

How Do Summer Youth Employment Programs Improve Criminal Justice Outcomes, and for Whom?

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Abstract: Cities across the U.S. have turned to summer youth employment programs (SYEPs) to improve the behavioral, economic, and academic outcomes of inner-city youth. This paper evaluates the impact of the Boston Summer Youth Employment Program using both experimental and non-experimental variation. Similar to previous studies of summer jobs programs in other cities, I make use of an embedded randomized controlled trial and find that the program reduces violent crime by 35 percent, as measured by the number of arraignments from administrative records during the 17 months after participation. In contrast to prior work, I also find a similar reduction in arraignments for property crimes (–29 percent). This study also provides exploratory evidence on the mechanisms driving these reductions in crime using self-reported responses of participants from a pre-/post-program survey. The results provide suggestive evidence that the beneficial impacts on violent and property crime are largely driven by improved conflict resolution skills versus other factors that would increase the opportunity cost of crime. 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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INTRODUCTION

Despite national violent crime and murder rates falling to historic lows over the past several decades, local policymakers and law-enforcement officials in Baltimore, Chicago, Cleveland, and St. Louis have raised concerns about recent upticks in violent crime. Gang-related killings in these and other cities have steadily increased over the past 35 years, from just 1 in 100 murders in 1980 to nearly 1 in 10 in 2015 (Economist Data Team, 2017). Youth are twice as likely as adults to be both victims and perpetrators of violence. Moreover, violent crime has a disproportionate impact on nonwhite youth, with the violent-crime arrest rate for African-American juveniles five times that of their white peers (Office of Juvenile Justice and Delinquency Prevention, 2017).

In response, mayors in U.S. cities such as Boston, Chicago, New York, and Washington, D.C., have turned to summer youth employment programs (SYEPs) as one potential way to reduce violence among youth, based on a variety of rationales (Boston Youth Violence Prevention Collaborative, 2013). Initially, the motivation was to keep youth off the street and out of trouble during program hours while improving “soft skills” such as self-efficacy, impulse control, and conflict resolution—the lack of which has been shown to be predictors of youth violence and delinquency (Lipsey & Derzon, 1998). Increasingly, policymakers are also seeking to use SYEPs as a vehicle to provide meaningful employment experiences that can lead to an alternative pathway—whether a career or some type of postsecondary education—that makes criminal activity less attractive. This new focus stems from the recognition that one of the major underlying causes of racial disparities in youth violence is the gap in economic opportunity arising from nonwhite teens being concentrated in neighborhoods with failing schools and few job opportunities (Wilson, 1996; Chetty, Hendren, & Katz, 2016). By providing access to skill

development, career mentoring, and employer networks, SYEPs aim to provide youth with the tools and experience needed to navigate the job market and, ultimately, stay out of trouble, based on the belief that “nothing stops a bullet like a job” (Cook & Ludwig, 2011).

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 prerecession 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 nonwhite 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 (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 criminal justice outcomes, to better understand how these impacts are achieved and for whom the benefits are the greatest. Using an embedded randomized controlled trial (RCT), I find that the program 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. Moreover, these medium-term criminal justice outcomes are greater in magnitude for males and appear to be linked to improvements in social skills among participants that occur during the summer, as measured by a pre-/post-program survey. 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, I provide an overview of the relevant literature and policy context. Next, I describe the Boston SYEP and the experimental design, and explain the data and methodology that I use to evaluate program outcomes. I then estimate the impact of the program on the medium-term criminal justice outcomes as well as the short-term behavioral changes in skills and attitudes, and analyze the relationship between the two. Finally, I 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. These earlier initiatives often failed to improve criminal behavior 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; Millenky 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 engaged in criminal activity. As such, SYEPs 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, SYEPs occur in summer months when youth are often idle, reducing opportunities for time that might otherwise be spent engaged in criminal activity, and creating fewer conflicts with academic studies compared to year-round employment programs.¹ Finally, the Boston SYEP incorporates several features that are designed to specifically address deficits arising from a lack of opportunities among at-risk youth such as a formal career readiness curriculum, greater exposure to private sector employers, and job-skill ladders across summers.

How Might SYEPs Improve Criminal Justice 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 employment,

¹ 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). Indeed, 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).

academic, and criminal justice outcomes down the road. There are four primary channels through which SYEPs are thought to lessen the propensity for youth to engage in criminal activity:

- (1) *Reducing opportunities to engage in delinquent or criminal behavior.* Summer jobs programs may “incapacitate” youth by limiting the time to engage in criminal activity or by disrupting “routine activities” that provide likely offenders with suitable targets and a lack of supervision or guardianship (Cohen & Felson, 1979). By providing youth with a set of socially productive activities, SYEPs may decrease the risk of exposure to, or participation in, violence and delinquent behavior (Wilson, 1996).
- (2) *Improving behavior correlated with delinquency and crime.* Although most criminal offending ceases as youth move from adolescence into adulthood (Monahan, Steinberg, & Piquero, 2015), strong, supportive, and sustained relationships with adults and peers are critical to that process (Nagaoka et al., 2015). SYEPs help develop these relationships by placing youth in jobs that are supported by mentors and program staff. In addition, the early work experience provided by SYEPs gives youth the opportunity to engage in tasks that help them develop a sense of agency, identity, and competency that is necessary for adult roles and success. Some SYEPs, including the Boston program, also offer programming aimed at improving self-efficacy and conflict resolution, behaviors inversely correlated with youth delinquency and violence (Lipsey & Derzon, 1998).
- (3) *Making crime less attractive by improving future opportunities.* Early work experience can also improve current job readiness skills as well as raise career and academic aspirations—all of which can lead to better long-term employment outcomes, particularly for disadvantaged youth with less access to job opportunities. Labor force attachment at an early stage in one’s career typically predicts better labor market outcomes in terms of both employment and

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

(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.² In addition, by providing youth with a steady source of income, SYEPs may reduce the motivation for youth to engage in delinquent activities related to theft. 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.³

Understanding the mechanisms by which the summer jobs program can lead to better outcomes down the road is important for both policymakers and practitioners to maximize resource allocation. I 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 reductions to criminal activity during the 17 months after participation.

Summer Jobs Programs: What Do We Know So Far?

Although SYEPs have the potential to enhance youth outcomes along several dimensions, researchers have only recently focused on evaluating early work experiences provided by

² Note that it is often not possible to parse out the income effect associated with SYEPs from other changes related to the program experience itself. Nonetheless, I lay out the main arguments supporting why one might expect SYEPs to improve outcomes independent of the income effect.

³ Unlike recessions, where unemployment may be negatively correlated with property crime because of a decrease in suitable targets and an increase in guardianship in the aggregate, relatively high unemployment among only youth would have the opposite effect (Cantor & Land, 1985).

summer jobs programs. The results are encouraging. These studies typically use an RCT design to compare impacts for youth who were randomly selected into the program to youth who applied but were not selected. For example, 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, 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.

Other studies find that the New York City SYEP is associated with modest improvements in test taking and school attendance, but not high school graduation or college matriculation. For example, 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. Another study 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 (Leos-Urbel, 2014). 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).

Finally, several studies examine the link between summer jobs programs and subsequent employment and earnings. 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 subgroup 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. In Chicago, Heller (2014) finds no significant difference in criminal justice outcomes for participants randomly assigned to a social-emotional learning curriculum. In a follow-up study, Davis & Heller (2017b) find that employment benefiteres commit *more* property crime than their control counterparts, yet employment non-benefiteres commit *fewer* violent crimes, suggesting that SYEPs do not achieve better criminal justice outcomes by improving human capital and labor market opportunities that increase the opportunity cost of crime. A better understanding of which mechanisms within SYEPs work better than others could help policymakers target program elements to particular groups of youth to enhance outcomes. I build on this literature by linking survey data on changes in self-reported behaviors over the summer to administrative records on subsequent criminal justice outcomes to shed light on what works for whom, under which 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 roughly 10,000 youth each summer with about 900 local employers. Participants work a maximum of 25 hours per week for a six-week period from 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 where the employer pays the youth directly. 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.⁴

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.⁵ The enrollment period typically spans February through June, and applicants are notified of their lottery status and job assignment in late June. ABCD uses a computerized system with a simple random-assignment algorithm to select youth based on their applicant ID numbers and the number of available slots 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

⁴ 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.

