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

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Abstract: Expanding Summer Youth Employment Programs (SYEPs) has been suggested as one way to address youth violence. Prior studies showed SYEPs reduce violent crime during and in the months after the program, but little is known about the long-term impacts and how they can be sustained. We use an embedded lottery design to study a cohort of 4,219 youth who applied to the Boston SYEP during the summer of 2015, extending a prior study that showed significant short-term reductions in recidivism. Using a unique dataset of arrests and field interrogation and observations (FIOs), we show that SYEP impacts on violent crime are sustained for up to 4.5 years for males, youth aged 18-24 years, and those with a prior arrest record. These effects appear to be driven by youth who receive a second summer of participation as well as lower co-offending rates, possibly due to positive peer interactions.

Keywords: Youth, Workforce Development, Peers, Crime

JEL Codes: J13, J08, K14, K42

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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.[1] This uptick in violent crime, and youth violence in particular, prompted a renewed interest to find innovative interventions, including the expansion of Summer Youth Employment Programs (SYEPs)[2]. Indeed, randomized controlled trials across 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[3] and also involved in 35 to 40 percent fewer violent crimes up to 18 months after the program ends, compared to a control group [4,5,6]. Citing this research evidence, President Biden urged cities and states to use their American Rescue Plan Act (ARPA) funds to launch or expand SYEPs as one potential way to curb youth violence and "build back better" after the pandemic[7]. In response, many jurisdictions adopted the mindset that "nothing stops a bullet like a job"[8], turning to SYEPs as one way to create meaningful summer employment opportunities for young people with the goal of reducing their involvement with the criminal justice system.

The initial violence prevention gains generated by SYEPs are very encouraging and certainly benefit communities in the near-term. Yet it may be the case that these impacts are short-lived without additional intervention and cities will be hard-pressed to maintain their investments in summer jobs now that the ARPA funding has expired and federal bills to support summer jobs have gained little traction[8]. With average program costs of approximately $2,000 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 [9]. This limits the ability for jurisdictions to target the program towards at-risk youth, and also creates a stark tradeoff between serving as many youth as possible versus a more tailored approach that provides a second “dose” of summer employment to sustain the program’s impact. Finally, little is known about the mechanisms by which SYEP program impacts are achieved, especially the role of peers, which could also 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.

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. On the one hand, violent offending typically increases through the mid-twenties[11], or even later for youth living in neighborhoods with high crime rates[12], 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[13], especially considering the salient causal role of peers in the etiology of delinquency[14, 15, 16]. Indeed, most studies find between 50 and 75 percent of juvenile crimes are committed in the company of others[17] with the greatest concentration of serious youth violence occurring among individuals who are involved in gangs and other high-risk co-offending networks[18].

Despite a growing body of research on the positive impact of summer job programs across a range of near-term outcomes, including criminal justice involvement, little is known about the long-term effects nor how these impacts can be sustained—both of which are crucial when attempting to replicate the program in other cities or bring it to scale in existing ones[19]. 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 at-risk youth who experience a 10 percent reduction in felony arrests and a 20 percent reduction in felony convictions [3]. However, that study only had access to adult criminal justice records, limiting the analysis to youth who were already at the age of majority prior to the program (aged 16 through 18 in New York), and likely resulting in downward biased estimates that fail to capture juvenile offending and miss the program's preventive impacts on younger youth. Moreover, while prior research suggests that summer job programs reduce violent crime by improving soft skills[5], other potential channels that may sustain these behavioral changes, such as repeat participation during a second consecutive summer or peer interactions that serve to disrupt co-offending networks, remain unexplored.

Our study builds on previous research by broadening the scope to understand whether SYEPs have long-lasting effects on criminal justice outcomes across different types of crime and groups of youth to better measure the program's full impacts and understand its underlying mechanisms. First, we estimate program effects on arrests separately by the most serious charge to understand whether having a summer job affects how youth perceive and respond to social interactions (e.g., violent crime) differently from how it affects more economic situations (e.g. property or drug crime) versus their general opportunities to engage in criminal activity (e.g., total arrests). 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, including males, older youth aged 18 to 24 years, and at-risk youth (defined as having any criminal justice involvement or 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 at-risk youth with prior criminal justice involvement. Finally, we explore whether the summer jobs program affects co-offending behavior among the participants as well as other criminal activity among their co-offending "peers" that they had interacted with prior to the program. As we discuss, the findings from this research carry both theoretical implications for understanding how early employment experiences affect youth development as well as practical implications for deciding how to allocate resources between SYEP and other interventions aimed at reducing youth violence and/or increasing economic opportunity.

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, extending a prior study that showed significant short-term reductions in recidivism[5] . 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 (e.g., 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.

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. 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 capture involvement with the criminal justice system during the 17 months prior and up to 4.5 years after SYEP participation for the full age-range of 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 neighborhood, demographic, and criminal justice indicators. Because SYEP participation is allocated via lottery, the results from these analyses can be interpreted as the causal impacts of the program, although additional replication across other cities is needed to ensure that they are fully generalizable.

