Nothing Stops a Bullet like a Job? The Long-Term Impact of Summer Youth Employment Programs on Criminal Justice Involvement[1]

Alicia Sasser Modestino[2], Aria Golestani[3], Brandon Turchan[4], Sebastian Ramirez[5], Benjamin Struhl[6], and Anthony A. Braga[7]

Abstract

Summer Youth Employment Programs (SYEPs) have been shown to reduce youth violence in the near-term, yet little is known about how to sustain these impacts. Employing a lottery design, we find the Boston SYEP significantly reduces violent crime arrests for up to 4.5 years. These impacts are driven by older youth and those with a prior arrest, suggesting SYEPs produce safer communities by providing jobs to high-risk youth. Using a unique dataset of field interrogation and observations, we show these effects are driven by lower co-offending as well as receiving a second summer of participation, possibly disrupting negative peer interactions.

JEL Codes: J13, J21, K42

[1]We are grateful to Bocar Ba, Sarah Tahamont, and the participants of the 2024 Association for Public Policy & Management Conference for helpful comments and suggestions.

[2]Aicia Modestino is Associate Professor of Economics and Public Policy in the School of Public Policy and Urban Affairs at : Northeastern University. Email: a.modestino@northeastern.edu.

[3]Aria Golestani is a Criminal Justice Innovation Fellow at the Social-Science Research Council. Email: ariagolestani.io@gmail.com.

[4]Brandon Turchan is Assistant Professor of Criminology in the School of Criminal Justice at : Michigan State University. Email: turchanb@msu.edu.

[5]Sebastian Ramirez is a doctoral student in the School of Public Policy and Urban Affairs at Northeastern University. Email: ramirezramirez.d@northeastern.edu.

[6]Ben Struhl is the Executive Director of the Crime and Justice Policy Lab at the University of Pennsylvania. Email: bstruhl@sas.upenn.edu.

[7]Anthony Braga is the Jerry Lee Professor of Criminology and the Director of the Crime and Justice Policy Lab in the Department of Criminology at the University of Pennsylvania. Email: abraga@upenn.edu.

Please not that an Online Appendix has been included.

Disclosure Statements

Funding Organizations: This work was supported with funding from Arnold Ventures Grant ID: 21-06247.

Review of the Work: Both City of Boston's Office of Workforce Development and the Boston Police Department have requested to review the results of the study prior to its circulation to ensure that the confidentiality of the data was not unintentionally compromised.

Institutional Review Board (IRB) Approval: IRB approval was granted by Northeastern University IRB #20-01-10.

Pre-registration: This work was pre-registered with the Open Science Framework (OSF) on September 10, 2022 (Modestino and Braga, 2022) [https://osf.io/et376/].

Data Replication Statement: The data used in this research study are administrative data that are confidential in nature and cannot be made publicly available. The personally identifiable information of the youth were obtained from the City of Boston's Office of Workforce Development (https://www.boston.gov/departments/workforce-development). The criminal justice records were obtained from the Boston Police Department (https://police.boston.gov/). The data can be obtained by filing a separate request directly with each entity and the authors are willing to assist (Alicia Modestino a.modestino@northeastern.edu).

Disclosure Statement

Nothing Stops a Bullet like a Job? The Long-Term Impact of Summer Youth Employment Programs on Criminal Justice Involvement

Proprietary Data

The data used in this research are proprietary. The PII of the youth were obtained from the City of Boston's Office of Workforce Development. The criminal justice records were obtained from the Boston Police Department. Both institutions have requested to review the results of the study prior to their dissemination to ensure that the confidentiality of the data was not unintentionally compromised.

Foundation Funding

This work was funded by Arnold Ventures, a foundation which is interested in generating causal evidence using randomized controlled trials (RCTs). During the past three years I have also received funding from the William T. Grant Foundation which is interested in generating research evidence that can be used to improve the lives of young people. Although William T. Grant did not fund this project, they would be an interested third party to this research. I have also received funding from the City of Boston and Action for Boston Community Development (ABCD) related to youth workforce development. Although neither the City nor ABCD funded this project, they would be an interested third party to this research.

I have no other relevant or material financial interests that relate to the research described in this paper.



_____

Alicia Sasser Modestino

July 29, 2025

Disclosure Statement for Aria Golestani

Nothing Stops a Bullet like a Job? The Long-Term Impact of Summer Youth Employment Programs on Criminal Justice Involvement

Proprietary Data:
The data used in this research are proprietary. The PII of the youth were obtained from the City of Boston's Office of Workforce Development. The criminal justice records were obtained from the Boston Police Department. Both institutions have requested to review the results of the study prior to their dissemination to ensure that the confidentiality of the data was not unintentionally compromised.

Funding Organizations: Arnold Ventures funded my postdoc for this work. Arnold Ventures currently funds me for my other projects as well.

Positions: Nothing to disclose

Relatives/Partners: Nothing to disclose

Review of the Work: Nothing to disclose.



Aria Golestani

July 23, 2025

Disclosure Statement

Nothing Stops a Bullet like a Job? The Long-Term Impact of Summer Youth Employment Programs on Criminal Justice Involvement

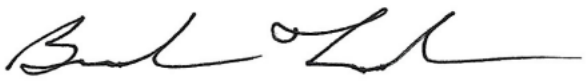
Proprietary Data

The data used in this research are proprietary. The PII of the youth were obtained from the City of Boston's Office of Workforce Development. The criminal justice records were obtained from the Boston Police Department. Both institutions have requested to review the results of the study prior to their dissemination to ensure that the confidentiality of the data was not unintentionally compromised.

Foundation Funding

This work was funded by Arnold Ventures, a foundation which is interested in generating causal evidence using randomized controlled trials (RCTs).

I have no other relevant or material financial interests that relate to the research described in this paper.



_____

Brandon Turchan

July 23, 2025

Disclosure Statement

Nothing Stops a Bullet like a Job? The Long-Term Impact of Summer Youth Employment Programs on Criminal Justice Involvement

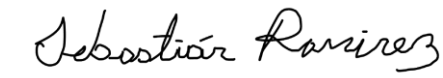
Proprietary Data

The data used in this research are proprietary. The PII of the youth were obtained from the City of Boston's Office of Workforce Development. The criminal justice records were obtained from the Boston Police Department. Both institutions have requested to review the results of the study prior to their dissemination to ensure that the confidentiality of the data was not unintentionally compromised.

Foundation Funding

This work was funded by Arnold Ventures, a foundation which is interested in generating causal evidence using randomized controlled trials (RCTs).

I have no other relevant or material financial interests that relate to the research described in this paper.



Sebastián Ramírez Ramírez

July 23, 2025

Disclosure Statement

Nothing Stops a Bullet like a Job?
The Long-Term Impact of Summer Youth Employment Programs on Violent Crime

Proprietary Data

This research used proprietary data. The PII of study youth were acquired from the City of Boston's Office of Workforce Development. The Boston Police Department provided criminal justice records for the study youth. Both institutions have requested to review the results of the study prior to their dissemination to ensure that the confidentiality of the data was not unintentionally compromised.

Foundation Funding

This work was funded by Arnold Ventures, a foundation which is interested in generating causal evidence using randomized controlled trials (RCTs). During the past three years, I have also received funding from the U.S. National Institute of Justice, Bloomberg Philanthropies, Annie Casey Foundation, Joyce Foundation, Massachusetts Executive Office of Public Safety and Security, and Pew Charitable Trusts. Although they did not support this work, these funders supported work in the general youth violence prevention area. As such, they would be an interested third party to this research.

I have no other relevant or material financial interests that relate to the research described in this paper.



_____

Ben Struhl

July 29, 2025

Disclosure Statement

Nothing Stops a Bullet like a Job?
The Long-Term Impact of Summer Youth Employment Programs on Violent Crime

Proprietary Data

This research used proprietary data. The PII of study youth were acquired from the City of Boston's Office of Workforce Development. The Boston Police Department provided criminal justice records for the study youth. Both institutions have requested to review the results of the study prior to their dissemination to ensure that the confidentiality of the data was not unintentionally compromised.

Foundation Funding

This work was funded by Arnold Ventures, a foundation which is interested in generating causal evidence using randomized controlled trials (RCTs). During the past three years, I have also received funding from the U.S. National Institute of Justice, Bloomberg Philanthropies, Annie Casey Foundation, Joyce Foundation, Massachusetts Executive Office of Public Safety and Security, and Pew Charitable Trusts. Although they did not support this work, these funders supported work in the general youth violence prevention area. As such, they would be an interested third party to this research.

I have no other relevant or material financial interests that relate to the research described in this paper.



_____

Anthony Braga

July 23, 2025

I. Introduction

During the COVID-19 pandemic, many American cities faced a sharp increase in violent crime, including Boston, where gun violence alone jumped 29 percent between 2019 and 2020 (The Boston Globe 2020). This uptick in violent crime prompted a renewed interest in finding innovative interventions to address youth violence in particular, including ways to scale-up Summer Youth Employment Programs (SYEPs) (Grawert and Kim 2023). This was highlighted in a speech by President Biden who urged cities and states to "build back better" after the pandemic by using their American Rescue Plan Act (ARPA) funding to expand SYEPs as one potential way to curb youth violence (Biden 2021). In response, many jurisdictions adopted the mindset that "nothing stops a bullet like a job" (Ludwig and Schnepel 2024), increasing the number of meaningful summer employment opportunities for young people with the hope of reducing their involvement with the criminal justice system and making communities safer during the summer months and beyond (American Youth Policy Forum 2021).

Yet it may be the case that any impacts are short-lived without additional intervention and/or targeting. Many cities will be hard-pressed to maintain their pandemic-era SYEP expansions once the ARPA funding has expired and federal bills to directly support summer jobs have gained little traction (Scott 2021).[1] With average program costs of upwards of $2,500 per youth, most SYEPs are typically oversubscribed, even during periods of federal assistance, such that cities often allocate jobs via lottery or on a first-come first-served basis (Heller and Kessler 2017). This limits the ability for jurisdictions to target the program towards high-risk youth, creating a stark tradeoff between serving as many youth as possible versus a more tailored approach that can provide a second "dose" of summer employment to sustain the program's impact for those who are most likely to be involved in violence.

Thus, understanding whether SYEPs improve youth outcomes in the long run is crucial for cities weighing the potential costs and benefits of future investments in summer jobs, especially given the logistical challenges of placing thousands of youth across hundreds of local employers every summer. The initial violence prevention gains generated by SYEPs are very encouraging and do indeed seem to benefit communities in the near-term. Randomized controlled trials across

[1] ARPA funds for state and local governments had to be obligated (committed to projects) by December 31, 2024, and must be spent by December 31, 2026 (Rochford et al., 2024).

four major U.S. cities have shown that SYEPs reduce criminal justice involvement with participants being 23 percent less likely to be arrested for a felony during the summer in which they work (Kessler et al. 2022) and also involved in 35 to 40 percent fewer violent crimes up to 18 months after the program ends, compared to a control group (Heller 2014; Heller 2022; Modestino 2019).

Despite this growing body of research on the positive impact of summer job programs across a range of near-term outcomes, little is known about the long-term SYEP effects and how the observed short-run impacts can be sustained—both of which are crucial for replicating the program across new cities or bringing it to scale within existing ones (Li and Jackson-Spieker 2022). To our knowledge, only one prior study exists, finding no significant impact of the New York City SYEP five years post-program participation, except for youth with a prior arrest who experience a 10 percent reduction in felony arrests (Kessler et al. 2022). However, that study only had access to adult criminal justice records, which limited the analysis to youth who were already at the age of majority prior to participating in the program (aged 16 to 18 years in New York), potentially resulting in downward biased estimates that fail to capture juvenile offending and understate the program's impacts on younger youth.

Moreover, little is known about the mechanisms by which SYEP program impacts are achieved, especially the role of peers, which could have important consequences for the degree to which programs can be targeted while achieving the same impressive reductions in criminal justice involvement across multiple jurisdictions. On the one hand, violent offending typically increases through the mid-twenties (Sampson and Laub 2003), or even later for youth living in neighborhoods with high crime rates (Monahan, Steinberg, and Piquero 2015), making it questionable whether a six-week intervention would have long-lasting effects. On the other hand, for youth who are at greater risk for criminal justice involvement, having a summer job can be transformational (Mortimer 2010), especially considering the salient influence of peers in the etiology of delinquency (Haynie 2001; Haynie 2002; McGloin and Thomas 2019). Indeed, most studies find between 50 and 75 percent of juvenile crimes are committed in the company of others (Warr 2002) with the greatest concentration of serious youth violence occurring among individuals who are involved in gangs and other high-risk co-offending networks (Papachristos et al. 2015). Although prior research suggests that summer job programs reduce violent crime by improving soft skills (Modestino 2019), other potential channels that may sustain these

behavioral changes, such as repeat participation during a second consecutive summer or positive peer interactions that serve to disrupt co-offending networks, remain unexplored.

Our study builds on and broadens the scope of this prior literature through both improved measurement of the long-term impacts of SYEPs on criminal justice outcomes and identification of potential underlying mechanisms: (1) across different types of crime, (2) among different groups of youth, (3) between youth and their peers. First, we estimate program effects on arrests separately by the most serious charge to understand whether having a summer job affects how youth respond to general opportunities to engage in criminal activity (that is, total arrests) relative to situations that lead to interpersonal violence (that is, violent crime arrests) and economic crimes (that is, property or drug crimes). Second, we compare the program's impacts across key sub-groups that are often targeted by criminal justice interventions to see if there is a differential impact for males, older youth aged 18 to 24 years, "at-risk" youth (having either a noncustodial police contact or a prior arrest), and "high-risk" youth (having a prior arrest). Third, we assess whether randomly receiving a second "dose" of SYEP during the following summer can help sustain the program's impacts for the longer-term, particularly for high-risk youth with a prior arrest. Finally, we explore whether the summer jobs program affects co-offending behavior among the participants as well as other criminal activity among their prior co-offending "peers" that they had interacted with before the start of the program.

To carry out this analysis, we make use of an embedded lottery design to study a cohort of 4,219 youth who applied to the Boston SYEP during the summer of 2015. We obtain personally identifying information (PII) for each youth that also includes their demographic characteristics and program experiences from Boston's Office of Workforce Development (OWD). We link these data to information provided by the Boston Police Department (BPD) from 2014 to 2019 on both juvenile and adults arrests as well as noncustodial police contacts, or "field interrogation and observation" reports (FIOs). This allows us to observe involvement with the criminal justice system during the 17 months prior, and up to 4.5 years after, SYEP participation for both juvenile and adult participants and at a much more granular level than prior studies. We use both intent-to-treat (ITT) and two-stage-least-squares (2SLS) methods to estimate program impacts over the full study period as well as cumulatively by month to examine how the trajectory of both formal (arrests) and more informal (FIO) contacts with police evolve over time, conditional

on a set of baseline pre-program demographic, criminal justice, and neighborhood characteristics.