⁵ The other intermediary that uses random assignment, the Department of Youth Employment and Engagement (DYEE), does so only on a partial basis where 60 percent of the jobs for a given employer are assigned randomly and the other 40 percent are selected by the employer. In addition, DYEE chose not to implement the survey during the summer of 2015, so it is not possible to test program mechanisms.

who applied to ABCD in 2015, a total of 1,186 (or 28 percent) were offered a job via simple 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.⁶ 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 upward of 18 percent acknowledged receiving cash public assistance of some form.⁷ 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. In Table 1, the only statistically significant difference is the share of Asian youth being slightly higher in the treatment group (7 percent) versus the control group (5 percent). I note that having at least one statistically significant difference at the $p < 0.10$ level would be expected by random chance when

⁶ Table A1 shows that ABCD draws applicants from all 18 Boston neighborhoods, with greater representation among those with higher shares of youth aged 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). All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher’s website and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

⁷ Cash public assistance includes Emergency Aid to the Elderly, Disabled, and Children; Supplemental Security Income; Social Security Disability Insurance; Transitional Aid to Families with Dependent Children; unemployment insurance, or workers’ compensation.

testing 15 different characteristics.⁸

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 those responding to an end-of-summer survey of both the treatment and control groups. Note that only 26.4 percent of those in the control group responding to the survey had worked during the summer, perhaps indicating their comparative inability to secure jobs even with Boston's relatively low unemployment rate of 4.4 percent during July 2015.⁹ 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. I next explore whether the program also had any meaningful positive impacts on youth avoidance of violence and criminal behavior.

DATA AND EMPIRICAL METHODOLOGY

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

⁸ I also test for baseline equivalence using separate models estimating the effect of winning the lottery on preexisting applicant characteristics for different age/gender/race groupings (see Table A3).

⁹ Quarterly wage record data provided by the Massachusetts Department of Unemployment Assistance show a similar proportion of youth in the control group (28.2 percent) worked during the third quarter (July–September) of 2015.

versus those who do not (Hotz et al., 2002; Bacolod & Hotz, 2006). To address this potential bias, I 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 on court arraignments that capture the 17 months following the intervention to assess SYEP impacts on medium-term criminal justice outcomes. The second phase of the analysis is more exploratory and 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 avoid the criminal justice system.

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 high-crime neighborhoods, where even typical developmental tendencies to engage in delinquent behavior during adolescence are more likely to result in arrest and arraignment because of more intense policing efforts (Moffitt, 1993). As such, I test for heterogeneous impacts where one might expect to see a disproportionate impact based on a greater likelihood of offending—specifically among males, older youth, those receiving public assistance, homeless individuals, and prior offenders.

Using Administrative Data to Assess SYEP Impacts on Criminal Justice Outcomes

Data for the first phase of the analysis come from criminal-justice records obtained from the Massachusetts Department of Criminal Justice Information Services (DCJIS) and the Office

of the Commissioner of Probation, which provide information on all court-related activity within the state of Massachusetts, including both adult and juvenile records.¹⁰ This rich data source contains information on each criminal charge up through November 2016, including the arraignment date, the seriousness of the crime (e.g., misdemeanor or felony), and a literal description of the crime that can be used to create categories by type (e.g., violent, property, drug, gun, and other). 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 are less willing to admit wrongdoing to keep their job. However, arraignment records measure criminal activity only to the extent that an individual was arrested, booked, and appeared before a judge and as such may reflect both criminal and police behavior.¹¹

Table 2 shows that there was no significant preexisting difference in the baseline criminal justice outcomes between youth in the treatment versus control groups. Prior to notification of their lottery status, the difference in the number of arraignments was trivial between the treatment group (0.12 arraignments per youth) and control group (0.14 arraignments per youth). Moreover, there were no significant differences in the number of arraignments by type of crime. Finally, similar proportions of youth in both groups had a criminal record prior to the start of the program, with 4.1 percent of the treatment group and 3.6 percent of the control group having

¹⁰ Without a national database of arrests, it is difficult to assess the extent to which having only state-level data is a limitation of the study. However, to bias the results it would have to be the case that treatment increases time spent outside the state and so reduces arrests without reducing criminal activity. All summer jobs were within the greater Boston area, so treatment did not directly encourage out-of-state travel. Thus, it seems implausible that differential censoring can explain the entire observed decrease in violent and property crimes.

¹¹ The arraignment data do not capture criminal activity that went undetected by police, nor encounters with the police that did not result in official documentation. In addition, an arraignment may result in a variety of outcomes including dismissal, community service, probation, and incarceration—or in the case of juveniles, placement with the Department of Youth Services.

been arraigned in court before June 2015.¹²

To assess the impact of the Boston SYEP on criminal justice outcomes, I compare criminal records 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, I 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 take-up to assess the degree to which SYEP could reduce violence 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, I also provide treatment-on-the-treated (TOT) estimates using a two-stage-least-squares method in the online appendix.

I measure two primary outcomes of interest during the post-intervention period: the number of arraignments per youth and whether an individual has been arraigned for any crime. Note that although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned (Bloom, 2006), I 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 criminal justice outcome, $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, $X_{i(t-1)}$ is a set of preexisting baseline criminal justice outcomes and demographic characteristics, and μ_{it} is a stochastic error term. I use both OLS as well as

¹² This is considerably lower than what Heller (2014) finds, where roughly 20 percent of Chicago SYEP youth had been arraigned prior to the start of the program. However, that program was primarily designed as a violence-reduction intervention, and program operators focused on recruiting a population of youth at high risk of violence.

alternative nonlinear methods to relax the linear functional form assumption.¹³

Finally, I exploit the richness of the data to perform several analyses aimed at testing some of the proposed program mechanisms discussed above. First, I track the cumulative number of arraignments occurring after random assignment for both treatment and control groups to determine whether the results are driven primarily by reductions in crime during the program, when youth are kept comparatively busy. Second, I compare a measure of recidivism between the two groups to assess whether SYEPs operate primarily as a preventive or a rehabilitative intervention. Finally, I estimate program effects separately by offense type (violent, property, drug, gun, and other) to test whether SYEPs affect youth behavior regarding social interactions differently from economic and situational factors. For example, because violent crime tends to arise from interpersonal conflicts, one might expect that improvements in cognitive and emotional skills would be more highly correlated with reductions in arraignments for violent crime. In contrast, nonviolent crimes, which involve property or drugs more often than interpersonal conflict, may be relatively more responsive to economic and situational factors such as improving future employment prospects or providing direct income support. Given that SYEPs may operate through either one pathway or both, I further test these assumptions in the second more exploratory phase of the analysis described in the next section.

Using Survey Data to Explore SYEP Program Mechanisms

To explore program mechanisms, I link the criminal justice 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. The survey was originally

¹³ To analyze differences in the number of arrests—a count variable—I 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 being arrested, I use a probit estimator.

developed by the Youth Violence Prevention Collaborative to measure individual behaviors correlated with youth violence, such as social skills and community engagement. I 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 criminal activity, the goal here is to provide a glimpse into *how* the program achieves these outcomes. Because I 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 Changes in Skills and Attitudes

I initially explore how the Boston SYEP affects youth behavior over the course of the summer. To identify significant changes in behaviors and attitudes, I identify survey responses 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.¹⁴ For binary questions such as those pertaining to job readiness skills, improvement was measured as moving from “no” to “yes.” For questions measured on a Likert scale, such as those pertaining to social skills and community engagement, improvement was measured as any upward movement along the scale (e.g., switching from “Agree” to “Strongly Agree”).¹⁵

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

¹⁴ 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.

¹⁵ As a robustness check, I also measure improvement as a “meaningful” positive change that is larger in magnitude and defined as switching from Disagree or Strongly Disagree to Agree or Strongly Agree. Table A13 in the online appendix shows that defining improvement using this more meaningful definition yields even stronger relationships between the short-term program impacts and the reduction in the number of arraignments.

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, which could be due to differences in both observable as well as unobservable characteristics across the two groups of survey responders. In terms of observable characteristics, those who chose to respond to the survey in 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 compared to the treatment group (see Table A5). I argue that this selection bias on the observable characteristics goes against finding significant improvements for the treatment group relative to the control group, given that the survey respondents in the control group exhibit characteristics that are on average associated with better economic, academic, and criminal justice outcomes.¹⁶ Similarly, I would argue that the selection bias on the unobservable characteristics among the control group is also likely to be positive.¹⁷ Nonetheless, to minimize selection bias due to survey response rates, I control for observable

¹⁶ 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 & Gonzalez-Barrera, 2014), and standardized test scores are lower among African-American children and those living in single parent households (Jencks and Phillips, 1998). Being older, male, or living in a single-parent home have been shown to be significant predictors of re-offending among youth (Cottle, Lee, & Heilburn, 2001).

