PREDICTING AND PREVENTING GUN VIOLENCE:
AN EXPERIMENTAL EVALUATION OF READI CHICAGO

Monica P. Bhatt
Sara B. Heller
Max Kapustin
Marianne Bertrand
Christopher Blattman

ABSTRACT

Gun violence is the most pressing public safety problem in American cities. We report results from a randomized controlled trial (N = 2,456) of a community-researcher partnership—the Rapid Employment and Development Initiative (READI Chicago)—which provided 18 months of a supported job alongside cognitive behavioral therapy and other social supports. Algorithmic and human referral methods identified men with strikingly high scope for gun violence reduction: for every 100 people in the control group, there were over 11 shooting and homicide victimizations during the 20-month outcome period. Take-up and retention rates were comparable to programs for people facing far lower mortality risk. There is no statistically significant change in an index combining three measures of serious violence, the study's primary outcome. But one component, shooting and homicide arrests, shows a suggestive decline of 64 percent (p = 0.15). Because shootings are so costly, READI generates social savings between $174,000 and $858,000 per participant, implying a benefit-cost ratio between 3.8 and 18.8 to 1. Moreover, participants referred by outreach workers—a pre-specified subgroup —show enormous declines in both arrests and victimizations for shootings and homicides that remain statistically significant even after multiple testing adjustments. These declines are concentrated among outreach referrals with high predicted risk, suggesting that human and algorithmic targeting may work better together.

Monica P. Bhatt
University of Chicago
Crime and Education Labs
33 North LaSalle Street
Suite 1600
Chicago, IL 60602
mbhatt@uchicago.edu

Sara B. Heller
University of Michigan
Department of Economics
611 Tappan Street, Lorch Hall Room 238
Ann Arbor, MI 48109
and NBER
sbheller@umich.edu

Max Kapustin
Department of Economics
Brooks School of Public Policy Cornell
University
Martha Van Rensselaer Hall
Ithaca, NY 14853
kapustin@cornell.edu

Marianne Bertrand
Booth School of Business
University of Chicago
5807 South Woodlawn Avenue
Chicago, IL 60637
and NBER
marianne.bertrand@chicagobooth.edu

Christopher Blattman
Harris School of Public Policy
The University of Chicago
1307 E 60th St
Chicago, IL 60637
and NBER
blattman@uchicago.edu

1   Introduction

Over 170 Americans are shot each day, with young Black men dying of gun homicide—by far their leading cause of death—at almost 20 times the rate of their White peers (CDC, 2020).[1] In previous decades, crime prevention policy often relied on a style of policing that prioritizes street stops and low-level arrests, and on high rates of incarceration (Harcourt, 2005; Raphael and Stoll, 2013). But there is growing evidence that these strategies exact high social costs,[2] most of which are borne by the same communities of color that already bear the burden of gun violence itself. This has fueled demand for ways to reduce shootings without the harms of aggressive law enforcement.

One feature of community gun violence—its concentration—may be key to reducing it. In Chicago, for instance, six neighborhoods accounted for a third of the shootings in 2021, despite containing less than a tenth of the city's population. Within these neighborhoods, moreover, the number of people involved in gun violence is small (e.g., Braga, 2003). If the *ex ante* risk of being a shooting victim or offender is concentrated enough, then intervening with a small set of people could meaningfully and cost-effectively reduce shootings (Abt, 2019; Green et al., 2017; Heller et al., 2022).[3]

Any individually-targeted intervention must overcome two key challenges. The first is identifying and engaging people at high risk of gun violence. This a difficult prediction problem given the complex determinants of shootings (Berk et al., 2009; Chandler et al., 2011; Heller et al., 2022; Wheeler et al., 2019), and a difficult practical problem given the extreme levels of trauma and disconnection in this population (Fagan and Wilkinson, 1998; Anderson, 1999).

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[1] Rates are for non-Hispanic Black and White men ages 18-34. These numbers exclude suicides, accidents, and shootings by police.

[2] See, e.g., Ang (2021); Geller et al. (2014); Jones (2014); Chalfin et al. (2022); Pattillo et al. (2004)

[3] A complement to this targeted, individual-focused approach is to address the "root causes" of gun violence, such as concentrated disadvantage and easy access to guns. A targeted approach can be implemented—and begin to help—quickly, while making structural changes to address ingrained social challenges will take more time (see, e.g., Haveman et al. (2015) and *New York State Rifle & Pistol Association, Inc. v. Bruen*, 597 U.S. (2022)).

The second challenge is finding effective ways to reduce these individuals' risk of being involved in a shooting. Many cities are now funding social services that try to overcome both challenges.[4] Unfortunately, we know relatively little about how to find, let alone engage, those at very high risk of gun violence without law enforcement involvement. Nor is there much rigorous evidence on what social services actually reduce this risk (see literature discussion in Section 2.1 and Appendix A.1).

This paper experimentally evaluates a community-researcher partnership designed to tackle both challenges and thereby reduce gun violence: the Rapid Employment and Development Initiative (READI). READI operated in five of Chicago's highest-violence neighborhoods. The program sought to identify men at the very highest risk of being involved in a shooting using three referral pathways: (1) a machine learning algorithm based on administrative arrest and victimization records, (2) referrals from outreach workers in the communities served, and (3) screening among those leaving prison and jail. Over three years, 2,456 men were randomly assigned either to a READI offer or to a control group free to pursue other available services.

Men assigned to READI were offered 18 months of subsidized, supported work combined with group cognitive behavioral therapy (CBT). The job was designed to provide several elements: a stable source of income to deter illegal work, an incentive to participate in the therapy, a place for participants to build and reinforce new skills and norms, and a reason to spend less time in dangerous settings. Meanwhile, the CBT-informed programming was designed to foster several complementary behavior changes: to help participants reflect on their own thinking, slow down in key moments of conflict, practice less harmful responses in dangerous situations, and adapt their behavior to a legal workplace and identity. Due to the significant barriers to participation this population faces, READI also provided participants with referrals to housing, substance abuse, mental health, and legal services when needed.

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[4] See, e.g., https://www.chicago.gov/content/dam/city/sites/public-safety-and-violenc-reduction/pdfs/OurCityOurSafety.pdf, https://www.phila.gov/2021-04-14-how-the-city-is-addressing-gun-violence-2021-update-to-the-roadmap-to-safer-communities/, https://www.oaklandca.gov/topics/oaklands-ceasefire-strategy, and https://monse.baltimorecity.gov/sites/default/files/MayorBMS_Draft_ViolenceReductionFrameworkPlan.pdf.

Since READI was in the field from August 2017 through October 2021, service delivery was forced to change at the onset of the COVID-19 pandemic; see Section 2.3 for details. Because our outcome window is 20 months, about 76 percent of person-day post-randomization observations occurred before the pandemic.

With respect to the first challenge—identifying and engaging people at very high risk of shooting involvement—READI was a definitive success. Prior to program referral, 35 percent of men in the study had been shot and 98 percent had been arrested, with an average of over 17 prior arrests. Staggeringly, in the 20 months after randomization, there were 11.4 shooting and homicide victimizations for every 100 men in the control group—54 times more than among average Chicagoans, and 2.8 times more than among other similarly-aged men in the five neighborhoods where READI operated.

Despite their many barriers to participating, 55 percent of men assigned to the treatment group attended at least one day of programming. Those who continued to work also remained highly engaged—active participants worked 75 percent of the weeks available to them while in-person programming was occurring. This rate of engagement is comparable to that of interventions for much lower risk populations (such as high school boys) and for much shorter transitional job programs (Heller et al., 2017; Redcross et al., 2016).

On the second challenge—reducing serious violence—the results provide reason for both caution and optimism. We track three main measures of serious violence involvement over a 20-month outcome period using matched administrative data: (1) shooting and homicide victimizations; (2) shooting and homicide arrests; and (3) other serious violent-crime arrests, such as armed robbery and aggravated battery.[5] Our primary pre-specified outcome is a standardized index that averages all three measures of serious violence with equal weights. We also specified several secondary analyses, including how READI affects the index components, as well as an index of all crime and violence weighted by their social costs.

There is no detectable impact of READI on the primary outcome—the simple average

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[5] Our pre-analysis plan is available at `https://osf.io/ap8fj/`.

of the three serious violence measures. The estimated effect of treatment on the treated (TOT) is -0.053 standard deviations ($p = 0.21$). When we break the index into its three components, however, we find suggestive evidence that READI reduced arrests for shootings and homicides. Relative to control compliers, READI participants had 64 percent fewer shooting and homicide arrests (2.1 fewer per 100 participants). This result is statistically significant on its own, but not after adjusting inference for the three hypothesis tests involved in breaking the index into its components (unadjusted $p = 0.05$, adjusted $p = 0.15$). Results for the other two components are less precise. Point estimates show that participants had 18 percent fewer shooting and homicide victimizations but 13 percent more arrests for other types of serious violent crime. Their confidence intervals, however, are too wide to draw clear conclusions (for both, adjusted $p = 0.7$).

When we weight incidents of crime and violence by the costs they impose on society, we estimate that READI reduces these social harms by at least $174,000, and perhaps by as much as $858,000, per participant—about a 50 percent decline ($p = 0.03$). The additional precision comes from the fact that, counter to our expectations, the index components did not move in the same direction, and the large decreases in violence are concentrated in the most socially costly outcomes. Using a range of assumptions, our estimates imply READI's benefit-cost ratio is between 3.8 and 18.8 to 1.

READI also generated heterogeneous treatment effects. Impacts on our primary outcome by referral pathway (a pre-specified subgroup analysis) differ significantly from each other ($p = 0.03$). Participants referred by outreach workers saw serious violence involvement fall by 0.13 standard deviations (adjusted $p = 0.02$), driven by large and statistically significant reductions in shooting and homicide arrests (79 percent, adjusted $p = 0.02$) and victimizations (45 percent, adjusted $p = 0.06$) relative to control compliers in the same pathway. We can therefore say with confidence that READI was more effective at reducing violence, including shooting and homicide involvement, among outreach referrals.

It is harder to say *why* impacts were larger for this group. The pattern of results is

consistent with those men receiving a higher dose of programming (see Section 4.3), but also with them being more responsive to it. Outreach workers were instructed to refer men at highest ex ante risk of gun violence involvement (i.e., selection on $\hat{Y}$). Interviews suggest that, as anticipated, outreach workers also considered referrals' expected gains from treatment (i.e., selection on β). Program staff frequently reported filtering out high-risk men who they felt were not "ready," e.g., not as open to a change of lifestyle or facing too many barriers to participation, and thus unlikely to engage in the programming.

In exploratory analysis, we contrast the outreach and algorithmic screening methods to unpack treatment heterogeneity by referral pathway. Men referred by the algorithm had, on average, a higher predicted risk of future gun violence involvement than men referred by outreach workers. Yet at all levels of predicted risk, outreach referrals were subsequently involved in gun violence at higher rates than algorithm referrals. This suggests that outreach workers' screening methods successfully incorporated risk factors for Y that the algorithm could not observe. However, outreach workers were only partially successful in identifying men with a high β; violence declined only among the subset of outreach referrals who also had high predicted risk. Together, these patterns suggest that outreach workers were not simply selecting on expected gains. Rather, the combination of high observable risk *and* outreach-identified unobservables appears to predict treatment responsiveness. In other words, human and algorithmic referral mechanisms worked better together than either did alone.

From the perspective of scientific hypothesis testing, the mixed program impacts we document make it difficult to give a definitive answer about how READI changes behavior. It likely reduced shooting and homicide offending (as measured by arrests) overall, as well as drastically lowered shooting and homicide victimization for men referred by outreach. But we fail to reject the null for all forms of serious violence across all subgroups. Future research replicating and refining READI's approach would be valuable to learn whether a combination of work and CBT can reduce gun violence among the men at highest risk of it.

From a policy perspective, however, binary hypothesis tests may not be the most useful

basis for decision-making. As others have argued, policymakers should weigh the importance of the outcome, the uncertainty of the estimates, and the availability of other interventions and evidence (Imbens, 2021; Manski, 2019; Ziliak and McCloskey, 2008). From this perspective, a few aspects of the READI results are worth highlighting. For the primary index of serious violence, 79 percent of the treatment effect's confidence interval is below zero. Policymakers can weigh this against their level of uncertainty about the effectiveness of other social service approaches to reduce gun violence, as well the increased social costs that aggressive law enforcement responses can generate. They can also use our estimate of READI's benefit-cost ratio—in essence, an importance-weighted sufficient statistic, as in Viviano et al. (2021)—which suggests that society values READI's impact on violence between 3 and 18 times more than the cost of running the program.

One clear lesson from these results is the potential for a targeted intervention to affect the total amount of gun violence in a city. Despite being less than 0.01 percent of Chicago's population, the 2,456 men in the study sample would have contained about 6 percent of Chicago's shooting and homicide victims during an average 20-month period in the absence of READI, costing society between $700 million and $3.5 billion.[6] And despite being disconnected from and distrustful of many social institutions, these men proved willing to engage in READI. The fact that it is possible to identify and engage a relatively small group at such elevated risk of socially costly outcomes emphasizes the potential of continuing to experiment with approaches to help this extremely disconnected and under-served population.

2 Experimental sample and intervention

2.1 Context & research questions

READI was designed in response to an unprecedented 60 percent spike in Chicago's homicide rate from 2015 to 2016 (Kapustin et al., 2017).[7] As in many cities, Chicago's shootings are

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[6] We calculate the 6 percent figure using the number of shooting and homicide victimizations in the control group as the counterfactual for the number in the treatment group without READI, compared to an average 20-month period between August 2017 and August 2021 (see Section 7 for details).

[7] Though Chicago is sometimes perceived to be the "murder capital" of the U.S., its homicide rate is not an outlier among other large American cities. From 2016–21, Chicago experienced an average of 23 homicides

extremely concentrated in neighborhoods with many low-income residents and historically high rates of violence. The neighborhoods where READI was introduced, for example, had homicide rates 3 to 6 times the city average.[8] Within these neighborhoods, gun violence appears to be further concentrated among a small group of people; a wide range of evidence suggests that only a tiny percentage of the population engage in serious violent crime (Green et al., 2017; Braga, 2003; Farrington et al., 2006; Wolfgang and Tracy, 1982; Abt, 2019).

Using targeted interventions to tackle concentrated gun violence is a longstanding idea, both in policing and among community violence interventions (CVIs) (see, e.g., Sherman and Rogan, 1995; Braga et al., 2001, 2018, 2014; Skogan et al., 2008; Butts et al., 2015a). Appendix A.1 briefly discusses the research about targeted policing as well as CVI approaches. The most commonly adopted style of CVI to reduce shootings, community-wide violence interruption, is also among the most challenging to causally evaluate due to the difficulty of finding comparison communities that provide a valid counterfactual (see, e.g., Farrell et al., 2016; Roman et al., 2018). Existing evidence about its success is "mixed at best" (Butts et al., 2015a, p. 47). A newer set of CVIs provide preventative services to specific people at high risk risk of gun violence, but there is no causal evidence so far of their effectiveness.

Separately, some causal evidence exists on READI's two main program components: supported work and CBT. Studies of transitional jobs programs suggest that they are unlikely to reduce crime and violence on their own, but that strategies combining work and enhanced services such as CBT are more effective (Cummings and Bloom, 2020; MDRC, 2013; Redcross et al., 2016). CBT-informed programming alone can reduce violence involvement, sometimes more effectively when paired with an economic intervention (Heller et al., 2017; Blattman et al., 2017; Lipsey et al., 2007; Wilson et al., 2005; Arbour, 2022). While both of READI's core program elements have shown promise, neither has been convincingly evaluated on the

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per 100,000 people, the vast majority due to guns. While this is more than 8 times higher than rates in Los Angeles or New York, it is comparable to Philadelphia and Milwaukee, and around half of the rates in cities such as St. Louis or Baltimore.

[8] For evidence on the concentrated, place-based nature of much violent crime, see Weisburd (2015); Abt (2019); Blattman et al. (2021).

highest risk and most disconnected population that READI aimed to serve.

Any CVI that seeks to intervene at the individual level must meet two criteria if it is to meaningfully reduce gun violence: (1) identify and engage a group of people at high enough *ex ante* risk of gun violence for it to be feasible to reduce shootings among them, and (2) reduce their risk of being involved in a shooting. Both criteria pose significant challenges.

First, identifying this population is a difficult prediction problem (Berk et al., 2009; Chandler et al., 2011; Heller et al., 2022; Wheeler et al., 2019). Existing research provides little guidance about whether and which observable and unobservable characteristics can be used to identify specific people at very high ex ante risk, particularly given the randomness inherent in human behavior and in the likelihood of being involved in gun violence itself (i.e., whether a person is hit or missed). To the extent that gun violence involvement risk is transitory, this population may also change over time.

Once identified, finding these individuals can be extraordinarily challenging, especially if they keep a low profile to avoid encounters with police or their opposition. Once found, they may be disconnected from—and skeptical of—societal institutions and other offers to help, in addition to facing many logistical barriers to participation such as housing instability, substance abuse, and safety concerns about exposing themselves to certain people or locations (Fagan and Wilkinson, 1998; Anderson, 1999).

Most CVIs use outreach workers' local relationships and expertise to find and engage clients—usually young men with high rates of recent violence exposure. But without a randomized comparison group, studies of these programs cannot tell us clients' gun violence risk in the absence of services. It is also unclear how many people at high violence risk are missed by relying solely on expert referrals, or whether those referrals have the highest gains from participation. Beyond the challenges of identifying, finding, and engaging the relevant population, we lack strong evidence about what kinds of interventions could reduce their shooting involvement risk.

As a result, the READI study was not designed with a sole focus on impact evaluation.

Rather, we set out to answer three main research questions: (1) Can we identify a group of men at high enough risk of future gun violence that there is scope to reduce shootings?; (2) Will they participate in a pro-social intervention?; and (3) Will a combination of supported work and CBT reduce their involvement in serious violence?

2.2 Sample selection, referral pathways, and randomization

READI was a partnership between several organizations: Heartland Alliance, an anti-poverty and human rights non-profit based in Chicago which designed, developed, and managed READI; four organizations specialized in outreach; three organizations specialized in employment and CBT-based programming;[9] the principal investigators; and the University of Chicago Urban Labs. All parts of the program were developed collaboratively with detailed input from community workers, and eventually from participants themselves via a Participant Advisory Council.

Eligibility READI aimed to recruit men 18 and over at the highest risk of gun violence involvement.[10] It focused on five Chicago community areas with some of the highest levels and rates of gun violence (from a total of 77, as seen in Figure 1). As some of the neighborhoods are contiguous, providers grouped these five into three geographic service areas: Austin/West Garfield Park, North Lawndale, and Englewood/West Englewood.[11] Despite containing only nine percent of Chicago's population, these five neighborhoods accounted

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[9] Program implementation started in partnership with Centers for New Horizons, Cure Violence, Heartland Human Care Services, the Institute for Nonviolence Chicago, Lawndale Christian Legal Center (LCLC), North Lawndale Employment Network, and UCAN. As of September 2018, Heartland Alliance took over outreach services from Cure Violence in Englewood. And in April 2021, UCAN took over outreach services from LCLC in North Lawndale.

[10] There were three main reasons for the age floor. First, in the year prior to READI, men 18 and over made up 87 percent of shooting victims. Second, since READI offered full-time work, excluding youth was a way to avoid crowding out high school attendance. And third, conversations with city violence-prevention staff and our own review suggested that most of the city's existing violence prevention programs served youth, leaving a service gap for the population comprising the vast majority of shooting victims. As described below, some of the referral pathways into READI also set an age ceiling of 40. READI focused on men because they are disproportionately involved in gun violence; because the most frequent forms of lethal violence among women have a different set of causes and would likely require a different kind of intervention; and because there were not enough resources to operate two different versions of the program.

[11] READI continues to operate in these neighborhoods using a modified program model for non-study participants.

for 32 percent of homicides in 2016, up from 25 percent in 2015.

Given READI's explicit goal of serving men at the highest risk of gun violence, it is worth emphasizing that being at high ex ante risk for an outcome does not necessarily mean being responsive to an intervention designed to reduce it—that is, having a high $\hat{Y}$ is not necessarily the same as having a high β. Understanding the relationship between $\hat{Y}$ and β is nonetheless important for future interventions. To encourage variation in our study participants that would allow us to assess this relationship, we designed three different pathways through which to recruit participants on a rolling basis, described below, with additional details in Appendix A.3.

Referral pathways   The *algorithm pathway* used administrative police data to predict the risk of being involved in gun violence as a victim or an arrestee over the next 18 months (the "risk score").[12] Each time program slots became available for algorithm referrals, men with the highest risk score at that time who met READI's eligibility criteria were referred for randomization. This algorithmic approach is useful insofar as observables can successfully predict a person's involvement in gun violence, but it will miss risk driven by unobservables or fast-moving situations that are not reflected in the police data used in the algorithm.

