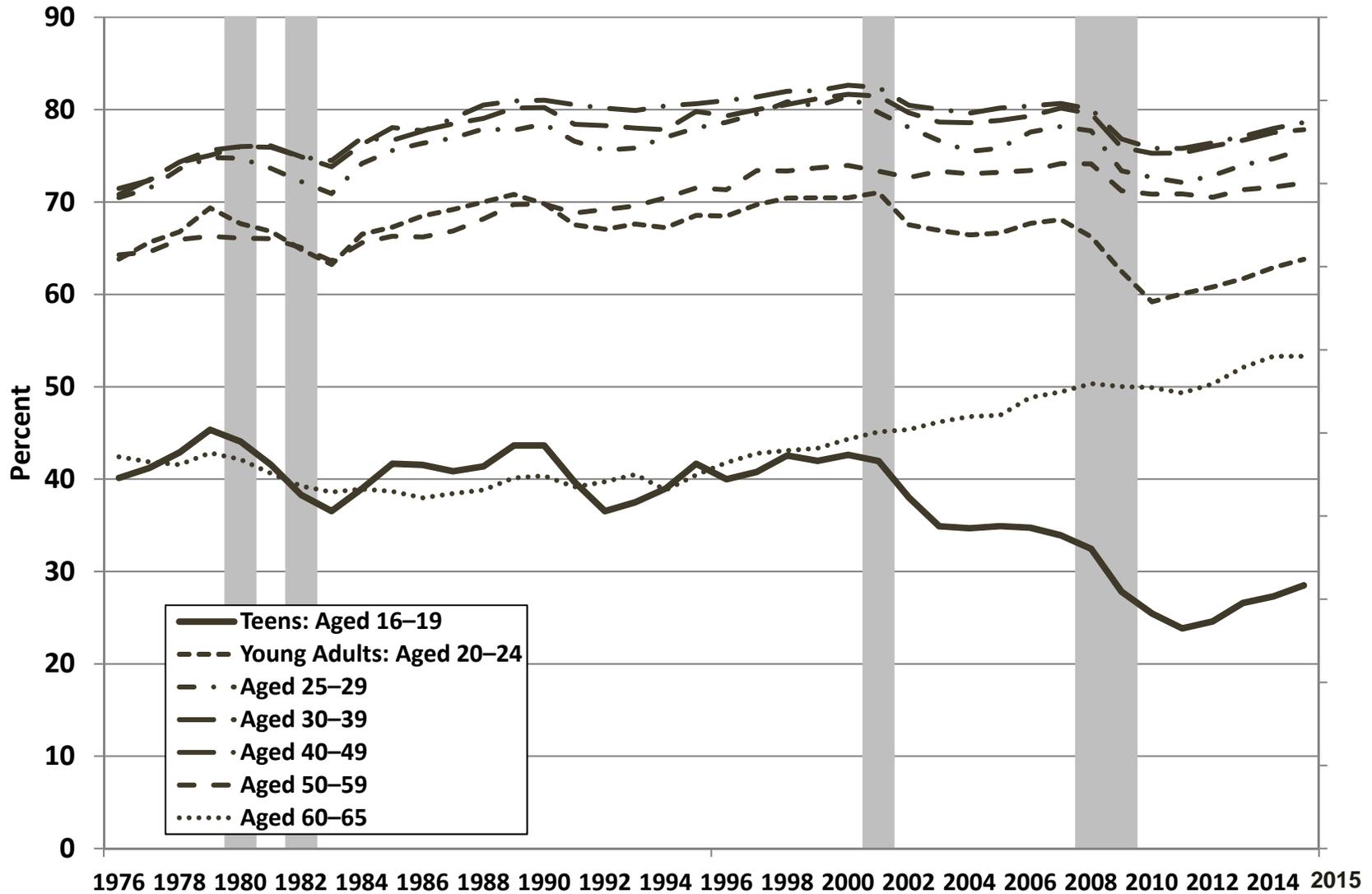
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