¹⁷ First, if it's the case that the observable characteristics are positively correlated with the unobservable characteristics, then selection on the unobservables among the control group would also be positive. Second, youth in the control group who responded to the survey are likely to be more intrinsically motivated than those who did not, again suggesting that they are more positively selected than the control group.

characteristics using equation (2):

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

where M_{it} is one of the short-term behavioral skills or attitudes (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. Because the selection bias among survey respondents in the control group is correlated with better outcomes, the coefficient π_2 is likely to provide more conservative estimates of the difference in post-survey behavioral outcomes between the treatment and control groups that would be, if anything, smaller in magnitude.

Finally, it is important to acknowledge the other limitations of self-reported survey data such as those raised in Meyer, Mok, and Sullivan (2015). For example, self-reported data are 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 nonresponse for the survey questions used in the analysis was less than 5 percent for both the treatment and control groups.

Linking Short-Term Behavioral Changes in Skills and Attitudes to Criminal Justice Outcomes

Ideally, a full mediation analysis would be used to generate evidence for how the Boston SYEP program improved criminal justice outcomes (Baron & Kenny, 1986; Keele, Tingley, & Yamamoto, 2015). However, this is not possible due to data limitations.¹⁸ Nevertheless, it is possible to explore whether short-term behavioral changes in skills and attitudes are correlated with better criminal justice outcomes to shed light on the program's mechanisms by modifying equation (1):

¹⁸ 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, I can only link the survey responses to the arraignment data for youth in the treatment group who responded to the survey, ruling out a full mediation analysis.

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

On the left-hand side, the dependent variable is one of the medium-term criminal justice outcomes (e.g., number of crimes per youth), while on the right-hand side is a dummy variable ($\Delta M_i * SYEP_i$) indicating positive improvement for a specific short-term behavioral skill or attitude (e.g., ability to resolve conflicts with a peer) for the treatment group. A negative and significant coefficient on $\Delta M_i * SYEP_i$ indicates that improvement in the short-term behavioral skill during the summer of participation is associated with a larger reduction in criminal arraignments (e.g., is positively correlated with the reduction). Moreover, if the coefficient on the stand-alone $SYEP_i$ dummy in equation (3) is smaller in magnitude than that in equation (1), this would suggest that ΔM_i plays a role in reducing arraignments separate from simply being assigned to treatment. I also test whether these same short-term behavioral changes are driving the reduction in crime 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. I argue that this assumption is plausible if the analysis is restricted to those short-term behavioral skills and attitudes 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 & Page, 2014).

RESULTS

Assessing SYEP Impacts on Criminal Justice Outcomes Using Administrative Data

I find that the Boston SYEP has a significant impact on reducing the frequency of

criminal arraignments among youth, but not the likelihood of ever being arraigned. I test the program's impact on each of these outcomes separately in Figure 3. The bar chart on the left plots post-program means for the treatment versus the control group by type of crime. The bar chart on the right plots the ITT estimate of the difference along with the 95 percent confidence interval. Panel A shows that despite there being no significant difference in the overall number of arraignments per youth, violent-crime arraignments among the treatment group were 35 percent lower relative to the control group, with roughly -0.031 fewer arraignments per youth. A similar impact was found for property crimes (-0.022 fewer arraignments per youth or a relative decline of -29 percent). There were no significant changes in arraignments for the other types of crimes (gun, drug, or other), although there was a slight uptick in drug and other crimes—such as disturbing the peace—which may be why the decline in the overall number of arraignments is not statistically significant.¹⁹ For the former, it could be that the additional income from working is spent on crime-inducing goods such as drugs. For the latter, it may be that incidents such as disturbing the peace continue to occur as frequently as before but no longer escalate into violent crimes. Interestingly, similar reductions in arraignments were observed regardless of the seriousness of the crime (e.g., misdemeanor versus felony).

Table 3 shows that controlling for preexisting baseline criminal justice outcomes and demographic characteristics in equation (1) does little to change the magnitude or significance of the ITT estimates, even when using a nonlinear specification such as a Poisson quasi-maximum likelihood estimator. In the preferred specification in column (4), the impacts on both violent (–

¹⁹ While it is impossible to say for certain due to a lack of data, many studies assert that violent and property crimes are better measured by arraignment data than other types of crimes, such as nonviolent drug and gun crimes, where often there is no victim (Tourangeau & McNeeley, 2003). This claim is supported even within the limited clearance data that exist, which show that violent crimes are 2.5 times more likely to result in an arrest than property crimes—likely because violent crimes involve a victim and may result in an incident or injury that requires police and medical response (Gramlich, 2017).

0.035 fewer arraignments per youth) and property crime (–0.030 fewer arraignments per youth) are very similar to the unadjusted estimates without controls. They are also similar to the TOT estimates that measure the program’s impacts on youth who chose to participate (see Table A7), reflecting the relatively high take-up rate (83.6 percent) among applicants. Both the ITT and TOT estimates are very similar to an earlier evaluation of the Chicago SYEP, which found no significant decreases in the total number of arrests but a 43 percent reduction (–0.0395 fewer arraignments per youth) in violent crime arrests (Heller, 2014).

In terms of the propensity to commit *any* crime during the post-period, panel B of Figure 3 shows that there was no significant reduction in the percent of youth arraigned for the treatment group versus the control group post-program. During the 17-month follow-up period, approximately 5.1 percent of the treatment group (n=53 youth) had been arraigned for any crime compared to 5.4 percent of the control group (n=165 youth), with no differential arraignment rate by type of crime (see Table A8).²⁰ Thus, it appears that SYEPs primarily affect youth on the intensive—rather than the extensive—margin, such that participants commit *fewer* crimes but are not less likely to have *ever* committed a crime during the post-period. This finding is consistent with the Chicago SYEP evaluation that finds fewer violent crime arrests among the treatment group during the post-program period, despite similar proportions of youth in the treatment and control groups being arrested post-program (Heller, 2014).

Although some might conclude that because the program does not reduce the likelihood of ever being arraigned, only the frequency of arraignments, SYEPs are not effective, most criminologists would disagree. First, the reduction in the number of crimes is an economically meaningful impact—particularly for violent and property crimes that are costly to both

²⁰ This is true even when using a more restricted post-program window of observation that starts six months after the end of the program.

individuals and society. A back-of-the-envelope calculation suggests that the Boston SYEP's benefits may already outweigh the costs. The cost of administering the program for the City of Boston was about \$2,000 per participant on average, which includes 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, leaving an administrative program cost of \$600 per youth. Applying estimates of the social costs of crime (tangible losses plus quality of life) from Miller, Cohen, Wiersema (1996) to each arraignment indicates that the estimated cost savings from the reduction in criminal activity is \$1,793 for violent crimes and \$135 for property crimes for a combined total cost savings of \$1,928 per youth (see Table A16). This benefit to victims clearly outweighs the program costs of \$600 per participant—not to mention the cost to the criminal justice system of arresting, trying, and potentially incarcerating the offender as well as the opportunity costs of lower economic productivity for both individuals and their communities arising from lower levels of education and employment associated with time spent in youth detention.

Second, these findings are consistent with prior research in criminology regarding the likelihood of youth to participate in delinquent and criminal activities over time. This likelihood increases with age as part of the normal course of adolescence through age 25, after which delinquency and criminal activity naturally decrease due to maturity, without any intervention (Monahan, Steinberg, & Piquero, 2015). Indeed, the relative reduction in the number of arraignments for violent and property crimes was driven by a lack of increase over time among the treatment group, yet the percentage of youth being arraigned for any crime increased over time for both the treatment and control groups pre- versus post-program (see Table A10).

Third, one of the potential rationales from the criminology literature through which

SYEPs may reduce crime is by “disrupting routine activities” that provide likely offenders with suitable targets and a lack of supervision or guardianship (Cohen & Felson, 1979). Thus, it is likely that SYEPs do not completely incapacitate youth who are likely to engage in criminal activity but rather disrupt the frequency with which they do so.²¹ This rationale is supported by focus group discussions with SYEP participants who had been court-involved prior to the program. When asked whether the program reduced the opportunity to engage in crime, the respondents acknowledged that having a summer jobs prevented them from interacting with other gang-related youth during the week because they had to get up early the next day for work.

