

Nothing Stops a Bullet like a Job? The Long-Term Impact of Summer Youth Employment Programs on Criminal Justice Involvement¹

Alicia Sasser Modestino², Aria Golestani³, Brandon Turchan⁴, Sebastian
Ramirez⁵, Benjamin Struhl⁶, and Anthony A. Braga⁷

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 among males, older youth and those with a prior arrest, suggesting SYEPs produce safer communities by providing jobs to youth who are more likely to have contact with police. 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.

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¹Alicia Modestino is a Professor of Economics and Public Policy in the School of Public Policy and Urban Affairs at : Northeastern University. Email: a.modestino@northeastern.edu.

²Aria Golestani is a Criminal Justice Innovation Fellow at the Social-Science Research Council. Email: ariagolestani.io@gmail.com.

³Brandon Turchan is an Assistant Professor in the School of Criminal Justice at Michigan State University. Email: turchanb@msu.edu.

⁴Sebastian Ramirez is a doctoral student in the School of Public Policy and Urban Affairs at Northeastern University. Email: ramirezramirez.d@northeastern.edu.

⁵Ben Struhl is the Executive Director of the Crime and Justice Policy Lab at the University of Pennsylvania. Email: bstruhl@sas.upenn.edu.

⁶Anthony 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 note that an Online Appendix has been included.

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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).

1. 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 solutions to address violence, especially among youth (Council of State Governments 2021). In response, the Biden administration urged cities and states to use their American Rescue Plan Act (ARPA) funding to start or expand Summer Youth Employment Programs (Biden 2021) with the hope that "nothing stops a bullet like a job" (Dickerson 2010). Since then, many jurisdictions have increased summer job opportunities for young people, aiming to make their communities safer during both the summer months and in the longer run (American Youth Policy Forum 2021).

These SYEP program expansions were based on a growing body of research evidence across multiple cities consistently showing that SYEPs reduced youth violence—at least in the short-term (Li and Jackson-Spieker, 2022). For example, compared to a control group of similarly situated youth, SYEP participants in Boston and Chicago were shown to commit 35 to 40 percent fewer violent crimes up to 18 months after the program ended (Heller 2014; Heller 2022; Modestino 2019). Similarly, in New York, youth were 23 percent less likely to be arrested for a felony during the summer in which they were randomly selected to work (Kessler et al. 2022). Yet these studies stand in contrast to the adult literature which suggests that while workforce development policies can reduce property crime by alleviating economic hardship, they typically have little impact on reducing violent crime (Ludwig and Schnepel, 2024).

Thus, it seems reasonable to ask whether the reduction in youth violence observed among SYEP participants can be sustained in the longer-term, especially when compared to less costly alternative interventions. With average program expenses of roughly \$2,500 per youth in wages alone each summer, many cities may be hard-pressed to maintain their pandemic-era SYEP expansions once their ARPA funding expires.¹ Moreover, bipartisan bills to support SYEPs with federal funding have consistently failed to gain any traction over the past several years (U.S.

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).

Congress, 2021, 2023, and 2025). In contrast, the “Becoming a Man” program, which now operates in eight cities, has been shown to achieve similar reductions in violence but with a lower financial and administrative burden than SYEPs which often entail placing thousands of youth across hundreds of local employers every summer (Heller et al., 2017). Thus, understanding the long-term impacts of SYEP participation on criminal justice involvement, and youth violence in particular, can help policymakers weigh the potential costs and benefits of maintaining their recent SYEP investments (Li and Jackson-Spieker 2022).

Even during periods of generous federal assistance, most SYEPs are oversubscribed such that cities often must allocate jobs via lottery or on a first-come first-served basis (Heller and Kessler 2017). This creates a stark tradeoff between serving as many youth as possible versus a more tailored approach that targets youth who are at high risk of engaging in violence. For example, some programs limit the percentage of total youth that can participate for more than one summer and/or reserve a certain percentage of summer jobs for high-risk youth in an effort to maximize their program’s impact.² Thus, knowing which types of youth benefit the most in the long run from having a summer job and whether multiple summers of participation can help sustain program impacts can inform cities that are aiming to reduce youth violence how to make use of their limited resources (Li and Jackson-Spieker 2022).

However, much less is known about the *long-term effects* of SYEP participation on criminal justice involvement among youth, and violent crime in particular. An earlier study by Gelber et al. (2016) documented that participants in the New York City SYEP were 10 percent less likely to be incarcerated compared to the control group up to five years after having a summer job. However, the authors note that the exclusion of youthful offenders was a particularly important data limitation in their context, because most SYEP applicants were 18 or younger at the time of SYEP application, resulting in a particularly low incidence for estimating impacts (roughly one percent of applicants were ever incarcerated). In a follow-up study using arrest data, Kessler et al. (2022) found that high-risk youth who had been arrested prior to the program were 10 percent less likely

² For example, in Massachusetts the state’s YouthWorks program serves young adults between the ages of 14-25 whose household income does not exceed the annual equivalent of 200% of the federal poverty guidelines and also meet at least one of 11 demographic risk factors which include “Involved with the justice system.” In the past, state guidelines have also prioritized new applicants, capping total statewide or local program repeat participants at around 20% of enrollment to maximize new access, though rules can vary by regional funding allocation. See <https://www.massshareberkshirecc.com/youth/youth-works/> for details.

to be arrested for a felony up to five years later, although the effect was not statistically significant. However, that study was again limited to youth who were already at the age of majority prior to participating in the program due to only having access to adult (not juvenile) criminal justice records. Moreover, neither of these prior studies was able to distinguish between violent and other types of crimes. Thus, the existing estimates may understate SYEP impacts on violent crime, particularly if summer jobs have greater impacts on the behavior of juveniles.

Moreover, even less is known about the *mechanisms* through which summer jobs programs reduce criminal justice involvement. Understanding key program elements could provide insight into how to sustain the program's documented short-run impacts on youth violence. Although prior research suggests that SYEPs reduce violent crime by improving soft skills and community engagement during the summer (Modestino 2019), it is still unclear how those short-term outcomes are achieved or how long they can be sustained. Given that 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), knowing how SYEPs might achieve longer-lasting effects through the transition into adulthood is important, especially for cities aiming to target their programs.

Indeed, a summer job can be transformational for youth who are at greater risk for criminal justice involvement (Mortimer 2010), in part due to the salient influence of *peers* in the etiology of delinquency (Haynie 2001; Haynie 2002; McGloin and Thomas 2019). Most studies find that 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). Thus, understanding whether the reduction in youth violence is achieved by disrupting these prior co-offending networks of peers could yield insights into how SYEPs could be used to not only achieve impressive reductions in criminal justice involvement among participants— but also to create possibly beneficial spillover effects on the youth with whom they had previously co-offended with. For example, if repeat participation during a second consecutive summer is needed to sustain positive behavioral changes and new peer relationships, then targeting the program to ensure that high-risk youth receive a second summer of participation could be more efficient and effective than using a simple lottery design each summer.

Our study builds on and broadens the scope of the prior literature through both improved measurement of the long-term impacts of SYEPs on criminal justice outcomes and better identification of the underlying mechanisms related to peers and dosage effects. In terms of measurement, our analysis makes use of both juvenile and adult 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 to, and up to 4.5 years after, SYEP participation for both juvenile and adult participants by type of arrest (for example, violent crime arrests) and at a more granular level (including FIO reports) than prior studies. To examine the underlying mechanisms, we also compare the trend in arrests versus FIO reports for the treatment and control group reflecting changes in formal versus informal police contacts over time. The FIO data also allows us to identify prior co-offending “peers” of both the treatment and control group who did not apply to the program. We then explore whether the reduction in criminal justice activity stems from less co-offending by SYEP participants and whether that change has an impact on the outcomes of their prior “peers.” Finally, we make use of the 2016 program lottery to assess whether randomly receiving a second SYEP “dose” during the following summer can help sustain and/or enhance the program’s impacts on youth violence for the longer-term, particularly for high-risk youth with a prior violent crime arrest.

We find that being offered an SYEP job leads to lower criminal justice involvement for up to 4.5 years, but only among subgroups who are more likely to come into contact with police. This includes youth identifying as male who experience an 18 percent decrease in the likelihood of *ever* being arrested post-program and a 23 percent decrease in the *number* of arrests. This relative improvement primarily reflects a large drop in both the incidence and number of less serious crimes (misdemeanors). In terms of type, the most consistent long-term impacts are on violent crime arrests which fall by 28 percent among males, with 18-24 year olds and high-risk youth (those with a prior violent-crime arrest) experiencing even larger reductions of 53 percent and 79 percent respectively during the full follow-up period. Delving further reveals that the program reduces co-offending among high-risk youth by 30 percent but has no spillover effects on the outcomes of their prior co-offending peers outside of the SYEP program. Finally, youth who applied for and were randomly selected to participate for a second summer further experienced significant decreases in the number of property and drug arrests.

Our results provide important insights into SYEP design and implementation that are relevant to effective youth violence reduction policy and practice. Although prior studies have shown that SYEPs reduce violent crime among the general population of youth in the short run, we show that the long-term impacts appear to be concentrated among males, older youth age 18-24 years, and especially youth that are at high-risk of repeat violence. While it might be tempting to conclude that targeting the program heavily towards these groups would further improve public safety at the community level, the peer effects suggest that having a mix of youth in the program potentially promotes new relationships that may help sustain violence reduction. Indeed, among the group of high-risk youth, it may be that the reduction in violence is driven by the program disrupting their prior co-offending networks of peers, although more qualitative research is needed to confirm this channel and uncover how it occurs. Taken together, these findings suggest that cities seeking to maximize the program's impacts on violence reduction may want to reserve a meaningful, yet limited, proportion of summer jobs for high-risk youth to participate for at least one, and/or perhaps for a second, dose of participation.

2. 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, youth participants work 20 to 25 hours per week and are paid the prevailing Massachusetts minimum wage. Youth also receive 20 hours of workforce development training, which includes evaluating personal strengths and career interests; practicing social-emotional learning (SEL) such as communication, collaboration, and conflict resolution; and developing 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 their socioeconomic, school enrollment, or court-involved status. Youth typically apply through one of the four intermediary organizations under contract with the Boston Mayor's Office of Workforce Development (OWD) that are responsible for reviewing applications, matching applicants with jobs, supervising job placements, and delivering the workforce development 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 school-aged, low-income population.³

Our experimental design relied on ABCD because it was the intermediary that used a lottery system during the summer of 2015 to assign participation due to the high number of applications it received for the limited number of SYEP jobs that were available. Youth applied from late February through early June after which ABCD used 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 was determined by the amount of funding available that year. This simple randomization method assigned the offer to participate in the program at random, creating a robust control group of youth who applied to the SYEP but were not chosen. Thus, we were able to control for both the initial selection of participants into the program as well as any behavioral changes that youth would typically undergo during the summer as part of the normal course of adolescent development.

