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

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Abstract: Summer youth employment programs (SYEPs) are believed to have the potential to improve the behavioral, economic, and academic outcomes of the populations they serve, particularly for inner-city, low-income, and non-white youth. As part of an embedded randomized controlled trial, during the summer of 2015, I collected survey data for both the treatment and control groups from the Boston SYEP and linked this to administrative data on criminal justice outcomes. In terms of short-term program effects, participants in the program reported improved social skills and attitudes toward their communities, enhanced job-readiness skills, and higher academic aspirations. In terms of longer-term outcomes, those in the treatment group exhibited significant reductions in the number of arraignments for violent crimes (-35 percent) and property crimes (-57 percent) during the 17 months after program participation. In all cases, these gains were significant relative to the control group, and many of the largest gains were among African American and Hispanic males. Moreover, the reductions in subsequent criminal activity were greater for youth in the treatment group who reported positive improvements in social skills during the summer of participation, including how to manage their emotions, ask for help, and resolve conflict with a peer. These results give researchers some insights into a broader set of short-term program effects while also providing a look inside the "black box" as to how SYEPs affect youth over the course of a summer.

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INTRODUCTION

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

In response, mayors in a number of U.S. cities, including Boston, Chicago, New York, and Washington D.C., have looked 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 alternative pathways for youth—whether it be a career or some type of postsecondary education—making 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 diminished economic opportunity arising from non-white teens being disproportionately located in neighborhoods with few job opportunities and failing

schools (Wilson, 1996; Chetty, Hendren, & Katz, 2016). By providing access to employer networks, career mentoring, and skill development, SYEPs aim to provide youth with the tools and experience needed to navigate the job market on their own, with the belief that "nothing stops a bullet like a job" (Cook & Ludwig, 2011, p. 44).

Moreover, SYEPs continue to be an important vehicle 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's 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, Snyder, Berrigan, & Knoll, 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; Schwartz, Leos-Urbel, & Wiswall, 2015; Heller, 2014). While the results of this research have demonstrated encouraging results in some cities, an important limitation of this work has been a lack of information on the mechanisms driving these improved outcomes. This paper adds to this early literature by evaluating the effects of the Boston SYEP on both short-term behavioral impacts as well as longer-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 (-57 percent) crimes among youth in the treatment group relative to the control group during the 17 months after participation. Moreover, these longer-term criminal justice outcomes appear to be linked to improvements in social skills that occur during the summer among participants, as measured by a pre-/post-program survey. Finally, both the short- and longer-term impacts are greater in magnitude for African American and Hispanic males. These results give researchers some insights into a 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 for whom the impacts are the greatest.

This paper is organized as follows: In the first two sections, I provide an overview of the relevant literature and policy context. Next, I explain the Boston SYEP and the lottery process and describe the data and methodology used for this study. I then measure the program's impact on the longer-term criminal justice outcomes as well as the short-term behavioral effects along with an analysis of the relationship between the two. Finally, I conclude with a discussion of the policy implications and next steps for future work.

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 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; 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 engaged in criminal activity. 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, SYEPs occur during the summer months when youth are often idle, reducing opportunities for time that might otherwise be spent engaged in criminal activity. Finally, 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 deficits arising from a lack of opportunities among at-risk youth.

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, academic, and criminal justice outcomes down the road. There are four primary channels through which SYEPs are thought to affect 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 they have to engage in criminal activity or disrupting "routine activities" that provide likely offenders with suitable targets and a lack of supervision or guardianship (Cohen & Felson, 1979; Felson, 1987). In this way, summer employment provides youth with a set of socially productive activities, possibly decreasing the risk of exposure to, or participation in, violence and delinquent behavior (Wilson, 1996).

(2) *Improving behaviors that are 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, Farrington, Ehrlich, & Heath, 2015). SYEPs help develop these relationships by placing youth in jobs that are supported by mentors and program staff. In addition, early work experience, such as that provided by SYEPs, gives participants 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, such as the Boston program, also include a formal curriculum aimed at improving behaviors such as self-efficacy and conflict resolution that are directly correlated with youth violence and delinquency (Lipsey & Derzon, 1998).

(3) *Making crime less attractive by improving future opportunities.* Early work experience can also improve job readiness skills as well as raise career and academic aspirations—both of which can lead to better employment outcomes. Typically, studies find that labor force attachment at an early stage in one's career predicts better labor market outcomes in terms of both employment and earnings later in life (Carr, Wright, & Brody, 1996; Ruhm,

1997; Painter, 2010; Baum & Ruhm, 2014; Sum et al., 2014). Research also shows that greater exposure to employment also gives youth experiences that can shape their goals—whether it be to complete high school, obtain career training, or attend college (Duckworth, Peterson, Matthews, & Kelly, 2007; Heckman, 2008; Lillydahl, 1990; Mortimer, 2010). In addition, the Boston SYEP curriculum specifically helps teens develop work-readiness skills such as exploring career pathways, writing a resume and cover letter, searching for jobs, completing online applications, and interviewing. Finally, SYEPs occur during the summer months when youth are often idle, creating fewer conflicts with academic studies compared to year-round employment programs.[1]

(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 alike to maximize resource allocation. I will advance these theories by shedding light on how the Boston SYEP

[1] The evidence regarding the impacts of early work experience on academic performance during the school year is mixed. It has been shown that when students work too many hours, this ultimately decreases high school graduation and college attendance rates and inhibits later economic success (Mortimer, 2010; Painter, 2010; Stasz & Brewer, 1999). Indeed, the association between hours of work and performance in school appears to follow 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).

[2] Note that it is often not possible to parse out any effect of the income associated with SYEPs from other changes related to the experience itself. Nonetheless, I lay out the main arguments supporting why I 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).

affects youth behaviors during the course of the summer and whether these short-term improvements are related to longer-term reductions to criminal activity.

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 summer jobs programs and have found some encouraging results. 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. For example, Heller (2014) found that participating in Chicago's One Summer Plus program decreased violent crime by 43 percent over 16 months for youth in the treatment group relative to the control group, with much of the decline occurring during the year after participation. Similarly, Gelber et al. (2014) used an embedded RCT to show that participating in the New York City SYEP reduced the probability of incarceration and mortality—the latter achieved by reducing death from "external causes," including homicides, suicides, and accidents.

Other studies have found that SYEP participation is associated with modest improvements in test taking and school attendance, but not college matriculation. For example, Schwartz et al. (2015) found small but significant increases in the share of New York City SYEP participants taking and passing statewide high school exams relative to the control group. A related study demonstrated significant increases of one to two percent in the treatment group's school attendance 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 has indicated that the New York City program did not have a positive effect on college enrollment (Gelber et al., 2014).

Finally, two studies have looked at the link between summer jobs programs and subsequent employment and earnings, finding no persistent positive relationship. The first study found that the New York City SYEP caused average earnings and the probability of employment to increase in the year of program participation but that these effects faded after three years (Gelber et al., 2014). The second study found that the District of Columbia SYEP actually reduced "employability" after the program ended (Sachdev, 2011), but was not based on an RCT design.

While the results of this research have demonstrated encouraging results in some cities—particularly for criminal justice and academic outcomes—a limitation of this work has been a lack of information on the *mechanisms* driving these improved outcomes. Indeed, it is important for policymakers to understand whether SYEPs affect adolescent behaviors that are correlated with criminal activity beyond the income effect of having a summer job. I build on this early SYEP literature by linking self-reported survey data on behavioral impacts to administrative data on criminal justice outcomes to shed light not only on what works but also on what works for whom, under what conditions, and why.

THE BOSTON SYEP INTERVENTION

Introduced in 1990, the Boston SYEP relies on city, state, and private funding (totaling nearly $10 million annually) to connect about 10,000 city teens each summer with roughly 900 local employers. During the summer, participants work a maximum of 25 hours per week for a six-week period, from the beginning of July to mid-August, and are paid the Massachusetts minimum wage. Students 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 youth with job-readiness training using a

hands-on, competency-based work-readiness curriculum called Signal Success.[4] Participants receive at least 20 hours of class time that covers 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.

All Boston city residents aged 14 to 24 years are eligible for the program and our data indicate that applicants are fairly representative of the city's high-school aged population.[5] Youth apply directly to the program through one of the four intermediaries under contract with the City of Boston's Office of Workforce Development (OWD). The intermediaries are responsible for reviewing applications, supervising job placements, and delivering the program's career-readiness curriculum. Youth typically apply to the intermediary in their neighborhood, and our data indicate that less than 5 percent of youth apply to more than one agency.

My analysis will be restricted to youth who applied for a job for summer 2015 through Action for Boston Community Development (ABCD), one of the two intermediaries that make use of random assignment because of the high number of applications they receive for the limited number of SYEP jobs available.[6] 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 random-assignment algorithm to select applicants based on

[4] The curriculum was developed by the Commonwealth Corporation, a state agency, as a result of its 2012 study on teen unemployment and is currently being piloted as part of the regular high school course offerings within the public school system.

[5] Our data indicate that approximately 75 percent of SYEP applicants are Boston Public School (BPS) students—similar to the proportion of high school-aged residents that are enrolled in BPS (Boston Foundation, 2006). The remaining 25 percent of SYEP applicants are either high-school aged students who attend parochial or private school or are older (e.g., age 19-24 years) who are no longer in school.

[6] The other intermediary that uses random assignment, Youth Employment and Engagement (YEE), 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. In addition, YEE did not implement the survey during the summer of 2015 and so we cannot test any of the potential mechanisms for this group.

their applicant ID numbers and the number of available slots as determined by the amount of funding ABCD receives each year. This system effectively assigns the offer to participate in the program at random, creating a control group of youth who apply to the SYEP but are not chosen. Of the 4,235 youth who applied to ABCD in 2015, a total of 1,186 were offered a job via random assignment (28 percent), leaving 3,049 individuals in the control group. Of those selected by the lottery, 83.6 percent accepted a job offer, with only a handful of youth dropping out of the program during the summer.

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.[7] On average, over 85 percent of applicants were in school at the time they applied, with a mean average age just shy of 16 years. A slightly higher percentage of applicants were female, and 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.[8] 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. Across all but one of these observed characteristics, there is no statistically significant difference between those in the treatment group versus the control group (see Table 1). I note that having at least one

[7] It should be noted that compared with the BPS high school population, ABCD applicants are more likely to be African American but less likely to be of limited English ability or live in a household receiving public assistance. This suggests that SYEP applicants may not be fully representative of the overall Boston youth population, which would limit our ability to draw conclusions about the program beyond the study sample.
[8] 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 at the $p<0.10$ level would be expected by random chance when testing 15 different characteristics. I also test for baseline equivalence using separate models estimating the effect of winning the lottery on preexisting student characteristics and outcomes and include estimates for age/gender/race groupings to demonstrate that the sample is indeed balanced across these cells even though there was no blocking to produce this result when the random assignment was performed (Cullen, Jacob, & Levitt, 2006).[9]

To determine whether the Boston SYEP provides a meaningful intervention, Table 2 provides information about the summer employment rates and experiences among those responding to an end-of-summer survey for both the treatment group versus the control group. While all of the respondents in the treatment group worked during the summer, only 26.4 percent of those responding in the control group had worked—perhaps indicating the difficulty for Boston-area youth to secure their own employment during the summer even with a relatively low unemployment rate for the city of Boston. Youth in the control group who were able to find a job generally worked fewer hours per week than SYEP participants, 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. However, participants were significantly more likely than those in the control group to report that they would consider a career in the type of work that they did, had an adult to use as a reference in the future as well as someone they considered a mentor, and felt generally better prepared to enter a new job.