We find that being offered a job through the Boston SYEP leads to lower criminal justice involvement for up to 4.5 years, but only among youth who are more likely to come into contact with police. In terms of the number of arrests, we find that the Boston SYEP's largest long-term impacts are the reduction in youth violence, suggesting that the program's short-term behavioral effects observed in prior studies are in fact sustained over time, but only for specific subgroups. Among males there is a 28 percent decrease in violent crime arrests over the entire 4.5 years, with 18-24 year olds and high-risk youth (for example, with a prior violent-crime arrest) experiencing even larger reductions in violent crime arrests of 53 percent and 79 percent respectively during the full follow-up period. Delving further reveals that the program reduces co-offending by 30 percent and youth who were randomly selected to participate the following summer experienced even larger drops in criminal justice involvement that also included a reduction in felonies as well as other types of crime (for example, drug offenses).

These findings run counter to the narrative that SYEPs primarily reduce violent crime among the general population of youth. Instead, we find that SYEPs are successful in engaging youth that are at high-risk of repeat violence, providing job opportunities to those high-risk youth that want them, and reducing the likelihood of future violent offending. Our results offer important theoretical insights for violence reduction policy as well as practical implications for ultimately producing safer communities. Understanding how program impacts might be sustained or even enhanced beyond the first year through targeting, repeat participation, and/or peer effects can help guide city and state officials facing stark trade-offs between allocating resources to as many youth as possible versus serving those who might benefit the most.

II. Policy Context and Experimental Design

Introduced in the early 1980s, the Boston SYEP relies on approximately $10 million in city, state, and private funding to connect upwards of 10,000 youth each summer with roughly 900 local employers. Youth are placed in either a subsidized position (for example, with a local nonprofit, community-based organization, or city agency) or in a job with a private-sector employer, with roughly one-third working in a daycare or day camp. For six weeks, from early July through mid-August, participating youth work 20 to 25 hours per week and are paid the

prevailing Massachusetts minimum wage. Youth also receive 20 hours of job-readiness training, which includes evaluating personal strengths and career interests; developing soft skills, such as communication, collaboration, and conflict resolution; and learning job readiness skills such as how to search for a job, draft a resume and cover letter, and answer typical interview questions.

The Boston SYEP is open to all city residents aged 14 to 24 years regardless of socioeconomic, school enrollment, or court-involved status. Youth typically apply through one of the four intermediary organizations under contract with OWD that are responsible for reviewing applications, matching applicants with jobs, supervising job placements, and delivering the career-readiness curriculum. Our pre-specified sampling method is restricted to youth who applied to the Boston SYEP for the summer of 2015 through Action for Boston Community Development (ABCD), a large and established nonprofit that serves 18 of Boston's 23 neighborhoods, focusing on a predominately young, school-aged, and low-income population.[2] We focus on ABCD because it is the only intermediary that makes use of simple random assignment due to the high number of applications it receives for the limited number of SYEP jobs that are available.

For our experimental design, we rely on ABCD's lottery assignment mechanism that effectively controls for selection into the program while also accounting for changes that might occur during the normal course of adolescent development. The application period extends from late February through early June, and youth are notified of their lottery status and job assignment in late June. ABCD uses a computerized system with a simple random-assignment algorithm to select youth based on their applicant ID numbers and the number of available slots, which is determined by the amount of funding each year. This system effectively assigns the offer to participate in the program at random, creating a control group of youth who apply to the SYEP but are not chosen. Of the 4,219 youth who applied to ABCD in 2015, a total of 1,186 (or 28 percent) were offered a job via simple random assignment, leaving 3,033 individuals in the control group. Of those selected by the lottery, 83.6 percent accepted the job offer, with only a handful dropping out of the program after accepting. Initially, there is little crossover across intermediaries, with only 3.0 percent of the ABCD control group obtaining a job through one of the other three intermediaries during the summer of 2015. However, over the full sample period,

[2] See Section A of the Online Appendix for more details about the characteristics of Boston SYEP applicants and how they compare to the City's broader youth population.

upwards of 20 percent of the control group subsequently participates in the Boston SYEP through one or more intermediaries, including 3.5 percent that later participate through ABCD.

III. Data

A. Criminal Justice data

To assess the impact of the Boston SYEP on criminal justice outcomes, we use both arrest and FIO data from the Boston Police Department (BPD) for the period 2014 through 2019. The benefit of using administrative data is that one avoids the problems of self-reported data such as social desirability bias, which might be significant if individuals are less willing to admit wrongdoing when applying to or working at a job. The BPD arrest and FIO data also have the advantage of allowing us to detect more frequent and less serious interactions with the criminal justice system than other administrative records (for example, court arraignments or convictions) which measure criminal activity only to the extent that an individual was arrested, booked, and appeared before a judge. Both arrest and FIO data are matched to the City of Boston's SYEP program records using each subject's name, race, date of birth, and address of residence as well as key demographic information (for example, gender, race, ethnicity).

Using the BPD arrest data allows us to compare our results directly with prior studies evaluating outcomes from other cities such as New York (Kessler et al. 2022). The arrest data contain information on each criminal charge including the arrest date, the seriousness of the crime (for example, misdemeanor or felony), and a literal description of the crime that can be used to create categories by type of crime (for example, violent, property, drug, gun, and other).

Using the FIO data allows us to document more informal criminal or suspicious activity that occur at an even higher frequency than arrests. The FIO records document a wide range of criminal justice behaviors including not only when an individual is stopped and frisked, but also when an officer engages in a consensual encounter with an individual or when an officer simply observes an individual and needs to document that observation for intelligence purposes.[3] For example, an officer may record an observation of a known gang member loitering in a high drug activity area.

[3]For more information, see the Boston Police Department Field Interrogation and Observation (FIO) Reports Dataset at https://data.boston.gov/dataset/boston-police-department-fio.

One caveat to keep in mind is that both arrest and FIO records measure criminal activity only to the extent that an individual was arrested, stopped, or observed by an officer and, as such, may reflect both criminal and police behavior. Indeed, one prior study found that the BPD uses FIO reports to direct officer deployments in crime "hot spots" and to focus law enforcement attention on active offenders, with only 5 percent of the individuals with any FIO report accounting for more than 40 percent of total FIO reports (Fagan et al. 2016). We control for these spatial differences in FIO encounters by including neighborhood Census tract characteristics such as demographic composition, median household income, labor force participation rate, unemployment rate, poverty rate, public assistance rate, and the percentage of households that are cost burdened (spend more than 30 percent of their income on rent).

B. Measuring Outcomes

The primary set of outcomes that we construct from the BPD data is the incidence and number of arrests. To identify crimes by type, we categorize charges associated with each arrest based on the offense code and corresponding offense description using the Text-based Offense Classification (TOC) tool from the Criminal Justice Administrative Records System (CJARS).[4] Violent crimes include all crimes against a person: assault, homicide, fatal and non-fatal shootings, sexual offenses, robbery, threats, kidnapping, and arson (when someone is known to be home). Property crimes include nonviolent offenses such as forgery, burglary, larceny, motor vehicle theft, shoplifting, and destruction of property. Drug crimes include both possession and dealing.

As a second set of outcomes, we also measure the incidence and number of FIOs. Prior to 2016, the FIO data was coded to include each contact between the BPD and one or more individuals as well as each individual involved in these contacts. Due to a reporting change by the department in 2016, FIO reports also contain detailed characteristics of each encounter, including location, basic demographic information about the individuals involved (for example, race, gender, age), and a brief statement about the encounter. We are able to use this information to learn more about the co-offending peers of SYEP applicants.

[4] The TOC is designed to harmonize criminal justice offense codes using a hierarchical classification framework. For more information, see https://cjars-toc.isr.umich.edu/.

We then link the SYEP program records to BPD arrest and FIO records using name and date of birth. Among the 4,219 youth in our sample, 737 of them (17.5 percent) were matched to the BPD administrative data, indicating these youth had been involved in some type of activity that resulted in any FIO report or arrest by law enforcement during the full observation period (for example, January 2014 through December 2019). We use the incidence of any criminal justice involvement (arrest or FIO) during the pre-program period (January 2014 through May 2015) to define youth as "at-risk" of future criminal justice involvement while those with a prior arrest are defined as "high-risk."[5] We also count both the incidence and number of FIOs and arrests by type of offense for each month during the post-period (June 2015 through December 2019).[6]

IV. Empirical Methodology

To assess the impact of the Boston SYEP on long-term criminal justice outcomes, we compare arrest and FIO records during the 4.5-year observation period following the intervention for youth offered an SYEP placement (the treatment group) to those for youth who were not offered a placement (control group). Because SYEP participation is allocated via lottery, we obtain causal estimates using a simple comparison of means on the outcome of interest. This "Intent to Treat" (ITT) estimate measures the impact of offering the program on the outcome. In many cases, this is the policy-relevant estimate because program administrators want to account for take-up to assess the degree to which SYEP could reduce violence among the pool of applicants, not just the participants. Nonetheless, because not all youth accept the offer, the ITT estimate will understate the effects of the program for those youth who choose to participate. As such, we also provide treatment-on-the-treated (TOT) estimates using a two-stage-least-squares method (see tables D.1 and D.2 in the Online Appendix).

We measure two primary outcomes of interest as specified in our pre-analysis plan along with an additional set of secondary outcomes that were designated as exploratory. Our primary outcome of interest during the post-intervention period is the program's impact on violent crime arrests including both the number of violent crime arrests per youth and whether an individual

[5] This is similar to, but likely more precise than, the "at-risk" subgroup constructed by Kessler et al. (2022) which used observable characteristics to predict at-risk status for youth.
[6] For more details and descriptive tables showing baseline criminal justice outcomes for youth in the treatment and control groups as well as their peers, see Section A in the Online Appendix for more details.

has ever been arrested for a violent crime during the follow-up period. We examine these outcomes in terms of levels as well as relative to other types of crime to account for shifting behavior across types of criminal activity. Note that although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned (Bloom 2006), we also use a regression framework to control for individual and neighborhood characteristics and improve the precision of our estimates using the following equation:

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

where Y_{it} is the criminal justice outcome, $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, $X_{i(t-1)}$ is a set of demographics, baseline criminal justice outcomes, and preexisting neighborhood characteristics, and μ_{it} is a stochastic error term. The set of pre-program demographic covariates is the same as prior studies which includes age, gender, race, language spoken at home, homeless, receiving public assistance, and disabled (Modestino 2019). The baseline criminal justice outcomes correspond to the pre-program criminal justice characteristics that reflect the outcome being estimated. For example, we control for having ever been arrested for a violent crime during the pre-period when estimating the program's impacts on violent crime in the post-period.

We use both OLS as well as alternative nonlinear methods to relax the linear functional form assumption. For example, to analyze differences in the number of violent crime arrests–a count variable–we use a Poisson quasi-maximum likelihood estimator (QMLE); to analyze differences in the likelihood of an arrest, we use a probit estimator. We also track the cumulative number of arrests occurring after random assignment for both treatment and control groups to study the dynamics of the program's impacts over time.

Our secondary outcomes focus on three main analyses: (1) differential program impacts on subgroups, (2) behavioral mechanisms that appear to sustain impacts, and (3) peer co-offending behavior and spillover effects. First, we test whether being randomly selected to participate in the Boston SYEP has a long- term impact on violent crime up to 4.5 years after participating in the program for males, older youth, youth with a prior violent crime arrest, or our alternative "at-risk" group. These subgroups were selected based on pre-existing contextual factors that have been shown to be correlated with criminal activity. For example, males typically offend at a higher rate than females. Similarly, older youth tend to be more likely to commit crimes because

they have less supervision and more opportunity—especially if they can drive and are no longer in school. Finally, youth who have previously been in contact with law enforcement are at higher risk for being arrested in the future (Cottle, Lee, and Heilbrun 2001) .

To explore the behavioral mechanisms of the program, we look at both the monthly cumulative impact over time for criminal justice involvement as well as the differential impact of receiving a second summer of participation. The time path of criminal justice involvement is examined to determine whether participating in the SYEP disrupts some of the activities that youth are involved in during the summer months to the point that it also reduces the frequency they engage in delinquent behavior even after the program has ended, and for how long this impact is sustained. Looking at the impact of receiving a second consecutive summer of participation provides us with the opportunity to study whether receiving an additional "dose" of SYEP yields greater benefits that are sustained longer.

Finally, we test whether the Boston SYEP has an impact on co-offending among treated youth and whether this has any spillover effects on their prior peers. It could be the case that the program reduces the likelihood that individuals in the treatment group are involved in co-offending events during the follow-up period (either through arrests or FIOs) which might provide some insight into how programs impacts are achieved (for example, SYEP participants are perhaps less subject to negative peer interactions). Conversely, it could be the case that the positive behaviors and/or the prosocial ties formed by SYEP treated youth are spread to their prior co-offending peers to ultimately reduce the incidence or frequency of violent crime among that group as well.

V. Results

Table 1 provides descriptive statistics for the preexisting characteristics of SYEP lottery applicants which largely reflect those of the Boston youth population. On average, approximately 88 percent of applicants were in school at the time they applied, with a mean age just shy of 16 years. A slightly higher percentage of applicants were female, and just over 50 percent were African American. Although over 95 percent indicated that their preferred language was English, roughly 7 percent identified as having limited English ability. In addition, nearly 7 percent reported being homeless and upwards of 18 percent acknowledged receiving cash public assistance of some form. Less than 5 percent listed themselves as having a disability.

Administrative wage record data confirm that only one-quarter of youth in the control group had found a job on their own, working fewer hours per week on average than those in the treatment group. Thus, the Boston SYEP provided a meaningful intervention in terms of the likelihood, intensity, and type of employment obtained during the summer of 2015.

[Insert Table 1 Here]

Based on these observable characteristics, the youth selected by lottery appear to be almost identical to those not selected, confirming that the lottery was indeed random. In Table 1, the only statistically significant difference is the share of Asian youth being slightly higher in the treatment group (7 percent) versus the control group (5 percent), yet this variation would be expected by random chance when testing 16 different characteristics. Baseline criminal justice activity in the 17 months leading up to the program is also balanced across the treatment and control groups with roughly 3 percent of youth ever having been arrested, and only 5 percent ever having been reported in the FIO data prior to the program. Youth also had similar rates and numbers of arrests across all types of crime, seriousness of the offenses (for example, misdemeanor versus felony), and co-offending behavior (see Table A.3 in the Online Appendix).