The program's initial take-up and participation rates were very high while the cross-over between the control and treatment groups was quite limited. Of the 4,219 youth who applied to ABCD in 2015, a total of 1,186 (or 28 percent) were notified of their job assignment in late June, leaving 3,033 youth 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 was little crossover, with only 3.0 percent of the control group obtaining a job through one of the other three intermediaries during the summer of 2015.⁴ However, over the full study period, upwards of 20 percent of the control group subsequently participated in the Boston SYEP through one or more intermediaries, including 3.5 percent that participated through ABCD.⁵

³ 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.

⁴ Administrative wage record data confirm that only one-quarter of youth in the control group had found a job on their own during the summer of 2015. Among youth in the control group who did find a job, they worked 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 and intensity of employment during the summer of 2015.

⁵ Tables D3 and D4 in the online appendix show that our estimates of the program's impacts are even larger when we account for youth in the initial control group eventually receiving treatment in a subsequent summer.

3. Data

3.1. Criminal Justice data

To assess the impact of the Boston SYEP on criminal justice outcomes, we used both arrest and FIO data obtained from the Boston Police Department (BPD) for the period 2014 through 2019. Both the arrest and FIO data were matched to the City of Boston's SYEP program records using each subject's name, date of birth, and address of residence as well as key demographic information (for example, gender, race, ethnicity). These administrative data enabled us to avoid some common issues with self-reported data such as social desirability bias, which might affect the willingness of youth 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 behavioral changes related to interactions with the criminal justice system with greater frequency than other administrative records, such as court arraignments or convictions, which can only measure criminal activity that leads to an individual being arrested, booked, and appearing before a judge.⁶

Moreover, each BPD data source offered different advantages for studying program outcomes. The BPD arrest data allowed us to compare our estimates directly with prior studies evaluating similar long-term outcomes from other cities such as New York (Kessler et al. 2022). The arrest data also contained 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 we used to create categories by type of crime (for example, violent crimes versus property, drug, gun, and other crimes).

The FIO data allowed us to also document more informal criminal or suspicious activity that occurred at an even higher frequency than arrests and was particularly useful in identifying prior co-offending peers that did not apply to the ABCD program in 2015. This is because the FIO records documented a wide range of criminal justice behaviors including not only when an individual is stopped and frisked, but also when an officer engaged in a consensual encounter with an individual or when an officer simply observed an individual and needed to document that

⁶ Although for minor youth it is likely that the arrest data closely match the arraignment data since police cannot hold a youth in secure police custody for longer than six hours for booking and identification. Within that window, the juvenile must be released to parents, taken to juvenile court, or transferred to the Department of Youth Services Overnight Arrest Program if court is closed. See <https://archives.lib.state.ma.us/server/api/core/bitstreams/cfc64a0f-19e8-4c9f-b47c-cdde67ddab0d/content> for more information.

observation for intelligence purposes.⁷ For example, an officer may have recorded an observation of a known gang member loitering in a high drug activity area, along with the names of other known individuals who were present at the time.

These data do have some limitations, however. Police decision-making practices introduce some bias in who gets arrested or stopped, and official records capture only a fraction of actual behavioral patterns (Black 1970). Nonetheless, studies that have compared victim reports of the demographics of offenders with those of arrestees find the two tend to be closely related (e.g., see Hindelang 1978). Another caveat to keep in mind is that arrest and FIO records reflect the interaction of both criminal and police behavior. For example, the BPD often uses FIO reports to direct officer deployments in crime "hot spots" and to focus law enforcement attention on active offenders (Fagan et al. 2016). This is because criminal activity, particularly violence, is highly concentrated within certain neighborhoods, with only 5 percent of the individuals that have any FIO report accounting for more than 40 percent of the total number of FIO reports. To control for these spatial differences, we included co-variates for 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 were cost burdened (defined as spending more than 30 percent of their income on rent).

3.2. Measuring Outcomes

Our primary set of outcomes constructed from the BPD data include the incidence and number of arrests by type of crime. We categorized the type of 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).⁸ For example, CJARS classifies as "violent" any crime against a person such as assault, homicide, fatal and non-fatal shootings, sexual offenses, robbery, threats, kidnapping, and arson (when someone is known to be home). Property crimes are defined as nonviolent offenses such as forgery, burglary, larceny, motor vehicle theft, shoplifting, and destruction of property while drug crimes

⁷ 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>.

⁸ 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/>.

include both possession and dealing.

As a second set of outcomes, we also measured the incidence and number of FIOs. Prior to 2016, the FIO data only captured basic information for each contact between the BPD and one or more individuals involved in these contacts. These data allowed us to identify the prior co-offending peers of youth in the treatment and control groups who did *not* apply for a summer job in 2015. As of 2016, the BPD expanded the FIO reports to include more 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 itself. We used this expanded dataset to construct a set of covariates which we use when estimating the criminal justice outcomes of those prior co-offending peers during the post-program period.

We linked the SYEP program records to both the 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 during the full observation period (January 2014 through December 2019), indicating these youth had been involved in some type of activity that resulted in an arrest or FIO report by law enforcement at some point. We used 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 a subset of those youth with a prior arrest were defined as "high-risk."⁹ We then tracked 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) to test whether the program's impacts were reversed, sustained, or continued to accumulate over time.¹⁰

4. Empirical Methodology

To assess the impact of the Boston SYEP on long-term criminal justice outcomes, we compared 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 youth who were not offered a placement

⁹ Our "high-risk" subgroup is defined similarly to that constructed by Kessler et al. (2022) but with a slightly higher proportion of youth (three percent) than in that prior study (two percent), likely because of the inclusion of juvenile arrest records. Our "at-risk" subgroup is completely novel and consists of roughly 6 percent of our sample, likely due to the ability to track less formal contacts with police. See Table A3 in the online appendix

¹⁰ Roughly 6 percent of both the treatment and control groups were identified as "at-risk" while about 3 percent of both groups were identified as "high-risk." See Section A in the online appendix for descriptive tables showing baseline criminal justice outcomes for youth in the treatment and control groups as well as their peers.

(control group). Because SYEP participation was allocated via lottery, we obtained causal estimates using a simple comparison of means on the outcome of interest. This “Intent to Treat” (ITT) estimate measured the impact of the SYEP *offer* which accounts for both program selection and take-up to assess whether SYEPs can effectively reduce violence among the pool of applicants, not just those who choose to participate. Nonetheless, because not all selected youth accepted the offer, the ITT estimate may understate the effects of the program on participants, so we also estimated treatment-on-the-treated (TOT) impacts.¹¹

We measured 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 was the program’s impact on violent crime arrests including both the prevalence (whether an individual has ever been arrested for a violent crime) as well as the intensity (the number of violent crime arrests per youth). We examined these outcomes in levels as well as relative to other types of crime to account for shifting behavior across types of criminal activity. Note that although the inclusion of covariates is not necessary to derive unbiased impact estimates when treatment is randomly assigned (Bloom 2006), we also used a regression framework to control for individual and neighborhood characteristics to improve the precision of our estimates using the following equation:¹²

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

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 was the same as in prior Boston SYEP studies including age, gender, race, and language spoken at home, as well as housing, public assistance, and disability status (Modestino 2019). In each specification, the baseline criminal justice outcomes corresponded to the outcome that was being estimated. For example, we controlled for having ever been arrested for a violent crime during the pre-period when estimating impacts on violent crime in the post-period.

¹¹ TOT estimates using a two-stage-least-squares method can be found in D1 and D2 in the online appendix.

¹² We also use alternative estimation 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.

Our secondary outcomes focused on two main analyses: (1) studying differential program impacts by subgroups, and (2) examining behavioral mechanisms that may sustain SYEP impacts in the longer-term. For our subgroup analysis, we identified the relevant sub-groups of interest based on pre-existing contextual factors that have been shown to be correlated with criminal activity. For example, youth who identify their gender as male typically offend at a higher rate than those that identify as female. Similarly, older youth tend to be more likely to commit crimes because they have less supervision and more opportunity than younger youth—especially if they can drive and are no longer in school. Finally, youth who have previously been in contact with police are at higher risk of being arrested than those who have no prior police contact (Cottle, Lee, and Heilbrun 2001). We estimate program effects by subgroups using equation (2):

$$(2) \quad Y_{it} = SYEP_i \pi_2 + GROUP_i \pi_3 + SYEP_i * GROUP_i \pi_4 + X_{i(t-1)} \beta_1 + \mu_{it}$$

where π_4 is the coefficient of interest which captures the separate impact of being treated on a particular subgroup. We also include Holm-adjusted p -values across arrest outcomes within subgroups to account for multiple hypothesis testing.

To explore the behavioral mechanisms of SYEPs, we studied the dynamics of the program's impact in three ways. First, we examined the time path of criminal justice involvement to determine whether participating in the SYEP reduces the frequency with which youth engage in delinquent behavior even after the program has ended, and whether this impact was reversed, sustained, or continued to grow over time. We estimate those impacts by criminal justice involvement (for example, formal arrests versus informal FIO reports) and type of crime for different subgroups, including the set of prior co-offending peers.

Second, we estimated the impact of receiving a second consecutive summer of participation to study whether receiving an additional SYEP “dose” yielded benefits that were sustained longer or even enhanced. To do this, we restrict the sample to youth in the treated in 2015 and estimate the impact of being randomly selected for summer job in 2016 using equation (3):

$$(3) \quad Y_{it} = SYEP_{i,2016} \pi_5 + X_{i(t-1)} \beta_1 + \mu_{it}$$

where π_5 is the coefficient of interest which captures the impact of being treated for a second consecutive summer. We also present estimates where we further restricted the sample to only treated youth who applied in 2016 to account for selection into the application process.

