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NOUR JULNAR ABDUL-RAZZAK

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To my son Yousef, your kind heart lights up my world. To my husband Ahmed, I'm so grateful for your companionship and endless encouragement. And to my parents, for their unconditional love and guidance. You taught me the value of searching for the truth.

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ABSTRACT

My research aims to evaluate and develop important insights around social policy interventions focused towards youth at risk of becoming involved in violence and the criminal justice system. The dissertation will consist of three essays that build on one another with the goal of answering closely related and complementary questions. The first essay seeks to evaluate a promising behaviorally-informed program for youth disconnecting from school in a large-scale randomized controlled trial (RCT). The second essay focuses on measuring any possible spillovers from this intervention on the peers of the study youth through a novel use of administrative data. The last essay seeks to learn how this program achieved longer-term behavioral change for youth through an additional field experiment run in schools.

First, I seek to understand what type of programs can be effective for a population of youth who are disconnecting from schools and are unlikely to fully benefit from an in school intervention. My first essay provides evidence on how to engage and achieve long-term behavioral change for youth at risk of becoming involved in the criminal justice system outside of institutional settings.[1] Specifically, I evaluate a community-based program called *Choose to Change: Your Mind, Your Game* that uniquely pairs an advocate from the community providing holistic mentorship with a behavioral health component (trauma-informed cognitive behavioral therapy) to help youth process their past traumatic experiences and develop decision-making tools that can help them navigate difficult situations. Evaluated through a large scale RCT in Chicago's most violent neighborhoods, results to date show the program to be highly effective at reducing total and violent arrests in number of arrests per person and having any arrest up to two years post random assignment, in addition to improving school engagement.

Given the importance of peer connections among adolescents, my second essay seeks to understand how such programs can have ripple effects in the larger community and neighborhood. Specifically, we seek to measure if this crime-reducing intervention had spillovers on

1. This paper is co-authored with Kelly Hallberg, hallberk@uchicago.edu

the peers of the study youth. Using exogenous variation in a peer's treatment status created by the crime-reducing RCT, we estimate peer effects using administrative co-offending data from the Chicago Police Department and cohabiting data from Chicago Public Schools.[2] We find little evidence that the intervention influenced peers criminal behavior among the co-offending network of youth. We find some evidence that the intervention impacted the cohabiting peers of the study youth when the study person is older than the peer, by reducing the probability of having any violent arrest within 12 months. Future work is needed to better understand the spillovers of crime reducing interventions among adolescents.

Lastly, my third essay seeks to understand why such crime-reducing programs can work for participants despite the strong peer influences among adolescents. Based off of qualitative work, this paper sought to explore if the crime-reducing RCT achieved such results because they helped youth increase their self-worth to respond differently to the peer pressures of their immediate communities. I test this idea in a field experiment in partnership with the Chicago Police Department (CPD) where C2C study youth are given a chance to sign up for CPD youth programming with randomly varied privacy assurances on the sign-up forms (the "public" forms suggested their peers might found about their interest in CPD). Given the stigma of working with CPD among these communities, I exploit the randomized experimental design of C2C to compare this public-private difference for the treatment versus control groups. I find suggestive evidence that a public form decreased take-up in the C2C control group, but significantly increased take-up in the C2C treatment youth, suggesting a response to peer pressure in a way that counteracts social norms.

2. This paper is co-authored with Kelly Hallberg, hallberk@uchicago.edu

CHAPTER 1

A COMMUNITY-BASED APPROACH TO ENGAGING DISCONNECTING YOUTH: EXPERIMENTAL EVIDENCE IN REDUCING YOUTH CRIME

1.1 Abstract

High levels of both crime and incarceration remain key urban problems that plague already economically disadvantaged communities, in Chicago and around the country. We have seen that interventions informed by behavioral science that focus on decision-making can decrease crime involvement, but engaging youth at high-risk of violence involvement *outside* of institutional settings (e.g. schools) has remained a challenge. This project studies a new intervention informed by behavioral science that seeks to effectively target and engage adolescents at high risk for violence involvement in Chicago's most economically and racially segregated neighborhoods. *Choose to Change (C2C): Your Mind, Your Game* is a community-based intervention that aims to reduce criminal behavior and improve academic outcomes for vulnerable young people. This five-month program combines a behaviorally-informed intervention (a version of trauma-informed cognitive behavioral therapy, or CBT) with holistic mentorship and advocacy, which seeks to recruit and retain youth to maximize participation in the C2C's programming. Results from our ongoing randomized controlled trial (RCT) demonstrate that participation in C2C reduces the probability of any arrest by 46% within 24 months of randomization and the probability of any violent crime by 44% within the same time frame. In school, significant reductions in school misconducts (by 30%) and increases in days attended (by 5-7%) indicate an increase in school engagement. This project demonstrates the viability of a new intervention that can achieve longer-term crime and violence reductions among vulnerable youth outside of an institutional setting.

1.2 Introduction

Rates of gun violence remain far higher in the United States than in other developed nations. Homicide rates in American cities like Baltimore, Chicago, and St. Louis can range from 25 to 50 per 100,000, far higher than cities like London (1 or 2 per 100,000 in recent years). This violence and the primary policy response to it – incarceration – impose substantial societal costs (Johnson and Raphael 2009; Aizer and Doyle 2015). Gun violence is also disproportionately concentrated among young people ages 10-24. In Chicago, for example, 60% of those arrested for violent crimes were under the age of 25. Efforts to prevent violent crime without increasing incarceration, therefore, must successfully engage young people in an effort to change their behavior.

While social policy efforts that focus on changing the costs and benefits of crime in the U.S. have been met with mixed results (Lipsey, 2009; Lochner 2011; Heller et al 2011), a growing body of literature suggests the effectiveness of behaviorally-informed interventions that focus on decision-making can effectively reduce crime among youth (Landenberger & Lipsey, 2005; Heller, 2014; Heller et al., 2017; Blattman et al. 2017). Rather than attempting to change the incentives youth face when deciding whether to engage in criminal activity, these interventions target the decision making process itself, encouraging young people to take a step back and reconsider the choices or alternatives that even come to mind in a given situation (Nisbett and Ross 1991; Kahneman and Egan 2011; Shah and Ludwig 2016). By making decision making less automatic and building cognitive skills, these approaches have been shown to successfully reduce youth involvement in violent crime and overall crime by substantial margins (Landenberger and Lipsey, 2005; Heller, 2014; Heller et al., 2017; Blattman et al. 2017).

However, a few challenges remain in extending the impact of these promising approaches. First, behaviorally informed programs must successfully enroll and retain youth who are at higher risk for criminal involvement. To date, most of the successful interventions that have been tested in the field have been implemented in institutional settings (e.g. schools and

detention centers). Yet the same factors that cause youth to be at elevated risk for crime make them less likely to engage key social institutions, such as schools and social programs. Programs that have been implemented in detention facilities have been shown to be effective, but if the goal is prevention, effective programs need to engage youth before they are justice involved.

Second, even programs that have successfully engaged higher risk youth in behaviorally informed interventions – such as a summer jobs program that incorporated a social and emotional learning curriculum – while promising in the short term, see fade-out of violence reduction effects within 16-24 months of randomization (Heller 2012; Davis and Heller 2018; Blattman et al. 2017). These findings are consistent with the broader literature which finds that many adolescent programs focused on building cognitive and character skills that have been rigorously studied to date experience fade-out within a few years of random assignment (Heckman and Kautz 2013).

This paper presents mid-study findings from a randomized controlled trial designed to study the effectiveness of a program that harnesses relationships to engage young people in a behaviorally-informed intervention and sustain the intervention's effects. *Choose to Change (C2C): Your Mind, Your Game* is a community-based intervention that aims to reduce criminal behavior and improve academic outcomes for vulnerable young people. In October of 2015, two program providers in Chicago, Children's Home & Aid (CHA) and Youth Advocate Programs (YAP), began working together to provide C2C services to youth ages 13-18 in Chicago's most economically disadvantaged and segregated neighborhoods. Over the past three years, five cohorts of youth have been served, bringing the total number of youth served to almost 500. Through a randomized controlled trial (RCT) we evaluate the effectiveness of this intervention with 1537 youth randomized to date into the program with the study and the program continuing to enroll youth.[1]

The five-month C2C program combines a behaviorally-informed intervention (a version

1. The program has expanded to the West Side of Chicago in March of 2019.

of trauma-informed cognitive behavioral therapy, or CBT) with holistic mentorship and advocacy, which seeks to recruit and retain youth to maximize participation in the C2C's programming.[2] The advocate, often a "near-peer" and pro-social adult, is the first point of contact for youth. Advocates come from the very same neighborhoods the youth live in and have confronted many of the same challenges. Before any CBT services or other services are offered, the advocate spends time developing a concrete personal relationship with the youth, which is fundamental to generating trust. They engage with youth and their families in prosocial activities, helping meet basic necessities while also providing motivation and direction on long-term goals such as employment. Intuitively, this kind of human connection – or what the sociology literature calls "social capital" (Coleman 1988) – can be a key to increasing a youth's connections to pro-social institutions (Tolan et al. 2013; Dubois et al. 2011; Deluca et al. 2016; Barker 2007). Advocates also take the youth they serve to regular CBT sessions, run by clinically trained therapists, and encourage their active participation. In these sessions, youth learn how to identify events and circumstances that overwhelm them and trigger a traumatic response and develop strategies to cope with these situations in the moment. Advocates reinforce these lessons individually and during group activities through the week when they engage with the young people outside of the sessions.

In this paper, we present evidence that the C2C program is able to successfully engage youth at higher-risk for violence involvement, leads to a substantial reduction in overall and violent crime involvement, and improves academic outcomes.

First, we find that despite having a high percentage of youth only marginally connected to school (or not at all), C2C achieved a take-up rate of 62% to date.[3] To demonstrate that youth on the higher end of the risk distribution signed up for programming, we benchmark our take-up rates using a predictive model that utilizes administrative Chicago Police

2. The program duration was five months for the first four cohorts and has expanded to six months for the fifth and subsequent cohorts.

3. We find that 22% of the study sample were not actively enrolled at some point in the prior school year. On average, youth in the program are missing more than 28% of the 180-day school year prior to the intervention.

Department (CPD) information about the arrest and victimization histories of each person recently arrested in Chicago, along with information about the arrests and victimizations of their associates, to predict the probability of future violence involvement. We find that 46% of C2C youth were in the top decile of predicted risk for violence involvement within the citywide distribution.[4] When we compare this to other in-school interventions, we find that C2C was able to engage a riskier population, and achieve slightly take-up rates despite serving a more acutely risky population that are marginally connected to school.[5]

Second, drawing on the RCT design and administrative data from CPD, we find that C2C was able to successfully decrease total and violent arrests (both the number of arrests and the probability of ever being arrested) among those that participated. We find that among all cohorts of youth, we see significant reductions by 42-44% of the control complier mean (CCM) in the probability of any violent arrest or number of violent arrests during the program period among participants.[6] We see similar reductions in the probability of having any arrest during the program period.[7] Lastly, using data from roughly half of the study population, we document significant longer term reductions in the probability of any arrest. Specifically we find that C2C reduces the probability of any arrest by 46% of the CCM within 24 months of randomization[8] and the probability of any violent crime by 44% of the CCM within the same time frame.[9] Future iterations of this paper will include the long-term results from the full study sample, as the RCT randomization was staggered over five cohorts and a larger sample will enable us to more precisely estimate the long-term violent arrest outcomes.

Lastly, we use administrative data from Chicago Public Schools (CPS) to determine if

4. This model uses data from the first cohort only.

5. For BAM, the in-school intervention studied in Heller et al 2017, only 21% of the study youth were in the top decile of risk for violent offense.

6. These are treatment-on-the treated (TOT) results and are significant at the 10% margin.

7. This percent decrease is 38% of the CCM exactly and statistically significant at the 5% level.

8. statistically significant at the 1% level

9. This is statistically significant at the 10% level

the program improved academic outcomes for youth. Given the tenuous connection many study youth have to school, the data available has high rates of missingness. With a robust set of imputation methods utilized, we find evidence that the program led to a 30% reduction in misconduct incidents in the year after randomization and an increase in days attended the year after randomization by 5-7%. Future iterations of the paper will be able to better understand longer-term outcomes like graduation once more time has passed.

The results presented in the paper today do not use the full study sample up to two years post random assignment given the staggered nature of cohorts with more time needed to pass to obtain outcomes on the full sample. Nonetheless, this work suggests C2C's unique ability to achieve lasting, significant and positive behavior change among vulnerable youth.

1.3 Literature Review and Contribution

This paper contributes to the literature in several ways. First, it builds on the growing CBT and behavioral focused intervention literature that has demonstrated these approaches are effective in reducing crime and violence (Heller et al. 2017; Blattman et al. 2017). As past research has shown, ensuring high-risk youth have access to mental health services is critical to helping youth achieve the social-emotional skillset they need to succeed in life (Herrera et al. 2013; Cunha et al 2006, 2010). For example, in a large scale RCT in Chicago, Heller et al. (2017) found that an in-school CBT program for young men called Becoming a Man (BAM) reduced arrest rates by 28-35% and arrests for violent crimes by 45-50%. A program based on comparable principles implemented among male youth in the Cook County Juvenile Temporary Detention Center, reduced re-admission rates by 21%. Similarly, Blattman et al. (2017) found that CBT reduced anti-social behavior among criminally engaged young adult men in an RCT conducted in Liberia.[10]

Further, it extends this literature by demonstrating that near peer mentors can support the engagement and retention of youth in behaviorally informed interventions outside of an

10. More details on these and other relevant studies can be found in the Appendix.

institutional setting. This is of critical importance because the same risk factors that put youth at elevated risk for violence involvement tend to also lead to decreased engagement in key social institutions like schools and social programs (Levitt 1997). We hypothesize that in addition to supporting sustained engagement with the program, the mentor facilitates individual and group practice of the cognitive skills learned in the CBT sessions. Such a mechanism would echo the findings of Blattman et al. (2017) who found that CBT when combined with a cash transfer facilitated cognitive skill building that mitigated program fade out at one year post-program completion.

Lastly, recent literature has shown that youth summer employment programs can generate large impact on youth behavior, particularly violent crime and incarceration (Davis and Heller 2017; Gelber et al 2015; Heller 2014). Davis and Heller (2017) has shown how a recent Chicago summer youth employment program was able to successfully engage youth out of school and involved in the criminal justice system, and still achieve reductions in violent crime one year post randomization (with fade-out shortly after). However, the success of that intervention was limited to violent crime. No significant impact was found on other types of crime or education outcomes. While the employment literature demonstrates how a summer job can engage youth out of school and already involved in the justice system, it also displays the limitation of a summer job in achieving holistic behavior change for disadvantaged youth. In contrast, this paper contributes to this literature by demonstrating that the combined services of a mentor/advocate with CBT can achieve substantial behavioral change in multiple domains of a youth's life including educational outcomes such as attendance. We also provide evidence that combining mentoring with a behaviorally-informed intervention may mitigate the fade out often seen in rigorous studies of youth social programs (Heller 2014; Davis and Heller 2017; Heckman and Kautz 2013).

1.4 Intervention Details

1.4.1 Trauma-Informed CBT

The specific form of group-CBT offered by Children's Home & Aid in C2C is called SPARCS, or Structured Psychotherapy for Adolescents Responding to Chronic Stress. Led by a trained therapist, SPARCS is designed specifically for youth who have been traumatized, often repeatedly, and continue to live with high levels of environmental stressors, such as neighborhood violence or concentrated poverty. Although not every youth served in C2C has symptoms stemming from traumatic events, the stress of living in high-violence, high-poverty communities such as Englewood and West Englewood–with homicide rates of 78 and 45 per 100k respectively (City of Chicago data portal, September 18, 2017)–suggests that some trauma-informed care would be beneficial for most youth. Using data collected at intake from a trauma history checklist and interview from half of the participants, on average youth had experienced 7.2 of the 20 possible traumas.[11] The type of traumas that there most often endorsed by youth tended to fall in the category of community and family violence, with 56% of youth noting that they have been told about someone's killing or injury, 55% of youth of youth have seen or heard someone being badly hurt, 50% have been hurt or injured themselves and 45% have seen someone dead or dying or had watched or heard someone being killed.

Exposure to violence and adverse experiences has been linked by several studies to poor health outcomes, earlier deaths, and a wide variety of emotional and behavioral symptoms (Carrion et al. 2002; Collins et al. 2010; Bryant and Philips 2013; De Bellis, 2001). Higher levels of violence exposure are predictive of more aggressive and less prosocial behavior among adolescents (McMahon et al, 2013). Children who report community violence exposure may in turn be vulnerable to academic difficulties through poor attendance (Bowen et al. 1999)

11. Not all youth enrolled in the program completed the interview. 137 of the 217 youth (63%) enrolled in year 1 of the program completed the checklist.

or because their functioning at school is hindered by disruptive behavior problems, such as aggression and hyperactivity (Schwartz and Gorman, 2003). In a school setting, this can have a profound spillover effect: negative behavior by individual students has been shown to lead to reduced test performance and increased likelihood of misconduct incidents for all students (Figlio, 2007). In Chicago, African American children were found to perform worse on assessments when a homicide occurred near their home close to the test date, an effect that can translate to lower cognitive functioning for one week out of every month because of the concentration of local homicides (Sharkey, 2010).

SPARCS aims to help youth understand and change the way stress or past traumatic experiences can influence their decision-making. Therapists stress how these "emotional leftovers" from bad experiences can make youth more willing to engage in short-term coping strategies that often lead to worse outcomes. Through exercises and conversation, youth learn to better regulate their emotions, engage in helpful coping mechanisms, and build problem-solving and communication skills.[12] Therapy is conducted in 1-hour sessions with 12-16 sessions total for full treatment.[13]

The first part of the sessions focus on understanding trauma and the link between their feelings and past experiences with current perceptions and actions. There is one particular exercise conducted early on that highlights how trauma and chronic stress can impact emotions. The therapist displays three bottles that look similar from the outside, however one bottle is carbonated and has been shaken, another bottle has flat water, and the last bottle has carbonated water (but unshaken). The therapist then opens up the first shaken carbonated bottle with water exploding everywhere and says this is what happens when we

12. Specific symptoms that some youth might have include affect and impulse dysregulation (easily triggers, problems expressing anger), self-perception (sense of ineffectiveness, feeling damaged), issues with relationships (inability to trust, problems relating to others), somatization (physical symptoms that are exacerbated by stress that may or may not have concrete physiological causes), and symptoms of meaning/sense of purpose (foreshortened future, hopeless and pessimistic).

13. During the school year, the sessions are often offered during lunchtime or after school and advocates bring lunch for the youth to encourage participation. During the summer, group sessions are held at the YAP office.

bottle up our feelings, things explode and can get messy. She then opens up the flat water bottle, and comments that for some people they could feel completely numb, they don't feel sad or angry or do not tend to feel happy or get excited about things. She emphasizes how it's important to feel, but chronic stress and trauma can rob some people of their emotions. Lastly, she opens up bottle number 3 slowly (with no mess) and emphasizes that with the right tools we can cope with the stress that we have been exposed to.

They then move on to learning a new set of coping skills and tools. As an example, one of the first exercises that youth participate in is a check-in called "SOS Thermometer" which stands for slow down (sit back, breathe, take one thought at a time), orient yourself (where are you? who are you with?), and self-check (rate how distressed one feels, and how in control one feels). This check-in process that happens at the beginning of each session and during role-playing activities helps youth understand how they feel and what they are going through in the moment. Therapists try to emphasize that the first step in figuring out what to do when dealing with stressful moments is to slow down and give oneself time to think.

The curriculum also includes therapists walking through specific situations to help youth think through their thoughts and actions. When in a hypothetical situation or role-playing, the therapists teach a series of skills: LET M' GO and MAKE a LINK (see Figure 1.4 and 1.5 in Appendix). A key part of the methodology is that the therapist does not tell the youth how they should feel or what they should do. Together, they work through a situation and concentrate on the goal or desired outcome. If a youth suggests an inappropriate action, the therapist validates their emotions but also helps the youth think through the consequences of such actions. SPARCS therapists balance encouragement to change with the validation of the youth's current perspective and coping strategies. They reassure youth that their current maladaptive behavior makes sense, while at the same time helping them find a more positive way of behaving and coping (Bayles et al. 2014). The MAKE a LINK exercise helps youth increase their communication skills. Many of the youth in the program have difficulty

trusting others and often feel alienated. These skills are helpful tools for learning how to ask for what one wants, cultivating meaningful relationships, reaching out for support, and managing interpersonal challenges.

Another major element of SPARCS is incorporating mindfulness practices.[14] Because many youth who have been impacted with trauma often operate at one speed moving through life, the idea behind mindfulness is to help youth learn to become more aware of their emotions and thoughts. They learn to recognize what state of mind they are in and how this impacts their actions and reactions. By focusing one's attention, being observant, and describing one's experience a youth learns to slow down and consequently respond to a situation less impulsively.

All of these different techniques are molded to fit the situation the youth are in and tailored to fit the culture and age group of the participants. They often have discussions about the violence that is impacting their communities, especially when peers and neighbors are involved in the violence. SPARCS helps youth identify and understand their triggers, and develop skills to navigate the challenging environment these youth encounter on a daily basis.

1.4.2 Wraparound Services

While SPARCS intends to equip youth with a set of tools to navigate the stressors in their lives and make better choices, many high-risk youth might be unwilling to give the sessions a chance. Advocates, or "super mentors" from YAP are the key to getting youth engaged in the program initially and participating in SPARCS sessions. Advocates are the first point of contact for the program and are persistent in recruitment. They have a policy of trying multiple forms of contact, including house visits, before they give up on a youth.

While advocates are responsible for encouraging SPARCS attendance and often help the

14. Mindfulness teaches awareness of thoughts, bodily sensations, feelings, the surrounding environment. They teach 3 states of mind: emotion mind (always reacting without thinking), reasonable mind (always logical and no emotion), and wise mind(a balance between emotion and reasonable mind).

youth physically attend many of the sessions, they also provide a wide range of services that support the youth in the challenges they face. This includes case management, crisis intervention services (available 24/7), skill development, educational support, vocational work, and connections with social and professional supports through the development of individual service plans.

In the beginning, each youth and family is assigned an advocate. Services begin with the advocate convening a youth family meeting with the youth, parents, other family members, and any others who might be able to support the youth. The advocate is the first point of contact for the C2C program, and introduces the program to the youth, as well as obtains consent from the youth and guardian. During this initial intake meeting, the parents and youth start by identifying the areas of life that are most important to them, and their strengths in these areas. They list community groups they'd like to be a part of or the types activities they enjoy, and generally talk about what is going well in all aspects of their life. They then discuss areas of their life in which they would like to see improvement. Through this meeting, the advocate is able to get a picture of the youth's life and how they can advocate on the youth's behalf to help the youth reach the best version of themselves. The advocate, with the cooperation and input of the family and youth, then draws up a service plan (including action steps and goals) based on the improvement areas and strengths identified. The model is rooted in the fact that all youth, adults and families have strengths that can and should be developed. The principle of strength-based services encourages teams to create goals that reflect building family and youth assets, capacities and resilience, rather than focus on deficits and problems.

These plans are usually focused on increasing stability in relationships and living situations, and helping the youth and families live and thrive in their natural settings. The goal is to mitigate patterns of violence by addressing the youth's home, school, employment and community needs. At the same time, the model is flexible in that it allows for service level changes as the client's needs evolve. Advocates meet with their YAP supervisor weekly

to plan how they are going to work on the youth's and family's goals for the next week. Advocates are often acting in loco parentis, bringing order and resources into the home lives of clients that lack either or both.

Throughout the intervention, the advocate engages in one-on-one meetings with the youth, family meetings, and weekly recreational groups. Group activities tend to be fun oriented outings such as playing basketball at a local gym, going to see movies, eating at various restaurants, visiting a recording studio, attending concerts, going to theme parks, go-kart racing, laser tag, shopping, etc. These activities provide a cost-effective way for advocates to engage with their clients and are an opportunity for youth to interact in positive ways with new peers. They are also an efficient way to keep clients occupied during high-risk times, often in activities outside of their neighborhood. Typically, advocates spend about 5-12 hours per week with the youth, and this levels off as needs change (average for the 5 months is 8 hours per week). However, all advocates understand that the group and individual activities they set up with their clients are not what they believe to be the major goal of the program. These interactions served as the site where they can get to know their clients, build trust, find out how to motivate them, support them and encourage them to follow the advice that the advocate provides. Advocates often call their clients, as well as use texting to check-in and set times for group or individual activities, while also being on call 24/7.

The goal is that the relationship between the youth and the advocate serves as a motivational guide to redirect the youth's thinking and actions. Advocates and youth share similar backgrounds, which helps youth develop functional and trusting relationships that are key to helping them continue to engage in the program. Advocates are often local experts who understand the neighborhoods needs and strengths. They serve anywhere between 5-15 youth at time to ensure small caseloads. Their clients tend be a mix of youth enrolled in middle school and high school, although YAP tries to assign youth who attend the same school to the same advocate. This makes school visits more efficient, and gives advocates

an opportunity to interact with several of their clients' teachers, school administrators and attend SPARCS sessions scheduled at the school.

When the program months overlap with the school year, advocates spend at least some time each week at schools that their clients attend. Although some of the youth are not regularly attending school initially, the advocates encourage attendance and participation. In this venue, they communicate directly with school staff, asking administrators and security guards about any disciplinary incidents involving their clients, checking in on grades, collecting homework if their clients are suspended, working with teachers and counselors to get past assignments so their clients can catch up. Schools (including alternative schools) tended to be the largest referral source for the C2C program, so school staff were aware of the challenges of working with the C2C youth. Describing his work at various schools, one advocate shared, "Having a partnership with the schools is very important because it allows you to set up a plan. It also gives the school momentum with the clients because they see someone else outside of the family being involved in this kid's life, maybe we [as school staff] can work with [the client]."

Recognizing that many of their clients need assistance meeting basic needs, advocates work with their clients individually to provide them with the resources they need. Advocates note that they schedule and transport clients to doctor's appointments, particularly for clients who are pregnant or who have children, accompany them to scheduled court dates, and help them obtain a driver's license or copy of their birth certificate. If youth need school supplies, or even basic necessities, YAP has flex funds to pay for emergency needs. Advocates often buy their clients winter coats when they have none and as well basic clothing items like socks and underwear.

Likewise, wherever possible, YAP tries to provide their clients with employment opportunities. They will help the youth find a job, prepare and transport them to any interviews, and if funding permits, will even subsidize wages to encourage hiring. A good proportion of youth participate in Chicago's summer youth employment program through the assistance

of their advocates.

The core of the model is to build the social capital for the youth and create an engaged and sustainable family team working in partnership with the advocate. YAP and the advocate also ensures that any mental health, drug or alcohol and other needed services are arranged and utilized by the youth and the family as agreed upon the services plan. Because the program is very individualized to the youth and the family needs, what services are provided is highly dependent on the needs of the youth and the family. If housing is an issue, YAP can provide housing assistance through housing subsidies or housing advocacy. They can help parents with household management strategies and even help them make appropriate plans for childcare.

1.4.3 Why SPARCS + YAP?

Given that SPARCS and YAP could be effective in different ways in terms of engaging at-risk youth and reducing crime, there are reasons to expect combining these two elements will lead to positive interaction effects. During the C2C program, the YAP advocates typically will attend the SPARCS sessions with the youth. As many advocates have indicated through interviews, they try to reinforce the SPARCS skills and ideas in their individual and group interactions with youth. This allows for more "learning by doing" and creates opportunities for youth to develop new habits that would benefit them as the program ended. Furthermore, since the advocates spend a majority of the time with the youth, they can highlight key moments when SPARCS techniques can be useful. Youth will typically call or text the advocate if a stressful or concerning issue has come up. Advocates can use these opportunities to remind the youth of helpful coping strategies. This is strengthened as the SPARCS therapists and YAP advocates try to maintain a close relationship to track the progress of the youth. One advocate mentioned that the SPARCS therapist, "gives me insight on what I should be looking for before the next [session]. I communicate with him on what I saw."

Likewise, as advocates are often from the same violent neighborhoods, many of the advo-

cates themselves have indicated the benefits of attending SPARCS. In interviews, advocates have highlighted how they did not realize that past experiences could have impacted how they approach situations, and try to incorporate the various SPARCS techniques in their own lives.

Through conversations with advocates and SPARCS therapists, we have also found that the advocate-youth relationship is key to engaging the youth, and is strengthened with the SPARCS element. Anecdotally, many program participants mentioned that if it weren't for their advocate, they would have been unwilling to even give the therapy a chance. Therapists have indicated that because the youth trust the advocate, the youth are then willing are willing to show up for group sessions and give the therapy a chance. At the same time, seasoned advocates in YAP have indicated that normally it takes youth in YAP a long time to feel comfortable talking about their feelings or sharing what they are going through, and some never even get to this point. However, in C2C youth are sharing and communicating much earlier in the relationship because of the practice they are getting in the SPARCS sessions. Because of this, the youth-advocate bond is further strengthened.