To capture some of these unobservables and allow for possible selection on treatment responsiveness (β), the *outreach pathway* sourced referrals from outreach workers with extensive on-the-ground experience in the READI neighborhoods. These workers are privy to local information that may be absent from police records, but may be limited by the scope of their social networks or their incentives to offer particular people services and fill caseloads. They were instructed to refer men at the highest risk of gun violence.

Finally, the *re-entry pathway* identified men leaving jail or on parole who may be missed by both outreach workers and the algorithm, and who may be at a particularly sensitive transition point. Because of the complicated logistics involved with operating within carceral fa-

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[12] We predict risk scores for those with sufficient recent police contact; see Appendix A.3.1 for more detail. Heller et al. (2022) also provides a full description and analysis of a related prediction model, incorporating the lessons from READI's prediction model.

cilities, the re-entry pathway took the longest to become operational. Also, because COVID-19 brought study recruitment to an early end, this pathway is considerably smaller than the other two and than we initially intended. As such, we largely focus on differences between the first two pathways, though we report re-entry results separately for completeness.

Having three referral pathways allows us to assess how the different ways of identifying men at the highest risk of shooting and being shot performed: whether observables are enough to predict future gun violence with a machine learning algorithm, whether the algorithm identified a different set of people than the outreach workers, and why those groups might differ (i.e., whether on-the-ground knowledge could capture unobservables in a way that improved program targeting, and whether human decision-makers chose to select not only on propensity for violence but also expected responsiveness to the program).

Randomization READI solicited referrals on a rolling basis from August 2017 to March 2020. New referrals ended unexpectedly early due to the onset of the pandemic, shifting the sample size to 2,456 from the original target of 3,000. Rolling referrals were made from all three pathways—since August 2017 for outreach referrals, December 2017 for algorithm referrals, and August 2018 for re-entry referrals—as program slots became available, both to accommodate READI's growing capacity to absorb new entrants over time, and to focus on the people at highest risk, who may change over time. The randomization process varied slightly by pathway but followed the same general structure (see Appendix A.3). After receiving referrals via the outreach or re-entry pathways and matching them to administrative data, or after identifying men with the highest predicted risk of future gun violence involvement via the algorithm pathway, we first screened out anyone who had previously been randomized or failed to meet eligibility criteria.[13]

For outreach referrals, if the number of remaining referrals was greater than twice the

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[13] Referrals had to live or spend considerable time in a READI neighborhood; identify as male; and be between 18 and 40. No age cap applied to outreach referrals to give maximum discretion to human expertise, though in practice, only 5 percent were over 40 at baseline. We used all available data at the time of randomization to screen out those who had either been incarcerated or died since their referral, though we received carceral and homicide data with a lag, so the screening was imperfect.

number of open program slots, we randomly dropped referrals to obtain a group exactly twice the size of the number of slots. Unused referrals were returned to the outreach organization so that they could be referred in a future round. In all cases, we randomized at the individual level with a treatment probability of one half, within strata defined by neighborhood, referral pathway, and randomization date.[14]

2.3 The READI program

READI is a bundled intervention designed to disrupt four different proximate causes of gun violence. First is the instrumental use of violence in illegal markets, where illicit organizations lack legal ways to enforce contracts or compete for market share. Second is rational reputation building, where people use violence to signal strength as a way to deter future attacks in a dangerous environment. Third is reciprocity, where violence is a means to punish real or perceived slights, especially in settings where the legal system is not viewed as legitimate or just. And fourth is "irrational" behavior arising from mistakes and misperceptions—instances where fast decision-making combined with fear, anger, or persistently biased beliefs about others' intentions can result in quickly escalating altercations.[15]

READI's key components were designed to address these proximate causes of gun violence. Note, however, that because it was designed and implemented extremely quickly to respond to crisis-level violence in Chicago, READI's development was a learning process with the model changing slightly over time.

Initial outreach For all three pathways, engagement began with outreach workers trying to locate men assigned to READI and convince them to participate. These men were usually highly mistrustful of organized programming. Once located, outreach tried to convince these men to join READI, including by helping them obtain proper documentation to work legally or negotiate a truce with members of opposition groups in the program. Once a

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[14] In practice there is slight variation in treatment probability within strata, see Appendix A.3.4 for details. We include strata fixed effects in all analyses.

[15] For more extended references, see reviews by Blattman (2022) and Abt (2019).

person was willing and ready to begin, the outreach worker connected them to the READI employment organization for their neighborhood. Attending an orientation seminar and signing employment paperwork denotes the beginning of formal participation in READI—what we define as "taking up" the program.

Supported, subsidized work To provide an alternative to work in illegal and violence-prone markets, and to provide a positive incentive to participate, READI's first major component was supported, subsidized work. Participants could earn money on worksites 5 days per week (29.5 hours total), for up to 18 months.[16] READI was explicitly not intended as a transitional job program to rapidly place men into full-time work, but rather focused on violence reduction. Nonetheless, it was informed by best practices in the transitional jobs literature, including a "career pathway" approach with four stages based on a participant's progress (see Appendix Figure A.1). During the first stage, participants were typically assigned to crews performing outdoor work (such as park cleanup) or other basic services (such as packing meals for food pantries). They received transportation to and from the worksite, as safety was a core challenge for participants. Later stages offered participants a greater variety of jobs and more independence, potentially including subsidized placements with local employers (e.g., some participants eventually worked in a local vehicle seat factory).

This tiered structure allowed participants to expand their skill set and earning potential over time, which prior research suggests is important for keeping them engaged. Participants in the first stage received a minimum hourly wage, initially $11 but rising during the course of the study to match changes in the local minimum wage. Advancing to each stage was accompanied by a wage increase, among other benefits.

Given the mixed results of prior jobs programs, even with populations at much lower risk of violence (Cummings and Bloom, 2020; MDRC, 2013; Redcross et al., 2016), the

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[16] READI recognized that its target population faced many barriers to employment, so men who stopped attending work were allowed to later resume participating. If a participant disengaged from work, or committed an offense on site, outreach and program staff continued to engage them. Participants had opportunities to return to the program after the problems had been resolved, with restorative justice processes used to reintegrate any violent offenders back into the program.

employment component was not expected to reduce violence on its own. Nonetheless, over dozens of interviews and focus groups, participants and program staff said the job was a crucial incentive to participate (see Section 3.3 for qualitative data and methods). The job was also designed to complement therapy by providing a place to practice and reinforce new thinking and behaviors, guided by staff at the worksites. Finally, given the level of criminal involvement the population was expected to have, simply keeping participants busy during the week could potentially have a substantial incapacitation effect.

CBT-informed curriculum   Both to reduce the kind of "hot" decision-making that leads altercations to escalate and to help build new social identities and norms around violence, social interactions, and illegal employment, the second major element of READI involved cognitive behavioral therapy (CBT). CBT is an approach for reducing maladaptive beliefs and behaviors, and promoting positive ones. Its methods can be applied to a wide range of thoughts and behaviors, and CBT-informed therapies have been successful at reducing symptoms of depression, anxiety, phobias, traumatic stress, and hostility (Beck, 1979; Beck and Dozois, 2011).

CBT-informed therapies like READI teach participants that thoughts can influence actions, and help them practice actions designed to shape those thoughts. In 90-minute sessions for three mornings each week, counselors facilitated conversations and conducted exercises with small groups of men designed to help them become more conscious of their automatic thoughts, particularly inaccurate or negative ones about themselves or others that could lead to violence. Counselors taught participants techniques to recognize these thoughts and respond to them in ways that are more constructive and less harmful. Both in group sessions and outside of them as "homework" (including at worksites), men had opportunities to practice these techniques on tasks of increasing difficulty. Through such "learning by doing," CBT can gradually modify participants' behavior and thinking.

Participants also received individual and small-group professional development and information sessions the remaining two mornings per week. They received a $25 gift card for

each CBT and personal development session attended.[17] Afterward, they departed to their worksites for the day. The CBT sessions were available for all 18 months of READI and were a requirement for participation in supported work.

Ongoing outreach support & referrals to other services   Throughout our qualitative data collection, READI staff and participants both emphasized the extreme struggles participants faced in continuing with READI day-to-day. Many had to cross rival territories or leave places of safety to attend the program. Some had active rivalries or developed new ones. There were also frequent fights, confrontations with program staff, and other serious disturbances on site—including occasional incidents of gun violence from outsiders and participants. Outreach workers provided almost continuous support, applying their training in conflict mediation, de-escalation, and restorative justice, including at worksites.

In addition to these safety and security issues, participants faced other serious challenges including episodic homelessness, family quarrels, financial difficulties, arrests and other legal troubles, parole commitments, physical and mental health struggles, and other issues that hindered daily participation. Outreach and program staff helped participants navigate these problems, including by making referrals to organizations providing substance abuse treatment, housing, or legal services.[18] Often, these situations served as opportunities to employ some of the new skills and techniques acquired in the CBT sessions, to entrench the new behaviors as habits.

Services available to the control group   Men in the study were free to access alternative programming. It is reasonable to assume that some did, particularly men in the control group who lacked access to READI. To our knowledge, no other provider offered programming of READI's length or intensity, with a similar combination of services, delivered to men facing

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[17] Gift cards were for specific vendors, including stores such as Walmart and Kroger, fast food chains such as McDonald's and Subway, the Chicago Transportation Authority, and retail stores such as Foot Locker.

[18] Criminal legal services were focused on dealing with a client's criminal prosecution. As such, they were unlikely to affect our main measure of violent offending, which occurs prior to prosecution at the point of arrest.

a similarly high level of risk during the study period and in the READI neighborhoods.[19] Though the number of organizations in Chicago offering individual programming intended to reduce gun violence has grown since READI started in August 2017, few provide jobs, let alone for 18 months, nor in combination with CBT.

Unfortunately, due to their level of risk, mobility, and distrust, it is impossible to survey control group men to learn about their program participation directly. Service providers also tend to be quite protective of client identities. We can, however, indirectly learn about this from the control group's primary conduit to alternative programming: outreach workers. Members of the research team interviewed and collected field observation notes with 90 percent of these staff. Several described an intense effort to assist the highest-risk men, largely because they felt these men were in life-or-death situations. These outreach staff tried to stay in touch with control group men, mentor them, and connect them to temporary work agencies or mentorship programs. While there are scattered reports of success at these efforts, one outreach worker expressed a common view that "there is nothing like READI around here." Such efforts were heterogeneous, however, with some staff reporting that they did not have the bandwidth to provide continued support to control group members who often quickly disappeared.

Any such support provided to control group men, however modest, would likely lead us to underestimate READI's treatment effects and overstate its costs relative to the counterfactual. This is probably most acute for men referred by outreach workers, because their pre-existing relationships could have facilitated the ongoing contact needed to access alternative services. In contrast, men referred by the algorithm were the least likely to have received other programming, as they were typically much less engaged with outreach workers, who had no information about the men randomized to the control group from the algorithm pathway.

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[19] The closest comparable program is Chicago CRED, which provides jobs, life coaching, trauma counseling, and education to a similar population of young men but operates mostly in the Roseland and West Pullman neighborhoods on Chicago's South Side: `https://www.ipr.northwestern.edu/documents/reports/ipr-n3-rapid-research-reports-cred-outreach-jan-22-2021.pdf`

Impacts of the 2020-22 pandemic on program delivery   In response to public health orders introduced in March 2020 to limit the spread of SARS-CoV-2, all randomization stopped and all in-person READI programming was suspended. The CBT sessions transitioned to being held online. The supported work was paused altogether, with participants receiving "standby pay" from March through July 2020 to ease the transition. Standby pay was offered at an amount equal to each person's average weekly wages during the prior month. Starting in August 2020, in-person work and CBT sessions resumed. Participants had the option of going to work in person or earning up to $50 per day attending professional development sessions remotely.

The pandemic's impact on the study is multi-faceted. On one hand, the physical and economic hardship it caused affected men in both the treatment and control groups, and therefore did not undermine the study's internal validity. On the other hand, our sample size is almost 20 percent smaller than initially intended because COVID-19 forced an early end to randomization. The disruption to services for those already participating was severe. Some men struggled to join remote CBT sessions due to unreliable internet connectivity, a lack of private space, or fear of showing others their location onscreen. The resumption of in-person work after a pause of many months made it difficult to re-engage some men.

While we cannot quantify the impact of these factors on the study's estimates, we certainly did not evaluate an uninterrupted delivery of READI with the statistical power and implementation fidelity we initially anticipated. Importantly, because recruitment began in August 2017 and our outcome window is 20 months, approximately 76 percent of post-randomization person-day observations occurred before the onset of the pandemic. Since the meaning of "participation" changed with the pandemic, in the main text we report overall program hours and earnings, as well as program retention limited to the pre-COVID period. Appendix A.5.3 provides more detail, with Appendix Table A.2 breaking out overall participation pre- and post-COVID, and Appendix Figure A.3 showing retention including the COVID period.

3 Data and empirical strategy

3.1 Outcomes and data

The inherently unobservable or "latent" outcome of interest is a person's involvement in serious violence, either as a perpetrator or a victim. We proxy for both kinds of violence involvement using administrative arrest and victimization records from the Chicago Police Department (CPD). The main advantage of police data is that we can use them to follow a large number of study members over a long period of time, with minimal sample attrition and relatively low cost. This makes the study feasible to conduct, as it would not be possible to track behavior via surveys over long periods of time with such a disconnected and difficult-to-locate population. Administrative records also avoid social desirability bias that might prevent people from honestly disclosing their serious violence involvement.

Police data also have serious limitations. One is that victimizations only appear when victims are willing to report incidents to the police. That said, because all healthcare workers in Illinois are legally required to report shooting victimizations to local police (20 ILCS 2630/3.2), and because shooting victims are very likely to seek medical care, underreporting of non-fatal shooting victimizations is likely to be minimal.[20] Similarly, homicide victimization is widely thought to be mostly free of underreporting in police data (Loftin and McDowall, 2010; Carr and Doleac, 2016). A more serious challenge is the use of arrests. Arrests very likely understate offending due to low clearance rates, even for the most serious crimes: only 26 percent of homicides and 5 percent of non-fatal shootings in Chicago in 2016 resulted in an arrest (Kapustin et al., 2017). Arrests are also subject to potentially biased police decisions and may be mistaken or wrongful. The chance of an offense resulting in an arrest (or the chance of a false arrest) may also vary by demographic group or neighborhood. But while these issues could make it harder to detect treatment effects by introducing error

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[20] Some underreporting may still occur if shooting victims self-treat or seek care from medical providers outside of Chicago who may not report such victimizations to the CPD. However, based on our conversations with violence prevention, medical, and law enforcement practitioners in Chicago, we think the magnitude of such underreporting is likely to be small.

into arrest measures, they affect both treatment and control groups, and so should not bias the treatment–control difference. The key assumption for treatment–control differences in arrests to successfully proxy for treatment–control differences in offending is that treatment does not change the probability of arrest conditional on actual criminal behavior.[21]

Finally, while future work may incorporate state-level police data, the data in this paper do not capture victimizations or arrests outside of Chicago. However, if READI's steady paycheck increased the time the treatment group spent within Chicago, then more of their violence involvement would show up in our data, causing us to understate treatment effects.

3.2 Primary outcomes

To proxy for the latent variable of interest, we pre-specified a single primary outcome, *Serious violence involvement*, which is an index that standardizes and averages arrests and reported victimizations for serious violent crimes over the 20 months post-randomization.[22] The three components of this index are: (1) *Shooting and homicide victimizations*, (2) *Shooting and homicide arrests*, and (3) *Other serious violent-crime arrests*.[23]

Though READI's emphasis is on reducing shootings and homicides specifically, we included arrests for serious but not gun-related violent offenses in the primary outcome. We did this because we initially underestimated READI's ability to identify people with future shooting involvement, and were worried a sole focus on shootings would leave too few incidents to detect program effects. Similarly, in our pre-analysis plan we pooled shooting

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[21] In theory, this assumption could fail if treatment changed time use in a way that affects the probability of arrest, or if it teaches participants to interact more constructively with police officers. Both are more likely for lesser offenses, such as disobeying an officer, where there is more room for police discretion. The lack of program impacts on arrests for minor offenses (Appendix A.5.7), combined with evidence that additional education does not change the probability of arrest conditional on crime (Lochner, 2004; Lochner and Moretti, 2004), makes this issue unlikely to be problematic for the serious violence measures that are our focus. For additional discussion, see Appendix A.2.5.

[22] The intent of the 20-month follow-up was to allow for an average of 2 months to locate and recruit treatment men, plus the 18-month period during which participants were eligible to work and attend CBT sessions. Although there is variation in when participants actually took up the program, these estimates generally focus on treatment effects during the program. A post-program, 40-month analysis is currently in progress.

[23] The last component includes arrests for the other violent offenses historically included in "Part I" of the Uniform Crime Reporting (UCR) program: aggravated assault and aggravated battery (excluding homicide, manslaughter, and non-fatal shootings), robbery, and criminal sexual assault.

and homicide arrests and other serious violent-crime arrests into a single component, as we did not anticipate having sufficient power to detect effects on shooting and homicide-specific outcomes. However, upon seeing the very high level of risk that men in the study sample experience, and given READI's focus on preventing shootings and homicides, we opted to separate these incidents from arrests for other serious violent crimes when constructing the index. This decision has a negligible impact on our estimates of READI's effect on the index itself.[24] When estimating effects separately for the three (rather than two) components of the index, the multiple hypothesis adjustment (described below) penalizes our inference for the additional hypothesis test.

An index is useful for increasing power to the extent that all underlying components move in the same direction. But because READI may affect the components of the index differently, and to better understand what behavior may be changing, we also pre-specified that we would: (1) estimate effects separately for each component, correcting for the increased probability of Type I error that comes from testing multiple hypotheses; and (2) calculate an index where all arrests and victimizations are weighted by their social cost. Viviano et al. (2021) offer a framework that suggests the latter approach, with aggregated social costs providing a sufficient statistic to capture the importance of different outcomes, may be a more helpful way to summarize results across multiple outcomes.

We implement two multiple hypothesis testing adjustments. First, we control for the family-wise error rate (FWER) among the three index components by using a free step-down resampling method (Anderson, 2008; Westfall and Young, 1993).[25] Second, we increase power by allowing for some proportion of null hypothesis rejections to be false, controlling for the false discovery rate (FDR) (Benjamini and Hochberg, 1995). We report the q-value, an analog to an adjusted p-value, which reports the smallest proportion of false null rejections

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[24] ITT estimates (p-values): -0.029 standard deviations ($p = 0.21$) with a three-component index versus -0.029 standard deviations ($p = 0.31$) with a two-component index.

[25] We report the adjusted p-value under strong FWER control. When we apply these adjustments to our heterogeneity tests, we treat the primary outcome index across the three referral pathways as a family, and then treat the index components within each referral pathway as its own family.

we would have to accept to reject the hypothesis under the FDR control procedure.

3.3 Qualitative data collection

Between August 2017 and March 2020, two of the investigators and a qualitative research team coordinated and conducted: (1) 220 hours of formal field observation across all READI sites; (2) 16 focus groups with over 90 percent of READI staff; (3) 23 semi-structured, recorded interviews with program participants across all three community areas; (4) 72 participant surveys in one community area; and (5) dozens of hours of informal qualitative observation and field site visits. The research team transcribed, read, and coded over 3,000 pages of recordings and field notes. We analyzed the frequency of responses and content of themes to determine the most common responses to questions. In March 2020, additional qualitative data collection was halted by the pandemic.

3.4 Estimating treatment effects

We estimate intent-to-treat (ITT) effects via the ordinary least squares regression:

$$Y_i = \beta T_i + \lambda \mathbf{X}_i + \gamma_s + \varepsilon_i \tag{1}$$

where Y_i is the 20-month outcome for individual i, T_i indicates assignment to an offer of READI, $\mathbf{X}_i$ is a vector of pre-randomization characteristics,[26] and γ_s is a vector of randomization strata fixed effects. We estimate heteroskedasticity-robust standard errors.

The ITT estimate represents the effect of having an offer to participate in READI. As such, the ITT will understate the effect of READI participation. We therefore also estimate the treatment effect on the treated (TOT) by using random assignment as an instrument for participation. We define participation as a binary indicator equal to 1 for those who

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[26] Pre-randomization characteristics include arrests for shootings and homicides, other serious violent crimes, other less serious violent crimes, property crimes, drug crimes, and other crimes; non-fatal shooting, other (non-shooting) violent, and non-violent victimizations; days spent in jail or prison in the prior 30 months; indicators for being in jail or prison at the time of randomization; baseline risk score; age; and race. For detailed information on pre-randomization characteristics and evidence of robustness to the inclusion of different covariates, see Appendix A.5.1.

attended the initial orientation and signed employment paperwork. To provide a sense of the proportional change for compliers, we report control complier means (CCMs) (Heller et al., 2017; Kling et al., 2007).