Third, we tested whether the Boston SYEP had an impact on co-offending among treated youth and whether changes in this behavior had any spillover effects on their prior peers. It may be that

the program reduced the likelihood that individuals in the treatment group were involved in co-offending events during the follow-up period (either arrests or FIOs) which might provide some insight into how program impacts can be sustained. For example, SYEP participants may either have less time to participate in co-offending activities and/or form new pro-social ties through the program and subsequently be less subject to negative peer interactions that involve co-offending. We estimate these impacts using equations (1) and (2) where the dependent variable is now an indicator for co-offending with any other individuals during the post-period.

However, any changes in co-offending among the treatment group associated with participating in the program might also affect the behavior of their prior co-offending peers as well. Here we define peers as individuals, aged 14 to 30 years, who did not apply to ABCD during the summer of 2015, but with whom an SYEP applicant (treatment or control) was either arrested or included in an FIO report prior to the program.¹³ For example, less co-offending among the treatment group could reduce the incidence or frequency of offending among their group of prior peers who did not apply to the program. 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.¹⁴ We estimate the post-program impacts on a sample of prior co-offending peers of youth in the treatment group, using equation (4):

$$(4) \quad Y_{it} = PEER_TREATED_i \pi_6 + X_{i(t-1)} \beta_1 + \mu_{it}$$

where Y_{it} is the criminal justice outcome, $PEER_TREATED_i$ is a dummy variable indicating that the individual is a peer of at least one youth in the treatment group, $X_{i(t-1)}$ is a set of demographics,

¹³ Tables A4 and A5 in the online appendix show that the prior co-offending peers of the treatment and control groups were balanced in terms of the demographic characteristics and prior criminal justice history that were obtained from the BPD data. We excluded incidents involving more than 10 people since those tend to be large gatherings of youth where it was unclear whether the individuals are intentionally co-offending as “peers” (for example, underage drinking at a party). We also excluded any peers who overlapped with individuals in both the SYEP treatment and control groups since it was unclear which “peer” effect would be more impactful. However, we also obtain similar results without these restrictions.

¹⁴ 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.

baseline criminal justice outcomes and μ_{it} is a stochastic error term. The coefficient of interest is π_6 which captures the impact of the program on individuals who previously had been arrested or observed in an FIO report with a youth in the SYEP treatment group but had not applied to the program during the summer of 2015.

5. Results

Table 1 provides descriptive statistics for the preexisting demographic 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 lower percentage of applicants identified their gender as male, just over 50 percent were African American, and over 95 percent indicated that their preferred language was English. 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.

[Insert Table 1 Here]

Based on these observable characteristics, youth selected by ABCD's lottery system 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 amount of variation would be expected by random chance across these 12 different characteristics. Baseline criminal justice activity in the 17 months leading up to the program was also balanced across the treatment and control groups with roughly 6 percent ever having been arrested or reported in the FIO data prior to the program. Youth also had similar rates of incidence and numbers of arrests across all types of crime, the seriousness of the offenses (for example, misdemeanor versus felony), and their co-offending behavior prior to the start of the program.¹⁵

5.1. Estimating Long-Term Program Impacts

¹⁵ See Table A.3 in the online appendix for a comparison of baseline criminal justice activity prior to the program across the treatment and control groups.

Table 2 reports the ITT estimates from equation (1) showing the program’s impact on arrests by youth subgroups. We find that being offered a job through the Boston SYEP led to lower criminal justice involvement over the long-term, but only among subgroups who were more likely to come into contact with police. For example, youth identifying as male experienced 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 0.150) and a 23 percent decrease in the *number* of total arrests (a decrease of -0.075 from a control group mean of 0.331).¹⁶ This relative improvement primarily reflected a large drop in both the incidence and number of less serious crimes (misdemeanors), although older youth also experienced 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 of the program’s impact in terms of total arrests comes from youth who were at the age of majority when participating in the program (age 18 and older in Massachusetts). Similar to Kessler et al. (2022), we find no statistically significant reductions in the overall incidence or total number of arrests for our “at-risk” or “high-risk” groups.¹⁷

[Insert Table 2 Here]

However, with our more refined dataset we were able to extend this prior literature to explore the Boston SYEP’s long-term impacts by type of crime, potentially revealing some insights into the program’s mechanisms. 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 whose 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 from a store) tend to reflect material needs, immediate wants, or thrill-seeking (Wright and Decker 1994; Clarke and Cornish 1986).

¹⁶ These effects are measured in number of arrests per 1,000 youth, so 0.331 represents 331 arrests per 1,000 youth.

¹⁷ This finding is consistent with prior studies of the Boston SYEP (and other cities that found no reduction in the overall incidence or total number of incidents when using arraignment data (Modestino 2019).

In terms of intensity (number of arrests), the Boston SYEP reduced both violent and property arrests, 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 youth who identified as male, there was a 27 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 previously observed at only 18 months post-program (Modestino 2019). Males also exhibited a 48 percent reduction in property crimes in the longer-term, exceeding the 29 percent reduction in property crime arraignments that had been observed in the short-term (Modestino 2019). Thus, we cannot rule out either social emotional learning or meeting material needs as a channel by which SYEPs reduce criminal justice involvement. Interestingly, there appears to be no significant decrease in drug arrests, although those occur with a much lower frequency among Boston SYEP applicants.

It is note-worthy that the reduction in violence was largely driven by youth who had been arrested prior to the program. These high-risk youth experienced even larger reductions in violent crime arrests of 79 percent during the full 4.5 year 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.

5.2. Testing Program Mechanisms

With this unique dataset, our secondary goal was to better understand the mechanisms by which the Boston SYEP sustains these impacts in the longer-term. We examine three potential channels: the timing of behavioral changes, the impact of a second consecutive summer of SYEP participation, and the role of co-offending with prior peers who did not apply to the program.

5.2.1. Timing of Program Impacts

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 could be that SYEPs 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 in panels A and B 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 both arrests and FIO reports demonstrates that most of the reduction in criminal activity accrued *after* the program ended (month two), consistent with prior studies (Heller 2014; Kesler et al. 2022; Modestino 2019). The reduction in arrests steadily accumulated over time, with sharper drops at months 15 and 27—possibly leveling off during the school year before an additional dose of SYEP—after which the long-term impacts were statistically significant for the remainder of the post-program observation period.¹⁸ In contrast, the time path of informal police contacts, as measured by the FIO reports, did not start to decline until 12 months after the program ended. Other estimates reveal that the number of FIO reports appeared to *increase* for high-risk youth despite a significant decrease in violent crime arrests, perhaps because they remained under BPD surveillance—either simply as a prior known suspect or because they were still actively observed in the FIO reports along with their prior co-offending “peers.”¹⁹

[Insert Figure 1 Here]

5.2.2 Dosage Effects: Receiving a Second Consecutive Summer of Participation

We next explored whether a second consecutive summer of participation can help sustain and/or enhance the program’s impacts. Using equation (3), we limited the sample to all youth in the treatment group—those who initially won the SYEP lottery in 2015—and estimated the marginal effect of a second summer of treatment in 2016 on long-term outcomes as shown in Table 3. While there did not appear to be any further significant decrease in the *incidence* of ever being arrested, the reduction in the *number* of total arrests for youth in the treatment group who received a second

¹⁸ Figure E1 shows that the reduction in misdemeanors and property arrests accelerates between months 18 and 36, dropping off even more sharply before level off through the remainder of the follow-up period.

¹⁹ See Figure E.2. for the time path of arrests versus FIOs for males versus high-risk youth.

summer was twice as large (47 percent) as the effect experienced by youth receiving just one summer of participation. The same was true for violent arrests—a second dose of participation did not further decrease the likelihood of *ever* being arrested for a violent crime but reduced the *number* of violent crime arrests by 58 percent. Moreover, there was also a large and significant reduction in both the incidence and number of both property and drug arrests among youth having participated for a second summer. Perhaps the consistency of maintaining employment from one summer to the next is needed to alleviate economic hardship to the point that it has a meaningful impact on these other types of crime. All of these findings persisted when we further restricted our analysis to the sub-sample of youth in the treatment group who chose to apply to the program in 2016, suggesting that the added dosage impacts do not simply reflect greater intrinsic motivation on the part of youth who *applied for* a second summer, but rather the experience of being *randomly selected* for another summer of program participation.

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, an examination of the SYEP program data revealed that repeat applicants were 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 chose to apply for a second time, repeat applicants were on average more likely to identify as male and having lived 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]

5.2.3 Peer Effects: Co-offending and Spillovers

Finally, we examined whether the Boston SYEP 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 research suggests that deviant peers socialize the acceptance of delinquent values and behaviors by otherwise non-offending youth (Akers 1998; Matsueda 1988). Other research suggests 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, SYEPs might reduce the likelihood that individuals in the treatment group were involved in co-offending events during the follow-up period because they were less subject to negative peer interactions than the control group.

We find that much of the program's impact on reducing arrests among particular subgroups stemmed from decreases in their co-offending behavior. Table 4 estimates the impact of the program on co-offending using equations (1) and (2) by replacing the dependent variable with an indicator of whether the individual was arrested and/or included in an FIO report with at least one other person.²⁰ Relative to the control group, co-offending decreased among males in the treatment group by 32 percent for arrests over the entire 4.5 year follow-up period. Among high-risk youth, the likelihood of being arrested with others fell even further (by 52 percent) and was also accompanied by a 30 percent decrease in being listed in an FIO report in the company of other known offenders. These findings suggest that youth with a prior arrest experienced some degree of disruption to their 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 in the evening because they had to get to work early each morning (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, including co-offending, for various subgroups, especially among high-risk youth that had previously been arrested. Do the new positive behaviors of high-risk participants also influence the prior co-offending "peers"? It is possible that some of the positive work habits and social interactions developed by high-risk youth in the treatment group might have "spilled over" onto their prior co-offending peers. Alternatively, if SYEP treated youth have completely severed their social ties with their former co-offending peers, then perhaps their prior co-offending peers were not influenced by the positive changes in behavior stemming from the program.