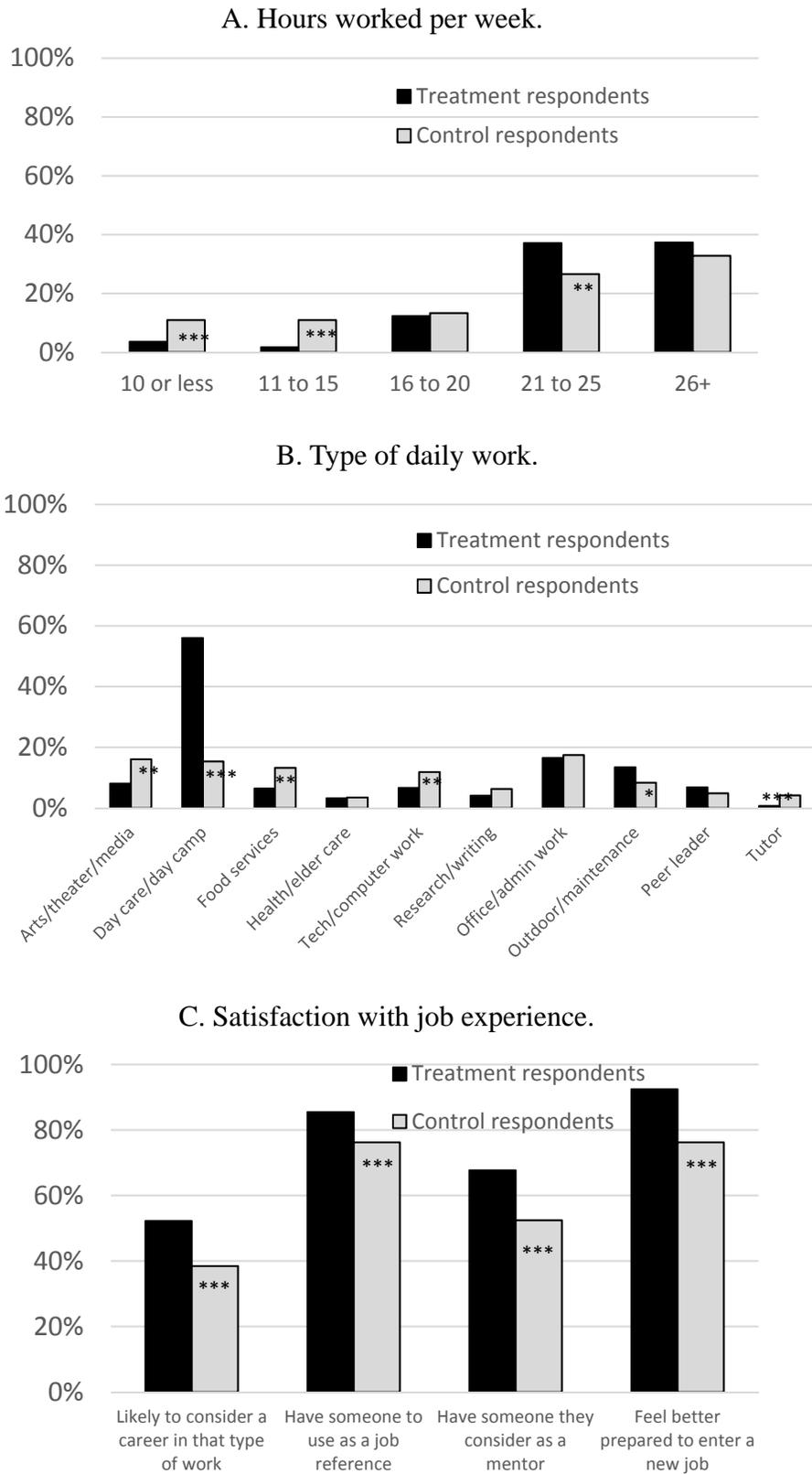
**Figure 1.** U.S. Employment-to-Population Ratio by Age Group, 1976–2015.