3.4.1 Threats to internal validity

Differential mortality and incarceration   One consequence of evaluating programs for such a high risk sample is that any death or imprisonment will censor our dependent variables, possibly in ways that are correlated with treatment assignment. For example, if treatment delayed the risk of a homicide, then treatment men would have more opportunities than control men to be arrested or non-fatally victimized. In some respects, this change in time available is part of the treatment effect. Overall counts of incidents still reflect how much violence there was among each group, which is why we made these counts our primary outcomes.

Still, if we are interested in whether READI changes violent behavior and not just the number of events, then such a shift in incapacitation could mask changes in outcomes conditional on being free to engage in one's normal activities. Recognizing this risk in advance, we pre-specified that we would test for differential rates of incarceration and death. Although we do not find significant differences in the overall amount of time the treatment and control groups are incapacitated, we still formally verify in Appendix A.5.2 that our main results are robust to adjustment for differential censoring.

Spillovers   The ITT and TOT are always interpretable as the average difference between the treatment and control groups (or compliers) after READI was implemented. But interpreting them as estimates of the direct effect of being offered or receiving READI relies on the Stable Unit Treatment Value Assumption (SUTVA)—that one person's treatment status does not affect another's potential outcomes. Given how social crime and violence can be, and that gun violence in particular is often a response to others' behavior, it is plausible

that SUTVA could fail due to such spillovers.[27]

In a separate project, Craig et al. (in progress) are rigorously estimating these kinds of social spillovers. By combining multiple measures of social networks at baseline with the variation in treatment exposure generated by randomization, they can identify the causal effect of exposure to a treated peer while overcoming the typical challenges of endogenous ties and common shocks in the peer effects literature. While that project will eventually use several different measures of social networks and combine READI with three other RCTs, Craig et al. (2022) reports early READI-specific results to aid with the interpretation of the estimates reported in the current paper. They show that within the study population, there is no definitive sign of adverse spillovers (if anything, the results here may mask declines in other serious violent-crime arrests). But given the sample size, the analysis is somewhat underpowered.[28]

4 Descriptive statistics, realized risk, and take-up

4.1 Baseline characteristics and balance

Table 1 reports baseline summary statistics and tests of balance. The average age of study men at referral was 25, and nearly all (97 percent) are Black.[29] Summary statistics on past arrest and victimization records confirm that the various referral pathways successfully identified men with very high levels of prior violence involvement. The baseline counts of outcomes going back to 1999 (2010 for shooting victimization) that are part of the primary index show that for every 100 men in the sample, there were an average of 7.6 prior arrests for shootings or homicides, over 46 prior shooting victimizations, and 89 arrests for other

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[27] For additional discussion on how spillovers might affect the interpretation of our point estimates, see Appendix A.5.4.

[28] They find clearer evidence of a READI-driven decline in drug-crime arrests that is masked in the ITT estimate reported here, because members of the control group who have co-arrest ties to the READI treatment group also show reduced drug-crime arrests. Combined with declines in these arrests among the peers of the treatment group who were outside the study sample entirely, it is likely the ITT estimate reported in this paper misses a net decline in drug-crime arrests from the intervention.

[29] The racial composition is largely a function of which neighborhoods and community organizations participated in READI, though as Heller et al. (2022) show, Black men in Chicago have a disproportionately high risk of being shot.

serious violent crimes—aggravated assault and battery, sexual assault, and robbery. The risk score shows that, based on our algorithm, we expected about 11.4 percent of the sample to be either arrested for or the victim of a serious gun crime in the 18 months after referral.

Prior involvement in other kinds of crime and violence was also high. The average study member experienced over 17 arrests prior to randomization (see Appendix A.2 for explanation of the different offense types), 3.4 reported victimizations, and had spent roughly 175 days in jail or prison in the prior 30 months, with 4 percent being incarcerated at the time of randomization.

Of the 17 baseline variables in Table 1, two have treatment–control differences with $p < 0.1$—no more than would be expected by chance. A joint test of significance has a p-value of 0.31. We control for these baseline variables in our analysis in part to account for any chance differences.

4.2 Pathway differences and realized risk

The top panel of Table 2 reports baseline summary statistics by referral pathway (Appendix Table A.1 shows baseline balance by pathway). Relative to outreach referrals, algorithm referrals had significantly longer arrest and victimization histories on basically all measures, despite being about a year younger. Their number of prior serious violence incidents was between 25 and 119 percent larger, depending on the measure, with a predicted level of future gun violence involvement of 14 versus 9 percent. They also had about twice the number of prior reported victimizations. The only measure on which algorithm referrals looked less involved in crime than outreach referrals is on days incarcerated in the past 30 months (129 versus 155 days), possibly because being incarcerated during the baseline period likely reduced someone's predicted risk score—and thus their chance of being referred by the algorithm—by reducing the number of recently observed incidents. In general, re-entry referrals were between algorithm and outreach referrals, though they were about a year older at baseline than the latter. These observable differences across referrals suggest that

the pathways identified different kinds of people.[30]

Because a core question about the referral mechanisms was whether they could anticipate future risk, the bottom panel of Table 2 shifts from baseline characteristics to realized risk in the control group over the 20-month outcome period. It shows that READI's referral pathways successfully identified men at high risk of future arrest and victimization. Almost two-thirds of the control group were arrested during the 20 months after randomization, 1.7 times on average. About a third reported at least one victimization. The best-measured and most severe indication of gun violence involvement—being shot or killed—is shockingly high: there were 11.4 shooting or homicide victimizations for every 100 control group members during the 20-month follow-up period. This is 54 times higher than the average Chicagoan, and 2.8 times higher than other men 18–34 living in the same neighborhoods READI serves. In short, READI's referral pathways identified a group at immensely high risk of being shot.

The algorithm was also successful at predicting a broader measure of gun violence. It was trained to predict involvement in a gun crime as an arrestee or a victim in the next 18 months (the table's last row). About 15 percent were actually arrested or reported being victims of a gun crime in that period—a bit higher than predicted at baseline, where the average risk score was 11.4 percent. This could partly be due to rising violent crime rates city-wide during the outcome period relative to the data on which the algorithm was trained, and partly from the role of unobservables discussed below.

On general measures of criminal legal involvement such as arrests and victimizations, the realized risk levels of referrals from the three pathways mirror the significant differences in their baseline characteristics (top panel of Table 2). Algorithm referrals were more likely to be arrested and victimized on both the extensive and intensive margins than outreach referrals, who in turn had higher rates and counts than re-entry referrals. Yet on measures of gun violence specifically, men identified across the three pathways did not significantly

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[30] Consistent with this is the fact that referrals from one pathway rarely included people who had previously been referred via another pathway; for example, only 35 initial outreach referrals had already been randomized via the algorithm pathway at the time of referral.

differ on realized risk, despite their significant differences in predicted risk.

The fact that realized rates of gun violence involvement were fairly similar across the pathways in the control group despite the differing risk levels predicted by observables suggests that the people making referrals using on-the-ground knowledge were leveraging unobservables that predict gun violence. A key question, given the similarity of gun violence involvement across pathways, is whether the human decision-makers were also selecting on expected gains from program participation, and not just levels of the outcome. We address this question in Section 5.2 below.

4.3   Take-up and participation

Given the extraordinary risk of violence in the study population, their interest in participating in a program with rules and restrictions was not self-evident. Table 3 reports rates of participation among men in the treatment group, overall and by pathway. Among all men randomized to READI offers, 55 percent started the program (defined as attending orientation). This take-up rate is comparable to interventions working with much less disconnected populations, such as teenage boys still attending Chicago public high schools in similar neighborhoods (Heller et al., 2017).

As expected, take-up was highest (78 percent) among outreach referrals. These men likely already knew some program staff and had been screened on an interest in, and "readiness" for, programming. Recruiting re-entry participants was also relatively successful, with take-up of 60 percent. Fewer of the algorithm referrals actually participated (37 percent). Interviews suggest that these men were much harder to locate, so the lower take-up rate reflects a combination of being unable to find the men as well as them declining to participate conditional on being found (see Appendix A.3.1).

The rest of the table breaks out earnings and hours by activity, reflecting participation within the 20-month outcome period.[31]   While we have to use some extrapolation due to

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[31] Some participants took up the program more than 2 months after randomization, so they could have continued the 18-month program after these 20 months.

incomplete records on CBT and training attendance, our estimates suggest that participants earned an average of about \$9,600, or about 40 percent of the total possible earnings theoretically available over a full 18-month period (i.e., uninterrupted by COVID-19). Appendix Table A.2 breaks the totals out by the different periods relative to the pandemic.

Figure 2 reports two measures of job retention among participants, weekly from the time they first attended orientation. The first, shown by the solid line, is the proportion of participants who worked at least one day after the time noted on the x-axis, as measured through payroll data from Heartland Alliance (the employer of record for READI participants). The top panel shows that, in the first few weeks after orientation, roughly 15 percent of participants stopped returning to work. Afterwards, the decline in participation becomes roughly linear and relatively slow. About 75 percent of participants continued showing up to work after the 20-week mark, and a little over half continued to work after one year.[32]

The second measure, shown by the dotted line, is the proportion of weeks worked by participants who are still working. Unlike the first measure, which captures the extensive margin (*whether* people still work), this second measure captures the intensive margin (*how much* people work). After the initial fall-off, participants consistently worked about 75 percent of the weeks that they could have.

The bottom panel of Figure 2 shows retention by pathway, excluding re-entry since the Figure is limited to pre-COVID data, and too few re-entry participants started early enough to have 18 months of follow-up data in that period. The overall patterns are quite similar for algorithm and outreach referrals, with the key difference being a faster fall-off in early participation among those recruited via the algorithm pathway. This resulted in the larger dosage for outreach referrals shown in Table 3.

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[32] Figure 2 excludes the period starting in March 2020 when READI's in-person employment programming was suspended for several months. An analogous version of the plot including this period shows similar patterns (Appendix Figure A.3), but with slightly lower participation towards the end of the program period.

5 Results

5.1 Average treatment effects

Table 4 reports estimates of average treatment effects on our pre-specified primary outcome index and its components. The point estimate for being randomly assigned to the READI group (the ITT) is a 0.029 standard deviation reduction in the serious violence index (a 0.053 standard deviation reduction for the effect of participating, the TOT), but the result is not statistically significant ($p = 0.21$).

The second panel of the table shows the pre-specified secondary results: how the program affected the three components of the index. The two measures of shooting and homicide involvement—both arrests and victimizations—have negative and substantively large point estimates. There are 2.1 fewer shooting or homicide victimizations for every 100 participants, a 18 percent decline relative to the control complier mean. But the confidence interval is too wide to rule out a similarly-sized increase instead. The decline in arrests is close to traditional statistical significance thresholds, with a proportionally huge 64 percent decline (2.1 fewer per 100 participants) that is statistically significant on its own, but not after adjusting inference for the three hypothesis tests across components (unadjusted $p = 0.05$, adjusted $p = 0.15$).[33]

The small reduction in the overall index reflects the fact that not all forms of violence move in the same direction. There are just under 1 additional arrests for other serious violent crimes for every 100 participants, a 13 percent increase, though the standard errors are even larger relative to the point estimate than for the shooting and homicide components. As we will see below, however, the disproportionately high costs of shootings and homicides means that weighting arrests and victimizations by their social cost leads to different conclusions, which is relevant for policymakers to consider along with the lack of clear change in our

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[33] We focus on reporting adjusted p-values that strongly control the FWER in the text, which is a conservative inference adjustment. The tables also report the less-conservative q-values from controlling the FDR (i.e., what proportion of false rejections we would have to accept to reject the null).

primary outcome.

Appendix A.5 shows that these results are robust to: using a count model; the inclusion of different (or no) baseline covariates; adjustments for the possibility that incarceration or death are generating censoring; and limiting the analysis to the pre-COVID period (if anything, treatment may have been more protective against shooting and homicide victimizations during the pandemic). That section also shows that there are few statistically significant changes in other types of arrests, victimizations, or incarceration outcomes. Lastly, Appendix A.5.5 shows how treatment effects accrue over time.

A natural mechanism question, given the high level of criminal legal and violence involvement among the READI population, is whether the large decline in shooting and homicide arrests is simply a direct result of incapacitation from the program itself—whether keeping people busy during the workday mechanically reduced violence during that time. Appendix A.5.6 reports estimated effects on arrests and victimizations separately by day and time, as measured by the time of incident (not the time of arrest). It shows that while point estimates on the total number of arrests and victimizations are negative and substantively large during work days, there is no indication that declines in serious violence involvement are concentrated during the work day. The fall in incidents underlying shooting and homicide arrests happens during weekends, suggesting that incapacitation does not seem to be driving the change in this outcome.[34] This is corroborated by interview data, in which participants report changing with whom and where they spend time outside of READI hours.

5.2   Heterogeneity analysis

In this section, we examine the heterogeneity of impacts by pre-specified subgroups. Because they were built into the experimental design and prediction process, we focus the main text on differences across referral pathways and risk levels.[35] While our pre-analysis plan noted

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[34] Appendix A.5.7 shows that there may be some role for incapacitation in reducing arrests for drug crimes.

[35] Appendix A.5.8 summarizes heterogeneity by two other pre-specified subgroups, neighborhood and age. Briefly, we find no evidence of heterogeneity by age, but there is some variation by neighborhood: significantly larger declines in two neighborhoods (Austin/West Garfield and Greater Englewood) but a positive point estimate in the third (North Lawndale).

that these tests would be in the spirit of exploratory analysis, as we did not anticipate being powered to detect moderate heterogeneity, we still adjust our inference procedure for multiple testing given the number of tests across referral pathways.

Table 5 shows significant heterogeneity across referral pathways: we can reject the null that the estimated effects on the primary outcome index are the same across pathways ($p = 0.03$). There is a clear, statistically significant decline in serious violence within the outreach pathway. The decline in the index of 0.13 standard deviations for participants remains statistically significant after adjusting inference for the three tests across pathways (adjusted $p = 0.02$). Breaking the index into components shows a similar but more precise pattern as in the overall results: large declines in both arrests (79 percent) and victimizations (45 percent) for shooting and homicides, which both remain statistically significant after adjusting for the three tests across outcomes (adjusted $p = 0.02$ and 0.06, respectively), and a small decline (but large standard error) on other violent-crime arrests.

No results in the other referral pathways approach statistical significance. In many cases this is because the tests are under-powered, not because point estimates are substantively small. Standard errors for the TOT in the algorithm pathway are particularly large, despite the larger sample there, because of the weaker first stage resulting from lower program take-up. Understanding why the outreach referral pathway is more responsive to treatment is important both for thinking through mechanisms and for future policy. We focus the analysis here on the role of risk given its centrality to our experimental design. But we also note that the pathway differences cannot be explained simply by the differences across all observables seen in Table 2. In Appendix A.5.9, we show that re-weighting algorithm and re-entry referrals to look more like outreach referrals on observables does little to make the estimated treatment effects converge.

Table 6 shows that the difference across pathways is not clearly driven by the predicted differences in baseline risk. It reports separate effects for those over and under the median

baseline risk score, as well as the 231 people with missing scores.[36] There are some indications that READI's effects may be concentrated among those with higher baseline risk: the point estimate on the primary index is large and negative only for that group, and the only statistically significant estimate among the components is the decline in shooting and homicide arrests for those at above-median risk. Overall, however, there is not enough power to differentiate the groups. We cannot reject the null that the three risk groups have the same effect ($p = 0.33$), and the confidence interval is wide enough to include large proportional declines from a lower baseline for the under-median risk group.

To better understand whether the heterogeneity by referral pathway reflects outreach workers selecting on treatment responsiveness or unobserved risk factors or both, Figure 3 presents several exploratory results separately by pathway and baseline predicted risk quartile.[37] The top left panel shows average rates of actual gun violence involvement, the outcome predicted by the algorithm, for the control group. Marker sizes correspond to the share of referrals within each pathway in each quartile. Two points are worth noting. First, most outreach referrals (75 percent) have below-median (or missing) risk, while most algorithm referrals (67 percent) have above-median risk. Second, while both pathways referred men whose realized risk was greater than their predicted risk (Table 2), outreach referrals had higher realized risk than algorithm referrals across the predicted risk distribution. Taken together, these show that outreach workers successfully identify unobservable risk factors at all risk levels, but usually refer men at lower predicted risk.

One hypothesis for how outreach workers select men to refer is that they do so on the basis of gains (β), which may or may not be correlated with the level of ex ante risk ($\hat{Y}$). Our qualitative evidence, in which outreach workers describe selecting men who are "ready for

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[36] Missing a risk score (which can only happen in the outreach and re-entry pathways) indicates that someone had too little recent police contact to meet the criteria for inclusion in the algorithm: at least one arrest or two victimizations in the last 50 months. Consistent with idea that less recent police contact indicates lower risk, the control means for the missing group are considerably lower, as shown below.

[37] The quartiles are defined across the full distribution of risk scores in the study sample. Average predicted risk within each quartile is very similar across pathways. For distributions of risk scores within referral pathway, see the top left panel of Appendix Figure A.5.

READI," is consistent with this hypothesis. In interviews, outreach workers described "being ready" as being willing to change. Their examples of not being ready and willing included men who were still focused on settling a score, men who preferred illegal work (such as drug dealing), men who feared for their safety if they left their block or associated with opposition members, men who could not overcome their impatience with or skepticism of the therapy and jobs, and men unwilling to attend the program sober.

At the same time, the bottom panel of Figure 3 is inconsistent with a pure "selection on gains" hypothesis. That panel shows ITT estimates for the primary outcome index by pathway and risk quartile (estimates for each component separately are in Appendix Figure A.5). The top right panel shows that take-up rates are relatively flat across quartiles in each pathway, such that patterns for the TOT estimates are similar.

For the bulk of outreach referrals with below-median (or missing) risk, the point estimates are very close to zero. The large estimated declines in serious violence for outreach referrals in Table 5 are driven by the smaller group of above-median risk outreach referrals. Yet there do not appear to be parallel declines among the above-median risk algorithm referrals. These results suggest that it is neither predicted risk nor the unobservables identified by outreach workers alone that predict treatment responsiveness, but the interaction of the two.

On average, outreach workers appeared able to identify men more responsive to programming. This could be due to some unobserved "readiness for change" or to having fewer barriers to participation like housing or transport. Outreach referrals may also have responded more due to the higher dose of programming this group received (Section 4.3), which itself could be a result of outreach workers' effort. Outreach workers often aimed to continue engaging with participants throughout the program, including with conflict mediation and de-escalation assistance when necessary. This could have been more successful with the participants they referred thanks to their pre-existing relationships. The role of ongoing interaction and relationships with caseworkers does seem to play a role in program success in other settings, such as sectoral employment training programs (see, e.g., Katz et al., 2022).

Importantly, however, this pattern does not appear to hold for the majority of outreach referrals. Only the subset of outreach referrals whom the algorithm also identified as being at highest risk appeared the most responsive. We emphasize that this finding is exploratory: our sample is too small to determine whether all of these subgroup results differ from each other significantly, and the differences across the risk distribution could be correlated with other factors (e.g., it is possible that more of the lower-risk outreach referrals received alternative programming that shrank their observed treatment effect). Nonetheless, that the combination of human and algorithmic screening seems to outperform either referral mechanism individually may provide some guidance for future CVI design and for research.

6   Social benefits and costs

In many settings, the statistical uncertainty about declines in primary outcome components, combined with the lack of improvements in other secondary outcomes, might lead us to question the value of the intervention. But there is a crucial difference about this setting: here, the outcomes of interest are literally measures of life and death. Shootings and homicides generate such enormous costs to individuals, families, and communities that even limited improvement on a small number of outcomes could generate large benefits to society. Weighting outcome measures by their social value can help inform how to think about the value of the program, and can provide a useful alternative to multiple testing adjustments when policymakers value changes in the different outcomes differently (Viviano et al., 2021).

In this section, we aim to quantify READI's benefits to provide additional insight into how we should think about the observed changes in serious violence involvement, and then compare them to the program's administrative costs. Our main focus is on the social costs surrounding crime and violence. Our calculations are necessarily a rough approximation, both because of the uncertainty involved with assigning costs to crime and the loss of life (Dominguez and Raphael, 2015), and because we ignore a range of other difficult-to-measure costs and benefits (e.g., the social value of investing in and generating jobs in historically

under-served neighborhoods, the value of the work that READI participants do, gains in unmeasured outcomes like mental or physical health, and the opportunity cost of program spending).[38] Our intention is not to provide a definitive cost-benefit analysis, but rather to provide a basic sense of how the substantive importance of the outcomes READI affects might shape how we think about the program's impacts.

Because there is so much uncertainty about the social costs of crime (including the statistical value of life), and because there are numerous choices about how to translate imperfect measures like arrests into assessments of the true amount of crime, Table 7 presents sets of less and more inclusive estimates for how READI affects social costs. Since there is only one outcome here and the analysis was pre-specified, we do not adjust for multiple testing.