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 upward 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 confirms 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. As such, the Boston SYEP provided a meaningful intervention in terms of the likelihood, intensity, and type of employment obtained during the summer of 2015.

Based on these observable characteristics, the youth selected by lottery appear to be almost identical to those not selected, confirming that the lottery is indeed random. In Table 1, the only statistically significant difference is the share of Asian youth being slightly higher in the treatment group (7 percent) versus the control group (5 percent), yet this variation would be expected by random chance when testing 16 different characteristics. We follow youth in the administrative BPD data from January 2014 through December 2019, constructing both the incidence and number of FIOs and arrests by type (see Supplementary Note A for details on data collection and variable construction). 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 (e.g., misdemeanor versus felony), and co-offending (see Supplementary Note A).

SYEP Long-Term Impacts on Youth: Replication and Extension of Prior Research

Following our pre-analysis plan, we start with replicating the prior work of Kessler et. al.[3] using 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 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 [3], 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, we also find no statistically significant reductions in criminal justice activity for our alternative at-risk group defined as having any prior arrest or FIO recorded.

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 [20,21,22]. In terms of the number of arrests, the greatest impacts are on the long-term reduction in youth violence which suggests 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 were observed at only 18 months post-program[5]. Both 18-24 year olds and at-risk youth experience even greater reductions in violent crime arrests of 53 percent and 79 percent respectively during the full follow-up period. 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.

Dosage and timing of program impacts

We next examine whether it's 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 youth in the treatment group who receive a second summer are similar in magnitude than those experienced by youth receiving just one summer of participation—and even larger for males. In fact, there is now a large and significant reduction in both the incidence and number of drug arrests. These impacts largely persist when we further restrict the sample to the sub-sample of youth in the treatment group who also 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 received a second consecutive SYEP dose.

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[23]. Thus, it is likely that SYEPs do not completely incapacitate youth who are likely to engage in criminal activity but rather disrupt the frequency with which they do so. To test this, Fig. 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 Fig. 1 across various types of crime demonstrates that most of the reduction in criminal activity accrues well after the end of the program at month two, consistent with prior studies [4,5]. For misdemeanors and property arrests, the reduction in crime steadily accumulates