*Source:* Author's calculations from the U.S. Census Bureau, Current Population Survey, various years.

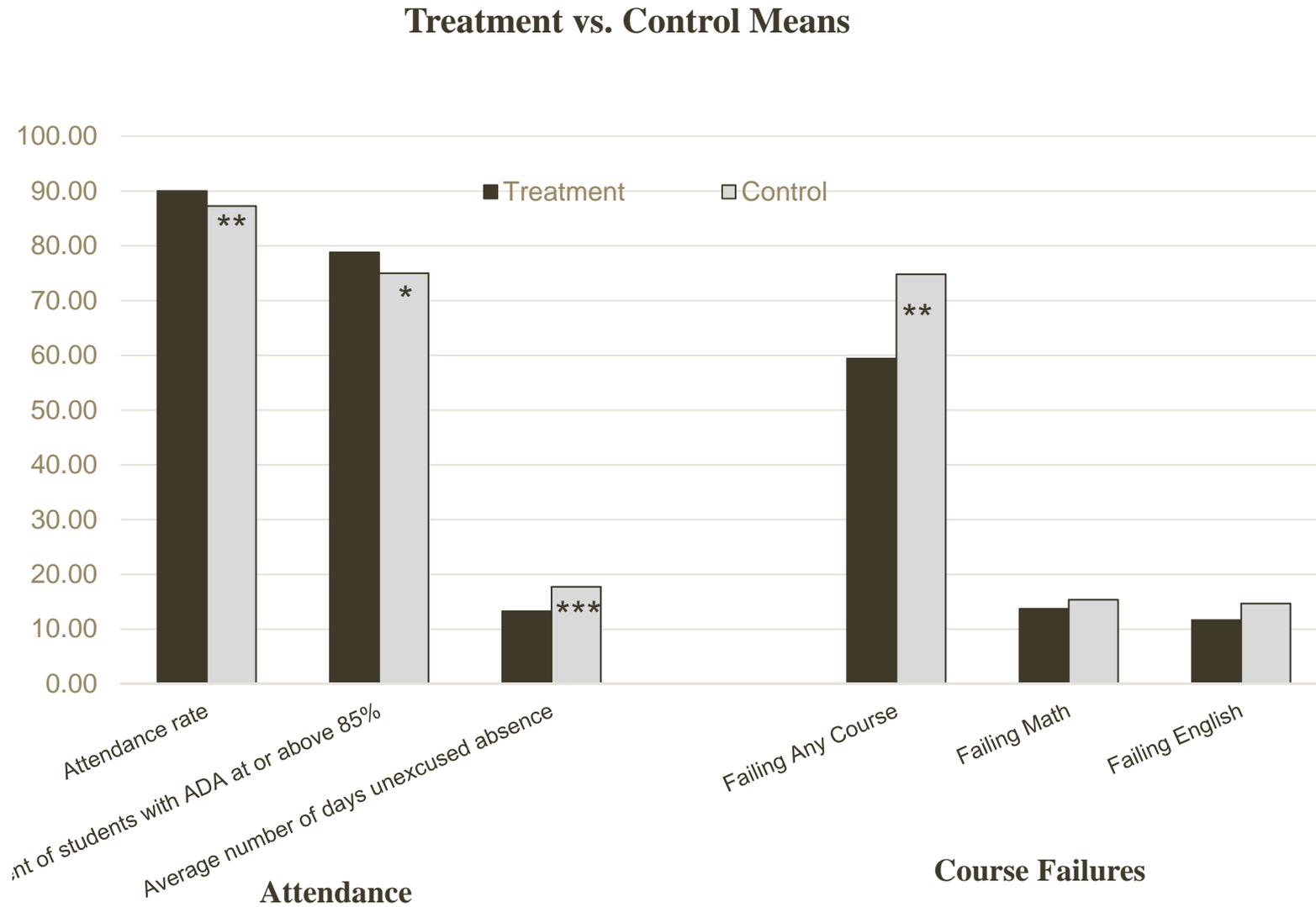
*Note:* Shaded bars represent recession periods as defined by the National Bureau of Economic Research.