Moreover, the decrease in criminal activity was not limited to the duration of the SYEP, as would be expected if its primary mechanism was to entirely “incapacitate” youth during the summer. If this were the case, treatment group participants would return to their prior behavioral patterns once the program ended. Instead, the number of arraignments for the treatment group continued to fall relative to the control group during the post-program period. Figure 4 graphs the cumulative ITT estimate over time from equation (1) controlling for preexisting baseline criminal justice outcomes and demographic characteristics, with each point adding an additional month of data to the prior effect. For violent crimes, the fall in arraignments becomes statistically different from zero at month six—a full four months after the end of the program—and continues to accumulate through month 17, at the end of the post-program data window. For property crimes, the fall in arraignments becomes significant during the program, levels off during the school year, and then decreases again during the subsequent summer. The downward slope of

²¹ Among youth arraigned at least once during the 17-month post-program period, there was a significant reduction in the *total* number of arraignments per youth for the treatment group compared to the control group (–1.3 fewer arraignments) as well as for both violent and property arraignments (see Table A11). Moreover, while the re-arrest rate is similar for both the treatment and control groups (about 35 percent), the number of post-program arraignments for youth in the treatment group with a prior criminal record is significantly lower for violent and property crimes relative to youth in the control group with a prior criminal record (see Table A12).

both effects makes it clear that most of the reduction in criminal activity accrues well after the end of the program at month two. This is consistent with the Chicago SYEP evaluation in which the cumulative reduction in violent crime becomes significant six months after the program's start and then continues to fall during the observation period through month 16 (Heller, 2014).

Of course, SYEPs do not change the household, neighborhood, or school environment of participating youth—*contextual* factors that also are important in explaining criminal activity. It is likely that for a subset of youth, such contextual factors would make them predisposed to engage in criminal activity. For example, males typically offend at a higher rate than females. Similarly, youth tend to be more likely to commit crimes as they age because they have less supervision and more opportunity—especially if they can drive and are no longer in school. Finally, youth with less advantageous socioeconomic characteristics, such as those living in poverty or who are homeless, have been shown to be more likely to engage in crime (HUD, 2016; Baron & Hartnagel, 1998). Finally, youth who have previously been arrested are at higher risk for being arrested in the future (Cottle, Lee, & Heilburn, 2001).

As such, it is natural to ask whether SYEPs might have a disproportionate effect on subgroups that are more likely to engage in crime. It should be noted that these subgroup analyses were not prespecified, but rather are exploratory, and are subject to the usual bias arising from multiple hypothesis testing. Still, exploratory subgroup analyses can be useful for generating new hypotheses and for robustness checking. Table 4 reports the ITT estimate of the differential program impact on the reduction in crime for males, youth aged 18–24, those receiving public assistance, homeless youth, and those who had ever been arraigned prior to participating in the summer jobs program. Among the five subgroups, males are the only subgroup for which there is consistent evidence that the Boston SYEP has a greater impact on

reducing arraignments for violent crimes (-0.071 arraignments per youth). Although I find some suggestive evidence that the program has a greater impact on reducing property crime among older youth and reducing violent crime among both homeless youth and those who had been arraigned prior to the program, these estimates do not remain statistically significant when accounting for multiple hypothesis testing using the family-wise error rate (FWER).

What might be driving the reduction in crime observed for the full sample? It could be the case that participating in the SYEP disrupts some of the activities that youth are involved in during the summer months to the point where it also reduces the frequency to engage in delinquent behavior even after the program has ended. Alternatively, it could be the case that the Boston SYEP affects youth behaviors during the summer that are correlated with delinquency and crime. If such behavioral changes are lasting, then this could explain why the reduction in the number of arraignments accumulates over time. I explore this idea further in the next section by identifying which new skills SYEP participants learn over the summer and how these changes are correlated with the relative reduction in arraignments after the program ends.

Exploring SYEP Program Mechanisms Using Survey Data

Assessing Short-Term Behavioral Changes in Skills and Attitudes

The self-reported survey data indicate that youth participating in the Boston SYEP experienced significant improvements across a variety of behavioral skills and attitudes that could plausibly be correlated with the subsequent reduction in criminal arraignments. Table 5 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 I identify short-term behavioral changes in skills and attitudes for those measures 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. 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 measurable improvements in social skills and behaviors that have been shown to be inversely correlated with delinquency and crime—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.

SYEP participants also indicated sizable growth in job readiness skills and academic aspirations during the summer, many of which were significantly greater than those reported by the control group at the end of the summer (see Table 5). This included large increases in the percent reporting that they had prepared a resume and a cover letter, practiced interviewing skills with an adult, developed answers to typical interview questions, and assembled all the key information to apply for a job. And while there was no significant change among participants with regard to their plans to pursue some type of postsecondary education or training after high school, there was a significant shift toward wanting to pursue a four-year college degree. In contrast, the percentage of participants indicating that they planned to work in the fall increased was lower than that of the control group (see Table 5). 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 have been employed during the summer.

Linking Short-Term Behavioral Changes in Skills and Attitudes to Criminal Justice Outcomes

Although participants demonstrated significant gains over the summer in a variety of behavioral skills and attitudes, only those related to better social skills appear to be correlated with subsequent reductions in criminal arraignments. Table 6 reports the results of the mediation analysis specified in equation (3) that estimates the program's impact on the number of arraignments per youth, while separately controlling for improvements in each of the short-term behavioral skills and attitudes (ΔM_i) as well as the preexisting baseline criminal justice outcomes and demographic characteristics. Improvements in social skills such as managing emotions, asking for help, and resolving conflict with a peer were associated with a larger reduction in criminal arraignments for both violent and property crimes. Moreover, the SYEP_i dummy was no longer statistically significant when these variables were included, suggesting that improvements in social skills play a mediating role in reducing subsequent arraignments. In contrast, improvements in other short-term program measures such as job readiness and academic aspirations did not play a meaningful role in reducing the number of arraignments per youth.²²

Although these findings are suggestive, they are largely consistent with recent evidence regarding SYEP program mechanisms. For example, Davis & Heller (2017b) find that youth with positive employment impacts commit more property crime than their control counterparts, yet non-employment beneficiaries show a decline in violent crime. The authors conclude that their results are not consistent with the usual rationales that improved human capital and better labor

²² These findings are robust to alternative measures and specifications. Using more extreme measures of improvement in the short-term behavioral changes in social skills increases the magnitude of the mediating impacts, providing suggestive evidence of dosage effects (see Table A13). Using Treatment-on-the-Treated as the measure of impact (see columns 3 and 4 of Table 6) or restricting the analysis to only the sample of participants who responded to the survey (see Table A14) yield similar results.

market opportunities create a higher opportunity cost of crime. They discuss several alternative mechanisms and conclude that SYEPs can generate substantively important behavioral change, but “for different outcomes, different youth, and different reasons than those most often considered in the literature.” The mediation analysis in this paper shows that changes in social skills are correlated with reductions in crime among the participants, but changes in other skills and attitudes such as job readiness and academic aspirations are not—again contradicting that SYEPs reduce crime by improving human capital and employment opportunities. Instead, it appears that the program’s impact on crime stems from behavioral changes that are likely to differ across groups. Yet neither Davis & Heller (2017b) nor 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

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 criminal justice outcomes. Similar to previous studies, I find that the Boston SYEP has a significant impact on reducing the frequency of criminal arraignments among youth, but not on the likelihood of ever being arraigned. Youth who were randomly selected into the SYEP treatment group experienced significant declines in the number of arraignments for both violent (–35 percent) and property (–29 percent) crime, compared to those in the control group, with a disproportionate impact on males. Moreover, the decrease in criminal activity was not limited to the duration of the program, as would be expected if the program’s primary mechanism were to “incapacitate” youth during the summer. For example, the impact on the number of arraignments for violent crimes does not become statistically significant until six months after the program

ends, suggesting that there may be long-lasting effects that change youth behavior.