A. SYEP Long-Term Impacts on Youth Criminal Justice Outcomes

Following our pre-analysis plan, we use both juvenile and adult arrest records to compare outcomes during the 4.5-year observation period following the intervention for youth offered an SYEP placement (the treatment group) to those for youth who were not offered a placement (control group). Table 2 reports the ITT estimates of the program's impact over the entire 4.5 year follow-up period by type of offense and youth subgroup. We find that being offered a job through the Boston SYEP leads to lower criminal justice involvement over the long-term, but only among subgroups who are more likely to come into contact with police. This includes males who experience an 18 percent decrease in the likelihood of ever being arrested post-program (a decline of -0.027 percentage points from a control group mean of 0.1499) and a 23 percent decrease in the *number* of arrests (decrease of -0.075 from a control group mean of 0.33 which represents 33 arrests per 1,000 youth). This relative improvement primarily reflects a large drop in both the incidence and number of less serious crimes (misdemeanors), although older youth also experience a reduction in the likelihood of ever committing a felony during the 4.5 year

post-period. These results are similar in both magnitude and significance to those of Kessler et al. (2022), confirming that much, although not all, of the program's impacts come from youth who were at the age of majority when participating in the program (age 18 and older in Massachusetts). Similar to Kessler at al. (2022), we find no statistically significant reductions in criminal justice activity for our alternative "at-risk" group defined as having either a prior arrest or FIO recorded.

[Insert Table 2 Here]

With our more refined dataset we are also able to extend the prior literature to better understand the program's mechanisms by exploring the long-term impacts by type of crime. Programmatic interventions tend to have differential impacts across types of crime depending on which underlying causes they address (Deming 2011; Jacob and Lefgren 2003; Kling, Ludwig, and Katz 2005). For example, violent crime involving conflicts between people stems from a combination of antisocial behavior (Dodge and Schwartz 1997) and thinking "fast" rather than slow (Kahneman 2011), particularly among youth where impulse control is not as developed (Ludwig and Shah 2014). In contrast, property crimes that do not involve violence (for example, burglary of a home or larceny of a store) tend to reflect material needs, immediate wants, or thrill-seeking (Wright and Decker 1994; Clarke and Cornish 1986).

In terms of the number of arrests, we find that the Boston SYEP's largest long-term impacts are the reduction in youth violence, suggesting that the program's short-term behavioral effects observed in prior studies are in fact sustained over time, but only for specific subgroups. Among males there is a 28 percent decrease in violent crime arrests over the entire 4.5 years, roughly on par with the 35 percent reduction in violent crime arraignments that was observed at only 18 months post-program (Modestino 2019). Males also exhibit a 47 percent reduction in property crimes, but there is no significant decrease in drug crimes for any of the subgroups that we study. It is note-worthy that the reduction in violence is driven by older youth and those with arrest records prior to the program. Both 18-24 year olds and high-risk youth (for example, with any prior arrest) experience even larger reductions in violent crime arrests of 53 percent and 79 percent respectively during the full follow-up period. This runs counter to the conventional wisdom that SYEPs are primarily a preventive measure that reduces violence among the general population of youth (Heller 2014). In fact, prior research in Boston and elsewhere shows that

citywide violence is driven by a small number of high-risk youth who are generally well known to the authorities and often caught up in high risk social networks (for example, gangs) (Braga and Cook 2023; Papachristos et al. 2015). Thus, SYEPs appear to offer a more direct route to producing safer communities by providing jobs to high-risk youth who can often be difficult to reach and engage in programming.

B. Dosage and timing of program impacts

We next examine whether it is sufficient for youth to receive only one summer of participation or if a second consecutive summer helps to sustain the program's impacts. In Table 3, we limit the sample to all youth in the treatment group—those who initially won the SYEP lottery in 2015—and include an indicator for whether an individual also won the lottery in 2016 to look at the marginal effect of a second summer of treatment on long-term outcomes. The reduction in the number of arrests for the full sample of youth in the treatment group who receive a second summer is larger in magnitude than those experienced by youth receiving just one summer of participation. In fact, there is now also a large and significant reduction in both the incidence and number of drug arrests. These impacts largely persist when we further restrict our analysis to the sub-sample of youth in the treatment group who chose to apply to the program in 2016, suggesting that these findings do not simply reflect greater intrinsic motivation on the part of those who both *applied for and received* a second consecutive SYEP dose.

While these results are suggestive, we acknowledge that we cannot attribute an entirely causal interpretation since repeat participation is conditional on having applied for a second time. However, SYEP program data reveal that repeat applicants are not a particularly advantaged group, at least on the observable characteristics. Among the roughly 24 percent of eligible lottery winners from the first summer who choose to apply for a second time, repeat applicants are on average more likely to be male and living in a household that receives public assistance—characteristics that are often positively, rather than negatively, correlated with criminal justice involvement. Still, we caution against taking these results as purely causal evidence that a second summer of participation produces program outcomes that are stronger or more persistent.

[Insert Table 3 Here]

One of the potential rationales through which SYEPs may reduce crime is by "disrupting routine activities" that provide opportunities for likely offenders to come in contact with suitable targets in the absence of supervision or guardianship (Cohen and Felson 1979). Rather than completely "incapacitating" youth who are likely to engage in criminal activity by occupying them with a job for part of the day, it is likely that SYEPs can disrupt the frequency with which youth have the opportunity to do so. To test this, Figure 1 graphs the cumulative ITT estimate over time for males with each point adding an additional month of data to the prior effect (treatment-control) through 55 months, nearly 4.5 years post-program. The downward slope of program impacts in Figure 1 across various types of crime demonstrates that most of the reduction in criminal activity accrues *after* the program ends (month two), consistent with prior studies (Heller 2014; Kesler et al. 2022; Modestino 2019). For misdemeanors and property arrests, the reduction in crime steadily accumulates over time, with a sharp drop at months 15 and 27—possibly leveling off during the school year before an additional dose of SYEP—after which the long-term impacts become statistically significant for the remainder of the post-program observation period. In contrast, after an initial decline during and immediately after the program, the time path of felonies and violent crime arrests is much less steep with the cumulative reduction in violent crime arrests becoming statistically significant at 48 months (4 years) post randomization.

[Insert Figure 1 Here]

Figure 2 compares the trajectory of arrests to that of more informal FIO contacts with police to better understand the interaction between changes in youth behavior and policing. Panel A shows that for males, while arrests start to decline immediately, FIOs initially increase, and do not start to decline until 12 months post-randomization when there is a consistent downward trend in total arrests. BPD may have previously viewed SYEP treated youth as initially likely to engage in suspicious activities but these perceptions seem to change over time in the absence of increased criminal activity as indicated by the decline in arrest outcomes. In contrast, for high-risk youth with a prior arrest, the number of FIOs levels off but does not decline proportionally to the number of arrests, even after there is a consistent decline starting in month 24, perhaps due to these youth still being considered prime suspects by BPD. As a result, when we combine both

the number of arrests and FIO reports, we find no significant long-term impact of the Boston SYEP on overall criminal justice involvement, even among high-risk youth.

[Insert Figure 2 Here]

C. Co-offending and peer spillovers

Finally, we explore whether the summer jobs program affected co-offending behavior among the participants as well as criminal activity among their co-offending "peers" that they interacted with prior to the program. Some observers suggest deviant peers socialize the acceptance of delinquent values and behaviors by otherwise non-offending youth (Akers 1998; Matsueda 1988). Others suggest that delinquency can naturally emerge through implicit and explicit encouragement of antisocial acts when youth socialize with each other in unstructured and unsupervised settings (Osgood et al. 1996). Thus, it could be the case that the program reduces the likelihood that individuals in the treatment group are involved in co-offending events during the follow-up period because they are less subject to negative peer interactions.

Table 4 estimates the differential impacts of the program on the incidence of co-offending for the treatment versus the control group both in terms of being arrested as well as being included in an FIO report with at least one other person. We find similar patterns as before, where participating in the program decreases arrests with co-offenders by 36 percent for males and by 52 percent for high-risk youth. Moreover, there is a small 3 percent reduction in being included with others in an FIO report post-program for high-risk youth, suggesting that those with a prior arrest are potentially disrupting social ties with their prior co-offending peers. This rationale is supported by focus groups with SYEP participants who had been court-involved prior to the program. When asked whether the program reduced the opportunity to engage in criminal activity, respondents acknowledged that having a summer job prevented them from interacting with gang-related youth during the week because they had to get up early the next day for work (Modestino 2019).

[Insert Table 4 Here]

So far, we have documented that the Boston SYEP reduces the number and incidence of certain types of crime for various subgroups, including co-offending groups, especially among

high-risk youth. Do these better behaviors also influence the prior co-offending "peers" of the treatment group? It is possible that good work habits and healthy social interactions developed by high-risk youth in the treatment group "spill over" onto their prior co-offending partners. Alternatively, if SYEP treated youth have broken these social ties with their former co-offending peers, then it could be the case that these peers are not influenced by the positive changes in behavior stemming from the program. Or worse—these former co-offending peers, driven by a need for belonging, might develop closer ties to other gang-related youth.[7]

Table 5 estimates the post-program impacts for peers of youth in the treatment group. We find no significant impacts of the program on the peers of the treatment group. If anything, the incidence of ever being arrested for a violent crime may even increase, although our estimates are too imprecise to draw any substantive conclusions.

[Insert Table 5 Here]

Figure 3 shows the trajectory of both violent and property crime for high-risk youth and their co-offending peers. Violent crime arrests drop significantly among high-risk youth immediately after the program ends and continue to decline throughout the 55-month post-program period. There is also an initial drop in violent crime among peers that then reverses, only to drop sharply between months 12 and 15, which would align with the timing of the next summer jobs cohort. In contrast, property crime arrests show no significant decline for high-risk youth and follows a similar path for peers.

[Insert Figure 3 Here]

D. Robustness to Alternative Specifications

Several additional supplementary analyses were performed to confirm the robustness of our findings. This includes an analysis that adds each set of controls sequentially as well as a

[7]The available evidence for community violence interventions (such as street outreach workers or "violence interrupters") that seek to reduce violence by youth gangs and other criminally active groups is mixed with evaluations suggesting positive, negative, and null effects on youth offending behaviors (for example, see Hureau et al. 2023). Programs associated with crime reduction impacts tend to promote educational and vocational opportunities that break youth social ties within their co-offending networks while programs showing backfire effects (that is, increased offending) tend to enhance social cohesion within gangs by promoting group-based programming that strengthens social connections amongst high-risk youth.

Gelbach decomposition (Gelbach 2016) to explore the amount of the program impact that can be explained by demographic characteristics, neighborhood characteristics, and previous criminal justice history (see section B of the Online Appendix). The decomposition reveals that very little of the covariates explain any of the FIO variation between the treatment and control groups. In contrast, the baseline criminal justice history has some explanatory power but also does not explain much of the variation.

Our conclusions are substantively similar when using non-linear specifications to estimate the program's impacts. First, we use a Poisson quasi-maximum likelihood estimator (QMLE) where the consistency of this estimator only requires the correct specification of the conditional mean, not the entire distribution. We also use Huber-White robust standard errors to allow for over-dispersion, relaxing the Poisson distributional constraint that the mean equals the variance. To analyze differences in the likelihood of being arrested, a 0/1 dependent variable, we use a probit estimator. See Section C of the Online Appendix for these results.

We also present alternative estimates that are often stronger when we account for noncompliers among both the treatment and control groups. Among the treatment group, these are youth who won the lottery but did not take up the job offer. Among the control group, these are youth who lost the lottery in 2015 but re-applied and won the lottery during the subsequent post-program follow-up period. See Section D of the Online Appendix for these results.

Finally, the limitations of this study point to important directions for future research. First, although the results from our analyses can be interpreted as the causal impacts of the Boston SYEP, additional replication across other cities is needed to ensure that they are fully generalizable. Second, although we have better data than prior studies, our sample size is insufficient for measuring important outcomes like co-offending and receiving a second summer of participation across subgroups. Third, there is a higher degree of measurement error when using arrests versus other types of criminal justice data (for example, arraignments) due to names and date of birth being more likely to contain errors or omissions. Finally, although we collected qualitative data during the summer of 2015, a longitudinal follow-up study would provide greater insight into how the program's longer-term impacts are being sustained over time.

VI. Conclusion

This study used an embedded randomized controlled trial to provide causal estimates of the long-term impacts of summer job programs on criminal justice outcomes among the 4,219 youth who applied to the Boston SYEP during the summer of 2015. Using a novel dataset that measures both juvenile and adult criminal justice involvement, including informal observations and interactions with police, we follow youth through the end of 2019 (4.5 years post-randomization). We find that participating in the Boston SYEP significantly lowers both the likelihood of ever being arrested and the number of arrests for certain subgroups of youth (for example, males, 18-24 year olds, and those previously arrested) and certain types of crime (for example, violent, property, and misdemeanor crimes). Across all subgroups, the program has a larger impact on the number of arrests, rather than the incidence. For example, among males, we find the incidence of ever being arrested falls by 18 percent and the total number of arrests drops by 23 percent. This finding is consistent with other youth interventions that operate by increasing the presence and influence of protective factors (for example, individual, family, peer, community) to reduce, but not eliminate, delinquent behaviors (National Institute of Justice 2024).

These SYEP program impacts appear to accumulate over time, particularly between months 12 and 36—literally putting youth on a better path. After three years, the differential impact does not grow by much but also does not shrink over the next 18 months, suggesting that the improvements are indeed stable. Given that the decrease in criminal activity was not limited to the duration of the program, it appears that the program shifts the behavior of participants and these behavioral changes persist even after the program has ended. Thus, it is likely that SYEPs do not simply occupy (or incapacitate) youth who are likely to engage in criminal activity but rather disrupt the frequency with which they do so.

Examining which groups of youth are most likely to be affected can provide some clues as to how SYEPs achieve these long-lasting behavioral changes. In particular, the number of violent crime arrests among high-risk youth who had been arrested prior to the program dropped by 79 percent, starting with a sharp decrease immediately after the program, which continued to accumulate through 4.5 years post-randomization with no sign of leveling off. This high-risk sub-group has characteristics that reflect those of hard-to-reach “opportunity youth”—those who are disconnected from both school and work—with roughly 40 percent aged 18 years or older

and greater proportions experiencing homelessness and receiving public assistance than the overall youth population. Summer jobs programs might provide a useful vehicle for reconnecting these hard-to-reach youth with more productive experiences that offer skill building, adult mentors, and positive peer interactions during their active offending years.[8]

We also find that the Boston SYEP exhibits heterogenous long-term impacts by types of crime, potentially revealing the underlying causes that the program addresses. While some groups of youth also experience a decrease in property and drug crimes, the most consistent long-term impact is the reduction in youth violence. This suggests that SYEPs have a sustained impact on criminal activity that is driven by interpersonal interactions, such as those that have found to be a primary source of gang-related shootings (Papachristos 2009; Papachristos et al. 2013). This hypothesis is supported by pre- and post-program survey data collected during the summer of 2015 that linked the short-term reductions in violent crime with self-reported improvements in managing emotions, resolving conflicts with a peer, and asking adults for help, suggesting that summer jobs primarily reduce violent crime through improvements in social-emotional skills (Modestino 2019).