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

[9] See Appendix Table A1. 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/cgibin/jhome/34787.

versus teens who don't (Hotz, Xu, Tienda, & Ahituv, 2002; Bacalod & Hotz, 2006). To address this potential source of bias, my empirical methodology relies on an embedded RCT design that effectively controls for selection while also accounting for changes that might occur during the normal course of youth development. The first phase of the analysis uses administrative data on criminal-justice outcomes that capture the subsequent 17 months after the intervention to assess SYEP impacts on long-term criminal justice outcomes. The second phase of the analysis uses survey data on self-reported behavioral outcomes that occur during the course of the summer to provide insight into program mechanisms that may lead to better criminal justice outcomes.

Although it's questionable to some observers whether a six-week intervention could provide a meaningful turning point to affect youth life-course development, it may be the case that the impact is 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" (Graham, 2014, para. 12). 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 greater policing efforts (Moffitt, 1993). As such, I test for heterogeneous impacts by age/gender/race groupings for both the short-term and longer-term outcomes.

Assessing SYEP Impacts on Criminal Justice Outcomes Using Administrative Data

Data for the first phase of the analysis come from criminal-justice records obtained from the Massachusetts Department of Criminal Justice Information Service (DCJIS) and the Office of the Commissioner of Probation, which provide information on all court-related activity for an individual including both adult and juvenile records. 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 for the type of crime (e.g., violent, property, drug, gun, and other). Similar proportions of ABCD youth were found to have 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 been arraigned in court before July 2015.[10]

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 SYEP placements (the treatment group) with the records for youth not offered placements (control group). Because SYEP participation is allocated via lottery, I am able to obtain causal estimates using a simple comparison of means on the outcome of interest. This "Intent to Treat" (ITT) estimate measures the impact of *offering* the program on the outcome. In many cases, this is the policy relevant estimate because program administrators want to account for program take-up to assess the degree to which SYEP could reduce violence among all the applicants, not just the participants.[11]

I measure two outcomes of interest: whether an individual has been arraigned for any crime during the post-intervention period, and the number of arraignments per youth during the post-intervention period. 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 include individual characteristics and improve the precision of my estimates using equation (1) below:

[10] It should be noted that the criminal record data measures criminal activity only to the extent that an individual was arrested and booked. It does not capture criminal activity that went undetected by police nor encounters with the police that did not result in official documentation. In addition, arraignments 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.

[11] Nonetheless, because not all youth end up participating, the ITT will understate the effects of actually participating in the program for those youth who choose to participate. I also provide estimates of treatment-on-the-treated.

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

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 pre-existing demographic characteristics collected by ABCD when youth applied to the program, and μ_{it1} is a stochastic error term

Finally, I exploit the richness of the data to perform several analyses aimed at testing some of the proposed program mechanisms that were discussed above. First, I track the cumulative number of arraignments occurring after random assignment for both the treatment and control groups to determine whether the results are driven primarily by reductions in crime during the program, when youth are afforded fewer opportunities for crime. Second, I also compare a measure of recidivism between the two groups to assess whether SYEPs operate primarily as a preventive or a rehabilitative intervention. Finally, given that SYEPs could affect youth behavior with regard to social interactions differently from economic and situational factors, I estimate program effects separately by offense type (violent, property, drug, gun, and other). 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. I further test these assumptions in the second part of the analysis.

Exploring SYEP Program Mechanisms Using Survey Data

For this part of the analysis, I link the criminal justice outcomes to the short-term behavioral impacts observed during the course of the summer for the treatment group, as measured by a pre-/post-program survey. Whereas the first part of the analysis using administrative data established

the causal impacts of the Boston SYEP on criminal activity, the goal here is to provide a glimpse inside the black box with regard to *how* the program achieves these outcomes. Because it is necessary to rely on self-reported survey data to assess the short-term behavioral impacts, this second part of the analysis should be regarded as more exploratory in nature.

The survey was originally developed and implemented by the Youth Violence Prevention Collaborative, an initiative that began funding summer employment opportunities in 2012 in Boston neighborhoods that had been identified by the Boston Police Department as having a high number of fatal and nonfatal shootings. The goal was to measure personal and social behaviors that are correlated with youth violence to determine whether summer employment could reduce the exposure of economically disadvantaged teens to risky, violent, and delinquent behaviors. I built on this original framework to expand the survey's content, adding questions related to job readiness as well as postsecondary aspirations. The survey was administered to participants at both the beginning and the end of the summer to measure changes over time, and to the control group at the end of the summer to provide a point of comparison.[12]

To explore the whether 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, due to program restrictions the pre-survey was not administered to the control group. Therefore, I first determine whether the program significantly affected the self-reported outcomes of participants over time and then test whether the post-program measurements of these outcomes were significantly different from those of the control group. Finally, I link the self-reported survey outcomes of the participants to the administrative data to determine whether any of the short-term behavioral impacts that were

[12] Please see the Data Appendix for more details about the survey data. 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/cgibin/jhome/34787.

shown to be significant are correlated with improvements in the longer-term criminal justice outcomes.

Assessing Short-Term Behavioral Impacts

To initially determine whether the program has an impact on youth behavior during the summer, I estimate changes over time for the treatment group by performing a simple comparison of means on the outcome of interest based on a matched sample of the pre- and post-survey responses. I then compare the outcomes measured at the end of the summer for both the treatment and control groups to indicate whether these improvements are attributable to the Boston SYEP versus that which would occur during the course of the typical adolescent's development. Outcomes for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group are identified as the short-term behavioral impacts of the Boston SYEP program.

There are several potential sources of bias arising from this analysis. First, it might be the case that the individuals in the treatment group that responded to the survey differ from those that 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 application characteristics for the entire treatment group versus those responding to both the pre- and post-survey (see Table A2 in the appendix).[13] Thus, changes in behavioral outcomes measured over the course of the summer for the treatment group are likely to be unbiased. Since these variables are self-reported, it could still be the case that measurement error might exist. However, if we assume that the measurement error is random, this would reduce efficiency but not cause bias.

[13] 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/cgibin/jhome/34787.

A second source of bias could arise from the differential response rates of the treatment versus the control groups. Indeed, although the number of respondents among the control group was similar (N=664), this represented a response rate of only 21.8 percent. Moreover, although the control group was randomly selected, those who chose to respond to the post-survey were not. Survey respondents from the control group exhibited characteristics indicating *positive* selection relative to the treatment group. They were more likely to be older, identify as white or Asian, and indicate that they live in a two-parent household (see Table A3 in the appendix).[14]

For the purposes of my analysis, I note that this bias goes *against* our finding an impact for the SYEP, given that the survey respondents in the control group exhibit more positive demographic characteristics, setting a high bar for comparison. Nonetheless, to minimize this selection bias regarding the differential survey response, I control for observable characteristics using a regression framework similar to that used for the longer-term outcomes:

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

where M_{it} is one of the short-term program outcomes (e.g., social skills), $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, and X_{it} is a set of demographic characteristics collected at the time of the survey. Because survey respondents in the control group were positively selected, the coefficient π_2 is likely to provide downward-biased estimates of the program's impact on each short-term behavioral outcome.

Linking Short-Term Behavioral Impacts to Criminal Justice Outcomes

Ideally, a full mediation analysis would be used to generate evidence for how the SYEP program achieved its effects using measures of the mediating variable as well as the dependent and independent variable (Baron & Kenny 1986). First, a significant relationship is estimated

[14] 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/cgibin/jhome/34787.

between the dependent variable of interest (Y_{it}) and the independent variable ($SYEP_i$) using equation (1) above. Second, a significant relationship is estimated between the hypothesized mediating variable (M_{it}) and the independent variable ($SYEP_i$) using equation (2) above. Third, the mediating variable (M_{it}) is shown to be significantly related to the dependent variable (Y_{it}) when both the independent variable and mediating variable are include as predictors:

$$Y_{it} = SYEP_i \pi_3 + X_{i(t-1)} \beta_3 + M_{it} \gamma + \mu_{it3} \quad (3)$$

If M_{it} is a valid mediator, then the coefficient on $SYEP_i$ in equation (3) must be smaller (in absolute value) than the coefficient on $SYEP_i$ in equation (1) without the mediating variable.

Due to data limitations, I am unable to undertake the typical mediation analysis described above. This is because the post-survey was administered to the control group anonymously, rather than confidentially as was done for the treatment group. As such, I can only link the survey responses to the longer-term criminal justice outcomes for youth in the treatment group who responded to the survey. Nevertheless, it is still possible to explore whether improvement in the short-term behavioral impacts are correlated with subsequent improvement in the criminal justice outcomes to shed light on the program's mechanisms. I do this in three ways. First, I modify equation (3) as follows:

$$Y_{it} = SYEP_i \pi_4 + X_{i(t-1)} \beta_4 + \Delta M_i \delta + \mu_{it4} \quad (4)$$

On the left-hand side, the dependent variable is one of the longer-term criminal justice outcomes (e.g., number of crimes per youth) while on the right-hand side is a dummy indicating positive improvement for a specific short-term program impact ΔM_i (e.g., ability to resolve conflicts with a peer). A negative and significant coefficient on ΔM_i indicates that improvement in the short-term program impact observed during the summer of participation is negatively correlated with longer-term criminal behavior. Moreover, if the coefficient on the $SYEP_i$ dummy in equation (4)

is smaller in magnitude than that in equation (1), this would suggest that ΔM_i plays a role in achieving the longer-term impact separate from simply being assigned to treatment. Note that this specification implicitly assumes that there was no change in the short-term program measures for youth in the control group. I argue that this assumption is plausible if the analysis is restricted to those short-term program impacts for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group by the end of the summer.

However, only youth in the treatment group who actually participated will have responded to the survey. As such, it is still possible that the observed changes in the short-term program measures from the survey correlate with other unobserved factors (e.g. motivation to participate) that are driving the longer-term reduction in criminal behavior. To address this, I modify equation (4) and use two-stage least squares to estimate the impact of the short-term behavioral impacts on the longer-term criminal justice outcomes using the SYEP treatment dummy as an instrument for participation and include ΔM_i as a control.[15] Again, if the coefficient on ΔM_i is negative and significant and the coefficient on the $SYEP_i$ dummy is smaller in magnitude than that in equation (1), this would suggest that ΔM_i is a potential mediator.

Finally, I test whether these same short-term program measures are driving the reduction in crime among program participants completing both surveys using the following equation:

$$Y_{it} = X_{i(t-1)}\,\beta_5 + \Delta M_i\,\zeta + \mu_{it} \qquad (5)$$

[15] Please see the Data Appendix for more details about the analysis of treatment on the treated. 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/cgibin/jhome/34787.

RESULTS

Assessing SYEP Impacts on Criminal Justice Outcomes Using Administrative Data

Similar to previous studies, I find that the Boston SYEP has a significant impact on reducing the frequency of criminal arraignments among youth. Violent-crime arraignments among the treatment group was 35 percent lower relative to the control group, with roughly 2.5 fewer arraignments per 100 youth (Figure 2). Although the magnitude was similar for property crimes (-2.4 fewer arraignments per 100 youth), the percentage decline was even greater (-57 percent). There were no significant changes in arraignments for other types of crimes (gun, drug, or other). Interestingly, similar reductions in arraignments were observed regardless of the seriousness of the crime (i.e., misdemeanor versus felony).