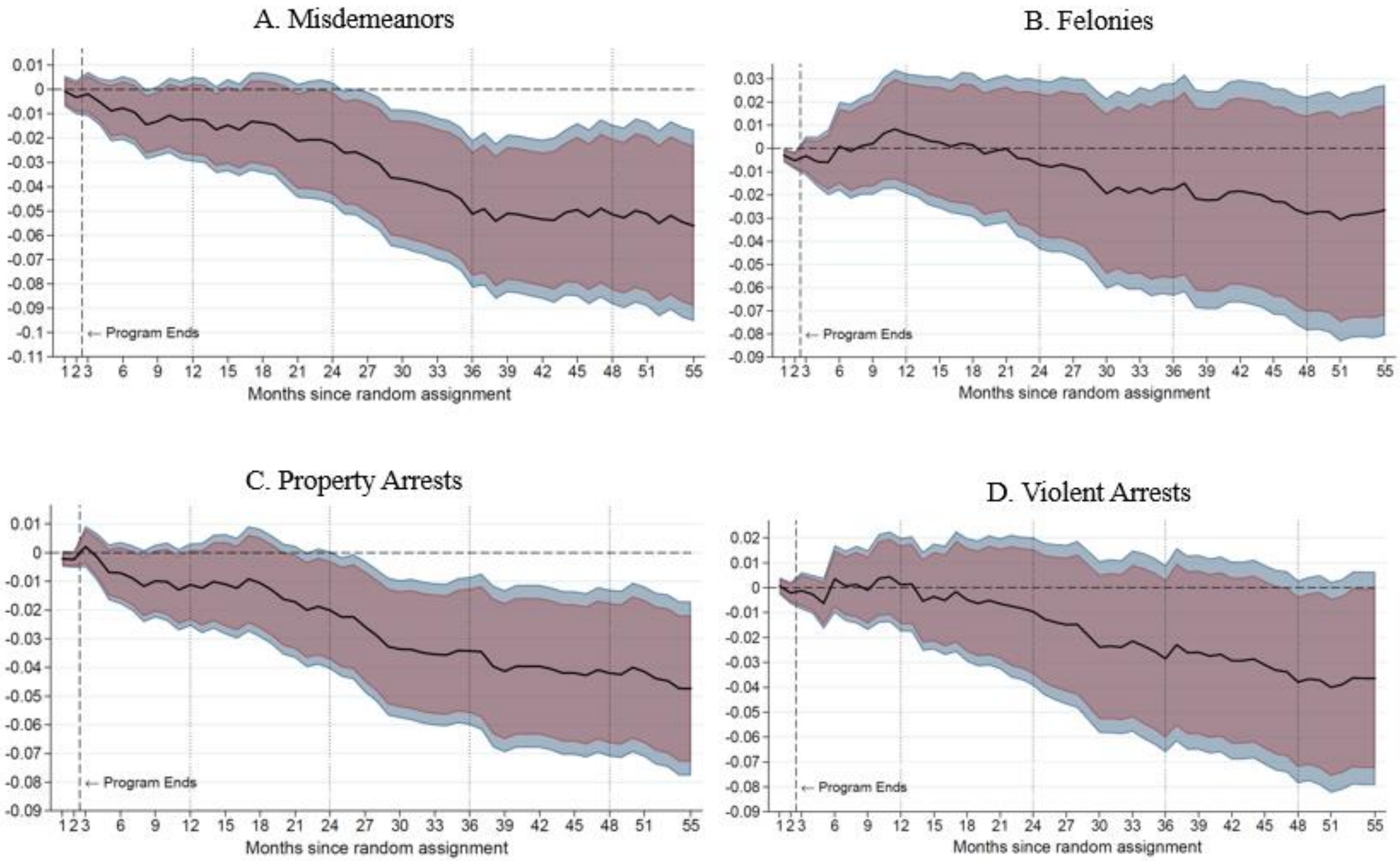

Figure 1.Cumulative time path of program impacts by month for males. This figure shows the cumulative treatment effect of the decline in arrests from random assignment in month zero through 55 months. The vertical dotted line shows the end of the program. Estimates include all neighborhood, demographic and baseline covariates. Confidence intervals using heteroskedasticity-robust standard errors are shown for both 90% (purple) and 95 percent (blue).

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 only after 48 months (4 years) post randomization.

Fig. 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 youth as initially engaged in suspicious activities but these perceptions may have changed over time in the absence of increased criminal activity as suggested by the decline in arrest outcomes. In contrast, for at-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. 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 at-risk youth.

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. 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

