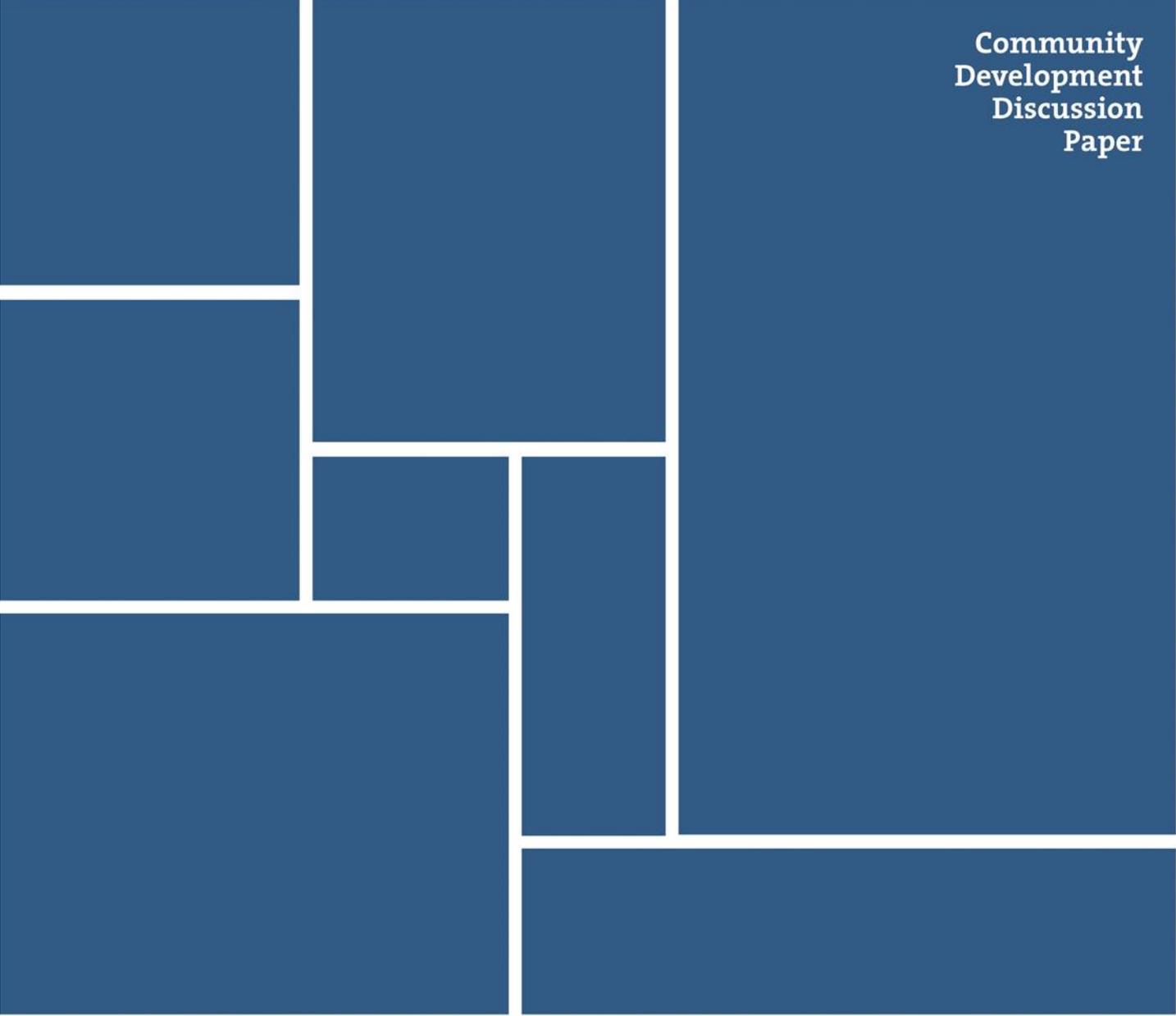


How Do Summer Youth Employment Programs Improve Criminal Justice Outcomes, and for Whom?

Alicia Sasser Modestino



Community
Development
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Paper

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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 and how to 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, policymakers and law-enforcement officials have raised concerns about recent upticks in these types of 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, 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). These include direct mechanisms such as keeping youth off the street and out of trouble during program hours, strengthening social bonds through community-based jobs, and improving “soft skills” such as self-efficacy, impulse control, and conflict resolution through a specialized curriculum—the lack of which have 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 an alternative pathway for youth that makes crime appear less attractive—whether it be a career or some type of postsecondary education. This indirect channel stems from the recognition that one of the major underlying causes of racial disparities in violence among youth is the diminished economic opportunity that arises from non-white youth being disproportionately located in neighborhoods with few job opportunities and failing schools, which negatively affects their outcomes later in life (Wilson, 1996; Chetty, Hendren, & Katz, 2016). By providing greater exposure to private-sector employers, formal career-readiness instruction, and job-skill ladders across summers, SYEPs aim to provide youth with the tools and experience needed to navigate the job market on their own and raise their future aspirations, with the belief that “nothing stops a bullet like a job” (Cook & Ludwig, 2011, p. 44).

Moreover, SYEPs have become an increasingly important tool for employing youth in high-poverty and high-crime neighborhoods even as the economy has recovered from the Great Recession. Employment among youth is still below pre-recession levels and has been falling steadily since 2000, with less than one-third of teens aged 16 to 19 years currently employed today (Figure 1). Employment rates are even lower among non-white teens from low-income families in high-poverty neighborhoods, and more than half of unemployed teens report that they are looking to get their first job, suggesting that there may be fewer pathways for teens to enter the labor market—especially for those not enrolling in college (Sum et al., 2014; Dennett & Modestino, 2013). 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 & McHugh, 2015). And employer expectations for work readiness, communication, and other soft skills—qualifications that are difficult for youth to demonstrate without a track record of work experience—have risen (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 long-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. In addition, it’s not clear whether SYEPs have the potential to reduce differences in criminal behavior across different racial and ethnic or otherwise disadvantaged groups.

This paper seeks to assess the impact of early work experience provided by the Boston SYEP on low-income inner-city youth in terms of both short-term behavioral outcomes and longer-term criminal activity. The paper’s primary contribution to the literature is to better understand not only whether SYEPs improve criminal justice outcomes among youth but also how these impacts are achieved and for whom these impacts are the greatest. Identification of program impacts comes from an embedded randomized controlled trial (RCT) where youth are randomly assigned to jobs because the program is oversubscribed. Using a pre-/post-program

survey, I measure how the program affects teen behavior during the summer and how these short-term program impacts relate to reductions in crime during the subsequent 17 months after participation, based on administrative records on criminal activity. By linking the survey responses to the administrative data, I can determine which of the short-term behavioral impacts lead to greater reductions in criminal activity after the program has ended. In addition, I explore how the SYEP impacts vary across different racial/ethnic groups to determine the program's potential to reduce inequality across groups.

I find that the Boston SYEP reduces the number of arraignments for violent and property crimes among youth and that this improvement appears to be linked to changes in behavioral factors such as social skills and community engagement. Individuals in the treatment group exhibited significant reductions relative to the control group in the number of arraignments for violent crimes (-35 percent) and property crimes (-57 percent)—with larger improvements observed among African American and Hispanic males. The reductions in criminal activity occurred over the 17 months after participation, suggesting that incapacitation of youth during the summer is not the driving mechanism. Rather, youth in the treatment group who reported positive improvements in social skills and community engagement during the summer of participation showed larger reductions in criminal activity relative to the control group—indicating that the program may reduce crime by directly affecting behaviors that have been associated with criminal activity. No such relationship was found for youth showing improvements in other measures of job readiness or academic aspirations, suggesting that SYEPs do not affect criminal activity via the indirect channel of making crime relatively less attractive by providing an alternative pathway. 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.

This paper is organized as follows: In the first sections, I provide an overview of the relevant literature and policy context. Next, I describe the Boston SYEP and the lottery process, followed by a description of the data and methodology used for this study. I then present both the short-term program effects and the longer-term criminal justice outcomes, 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 theory on the impacts of early work experience both in general and in terms of the specific experience provided by summer jobs programs. Previous studies of early work experience have been criticized for their inability to address positive selection into employment. By using an RCT, I will eliminate this potential source of bias. Moreover, I will advance these theories by shedding light on the *mechanisms* by which the Boston SYEP affects youth during the course of the summer and whether these short-term program improvements are related to long-term reductions to criminal activity.

Early work experience: how might SYEPs improve criminal justice outcomes?

Summer jobs programs can affect the propensity for youth to engage in criminal activity both directly and indirectly. Direct mechanisms include “incapacitating” youth by limiting the time they have to engage in criminal activity, thereby reducing the likelihood of risky behavior such as physical violence and drug use. Having a summer job can potentially disrupt “routine activities” that provide likely offenders with suitable targets and a lack of supervision or guardianship (Cohen & Felson, 1979; Felson, 1987). If SYEPs primarily operate by reducing opportunities to engage in delinquent or criminal behavior, we would expect to see a reduction in the number of criminal arraignments rather than a decrease in the share of youth engaging in any criminal activity.

Summer jobs programs may also directly affect youth behaviors that are correlated with criminal activity. For example, the Boston SYEP incorporates a career curriculum aimed at developing soft skills such as self-efficacy, impulse control, and conflict resolution—the lack of which have been shown to predict youth violence and delinquency (Lipsey & Derzon, 1998). From the perspective of youth-development researchers and practitioners, youth need activities that enable them to develop the sense of agency, identity, and competency necessary for adult roles and success. There are continued changes and growth in brain functioning and maturation from mid-adolescence to the mid-20s, and most criminal offending ceases as youths 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). 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).

In addition, most SYEP participants are placed in community-based jobs at day cares and summer camps, which may have a direct impact on the way teens interact with younger children, peers, or adults in their neighborhood. 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). Although it's questionable 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 troubled 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).

Finally, the direct income support that SYEPs provide to low-income youth (and their families) through wages earned from employment in the program can help reduce poverty and also alleviate the tendency to engage in property crime.¹ This mechanism may be particularly important for youth as employment rates for this population have been declining relative to that of other age groups. 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). Thus, by providing youth with a source of income, SYEPs may reduce the motivation for youth to engage in delinquent activities related to theft.