Appendix A.6 provides additional detail about our calculations. In brief, for both sets of estimates, we assign to each arrest and victimization of a READI study member an estimated cost of crime depending on its type.[39] The less inclusive estimates use the lower-end estimates of the cost of crime from Cohen and Piquero (2009), and only count the harm from crime when directly observed in the data (i.e., from each observed arrest or victimization of a READI study participant). The more inclusive estimates use the higher willingness-to-pay estimates of the cost of crime (Cohen and Piquero, 2009);[40] extrapolate how much crime

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[38] Because READI was largely funded by private philanthropy, there was no deadweight loss from raising tax dollars. Future versions of the program might use public funding, and so might benefit from weighing the marginal value of public funds (Hendren and Sprung-Keyser, 2020). Another complicated issue surrounds the time coverage of our calculations. When a crime is committed in our data, we use estimates of social costs that span a victim's lifetime. But due to their involvement in crime, it is at least theoretically possible that saving the life of a sample member could generate other, future costs in terms of increased offending after our 20-month outcome period. Because it is difficult to anticipate how READI will affect future behavior, we do not attempt to extrapolate beyond the crimes we observe in our 20-month outcome period. But it will be important to follow the population over time to better assess total costs and benefits.

[39] In principle, an arrest may be for an offense that resulted in multiple people being victimized, and multiple arrests may be associated with a single victim. In practice, we cannot reliably measure the number of victims associated with an arrest due to data limitations. As a result, we assign a single cost to each arrest and victimization in the data, which is standard in the cost of crime literature. While this will underestimate the social costs associated with some arrests (those with multiple victims per arrestee), it will overestimate the social costs associated with other arrests (those with multiple arrestees per victim).

[40] There is debate about whether and how to use willingness-to-pay estimates in valuing crimes that involve the loss of life, since willingness-to-pay is shaped by ability-to-pay and therefore varies by factors like

is likely to have occurred for every arrest, given clearance rates in Chicago;[41] and scale each victimization up by average reporting rates for each crime type. When we compare the benefits of reduced crime to the administrative costs of the program, we treat program wages as a transfer from society to participants. The net READI costs are thus the administrative costs less payments to READI participants.

Table 7 reports program impacts on these different costs. The first thing to note is how much cost society incurs from the level of crime and violence in READI's absence: between $352,000 and $1,812,000 on average, depending on how inclusive the costs are, for each control complier. The second thing to note is that despite the uncertainty about how each element of our primary index responds to READI, the severity of shootings and homicides is so great that their higher weights in the cost of crime calculations generate less uncertainty about program benefits. We estimate that READI saved society a total of between $174,000 and $858,000 for each participant ($p = 0.03$), about a 50 percent decline. Compared to the cost of offering READI over 20 months (about $46,000 for each participant), these benefits are at least 3.8—and perhaps as much as 18.8—times as large as the program's costs.

7 Discussion

READI was developed and launched in response to the crisis-level gun violence Chicago faced in 2016. Few services were available then for the people at highest risk of being involved in this violence, and few studies could provide rigorous answers about which services effectively reduce that risk. We therefore designed this study to determine whether it is possible (and if

income. As Sunstein (2013) argues, it may be appropriate to consider an individual's own willingness-to-pay for a reduced mortality risk when they are being asked to fund the policy directly. But when external organizations are funding the intervention, as is the case with READI, there is a clear rationale for treating the value of life equally across people, or even up-weighting the protection of lower-income lives based on redistributive welfare gains. As such, we follow all federal agencies in using a unitary value for the harm from a loss of life. There is still some uncertainty about the appropriate dollar figure to use, informed by considerable research outside of crime on issues like climate change and transportation policy that involve a risk of mortality (see, e.g., summary in Sunstein, 2013). We view the different dollar figures across the more and less inclusive columns as a way to reflect some of that uncertainty.

[41] In particular, for each crime type's clearance rate C, we assume there are $1/C$ crimes committed for each observed arrest, and C offenders arrested for every observed victimization. See Appendix A.6 for further detail.

so, how) to find enough people facing a high enough risk of gun violence for social services to make a cost-effective difference; whether they could be engaged; and whether the combination of jobs and CBT could reduce their involvement in serious violence.

The answer to the first two questions is a definitive yes. The extraordinary risk of becoming a shooting or homicide victim in READI's control group—with 11.4 shootings and homicides per 100 people over 20 months—shows that it is possible to find enough future victims for an intervention targeted toward this group to make a cost-effective difference. Importantly, the fact that READI's referral mechanisms identified men who are observably different from each other but face a similar level of risk suggests that diversifying recruitment methods can help find a more complete set of people at risk of serious violence than any single method can on its own.

To put this level of risk in context, consider the number of shootings and homicides in the READI sample relative to Chicago as a whole. According to police records, between August 2017 and August 2021 there were about 4,600 fatal and non-fatal shootings in an average 20-month period.[42] The READI control group experienced about 140 shooting and homicide victimizations over the 20-month outcome period, or 3 percent of the city's total. The entire READI sample, therefore, would have experienced 6 percent of Chicago's shootings and homicides, despite containing under 0.01 percent of its population. And the costs borne by society from this group's involvement in crime and violence under the status quo, based on available estimates (and driven mostly by the cost of a gunshot injury (Cook et al., 2000)), would be at least $700 million, or about $285,000 per person.

If READI (or other programs) expanded, it would capture a greater share of the city's shooting and homicide victims. The size-to-risk ratio depends on how quickly risk declines as the number of referrals increases. Using the results of an algorithm that predicts shooting victimization risk from Heller et al. (2022) to establish a gradient, a conservative estimate is that a program serving 4,000 people could capture 10 percent of Chicago's shooting and

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[42] https://www.chicago.gov/city/en/sites/vrd/home.html

homicide victims.[43] This is considerably larger than most existing social service programs, but smaller than, for example, the largest high school in Chicago.

The 55 percent take-up rate confirms that this group, despite the risks and barriers they face, shows up. Given the 50 percent take-up among the lower-risk high school boys in Heller et al. (2017), and the fact that fewer than half the participants in shorter transitional jobs programs for re-entering populations worked more than 2 months (e.g., Redcross et al., 2016), we expected take-up rates of between 25 and 30 percent when making power calculations at the study's outset. The much higher take-up rate reveals considerable demand for READI's combination of safe and reliable work with CBT and supportive staff. Our interviews with participants suggest that many of them started out coming for the paycheck, but stayed due to how the combination of CBT-related skills and caring program staff improved their well-being.

READI's impacts on serious violence, however, are mixed. There is no statistically significant decline in our pre-specified primary outcome. This means we cannot conclude with certainty that the version of READI evaluated here, including the disruption to services caused by the pandemic, decreases serious violence. But there is suggestive evidence that READI reduced arrests for shootings and homicides, with the estimated effect being just beyond traditional statistical significance cutoffs after adjusting for multiple testing. For men referred by outreach workers, the declines in arrests and victimizations for shootings and homicides clearly pass standard statistical significance thresholds. Our analysis suggests that a combination of human intelligence and machine-driven risk prediction may more effectively anticipate treatment responsiveness than either method alone.

These estimates do not capture READI's complete impact on participants owing to sev-

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[43] This estimate is conservative because it relies solely on results from a prediction algorithm, whereas READI also relies on outreach workers who successfully use unobservables to identify additional likely future victims. We note that throughout this discussion, we focus on victim counts because we have reliable measures of shooting and homicide victimization. Relying on arrests to capture the level of (rather than the treatment–control difference in) offending is problematic, since low clearance rates and mistaken arrests make it impossible to know how many shooting offenders are in the sample. In practice, there tends to be a fair amount of overlap between victims and arrestees.

eral measurement challenges. For example, while crimes like aggravated assault and robbery are far more common than shootings and homicides, reported victimizations for the latter are three times higher than arrests for the former, driven entirely by differences in measurement. And arrests occurring outside of Chicago remain, for now, unobserved. But administrative data will always yield an incomplete view of a program's impact. Before the pandemic cut collection short, our qualitative work aimed to capture some of how staff, participants, and the community experienced READI. It was apparent in the information we collected that the investment in the community (both the urban renewal work participants performed and the READI-induced hiring at community organizations) were valuable to those who took part in READI, and that at least some participants reported unmeasured impacts on relationship quality, self confidence, and community integration.

From a scientific perspective, the statistically insignificant decline in serious violence involvement, as measured by our primary outcome, merits caution. Science is rightfully conservative about overturning null hypotheses based on a single imprecise finding. Further testing—especially without an intervening pandemic—would have a high payoff. From a policy perspective, however, the binary conclusion from one hypothesis test is not dispositive; $p > 0.1$ is not the same as a true zero. As a number of economists have emphasized (Imbens, 2021; Manski, 2019; Ziliak and McCloskey, 2008), policymakers should attend to the range of plausible magnitudes consistent with a point estimate, along with the availability of other credible evidence and the benefits and costs of the status quo versus new investments. In this vein, it is worth emphasizing that about 79 percent of the confidence interval for READI's estimated impact on the serious violence index is below zero; 85 percent of the confidence interval for shooting and homicide arrests is negative. Weighting outcomes by their social costs shows that READI's estimated benefits to society were clearly positive, and at least 3.8 times its cost. For policymakers trying to reduce the enormous harms of current levels of gun violence, and given the absence of other convincing evidence about how to reduce shootings without the collateral costs that often accompany aggressive policing, the findings

we report here should still be useful inputs into decision-making, despite the need for more evidence to generate firm scientific conclusions.

READI is a demonstration that investments of this scale are possible, even among a population deeply disconnected from every institution other than the criminal legal system, with a potentially large social payoff given the concentration of socially costly outcomes among a relatively small group of people. The results suggest that the combination of work and CBT has the potential to generate cost-effective decreases in serious violence. But future research could usefully experiment with refinements to the program model, targeting strategies, and other ways to reduce the extraordinarily high risk of gun violence among a small and under-served group.

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8   Figures and Tables

Table 1: Baseline characteristics

	Control Mean	Treatment Mean	Pairwise p-value
N	1232	1224	
Demographics			
Black	0.969	0.971	0.864
Age	25.3	25.1	0.443
Primary Outcome Components, Counts			
Shooting Victimizations	0.478	0.436	0.124
Shooting & Homicide Arrests	0.079	0.074	0.625
Other Serious Violent-Crime Arrests	0.908	0.869	0.364
Risk Prediction			
Risk Score	0.115	0.114	0.825
Missing Risk Score	0.108	0.080	0.008
Arrest Counts			
All Arrests	17.1	17.7	0.195
Less Serious Violent-Crime Arrests	1.6	1.6	0.996
Drug Crime Arrests	4.8	5.2	0.058
Property Crime Arrests	1.6	1.7	0.572
Other Crime Arrests	8.0	8.2	0.353
Victimization Counts			
All Victimizations	3.5	3.3	0.315
Other (Non-Shooting) Violent Victimizations	2.4	2.3	0.48
Non-Violent Victimizations	0.577	0.560	0.713
Incarceration Measures			
Days Incarcerated, Past 30 Months	177.0	173.9	0.707
Incarcerated at Randomization	0.041	0.039	0.745
Joint Test			
p-value on F-test			0.307

Notes: Pairwise p-value from test of treatment-control difference using heteroskedasticity-robust standard errors and controlling for randomization strata fixed effects. Arrest and victimization measures include all available CPD data from 1999 (2010 for shooting victimizations) through the time of randomization, with counts winsorized at the top 99$^{\text{th}}$ percentile. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. Less serious violent-crime arrests include simple assault and battery. Other (non-shooting) violent victimizations include sexual assault, robbery, aggravated assault and battery, and simple assault and battery. Non-violent victimizations include all other incidents such as burglary, stalking, and threats. Risk score is missing for 231 individuals who did not have at least one arrest or two victimizations within the 50 months prior to randomization (risk score N for control = 1,099 and for treatment = 1,126). Race is missing for 38 individuals (race N for control = 1,211 and for treatment = 1,207). Joint test excludes all arrests and all victimizations since they are linear combinations of other variables.

Table 2: Baseline characteristics and realized risk by pathway

	All	Algorithm	Outreach	Re-entry	P-value, Test of Pathway Difference
Baseline					
N	2456	1232	878	346	
Age	25.2	24.6	25.6	26.4	<.001
Black	0.970	0.964	0.986	0.953	<.001
Shooting Victimizations	0.462	0.636	0.290	0.278	<.001
Shooting & Homicide Arrests	0.076	0.080	0.064	0.092	0.167
Other Serious Violent-Crime Arrests	0.889	1.0	0.664	1.0	<.001
Ever Shot or Killed	0.346	0.461	0.234	0.225	<.001
Ever Arrested	0.977	1.000	0.945	0.974	<.001
All Arrests	17.4	20.4	13.6	16.2	<.001
Ever Victimized	0.834	0.908	0.768	0.743	<.001
All Victimizations	3.4	4.5	2.3	2.3	<.001
Days Incarcerated, Past 30 Months	175.4	129.2	155.1	391.6	<.001
Predicted Involvement in a Violent Gun Crime	0.114	0.137	0.089	0.080	<.001
Realized Risk Among Controls					
N	1232	616	438	178	
Shooting & Homicide Victimizations	0.114	0.109	0.119	0.124	0.837
Shooting & Homicide Arrests	0.026	0.021	0.032	0.028	0.575
Other Serious Violent-Crime Arrests	0.054	0.057	0.050	0.056	0.912
Ever Shot or Killed	0.106	0.107	0.105	0.107	0.994
Ever Arrested	0.636	0.698	0.578	0.562	<.001
All Arrests	1.7	1.9	1.4	1.3	<.001
Ever Victimized	0.314	0.352	0.285	0.253	0.010
All Victimizations	0.525	0.669	0.397	0.343	<.001
Days Incarcerated	76.9	85.8	60.9	85.3	0.010
Involved in a Violent Gun Crime	0.151	0.166	0.142	0.124	0.290

Notes: Top panel shows baseline characteristics. Arrest and victimization measures include all available CPD data from 1999 (2010 for shooting victimizations) through the time of randomization, with counts winsorized at the top 99th percentile. "Predicted Involvement in a Violent Gun Crime" shows the risk score: the predicted probability of being a victim or an arrestee in a violent crime involving a gun during the next 18 months. "Involved in a Violent Gun Crime" shows the actual realized rate of that same outcome over the 18 months after randomization. With the exception of that 18-month outcome period, the rest of the bottom panel shows control means during the 20-month outcome period. P-value from joint test of the null that referral pathway means are equal using heteroskedasticity-robust standard errors. In practice, counts and indicators for shooting and homicide victimizations at baseline include only non-fatal shooting victimizations. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. N for the risk score is 2,225 for All, 1,232 for algorithm referrals, 717 for outreach referrals, and 276 for re-entry referrals.

Table 3: Participants' earnings and hours by pathway

	Take-up rate	Work		CBT/Trainings		Total	
		Earnings	Hours	Earnings	Hours	Earnings	Hours
All Participants	55%	$7,040	559	$2,609	159	$9,648	718
Algorithm	37%	$6,492	517	$2,343	142	$8,836	659
Outreach	78%	$7,640	614	$2,825	172	$10,465	786
Re-entry	60%	$6,632	505	$2,543	157	$9,175	662

Note: Take-up defined as attending the first day of READI orientation. Earnings and hours are limited to men who took up and appear in the payroll data. This excludes 124 men who took up, most of whom did so prior to consent forms to allow the release of their payroll data being distributed. Earnings and hours worked are averages per participant over the 20 months post-randomization, ignoring participation after 20 months to match the outcome period. The maximum possible hours someone could have participated in both a job (29.5 hours/week) and CBT/trainings (7.5 hours/week) over 18 months was 2,664 hours, although those in the program during COVID-19 had a lower maximum. Work earnings and hours correspond to time spent at a worksite. CBT/trainings includes time spent in group CBT sessions, professional development (PD) sessions, and online trainings. From March 2020 to July 2020, participants received weekly standby pay (included in "work" earnings), which was calculated from the average weekly earnings in the month prior to COVID. Beginning in August 2020, participants were given the option to return to in-person work or complete online trainings and PD, which most participants opted to do (see Appendix A.5.3 and Appendix Table A.2). Earnings records for CBT/PD before March 2020 are incomplete; table shows totals extrapolated based on available data.

Table 4: READI's estimated effects on serious violence involvement

	Estimates				P-values		
	CM	ITT	CCM	TOT	Observed ITT	FWER	FDR-q
Primary Index of Serious Violence							
	0	-0.0289 (0.0232)	0.0173	-0.0530 (0.0409)	0.213		
Primary Outcome Components, Counts							
Shooting & Homicide Victimizations	0.1144	-0.0112 (0.0136)	0.1178	-0.0205 (0.0240)	0.411	0.646	0.617
Shooting & Homicide Arrests	0.0260	-0.0114 (0.0059)	0.0329	-0.0209 (0.0104)	0.054	0.148	0.161
Other Serious Violent-Crime Arrests	0.0544	0.0038 (0.0100)	0.0544	0.0070 (0.0177)	0.704	0.703	0.704

Notes: N = 2,456. Primary index standardizes each of the three components shown in the bottom panel based on the control group's distribution and takes their unweighted average. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. Multiple hypothesis testing adjustments define the three components of the primary index as a family. FWER p-values control for the family-wise error rate using the Westfall and Young (1993) method for step-down resampling. FDR q-values control for the false discovery rate using the Benjamini and Hochberg (1995) method. CM shows control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses.

Table 5: READI's estimated effects on serious violence involvement, by pathway

	CM	ITT	CCM	TOT	Observed ITT	FWER	FDR-q
			Estimates			P-values	
Primary Index of Serious Violence by Pathway							
Algorithm	-0.0121	0.0328	-0.0586	0.0904	0.337	0.557	0.337
		(0.0342)		(0.0908)			
Outreach	0.0107	-0.1031	0.0545	-0.1324	0.006	0.022	0.019
		(0.0377)		(0.0467)			
Re-entry	0.0156	-0.0590	0.0563	-0.0987	0.317	0.557	0.337
		(0.0590)		(0.0954)			
Primary Outcome Components by Pathway, Counts							
Algorithm (N = 1232)							
Shootings & Homicides Victimizations	0.1088	0.0175	0.0806	0.0483	0.380	0.607	0.570
		(0.0200)		(0.0530)			
Shootings & Homicides Arrests	0.0211	-0.0033	0.0181	-0.0092	0.689	0.680	0.689
		(0.0084)		(0.0222)			
Other Serious Violent-Crime Arrests	0.0568	0.0168	0.0471	0.0462	0.279	0.607	0.570
		(0.0155)		(0.0411)			
Outreach (N = 878)							
Shootings & Homicides Victimizations	0.1187	-0.0469	0.1332	-0.0603	0.029	0.058	0.044
		(0.0215)		(0.0266)			
Shootings & Homicides Arrests	0.0320	-0.0254	0.0414	-0.0326	0.009	0.023	0.028
		(0.0097)		(0.0121)			
Other Serious Violent-Crime Arrests	0.0502	-0.0044	0.0581	-0.0057	0.790	0.802	0.790
		(0.0164)		(0.0203)			
Re-entry (N = 346)							
Shootings & Homicides Victimizations	0.1236	-0.0220	0.1469	-0.0369	0.540	0.814	0.808
		(0.0360)		(0.0579)			
Shootings & Homicides Arrests	0.0281	-0.0041	0.0368	-0.0068	0.808	0.826	0.808
		(0.0169)		(0.0272)			
Other Serious Violent-Crime Arrests	0.0562	-0.0217	0.0565	-0.0365	0.269	0.662	0.807
		(0.0197)		(0.0318)			

Notes: N = 2,456. Estimates for each outcome are from a single regression that interacts pathway indicators with treatment. Primary index standardizes each of the three components shown in the bottom panel based on the control group's distribution and takes their unweighted average. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. For multiple hypothesis testing adjustments within the primary index, we define the three different pathways as a family. For the component adjustments, we define the three different outcomes within each pathway as a family. FWER p-values control for the family-wise error rate using the Westfall and Young (1993) method for step-down resampling. FDR q-values control for the false discovery rate using the Benjamini and Hochberg (1995) method. CM shows control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses. F-tests of the null hypothesis that treatment effects are equal across the three pathways are as follows: Primary Index: $p = 0.0280$; Shooting & Homicide Victimizations: $p = 0.0894$; Shootings & Homicide Arrests: $p = 0.206$; Other Serious Violent-Crime Arrests: $p = 0.288$.