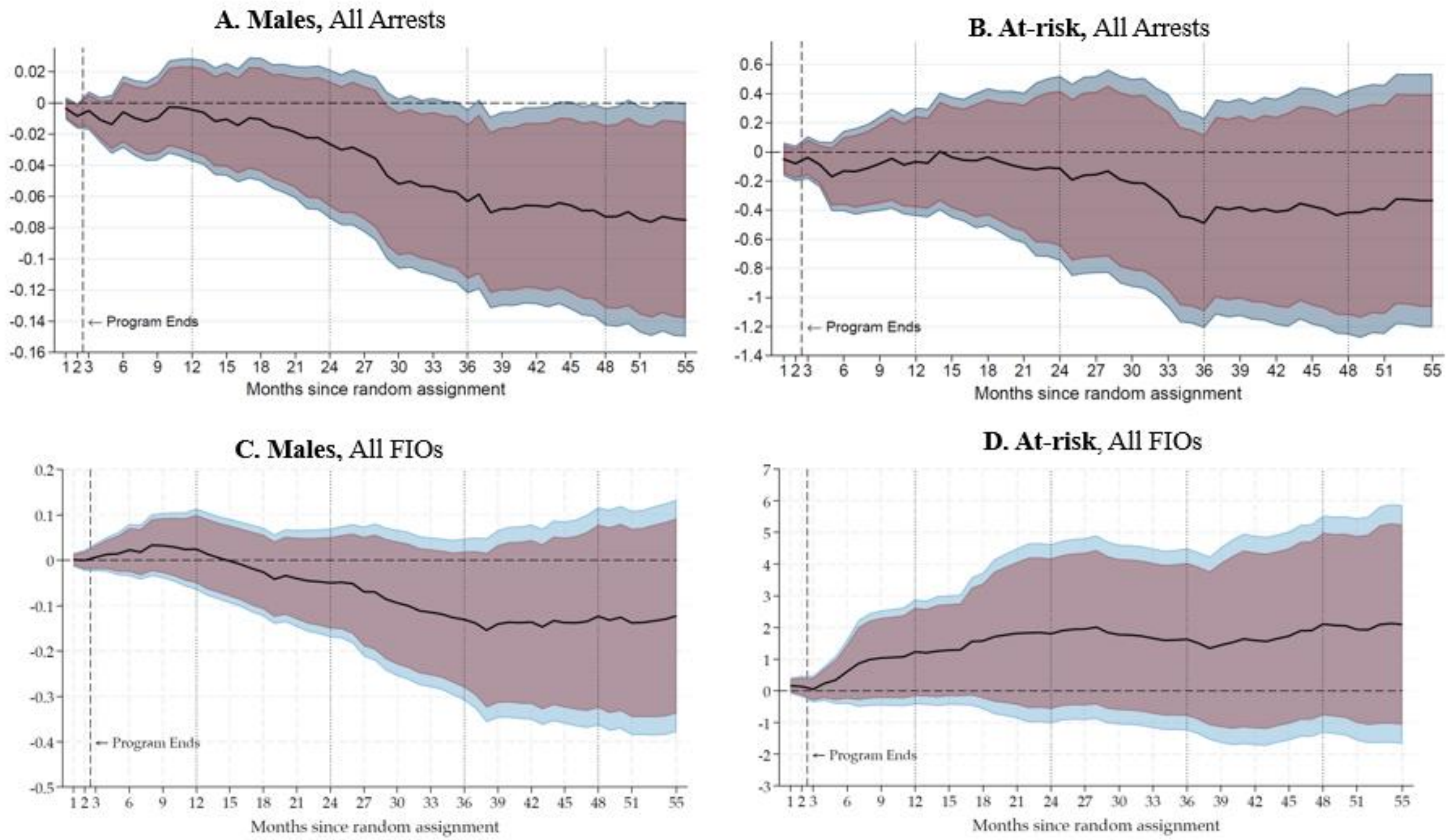

Figure 2. Time path of program impacts by month for arrests versus FIOs. This figure shows the cumulative treatment effect from random assignment in month zero through 55 months, comparing arrests and FIOs for all males and at-risk youth with a prior criminal justice history. The vertical dotted line 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).

arrested as well as being included in an FIO report with at least one other person. Although there is no significant impact on the full sample of youth, that participating in the program decreases arrests with co-offenders by 36 percent for males and by 52 percent for at-risk youth. Again, there is little to no reduction for youth being included with others in an FIO report post-program, except for a small 3 percent reduction for at-risk youth.

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, especially among at-risk youth. Do these better behaviors also influence the prior co-offending "peers" of the treatment group? Table 5 estimates the post-program impacts for peers of the treatment group and reveals that there are no significant impacts of the program on peers of the treatment group and if anything, the incidence seems to increase. Fig. 3 shows the trajectory of both violent and property crime for at-risk youth and their co-offending peers. Violent crime arrests drop significantly among at-risk youth immediately after the program ends and continue to decline throughout the 55-month post-program period. There is an initial drop in violent crime among peers as well 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 at-risk youth and follows a similar path for peers.

Supplementary analyses

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 Gelbach[25] decomposition that shows the amount of the program impact that can be explained by demographic characteristics, neighborhood characteristics, and previous criminal justice history (see Supplementary Note B). Our conclusions are substantively similar when using non-linear