More importantly, our results speak directly to *how* SYEPs change youth behavior and what can be done to sustain program impacts in the longer-term. Perhaps most insightful are the findings related to co-offending and peers, given that youth tend to commit delinquent acts as they live their lives–primarily in groups (Zimring 1981). Indeed, we find summer job programs reduce co-offending among both males and high-risk youth by roughly 30 percent. In addition, youth winning the summer jobs lottery for a second consecutive summer experience larger (up to 50%) and more consistent reductions across most types of arrests, including felonies. City leaders focused on youth violence prevention might want to invest in additional outreach and target a second summer of participation at high-risk youth who can be easily identified by law enforcement and are often involved in gang-related activities. However, positive peer interactions matter so targeting the program *exclusively* to those with the greatest need could have the unintended consequence of reducing the program's effectiveness. It is likely that a mix of youth with different strengths and weaknesses is the most effective approach to capture the

[8] Moreover, prior research shows that SYEPs also instill good work habits that increase school attendance and boost course GPAs, leading to a 22 percent decrease in school drop out in the year following the program and a 7 percent increase in high school graduation (Modestino and Paulsen 2022).

benefits of peers while also limiting any stigma that might be associated with the program. Similarly, additional targeting of high-risk youth would also require expanding employer partnerships to provide meaningful jobs with proper supervision at trauma informed workplaces.

Given the magnitude and trajectory of improvements so far, with SYEP participants just at the start of their adult lives, additional SYEP investments are likely to pay off in terms of lower victimization, incarceration, and lost productivity. It also appears that early intervention is perhaps even more important than previously thought, especially since there are very few employers outside of SYEPs that are willing to hire 14 and 15-year olds who have often aged out of camps and other structured summertime activities. Indeed, comparing the trajectories of arrest and FIO records suggests that police continue to observe high-risk youth until there is a consistent decrease in criminal activity as evidenced by fewer arrests, suggesting that SYEPs can be thought of as a protective intervention. Finding ways to link SYEPs to other existing year-round work-based learning such as co-ops and internships could be a viable way to help high-risk youth continue to sustain beneficial post-program behavioral changes, particularly for groups of youth still experiencing COVID-related learning and developmental deficits.

REFERENCES

Akers, Ronald L. 1998. *Social Learning and Social Structure: A General Theory of Crime and Deviance.* Boston: Northeastern University Press.

American Youth Policy Forum. 2021. "Saving Summer Jobs: How Can Summer Youth Employment Programs Improve Youth Outcomes during COVID-19?" Webinar, April 2022. https://aypf.org/event/saving-summer-jobs-how-can-summer-youth-employment-programs-improve-youth-outcomes-during-covid-19/

Analyze Boston. n.d. City of Boston Open Data Portal. https://data.boston.gov/.

Biden, Joseph R. 2021. "Remarks by President Biden and Attorney General Garland on Gun Crime Prevention Strategy." The White House, June 23. https://www.whitehouse.gov/ (accessed January 2023).

Bloom, Dan. 2006. "Employment-Focused Programs for Ex-Prisoners: What Have We Learned, What Are We Learning, and Where Should We Go from Here?" MDRC, July.

Boston Police Department. Field Interrogation and Observation (FIO) Reports Dataset (n.d.); https://data.boston.gov/dataset/boston-police-department-fio (accessed June 12, 2025).

Braga, Anthony A., and Philip J. Cook. 2023. *Policing Gun Violence: Strategic Reforms for Controlling Our Most Pressing Crime Problem.* New York: Oxford University Press.

Clarke, Ronald V., and Derek B. Cornish. 1986. *The Reasoning Criminal: Rational Choice Perspectives on Offending.* New York: Springer-Verlag.

Cohen, Lawrence E., and Marcus Felson. 1979. "Social Change and Crime Rate Trends: A Routine Activity Approach." *American Sociological Review*, 4: 588–608.

Cottle, Cindy C., R. J. Lee, and Kirk Heilbrun. 2001. "The Prediction of Criminal Recidivism in Juveniles: A Meta-Analysis." *Criminal Justice and Behavior,* 28: 367–394.

Deming, David J. 2011. "Better Schools, Less Crime?" *Quarterly Journal of Economics,* 126 (4): 2063–2115.

Dodge, Kenneth A., and David Schwartz. 1997. "Social Information Processing Mechanisms in Aggressive Behavior." In *The Development and Treatment of Childhood Aggression*, 171–187. Mahwah, NJ: Erlbaum.

Fagan, Jeffrey, Anthony A. Braga, Rod K. Brunson, and April Pattavina. 2016. "Stops and Stares: Street Stops, Race, and Surveillance in the New Policing." *Fordham Urban Law*

Journal, 42 (5): 621 – 696.

Gelbach, Jonah B. 2016. "When Do Covariates Matter?" *Journal of Labor Economics*, 34: 509–543.

Grawert, Ames, and Noah Kim. 2023. *Myths and Realities: Understanding Recent Trends in Violent Crime*. Brennan Center for Justice at NYU Law, May 9. https://www.brennancenter.org/our-work/research-reports/myths-and-realities-understanding-recent-trends-violent-crime.

Haynie, Dana L. 2001. "Delinquent Peers Revisited: Does Network Structure Matter?" *American Journal of Sociology*, 106: 1013–1057.

Haynie, Dana L. 2002. "Friendship Networks and Delinquency: The Relative Nature of Peer Delinquency." *Journal of Quantitative Criminology*, 18: 99–134.

Heller, Sara B. 2014. "Summer Jobs Reduce Violence Among Disadvantaged Youth." *Science*, 346: 1219–1223.

Heller, Sara B. 2022. "When Scale and Replication Work: Learning from Summer Youth Employment Experiments." *Journal of Public Economics*, 209: 104617.

Hureau, David M., Anthony A. Braga, Tracey Lloyd, and Christopher Winship. 2023. "Streetwork at the Crossroads: An Evaluation of a Street Gang Outreach Intervention and Holistic Appraisal of the Research Evidence." *Criminology*, 61: 758 – 794.

Jacob, Brian A., and Lars Lefgren. 2003. "Are Idle Hands the Devil's Workshop? Incapacitation, Concentration, and Juvenile Crime." *American Economic Review*, 93 (5): 1560–1577.

Kahneman, Daniel. 2011. *Thinking, Fast and Slow*. New York: Farrar, Straus, and Giroux.

Kessler, Judd B., Sarena Tahamont, Alexander Gelber, and Adam Isen. 2022. "The Effects of Youth Employment on Crime: Evidence from New York City Lotteries." *Journal of Policy Analysis and Management*, 41: 710–730.

Kessler, Judd B., and Sara B. Heller. 2017. "How to Allocate Slots: The Market Design of Summer Youth Employment Programs." Preprint. https://users.nber.org/~kesslerj/papers/HellerKessler_SYEP_2017.pdf.

Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz. 2005. "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment." *Quarterly Journal of Economics*, 120 (1): 87–130.

Li, Yiping and Kalila Jackson-Spieker. 2022. "The Promises of Summer Youth Employment

Programs: Lessons from Randomized Evaluations." Evidence Review, Abdul Latif Jameel Poverty Action Lab, September 22. https://www.povertyactionlab.org/sites/default/files/publication/SYEP_Evidence_Review-9.22.22.pdf

Ludwig, Jens, and Priscilla Shah. 2014. "Thinking Fast and Slow: The Long-Run Effects of Cognitive Behavioral Therapy on Violent Crime." NBER Working Paper No. 21178.

Ludwig, Jens, and Katherine Schnepel. 2024. "Does Nothing Stop a Bullet Like a Job? The Effects of Income on Crime." University of Chicago, Becker Friedman Institute for Economics, Working Paper No. 2024–42.

Matsueda, Ross L. 1988. "The Current State of Differential Association Theory." *Crime and Delinquency*, 34: 277–306.

McGloin, Jean Marie, and Kyle J. Thomas. 2019. "Peer Influence and Delinquency." *Annual Review of Criminology*, 2: 241–264.

Modestino, Alicia S. 2019. "How Do Summer Youth Employment Programs Improve Criminal Justice Outcomes, and for Whom?" *Journal of Policy Analysis and Management*, 38: 600–628.

Modestino, Alicia S., and Anthony A. Braga. 2022. "The Long-Term Impact of Summer Youth Employment Programs on Outcomes." Open Science Framework (OSF) Registry, April 25. https://doi.org/10.17605/OSF.IO/C7GY3.

Modestino, Alicia S., and Rick Paulsen. 2022. "School's Out: How Summer Youth Employment Programs Impact Academic Outcomes." *Education Finance and Policy*, 18: 97–126.

Monahan, Kathryn C., Laurence Steinberg, and Alex R. Piquero. 2015. "Juvenile Justice Policy and Practice: A Developmental Perspective." *Crime and Justice*, 44: 577–619.

Mortimer, Jeylan T. 2010. "The Benefits and Risks of Adolescent Employment." *Prevention Researcher*, 17: 8–11.

National Institute of Justice. 2024. "Five Things about Youth and Delinquency." https://www.ojp.gov/pdffiles1/nij/309129.pdf.

Osgood, D. Wayne, Jeff M. Wilson, Patrick M. O'Malley, Jerald G. Bachman, and Lloyd D. Johnston. 1996. "Routine Activities and Individual Deviant Behavior." *American Sociological Review*, 61: 635–655.

Papachristos, Andrew V. 2009. "Murder by Structure: Dominance Relations and the Social Structure of Gang Homicide." *American Journal of Sociology*, 115: 74 – 128.

Papachristos, Andrew V., Anthony A. Braga, David M. Hureau, and Louis Grossman. 2015. "The Company You Keep? The Spillover Effects of Gang Membership on Individual Gunshot Victimization in a Co-Offending Network." *Criminology*, 53: 624–649.

Papachristos, Andrew V., David M. Hureau, and Anthony A. Braga. 2013. "The Corner and the Crew: The Influence of Geography and Social Networks on Gang Violence." *American Sociological Review*, 78: 417 – 447.

Rochford, Patrick, Julia Bauer, and Michael Wallace. 2024. "What to Know About the 2024 End-of-Year ARPA Obligation Deadline." National League of Cities, July 18. https://www.nlc.org/article/2024/07/18/what-to-know-about-the-2024-end-of-year-arpa-obligation-deadline/#:~:text=The%20passage%20of%20the%20American,place%20orders%20and%20negotiate%20contracts.

Sampson, Robert J., and John H. Laub. 2003. "Desistance from Crime over the Life Course." In *Handbook of the Life Course*, edited by Jeylan Mortimer and Michael J. Shanahan, 295–309. New York: Kluwer Academic/Plenum.

Scott, Robert. 2021. "Opening Doors for Youth Act: Investing in Our Nation's At-Risk and Opportunity Youth," July.

The Boston Globe. 2020. "In the Year of COVID-19, Boston Homicides and Shootings Spiked. Why?" *The Boston Globe*, December.

Warr, Mark. 2002. *Companions in Crime: The Social Aspects of Criminal Conduct.* Cambridge: Cambridge University Press.

Wright, Richard, and Scott Decker. 1994. *Burglars on the Job: Streetlife and Residential Break-ins.* Boston: Northeastern University Press.

Zimring, Franklin E. 1981. "Kids, Groups, and Crime: Some Implications of a Well-Known Secret." *Journal of Criminal Law and Criminology*, 72: 867–885.

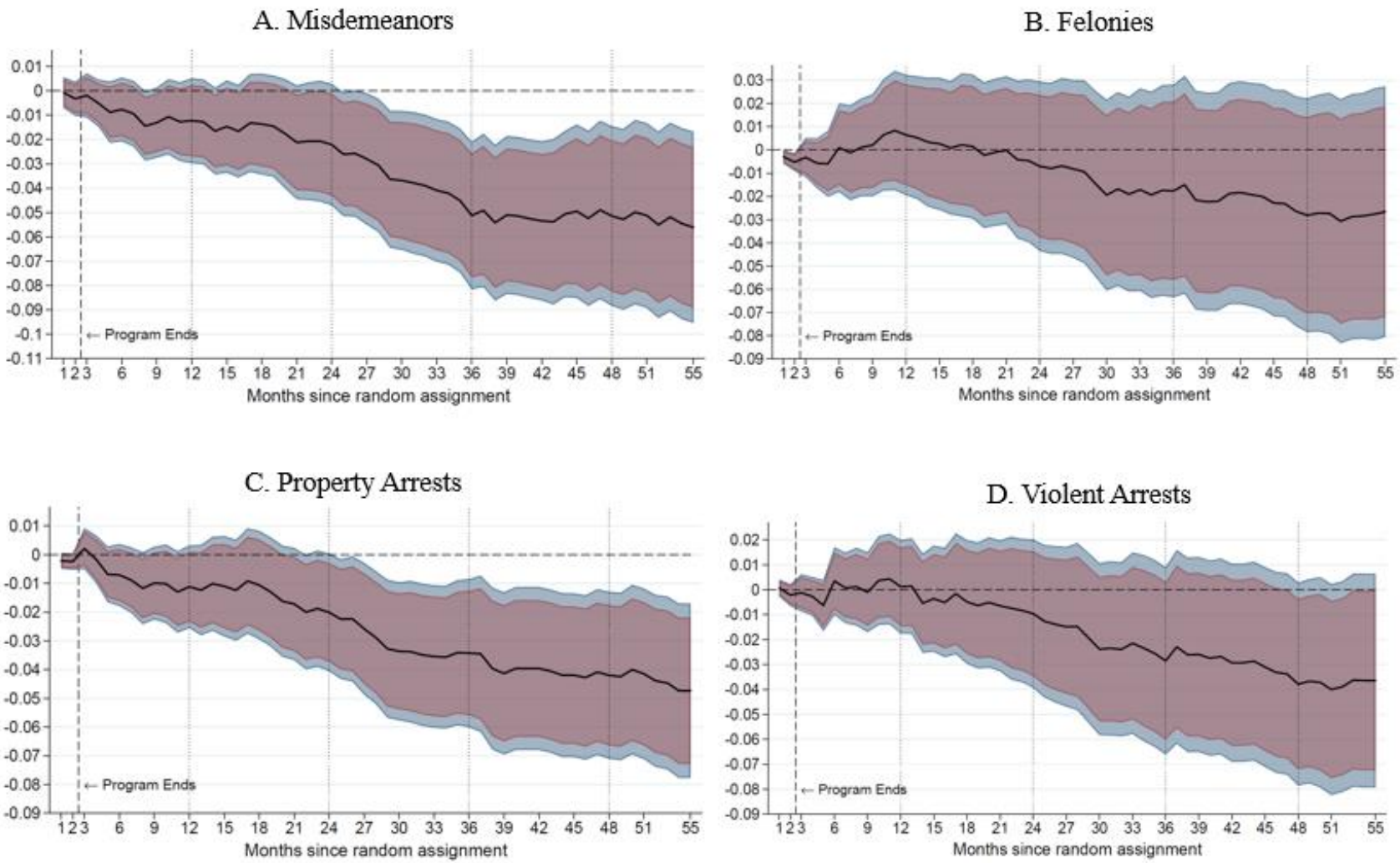


FIGURE 1.CUMULATIVE TIME PATH OF PROGRAM IMPACTS BY MONTH FOR MALES.