The decrease in criminal activity was not limited to the duration of the SYEP program, as would be expected if the program's primary mechanism were to "incapacitate" youth during the summer by giving them less opportunity to engage in delinquent behavior. If this were the case, treatment group participants would return to their prior behavioral patterns once the program ended, so that we would observe a relative decrease in arraignments for the treatment group during the program, after which there would be no significant difference between the treatment and control groups. Instead, the number of arraignments for the treatment group continued to fall relative to the control group during the post-program period. Figure 3 graphs the cumulative treatment effect over time, with each point adding an additional month of data to the prior effect. For violent crimes, the drop 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 drop in arraignments becomes significant at month four, after which it accumulates more slowly through

month 13. The downward slope of the effect makes it clear that the majority of the reduction in criminal activity accrues well after the end of the program, at month two.

Across subgroups, significant reductions in arraignments for both violent and property crime were found among African-American and Hispanic males of varying ages. Among African-American males aged 14–18 years, youth in the treatment group experienced 6.1 fewer arraignments per 100 youth during the post-program period compared to their peers in the control group, primarily due to a decline in violent crime (see Table 3). Older African-American males aged 19-24 years demonstrated a relative drop in property crimes (-8.9 fewer arraignments per 100 youth). Similarly, Hispanic males aged 19–24 years posted the largest relative reduction in overall criminal activity (-13.9 fewer arraignments per 100 youth) including significantly fewer violent and property crimes. No significant impacts were detected for females who have much lower arraignment rates.

Yet in terms of the propensity to commit *any* crime during the post-period, there was no significant reduction in the percent of youth arraigned for the treatment group versus the control group. During the 17-month follow-up period, approximately 5.1 percent of the treatment group (n=53 youth) was arrested for any crime compared to 5.4 percent of the control group (n=165 youth). Thus, it appears that the SYEP affects youth primarily 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. Indeed, Table 4 shows that 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 for both the treatment and control groups. Moreover, the re-arraignment rate for both

groups was very similar: 43 percent of individuals with a prior criminal record in the treatment group was arraigned during the post-period compared to 48 percent in the control group.

What might be driving this result? It could be the case that participating in the SYEP disrupts some of the activities that youth might be 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. Yet we see little reduction in the number of arraignments *during* the program—the impacts do not become significant until four to six months after the program has ended. Alternatively, it could be the case that the Boston SYEP affects youth behaviors during the course of the summer that are correlated with delinquency and crime. If such behavioral changes are lasting, then this could explain why we observe a cumulative reduction in the number of arraignments over time. We explore this idea 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 relative reduction in arraignments over the longer-term.

Exploring SYEP Program Mechanisms Using Survey Data

Assessing Short-Term Behavioral Impacts

In previous work, I assessed the short-term behavioral impacts of the Boston SYEP using data from a survey that was administered to both the treatment and control groups to assess social skills and community engagement, job readiness skills, future work plans, and aspirations to attend college (Modestino and Nguyen 2016). The survey was administered to participants at both the beginning and the end of the summer to measure changes over time, and to the control group at the end of the summer to provide a point of comparison. Outcomes for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group were attributed to the Boston SYEP program.

During the course of the summer, participants' attitudes toward their communities improved greatly and these outcomes were significantly better than those reported by the control group at the end of the summer. For example, the percent of participants who said that over the past 30 days they *always* had a lot to contribute to the groups to which they belonged jumped by 15 percentage points (see Table 5). Similar positive improvements were also seen in the share of teens who said they always felt connected to their neighborhood. In retrospect, these large improvements are perhaps not so surprising, given that most SYEP job placements are with community-based organizations in the neighborhoods in which participants live, providing an opportunity for youth to engage in their communities in a positive way.

Although smaller in magnitude, participants also showed measurable improvements in social skills and behaviors that have been shown to be correlated with delinquent and criminal behavior and this progress was significant relative to the control group. For example, by the end of the summer a greater share of youth reported knowing how to manage their emotions, how to ask for help when they needed it, and how to constructively resolve conflict with a peer (see Table 5). In addition, there was a large and significant reduction in the percentage of youth indicating that they needed to improve their conflict resolution skills (-15.6 percentage points).

Participants also indicated sizeable growth in job readiness skills during the summer, many of which were significantly greater those reported by the control group at the end of the summer. These included large increase in the percent of participants reporting that they had prepared a resume (+29.3 percentage points) and a cover letter (+20.4 percentage points). More modest but significant improvements were also observed in the percent of participants who had practiced interviewing skills with an adult, developed answers to typical interview questions, and assembled all the key information to apply for a job (e.g. social security number). There was also

a small and marginally significant reduction in the share of youth indicating that they needed to improve their job readiness skills.

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, this was actually lower the share reported by the control group at the end of the summer (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 report being employed during the summer. In fact, by enabling youth to shift their work experiences to a part of the year when they are not also attending school, the Boston SYEP might enable youth to increase the time and attention that they can devote to academics during the school year.

In terms of academic aspirations, the Boston SYEP appears to affect college-going plans on the intensive margin rather than the extensive one. 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 (+4.9 percentage points). As a result, the percentage of youth in the treatment group aspiring to attend a four-year college was significantly higher than that reported by the control group at the end of the summer.

In sum, the 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 longer-term reduction in subsequent arraignments. In addition, the largest gains in these short-term measures were among males, non-white youth, and younger teens—similar to what was observed in the arraignment data (see Figures A1-A3 and Table A4 in the appendix).[16]

[16] 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/cgibin/jhome/34787.

Evaluation of Program Mechanisms

There are a number of rationales that have been offered as to why summer jobs programs might improve delinquent behaviors that are correlated with criminal activity: (1) reducing opportunities to engage in delinquent or criminal behavior; (2) improving behaviors that are correlated with crime; (3) making crime less attractive by improving future employment prospects; and (4) providing direct income support to youth and their families. While it is hard to say for certain which of these channels is the primary one without a full analysis of mediator effects, in this section I present some suggestive evidence regarding the relationship between changes in youth behavior for participants and subsequent arraignments after the program ends.

Although participants demonstrated significant gains in a variety of short-term program outcomes according to the survey data, only those related to better social skills appear to be correlated with subsequent reductions in criminal arraignments. Table 6 reports the results of estimating the program's longer-term impacts when including a dummy variable indicating positive improvement for each of the specific short-term program impacts (ΔM_i) for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group. Improvements in social skills such as managing emotions, asking for help, and resolving conflict with a peer were found to be negatively correlated with the number of arraignments per youth for both violent and property crimes. Moreover, the $SYEP_i$ dummy was no longer statistically significant when these social skill measures were included, suggesting that improvements in social skills play a mediating role in reducing subsequent arraignments that is separate from simply being assigned to treatment. This finding is robust to using the SYEP dummy as an instrument for participation when estimating the program's long-term impact on criminal activity for those that were actually treated. In

contrast, improvements in other short-term program measures such as job readiness and academic aspirations did not appear to play a meaningful role in reducing the number of arraignments per youth.

As a final robustness check of the program's mechanisms, Table 7 report the relationship between improvements in the same short-term program measures and the number of arraignments per youth for program participants completing both the pre- and post-program surveys.[17] Again, the same relationship is observed where participants who reported significant gains in social skills during the summer also experienced a reduction in arraignments during the 17 months after the program ended. Although these findings are suggestive, this analysis does not 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 jobs programs on low-income inner-city youth in terms of both short-term program outcomes as well as longer-term delinquent and criminal behavior. I find that the Boston SYEP impacts youth in many of the ways that it was designed to—both in the short-term and in the longer-term. In terms of short-term impacts achieved during the summer, participants in the program reported improvements in community engagement and social skills, job-readiness skills, and their aspirations to attend college that were significant relative to the control group. These impacts were greater in magnitude for males, non-white youth and younger teens. In terms of the longer-term program impacts, youth who were randomly selected into the SYEP treatment group experienced significant declines in the number of arraignments for both violent crime (-35 percent) and property crime (-57

[17] I find similar impacts when restricting the sample to all participants regardless of survey completion.

percent), compared with those in the control group, with even greater improvements observed for African American and Hispanic 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 by limiting their opportunity to engage in delinquent behavior during the summer. Instead, the impact of the program on the number of arraignments does not become statistically significant until four to six months after the program ends—again suggesting that the program may have long-lasting effects that change youth behavior. In addition, state wage and employment record data show that youth in the treatment group were *less* likely to be employed during the year following their participation in the program relative to the control group, which would also argue against the incapacitation mechanism.

By linking the survey data on short-term program impacts to administrative data on longer-term criminal justice outcomes, I am able to 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 the longer-term reduction in arraignments for both violent and property crimes. Moreover, the treatment dummy is no longer statistically significant with inclusion of these short-term program measures, suggesting that they play a mediating role in reducing subsequent arraignments. Other short-term program measures such as job readiness and academic aspirations that reflect the opportunity cost of engaging in crime did not appear to play a role in reducing delinquent or criminal behavior in the long run. To my knowledge, this is the first study that provides any insight into *how* SYEPs affect youth during the summer and how

these short-term program effects produce improvements in longer-term criminal justice outcomes.

However, there are a number of remaining questions that pertain to different features of the program that are important to answer as practitioners seek to improve summer jobs programs. For example, it's difficult to tell whether the program's impact stems from learning new skills on the job or through the career-readiness curriculum—an important distinction for other cities, such as Los Angeles and Philadelphia, that are considering adding a similar curriculum 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 be helpful for 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" (i.e., number of summers) needed to achieve meaningful impacts while also helping to alleviate oversubscribed programs.

Moreover, it's not clear how the Boston SYEP compares with other interventions that do not involve the added direct costs of subsidized wages as well as the indirect 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, 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 (about $2,000 per participant) with a benefit-cost ratio of up to 30-to-1 (Heller et al., 2017). Yet

given the costs of juvenile detention (about $241 per day per youth[18]), it may be that the benefits associated with the reduction in criminal activity is enough to justify the relatively low cost of SYEPs compared with other year-round workforce-development programs for youth (roughly $6,500 per participant[19]). Not to mention 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 as well as the disruption of the process that normally allows many youth to "age out" of delinquent behavior.

SYEPs also have other advantages over alternative programs, providing benefits to individuals, families, and even communities that may outweigh the costs. First, unlike year-round programs, SYEPs occur during the summer months, when youth are likely to be idle, and thus 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 contribute directly to their household's expenses, according to our survey. Fourth, SYEPs supply a low-cost source of labor for many community-based programs serving cities, particularly summer camps that provide inexpensive daycare for working parents.

Finally, it appears that the program's impacts on delinquent and criminal activity are driven primarily by improvements among non-white and court-involved youth such that targeting SYEPs may help level the playing field for these groups. This is particularly important because the consequences of typical adolescent delinquent behavior is more likely to result in arraignment for inner-city youth of color, limiting opportunities that otherwise remain open to

[18] According to the American Correctional Association's 2008 Directory.
[19] According to the Boston Mayor's Office of Workforce Development.

teens living in neighborhoods that are policed less aggressively. In addition, a sample of court-involved youth served by another Boston SYEP intermediary demonstrated an even larger reduction in the number of arraignments and a lower re-arraignment rate. While this population was not randomly selected, the results suggest that the summer jobs program has the potential to affect behavior even when over 40 percent of youth had been arrested 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 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 survey data can provide a collage of evidence to 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 efficiency throughout the program. For policymakers, being able to articulate which short-term program outcomes translate into better long-term outcomes may lead to a more effective intervention that can be scaled up to produce better outcomes at a reduced cost and serve as the basis for pay-for-performance contracting as is currently being explored in Boston. Whether SYEPs should be a light-touch/high-volume intervention or something deeper and more targeted can be determined only with better insights into how SYEPs impact youth and for whom those impacts are the greatest. As such, the findings from this paper, as well as the larger Boston SYEP evaluation, will hopefully help guide program development aimed at employing youth in cities across the nation.