**Figure 2.** Summer Employment Experiences for SYEP Survey Respondents by Lottery Outcome.



Source: Author's calculations based on survey data provided by the City of Boston, Office of Workforce Development.  
 Note: \*Indicates that the difference is statistically significant at the 10 percent level; \*\* at the 5 percent level; and \*\*\* at the 1 percent level.

**Figure 3.** ITT Estimates of the Impact of the Boston SYEP on Course Failures: Boston Public School Students



Source: Author's calculations based on data provided by the Massachusetts Department of Education.

Note: \*Indicates difference is statistically significant at the 10 percent level; \*\* at the 5 percent level; and\*\*\* at the 1 percent level.

**Table 1.** SYEP applicant characteristics by lottery outcome.

	<b>Selected (treatments)</b>		<b>Not Selected (controls)</b>		<b>Treatment-Control</b>	
	<b>(1)</b>		<b>(2)</b>		<b>(3)</b>	
Number of youth	1,186		3,049		-1,863	
	<b>Mean</b>	<b>Std. Error</b>	<b>Mean</b>	<b>Std. Error</b>	<b>Difference</b>	<b>P-value</b>
Age	15.9	(0.058)	15.8	(0.033)	0.073	(0.258)
Percentage 14-17 years	0.79	(0.008)	0.80	(0.007)	-0.008	(0.292)
Percentage female	0.53	(0.014)	0.54	(0.009)	-0.008	(0.640)
Percentage in school	0.88	(0.010)	0.88	(0.006)	-0.008	(0.497)
Percentage African American	0.51	(0.015)	0.54	(0.009)	-0.027	(0.197)
Percentage Asian	0.07	(0.007)	0.05	(0.004)	0.015	(0.088)
Percentage White	0.10	(0.009)	0.08	(0.005)	0.012	(0.211)
Percentage other/two or more races	0.33	(0.014)	0.33	(0.009)	0.000	(0.983)
Percentage Chinese	0.00	(0.001)	0.00	(0.001)	0.001	(0.557)
Percentage English	0.95	(0.006)	0.95	(0.004)	-0.004	(0.620)
Percentage Spanish	0.03	(0.005)	0.03	(0.003)	0.006	(0.287)
Percentage other language	0.01	(0.003)	0.02	(0.002)	-0.003	(0.465)
Percentage limited English ability	0.07	(0.007)	0.07	(0.005)	0.000	(0.969)
Percentage homeless	0.07	(0.007)	0.07	(0.005)	-0.002	(0.822)
Percentage receiving public assistance	0.19	(0.011)	0.17	(0.007)	0.015	(0.240)
Percentage disabled	0.04	(0.006)	0.03	(0.003)	0.007	(0.276)

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

**Table 2**  
**ABCD Applicant Characteristics by Lottery Outcome for Individuals Matched in DOE Database**  
**Summer 2015 Cohort**

	Treatments		Not Selected (Controls)		Treatment-Control	
	(1)		(2)		(3)	
Total selected by random assignment	1,186		3,049		-1,658	
Number in school at time of application	1,067		2,725		-1,658	
Number in grades 8-11 at time of application	903		2,341		-1,438	
Number matched for 2014-15 school year	696		1,855		-1,159	
Match rate	77.1%		79.2%		-2.2	
Number matched for 2015-16 school year	673		1,805			
Match rate	74.5%		77.1%		-2.6	
Number matched for both years	611		1,638			
Match rate	67.7%		70.0%		-2.3	
<b>PERCENT IN EACH CATEGORY:</b>						
	Mean	Standard Error	Mean	Standard Error	Difference	p-value
Age	15.3	(0.054)	15.4	(0.032)	-0.09	(0.156)
Percentage less than 16 years	0.64	(0.019)	0.62	(0.012)	0.03	(0.244)
Percentage female	0.54	(0.020)	0.56	(0.012)	-0.02	(0.357)
Percentage African-American	0.53	(0.020)	0.55	(0.012)	-0.02	(0.479)
Percentage Asian	0.09	(0.012)	0.06	(0.006)	0.03	(0.026)
Percentage white	0.10	(0.012)	0.08	(0.007)	0.02	(0.137)
Percentage other/two or more races	0.29	(0.018)	0.32	(0.012)	-0.03	(0.137)
Percentage Chinese	0.00	0.000	0.00	(0.001)	0.00	(0.318)
Percentage English	0.97	(0.007)	0.96	(0.005)	0.01	(0.364)
Percentage Spanish	0.01	(0.005)	0.02	(0.003)	0.00	(0.904)
Percentage other language	0.01	(0.005)	0.02	(0.003)	-0.01	(0.292)
Percentage limited English ability	0.05	(0.009)	0.06	(0.006)	-0.01	(0.463)
Percentage homeless	0.05	0.000	0.05	(0.005)	0.00	(0.926)
Percentage receiving public assistance	0.18	(0.015)	0.16	(0.009)	0.01	(0.498)
Percentage disabled	0.03	(0.007)	0.03	(0.004)	0.00	(0.676)