We find no significant impact of the Boston SYEP on the prior co-offending peers of the

²⁰ This includes co-offending with any other person, not just a prior co-offending peer.

treatment group.²¹ If anything, the incidence of ever being arrested for a violent crime appeared to increase, although our estimates were too imprecise to draw any substantive conclusions.²² Panels C and D of Figure 1 compare the time path of violent crime for high-risk youth versus their co-offending peers. Violent crime arrests dropped significantly among high-risk youth immediately after the program ended and continued to decline throughout the 55-month post-program period. Among their peers, there was also an initial drop in violent crime that then was reversed, only to drop sharply between months 12 and 15, which would align with the timing of the next summer jobs cohort.²³

5.3 Robustness to Alternative Specifications

Several additional supplementary analyses were performed to confirm the robustness of our findings. This included an analysis that adds each set of controls sequentially as well as a formal decomposition (Gelbach 2016) to examine the proportion 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 revealed that very little of the covariates explained any of the FIO variation between the treatment and control groups. In contrast, the baseline criminal justice history has some explanatory power but also did not explain much of the variation, as would be expected since treatment was assigned at random.

Our conclusions are substantively similar when using non-linear specifications to estimate program impacts. First, we used a Poisson quasi-maximum likelihood estimator (QMLE) where the consistency of this estimator only required the correct specification of the conditional mean, not the entire distribution. We also used 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 used 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

²¹ See Table A4 in the online appendix for a comparison of the demographic characteristics and baseline criminal justice outcomes of the prior co-offending peers of the treatment and control groups. These “peers” exclude other SYEP applicants as well as prior co-offenders who may be a peer of members in both the treatment and control groups.

²² See Table E.2 in the online appendix for the ITT estimates of the program’s impact on total arrests and arrests by both type and seriousness of the crime.

²³ See Figure E.3 in the online appendix for a further comparison of the trajectory of both violent and property crime for high-risk youth and their co-offending peers.

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.

We note some remaining limitations of this study, some of which 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 the FIO data allow us to measure peer effects at a more granular level than prior studies, our sample size is insufficient for measuring important outcomes like co-offending across subgroups, making the case for police departments in cities with larger programs to make that data available to researchers. Third, there is a higher degree of measurement error when using arrests versus other types of criminal justice data (for example, arraignments), although those other sources typically do not contain as much information on peers. Finally, although we collected some qualitative data during the summer of 2015, a follow-up qualitative study of both treated and untreated youth—and possibly their prior co-offending peers—would provide greater insight into how the program’s longer-term impacts are being sustained over time.

6. 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 measured both juvenile and adult criminal justice involvement, including informal observations and interactions with police, we followed youth through the end of 2019 (4.5 years post-randomization). We find that participating in the Boston SYEP significantly lowered both the likelihood of ever being arrested and the number of arrests in the longer-term for certain subgroups of youth who are likely to be court-involved (for example, youth who identify as male, 18-24 year olds, and those previously arrested) and for certain types of crime (for example, violent, property, and misdemeanor crimes). For high-risk youth, the program appears to primarily reduce the number of arrests, rather than the incidence, as is often found in prior SYEP studies (Heller 2014, Gelber et al. 2016, Modestino 2019). Similar to Kessler et al. (2021), we find no decrease in the incidence of high-risk youth ever being arrested for a violent crime, yet a 79 percent drop in the number of

violent crime arrests. 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 accumulated over time, particularly between months 12 and 36—literally putting youth on a better path. After three years, the differential impact did not grow by much but also did not shrink over the next 18 months, suggesting that the observed short-term improvements were sustained. Given that the decrease in criminal activity was not limited to the duration of the program, it appears that the program shifted the behavior of participants and these behavioral changes persisted even after the program had 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 and the time path of these behavioral changes 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 sharply immediately after the program ended and 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 job 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.²⁴

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. The most consistent long-term impact was the reduction in youth violence. This suggests that SYEPs may sustain reductions in criminal activity by changing interpersonal behavior, 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

²⁴ 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).

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). In addition, youth winning the summer jobs lottery for a second consecutive summer experienced larger (up to 50%) and more consistent reductions across most types of arrests, including property and drug crimes. City leaders focused on more widespread crime prevention beyond violence 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.

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 reduced co-offending among both males and high-risk youth by roughly 30 percent, but this does not translate into reductions for their peers. This suggests that SYEP participants do indeed sever or greatly limit their ties with prior co-offending peers.

While it might be tempting to exclusively target SYEP towards only high-risk youth within a city or neighborhood, there are several reasons why this approach may have the unintended consequence of reducing the program’s effectiveness. First, the take-up rate might not be as high among this group of youth which could leave jobs unfilled while other willing youth are left unemployed. Second, additional targeting of high-risk youth would likely require expanding employer partnerships to ensure proper placement and supervision of court-involved youth at job sites that do not require a CORI check. Third, our qualitative evidence suggests that positive peer interactions matter so it is likely that a mix of youth with different strengths and weaknesses may be needed to produce such benefits while also limiting any stigma and ensuring continued widespread support for the program. Regardless of the reason, cities may need to consider pairing SYEPs with other prevention approaches to address the remaining high-risk youth in those networks, such as Baltimore’s successful Group Violence Reduction Strategy (GVRS) (Kapustin et al., 2026).

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 intervening when young

adults are most likely to engage in criminal activity is perhaps even more important than previously thought, especially for high-risk youth. 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. Even though high-risk youth are likely to “age out” of criminal justice involvement, SYEPs can play an important role in accelerating that process or at least minimizing the amount of violent crime during this period to produce better long-term outcomes for youth and the communities that they live in.

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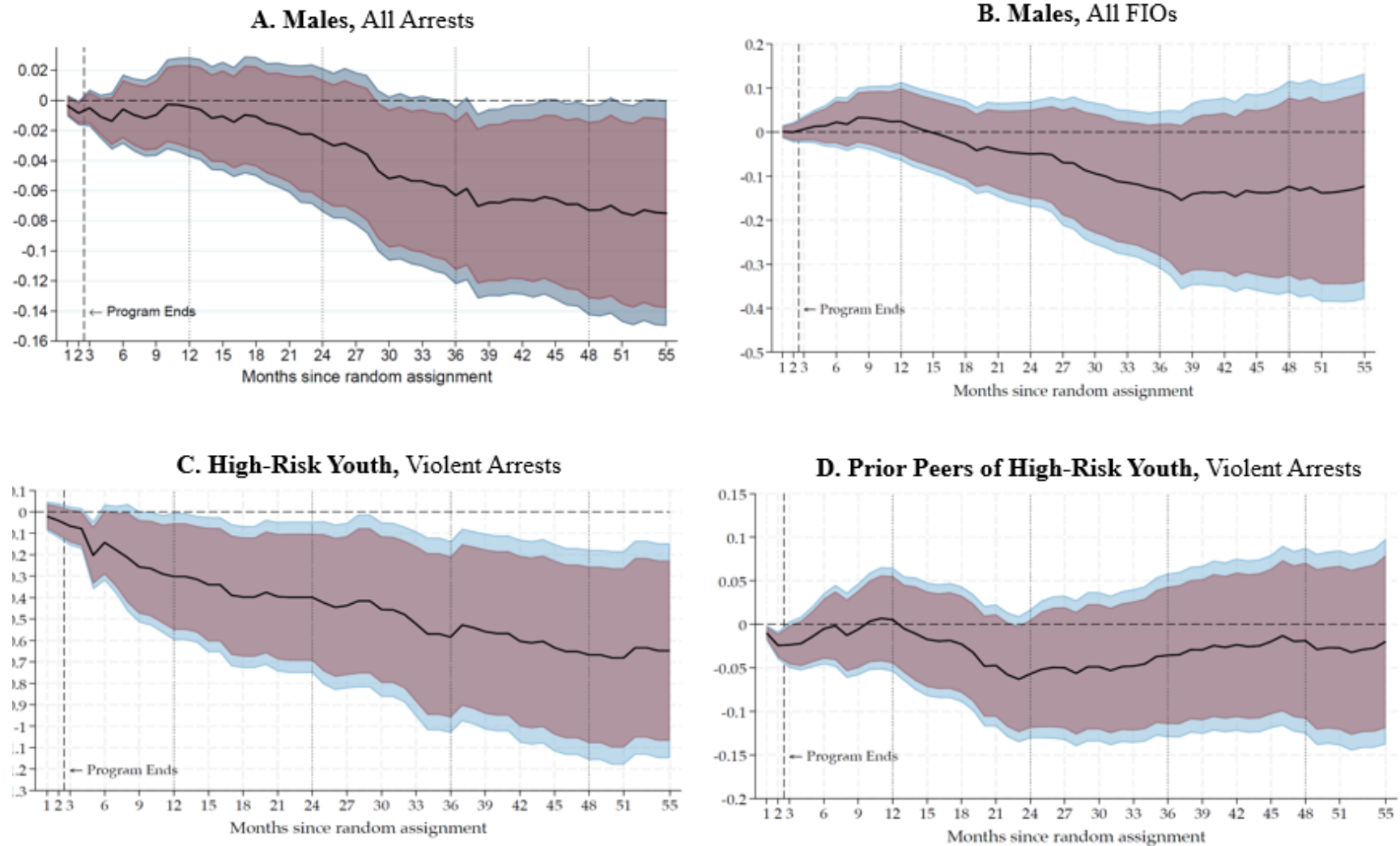


FIGURE 1. CUMULATIVE TIME PATH OF PROGRAM IMPACTS BY MONTH FOR MALES, HIGH-RISK YOUTH AND PRIOR CO-OFFENDING PEERS.

Notes: This figure shows the cumulative treatment effect of the decline in arrests (or FIO reports) from random assignment in month zero through 55 months. The vertical dotted line shows the end of the program. High-risk youth are those with an arrest record prior to the program. Prior peers of high-risk youth are individuals, aged 14 to 30 years, who did not apply to ABCD during the summer of 2015, but with whom an SYEP applicant (treatment or control) was either arrested or included in an FIO report prior to 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). *Source:* Authors' calculations based on arrest records from the Boston Police Department.

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

	(1)		(2)		(3)	
	Treatment		Control		Treatment – Control	
	Mean	SD	Mean	SD	Diff.	<i>p</i> -value
Age (mean)	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)
<i>Race:</i>						
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 based on program records from the Office of Workforce Development (OWD).