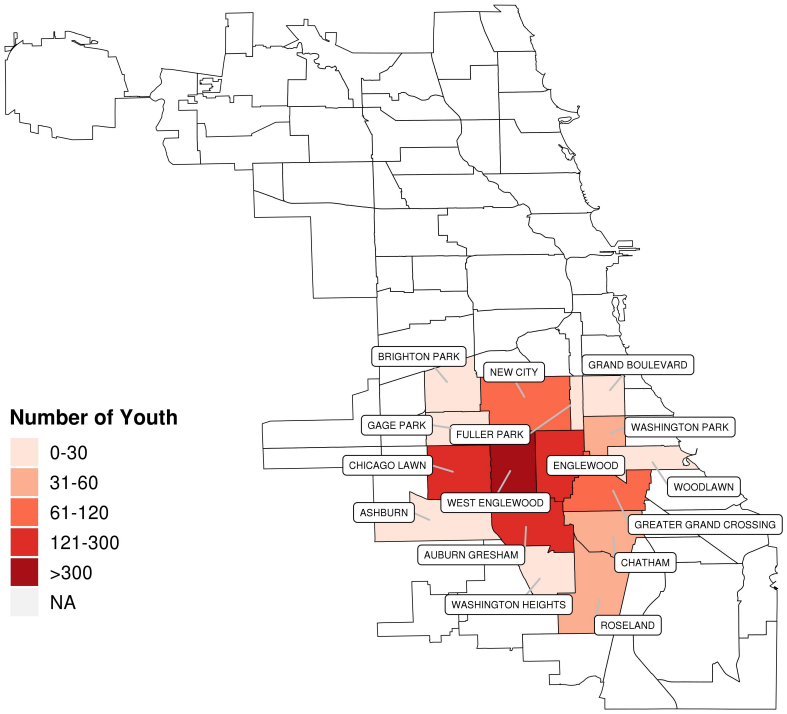
Lastly, we find that this model has helped youth stay connected to social services even after program end. Given the transformative impact this program had on many youth, youth are more willing to sign up for other services and programs in the neighborhood after C2C ends. An individualized discharge plan is designed for every youth, with advocates helping identify programs the youth could most benefit from and individuals in the community that can continue to provide support. Many youth from C2C end up in other behavioral informed programs run by CHA or programs like "After School Matters", an after-school and summer opportunity program.

With this bundled treatment costing about $4500-5500 per youth, we will only be able to identify the effect of the program as a whole. We will not able to detect the relative contribution of the SPARCs or YAP programming or the added interaction effect of combining these two program components.

1.5 Study Population

Over the past three years, the research team has worked closely with the program providers to reach out to referral agencies able to identify eligible youth who might benefit from C2C. The study population was drawn from a boundary area slightly larger than the Englewood and West Englewood neighborhoods on the south side of Chicago. Since the YAP advocates frequently travel to the youth's place of residence and school, geographic boundaries were drawn at the start of the program in order to ensure advocates were not driving excessive distances to reach their youth. Specifically, the boundary was no farther south than 87th Street, no farther north than 51st Street, no farther east than Cottage Grove, and no farther west than California Ave. This includes parts of the following neighborhoods: Ashburn, Auburn Gresham, Chatham, Chicago Lawn, Englewood, Fuller Park, Gage Park, Grand Boulevard, Greater Grand Crossing, New City, Roseland, Washington Heights, Washington Park, West Englewood, and Woodlawn. Figure 1.1 shows a map of where these youth were drawn based on their address of residence at the time of referral.

Figure 1.1: Cohort 1-5 Communities Served



The three years of programming for all 5 cohorts began in November 2015 and wrapped up services in September of 2018. The analysis presented in this paper covers cohort 1 (served from November 2015 to March 2016), cohort 2 (served from April 2016 to August 2016), cohort 3 (served from November 2016 to March 2017), cohort 4 (served from April 2017 to September 2017), and cohort 5 (served from April 2018 to October 2018) with a sample size of 488 participants and 1537 youth randomized. Randomization and enrollment were done on a rolling basis, and so the dates mentioned above are approximate while all youth did receive the minimum length of programming of 5 months. By cohort 5, the model changed slightly to allow more time for discharge and the official program length was extended to 6

months. [15]

Across all 5 cohorts, 1537 were randomized into treatment and control groups stratified by referral source. For all cohorts, in the months preceding engagement with youth, YAP and CHA conducted outreach to community service providers and schools in the greater Englewood area to identify and recruit youth who would meet the target population for this program. Specifically, referral sources were asked to identify youth between the ages of 13-18[16] who were:

- actively gang-affiliated or at risk or gang engagement
- on juvenile probation
- previously found guilty of weapons offenses
- seriously disruptive in school through chronic truancy, serious misconduct and/or frequent suspensions
- direct victim of or witness to traumatic violence

The study team received referrals from CPS, including neighborhood and charter high schools (50%) and alternative schools (15%), the Student Outreach and Re-engagement Centers (23%) focused on re-engaging chronically truant and out-of-school youth and the Office of Safety and Security (8%), as well as Cook County Juvenile Probation (4%).

Table 1.1 shows that for these first five cohorts, the program succeeded in identifying disadvantaged youth in need of services. Of those eligible youth referred during the first five cohorts, youth were almost 16 years old and in the first few years of high school. About 94% of the youth are Black and almost all youth qualify for free or reduced lunch. On average,

15. A sixth cohort began services in December of 2018, and future cohorts are likely as more funding is provided.

16. Although the program targeted 13-18 year olds with respect to the program letters and advertisement sent out to the referral agencies, occasionally youth outside of this age range was served if age was not available prior to randomization.

study youth had missed about 10 weeks of school, or more than 28% of the 180-day school year prior to the intervention. A significant percentage had disciplinary incidents in school (33%), and had been previously arrested (37%). About 21% experienced homelessness and 22% were not enrolled in school at some point during the prior school year (with 13% not enrolled in the prior semester). Randomization was successful in balancing the treatment and control groups across the vast majority of baseline characteristics (see Table 1.1). An F-test for joint significance of all baseline characteristics shows we cannot reject the null hypothesis that treatment and control groups are equal. [17]

Table 1.1: C2C Baseline Check, Cohorts I, II, III, IV & V

Variable	Mean T	Mean C	Diff	p-value	Obs
Age	15.79	15.79	0	.775	1537
% female	.44	.41	.03	.164	1537
% black	.93	.950	-.02	.342	1537
% hispanic	.05	.05	0	.421	1537
School grade	9.87	9.78	.09	.111	1471
Free/reduced lunch status	.95	.95	0	.653	1537
Learning disability	.16	.18	-.02	.36	1449
Homeless	.2	.22	-.02	.43	1449
Prior GPA	1.99	1.91	.08	.309	1238
Not enrolled	.12	.14	-.02	.192	1537
Not enrolled at any point during prior school year	.2	.23	-.03	.255	1537
Any previous school incident	.32	.34	-.02	.598	1328
Previous days present at school	134.37	128.62	5.75**	.017	1431
Any prior arrest	.36	.39	-.03	.283	1537
Any prior violent arrest	.23	.26	-.03	.132	1537
Any prior property arrest	.15	.19	-.04**	.04	1537
Any prior drug arrest	.07	.06	.01	.533	1537
Any prior other arrest	.23	.26	-.03	.14	1537
Joint F Test		P-value = 0.431			

Significance between the mean differences for the treatment vs. control groups was estimated using a linear regression with block level and cohort fixed effects. Standard errors are robust. A joint F test was performed to test their joint significance. *** p<0.01, ** p<0.05, * p<0.1

17. An F-test including and not including CPS variables returned similar F-statistics despite the number of missing baseline variables we have for CPS variables such as GPA and attendance. For controls in the regressions we include demographic covariates (age/race/gender dummies), school grade, not enrolled at any point during the prior school year indicator, prior enrollment in free lunch benefits, and prior arrest records.

1.6 Data and outcomes measured

1.6.1 Criminal justice outcomes

To measure the impact of C2C on criminal behavior, we match our study youth to Chicago Police Department (CPD) arrest data using a probabilistic match based on first name, last name, and date of birth.[18] We breakdown number of arrests by crime type (violent, property, drug, or other) as the program could have differential impacts on type of arrest.[19] Furthermore, we look to see how the impact of the intervention changes over time by measuring outcomes at year intervals up to 24 months post randomization while also presenting results at 6 months–during the program period. Currently, we are only able to measure 24 months post for roughly half of the study population given the timing of the cohorts and when youth were randomized. Examining these time intervals will provide some insight into whether the program succeeded mainly due to an incapacitation effect (by keeping the youth busy during the program) or if the effects persist once the program has ended. All of these outcomes have been pre-registered with the AEA's RCT registry.

For our criminal justice outcomes we show both the number of arrests after a C2C peer's randomization, and the probability of having any arrest after a C2C peer's randomization. Given that the number of arrest variable is skewed with a mass at zero, an analysis of the average change might miss some important movement like engagement with the juvenile justice system at all. Therefore we also look at the extensive margin, as any engagement with juvenile justice system can have negative consequences for youth and society.

Using administrative arrest records we are able to avoid the issues that come with self-reported survey data such as sample attrition, high-mobility and under-reporting of socially undesirable behavior (Kling et al 2007; Kling et al 2005). However, we should note that arrest records are not a perfect measure of criminal behavior. For arrests to be an accurate

18. Appendix section 1.10.2 discusses details of matching.

19. The type of arrests that are decreased will also impact the benefit cost ratio calculated for this program.

measure of criminal behavior we'd have to assume that the intervention did not change the chances a crime results in arrest. Given that we are seeing a substantial change in violent crime arrests, it is unlikely that officers are exercising discretion in these type of crimes (which includes assaults, homicides, sexual offenses, robbery, etc). However, even if in some contexts a change in behavior is coming from de-esclating a situation to avoid arrest (particularly those in the "other" arrest category that includes trespassing, disorderly conduct, vandalism), given the harms associated with the criminal justice system this would still certainly have some social benefit.

One important note about arrest outcomes is that they solely rely on administrative CPD data. Therefore we do assume that if a youth is not matched to CPD they essentially have "zero" arrests. However, if youth are moving out of CPD jurisdiction, these zero arrests would be incorrect and would be a concern if differential attrition occurred across treatment conditions. To assess whether study youth might be moving out of Chicago at differential rates, we use Chicago Public Schools (CPS) transfer data given that we have CPS data for 99.5% of all study youth. CPS records all youth who transfer, and in particular note if they have transferred outside of Chicago. Using this indicator, we find no evidence of a differential rate of departure for those assigned to treatment compared to the control group and the rates of moving out at all are small even up to two years post randomization (please see table 1.14 in the Appendix). Specifically, we find that one semester after randomization, about 6.8% of study youth are recorded as transferring out of Chicago, two semesters after randomization, the mean is about 8%, three semesters after randomization the mean is about 7.3%, and four semester after randomization, the mean is about 5.4%. It is also very possible for a youth to move out of Chicago temporarily and then move back into CPS.[20] All results we discuss today are robust to not including these youth who are flagged in CPS data as transferring out

20. Further iterations of this paper will also use data from Illinois State Police data to compare to CPD outcomes as a robustness check, however there are some concerns about the quality of matches among Illinois State Police data.

to address any potential concerns about attrition and how the arrest variables are created.[21]

1.6.2 School outcomes

To measure the impact of the program on schooling outcomes, we will use longitudinal student-level CPS data for baseline characteristics and outcomes. When considering what would be our primary outcomes we thought carefully about the type of impact we might see on the education front conditional on the quality of the data we have in CPS. Specifically, our primary outcomes include attendance, persistence in school (either graduated or still enrolled), and graduation. The program focused on getting youth re-enrolled in school if they were not, increasing their attendance and general engagement, and helping students catch up on credits if they were behind. However, they had less capacity to help with tutoring or other substantive ways to actually increase GPA or test scores. And if these youth were on the margin of dropping out, it is possible that we would observe a negative impact on GPA if the program succeeds in bringing them back to school. Therefore our secondary outcomes include percent of course failures, GPA, and misconduct incidents in schools. Ideally, we would want misconducts or any other behavioral outcome measure in school as part of our primary outcomes where the program is likely to impact. However, charter and alternative schools are not required to report misconduct data to CPS central (our source for data). Given that 25-31% of students are in schools that do not report misconduct information, many of these alternative schools, we are slightly cautious in how we interpret those results.

One potential concern is the amount of missing values we have for the CPS baseline and outcomes variables we listed. Given the tenuous connection many study youth have to school and the fact that many attend non-traditional schools, missing values for baselines and school outcomes is a concern. Table 1.15 in the Appendix displays the percent missing values for each outcome in each time horizon, and the cohorts of data each time horizon

21. These results are available upon request.

covers. About 25% of baseline GPA is missing, however missingness becomes a bigger issue with our outcomes, as some semesters are missing up to 46% of GPA. Other variables such as attendance and school misconducts are missing as well, but at lower rates ranging from 22% to 45%. Students could be missing data because they attend a charter school or a non-traditional school that does not report to CPS, or a district school yet does not attend enough to have data.[22] We account for the missingness in five ways and look for consistency across these various methods. First we show results after doing a set of logical imputations where we use information in the data to confirm if a student has dropped out, and report regressions with only those have non-missing values after this imputation. This assumes that data are missing completely at random. Our second approach imputes all missing with zeros to assume a "worst case" scenario situation with the missigness (that they dropped out). Our third approach imputes with treatment and control means. Our fourth approach uses a combination a combination of imputed group means with logical imputation, imputing zeros when it's likely that youth have dropped out.[23] Lastly, we use a Bayesian approach, multiple imputation, to relax the missing completely at random assumption.[24] Section 1.11 in the Appendix includes more details about each of these imputation methods.

1.6.3 Take-up

Of those assigned to treatment, 62.2% took up treatment over the course of the first five cohorts of program delivery. Considering the highly at-risk population referred and that participation is optional, these take-up rates show considerable promise in C2C's ability to

22. While almost all students who attend a charter are missing data, almost half of the missing values are coming from district schools as well which might include alternative schools

23. Specifically, we impute missing cases with treat/control means if a study participant meets at least one of the following criteria: (i) appears as actively enrolled in CPS on a given semester/year; (ii) has any kind of transfer status within CPS records for that semester/year; (iii) is currently enrolled on charter school or an option school; (iv) is currently enrolled on a school that does not report GPA; (v) attendance is at least 70 days for the given year. For all missing data that do not meet at least one of the conditions described above, we impute them with zeroes.

24. All regressions that include any imputation include an indicator for missingness.

engage youth most in need of services.

To demonstrate that youth on the higher end of the risk distribution signed up for programming, we can benchmark the take-up rates with the Becoming a Man (BAM) study (by Heller et al. 2017) that worked in the same high-violent Chicago neighborhoods and offered youth in-school CBT programming.

First of all, to be in the BAM study you had to have been actively enrolled and attending school enough to benefit from an in-school intervention. Youth who rarely attended school (missing either 60% of school days or had failed 75% of classes) or had serious disabilities were excluded from BAM randomization. Among this in-school population, BAM take-up was about 50%. In contrast, 25% of the C2C study youth were either missing more than 60% of school days or failing more than 75% of classes. Among this disconnected population, C2C still achieved a take-up of 49%.[25] These numbers demonstrate that C2C is working with a larger population that is disconnected from school and are helpful in contextualizing the take-up rate differences between these two programs. Furthermore, while BAM and other in-school interventions typically work in neighborhood schools, half of the C2C referrals did not come from neighborhoods schools and instead were referred from alternative high schools, probation, or school re-engagement centers.

As another risk factor, we can look at the percentage of youth who have a prior involvement in the juvenile justice system. C2C was able to achieve a higher take-up rate among youth who have a prior arrest record. For youth with a prior arrest record, the BAM take-up was 44%. In C2C, the take-up among youth with a prior-arrest record was 52%.

Lastly, we also use a predictive model that uses administrative CPD information about the arrest and victimization histories of each person recently arrested in Chicago, along with information about the arrests and victimizations of their associates, to predict the probability of future violence involvement. We obtained predicted scores for violence involvement for

25. We can also look at youth who were not enrolled at any point during the prior school year–about 22% of the C2C study and find a take-up rate of 46% among this population.

the first cohort of C2C youth and the second study of BAM at the time of randomization for both of these studies. We find that of the youth referred for services for cohort 1, 46% of the youth are in the top decile of risk of committing a violent offense. This is in sharp contrast to the youth offered the in-school intervention BAM, where only 21% of the study youth are in the top decile of risk for violent offense.[26] Using this risk model, we find that of the BAM youth in the top decile of predicted risk for violence involvement within the citywide distribution, 45% randomized to treatment signed up for the program. By comparison, within C2C, of those in the highest decile of predicted risk of violence involvement, the C2C program achieved a 47% take-up rate. This is slightly higher than BAM, even though as we have demonstrated earlier, C2C is working with many youth disengaged from school unlike BAM.

These take-up rate comparisons serve the purpose of demonstrating that C2C was able to not only receive referrals from a higher-risk population, but also successfully engage these youth only marginally connected to school in a behavioral informed intervention.

1.7 Analysis Approach

To estimate the intent to treat (ITT) effects, we run the following regression:

$$Y_{ijt} = \alpha + \beta^{ITT} Z_{ij} + \gamma X_{i(t-1)} + \phi_j + \epsilon_{ijt} \tag{1.1}$$

Where Y_{ij} represents the outcomes of interest during the post-randomization period for individual i, in block j. Z_{ij} represents the random assignment indicator for each youth within each block (j). X_i are the baseline covariates to improve precision by accounting for residual variation in the outcome of interest, and, ϕ_j are the set of dummy variables indicating the observation's randomization block (by referral source). The ITT captures the impact of

26. A limitation of this risk model is that risk scores could not be obtained for youth not found in CPD data. Therefore, we assume a zero risk of violent offense in these summary statistics although that may be an understatement.

being offered the C2C program, and may be more relevant for policy purposes. However, given that not everyone who is offered the program participates, we present the effects of participating in the program as well.

To estimate the effect of participating in the program, we estimate the effect of the treatment on the treated (TOT) by two-stage least squares using the random assignment indicator as an instrument for participation.[27]:

$$P_{ijt} = \alpha_1 + \beta_1 Z_{ij} + \gamma_1 X_{i(t-1)} + \phi_j + \epsilon_{ijt1} \tag{1.2}$$

$$Y_{ijt} = \alpha_2 + \beta_2 P_{ij} + \gamma_2 X_{i(t-1)} + \phi_j + \epsilon_{ijt2} \tag{1.3}$$

Where P_{ij} indicates participation in the program by a youth in a block after randomization. β_2 approximates the TOT given that there is no control crossover.

Most of the outcomes we seek to learn about involve variables that take only a limited number of values (e.g: binary variable for being arrested at all or number of arrests within a time frame). Some may argue that nonlinear models such as probit and Tobit are preferred in these cases when the outcome of interest in not continuous. All of our main results will use ordinary least squares given that OLS gives us the average causal effect without additional distributional or functional form assumptions. Likewise, we know that OLS will also always give us the minimum mean squared error linear approximation to the conditional expectation function, and IV will capture the local average treatment effect (or in this case the treatment on the treated given no control crossover) even in the cases where we have dependent variables that take limited values (Angrist and Pischke 2009). To address any potential concerns we do estimate our results using a probit model and find similar effect sizes.[28]

27. This analysis requires the typical relevance and exogeneity assumptions of instrumental variables. In order for the random assignment variable to be a valid instrument, it must be correlated with program participation and uncorrelated with observables. It must shift participation in a uniform direction across people (the monotonicity assumption).

28. These probit results available upon request.

We define take-up as participating in at least one SPARCS session or receiving at least five hours of YAP services. Across cohort 1-5 , the take-up rate was about 62.2%. Among the youth who participated, they attended an average of 9 SPARCS sessions and received 170 YAP hours total (see Table 1.2). These YAP hours include both individual and group time with the advocate. Typically it took about a month from the time a youth was randomized for the youth to begin services.

Table 1.2: SPARCS sessions & YAP hours, Cohorts I, II, III, IV and V

Cohort	Avg. SPARCS sessions	Avg. YAP hours	Obs.
Cohort I	5.88	148.55	104
Cohort II	8.5	183.47	112
Cohort III	11.74	158.62	118
Cohort IV	11.6	175.86	94
Cohort V	5.61	192.53	60
Cohort I, II, III, IV, V	9.06	169.59	488

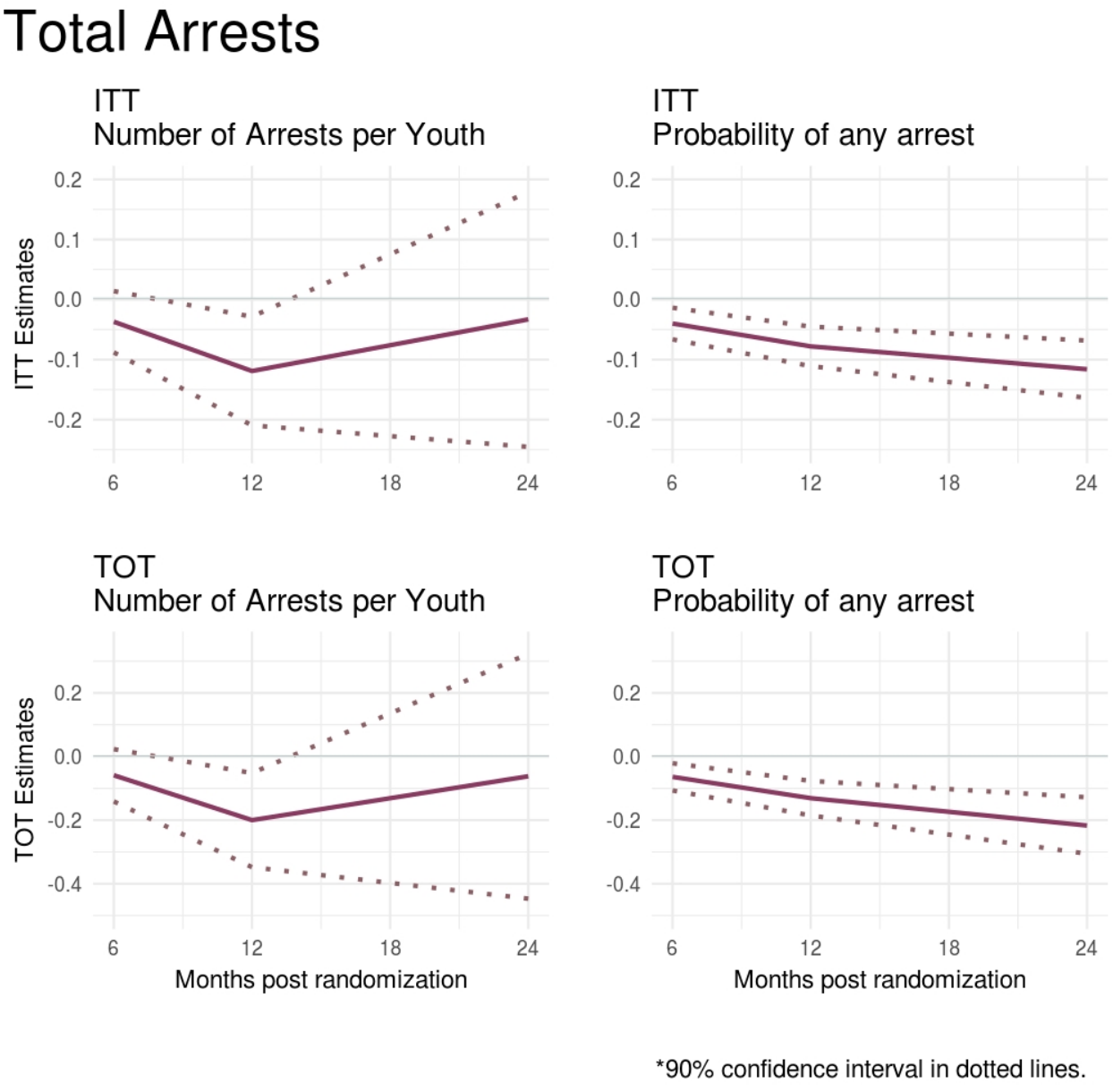
* Averages shown for program participants only.

1.8 Results

Graphs 1.2 and 1.3 display the ITT and TOT results for total and violent arrests at 6, 12, and 24 months post randomization. While we are interested in the long-term impact C2C can have on these youth, we display the 6 month interval to demonstrate that most of these reductions in arrests came after the program end, suggesting that the results are not driven by incapacitation effects of the intervention itself. We show both the probability of any arrest and the number of arrests per person. Given the timing of the intervention and current data availability, we can follow almost all of cohort 1 and 2 up to 24 months post randomization. For cohorts 3 and 4, most outcomes end around 12 months post randomization. Cohort 5 is followed for about 6 months. We see visually consistent significant reductions across both margins for total and violent arrests, with the standard errors increasing over time as

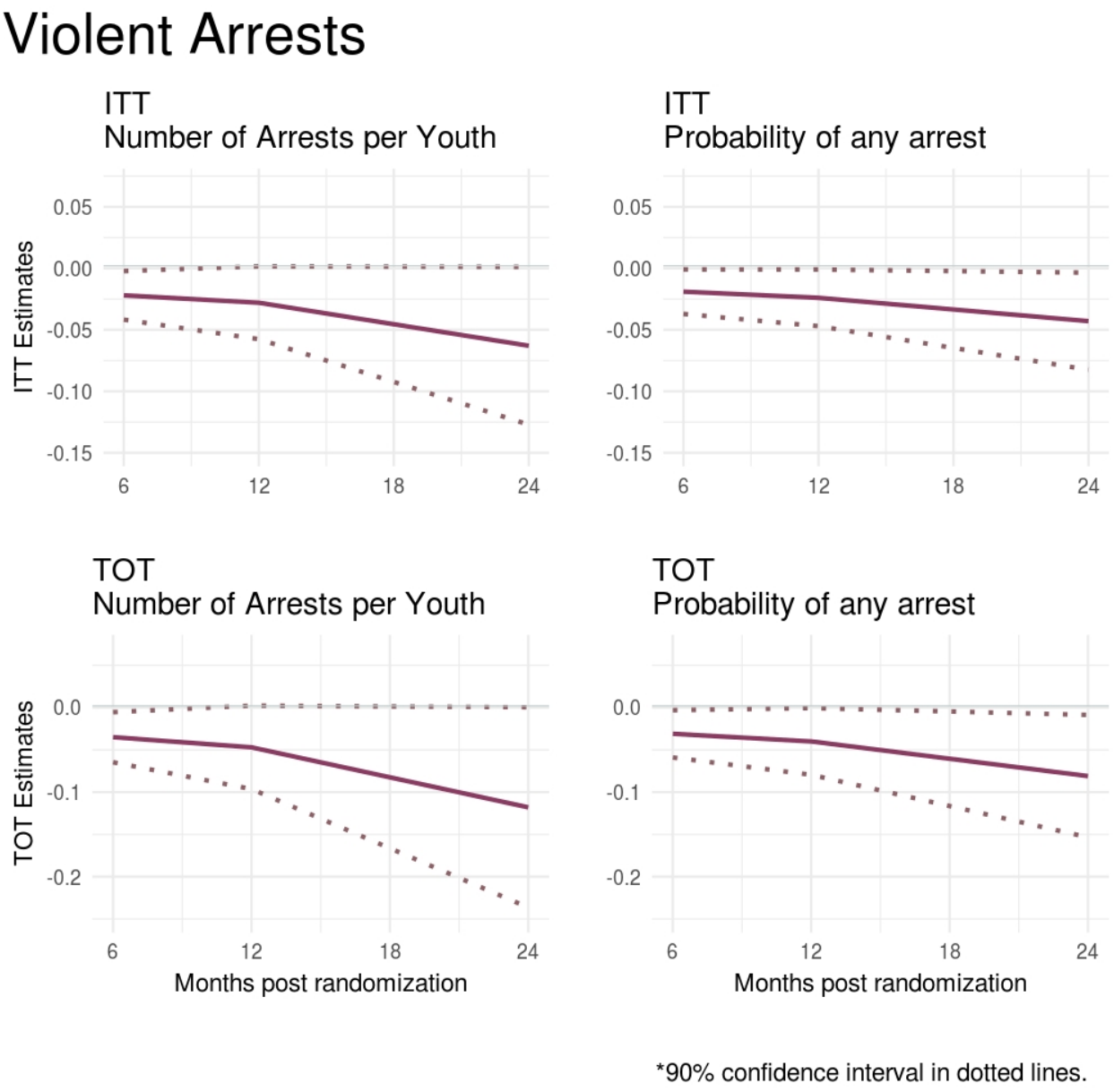
the sample size drops. Except for number of total arrests, all other estimates appear to be growing over time, even after program end.

Figure 1.2: Time Path for Changes in Any Arrest



This graph shows the total arrest ITT and TOT estimates (and the 90% confidence interval) for cohort 1-5 (where only cohorts 1 and 2 go up to 24 months post-randomization). Both number of arrests per person and extensive margin (probability of any arrest) results are shown.

Figure 1.3: Time Path for Changes in Violent Arrests



This graph shows the violent arrest ITT and TOT estimates (and the 90% confidence interval) for cohort 1-5 (where only cohorts 1 and 2 go up to 24 months post-randomization). Both number of arrests per person and extensive margin (probability of any arrest) results are shown.

Table 1.3- 1.6 shows more details including the percent changes benchmarked against the control group (the full set of details for these findings can be found in table 1.16 in

the Appendix.). These tables include the mean of the control group, the ITT, the control complier mean (CCM), and the TOT, and the percent change.[29] To help benchmark the TOT, we estimate the mean for those who would have taken up treatment had they been offered it–the CCM (Katz et al. 2001). It is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT.