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

*Notes:* Standard errors are in parentheses.

**Table 3.** Baseline schooling outcomes by lottery outcome prior to SYEP participation.

	Treatment group		Control group		Treatment-Control	
	(1)		(2)		(3)	
	Mean	SE	Mean	SE	Difference	p-value
<b>A. Attendance</b>						
Attendance rate	0.923	(0.004)	0.916	(0.003)	0.01	(0.175)
Attendance rate >=85%	0.891	(0.013)	0.868	(0.009)	0.02	(0.142)
Average days attended	164.30	(0.927)	163.32	(0.632)	0.98	(0.384)
Unexcused absences	10.57	(0.578)	10.79	(0.432)	-0.21	(0.193)
<b>B. Course performance</b>						
Overall GPA	2.12	(0.040)	2.12	(0.024)	0.00	(0.935)
Percentage failing any course	0.60	(0.021)	0.61	(0.012)	-0.01	(0.688)
Math GPA	2.18	(0.056)	2.11	(0.033)	0.07	(0.263)
Percentage failing a math course	0.15	(0.016)	0.16	(0.010)	0.00	(0.855)
English GPA	2.15	(0.050)	2.16	(0.030)	-0.01	(0.799)
Percentage failing an ELA course	0.13	(0.014)	0.13	(0.009)	-0.01	(0.633)
<b>C. Disciplinary action</b>						
Percentage ever had an incident	0.18	(0.015)	0.15	(0.009)	0.03	(0.082)
Number of incidents	0.30	(0.036)	0.25	(0.019)	0.05	(0.251)
Days suspended	0.26	(0.032)	0.22	(0.017)	0.04	(0.304)
Number of youth	611		1638		-1027	

*Source:* Author's calculations based on administrative records from the Massachusetts Department of Education.

*Note:* The pre-program period is defined as the 2014-15 school year.

**Table 4.** ITT program effect on school outcomes: Attendance, course performance, and disciplinary actions

	Coefficient on Treatment Dummy		
	(1)	(2)	(3)
<b>A. Attendance</b>			
Attendance rate	0.013 ** (0.005)	0.011 ** (0.005)	0.008 ** (0.004)
Attendance rate >=85%	0.029 * (0.015)	0.029 * (0.015)	0.023 * (0.014)
Increased attendance rate	0.043 * (0.025)	0.044 * (0.025)	0.050 ** (0.025)
Decreased attendance rate	-0.067 ** (0.025)	-0.068 ** (0.025)	-0.071 ** (0.025)
Average days attended	2.060 * (1.151)	1.768 (1.139)	1.027 (1.014)
Unexcused absences	-1.869 ** (0.868)	-1.843 ** (0.851)	-1.218 * (0.703)
<b>B. Course Performance</b>			
Overall GPA	0.074 (0.051)	0.050 (0.040)	0.035 (0.040)
Percentage failing any course	-0.013 (0.025)	-0.007 (0.024)	-0.002 (0.024)
Math GPA	0.085 (0.071)	0.023 (0.068)	0.003 (0.067)
Percentage failing a math course	-0.006 (0.019)	(0.002) (0.020)	(0.004) (0.020)
English GPA	0.011 (0.063)	0.006 (0.056)	(0.021) (0.056)
Percentage failing an ELA course	-0.020 (0.018)	-0.016 (0.018)	-0.014 (0.018)
<b>C. Disciplinary Incidents</b>			
Percentage ever had an incident	-0.010 (0.016)	-0.017 (0.015)	-0.014 (0.015)
Number of incidents	0.026 (0.041)	0.011 (0.038)	0.015 (0.038)
Days suspended	0.001 (0.029)	-0.009 (0.027)	-0.006 (0.027)
Includes baseline outcomes	No	Yes	Yes
Includes demographic characteristics	No	No	Yes
Number of youth	2249	2249	2249