Table 6: READI's estimated effects on serious violence involvement, by baseline risk level

		Estimates				P-values		
		CM	ITT	CCM	TOT	Observed ITT	FWER	FDR-q
Primary Index of Serious Violence by Risk Level								
Over Median		0.0822	-0.0688 (0.0402)	0.1733	-0.1526 (0.0859)	0.087	0.230	0.260
Under Median		-0.0495	0.0080 (0.0321)	-0.0661	0.0135 (0.0510)	0.805	0.947	0.805
Missing		-0.1432	-0.0141 (0.0485)	-0.1424	-0.0199 (0.0688)	0.772	0.947	0.805
Primary Outcome Components by Risk Level, Counts								
Over Median (N = 1112)								
	Shootings & Homicides Victimizations	0.1470	-0.0242 (0.0224)	0.1682	-0.0536 (0.0478)	0.281	0.477	0.421
	Shootings & Homicides Arrests	0.0358	-0.0195 (0.0101)	0.0552	-0.0433 (0.0217)	0.054	0.153	0.162
	Other Serious Violent-Crime Arrests	0.0771	-0.0042 (0.0175)	0.1002	-0.0093 (0.0373)	0.811	0.809	0.811
Under Median (N = 1113)								
	Shootings & Homicides Victimizations	0.0943	0.0029 (0.0193)	0.0896	0.0050 (0.0307)	0.879	0.880	0.879
	Shootings & Homicides Arrests	0.0203	-0.0066 (0.0083)	0.0222	-0.0107 (0.0132)	0.431	0.685	0.647
	Other Serious Violent-Crime Arrests	0.0407	0.0137 (0.0137)	0.0290	0.0226 (0.0217)	0.319	0.685	0.647
Missing (N = 231)								
	Shootings & Homicides Victimizations	0.0602	-0.0169 (0.0355)	0.0700	-0.0246 (0.0504)	0.635	0.951	0.718
	Shootings & Homicides Arrests	0.0075	0.0048 (0.0132)	0.0077	0.0075 (0.0186)	0.714	0.951	0.718
	Other Serious Violent-Crime Arrests	0.0150	-0.0055 (0.0154)	0.0083	-0.0083 (0.0218)	0.718	0.951	0.718

Notes: N = 2,456. Estimates for each outcome are from a single regression that interacts indicators for above/below-median risk score and missing risk score with treatment. The median risk score in the study sample, the predicted probability of being arrested for or the victim of a violent gun crime in the next 18 months, is 0.11. Primary index standardizes each of the three components shown in the bottom panel based on the control group's distribution and takes their unweighted average. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. For multiple hypothesis testing adjustments within the primary index, we define the three different risk score groups as a family. For the component adjustments, we define the three different outcomes within each risk score group as a family. FWER p-values control for the family-wise error rate using the Westfall and Young (1993) method for step-down resampling. FDR-q values control for the false discovery rate using the Benjamini and Hochberg (1995) method. CM shows control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses. F-tests of the null hypothesis that treatment effects are equal across the three risk score groups are as follows: Primary Index: $p = 0.333$; Shooting & Homicide Victimizations: $p = 0.653$; Shootings & Homicide Arrests: $p = 0.328$; Other Serious Violent-Crime Arrests: $p = 0.582$

Table 7: READI's estimated effect on the social costs of crime

	Less Inclusive Estimates				More Inclusive Estimates			
	CM	ITT	CCM	TOT	CM	ITT	CCM	TOT
Social Cost of Victimization								
READI Sample Victims	$147,137	-$22,313	$148,853	-$40,961	$390,138	-$55,647	$398,573	-$102,154
		($33,223)		($58,647)		($82,136)		($144,998)
READI Sample Offenders	$122,653	-$65,988**	$182,248	-$121,138**	$1,032,828	-$404,743**	$1,386,751	-$743,012**
		($28,736)		($50,853)		($198,943)		($351,862)
Social Cost of Punishment								
Legal System Costs	$10,498	-$4,287**	$14,369	-$7,870**	$14,399	-$4,855**	$18,361	-$8,913**
		($1,888)		($3,341)		($2,038)		($3,606)
Productivity Loss from Incarceration	$4,374	-$1,988**	$6,155	-$3,650**	$6,183	-$2,253**	$8,008	-$4,137**
		($881)		($1,560)		($952)		($1,684)
Total Social Cost of Crime	$284,662	-$94,576**	$351,625	-$173,618**	$1,443,548	-$467,498**	$1,811,693	-$858,215**
		($45,393)		($80,255)		($216,633)		($383,158)
Social Cost of Program								
Administrative Costs		$30,124		$55,274		$30,124		$55,274
Transfer to Participants		-$5,256		-$9,644		-$5,256		-$9,644
Net READI Costs		$24,868		$45,630		$24,868		$45,630
Benefit-cost Ratio		3.8:1		3.8:1		18.8:1		18.8:1

Notes: N = 2,456. Outcome variables in top panel are different measures of social costs from crime. Negative point estimates show gains from reduced crime. Crime cost estimates, from Cohen and Piquero (2009) and inflated to 2017 dollars, aim to quantify harm to victims from each crime, the productivity loss to offenders from their involvement in the legal system, and the cost to the government of running the legal system. The less inclusive estimates use bottom-up crime cost estimates and only assign costs to observed arrests and victimizations (ignoring both offending that does not result in arrest and arrests of non-study individuals who victimized READI participants). The more inclusive estimates use willingness-to-pay crime cost estimates and extrapolate social harm based on estimates of clearance rates (i.e., for each crime type's clearance rate, C, we assume there are $1/C$ crimes committed for each observed arrest and C arrests made for each observed victimization), as well as victimization reporting rates. Program costs calculated from spending reported by Heartland Alliance over the 20-month outcome period. CM shows control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$
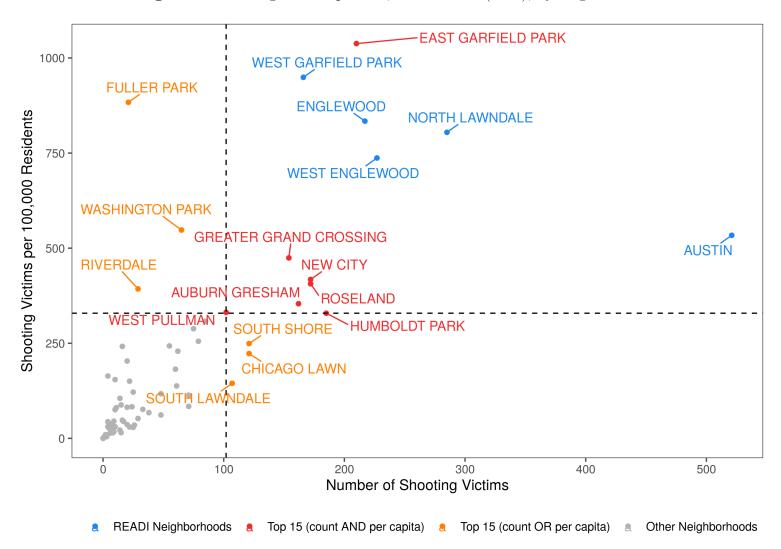
Figure 1: Shooting victims per 100,000 residents (2016), by neighborhood

Notes: Plot shows counts and rates of shooting victims in each of Chicago's 77 community areas in 2016. Dashed lines represent top 15 neighborhoods for each dimension.
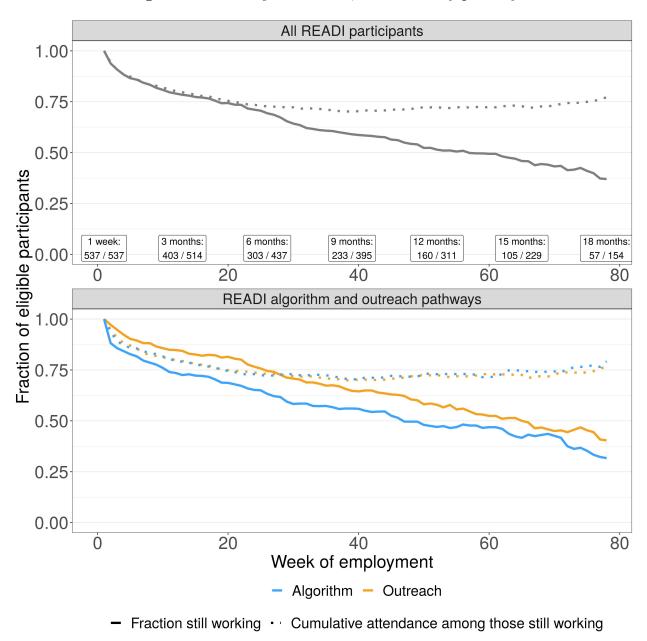
Figure 2: READI job retention, overall and by pathway

Notes: Figure shows two measures of job retention for men who started READI measured from payroll data. The solid line shows the proportion of participants who work at least once after the time shown on the x-axis conditional on observing them for that long. The boxes show the number of workers contributing to each point. The dotted line shows the average proportion of possible weeks worked among those still working at each point in time. At 18 months after first taking up, N = 19/60 algorithm referrals and N = 38/94 outreach referrals are still observed working. Since COVID-19 changed both what "participation" looked like in practice as well as payroll policies, we report these measures of retention using only data through the start of the pandemic. Because there were too few re-entry participants with sufficient pre-COVID data to measure retention, they are omitted. For a description of the pandemic's impact on READI, see Section 2.3 and Appendix A.5.3. For retention measures inclusive of the pandemic period, see Appendix Figure A.3.
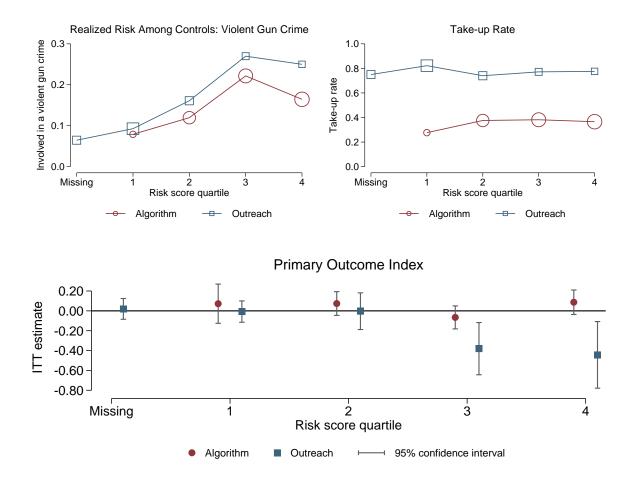
Figure 3: READI realized risk, take-up, and estimated effects by pathway and risk level

Notes: Top left panel shows the realized rate of involvement in a violent gun crime as a victim or an arrestee during the 18 months after randomization by quartile of the risk score, which is the predicted probability at baseline of the same outcome, separately for algorithm and outreach referrals. Top right panel shows the take-up rate, defined as the share of the treatment group attending the first day of READI orientation, by quartile of the risk score, separately for algorithm and outreach referrals. Markers in both of the top panels are weighted to reflect the share of algorithm and outreach referrals, respectively, in each risk score bin. The bottom panel shows coefficient estimates and 95 percent confidence intervals (using heteroskedasticity-robust standard errors) on three-way interactions of pathway indicators, risk quartile indicators, and an indicator for being randomized to receive a READI offer, from a regression of the primary index on baseline covariates, randomization strata fixed effects, and all two-way interactions of pathway indicators, risk quartile indicators, and an indicator for being randomized to receive a READI offer. N = 231 for the missing risk score group (of whom 161 are outreach referrals).

ONLINE APPENDIX

Predicting and Preventing Gun Violence: An Experimental Evaluation of READI Chicago

Monica P. Bhatt, Sara B. Heller, Max Kapustin, Marianne Bertrand & Christopher Blattman

A.1 Research on policing and CVIs

Targeted interventions for reducing concentrated gun violence have a long history. For instance, "hot spots" and "proactive" policing models focus resources on small geographic areas with a high risk of gun crime occurring. "Focused deterrence" and other related strategies apply sanctions and provide services to specific people and groups at high risk of involvement in future gun violence, often with community involvement. Both approaches show some success in reducing violence overall and gun violence specifically (e.g., Grogger, 2002; Sherman and Rogan, 1995; Braga et al., 2001, 2014, 2018; Ridgeway et al., 2019; Wood and Papachristos, 2019). But increased police attention can impose significant costs on its targets (Ang, 2021; Geller et al., 2014; Jones, 2014; Chalfin et al., 2022). It can also cause harassment of or harm to residents of hot spots who are not engaged in any criminal activity but are nonetheless subjected to greater police scrutiny. This concern has renewed interest in community violence interventions (CVIs)—programs such as READI that offer alternatives to law enforcement-focused approaches.

Among the most widely studied and adopted CVIs focused on shootings is violence interruption, exemplified by organizations such as Cure Violence.[1a] Violence interruption relies on outreach workers to mediate active disputes, foster community norms of non-violence, and refer the highest-risk men to available services (Butts et al., 2015a). In addition to

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[1a] See, e.g., Skogan et al. (2008); Wilson and Chermak (2011); Butts et al. (2015b); Fox et al. (2015); Webster et al. (2012); Picard-Fritsche and Cerniglia (2013); Buggs et al. (2022). For an overview, see Butts et al. (2015a).

community-wide education and mobilization efforts, neighborhood-based outreach workers identify and leap into imminent or active disputes, especially an initial shooting, and try to avert cycles of retaliatory violence by helping involved parties cool emotions, step back from immediate conflict, and find non-violent ways to respond, save face, or achieve justice. There are parallels to the emotional regulation and slower thinking encouraged by CBT-informed programs such as READI. By its nature, however, mediation and other violence interruption techniques tend to be more immediate and reactive. This interruption approach and the READI model are distinct and, in principle, complementary—akin to emergency and preventive medicine. Reviewing the literature on interventions adopting the Cure Violence model, Butts et al. (2015a) find the evidence of their effectiveness to be "mixed at best." Some studies report large reductions in homicides and shootings in specific neighborhoods of Chicago and Baltimore that researchers attribute to the intervention (Skogan et al., 2008; Webster et al., 2012), while others find no such impacts or even evidence of adverse effects (Wilson and Chermak, 2011). A recent study by Buggs et al. (2022) tries to overcome the confounding challenges in this literature by using synthetic control methods to construct more reliable comparison neighborhoods. Overall, neighborhood-level interventions are quite challenging to evaluate given the difficulty of finding comparable comparison groups when many determinants of violence are unobserved to the researcher.

The READI model is closer to a relatively new set of CVIs that deliver intensive preventative services to people thought to be at high risk of shooting involvement. These programs—which include Advance Peace, Roca, Turn90, CRED, and READI—all rely on local outreach workers to find and engage clients, usually young men of color, with extensive prior criminal legal system involvement and exposure to violence and trauma. Although they differ somewhat in the composition and duration of the services they provide, most offer some combination of financial assistance or supported employment, as well as counseling or CBT. Advance Peace, operating in high-violence neighborhoods within several California cities, provides clients with 18 months of mentorship, life skills coaching, and financial support (Corburn and Fukutome, 2199). Roca, operating in several Massachusetts sites and Baltimore, Maryland, provides clients intensive case management, supported education or employment opportunities, and CBT, with an average engagement of approximately two years (Abt Associates, 2199). Turn90 (formerly Turning Leaf), operating in Charleston, South Carolina, provides men returning from prison with 150 hours of CBT, case management, and a transitional job. CRED, operating in several Chicago neighborhoods, provides clients with life coaching and counseling, academic support, and a transitional job.

These newer CVIs are promising and represent potentially cost-effective models, but there is no causal evidence of their effectiveness so far. An evaluation of Advance Peace

follows the approach taken by studies of violence interruption programs and measures its impact at the city- and neighborhood-level, but lacks a comparison group (Corburn et al., 2022). An implementation evaluation of Roca that similarly lacks a comparison group reports the relationship between participants' outcomes and the intensity of their engagement with programming (Abt Associates, 2199), though our understanding is that a separate impact evaluation remains underway. A report summarizing preliminary evidence on CRED finds that participants experience 50 percent fewer shooting victimizations in the 18 months after starting programming than they did before, but a quasi-experimental comparison with similar young men not in CRED does not find statistically significant effects.[2a] As a result, the impact of this type of programming remains an open question.

A.2 Data details

A.2.1 Chicago Police Department

We use data from the Chicago Police Department (CPD) as our primary measure of crime and violence. We combine information from six different CPD databases: arrests from 1999 to present (including juvenile arrest information), victimizations reported to CPD from 1999 to present, reported crimes from 1999 to present (which are not tied to individual victims), shooting victims from 2010 to present, homicide victims from 1999 to present, and homicide offenders from 1999 to present. Arrest records are tied to fingerprints and attached to unique identifying numbers, which appear in other databases when they are known. But victim records are identified by name and date of birth, without these identifiers. To deduplicate and link these administrative records, we use Name Match, a probabilistic matching algorithm (McNeill and Jelveh, 2022; Tahamont et al., 2021).

We code arrest and victim records into categories using the FBI offense codes in the data,[3a] separating shootings from other assaults using accompanying flags in the data (since FBI codes do not separate shootings from other non-fatal assaults). Our measures of age and race/ethnicity also come from CPD records, with the latter likely measured with error since it captures officer impressions rather than self-identification.

A.2.2 Incarceration data: jail and prison

We have two sources of incarceration data: records from the Cook County Sheriff's Office (CCSO) capturing who is housed in the Cook County Jail each day from 2015 to present, and records from the Illinois Department of Corrections (IDOC) capturing entry and exit dates in state carceral facilities from 1999 to present. CCSO records were associated with

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[2a] https://www.ipr.northwestern.edu/documents/reports/ipr-n3-rapid-research-reports-cred-impact-aug-25-2021.pdf

[3a] https://ucr.fbi.gov/crime-in-the-u.s/2011/crime-in-the-u.s.-2011/offense-definitions

the same identifying number used by the Chicago Police, so we could match those records directly. We used probabilistic matching based on name and date of birth to match IDOC records to our study sample.

We use the carceral data to generate baseline covariates for the number of days incarcerated in the 30 months prior to randomization and measures of incarceration during the 20-month outcome period. We also used these records to check whether someone referred to READI is currently incarcerated. Since we only received periodic updates of these data (for IDOC annually during the first half of the study and bi-annually during the second half), some individuals who had been recently incarcerated were included in randomization.

A.2.3 Data from READI providers

Each community organization reported program attendance separately, and Heartland provided us with centralized payroll and participation information. Overall take-up decisions are measured from monthly data on who is currently being served. For most of the study, we also have weekly payroll records capturing the weeks an individual worked on a job site and the amount earned. Heartland required individuals to consent to having this payroll data shared with researchers, but in the very beginning of the study, not everyone was consented. As a result, we are missing payroll data for 117 individuals (though we can still see whether they participated at all based on the monthly service records).

Participation in CBT sessions was less systematically recorded by program providers. In some cases, we were only provided with sign-in sheets that showed actual signatures, the legibility of which prevents us from clean records about participation. For most providers between July 2018 and July 2019, we received legible sign-in sheets, from which we handcoded participation. During this time period, participation in CBT and training sessions is very closely correlated with when people appear in the payroll data. So we use the better-measured payroll data to extrapolate participation in CBT and trainings for the periods and providers that have incomplete CBT attendance data, up until March 2020. Starting in April 2020, we received Excel files containing attendance records reflecting who received stipends each week, although they are not always available every week for every provider. We perform a similar extrapolation for these records.

A.2.4 Data linkage

A critical prerequisite to computing baseline and outcome measures was the ability to link READI referrals to CPD arrest, victimization, shooting, and homicide data. Among these CPD data sources, only individuals in the arrest data are tracked over time via a unique person identifier (called an IR Number). As a result, we must use a probabilistic record linkage algorithm to associate each unique study member with all of their records across the

CPD data. For details on the algorithm itself, see McNeill and Jelveh (2022). In this section, we describe the basics of the linking procedure.

The high-level goal of the record linkage algorithm is to divide the records found in five data sources—READI study members, CPD arrests, victimizations, shootings, and homicides—into clusters, or groups of records that refer to the same person. To do this, the algorithm first compares the identifying fields (name, date of birth, age, home address, gender, race) for individual pairs of records and predicts whether the two records refer to the same person or not. To inform this prediction, the algorithm uses examples from the CPD arrest dataset to learn how identifying fields compare in records that should and should not be linked. Specifically, it leverages the fact that an arrestee's IR Number can be used to generate ground truth examples of records that refer to the same person and records that do not refer to the same person.

While aggregating these predicted links into groups of records that refer to the same person, the algorithm follows researcher-specified rules that are customized to fit the context of the data. For our linkage, we specify the following constraints. First, a cluster can have at most one post-2010 IR number. Second, a homicide victimization record cannot link to another record if the homicide record's event date came before the other record's event date. Third, some victimization records do not have date-of-birth information. To reduce the chance of this causing false positive links, we introduce a constraint that if at least one record in a record pair is missing date-of-birth information, the age fields must be within 3 years. We also enforce that if at least one record in a potential cluster is missing exact date-of-birth information, all other records in the cluster not missing date-of-birth information must have similar dates of birth. Additionally, if we could not find a referral in CPD records, we consulted Chicago Public Schools records to identify the person.