These results show that being offered the C2C program decreases the probability of having any arrest by 27.2% compared to the control mean within one year of randomization ($p < .01$), and a 26.4% reduction by two years post-randomization also significant at the 1% level.[30] The total number of arrest results are consistent with the extensive margin results generally except with the 24 month post time frame where the percent change is much smaller. What is interesting to note is that two years post randomization almost 44% of the control group have some arrest, indicating the program certainly targeted the right group of youth and helped reduce this by 11.6 percentage points. When we account for participation in the program, we see equally significant reductions in the probability of any arrest with the percent change ranging from 37.9% at 6 months to increasing to 46.4% at 24 months post randomization.

With respect to violent crime reductions (Table 1.5 and 1.6), the percent changes are similar in magnitude to the overall arrest category however our standard errors are slightly larger with estimates significant at the 10% level consistently across all time horizons.[31] We do find that just being offered the C2C program reduces the probability of any violent arrest by almost 30% of the CM initially to 22% at 24 months post randomization. When we account for participation again we see reductions on both margins ranging from 39% to 46% of the CCM. For violent arrests, we do see more consistent reductions across both the number of arrests per youth and the extensive margin (probability of any arrest) for violent

29. The percent change is defined as TOT/CCM for the TOT results and IT/CM for the ITT results.

30. Typically the program starts up about one month after randomization.

31. Violent arrest covers: assaults, homicides, sexual offenses, robbery, and kidnappings

arrests, both in terms of magnitudes and significance. Table 1.16 includes a few more details regarding these results.

Table 1.3: ITT All Arrests

	Number of Arrests Per Person			Ever Arrested			
Months	ITT	CM	% Change	ITT	CM	% Change	N
6	-.037	0.275	13.5	-.04**	0.181	22.1	1537
	(0.031)			(0.016)			
12	-.119**	0.592	20.1	-.078***	0.287	27.2	1398
	(0.055)			(0.02)			
24	-.033	1.178	2.8	-.116***	0.439	26.4	784
	(0.129)			(0.029)			

We show the number of arrests per person and the probability of ever being arrested at various time intervals (6, 12, and 24) post randomization. ITT estimates were calculated using randomization strata fixed effects, cohort fixed effects, and robust standard errors. TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment. CM is the control mean, and CCM is the control complier mean–those who would have taken up treatment had they been offered it. CCM is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT. We include demographic covariates (age/race/gender dummies), school grade dummies, prior enrollment in free lunch benefits, prior arrest records, and cohort fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: TOT All Arrests

	Number of Arrests Per Person			Ever Arrested			
Months	TOT	CCM	% Change	TOT	CCM	% Change	N
6	-.059	0.197	29.9	-.064**	0.169	37.9	1537
	(0.05)			(0.026)			
12	-.2**	0.481	41.6	-.131***	0.283	46.3	1398
	(0.09)			(0.033)			
24	-.062	0.755	8.2	-.217***	0.468	46.4	784
	(0.234)			(0.054)			

We show the number of arrests per person and the probability of ever being arrested results at various time intervals (6, 12, and 24) post randomization. ITT estimates were calculated using randomization strata fixed effects, cohort fixed effects, and robust standard errors. TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment. CM is the control mean, and CCM is the control complier mean–those who would have taken up treatment had they been offered it. CCM is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT. We include demographic covariates (age/race/gender dummies), school grade dummies, prior enrollment in free lunch benefits, prior arrest records, and cohort fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: ITT Violent Arrests

	Number of Arrests Per Person			Ever Arrested			
Months	ITT	CM	% Change	ITT	CM	% Change	N
6	-.022*	0.068	32.4	-.019*	0.064	29.7	1537
	(0.012)			(0.011)			
12	-.028	0.124	22.6	-.024*	0.105	22.9	1398
	(0.018)			(0.014)			
24	-.063	0.271	23.2	-.043*	0.196	21.9	784
	(0.039)			(0.024)			

We show the number of arrests per person and the probability of ever being arrested at various time intervals (6, 12, and 24) post randomization. ITT estimates were calculated using randomization strata fixed effects, cohort fixed effects, and robust standard errors. TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment. CM is the control mean, and CCM is the control complier mean–those who would have taken up treatment had they been offered it. CCM is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT. We include demographic covariates (age/race/gender dummies), school grade dummies, prior enrollment in free lunch benefits, prior arrest records, and cohort fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table 1.6: TOT Violent Arrests

	Number of Arrests Per Person			Ever Arrested			
Months	TOT	CCM	% Change	TOT	CCM	% Change	N
6	-.035*	0.080	43.8	-.031*	0.074	41.9	1537
	(0.018)			(0.017)			
12	-.047	0.131	35.9	-.04*	0.103	38.8	1398
	(0.03)			(0.024)			
24	-.118*	0.258	45.7	-.081*	0.183	44.3	784
	(0.072)			(0.044)			

We show the number of arrests per person and the probability of ever being arrested at various time intervals (6, 12, and 24) post randomization. ITT estimates were calculated using randomization strata fixed effects, cohort fixed effects, and robust standard errors. TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment. CM is the control mean, and CCM is the control complier mean–those who would have taken up treatment had they been offered it. CCM is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT. We include demographic covariates (age/race/gender dummies), school grade dummies, prior enrollment in free lunch benefits, prior arrest records, and cohort fixed effects. *** p<0.01, ** p<0.05, * p<0.1

While we concentrate on the impacts on total and violent arrests given the societal benefits these hold, we also look at 3 other categories: property, drug, and "other". Depending on the time frame, about 23-25% of the total arrest are considered violent arrests, roughly 16% are property, 6-8% are drug and 53% are "other."[32] This other category typically covers arrests such as trespassing, vandalism, probation violations and weapons charge. Table 1.7 shows the other category arrest results. We find that the total arrest reductions were likely driven by the violent arrests and the "other" arrest category where we also see significant reduction at the 6 and 12 month interval (56% reductions in the probability of any other

32. property covers larceny, burglary, arson, and motor vehicle thefts; drugs covers drug dealing and drug possession

arrest at 12 months post). The other arrests category are driven by reductions in trespassing and disorderly conduct. In the Appendix (Table 1.17), you'll find these results without baselines covariates and find very similar estimates in magnitude.

The consistency, magnitude and significance of these results are quite large given the short intervention (5 months), the duration of impact (up to two years post randomization) and the fact that over this same time period of program operation, juvenile arrest rates overall and violent arrests in particular were cut in half across the city of Chicago as well (see 1.6 and figure 1.7).

Table 1.7: C2C Arrest Outcomes, Cohorts I , II, III, IV, and V–Other Arrest Categories

Variable	Mean T	Mean C	ITT	CCM	TOT	Obs
Number of Arrests Per Person						
property arrests before 6 months	.043	.046	.003	.022	.005	1537
			(.012)		(.019)	
property arrests before 12 months	.087	.112	-.013	.082	-.023	1398
			(.021)		(.034)	
property arrests before 24 months	.212	.189	.037	.079	.07	784
			(.047)		(.086)	
drug arrests before 6 months	.026	.017	.01	-.007	.017	1537
			(.009)		(.014)	
drug arrests before 12 months	.035	.038	-.001	.015	-.001	1398
			(.013)		(.021)	
drug arrests before 24 months	.076	.093	-.03	.075	-.056	784
			(.025)		(.045)	
other arrests before 6 months	.101	.144	-.029	.101	-.046	1537
			(.021)		(.033)	
other arrests before 12 months	.212	.318	-.077**	.254	-.13**	1398
			(.035)		(.058)	
other arrests before 24 months	.617	.625	.022	.344	.042	784
			(.084)		(.153)	
Probability of Any Arrest						
Property arrest before 6 months	.033	.041	-.003	.028	-.005	1537
			(.009)		(.015)	
Property arrest before 12 months	.062	.083	-.015	.072	-.025	1398
			(.014)		(.023)	
Property arrest before 24 months	.121	.147	-.026	.142	-.049	784
			(.024)		(.044)	
Drugs arrest before 6 months	.018	.012	.007	-.005	.011	1537
			(.006)		(.009)	
Drugs arrest before 12 months	.024	.029	-.004	.013	-.006	1398
			(.008)		(.014)	
Drugs arrest before 24 months	.045	.067	-.026*	.063	-.049*	784
			(.016)		(.028)	
Other arrest before 6 months	.073	.111	-.029**	.095	-.048**	1537
			(.014)		(.022)	
Other arrest before 12 months	.121	.197	-.064***	.191	-.107***	1398
			(.017)		(.029)	
Other arrest before 24 months	.237	.292	-.041	.254	-.077	784
			(.027)		(.05)	

ITT estimates were calculated using randomization strata fixed effects, cohort fixed effects, and robust standard errors. TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment. CCM is the control complier mean–those who would have taken up treatment had they been offered it. It is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT. We include demographic covariates (age/race/gender dummies), school grade, prior enrollment in free lunch benefits, prior arrest records, and cohort fixed effects. *** p<0.01, ** p<0.05, * p<0.1

To control for multiple hypothesis testing within the different arrest categories tested, we present various methods in Table 1.18 in the Appendix: the family wise error rate (FWER), the false discovery rate (FDR), and the Sidak-Holm adjustment.[33] The FWER is the probability of rejecting at least one true null hypothesis in a family of hypothesis tests using the free step down bootstrap resampling method (Westfall and Young 1993).[34] We define our

33. Future iterations will also include an adjustment using randomization inference.

34. Following the recommendation, we use a bootstrap resampling technique that simulates data under

"family" of outcomes to be violent, property, drug or other arrests given that total arrests is a linear combination of the four categories.[35] While the FWER control limits the probability of making any type I error, it is well suited in cases in which the cost of a false rejection is high (Anderson 2008). In certain circumstances we might be more willing to tolerate type I errors in exchange for greater power. For example, in this paper we do not make any conclusions on program efficacy based on a single outcome. FWER adjustments can be severe as the number of tests grows, and so an alternative method that affords better power is the FDR or the expected proportion of rejections that are type I errors. It reduces the penalty of testing additional hypotheses. Lastly, we include the Sidak-Holm step down correction that assumes outcomes are independent (based on the Bonferroni-Holm) and generally thought to be more conservative when outcomes are correlated than the FDR and the FWER.[36].

Once we adjust for multiple hypothesis testing, our violent outcome results are no longer significant at conventional levels (they are now just above the 10 percent level). Given the strength of the total arrest outcomes (that would be robust to any p-value adjustment), we feel confident that C2C is reducing the probability of any engagement with the juvenile justice system as measured by arrests up to two years post random assignment.[37]

1.8.1 Sub-Group Analysis

Next, we begin to explore if the C2C program is more effective for certain young people compared to others. These sub-sample results were explicitly identified prior to analysis in our pre-analysis plan. Table 1.19 in the Appendix shows the TOT results when including an

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the null hypothesis using 100,000 replications.

35. Future iterations of the paper will define the family as including total arrest as well as a robustness check.

36. The Sidak-Holm adjusted p-values are calculated as: Sort the J unadjusted p-values so that $p(1) < p(2) < ... < p(J)$. Then calculate $[1-(1-p(1))^J, max[p(1), 1-(1-p(2))^{J-1}], ..., max[p(J-1), p(J)]]$. If the calculation yields a value larger than 1, then the adjusted p-value is set equal to 1.

37. Had we included the total arrest outcomes in our family of tests, they would have remained significant regardless of any of the adjustments used given the strength of the findings.

interaction term with being a female (the first column) and the TOT results when including an interaction with having a prior arrest (second column), and a F-statistic that tests the null hypothesis of joint zero values for both terms. We concentrate on the overall and violent arrest categories given the significant reductions we saw in the overall group. We generally see males responding stronger to the intervention compared to females with larger and significant reductions in almost any category and time frame.

When looking at the differential impact of C2C if you had a prior arrest, we find in almost every category that the C2C program had larger impacts for youth who already had a prior arrest prior to entering the program. This suggests that the program was able to engage and alter the behavior of youth more likely to re-offend. Although the magnitudes are often smaller (yet still significant at conventional levels often), C2C was also successful at preventing youth at higher risk of criminal involvement from engaging with the juvenile justice system at all.

1.8.2 Academic Results

Lastly, we evaluate if the C2C program increased school engagement and improved school outcomes for youth. Table 1.8 shows the pooled cohort 1-4 results across the different imputation methods discussed beginning with just the logical imputation/basic imputation (that only accounts for youth who have confirmed drop out codes) to multiple imputation methods. We have no schooling data for cohort 5 yet given the timing of when they completed the program. We do see consistent increases in the number of days attended within a year after random assignment across the different imputation methods, with TOT estimates ranging from almost 5.5 more days to 8 more days attended. Given the control means however this does not translate into a very large percentage increases (ranging from 5% to 7% increases). We do not find any significant or consistent results in the second year post random assignment, although the sample size does drop as well.

When we turn to looking at the impact of C2C on school persistence and graduation (see

table 1.9), we do not yet detect any impact on either of these dimensions. School persistence is defined as either being actively enrolled in CPS at that moment in time or have already graduated by then (we include two different measures: one that does not include transfers to school outside of CPS as having persisted in school, and one that does include them). Currently, we use a measure that only captures enrollment at a snapshot in time (at one year post randomization or two years post randomization), which may not be a very accurate measure of consistent enrollment for the purposes of persistence in school. Future iterations of this paper will include robustness checks that use alternative CPS data sources to confirm these findings.

For graduation, we have 3 different indices: the first one allows an extra year to graduate on track based on the school grade the person was in when they were randomized into treatment.[38] Our second index does not allow an extra year to graduate on time, and our last index just gives a binary indicator for any students having graduated by the beginning of the school year 2018-2019.[39] Across these different graduation measures, we do see slight positive coefficients, however the standard are very large and we are unable to make any conclusions at this point. Given that graduation is a long-term outcome, especially because most of the study youth entered between 9 and 10th grade, we likely need more time to be able to assess this result.

38. For students who have not yet had an opportunity to graduate on track plus one year are given a missing value and not included in this analysis.

39. Although given the previous two indices, we should note that only about half of the sample could have graduated by that date.

1.8.3 Aggregated Academic Outcomes

Table 1.8: C2C Attendance Outcomes, all imputation methods

		BASIC		ZERO		T/C MEAN		MIXED		MICE	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Days present in school	Estimate	4.91*	7.854*	3.616	6.096	4.775**	8.05**	4.775**	8.05**	3.305	5.572
1 year after randomization	(SE)	(2.939)	(4.545)	(2.389)	(3.921)	(2.38)	(3.902)	(2.38)	(3.902)	(2.912)	(4.808)
	CM/CCM	104.045	114.375	81.933	91.493	103.975	111.452	103.975	111.452	98.212	110.478
	Obs	N = 959		N = 1236		N = 1236		N = 1236		N = 1236	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Days present in school	Estimate	-.119	-.21	.424	.829	1.814	3.543	1.814	3.543	1.067	2.083
2 years after randomization	(SE)	(4.78)	(7.973)	(3.314)	(6.239)	(3.299)	(6.207)	(3.299)	(6.207)	(4.499)	(8.572)
	CM/CCM	103.829	120.065	67.586	85.578	101.886	110.796	101.886	110.796	85.922	101.024
	Obs	N =417		N =643		N = 643		N = 643		N =643	

*** p<0.01, ** p<0.05, * p<0.1

Imputation methods include basic/logical imputations where we use information in the data to confirm if a student has dropped out, and report regressions with only those have non-missing values after this imputation. Our second approach imputes all missing with zeros to assume a "worst case" scenario situation with the missingness (that they dropped out). Our third approach imputes with treatment and control means. Our fourth approach uses a combination a combination of imputed group means with logical imputation, imputing zeros when it's likely that youth have dropped out. The fifth approach uses a Bayesian approach, multiple imputation, to relax the missing completely at random assumption.

Table 1.9: C2C School Persistence and Graduation Outcomes

Variable	Mean T	Mean C	ITT	CCM	TOT	Obs
Persistence Index year 1, transfers not included	.614	.623	-.016	.713	-.027	1392
			(.024)		(.04)	
Persistence Index year 2, transfers not included	.583	.59	-.025	.731	-.047	778
			(.033)		(.06)	
Persistence Index year 1, out of Chicago transfers included	.767	.756	.012	.791	.019	1392
			(.021)		(.034)	
Persistence Index year 2, out of Chicago transfers included	.695	.699	-.021	.825	-.039	778
			(.03)		(.055)	
Graduated on track, index 1 (+1 extra year allowed)	.78	.769	.01	.872	.015	457
			(.035)		(.05)	
Graduated on track, index 2 (+0 extra year allowed)	.531	.527	.003	.664	.006	631
			(.036)		(.059)	
Graduated by day20 SY 1819	.305	.266	.019	.345	.032	1392
			(.019)		(.031)	

*** p<0.01, ** p<0.05, * p<0.1

We then look at the impact of C2C on GPA and failed courses (see table 1.10), we find no consistent findings across time (during the first year after randomization or during the second year after randomization) or imputation method generally.

Lastly, we look at behavioral change (total misconducts and violent misconducts) within a school (see table 1.11 and 1.12). We find consistent significant reductions across almost all imputation methodologies in the number of school incidents during the program period (one semester after randomization) by about 28% of the CCM (using the multiple imputation method). The decrease continues through the second semester after randomization, where we see more statistically robust and consistent reductions in the number of school incidents per person by about 29% of the CCM. These changes appear to fade out by two years post randomization (4 full semesters). Given the large and significant reductions among arrests, we are encouraged to see C2C having significant impact on behavioral changes within school as well.

In terms of violent incidents, we generally see little impact on the number of total violent incidents as the magnitudes are small and not statistically significant at almost every time period. When we reach two years post randomization, we do see some marginally statistically significant decreases however these are not robust to the various imputation methods.

Table 1.10: C2C GPA & % Failed Courses Outcomes, all imputation methods

		BASIC		ZERO		T/C MEAN		MIXED		MICE	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
% of failed 1 year after randomization	Estimate	-.004	-.006	-.004	-.007	-.008	-.013	-.007	-.012	-.005	-.008
	(SE)	(.022)	(.034)	(.015)	(.024)	(.014)	(.023)	(.014)	(.023)	(.021)	(.034)
	CM/CCM	.29	.206	.197	.145	.315	.25	.314	.249	.351	.241
	Obs	N =734		N =1333		N =1333		N =1333		N =1333	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
% of failed 2 years after randomization	Estimate	-.003	-.006	.007	.014	.007	.013	.005	.01	.013	.026
	(SE)	(.039)	(.068)	(.023)	(.044)	(.022)	(.042)	(.022)	(.042)	(.032)	(.061)
	CM/CCM	.443	.354	.245	.193	.444	.375	.444	.378	.483	.351
	Obs	N =318		N =652		N =652		N =652		N =652	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
GPA 1 year after randomization	Estimate	.009	.014	.047	.078	.033	.055	.036	.06	.041	.068
	(SE)	(.07)	(.105)	(.057)	(.092)	(.044)	(.072)	(.044)	(.072)	(.062)	(.102)
	CM/CCM	1.797	2.043	1.079	1.314	1.717	1.905	1.713	1.9	1.612	1.903
	Obs	N =734		N =1333		N =1333		N =1333		N =1333	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
GPA 2 years after randomization	Estimate	.017	.033	.022	.042	.03	.059	.026	.05	-.035	-.069
	(SE)	(.119)	(.21)	(.08)	(.149)	(.064)	(.121)	(.064)	(.121)	(.092)	(.174)
	CM/CCM	1.405	1.668	.754	.95	1.382	1.515	1.382	1.524	1.318	1.642
	Obs	N =318		N =652		N =652		N =652		N =652	

*** p<0.01, ** p<0.05, * p<0.1

Imputation methods include basic/logical imputations where we use information in the data to confirm if a student has dropped out, and report regressions with only those have non-missing values after this imputation. Our second approach imputes all missing with zeros to assume a "worst case" scenario situation with the missingness (that they dropped out). Our third approach imputes with treatment and control means. Our fourth approach uses a combination a combination of imputed group means with logical imputation, imputing zeros when it's likely that youth have dropped out. The fifth approach uses a Bayesian approach, multiple imputation, to relax the missing completely at random assumption.

Table 1.11: C2C School incidents–cumulative outcomes

		BASIC		ZERO		T/C MEAN		MIXED		MICE	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Number of school incidents	Estimate	-.128	-.211	-.104*	-.172*	-.133**	-.221**	-.133**	-.221**	-.117*	-.195*
1 sem after randomization	(SE)	(.08)	(.129)	(.062)	(.102)	(.062)	(.102)	(.062)	(.102)	(.068)	(.111)
	CM/CCM	.635	.776	.476	.619	.601	.74	.601	.74	.538	.68
	Obs	N =1072		N =1389		N =1389		N =1389		N =1389	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Number of school incidents	Estimate	-.247*	-.401*	-.176*	-.293*	-.248***	-.413***	-.248***	-.413***	-.205**	-.343**
2 sem after randomization	(SE)	(.133)	(.211)	(.096)	(.157)	(.096)	(.158)	(.096)	(.158)	(.104)	(.17)
	CM/CCM	1.144	1.401	.795	1.039	1.038	1.296	1.038	1.296	.915	1.172
	Obs	N =913		N =1333		N =1333		N =1333		N =1333	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Number of school incidents	Estimate	-.224	-.379	-.119	-.232	-.197	-.384	-.197	-.384	-.152	-.296
2 years after randomization	(SE)	(.328)	(.528)	(.211)	(.397)	(.211)	(.398)	(.211)	(.398)	(.229)	(.433)
	CM/CCM	2.055	2.718	1.381	2.09	1.747	2.453	1.747	2.453	1.611	2.334
	Obs	N =410		N =652		N =652		N =652		N =652	

*** p<0.01, ** p<0.05, * p<0.1

Imputation methods include basic/logical imputations where we use information in the data to confirm if a student has dropped out, and report regressions with only those have non-missing values after this imputation. Our second approach imputes all missing with zeros to assume a "worst case" scenario situation with the missingness (that they dropped out). Our third approach imputes with treatment and control means. Our fourth approach uses a combination a combination of imputed group means with logical imputation, imputing zeros when it's likely that youth have dropped out. The fifth approach uses a Bayesian approach, multiple imputation, to relax the missing completely at random assumption.

Table 1.12: C2C Violent School Incidents–cumulative outcomes

		BASIC		ZERO		T/C MEAN		MIXED		MICE	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Number of violent school incidents	Estimate	.012	.021	.01	.016	.011	.018	.011	.018	.009	.015
1 sem after randomization	(SE)	(.012)	(.02)	(.01)	(.015)	(.01)	(.015)	(.01)	(.015)	(.011)	(.017)
	CM/CCM	.035	.026	.026	.021	.034	.026	.034	.026	.032	.026
	Obs	N =1072		N =1389		N =1389		N =1389		N =1389	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Number of violent school incidents	Estimate	.005	.008	.003	.005	0	-.001	0	-.001	.003	.004
2 sem after randomization	(SE)	(.02)	(.031)	(.013)	(.022)	(.013)	(.022)	(.013)	(.022)	(.015)	(.025)
	CM/CCM	.079	.072	.053	.054	.07	.073	.07	.073	.062	.064
	Obs	N =913		N =1333		N =1333		N =1333		N =1333	
		ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Number of violent school incidents	Estimate	-.064	-.109	-.037	-.071	-.05*	-.097*	-.05*	-.097*	-.037	-.072
2 years/4 sem after randomization	(SE)	(.042)	(.067)	(.028)	(.053)	(.028)	(.053)	(.028)	(.053)	(.032)	(.06)
	CM/CCM	.171	.227	.116	.168	.148	.208	.148	.208	.132	.182
	Obs	N =410		N =652		N =652		N =652		N =652	

*** p<0.01, ** p<0.05, * p<0.1

Imputation methods include basic/logical imputations where we use information in the data to confirm if a student has dropped out, and report regressions with only those have non-missing values after this imputation. Our second approach imputes all missing with zeros to assume a "worst case" scenario situation with the missingness (that they dropped out). Our third approach imputes with treatment and control means. Our fourth approach uses a combination a combination of imputed group means with logical imputation, imputing zeros when it's likely that youth have dropped out. The fifth approach uses a Bayesian approach, multiple imputation, to relax the missing completely at random assumption.

Lastly, using CPS leave code information we can document the various reasons for why a CPS student might leave the CPS system. As part of this, CPS also records if a student is deceased or legally committed to a correctional facility. While we did not pre-register the following outcomes, we were disheartened to see any impact on mortality given the age of the youth in the program. By mid-year of the school year 2018-2019, we find 11 study youth were recorded in CPS as deceased, all non-participants of C2C (7 control youth, and 4 non-participating treatment youth). We also see that C2C youth are less likely to be committed to a correctional facility, although neither of these outcomes are significant at conventional levels. These outcomes suggest the reality and magnitude of the violence the study youth are faced with in these Chicago neighborhoods, and how C2C may be able to have larger life impacts on youth beyond behavior and school outcomes.

Table 1.13: C2C Impact on Mortality and Incarceration

Variable	Mean T	Mean C	ITT	CCM	TOT	Obs
Deceased as of Midyear 1819	.005	.009	-.005	.008	-.008	1531
			(.004)		(.007)	
Incarcerated as of Midyear 1819	.021	.035	-.013	.033	-.021	1531
			(.008)		(.013)	

*** p<0.01, ** p<0.05, * p<0.1

Overall, we find these initial educational outcomes encouraging, although given the timing of when youth entered the program and the limited data we have right now it is likely we need to wait for more time to pass before we are able to see any conclusive movement on outcomes like graduation that could shape longer term outcomes.[40]

1.9 Conclusion

This paper evaluates a unique behavioral-informed intervention provided outside of institutional settings that seeks to engage youth at risk of criminal involvement and improve

40. Future iterations of the paper will adjust these standard errors for multiple hypothesis testing as well.

overall life outcomes. To date we document that the program has been successful at significantly decreasing arrests (both the number of arrests and the probability of any arrest) at 12 months post randomization, and at the extensive margin for 24 months post randomization. Given the impact fade-out most social programs experience in behavioral change, we find it incredibly encouraging we see such strong results at preventing any arrests up to two years post random assignment. We also find very encouraging results regarding reductions in violent arrests at the both the extensive margin and the number of arrests per person lasting up to two years post random assignment, although these findings are not yet robust to multiple hypothesis testing. Lastly, we find encouraging preliminary results on education engagement outcomes, showing how the program helped youth reduce their misconduct behavior in school and increase days attended in school.

While more time needs to pass to better understand the longer-term impacts of the C2C program, these results to date demonstrate the promise of this intervention for affecting lasting change among youth who are at elevated risk for violence and criminal involvement.

1.10 Appendix

1.10.1 CBT and Wraparound Literature

Previous literature has found cognitive behavioral therapy (CBT) to be effective in addressing specific psychiatric disorders, including conduct problems (Kendall and Wilcox 1980; Kendall, et al. 1990; Kazdin 1995; Koegl, et al. 2008). While meta-analytic reviews have found mixed results on the effectiveness of CBT in reducing crime and delinquency among youth (Landenberger and Lipsey 2005; Lipsey and Cullen 2007; Greenwood 2008; Drake, Aos and Miller 2009; Lipsey 2009), this literature also has some limitations as many of these studies had used self-reported behavior outcomes, small sample sizes, or experienced attrition (see Appendix in Heller et al. 2017). More recent work by Heller et al. (2017) and Blattman et al. (2017) use stronger research designs and find more consistent evidence

for the effectiveness of CBT. Heller et al. (2017) found that an in-school CBT program for young men called Becoming a Man (BAM) was effective at reducing arrest and improving graduation rates among students enrolled in high schools located in areas of Chicago with disproportionately high violence and poverty rates. They found that participation in the program led to a 28-35% reduction in total arrests and a 45-50% reduction in violent arrests. CBT was also implemented among a higher risk population of male youth held in pre-trial detention in the Cook County Juvenile Temporary Detention Center (JTDC). The program was found to reduce re-admission rates to the facility by 21%. Heller and her co-authors hypothesize that these programs improved youth outcomes by helping youth change their behavior through slowing down their thinking and relying less on automatic thoughts and behaviors known in the psychology literature as automaticity. Supporting these findings, Blattman et al. (2017) conducted an RCT in Liberia in which criminally engaged men received either CBT, cash grants, both or nothing. The authors found that CBT alone led to a reduction in anti-social behavior but that CBT followed by cash grants led to a larger and more persistent effect in reduced crime and violence.

CBT has been adapted to be delivered specifically to populations dealing with the effects of traumatic life experiences. This is referred to as Trauma-Focused CBT and is designed to address negative emotions and behavioral responses and improve coping abilities. A meta-analysis of studies found that on average there was a positive but modest effect on post-traumatic stress, depressive symptoms, anxiety symptoms and externalizing behavior problems (Silverman et al. 2008). With respect to the specific kind of TF-CBT offered in this program, SPARCS (Structured Psychotherapy for Adolescents Responding to Chronic Stress), no rigorous research has been conducted to date to test the effectiveness of this therapy in particular. One key difference between SPARCS and other forms of TF-CBT is that SPARCS is not exposure based.[41] It is intended for an adolescent group (where

41. Exposure based means describing and sharing the personal traumatic event during the sessions is an important aspect of the therapy. Furthermore, in exposure based therapy (if appropriate) the youth goes through "in vivo mastery of trauma reminders", or exposure to situations that are no longer dangerous but

TF-CBT is mostly individual therapy) and focuses less on discrete traumas with the goal of helping youth cope with chronic traumatization. Additionally, literature has not explored the impact of TF-CBT or SPARCS on school engagement outcomes or crime in a rigorous manner.