Indirect mechanisms include teaching adolescents how to be successful employees, facilitating connections to employer networks, and providing work experience that can help youth better navigate the job market on their own. It is widely believed that summer work experience may improve future employment outcomes, thereby making crime less attractive, particularly for low-income and non-white youth, who have less access to job opportunities. Through SYEPs, youth have the opportunity to develop relationships with employers, who can

¹ 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.

serve as a reference, and learn work-readiness skills such as writing a resume and cover letter, searching for jobs, completing job applications, and interviewing.

Research suggests that moderate levels of teen employment (less than 15–20 hours per week) may have beneficial effects on future employment. 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). Conversely, unemployment at a young age has been shown to have adverse effects on wages up to 10 and 20 years later (Mroz & Savage, 2006; Gregg & Tominey, 2005). Yet others contend that teen employment is not a determining factor in later labor-market success, citing selection effects and the preexisting characteristics of teens who work versus teens who don’t (Hotz, Xu, Tienda, & Ahituv, 2002; Bacalod & Hotz, 2006).

Finally, summer jobs programs introduce youth to different career paths, potentially raising their future aspirations and making crime less attractive relative to legitimate paid work. The Boston SYEP seeks to accomplish this goal by making career exploration an explicit part of its career-readiness curriculum and placing a high share of youth in private-sector jobs that offer a wider variety of occupations than what is typically found in community-based organizations. Research shows that greater exposure to employment gives youth experiences that can shape their goals—whether it be to complete high school, obtain career training, or attend college (Duckworth, Peterson, Matthews, & Kelly, 2007; Heckman, 2008; Lillydahl, 1990; Mortimer, 2010). Work experience may also provide an opportunity for teens to explore and test school-based knowledge, possibly increasing comprehension and academic achievement, thereby reinforcing their career goals. What’s more, SYEPs provide employment experiences during the summer months, when youth are often idle, creating fewer conflicts with homework and study time during the academic year.²

Summer jobs programs: what do we know so far?

² 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).

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. In terms of direct impacts on criminal activity, research on summer jobs programs in Chicago and New York City found that these programs can reduce violence as well as the probability of incarceration and mortality. An RCT among disadvantaged high school youth in Chicago’s One Summer Plus program found that the SYEP decreased violence by 43 percent over 16 months, with much of the decline occurring during the year after participation (Heller, 2014). In New York City, one study found that participation reduced the probability of incarceration and mortality, most likely by reducing death by “external causes,” which includes homicides, suicides, and accidents (Gelber et al., 2014).

In terms of indirect channels that might affect criminal activity, other studies found that participation in the New York City SYEP was associated with modest improvements in test taking and school attendance, but not college matriculation. One study found small but significant increases in the share of participants taking and passing statewide high school exams relative to the control group (Schwartz et al., 2015). A related study found average increases of 1 to 2 percent in participants’ school attendance the following year, with larger increases for students aged 16 years and older with low baseline attendance (Leos-Urbel, 2014). A separate New York City study found that the 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 it did not use 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 be able to distinguish between the income effect of having a summer job on criminal justice outcomes and other aspects of SYEPs, which may affect adolescent behaviors that are correlated with criminal activity either directly or indirectly. I build on this literature in

several fundamental ways to shed light not only on what works but also on what works for whom, under what conditions, and why by focusing on the following research questions:

- Does the Boston SYEP improve long-term criminal justice outcomes in terms of propensity to commit a crime, frequency of crimes committed, and recidivism?
- Does the Boston SYEP positively impact short-term behavioral outcomes related to social skills, community engagement, job readiness, and aspirations among participants?
- Are any of the short-term behavioral impacts observed during the summer correlated with the reduction in criminal activity observed in the period following participation?
- Do these outcomes vary for different demographic groups by age, gender, race, and ethnicity?

To explore these questions, I make use of an embedded RCT to assess the causal impacts of the Boston SYEP. In the first phase, I produce intent-to-treat estimates of the program on various types of criminal activity using administrative data for the 17 months after program participation. In the second phase, I link these long-term program outcomes to self-reported survey data on behavioral outcomes to better understand the mechanisms driving the reductions in criminal activity. In the next two sections, I provide further details on the Boston SYEP intervention as well as the data and methods used in producing these estimates.

Policy context: the Boston SYEP intervention

Introduced in 1990, the Boston SYEP has become a model program for the nation. It 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 additional job-readiness training using a hands-on, competency-based work-readiness curriculum called Signal Success. The primary goal of the curriculum is to help teens find and

maintain employment in the private sector after the SYEP has ended.³ The curriculum consists of 13 required and eight elective 75-minute sessions, which can be delivered one at a time or combined into two- to three-hour blocks. Topics include understanding workplace safety, evaluating learning strengths and skills/interests, practicing soft skills (e.g., dependability, communication, collaboration, and initiative), and learning how to find and apply for jobs and be successful in an interview. Electives include financial capability (required for summer 2015), completing online applications, and drafting resumes.⁴ Participants must receive at least 20 hours of class time that results in the creation of a portfolio and a transition plan.

All Boston city residents aged 14 to 24 years are eligible for the program, and our data indicate that approximately 80 percent of SYEP applicants are Boston Public School (BPS) students—similar to the proportion of high school-aged individuals (15–19 years) enrolled in BPS in the Boston area (Boston Foundation, 2006).⁵ Youth apply directly through one of the four intermediaries under contract with the City of Boston’s Office of Workforce Development (OWD) (see Table 1). The intermediaries are responsible for reviewing applications, supervising job placements, and delivering the program’s career-readiness curriculum. Youth typically apply to the particular organization that serves their neighborhood, and audits in prior years have confirmed that only a handful of youth apply to more than one agency. Community-based organizations differ in the population they serve and how they deliver the intervention with respect to applicant selection, the share of private-sector placements, and the administration of the career-readiness curriculum. Each summer, two of the four intermediaries receive more applications than the number of SYEP jobs available and randomly allocate spots in the program by lottery.⁶

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

³ 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.

⁴ Other electives include respect for others, sexual harassment, workplace check-in, planning for future success, education and training, occupational goals, and final projects (e.g., presentation, resume, and occupational research).

⁵ The remainder of high school-aged students attend either parochial or private school and the remaining SYEP applicants are older individuals who are not in school.

⁶ The other two intermediaries do not use random assignment because of the types of jobs and/or the populations they serve.

limited number of SYEP jobs available.⁷ 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 their applicant ID numbers; the number of available slots is determined by the funding ABCD receives for that year. The 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. As a result, the individuals in the control group should be statistically identical to the participants on both the observable and unobservable characteristics.

Of the 4,235 youth who applied to ABCD, a total of 1,186 were offered a job via random assignment (28 percent). This random assignment process yields a control group of 3,049 individuals, which provides sufficient power to compare outcomes across demographic groups such as age, gender, and race/ethnicity. Of those selected by the lottery, 83.6 percent participated in the program. Among lottery winners who do accept the ABCD job offer, participation is quite high, with only a handful of youth dropping out of the program during the summer.

Table 2 provides descriptive statistics for the preexisting characteristics of SYEP lottery applicants collected by ABCD at the time the youth applied. Based on observable characteristics, the youth selected by the ABCD lottery appear to be almost identical to those not selected, indicating that the lottery is indeed random. Across all but one of these observed characteristics, there is no statistically significant difference between those selected by ABCD to participate in the SYEP and those not selected. I note that having at least one statistically significant difference at the $P < 0.10$ level would be expected by random chance when testing 15 different characteristics.

Overall, ABCD serves a predominately low-income school-aged population. On average, over 85 percent of youth were in school at the time they applied, with a mean average age just shy of 16 years (see Table 2). 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

⁷ The other intermediary that uses random assignment, Youth Employment and Engagement (YEE), does so only 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 2016 and so we cannot test any of the potential mechanisms for this group.

cash public assistance of some form—suggesting that the program serves a significant proportion of low-income youth.⁸ Less than 5 percent listed themselves as having a disability. It should be noted that compared with the BPS high school population, SYEP 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.

If the ABCD lottery is random, then the lottery outcome should not predict preexisting applicant characteristics. In order to verify that the ABCD lottery is in fact random, I test for baseline equivalence in Table 3 using separate models estimating the effect of winning the lottery on preexisting student characteristics and outcomes (Cullen, Jacob, & Levitt, 2006).⁹ I replicate the results from our simple comparison of means in column 1 for comparison with estimates for the age/race/gender groupings, within which I will later compare outcomes. The models in column 1 confirm again that the lottery is random, with just one coefficient showing significance at the 10 percent level, as would be expected by random chance when testing 15 different outcomes. Moreover, none of the coefficients from the models within each age/race/gender grouping is significant, indicating that the sample is indeed balanced across these cells.

Data and methodology

Although the Boston SYEP is a stand-alone program that occurs during the summer and is separate from youth workforce-development programs that occur during the academic year, one might be concerned that the program could overlap with other summer programming for youth. To reduce the potential for contamination, I use an embedded RCT along with a mixed-methods approach to ensure that the outcomes I observe are well identified and due to the SYEP intervention rather than other funded programs. Specifically, I use both administrative data on criminal-justice outcomes that capture the subsequent 17 months after the intervention for both treatments and controls, and survey data on self-reported behavioral outcomes that occur during

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.

⁹ Analyses presented use linear probability models. Logistic regression with marginal effects yields similar results.

the course of the summer. By linking the longer-term criminal-justice outcomes from administrative records to self-reported short-term program impacts on behavioral outcomes from the survey responses, I provide deeper insights into the unique features of the program as well as the relationship between the program's short-term impacts and longer-term youth 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 both as an adult and as a juvenile. There was no selection bias associated with the retrieval of the criminal justice records. For the ABCD treatment and control groups, similar proportions were found to have a criminal record prior to the start of the program (4.1 percent for the treatment group and 3.6 percent for the control group).

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). 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.

To assess the impact of the Boston SYEP on criminal justice outcomes, I derive intent-to-treat estimates by comparing 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), exploiting the randomized selection of ABCD program participants. 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 between those assigned to SYEP (treatment group) and those not (control group), where the treatment is a placement offer from SYEP. Specifically, I estimate the impact of the Boston SYEP on subsequent criminal justice outcomes after participation using equation (1) below:

$$Y_{it} = \beta_0 + \beta_1 SYEP_{it} + \beta_2 X_{it} + \mu_{it} \quad (1)$$

Here, our primary outcome of interest, Y , is either whether an individual has been arraigned for a crime during the post-intervention period or the number of arraignments during the post-intervention period. Note that although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned, equation A includes individual characteristics to improve the precision of our estimates (Bloom, 2006).