*Source:* Author's calculations based on data provided by the Massachusetts Department of Education.

*Note:* Demographic characteristics include age, gender, race, limited English, public assistance, homelessness, disabled status, and a dummy variable indicating that the student attends Boston Public Schools. Robust standard errors are in parentheses. \*Indicates difference is statistically significant at the 10 percent level, \*\* at the 5 percent level, and \*\*\* at the 1 percent level.

**Table 5.** ITT program effect on MCAS test scores.

	Coefficient on Treatment Dummy		
	(1)	(2)	(3)
	<b>Panel A. Participation in Statewide MCAS testing</b>		
	All Youth	Treatment	Control
Number of 9th graders	859	215	644
Number taking MCAS post-program	712	185	527
Percent taking MCAS post-program	82.9%	86.0%	81.8%
	<b>Panel B. MCAS Performance</b>		
<b>B. Mathematics</b>	No covariates		With covariates
Scaled score	-1.138 (1.521)		-1.306 (1.362)
Percentage proficient or better	-0.035 (0.041)		-0.034 (0.038)
<b>C. English Language Arts</b>			
Scaled score	-1.132 (0.962)		-1.036 (0.877)
Percentage proficient or better	-0.010 (0.032)		-0.010 (0.031)

*Source:* Author's calculations based on data provided by the Massachusetts Department of Education.

*Note:* Demographic characteristics include age, gender, race, limited English, public assistance, homelessness, disabled status, and a dummy variable indicating that the student attends Boston Public Schools. Robust standard errors are in parentheses. \*Indicates difference is statistically significant at the 10 percent level, \*\* at the 5 percent level, and \*\*\* at the 1 percent level.

**Table 6.** ITT program effect on academic outcomes by subgroup.

	Coefficient on Treatment Dummy* Group Dummy						
	Marginal Students	Age 16+	Male	African American	Limited English	Homeless	Public Assistance
	(1)	(2)	(3)	(4)	(5)	(6)	
<b>A. Attendance</b>							
Attendance rate	0.001 (0.028)	0.024 * (0.013)	----	----	0.001 (0.018)	-0.010 0.460	0.025 * (0.013)
Attendance rate >=85%	0.022 (0.076)	0.038 (0.038)	----	----	-0.012 (0.088)	-0.017 (0.098)	0.059 (0.038)
Increased attendance rate	0.213 *** (0.068)	0.090 * (0.053)	----	----	-0.021 (0.112)	0.011 (0.130)	0.129 ** (0.058)
Decreased attendance rate	-0.152 ** (0.068)	-0.114 ** (0.054)	----	----	0.022 (0.114)	-0.065 (0.125)	-0.173 *** (0.059)
Average days attended	-4.769 (5.770)	3.456 (2.963)	----	----	6.574 (4.373)	-2.298 (6.341)	4.353 (2.897)
Unexcused absences	1.956 (4.928)	-2.295 (2.215)	----	----	2.519 (3.157)	3.619 (3.408)	-4.812 ** (2.206)
<b>B. Course Performance</b>							
Overall GPA	0.027 (0.053)	0.106 (0.106)	----	----	0.199 (0.126)	0.179 (0.209)	-0.013 (0.083)
Percentage failing any course	-0.026 (0.031)	(0.042) (0.051)	----	----	-0.184 * (0.115)	-0.154 (0.112)	0.005 (0.050)
<b>C. Disciplinary Incidents</b>							
Percentage ever had an incident	----	-0.039 (0.027)	-0.003 (0.029)	0.005 (0.029)	----	0.074 (0.072)	-0.018 (0.028)
Number of incidents	0.047 (0.168)	-0.078 (0.077)	0.054 (0.079)	0.052 (0.072)	----	0.265 (0.209)	-0.071 (0.058)
Days suspended	0.018 (0.130)	-0.026 (0.052)	0.044 (0.055)	0.022 (0.052)	----	0.250 (0.169)	-0.023 (0.045)
Number of youth		2,249	2,249	2,249	2,249	2,249	2,249