By linking the criminal justice records to self-reported survey data on short-term behavioral changes in skills and attitudes, I shed light on how the program achieves better outcomes among the youth being served. I find that 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 reductions in arraignments for both violent and property crimes. Other short-term improvements in job readiness skills and academic aspirations do not play a role in reducing subsequent delinquent or criminal behavior. These findings are consistent with recent work by Davis & Heller (2017a, 2017b) that explores heterogeneity in SYEP outcomes across youth and concludes that the results do not seem consistent with theories of improved human capital and better labor market opportunities creating a higher opportunity cost of crime, but rather support theories related to behavioral change.

However, 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 crime stems from learning new skills on the job or through the career-readiness curriculum (or both)—an important distinction for other cities looking to adopt similar curricula as part of their programs. In addition, understanding the intensity needed to produce better outcomes would help mayors seeking to serve the greatest number of youth, given funding constraints. For example, the state portion of the Boston SYEP funding stipulates 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 direct costs of subsidizing wages as well as indirect administrative costs related to soliciting commitments from employers, matching teens to jobs, and supervising youth at multiple job sites. For example, a recent RCT evaluation of Chicago's Becoming a Man intervention found that the program achieved larger impacts (e.g., reducing violent crime arrests by up to 50 percent) for nearly the same participant cost as the Boston SYEP, resulting in a benefit-cost ratio of up to 30-to-1 (Heller et al., 2017). In contrast, my estimates indicate a benefit-cost ratio for the Boston SYEP that is roughly 3-to-1, although this does not include the added direct costs of arresting, trying, and potentially incarcerating offenders nor the opportunity costs of lower economic productivity for both individuals and their communities due to time forgone in youth detention.

Yet SYEPs have other advantages over alternative programs, providing benefits to individuals, families, and even communities that may further 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 postsecondary education. Third, SYEPs help families at or near the poverty line by providing income to youth—with one in five participants helping to cover household 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 day care for low-income working parents.

Finally, it is important to recognize that the consequences of typical adolescent delinquent behavior are more likely to result in arraignment for inner-city youth, limiting

opportunities that otherwise remain open to teens living in neighborhoods that are policed less aggressively. For example, a sample of gang-involved youth served by Youth Options Unlimited, another Boston SYEP intermediary, demonstrated an even larger reduction in the number of arraignments. While this population was not randomly selected, the results suggest that summer job programs have the potential to affect criminal justice outcomes even among populations where over 40 percent had been arraigned prior to the program. Given that the Workforce Innovation and Opportunity Act of 2014 specifically requires youth workforce-development programs to increase the share of at-risk youth they serve, understanding for whom the program provides the most benefits can guide cities in using limited resources effectively.

Taken together, the outcomes measured by the administrative data and the insights provided by the self-reported survey data can help inform both practitioners and policymakers. For practitioners, understanding what teens learn during the summer through their participation in SYEPs can help establish best practices and improve program efficacy *and* efficiency. For policymakers, articulating how improvements in certain behavioral skills and attitudes translate into a reduction in crime 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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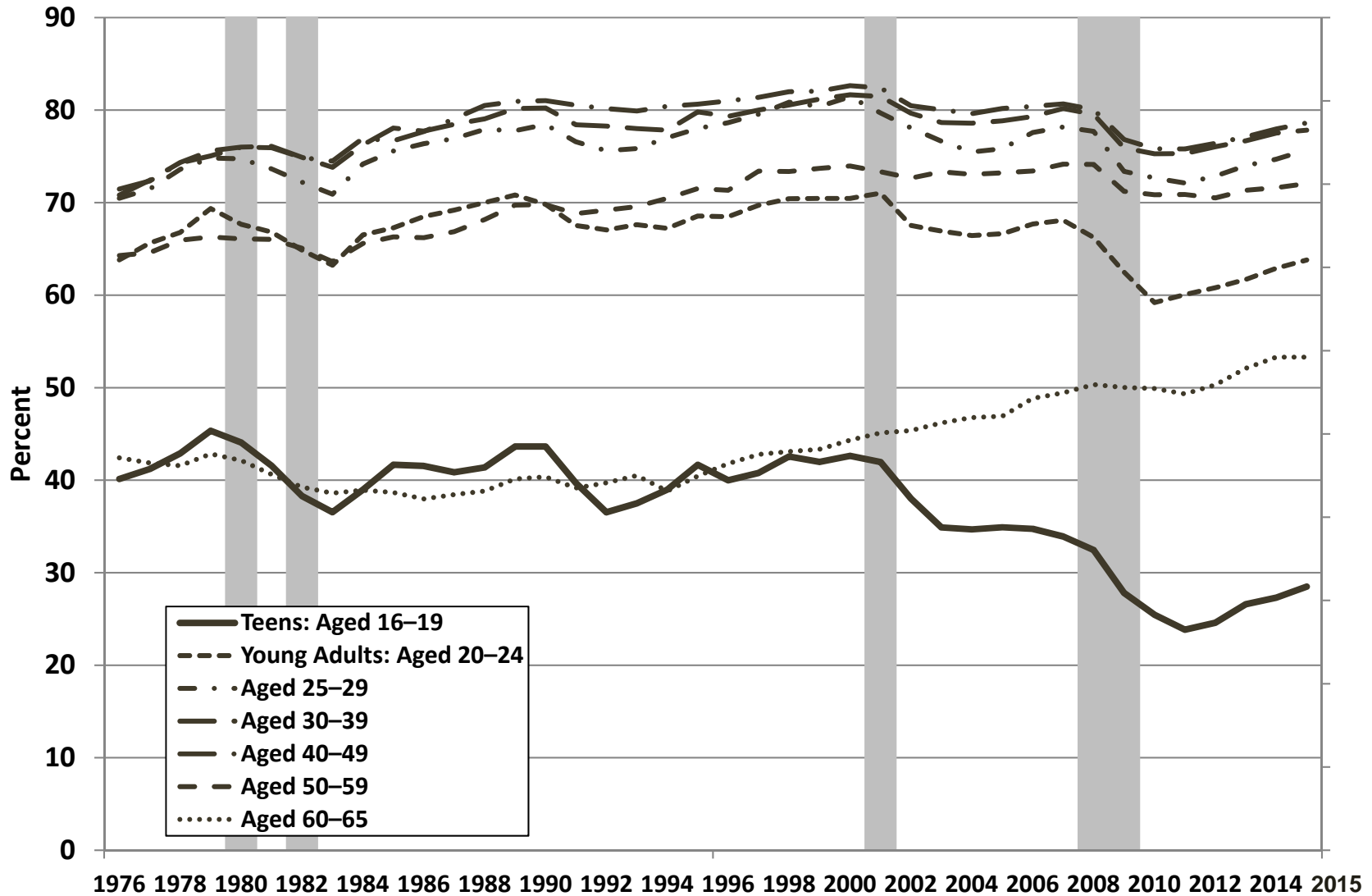
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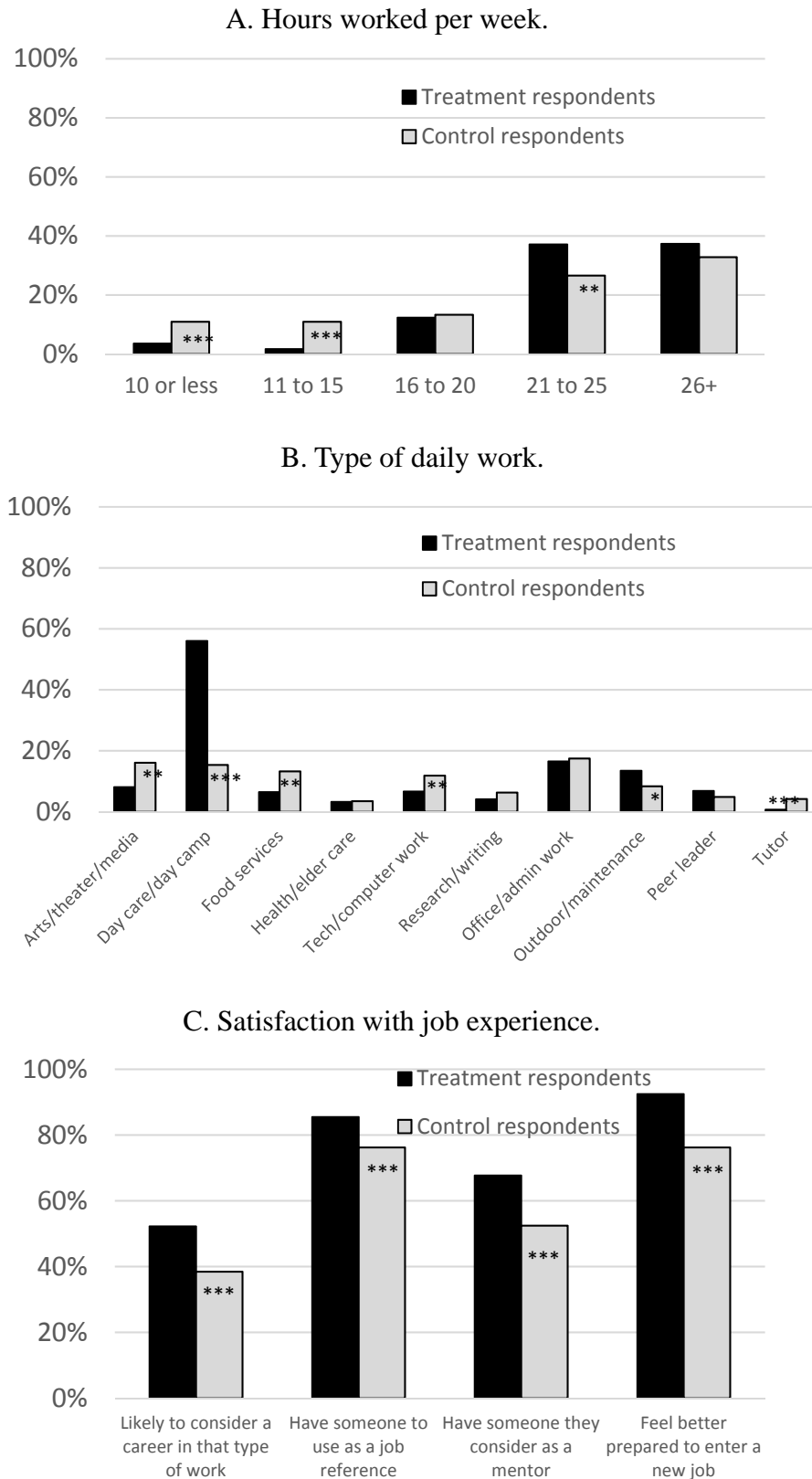
Figure 1. U.S. Employment-to-Population Ratio by Age Group, 1976–2015.