The other program element, wraparound services, has been tested with various populations but with mixed or limited results. In a survey of the literature, Bruns and Walker (2015) found that among the most rigorous studies, positive effects were found in some areas such as behavior and housing stability and mixed results on areas such as school engagement (Carney and Buttell, 2003; Clark, et al. 1996; Evans et al. 1998; Rast et al. 2008). The most relevant RCT involved juvenile justice youth from Columbus, Ohio in 2003 (Carney and Buttell 2003). With the comparison group being youth who received conventional juvenile court services, youth who received wraparound were less likely to be absent from school, get expelled or suspended from school, run away from home, or get picked up by the police as frequently.

Only one study has attempted to study YAP's wraparound model in particular. Chandler, Levitt and List (2011) used a quasi-experimental design (constructing a control group with propensity score matching[42]) to evaluate the effectiveness of wraparound services provided by YAP in reducing victimization, recidivism and improving academic outcomes. However, the study results were inconclusive and did not find improvements in school behavior and education outcomes. In addition, with only 155 youth in the treatment group and approximately equivalent in the control group, the study did not have sufficient statistical power to measure victimization outcomes.

In assessing if the wraparound portion of C2C can be successful (both at engagement and impact), the mentorship literature can provide helpful insights since a large portion of

remind the youth of the original trauma

42. Most of the control group was constructed using propensity score matching. Some of the youth in the control group were selected from a predictive model (to predict risk of being shot), selecting youth from right below the threshold that was used to make referrals.

YAP in particular revolves around mentorship and advocacy. While the mentorship literature in general produces mixed results on effectiveness in improving various life outcomes, meta-analytic reviews suggest that when mentorship is paired with advocacy and emotional support, higher risk youth see improvements in delinquency and academic functioning (Tolan et al. 2013; DuBois et al. 2011). Other recent studies have also highlighted the importance of adult mentors being able to properly address the mental health needs of the youth they serve (Herrera et al. 2013). This literature further highlights the importance of combining advocacy/holistic mentorship with mental health services.

We'd like to highlight that the combination of mentoring/advocacy with therapy is not the only key innovation element of the program. Other programs such as multisystemic therapy (MST) do in some cases offer a combination of CBT and mentoring with an individualized service plan. However, MST differs from C2C in several key aspects. Where MST is typically three months, and C2C is at least 5 months (6 months in the current version), the most important difference between these two models is the first point of contact for the program and who delivers the services. The main point of contact for MST is a trained therapist, with the trained therapist essentially serving as both the therapist and the mentor. However, in C2C the community based advocate is the first point of contact and ensures that a relationship is formed before a trained therapist offers any therapeutic services. This we believe is the key innovation of C2C, and what we believe is essential to engagement. In speaking to other youth violence program providers in Chicago that have tried to deliver something similar to C2C in the last few years but where a case worker or a therapist is the first point of contact, engagement has remained to be a challenge.

1.10.2 Matching to CPD

To match our study participants to Chicago Police Department data, we design an algorithm using name, date of birth and address. Specifically, if name and DOB are an exact match, we keep the match. However, given that CPD data is not accurate in every arrest record and

that typos in name and DOB exist, we created several other pathways to ensure we would not miss accurate matches and designed the algorithm in a way to minimize false-positive arrests and manual review of particular cases. We allow for highly similar names and highly similar date of births (they differ on regular time intervals that may be attributed to a typo such as 1 years/months/day). Highly similar names are defined by having a rank value above the median value of similar string matches (using a string distance function). Matches are kept automatically if they have the same date of birth and same name or highly similar names and same date of birth. If the date of birth is the same, but the name match is not highly similar (so below the median), the case goes to a manually review process that also takes into account address information. Manual reviews are only kept if at least two independent reviewers (out of 3) consider the match a good match. We consider these type of "low name matches" with exact date of birth because the algorithm only looks at string distance and this does not account for phonetically similar names but are spelled different.

If the date of birth is highly similar, we got through a different set of decision nodes before any matches are kept. In this context, we account for name matches that are similar but also "common" names found in the CPD data base (specifically among the 100 most common names found in CPD arrest files for people born during or after 1995). If date of birth is highly similar yet the name is a common CPD name, the match goes to manual review (or dropped if name is not highly similar by string distance function and the address does not match). If date of birth is highly similar but the name is not a common CPD name, then the match is kept if the name is highly similar and the address matches, or sent to manual review otherwise. Only low name matches and non-address matches are dropped.

Lastly, matches where the date of birth is not highly similar are either dropped out right (given how closely the name matches) or sent to manual review based on address.

Figure 1.4: SPARCS Lesson: LET M' GO

The LET 'M GO steps are to answer the following questions:

Why am I losing it?	**L**osing It
What am I feeling?	**E**motions
What am I thinking?	**T**houghts
What's really important to me?	'**M**eaning
What do I want?	**G**oals
What are my choices?	**O**ptions

Figure 1.5: SPARCS Lesson: MAKE a LINK

"MAKE a LINK" stands for:

Mindfully connect
Act confident
Keep a calm and gentle manner
Express Interest
Ask for what you want
Let them know you get their point of view
Include your feelings
Negotiate—give to get
Keep your self-respect

Table 1.14: C2C Transfers out of Chicago, Cohorts I, II III, IV and V

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	sem1	sem2	sem3	sem4	sem1	sem2	sem3	sem4
treat	**0.00335**	**-0.00848**	**-0.0146**	**-0.00118**	**0.00476**	**-0.00583**	**-0.0104**	**0.00150**
	(0.0131)	(0.0142)	(0.0178)	(0.0206)	(0.0130)	(0.0140)	(0.0174)	(0.0204)
black					0.0639***	0.0752***	0.0630***	0.0707**
					(0.0179)	(0.0177)	(0.0236)	(0.0305)
female					-0.0218	-0.0178	-0.0127	-0.0364
					(0.0147)	(0.0162)	(0.0205)	(0.0243)
hispanic					0.0222	-0.00996	-0.0343	-0.0281
					(0.0280)	(0.0248)	(0.0287)	(0.0311)
bl_FRL					-0.0584	-0.0842**	-0.0778	-0.0804
					(0.0368)	(0.0424)	(0.0510)	(0.0642)
any_viol_prior					-0.0111	-0.0170	-0.0163	-0.0142
					(0.0181)	(0.0197)	(0.0241)	(0.0283)
any_prop_prior					-0.0146	-0.0222	-0.0350	-0.0448
					(0.0204)	(0.0198)	(0.0258)	(0.0280)
any_drugs_prior					-0.0329	-0.0480**	-0.0489*	-0.0615**
					(0.0227)	(0.0233)	(0.0290)	(0.0264)
any_other_prior					-0.0215	-0.0133	-0.0115	-0.0313
					(0.0188)	(0.0223)	(0.0254)	(0.0285)
bl_ever_notenrolled					0.0197	0.0263	0.0179	0.0255
					(0.0210)	(0.0225)	(0.0254)	(0.0328)
cohort2	-0.0146	0.0100			-0.00983	0.0157		
	(0.0244)	(0.0250)			(0.0247)	(0.0251)		
cohort3	-0.0132	0.00898			-0.0190	0.00106		
	(0.0229)	(0.0247)			(0.0231)	(0.0249)		
cohort4	-0.0203	-0.0144			-0.0258	-0.0247		
	(0.0245)	(0.0243)			(0.0260)	(0.0253)		
Baseline Covariates	NO	NO	NO	NO	YES	YES	YES	YES
Randomization Blocks	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,392	1,392	1,049	778	1,392	1,392	1,049	778
R-squared	0.070	0.070	0.078	0.087	0.115	0.128	0.141	0.142

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.15: C2C Outcomes, missing counts after basic imputation

Variable	Non-missing obs	Valid Obs*	Cohorts**	% missing
Days present in school 1 year after program	959	1236	I, II, IIIa, IIIb, IV	22.41%
Days present in school 2 year after program	417	643	I, II	35.1%
% of failed courses 1 semester after program	932	1389	I, II, IIIa, IIIb, IV	32.9%
% of failed courses 2 semesters after program	790	1333	I, II, IIIa, IIIb, IV	40.7%
% of failed courses 3 semesters after program	545	921	I, II, IIIa,	40.8%
% of failed courses 4 semesters after program	353	652	I, II	45.9%
GPA 1 semester after program	932	1389	I, II, IIIa, IIIb, IV	32.9%
GPA 2 semesters after program	790	1333	I, II, IIIa, IIIb, IV	40.7%
GPA 3 semesters after program	545	921	I, II, IIIa	40.8%
GPA 4 semesters after program	353	652	I, II	45.9%
Number of school incidents 1 semester after program	1072	1389	I, II, IIIa, IIIb, IV	22.8%
Number of school incidents 2 semesters after program	969	1333	I, II, IIIa, IIIb, IV	27.3%
Number of school incidents 3 semesters after program	729	921	I, II, IIIa	20.8%
Number of school incidents 4 semesters after program	555	652	I, II	14.9%
Had any school incidents 1 semester after program	1072	1389	I, II, IIIa, IIIb, IV	22.8%
Had any school incidents 2 semesters after program	969	1333	I, II, IIIa, IIIb, IV	27.3%
Had any school incidents 3 semesters after program	729	921	I, II, IIIa	20.8%
Had any school incidents 4 semesters after program	555	652	I, II	14.9%
Number of violent school incidents 1 semester after program	1072	1389	I, II, IIIa, IIIb, IV	22.8%
Number of violent school incidents 2 semesters after program	969	1333	I, II, IIIa, IIIb, IV	27.3%
Number of violent school incidents 3 semesters after program	729	921	I, II, IIIa	20.8%
Number of violent school incidents 4 semesters after program	555	652	I, II	14.9%
Had any violent school incidents 1 semester after program	1072	1389	I, II, IIIa, IIIb, IV	22.8%
Had any violent school incidents 2 semesters after program	969	1333	I, II, IIIa, IIIb, IV	27.3%
Had any violent school incidents 3 semesters after program	729	921	I, II, IIIa	20.8%
Had any violent school incidents 4 semesters after program	555	652	I, II	14.9%

*This includes all potential cases that could have been non-missing, excluding those that graduated the semester before a given outcome was realized. For example, for GPA 4 semesters after program we exclude all cohort 1 participants that graduated by SS SY 1617 or before, all cohort 2 participants that graduated by FS 1718 or before, etc.

**Cohort IIIa stands for cohort III's participants that were randomized durng the first semester of academic year 1617; Cohort IIIb stands for those randomized during the second semester of academic year 1617.

1.10.3 Imputation Methodology

Basic

The "basic" imputation or logical imputation is the procedure that underlies all other imputation methods. We start by creating a school-level "does not report" binary indicator for misconduct/GPA/attendance data; this is an indicator that takes the value of "1" if a school did not report any misconduct/GPA/attendance data to CPS on a given school year, ranging from school year 1516 to school year 1718. We then proceed to impute with zeroes all outcomes and baseline misconduct related variables - e.g. number of school incidents - only if the school a study participant was enrolled in at the beginning of that year reported misconduct data to CPS at least once. The following step consists on imputing the remaining misconduct records and GPA with zeroes if a study participant appears as inactive in CPS and has a "dropped out" or "can't be found" leave code on that particular semester/year (we do the same with % failed courses, but imputing with "1" instead).

Finally, for all imputation methods, if a study participant appears as graduated on a given semester for the first time, we leave all outcomes that happened after that first graduation semester as missing. As a robustness check, we impute any outcome after graduation with the value of the variable right before graduation (ex: GPA from the last semester before they graduated would be imputed for all future semesters after graduation). This ensures we are not changing the sample of the analysis based off a post-treatment outcome. Results are robust to this alternative imputation and are available upon request.

Zero

We impute all missing data after the "basic" imputation with zeroes.

T/C mean

We impute all missing data after the "basic" imputation with treatment & control means. We run this imputation for each cohort, independently. That is, missing data in cohort i is imputed with the the treatment group's mean drawn from cohort i only.

Mixed

This method implements a mixed imputation strategy, filling in missing data cases with cohort level treatment/control means, conditional on certain parameters; the remaining missing cases are then imputed with zeroes. We impute missing cases with treat/control means if a study participant meets at least one of the following criteria: (i) appears as actively enrolled in CPS on a given semester/year; (ii) has any kind of transfer status within CPS records for that semester/year; (iii) is currently enrolled on charter school or an option school; (iv) is currently enrolled on a school that does not report GPA; (v) attendance is at least 70 days for the given year. For all missing data that do not meet at least one of the conditions described above, we impute them with zeroes.

Multiple Imputation

We use a Multiple Imputation with Chained Equations approach. We start by regressing each outcome/baseline variable that has at least one missing observation on set of baseline covariates, randomization blocks and cohort indicators, for observations with non-missing data. We do this separately for treatment and control groups so as not to introduce correlation between the predictions and the treatment indicator. We use the resulting parameters to predict the missing values for the outcome/baseline variables, creating an initial imputed dataset. Using the imputed dataset, we re-estimate the regression parameters, update their distributions, take new draws from these distributions, and repeat the process. After a given number of iterations, we generate a usable imputed dataset. We repeat this process 25 times,

reporting coefficients that are averages across the 25 imputed data sets. After the imputation is done, we run our ITT/TOT regression estimates and account for the within and across imputation variances.

1.10.4 CPS Outcome Variable Description

• **Days present in school 1/2 years after program**: the total number of days a study participant attended on any school that reports attendance to CPS, 1/2 years after the semester on which that person was randomized into the treatment or control group.

• **Persistence Index year 1/2, transfers not included**: and index identifying those study participants that are either actively enrolled in CPS or have already graduated 2/4 semesters after the semester on which that person was randomized into the treatment or control group.

• **Persistence Index year 1/2, out of Chicago transfers included** : and index identifying those study participants that are either actively enrolled in CPS or have already graduated 2(4) semesters after the semester on which that person was randomized into the treatment or control group. Study participants that appear as either having transferred to a non-public school in Chicago or to a school outside of Chicago are considered in the same way as enrolled/graduated students in this version of the index.

• **% of failed courses 1/2 years after program**: the number of failed courses divided by the number of graded courses a study participant enlisted on a given academic semester, 1/2 years after the semester on which that person was randomized into the treatment or control group. A course is considered as "failed" if it the grade obtained was an "F".

• **Graduated on track, index 2 (no extra year allowed)** : A binary indicator identifying those study participants that graduated on-track, based on their school grade at the beginning of the semester on which that person was randomized into the treatment or control group. For example, if a student was enrolled in 11th grade at baseline, then that student will be considered as graduated "on-track" if he/she appears as graduated by the end of the

following academic year.[43] Students that graduate before their "on-track" expected graduation are considered as graduated "on-track" for the purpose of this indicator. Students that have not yet graduated and for which we don't observe the "on-track" graduation status - because of data not being available at this time - are given a missing value.

• **Graduated on track, index 2 (+1 extra year allowed)** : A binary indicator identifying those study participants that graduated on-track, allowing for an extra year to graduate, based on their school grade at the beginning of the semester on which that person was randomized into the treatment or control group. For example, if a student was enrolled in 11th grade at baseline, then that student will be considered as graduated "on-track (+ 1)" if he/she appears as graduated by the end of either the following academic year, or the year after that. Students that graduate before their "on-track (+1)" expected graduation are considered as graduated "on-track (+1)" for the purpose of this indicator. Students that have not yet graduated and for which we don't observe the "on-track (+1)" graduation status - because of data not being available at this time - are given a missing value.

• **Graduated by day20 SY 1819**: A binary indicator identifying those study participants that appear as graduated on CPS records by the beginning of school year 1819.

• **GPA 1/2 years after program** : the sum of the numeric grades obtained in all graded courses taken on a given year[44], divided by number of graded courses taken, 1/2 years after the semester on which that person was randomized into the treatment or control group.

• **Number of school incidents 1/2/4 semesters after program**: The total number of recorded school incidents a study participant was involved in, looking into the future up to 1/2/4 semesters after the semester on which that person was randomized into the treatment or control group. These counts are built in a cumulative way, that is, the number of school incidents 2/4 semesters into the future include all incidents that happened during the focal

43. We actually look at files the beginning of the year after the one "on-track" to include those students that might have graduated over the summer

44. Letters to numbers equivalency for grades obtained in courses are the following: "A" = 4; "B" = 3; "C" = 2; "D" =1; "F" = 0.

semester or on the semesters before that, starting with the semester after randomization.

• **Number of violent school incidents 1/2/4 semesters after program**: The total number of recorded **violent** school incidents a study participant was involved in, looking into the future up to 1/2/4 semesters after the semester on which that person was randomized into the treatment or control group. These counts are built in a cumulative way, that is, the number of violent school incidents 2/4 semesters into the future include all violent incidents that happened during the focal semester or on the semesters before that, starting with the semester after randomization.

Table 1.16: C2C Arrest Outcomes, Cohorts I , II, III, IV, and V–All and Violent Arrest Categories

Variable	Mean T	Mean C	ITT	CCM	TOT	Obs
Number of Arrests Per Person						
arrests before 6 months	.21	.275	-.037	.197	-.059	1537
			(.031)		(.05)	
arrests in between 6 and 12 months	.201	.305	-.084**	.272	-.141**	1398
			(.034)		(.056)	
arrests before 12 months	.426	.592	-.119**	.481	-.2**	1398
			(.055)		(.09)	
arrests before 24 months	1.101	1.178	-.033	.755	-.062	784
			(.129)		(.234)	
violent arrests before 6 months	.04	.068	-.022*	.08	-.035*	1537
			(.012)		(.018)	
violent arrests in between 6 and 12 months	.049	.055	-.007	.044	-.011	1398
			(.013)		(.021)	
violent arrests before 12 months	.091	.124	-.028	.131	-.047	1398
			(.018)		(.03)	
violent arrests before 24 months	.196	.271	-.063	.258	-.118*	784
			(.039)		(.072)	
Probability of Any Arrest						
Any arrest before 6 months	.128	.181	-.04**	.169	-.064**	1537
			(.016)		(.026)	
Any arrest before 12 months	.192	.287	-.078***	.283	-.131***	1398
			(.02)		(.033)	
Any arrest before 24 months	.31	.439	-.116***	.468	-.217***	784
			(.029)		(.054)	
Violent arrest before 6 months	.038	.064	-.019*	.074	-.031*	1537
			(.011)		(.017)	
Violent arrest before 12 months	.076	.105	-.024*	.103	-.04*	1398
			(.014)		(.024)	
Violent arrest before 24 months	.141	.196	-.043*	.183	-.081*	784
			(.024)		(.044)	

ITT estimates were calculated using randomization strata fixed effects, cohort fixed effects, and robust standard errors. TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment. CCM is the control complier mean–those who would have taken up treatment had they been offered it. It is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT. We include demographic covariates (age/race/gender dummies), school grade, prior enrollment in free lunch benefits, prior arrest records, and cohort fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table 1.17: C2C Arrest Outcomes, Cohorts I , II, III, IV and V with no covariates

Variable	Mean T	Mean C	ITT	CCM	TOT	Obs
Number of Arrests Per Person						
arrests before 6 months	.21	.275	-.063*	.239	-.101*	1537
			(.036)		(.057)	
arrests before 12 months	.426	.592	-.16***	.549	-.268***	1398
			(.061)		(.102)	
arrests before 24 months	1.101	1.178	-.055	.795	-.102	784
			(.146)		(.268)	
violent arrests before 6 months	.04	.068	-.028**	.091	-.046**	1537
			(.012)		(.019)	
violent arrests before 12 months	.091	.124	-.032	.137	-.053*	1398
			(.02)		(.032)	
violent arrests before 24 months	.196	.271	-.069*	.269	-.129*	784
			(.042)		(.077)	
property arrests before 6 months	.043	.046	-.002	.03	-.003	1537
			(.012)		(.02)	
property arrests before 12 months	.087	.112	-.024	.099	-.04	1398
			(.021)		(.035)	
property arrests before 24 months	.212	.189	.026	.101	.048	784
			(.046)		(.086)	
drug arrests before 6 months	.026	.017	.009	-.004	.014	1537
			(.01)		(.016)	
drug arrests before 12 months	.035	.038	-.002	.018	-.004	1398
			(.013)		(.022)	
drug arrests before 24 months	.076	.093	-.016	.049	-.03	784
			(.028)		(.052)	
other arrests before 6 months	.101	.144	-.041*	.122	-.067*	1537
			(.023)		(.036)	
other arrests before 12 months	.212	.318	-.102***	.295	-.171***	1398
			(.039)		(.064)	
other arrests before 24 months	.617	.625	.004	.378	.008	784
			(.093)		(.172)	
Probability of Any Arrest						
Any arrest before 6 months	.128	.181	-.052***	.19	-.085***	1537
			(.018)		(.028)	
Any arrest before 12 months	.192	.287	-.093***	.309	-.157***	1398
			(.021)		(.036)	
Any arrest before 24 months	.31	.439	-.123***	.481	-.23***	784
			(.032)		(.059)	
Violent arrest before 6 months	.038	.064	-.026**	.084	-.041**	1537
			(.011)		(.018)	
Violent arrest before 12 months	.076	.105	-.029*	.112	-.049*	1398
			(.015)		(.025)	
Violent arrest before 24 months	.141	.196	-.052**	.199	-.097**	784
			(.025)		(.047)	
Property arrest before 6 months	.033	.041	-.007	.034	-.011	1537
			(.01)		(.015)	
Property arrest before 12 months	.062	.083	-.02	.081	-.034	1398
			(.014)		(.023)	
Property arrest before 24 months	.121	.147	-.024	.139	-.046	784
			(.024)		(.044)	
Drugs arrest before 6 months	.018	.012	.006	-.004	.01	1537
			(.006)		(.01)	
Drugs arrest before 12 months	.024	.029	-.005	.015	-.008	1398
			(.008)		(.014)	
Drugs arrest before 24 months	.045	.067	-.021	.054	-.04	784
			(.016)		(.029)	
Other arrest before 6 months	.073	.111	-.037**	.106	-.059***	1537
			(.014)		(.023)	
Other arrest before 12 months	.121	.197	-.074***	.209	-.125***	1398
			(.018)		(.031)	
Other arrest before 24 months	.237	.292	-.05*	.27	-.093*	784
			(.029)		(.054)	

ITT estimates were calculated using randomization strata fixed effects, cohort fixed effects, and robust standard errors. TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment. CCM is the control complier mean–those who would have taken up treatment had they been offered it. It is calculated by taking the mean of the outcome for those that comply with the treatment minus the TOT. These results include only cohort fixed effects and randomization blocks as controls. *** p<0.01, ** p<0.05, * p<0.1

Figure 1.6: Total CPD Juvenile Arrests over Time

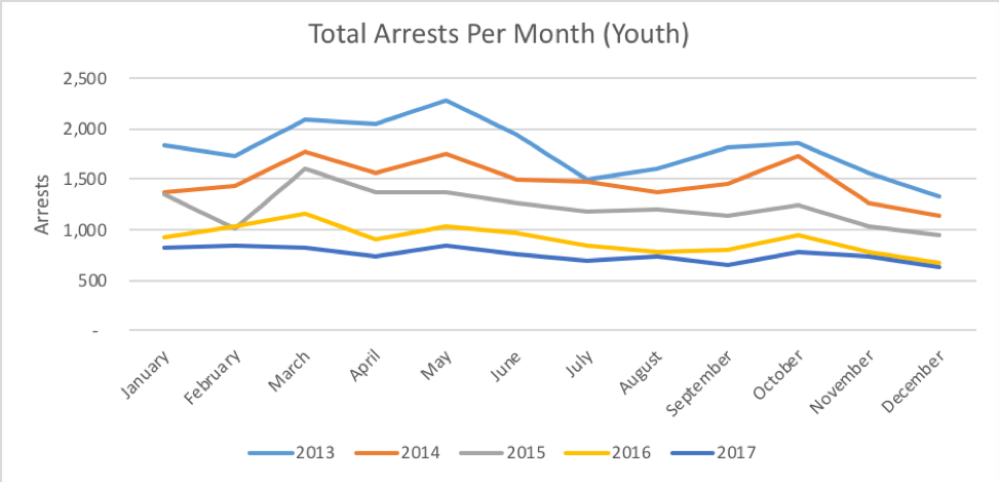

Figure 1.7: Total CPD Juvenile Violent Arrests over Time

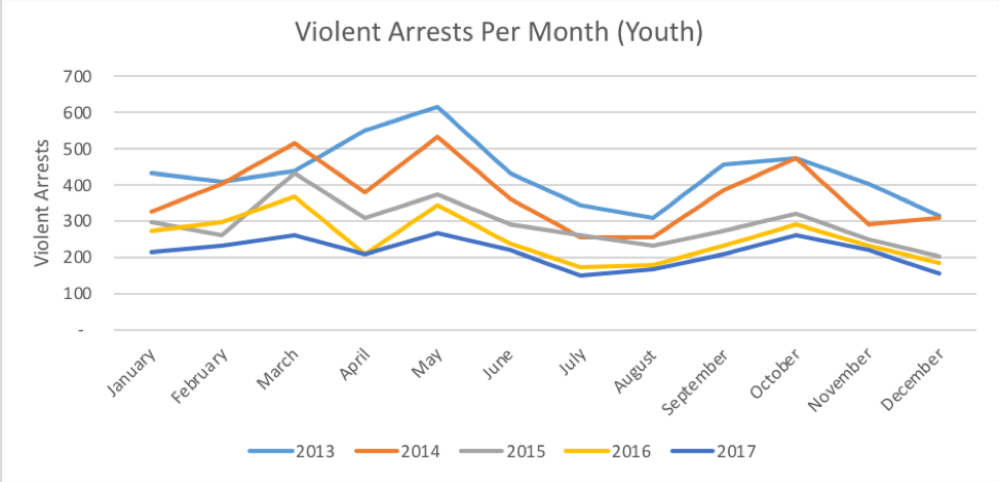

Table 1.18: C2C Arrest Outcomes, Cohorts I , II, III, IV, and V–Multiple Hypothesis Correction

Variable	CM	ITT	CCM	TOT	Unadjust. P-value	FWER	PSIDAK	FDR	Obs
Num. of Arrests Per Person									
arrests before 6 months	.275	-.037	.197	-.059	.236	-	-	-	1537
		(.031)		(.05)					
violent arrests before 6 months	.068	-.022*	.08	-.035*	.058	.223	.213	.234	1537
		(.012)		(.018)					
property arrests before 6 months	.046	.003	.022	.005	.776	.785	.779	.776	1537
		(.012)		(.019)					
drug arrests before 6 months	.017	.01	-.007	.017	.243	.446	.427	.325	1537
		(.009)		(.014)					
other arrests before 6 months	.144	-.029	.101	-.046	.16	.422	.407	.321	1537
		(.021)		(.033)					
arrests before 12 months	.592	-.119**	.481	-.2**	.026	-	-	-	1398
		(.055)		(.09)					
violent arrests before 12 months	.124	-.028	.131	-.047	.121	.333	.316	.242	1398
		(.018)		(.03)					
property arrests before 12 months	.112	-.013	.082	-.023	.508	.768	.755	.677	1398
		(.021)		(.034)					
drug arrests before 12 months	.038	-.001	.015	-.001	.951	.953	.951	.952	1398
		(.013)		(.021)					
other arrests before 12 months	.318	-.077**	.254	-.13**	.026	.102	.098	.103	1398
		(.035)		(.058)					
arrests before 24 months	1.178	-.033	.755	-.062	.79	-	-	-	784
		(.129)		(.234)					
violent arrests before 24 months	.271	-.063	.258	-.118*	.099	.345	.322	.396	784
		(.039)		(.072)					
property arrests before 24 months	.189	.037	.079	.07	.416	.670	.666	.555	784
		(.047)		(.086)					
drug arrests before 24 months	.093	-.03	.075	-.056	.213	.529	.513	.427	784
		(.025)		(.045)					
other arrests before 24 months	.625	.022	.344	.042	.784	.796	.791	.784	784
		(.084)		(.153)					
Prob. of Any Arrest									
Any arrest before 6 months	.181	-.04**	.169	-.064**	.013	-	-	-	1537
		(.016)		(.026)					
Violent arrest before 6 months	.064	-.019*	.074	-.031*	.069	.205	.193	.139	1537
		(.011)		(.017)					
Property arrest before 6 months	.041	-.003	.028	-.005	.726	.729	.724	.726	1537
		(.009)		(.015)					
Drugs arrest before 6 months	.012	.007	-.005	.011	.232	.428	.411	.31	1537
		(.006)		(.009)					
Other arrest before 6 months	.111	-.029**	.095	-.048**	.028	.118	.107	.113	1537
		(.014)		(.022)					
Any arrest before 12 months	.287	-.078***	.283	-.131***	.000	-	-	-	1398
		(.02)		(.033)					
Violent arrest before 12 months	.105	-.024*	.103	-.04*	.093	.265	.249	.186	1398
		(.014)		(.024)					
Property arrest before 12 months	.083	-.015	.072	-.025	.271	.483	.466	.362	1398
		(.014)		(.023)					
Drugs arrest before 12 months	.029	-.004	.013	-.006	.64	.651	.641	.641	1398
		(.008)		(.014)					
Other arrest before 12 months	.197	-.064***	.191	-.107***	.000	.001	.001	.001	1398
		(.017)		(.029)					
Any arrest before 24 months	.439	-.116***	.468	-.217***	.000	-	-	-	784
		(.029)		(.054)					
Violent arrest before 24 months	.196	-.043*	.183	-.081*	.068	.246	.230	.164	784
		(.024)		(.044)					
Property arrest before 24 months	.147	-.026	.142	-.049	.262	.270	.258	.262	784
		(.024)		(.044)					
Drugs arrest before 24 months	.067	-.026*	.063	-.049*	.085	.247	.233	.164	784
		(.016)		(.028)					
Other arrest before 24 months	.292	-.041	.254	-.077	.123	.247	.233	.164	784
		(.027)		(.05)					

Multiple hypothesis corrections using the family wise error rate (FWER), the false discovery rate (FDR), and the Sidak-Holm adjustment (PSIDAK).