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

	(1) All Youth		(2) Male		(3) Age 18–24		(4) At-risk: any prior		(5) 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. (0.009) [0.958]	[0.150]	-0.027* (0.015) [0.192]	[0.127]	-0.018. (0.022) [0.519]	[0.533]	-0.044. (0.071) [0.786]	[0.659]	-0.060. (0.123) [0.647]
Any Drug Arrest	[0.010]	-0.001. (0.003) [0.958]	[0.017]	-0.003. (0.006) [0.698]	[0.012]	0.013. (0.010) [0.436]	[0.095]	-0.017. (0.034) [0.786]	[0.121]	-0.076. (0.051) [0.428]
Any Property Arrest	[0.036]	-0.002. (0.006) [1.000] [0.958]	[0.061]	-0.018* (0.010) [0.448] [0.192]	[0.035]	-0.010. (0.013) [1.000] [0.519]	[0.201]	0.079. (0.058) [1.000] [0.706]	[0.264]	0.162. (0.099) [0.728] [0.428]
Any Violent Arrest	[0.059]	-0.003. (0.008) [0.958]	[0.082]	-0.013. (0.012) [0.466]	[0.082]	-0.039** (0.018) [0.187]	[0.312]	-0.055. (0.068) [0.786]	[0.429]	-0.168. (0.125) [0.428]
Any Felony Arrest	[0.064]	-0.000. (0.008) [0.958]	[0.100]	-0.012. (0.013) [0.475]	[0.090]	-0.034* (0.018) [0.211]	[0.377]	-0.028. (0.067) [0.786]	[0.473]	-0.061. (0.126) [0.647]
Any Misdemeanor Arrest	[0.055]	-0.006. (0.007) [0.958]	[0.090]	-0.033*** (0.012) [0.040]**	[0.073]	-0.008. (0.019) [0.658]	[0.342]	-0.080. (0.062) [0.706]	[0.462]	-0.085. (0.109) [0.647]
B. Intensity: Number of arrests per youth										
Arrests	[0.199]	-0.017. (0.023) [0.551]	[0.331]	-0.075** (0.038) [0.116]	[0.249]	-0.077. (0.048) [0.260]	[1.377]	0.069. (0.245) [0.907]	[2.066]	-0.334. (0.443) [0.529]
Drug Arrests	[0.015]	-0.003. (0.004) [0.551]	[0.021]	-0.004. (0.008) [0.623]	[0.017]	0.013. (0.013) [0.480]	[0.161]	0.065. (0.069) [0.811]	[0.253]	-0.090. (0.069) [0.452]
Property Arrests	[0.054]	-0.012. (0.010) [0.551]	[0.097]	-0.047*** (0.015) [0.016]**	[0.055]	-0.018. (0.025) [0.544]	[0.362]	0.190. (0.124) [0.710]	[0.505]	0.335. (0.235) [0.452]
Violent Arrests	[0.089]	-0.012. (0.014) [0.551]	[0.131]	-0.036* (0.022) [0.168]	[0.117]	-0.063** (0.030) [0.227]	[0.523]	-0.162. (0.127) [0.710]	[0.824]	-0.648** (0.256) [0.092]*
Felony Arrests	[0.115]	-0.006. (0.016) [0.700]	[0.191]	-0.027. (0.028) [0.468]	[0.142]	-0.056. (0.035) [0.260]	[0.769]	0.096. (0.176) [0.907]	[1.176]	-0.325. (0.302) [0.452]
Misdemeanor Arrests	[0.083]	-0.015. (0.013) [0.551]	[0.141]	-0.056*** (0.020) [0.018]**	[0.107]	-0.025. (0.027) [0.480]	[0.608]	-0.060. (0.134) [0.907]	[0.890]	-0.054. (0.231) [0.815]
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 in subgroup		4219		1950		857		270		121
Number of youth		4219		4219		4219		4219		4219

Notes: CM, control mean in brackets for each subgroup; ITT, intent to treat OLS estimates on the interaction between the SYEP and subgroup dummy with robust standard errors in parentheses. Holm-adjusted p-values across arrest outcomes within subgroups in braces. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

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

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 (0.017)	[0.098]	-0.032 (0.021)
Violent arrest	[0.060]	-0.017 (0.015)	[0.052]	-0.009 (0.017)
Property arrest	[0.038]	-0.019* (0.010)	[0.046]	-0.028** (0.013)
Drug arrest	[0.011]	-0.013*** (0.005)	[0.018]	-0.020** (0.008)
Felony arrest	[0.063]	-0.015 (0.015)	[0.080]	-0.035* (0.019)
Misdemeanor arrest	[0.056]	-0.025** (0.013)	[0.052]	-0.022 (0.015)
B. Intensity: Number of arrests per youth				
All arrests	[0.197]	-0.093*** (0.036)	[0.199]	-0.090** (0.044)
Violent arrests	[0.093]	-0.054** (0.022)	[0.083]	-0.038 (0.027)
Property arrests	[0.051]	-0.029* (0.015)	[0.052]	-0.029* (0.015)
Drug arrests	[0.012]	-0.014** (0.005)	[0.021]	-0.022** (0.009)
Felony arrests	[0.116]	-0.044* (0.027)	[0.135]	-0.062* (0.035)
Misdemeanor arrests	[0.081]	-0.053*** (0.018)	[0.064]	-0.033* (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, intent to treat OLS 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 based on arrest records from the Boston Police Department.

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	-0.006 (0.009)	-0.018 (0.019)	-0.012 (0.023)	-0.087 (0.064)	-0.203** (0.087)
<i>CM: youth in sub-group</i>	0.11	0.19	0.13	0.56	0.64
<i>CM: youth not in sub-group</i>	0.11	0.05	0.11	0.08	0.10
B. Any arrest involving multiple people	-0.009 (0.006)	-0.025** (0.012)	0.012 (0.015)	-0.037 (0.054)	-0.152** (0.071)
<i>CM: youth in sub-group</i>	0.04	0.07	0.04	0.24	0.29
<i>CM: youth not in sub-group</i>	0.04	0.02	0.04	0.03	0.03
C. Any FIO involving multiple people	-0.004 (0.009)	-0.014 (0.018)	-0.009 (0.021)	-0.094 (0.063)	-0.168* (0.094)
<i>CM: youth in sub-group</i>	0.09	0.16	0.11	0.49	0.56
<i>CM: youth not in sub-group</i>	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, intent to treat estimates with OLS 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 based on arrest and FIO records from the Boston Police Department.

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 offense 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 then 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) Peer Effects: Co-offending and Spillovers

Criminal Justice Co-offenders – we identify individuals as co-offenders if they are observed engaging in criminal activity together as recorded by either the BPD arrest or FIO data.

Criminal Justice Peers – we define peers as a subset of co-offenders, aged 14 to 30 years, who did not apply to ABCD during the summer of 2015, but with whom an SYEP applicant (treatment or control) was either arrested or included in an FIO report prior to the program. We exclude incidents involving more than 10 people since those tend to be large gatherings of youth (for example, underage drinking at a party) where it is unclear whether the individuals are in fact intentionally working together as “peers.” We also exclude potential peers who overlapped with individuals in both the SYEP treatment and control groups since it is unclear which “peer” effect would be more impactful.

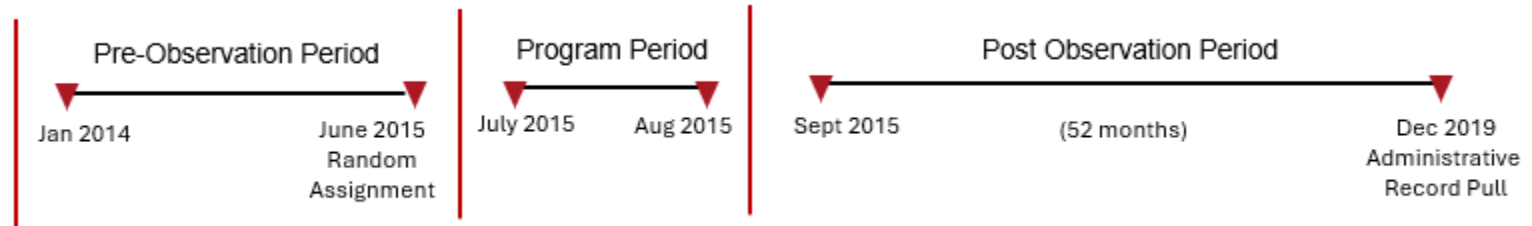
3. Additional Balance Checks

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. Table A4 shows that the prior co-offending peers of the

treatment and control groups are largely balanced in terms of the limited demographics and criminal justice history we can obtain from the BPD data. Of the 14 characteristics examined, only two are marginally significant at the 10 percent level which is not unusual given that the program was assigned at random.

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 program records from the Office of Workforce Development and 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 program records from the Office of Workforce Development and 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 and FIO 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.; statistical significance is indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table A.4. Baseline demographic characteristics and criminal justice history of *prior co-offending peers*, by lottery outcome of SYEP applicants

	Treatment		Control		Treatment–Control	
	(1)		(2)		(3)	
	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	<i>p</i> -value
A. Demographic characteristics						
Age (mean)	19.060	(3.367)	18.898	(3.424)	0.162	(0.62)
Percent aged 18-24 years	0.695	(0.462)	0.677	(0.468)	0.018	(0.68)
Percent male	0.921	(0.271)	0.879	(0.326)	0.041	(0.13)
<i>Race/Ethnicity:</i>						
Percent Black	0.417	(0.495)	0.483	(0.500)	-0.066	(0.17)
Percent White	0.126	(0.333)	0.121	(0.326)	0.005	(0.87)
Percent Hispanic	0.325	(0.470)	0.271	(0.445)	0.054	(0.23)
B. Incidence: Percent of youth arrested						
Any Arrest	0.265	(0.443)	0.318	(0.466)	-0.053	(0.22)
Drug arrests	0.033	(0.180)	0.034	(0.183)	-0.001	(0.94)
Property arrests	0.079	(0.271)	0.153	(0.360)	-0.073*	(0.01)
Violent arrests	0.126	(0.333)	0.118	(0.233)	0.008	(0.81)
C. Intensity: Number of arrests per youth						
Number of arrests	0.490	(1.019)	0.596	(1.171)	-0.106	(0.30)
Drug arrests	0.046	(0.291)	0.049	(0.294)	-0.003	(0.92)
Property arrests	0.126	(0.533)	0.224	(0.697)	-0.098*	(0.08)
Violent arrests	0.172	(0.500)	0.175	(0.576)	-0.003	(0.96)
F-test (Prob>F)					0.81	(0.56)
Number of youth	151		406		557	

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

Note: SYEP, summer youth employment program; Std. Dev., 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.; Peers are defined as individuals who did not apply to ABCD during summer 2015, but with whom an SYEP applicant was either arrested or included in an FIO report prior to the program during an incident involving less than 10 people, excluding those who overlapped with both the SYEP treatment and control groups. Statistical significance is indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