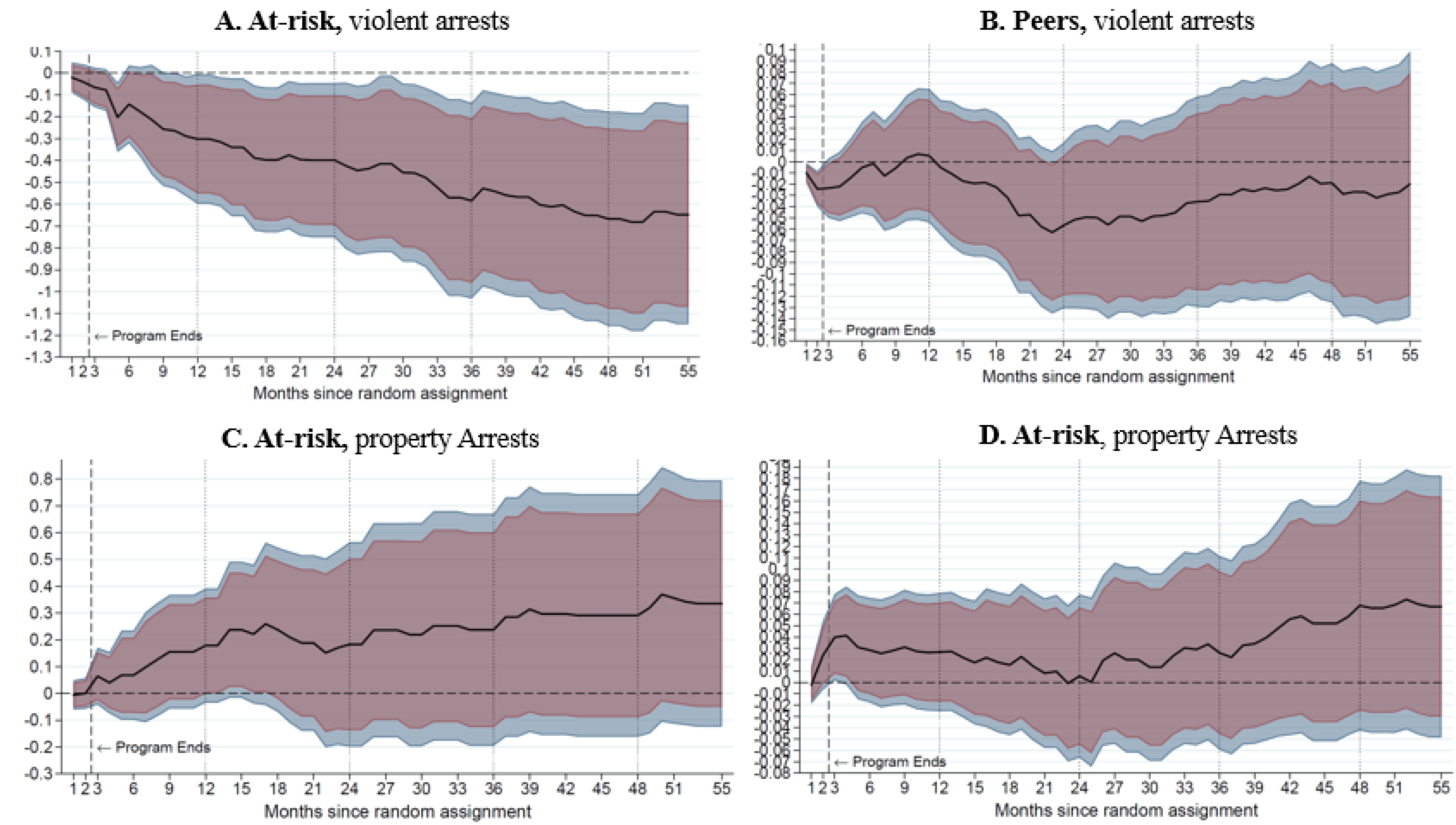

Figure 3.Time path of program impacts by month for at-risk youth versus peers. This figure shows the cumulative treatment effect in number of arrests from at-risk youth with a prior criminal justice history and their peers from random assignment in month zero through 55 months. The vertical dotted line 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).

specifications to estimate the program's impacts (see Supplementary Note C). We also present alternative estimates that are often stronger when we account for noncompliers among the treatment group (youth who won the lottery but did not take up the job offer) and the control group (youth who lost the lottery in 2015 but re-applied and won the lottery during the subsequent post-program follow-up period), as shown in Supplementary Note D.

Discussion

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 4,219 youth who applied to the Boston SYEP during the summer of 2015. Using a novel dataset that captures a 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 does not lead to lower criminal justice involvement across the entire sample of youth, but significantly lowers both the likelihood of ever being arrested and the number of arrests for certain subgroups of youth (e.g., males, 18-24 year olds, and those previously arrested) and certain types of crime (e.g., violent, property, and misdemeanor crimes).

Although these effects might seem small, in fact they represent a significant decline that is comparable to other interventions. For example, the -0.027 percentage point reduction in the arrest rate for males represents an 18 percent decrease from the control mean of 0.097. However, across all subgroups, the program has a larger impact on the number of arrests, rather than the incidence, with the total *number* of arrests dropping by 23 percent among males. This finding is consistent with other youth interventions that operate by increasing the presence and influence of protective factors (e.g., individual, family, peer, community) to reduce, but not eliminate, delinquent behaviors[26].

For the affected subgroups and types of crime, SYEP program impacts appear to accumulate over the next 4.5 years, 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 this persists even after the program has ended. Thus, it is likely that SYEPs do not simply incapacitate youth who are likely to engage in criminal activity but rather disrupt the frequency with which they do so. 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[5].

Examining which groups of participants are most likely to be affected can provide some clues as to how SYEPs achieve these long-lasting behavioral changes. Our analysis focused on subgroups of youth that prior research had identified as having benefitted in the short-term from participating in SYEPs across multiple cities. In particular, the number of violent crime arrests among at-risk youth who had been arrested prior to the program dropped by 79 percent, starting with a sharp decrease immediately after the program, that continued to accumulate through 4.5 years post-randomization with no sign of leveling off. Our at-risk sample of youth have characteristics that reflect those of hard-to-reach "opportunity youth"—youth 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. Summer jobs programs might provide a useful vehicle for reconnecting these youth with more productive experiences that offer skill building, adult mentors, and positive peer interactions during their active offending years.

Similar to other interventions, it appears that SYEPs differential impacts across types of crime which may depend on which underlying causes they address [20, 21, 22] . For example, violent crime involving conflicts between people stems from a combination of antisocial behavior [27] and thinking "fast" rather than slow[28], particularly among youth where impulse control is not as developed [29]. In contrast, property crimes that do not involve violence (e.g., burglary of a home rather than theft of a person) tend to reflect material needs, immediate wants, or thrill-seeking[30, 31]. SYEPs across multiple cities have consistently shown significant short-term reductions in youth violence that appear to persist for males, youth at or above the age of majority, and at-risk youth that have previously been arrested. While some groups also experience a decrease in property and drug crimes, it appears that summer jobs primarily reduce violent crime through improvements in social-emotional skills. This hypothesis is supported by pre- and post-program survey data showing that the reductions in violent crime are driven by self-reported improvements in managing emotions, resolving conflicts with a peer, and asking adults for help[5.]