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.

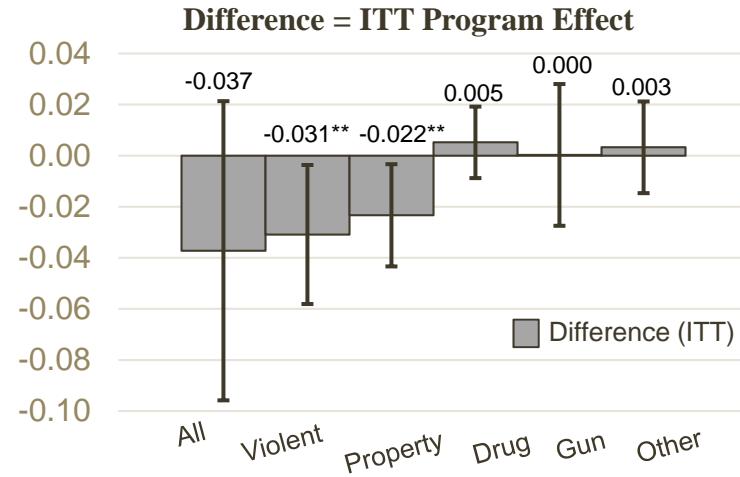
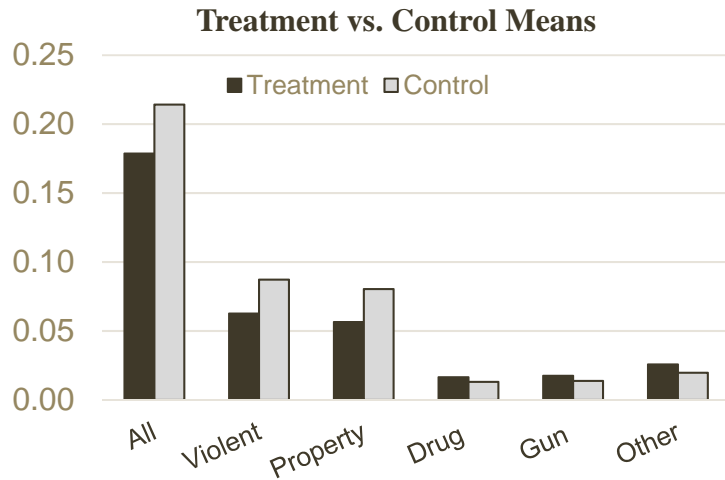
Figure 2. Summer Employment Experiences for SYEP Survey Respondents by Lottery Outcome.



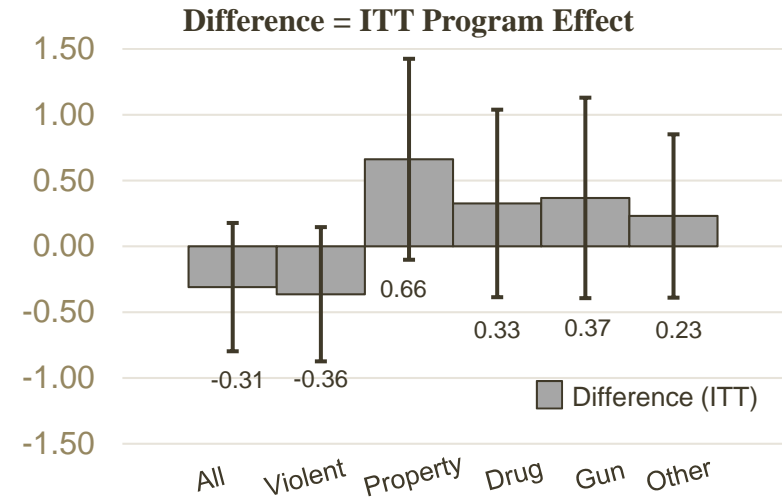
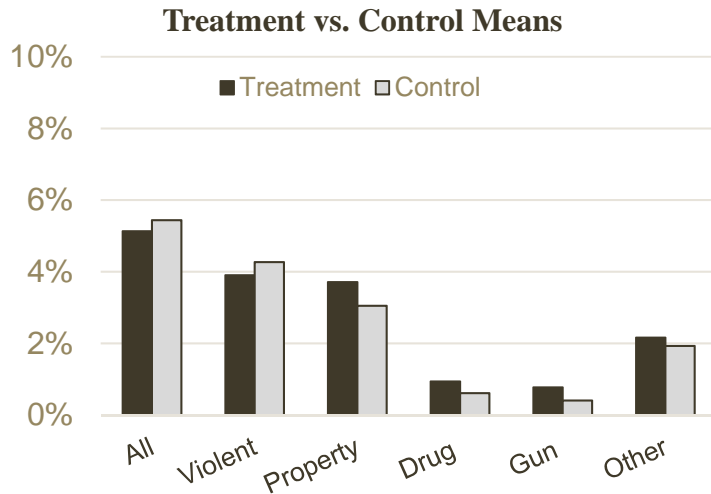
Source: Author's calculations based on survey data provided by the City of Boston, Office of Workforce Development.
Note: *Indicates that the difference is statistically significant at the 10 percent level, ** at the 5 percent level, and *** at the 1 percent level.

Figure 3. ITT Estimates of the Impact of the Boston SYEP on Criminal Activity.