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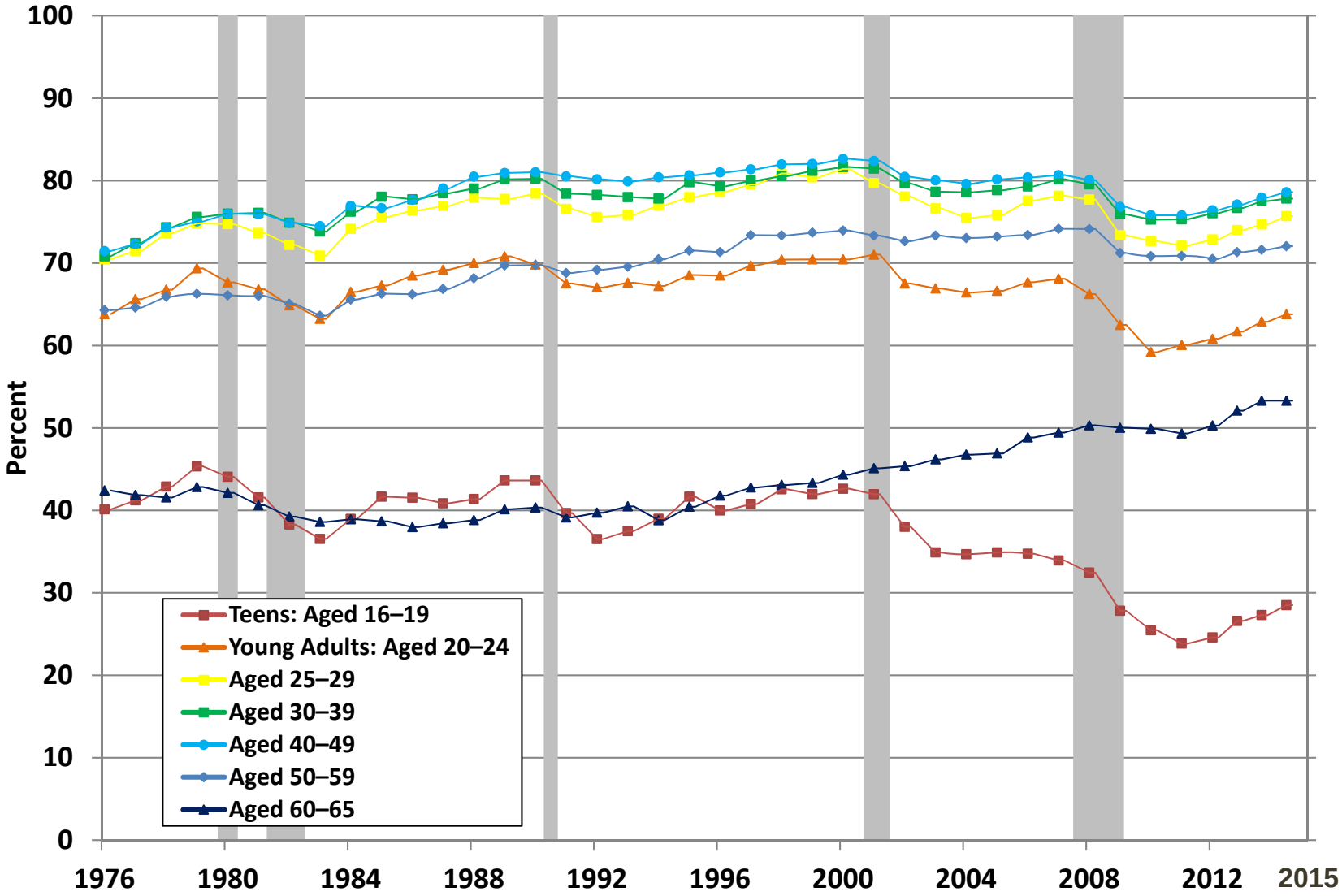
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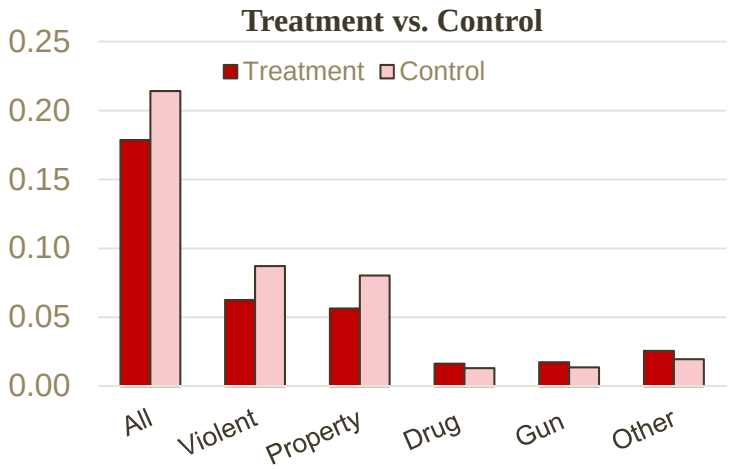
Figure 1. U.S. Employment-to-Population Ratio by Age Group, 1976–2015.

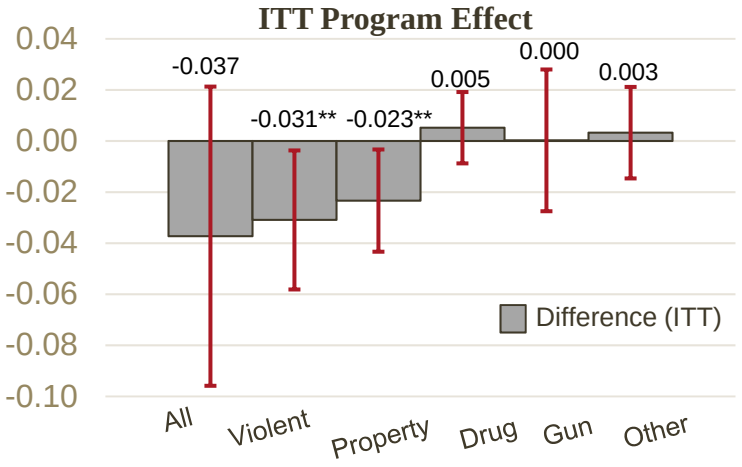


Source: Author's calculations from the U.S. Census Bureau, Current Population Survey, various years.
Note: Shaded bars represent recession periods as defined by the National Bureau of Economic Research.

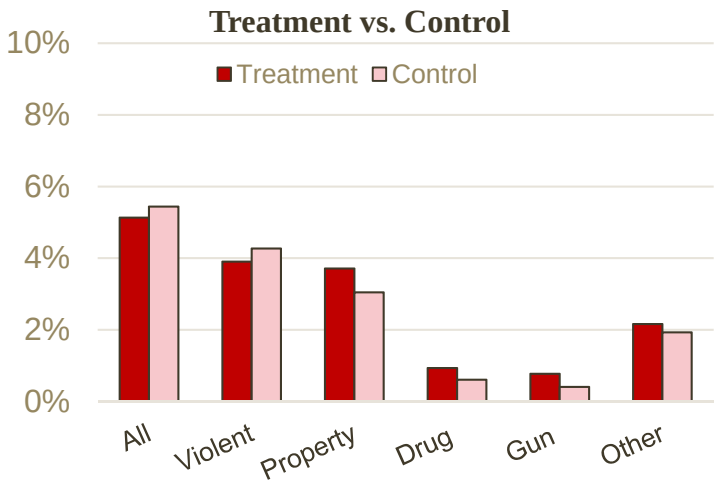
Figure 2. ITT Estimates of the Impact of the Boston SYEP on Criminal Activity.

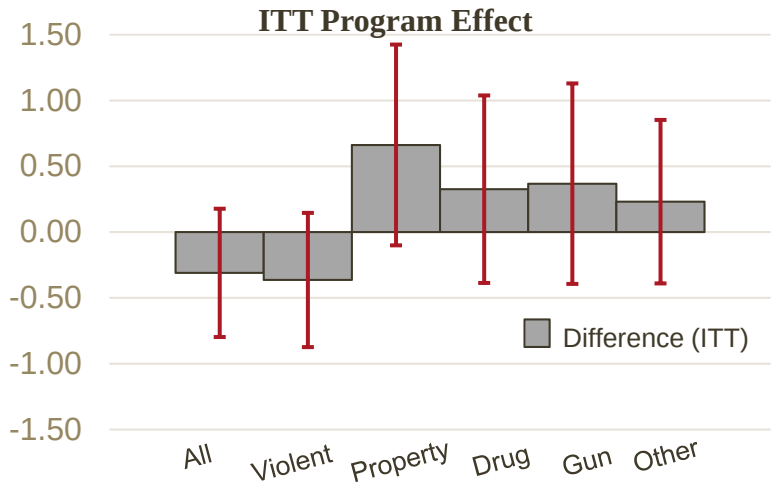
Panel A. Number of Arraignments Per Youth