However, understanding *how* SYEPs change youth behavior and what can be done to sustain program impacts has been less studied. We find that 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. While these results are suggestive, we acknowledge that we cannot attribute an entirely causal interpretation to these results for repeat participants since this is conditional on having applied for a second time, which may indicate a greater intrinsic motivation or ability. However, SYEP program data reveal that the group of repeat applicants is not as exclusive as one might imagine. Among the roughly 24 percent of eligible lottery winners from the first summer who choose to apply for a second time, youth 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.

Perhaps more insightful are the findings related to co-offending and peers, given that youth tend to commit delinquent acts as they live their lives – in groups[32]. Some observers suggest deviant peers socialize the acceptance of delinquent values and behaviors by otherwise non-offending youth [33, 34]. 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[35]. 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. Indeed, we find summer job programs reduce co-offending among both males and at-risk youth by roughly 30 percent. Similarly, it could also be the case that the newly acquired positive behaviors or prosocial ties formed by treated youth are spread to their former peers to ultimately reduce their offending as well. However, we do not find any significant impact of the Boston SYEP on the relative arrest rates or number of arrests for the pre-program co-offending peers of the treatment versus the control group.

The limitations of this study point to important directions for future research. First, even though 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. Second, our main estimates are likely biased downward given that roughly 3 percent of the control group (N=102) crosses over and wins the SYEP lottery in summer 2016, although this is a limitation that all longitudinal SYEP studies face and we address this as part of our robustness checks (see Supplementary Note D). Third, there is a higher degree of measurement error when using arrest versus other types of criminal justice (e.g., arraignment) data due to a lower quality match that can be obtained as names and date of birth are 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.

While programs differ in terms of the population to be served, the employment opportunities that exist, and the community supports that can be offered, there are a few useful insights that can be drawn to guide future municipal investments. First, the youth who benefit the most are those with the greatest need. Ensuring sufficient outreach to recruit males, older youth, and those with prior arrest records through diversion programs can be crucial if one of the goals of the program is to reduce youth violence. However, developing intentional job matching processes so that at-risk youth are placed within trauma informed workplaces will be needed to encourage take-up, prevent dropout and set youth up to succeed.

Second, positive peer interactions matter so targeting the program exclusively to those with the greatest need could actually reduce the program's effectiveness. It's likely that a mix of youth with different strengths and weaknesses is the most effective approach while also limiting any stigma associated with the program. 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 at-risk youth until there is a consistent decrease in criminal activity as evidenced by fewer arrests.

Third, offering a second dose of summer employment can enhance outcomes but may not be feasible from a budget standpoint given that programs already braid together funding from multiple sources to serve as many youth as possible and are still oversubscribed. That said, finding ways to link SYEPs to other year-round work-based learning such as co-ops and internships could be a viable way to help youth continue to sustain those post-program behavioral changes, particularly as youth continue to suffer COVID-related learning and developmental deficits.

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. 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[36].

Methods

Experimental Design

The Boston SYEP is open to all city residents aged 14 to 24 years regardless of socioeconomic, school enrollment, or court-involved status, and youth typically apply through one of the four intermediary organizations under contract with the Boston Mayor's Office of Workforce Development (OWD). The intermediaries 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 summer 2015 through Action for Boston Community Development (ABCD), a large and established nonprofit that works in 18 of Boston's 23 neighborhoods and serves a predominately young, school-aged, and low-income population (See Supplementary Note A). We focus on ABCD because it is one of the two intermediaries that make use of random assignment due to the high number of applications it receives for the limited number of SYEP jobs that are available. The study design and analysis plan were preregistered on Open Science Framework (OSF) on September 10, 2022 [https://osf.io/et376/].

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 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. 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 and only 3.5 percent subsequently participating in the Boston SYEP through ABCD in later years (2016, 2017, or 2018).

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 in the treatment group 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 capture more frequent and less serious interactions with the criminal justice system than the state arraignment records which measure criminal activity only to the extent that an individual was arrested, booked, and appeared before a judge.

Using the BPD arrest data allows us to compare our results directly with prior studies evaluating outcomes from other cities such as Chicago and New York. The arrest data contain information on each criminal charge including the arrest date, the seriousness of the crime (e.g., misdemeanor or felony), and a literal description of the crime that can be used to create categories by type (e.g., violent, property, drug, gun, and other)[37]. Under a data use agreement with the BPD, we also obtained key demographic information including the subject's name, race, date of birth, and address of residence to match with SYEP program records.

Using the FIO data allows us to document more informal criminal or suspicious activity than arrests that occur at a higher frequency. 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 observes an individual and needs to document that observation for intelligence purposes [38]. For example, an officer may record an observation of a known gang member loitering in a high drug activity area. Under a separate data use agreement with the BPD, we also obtained key demographic information from the FIO reports including the subject's name, race, date of birth, and address of residence to match with SYEP program records.

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 5 percent of the individuals with an FIO accounting for more than 40 percent of total FIO reports[24]. We account for these spatial differences in FIO encounters by including neighborhood Census tract characteristics including 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.