It is important to our research design and maintaining randomization that treatment and control individuals are balanced on baseline measures of criminal legal system involvement. Therefore, it was important to ensure that links formed between a study member and their pre-period records were unaffected by post-randomization CPD records. To operationalize this requirement, our record linkage procedure clustered predicted links in chronological order and enforced one final constraint: that once a READI study member was linked to a cluster, the cluster could not be linked to any more CPD records from before that individual's randomization date. This process makes it impossible for post-period CPD involvement to inform which pre-period CPD records are linked to a study member.

A.2.5 Arrest data & bias

As discussed in Section 3.1, the key assumption for treatment–control differences in arrests to successfully proxy for treatment–control differences in offending is that treatment does not change the probability of arrest conditional on actual criminal behavior. There are two ways this assumption might not hold. First, program-driven changes in time use may affect the probability of interacting with a police officer. For example, if treatment decreases idle time spent outside, it might lower the probability police happen to notice someone who has an outstanding warrant, or that they arrest someone for a crime they did not commit (e.g., for loitering or as part of a general round-up). Second, conditional on an interaction, treatment may affect the likelihood of talking one's way out of—or into—an arrest. For example, how a person responds to an officer could determine whether a street stop results in a disobeying an officer or disorderly conduct arrest.

There are several reasons to think these issues are unlikely to be a major problem in our setting. First, if improving skills or employing people during the day actually reduced the probability of arrest, we would expect to see consistent arrest declines in other programs that accomplish these changes. But many jobs programs have shown, on average, no decline in arrests (Bloom et al., 1997; MDRC, 1980; Redcross et al., 2016), and there is evidence from surveys that while education does decrease crime, it does not change the probability of arrest conditional on crime (Lochner, 2004; Lochner and Moretti, 2004). Second, if READI were reducing police interaction or improving the quality of the interactions, we would expect those changes to be most salient for the kinds of arrests that involve the most police discretion. Yet as we show in Appendix A.5.7, we can rule out even relatively small declines in our "other" arrest category, which includes most discretionary arrests such as vandalism, trespassing, loitering, disorderly conduct, disobeying an officer, and so forth. Combined with the increased scrutiny that serious violence charges like shootings and homicides entail from prosecutors following an arrest, it seems quite unlikely that READI is substantially changing the probability of being arrested conditional on committing (or not committing) a serious violent crime.

A.3 Randomization details

Randomization occurred between August 2017 and March 2020. Because of the onset of the COVID-19 pandemic, we stopped randomization, resulting in a study sample approximately 20 percent smaller than initially intended. Intake at a given time was based on provider capacity, determined by funding levels and the current caseloads of READI outreach workers. Below we describe the referral and randomization process for each referral pathway, since processes differed based on the source of the referral.

The main text reports a joint test of significance of baseline covariates showing that randomization successfully balanced observable characteristics, and Appendix Table A.1 reports a similar test by pathway. The covariates included in the baseline test are age; the baseline versions of our three primary index components (counts of arrests for shooting and homicide, victimizations for shootings [no baseline homicide victims], and arrests for other serious violence); counts of arrests for less serious violence, property, drug, and other crimes; counts of non-shooting and non-violent victimizations (separately); days incarcerated in the prior 30 months; an indicator for being incarcerated at baseline; the predicted risk of future gun violence (risk score with 0s imputed for missing); an indicator for missing risk score; and an indicator for not being Black (other race/ethnicity or missing).

A.3.1   Algorithm referral pathway

Referrals from the algorithm pathway, which began in December 2017 and occurred over 13 rounds through October 2019, started with the output from a machine learning algorithm trained to predict gun violence involvement in the next 18 months. Because READI recruited on a rolling basis, and because the relationship between the predictors and gun violence involvement may not be static, new models were trained 10 times over the referral period using the most up-to-date records available from CPD. To be in the prediction sample, a person had to have at least one arrest or at least two victimizations in the data during the 50 months prior to the prediction date. Excluding those with a single victimization record serves to remove a large number of people at very low risk of future gun violence (e.g., tourists who report a mugging). It also improves data quality: Non-shooting victimization records do not always have reliable dates of birth, so single cases in victim records can involve considerably more matching error. Heller et al. (2022) describes much more about a related prediction model.

One key difference relative to Heller et al. (2022), which focuses solely on shooting victimization, is that the READI model trained gradient-boosted decision trees to predict a broader outcome: whether someone would be either arrested for, or the victim of, a violent crime involving a gun during the next 18 months (to match READI's basic service period). Violent crimes involving a gun included homicide, assault, battery, or robbery with a firearm.[4a] We included arrests here to proxy for gun violence offenses like homicides, non-fatal shootings, and armed robberies, despite concerns about arrests capturing both individual behavior and police decision-making. The reason is that those in charge of READI were very interested in serving potential offenders as well as potential victims. Since READI provided what was perceived as a valuable opportunity rather than punishment or police involvement, the costs

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[4a] The predicted outcome excludes suicides and incidents where someone is shot or killed by a police officer.

of using information influenced by potential police bias to refer men to READI seemed relatively low. Additionally, in practice, arrests are a very incomplete measure of such behavior due to the low arrest or "clearance" rate in Chicago for these offenses. For example, in 2016, only 26 percent of homicides and 5 percent of non-fatal shootings in Chicago resulted in an arrest (Kapustin et al., 2017). As a result, the large majority of the outcomes we predict are gun violence victimizations (91 percent).

The model used over 1,400 features built from arrest and victimization records to predict future violence. The model continued to develop over the course of READI. In the beginning of the study, we did not have permission to use either juvenile records or information on domestic violence victimizations to train the model or generate predictions. We obtained permission to use juvenile records in February 2019. Other refinements were made over time as we continued to learn from studying the prediction model. We never used race or ethnicity as predictors in the model, since there are open legal questions about the role of such features in determining service provision.

The output of a given model is a set of predicted probabilities of gun violence involvement in the 18 months following the model's prediction date for around 300,000 people. To identify potential candidates for randomization and referral to READI, we imposed four restrictions on the output of the models. First, we limited our focus to adult men under the age of 40. Second, we limited our focus to men with a high likelihood of living in one of the neighborhoods where READI operates (identified from the home address and incident locations in the CPD data). Third, because the referral process involves sharing information about a person derived from confidential police records with an outreach organization, we excluded anyone about whom publicly available information was unavailable. In practice, this meant limiting to only men with at least one adult (18+) arrest since January 1, 2014, as these arrest records are public.[5a] Finally, we removed anyone who died in the period after their predicted risk was generated, or who was incarcerated in an Illinois Department of Corrections (IDOC) prison or the Cook County Department of Corrections (CCDOC) jail when randomization and referral were about to occur. After imposing these restrictions, men not already in the study were ranked in descending order of their predicted probability of future gun violence involvement, separately within each of the three READI neighborhood groups.

If a program provider needed X individuals for a given randomization round, we selected the 2X individuals with the highest predicted risk in their area. We then randomized this group, so everyone had a 50 percent chance of being offered READI.[6a] Once the treatment

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[5a] http://publicsearch1.chicagopolice.org

[6a] There are nine cases where people who were ineligible for READI based on their baseline characteristics

group was selected, we compiled publicly available information on each individual (name, age, mugshot, and locations of recent arrests) into a referral sheet and provided these to the outreach organizations.[7a]

Upon receiving these referral sheets, workers at the assigned READI outreach organization used their social networks, social media, and public databases to locate the individuals. This process could take weeks or months. Even after the search process, a significant share of referred men could not be found: according to self-reported data by outreach workers, of the 775 men referred through December 2018, 14 percent could not be located. For those men who could be found, outreach workers would explain the nature of READI, attempt to develop a relationship, and persuade them to join. Those who refused were still eligible to enroll at a future date, and outreach workers often remained in close contact.

A.3.2 Outreach referral pathway

The second referral pathway relies on the experience and knowledge of front-line staff at READI's partner outreach organizations to identify men currently involved in gun violence in their neighborhood. These outreach organizations are community-based non-profits with longstanding roots in their neighborhood, and they typically have been involved in a range of violence-reduction activities and youth programming for some years. Their front-line workers are recruited from the communities they serve and often have backgrounds similar to those of program participants, including exposure to secondary trauma (Hureau et al., 2022). This experience provides them with extensive information networks, contacts, and the familiarity and credibility to approach high-risk young men. The partner organizations

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slipped through our initial screening process but could later be identified: two women, three people who were deceased by the date of randomization, and four duplicates who had already been randomized. Since eligibility status is a baseline characteristic that can be observed for both treatment and control groups, we can drop these nine cases from the study without undermining randomization. This means that in a few strata, treatment probability varies very slightly from 50 percent. To account for this aspect of the experimental design, we control for randomization strata fixed effects in all analyses. As described below, re-entry referrals recruited and consented in the jail were only randomized upon discharge, which often occurred one-at-a-time rather than in groups. We group everyone in this pathway who was randomized individually into separate strata based on their quarter of random assignment. Each person had exactly a 0.5 probability of being assigned to READI, but the realization of multiple Bernoulli trials means these strata also have some variation around 0.5.

[7a] In the beginning of the study, the referral sheets included only a referral's name and age. This limited information proved very difficult for outreach workers to use successfully, since many of the men being referred did not go by their legal names, nor was there any indication of where an outreach worker might find them. We then added mugshots and locations of recent arrests to the sheets, both of which were publicly available via CPD's online adult arrest record search. Starting in February 2019, to further assist outreach workers in finding referrals, we had research staff go to the courthouse in Chicago to use terminals that contain additional information such as referrals' three most recent home addresses and any upcoming court dates, which were added to the sheets. Outreach workers reported that this additional information helped them locate and recruit these individuals.

employed READI-dedicated outreach workers to search for, identify, screen, and recruit READI participants.

Outreach workers were instructed to use community contacts and their organization's sources to identify the men they believed to be at highest risk for being involved in gun violence in the coming months. After identifying someone as a potential candidate, an outreach worker discussed the possibility of READI participation with them, explained the study, and administered a 10-question risk assessment. The questionnaire asked whether the person was a victim of a violent crime; had previously been incarcerated for a gun-related offense; was an active member of a street gang; had substance abuse issues; was unstably housed; was promoting violence on social media; and had recently been arrested. Outreach workers were instructed that a referral had to meet, at a minimum, more than one of these conditions. Outreach organizations then discussed the set of interested candidates and decided whether to nominate them for random assignment.

Each time a program provider was ready for new outreach referrals, they provided us with an even-numbered list of people who had been through this screening process. The research team then vetted the list, returning anyone who we either could not locate in available administrative data or had been previously randomized.[8a] The provider then either corrected identifying information for those whom we could not find or provided new names to replace anyone previously randomized.

Once we had a vetted list with exactly twice the number of names as available slots, we randomized with a 50 percent treatment probability. We then returned the list of selected individuals to the providers so they could start the intake process. This referral process occurred 57 times, from August 2017 through March 2020.

The outreach referral process creates several key differences between outreach referrals and algorithm referrals. First, because outreach workers typically knew the location and interest of a person before randomization, we expected (and later confirmed) higher rates of take-up for men referred through this pathway. Second, because outreach workers knew whether a person they referred was randomized *not* to receive a READI offer, they may have worked to find such a person alternative programming. In contrast, a person identified by the algorithm pathway and randomized to the control group was not contacted by the research team, nor was their identity shared with outreach, making it less likely that they were referred to alternative programming. Lastly, while the algorithm pathway mechanically identified men solely on the basis of their risk of future involvement in gun violence as predicted

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[8a] We attempted to match each name and date of birth to our administrative records using both exact and probabilistic matching. If someone was not found in CPD records, we also referenced Chicago Public Schools data to try to identify the individual.

using CPD records, outreach workers could have identified men based on some combination of risk of future gun violence involvement and being ready and willing to participate in programming, among other potential factors.

A.3.3 Re-entry referral pathway

The third and final pathway focuses on men being released from incarceration, including those released on condition of parole from an Illinois Department of Corrections (IDOC) prison facility, and those exiting the Cook County Jail (including via the use of electronic monitoring). Similar to outreach referrals, this pathway relies on the knowledge and experience of implementation partners, in this case parole officers and others working in detention facilities. They were told about the population READI aimed to serve and asked to refer those at high risk of gun violence. As with outreach referrals, location and interest in program participation were typically ascertained prior to nomination for random assignment for this pathway.

The re-entry pathway consisted of referrals from three different carceral settings. First, for people leaving IDOC custody to parole, parole agents received a set of criteria developed to aid in identifying READI's target population of high-risk men, including having been a victim or offender in a violent or gun crime, as well as being over 18 and living or being paroled to a READI neighborhood. The agents shared those referrals in batches with the research team, at which point the process looked identical to the vetting and randomization for outreach referrals.

Second, at the Cook County Jail, an embedded program staff member identified eligible individuals from among those housed in Division 6 or participating in the Sheriff's Anti-Violence Effort (SAVE), which serves men 18-24 years old from Chicago's 15 highest-violence neighborhoods.[9a] To be eligible, individuals had to be between ages 18 and 40, live in READI zip codes, and have a predicted probability of future gun violence involvement using the algorithm described above that was at or above the 95th percentile. Once this group was identified, Heartland Alliance staff consented eligible individuals while they were in the jail. Because release dates were unpredictable, consented individuals did not become part of the study and randomized until they were released. The first set of referrals occurred in a group, but after that individuals were randomized one-by-one at the point of release, with a treatment probability of 50 percent. Those assigned to the treatment group were then assigned to outreach workers based on the neighborhood to which they were returning.

[9a] Division 6 of the Cook County Jail houses minimum- and medium-security individuals. Following conversations with jail administrators about the population READI was designed to serve, the need to identify individuals likely to be released from the jail (rather than go on to serve a prison sentence), and logistical constraints to in-facility recruitment, it was determined that Division 6 was the most promising source of eligible individuals.

Lastly, a small group of referred individuals entered the custody of the Illinois Department of Juvenile Justice (IDJJ) as youth but were exiting custody as adults. Individuals leaving IDJJ custody enter Aftercare, the community supervision program run by IDJJ that is analogous to adult parole. Aftercare specialists referred people 18 or older living in READI community areas to Heartland Alliance staff, who then met with those individuals to recruit and consent them. If the referred individuals wished to participate, READI shared their information with the research team for random assignment, which occurred either in pairs or one-by-one, with a treatment probability of 50 percent.

A.3.4 Variation in treatment probability within strata

There are nine cases where people who were ineligible for READI based on their baseline characteristics slipped through our initial screening process but could later be identified: two women, three people who were deceased by the date of randomization, and four duplicates who had already been randomized. Since these characteristics are observable for both treatment and control groups, we can drop these nine cases from the study without undermining randomization. Some of the resulting randomization strata have an uneven number of people in them, with treatment probabilities not exactly equal to 0.5. To account for this aspect of the experimental design, we control for randomization strata fixed effects in all analyses. Re-entry referrals recruited and consented in the jail were only randomized upon discharge, which often occurred one-at-a-time rather than in groups. We group everyone in this pathway who was randomized individually into separate strata based on their quarter of random assignment; each person had exactly a 0.5 probability of being assigned to READI, but the realization of multiple Bernoulli trials means these strata also have some variation around 0.5.

A.4 Program details

A.4.1 Jobs and wages

READI jobs were designed to help participants with very little work readiness stay involved in the program. As such, jobs and wages were designed in stages with progressively broader goals set for each stage. Appendix Figure A.1 shows the initial conception of how these stages would work, along with how wages would grow as participants advanced across stages. From the time of its conception, READI's designers knew that relatively few participants could be expected to make it all the way to Stage 4. This was not a typical "transitional jobs" program, where the goal was to establish long-term employment. Indeed, increasing employment was not a primary outcome of the intervention; the focus was always on preventing violence.

Appendix Figure A.2 reflects how wages actually grew over time, with the number of

weeks after initial take-up on the x-axis and change in wages on the y-axis (the graph adjusts for the rising minimum wage in Chicago, so it shows only progression through stages with increased wages). The average participant who persisted in the program earned 40–50 cents more per hour by the end of READI than at the beginning, which is about 1 additional stage.

A.4.2 CBT

In READI, participants took part in 90-minute group CBT sessions three mornings per week. Facilitators delivered a version of the University of Cincinnati's Cognitive Behavioral Interventions Core Curriculum (CBI-CC) modified to be culturally relevant and targeted to participants' literacy levels. Staff also incorporated aspects of the Seeking Safety curriculum to address substance abuse and symptoms of traumatic stress. Sessions were co-facilitated by a senior coach and an experienced CBT facilitator who is a member of the READI program staff. Cross-community meetings of CBT staff aimed to ensure that the model was being delivered consistently across READI's sites, though in practice there was considerable variation across providers and staff members, as well as over time.

A.4.3 Safety during READI programming

Maintaining participant and staff safety during programming was a major focus of program operations. To avoid reports about participant locations to non-participating rivals, READI participants were banned from having their cell phones at their worksites. They were also forbidden from carrying weapons, enforced by metal-detecting wands. READI provided group transportation from morning CBT sessions to the worksites, to avoid the dangers involved in crossing gang lines. Staff would also sometimes rearrange work crews to help manage ongoing personal conflict and group rivalries. READI program operators learned over time what was more and less successful in ensuring safety, and program rules continued to develop over the course of the program. For example, the crew chiefs on worksites would sometimes call the outreach workers to help resolve conflicts while participants were at work. And work site assignments were rotated so that no one could consistently anticipate where participants would be.

A.5 Additional results

This section provides further discussion and details of results referenced in the main text.

A.5.1 Covariates and functional form

The main text uses the same set of baseline covariates for all regressions. To minimize any finite sample bias stemming from mis-specification, we specify baseline covariates as a set of indicator variables (dividing continuous variables into quartiles, with 0 as a separate

category, and dividing count variables into an indicator for 0 (left out), an indicator for 75th percentile and above, and then diving the remaining variation into roughly equal groups). The indicators are for: at least one arrest for a shooting or homicide; 1 or more than 1 shooting victimization; 1 or more than 1 other (non-violent) victimization; 1, 2-3, or more than 3 non-shooting (violent) victimizations; 1 or more than 1 other serious violent-crime arrests; 1-2 or more than 2 less serious violent-crime arrests; 1-2 or more than 2 property crime arrests; 1-2, 3-4, 5-8, or more than 8 drug crime arrests; 0-3, 4-7, 8-11, or more than 11 other arrests; quartiles 1, 2, 3, and 4 of days in jail during the past 30 months (0 as baseline category); quartiles 1, 2, 3, and 4 of days in prison during the past 30 months (0 as baseline category); quartiles 2, 3, and 4 of age; quartiles 2, 3, and 4 of baseline risk score, and an indicator for missing baseline risk score; and an indicator for having a race/ethnicity other than Black (including missing).

To ensure that results are not sensitive to the way covariates are included or functional form, Appendix Table A.3 reports several sets of alternative results. The first panel shows the main outcomes without any covariates, other than the randomization strata fixed effects. Although the results are slightly less precise, as expected when excluding covariates that explain some of the residual variation in the outcomes, they are nevertheless substantively quite similar to those referenced in the main text. The second panel shows results with covariates selected by the post-double selection LASSO (Belloni et al., 2014a,b), first partialling out the randomization strata fixed effects. The LASSO selects the indicator for having a missing risk score in the index regression, and does not select any covariates for any of the three index component regressions. Since the main components measures are count variables, the final panel shows results using a Poisson specification, with robust standard errors to relax the assumption that the mean equals the variance. To be comparable to the ITT reported in other panels, we show the average marginal effect from Poisson models that include the standard covariates (excluding month of randomization fixed effects to ensure models converge). The results are quite similar.

A.5.2 Scaling for incapacitation

Our main outcome measures are counts of serious violent-crime arrests and victimizations within the 20-month outcome period. The fact that people may be incapacitated and therefore unable to be involved in a serious violent crime, however, means that these counts capture a combination of individual behavior and "incapacitation," in this case either through incarceration or death. We pre-specified our interest in counts as outcomes, because in some respects the number of incidents are what matters most for the social cost of violence; if someone in the control group is incarcerated for the full outcome period, they are unable

to engage in violence. As long as we account for the social cost of incarceration in our benefit-cost comparison, we can ask whether the benefits of READI outweigh its costs.

As we noted in our pre-analysis plan, however, there is an additional question of substantive interest: whether READI changes behavior during the time someone is free to choose how to spend their time. In some respects, the results in Appendix Table A.8 (discussed below) suggest that adjusting for incapacitation should not matter much, since there is no significant difference in the number or percent of days the treatment and control groups are incapacitated. But it is possible that treatment heterogeneity could be masking differential incapacitation changes that affect the serious violence results.