I further explore program impacts by seriousness as well as type of crime and also disaggregate the analysis by age, gender, and race. I also map out the cumulative number of arraigned crimes occurring for each group after random assignment to determine whether the results are driven by reductions in crime during the program, when youth are afforded fewer opportunities for crime. Finally, I also compare measures of recidivism between the two groups to assess whether SYEPs operate primarily as a preventive or a rehabilitative intervention.

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 described above 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 Boston neighborhoods that had been identified by the Boston Police Department as having a high number of fatal and nonfatal shootings. Starting in summer 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. The 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 OWD, I built on this original framework to expand the survey's content and scope during the summer of 2015. In terms of content, I added questions related to job readiness, postsecondary aspirations, and financial capability. In terms of scope, OWD engaged ABCD to conduct pre- and post-program surveys to measure changes over time for participants. The pre-program survey was administered to participants during orientation, just after July 4, and the post-program survey was administered in mid-August, when participants pick up their last paycheck. In addition, OWD worked with ABCD to administer the survey to the control group in order to compare the experiences of participants with the counterfactual experiences of those who had applied but not been selected by the SYEP lottery.

To explore the mechanisms by which the Boston SYEP affects criminal activity, I first determine whether the program significantly affected the self-reported outcomes of participants and whether the post-program measurements of these outcomes were significantly different from those in the control group. I then link the self-reported survey outcomes to the administrative data to determine whether any of the short-term behavioral impacts correlate with the longer-term criminal justice outcomes. There are several measurement issues inherent in using the survey data that give rise to potential bias, but I argue below that the bias goes against finding an effect.

Assessing short-term behavioral impacts

To initially determine whether the program has an impact on youth behavior, 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-program survey responses.¹⁰ To determine whether these improvements are attributable to the Boston SYEP, I then compare the outcomes measured at the end of the summer for both the treatment and control groups.

There are several potential sources of bias arising from this analysis. First, it might be the case that individuals responding to the survey differ from those who do not. Fortunately, the high

¹⁰ Although nearly the same number of individuals answered the pre- and post-program 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-program survey sample and the matched sample for ABCD yields no statistically significant differences. See Table A1 in the data appendix for more detail.

response rate among the treatment group (66.9 percent, n=663) was sufficient such that there were no significant differences in application characteristics among respondents versus nonrespondents. 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, but only in the dependent variable.

Assuming that the measurement error is random, this would reduce efficiency but not cause bias.

In contrast, although the number of respondents among the control group was similar (n=664), this represented a response rate of only 21.8 percent—despite a small material incentive.¹¹ This is likely because the control group was contacted via email (rather than in person) and also may have felt less compelled to answer because they had not been initially selected for a summer job placement. Moreover, although the control group was randomly selected, those who chose to respond to the post-program survey were not. As a result, survey respondents from the control group exhibited characteristics indicating positive selection relative to the treatment group. They were more likely to be over age 16, identify as white or Asian, and indicate that they live in a two-parent household (see Table 4). In addition, survey responders in the control group were significantly more likely to indicate that their motivation for working during the summer was to learn about college, whereas survey responders in the treatment group were more likely to want to work to make money, have something to do, or stay out of trouble.

For the purposes of this phase of the analysis, I note that this bias goes *against* our finding an impact for the SYEP, given that the control group is likely to set a high bar for comparison, having been more positively selected. To minimize this selection bias regarding the survey response, I will control for all observable characteristics by using a regression framework similar to that of equation (1) above. Because survey respondents in the control group were positively selected, I expect the coefficient on the SYEP dummy to provide downward-biased estimates of the impact of the program on the short-term behavioral outcome of interest. Finally, I test for heterogeneous treatment effects through separate models stratified by subgroups based on demographic characteristics including age, race/ethnicity, and gender.

Finally, when combining the survey with the administrative data, I will only make use of the survey data for the treatment group in order to detect whether program impacts are larger for

¹¹ The control group was offered the chance to win a free iPad mini for completing the survey.

individuals exhibiting improvements in key short-term behavioral indicators during the summer. I discuss this in further detail below.

Linking short-term behavioral impacts to criminal justice outcomes

To assess whether improvement in these short-term behavioral impacts mediates subsequent improvement in the criminal justice outcomes, I modify equation (1) as follows:

$$Y_{it} = \beta_0 + \beta_1 SYEP_i + \beta_2 X_{it} + \beta_3 \Delta ST_i + \mu_{it} \quad (2)$$

On the left-hand side, the dependent variable is one of the longer-term criminal justice outcomes (e.g., propensity to commit a crime) while on the right-hand side we add an “effectiveness” dummy indicating positive improvement for a specific short-term program effect ΔST_i (e.g., ability to resolve conflicts with a peer). A negative and significant coefficient on ΔST_i indicates that the impact on the criminal justice outcome is greater for youth that showed improvement in that particular short-term behavioral effect during the summer of participation. By linking the survey responses to the administrative data, this second phase of the research aims to understand which *specific* short-term behavioral impacts lead to improvements in *specific* criminal justice outcomes.

To reduce the chance of detecting spurious outcomes from making multiple comparisons, I estimate equation B only for those short-term behavioral impacts that were found to be significant changes over the summer for the participants, and significantly different relative to the control group by the end of the summer. However, it is still possible that the observed changes in the short-term behavioral measures from the survey correlate with other unobserved changes that are driving the reduction in crime. This could generate an upward bias in the absolute value of the estimated effect. However, as we shall see in the next section, the estimated effects of the short-term behavioral impacts vary according to our theory regarding direct versus indirect mechanisms such that it’s likely they represent some aspect of the direct channel that we are measuring.

Results: how does the Boston SYEP affect criminal justice outcomes?

In this section, I provide evidence as to whether the Boston SYEP affects longer-term outcomes by estimating changes over time in criminal activity for the treatment versus the control group as measured by court records for the 17 months after participation in the program. I then examine changes in short-term behavioral impacts that occur during the summer for the treatment group and assess whether these outcomes are significantly different from the control group. Finally, I link the short-term behavioral impacts to the longer-term criminal justice outcomes to explore whether summer jobs programs operate primarily through direct or indirect mechanisms. Direct mechanisms include keeping youth off the streets, providing them with productive activities, and improving social skills such as self-efficacy, impulse control, and conflict resolution. Indirect mechanisms are those that seek to make crime less attractive by providing alternative pathways to careers and postsecondary education through increasing job readiness and raising academic and career aspirations. By linking the survey data on self-reported behavioral outcomes that occur during the summer of participation to the administrative data on criminal justice activity, the results presented here provide a glimpse inside the black box as to how SYEPs affect youth over the course of a summer.

Given that social interventions tend to have differential impacts by type of crime, I categorize arraignments by offense type (violent, property, drug, gun, and other). This categorization is particularly important when linking the short-term behavioral impacts to the longer-term criminal justice outcomes. For example, because violent crime tends to arise from interpersonal conflicts, one would expect that improvements in cognitive and emotional skills such as self-efficacy, impulse control, and conflict resolutions would be highly correlated with reductions in arraignments for violent crime in particular. 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 the additional income from having a summer job or increased future aspirations. Given that SYEPs could affect youth behavior with regard to social interactions differently from how they affect youth behavior in response to economic and situational factors, I estimate program effects separately by crime type.

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. During the 17-month follow-up

period, approximately 5.1 percent of the ABCD control group (n=159 youth) was arraigned for any crime, with an average of 2.1 arraignments per 100 youth. The main intent-to-treat results are presented in the top panel of Figure 2. Violent-crime arraignments among the treatment group decreased 35 percent relative to the control group, with roughly 2.5 fewer arraignments per 100 youth. The percentage decline was even greater for property crimes (-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).

Somewhat surprisingly, 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 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 around month six—a full four months after the end of the program—and continues to grow through month 17, at the end of the post-program data window. For property crimes, the drop in arraignments becomes significant around month four, after which it flattens out 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 black and Hispanic males of varying ages. For example, Table 5 shows that among black males aged 14–18 years, the total number of arraignments fell by 6.1 per 100 youth, primarily driven by a drop in violent crime. Yet the reduction in arraignments among older black males aged 19–24 years was driven primarily by a reduction in property crimes (-8.9 crimes per 100 youth). In contrast, the drop in arraignments among Hispanic males aged 19–24 years (-13.9 crimes per 100 youth) was driven by a fall in both violent and property crimes.

Yet in terms of the propensity to commit any crime, the bottom panel of Figure 2 reveals that there was no significant impact on the share of youth arraigned for the treatment group versus the control group. During the 17-month follow-up period, approximately 5.4 percent of the ABCD treatment group (n=53 youth) was arrested for any crime. Thus, it appears that the SYEP affects youth primarily on the intensive, rather than the extensive, margin such that youth commit fewer crimes but are not less likely to engage in criminal activity. Indeed, Table 6 shows that the relative reduction in arraignments for violent and property crimes was driven by a lack of increase over time among the treatment group. Yet the share of youth being arraigned for a crime increased across both the treatment and control groups. Moreover, the overall re-arraignment rate for both the treatment and control groups is very similar—in the range of 43 to 48 percent—and not significantly different for any type of crime.

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 behavior—either by strengthening social bonds through community-based jobs or improving soft skills such as self-efficacy, impulse control, and conflict resolution through a specialized curriculum—which could potentially reduce delinquent and violent behavior. 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—either through work experience or the program curriculum—and how these changes correlate with the longer-term outcomes.

Exploring SYEP program mechanisms using survey data

To determine whether the Boston SYEP provides a meaningful intervention, Table 7 provides information about the summer employment rates and experiences among those in the treatment group responding to the survey as well as the counterfactual experiences of 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. Although workers in the treatment group worked more hours per week than workers in the control group, they had less variation in the types of daily work they did, being far more likely to work at a day care or day camp or doing outdoor, maintenance, or conservation work. However, responders in the treatment group were 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 someone to use as a job reference in the future, had someone they considered a mentor, and felt better prepared to enter a new job. When asked about what they had learned because of the SYEP, more than 40 percent of those in the treatment group “agreed strongly” that they had learned how to manage their emotions and temper, how to ask for help when they needed it, and how to constructively resolve a conflict with a peer.