Table 1.19: C2C Arrest TOT, Cohorts I , II, III, IV, V by Gender and Prior Arrest

Variable		TOT Gender	TOT Prior Arrest
arrests before 6 months	Partic	-.045	-.023
	Partic*Female/Prior Arrest	-.022	-.104
	Joint F-Test P-value	.266	.488
arrests before 12 months	Partic	-.214	-.136**
	Partic*Female/Prior Arrest	.041	-.185
	Joint F-Test P-value	.06	.026
arrests before 24 months	Partic	.14	-.404**
	Partic*Female/Prior Arrest	-.504	1.059
	Joint F-Test P-value	.317	.036
violent arrests before 6 months	Partic	-.051*	-.002
	Partic*Female/Prior Arrest	.037	-.105**
	Joint F-Test P-value	.179	.114
violent arrests before 12 months	Partic	-.071	-.005
	Partic*Female/Prior Arrest	.054	-.129
	Joint F-Test P-value	.302	.236
violent arrests before 24 months	Partic	-.211*	-.074
	Partic*Female/Prior Arrest	.235*	-.136
	Joint F-Test P-value	.145	.252
Any arrest before 6 months	Partic	-.083*	-.023
	Partic*Female/Prior Arrest	.045	-.127*
	Joint F-Test P-value	.053	.054
Any arrest before 12 months	Partic	-.158***	-.081***
	Partic*Female/Prior Arrest	.06	-.155*
	Joint F-Test P-value	0	0
Any arrest before 24 months	Partic	-.187***	-.185***
	Partic*Female/Prior Arrest	-.076	-.099
	Joint F-Test P-value	0	0
Violent arrest before 6 months	Partic	-.044	.001
	Partic*Female/Prior Arrest	.03	-.101**
	Joint F-Test P-value	.204	.115
Violent arrest before 12 months	Partic	-.055	-.002
	Partic*Female/Prior Arrest	.034	-.119*
	Joint F-Test P-value	.244	.155
Violent arrest before 24 months	Partic	-.13**	-.037
	Partic*Female/Prior Arrest	.122	-.139
	Joint F-Test P-value	.137	.18

TOT estimates were computed using a 2SLS regression, where the participation rate was instrumented by the treatment random assignment, and the interaction term of Subgroup×Partic was instrumented with an interaction of the treatment random assignment and the sub group indicator. We include demographic covariates (age/race/gender dummies), school grade, prior enrollment in free lunch benefits, and cohort fixed effects. *** p<0.01, ** p<0.05, * p<0.1

CHAPTER 2
MEASURING SPILLOVERS FOR A YOUTH VIOLENCE INTERVENTION

2.1 Abstract

Given that the vast majority of all adolescent offenses are committed in groups, understanding how a crime-reducing intervention can have spillovers or impact on the peers of the study youth becomes important not just in measuring the full impact of any program but also integral to understanding how youth engage in crime together. This paper overcomes methodological and measurement challenges by using administrative arrest and school data to measure social networks and a randomized controlled trial (RCT) that evaluated a community-based intervention intended to decrease crime and increase school engagement among high-risk youth in Chicago. Using exogenous variation in a peer's treatment status created by the crime-reducing RCT, we estimate peer effects using administrative co-offending data and administrative school data to identify a network of social ties. We find little evidence that the intervention influenced peers criminal behavior among the co-offending network of youth. We find some evidence that the intervention impacted the cohabiting peers of the study youth when the study person is older than the peer, by reducing the probability of having any violent arrest within 12 months. Future work is needed to better understand the spillovers of crime reducing interventions among adolescents.

2.2 Introduction

Across policy and research circles, many are skeptical that social interventions that target disadvantaged individual youth can be successful at reducing crime without changing the entire social system in which they operate, in part because of the importance of peer influences among youth (Garbarino 1999). Peer influences may discourage participation in

potentially beneficial programs as well as counteract these programs positive effects over time. The strength of peer pressure among adolescents has been extensively documented, as evidenced in part by the fact that the vast majority of youth criminal offending occurs in groups (Carrington 2009; Reiss 1988; Sutherland 1947; Sarnecki 2001; and Warr 2002).

Understanding how these peer influences moderate program impacts, and conversely, how intervening with some members of a social network influences other peers within the network, are critical for social policies intending to prevent youth violence. These peer influences are also relevant for benefit-cost analyses of these programs. For example, if participants simply stop spending time with their "higher-risk" peers, there may be no spillover effect of the program. Or if members of a criminogenic network are easily replaceable, we might find negative spillovers. But, if the pro-social behavior of program youth leads them to change the behavior of their friends or siblings, or make it more difficult for peers to recruit enough people with particular attitudes or skills to engage in some crime, there could be beneficial spillovers. Precisely measuring these crime spillovers becomes important in shedding light on how youth engage in crime together and which youth can be engaged to maximize crime reduction.

One challenge for understanding impacts on peer networks has been robustly identifying spillovers and the measurement of these networks. Manski (1993) highlighted that peers could have similar outcomes for a variety of reasons. They could seek out individuals who are similar to them, peers could be influenced by similar outside factors, or peers' behavior could affect one another. Constructing social networks and peer affiliations can also be challenging as this is typically done with in-person surveys, which in the U.S. context is often extremely expensive (Blume et al. 2015; Bramoulle et al. 2009; Shepherd and Paluck 2015). Another difficulty is eliciting honest and accurate information from high-risk youth about their peers using this method (Roman et al 2012).

In this paper, we tackle the challenge of identifying and measuring peer effects by using administrative data to measure social networks and a randomized controlled trial that

evaluated a community-based intervention intended to decrease crime and increase school engagement among high-risk youth in Chicago. *The Choose to Change: Your Mind, Your Game* (C2C) intervention combines wraparound services with trauma-focused CBT to improve life outcomes for at-risk youth in Chicago. Results to date has shown it to be effective at significantly decreasing total arrests and violent crime by almost 50%, as well as increasing school engagement among youth at risk of criminal involvement.

We overcome various measurement challenges with costly in-person surveys by using insights from previous work that co-offending networks can be constructed at much lower cost using administrative arrest records (Papachristos et al. 2015; Schaefer 2012; Chen et al. 2004; Xu and Chen 2005). To identify peer effects and build a baseline peer network for C2C youth, we exploit the exogenous variation in a peer's treatment status (as determined by their connection to either a treatment or control study youth prior to program start through a co-offense) created by the RCT at the start of programming. Given that treatment is randomly assigned, the peers of the treatment youth should have the same offending behavior as peers of the control youth on average. Any outside factors that impact youth or characteristics that led youth to associate in the first place should be on average equal between these two groups. Therefore, when we compare the peers of the treatment youth to the peers of the control youth (defined prior to program start), we can attribute any differences between these groups to peer effects of the C2C program. Using Chicago Police Department (CPD) arrest records, we estimate the impact of C2C on the behavior of these baseline peers. In addition to looking at social ties of C2C youth using arrest networks, we can also use administrative school records to identify cohabiting peers (i.e. siblings or other possible family members) as an additional network of interest.

We are not the first to estimate peer and spillover effects using an RCT, which has been done using survey data across the development, education, and labor economics literature (Miguel and Kremer 2004; Duflo et al. 2011; Breza and Chandrasekhar 2015; Cai et al. 2015; Oster and Thornton 2012; Crepon et al. 2013). However, to our knowledge, we are

the first to measure peer effects in a cost-effective way using administrative data to measure networks within the context of an RCT.

We find little evidence that the intervention influenced peers criminal behavior among the co-offending network of youth. We find some marginally significant increases in drug arrests 12 and 24 months post randomization among the the treatment peers of the C2C youth. However, these findings are not robust to various robustness checks including how we define a co-offending peer, how we define the outcome, or with multiple hypothesis testing. Among the cohabiting peers of C2C youth, we do find some more consistent evidence that being connected to a cohabiting peer that was older and offered the C2C program reduces a person's likelihood of having any violent arrest by 1.1 percentage points or 46% of the control mean ($p < .05$). However, we are cautious in how we interpret these results as they are not robust to multiple hypothesis testing either. Future work is needed to better understand the nature of spillovers in the context of a crime reducing intervention.

2.3 Literature Contribution

This paper contributes to the network and spillover literature more broadly, providing credible estimates of peer effects using rich co-offending data in the context of a crime reducing RCT. One challenge for understanding impacts from and on peer networks has been the measurement of these networks. While research has shown that co-offending relationships are important in crime perpetration, few papers have used these networks as a means of crime prevention to test spillovers. Typically, peer and social networks are measured with in-person surveys, which in the U.S. context is often prohibitively expensive (Blume et al. 2015; Bramoulle et al. 2009; Paluck et al. 2016; Shepherd and Paluck 2015). Another difficulty is eliciting honest and accurate information from high-risk youth about their peers using this method (Roman et al 2012). Other research has used proxies such as neighborhood (Kling et al 2005; Ludwig et al 2001; Ludwig and Kling 2007; Damm and Dustman 2014), prison facility and region/language (Bayer et al. 2009; Drago and Galbiati 2012) to

construct the relevant peer group. Our research takes a different approach by looking at who the program youth has co-offended with in the past in order to identify the persons most likely to be impacted by spillovers from a crime-reducing intervention. This is based off of insights from previous work using co-offending networks as they can be constructed at much lower cost using administrative arrest records (Papachristos et al. 2015; Schaefer 2012; Chen et al. 2004; Xu and Chen 2005).[1]. Furthermore, because we are able to utilize the exogenous variation created by a randomized control trial in a peer's treatment status, we do not need to use instruments based on network structure to obtain identification of peer effects as past work has done (Lindquist and Zenou 2014)

For the cohabiting peer analysis we conduct, we contribute to the growing set of literature that seeks to understand the role of the family in skill formation and human development. These set of papers concentrate on the various ways sibling spillovers could be present in school choice and academic achievement (Karbownik and Ozek 2018; Nicoletti and Rabe 2018; Qureshi 2018; Joensen and Nielsen 2018; Dustan 2018). Identification strategies vary from using discontinuities in school age cutoffs, to policy changes that only impacted older siblings. What is consistent across these papers is the understanding that older siblings can influence the behavior of their younger siblings through direct exposure and social influence rather than through parental resource reallocation. We contribute to this literature by demonstrating sibling/cohabiting spillovers outside of the educational space in the context of a crime reducing intervention. At the same time, we also expand the model of family skill formation by not limiting the spillovers to just siblings but include anyone cohabiting with study youth. This change in definition allows us to measure the externalities in other household compositions, especially in contexts where a nuclear family structure might be less common.

The appendix highlights more details about the various literatures this paper contributes to.

1. See Appendix for more details

2.4 Data and outcomes measured

2.4.1 Overview of C2C

In this paper, we estimate the role of peer effects using a randomized control trial that sought to decrease crime and increase school engagement among treated youth. The C2C intervention combines trauma-informed cognitive behavioral therapy that helps youth understand and change the way stress or past traumatic experiences can influence their decision-making with an advocate that provides mentorship and support for pro-social behavior. The advocate, a "near-peer" and pro-social adult, is the first point of contact for youth. Advocates come from the very same neighborhoods the youth live in and have confronted many of the same challenges. Before any CBT services or other services are offered, the advocate spends time developing a concrete personal relationship with the youth, which is fundamental to generating trust. They engage with youth and their families in prosocial activities, helping meet basic necessities while also providing motivation and direction on long-term goals. Advocates also take the youth they serve to regular CBT sessions, run by clinically trained therapists, and encourage their active participation. In these sessions, youth learn how to identify events and circumstances that overwhelm them and trigger a traumatic response and develop strategies to cope with these situations in the moment. Advocates reinforce these lessons individually and during group activities through the week when they engage with the young people outside of the sessions.

The program targets youth considered higher-risk for violence (either as victims or perpetrators): those who are gang affiliated, on juvenile probation, guilty of weapons offenses, disruptive or disengaged from school, or have a history of being a victim or witness to trauma. Results show that participation in C2C reduces the probability of any arrest by 47% within 24 months of randomization and the probability of any violent crime by 44% within the same time frame.

Youth were served in cohorts of 70 to 100 youth over the last three years with a total of 5

cohorts of programming beginning in November 2015 and wrapping up services in September of 2018.[2] The analysis presented in this paper covers cohort 1 (served from November 2015 to March 2016), cohort 2 (served from April 2016 to August 2016), cohort 3 (served from November 2016 to March 2017), cohort 4 (served from April 2017 to September 2017), and cohort 5 (served from April 2018 to October 2018) with a sample size of 488 participants and 1537 total youth randomized.

2.4.2 Data

To measure peer effects, we proxy social networks with co-offending networks using administrative CPD data. In C2C cohorts 1-5, 37% (N=575) of the study sample (N=1,537) youth have a baseline arrest prior to program start. Of those youth, 77% had been arrested with another person (N=442). This is reflective of the literature that states the vast majority of youth offend in groups (Carrington 2009; Reiss 1988; Sutherland 1947; Sarnecki 2001; and Warr 2002). Given our experiment and data availability we will not be able to speak to how the program changed peer networks. However, we can estimate the spillovers of C2C on the peers of a sample of the study youth, as defined by the baseline network (prior to the program starting) using co-offending arrests.

Using CPD data we can identify people who were arrested in the same incident and consider these to be "associates" of justice involved study youth. Through this method, we have identified 1,784 individuals as associates of the 442 C2C study youth. On average, each C2C study youth has been arrested with 5 other peers at baseline (the median number is 3). More than 93% of the peers or associate youth are connected to just one other C2C study youth.

To measure the impact of C2C on the criminal behavior of the associates of C2C youth, our primary outcomes are limited to total and violent arrests–where we see the most behavioral change among the C2C youth and where we might expect changes among the peers as

2. Additional cohorts of youth have been served but will not be part of this analysis.

well. Secondary outcomes include other categories of crime such as drugs, property or other. It is possible that certain kinds of crimes are more likely to occur in groups, and therefore more subject to peer effects of an intervention aimed to reduce crime. At the same time, crimes that are more individual or typically done on the lower end of the gang hierarchy may see increases if these youth are easily replaceable in the network.

With a sample size of 1,784 first order peers connected to 442 C2C study youth (of those 225 control youth and 217 treatment youth), it's possible we are not powered to detect any changes in arrest outcomes among peers. We augment our C2C arrest network of peers by looking at second order associates, or youth who were arrested at baseline with our first order associates (which have direct co-offense connections to C2C youth). When we extend our network to include first and second order associates, we end up with 7,241 peers where each C2C study youth is now connected (either directly or indirectly) to an average of 24 associates (median is 10 associates). Given the likely weaker connection to second order associates, we also look at the subset of second order associates that might be more likely to be impacted by C2C: those who are closer in age to the C2C study youth or those who live closer at baseline.[3] Looking at the distribution of second order associates, we limit peers by being below the median in age distance or below the median in geographic distance (within 2.15 years and 2.01 square miles, respectively). With this limited subset we end up with 5,243 associates, where C2C study youth are on average connected to 18 peers (median is 8).

2.4.3 Cohabiting peers

Lastly, these co-offending networks capture only a small subset of the possible ways in which C2C could have spillovers both in terms of network size (since only 37% of C2C youth have a baseline arrest with only 29% have a baseline arrest connected to another person) and the actual ways in which the C2C program might have impact beyond the study youth.

3. We follow Bhuller et al. (2018a) in this line of reasoning

The wraparound aspect of the program that is delivered by advocates is intended to help stabilize the family unit, and often works with siblings and other family members of the youth in the program. Before any services are delivered, they ask the family: "What do you need? How can we help? How can we work together as equal partners" to better serve the youth. This includes having frequent child-family meetings, connection and referral to other supports within the community as needed, and transportation to appointments. Siblings or other cohabiting peers of participants often also come with the C2C participants to outings or gatherings when space is available. Furthermore, if the C2C participant undergoes some positive behavior change, it is possible they might become a positive role model for others living in their household, particularly younger siblings or cohabiting peers as documented by an past literature (Karbownik and Ozek 2018; Nicoletti and Rabe 2018; Qureshi 2018; Joensen and Nielsen 2018; Dustan 2018). Lastly, as C2C participants are further engaging in school and become less involved in the justice system as results have shown, this might also free up a parent(s) time to invest and engage with the other siblings in the home.

To identify cohabiting peers of our study youth, we use an algorithm that uses administrative Chicago Public Schools (CPS) data to identify youth who live in the same household and share the same guardian. CPS data does not identify siblings or family members within their administrative data sets, but using probabilistic matching, address information, and guardian information we construct a data set that intends to identify cohabiting peers of C2C study youth. Details of the matching can be found in the Appendix. We identify 4,370 cohabiting peers of C2C youth. Specifically, 1,232 C2C youth are identified to have a cohabiting peer in CPS data at baseline prior to randomization. Of those 600 of them are control youth, 632 are treatment youth. On average each C2C study youth is connected to almost 4 cohabiting peers (median is 3). 334 of these C2C youth are older than the cohabiting peers identified, and 898 are younger than the cohabiting peers.

2.5 Analysis

Given that exposure to the C2C program through an associate (either co-offending peer or cohabiting peer) is random we are able to identify causal peer effects of the C2C intervention on both of these networks. For the co-offending network, an associate is considered "treated" if they have a prior arrest with at least one study youth who had been offered treatment. "Control" associates are then only associated/had been arrested with control study youth prior to program start. Similarly, for the cohabiting network, a peer is considered "treated" if they have been identified to be cohabiting (for at least 2 consecutive years) with at least one C2C treatment youth prior to randomization. "Control" associates are then only cohabiting with control study youth prior to program start.

Specifically we run the following reduced form regression within each network of peers:

$$Y_{it} = \alpha + \beta OfferedC2C_b + \gamma X_{i(t-1)} + \phi TotalConnections_{i(t-1)} + \psi_j + \epsilon_{it} \qquad (2.1)$$

Where Y_{it} is our arrest outcomes for peer i connected to a C2C study person for each crime type during various time intervals, t. $OfferedC2C$ is an indicator for being connected to at least one (randomly assigned) treated C2C youth b at baseline. X are baseline characteristics for the peers. $TotalConnections$ are the total number of C2C study youth the peers are connected to at baseline. ϕ_j are the set of dummy variables indicating the C2C randomization block (by referral source). Pooled regressions across cohorts also include cohort level fixed effects. We can consider this β as the impact or peer effect if you have a peer/associate that was offered the C2C program.

Whether a friend participates in C2C is not exogenously determined and in this context associates or peers at baseline could certainly impact that decision. Therefore an instrumental variable approach is needed to determine the impact of being connected to a C2C participant and defendable as long as the usual assumptions hold–exclusion restriction and monotonicity.[4] To estimate the peer effects of associating/living with someone who actually

4. In this context, the exclusion restriction requires that the only way being connected to a C2C person

participated in C2C, we can estimate a two-stage least squares regression using $OfferedC2C$ as an instrument for participation:

$$Y_{it} = \alpha + \beta TakeupC2C_b + \gamma X_{i(t-1)} + \phi TotalConnections_{i(t-1)} + \psi_j + \epsilon_{it} \tag{2.2}$$

Where $TakeupC2C$ is defined as being connected to at least one C2C youth b who participated in C2C. For the arrest network analysis that includes first and second order associates we also include a fixed effect for the type of connection (first/second order connection).

We cluster standard errors by the C2C study youth an associate was connected to, with 383 clusters for the arrest network and 1,232 clusters for the cohabiting peer network. Of the $1,784$ co-offending associates, over 93% are connected to just one study youth. Within the cohabiting peer network, 87% of the 4,370 peers are connected to just one study youth. However, there are a handful of associates connected to more than one study youth. To account for this, all regressions also include a binary control if an associate is connected to more than one study youth.[5]

2.6 Randomization/balance check

Given that C2C selection is randomly assigned, the peers of the treatment youth should have the same offending behavior and school engagement behavior as peers of the control youth on average. Any outside factors that impact youth or characteristics that led youth to associate in the first place (either through co-offending or cohabiting) should be on average equal between these two groups. We can test this assumption by looking at balance across the associates of treatment and control groups with observables, specifically age, demographic information and prior arrest information.

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who was offered the program could impact a peer's outcomes is through the C2C person actually participating in the program.

5. Other ways of correcting the standard errors are being explored, especially to deal with the youth that are connected to more than one study youth. Currently, these youth are clustered with just one of their connections, and if a youth is connected to both a treatment and control youth, the treatment focal youth becomes the cluster given that the associate is being considered "treated" in these cases.

2.6.1 Co-offending peers balance

Table 2.1 shows the balance between peers exposed to treatment (N=885) and their "control" peers (N=899) control peers. We find balance on all demographic variables including age, percent Black, percent Hispanic, and prior arrest records. Only total C2C connections is not balanced and this is due to the way "treatment" and "control" youth are defined (with treatment youth including youth connected to both treatment and control study youth). An F-test for joint significance of all baseline characteristics shows that we cannot reject the null hypothesis that treatment and control groups are equal. What is interesting to note is that these associates are on average older than the C2C study youth they are connected to (where average age is 16). This could impact the spillovers we may be able to observe given the structure of how juvenile crime works and hierarchy of gang and social structures.

Table 2.1: Associate (first order/direct connection) C2C Baseline Check, Cohort I, II, III, IV and V

Variable	T Peers	C Peers	Diff	p-value	Obs
Age at time of C2C randomization	18.54	18.46	.08	.724	1784
% female	.23	.2	.03	.377	1784
% black	.95	.91	.04	.394	1784
% hispanic	.04	.08	-.04	.337	1784
Total C2C connections	1.12	1.05	.07***	.002	1784
Multiple connections	.08	.07	.01	.557	1784
Any prior violent arrest	.67	.63	.04	.275	1784
Any prior property arrest	.48	.49	-.01	.866	1784
Any prior drug arrest	.33	.31	.02	.737	1784
Any prior other arrest	.84	.86	-.02	.162	1784

Joint F Test	Number of obs. =	1784
	F(7 , 382) =	.834
	Prob> F =	.560

Significance between the mean differences for the treatment vs. control groups was estimated using a linear regression with block level and cohort fixed effects. Standard errors are clustered at the focal youth level. A joint F test was performed on all covariates except total C2C connections given how it is defined. *** p<0.01, ** p<0.05, * p<0.1

As described earlier, we also look at the impact of C2C on the second order peers:

peers who have co-offended with first order associates (those that directly co-offended with C2C study youth) at baseline, and therefore indirectly exposed to C2C study youth. Table 2.2 shows the balance for the larger set of peers that now include second order/indirect connections to C2C study youth, and table 2.3 shows the first and second order peers limited by being close in age and geography as described in the data section. We see that these youth overall are even older than order 1 peers alone, however a joint F-test indicates we can reject the null that treatment and control peers are balanced. It is possible that we see imbalance by chance on a few of these covariates. Regardless, we do control for all of these baseline covariates in the regressions to follow.

Table 2.2: Associate (first and second order) C2C Baseline Check, Cohort I, II, III, IV and V

Variable	T Peers	C Peers	Diff	p-value	Obs
Age at time of C2C randomization	21.07	21.14	-.07	.79	7241
% female	.16	.15	.01	.574	7241
% black	.93	.84	.09*	.1	7241
% hispanic	.05	.13	-.08	.116	7241
Total C2C connections	1.49	1.17	.32***	0	7241
Multiple connections	.2	.17	.03	.35	7241
Any prior violent arrest	.66	.63	.03*	.099	7241
Any prior property arrest	.46	.46	0	.47	7241
Any prior drug arrest	.47	.43	.04	.253	7241
Any prior other arrest	.88	.87	.01	.524	7241

Joint F Test	Number of obs. =	7241
	F(7 , 379) =	2.117
	Prob> F =	.041

Significance between the mean differences for the treatment vs. control group peers of the first and second order connections was estimated using a linear regression with block level and cohort fixed effects. Standard errors are clustered at the focal youth level. A joint F test was performed on all covariates except total C2C connections given how it is defined. *** p<0.01, ** p<0.05, * p<0.1

Table 2.3: Associate (first and second order limited) C2C Baseline Check, Cohort I, II, III, IV and V

Variable	T Peers	C Peers	Diff	p-value	Obs
Age at time of C2C randomization	19.32	19.81	-.49	.198	5243
% female	.18	.16	.02	.381	5243
% black	.93	.86	.07	.195	5243
% hispanic	.06	.13	-.07	.195	5243
Total C2C connections	1.53	1.17	.36***	0	5243
Multiple connections	.2	.18	.02	.458	5243
Any prior violent arrest	.67	.63	.04	.125	5243
Any prior property arrest	.48	.47	.01	.546	5243
Any prior drug arrest	.4	.38	.02	.703	5243
Any prior other arrest	.87	.87	0	.802	5243

Joint F Test	Number of obs. =	5243
	F(7 , 379) =	2.188
	Prob > F =	.035

Significance between the mean differences for the treatment vs. control groups peers of the first and second (limited) order was estimated using a linear regression with block level and cohort fixed effects. Standard errors are clustered at the focal youth level. A joint F test was performed on all covariates except total C2C connections given how it is defined. *** p<0.01, ** p<0.05, * p<0.1. The subset of second order peers included in this table were limited by being close in age (within 2 years of age) or living close to their C2C connection (within 2 miles).

2.6.2 Cohabiting peers balance

The next few tables show the baseline checks between the cohabiting peers of the C2C youth, both all cohabiting peers (Table 2.4) and just the cohabiting peers that are younger than the C2C study youth at baseline (Table 2.5). In both cases we find balance on a number of demographic variables as well as prior arrest history. One thing to note that this population of cohabiting youth have a much smaller baseline arrest record. On average for all the siblings, only 21% have a prior arrest compared to the C2C study youth where 37% have a prior arrest at baseline. Given that the program was intended to reach youth at higher risk of engaging in crime and violence (or being the victim of such acts), it is not a surprise that their cohabiting peers are less justice involved. When we limit the sample to the cases where the C2C study person is older than the peer, we see even smaller baseline arrest rates which

certainly can be explained by the age of these peers. On average the younger peers of the C2C study youth are almost 12 with about 6% having a prior arrest. For these cohabiting peers, it is likely that any behavior influence on arrests would operate through a prevention pathway.

Table 2.4: C2C Baseline Check, Cohorts I, II, III, IV & V Siblings/Cohabiting peers

Variable	T Peers	C Peers	Diff	p-value	Obs
Age as of C2C randomization	15.36	15.38	-.02	.842	4060
% female	.5	.54	-.04**	.015	4060
% black	.93	.92	.01	.718	4060
% hispanic	.07	.08	-.01	.812	4060
Free/reduced lunch	.97	.96	.01	.878	4060
Connected to Older C2C Sibling	.54	.560	-.02	.173	4060
Number of C2C connections	1.22	1.05	.17***	0	4060
Any prior arrest	.2	.22	-.02	.137	4060
Any prior violent arrest	.12	.13	-.01	.233	4060
Any prior property arrest	.08	.1	-.02	.279	4060
Any prior drug arrest	.05	.06	-.01	.672	4060
Any prior other arrest	.13	.14	-.01	.536	4060

Joint F Test	Number of obs. =	4060
	F(10 , 1111) =	1.532
	Prob > F =	.122

Significance between the mean differences for the treatment peer vs. control peer groups was estimated using a linear regression with block level and cohort fixed effects. Standard errors are clustered at the focal youth level. A joint F test was performed on all covariates except total C2C connections given how it is defined. *** p<0.01, ** p<0.05, * p<0.1

Table 2.5: C2C Baseline Check, Cohorts I, II, III, IV & V–C2C sibling is **Older** than peer

Variable	T Peers	C Peers	Diff	p-value	Obs
Age as of C2C randomization	11.69	11.91	-.22	.181	2221
% female	.52	.53	-.01	.58	2221
% black	.92	.92	0	.584	2221
% hispanic	.07	.08	-.01	.639	2221
Free/reduced lunch	.96	.96	0	.737	2221
Number of C2C connections	1.2	1.05	.15***	0	2221
Any prior arrest	.05	.06	-.01	.369	2221
Any prior violent arrest	.03	.04	-.01	.296	2221
Any prior property arrest	.02	.02	0	.716	2221
Any prior drug arrest	.01	.01	0	.624	2221
Any prior other arrest	.02	.03	-.01	.326	2221

Joint F Test	Number of obs. =	2221
	F(9 , 862) =	.471
	Prob > F =	.894

Significance between the mean differences for the treatment cohabiting peer vs. control cohabiting peer groups was estimated using a linear regression with block level and cohort fixed effects. Standard errors are clustered at the focal youth level. A joint F test was performed on all covariates except total C2C connections given how it is defined. *** p<0.01, ** p<0.05, * p<0.1

2.7 Results

2.7.1 Co-offending Peers

Table 2.6 shows the impact of an associate being offered C2C (the reduced form) and the instrumental variable (IV) results for the peer effects among the co-offending network of cohorts 1-5. The table reports the mean of the treatment peers (those connected to at least one C2C treatment youth), the mean of the control group (those connected to only C2C control youth), the reduced form, and the IV estimate. Our primary outcomes are limited to the total and violent arrest categories: the categories of arrests where we see significant reductions among the C2C study population directly and where we might be more likely to see changes among their peers as well. We also show both the number of arrests after a C2C peer's randomization, and the probability of having any arrest after a C2C peer's

randomization. Given that the number of arrest variable is skewed with a mass at zero, an analysis of the average change might miss some important movement like engagement with the juvenile justice system at all. Therefore we also look at the extensive margin, as any engagement with juvenile justice system can have negative consequences for youth.