A. Number of Arraignments Per Youth

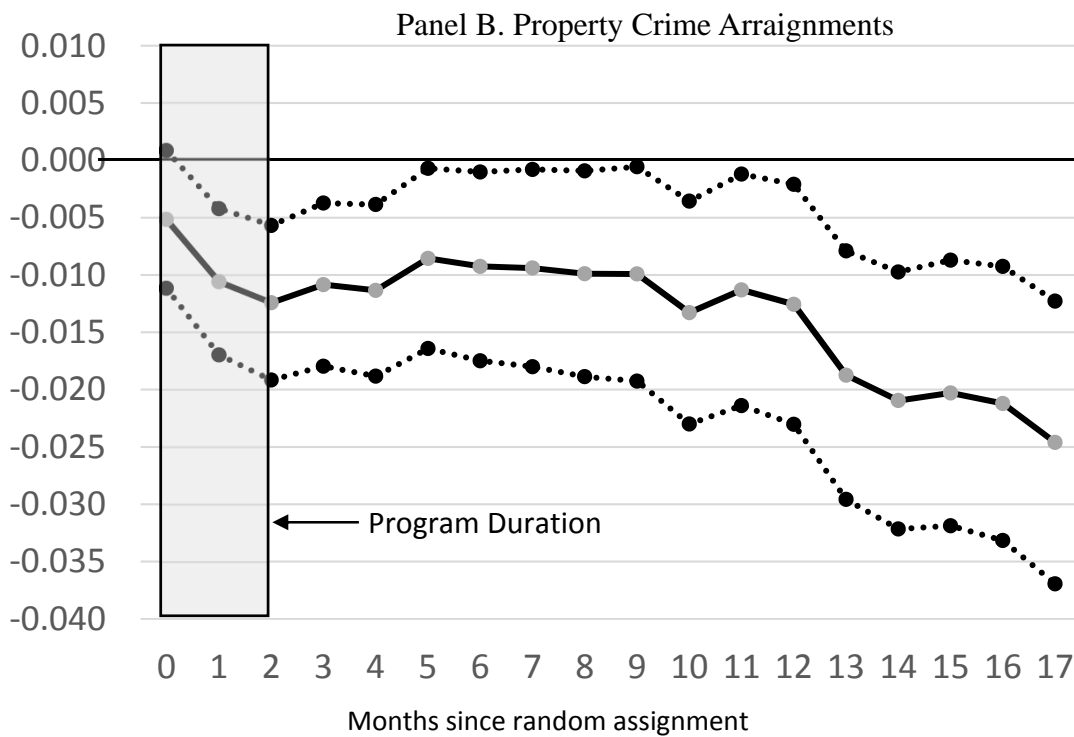
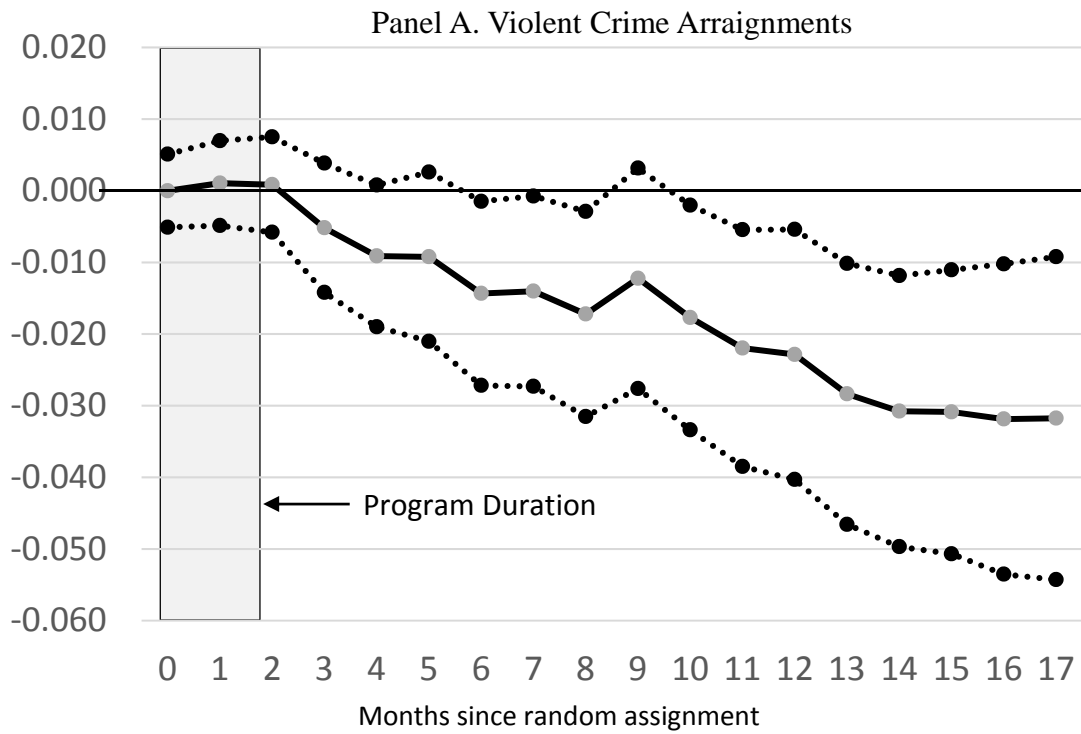


B. Percent of Youth Arraigned



Source: Author's calculations based on data provided by the Department of Criminal Justice Information Services and the Office of the Commissioner of the Probation.
 Note: The left hand bar charts plot the treatment versus control means while the right hand bar charts plot the ITT difference between the two groups estimated where the bars indicate the 95 percent confidence interval. *Indicates difference is statistically significant at the 10 percent level, ** at the 5 percent level, and *** at the 1 percent level.

Figure 4. ITT Estimate of Cumulative Decrease in Arraignments by Type.



Source: Author's calculations based on data provided by the Department of Criminal Justice Information Services and the Office of the Commissioner of the Probation.

Note: Each month is the OLS regression-adjusted cumulative difference in the number of arraignments between treatment and control youth since random assignment and lottery notification in month 0 (June). Regressions include baseline criminal justice outcomes and demographic characteristics. Dotted lines indicate the 95% confidence interval calculated using robust standard errors.

Table 1. SYEP applicant characteristics by lottery outcome.

	Selected (treatments)		Not Selected (controls)		Treatment-Control	
	(1)		(2)		(3)	
	Mean	Std. Error	Mean	Std. Error	Difference	P-value
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
N	1,186		3,049		4,235	

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

Table 2. Baseline criminal arraignments by lottery outcome prior to SYEP participation.

	Treatment group		Control group		Treatment-Control
	(1)		(2)		(3)
	Mean	SE	Mean	SE	Difference
A. Number of arraignments per youth					
All crimes	0.12	(0.031)	0.14	(0.016)	-0.01
Violent crimes	0.07	(0.018)	0.06	(0.009)	0.01
Property crimes	0.04	(0.012)	0.05	(0.006)	0.00
Drug crimes	0.00	(0.001)	0.01	(0.003)	-0.01
Gun crimes	0.00	(0.000)	0.00	(0.001)	0.00
Other crimes	0.01	(0.006)	0.02	(0.004)	-0.01
Misdemeanor	0.06	(0.018)	0.08	(0.010)	-0.01
Felony	0.05	(0.014)	0.05	(0.007)	0.00
B. Percent of youth arraigned for a criminal charge					
All crimes	4.1%	(0.006)	3.6%	(0.004)	0.47
Violent crimes	3.2%	(0.007)	2.8%	(0.004)	0.38
Property crimes	3.1%	(0.005)	2.1%	(0.003)	0.96
Drug crimes	0.7%	(0.002)	0.4%	(0.001)	0.34
Gun crimes	0.4%	(0.002)	0.2%	(0.001)	0.18
Other crimes	1.6%	(0.004)	1.2%	(0.003)	0.39
Misdemeanor	3.6%	(0.007)	3.2%	(0.004)	0.43
Felony	3.6%	(0.007)	2.9%	(0.004)	0.67
N	1,186		3,049		4,235

Source: Author's calculations based on administrative records from the Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.

Note: The pre-program is defined as the 17 months prior to random assignment (February 2014 through June 2015). No differences were statistically distinguishable from zero.

Table 3. ITT program effect on criminal arraignments by type of crime: Number of post-program arraignments per youth.

	OLS Regressions		Poisson Regressions	
	Without covariates	With covariates	Without covariates	With covariates
	(1)	(2)	(3)	(4)
All Crime	-0.037 (0.033)	-0.037 (0.033)	-0.041 (0.038)	-0.041 (0.038)
Violent crimes	-0.031 ** (0.015)	-0.031 ** (0.015)	-0.035 ** (0.017)	-0.035 ** (0.017)
Property crimes	-0.022 ** (0.011)	-0.023 ** (0.012)	-0.027 ** (0.014)	-0.030 ** (0.014)
Drug crimes	0.006 (0.007)	0.005 (0.007)	0.006 (0.006)	0.005 (0.007)
Gun crimes	0.001 (0.015)	0.000 (0.014)	0.001 (0.014)	0.000 (0.013)
Other crimes	0.004 (0.009)	0.003 (0.009)	0.004 (0.009)	0.002 (0.009)
Misdemeanors	-0.025 (0.019)	-0.025 (0.019)	-0.027 (0.023)	-0.026 (0.023)
Felonies	-0.016 (0.017)	-0.017 (0.018)	(0.017) (0.020)	(0.021) (0.020)
Includes baseline outcomes	No	Yes	No	Yes
Includes demographic characteristics	No	Yes	No	Yes
N	4,235	4,235	4,235	4,235

Source: Author's calculations based on data provided by the Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.