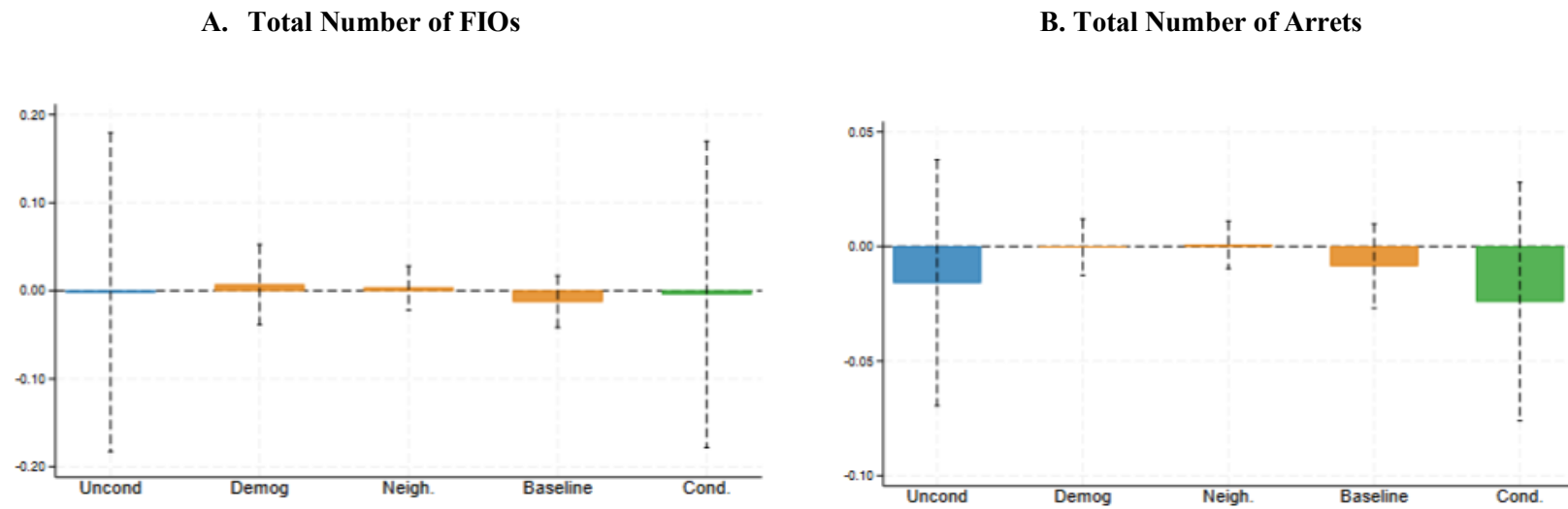
B. Explanatory Power of Independent Variables

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 justice 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



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 (0.010)	-0.001 (0.010)	-0.002 (0.010)	-0.001 (0.009)
Drug arrest	[0.010]	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)
Property arrest	[0.036]	-0.001 (0.006)	-0.000 (0.006)	-0.002 (0.006)	-0.002 (0.006)
Violent arrest	[0.059]	0.000 (0.008)	0.001 (0.008)	0.001 (0.008)	-0.003 (0.008)
Felony arrest	[0.064]	-0.002 (0.008)	0.001 (0.008)	0.001 (0.008)	-0.000 (0.008)
Misdemeanor arrest	[0.055]	-0.004 (0.008)	-0.002 (0.008)	-0.005 (0.008)	-0.006 (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; statistical 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 (0.027)	-0.011 (0.028)	-0.016 (0.028)	-0.017 (0.023)
Drug Arrests	[0.015]	-0.006 (0.005)	-0.006 (0.006)	-0.006 (0.005)	-0.003 (0.004)
Property Arrests	[0.054]	-0.008 (0.011)	-0.008 (0.011)	-0.012 (0.011)	-0.012 (0.010)
Violent Arrests	[0.089]	-0.004 (0.014)	-0.003 (0.014)	-0.005 (0.014)	-0.012 (0.014)
Felony Arrests	[0.115]	-0.004 (0.019)	-0.002 (0.019)	-0.003 (0.019)	-0.006 (0.016)
Misdemeanor Arrests	[0.083]	-0.012 (0.013)	-0.010 (0.013)	-0.013 (0.013)	-0.015 (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; statistical significance is indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C. Alternative 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 alternative 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 these nonlinear specifications does little 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 (0.010)	-0.001 (0.010)	-0.001 (0.010)	0.000 (0.009)	-0.028* (0.016)	-0.021 (0.023)
Violent Arrest	[0.059]	0.000 (0.008)	0.001 (0.008)	-0.000 (0.008)	-0.003 (0.008)	-0.016 (0.013)	-0.047** (0.021)
Property Arrest	[0.036]	-0.001 (0.006)	-0.000 (0.006)	-0.001 (0.006)	-0.002 (0.006)	-0.022* (0.011)	-0.013 (0.015)
Drug Arrest	[0.010]	-0.002 (0.003)	-0.001 (0.003)	-0.002 (0.004)	-0.001 (0.003)	-0.003 (0.007)	0.014* (0.009)
Felony Arrest	[0.064]	-0.002 (0.008)	0.001 (0.008)	0.001 (0.009)	0.001 (0.008)	-0.014 (0.015)	-0.040* (0.022)
Misdemeanor Arrest	[0.055]	-0.004 (0.008)	-0.002 (0.008)	-0.004 (0.008)	-0.005 (0.007)	-0.039*** (0.014)	-0.009 (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; statistical 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 (0.028)	-0.011 (0.028)	-0.018 (0.028)	-0.021 (0.025)	-0.104** (0.048)	-0.075 (0.057)
Violent arrests	[0.089]	-0.004 (0.015)	-0.003 (0.014)	-0.007 (0.015)	-0.019 (0.016)	-0.046* (0.027)	-0.067* (0.036)
Property arrests	[0.054]	-0.008 (0.012)	-0.009 (0.012)	-0.012 (0.012)	-0.013 (0.012)	-0.059*** (0.021)	-0.018 (0.022)
Drug arrests	[0.015]	-0.007 (0.006)	-0.007 (0.006)	-0.007 (0.006)	-0.004 (0.005)	-0.004 (0.009)	0.016 (0.013)
Felony arrests	[0.115]	-0.004 (0.019)	-0.001 (0.019)	-0.002 (0.019)	-0.006 (0.017)	-0.033 (0.033)	-0.061 (0.043)
Misdemeanor arrests	[0.083]	-0.012 (0.014)	-0.010 (0.014)	-0.014 (0.014)	-0.019 (0.014)	-0.074*** (0.026)	-0.025 (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; statistical 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):

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

$$Y_{it} = P_i \pi_3 + X_i(t-1)\beta_3 + \mu_{it2} \quad (D.2)$$

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 π_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). Interestingly, it does appear that there is some substitution across types of crime with large and significant increases in property crime among the at-risk and high-risk youth. However, those changes become insignificant once we account for cross-overs, including those at-risk and high-risk youth who are served by Youth Options Unlimited which works exclusively with court-involved youth.

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 often larger when we account for these youth that crossover from the control group into receiving treatment.

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 (0.011)	0.001 (0.011)	-0.002 (0.011)	0.000 (0.010)	-0.020 (0.017)	-0.019 (0.024)	0.052 (0.077)	0.069 (0.107)
Violent Arrest	[0.059]	0.001 (0.009)	0.001 (0.009)	-0.001 (0.009)	-0.002 (0.009)	-0.008 (0.014)	-0.035* (0.019)	0.028 (0.078)	-0.133 (0.122)
Property Arrest	[0.036]	-0.003 (0.007)	-0.003 (0.007)	-0.005 (0.007)	-0.004 (0.007)	-0.024** (0.011)	-0.017 (0.014)	0.146** (0.064)	0.269*** (0.089)
Drug Arrest	[0.010]	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.001 (0.004)	-0.002 (0.007)	0.008 (0.010)	-0.045 (0.034)	-0.064 (0.042)
Felony Arrest	[0.064]	0.004 (0.010)	0.006 (0.010)	0.004 (0.010)	0.005 (0.009)	-0.006 (0.015)	-0.031* (0.019)	0.065 (0.076)	0.053 (0.116)
Misdemeanor Arrest	[0.055]	-0.008 (0.008)	-0.007 (0.009)	-0.011 (0.009)	-0.011 (0.008)	-0.040*** (0.013)	-0.016 (0.021)	-0.055 (0.069)	-0.020 (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; statistical 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 (0.030)	-0.013 (0.029)	-0.023 (0.029)	-0.016 (0.025)	-0.090** (0.040)	-0.078 (0.053)	0.137 (0.253)	-0.173 (0.414)
Violent arrests	[0.089]	-0.005 (0.016)	-0.004 (0.016)	-0.008 (0.016)	-0.008 (0.016)	-0.029 (0.024)	-0.055* (0.030)	-0.094 (0.135)	-0.552** (0.231)
Property arrests	[0.054]	-0.007 (0.012)	-0.006 (0.012)	-0.012 (0.012)	-0.009 (0.011)	-0.051*** (0.016)	-0.025 (0.028)	0.281** (0.136)	0.655*** (0.222)
Drug arrests	[0.015]	-0.006 (0.005)	-0.005 (0.005)	-0.005 (0.005)	-0.004 (0.005)	-0.004 (0.009)	0.007 (0.014)	0.006 (0.065)	-0.100* (0.060)
Felony arrests	[0.115]	0.003 (0.021)	0.006 (0.021)	0.000 (0.021)	0.003 (0.018)	-0.022 (0.031)	-0.057 (0.041)	0.188 (0.201)	-0.325 (0.262)
Misdemeanor arrests	[0.083]	-0.020 (0.014)	-0.018 (0.014)	-0.023* (0.014)	-0.022 (0.013)	-0.074*** (0.021)	-0.027 (0.030)	-0.090 (0.138)	0.036 (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; statistical 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 (0.010)	-0.002 (0.010)	-0.003 (0.010)	-0.001 (0.009)	-0.020 (0.015)	-0.024 (0.022)	-0.037 (0.070)	-0.043 (0.115)
Violent arrest	[0.058]	0.002 (0.008)	0.003 (0.008)	0.002 (0.008)	-0.001 (0.008)	-0.006 (0.012)	-0.048*** (0.017)	-0.047 (0.067)	-0.166 (0.122)
Property arrest	[0.036]	-0.002 (0.006)	-0.001 (0.006)	-0.003 (0.006)	-0.003 (0.006)	-0.018* (0.010)	-0.004 (0.013)	0.071 (0.058)	0.139 (0.099)
Drug arrest	[0.010]	-0.002 (0.003)	-0.001 (0.003)	-0.002 (0.003)	-0.001 (0.003)	-0.005 (0.006)	0.012 (0.009)	-0.006 (0.034)	-0.033 (0.061)
Felony arrest	[0.065]	-0.003 (0.008)	-0.000 (0.008)	-0.001 (0.008)	-0.000 (0.008)	-0.012 (0.013)	-0.044** (0.017)	-0.034 (0.068)	-0.051 (0.120)
Misdemeanor arrest	[0.055]	-0.002 (0.007)	-0.001 (0.007)	-0.003 (0.008)	-0.004 (0.007)	-0.025** (0.012)	-0.008 (0.018)	-0.029 (0.064)	0.015 (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; statistical 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 (0.026)	-0.018 (0.026)	-0.023 (0.026)	-0.018 (0.022)	-0.074** (0.038)	-0.096** (0.047)	0.094 (0.243)	-0.275 (0.428)
Violent arrests	[0.089]	-0.003 (0.014)	-0.002 (0.014)	-0.005 (0.014)	-0.010 (0.013)	-0.026 (0.022)	-0.076*** (0.029)	-0.136 (0.126)	-0.586** (0.246)
Property arrests	[0.055]	-0.010 (0.010)	-0.010 (0.010)	-0.014 (0.011)	-0.013 (0.010)	-0.047*** (0.016)	-0.016 (0.025)	0.164 (0.124)	0.240 (0.216)
Drug arrests	[0.015]	-0.005 (0.005)	-0.005 (0.005)	-0.005 (0.005)	-0.002 (0.004)	-0.007 (0.008)	0.011 (0.012)	0.102 (0.073)	0.006 (0.095)
Felony arrests	[0.117]	-0.009 (0.018)	-0.007 (0.018)	-0.009 (0.018)	-0.007 (0.015)	-0.030 (0.027)	-0.074** (0.035)	0.100 (0.176)	-0.311 (0.295)
Misdemeanor arrests	[0.084]	-0.012 (0.013)	-0.011 (0.013)	-0.014 (0.013)	-0.015 (0.012)	-0.050** (0.020)	-0.026 (0.026)	-0.017 (0.135)	0.029 (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; statistical significance is indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E. Testing Mechanisms