Measuring Outcomes

The primary outcome 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. 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 outcome, 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, all encounters after 2016 contain detailed characteristics of encounters, including their location, demographic information about the individuals involved (e.g., race, gender, age), and a brief statement about the encounter. 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, similar to the subgroup of interest constructed by Kessler et al[3] using observable characteristics to predict at-risk youth.

Linking BPD and SYEP Data

We 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 were matched to the BPD data, indicating which youth had been involved in some type of activity that resulted in either an observation or an arrest by law enforcement. See Supplementary Note A for more details and descriptive tables showing baseline criminal justice outcomes for youth in the treatment and control groups as well as their peers.

Model

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 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 Supplementary Note C).

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 impact on violent crime arrests including both the number of violent crime arrests per youth and whether an individual 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[39], we also use a regression framework to control for individual characteristics and improve the precision of our estimates using the following equation:

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

where Y_{it} is the criminal justice outcome, $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, $X_{i(t-1)}$ is a set of preexisting neighborhood characteristics, demographics, and baseline criminal justice outcomes and μ_{it} is a stochastic error term. The set of pre-program demographic covariates is the same as prior studies[5] which includes age, gender, race, language spoken at home, homeless, receiving public assistance, and disabled. The baseline criminal justice outcomes correspond to the pre-program criminal justice characteristics that reflect the outcome being estimated (e.g. we control for having ever been arrested for a violent crime during the pre-period when estimating the program's impacts on violent crime).

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, and (3) co-offending behavior and spillover peer 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, or youth with a prior violent crime arrest relative to the full treatment group. This is because SYEPs do not change the household, neighborhood, or school environment of participating youth—contextual factors that are also important in explaining 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 arrested are at higher risk for being arrested in the future [40].

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 could reveal whether participating in the SYEP disrupts some of the activities that youth are involved in during the summer months to the point where it also reduces the frequency to engage in delinquent behavior even after the program has ended. Looking at a second consecutive summer of participation provides us with the opportunity to study whether receiving a second SYEP "dose" yields 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 peers. It could be the case that the program reduces the likelihood that individuals in the treatment group are involved in co-offending events (either through arrests or FIOs) during the follow-up period which might provide some insight into how the programs impacts are achieved (e.g., youth are perhaps less subject to negative peer interactions). Conversely, it could be the case that the positive behaviors of treated persons are spread to peers and/or that the prosocial ties formed by individuals in the summer jobs program are then leveraged or shared to their peers to ultimately reduce peer offending.

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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 age 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)
Percemt 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	

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.; statistcical significance is indicated by $*$ p < 0.1, $**$ p < 0.05, and $***$ p < 0.01.

Table 2. Cumulative five-year 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**		**At-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

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 3. Cumulative five-year ITT program impact on youth with 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

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

Table 4. Cumulative five-year 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**	**At-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

FIO, field, interrogation, and observation; CM, control mean; 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 5. Cumulative five-year 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 (0.032)	[1.089]	-0.128 (0.142)
Violent arrests	[0.184]	0.031 (0.032)	[0.337]	-0.028 (0.061)
Property arrests	[0.145]	0.015 (0.029)	[0.232]	0.052 (0.064)
Drug arrests	[0.085]	0.004 (0.025)	[0.133]	-0.025 (0.038)
Felony arrests	[0.275]	0.016 (0.034)	[0.648]	-0.078 (0.103)
Misdemeanor arrests	[0.224]	-0.006 (0.033)	[0.441]	-0.093 (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

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

Supplementary Information

A. Data Collection and Variable Construction

1. Data Collection

We rely on a lottery assignment conducted for youth who applied to the Boston 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 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).

Moreover, 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

We manipulate the administrative program data in the following way to create the independent variables that are used as covariates.

Independent Variables – Pre-Program Demographics
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

We manipulate the administrative data in the following way to create measured outcomes to be used as the dependent variables in our analysis.

Dependent Variables – Primary Outcomes

Number of Arrests – we calculate the number of times that the individual was arrested as captured 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 count the number of post-program arrests since the program lottery notification date of June 2015.

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.

Dependent Variables – Secondary Outcomes

Number of Co-Offending Arrests - we calculate the number of times that the individual was arrested for group-related criminal offending as captured 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 (as opposed to crimes likely to be carried out in isolation) violent crime offending includes assault, homicide, fatal and non-fatal shootings, robbery, and threats. 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 count the number of post-program arrests since the program lottery notification date of June 2015.

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 captured by the Boston Police Department Field Interrogation and Observation database. We then count the number of pre-program FIO contacts during the year prior to participation in the program to serve as a baseline control. We count the number of post-program FIO contacts since the program lottery notification date of June 2015.

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.