Pre-program versus post-program survey comparisons for treatment group

Direct mechanisms: social and community engagement. Across all ABCD participants, youth attitudes toward their communities shifted significantly toward better social outcomes. 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 8). Similar positive improvements were also seen in terms of the share of teens who said they always felt connected to their neighborhood. Both measures showed large and significant gains across all demographic groups, although here the gains were largest for males, Hispanics, and younger teens (see Figure 4). In retrospect, these large improvements are perhaps not all that surprising, given that most of the SYEP job placements are with community-based organizations in the neighborhoods in which many of the participants live, providing an opportunity for youth to engage in their communities in a positive way. In contrast, no significant increases were observed in the percentage of teens who said they always felt safe walking around their neighborhood. Apparently, while the Boston SYEP may impact the attitudes of participants, it is unlikely to affect the actual neighborhood conditions in which they live.

Indirect mechanisms: job readiness and future aspirations. In terms of job-readiness skills, Table 8 shows that there were large significant increases among ABCD participants in the percent reporting they had prepared a resume (+29.3 percentage points) and a cover letter (+20.4

percentage points). There were also positive but more modest improvements in the percent of participants who had searched for jobs online (+12 percentage points), had practiced interviewing skills with an adult (+10.1 percentage points), and had developed answers to typical interview questions (+9 percentage points). Although nearly all demographic groups saw similar improvements, the biggest gains were observed for younger ABCD youth, who were much less likely to have been exposed to the program in prior summers (see Figure 5).

In terms of future aspirations, the share of ABCD participants indicating that they planned to work in the fall jumped significantly, by 7.4 percentage points (see Table 8), and this increase was largest among Hispanics and whites/Asians and among younger teens (see Figure 6). Although there was little shift in terms of whether participants planned to enroll in an education or training program after high school, there was a shift in the type of program they expected to pursue. The percent of participants indicating that they planned to enroll in a four-year college or university increased by 4.9 percentage points and declined for all other categories, including attending a vocational or technical program, a training program, or a two-year college (see Table 8). The largest impact was found for African American males (see Figure 6).

Post-program survey comparisons for treatment group versus control group

Based on the pre- and post-program survey responses of participants, it appears that the Boston SYEP impacts youth in many of the ways that it was designed to. Yet can the improvements in these outcomes be attributed exclusively to the Boston SYEP versus other experiences that youth may have had during the course of the summer? To test this, I compare the post-program survey responses of the ABCD participants (treatment group) with those who applied to the Boston SYEP but were not randomly selected to participate (control group). However, as noted before, although the control group was randomly selected, those who chose to respond to the post-program survey were not. Relative to the treatment group, survey respondents from the control group were more positively selected: they were older and more likely to identify as white or Asian, to be from a two-parent household, and to have English as the primary language spoken at home. Therefore, to minimize selection bias regarding the survey response, I control for observable characteristics and also make comparisons between treatment and control groups within race/gender/age cells. Note that all models report the marginal effects

from a probit regression of the outcome of interest on a dummy variable for treatment controlling for age, gender, race, two-parent family, and English as the primary language.

Direct mechanisms: social and community engagement. Compared with the indirect measures, the impact of the Boston SYEP on participants' attitudes toward their community was the most prominent. Controlling for all observable characteristics, Table 9 shows that youth in the treatment group were far more likely to report feeling that they always had a lot to contribute to the groups they belonged to (+15.6 percentage points). Similarly large positive effects were found for feeling connected to the people in one's neighborhood (+21.2 percentage points) and for feeling safe walking around the neighborhood (+19.3 percentage points). These findings were strongly consistent across age/race/gender cells.

Indirect mechanisms: job readiness and future aspirations. Among the indirect effects, the treatment group outperformed those in the control group across most of the job-readiness measures that correspond with the career-readiness curriculum. Table 9 demonstrates that youth in the treatment group were 20 percentage points more likely than the control group to have a resume or cover letter, and these impacts were fairly uniform across all age/race/gender cells. Overall, African American males showed the most improvement across the board, while Hispanic females had the least improvement across multiple measures. Surprisingly, Hispanic females in the treatment group were less likely than those in the control group to indicate that they could pass a criminal background check (-7.6 percentage points) or a drug test (-5.2 percentage points), even though there were few participants with a criminal record in the ABCD group.

In terms of career and academic aspirations, youth who participated in the Boston SYEP were 7.4 percentage points less likely to indicate that they planned to work in the fall—even when controlling for observable characteristics. This was largely driven by Hispanic females (see Table 9). This diminished intention for future labor force participation among SYEP participants may be because they were able to gain some work experience during the summer and thus did not feel a need to work in the fall, compared with those in the control group, who were far less likely to report being employed. In fact, the SYEP program, by enabling youth to shift their work experiences to a part of the year when they are not also attending school, might consequently

provide the additional benefit of increasing the time and attention that these students can devote to academics during the school year.

As before, I find that the Boston SYEP affects college-going plans on an intensive margin rather than an extensive one. While there were no significant differences between the treatment and control groups in terms of their plans to attend an education or training program after high school, youth in the treatment group were more likely to report wanting to go to a two-year (+6.2 percentage points) or four-year (+11.0 percentage points) college. The largest impact was found for African American and Hispanic females (see Table 9). This finding is consistent with other research that has documented an upward trend in college attendance among non-white women relative to men.

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. For example, it has been asserted that employment provides youth with a set of socially productive activities, possibly decreasing the risk of exposure to, or participation in, violence and delinquent behavior. Yet it has been difficult to disentangle these effects from that of simply providing youth with additional income and/or incapacitating youth during the summer by limiting their time to engage in criminal activity. To test this, I estimate equation B by including a dummy variable equal to one for youth in the ABCD treatment group who reported improvements among the community-engagement, job-readiness, and future-aspiration measures that were shown to be significant during our analysis of the short-term behavioral impacts using the survey data. A negative and significant coefficient on this dummy variable indicates that those in the treatment group who reported greater improvements in behavior during the summer experienced larger reductions in arraignments relative to the control group during the 17 months after the program ended.

Table 10 shows that there were large and significant reductions in criminal activity among participants reporting improvements in the social- and community-engagement measures that correspond with the direct short-term behavioral impacts but no impact related to the indirect measures. Significant reductions in total arraignments were observed among youth indicating improvements in contributing to the groups that they belong to, managing their emotions,

resolving conflict with a peer as well as those indicating that they wanted to improve their conflict-resolution skills. In particular, learning to manage one's emotions and resolve conflicts were both correlated with significant reductions in both violent and property crimes—perhaps indicating that the Boston SYEP reduces delinquent behavior primarily through improvements in impulse control and conflict resolution. In contrast, indirect mechanisms such as job readiness and career/academic aspirations did not appear to play a meaningful role in reducing the number of arraignments for violent or property crimes. Of the eight measures listed in Table 10 that were shown to be affected by the program during the course of the summer, only the ability to prepare a resume was correlated with a significant reduction in property crime.

Discussion and conclusion

This paper seeks to assess the impact of early work experience provided by summer jobs programs on low-income inner-city youth in terms of both short-term behavioral outcomes and longer-term criminal activity. The paper's primary contribution to the literature is to better understand not only whether SYEPs improve criminal justice outcomes among youth but also how these impacts are achieved and for whom these impacts are the greatest. Policymakers have provided various rationales to support summer jobs programs as an intervention that can affect both short- and longer-term outcomes for youth. These rationales include direct mechanisms such as keeping youth off the streets, providing them with productive activities, and improving social skills such as self-efficacy, impulse control, and conflict resolution as well as indirect mechanisms that seek to make crime less attractive by providing alternative pathways to careers and postsecondary education. By linking survey data on self-reported behavioral outcomes that occur during the summer of participation to administrative data on criminal justice activity, the results presented here provide a glimpse inside the black box as to how SYEPs affect youth over the course of a summer.

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 having greatly improved their attitudes toward their communities and gained some additional job-readiness skills, and they were more likely than the control group to raise their sights toward enrolling in college. Improvements in community engagement measures—those most likely to be directly related to delinquent

behavior—were the largest in magnitude and the impacts were fairly uniform across all subgroups. In comparison, other indirect measures related to job readiness and future aspirations—those more likely to have an indirect affect by making crime less attractive—were smaller in magnitude and the greatest impacts were observed among younger and non-white youth.

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 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 relative decline in the number of arraignments persisted during the 17 months after participation.

However, it appears that the program operates primarily by reducing criminal activity on the intensive margin in terms of the frequency of being arraigned for a crime—rather than on the extensive margin of the likelihood of ever being arraigned. This finding could be because having a summer job disrupts some of the "routine" (possibly delinquent) activities that youth would typically be involved in during the summer. Yet the impact of the Boston SYEP on the number of arraignments does not become statistically significant until four to six months after the program ends—suggesting that the program may have long-lasting effects that change youth behavior.

Indeed, the exploratory analysis using the self-reported short-term outcomes from the pre-/post-program survey indicates that participants show improvements along a number of dimensions during the summer, some of which are correlated with the subsequent reduction in arraignments. For example, participants reporting significant improvements in direct short-term measures related to social and emotional skills—such as learning to manage their emotions and resolve conflicts with a peer—experienced larger decreases in both violent and property crimes. Indirect short-term mechanisms such as job readiness and academic aspirations did not appear to play a role in reducing longer-term delinquent behavior. 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 the Becoming a Man program in Chicago achieved very similar impacts (e.g., reducing violent crime arrests by 45 percent to 50 percent) with a benefit-cost ratio from 5-to-1 up to 30-to-1 or more (Heller et al., 2017). Yet given the costs of juvenile detention (about \$241 per day per youth¹²), it may be that the benefits associated with the reduction in arrests alone is enough to justify the relatively low cost of SYEPs (about \$2,000 per participant, similar to that of Becoming a Man) compared with other year-round workforce-development programs for youth (roughly \$6,500 per participant¹³). And this cost-benefit calculation does not include the costs arising from youth detention in terms of reducing future economic productivity for both individuals and their

¹² According to the American Correctional Association's 2008 Directory.

¹³ According to the Boston Mayor's Office of Workforce Development.

communities because of lost opportunities for education and employment as well as the disruption of the process that normally allows many youth to “age out” of delinquent behavior.