1. Alternative Methods to Measure Dosage

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 (see Table E.1, Panel A). 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 E.1, Panel B).

2. Peer Spillover Effects

Table E.2 estimates the post-program impacts on the prior co-offending peers of youth in the treatment group versus those of youth in the control group. We find no significant impacts of the Boston SYEP on the prior co-offending 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.

3. Alternative Comparisons of Cumulative Time Path of Program Impacts

We explore how SYEP impacts accumulate over time in three different ways to better understand the program's mechanisms. First, we compare changes over time by type of crime. Figure E.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 E.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.

Second, we compare both formal (arrests) versus informal (FIOs) police contacts to better understand the interaction between changes in youth behavior and policing. Panel A of Figure E.2 shows that for youth who identify as male, 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 in arrests 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 as an outcomes

of total police contacts, we find no significant long-term impact of the Boston SYEP on overall criminal justice involvement, even among high-risk youth.

Third, Figure E.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.

Table E.1. 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 (0.019)	0.154	0.024 (0.044)	
Treated 2015 and 2016	0.073	-0.030 (0.021)	0.127	-0.098 (0.045)	**
Violent arrest					
Treated 2015	0.050	-0.007 (0.015)	0.072	-0.007 (0.028)	
Treated 2015 and 2016	0.047	-0.009 (0.017)	0.064	-0.044 (0.027)	
Property arrest					
Treated 2015	0.027	0.015 (0.013)	0.039	0.004 (0.016)	
Treated 2015 and 2016	0.022	-0.027 (0.013)	0.030	-0.033 (0.015)	**
Drug arrest					
Treated 2015	0.007	0.012 (0.008)	0.011	0.014 (0.009)	
Treated 2015 and 2016	0.006	-0.020 (0.008)	0.008	-0.023 (0.009)	**
Felony arrest					
Treated 2015	0.054	0.022 (0.017)	0.091	0.031 (0.033)	
Treated 2015 and 2016	0.048	-0.029 (0.019)	0.079	-0.057 (0.035)	*
Misdemeanor arrest					
Treated 2015	0.044	0.001 (0.014)	0.062	(0.007) (0.020)	
Treated 2015 and 2016	0.037	-0.023 (0.015)	0.048	-0.040 (0.020)	**
B. IV estimates using number of summers treated as an instrument					
Any arrest	0.070	-0.015 (0.010)	0.121	-0.050 (0.020)	**
Violent arrest	0.046	-0.008 (0.008)	0.063	-0.030 (0.012)	**
Property arrest	0.020	-0.010 (0.005)	0.028	-0.018 (0.007)	**
Drug arrest	0.005	-0.007 (0.002)	0.006	-0.008 (0.003)	***
Felony arrest	0.045	-0.009 (0.008)	0.075	-0.022 (0.015)	
Misdemeanor arrest	0.036	-0.013 (0.007)	0.047	-0.027 (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; statistical significance is indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE E.2. CUMULATIVE ITT ESTIMATES OF IMPACT ON PRIOR CO-OFFENDING PEERS OF SYEP APPLICANTS

	Incidence: Percent of Youth		Intensity: Number of Arrests	
	(1) CM	(2) ITT	(3) CM	(4) ITT
Any Arrest	[0.414]	0.033 (0.038)	[1.296]	-0.141 (0.171)
Violent Arrests	[0.219]	0.039 (0.038)	[0.401]	-0.034 (0.073)
Property Arrests	[0.172]	-0.001 (0.033)	[0.276]	0.046 (0.077)
Drug Arrests	[0.101]	-0.004 (0.029)	[0.158]	-0.033 (0.045)
Felony Arrests	[0.328]	0.017 (0.039)	[0.771]	-0.082 (0.127)
Misdemeanor Arrests	[0.266]	-0.031 (0.038)	[0.525]	-0.107 (0.089)
Controlling for:				
Demographic characteristics		Y		Y
Baseline criminal justice history		Y		Y
Number of youth	557	557	557	557

Notes: SYEP, summer youth employment program; CM, control mean; ITT, intent to treat OLS estimates with robust standard errors in parentheses. Prior co-offending peers are individuals, aged 14 to 30 years, who did not apply to ABCD during the summer of 2015, but with whom an SYEP applicant (treatment or control) was either arrested or included in an FIO report prior to the program. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

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

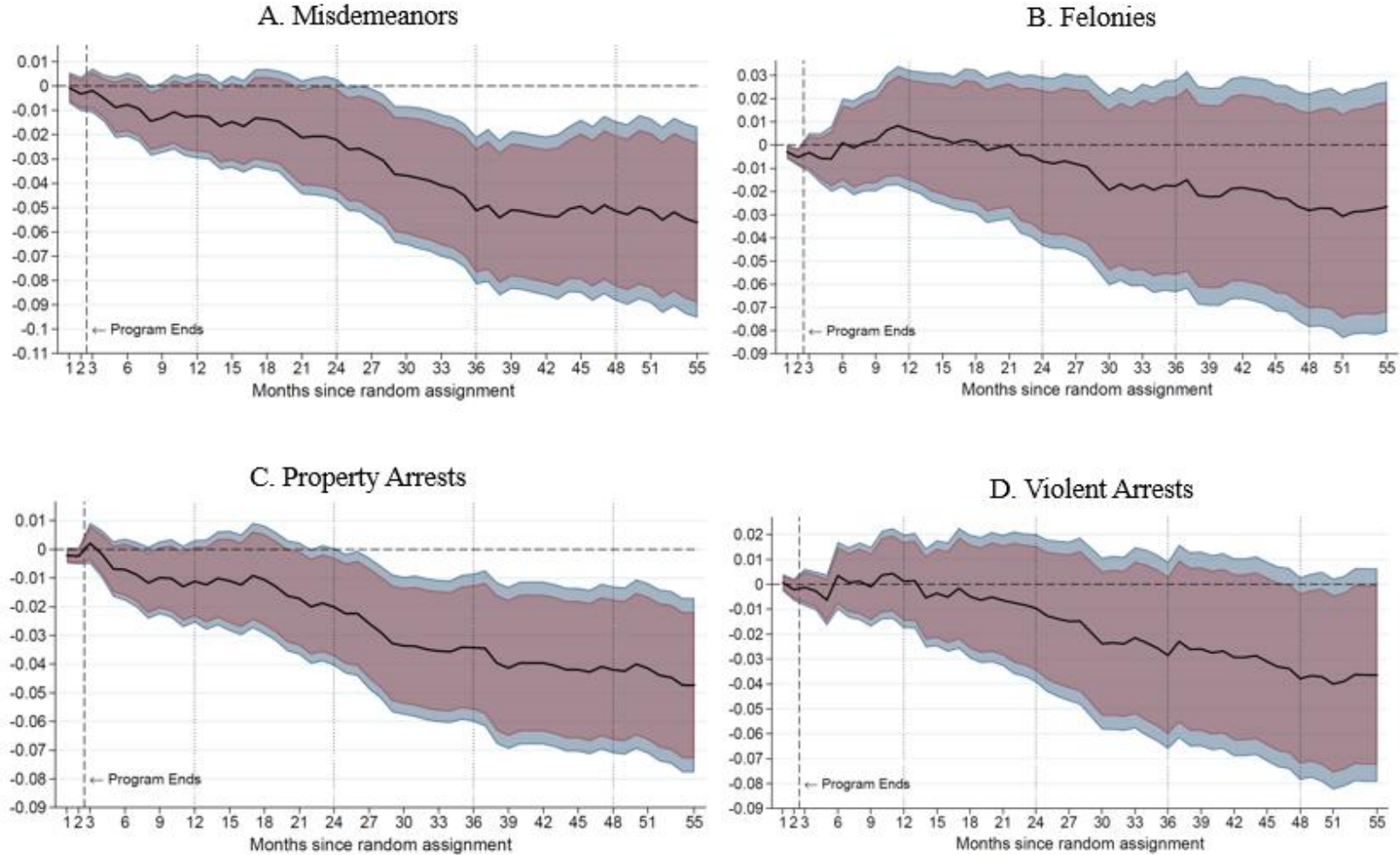


FIGURE E.1. CUMULATIVE TIME PATH OF PROGRAM IMPACTS BY MONTH FOR MALES BY ARREST TYPE.