Testing for program effects conditional on incapacitation requires a different analysis strategy. Survival analysis is a common way to assess whether time to initial incidents changes. But in our case, survival analysis is complicated by competing risks: we are most interested in serious violence outcomes, but individuals' data can be censored by other types of failures (incarceration for more minor arrests or probation/parole violations). Since these types of censoring are not ignorable—it is plausible READI could affect them—standard survival analysis can not help us to isolate changes in serious violence involvement alone.

An alternative is to scale the observed counts of violent incidents to account for the number of days an individual has free. Although we could do this directly by using rates as outcomes (number of incidents/number of days free), the results would be sensitive to outlier weights that result from people being incarcerated or killed almost immediately after randomization (i.e., that have a very small denominator). To avoid giving the cases with a single incident in a short period of time undue weight, we instead build a panel data set, where each row is a person-day in the post-randomization period during which a person is not incarcerated and not deceased, up to a maximum of 610 rows per person (corresponding to 20 months with 30.5 days per month). We regress each outcome on a treatment indicator and our usual baseline covariates and randomization strata, clustering standard errors on individual, and record the estimated coefficient and standard error scaled by 610. This strategy effectively upweights individuals for whom we observe more post-randomization information.

The results are in Appendix Table A.4. As expected given the lack of treatment effect on the number of days incapacitated, results are relatively similar to our main analysis of counts. The biggest difference is that the shooting and homicide victimization point estimate gets somewhat bigger, driven by the fact that homicide victimization is the source of some of the incapacitation.[10a]

[10a]Note that, for purposes of this analysis, we do not consider someone incapacitated following a non-fatal shooting, although some victims may be hospitalized following such incidents. We do this because we

A.5.3 COVID-19

As the main text describes, the onset of the COVID-19 pandemic brought dramatic changes to program delivery. Transitional jobs were stopped entirely from mid-March 2020 until August 2020, since in-person operations were not safe to continue. To prevent simply cutting off current READI participants, program operators continued to pay participants the average amount that they had earned over their last 3 weeks prior to the onset of the pandemic. CBT sessions shifted online, which likely diminished their quality given internet connection struggles, challenges finding privacy to discuss personal issues, and safety issues involved with potential disclosure of a person's location to members of rival groups (without the safety safeguards that READI operators had in place). COVID-19 also brought change to the crime environment in Chicago, with many types of crime and arrests decreasing as people stayed home, but a huge spike in gun violence.

Appendix Figure A.3 shows program retention as in the main text, but here including data from the COVID-19 period. The patterns are generally similar, with somewhat more fall-off in program participation during the end of the program, as COVID-19 deterred some individuals from returning.

Appendix Table A.5 reports READI's estimated impact separately in the pre- and post-COVID periods. We are largely under-powered to test the difference between the two periods, although the decline in shooting and homicide victimization is suggestively different (unadjusted for multiple testing), with a much larger protective effect during COVID-19. This change seems to be unique to potentially lethal victimizations, though, as the point estimate on all victmizations during COVID-19 is actually positive.

A.5.4 Spillovers

A priori, it is not clear what impact spillovers would have on the interpretation of our point estimates. If exposure to treated peers has the same effect regardless of one's own treatment status, then exposure should be balanced across treatment and control groups. In this case, the ITT might miss the net social effects of READI by failing to account for level shifts in both treatment and control groups. But the ITT would still be an unbiased estimate of the average difference between being offered treatment or not, inclusive of social spillovers. If being exposed to treated peers interacts with one's own treatment status, however, then the ITT could either over- or understate the net effects of READI. For example, if members of the control group pick up the guns that the treatment group puts down, then the displacement would lead the ITT to overstate the net benefits of READI. Alternatively, if the control group

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observe cases in the data where a non-fatal shooting victim is involved in another incident within days of being shot. We are trying to obtain hospital records which should allow us to measure this kind of incapacitation directly.

learns some of the treatment group's positive behaviors, or if the drop in violence among treated men cools off what would have been an escalating cycle of violence in a neighborhood, then the ITT may understate the net benefits of READI.

As discussed in the main text, the evidence in Craig et al. (2022) provides some weak evidence that the ITT effect on serious violent-crime arrests that are not shootings or homicides may be an inflated measure of READI's direct effect (i.e., that the actual direct effect on these kinds of arrests could be negative despite the positive ITT estimate, due to differential spillovers onto treatment versus control peers of treated individuals). But the clearest evidence of a SUTVA violation is for drug-crime arrests, where it appears that READI actually decreases these arrests both among those directly treated and among controls who are exposed to treated peers.

A.5.5 Dynamic treatment effects

Appendix Figure A.4 shows how treatment effects accrue over time during the 20-month outcome period. The top left panel shows cumulative participation rates, and the other panels show cumulative ITT effect estimates. The estimated decline in shooting and homicide arrests accrues over time, becoming statistically significant at about the year mark. The estimated decline in shooting and homicide victimization, while noisier, follows a similar pattern.

A.5.6 Incapacitation as mechanism

To test whether keeping people busy during the workday mechanically reduced violence during that time, we separately code violent incidents based on the day and time they occur (measuring the time of the incident rather than the time of an arrest). Appendix Table A.6 shows results separately for incidents that occur during the work day (8am–6pm Monday through Friday), weekend (Friday 6pm–Sunday 11:59pm), and weekday mornings and nights (Monday through Friday 6pm–8am). While point estimates on the total number of arrests and victimizations are negative and substantively large during work days, there is no indication that declines in serious violence involvement are concentrated during the work day. Ignoring multiple testing issues, arrests for non-shooting violent crimes actually increase during that time.

Instead, the decline in arrests for shootings and homicides are driven by declines in weekend incidents, which is when most of these incidents occur. Though the small number of violent events in each cell makes this analysis noisy, all point estimates for weekend violence are negative, and the arrest decline is statistically significant ($p = 0.003$). We conclude that it is not the incapacitation effect of READI activities that decreases shooting and homicide arrests. Rather, the treatment group appears to change its behavior outside

the work day. Appendix A.5.7 discusses incapacitation for outcomes other than our main violence measures.

A.5.7 Other outcomes

The three main components of our primary index are shooting and homicide victimizations, shooting and homicide arrests, and other serious violent-crime arrests. Appendix Table A.7 separates each of these components into smaller categories, with the top panel showing counts and the bottom panel showing indicators for whether the outcome ever happened during the outcome period. We did not design the study to have enough power to separately identify effects on these smaller subcategories; homicide arrests are the only category with statistically significant effects, ignoring multiple testing adjustments. Nonetheless, the table is useful for showing what is underlying the effects reported in the main text.

Fatal and non-fatal shooting victimizations, homicide arrests, and aggravated assault and battery arrests all have negative point estimates. The reason other serious violent-crime has a positive point estimate is that the robbery and sexual assault point estimates are positive and proportionally large. The control means reflect lower clearance rates for these less serious crimes, with more homicide arrests than robbery arrests (0.022 versus 0.015), despite robberies being much more common. The result has too little power for us to be confident in whether these crimes are actually increasing. Results are quite similar in the bottom panel, which uses indicator variables rather than counts as the dependent variables.

These measures of serious violence involvement were our primary outcomes of interest. But we also pre-specified a secondary interest in other types of crime, as well as incarceration (and, for understanding potential censoring, days lost to homicide). Appendix Table A.8 shows these results. A few results have large enough point estimates to approach statistical significance on their own, with p-values between 0.1 and 0.22 for a decline in drug arrests and in days incapacitated, with a more precise decline in days lost to homicide (consistent with the large negative point estimate in shooting and homicide victimizations). Given the number of tests in this table, however, the general take-away should likely be that READI did not seem to affect these other measures of criminal involvement.

Appendix Table A.9 breaks down the non-primary arrest and victimization outcomes by time of day. As mentioned in the main text, there is some indication that work day incapacitation plays a role in reducing drug crimes; during the work day, for every 100 people there are 4.5 fewer arrests for drug crimes among the treatment group than the control group (a 27 percent decline, unadjusted $p = 0.02$). It is possible that some of the work day drop in drug crime arrests has to do with policing strategies. If police make arrest sweeps of drug markets during the day on weekdays, READI participants may be less likely to be caught

up in those sweeps even if their involvement in drug crime moves to other times. However, point estimates on drug arrests at other times are either very small (weekend) or negative (weekday morning and night). So the work day incapacitation does not seem to be purely a shift in when drug crimes and police enforcement happen. Other (non-shooting) violent victimizations also fall during the work day by 2.8 per 100 people (27 percent, unadjusted $p = 0.06$), consistent with the possibility that READI keeps the treatment group safer during the work day.

The only indication of significant substitution of criminal behavior to other times is an increase in less serious violent-crime arrests during the weekend (2.1 additional arrests per 100 people assigned to treatment, a 33 percent increase, unadjusted $p = 0.09$). Although this is balanced by negative point estimates at other times of the week, it could be consistent with the basic pattern of substitution from more to less serious violence seen in the primary index components.

A.5.8 Heterogeneity

The main text focuses on heterogeneity by pathway and baseline risk, which were built into our randomization and experimental design, as well as the interaction of the two. Our pre-analysis plan also pre-specified an interest in heterogeneity by geography and age (along with an acknowledgement that analysis would be exploratory since we would likely lack power to distinguish effects across groups).

Appendix Tables A.10 and A.11 report results for our primary index and its components by these groups. An F-test for whether the primary index varies by group finds significant heterogeneity by neighborhood ($p = 0.08$), but not for age ($p = 0.645$). Appendix Table A.10 shows that the neighborhood heterogeneity is driven by larger declines in two neighborhoods (Austin/West Garfield and Greater Englewood), but a positive point estimate in the third (North Lawndale). We are cautious about telling too strong a narrative about these results, since many factors—program providers, population, levels of violence, and policing, among others—all varied across neighborhoods.

One potential hypothesis for future exploration, based on our qualitative work, is that when North Lawndale's program started, many of the initial treatment participants were from one particular "clique" or "crew" in that neighborhood. As recruitment broadened, ongoing conflicts in the neighborhood may have spilled over into program time. It is possible that the potentially adverse effects in North Lawndale reflect the importance of managing local conflicts as part of the program's delivery, and the difficulty of running a program with members of groups that are currently in conflict with each other.

A.5.9 Assessing role of observables in treatment heterogeneity

To further explore the role of observables in explaining the pathway heterogeneity, beyond just the single observables in the previous subsection, we use the relationship between observables and outcomes in the algorithm and re-entry pathways to estimate what treatment effects would look like for each of those groups if they had the mean observables of the outreach pathway. We follow the simple correction on observables procedure in Andrews and Oster (2017). The idea is to assess whether making the other pathways look more like the outreach pathway on observables also makes the estimated ITT effects look more similar across pathways, i.e., if observable differences explain the differences in treatment effects.

We start by enforcing common support. We start by estimating the probability of being an outreach referral based on observables. To do so, using logistic regression we regress an indicator for being in the outreach pathway on our standard set of baseline covariates (without strata fixed effects). We then calculate predicted values from the regression to generate for each individual $\hat{p}_i^{outreach}$. We then plot the distributions of $\hat{p}_i^{outreach}$ for each pathway and use visual inspection to identify the ranges of values where there is sufficient density and overlap among algorithm and re-entry referrals, respectively, and outreach referrals. This process results in restricting algorithm and outreach referrals to those with $\hat{p}_i^{outreach} < 0.55$ (dropping 45 algorithm and 393 outreach referrals), and restricting re-entry and outreach referrals to those with $\hat{p}_i^{outreach} < 0.8$ (dropping 7 re-entry and 95 outreach referrals).

We then calculate what Andrews and Oster call T_i by reweighting each individual's observed Y_i by $1/D_i$ for treatment individuals and $-1/(1-D_i)$ for control individuals (where D_i is the probability of treatment within i's stratum). The average value of T_i within a referral pathway, $\bar{T}$, is the estimated ITT. The goal is to estimate versions of $\bar{T}$ for algorithm and re-entry referrals that are covariate-adjusted to resemble outreach referrals. To do this, we regress T_i on our baseline covariates separately for algorithm and re-entry referrals, then multiply the resulting coefficient estimates by the average values of the baseline covariates among the outreach referrals. Finally, we calculate the resulting adjusted versions of $\bar{T}$ for algorithm and re-entry referrals.

These $\bar{T}$s for algorithm and re-entry referrals, both unadjusted (OLS ITT) and adjusted (AO ITT), are reported in Appendix Table A.12. The covariate adjustment does very little to close the gap in the ITT estimates between the algorithm and re-entry pathways and the outreach pathway. Overall, these results suggest that observables have relatively little role in explaining treatment heterogeneity by pathway.

A.6 Details on benefit-cost comparison

Assigning dollar values to the harm generated by crime is not a straightforward task. It involves a number of complicated conceptual and ethical issues, including how to value the loss of life. Our analysis is motivated by the excellent discussion of these issues in Dominguez and Raphael (2015), which emphasizes the uncertainty inherent in the estimates, along with the care with which policymakers should use benefit-cost comparisons as an input into—but certainly not the sole input into—decision-making.

We do not aim to conduct a comprehensive benefit-cost analysis. In particular, we focus on the social costs of crime, setting aside other potential benefits and costs of the program: the opportunity cost of the dollars that fund the program, the work that program staff might have done in the absence of their jobs on READI, the benefits of investing in under-served neighborhoods, the value of the work READI participants did, other unmeasured benefits of the program, and so forth. We do not mean to trivialize the potential importance of these benefits and costs. But there is little empirical work to help guide our thinking on the social value of these aspects of the program, and we have few good measures of how much they changed. As such, assigning dollar values would necessarily involve a huge amount of speculation and extrapolation.

By taking a narrower focus on the costs of crime, we can rely on a longstanding literature that estimates the social harm from victimization, as well as the cost of running the criminal legal system and punishing offenders (although even these latter costs may be still be underestimated, since the broader harms that can come from policing, criminal records, and incarceration are an active area of ongoing research). This narrower focus is not intended to generate comprehensive estimates, and has a number of real limitations. But it also provides a concrete way to start to quantify the value of programs that aim to reduce violence, which as Dominguez and Raphael (2015) argue, should be a central input into decisions about how to direct resources.

Our aim in this section is to provide transparency about how these estimates are constructed, what is and is not included, and how sensitive the estimates are to the biggest outlier in terms of social costs: the cost of a lost life.

A.6.1 Cost of crime estimates

The literature on estimating the social harm from crime typically takes one of two different approaches. The first is a "bottom-up" approach, which uses observable prices from pieces of the harm a specific crime generates—medical and legal bills, mental health and social services, rising insurance costs, jury awards for pain and suffering, and the like—and adds them up to approximate the direct costs of different kinds of victimization. The seminal

estimates of this approach come from Miller (1996); we use the updated version of their estimates reported in Cohen and Piquero (2009), inflated to 2017 dollars.

The second is a "top-down" approach, which uses contingent valuation surveys to estimate a broader willingness to pay (WTP) to avoid different types of crime. These WTP measures in theory capture a broader set of social harms that what can be measured in the bottom-up approach, including the investments people make to avoid crime in the first place. In part for this reason, they tend to be somewhat higher than bottom-up estimates. We use the WTP measures from Cohen and Piquero (2009), inflated to 2017 dollars. Note that when people report their WTP to avoid an assault, for example, they are theoretically including the tax burden involved in arresting, trying, and punishing offenders as well as the productivity loss from incarceration. To avoid double-counting these pieces, we use the WTP measures in Cohen and Piquero (2009) after subtracting the bottom-up measures of legal costs and offender productivity loss.

The first 5 columns of Appendix Table A.13 report the dollar figures that correspond to both types of cost estimates by crime type. Note that these dollar figures represent a rough consensus from the literature, although there is certainly variation in the details (see, e.g., Chalfin, 2015). For this reason, along with the conceptual issues about what types of costs should be included, the standard errors in Table 7 under-estimate the amount of true uncertainty in our estimates.

A.6.2 Details on calculations

The left panel of Table 7 attempts to make a series of more conservative decisions, which likely understates the true harm from crime, while the right panel makes a series of more inclusive decisions which may come closer to estimating the true amount of harm, but are necessarily more speculative. The two sets of estimates can help give a sense for how much some of the measurement issues surrounding our outcomes might matter.

In both sets of estimates, we assign to each arrest and victimization of a READI study member an estimated cost of crime depending on its type.[11a] In the more conservative estimates, we use the lower bottom-up costs of crime. We also limit the calculation to incidents that appear as a victimization or an arrest in our data. That is, we only count a victimization as occurring if it is reported in our victimization data as having a victim in the READI study sample. For shooting and homicide victimizations, reporting rates are close

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[11a]Though an arrest may be for an offense that resulted in multiple people being victimized, data limitations prevent us from reliably measuring the number of victims associated with an arrest. As a result, we assign a single cost to each arrest and victimization in the data, which is standard in the cost of crime literature. While this will underestimate the social costs associated with some arrests (those with multiple victims per arrestee), it will overestimate the social costs associated with other arrests (those with multiple arrestees per victim).

to 100 percent, so this should be reasonably close to a complete measure. Lesser crimes are not as consistently reported to the police, so these counts understate the amount of actual victimization that occurs.

The more conservative estimates also only count an offense as occurring if it is reported in our arrest data with an arrestee in the READI study sample. This may overstate actual offending in cases where the person arrested did not actually commit the crime. But the magnitude of this overstatement is likely quite a bit smaller than the underreporting stemming from the fact that most offenses are not cleared—i.e., they do not result in an arrest. Clearance rates are often quite a bit lower than reported crime rates, so on net we expect arrest counts to understate the amount of offending that actually occurs among the READI sample. One implication of only counting offenses that appear as arrests in our data is that the conservative estimates do not include the costs of the legal system for those are arrested for victimizing a READI sample member, but are not in the READI sample themselves.

The more inclusive cost of crime estimates use WTP measures of the social harm of each crime type. They also make adjustments for the underreporting of victimizations to police and the fact that most offenses do not result in an arrest. To make these adjustments, we use the crime-specific reporting rates (call that rate R) from the Bureau of Justice Statistics (Morgan and Thompson, 2021). We calculate separate reporting rates for 2019 and 2020, then average these two values to estimate the overall reporting rate.

For shootings and homicides, we use clearance rates that are specific to our context, calculated from the Chicago Police Department data. We do not have clearance rate estimates for other crimes, so we use prior work that has estimated the number of crimes that actually occur for every observed arrest (call that rate C). In particular, we use the midpoint of the range given in the Belfield et al. (2006) benefit-cost analysis of Perry Preschool, giving preference to felony rates when available. The resulting victimization and clearance rates are reported in the last two columns of Appendix Table A.13.

To be clear on what enters each row of Table 7: The "READI Sample Victims" row assigns social costs to each incident where someone in the READI sample was reported as a victim in the data. The more inclusive estimates inflate each victimization by the average reporting rate for each kind of incident, and use the WTP social costs. The "READI Sample Offenders" row assigns social costs to each incident where someone in the READI sample was arrested. The more inclusive estimates inflate by both the clearance rate for each type of arrest (to account for reported offenses that do not result in an arrest), as well as the victimization reporting rates (to account for offenses by READI offenders that do not get reported to the police and so could not feasibly be cleared by an arrest).

We assign the average legal system cost and productivity loss from incarceration for each

type of crime. The more conservative estimates assign those costs to each observed arrest only. The more inclusive estimates add the legal and incarceration costs of the arrests that are implied by READI victimizations. That is, they assume that when a READI sample member is victimized, someone else is arrested at a rate equal to the clearance rate for that crime type.

It is not straightforward to calculate the exact cost per participant (or per random assignee), as program operators lump fixed and variable costs together, and use budget periods that have varying numbers of participants cycling through them. In addition, the COVID-19 pandemic shifted costs around in unexpected ways. We currently only have time windowed cost information pre-COVID, so our estimate of the administrative cost of the program is only from the pre-COVID period. To approximate the administrative costs of the program, we calculate average monthly spending on everything for the program (staff, participant wages, legal and insurance costs, and other operations). We then calculate the average number of participant-months during that period, use that to calculate the average monthly participant cost, and multiply by 20 to match the costs accrued during the average 20-month period. Note that this is not quite what we would want in principle, since it includes fixed start-up costs (spread only over the pre-COVID period), and the early part of the program may not be representative of the spending for the program as a whole.

We also only have an approximation of how many of these costs went directly to participants as a transfer via wages and CBT/training stipends. For these, we use approximations from the payroll data, which have their own imperfections. Given the measurement error, we have used what we believe is a lower-bound of payments to participants so that READI's net cost appears as high as possible (making it harder to find a positive benefit-cost ratio).