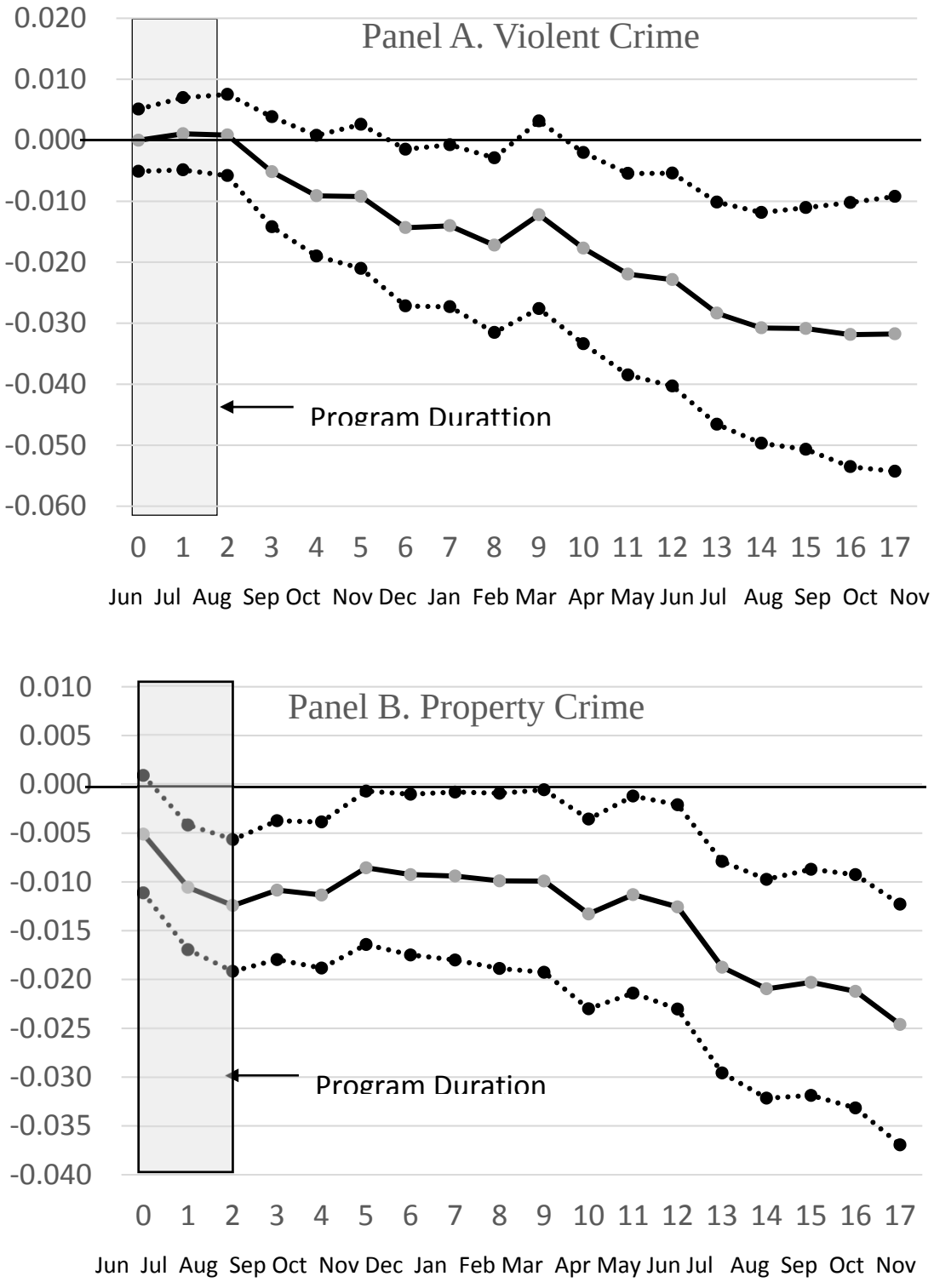
Panel B. Percent of Youth Arraigned





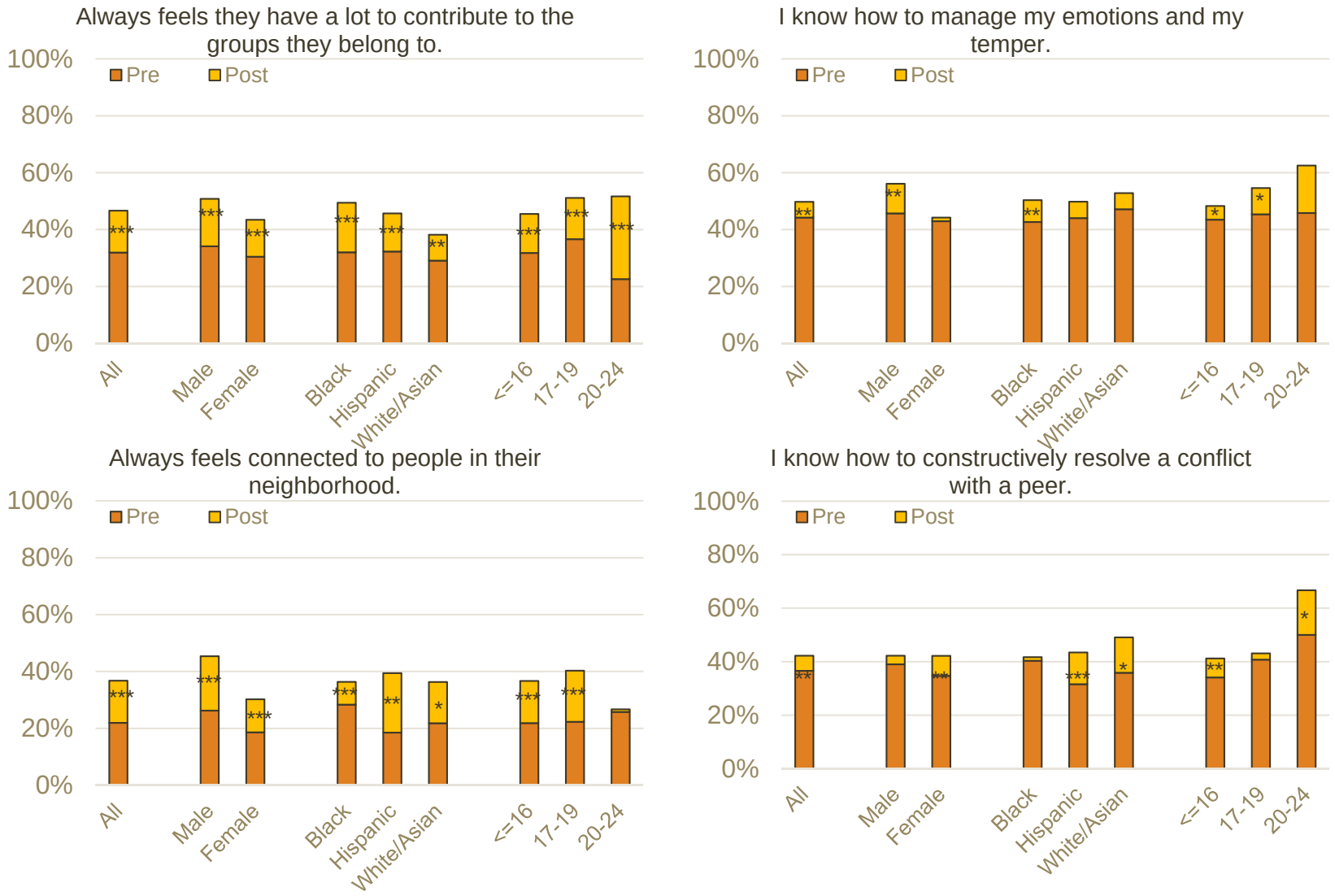
Source: Author's calculations based on data provided by the Department of Criminal Justice Information Services and the Office of the Commissioner of the Probation.
Note: *Indicates difference is statistically significance at the 10 percent level; ** at the 5 percent level; and *** at the 1 percent level.

Figure 3. ITT Estimate of Cumulative Decrease in Arraignments by Type.



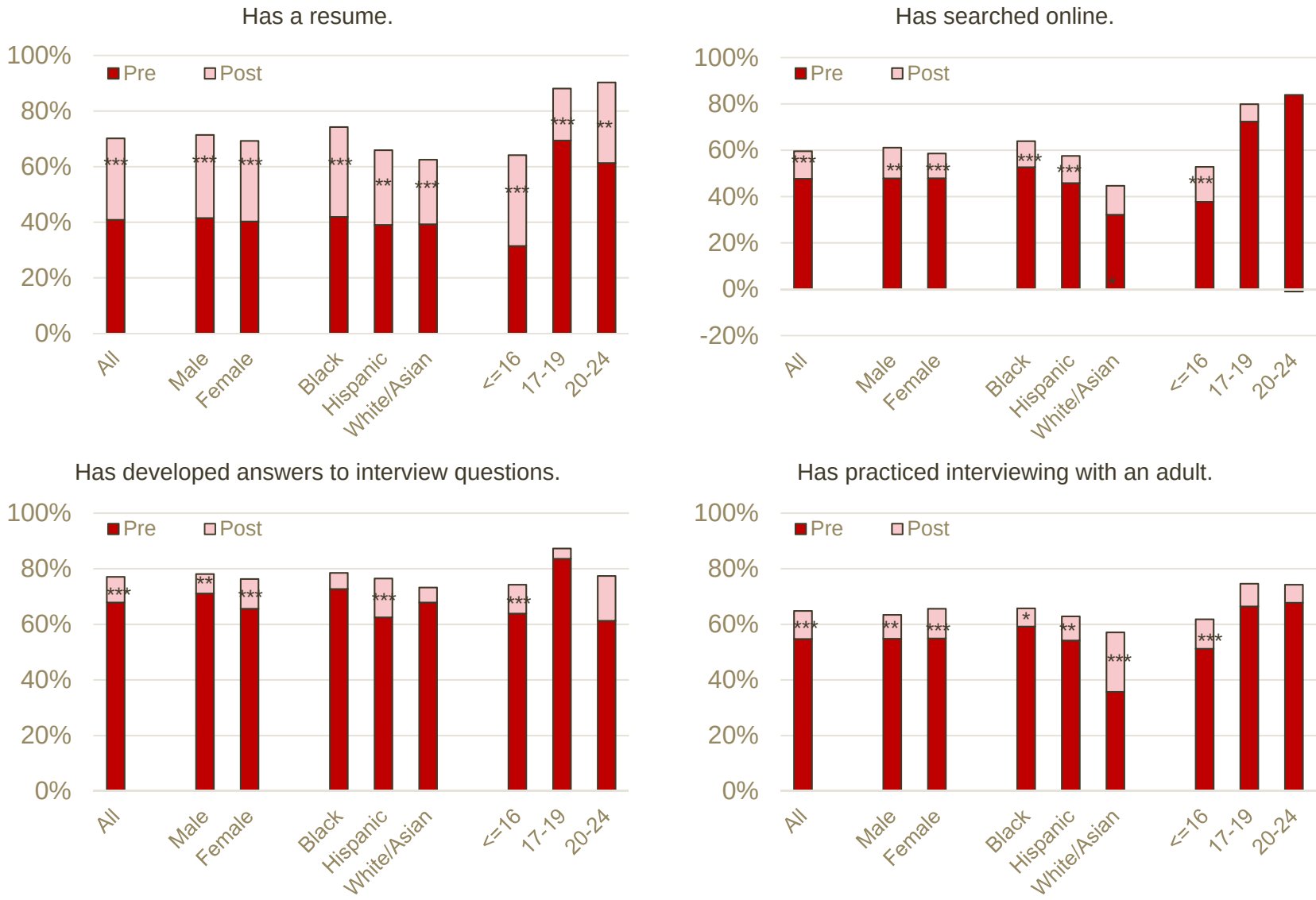
Source: Author's calculations based on data provided by the Department of Criminal Justice Information Services and the Office of the Commissioner of the Probation.
Note: Random assignment and lottery notification occurs at the end of June (month 0). Confidence intervals were calculated using robust standard errors.