Notes: This figure shows the cumulative treatment effect of the decline in arrests from random assignment in month zero through 55 months. The vertical dotted line shows the end of the program. Estimates include all neighborhood, demographic, and baseline covariates. Confidence intervals using heteroskedasticity-robust standard errors are shown for both 90% (purple) and 95 percent (blue).

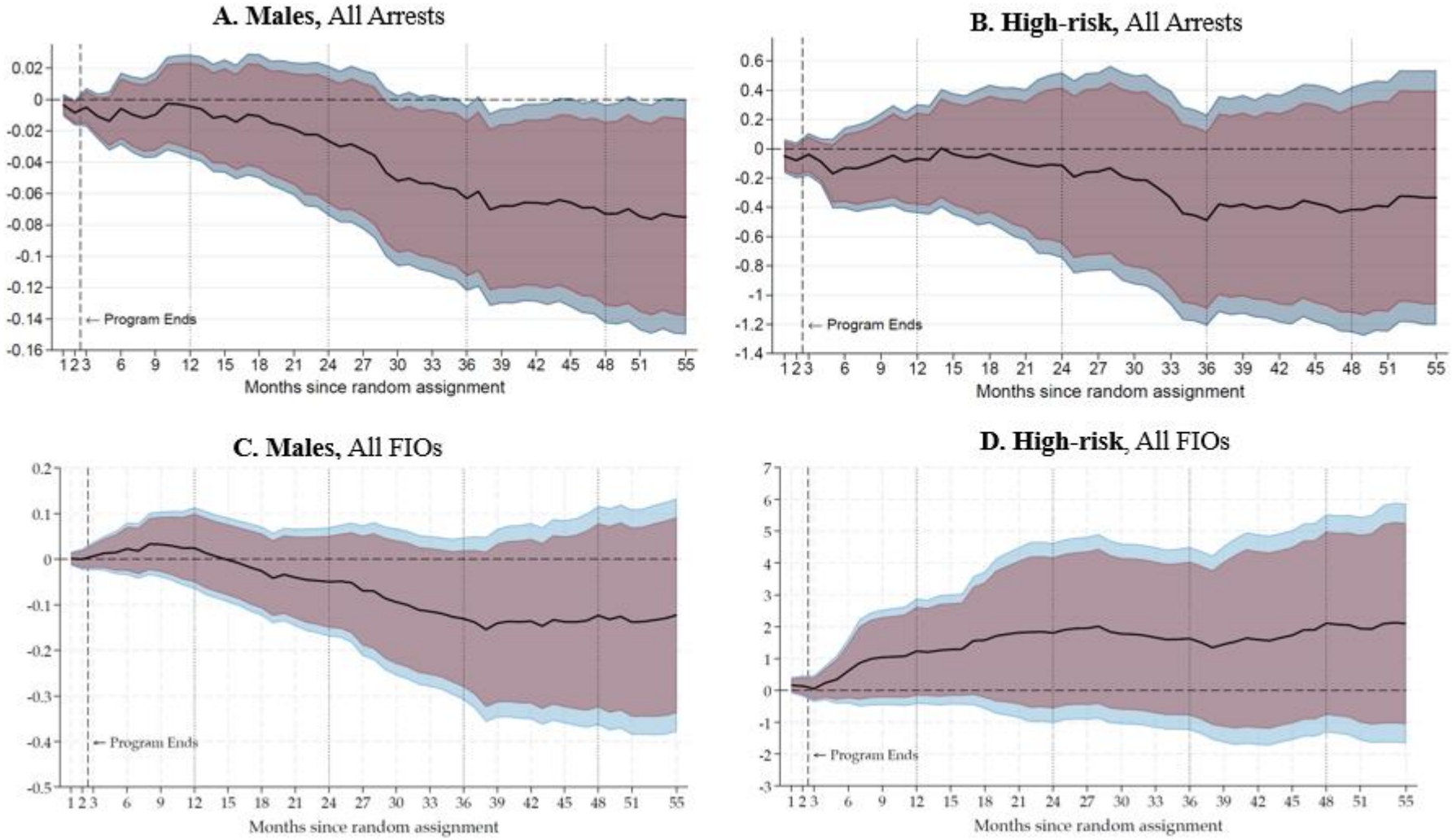


FIGURE 2. CUMULATIVE TIME PATH OF PROGRAM IMPACTS BY MONTH FOR ARRESTS VERSUS FIOS.

Notes: This figure shows the cumulative treatment effect from random assignment in month zero through 55 months, comparing arrests and FIOs for all males and high-risk youth with a prior criminal justice history. The vertical dotted line shows the end of the program. Estimates include all neighborhood, demographic, and baseline covariates. Confidence intervals using heteroskedasticity-robust standard errors are shown for both 90% (purple) and 95 percent (blue).

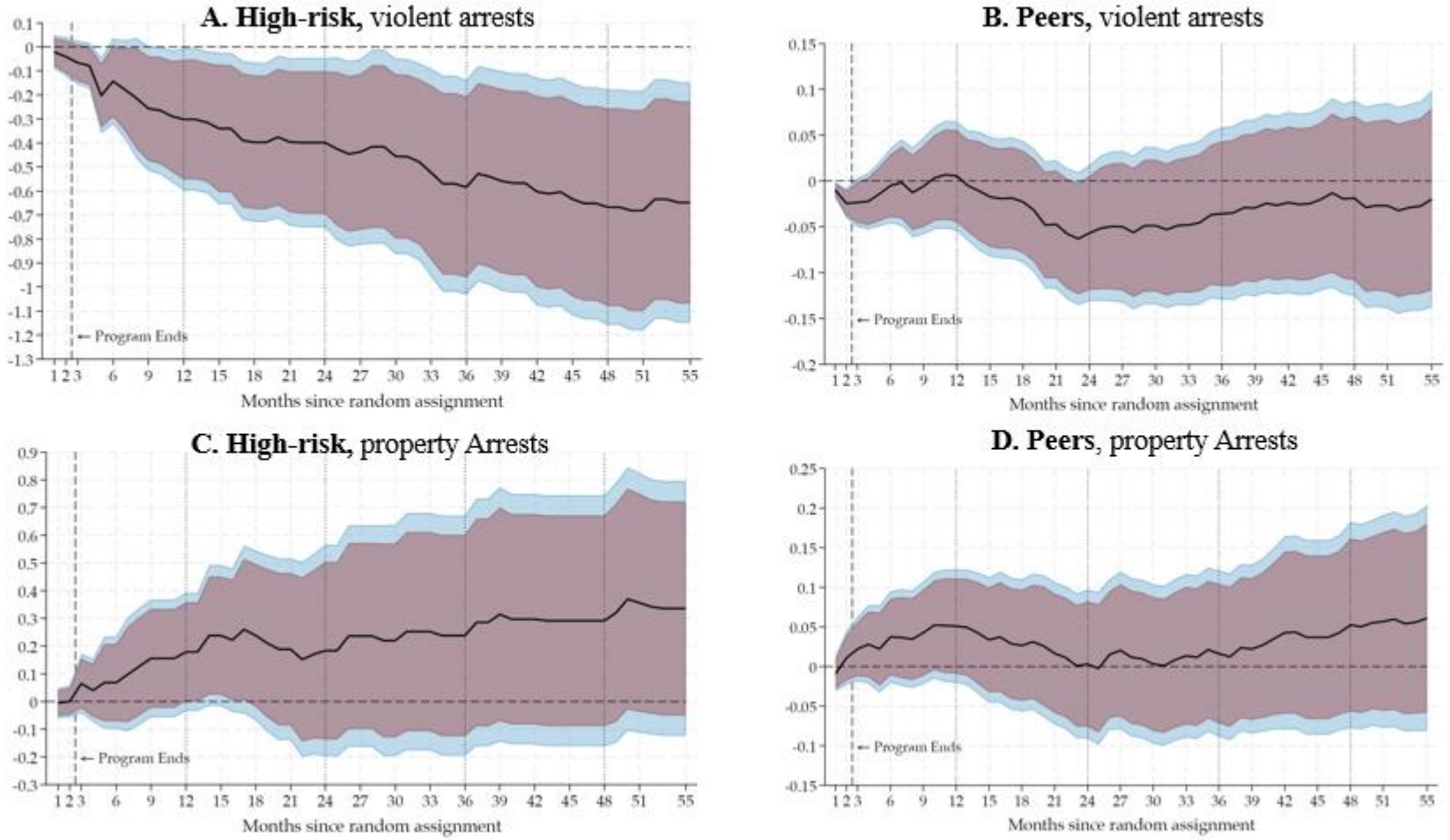


FIGURE 3. CUMULATIVE TIME PATH OF PROGRAM IMPACTS BY MONTH FOR HIGH-RISK YOUTH VERSUS PEERS.

Notes: This figure shows the cumulative treatment effect in number of arrests from high-risk youth with a prior criminal justice history and their peers from random assignment in month zero through 55 months. The vertical dotted line shows the end of the program. Estimates include all neighborhood, demographic, and baseline covariates. Confidence intervals using heteroskedasticity-robust standard errors are shown for both 90% (purple) and 95 percent (blue).

TABLE 1. BASELINE DEMOGRAPHIC CHARACTERISTICS OF SYEP APPLICANTS BY LOTTERY OUTCOME

	(1)		(2)		(3)	
	Treatment		**Control**		**Treatment – Control**	
	Mean	SD	Mean	SD	Diff.	*p*-value
Age	15.92	(1.99)	15.88	(1.84)	0.03	(0.63)
Percent aged 18-24 years	0.22	(0.41)	0.20	(0.40)	0.02	(0.15)
Percent male	0.47	(0.50)	0.46	(0.50)	0.01	(0.59)
Race:						
Percent Black	0.51	(0.50)	0.53	(0.50)	-0.02	(0.18)
Percent White	0.10	(0.30)	0.09	(0.28)	0.01	(0.20)
Percent Asian	0.07	(0.25)	0.05	(0.22)	0.02*	(0.07)
Percent Other	0.33	(0.47)	0.33	(0.47)	-0.00	(0.76)
Preferred language is English	0.95	(0.22)	0.95	(0.21)	-0.00	(0.74)
Percent with disability	0.04	(0.20)	0.03	(0.18)	0.01	(0.31)
Percent experiencing homelessness	0.07	(0.25)	0.07	(0.25)	-0.00	(0.85)
Percent receiving public assistance	0.19	(0.39)	0.17	(0.38)	0.01	(0.34)
Percent still in school	0.88	(0.33)	0.88	(0.32)	-0.01	(0.56)
F-test (Prob>F)					1.09 (0.37)	
Number of youth	1,186		3,033		4,219	

Notes: SYEP, summer youth employment program; SD, standard deviation; Diff, difference between the treatment and control groups; *p*-value is from a regression of the applicant characteristics on the treatment dummy; F-test represents the joint significance across all baseline characteristics. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source: Authors' calculations.

TABLE 2. CUMULATIVE ITT PROGRAM EFFECT BY OFFENSE TYPE AND SUB-GROUP

	(1)		(2)		(3)		(4)		(5)	
	All Youth		Male		Age 18-24 years		At-risk: any prior		High-risk: prior arrest	
	CM	ITT	CM	ITT	CM	ITT	CM	ITT	CM	ITT
A: Incidence: Percent of youth arrested										
Any arrest	[0.097]	-0.001	[0.150]	-0.027*	[0.127]	-0.018	[0.533]	-0.044	[0.659]	-0.060
		(0.009)		(0.015)		(0.022)		(0.071)		(0.123)
Drug arrest	[0.010]	-0.001	[0.017]	-0.003	[0.012]	0.013	[0.095]	-0.017	[0.121]	-0.076
		(0.003)		(0.006)		(0.010)		(0.034)		(0.051)
Property arrest	[0.036]	-0.002	[0.061]	-0.018*	[0.035]	-0.010	[0.201]	0.079	[0.264]	0.162
		(0.006)		(0.010)		(0.013)		(0.058)		(0.099)
Violent arrest	[0.059]	-0.003	[0.082]	-0.013	[0.082]	-0.039**	[0.312]	-0.055	[0.429]	-0.168
		(0.008)		(0.012)		(0.018)		(0.068)		(0.125)
Felony arrest	[0.064]	-0.000	[0.100]	-0.012	[0.090]	-0.034*	[0.377]	-0.028	[0.473]	-0.061
		(0.008)		(0.013)		(0.018)		(0.067)		(0.126)
Misdemeanor arrest	[0.055]	-0.006	[0.090]	-0.033***	[0.073]	-0.008	[0.342]	-0.080	[0.462]	-0.085
		(0.007)		(0.012)		(0.019)		(0.062)		(0.109)
B: Intensity: Number of arrests per youth										
All arrests	[0.199]	-0.017	[0.331]	-0.075**	[0.249]	-0.077	[1.377]	0.069	[2.066]	-0.334
		(0.023)		(0.038)		(0.048)		(0.245)		(0.443)
Drug arrests	[0.015]	-0.003	[0.021]	-0.004	[0.017]	0.013	[0.161]	0.065	[0.253]	-0.090
		(0.004)		(0.008)		(0.013)		(0.069)		(0.069)
Property arrests	[0.054]	-0.012	[0.097]	-0.047***	[0.055]	-0.018	[0.362]	0.190	[0.505]	0.335
		(0.010)		(0.015)		(0.025)		(0.124)		(0.235)
Violent arrests	[0.089]	-0.012	[0.131]	-0.036*	[0.117]	-0.063**	[0.523]	-0.162	[0.824]	-0.648**
		(0.014)		(0.022)		(0.030)		(0.127)		(0.256)
Felony arrests	[0.115]	-0.006	[0.191]	-0.027	[0.142]	-0.056	[0.769]	0.096	[1.176]	-0.325
		(0.016)		(0.028)		(0.035)		(0.176)		(0.302)
Misdemeanor arrests	[0.083]	-0.015	[0.141]	-0.056***	[0.107]	-0.025	[0.608]	-0.060	[0.890]	-0.054
		(0.013)		(0.020)		(0.027)		(0.134)		(0.231)
Controlling for:										
Neighborhood characteristics		Y		Y		Y		Y		Y
Demographic characteristics		Y		Y		Y		Y		Y
Baseline criminal history		Y		Y		Y		Y		Y
Number of youth		4219		1950		857		270		121

Notes: CM, control mean in brackets; ITT, OLS intent to treat estimates with robust standard errors in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source: Authors' calculations.