We also look to see if the peer effects change over time by measuring associate arrests during the same time intervals studied for the direct effects (during C2C programming-6 months and up to 24 months post). The time intervals were calculated using the date the C2C peer was first randomized into treatment if an associate is connected to more than one C2C youth. Given that each C2C cohort started at a different point in time from October 2015 to March 2018, we are only able to observe two years post randomization for half of the direct C2C study youth, with a similar drop in observations for the peers.

Generally, we see no significant changes in the behavior of C2C's co-offending network in Table 2.6.[6] While most of the point estimates for total and violent arrests are negative, the standard errors are all very large and we are unable to say anything for certain during the program period or after. Once we break these arrests down by other categories, see Table 2.11 in the appendix we do find marginally significant (at the 10 percent level) increases in the number of drug arrests at 12 and 24 months post by 50% and 41%, respectively using the reduced form results and the control mean as a benchmark. The estimates are also positive for any drug arrest but not but not statistically significant at any conventional levels.[7]

Given a sample size of 1,784 individuals and the likelihood that any spillovers would be much smaller in size than the direct impact, we may not be powered to detect any significant or large changes in criminal behavior among the order one co-offending peers. The next few tables extend the analysis to include second order peers, or youth who were arrested at baseline with our first order associates (which have direct co-offense connections

6. We document a strong first stage for the IV regressions as being connected to a C2C person who was offered the program increases the chances of being connected to a participant by 39%.

7. These findings are not robust to when we adjust for multiple hypothesis testing however using the False Discovery Rate.

to C2C youth). Table 2.7 shows the results with all second order peers included and table 2.8 shows the impact on the order one and order two peers who live within two miles of the C2C connection and are within two years of age at baseline as well (those youth who are more likely to be in touch with the C2C focal youth). Again we don't see any consistent story although there does seem to be a marginally statistically significant increase among treatment peers in the probability of having any violent arrest by 1.2 percentage points (or 25% of the control mean) during the C2C program period (see table 2.8).[8]

We are cautious in how we interpret any of these increases given that we do not see any consistent impact on overall arrests (both extensive margin and number of arrests) or consistency by how we define the set of co-offending peers, and given that these results are not robust to multiple hypothesis testing.

8. This finding is also not robust to multiple hypothesis testing

Table 2.6: C2C Spillover Analysis, Cohort I, II, III, and IV Order One Associates

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
arrests before 6 months	.445	.415	.028	.069	1784
			(.048)	(.12)	
arrests before 12 months	.841	.828	-.015	-.037	1713
			(.081)	(.201)	
arrests before 24 months	1.539	1.528	-.023	-.08	1208
			(.146)	(.509)	
violent arrests before 6 months	.085	.068	.016	.041	1784
			(.016)	(.04)	
violent arrests before 12 months	.157	.157	-.006	-.016	1713
			(.022)	(.055)	
violent arrests before 24 months	.284	.289	-.007	-.024	1208
			(.04)	(.139)	
Any Arrest					
Any arrest before 6 months	.264	.264	-.009	-.021	1784
			(.023)	(.057)	
Any arrest before 12 months	.395	.399	-.02	-.05	1713
			(.025)	(.062)	
Any arrest before 24 months	.525	.544	-.032	-.114	1208
			(.031)	(.111)	
Any violent arrest before 6 months	.078	.062	.013	.033	1784
			(.014)	(.034)	
Any violent arrest before 12 months	.134	.131	-.004	-.011	1713
			(.018)	(.044)	
Any violent arrest before 24 months	.213	.219	-.01	-.037	1208
			(.026)	(.092)	

First Stage	Number of obs. =	1784
	Estimate=	.399
		(.043)
	F(1 , 382) =	84.07
	Prob > F =	0

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

Table 2.7: C2C Spillover Analysis, Cohort I, II, III, and IV Order 1 and 2 Associates

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
arrests before 6 months	.352	.317	.004	.01	7241
			(.023)	(.058)	
arrests before 12 months	.673	.633	-.032	-.084	7065
			(.04)	(.108)	
arrests before 24 months	1.246	1.157	-.064	-.215	5447
			(.074)	(.259)	
violent arrests before 6 months	.06	.053	.002	.005	7241
			(.007)	(.017)	
violent arrests before 12 months	.118	.108	-.004	-.011	7065
			(.011)	(.028)	
violent arrests before 24 months	.204	.192	-.008	-.027	5447
			(.019)	(.064)	
Any Arrest					
Any arrest before 6 months	.232	.205	.01	.026	7241
			(.011)	(.028)	
Any arrest before 12 months	.346	.329	-.012	-.032	7065
			(.013)	(.035)	
Any arrest before 24 months	.48	.464	-.019	-.063	5447
			(.017)	(.059)	
Any violent arrest before 6 months	.055	.045	.004	.011	7241
			(.005)	(.014)	
Any violent arrest before 12 months	.101	.088	.002	.006	7065
			(.008)	(.021)	
Any violent arrest before 24 months	.156	.147	-.007	-.023	5447
			(.013)	(.046)	

First Stage	Number of obs. =	7241
	Estimate=	.391
		(.053)
	F(1 , 379) =	54.043
	Prob> F =	0

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

Table 2.8: C2C Spillover Analysis, Cohort I, II, III, and IV Order 1 and 2 (limited) Associates

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
arrests before 6 months	.387	.343	.008	.02	5243
			(.025)	(.065)	
arrests before 12 months	.731	.682	-.033	-.086	5109
			(.045)	(.12)	
arrests before 24 months	1.327	1.215	-.061	-.205	3955
			(.082)	(.278)	
violent arrests before 6 months	.073	.057	.009	.024	5243
			(.008)	(.021)	
violent arrests before 12 months	.136	.118	.003	.007	5109
			(.012)	(.031)	
violent arrests before 24 months	.225	.204	-.011	-.037	3955
			(.02)	(.069)	
Any Arrest					
Any arrest before 6 months	.254	.222	.011	.029	5243
			(.013)	(.032)	
Any arrest before 12 months	.365	.346	-.012	-.032	5109
			(.014)	(.038)	
Any arrest before 24 months	.491	.475	-.023	-.078	3955
			(.019)	(.068)	
Any violent arrest before 6 months	.067	.048	.012*	.03*	5243
			(.007)	(.017)	
Any violent arrest before 12 months	.116	.096	.008	.02	5109
			(.009)	(.024)	
Any violent arrest before 24 months	.176	.156	-.005	-.016	3955
			(.014)	(.049)	

First Stage		
	Number of obs. =	5243
	Estimate=	.389
		(.05)
	F(1 , 379) =	60.178
	Prob> F =	0

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

2.7.2 Cohabiting Peers

We now turn to the spillover results for the cohabiting peers of the C2C youth. Unsurprisingly and in line with past literature we don't find any impact on the cohabiting peers when we include all peers, those younger and older than their C2C connection (see Table 2.9). However, once we limit the sample to only the cohabiting peers where the C2C connection is older, we begin to see more consistent change. Specifically, we see significant reductions in violent arrests both on the number of arrests and the extensive margin 6 and 12 months post C2C randomization. Given the control means, the estimates themselves are actually

quite small in comparison to the direct results we document. For example, we see that being connected to a cohabiting peer that was offered C2C reduces a person's likelihood of having any violent arrest by 1.1 percentage points or 46% of the control mean (which is 2.4%). As we saw with the baseline covariates, these younger cohabiting peers of the C2C youth are quite young (average age is almost 12) and therefore unlikely to have high arrest rates. Lastly, we don't find any spillovers looking at the other categories of arrests (see Table 2.15 and Table 2.14 in the Appendix).

We are a more confident of these results given the consistency across both margins and the slightly stronger finding, however these findings are not robust to multiple hypothesis testing.

Table 2.9: C2C Arrest outcome Table, Cohorts I, II, III, IV & V Cohabiting Peers, Overall

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
arrests before 6 months	.056	.067	-.001	-.002	4060
			(.012)	(.02)	
arrests before 12 months	.121	.149	-.004	-.007	3759
			(.024)	(.04)	
arrests before 24 months	.274	.26	.042	.078	2132
			(.048)	(.088)	
violent arrests before 6 months	.012	.015	-.001	-.002	4060
			(.005)	(.007)	
violent arrests before 12 months	.029	.032	.003	.004	3759
			(.008)	(.013)	
violent arrests before 24 months	.053	.048	.014	.025	2132
			(.016)	(.03)	
Any Arrest					
Any arrest before 6 months	.041	.042	.005	.008	4060
			(.007)	(.01)	
Any arrest before 12 months	.067	.074	0	0	3759
			(.008)	(.013)	
Any arrest before 24 months	.119	.1	.024*	.044*	2132
			(.013)	(.024)	
Any violent arrest before 6 months	.011	.013	-.001	-.001	4060
			(.004)	(.006)	
Any violent arrest before 12 months	.024	.028	0	0	3759
			(.006)	(.009)	
Any violent arrest before 24 months	.04	.038	.009	.017	2132
			(.01)	(.018)	

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

Table 2.10: C2C Arrest outcome Table, Cohorts I, II, III, IV & V Older Siblings

Number of Arrests per person					
Variable	Mean T	Mean C	Reduced Form	IV	Obs
arrests before 6 months	.027	.055	-.023	-.038	2221
			(.015)	(.024)	
arrests before 12 months	.069	.111	-.024	-.039	2048
			(.027)	(.045)	
arrests before 24 months	.171	.162	.015	.029	1096
			(.056)	(.107)	
violent arrests before 6 months	.003	.011	-.007*	-.011*	2221
			(.004)	(.006)	
violent arrests before 12 months	.011	.028	-.013*	-.021*	2048
			(.007)	(.011)	
violent arrests before 24 months	.032	.036	.001	.002	1096
			(.013)	(.025)	
Probability of any arrest					
Any arrest before 6 months	.021	.027	-.001	-.002	2221
			(.007)	(.011)	
Any arrest before 12 months	.034	.048	-.005	-.009	2048
			(.008)	(.013)	
Any arrest before 24 months	.058	.063	.005	.011	1096
			(.013)	(.025)	
Any violent arrest before 6 months	.003	.009	-.005	-.008	2221
			(.003)	(.005)	
Any violent arrest before 12 months	.009	.024	-.011**	-.018**	2048
			(.005)	(.009)	
Any violent arrest before 24 months	.024	.031	-.003	-.005	1096
			(.01)	(.019)	

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

2.7.3 Discussion

To date, all of spillover estimates documented for both the co-offending and cohabiting peers have been suggestive given the lack of consistency in the results and not being robust to multiple hypothesis testing. However, in this section we discuss some of the possibilities behind why we might see increases in drug arrests among the co-offending peers and decreases in violent arrests among the cohabiting peers if these impacts are indeed true.

Given the structure of juvenile crime in Chicago, with group violence and drug distribution as the most common gang-related crimes (Flowers 1990), we suspect that drug activity may be an easily replaceable activity and less subject to positive peer pressure. Although the direct results of the C2C focal youth are not significant, there are consistent decreases in drug arrests among the C2C youth (see Table 1.7). This might have led to C2C peers

replacing their friends as they left behind that particular life. Using youth (ages 11-26) CPD arrest data from 2008-2018, we find that almost 35% of individual arrests are drug possession or drug dealing charges, making them less susceptible to the positive peer pressure C2C study youth could provide if they are still in fact friends. With more data available for these cohorts of peers over time, we hope to be able to analyze these results in greater detail.

As for the cohabiting peers reduction in violent arrests, work in the sibling spillover literature has documented the importance of older siblings in influencing the behavior of their younger siblings though direct exposure and social influence rather than through parental resource reallocation (Karbownik and Ozek 2018; Nicoletti and Rabe 2018; Qureshi 2018; Joensen and Nielsen 2018; Dustan 2018). In this context this is certainly possible, however we also know the C2C program itself often worked with the family of the youth including the cohabiting peers or siblings. Therefore, it is possible that these cohabiting peers could have direct impact with the advocates, going on outings with their C2C peer, and also being further influenced by having a more positive role model in their C2C peer. Given the age range of these peers, we will complement future iterations of this paper with behavior outcomes in school like attendance and misconducts where we might be more likely to see larger spillovers.[9]

2.7.4 Future Work and Conclusion

After each cohort of C2C youth, we held focus groups to try and learn more about how the program operated in practice. Qualitatively, we did not get consistent answers on how study youth interacted with their old set of friends. During the intervention it seems that some youth left behind their prior friendship network, as some focus group participants said things like "before I would be around people doing negative stuff... so [now] I would call him up instead". Some of the youth explicitly mentioned how they have left behind the street life, and have essentially made a new network of friends. Others, however, said that the program

9. These are also the same categories of outcomes where we see direct C2C impact.

did not impact who they interacted with on a daily basis. If the increases in arrests were robust, this would be consistent with the idea that C2C study youth simply left behind their old life and old friends, and others in the criminogenic network came in to readily fill their spots.

Given the suggestive evidence we were able to identify in this paper, more work is needed to better understand the spillover nature of crime-reducing interventions like *Choose to Change*. Future iterations of this paper will likely explore geographical spillovers or use CPS data to identify peers through co-suspension networks. Given the group nature of juvenile crime and how expensive social programs can be it is certainly worthwhile to understand the larger (positive and negative) impacts of these interventions.

2.8 Appendix

2.8.1 Crime Peer Effect Literature

Across development, education, and labor fields, research has used RCTs to robustly estimate peer and spillover effects (Miguel and Kremer 2004; Duflo et al. 2011; Breza and Chandrasekhar 2015; Cai et al. 2015; Oster and Thornton 2012; Crepon et al. 2013). However, to our knowledge, we are the first to use co-offending networks to measure peer effects within the context of an RCT.

The Co-offense Connection

Past research has used co-offending networks as a means to understand how criminal activities are concentrated or spread within social networks. The principle underlying using co-arrest data for building social networks is that disengagement, violence, and crime have been shown to often occur in groups and with people of the same age, especially during teen years (Ballester et al. 2004; Reiss 1988; Sutherland 1947; Sarnecki 2001; and Warr 2002). Although one may be concerned that using administrative data can lead to limited conclusions given the limited contextual information available in the data, research has shown that co-offending connections can be meaningful relationships.

Specifically, Papachristos et al. (2015) found that individual gunshot victimization is correlated with exposure to violence in one's social network as measured by co-arrests. Another study done in 2012 (Schaefer, 2012) used co-arrest data to better understand crime patterns by examining ties between neighborhoods through social and spatial distance. Schaefer found that different neighborhoods with students going to the same school districts were more likely to have co-offending ties. Co-offending ties have also been used by law enforcement to learn more about criminal networks. During a trial, Tuscan Police Department used weighting ties (based on the number of times a unique pair of individuals had co-offended) and network centrality measures to identify influential individuals and unique groups of actors that

closely reflected gangs and other criminal groups, as well as individual leaders (Chen et al. 2004; Xu and Chen, 2005). Furthermore, while the stability of these ties may not be strong or remain over time among some populations (Xu and Chen, 2005), repeat co-offenses were more common among youth who were arrested more frequently and engaged in crime with larger groups (McGloin et al. 2008). This appears closer to the population we are working with, as the average youth who has a baseline arrest in our peer sample had been arrested at least 8 times[10]

While one may think a survey directly asking youth about their peers would be preferable to administrative co-offending data, it is not necessary clear that building a social network using a survey would be more accurate among high-risk populations, especially with the goal of studying criminal activity. Roman et al (2012) explicitly cite this limitation when they surveyed high-risk youth in a Latino neighborhood in Maryland and elicited peer networks as well as self-reported criminal behavior. They believed that respondents would answer more honestly and be more willing to participate in the survey if they knew that researchers would not be looking into criminal records or police data. It is possible that given the high-risk population in the C2C study, youth would be unwilling to share identifying information on their friends or peers out of concern of being of "snitch", or if they did, the information would be unreliable. If this is the case, using administrative data to construct networks and for outcomes may be our best option in assessing spillovers for high-risk youth.

Crime Peer Effects

There is a growing literature in economics suggesting that social interactions and neighborhood contexts are important in influencing criminal behavior, and should be accounted for when analyzing the effects of an intervention that reduces crime. Glaeser's et al. (1996) early study found that the amount of social interactions is highest in petty crimes and moderate in

10. These are considered the "associates" of the C2C youth. The C2C youth they are connected to have been arrested on average 6 times prior to program start.

more serious crimes. The Moving to Opportunity (MTO) program implemented by the U.S. Department of Housing and Urban Development from 1994 to 1998 found that when low-income families were offered the chance to relocate to lower-poverty neighborhoods, violent crime decreased for both genders in the short run, but property crime increased among men (Kling et al 2005; Ludwig et al 2001). Recent research by Chetty et al. (2018) shows that program effects differ by age at the time of the move, with youth who moved prior to the age of 13 increasing college attendance, earnings and single parenthood rates. Outside the U.S., Damm and Dustman (2014) found that male refugee immigrants quasi-randomly assigned to neighborhoods in Denmark with more violent crimes were more likely to be convicted of a crime later in life.

Among already convicted and incarcerated individuals, research has also found strong evidence for peer effects. Bayer et al. (2009) found that among juvenile offenders who had committed a particular crime and exposed to peers in the same facility with a history of that same crime, they were more likely to recidivate with that crime. Drago and Galbiati (2012) used a 2006 prison pardon that commuted actual sentences to expected sentences for 40 percent of the Italian prison population. This generated exogenous variation in peers' incentives to reoffend, as former inmates were required to serve their residual sentence and the sentence for the new crime if they reoffended within 5 years of release. Using prison and geographical origin to construct peer groups for former inmates, they found that peers' residual sentence reduced individual recidivism. More recently, several papers have used the quasi-random assignment of judges in the U.S. and abroad to estimate peer, sibling and intergenerational impacts of incarceration (Bhuller et al. 2018a; Bhuller et al. 2018b; Norris et al. 2018), where they find that incarceration reduces the likelihood of criminal involvement for peers, siblings and children.

2.8.2 Matching Algorithm for Cohabiting Peers

To match cohabiting peers within CPS, we first limit the dataset to only those who are currently considered actively enrolled in CPS within the last 5 years. The files we use from CPS are cumulative and therefore include any person who has ever enrolled in CPS. We therefore first limit the dataset by deleting persons who have been inactive in CPS files for the last five years (prior to randomization of each C2C cohort). We then look to identify peers who match on address and guardian if this information is available. Specifically, if addresses match exactly, students are flagged as belonging in the same cohabiting group. For addresses that are sufficiently similar according to a string match probabilistic threshold [i.e. those below the 0.08 percentile determined sufficient by manual review], we use guardian and parent names to determine if there could be a typo or missing information in the address.[11] Matches below this threshold are dropped. If the match is above this threshold, we then classify several version of match-types, some more restrictive than others (see below for the type of different matches). In this algorithm, we built in knowledge that we know that many of the family structures of the families served by C2C are not traditional and therefore there could be students living in the same household with different guardians and they may not share the same last name.

- Manual review and type 2 match: these matches do not have the same exact address, but we consider them cohabitants because either:
  - Address is similar (using probability matching) AND match was accepted after manual review
  - Address is similar (using probability matching) AND guardians have an exact match (type 2 matches)
- Same guardian match: same address AND same guardian

11. We also use string distance match to determine close enough guardian name matches, and use a 2.3 percentile threshold.

- Probability matching type 1
  - Same address AND
  - the available guardian names are similar enough to be considered a match
- Probability matching type 2
  - same address AND
  - the available guardian names are similar, but not enough to be sure that the names are the same for all cases
- Probability matching type 3
  - Same address AND
  - the available guardian names are not similar AND
  - last names are different
- Last name match
  - same address AND
  - not enough guardian information OR available guardian names are not similar AND
  - same name
- Not enough guardian information
  - same address AND
  - not enough guardian information AND
  - last names are different

Table 2.11: C2C Spillover Analysis, Cohort I, II, III, and IV Order 1 Associates–Other Categories

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
property arrests before 6 months	.07	.068	-.002	-.004	1784
			(.018)	(.045)	
property arrests before 12 months	.133	.123	.013	.032	1713
			(.027)	(.067)	
property arrests before 24 months	.207	.217	.004	.013	1208
			(.047)	(.164)	
drug arrests before 6 months	.053	.034	.018	.044	1784
			(.011)	(.028)	
drug arrests before 12 months	.088	.058	.029*	.073*	1713
			(.016)	(.041)	
drug arrests before 24 months	.181	.118	.048*	.169*	1208
			(.027)	(.101)	
other arrests before 6 months	.237	.245	-.005	-.012	1784
			(.03)	(.075)	
other arrests before 12 months	.463	.49	-.05	-.126	1713
			(.052)	(.13)	
other arrests before 24 months	.867	.904	-.067	-.237	1208
			(.097)	(.336)	
Any Arrest					
Any property arrest before 6 months	.056	.05	.004	.009	1784
			(.013)	(.032)	
Any property arrest before 12 months	.095	.084	.013	.034	1713
			(.016)	(.04)	
Any property arrest before 24 months	.14	.138	.007	.025	1208
			(.024)	(.083)	
Any drug arrest before 6 months	.04	.033	.004	.009	1784
			(.008)	(.021)	
Any drug arrest before 12 months	.063	.053	.008	.021	1713
			(.011)	(.027)	
Any drug arrest before 24 months	.117	.101	.007	.026	1208
			(.016)	(.058)	
Any other arrest before 6 months	.172	.182	-.007	-.018	1784
			(.019)	(.048)	
Any other arrest before 12 months	.277	.292	-.023	-.058	1713
			(.022)	(.055)	
Any other arrest before 24 months	.406	.426	-.027	-.097	1208
			(.029)	(.103)	

First Stage		
	Number of obs. =	1784
	Estimate=	.399
		(.043)
	F(1 , 382) =	84.07
	Prob > F =	0

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

Table 2.12: C2C Spillover Analysis, Cohort I, II, III, and IV Order 1 and 2 Associates–Other Categories

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
property arrests before 6 months	.041	.035	0	0	7241
			(.006)	(.016)	
property arrests before 12 months	.075	.068	-.002	-.006	7065
			(.011)	(.029)	
property arrests before 24 months	.128	.132	-.019	-.065	5447
			(.018)	(.064)	
drug arrests before 6 months	.049	.035	.006	.016	7241
			(.007)	(.017)	
drug arrests before 12 months	.087	.071	.003	.007	7065
			(.012)	(.032)	
drug arrests before 24 months	.166	.129	.007	.023	5447
			(.02)	(.067)	
other arrests before 6 months	.202	.193	-.004	-.011	7241
			(.014)	(.035)	
other arrests before 12 months	.393	.385	-.028	-.075	7065
			(.023)	(.063)	
other arrests before 24 months	.748	.705	-.044	-.146	5447
			(.042)	(.146)	
Any Arrest					
Any property arrest before 6 months	.033	.03	-.001	-.002	7241
			(.005)	(.013)	
Any property arrest before 12 months	.056	.053	-.004	-.011	7065
			(.007)	(.019)	
Any property arrest before 24 months	.091	.093	-.014	-.047	5447
			(.011)	(.038)	
Any drug arrest before 6 months	.041	.03	.005	.013	7241
			(.005)	(.013)	
Any drug arrest before 12 months	.065	.056	.001	.003	7065
			(.007)	(.018)	
Any drug arrest before 24 months	.111	.092	.001	.002	5447
			(.01)	(.033)	
Any other arrest before 6 months	.154	.144	.002	.004	7241
			(.009)	(.022)	
Any other arrest before 12 months	.247	.237	-.01	-.025	7065
			(.01)	(.028)	
Any other arrest before 24 months	.376	.357	-.008	-.028	5447
			(.014)	(.047)	

First Stage	Number of obs. =	7241
	Estimate=	.391
		(.053)
	F(1 , 379) =	54.043
	Prob> F =	0

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

Table 2.13: C2C Spillover Analysis, Cohort I, II, III, and IV Order 1 and 2 (limited) Associates–Other Categories

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
property arrests before 6 months	.052	.039	.006	.015	5243
			(.008)	(.02)	
property arrests before 12 months	.095	.079	.003	.008	5109
			(.013)	(.035)	
property arrests before 24 months	.157	.147	-.012	-.041	3955
			(.022)	(.073)	
drug arrests before 6 months	.044	.036	.003	.008	5243
			(.008)	(.021)	
drug arrests before 12 months	.079	.075	-.002	-.006	5109
			(.013)	(.034)	
drug arrests before 24 months	.151	.13	-.001	-.003	3955
			(.021)	(.07)	
other arrests before 6 months	.217	.21	-.011	-.027	5243
			(.016)	(.042)	
other arrests before 12 months	.421	.41	-.036	-.095	5109
			(.027)	(.073)	
other arrests before 24 months	.795	.732	-.037	-.123	3955
			(.048)	(.164)	
Any Arrest					
Any property arrest before 6 months	.042	.034	.003	.007	5243
			(.006)	(.016)	
Any property arrest before 12 months	.069	.061	0	.001	5109
			(.009)	(.023)	
Any property arrest before 24 months	.108	.104	-.01	-.033	3955
			(.012)	(.042)	
Any drug arrest before 6 months	.036	.03	.002	.005	5243
			(.006)	(.015)	
Any drug arrest before 12 months	.059	.054	0	0	5109
			(.008)	(.02)	
Any drug arrest before 24 months	.1	.088	0	-.001	3955
			(.01)	(.034)	
Any other arrest before 6 months	.167	.155	.001	.001	5243
			(.01)	(.027)	
Any other arrest before 12 months	.261	.25	-.013	-.035	5109
			(.013)	(.034)	
Any other arrest before 24 months	.387	.364	-.008	-.028	3955
			(.017)	(.058)	

First Stage		
	Number of obs. =	5243
	Estimate=	.389
		(.05)
	F(1 , 379) =	60.178
	Prob > F =	0

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

Table 2.14: C2C Arrest outcome Table, Cohorts I, II, III, IV & V All Siblings–Other Arrest Categories

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
property arrests before 6 months	.011	.013	.001	.001	4060
			(.004)	(.007)	
property arrests before 12 months	.018	.026	-.001	-.002	3759
			(.007)	(.012)	
property arrests before 24 months	.047	.04	.018	.033	2132
			(.014)	(.026)	
drug arrests before 6 months	.003	.004	-.001	-.001	4060
			(.002)	(.003)	
drug arrests before 12 months	.01	.01	0	0	3759
			(.004)	(.006)	
drug arrests before 24 months	.024	.021	.003	.006	2132
			(.008)	(.014)	
other arrests before 6 months	.03	.035	0	0	4060
			(.007)	(.012)	
other arrests before 12 months	.064	.08	-.006	-.01	3759
			(.014)	(.023)	
other arrests before 24 months	.15	.15	.007	.013	2132
			(.029)	(.053)	
Probability of any arrest					
Any property arrest before 6 months	.01	.011	.002	.003	4060
			(.003)	(.005)	
Any property arrest before 12 months	.014	.017	.001	.001	3759
			(.004)	(.007)	
Any property arrest before 24 months	.027	.028	.004	.007	2132
			(.008)	(.014)	
Any drugs arrest before 6 months	.003	.004	0	0	4060
			(.002)	(.003)	
Any drugs arrest before 12 months	.009	.009	0	-.001	3759
			(.003)	(.005)	
Any drugs arrest before 24 months	.018	.018	.001	.001	2132
			(.006)	(.011)	
Any other arrest before 6 months	.024	.026	.001	.001	4060
			(.005)	(.008)	
Any other arrest before 12 months	.042	.043	.003	.004	3759
			(.006)	(.01)	
Any other arrest before 24 months	.084	.069	.015	.028	2132
			(.011)	(.02)	

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

Table 2.15: C2C Arrest outcome Table, Cohorts I, II, III, IV & V Older Siblings–Other Arrest Categories

Variable	Mean T	Mean C	Reduced Form	IV	Obs
Number of Arrests					
property arrests before 6 months	.009	.015	-.003	-.006	2221
			(.006)	(.01)	
property arrests before 12 months	.017	.031	-.009	-.015	2048
			(.01)	(.016)	
property arrests before 24 months	.041	.041	.004	.008	1096
			(.023)	(.043)	
drug arrests before 6 months	.001	.003	-.003	-.004	2221
			(.002)	(.003)	
drug arrests before 12 months	.005	.003	.002	.003	2048
			(.003)	(.005)	
drug arrests before 24 months	.013	.011	0	-.001	1096
			(.01)	(.019)	
other arrests before 6 months	.013	.026	-.011	-.017	2221
			(.009)	(.014)	
other arrests before 12 months	.037	.048	-.003	-.006	2048
			(.017)	(.027)	
other arrests before 24 months	.085	.074	.01	.02	1096
			(.031)	(.059)	
Probability of any arrest					
Any property arrest before 6 months	.009	.011	-.001	-.002	2221
			(.005)	(.008)	
Any property arrest before 12 months	.013	.02	-.005	-.008	2048
			(.006)	(.01)	
Any property arrest before 24 months	.017	.027	-.008	-.016	1096
			(.008)	(.016)	
Any drugs arrest before 6 months	.001	.002	-.002	-.003	2221
			(.001)	(.002)	
Any drugs arrest before 12 months	.003	.002	.001	.002	2048
			(.002)	(.004)	
Any drugs arrest before 24 months	.006	.009	-.004	-.008	1096
			(.005)	(.011)	
Any other arrest before 6 months	.01	.013	-.001	-.001	2221
			(.005)	(.008)	
Any other arrest before 12 months	.023	.019	.009	.015	2048
			(.007)	(.011)	
Any other arrest before 24 months	.037	.032	.012	.023	1096
			(.012)	(.023)	

All estimates were calculated using clustered standard errors (in parentheses). Instrumental variable estimates were computed using a 2SLS regression, where being connected to at least one C2C participant was instrumented with connected to at least one randomized treatment C2C youth. We include demographic variables (age/race/gender dummies), prior arrest record, total number of connections to C2C youth, if they are connected to more than one youth, and cohort fixed effects as controls. *** p<0.01, ** p<0.05, * p<0.1

CHAPTER 3

RESPONSES TO PEER PRESSURE: A THEORY BEHIND *CHOOSE TO CHANGE*

3.1 Abstract

Social pressure and concerns about social image can influence a wide range of behaviors, especially among adolescents. This paper finds suggestive evidence on how a crime-reducing intervention (C2C) in Chicago influenced responses to peer pressure for adolescent youth living in high-violent neighborhoods. Working in high schools with a high concentration of C2C study youth, this project partners with the Chicago Police Department (CPD) in offering youth an opportunity to become engaged in the various youth programming the department offers. Given the stigma of working with CPD in these communities, I expect that a public sign-up to learn more these programs comes with a high social cost. The field experiment in schools varied the privacy assurances on the CPD take-up forms to test the idea that C2C treatment youth are less susceptible to prevailing community norms and social pressures compared to C2C control youth. I find suggestive evidence (not statistically significant at conventional levels) that the public form had a negative impact on take-up in the C2C control group, highlighting the stigma in working with CPD. Among the C2C treatment youth we instead find that a public sign-up increased take-up rates by a significant amount, suggesting a response to peer pressure in a way that counteracts social norms.