Spillovers/Peer Effects

Criminal Justice Peers – we identify pairs of individuals as peers if they are observed engaging in criminal activity together as captured 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 their pre-program criminal justice history. 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 come from arrest records which 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

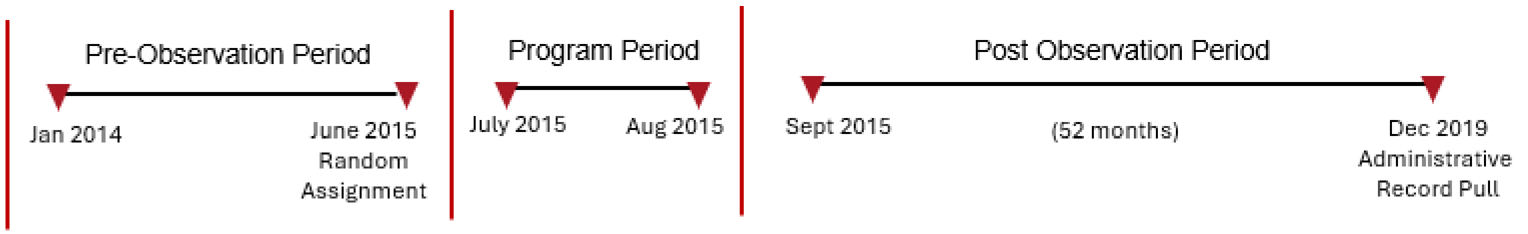


Table A1. Distribution of SYEP applicants across Boston neighborhoods by lottery outcome

	(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%

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%
Among those Age 14-17 years:			
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%

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	

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	

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	

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

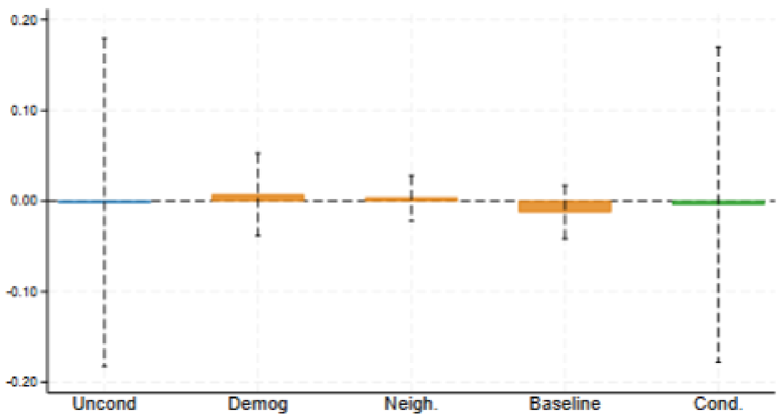
To assess the impact of the Boston SYEP on criminal justice outcomes, we compare criminal records during the period following the intervention for youth offered SYEP placements (the treatment group) with the records for youth not offered placements (control group). We measure two primary outcomes of 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 included in the main regressions are neighborhood characteristics associated with the Census tract that the individual resides in to account for spatial differences including 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 collected during the application process include age, gender, race/ethnicity, limited English, in school, public assistance, and homelessness. We also include baseline criminal justice outcomes captured by the administrative data during the pre-program period. To determine the number of baseline arraignments, pre-program is defined as the 17 months prior to random assignment (January 2014 through June 2015). To determine if an individual has ever been charged with a crime, the pre-program period is defined as any time prior to July 2015.

We sequentially add in each group of 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 represent 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

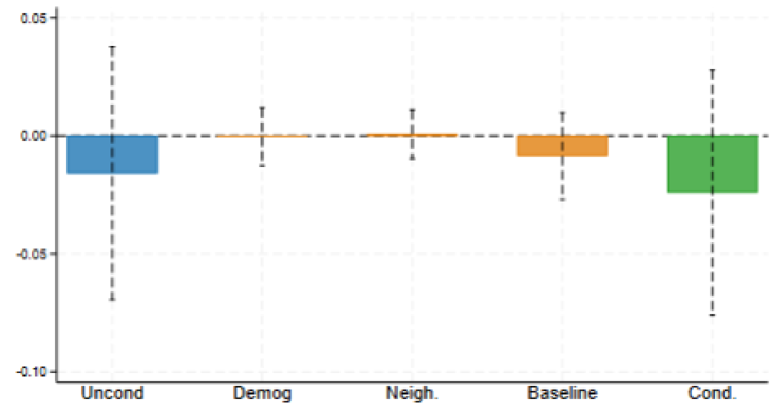


Table B.1. Cumulative five-year ITT program effect on incidence for all SYEP youth, adding controls

	(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

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 five-year 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

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 Gauss-Markov assumptions, we also explore the robustness of the results to alternative assumptions. Specifically, I relax the linear functional form assumption by using non-linear specifications. To analyze treatment-control differences in the number of arrests—a count variable—we use a Poisson quasimaximum 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 five-year 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

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 five-year 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

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):

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

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

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. 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 the treatment group 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 the treatment group.

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 five-year 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	At-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

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 five-year 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**	**At-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

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 five-year 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**	**At-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

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 five-year 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**	**At-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

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 five-year 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

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