TABLE 3. CUMULATIVE ITT PROGRAM IMPACT ON YOUTH WITH A SECOND CONSECUTIVE SYEP DOSE

	(1)		(2)	
	All 2015 lottery Winners		All 2015 lottery winners who also applied in 2016	
	CM	ITT	CM	ITT
A. Incidence: Percent of youth arrested				
Any arrest	[0.097]	-0.027	[0.098]	-0.032
		(0.017)		(0.021)
Violent arrest	[0.060]	-0.017	[0.052]	-0.009
		(0.015)		(0.017)
Property arrest	[0.038]	-0.019*	[0.046]	-0.028**
		(0.010)		(0.013)
Drug arrest	[0.011]	-0.013***	[0.018]	-0.020**
		(0.005)		(0.008)
Felony arrest	[0.063]	-0.015	[0.080]	-0.035*
		(0.015)		(0.019)
Misdemeanor arrest	[0.056]	-0.025**	[0.052]	-0.022
		(0.013)		(0.015)
B. Intensity: Number of arrests per youth				
All arrests	[0.197]	-0.093***	[0.199]	-0.090**
		(0.036)		(0.044)
Violent arrests	[0.093]	-0.054**	[0.083]	-0.038
		(0.022)		(0.027)
Property arrests	[0.051]	-0.029*	[0.052]	-0.029*
		(0.015)		(0.015)
Drug arrests	[0.012]	-0.014**	[0.021]	-0.022**
		(0.005)		(0.009)
Felony arrests	[0.116]	-0.044*	[0.135]	-0.062*
		(0.027)		(0.035)
Misdemeanor arrests	[0.081]	-0.053***	[0.064]	-0.033*
		(0.018)		(0.018)
Controlling for:				
Neighborhood characteristics		Yes		Yes
Demographic characteristics		Yes		Yes
Baseline criminal history		Yes		Yes
Number of youth	1186	1186	613	613

Notes: SYEP, summer youth employment program; ITT, OLS intent to treat estimates with robust standard errors in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source: Authors' calculations.

TABLE 4. CUMULATIVE ITT ESTIMATES OF PROGRAM IMPACT ON INCIDENCE OF CO-OFFENDING

	(1)	(2)	(3)	(4)	(5)
	All Youth	**Male**	**Age 18-24 years**	**At-risk: any prior**	**High-risk: prior arrest**
A. Any arrest or FIO involving multiple people					
Treatment	-0.006	0.002	-0.004	-0.001	-0.000
	(0.009)	(0.010)	(0.011)	(0.009)	(0.009)
Group		0.092***	0.010	0.310***	0.237***
		(0.011)	(0.018)	(0.058)	(0.057)
Treatment × Group		-0.018	-0.012	-0.087	-0.203**
		(0.019)	(0.023)	(0.064)	(0.087)
CM: youth in sub-group	0.11	0.19	0.13	0.56	0.64
CM: youth not in sub-group	0.11	0.05	0.11	0.08	0.10
B. Any arrest involving multiple people					
Treatment	-0.009	0.003	-0.011*	-0.006	-0.004
	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)
Group		0.034***	-0.007	0.158***	0.174***
		(0.007)	(0.013)	(0.031)	(0.050)
Treatment × Group		-0.025**	0.012	-0.037	-0.152**
		(0.012)	(0.015)	(0.054)	(0.071)
CM: youth in sub-group	0.04	0.07	0.04	0.24	0.29
CM: youth not in sub-group	0.04	0.02	0.04	0.03	0.03
C. Any FIO involving multiple people					
Treatment	-0.004	0.003	-0.002	0.003	0.002
	(0.009)	(0.008)	(0.010)	(0.008)	(0.009)
Group		0.087***	0.014	0.259***	0.258***
		(0.010)	(0.016)	(0.050)	(0.054)
Treatment × Group		-0.014	-0.009	-0.094	-0.168*
		(0.018)	(0.021)	(0.063)	(0.094)
CM: youth in sub-group	0.09	0.16	0.11	0.49	0.56
CM: youth not in sub-group	0.09	0.03	0.09	0.07	0.08
Controlling for all independent variables:	Y	Y	Y	Y	Y
Total number of youth	4219	4219	4219	4219	4219
Number of youth in subgroup of interest	4,219	1,950	857	270	121

Notes: FIO, field, interrogation, and observation; CM, control mean; ITT, OLS intent to treat estimates with robust standard errors in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source: Authors' calculations.

TABLE 5. CUMULATIVE ITT ESTIMATES OF PROGRAM IMPACT ON PEERS OF SYEP APPLICANTS

	(1)		(2)	
	Incidence: Percent of youth arrested		**Intensity: Number of Arrests**	
	CM	ITT	CM	ITT
All arrests	[0.348]	0.035	[1.089]	-0.128
		(0.032)		(0.142)
Violent arrests	[0.184]	0.031	[0.337]	-0.028
		(0.032)		(0.061)
Property arrests	[0.145]	0.015	[0.232]	0.052
		(0.029)		(0.064)
Drug arrests	[0.085]	0.004	[0.133]	-0.025
		(0.025)		(0.038)
Felony arrests	[0.275]	0.016	[0.648]	-0.078
		(0.034)		(0.103)
Misdemeanor arrests	[0.224]	-0.006	[0.441]	-0.093
		(0.033)		(0.076)
Controlling for:				
Demographic characteristics	Yes	Yes	Yes	Yes
Baseline criminal justice history	Yes	Yes	Yes	Yes
Number of youth	670	670	670	670

Notes: SYEP, summer youth employment program; CM, control mean; ITT, OLS intent to treat estimates with robust standard errors in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source: Authors' calculations.

Nothing Stops a Bullet like a Job? The Long-Term Impact of Summer Youth Employment Programs on Criminal Justice Involvement

Alicia Sasser Modestino, Aria Golestani, Brandon Turchan, Sebastian Ramirez, Benjamin Struhl, and Anthony A. Braga

Online Appendix

A. Data Collection and Variable Construction

1. Data Collection

We rely on a lottery assignment conducted for youth who applied to the Boston Summer Youth Employment Program (SYEP) for summer 2015 by Action for Boston Community Development (ABCD), a large and established nonprofit that works in all of Boston's 18 neighborhoods and serves a predominately young, school-aged, and low-income population. The enrollment period typically spans February through June, and applicants are notified of their lottery status and job assignment in late June. See Figure A1 for a timeline of the program and data collection. ABCD uses a computerized system with a random-assignment algorithm to select applicants based on their applicant ID numbers and the number of available slots as determined by the amount of funding ABCD receives each year. This system effectively assigns the offer to participate in the program at random, creating a control group of youth who apply to the SYEP but are not chosen.

During the summer of 2015, 4,235 youth applied to the Boston SYEP through ABCD. Of these, 1,186 (or 28 percent) were offered a job via random assignment, leaving 3,049 individuals in the control group. Of those selected by the lottery, 83.6 percent accepted a job offer, with only a handful dropping out during the program while it was in progress. According to quarterly wage record data provided by the Massachusetts Division of Unemployment Assistance, only 28.2 percent of youth in the control group had worked during the third quarter (July-September) of 2015.

We also check whether the applicants served by ABCD representative of all youth aged 14 to 24 years in the city of Boston. This question is important for demonstrating internal validity for the city of Boston and for city leaders potentially seeking to bring the summer jobs program to scale. Table A1 shows that ABCD draws applicants from all 18 of the city's neighborhoods with a greater representation among Dorchester (about 33 percent), Roxbury (about 10 percent), and Mattapan (about 9 percent). Applicants from other neighborhoods are also similarly represented in proportion to the distribution of youth as shown by the Census data: Hyde Park (about 6.6 percent), South Boston (about 6.4 percent), South End (about 6 percent), Roslindale (about 5.7 percent), Allston-Brighton (about 5 percent) and Jamaica Plain (about 4.5 percent).

In addition, data from the 2011 to 2015 5-Year American Community Survey indicate that ABCD applicants have similar gender and racial characteristics in comparison to the population of low-income Boston youth. Table A2 shows that although ABCD applicants are more likely to

be younger, within that younger age group (age 14 to 17 years), the breakdown by gender and race is very similar. In general, it
is reasonable to expect that youth applying to summer jobs programs would be younger given the greater difficulty that less experienced youth have in finding a job on their own.

2. Variable Construction

a. Independent Variables

We manipulate the administrative data collected by the Boston Summer Youth Employment Program to create the following independent variables based on pre-program demographics that are used as covariates in our analysis.

Gender – we create categories for Male, Female, Other, Missing
Race – we create categories for Asian, Black, White, Two/More Races, Other, and Missing
Language spoken at home – we create categories for whether English is the preferred language
Homeless – we create categories for Homeless, Not Homeless and Missing
Receiving public assistance - we create categories for Receiving Public Assistance, Not Receiving Public Assistance, and Missing
Disabled – we categories for Disabled, Not Disabled, and Missing

b. Dependent Variables

We manipulate the administrative data collected by the Boston Police Department to measure youth outcomes that are used as the dependent variables in our analysis.

(1) Primary Outcomes
Number of Arrests – we calculate the number of times that the individual was arrested as recorded by the Boston Police Department arrest data. To identify crimes by type, we categorize charges associated with each arrest based on the offense code. For example, violent crimes include all crimes against a person: assault, homicide, fatal and non-fatal shootings, sexual offenses, robbery, threats, kidnapping, and aggravated arson (arson when someone is known to be home). Property crime includes larceny,
burglary, non-aggravated arson, motor vehicle theft, and vandalism. Drug crimes include both possession and dealing. Note that status offenses (or "child in need of assistance") as well as revocations (e.g. rules violations) are not be included. We then count the number of pre-program arrests during the year prior to participation in the program to serve as a baseline control. We also count the number of post-program arrests since the program lottery notification date of June 2015 by month and cumulatively for the whole period.

Incidence of Violent Crime Arrest – we also construct a dummy variable with the categories of ever arrested as well as by offfense type separately for the pre- and post-program periods.

(2) Secondary Outcomes

Number of Co-Offending Arrests - we calculate the number of times that the individual was arrested for group-related criminal offending as recorded by the Boston Police Department arrest

data. To identify crimes by type, we will categorize charges associated with each arrest based on the offense code. For example, group-related violent crime offending (as opposed to crimes likely to be carried out in isolation) includes assault, homicide, fatal and non-fatal shootings, robbery, and threats. We count the number of pre-program arrests during the year prior to participation in the program to serve as a baseline control. We the count the number of post-program arrests since the program lottery notification date of June 2015 by month and cumulatively for the whole period.

Incidence of Co-Offending Arrests - we construct a dummy variable with the categories ever having a co-offending arrest separately for the pre- and post-program periods.

Number of Field, Interrogation and Observation (FIO) Contacts – we calculate the number of times that an individual experienced a noncustodial police contact as recorded by the Boston Police Department Field Interrogation and Observation database. We count the number of pre-program FIO contacts during the year prior to participation in the program to serve as a baseline control. We then count the number of post-program FIO contacts since the program lottery notification date of June 2015 by month and cumulatively for the whole period.

Incidence of Any Field, Interrogation and Observation (FIO) Contact – we construct a dummy variable with the categories of ever having an FIO Contact separately for the pre- and post-program periods.

(3) Spillovers/Peer Effects

Criminal Justice Peers – we identify pairs of individuals as peers if they are observed engaging in criminal activity together as recorded by either the BPD arrest or FIO data.

3. Additional Balance Checks

As discussed in the main text, randomization successfully balanced all observable demographic characteristics across treatment and control groups. We also check whether this is true for the pre-program criminal justice history for the two groups. Table A3 shows that based on observable characteristics collected at the time of the application to the program, the youth selected by the ABCD lottery appear to be almost identical to those not selected, confirming that the lottery is indeed random.

We also perform similar balance checks for the sample of peers associated with SYEP applicants in both the treatment and control groups. Peers are identified as individuals age 14 to 30 years of age with whom the SYEP applicant had either been arrested or had an FIO recorded during the pre-program period. Table A4 shows that the prior co-offending peers are largely balanced with the exception that the peers in the control group are more likely to be male. Again, it's not unusual to have one statistically different characteristic and we note that the demographic data from the FIO records are often subject to greater measurement error. More importantly, Table A5 shows that the peers of the treatment and groups had criminal justice histories that were not significantly different from one another.

Figure A1. Timeline of Boston SYEP program and data collection



Source: Authors' illustration of Boston SYEP program and data collection based on information from the City of Boston's Office of Workforce Development.

Table A1. Distribution of SYEP applicants across Boston neighborhoods by lottery outcome

	(1)	(2)	(3)
	Treatment	**Control**	**2010 Census**
	1,186	3,049	
Neighborhood			
Allston/Brighton	4.9%	5.1%	7.6%
Beacon Hill/Back Bay	0.4%	0.4%	2.0%
Charlestown	2.2%	2.4%	2.2%
Chinatown	0.8%	0.5%	1.7%
Dorchester	33.4%	32.8%	24.8%
East Boston	6.7%	6.6%	7.6%
Fenway	0.0%	0.0%	6.3%
Hyde Park	6.6%	6.5%	6.5%
Jamaica Plain	4.5%	4.7%	5.2%
Mattapan	9.1%	8.9%	4.7%
Mission Hill	1.8%	2.0%	2.6%
North End	0.1%	0.1%	0.4%
Roslindale	5.7%	5.7%	4.8%
Roxbury	10.3%	10.4%	11.4%
South Boston	6.4%	6.6%	3.7%
South End	6.1%	6.3%	3.1%
West End	0.1%	0.1%	0.4%
West Roxbury	0.8%	0.9%	5.1%

Source: Authors' calculations based on the 2010 Census, U.S. Bureau of the Census Population Estimates, 2010.

Table A2. SYEP applicant characteristics by lottery outcome versus Boston youth population

	(1)	(2)	(3)
	Treatment	**Control**	**5-Year ACS**
	1,186	3,049	
Age			
14-17 years	79.4%	80.2%	28.3%
18-24 years	20.6%	19.8%	71.7%
<u>Among those Age 14-17 years:</u>			
Gender			
Female	53.1%	53.9%	51.5%
Male	46.9%	46.1%	48.5%
Race			
African American	51.3%	54.0%	50.1%
Asian*	6.5%	5.0%	6.6%
White	9.6%	8.4%	9.5%
Other / Mixed-Race	32.5%	32.6%	33.8%

Source: Authors' calculations based on the 5-Year ACS, U.S. Census Bureau American Community Survey 5-Year Estimates, 2011-2015.