Note: Covariates include baseline criminal justice outcomes and demographic characteristics such as age, gender, race/ethnicity, limited English, in school, public assistance, homelessness, and disabled status. Coefficients for Poisson regressions are marginal effects. 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 4. ITT program effect on court involvement by subgroup: Number of arraignments per youth.

	All Youth	Male	Age 18-24	Public Assistance	Homeless	Prior Arraignment
	(1)	(2)	(3)	(4)	(5)	(6)
<u>All Crime</u>						
Treatment	-0.037 (0.033)	-0.007 (0.027)	-0.037 (0.033)	-0.033 (0.034)	-0.031 (0.033)	-0.012 (0.020)
Treatment * Group Dummy	-----	-0.065 (0.068)	0.000 (0.097)	-0.024 (0.097)	-0.202 (0.159)	-0.352 (0.403)
Unadjusted p-value		0.339	0.999	0.803	0.205	0.383
FDR q-value		0.339	0.999	0.803	0.210	0.383
FWER adjusted p-value		0.343	0.999	0.828	0.328	0.375
<u>Violent Crime</u>						
Treatment	-0.031 ** (0.015)	0.003 (0.017)	-0.023 (0.017)	-0.026 (0.017)	-0.026 * (0.015)	-0.002 (0.012)
Treatment * Group Dummy	-----	-0.071 (0.031)	-0.034 (0.038)	-0.027 (0.042)	-0.144 (0.080)	-0.341 (0.152)
Unadjusted p-value		0.022 **	0.371	0.523	0.070 *	0.025 **
FDR q-value		0.066 *	0.557	0.803	0.187	0.075 *
FWER adjusted p-value		0.073 *	0.648	0.828	0.212	0.148
<u>Property Crime</u>						
Treatment	-0.023 ** (0.012)	0.002 (0.012)	-0.007 (0.012)	-0.017 (0.013)	-0.018 (0.012)	-0.005 (0.007)
Treatment * Group Dummy	-----	-0.048 (0.024)	-0.061 (0.031)	-0.019 (0.034)	-0.082 (0.066)	-0.266 (0.149)
Unadjusted p-value		0.050 **	0.047 **	0.571	0.212	0.073 *
FDR q-value		0.075 *	0.141	0.803	0.212	0.110
FWER adjusted p-value		0.145	0.222	0.828	0.328	0.177
Number in subgroup	-----	1,961	848	746	288	272
Rest of sample	4,235	2,274	3,387	3,489	3,947	3,963
Total N	4,235	4,235	4,235	4,235	4,235	4,235

Source: Author's calculations based on data provided by the Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.

Note: Covariates include age, gender, race/ethnicity, limited English, in school, public assistance, homelessness, and disabled status.

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. Assessing Short-Term Behavioral Changes in Skills and Attitudes for SYEP treatment versus control group.

CATEGORY	Treatment			Treatment-Control
	(1)	(2)	(3)	(4)
	Pre-program	Post-program	Difference: post-pre	Difference: post
<u>Community engagement and social skills</u>				
I have a lot to contribute to the groups I belong to	31.9%	46.6%	14.7 ***	15.6 ***
I feel connected to people in my neighborhood	22.0%	36.8%	14.8 ***	21.2 ***
I feel safe walking around my neighborhood	42.9%	46.7%	3.8	19.3 ***
I have a positive role model in my life	91.6%	92.6%	1.0	0.5
I know how to manage my emotions and my temper	44.2%	49.7%	5.5 **	6.5 **
I know how to ask for help when I need it	44.5%	48.7%	4.2 *	11.6 ***
I know how to constructively resolve a conflict with a peer	36.6%	42.2%	5.7 **	13.6 ***
I need to improve my conflict resolution skills	21.6%	6.0%	-15.6 **	-13.0 **
<u>Job readiness skills</u>				
I have all key information to apply for a job	81.0%	88.2%	7.2 **	9.4 ***
I have prepared a resume	40.9%	70.1%	29.3 ***	24.5 ***
I have prepared a cover letter	23.4%	43.7%	20.4 ***	21.7 ***
I have asked an adult to serve as a reference	70.9%	74.5%	3.6	-0.1
I have reviewed at least one job application form	74.8%	82.4%	7.5 **	3.9
I have completed at least one online job application form	66.1%	70.9%	4.8 *	-3.3
I have searched for jobs online	47.7%	59.6%	11.9 ***	2.5
I have asked an adult for help in finding job opportunities	83.0%	84.6%	1.7	7.1 ***
I have developed answers to the usual interview questions	67.9%	77.1%	9.2 **	6.9 ***
I have practiced my interviewing skills with an adult	54.8%	64.9%	10.1 ***	6.4 **
I need to improve my job readiness skills	33.2%	29.4%	-3.8 *	-5.3 *
<u>Future work plans and academic aspirations</u>				
I plan to work in the fall	40.6%	48.0%	7.4 **	-7.4 **
I plan to enroll in an education or training program after high school	67.4%	70.3%	2.9	0.3
I plan to enroll in a four-year college or university	68.1%	73.0%	4.9 *	11.0 ***
I plan to enroll in a two-year college	12.9%	12.4%	-0.5	6.2 ***
I need to improve my academic skills	43.6%	43.4%	-0.2	12.9 ***
N	663	663	663	1,327

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

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

Table 6. Relationship between SYEP impact on short-term behavioral changes and subsequent criminal activity: Number of arraignments per youth.

Dummy variable indicating improvement:	Intent-to-treat estimates				Treatment-on-the-treated estimates			
	(1)		(2)		(3)		(4)	
	Violent crimes		Property crimes		Violent crimes		Property crimes	
	Coefficient	Standard error	Coefficient	Standard error	Coefficient	Standard error	Coefficient	Standard error
<u>Community engagement and social skills</u>								
Contributing to the groups they belong to	-0.012	(0.011)	-0.004	(0.011)	-0.010	(0.014)	-0.001	(0.013)
Connecting to people in their neighborhood	-0.001	(0.012)	0.008	(0.012)	0.002	(0.014)	0.012	(0.013)
Managing emotions	-0.031	(0.011) ***	-0.017	(0.011)	-0.032	(0.014) **	-0.015	(0.014)
Asking for help	0.004	(0.011)	-0.021	(0.011) **	0.008	(0.014)	-0.031	(0.014) **
Resolving conflict with a peer	-0.048	(0.023) ***	-0.025	(0.010) **	-0.051	(0.020) **	-0.039	(0.019) **
Improving conflict resolution skills (overall)	-0.106	(0.044) **	-0.037	(0.021) *	-0.109	(0.029) ***	-0.045	(0.027) *
<u>Job readiness skills</u>								
Having key information to apply for a job	-0.004	(0.013)	0.013	(0.013)	0.009	(0.030)	0.041	(0.029)
Preparing a resume	0.009	(0.011)	-0.008	(0.011)	0.018	(0.016)	-0.005	(0.015)
Preparing a cover letter	-0.005	(0.011)	-0.005	(0.011)	-0.002	(0.014)	-0.002	(0.013)
Developing answers to interview questions	-0.025	(0.014) *	-0.010	(0.013)	-0.029	(0.024)	-0.006	(0.022)
Practicing interviewing with an adult	0.013	(0.011)	0.014	(0.010)	0.023	(0.017)	0.023	(0.017)
Improving job readiness skills (overall)	-0.015	(0.013)	-0.011	(0.012)	-0.013	(0.018)	-0.009	(0.017)
<u>Academic aspirations</u>								
Planning to attend a four-year college	0.006	(0.012)	0.001	(0.011)	0.013	(0.016)	0.007	(0.015)
Includes baseline outcomes	Yes		Yes		Yes		Yes	
Includes demographic characteristics	Yes		Yes		Yes		Yes	
N	4,235		4,235		4,235		4,235	

Source: Author's calculations based on data provided by Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.

Note: Covariates include baseline outcomes and demographic characteristics such as age, gender, race/ethnicity, limited English, in school, public assistance, homelessness, and disabled status. Robust standard errors are in parentheses. See Table A5 in the appendix for the baseline treatment-on-the-treated results.

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