However, SYEPs 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 labor market outcomes or career/academic aspirations. Third, SYEPs help families at or near the poverty line by providing income to youth, of which 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 organization programs serving cities, particularly summer camps that provide inexpensive daycare for working parents when school is not in session.

Finally, it appears that the program’s impacts on criminal activity are driven primarily by improvements in social behaviors among non-white and court-involved youth such that targeting SYEPs may help level the playing field for these groups. Indeed, a sample of court-involved youth served by one of the other Boston SYEP intermediaries 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 disrupt criminal activity even among groups where 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. 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 teens living in neighborhoods that are policed less aggressively.

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 how the summer jobs program can help reduce criminal activity and for which groups the impact is greatest may lead to a more effective intervention that can be scaled up to produce better outcomes at a reduced cost. Some proponents suggest that the program could target disadvantaged youth more effectively, with added layers of instruction, mentoring, and support services. Others see the program's greatest value in exposing as many youth as possible to the labor market and in helping to place young people in environments where they can develop non-cognitive skills and work readiness. 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 could have important ramifications for similar programs, policies, and practices across the nation aimed at employing youth in other cities.

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Data Appendix

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

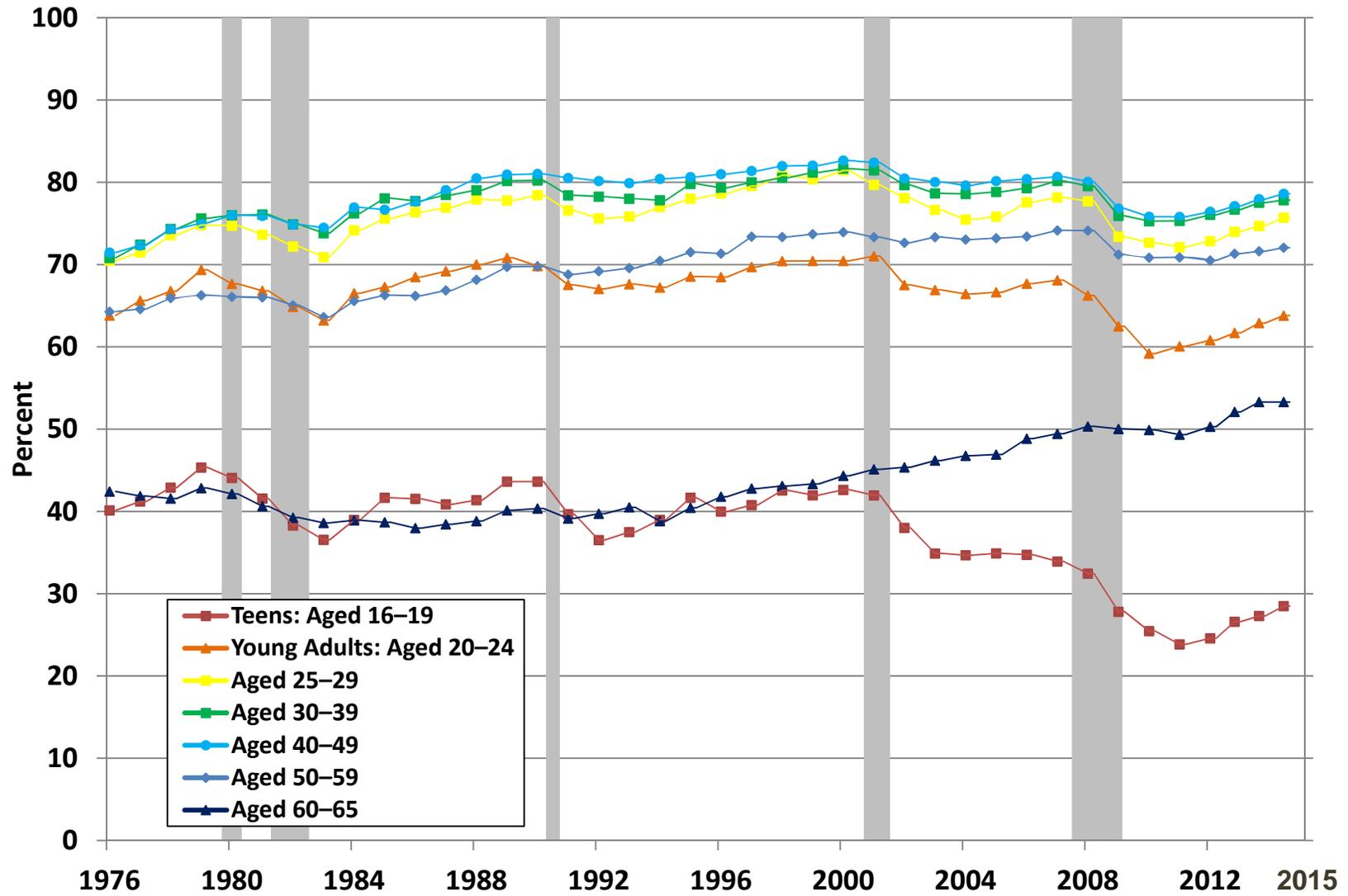
The survey was originally developed and implemented by the Youth Violence Prevention Collaborative, an initiative that began funding summer employment opportunities in Boston neighborhoods that had been identified by the Boston Police Department as having a high number of fatal and non-fatal shootings. 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.

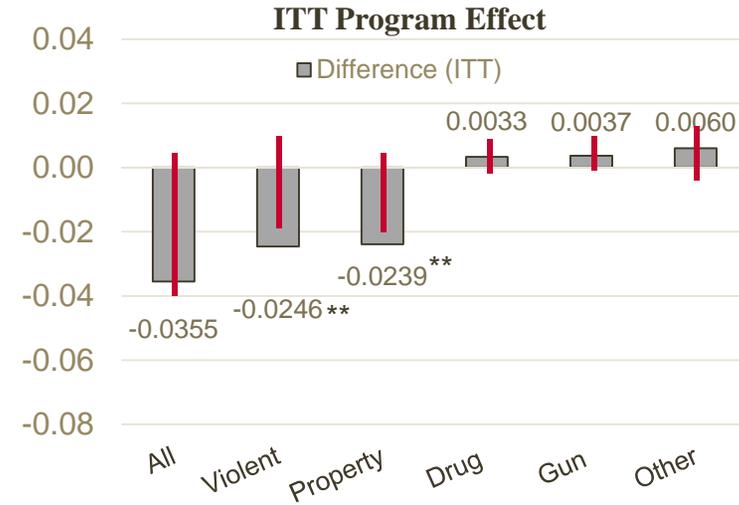
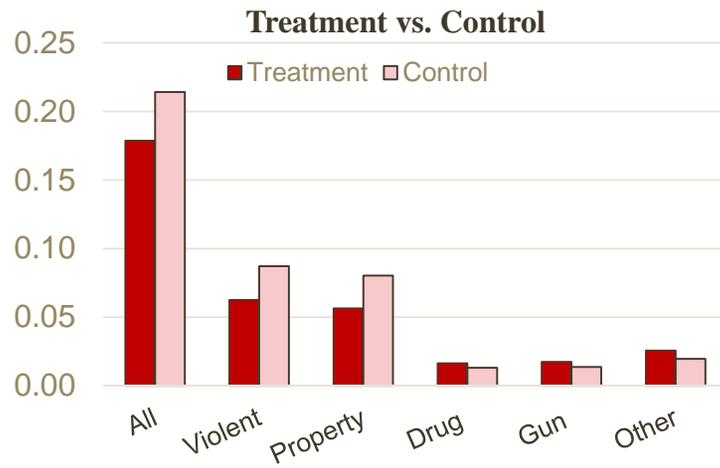
Figure 1
U.S. Employment-to-Population Ratio by Age Group, 1976–2015



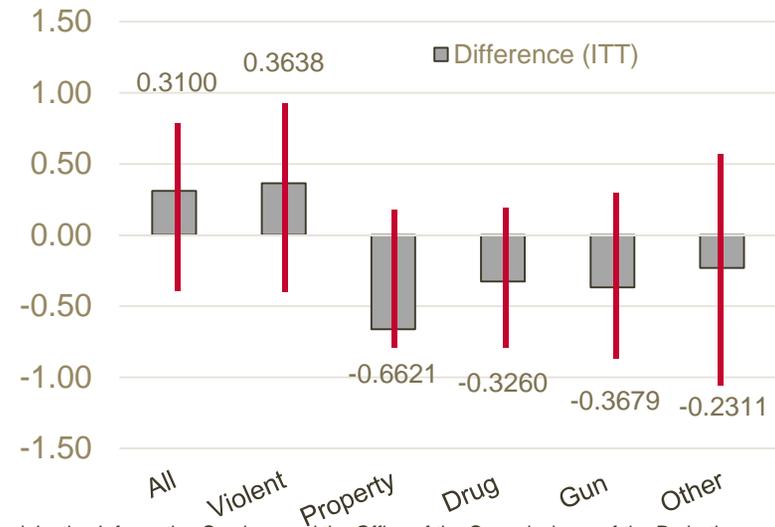
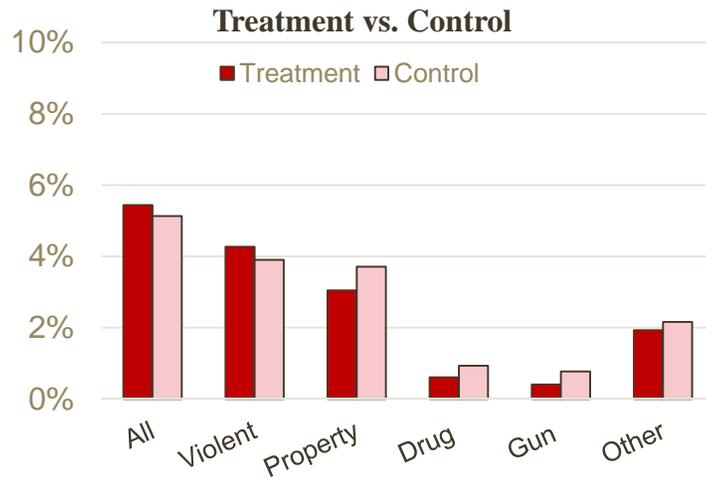
Source: Author's calculations from the U.S. Census Bureau, Current Population Survey, various years.