Notes: This figure shows the cumulative treatment effect of the decline in arrests by type 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).

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

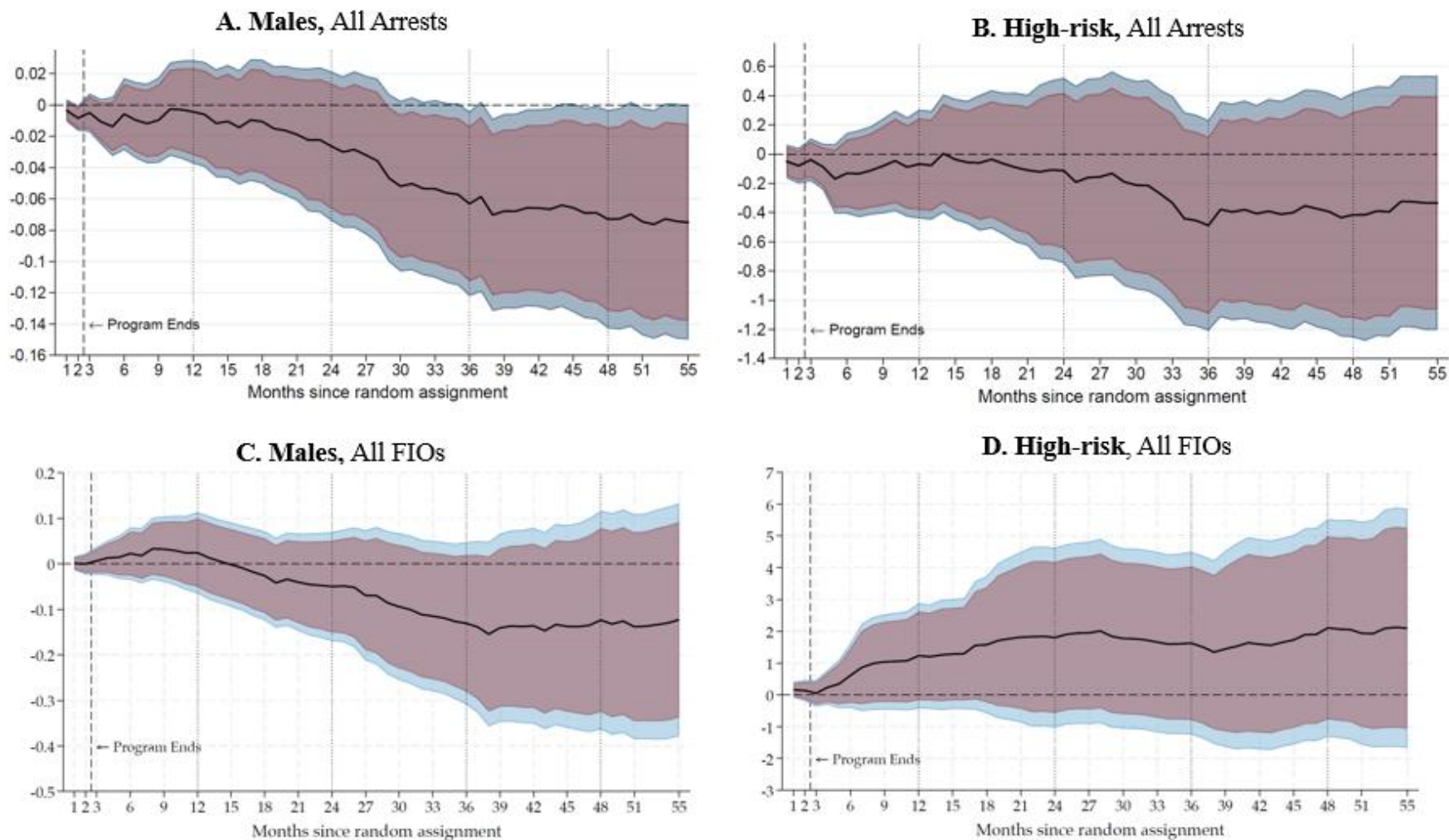


FIGURE E.2. CUMULATIVE TIME PATH OF PROGRAM IMPACTS BY MONTH FOR ARRESTS VERSUS FIOs.

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 arrest. 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).

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

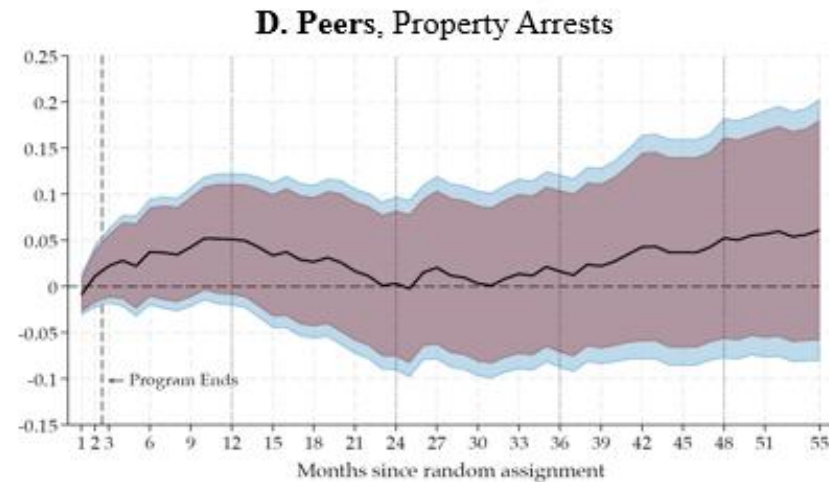
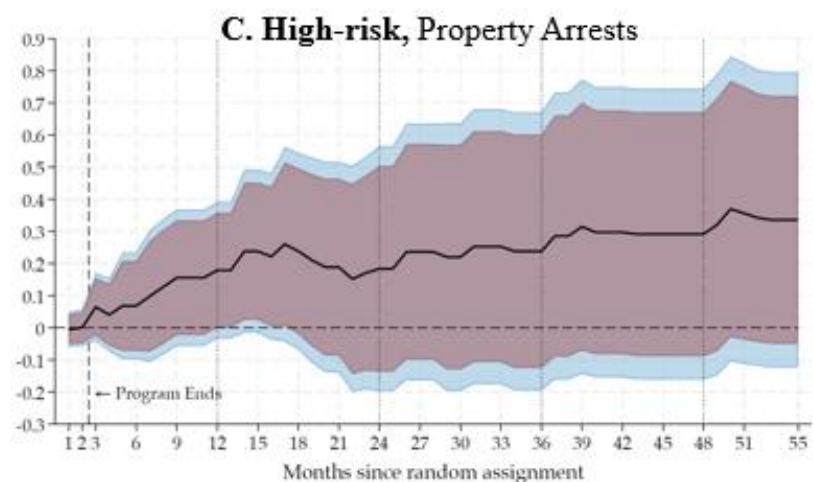
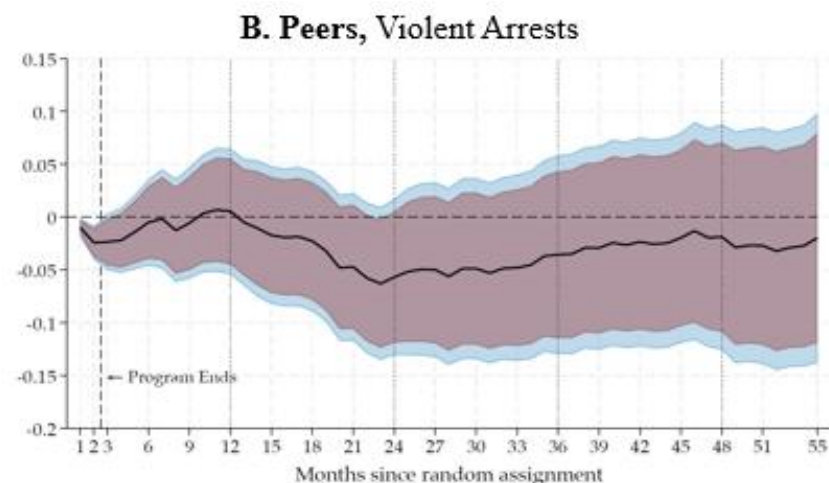
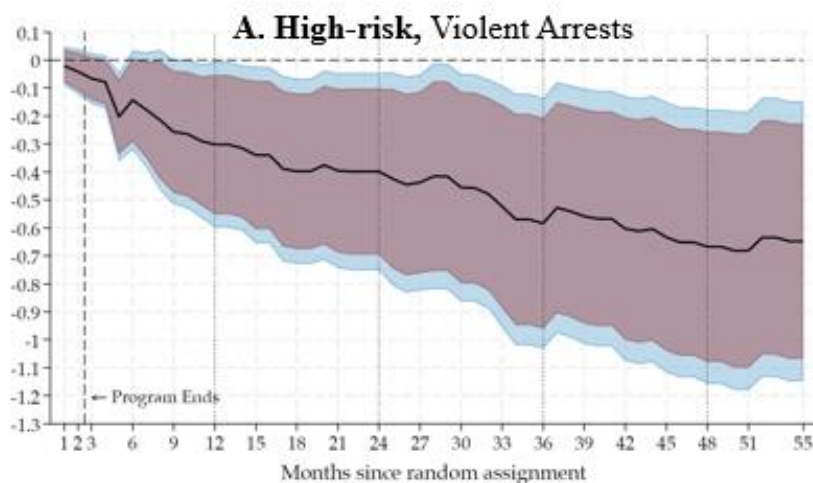


FIGURE E.3. CUMULATIVE TIME PATH OF PROGRAM IMPACTS BY MONTH FOR HIGH-RISK YOUTH VERSUS THEIR CO-OFFENDING PEERS.

Notes: This figure shows the cumulative treatment effect in number of arrests from high-risk youth with a prior arrest and their prior co-offending 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).

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