3.2 Introduction

Young people who grow up in high-poverty, distressed, and dangerous urban neighborhoods have to navigate social environments every day that exert pressure on them to engage in high-risk activities. It's been documented in literatures across economics and other social sciences that peers can shape decisions from educational investments (Bursztyn and Jensen

2015; Bursztyn et al 2017a; Austen-Smith & Fryer 2005; Fryer & Torelli 2010) to voting and investment decisions (Funk 2010; Gerber al 2008; Bursztyn et al 2017b). Yet little is known about what interventions or tools can be used to counteract social pressure concerns. In this paper I document suggestive evidence from a field experiment that demonstrates how a South-Side Chicago intervention may helped youth act against prevailing social norms in high-violent neighborhoods.

Although a wide range of behaviors is influenced by social pressure or concerns about social image (Bursztyn and Jensen 2017), adolescents are especially susceptible to such influences (Coleman 1961; Lavecchiea et al 2015; Steinberg 2015; Dodge 2008). As adolescents are going through physical changes, intellectual changes, and social and environment changes they often struggle to determine the nature of his/her self and how that relates to peers. Social pressure or image concerns can play a large role in adolescent delinquent and criminal behavior, as partly evidenced in part by the fact that the vast majority of youth criminal offending occurs in groups (Carrington 2009; Reiss 1988; Sutherland 1947; Sarnecki 2001; and Warr 2002).

In exploring what interventions or tools can be used to counteract the pressures youth face, we turn to a randomized controlled trial evaluation of a crime-reducing intervention. In previous work, I find that a new community-based intervention in Chicago targeted towards youth at high risk for violence involvement and academic disengagement was successful reducing total and violent arrests by almost 50% amount 24 months post-randomization. This five-month intervention called *Choose to Change (C2C): Your Mind, Your Game* combines trauma-informed cognitive behavioral therapy that helps youth understand and change the way stress or past traumatic experiences can influence their decision-making with an advocate that provides mentorship and support for pro-social behavior. The remarkable fact about the success of this program is that decreases in arrests continue 18 months after the program ends, suggesting that this program was successful at facilitating long-term change and habits. And yet these youth still live in the same distressed and violent neighborhoods

with the same social pressures. How did this happen, and what can we learn from this about how to develop even more effective youth interventions in the future?

I show suggestive evidence that one way this program may have succeeded in reducing arrests among participants is by changing the way C2C youth respond to peer pressure or specific community norms. Between May of 2018 and March of 2019, I ran a series of field experiments in partnership with Chicago Police Department (CPD) to test this idea that C2C youth are better able to resist peer pressure. Working in Chicago Public School (CPS) high schools that have the highest concentration of C2C study youth, I worked with CPD in offering students a chance to learn more about the various youth program CPD runs that help youth prepare for a career in law enforcement. Given that the stigma of working with CPD is high in certain neighborhoods in Chicago, in line with research on majority black communities and police (Kirk and Papachristos 2011; Sampson 2012; Weitzer 2002; Desmond et al 2016), I expected that a public sign-up to learn more these programs comes with a high social cost. After CPD gave a presentation, sign-up forms randomly varied the privacy emphasis if a student's decision to sign-up and learn more about CPD youth programming would be kept private from other students in the school. Specifically, the difference in these privacy assurances is that the "private" form emphasized that the decision to sign-up and learn more is kept private from everyone including other students in the school, while the "public" form said "except other students in your school." As an additional (possibly more salient) test for the peer pressure mechanism related to CPD, the forms also offered youth a chance to attend a baseball or basketball game with CPD officers. Under the "private" condition, winners of the game lottery are announced via email. Under the "public" condition, the form indicated that winners of the game lottery may be announced in school. Presenting to schools with a high concentration of C2C youth and randomizing who received the public or private forms, the intention was to capture within control study youth how take-up could differ by form and compare this to the C2C treatment youth take-up difference. I hypothesized that the C2C treatment youth would respond differently to the

privacy assurances compared to the C2C control group.

Although these results are not always statistically significant at conventional levels, I find some suggestive evidence that youth are responding to peer pressure and social norms regarding CPD. Among the non-C2C youth, I find some suggestive evidence that private take-up for CPD programming was 5.4 percentage points more compared to the public form, although this is not statistically significant at conventional levels.[1] When I look at take-up for participating in the CPD baseball/basketball game, there was a slightly larger difference, specifically a 7.3 percentage points increase when comparing the private take-up to the public-take-up. Interestingly, I find that among a school that had a program already operating with CPD to reduce stigma, public forms may increase take-up (again not significant at conventional levels) demonstrating that here students still change their behavior in response to perceived peer and social norms although in this case it means social acceptability to engage with CPD.

When I turn to look at these differences in take-up by C2C treatment status, I find evidence that while youth are still responding to social pressure they do so in a way that counteracts social norms. Although only some of these results are statistically significant at conventional levels given the small sample size of the experiment, they do suggest an interesting finding that hopes to be explored with more research. Among the control youth in C2C I find consistent evidence of CPD stigma with an increase in take-up if the youth received a private form by 9.1 percentage points for CPD youth programming, and an increase by 25 percentage points for the CPD game lottery, although these differences are not statistically significant at conventional levels. When we look at the impact of the forms within the C2C treatment group, here we surprisingly find large and positive impacts of the public form on take-up, where receiving the public form increases CPD program take-up by 22 percentage points (depending on the specification used, but not significant at conventional levels) and

1. I run various specifications to account for some compliance issues. This result is from the preferred specification. More details in the main section about this.

CPD lottery take-up by 24 percentage points ($p < .05$ for some specifications).[2] While these results are still suggestive considering the small sample sizes, they do suggest the social costs and stigma of interacting with CPD and how the C2C treatment youth are not susceptible to this particular pressure. While the strong response to the public form indicates concern for social image, they act in a way that is contrary to social norms. Given the very promising results we have documented to date about the effectiveness of C2C in reducing crime and violence, it's encouraging to document a specific context in which C2C treatment youth defied a specific social norm. We take this as suggestive evidence that they might be doing this in other contexts related to conflict and crime among their peers.

Although these findings to date are promising and suggestive evidence for now, this research will continue in Fall of 2019 with more field experiments in schools to increase the sample size and precision of the results. If these results hold, this paper seeks to contribute to the literature in a few ways.

First, I document an intervention that is influencing the way youth respond to peer pressure and social norms, although not always in a way we would have expected a priori. While changing the social norms youth prescribe too or the peers they associate with may work in some contexts (Paluck et al 2016; Tankard & Paluck 2016; Shepherd & Paluck 2015; Kling et al 2005; Chetty et al 2018), I find that an alternative approach may be used. If an individual is able to act in a way that is contrary to the prevailing norms, it matters less then if you are unable to change all relevant social norms or peer influences as research has documented how family members could be a negative source for social influence (Dodge et al 2007). This is especially relevant in a city like Chicago where social media has amplified the audience of people watching and expectations to respond in specific ways has exacerbated the gun violence (Blandfort et al 2018).

Second, this paper attempts to document one possible mechanism for how C2C worked to achieve significant decreases in crime increases in school engagement. Many policy ex-

2. This result holds at this significance using 2 of the 3 specifications run.

periments tell us little about underlying mechanisms of action (Ludwig et al 2011). But in order to have impact at large scale, policymakers need to know why an intervention changes behavior to strengthen efficacy of program design and ensure fidelity. After all, absent some understanding of why a program of bundled activities works, we cannot answer the question "fidelity to what?" While I will not be able to determine what aspect of C2C in particular led to these changes in responses to a social norm through this specific experiment, interviews with youth and program providers can help us understand a little more. We do see from our conversations with the program participants that C2C's trauma-informed therapy portion first helps youth think about their identity in a more positive manner, while the advocates help the youth believe in themselves and their capacity for change. As they shared with an interviewer, youth often said,"I feel like a better version of me." Participants have also indicated how after the program they eventually often become mentors to their peers in their communities. This new level of confidence combined with being seen as a new role model among their peers may have led to the findings I document to date.

Lastly, I document interesting insights in police stigma among adolescents living in high violent communities. While Chicago has especially struggled recently in police-community relationships, these presentations demonstrated that police stigma is not insurmountable and that many adolescents are willing to not only engage in conversation with police officers on heated topics but also willing to consider a career in public safety.

The rest of the paper is organized as follows: Section 3.3 discusses in more depth the literature in this field and a theoretical model to help frame social image. Section 3.4 discusses the experimental set up and strategy. Section 3.5 highlights results and Section 3.6 focuses on a discussion of these findings. I conclude with thoughts for future research in Section 3.7.

3.3 Literature Review

Over the years a number of studies across disciplines have documented the importance of peer pressure or concerns about social image in impacting behavior (Asch 1951; Nisbett and Ross 1991; Goffman 1959; Akerlof and Kranton 2000;2010; Beneabou and Tirole 2006). I define peer pressure using Bursztyn and Jensen's (2015) definition: an action taken that deviates from what a person would privately consider to be optimal (what a person would do if the action was not observable) to achieve social gains or avoid social costs. To further guide this concept of social image used in this paper, I replicate a simple model below developed by Bursztyn and Jensen (2017a) to illustrate later concepts. They consider an individual i with reference group j undertaking a visible action $a_i \in 0, 1$ that might reveal information about i's type $\sigma_i \in l, h$. Assuming type h is more socially desirable within reference group j than being type l, they define the social image term S_{ij} in i's utility function:

$$S_{ij} = \lambda_{ij} E_i(\omega_j) Pr_{-i}(\sigma_i = h \mid a_i) \tag{3.1}$$

$Pr_{-i}(\sigma_i = h \mid a_i)$ represents the probability that other people in the reference group think that $i's$ type is h conditional on observing $i's$ choice of action, a. The term $E_i(\omega_j)$ represents $i's$ expectation about how social desirable (ω_j) it is being seen by group j as type h. The model assumes that being type h is socially desirable, so $\omega_j > 0$. Lastly, λ_{ij} measures how much the individual cares about being perceived by group j as being of type h. If $\lambda_{ij} > 0$, the person is a conformist, if $\lambda_{ij} < 0$, then the person is a contrarian. And lastly if $\lambda_{ij} = 0$ than the individual does not care about their image or what others think about their type. For any nonzero amount, the individual cares about social image and is subject to social pressure from group j. Bursztyn and Jensen (2017a) discuss more details about this conceptual framework and embed a random utility model to further illustrate concepts.

The field experiment run in this paper essentially manipulates $Pr_{-i}(\sigma_i = h \,|\, a_i)$, whether information is kept fully private or shared with others. The general idea is that if the

probability that others update their assessment of one's type is zero because it's completely private (peers cannot observe the outcome), the individuals choose what they privately consider optimal. But once behavior becomes observable to others, social image concerns come into play. In this experiment, I therefore compare how C2C treatment youth respond to social image concerns compared to C2C control youth.

As research has shown, social pressure might lead to undesirable outcomes especially with respect to youth criminal behavior (Dodge 2008, Dodge et al. 2007). These pressures are not limited to criminality but have shown to influence decisions with respect to education investments as well (Fordam & Ogbu; Austen-Smith & Fryer 2005, Fryer & Torelli 2010; Bursztyn & Jensen 2015). Understanding how to counteract these social pressures becomes incredibly important when looking to social policy interventions that seeks to engage at-risk youth. Using the framework outlined earlier, this could involve changing the ability to infer types from behavior, whether something is socially desirable, or how much people care about the opinions of them.

While relatively few scholars have focused on this, some previous research has shown some success in changing the impacts of social pressure. Paluck's work is especially relevant to youth as she has shown how to use peer influence as a vehicle for social change by influencing the social norms or the perceptions of social norms youth prescribe to (Paluck et al 2016; Tankard & Paluck 2017). Paluck shows how changing the public behavior of highly connected and salient actors within a school network can lead to changes among their peers' perceptions of norms. Using the framework discussed earlier, this would essentially be changing individual i's beliefs about whether something is socially desirable, $E_i(\omega_i)$, which would change the effects of social pressure. However, this method maybe limited to areas or groups where social norms are readily adaptable. Given the context youth in violent neighborhoods grow up in, it may be difficult to change the various social norms that are influencing educational decisions or potential delinquent behavior.[3] Research has also shown

3. For example, Evans et al 2018 shows how historical violent crack markets permanently changed young

that among youth living in under-resourced and high violent neighborhoods youth are not just influenced by peers but by negative family influences as well (Dodge et al 2007).

This paper focuses on a different component of social pressure: testing to see if it's possible to change how much people care about the opinions of them (λ_i in the model). Little empirical work exists on whether this can be influenced or changed, however some work has shown how such concerns over social image interact with views of the self. For example, Bursztyn et al. (2017b) provides evidence that having higher self-esteem results in lower demand for a conspicuous status good, inferring that self and social image appear to be substitutes. Similarly, and in line with decades of sociologist work, Lavecchia et al 2016 highlight that strengthening positive identities among adolescents can influence behavior. Such research has shown that individuals behave differently when prompted or "primed" to think of themselves in a specific way. For example, Cohen et al 2006 show how a values-affirmation intervention among low-income and minority students increased grades among the treatment group in the short and long run. Studies have also shown the unintended consequences of negative labels on adolescents who are struggling and how labeling can lead to self-fulfilling prophecies (Dodge et al 2007; Harris 1994). Economists have also devoted increasing attention to the role that a person's sense of self, or identity, plays in explaining behavior (Akerlof & Kranton 2000; Benabou & Tirole 2003, 2006).

3.4 Experiment Setup

3.4.1 Intervention

The Chicago Police Department runs various youth programs geared towards mentoring and helping youth prepare for a career in public safety and law enforcement. This includes a Chicago Police and Fire Training Academy that trains high school students in preparation for

black males' rates of gun possession and their norms around carrying guns.

the police academy or the fire academy. [4] The CPD Police Cadet Program is another career enhancing program that provides older adolescents (18 and above) with paid work experience and prepares participants directly for the police exam. CPD also runs a Peer Jury program that allows young community members to determine the consequences of minor offenses done by guilty youth. Lastly, they also run a mentoring program called "Youth Explorers" for youth as young as eight. CPD was looking to increase their school-based recruitment for these programs and enhance community engagement in general they were open to the opportunity of partnering with me to study the impacts of peer pressure in this particular context.

Given CPD's desire to recruit from the communities they serve, I worked with them in designing a school-based recruitment strategy that includes two police officers delivering a presentation that summarizes the different CPD youth opportunities. I worked with two police officers in particular to deliver the school presentations. These two police officers were familiar with the programs CPD runs and had relationships with many Englewood schools to facilitate buy-in from school administrators.

The presentations lasted about 10-15 minutes with the officers discussing each of the CPD youth programs (please see Appendix for presentation 3.2). Following these presentations, I would then use the sign-up form passed to all students as means to test the peer pressure mechanism. Sign-up forms were passed out immediately following the presentation and students were asked to hold all discussion and questions until after the form was returned.[5]

Both forms summarized the programs discussed, and then asked students if they would be interested in learning more about any of them (and if so, which ones).[6] The "private" form stated that "your interest in any of the above programs will be kept completely private from everyone including other students in your school". The "public" form varied this

4. The program includes summer internships and financial assistance for college.

5. This was done to ensure that each youth would make a private decision and not be influenced by a peer asking a question.

6. Forms can be found in Appendix, 3.3.

language only slightly and stated that "your interest in any of the above programs will be kept completely private from everyone except other students in your school." These sentences and differences were also bolded and underlined to increase the changes that students would actually read it. Given the fact that these programs require an application and have specific requirements, students could not sign up on the spot for the actual program. Students were told if they showed interest in any of the programs, a CPD officer would reach out to follow-up.

As an additional test for the peer pressure mechanism related to CPD, the forms also offered youth a chance to attend a baseball game with CPD officers in Summer of 2018 for the first set of presentations and a basketball game in Spring of 2019 for the second set of presentations. Under the "private" condition, winners of the game lottery are announced via email. Under the "public" condition, the form said that winners of the game lottery "may be announced in school." The peer social group was the same across the forms and the C2C control and treatment group in that it was everyone in the school according to the treatment. Presentations were held for the entire school if the school population was small enough (which was more common in the 2018 presentations for the small schools that were shutting down), or by grade level.[7] The ideal presentation size was about 15-20 students to ensure conditions were quiet and private for all students so the treatment manipulation only came from reading the form. The goal was to set up each presentation to ensure youth had plenty of personal space to make these decisions without the influence of peers looking over on each other's forms with either personal desks or plenty of space between chairs. In some cases, I also randomized seating to ensure youth would not be sitting next to their friends and therefore are less likely to talk or look over at others. This succeeded for the majority of situations, however roughly 25% of the time I was unable to maintain this required setup. This often happened due to a school administration deciding last minute to change the number of students per presentation and lacking the facilities to set up a situation where

7. A handful of presentations were done at the classroom level.

each student could have personal space. In these cases, students were frequently talking to each other before and after the forms went out and looking at each other's forms. I account for these problematic sessions in a few ways. First, I look at the just the impact of being offered either the private form or the public form on take-up. Second, given that the non-compliance was a result of administration oversight we could consider this non-compliance as random and drop these problematic sessions from the analysis. Lastly, we also run a specification where we consider these problematic sessions as non-compliant with the private form treatment and account for this in the analysis through an instrumental variable set-up. However, considering these non-compliant cases as "public" forms introduces additional variables into the experiment given that in our true "public" form scenario students still cannot talk to each other to coordinate on sign-up or learn about each other's preferences. This would be violated here.

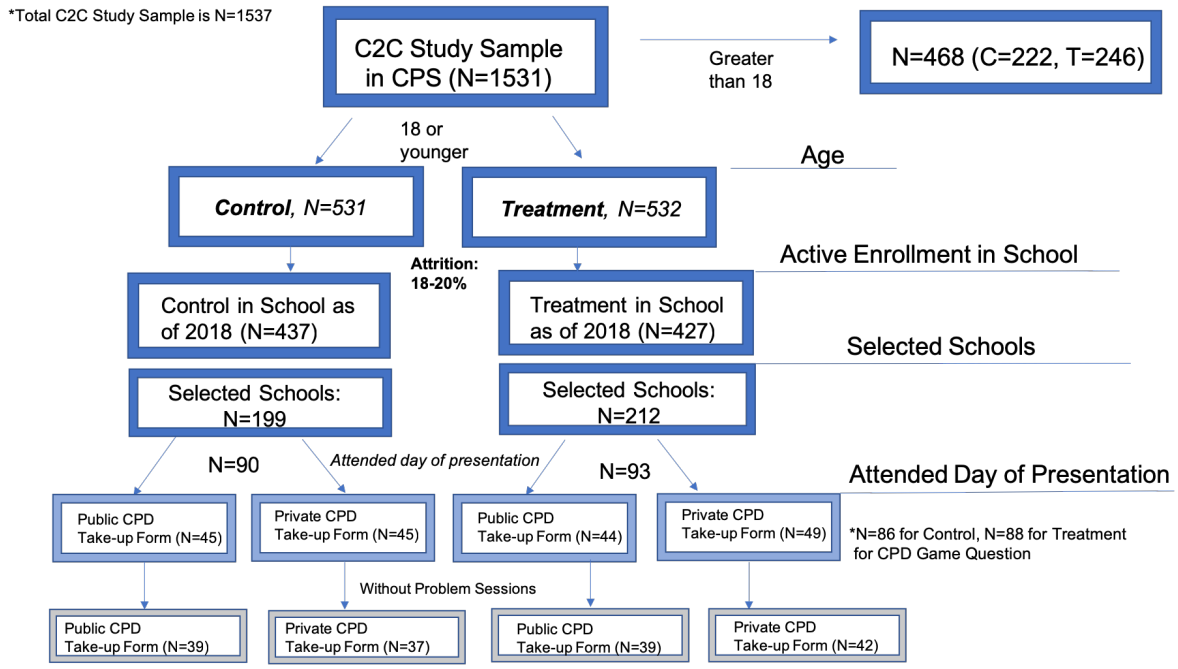
I also offered a small food incentive to youth to return the form completed. Two types of forms were randomly passed out to test the peer pressure mechanism. Given that the stigma of working with CPD is high in Chicago, similar to other police relationships in majority-Black communities (Kirk and Papachristos 2011; Sampson 2012; Weitzer 2002; Desmond et al 2016), I expect that a public sign-up to learn more these programs comes with a high social cost. I also expect any public sign-up for a fun activity involving CPD to come with a stigma, although possibly less so than actually showing interest in joining the police force.

Although the CPD youth program treatment is subtle, previous literature has shown that even using such subtle language can generate substantive differences in take-up (Bursztyn and Jensen 2015; Bursztyn et al 2017a). Although this method will likely underestimate the impact of peer pressure related to working with CPD, other more public treatments (such as a public sign-up sheet) would have required either a much larger sample if treatment was done at the classroom level or could have actually put youth at risk if there really is a significant stigma with working with CPD. All forms collected from the youth were indeed kept confidential and private, including the youth who won the baseball or basketball tickets.

The game lottery question was intended to provide a more salient public/private treatment given the clear differences in how winners are announced.

3.4.2 Experimental Sample and Procedures

Figure 3.1: Experiment Setup and Sample



Experimental procedure and sample selection is traced beginning with the total sample we could have had in these experiments and the sample size we ended up with.

Figure 3.1 displays the experimental sample and procedures for determining the population of youth who participated in these mechanism experiments. To determine what the starting off sample attrition would be for this type of experiment to be conducted in schools, by January 2018 1,061 of the 1,531 CPS study youth for C2C were 18 or under the age of 18 and should likely be enrolled in school. I find that of those 1,061 (N=531 for the control group, N=532 for the treatment group), 863 were classified as being actively enrolled as of the start of the presentations. This creates a baseline attrition rate of 18.7% for the sample, and is not differential between the C2C treatment and control groups.

I then decided to concentrate on reaching the 15 schools (including many "alternative" schools within CPS) that had the highest concentration of study youth at the time of the experiments in order to maximize C2C study participation. Over the course of the year of presentations, we were able to engage with and present at 10 of these schools, that according to our records should have had 411 C2C study youth enrolled at the time. However, a few issues came up in reaching this sample size. For the presentations during May 2018, attendance was very low during the last few weeks of the school year. Furthermore, CPS administration had decided in Spring of 2018 that four of the high schools that had the highest concentration of C2C youth would either shut down at the end of the 2018 school year or be allowed to stay open until their current students graduate. This further exacerbated attendance during the final month of the 2018 school year. Some of these schools also included last chance or alternative schools that students attend when they can no longer attend a neighborhood school for various reasons including misconduct, personal circumstances, chronic truancy, etc. Attendance at these type of schools are also known to be considerably lower than neighborhood CPS schools. Our C2C study sample size was 90 control youth and 93 treatment youth after all the presentations, with a total of 183.[8] Given that attendance on the day of the presentation is certainly not exogenous, I address generalizing these results to the other C2C study youth not represented in this sample in the concerns section.

Overall, 749 youth were presented to across the 10 schools, with 183 of them being C2C study youth. After the presentations concluded, I found out that at one high school, the officers giving the presentation currently run a mentoring program there with many of the students with one goal being trying to change CPD perception. Given that the officers' and CPD's presence in this school was treated very differently than the other schools, I look at the results within this school and without this school in the analysis.

8. For one school, we were unable to offer tickets to a game with CPD and therefore the sample size for the Game question is N=174, 86 for control and 88 for treatment.

3.4.3 Empirical Strategy

Presenting to schools with a high concentration of C2C youth and randomizing at the student level the type of form passed out ensured that roughly half of each study arm would receive the private form and half would receive the public form as we can see in figure 3.1. I would then compare the difference in take-up between the forms by study arm with the following regressions reduced form regressions:

$$Takeup_{CPD} = \alpha + \beta Private + \delta Treat + \eta Private \times Treat + \epsilon \tag{3.2}$$

$$Takeup_{game} = \alpha + \beta Private + \delta Treat + \eta Private \times Treat + \epsilon \tag{3.3}$$

Where $Treat$ is an indicator for being randomly assigned to the C2C treatment group, and $Private$ is and indicator for receiving the form that guarantees privacy. The η in the equations represents the differential impact the private form has among the treatment group when comparing the control group. $Takeup_{CPD}$ is an indicator for the youth who expressed interest in learning more about any of the CPD youth programming. $Takeup_{game}$ is an indicator for youth who decided to participate in the lottery. Using identifying information collected on the take-up forms (date of birth and name), I can identify which youth within schools that are part of the C2C study.

To account for the non-compliance in ensuring privacy during the experiments, I run an alternative specification where can estimate the a two-stage least squares regression, $Takeup_{CPD/game} = \alpha + \beta Comply_Private + \delta Treat + \eta Comply_Private \times Treat + \epsilon$ where $Comply_Private$ is an indicator for complying with the private form assignment which fails to happen in the problematic sessions described earlier. In these problematic sessions, if a student received a private form but the environment failed to create an actual private atmosphere, we do not consider this as complying with the private treatment form and therefore this student is coded as 0 for $Comply_Private$. $Comply_Private$ is instrumented with $Private$ and $Comply_Private \times Treat$ is instrumented with $Private \times Treat$. However,

considering these non-compliant cases as public forms introduces additional factors into the experiment given that in our true public form scenario students cannot talk to each other to coordinate on sign-up or learn about each other's preferences. This was violated in the problematic sessions, with possible coordination and social learning influencing take-up. Therefore, my preferred specification is to drop these problematic sessions all together under the assumption that this non-compliance was essentially random given that it was a result of the school administration altering the experimental setup.

3.4.4 Randomization

Tables 3.1 and 3.3 show that baseline balance was achieved across a number of observables between the private and public forms for both the overall sample and the C2C study sample. Tables 3.11 and 3.12 in the Appendix show that balance was achieved with the C2C study arms as well. I also show that we fail to maintain balance in the baseline covariates when we drop the problematic sessions in the overall sample (see Table 3.2), but maintain balance in the C2C study sample (see Table 3.4). Lastly, in the Appendix we see that youth in the C2C study are more likely to be older, black, female and receive free lunch compared to youth not in the C2C study but were present during the CPD presentations (see Table 3.10).