Figure 2
ITT Estimates of the Impact of the Boston SYEP on Criminal Activity, ABCD Summer 2015

Panel A. Average Number of Crimes Per Youth

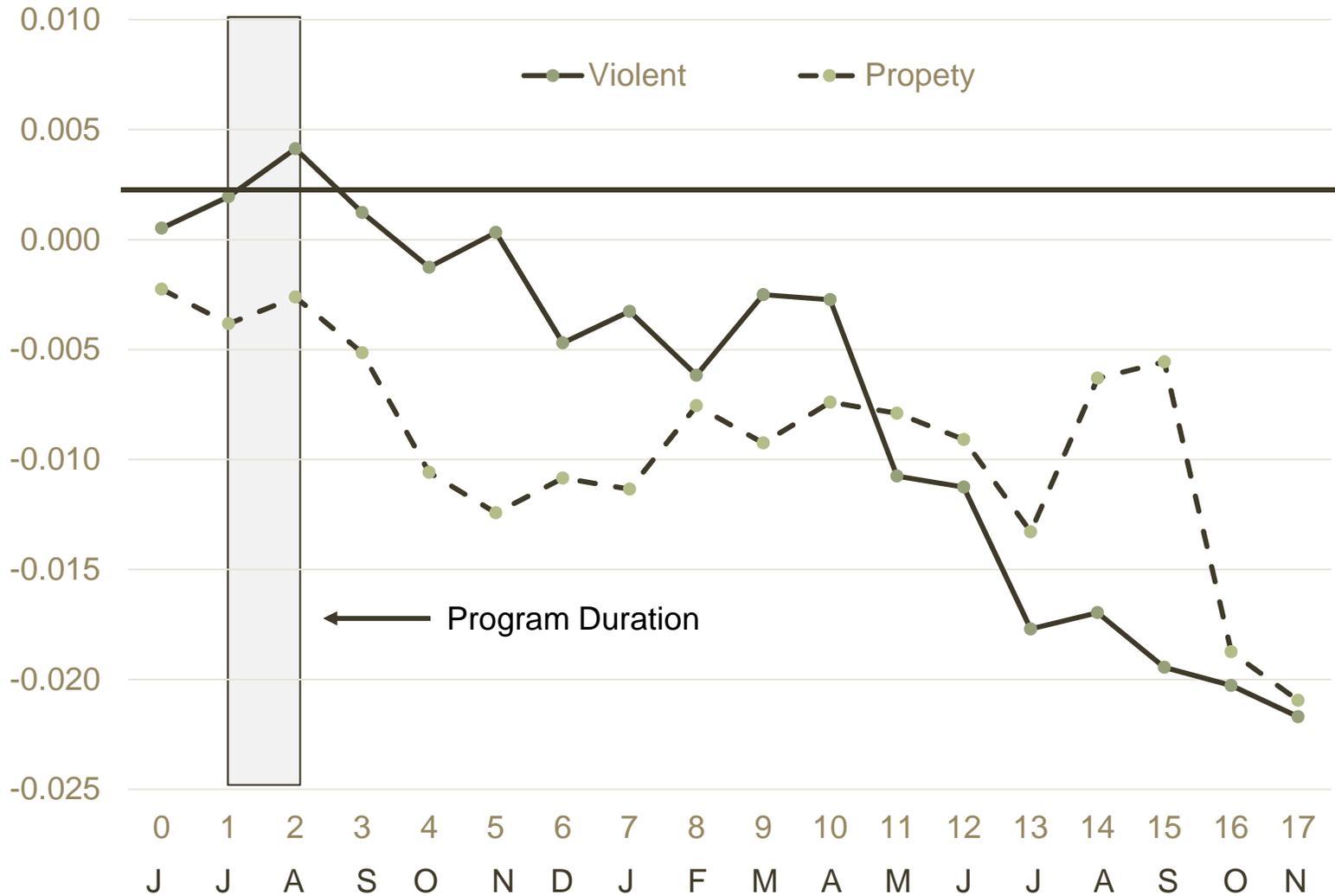


Panel B. Percent of Youth Arraigned



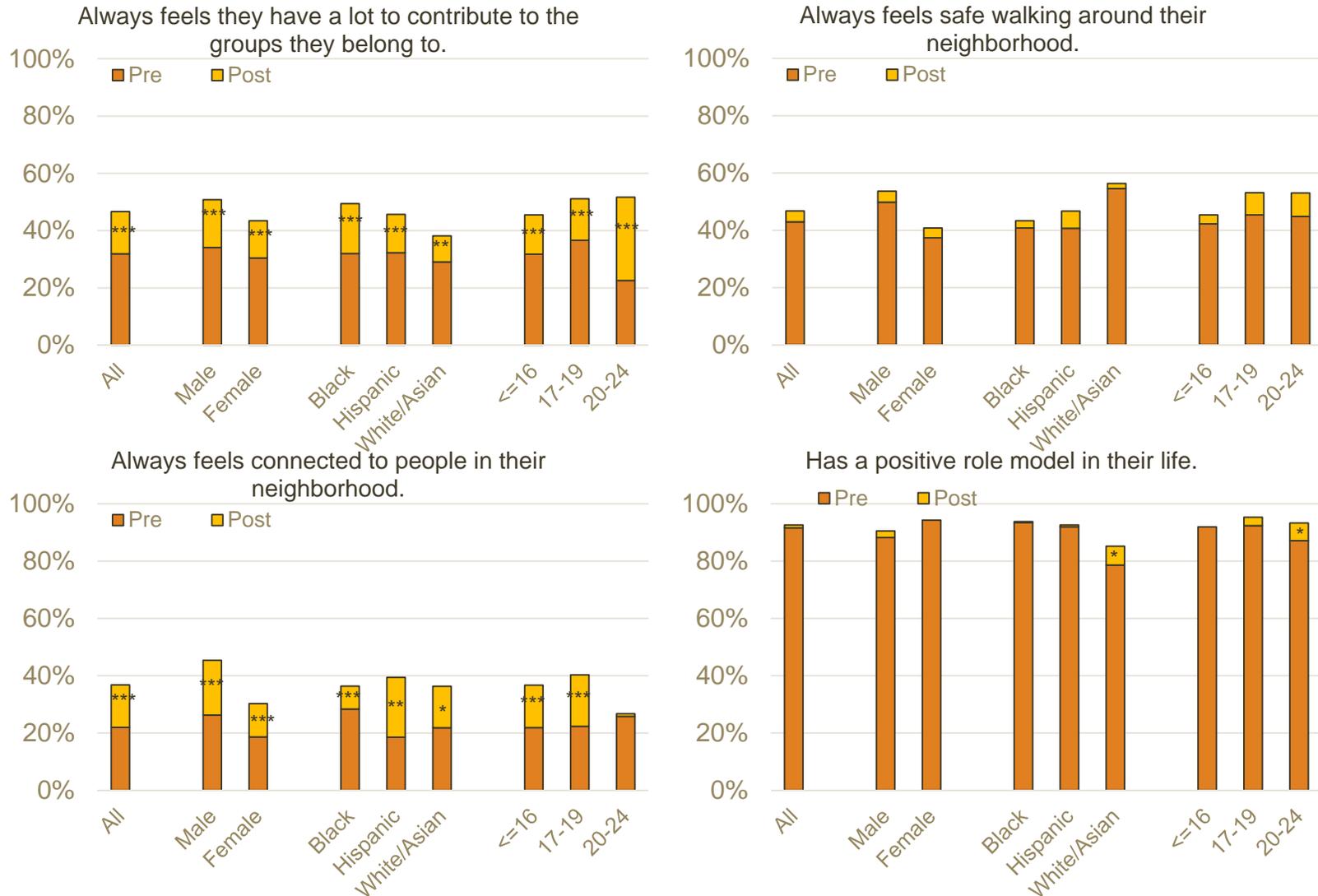
Source: Author's calculations based on administrative data provided by the Department of Criminal Justice Information Services and the Office of the Commissioner of the Probation.
 Note: *Indicates that the difference is statistically significance at the 10 percent level, ** at the 5 percent level, and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

Figure 3
ITT Estimate of Cumulative Decrease in Arrests by Type, ABCD Summer 2015



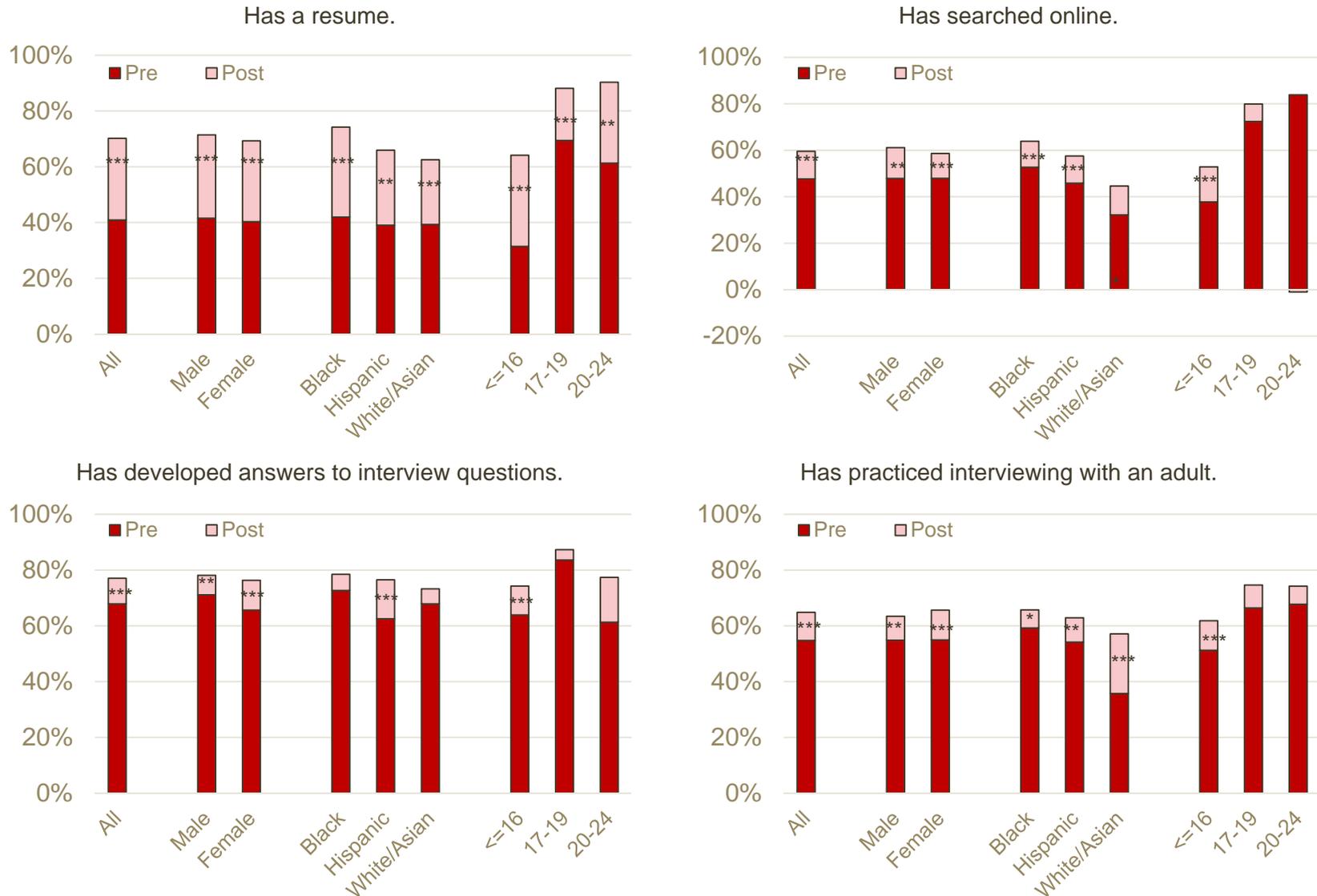
Source: Massachusetts Department of Criminal Justice Information Services and Office of the Commissioner of Probation.