Table A.3. Baseline criminal justice history of SYEP applicants, by lottery outcome

	(1)		(2)		(3)	
	Treatment		**Control**		**Treatment-Control**	
	Mean	SD	Mean	SD	Diff.	*p*-value
A. Incidence – Percent of youth FIO recorded or arrested						
Any FIO or arrest	0.060	(0.237)	0.066	(0.248)	-0.006	(0.49)
FIO only	0.047	(0.212)	0.054	(0.226)	-0.007	(0.37)
Arrest only	0.025	(0.157)	0.030	(0.171)	-0.005	(0.41)
Drug arrest	0.001	(0.029)	0.003	(0.051)	-0.002	(0.26)
Property arrest	0.008	(0.087)	0.013	(0.111)	-0.005	(0.17)
Violent arrest	0.019	(0.135)	0.015	(0.120)	0.004	(0.34)
Felony arrest	0.018	(0.132)	0.021	(0.144)	-0.003	(0.48)
Misdemeanor arrest	0.014	(0.119)	0.015	(0.120)	-0.000	(0.97)
FIO or arrest involving multiple people	0.043	(0.203)	0.046	(0.211)	-0.003	(0.63)
F-test (Prob>F)					1.47	
					(0.15)	
B. Intensity: Number of FIOs and arrests per youth						
FIOs and arrests combined	0.168	(1.132)	0.171	(0.991)	-0.003	(0.93)
FIOs only	0.127	(0.943)	0.122	(0.756)	0.005	(0.85)
Arrests only	0.040	(0.302)	0.049	(0.345)	-0.009	(0.45)
Drug Arrests	0.002	(0.058)	0.003	(0.051)	-0.001	(0.60)
Property Arrests	0.010	(0.123)	0.016	(0.153)	-0.006	(0.25)
Violent Arrests	0.023	(0.175)	0.022	(0.207)	0.001	(0.92)
Felony Arrests	0.024	(0.206)	0.029	(0.238)	-0.005	(0.53)
Misdemeanor Arrests	0.016	(0.138)	0.020	(0.179)	-0.004	(0.51)
FIOs and arrests involving multiple people	0.081	(0.520)	0.082	(0.463)	-0.001	(0.96)
F-test (Prob>F)					0.58	
					(0.77)	
Number of youth	1,186		3,033		4,219	

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; SD, standard deviation; Diff, difference between the treatment and control groups; *p*-value is from a regression of the baseline criminal history on the treatment dummy; F-test represents the joint significance across all baseline characteristics.; statistcical significance is indicated by $* p < 0.1$, $** p < 0.05$, and $*** p < 0.01$.

Table A.4. Baseline demographic characteristics of peers, by lottery outcome of SYEP applicants

	(1)		(2)		(3)	
	Treatment		**Control**		**Treatment – Control**	
	Mean	SD	Mean	SD	Diff.	*p*-value
Age	18.89	(3.47)	18.87	(3.36)	0.02	(0.94)
Percent age 18-24 years	0.65	(0.48)	0.67	(0.47)	-0.02	(0.62)
Race:						
Percent Male	0.93	(0.26)	0.87	(0.34)	0.07***	(0.01)
Percent Black	0.41	(0.49)	0.41	(0.49)	-0.01	(0.86)
Percent White	0.14	(0.35)	0.15	(0.36)	-0.01	(0.82)
Percent Hispanic	0.30	(0.46)	0.26	(0.44)	0.04	(0.27)
F-test (Prob>F)					1.50 (0.18)	
Number of youth	187		483		670	

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; SD, standard deviation; Diff, difference between the treatment and control groups; *p*-value is from a regression of the peer characteristics on the treatment dummy; F-test represents the joint significance across all baseline characteristics.; statistcical significance is indicated by $*$ $p < 0.1$, $**$ $p < 0.05$, and $***$ $p < 0.01$.

Table A.5. Baseline criminal justice history of peers, by lottery outcome of SYEP applicants

	(1)		(2)		(3)	
	Treatment		**Control**		**Treatment – Control**	
	Mean	SD	Mean	SD	Diff.	*p*-value
FIOs and arrests	5.091	(7.690)	4.559	(7.621)	0.532	(0.42)
FIOs only	4.385	(6.952)	3.855	(6.901)	0.530	(0.37)
Arrests only	0.706	(1.389)	0.704	(1.356)	0.002	(0.99)
Violent arrests	0.235	(0.638)	0.240	(0.751)	-0.005	(0.94)
Drug arrests	0.059	(0.297)	0.056	(0.300)	0.003	(0.91)
Property arrests	0.166	(0.567)	0.251	(0.721)	-0.085	(0.15)
Felony arrests	0.380	(0.783)	0.354	(0.835)	0.026	(0.72)
Misdemeanor arrests	0.326	(0.800)	0.350	(0.822)	-0.024	(0.74)
FIOs and arrests involving multiple people	3.214	(4.344)	2.845	(4.401)	0.369	(0.33)
F-test (Prob>F)					1.37 (0.21)	
Number of youth	187		483		670	

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; SD, standard deviation; Diff, difference between the treatment and control groups; *p*-value is from a regression of the baseline criminal history on the treatment dummy; F-test represents the joint significance across all baseline characteristics.; statistcical significance is indicated by $*$ $p < 0.1$, $**$ $p < 0.05$, and $***$ $p < 0.01$.

B. Explanatory Power of Independent Variables

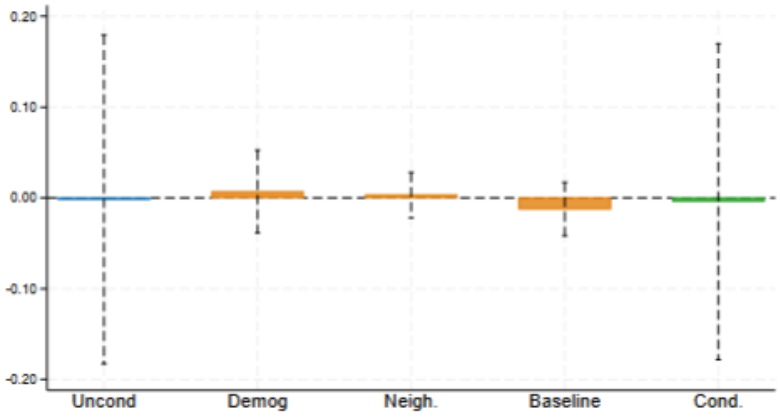
To assess the impact of the Boston SYEP on criminal justice outcomes, we compare criminal records during the period following the intervention for youth offered SYEP placements (the treatment group) with the records for youth not offered placements (control group). We measure two primary outcomes of interest: whether an individual has been arrested for any crime during the post-intervention period, and the number of arrests per youth during the post-intervention period. Because SYEP participation is allocated via lottery, we obtain causal estimates using a simple comparison of means on the outcome of interest.

Although baseline characteristics are not necessary for identification, we include them in the regression to improve the precision of estimates by accounting for residual variation in the outcomes. Baseline covariates include neighborhood, demographic, and criminal justice characteristics. Neighborhood characteristics are measured by the Census tract that the individual resides in to account for spatial differences in demographics, median household income, poverty rate, percentage receiving public assistance, percentage of households that spend more than 30 percent of their incomes on rent, labor force participation, and unemployment rate. Demographic characteristics are collected during the application process and include age, gender, race/ethnicity, limited English proficiency, school enrollment status, receipt of public assistance, and housing insecurity. We also include baseline criminal justice outcomes recorded by the administrative data from the Boston Police Department. To measure the baseline number of incidents, we aggregate the number of arrests and FIOs over our observable pre-program period which includes the 17 months prior to random assignment (January 2014 through June 2015). Similarly, we measure the baseline incidence of having any criminal justcie involvement during the pre-program period as any arrest or FIO between January 2014 and June 2015.

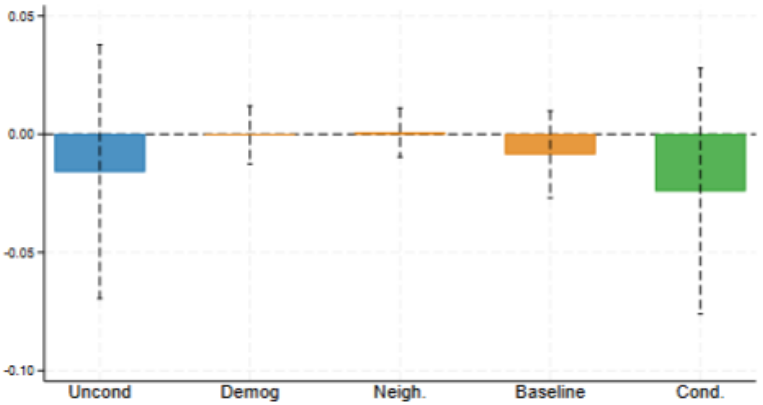
We sequentially add in each group of baseline controls to Tables B1 and B2. None of the substantive conclusions are different if these variables are excluded from the outcome regressions, but the covariates do improve precision. Figure B1 provides a formal Gelbach (2016) decomposition where the first bar is the unconditional coefficient on SYEP participation. The next three bars decompose the change into the amount that can be explained by demographic characteristics, neighborhood characteristics, and pre-program criminal justice history along with 95% confidence intervals of these estimates. The final bar is the residual effect after controlling for all characteristics and controls. The decomposition reveals that very little of the covariates explain any of the FIO variation between the treatment and control groups. In contrast, the baseline criminal justice history has some explanatory power but also does not explain much of the variation.

Figure B1. Gelbach decomposition of covariates

A. Total Number of FIOs



B. Total Number of Arrets



Source: Authors' calculations based on arrest records from the Boston Police Department.

Table B.1. Cumulative ITT program effect on incidence for all SYEP youth, adding controls

	(1)	(2)	(3)	(4)	(5)
	CM	ITT	ITT	ITT	ITT
Any arrest	[0.097]	-0.004	-0.001	-0.002	-0.001
		(0.010)	(0.010)	(0.010)	(0.009)
Drug arrest	[0.010]	-0.001	-0.001	-0.001	-0.001
		(0.003)	(0.003)	(0.003)	(0.003)
Property arrest	[0.036]	-0.001	-0.000	-0.002	-0.002
		(0.006)	(0.006)	(0.006)	(0.006)
Violent arrest	[0.059]	0.000	0.001	0.001	-0.003
		(0.008)	(0.008)	(0.008)	(0.008)
Felony arrest	[0.064]	-0.002	0.001	0.001	-0.000
		(0.008)	(0.008)	(0.008)	(0.008)
Misdemeanor arrest	[0.055]	-0.004	-0.002	-0.005	-0.006
		(0.008)	(0.008)	(0.008)	(0.007)
Controlling for:					
Neighborhood characteristics		N	Y	Y	Y
Demographic characteristics		N	N	Y	Y
Baseline criminal justice history		N	N	N	Y
Number of youth		4219	4219	4219	4219

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; CM, control mean in brackets; ITT, OLS intent to treat estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.2. Cumulative ITT program effect on intensity for all SYEP youth, adding controls

	(1)	(2)	(3)	(4)	(5)
	CM	ITT	ITT	ITT	ITT
Number of arrests	[0.199]	-0.016	-0.011	-0.016	-0.017
		(0.027)	(0.028)	(0.028)	(0.023)
Drug Arrests	[0.015]	-0.006	-0.006	-0.006	-0.003
		(0.005)	(0.006)	(0.005)	(0.004)
Property Arrests	[0.054]	-0.008	-0.008	-0.012	-0.012
		(0.011)	(0.011)	(0.011)	(0.010)
Violent Arrests	[0.089]	-0.004	-0.003	-0.005	-0.012
		(0.014)	(0.014)	(0.014)	(0.014)
Felony Arrests	[0.115]	-0.004	-0.002	-0.003	-0.006
		(0.019)	(0.019)	(0.019)	(0.016)
Misdemeanor Arrests	[0.083]	-0.012	-0.010	-0.013	-0.015
		(0.013)	(0.013)	(0.013)	(0.013)
Controlling for:					
Neighborhood characteristics		N	Y	Y	Y
Demographic characteristics		N	N	Y	Y
Baseline criminal justice history		N	N	N	Y
Number of youth		4219	4219	4219	4219

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; CM, control mean in brackets; ITT, OLS intent to treat estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C. Using Nonlinear Specifications

While ordinary least squares provides the best linear unbiased estimate of the treatment effect under the usual Gauss-Markov assumptions, we also explore the robustness of our results to using non-linear specifications. To analyze treatment-control differences in the number of arrests—a count variable—we use a Poisson quasi-maximum likelihood estimator (QMLE). The consistency of this estimator only requires the correct specification of the conditional mean, not the entire distribution (Wooldridge, 1997). We also use Huber-White robust standard errors to allow for over-dispersion, relaxing the Poisson distributional constraint that the mean equals the variance. To analyze differences in the likelihood of being arrested, a 0/1 dependent variable, we use a probit estimator. Tables C1 and C2 confirm that using that using these nonlinear specifications does litte to change our findings.

Table C.1. Cumulative Probit estimates of SYEP program effect on incidence, by offense type and sub-group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		All Youth				Male	Age 18-24 years
	CM	Probit	Probit	Probit	Probit	Probit	Probit
Any Arrest	[0.097]	-0.004	-0.001	-0.001	0.000	-0.028*	-0.021
		(0.010)	(0.010)	(0.010)	(0.009)	(0.016)	(0.023)
Violent Arrest	[0.059]	0.000	0.001	-0.000	-0.003	-0.016	-0.047**
		(0.008)	(0.008)	(0.008)	(0.008)	(0.013)	(0.021)
Property Arrest	[0.036]	-0.001	-0.000	-0.001	-0.002	-0.022*	-0.013
		(0.006)	(0.006)	(0.006)	(0.006)	(0.011)	(0.015)
Drug Arrest	[0.010]	-0.002	-0.001	-0.002	-0.001	-0.003	0.014*
		(0.003)	(0.003)	(0.004)	(0.003)	(0.007)	(0.009)
Felony Arrest	[0.064]	-0.002	0.001	0.001	0.001	-0.014	-0.040*
		(0.008)	(0.008)	(0.009)	(0.008)	(0.015)	(0.022)
Misdemeanor Arrest	[0.055]	-0.004	-0.002	-0.004	-0.005	-0.039***	-0.009
		(0.008)	(0.008)	(0.008)	(0.007)	(0.014)	(0.019)
Controlling for:							
Neighborhood characteristics		N	Y	Y	Y	Y	Y
Demographic characteristics		N	N	Y	Y	Y	Y
Baseline criminal justice history		N	N	N	Y	Y	Y
Number of youth		4219	4216	4210	4210	1946	798

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; CM, control mean in brackets; Probit, nonlinear intent to treat estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.2. Cumulative Poisson estimates of SYEP program effect on intensity, by offense type and sub-group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		All Youth				Male	Age 18-24 years
	CM	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson
Number of arrests	[0.199]	-0.016	-0.011	-0.018	-0.021	-0.104**	-0.075
		(0.028)	(0.028)	(0.028)	(0.025)	(0.048)	(0.057)
Violent arrests	[0.089]	-0.004	-0.003	-0.007	-0.019	-0.046*	-0.067*
		(0.015)	(0.014)	(0.015)	(0.016)	(0.027)	(0.036)
Property arrests	[0.054]	-0.008	-0.009	-0.012	-0.013	-0.059***	-0.018
		(0.012)	(0.012)	(0.012)	(0.012)	(0.021)	(0.022)
Drug arrests	[0.015]	-0.007	-0.007	-0.007	-0.004	-0.004	0.016
		(0.006)	(0.006)	(0.006)	(0.005)	(0.009)	(0.013)
Felony arrests	[0.115]	-0.004	-0.001	-0.002	-0.006	-0.033	-0.061
		(0.019)	(0.019)	(0.019)	(0.017)	(0.033)	(0.043)
Misdemeanor arrests	[0.083]	-0.012	-0.010	-0.014	-0.019	-0.074***	-0.025
		(0.014)	(0.014)	(0.014)	(0.014)	(0.026)	(0.029)
Controlling for:							
Neighborhood characteristics		N	Y	Y	Y	Y	Y
Demographic characteristics		N	N	Y	Y	Y	Y
Baseline criminal justice history		N	N	N	Y	Y	Y
Number of youth		4,219	4,219	4,219	4,219	1,950	857

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; CM, control mean in brackets; Poisson, nonlinear intent to treat estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

D. Accounting for Noncompliers

The "Intent to Treat" (ITT) estimate used in the main paper measures the impact of offering the program on the outcome. In many cases, this is the policy-relevant estimate because program administrators want to account for program take-up to assess the degree to which SYEP could reduce criminal activity among all the applicants, not just the participants. Nonetheless, because not all youth end up participating, the ITT will understate the effects of the program for those youth who choose to participate.