Table 3.1: C2C Mechanism Baseline Check–all found within CPS

Variable	Public	Private	Diff	Obs
Age as of July 2018/March 2019	16.58	16.53	-.05	686
Female	.45	.47	.02	686
Assigned to C2C Treatment	.49	.52	.03	183
Black	.76	.8	.04	691
Hispanic	.19	.15	-.04	691
Free Lunch	.85	.82	-.03	691

Significance between the mean differences for the private vs. public assignments was estimated using a linear regression with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 3.2: C2C Mechanism Baseline Check–all found within CPS and no problem sessions

Variable	Public	Private	Diff	Obs
Age as of July 2018/March 2019	16.58	16.56	-.02	523
Female	.42	.5	.08*	523
Assigned to C2C Treatment	.5	.53	.03	157
Black	.76	.84	.08**	527
Hispanic	.18	.1	-.08***	527
Free Lunch	.84	.8	-.04	527

Significance between the mean differences for the public vs. private assignments was estimated using a linear regression with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 3.3: C2C Mechanism Baseline Check–all C2C study youth

Variable	Treatment	Control	Diff	Obs
Age as of July 2018/March 2019	16.9	16.84	.06	183
Female	.51	.53	-.02	182
Private CPD Sign-up Form	.53	.5	.03	183
Black	.94	.93	.01	183
Hispanic	.03	.04	-.01	183
Free Lunch	.9	.92	-.02	183

Significance between the mean differences for the treatment vs. control assignments was estimated using a linear regression with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 3.4: C2C Mechanism Baseline Check–all C2C study youth and no problem sessions

Variable	Public	Private	Diff	Obs
Age as of July 2018/March 2019	16.73	16.8	.07	157
Female	.53	.54	.01	156
Assigned to C2C Treatment	.5	.53	.03	157
Black	.9	.96	.06	157
Hispanic	.05	.03	-.02	157
Free Lunch	.9	.9	0	157

Significance between the mean differences for the private vs. public assignments was estimated using a linear regression with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

3.5 Results

3.5.1 Overall, non-C2C study youth results

Before I turn to the main results comparing take-up by each C2C study arm, I wanted to show the impact of receiving the private form on take-up rates across the full field experiment sample with and without the C2C study youth to get a better sense of how CPD is perceived among youth in these particular schools. We might think that because C2C youth were referred to the program because there were at higher risk of becoming involved in violence, their response to CPD may not be in line with their peers.

Table 3.5 shows the overall take-up results for the CPD youth programming using our reduced form model, dropping the problem sessions, and with and without the high school that has a CPD connection. In the Appendix (Table 3.14) I report the alternative specifications including the IV. In my preferred specification, we see an increase in CPD program take-up if the youth received a private form, and this increases to 5.4 percentage points when we do not include the CPD connected school or any of the study youth. However, this is not statistically significant at any conventional levels.

I find similar differences across the various regressions run when I look at the impact of take-up in the lottery to go to a sports game with CPD officers across all youth who participated in the presentations (see Table 3.6) and within the non-study youth only. Specifically we see that private take-up increases take-up in the game by 2.3-7.3 percentage points, suggesting peer social concerns among the non study youth, although these results are not significant at conventional levels.[9]

Given the reputation of CPD in certain Chicago neighborhoods, I was surprised that we were unable to detect larger significant peer social concerns related to signing up for CPD programming or going to a game with officers. There are a few possible reasons for why this might be the case. First of all, the treatment itself may be too subtle so that I am under-

9. See Table 3.13 for alternative IV specification with little effect.

powered to detect smaller differences in take-up among this set of youth. Future work will be needed to obtain more precise results. Second, these non-study youth could be different from the C2C population overall (see Table 3.10 in the Appendix) and it is possible that among the non-study youth who are less likely to have previous criminal justice involvement we observe there is less of a stigma associated with CPD. Lastly, given the schools these students attend these youth may also lack career opportunities or free and fun events such as going to baseball game. Therefore any social cost of interacting with CPD may be worth the opportunity for these particular youth.

As discussed earlier, I learned after the presentations were completed one high school had a strong relationship with CPD as the officers presenting ran their own mentoring program there and were very familiar with all the students here. Given that the social norms here related to CPD (or these CPD officers in particular) are likely different compared to the other schools or communities, we might be able to learn something related to how youth respond to different social norms by looking at this sub-group analysis. I do find that receiving the public form in this school actually increased take-up by 6.6 percentage points for CPD youth programming (for all students in this school) and by 51.4 percentage points for the non-C2C study youth ($p < .05$). For the game, I see the public form increase take-up by 15.3 percentage points to 27.1 percentage points for the CPD game when we drop study youth (although neither of these findings are significant at conventional margins, they are larger than any of the take-up differences in the other schools). Interestingly, this case suggests peer pressure to conform to a social norm, although in this school this means social acceptability to engage with CPD.

Table 3.5: Private/Public Take-up in CPD Programs across all youth

Sample/Specification	Public	Private	Diff	St. Error	P-value	Obs
All schools, reduced form	.47	.446	-.024	.036	.508	749
No problem sessions	.489	.505	.016	.043	.704	553
No problem sessions, no CPD connection, no study youth	.438	.492	.054	.051	.296	379
School with CPD connection	.630	.564	-.066	.108	.540	85
School with CPD connection, no study youth	.8	.286	-.514**	.221	.035	17

Differences between take-up by type of form was estimated using a linear regression: $Takeup_{CPD} = \alpha + \beta Private + \epsilon$, with robust standard errors, where $Private$ is an indicator for receiving the private take-up form and $Takeup_{CPD}$ is 1 if they express interest in any of the CPD youth programs, 0 otherwise. *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Private/Public Take-up with CPD game across all youth

Sample/Specification	Public	Private	Diff	St. Error	P-value	Obs
All schools, reduced form	.504	.487	-.017	.037	.654	726
No problem sessions	.504	.527	.023	.043	.594	530
No problem sessions, no CPD connection, no study youth	.449	.522	.073	.052	.162	365
School with CPD connection	.717	.564	-.153	.105	.147	85
School with CPD connection, no study youth	.7	.429	-.271	.249	.292	17

Differences between take-up by type of form was estimated using a linear regression: $Takeup_{game} = \alpha + \beta Private + \epsilon$, with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

3.5.2 C2C study youth results

When I turn to look at these differences in take-up by C2C treatment status, the sample size does decrease considerably and most of these results become suggestive. Table 3.7 shows the various specifications we run to determine if the privacy guarantees on the form influenced take-up for any of the CPD youth programs. Across the specifications, we find that among the control group there is no significant impact of receiving a private form on take-up, although they are generally positive (biggest difference we see is 9.1 percentage points). This might be due to the subtle language used in the forms to indicate privacy guarantees.[10]

When I look at the C2C treatment group, surprisingly I find large and positive impacts of the public form on take-up, where receiving a public (private) form increases (decreases) take-up between 9 and 22 percentage points, depending on the specification used.[11] However,

10. See Table 3.15 in Appendix for alternative specifications that also do not lead to conclusive results.

11. The one exception is the results within the CPD connected school, which might have a different set of social norms related to CPD.

none of these results are statistically significant at conventional levels.

Table 3.7: C2C Private/Public CPD Programming Take-up Difference

	Public Take-up	Private Take-up	Diff	P-value	Obs
All Sessions					
C2C Assigned Control	.533	.489	-.044	.677	90
C2C Assigned Treatment	.591	.49	-.101	.333	93
Difference in take-up by private form btw T and C			-.057	.704	183
No problem sessions					
C2C Assigned Control	.513	.568	.055	.637	76
C2C Assigned Treatment	.641	.548	-.093	.397	81
Difference in take-up by private form btw T and C			-.148	.355	157
No problem sessions, no CPD connected school					
C2C Assigned Control	.455	.546	.091	.556	44
C2C Assigned Treatment	.7	.480	-.22	.137	45
Difference in take-up by private form btw T and C			-.311	.147	89
CPD connected School					
C2C Assigned Control	.588	.6	.012	.948	32
C2C Assigned Treatment	.579	.647	.068	.685	36
Difference in take-up by private form btw T and C			.056	.819	68

The regressions were estimated using a linear regression: $Takeup_{CPD} = \alpha + \beta Private + \delta Treat + \eta Private \times Treat + \epsilon$. η represents the difference in take-up by private form between treatment and control. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1.

Turning to the results for the CPD baseball and basketball game by C2C treatment, Table 3.8 compares the take-up rates by form within each treatment status across the various specifications previously described.[12] We find consistent, albeit not quite statistically significant at conventional levels, positive impacts of receiving the private form with magnitudes ranging from 5 to 25 percentage points. These results suggest the presence of peer social concerns among the control youth and a stigma attached to working with CPD.[13] One reason why we may see more consistent impacts on this question compared to the CPD program question is the salience of the treatment, with the lottery question intended to be more clear in its privacy guarantees.

When I turn to the C2C treatment group here, I also find consistent large and positive impacts of the public form on take-up, where receiving a public (private) form increases (decreases) take-up by roughly 24 percentage points.[14] This result is statistically significant

12. The sample size is slightly smaller for this question because for one school presentation, the date of the sports game had passed by the time we were able to get in and present.

13. The only exception to this are the results within the CPD connected school that has a different set of norms related to CPD.

14. Alternative specification in Appendix table 3.16 shows similar findings.

at conventional levels ($p < .05$) using two of the three specifications. We also find this take-up difference among the C2C treatment group to be statistically different from the take-up difference among the control youth ($p < .05$ or $p < .10$ depending on the specification).

While these results are not what I had initially expected (with the initial hypothesis being that the form would not make an impact on take-up among the treatment youth), they do suggest C2C's ability to help youth move against prevailing social norms in a public context. While we find suggestive evidence that there exists a stigma and social costs to interacting with CPD among the control group, C2C treatment youth are not susceptible to this pressure and in fact are more likely to interact with CPD when privacy is not ensured. These results suggest that C2C treatment youth care about their social image but seek to act in a way that is contrary to social norms.[15]

Another possibility is that C2C changed the set of peers or social group the C2C kids care about. While both forms stated that peers in that particular school could find out, presumably youth care more about specific groups of youth in their respective social circles. If C2C changed the type of youth they consistently hang out with, this could have changed the social norms or concerns they are subject to. For example, if C2C youth began hanging out with more future oriented individuals, this might make them more likely to sign up for CPD programming in a public context.

Future work includes increasing the sample size to ensure these results are robust, as well as more interviews with participants and qualitative work to learn more about the various ways in which peer pressure may be operating.

15. According to the model discussed earlier, this would mean $\lambda_{ij} < 0$.

Table 3.8: C2C Private/Public CPD Game Take-up Difference

	Public Take-up	Private Take-up	Diff	P-value	Obs
All Sessions					
C2C Assigned Control	.558	.605	.047	.666	86
C2C Assigned Treatment	.610	.384	-.226**	.033	88
Difference in take-up by private form btw T and C			-.273*	.071	174
No problem sessions					
C2C Assigned Control	.541	.658	.117	.318	72
C2C Assigned Treatment	.695	.451	-.244**	.03	76
Difference in take-up by private form btw T and C			-.361**	.026	148
No problem sessions and no CPD connected school					
C2C Assigned Control	.45	.7	.25	.115	40
C2C Assigned Treatment	.588	.348	-.24	.138	40
Difference in take-up by private form btw T and C			-.49**	.03	80
CPD connected school					
C2C Assigned Control	.647	.6	-.047	.791	32
C2C Assigned Treatment	.789	.588	-.201	.203	36
Difference in take-up by private form btw T and C			-.154	.516	68

The regressions were estimated using a linear regression: $Takeup_{Game} = \alpha + \beta Private + \delta Treat + \eta Private \times Treat + \epsilon$. η represents the difference in take-up by private form between treatment and control. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1.

3.5.3 Concerns and next steps

One of the benefits of the CPD program take-up question is that the treatment tested only changed one word in the question to vary the privacy assurance given. This subtle treatment of courses comes with the cost that it may have been too subtle for this particular population given a low baseline GPA for the C2C study youth. One concern for the take-up question regarding the CPD game lottery (winners announced in school–public condition vs. winners announced by email–private condition) is that these different methods of finding out could also influence take-up, irrespective of the peer concerns. For example, we did see an increase in days attended among the C2C treatment youth. Therefore, they might be more willing to say yes when the winners are announced in school because they are more likely to attend school all together.

Another concern one might have about these results is the dependency on attendance in school on the day we present. We might be concerned that the C2C youth who show up that day are not representative of the full sample and therefore we can't extrapolate these findings to the full C2C study. Table 3.9 shows the baseline characteristics of all the youth in the

study and the differences in these characteristics by whether or not they participated in these mechanism experiments. We do find that significant differences in baseline GPA, prior arrest records, prior misconducts, and days attended in school, where the youth who showed up for the mechanism experiment appear more connected to school with less behavioral issues. The significant differences in age and grade are less of a concern given the youth who participate in these mechanism experiments are bound to be younger since we conducted the experiments years after the first cohort has completed programming. While I have balance within the C2C study youth who show up on the given day of the experiment, given these significant differences more work is needed to increase the sample of the youth who participate in these mechanism experiments to ensure they are representative of the full study sample.

Given these potential concerns, future work will focus on increasing the sample size and the precision of the results, especially with the take-up question regarding CPD programming.

Table 3.9: C2C Baseline Check, Cohorts I, II, III, IV & V by Mechanism Participation

Variable	Mech Study	Non Mech Study	Diff	p-value	Obs
Age	15.32	15.86	-.54***	0	1531
% female	.51	.42	.09	.491	1531
% black	.950	.940	.01	.582	1531
% hispanic	.04	.05	-.01	.645	1531
School grade	9.53	9.86	-.33***	0	1470
Free/reduced lunch	.940	.96	-.02	.534	1531
Learning disability	.22	.16	.06	.175	1448
Homeless	.14	.22	-.08	.442	1448
Previous GPA	2.47	1.88	.59**	.016	1237
Inactive in CPS	.05	.14	-.09	.201	1531
Not enrolled at any point prior	.13	.23	-.1	.883	1531
Any previous school incident	.25	.34	-.09**	.027	1327
Previous days present	155.24	128.36	26.88***	.003	1430
Any prior arrest	.13	.4	-.27***	0	1531
Any prior violent arrest	.08	.26	-.18**	.023	1531
Any prior property arrest	.04	.19	-.15**	.016	1531
Any prior drug arrest	.01	.07	-.06	.570	1531
Any prior other arrest	.06	.27	-.21**	.018	1531
Joint F Test–Full sample	Number of obs. =	1531			
	F(10 , 1494) =	2.029			
	Prob > F =	.027			

Significance between the mean differences for the mechanism vs. non-mechanism groups was estimated using a linear regression with block level and cohort fixed effects. Standard errors are robust. A joint F test was performed to test their joint significance.
** p<0.01, ** p<0.05, * p<0.1

3.6 Discussion

3.6.1 How C2C Might Work

The results presented to date are promising in showing how C2C can help youth counteract prevailing social norms in a public context. Although future work will be done to increase the sample size, I do explore what elements of C2C could have led to these results. In terms of the context and environment many of these youth are faced with daily, one theme that came out in our conversations with youth and program providers is the role social media plays in the violence in Chicago.

Many news articles from *Nature* to the *Washington Post*, as well as academics (Patton 2019) have documented how social media has changed and exacerbated the nature of gun violence among young people in Chicago. It has changed how gangs recruit members, how they conduct business, initiative violence and retaliate (Nature 2018).[16] Oftentimes minor disputes such as insults on social media become deadly disputes. From our conversations with youth, they often mention they feel the need to respond in a specific way on social media so they don't look weak in front of their peers. While these social and peer pressures to respond to an insult in a given way certainly existed before, social media has amplified the audience of people watching. This makes it more pressing that we find interventions that can help youth withstand these pressures and respond in more positive ways.

While little research exists on how to change how much people care about others opinions (Bursztyn et al 2017a), some work has shown that more positive self-image reduces demand for social-image (Bursztyn et al 2017b). There are features within the trauma-informed therapy and the advocacy of C2C that work to improve self-esteem and creating a positive self-image. This section discusses these elements in more detail and pulls these insights from the various qualitative research conducted to date with the C2C participants and program providers.

16. `https://www.nature.com/articles/d41586-018-06169-8`

Advocacy

An advocate from Youth Advocate Programs (YAP) is the first point of contact for youth, and they are persistent in recruitment efforts. Advocates often come from the very same neighborhoods the youth live in and have confronted many of the same challenges. Before any CBT or other services are offered, the advocate spends time developing a strong personal relationship with the youth. Youth typically spend 5-12+ hours a week with their advocate in one-on-one and group activities that range from playing sports to visiting new places in Chicago. The advocate provides wraparound support, from meeting basic necessities to motivation and direction on larger goals (such as employment or college). Advocates help youth reconnect with school to achieve success in the classroom and help identify and cultivate key positive family relationships to ensure persistent support across many domains of a youth's life.

YAP program providers have told us that a unique component of their programming is that from the very beginning advocates focus on what they can do and how they can make their clients' lives better. Instead of emphasizing deficits or problems, they tell the client they are here to "be the best version of yourself" and "everyone really wants to see you succeed and see you win." They work on getting youth to believe in themselves and feel more confident about their decisions. By offering genuine care and support, advocates can also challenge the view youth have of their current and future self. Recent work by DeLuca et (2016) suggests that youth in high-poverty neighborhoods have developed quite a bit of grit, but just need something else to latch onto (typically a project/hobby) to create a positive identity. As one youth mentioned in a focus group, "I feel like me, but better." Participants have also indicated how after the program they eventually often become mentors to their peers in their communities. This new level of confidence combined with being seen as a new role model among their peers may have led to the counter-intuitive findings I document in this paper.

Trauma-Informed CBT

Advocates take the youth they serve to the trauma-informed CBT sessions, called SPARCS (Structured Psychotherapy for Adolescents Responding to Chronic Stress). Led by clinically trained therapists from Children's Home + Aid, SPARCS is designed specifically for youth who have been traumatized, often repeatedly, and continue to live with high levels of environmental stressors, such as neighborhood violence or concentrated poverty. A key component of SPARCS is helping youth understand that they are not "bad" or "crazy", but that stress and/or past traumatic experiences can influence their decision-making. Therapists often stress how "emotional leftovers" from bad experiences can make youth more willing to engage in short-term coping strategies that can ultimately lead to worse outcomes. Their goal is to help youth better understand and cope with past experiences so that they can make better choices in the future. SPARCS sessions incorporate many tangible skills such as teaching positive coping mechanisms, building problem-solving skills and working on communication skills. However, before any of these skills are taught they first help them change the way they see themselves.

Given the design of the experiment, I will not be able to highlight just one section of C2C that accomplished behavior change, but through hours of conversation and qualitative interviews, I believe this can give some insight into what was important to help youth counteract specific social pressures and norms. During focus groups, many youth highlighted how more confident and positive they felt. They would often share that they feel like better versions of themselves.

3.6.2 Police Stigma and Possible Future Research

These presentations allowed us to not only test how peer pressure is operating within C2C study youth, but also shed light on police stigma among adolescents living in high violent communities. It's been heavily documented that negative encounters with law enforcement as well as police misconduct leads to legal cynicism within black communities (Kirk and

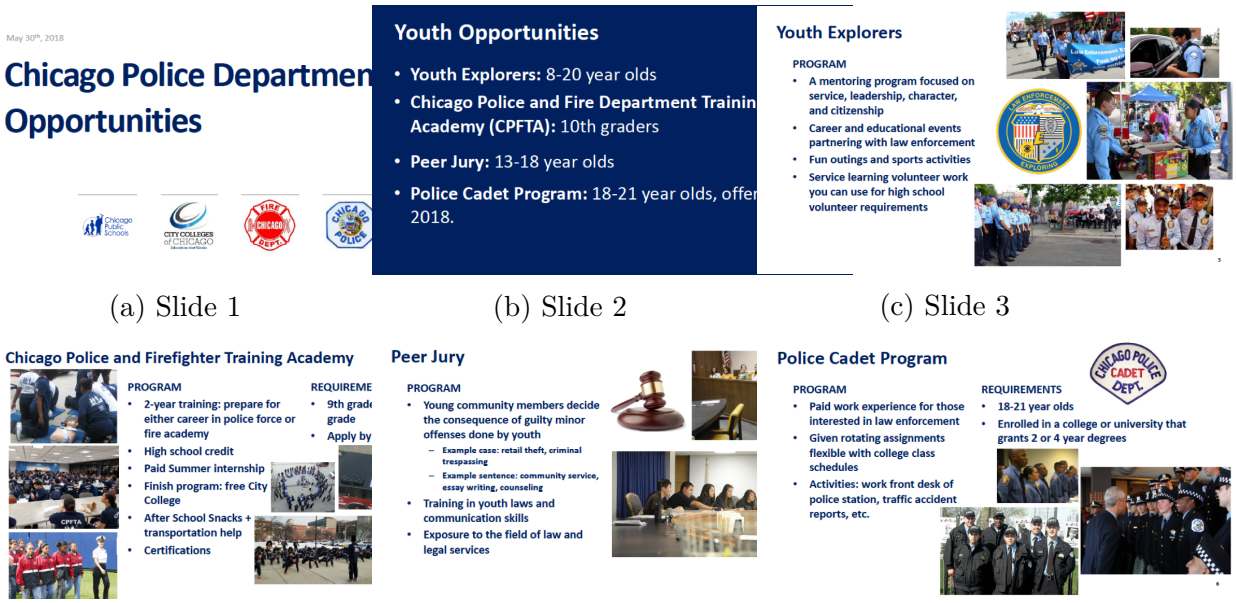
Papachristos 2011; Sampson 2012; Weitzer 2002; Papachristos et al 2015). Chicago has seen considerable strain in police-community relationships following the Laquan McDonald shooting and the subsequent critical Department of Justice investigation of CPD.

Many school officials in these high-violent neighborhoods were concerned that none of their students would be interested in working with CPD given the consistent negative interactions the youth encounter. However, the fact that 45% of all students who were given the presentation (averaging across both private and public forms, including problematic sessions) showed an interest in working with CPD shows that 1) youth in these communities need feasible career support and trajectory, and 2) that CPD stigma may not be insurmountable. Although there was significant variation across schools, the lowest average take-up was still 29%. Students also showed a keen desire to engage with police officers in discussing topics such as police brutality and frequent police stops. These Q&A sessions held after the presentation and after the forms were given out showed the promise that thoughtful community engagement could improve police-community relations. Although this research is not focused on police-community relations, these presentations highlighted the need for more research in understanding the dynamics at play and how such a relationship can be improved.

3.7 Conclusion

This paper demonstrates suggestive evidence that a crime-reducing intervention (C2C) achieved results in part through enabling youth to counteract prevailing social norms in a public context. Qualitative evidence from interviews with program providers and youth suggest that this may be due to C2C's ability to increase self-esteem and confidence, thereby reducing demand for social image that complies with prevailing norms. Future work will be needed to obtain more statistical precision for the results however the preliminary results presented to date are promising in showing how a social policy intervention might work in high-violent neighborhoods.

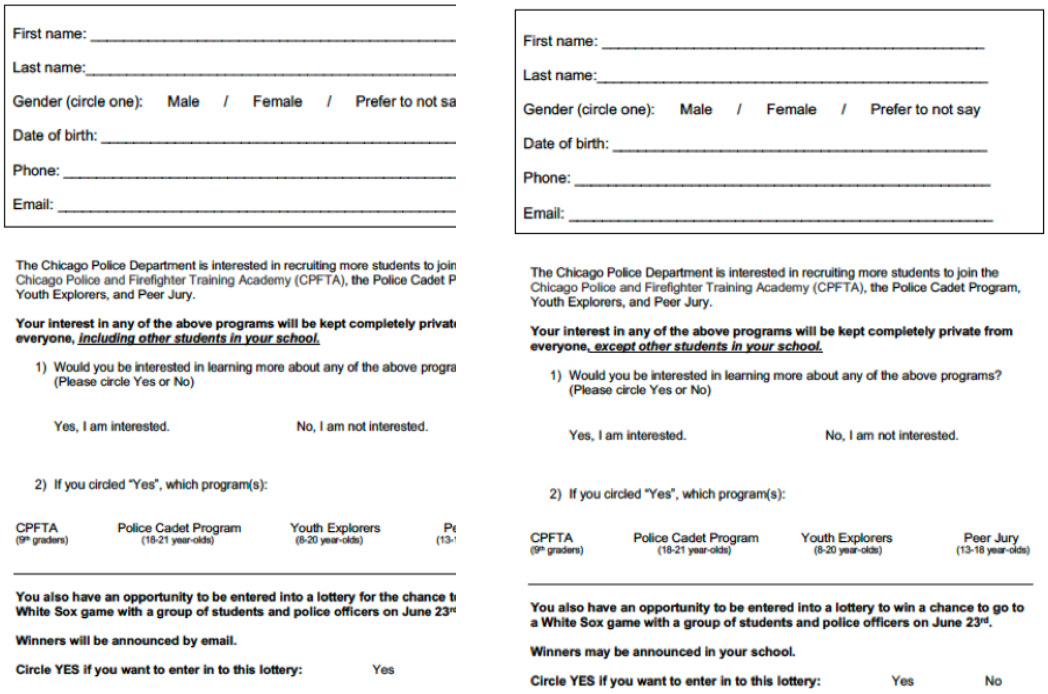
3.8 Appendix



(a) Slide 1 (b) Slide 2 (c) Slide 3

(d) Slide 4 (e) Slide 5 (f) Slide 6

Figure 3.2: CPD Youth Presentation to Schools-May 2018



(a) Private Form

(b) Public Form

Figure 3.3: Types of take-up forms passed out during experiment

Table 3.10: C2C Mechanism Baseline Check–Between C2C study youth and non-study youth

Variable	Non-Study	Study	Diff	Obs
Age as of July 2018/March 2019	16.44	16.87	.43***	686
Female	.44	.52	.08*	686
Black	.73	.93	.2***	691
Hispanic	.21	.04	-.17***	691
Free Lunch	.81	.91	.1***	691

Significance between the mean differences for the study vs. non-study assignments was estimated using a linear regression with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 3.11: C2C Mechanism Baseline Check–within C2C T status

Variable	Public	Private	Diff	Obs
Age as of July 2018/March 2019	16.75	17.04	.29	93
Female	.48	.53	.05	93
Black	.91	.96	.05	93
Hispanic	.02	.04	.02	93
Free Lunch	.89	.92	.03	93

Significance between the mean differences for the private vs. public assignments was estimated using a linear regression with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 3.12: C2C Mechanism Baseline Check–within C2C C status

Variable	Public	Private	Diff	Obs
Age as of July 2018/March 2019	16.98	16.71	-.27	90
Female	.59	.47	-.12	89
Black	.91	.96	.05	90
Hispanic	.07	.02	-.05	90
Free Lunch	.93	.91	-.02	90

Significance between the mean differences for the private vs. public assignments was estimated using a linear regression with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 3.13: Private/Public Game Take-up with CPD officers across all youth, alternative specifications

Sample/Specification	Public	Private	Diff	St. Error	P-value	Obs
IV	.504	.481	-.023	.051	.654	726
IV: all schools except school with CPD connection, non study youth	.473	.496	.023	.063	.716	535

Differences between take-up by type of form was estimated using a linear regression: $Takeup_{game} = \alpha + \beta Private + \epsilon$, with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 3.14: Private/Public CPD Take-up in Programs across all youth, alternative specifications

Sample/Specification	Public	Private	Diff	St. Error	P-value	Obs
IV	.47	.437	-.033	.05	.508	749
IV: all schools except school with CPD connection, non study youth	.429	.437	.008	.061	.899	549

Differences between take-up by type of form was estimated using a linear regression: $Takeup_{CPD} = \alpha + \beta Private + \epsilon$, with robust standard errors, where $Private$ is an indicator for receiving the private take-up form and $Takeup_{CPD}$ is 1 if they express interest in any of the CPD youth programs, 0 otherwise. *** p<0.01, ** p<0.05, * p<0.1

Table 3.15: C2C Private/Public CPD Programming Take-up Difference-alternative specification

	Public Take-up	Private Take-up	Diff	P-value	Obs
IV Specification					
C2C Assigned Control	.533	.479	-.054	.675	90
C2C Assigned Treatment	.591	.473	-.118	.332	93
Difference in take-up by private form btw T and C			-.064	.718	183
IV, no CPD connected School					
C2C Assigned Control	.5	.409	-.091	.616	58
C2C Assigned Treatment	.6	.352	-.248	.153	57
Difference in take-up by private form btw T and C			-.157	.531	115

The reduced form was estimated using a linear regression: $Takeup_{CPD} = \alpha + \beta Private + \delta Treat + \eta Private \times Treat + \epsilon$. The IV was estimated using $Takeup_{CPD} = \alpha + \beta Comply_Private + \delta Treat + \eta Comply_Private \times Treat + \epsilon$, where we instrument $Comply_Private$ and $Comply_Private \times Treat$ with $Private$ and $Treat \times Private$. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.16: C2C Private/Public CPD Game Take-up Difference-alternative specification

	Public Take-up	Private Take-up	Diff	P-value	Obs
IV					
C2C Assigned Control	.558	.615	.057	.66	86
C2C Assigned Treatment	.61	.344	-.266**	.036	88
Difference in take-up by private form btw T and C			-.324*	.075	174
IV, no CPD connected school					
C2C Assigned Control	.5	.65	.15	.419	54
C2C Assigned Treatment	.455	.21	-.245	.175	52
Difference in take-up by private form btw T and C			-.395	.127	106

The reduced form was estimated using a linear regression: $Takeup_{Game} = \alpha + \beta Private + \delta Treat + \eta Private \times Treat + \epsilon$. The IV was estimated using $Takeup_{Game} = \alpha + \beta Comply_Private + \delta Treat + \eta Comply_Private \times Treat + \epsilon$, where we instrument $Comply_Private$ and $Comply_Private \times Treat$ with $Private$ and $Treat \times Private$. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1.

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