Figure A1. Changes in Community Engagement and Social Skills: By Demographic Group, SYEP Participants



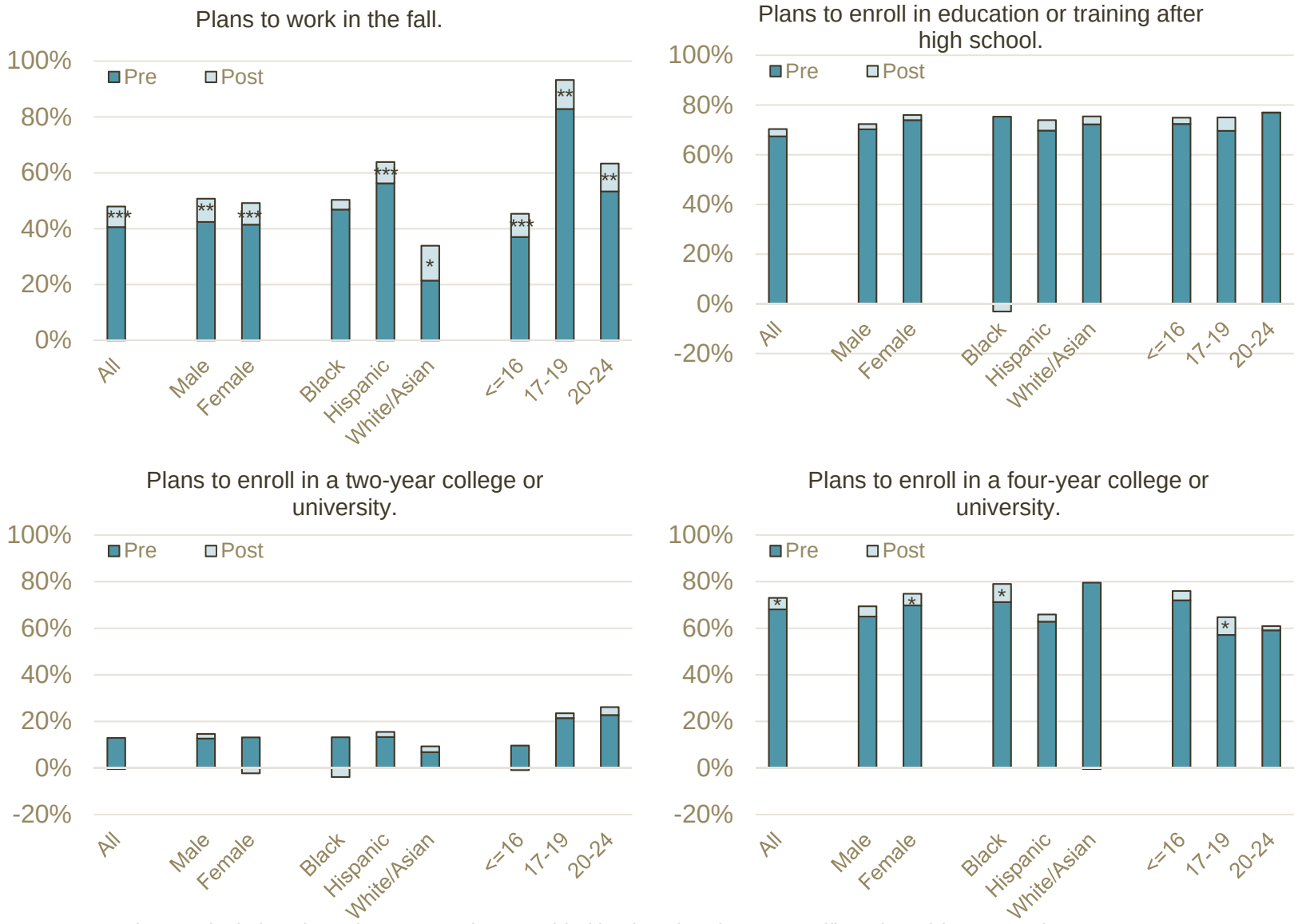
Source: Author's calculations based on survey data provided by the City of Boston, Office of Workforce Development.
Note: *Indicates that the difference is statistically significant at the 10 percent level; ** at the 5 percent level; and *** at the 1 percent level.

Figure A2. Change in Job Readiness Skills: By Demographic Group, SYEP Participants



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 A3. Change in Future Aspirations: By Demographic Group, SYEP Participants.



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.

Table 1. SYEP applicant characteristics by lottery outcome, Summer 2015.

	Selected (treatments)		Not Selected (controls)	
Total selected by random assignment	1,186		3,049	
PERCENT IN EACH CATEGORY:				
Age				
Mean	15.9	(0.058)	15.8	(0.033)
14-18 years	86.1%	(0.010)	88.0%	(0.006)
19-21 years	13.3%	(0.010)	11.5%	(0.006)
22-24 years	0.3%	(0.002)	0.0%	(0.000)
Gender				
Female	53.1%	(0.014)	53.9%	(0.009)
Male	46.9%	(0.014)	46.1%	(0.009)
Current education status				
In-school	87.6%	(0.010)	88.4%	(0.006)
Race				
African American	51.3%	(0.015)	54.0%	(0.009)
Asian*	6.5%	(0.007)	5.0%	(0.004)
White	9.6%	(0.009)	8.4%	(0.005)
Hispanic	32.5%	(0.014)	32.6%	(0.009)
Preferred language				
Chinese	0.2%	(0.001)	0.1%	(0.001)
English	95.1%	(0.006)	95.5%	(0.004)
Spanish	3.3%	(0.005)	2.7%	(0.003)
Other	1.4%	(0.003)	1.8%	(0.002)
Limited English ability				
Yes	7.1%	(0.007)	7.1%	(0.005)
Housing Status				
Homeless	6.7%	(0.007)	6.9%	(0.005)
Household Income Type				
Public assistance	18.7%	(0.011)	17.2%	(0.007)
Disabled				
Yes	4.0%	(0.006)	3.3%	(0.003)

Source: Author's calculations based on application data provided by the City of Boston Office of Workforce Development.
Note: Standard errors are in parentheses.
*Indicates difference is statistically significant at the 10 percent level.

Table 2. Summer employment rates and experiences for SYEP survey respondents by lottery outcome.

	Treatment		Control	
	(1)		(2)	
ALL RESPONDENTS:	**663**		**664**	
Employment rate				
Percent employed this summer***	100.0%	(0.000)	26.4%	(0.039)
Including this summer, how many summers have you been employed?				
Mean**	1.84	(0.046)	1.67	(0.061)
WORKERS:				
If worked, hours worked per week				
10 or less***	3.5%	(0.008)	10.9%	(0.028)
11 to 15***	1.7%	(0.006)	10.9%	(0.028)
16 to 20	12.3%	(0.014)	13.3%	(0.030)
21 to 25**	37.1%	(0.021)	26.6%	(0.039)
26+	37.3%	(0.021)	32.8%	(0.042)
Don't know	8.0%	(0.012)	5.5%	(0.020)
If worked, daily work involved (check all that apply)				
Arts/theater/photography/media**	8.1%	(0.012)	16.1%	(0.031)
Day care/day camp***	56.0%	(0.021)	15.4%	(0.030)
Food services**	6.5%	(0.010)	13.3%	(0.028)
Health care/elder care	3.2%	(0.007)	3.5%	(0.015)
Technology/computer work**	6.6%	(0.011)	11.9%	(0.027)
Library/research/writing	4.1%	(0.008)	6.3%	(0.020)
Office work/administrative work	16.5%	(0.016)	17.5%	(0.032)
Outdoor/maintenance/conservation*	13.5%	(0.014)	8.4%	(0.023)
Peer leader	6.8%	(0.011)	4.9%	(0.018)
Tutor***	0.7%	(0.004)	4.2%	(0.017)
If worked, more likely now to consider a career in that type of work				
Yes**	52.2%	(0.021)	38.5%	(0.041)
If worked, have someone to use as a job reference				
Yes**	85.5%	(0.015)	76.2%	(0.036)
If worked, have someone they consider as a mentor				
Yes***	67.7%	(0.020)	52.4%	(0.042)
If worked, feel better prepared to enter a new job				
Yes***	92.5%	(0.011)	76.2%	(0.036)

Source: Author's calculations based on application data provided by the City of Boston Office of Workforce Development.
Note: Standard errors are in parentheses.
*Indicates difference is statistically significant at the 10 percent level; ** at the 5 percent level; ***at the 1 percent level.

Table 4. Pre vs. post Program comparisons for criminal justice outcome measures: Treatments vs. controls.

	Treatment group						
	Number of arraignments per youth			Percent of youth arraigned for a criminal charge			Re-arraignment rate
	Pre	Post	Diff: Post-Pre	Pre	Post	Diff: Post-Pre	
All crimes	0.12	0.18	0.06 **	4.1%	5.1%	1.1 **	42.9%
Violent crimes	0.07	0.06	-0.01	3.2%	3.9%	0.7 *	50.0%
Property crimes	0.04	0.06	0.01	3.1%	3.7%	0.6 *	52.4%
Drug crimes	0.00	0.02	0.02	0.7%	0.9%	0.2	50.0%
Gun crimes	0.00	0.02	0.02	0.4%	0.8%	0.4	100.0%
Other crimes	0.01	0.03	0.02	1.6%	2.2%	0.5 *	58.3%
Misdemeanor	0.06	0.09	0.03 *	3.6%	4.2%	0.6 *	48.4%
Felony	0.05	0.09	0.03 *	3.6%	4.4%	0.7 *	48.3%
	Control group						
	Number of arraignments per youth			Percent of youth arraigned for a criminal charge			Re-arraignment rate
	Pre	Post	Diff: Post-Pre	Pre	Post	Diff: Post-Pre	
All crimes	0.14	0.21	0.08 **	3.6%	5.4%	1.8 **	47.6%
Violent crimes	0.06	0.09	0.03 **	2.8%	4.3%	1.4 **	57.0%
Property crimes	0.05	0.08	0.03 **	2.1%	3.0%	0.9 **	53.1%
Drug crimes	0.01	0.01	0.00	0.4%	0.6%	0.2	65.2%
Gun crimes	0.00	0.01	0.01	0.2%	0.4%	0.2	91.7%
Other crimes	0.02	0.02	0.00	1.2%	1.9%	0.7	66.0%
Misdemeanor	0.08	0.11	0.04 **	3.2%	4.4%	1.2 **	49.5%
Felony	0.05	0.10	0.04 **	2.9%	4.4%	1.4 **	52.7%

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: To determine the number of arraignments, pre-program is defined as the 17 months prior to random assignment (February 2014 through June 2015) and post-program is defined as the 17 months afer the program ends (September 2015 though November 2016). To determine if an individual has ever been charged with a crime, the pre-program period is defined as any time prior to July 2015 and the post-program period is defined as September 2015 through November 2016. The re-arraignment rate is calculated as the number of youth re-arraigned during the post-program period divided by the number of youth arraigned during the pre-program period.

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

Table 5. Change in short-term program otcomes for SYEP participants and controls, Summer 2015.

CATEGORY	SYEP participants (N=663)				SYEP controls (N=664)	
	Pre-program	Post-program	Difference: pre versus post		Pre-program	Post-program difference: participants - controls
Community engagement and social skills						
I have a lot to contribute to the groups I belong to (all of the time)	31.9%	46.6%	14.7	***	NA	15.6% ***
I feel connected to people in my neighborhood (all of the time)	22.0%	36.8%	14.8	***	NA	21.2% ***
I feel safe walking around my neighborhood (all of the time)	42.9%	46.7%	3.8		NA	19.3% ***
I have a positive role model in my life	91.6%	92.6%	1.0		NA	0.5%
I know how to manage my emotions and my temper	44.2%	49.7%	5.5	**	NA	6.5% **
I know how to ask for help when I need it	44.5%	48.7%	4.2	*	NA	11.6% ***
I know how to constructively resolve a conflict with a peer	36.6%	42.2%	5.7	**	NA	13.6% ***
I need to improve my conflict resolution skills	21.6%	6.0%	-15.6	**	NA	-13.0% **
Job readiness skills						
I have all key information to apply for a job	81.0%	88.2%	7.2	**	NA	9.4% ***
I have prepared a resume	40.9%	70.1%	29.3	***	NA	24.5% ***
I have prepared a cover letter	23.4%	43.7%	20.4	***	NA	21.7% ***
I have asked an adult to serve as a reference	70.9%	74.5%	3.6		NA	-0.1%
I have reviewed at least one job application form	74.8%	82.4%	7.5	**	NA	3.9%
I have completed at least one online job application form	66.1%	70.9%	4.8	*	NA	-3.3%
I have searched for jobs online	47.7%	59.6%	11.9	***	NA	2.5%
I have asked an adult for help in finding job opportunities	83.0%	84.6%	1.7		NA	7.1% ***
I have developed answers to the usual interview questions	67.9%	77.1%	9.2	**	NA	6.9% ***
I have practiced my interviewing skills with an adult	54.8%	64.9%	10.1	***	NA	6.4% **
I need to improve my job readiness skills	33.2%	29.4%	-3.8	*	NA	-5.3% *
Future work plans and academic aspirations						
I plan to work in the fall	40.6%	48.0%	7.4	**	NA	-7.4% **
I plan to enroll in any eduation or training program after high school	67.4%	70.3%	2.9		NA	0.3%
Share indicating that they plan to enroll in:					NA	
Four year college or university	68.1%	73.0%	4.9	*	NA	11.0% ***
Two year college	12.9%	12.4%	-0.5		NA	6.2% ***
I need to improve my academic skills	43.6%	43.4%	-0.2		NA	12.9% ***