Source: Author's calculations based on data provided by the Massachusetts Department of Education.

Note: Covariates include baseline school outcomes, Boston Public School dummy, age, gender, race/ethnicity, limited English, in school, public assistance, homelessness, and disabled.

Robust standard errors are in parentheses. \*Indicates difference is statistically significant at the 10 percent level, \*\* at the 5 percent level, and\*\*\* at the 1 percent level.

**Table 7.** Comparison of survey responses by demographic groups: SYEP treatments versus controls, Summer 2015

CATEGORY	All groups combined	"In-school" youth: Age 14-18 years			
		African American		Hispanic	
		Males	Females	Males	Females
<b>Community engagement and social skills</b>					
I have a lot to contribute to the groups I belong to	0.156 *** (0.029)	0.180 ** (0.068)	0.132 ** (0.057)	0.173 ** (0.088)	0.128 * (0.073)
I feel connected to people in my neighborhood	0.212 *** (0.025)	0.260 *** (0.059)	0.148 *** (0.050)	0.251 *** (0.084)	0.224 *** (0.065)
I know how to manage my emotions and my temper	0.065 ** (0.033)	0.162 ** (0.071)	0.089 (0.062)	0.037 (0.091)	0.034 (0.081)
I know how to ask for help when I need it	0.116 *** (0.030)	0.029 (0.070)	0.090 (0.058)	0.082 (0.090)	0.080 (0.075)
I know how to constructively resolve a conflict with a peer	0.136 *** (0.029)	0.133 ** (0.065)	0.057 (0.056)	0.151 * (0.086)	0.174 (0.070) **
<b>Job readiness skills</b>					
Have prepared a resume	0.245 *** (0.027)	0.317 *** (0.052)	0.187 *** (0.055)	0.313 *** (0.075)	0.238 *** (0.071)
Have prepared a cover letter	0.217 *** (0.028)	0.257 *** (0.061)	0.230 *** (0.055)	0.285 *** (0.085)	0.204 ** (0.071)
Have searched for jobs online.	0.025 (0.031)	0.152 ** (0.066)	-0.110 ** (0.057)	0.103 (0.090)	-0.018 (0.078)
Have developed answers to the usual interview questions	0.069 *** (0.026)	0.111 * (0.062)	0.056 (0.051)	0.088 (0.071)	0.031 (0.062)
Have practiced my interviewing skills with an adult	0.064 ** (0.031)	0.118 * (0.071)	0.074 (0.059)	0.069 (0.085)	0.012 (0.075)
Have asked an adult to serve as a reference.	-0.001 (0.027)	-0.016 (0.065)	-0.055 (0.052)	0.105 (0.074)	-0.056 (0.065)
Have asked an adult for help in finding job opportunities	0.071 *** (0.024)	0.041 (0.053)	0.026 (0.042)	0.135 ** (0.060)	0.068 (0.055)
Have completed at least one online job application.	-0.033 (0.028)	-0.003 (0.063)	-0.082 (0.052)	0.023 (0.078)	-0.090 (0.066)
Have learned how to be on time	0.090 *** (0.019)	0.085 ** (0.041)	0.055 * (0.034)	0.113 ** (0.046)	0.028 (0.034)
<b>Future work plans and academic aspirations</b>					
Plan to work in the fall	-0.074 ** (0.030)	0.080 (0.070)	-0.076 (0.057)	-0.038 (0.086)	-0.204 *** (0.063)
Plan to enroll in education or training program after high	0.003 (0.017)	-0.002 (0.040)	0.017 (0.034)	-0.007 (0.048)	0.011 (0.039)
Plan to attend a four year college or university	0.110 *** (0.081)	0.099 (0.065)	0.171 *** (0.052)	-0.103 (0.084)	0.169 ** (0.066)
Plan to attend a two year college	0.062 *** (0.019)	0.049 (0.041)	0.094 *** (0.033)	0.117 * (0.070)	0.018 (0.044)
I need to improve my academic skills	0.129 *** (0.029)	0.114 * (0.070)	0.211 *** (0.054)	0.185 ** (0.087)	0.024 (0.072)