Figure 4
Social and Community Engagement: By Demographic Group, ABCD Participants



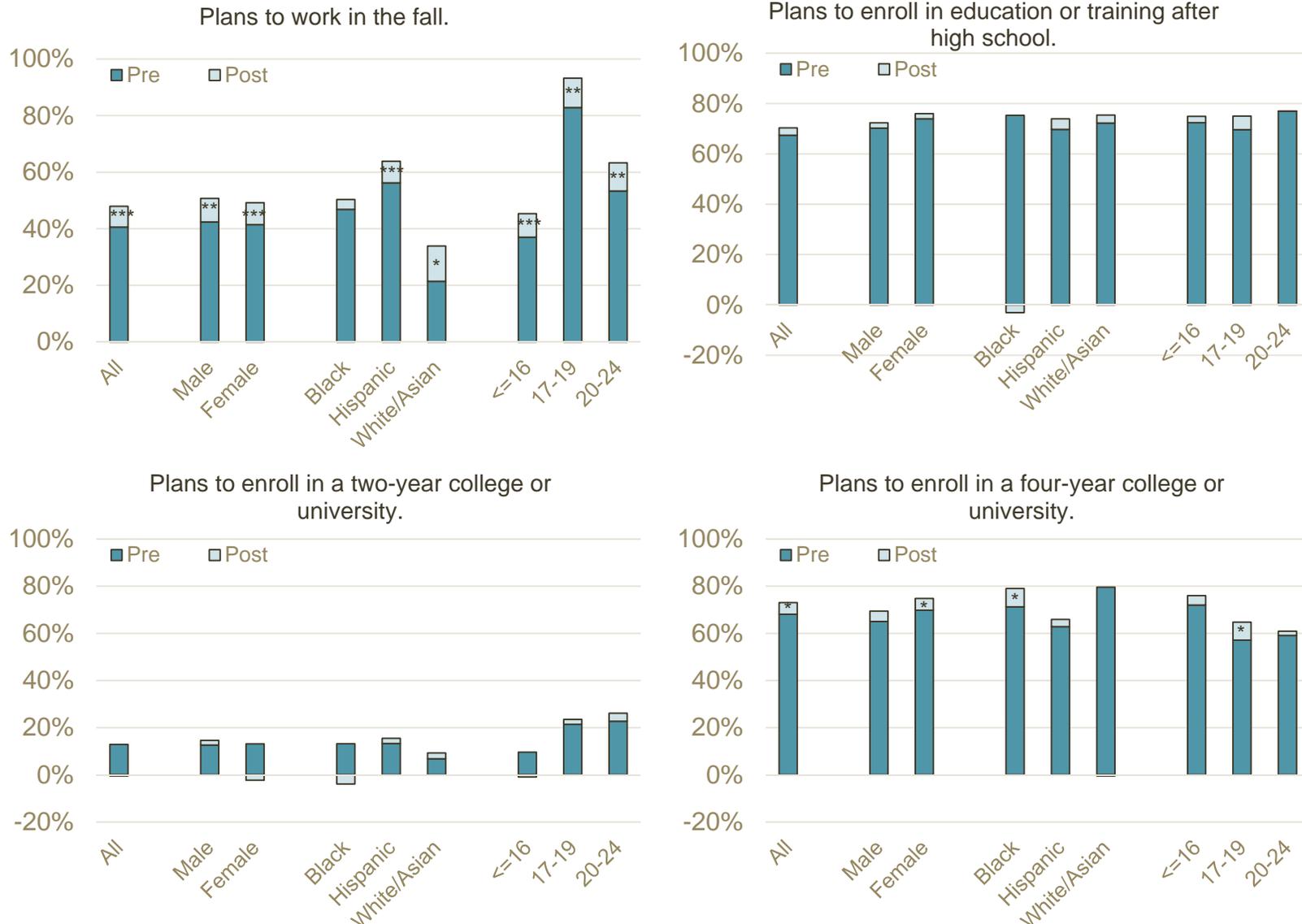
Source: Data for pre- versus post-program survey participants reflect the author's calculations based on survey data provided by the City of Boston, Office of Workforce Development.
 Note: *Indicates that the difference is statistically significant at the 10 percent level, ** at the 5 percent level, and *** at the 1 percent level.

Figure 5
Job Readiness Skills: By Demographic Group, ABCD Participants



Source: Data for pre- versus post-program survey participants reflect the 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 6
Future Aspirations: By Demographic Group, ABCD Participants



Source: Data for pre- versus post-program survey participants reflect the 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. Summary Statistics for Program Applicants and Participants by Intermediary, Summer 2015

Intermediary	Population served	Age group	Number of applicants	Number of participants	Random assignment used?	Career curriculum administered to all youth?	Percent of youth placed in private-sector jobs
Action for Boston Community Development (ABCD)	Younger, low-income youth	14-22	4235	1186	Yes	Yes	8%
Youth Employment and Engagement (YEE)	In-school and disconnected youth	14-22	7391	3209	Yes	No	10%
Boston Private Industry Council (BPIC)	Boston Public School students	14-18	2072	2072	No	Yes	63%
Youth Options Unlimited (YOU)	Court-involved and gang-related youth	15-24	141	141	No	Yes	0%
TOTAL			13839	6608	11626	51%	26%

Source: Author's calculations from data supplied by the City of Boston, Office of Workforce Development.

Table 2. ABCD 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)
Other/mixed-Race	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: Based on application data provided by the City of Boston Office of Workforce Development.

Notes:

Standard errors are in parentheses.

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

Table 3. Testing the Validity of the ABCD Lottery

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

Sources:

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

Notes:

Robust standard errors are in parentheses.

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

Table 4. 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)
Other / two or more races	2.7%	(0.007)	3.1%	(0.007)
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)

Sources:

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

Notes:

Standard errors are in parentheses.

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

Table 5. Program Effect on Court Involvement by Type of Crime: Number of Arraignments per Youth

	All groups combined	Youth: Age 14-18 years				Youth: Age 19-24 years			
		African American		Hispanic		African American		Hispanic	
		Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<u>Without covariates</u>									
All Crime	-0.015 (0.010)	-0.063 *** (0.022)	0.019 (0.019)	-0.005 (0.027)	0.033 (0.034)	0.046 (0.049)	0.015 (0.042)	-0.203 *** (0.033)	0.032 (0.061)
Violent crimes	-0.025 ** (0.007)	-0.035 ** (0.014)	0.025 (0.016)	-0.011 (0.016)	0.004 (0.006)	-0.027 (0.023)	-0.001 (0.034)	-0.047 ** (0.018)	-0.020 (0.014)
Property crimes	-0.024 ** (0.006)	-0.020 (0.014)	-0.004 (0.010)	(0.009)	0.019 (0.012)	-0.104 *** (0.023)	0.014 (0.023)	-0.101 *** (0.025)	-0.010 (0.010)
<u>With covariates</u>									
All Crime	-0.015 (0.010)	-0.061 *** (0.022)	0.022 (0.019)	0.002 (0.027)	0.033 (0.036)	0.065 (0.050)	0.018 (0.042)	-0.139 *** (0.042)	0.073 (0.059)
Violent crimes	-0.024 ** (0.007)	-0.033 *** (0.014)	0.026 (0.016)	-0.006 (0.016)	0.004 (0.006)	-0.018 (0.023)	0.001 (0.033)	-0.037 ** (0.018)	-0.009 (0.011)
Property crimes	-0.023 ** (0.006)	-0.017 (0.013)	-0.003 (0.010)	-0.005 (0.019)	0.019 (0.012)	-0.089 *** (0.023)	0.014 (0.024)	-0.043 ** (0.023)	-0.002 (0.003)
N	5934	1585	1313	865	698	413	366	161	121

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

Notes:

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

Robust standard errors are in parentheses.

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

Table 6. Program Effect on Court Involvement by Type of Crime, Pre vs. Post Program for ABCD 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.

Notes:

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.

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 after the program ends (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 7. Summer Employment Rates and Experiences for ABCD Survey Respondents by Lottery Outcome

	Treatment		Control	
	(1)		(2)	
<u>ALL RESPONDENTS:</u>	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)
<u>WORKERS:</u>				
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)
<u>PARTICIPANTS:</u>				
Percent reporting that they strongly agree that because of the summer program they have learned more...				
About how to be on time	54.0%		NA	
About new computer skills	26.9%		NA	
About how to solve problems	43.1%		NA	
About how to manage my money	47.9%		NA	
About how to organize my work for the day and keep to my schedule	51.0%		NA	
About how to manage my emotions and my temper	43.8%		NA	
About how to ask for help when I need it	48.3%		NA	
About how to constructively resolve a conflict with a peer	42.4%		NA	
About how to find new resources (health, finances, school, careers)	41.8%		NA	
About how to make eye contact when having a conversation	53.0%		NA	

Sources:

Data for post-program survey responses of treatment versus control group reflect the author's calculations based on survey data provided by the City of Boston, Office of Workforce Development.