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 particiapants completing both surveys. Difference in post-program responses for participants versus controls 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. See Table A4 in the appendix for the full regression results.
*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 in:	Intent-to-treat estimates				Treatment-on-the-treated estimates			
	Violent cimes		Property crimes		Violent cimes		Property crimes	
	Coefficient	Standard error	Coefficient	Standard error	Coefficient	Standard error	Coefficient	Standard error
Community engagement and social skills								
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) *
Job readiness skills								
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)
Academic aspirations								
Planning to attend a four-year college	0.006	(0.012)	0.001	(0.011)	0.013	(0.016)	0.007	(0.015)
Number of Observations	4235		4235		4235		4235	

Source: Author's calculations based on data provided by Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.
Note: Regressions also include SYEP treatment dummy and covariates for age, gender, race/ethnicity, limited English, in school, public assistance, and homelessness.
See Table A5 in the appendix for the baseline treatment-on-the treated results. 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. Relationship between short-term behavioral changes and subsequent criminal activity: SYEP participants

Dummy variable indicating improvement in:	Number of arraignments per youth			
	Violent cimes		Property crimes	
	Coefficient	Standard error	Coefficient	Standard error
Community engagement and social skills				
Contributing to the groups they belong to	-0.007	(0.012)	-0.003	(0.011)
Connecting to people in their neighborhood	0.007	(0.004)	0.019	(0.012)
Managing emotions	-0.031	(0.013) **	-0.021	(0.011) *
Asking for help	0.005	(0.012)	-0.025	(0.011) **
Resolving conflict with a peer	-0.049	(0.023) **	-0.042	(0.021) **
Improving conflict resolution skills (overall)	-0.109	(0.043) **	-0.053	(0.031) *
Job readiness skills				
Having key information to apply for a job	0.001	(0.019)	0.013	(0.015)
Preparing a resume	0.021	(0.013)	-0.015	(0.011)
Preparing a cover letter	0.003	(0.013)	-0.010	(0.010)
Developing answers to interview questions	-0.033	(0.019) *	-0.015	(0.016)
Practicing interviewing with an adult	0.019	(0.012)	0.020	(0.012)
Improving job readiness skills (overall)	-0.017	(0.013)	-0.014	(0.012)
Academic aspirations				
Planning to attend a four-year college	-0.005	(0.014)	0.002	(0.013)
Number of Observations	663		663	

Source: Author's calculations based on data provided by Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.
Note: Regressions also include covariates for age, gender, race/ethnicity, limited English, in school, public assistance, and homelessness.
Sample includes participants that answered both the pre- and post-survey.
*Indicates difference is statistically significant at the 10 percent level; ** at the 5 percent level; and*** at the 1 percent level.

Table A1. Testing the validity of the ABCD lottery.

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

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

Note: Robust standard errors are in parentheses.

*Indicates difference is statistically significant at the 10 percent level.

Table A2. ABCD applicant characteristics by survey response, Summer 2015.

	Treatment group All individuals		Treatment group Responding to pre-/post-program survey	
Total selected by random assignment	1,186		663	
Percent in each category:				
Age				
Mean	15.9	(0.058)	15.6	(0.083)
14-18 years	86.1%	(0.010)	88.7%	(0.013)
19-21 years	13.3%	(0.010)	11.1%	(0.013)
22-24 years	0.3%	(0.002)	0.2%	(0.002)
Gender				
Female	53.1%	(0.014)	54.7%	(0.016)
Male	46.9%	(0.014)	45.3%	(0.020)
Current education status				
In-school	87.6%	(0.010)	89.8%	(0.012)
Race				
African American	51.3%	(0.015)	54.6%	(0.016)
Asian	6.5%	(0.007)	6.5%	(0.010)
White	9.6%	(0.009)	9.9%	(0.012)
Hispanic	32.5%	(0.014)	28.9%	(0.018)
Preferred language				
Chinese	0.2%	(0.001)	0.0%	0.000
English	95.1%	(0.006)	97.3%	(0.006)
Spanish	3.3%	(0.005)	1.7%	(0.005)
Other	1.4%	(0.003)	0.8%	(0.004)
Limited english ability				
Yes	7.1%	(0.007)	6.7%	(0.009)
Housing status				
Homeless	6.7%	(0.007)	6.4%	(0.006)
Household income type				
Public assistance	18.7%	(0.011)	17.6%	(0.015)
Disabled				
Yes	4.0%	(0.006)	3.7%	(0.037)

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

Table A3. ABCD survey respondent characteristics by lottery outcome.

	Treatment group		Control group	
Total selected by random assignment	1186		3049	
Total participated in program	991		3049	
Total participated in pre-program survey	903		NA	
Total participated in post-program survey	801		955	
Total matched pre and post	663		NA	
Total number with complete responses	663		664	
Response rate	66.9%		21.8%	
Percent in each category:				
Age				
Mean***	15.7	(0.078)	16.4	(0.081)
14-18 years	88.2%	(0.014)	88.2%	(0.014)
19-21 years	11.6%	(0.012)	11.4%	(0.012)
22-24 years	0.2%	(0.006)	0.4%	(0.006)
Gender				
Female***	53.9%	(0.021)	65.2%	(0.021)
Male***	46.1%	(0.021)	34.8%	(0.021)
Race/ethnic group				
African American	51.5%	(0.021)	48.9%	(0.021)
Asian***	6.5%	(0.010)	12.0%	(0.014)
Hispanic***	36.1%	(0.020)	26.8%	(0.019)
White***	3.2%	(0.007)	9.2%	(0.012)
Living situation				
Single parent family**	63.7%	(0.020)	57.6%	(0.021)
Two parent family***	29.4%	(0.019)	37.8%	(0.021)
Other relative	8.1%	(0.012)	10.7%	(0.013)
Other	6.3%	(0.010)	4.4%	(0.009)
Language spoken at home				
Chinese	3.9%	(0.008)	5.5%	(0.010)
English	74.0%	(0.019)	70.3%	(0.020)
Spanish***	18.5%	(0.016)	10.7%	(0.013)
Other***	3.6%	(0.008)	13.5%	(0.015)
What are the major reasons you wanted to work this summer (choose two)?				
Make money***	86.2%	(0.015)	66.1%	(0.020)
Learn more about careers	26.8%	(0.019)	30.3%	(0.020)
Learn more about college**	4.1%	(0.008)	7.6%	(0.011)
Have something to do***	47.9%	(0.021)	33.2%	(0.020)
Stay out of trouble***	13.5%	(0.014)	7.6%	(0.011)
Other	4.7%	(0.009)	5.2%	(0.010)

Source: Author's calculations based on survey data provided by the City of Boston, Office of Workforce Development.
Note: 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 A4. 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
Community engagement and social skil					
I have a lot to contribute to the groups I belong to	0.156 ***	0.180 **	0.132 **	0.173 **	0.128 *
	(0.029)	(0.068)	(0.057)	(0.088)	(0.073)
I feel connected to people in my neighborhood	0.212 ***	0.260 ***	0.148 ***	0.251 ***	0.224 ***
	(0.025)	(0.059)	(0.050)	(0.084)	(0.065)
I feel safe walking around my neighborhood	0.193 ***	0.200 ***	0.195 ***	0.260 ***	0.174 **
	(0.028)	(0.066)	(0.053)	(0.078)	(0.070)
I have a positive role model in my life	0.005	0.012	-0.03	-0.02	0.000
	(0.011)	(0.027)	(0.028)	(0.043)	(0.028)
I know how to manage my emotions and my temper	0.065 **	0.162 **	0.089	0.037	0.034
	(0.033)	(0.071)	(0.062)	(0.091)	(0.081)
I know how to ask for help when I need it	0.116 ***	0.029	0.090	0.082	0.080
	(0.030)	(0.070)	(0.058)	(0.090)	(0.075)
I know how to constructively resolve a conflict with a peer	0.136 ***	0.133 **	0.057	0.151 *	0.174
	(0.029)	(0.065)	(0.056)	(0.086)	(0.070) **
I need to improve my conflict resolution skills	-0.130 ***	-0.151 **	-0.138 **	-0.098	-0.149 **
	(0.024)	(0.057)	(0.047)	(0.071)	(0.057)
Job readiness skill					
Have all key information to apply for a job	0.094 ***	0.064	0.080 **	0.080	0.059
	(0.021)	(0.053)	(0.042)	(0.057)	(0.055)
Have prepared a resume	0.245 ***	0.317 ***	0.187 ***	0.313 ***	0.238 ***
	(0.027)	(0.052)	(0.055)	(0.075)	(0.071)
Have prepared a cover letter	0.217 ***	0.257 ***	0.230 ***	0.285 ***	0.204 **
	(0.028)	(0.061)	(0.055)	(0.085)	(0.071)
Have asked an adult to serve as a reference.	-0.001	-0.016	-0.055	0.105	-0.056
	(0.027)	(0.065)	(0.052)	(0.074)	(0.065)
Have reviewed at least one job application form	0.039	-0.001	0.027	0.086	0.025
	(0.024)	(0.053)	(0.044)	(0.071)	(0.057)
Have completed at least one online job application.	-0.033	-0.003	-0.082	0.023	-0.090
	(0.028)	(0.063)	(0.052)	(0.078)	(0.066)
Have searched for jobs online.	0.025	0.152 **	-0.110 **	0.103	-0.018
	(0.031)	(0.066)	(0.057)	(0.090)	(0.078)
Have asked an adult for help in finding job opportunities	0.071 ***	0.041	0.026	0.135 **	0.068
	(0.024)	(0.053)	(0.042)	(0.060)	(0.055)
Have developed answers to the usual interview questions	0.069 ***	0.111 *	0.056	0.088	0.031
	(0.026)	(0.062)	(0.051)	(0.071)	(0.062)
Have practiced my interviewing skills with an adult	0.064 **	0.118 *	0.074	0.069	0.012
	(0.031)	(0.071)	(0.059)	(0.085)	(0.075)
Have appropriate professional clothes to wear to interview.	0.043 **	0.088 **	0.008	0.098 *	0.024
	(0.020)	(0.044)	(0.034)	(0.055)	(0.042)
Have made a plan for how to get to work every day	0.090 ***	0.085 **	0.055 *	0.113 **	0.028
	(0.019)	(0.041)	(0.034)	(0.046)	(0.034)
Can pass a criminal background check	-0.053 ***	-0.064	------	0.000	-0.076 *
	(0.016)	(0.044)	------	(0.037)	(0.043)
Can pass a drug test	-0.042 ***	-0.029	-0.023	------	-0.052 *
	(0.015)	(0.036)	(0.025)	------	(0.035)
I need to improve my job readiness skills	-0.053 *	-0.120 *	-0.020	-0.182 **	-0.009
	(0.030)	(0.066)	(0.055)	(0.077)	(0.073)
Future work plans and academic aspiration					
Plan to work in the fall	-0.074 **	0.080	-0.076	-0.038	-0.204 ***
	(0.030)	(0.070)	(0.057)	(0.086)	(0.063)
Plan to enroll in education or training program after high school	0.003	-0.002	0.017	-0.007	0.011
	(0.017)	(0.040)	(0.034)	(0.048)	(0.039)
Plan to attend a four year college or university	0.110 ***	0.099	0.171 ***	-0.103	0.169 **
	(0.081)	(0.065)	(0.052)	(0.084)	(0.066)
Plan to attend a two year college	0.062 ***	0.049	0.094 ***	0.117 *	0.018
	(0.019)	(0.041)	(0.033)	(0.070)	(0.044)
I need to improve my academic skills	0.129 ***	0.114 *	0.211 ***	0.185 **	0.024
	(0.029)	(0.070)	(0.054)	(0.087)	(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 A5. Treatment-on-the-treated estimates from two-stage least squares regressions: Number of arraignments per youth