Treatment on the Treated

To address this, we also provide estimates of treatment-on-the-treated (TOT). Under the usual relevance and exogeneity assumptions for instrumental variables, this latter set of effects can be recovered from the experimental data. I perform this estimation through a two-stage least squares strategy, in which random assignment ($SYEP_i$) is an instrument for actual participation (P_{it}), and P'_{it} is the predicted probability of participation from equation (D.1):

$$P_{it} = SYEP_i\,\pi_2 + X_i(t-1)\beta_2 + \mu_{it} \qquad (D.1)$$

$$Yit = P_i\,\pi_3 + X_i(t-1)\beta_3 + \mu_{it2} \qquad (D.2)$$

If all youth respond the same way to the program (i.e., if treatment effects are constant across youth), then equations (A.2) and (A.3) also yield an estimate of the average treatment effect (ATE) across this population of disadvantaged youth. Given that treatment effects are likely to be heterogeneous across youth, then the coefficient $\pi 3$ estimates a local average treatment effect—the effect of participation on those who comply with random assignment. Table D1 and D2 report the Treatment-on-the-Treated (TOT) estimates that show the effect of the Boston SYEP for those who choose to participate. The TOT estimates are only slightly larger than the ITT estimates, likely because the take-up rate is so high (about 85 percent).

Accounting for Cross-Overs

Typically, in studies of short-term program impacts, we do not need to worry about youth that crossover from the control group to become treated. However, over the course of 55 months, roughly 20 percent of the control group ends up participating in the Boston SYEP—either winning the lottery through ABCD or by being placed in a summer job through one of the other intermediaries that contracts with the city. To account for this, we follow Kessler et. al. (2021) and allow those youth to cross-over into treatment once they have participated in the SYEP for the first time. Tables D3 and D4 show that our estimates of the program's impacts are even larger when we account for these youth that crossover from the control group into receiving treatment.

Alternative Methods to Measure Dosage

Finally, we provide two alternative ways to measure the impacts of youth who apply for and are randomly assigned to a job for a second consecutive summer. First, we separately account for receiving one versus two summers and find that virtually all of the impact is coming from having a second dose of participation in the program. Second, we use the number of summers that youth were randomly assigned to the program (0, 1, or 2) as an instrument for being treated and find similar results (see Table D5).

Table D.1. Cumulative two-stage least squares estimates of SYEP program effect on incidence, by offense type and sub-group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Youth					Male	Age 18-24 Years	At-risk: any prior	High-risk: prior arrest
	CM	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Any Arrest	[0.097]	-0.001	0.001	-0.002	0.000	-0.020	-0.019	0.052	0.069
		(0.011)	(0.011)	(0.011)	(0.010)	(0.017)	(0.024)	(0.077)	(0.107)
Violent Arrest	[0.059]	0.001	0.001	-0.001	-0.002	-0.008	-0.035*	0.028	-0.133
		(0.009)	(0.009)	(0.009)	(0.009)	(0.014)	(0.019)	(0.078)	(0.122)
Property Arrest	[0.036]	-0.003	-0.003	-0.005	-0.004	-0.024**	-0.017	0.146**	0.269***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.011)	(0.014)	(0.064)	(0.089)
Drug Arrest	[0.010]	-0.002	-0.002	-0.002	-0.001	-0.002	0.008	-0.045	-0.064
		(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.010)	(0.034)	(0.042)
Felony Arrest	[0.064]	0.004	0.006	0.004	0.005	-0.006	-0.031*	0.065	0.053
		(0.010)	(0.010)	(0.010)	(0.009)	(0.015)	(0.019)	(0.076)	(0.116)
Misdemeanor Arrest	[0.055]	-0.008	-0.007	-0.011	-0.011	-0.040***	-0.016	-0.055	-0.020
		(0.008)	(0.009)	(0.009)	(0.008)	(0.013)	(0.021)	(0.069)	(0.099)
Controlling for:									
Neighborhood characteristics		N	Y	Y	Y	Y	Y	Y	Y
Demographic characteristics		N	N	Y	Y	Y	Y	Y	Y
Baseline criminal justice history		N	N	N	Y	Y	Y	Y	Y
Number of youth		4219	4219	4219	4219	1950	857	270	121

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: SYEP, summer youth employment program; CM, control mean in brackets; 2SLS, two-stage least squares estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D.2. Cumulative two-stage least squares estimates of SYEP program effect on intensity, by offense type and sub-group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Youth					Male	Age 18-24 Years	At-risk: prior arrest	High-risk: any prior
	CM	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
All Arrests	[0.199]	-0.018	-0.013	-0.023	-0.016	-0.090**	-0.078	0.137	-0.173
		(0.030)	(0.029)	(0.029)	(0.025)	(0.040)	(0.053)	(0.253)	(0.414)
Violent arrests	[0.089]	-0.005	-0.004	-0.008	-0.008	-0.029	-0.055*	-0.094	-0.552**
		(0.016)	(0.016)	(0.016)	(0.016)	(0.024)	(0.030)	(0.135)	(0.231)
Property arrests	[0.054]	-0.007	-0.006	-0.012	-0.009	-0.051***	-0.025	0.281**	0.655***
		(0.012)	(0.012)	(0.012)	(0.011)	(0.016)	(0.028)	(0.136)	(0.222)
Drug arrests	[0.015]	-0.006	-0.005	-0.005	-0.004	-0.004	0.007	0.006	-0.100*
		(0.005)	(0.005)	(0.005)	(0.005)	(0.009)	(0.014)	(0.065)	(0.060)
Felony arrests	[0.115]	0.003	0.006	0.000	0.003	-0.022	-0.057	0.188	-0.325
		(0.021)	(0.021)	(0.021)	(0.018)	(0.031)	(0.041)	(0.201)	(0.262)
Misdemeanor arrests	[0.083]	-0.020	-0.018	-0.023*	-0.022	-0.074***	-0.027	-0.090	0.036
		(0.014)	(0.014)	(0.014)	(0.013)	(0.021)	(0.030)	(0.138)	(0.226)
Controlling for:									
Neighborhood characteristics		N	Y	Y	Y	Y	Y	Y	Y
Demographic characteristics		N	N	Y	Y	Y	Y	Y	Y
Baseline criminal justice history		N	N	N	Y	Y	Y	Y	Y
Number of youth		4219	4219	4219	4219	1950	857	270	121

Source: Authors' calculations based on arrest records from the Boston Police Department.

Note: SYEP, summer youth employment program; CM, control mean in brackets; 2SLS, two-stage least squares estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D.3. Cumulative ITT program effect on incidence, by offense type and sub-group, accounting for cross-overs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		All Youth				Male	Age 18-24 Years	At-risk: prior arrest	High-risk: any prior
	CM	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
Any arrest	[0.097]	-0.004	-0.002	-0.003	-0.001	-0.020	-0.024	-0.037	-0.043
		(0.010)	(0.010)	(0.010)	(0.009)	(0.015)	(0.022)	(0.070)	(0.115)
Violent arrest	[0.058]	0.002	0.003	0.002	-0.001	-0.006	-0.048***	-0.047	-0.166
		(0.008)	(0.008)	(0.008)	(0.008)	(0.012)	(0.017)	(0.067)	(0.122)
Property arrest	[0.036]	-0.002	-0.001	-0.003	-0.003	-0.018*	-0.004	0.071	0.139
		(0.006)	(0.006)	(0.006)	(0.006)	(0.010)	(0.013)	(0.058)	(0.099)
Drug arrest	[0.010]	-0.002	-0.001	-0.002	-0.001	-0.005	0.012	-0.006	-0.033
		(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.009)	(0.034)	(0.061)
Felony arrest	[0.065]	-0.003	-0.000	-0.001	-0.000	-0.012	-0.044**	-0.034	-0.051
		(0.008)	(0.008)	(0.008)	(0.008)	(0.013)	(0.017)	(0.068)	(0.120)
Misdemeanor arrest	[0.055]	-0.002	-0.001	-0.003	-0.004	-0.025**	-0.008	-0.029	0.015
		(0.007)	(0.007)	(0.008)	(0.007)	(0.012)	(0.018)	(0.064)	(0.109)
Controlling for:									
Neighborhood characteristics		N	Y	Y	Y	Y	Y	Y	Y
Demographic characteristics		N	N	Y	Y	Y	Y	Y	Y
Baseline criminal justice history		N	N	N	Y	Y	Y	Y	Y
Number of youth		4219	4219	4219	4219	1950	857	270	121

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: CM, control mean in brackets; ITT, OLS intent to treat estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D.4. Cumulative ITT program effect on intensity, by offense type and sub-group, accounting for cross-overs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		All Youth				Male	18-24	At-risk: prior arrest	High-risk: any prior
	CM	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
All arrests	[0.201]	-0.021	-0.018	-0.023	-0.018	-0.074**	-0.096**	0.094	-0.275
		(0.026)	(0.026)	(0.026)	(0.022)	(0.038)	(0.047)	(0.243)	(0.428)
Violent arrests	[0.089]	-0.003	-0.002	-0.005	-0.010	-0.026	-0.076***	-0.136	-0.586**
		(0.014)	(0.014)	(0.014)	(0.013)	(0.022)	(0.029)	(0.126)	(0.246)
Property arrests	[0.055]	-0.010	-0.010	-0.014	-0.013	-0.047***	-0.016	0.164	0.240
		(0.010)	(0.010)	(0.011)	(0.010)	(0.016)	(0.025)	(0.124)	(0.216)
Drug arrests	[0.015]	-0.005	-0.005	-0.005	-0.002	-0.007	0.011	0.102	0.006
		(0.005)	(0.005)	(0.005)	(0.004)	(0.008)	(0.012)	(0.073)	(0.095)
Felony arrests	[0.117]	-0.009	-0.007	-0.009	-0.007	-0.030	-0.074**	0.100	-0.311
		(0.018)	(0.018)	(0.018)	(0.015)	(0.027)	(0.035)	(0.176)	(0.295)
Misdemeanor arrests	[0.084]	-0.012	-0.011	-0.014	-0.015	-0.050**	-0.026	-0.017	0.029
		(0.013)	(0.013)	(0.013)	(0.012)	(0.020)	(0.026)	(0.135)	(0.216)
Neighborhood Characteristics		N	Y	Y	Y	Y	Y	Y	Y
Demographic Characteristics		N	N	Y	Y	Y	Y	Y	Y
Baseline Criminal History		N	N	N	Y	Y	Y	Y	Y
Number of youth		4219	4219	4219	4219	1950	857	270	121

Source: Authors' calculations based on arrest records from the Boston Police Department.
Note: CM, control mean in brackets; ITT, OLS intent to treat estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D.5. Cumulative ITT program impact on all youth with second consecutive SYEP dose

	(1)	(2)		(3)	(4)	
	Incidence: Percent of youth			Intensity: Number of arrests per youth		
	CM	ITT		CM	ITT	
A. OLS estimates separating one versus two summers of treatment						
Any arrest						
Treated 2015	0.081	0.008		0.154	0.024	
		(0.019)			(0.044)	
Treated 2016 and 2016	0.073	-0.030		0.127	-0.098	**
		(0.021)			(0.045)	
Violent arrest						
Treated 2015	0.050	-0.007		0.072	-0.007	
		(0.015)			(0.028)	
Treated 2016 and 2016	0.047	-0.009		0.064	-0.044	
		(0.017)			(0.027)	
Property arrest						
Treated 2015	0.027	0.015		0.039	0.004	
		(0.013)			(0.016)	
Treated 2016 and 2016	0.022	-0.027	**	0.030	-0.033	**
		(0.013)			(0.015)	
Drug arrest						
Treated 2015	0.007	0.012		0.011	0.014	
		(0.008)			(0.009)	
Treated 2016 and 2016	0.006	-0.020	**	0.008	-0.023	**
		(0.008)			(0.009)	
Felony arrest						
Treated 2015	0.054	0.022		0.091	0.031	
		(0.017)			(0.033)	
Treated 2016 and 2016	0.048	-0.029		0.079	-0.057	*
		(0.019)			(0.035)	
Misdemeanor arrest						
Treated 2015	0.044	0.001		0.062	(0.007)	
		(0.014)			(0.020)	
Treated 2016 and 2016	0.037	-0.023		0.048	-0.040	**
		(0.015)			(0.020)	
B. IV estimates using number of summers treated as an instrument						
Any arrest	0.070	-0.015		0.121	-0.050	**
		(0.010)			(0.020)	
Violent arrest	0.046	-0.008		0.063	-0.030	**
		(0.008)			(0.012)	
Property arrest	0.020	-0.010	*	0.028	-0.018	**
		(0.005)			(0.007)	
Drug arrest	0.005	-0.007	***	0.006	-0.008	***
		(0.002)			(0.003)	
Felony arrest	0.045	-0.009		0.075	-0.022	
		(0.008)			(0.015)	
Misdemeanor arrest	0.036	-0.013	*	0.047	-0.027	**
		(0.007)			(0.010)	
Controlling for:						
Neighborhood characteristics		Y			Y	
Demographic characteristics		Y			Y	
Baseline criminal justice history		Y			Y	
Number of youth who won the 2015 lottery	1,287	1,287		1,287	1,287	

Source: Authors' calculations based on arrest records from the Boston Police Department.

Note: SYEP, summer youth employment program; ITT, OLS intent to treat estimates with robust standard errors in parentheses; statistcical significance is indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.