Notes:

Standard errors are in parentheses.

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

Table 8. Change in Short-Term Behavioral Outcomes Pre vs. Post-Program for ABCD Participants Responding to Both Surveys, Summer 2015

CATEGORY	ABCD participants N=663			
	Pre	Post	Difference	
<u>Social and community engagement</u>				
I have a lot to contribute to the groups I belong to (all of the time)	31.9%	46.6%	14.7	***
I feel connected to people in my neighborhood (all of the time)	22.0%	36.8%	14.8	***
I feel safe walking around my neighborhood (all of the time)	42.9%	46.7%	3.8	
I have a positive role model in my life	91.6%	92.6%	1.0	
<u>Job readiness skills</u>				
Have all key information to apply for a job	81.0%	88.2%	7.2	**
Have prepared a resume	40.9%	70.1%	29.3	***
Have prepared a cover letter	23.4%	43.7%	20.4	***
Have asked an adult to serve as a reference.	70.9%	74.5%	3.6	
Have reviewed at least one job application form	74.8%	82.4%	7.5	**
Have completely filled out at least one online job application form.	66.1%	70.9%	4.8	*
Have searched for jobs online using a job board or state web site.	47.7%	59.6%	11.9	***
Have asked an adult for help in finding job opportunities	83.0%	84.6%	1.7	
Have developed answers to the usual questions asked during an interview	67.9%	77.1%	9.2	**
Have practiced my interviewing skills with an adult	54.8%	64.9%	10.1	***
Have appropriate professional clothes to wear to an interview.	85.1%	89.6%	4.5	*
Have made a plan for how to get to work every day	91.9%	92.5%	0.6	
Can pass a criminal background check	91.0%	91.9%	0.9	
Can pass a drug test	91.7%	92.6%	0.9	
<u>Career and academic aspirations</u>				
Plan to work in the fall	40.6%	48.0%	7.4	**
Plan to enroll in any education or training program after high school	67.4%	70.3%	2.9	
If plan to enroll in education or training program, share indicating that they plan to enroll in:				
Four year college or university	68.1%	73.0%	4.9	*
Two year college	12.9%	12.4%	-0.5	
Attend a training program for a future career	9.5%	7.8%	-1.6	
Go to technical/vocational school	4.2%	2.4%	-1.8	
Join the military	3.1%	2.9%	-0.2	
Other	2.2%	1.5%	-0.7	

Sources:

Data for pre- versus post-program survey participants provided by the City of Boston, Office of Workforce Development.

Notes:

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

Table 9. Comparison of Survey Responses by Demographic Groups: ABCD Treatments versus Controls, Summer 2015

CATEGORY	All groups combined	"In-school" youth: Age 14-18 years			
		African American		Hispanic	
		Males	Females	Males	Females
Social and community engagement					
I have a lot to contribute to the groups I belong to	0.156 *** (0.029)	0.180 ** (0.068)	0.132 ** (0.057)	0.173 ** (0.088)	0.128 * (0.073)
I feel connected to people in my neighborhood	0.212 *** (0.025)	0.260 *** (0.059)	0.148 *** (0.050)	0.251 *** (0.084)	0.224 *** (0.065)
I feel safe walking around my neighborhood	0.193 *** (0.028)	0.200 *** (0.066)	0.195 *** (0.053)	0.260 *** (0.078)	0.174 ** (0.070)
Job readiness skills					
Have all key information to apply for a job	0.094 *** (0.021)	0.064 (0.053)	0.080 ** (0.042)	0.080 (0.057)	0.059 (0.055)
Have prepared a resume	0.245 *** (0.027)	0.317 *** (0.052)	0.187 *** (0.055)	0.313 *** (0.075)	0.238 *** (0.071)
Have prepared a cover letter	0.217 *** (0.028)	0.257 *** (0.061)	0.230 *** (0.055)	0.285 *** (0.085)	0.204 ** (0.071)
Have asked an adult to serve as a reference.	-0.001 (0.027)	-0.016 (0.065)	-0.055 (0.052)	0.105 (0.074)	-0.056 (0.065)
Have reviewed at least one job application form	0.039 (0.024)	-0.001 (0.053)	0.027 (0.044)	0.086 (0.071)	0.025 (0.057)
Have completely filled out at least one online job application form.	-0.033 (0.028)	-0.003 (0.063)	-0.082 (0.052)	0.023 (0.078)	-0.090 (0.066)
Have searched for jobs online using a job board or state web site.	0.025 (0.031)	0.152 ** (0.066)	-0.110 ** (0.057)	0.103 (0.090)	-0.018 (0.078)
Have asked an adult for help in finding job	0.071 *** (0.024)	0.041 (0.053)	0.026 (0.042)	0.135 ** (0.060)	0.068 (0.055)
Have developed answers to the usual interview	0.069 *** (0.026)	0.111 * (0.062)	0.056 (0.051)	0.088 (0.071)	0.031 (0.062)
Have practiced my interviewing skills with an adult	0.064 ** (0.031)	0.118 * (0.071)	0.074 (0.059)	0.069 (0.085)	0.012 (0.075)
Have appropriate professional clothes to wear to an interview.	0.043 ** (0.020)	0.088 ** (0.044)	0.008 (0.034)	0.098 * (0.055)	0.024 (0.042)
Have made a plan for how to get to work every day	0.090 *** (0.019)	0.085 ** (0.041)	0.055 * (0.034)	0.113 ** (0.046)	0.028 (0.034)
Can pass a criminal background check	-0.053 *** (0.016)	-0.064 (0.044)	----- -----	0.000 (0.037)	-0.076 * (0.043)
Can pass a drug test	-0.042 *** (0.015)	-0.029 (0.036)	-0.023 (0.025)	----- -----	-0.052 * (0.035)
Career and academic aspirations					
Plan to work in the fall	-0.074 ** (0.030)	0.080 (0.070)	-0.076 (0.057)	-0.038 (0.086)	-0.204 *** (0.063)
Plan to enroll in education or training program after high school	0.003 (0.017)	-0.002 (0.040)	0.017 (0.034)	-0.007 (0.048)	0.011 (0.039)
Plan to attend a four year college or university	0.110 *** (0.081)	0.099 (0.065)	0.171 *** (0.052)	-0.103 (0.084)	0.169 ** (0.066)
Plan to attend a two year college	0.062 *** (0.019)	0.049 (0.041)	0.094 *** (0.033)	0.117 * (0.070)	0.018 (0.044)

Sources:
Data for post-program survey responses for treatments and control groups provided by the City of Boston, Office of Workforce Development.

Notes:
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 10. Relationship Between SYEP Impact on Short-Term Behavioral Changes and Subsequent Criminal Activity

CATEGORY	Number of arraignments per youth					
	All		Violent		Property	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
<u>Social and community engagement</u>						
Youth increasing in feeling that they contribute to the groups they belong to	-0.031	(0.018) *	-0.009	(0.011)	0.012	-1.080
Youth increasing in feeling connected to people in their neighborhood	0.014	(0.019)	0.006	(0.012)	0.012	-0.012
Youth indicating they have learned how to manage emotions	-0.056	(0.018) ***	-0.032	(0.011) ***	-0.026	-0.011 **
Youth indicating they have learned how to ask for help	-0.026	(0.018)	0.002	(0.011)	-0.031	-0.010 ***
Youth indicating they have learned how to resolve conflict with a peer	-0.055	(0.018) ***	0.010	(0.012)	-0.025	-0.010 **
Youth indicating that they want to improve their conflict resolution skills	-0.128	(0.055) **	-0.101	(0.044) **	-0.023	-0.031
<u>Job readiness</u>						
Youth indicating they have learned how to prepare a resume	-0.033	(0.019) *	0.001	(0.012)	-0.021	-0.010 **
Youth indicating they have learned how to prepare a cover letter	-0.001	(0.021)	0.013	(0.014)	-0.009	-0.012
Youth indicating they have learned how to search for a job online	0.031	(0.022)	0.017	(0.014)	-0.009	-0.012
Youth indicating they have gained interview skills	-0.009	(0.022)	-0.004	(0.014)	0.006	-0.014
Youth indicating they have gained a job mentor	-0.027	(0.018)	0.008	(0.011)	-0.007	-0.011
<u>Career and academic aspirations</u>						
Youth increasing aspirations to work in the fall	0.023	(0.024)	0.002	(0.014)	0.002	-0.014
Youth increasing aspirations to attend two-year college	0.055	(0.039)	0.005	(0.022)	0.031	-0.026
Youth increasing aspirations to attend four-year college	0.005	(0.026)	0.007	(0.017)	0.000	-0.015
Number of Observations	5934		5934		5934	

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

Notes:

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

Robust standard errors are in parentheses.

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

Table A1. Distribution of Pre- and Post-Program Survey Respondents by Demographic Characteristics, Summer 2015

	ABCD			
	Pre-program survey only 791		Pre and post-program survey 663	
Total				
Age				
Mean	15.70	(0.069)	15.73	(0.078)
14-18	88.8%	(0.012)	88.2%	(0.014)
19-24	13.0%	(0.014)	11.8%	(0.014)
Gender				
Female	53.8%	(0.019)	53.9%	(0.021)
Male	46.1%	(0.018)	46.1%	(0.021)
Race/ethnic Group				
African American	49.9%	(0.019)	51.5%	(0.021)
Asian	6.0%	(0.009)	6.5%	(0.010)
Hispanic	38.1%	(0.018)	36.1%	(0.020)
White	3.8%	(0.007)	3.2%	(0.007)
Other/mixed-Race	2.4%	(0.006)	2.7%	(0.007)
Living situation				
Single parent family	65.2%	(0.018)	63.7%	(0.020)
Two parent family	25.7%	(0.016)	29.4%	(0.019)
Other relative	9.6%	(0.011)	8.1%	(0.012)
Other	6.4%	(0.009)	6.3%	(0.010)
Language spoken at home				
Chinese	4.3%	(0.008)	3.9%	(0.008)
English	74.1%	(0.017)	74.0%	(0.019)
Spanish	18.6%	(0.015)	18.5%	(0.016)
Other	2.98%	(0.006)	3.6%	(0.008)

Sources:

Data for pre- versus post-survey participants provided by the City of Boston, Office of Workforce Development.

Notes:

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.