	First stage	Second stage		
	Depvar=PART	All crimes	Violent crimes	Property crimes
PART	-----	-0.041	-0.036 **	-0.029 *
	-----	(0.039)	(0.018)	(0.016)
SYEP	0.908 ***	-----	-----	-----
	(0.005)	-----	-----	-----
Baseline crime outcome (e.g. pre-arrest)	-0.025 **	1.844 ***	0.861 ***	0.782 ***
	(0.012)	-0.085	-0.054	-0.050
Age	-0.008 ***	0.014	-0.003	0.007
	(0.002)	(0.011)	(0.006)	(0.005)
Male	-0.011 **	0.152 ***	0.059 ***	0.039 **
	(0.005)	(0.033)	(0.017)	(0.014)
Black	-0.002	0.111 ***	0.055 ***	0.036 **
	(0.005)	(0.033)	(0.017)	(0.014)
Limited English	-0.016 *	0.028	-0.019	0.040
	(0.009)	(0.064)	(0.033)	(0.028)
In school	-0.017 *	0.119 *	-0.002	0.048
	(0.009)	(0.063)	(0.032)	(0.027)
Public assistance	0.007	0.015	0.014	0.010
	(0.006)	(0.044)	(0.023)	(0.019)
Homeless	-0.029 **	-0.006	0.004	0.035
	(0.013)	(0.096)	(0.050)	(0.042)
Disabled	-0.032 **	-0.065	-0.042	-0.007
	(0.013)	(0.090)	(0.046)	(0.039)
F-statistic	2915.260			
Number of Observations	4235	4235	4235	4235

Source: Author's calculations based on data provided by Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.
Note: *Indicates difference is statistically significant at the 10 percent level; ** at the 5 percent level; and*** at the 1 percent level.

DATA AND METHODS APPENDIX

Data Sources

Administrative Data on Criminal Justice Outcomes

The main outcome data consist of adult arrest records from the Massachusetts Department of Criminal Justice Information Services and juvenile arrest records from the Massachusetts Office of the Commissioner of Probation. This rich data source includes information on each criminal charge up through November of 2016—the date that the data were pulled by the two agencies. This includes the arraignment date, the seriousness of the crime (e.g., misdemeanor or felony), as well as the offense code that can be used to create categories for the type of crime (e.g. violent, property, drug, gun, and other).

It should be noted that the criminal record data measures criminal activity only to the extent that an individual was arrested and "booked." It does not capture criminal activity that went undetected by police nor encounters with the police that did not result in official documentation. Data were matched using name and date of birth where name was that used at the time of the individual's first court arraignment. The arrest data therefore include all arrests of an individual in the state of Massachusetts, even if he or she submits an alias at the time of arrest. Similar proportions of youth were found to have been arraigned prior to the start of the program for the treatment group (4.1 percent) and the control group (3.6 percent).

To separate crimes by type, I identify categorize charges associated with each arrest based on the offense code. Violent crimes include all crimes against a person: assault, homicide, sexual offenses, robbery, threats, kidnapping, and aggravated arson (arson when someone is known to be home). Property crime includes larceny, burglary, non-aggravated arson, and motor vehicle theft. Drug crimes include both possession and dealing. Gun crimes include possession of a firearm, firearm violations, possession of ammunition, and carrying without a license. Other crimes include other offenses such as possession of alcohol by a minor, operating under the influence, trespassing, disturbing the peace, cruelty to animals, and parole violations. Note that status offenses (or "child in need of assistance") as well as revocations (e.g. rules violations) were not included. I then count the number of pre- and post-program incidents of each type, defining "post" as after the date of notification of the lottery at the end of June.

Note that the data are limited to arrests conducted within the state of Massachusetts. Without a national database of arrests, it is difficult to assess the extent to which this 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 actually reducing criminal activity. However, 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.

Administrative arrest data avoid limitations of self-reported crime like social desirability bias, which might be particularly problematic given that the treatment group received a fair amount of money from the program and so may be less willing to admit wrongdoing than the control group.

Nonetheless, official arrest records are not without limitations as measures of crime and violence. They tend to understate the overall amount of crime, since many crimes do not result in an arrest, and they capture both criminal and police behavior. However, the similarity of estimated program impacts across both administrative and self-reported crime data in another jobs-program evaluation, Job Corps, suggests that changes in police behavior or probability of being caught are unlikely to explain program effects. Moreover, because of the randomized design, the treatment and control groups are from similar neighborhoods and would be subject to the same policing behavior during the post-period.

Survey Data on Pre-/Post-Program Behavioral Outcomes

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

With the help of the Office of Workforce Development (OWD), I expanded the survey's content and scope during the summer of 2015. In terms of content, I added questions related to job readiness, post-secondary aspirations, and financial capability.8 In terms of scope, OWD engaged ABCD to conduct both a pre- and post-survey to measure changes over time for participants. The pre-survey was administered to participants during orientation just after July 4th and the post-survey was administered in mid-August when participants pick up their last paycheck. Surveys were administered to participants on-site using a paper based collection method. Although nearly the same number of individuals answered the pre- and post-surveys, these were not necessarily the same individuals as only 66.9 percent of individuals could be matched. However, testing for differential attrition between the pre- survey sample and the matched sample for both ABCD yields no statistically significant differences (see Table A1).

In addition, OWD also worked with ABCD to administer the post-survey to the control group to compare the experiences of participants to the counterfactual experiences of those who had applied but not been selected by the SYEP. The post-survey was administered to the control group on-line via email with a link to the survey web site using SurveyGizmo. The control group was offered the chance to win a free iPad mini for completing the survey.

Methodology

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 SYEP placements (the treatment group) with the records for youth not offered placements (control group). I measure two outcomes of interest: whether an individual has been arraigned for any crime during the

post-intervention period, and the number of arraignments per youth during the post-intervention period. Because SYEP participation is allocated via lottery, I am able to obtain causal estimates using a simple comparison of means on the outcome of interest. This "Intent to Treat" (ITT) estimate measures the impact of *offering* the program on the outcome. In many cases, this is the policy relevant estimate because program administrators want to account for program take-up to assess the degree to which SYEP could reduce violence among all the applicants, not just the participants. Nonetheless, because not all youth end up participating, the ITT will understate the effects of actually participating in the program for those youth who choose to participate. I also provide estimates of treatment-on-the-treated.

Intent-to-Treat Analysis

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

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

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 pre-existing demographic characteristics collected by ABCD when youth applied to the program, and μ_{it1} is a stochastic error term.

Although baseline characteristics are not necessary for identification, I include them in the regression to improve the precision of estimates by accounting for residual variation in the outcomes. Baseline covariates included in the main regressions are demographic characteristics including during the application process: age, gender, race/ethnicity, limited English, in school, public assistance, and homelessness. None of the substantive conclusions are different if these variables are excluded from the outcome regressions, but the covariates do improve precision.

Treatment-on-the-Treated Analysis

Nonetheless, because not all youth end up participating, the ITT will understate the effects of actually participating in the program for those youth who choose to participate. Under the usual relevance and exogeneity assumptions for instrumental variables, this latter set of effects can be recovered from the experimental data.[1] I perform this estimation through a two-stage least squares strategy, in which random assignment ($SYEP_i$) is an instrument for actual participation (P_{it}), and P'_{it} is the predicted probability of participation from equation (2):

$$P_{it} = SYEP_i\,\pi_2 + X_{i(t-1)}\,\beta_2 + \mu_{it2} \qquad (2)$$

$$Y_{it} = P'_i\,\pi_3 + X_{i(t-1)}\,\beta_3 + \mu_{it3} \qquad (3)$$

If all youth respond the same way to the program (i.e., if treatment effects are constant across youth), then equations (2) and (3) also yield an estimate of the average treatment effect (ATE) across this population of disadvantaged youth. Given that treatment effects are likely to be heterogeneous across youth, then the coefficient $\pi 3$ estimates a local average treatment effect—

[1] In order for the random assignment variable, $SYEP_i$, to be a valid instrument, it must be correlated with program participation, P_{it}, and uncorrelated with μ_{it3}.

the effect of participation on those who comply with random assignment.[2] Because there is no control crossover (no always-takers) in this setting, $\pi 3$ provides an estimate of the treatment-on-the-treated.

Exploration of Program Mechanisms

Ideally, a full mediation analysis would be used to generate evidence for how the SYEP program achieved its effects using measures of the mediating variable as well as the dependent and independent variable (Baron & Kenny 1986). First, a significant relationship is estimated between the dependent variable of interest (Y_{it}) and the independent variable ($SYEP_i$) using equation (1) above.

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

$$M_{it} = SYEP_{it}\, \pi_4 + X_{it}\, \beta_4 + \mu_{it4} \qquad (4)$$

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

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

$$Y_{it} = SYEP_i\, \pi_5 + X_{i(t-1)}\, \beta_5 + M_{it}\, \gamma + \mu_{it5} \qquad (5)$$

To be a valid mediator, the coefficient π_3 relating the independent variable to the dependent variable in equation (5) must be smaller (in absolute value) than the coefficient π_1 relating the independent variable to the dependent variable in the equation (1) without the mediating variable.[3]

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

First, I modify equation (5) as follows:

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

---

[2] When treatment effects are heterogeneous, $SYEP_i$, must also satisfy a monotonicity condition to be a valid instrument. In particular, random assignment must make everyone weakly more likely to participate and no one less likely.

[3] Researchers often test whether there is complete or partial mediation by testing whether $\pi 3$ is statistically significant, which is a test of whether the association between the independent and dependent variable is completely accounted for by the mediator.

On the left-hand side, the dependent variable is one of the longer-term criminal justice outcomes (e.g., number of crimes per youth) while on the right-hand side is a dummy indicating positive improvement for a specific short-term program impact ΔM_i (e.g., ability to resolve conflicts with a peer). A negative and significant coefficient on ΔM_i indicates that improvement in the short-term program impact observed during the summer of participation is negatively correlated with longer-term criminal behavior. Moreover, if the coefficient on the $SYEP_i$ dummy in equation (6) is smaller in magnitude than that in equation (1), this would suggest that ΔM_i plays a role in achieving the longer-term impact separate from simply being assigned to treatment. Note that this specification implicitly assumes that there was no change in the short-term program measures for youth in the control group. I argue that this assumption is plausible if the analysis is restricted to those short-term program impacts for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group by the end of the summer.

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

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

$$Y_{it} = P'_i \, \pi_7 + X_{i(t-1)} \, \beta_7 + \Delta M_i \, \zeta + \mu_{it7} \qquad (7)$$

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