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

Note: Each coefficient is the marginal effect from a separate probit regression of the outcome on a dummy variable for treatment controlling for age, gender, race, two parent family, and English as the primary language. Robust standard errors are in parentheses.

\*Indicates difference is statistically significant at the 10 percent level, \*\* at the 5 percent level, and\*\*\* at the 1 percent level.

**Table 8. Relationship Between SYEP Impact on Short-Term Behavioral Changes and Attendance  
Summer 2015 Cohort**

CATEGORY	Dependent Variable (Post 2015-16)					
	Attendance Rate		ADA>=85%		Unexcused Days	
	Coeff	SE	Coeff	SE	Coeff	SE
<b><u>Social and Community Engagement</u></b>						
Youth increasing in feeling that they contribute to the groups they belong to	0.190	(2.604)	0.082	(0.061)	-1.020	(2.923)
Youth increasing in feeling connected to people in their neighborhood	-0.807	(2.168)	-0.005	(0.073)	2.751	(3.387)
Youth indicating they have learned how to manage emotions	-2.724	(3.183)	-0.042	(0.092)	4.072	(4.189)
Youth indicating they have learned how to ask for help	3.169	(1.848) *	0.064	(0.070)	-4.500	(2.655) *
Youth indicating they have learned how to resolve conflict with a peer	3.500	(1.620) **	0.135	(0.059) **	-4.179	(2.161) **
<b><u>Job Readiness</u></b>						
Youth indicating they have learned how to prepare a resume	1.289	(1.187)	0.043	(0.034)	-4.588	(2.276) **
Youth indicating they have learned how to prepare a cover letter	2.783	(2.464)	0.004	(0.058)	-3.066	(2.557)
Youth indicating they have learned how to search for a job online	0.743	(2.259)	0.069	(0.056)	-3.431	(2.369)
Youth indicating they have gained interview skills	-1.073	(2.573)	-0.008	(0.059)	-1.499	(2.819)
Youth indicating they have gained a job mentor	4.045	(1.969) **	0.106	(0.051) **	-6.338	(2.728) **
Youth indicating they have learned how to be on time	2.273	(1.800)	0.068	(0.050)	-4.372	(2.384) *
Youth indicating they have learned how to organize their day and keep to schedule	1.618	(1.824)	0.021	(0.051)	-3.125	(2.419)
<b><u>Work and Academic Aspirations</u></b>						
Youth increasing aspirations to work in the fall	2.256	(1.879)	0.038	(0.059)	-1.192	(2.919)
Youth increasing aspirations to attend 2 year college	6.407	(1.509) ***	0.226	(0.027) ***	-8.001	(2.210) ***
Youth increasing aspirations to attend 4 year college	3.493	(1.699) **	0.083	(0.060)	-3.837	(2.542)
Youth starting to save for college tuition	6.826	(1.544) ***	-0.010	(0.141)	-9.246	(2.094) ***
Number of Observations	1220		1220		1220	

Source: Author's calculations based on data provided by the Massachusetts Department of Education..

Notes:

Regressions also include SYEP treatment dummy and covariates for age, gender, race/ethnicity, limited English, in school, public assistance, and homelessness.

Robust standard errors are in parentheses. \*Indicates difference is statistically significant at the 10 percent level, \*\* at the 5 percent level, and\*\*\* at the 1 percent level.