A.7 Appendix Tables and Figures

Table A.1: Baseline balance within pathways

	Algorithm			Outreach			Re-entry		
	Control Mean	Treatment Mean	Pairwise p-value	Control Mean	Treatment Mean	Pairwise p-value	Control Mean	Treatment Mean	Pairwise p-value
N	616	616		438	440		178	168	
Demographics									
Black	0.964	0.963	0.879	0.986	0.986	0.972	0.943	0.963	0.488
Age	24.8	24.4	0.136	25.6	25.6	0.89	26.1	26.7	0.53
Primary Outcome Components, Counts									
Shooting Victimizations	0.666	0.594	0.112	0.274	0.300	0.492	0.331	0.214	0.048
Shooting & Homicide Arrests	0.086	0.075	0.467	0.068	0.059	0.563	0.079	0.107	0.381
Other Serious Violent-Crime Arrests	1.1	0.950	0.057	0.637	0.691	0.45	0.978	1.0	0.834
Risk Prediction									
Risk Score	0.137	0.137	0.881	0.089	0.089	0.911	0.080	0.079	0.875
Missing Risk Score	0	0		0.212	0.155	0.027	0.225	0.179	0.189
Arrest Counts									
All Arrests	20.1	20.7	0.42	13.2	14.1	0.253	15.9	16.4	0.883
Less Serious Violent-Crime Arrests	1.9	2.0	0.626	1.1	1.1	0.984	1.5	1.3	0.264
Drug Crime Arrests	5.5	6.0	0.087	4.0	4.2	0.449	4.6	5.0	0.598
Property Crime Arrests	1.8	1.8	0.97	1.2	1.4	0.291	1.7	1.7	0.994
Other Crime Arrests	9.6	9.8	0.697	6.1	6.5	0.293	7.0	7.2	0.896
Victimization Counts									
All Victimizations	4.7	4.3	0.203	2.3	2.3	0.937	2.3	2.3	0.903
Other (Non-Shooting) Violent Victimizations	3.3	3.0	0.34	1.6	1.6	0.943	1.5	1.6	0.638
Non-Violent Victimizations	0.747	0.709	0.619	0.390	0.393	0.969	0.449	0.446	0.925
Incarceration Measures									
Days Incarcerated, Past 30 Months	134.8	123.7	0.303	156.9	153.3	0.818	372.4	411.9	0.296
Incarcerated at Randomization	0.050	0.049	0.892	0.037	0.023	0.214	0.022	0.048	0.197
Joint Test									
p-value on F-test			0.141			0.819			0.157

Notes: Pairwise p-value from test of treatment-control difference using heteroskedasticity-robust standard errors and controlling for randomization strata fixed effects. Arrest and victimization measures include all available CPD data from 1999 (2010 for shooting victimizations) through the time of randomization, with counts winsorized at the top 99th percentile. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. Less serious violent-crime arrests include simple assault and battery. Other (non-shooting) violent victimizations include sexual assault, robbery, aggravated assault and battery, and simple assault and battery. Non-violent victimizations include all other incidents such as burglary, stalking, and threats. Risk score is missing for 231 individuals who did not have at least one arrest or two victimizations within the 50 months prior to randomization (non-missing risk score Ns for algorithm pathway = 1,232, for outreach pathway = 717, and for re-entry pathway = 276). Race is missing for 38 individuals (non-missing race Ns for algorithm pathway = 1,232, for outreach pathway = 847, and for re-entry pathway = 339). Joint test excludes all arrests and all victimizations since they are linear combinations of other variables.

Table A.2: Participants' earnings and hours, by pathway and time period relative to the pandemic

	Work		Trainings		CBT/PD		Total	
	Earnings	Hours	Earnings	Hours	Earnings	Hours	Earnings	Hours
All Participants								
Pre-COVID	$5,658	457	$121	8	$1,695	102	$7,474	567
Standby Pay	$1,210	90	$1	0	$333	20	$1,544	110
Post-COVID	$172	12	$247	16	$213	13	$631	41
Algorithm								
Pre-COVID	$5,574	449	$99	7	$1,667	100	$7,340	555
Standby Pay	$842	63	$1	0	$278	17	$1,121	79
Post-COVID	$76	5	$139	9	$160	10	$374	24
Outreach								
Pre-COVID	$6,567	535	$162	11	$1,972	118	$8,701	664
Standby Pay	$935	69	$1	0	$298	18	$1,235	87
Post-COVID	$137	10	$219	15	$173	10	$529	35
Re-entry								
Pre-COVID	$3,433	269	$60	4	$947	57	$4,441	329
Standby Pay	$2,730	204	$1	0	$545	33	$3,276	236
Post-COVID	$468	33	$553	37	$437	26	$1,459	96

Notes: Take-up defined as attending the first day of READI orientation. Earnings and hours are limited to men who took up and appear in the payroll data. This excludes 124 men who took up, most of whom did so prior to consent forms to allow the release of their payroll data being distributed. The maximum possible hours someone could have participated in both a job (29.5 hours/week) and CBT/trainings (7.5 hours/week) over 18 months was 2,664 hours, although those in the program during COVID-19 had a lower maximum. Work earnings and hours correspond to time spend at a worksite. CBT/trainings includes time spent in group CBT sessions, professional development (PD) sessions, and online trainings. From March 2020 to July 2020, participants received weekly standby pay (included in "work" earnings), which was calculated from the average weekly earnings in the month prior to COVID. Beginning in August 2020, participants were given the option to return to in-person work or complete online trainings and PD, which most participants opted to do (see Appendix A.5.3). Earnings for CBT/PD before March 2020 are extrapolated based on available data. Pre-COVID period includes wages and hours worked from 10/06/17 to 3/16/20 (~127 weeks). Standby pay period includes wages from 3/17/20 to 8/10/20 (21 weeks), and was calculated based on each participant's average weekly wages from the month prior to COVID. Post-COVID period includes wages from 8/11/20 to 10/08/21 (~66 weeks).

Table A.3: Robustness to alternative specifications

	CM	ITT/AME	P-value
OLS: No covariates except randomization blocks			
Primary Index of Serious Violence	0	-0.0268	0.252
		(0.0234)	
Shootings & Homicide Victimizations	0.1144	-0.0099	0.465
		(0.0136)	
Shootings & Homicide Arrests	0.0260	-0.0107	0.066
		(0.0058)	
Other Serious Violent-Crime Arrests	0.0544	0.0035	0.733
		(0.0101)	
OLS: Covariates selected using post-double selection LASSO			
Primary Index of Serious Violence	0	-0.0306	0.180
		(0.0228)	
Shootings & Homicide Victimizations	0.1144	-0.0099	0.451
		(0.0132)	
Shootings & Homicide Arrests	0.0260	-0.0107	0.058
		(0.0057)	
Other Serious Violent-Crime Arrests	0.0544	0.0035	0.725
		(0.0098)	
Poisson: Standard covariates			
Shootings & Homicide Victimizations	0.1144	-0.0104	0.442
		(0.0135)	
Shootings & Homicide Arrests	0.0260	-0.0108	0.086
		(0.0063)	
Other Serious Violent-Crime Arrests	0.0544	0.0010	0.918
		(0.0099)	

Notes: N = 2,456. First panel shows main outcome results without covariates, other than the randomization strata fixed effects. Second panel shows main outcome results with covariates selected using the post-double selection LASSO (Belloni et al., 2014a,b), first partialling out the randomization strata fixed effects. Third panel shows average marginal effects from Poisson regressions with standard covariates but excluding randomization strata fixed effects for convergence. All regressions estimate heteroskedasticity-robust standard errors.

Table A.4: READI's estimated effects, adjusting for incapacitation

	Estimates				P-values	
	CM	ITT	CCM	TOT	Observed ITT	FDR-q
Primary Outcome Components, Counts						
Shooting & Homicide Victimizations	0.1320	-0.0152	0.1350	-0.0271	0.3288	0.4384
Shooting & Homicide Arrests	0.0302	-0.0142	0.0387	-0.0254	0.0394	0.1575
Other Serious Violent-Crime Arrests	0.0604	0.0034	0.0587	0.0061	0.7711	0.7711

Notes: N = 2,441. Treatment effects estimated with a person-day panel including only post-randomization observations where study members are neither incarcerated nor deceased. 15 study members are incapacitated for the entirety of the post-randomization period. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors clustered by individual reported in parentheses. Reported means and coefficients are scaled to represent the standard 20-month follow-up period (multiplied by 610 days). CM shows the control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression.

Table A.5: READI's estimated effects, pre- and post-COVID

	CM		ITT			P-values		
	Pre-COVID	Post-COVID	Treatment Pre-COVID	Treatment Post-COVID	Difference	Treatment Pre-COVID	Treatment Post-COVID	Difference
Primary Outcome Components, Counts								
Shooting & Homicide Victimizations	0.1008	0.1570	0.0003	-0.0473	-0.0476	0.982	0.115	0.154
			(0.0144)	(0.0300)	(0.0334)			
Shooting & Homicide Arrests	0.0268	0.0234	-0.0146	-0.0012	0.0134	0.027	0.919	0.330
			(0.0066)	(0.0119)	(0.0138)			
Other Serious Violent-Crime Arrests	0.0568	0.0468	0.0070	-0.0061	-0.0131	0.552	0.727	0.543
			(0.0117)	(0.0174)	(0.0215)			
All Events, Counts								
All Arrests	1.8818	0.9619	-0.0196	-0.1714	-0.1518	0.843	0.080	0.256
			(0.0985)	(0.0979)	(0.1337)			
All Victimizations	0.5404	0.4776	-0.0154	0.0074	0.0229	0.758	0.913	0.783
			(0.0500)	(0.0679)	(0.0831)			

Notes: N = 2,456. Treatment effects estimated with a person-day panel including post-randomization observations only. Regressions include a post-COVID indicator, an indicator for a treated person-day, and the interaction of the two, in addition to baseline covariates and randomization strata fixed effects. The pre-COVID ITT is the estimated coefficient on the treated person-day indicator. The post-COVID ITT is the linear combination of the estimated coefficients on the treated person-day indicator and its interaction with the post-COVID indicator. The difference is the estimated coefficient on the interaction of the treated person-day and post-COVID indicators. Heteroskedasticity-robust standard errors clustered by individual reported in parentheses. Reported means and coefficients are scaled to represent the standard 20-month follow-up period (multiplied by 610 days). The pre-COVID period runs through 3/15/20. The post-COVID period runs starts after 3/15/20. CM shows the control means; ITT shows the intent-to-treat estimates.

Table A.6: READI's estimated effects, by time of the week

	Work Hours (Mon-Fri 8am-6pm)			Weekend (Fri 6pm - Sun 11:59pm)			Weekday Mornings and Nights (Mon-Fri 6pm-8am)		
	CM	ITT	P-value	CM	ITT	P-value	CM	ITT	P-value
Primary Outcome Components, Counts									
Shootings & Homicide Victimizations	0.031	-0.002	0.778	0.059	-0.005	0.593	0.024	-0.004	0.517
		(0.007)			(0.010)			(0.006)	
Shootings & Homicides Arrests	0.006	0.001	0.708	0.016	-0.012	0.003	0.004	-0.000	0.962
		(0.003)			(0.004)			(0.003)	
Other Serious Violent-Crime Arrests	0.015	0.007	0.232	0.023	-0.006	0.356	0.016	0.003	0.592
		(0.006)			(0.006)			(0.005)	
All Events, Counts									
All Victimizations	0.183	-0.021	0.329	0.205	0.018	0.432	0.138	-0.007	0.674
		(0.021)			(0.023)			(0.017)	
All Arrests	0.604	-0.057	0.179	0.607	0.016	0.676	0.447	-0.016	0.634
		(0.042)			(0.038)			(0.033)	

Notes: N = 2,456. Weekdays, 6pm - 8am does not include events that occur after 6pm on Friday. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. CM shows the control means; ITT shows the intent-to-treat estimates. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses.

Table A.7: READI's estimated effects on primary outcome sub-components and binary outcomes

	CM	ITT	ITT P-value	CCM	TOT
Counts					
Shooting & Homicide Victimization					
Homicide Victimizations	0.025	-0.004 (0.006)	0.523	0.025	-0.007 (0.011)
Non-fatal Shooting Victimizations	0.089	-0.007 (0.012)	0.552	0.093	-0.013 (0.022)
Shooting & Homicide Arrests					
Homicide Arrests	0.022	-0.012 (0.005)	0.020	0.033	-0.022 (0.009)
Non-fatal Shooting Arrests	0.004	0.001 (0.003)	0.757	0	0.002 (0.005)
Other Serious Violent-Crime Arrests					
Aggravated Assault & Battery (Non-Shooting)	0.032	-0.004 (0.007)	0.512	0.041	-0.008 (0.012)
Robbery Arrests	0.020	0.005 (0.007)	0.479	0.013	0.009 (0.012)
Sexual Assault Arrests	0.002	0.003 (0.002)	0.177	0	0.006 (0.004)
Indicators					
Shooting & Homicide Victimization					
Ever: Homicide Victimizations	0.025	-0.004 (0.006)	0.523	0.025	-0.007 (0.011)
Ever: Non-fatal Shooting Victimizations	0.083	-0.005 (0.011)	0.671	0.086	-0.009 (0.019)
Shooting & Homicide Arrests					
Ever: Homicide Arrests	0.021	-0.011 (0.005)	0.025	0.031	-0.021 (0.009)
Ever: Non-fatal Shooting Arrests	0.004	0.001 (0.003)	0.757	0	0.002 (0.005)
Other Serious Violent-Crime Arrests					
Ever: Aggravated Assault & Battery (Non-Shooting)	0.032	-0.004 (0.007)	0.512	0.041	-0.008 (0.012)
Ever: Robbery Arrests	0.017	0.004 (0.005)	0.445	0.012	0.008 (0.010)
Ever: Sexual Assault Arrests	0.002	0.003 (0.002)	0.177	0	0.006 (0.004)

Notes: N = 2,456. Top panel shows effects on the sub-categories of arrest counts that comprise each component of the primary index. The bottom panel includes the same outcomes, but measured as binary indicators rather than counts. CM shows the control means; ITT shows the intent-to-treat estimates; CCM shows the control complier means, rounded to 0 when estimate is negative due to sampling error; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses.

Table A.8: READI's estimated effects on other outcomes

	CM	ITT	ITT P-value	CCM	TOT
Other Arrest & Victimization					
Counts					
Less Serious Violent-Crime Arrests	0.179	-0.003 (0.020)	0.888	0.176	-0.005 (0.036)
Property Crime Arrests	0.091	-0.006 (0.018)	0.757	0.091	-0.010 (0.031)
Drug Crime Arrests	0.373	-0.051 (0.036)	0.153	0.454	-0.094 (0.063)
Other Crime Arrests	0.935	0.011 (0.055)	0.846	0.950	0.020 (0.097)
Other (Non-Shooting) Violent Victimizations	0.285	-0.012 (0.032)	0.719	0.314	-0.021 (0.056)
Indicators					
Less Serious Violent-Crime Arrests	0.134	0.009 (0.014)	0.493	0.129	0.017 (0.024)
Property Crime Arrests	0.067	0.001 (0.010)	0.895	0.060	0.002 (0.018)
Drug Crime Arrests	0.220	-0.024 (0.016)	0.130	0.258	-0.045 (0.029)
Other Crime Arrests	0.485	-0.002 (0.019)	0.902	0.510	-0.004 (0.034)
Other (Non-Shooting) Violent Victimizations	0.163	0.001 (0.015)	0.937	0.176	0.002 (0.026)
Incarceration & Incapacitation					
Counts					
Percent of Days Incapacitated	0.140	-0.011 (0.009)	0.217	0.119	-0.020 (0.016)
Days Available in Follow-up	524.9	6.7 (5.440)	0.217	537.5	12.3 (9.583)
Days Incapacitated	85.1	-6.715 (5.440)	0.217	72.5	-12.326 (9.583)
Days Lost to Homicide	8.2	-3.413 (1.953)	0.081	9.0	-6.266 (3.446)
Days Incarcerated, Past 30 Months	76.9	-3.301 (5.209)	0.526	63.5	-6.060 (9.186)
Days in Jail (CCSO)	40.8	0.071 (3.491)	0.984	33.8	0.131 (6.163)
Days in Prison (IDOC)	36.1	-3.373 (3.467)	0.331	29.7	-6.191 (6.110)
Indicators					
Incapacitated	0.485	-0.009 (0.018)	0.615	0.462	-0.017 (0.033)
Incarcerated	0.470	-0.006 (0.018)	0.735	0.449	-0.012 (0.033)
Ever in Jail (CCSO)	0.451	-0.017 (0.019)	0.372	0.442	-0.030 (0.033)
Ever in Prison (IDOC)	0.210	-0.009 (0.015)	0.519	0.200	-0.017 (0.026)

Notes: N = 2,456. Table shows READI's estimated effects on secondary outcomes (counts and indicators) that measure other involvement in crime and violence. Days Incapacitated measure days during which an individual was either incarcerated or deceased. Days Incarcerated separates incarceration in the local jail (Cook County Sheriff's Office) and in a state prison (Illinois Department of Corrections). CM shows control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses.

Table A.9: READI's estimated effects on other outcomes, by time of the week

	Work Hours (Mon-Fri 8am-6pm)			Weekend (Fri 6pm - Sun 11:59pm)			Weekday Mornings and Nights (Mon-Fri 6pm-8am)		
	CM	ITT	P-value	CM	ITT	P-value	CM	ITT	P-value
Other Arrest & Victimization Counts									
Less Serious Violent Crime Arrests	0.054	-0.005	0.578	0.064	0.021	0.086	0.060	-0.018	0.096
		(0.010)			(0.012)			(0.011)	
Property Crime Arrests	0.028	0.009	0.360	0.032	-0.004	0.628	0.031	-0.011	0.161
		(0.010)			(0.008)			(0.008)	
Drug Crime Arrests	0.164	-0.045	0.015	0.127	0.008	0.681	0.083	-0.014	0.292
		(0.019)			(0.019)			(0.013)	
Other Crime Arrests	0.337	-0.023	0.425	0.345	0.010	0.716	0.253	0.025	0.311
		(0.029)			(0.026)			(0.024)	
Other (Non-Shooting) Violent Victimizations	0.105	-0.028	0.059	0.098	0.021	0.212	0.082	-0.005	0.739
		(0.015)			(0.017)			(0.014)	

Notes: N = 2,456. Weekdays, 6pm - 8am does not include events that occur after 6pm on Friday. CM shows control mean; ITT shows the intent-to-treat estimates. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses.

Table A.10: READI's estimated effects on serious violence involvement, by neighborhood

	CM	ITT	CCM	TOT	Observed ITT	FWER	FDR-q
			Estimates			**P-values**	
Primary Index of Serious Violence by Neighborhood							
Austin/West Garfield Park	0.0179	-0.0742 (0.0379)	0.0574	-0.1300 (0.0644)	0.051	0.145	0.152
Greater Englewood	0.0029	-0.0574 (0.0390)	0.0334	-0.1071 (0.0695)	0.141	0.261	0.211
North Lawndale	-0.0211	0.0449 (0.0431)	-0.0431	0.0853 (0.0789)	0.298	0.296	0.298
Primary Outcome Components by Neighborhood, Counts							
Austin/West Garfield Park (N = 852)							
Shootings & Homicides Victimizations	0.1119	-0.0178 (0.0211)	0.1142	-0.0311 (0.0356)	0.398	0.633	0.534
Shootings & Homicides Arrests	0.0350	-0.0213 (0.0105)	0.0456	-0.0373 (0.0177)	0.042	0.118	0.125
Other Serious Violent-Crime Arrests	0.0559	-0.0101 (0.0162)	0.0675	-0.0178 (0.0274)	0.534	0.633	0.534
Greater Englewood (N = 775)							
Shootings & Homicides Victimizations	0.1140	-0.0250 (0.0238)	0.1323	-0.0466 (0.0425)	0.293	0.498	0.440
Shootings & Homicides Arrests	0.0207	-0.0149 (0.0084)	0.0326	-0.0279 (0.0151)	0.077	0.210	0.230
Other Serious Violent-Crime Arrests	0.0648	-0.0021 (0.0188)	0.0563	-0.0039 (0.0335)	0.913	0.912	0.913
North Lawndale (N = 829)							
Shootings & Homicides Victimizations	0.1175	0.0088 (0.0255)	0.1077	0.0167 (0.0467)	0.729	0.928	0.814
Shootings & Homicides Arrests	0.0216	0.0026 (0.0111)	0.0182	0.0049 (0.0203)	0.814	0.928	0.814
Other Serious Violent-Crime Arrests	0.0432	0.0230 (0.0177)	0.0390	0.0439 (0.0323)	0.192	0.472	0.577

Notes: N = 2,456. Estimates for each outcome are from a single regression that interacts neighborhood indicators with treatment. Primary index standardizes each of the three components shown in the bottom panel based on the control group's distribution and takes their unweighted average. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. For multiple hypothesis testing adjustments within the primary index, we define the three neighborhoods as a family. For the component adjustments, we define the three different outcomes within each neighborhood as a family. FWER p-values control for the family-wise error rate using the Westfall and Young (1993) method for step-down resampling. FDR-q values control for the false discovery rate using the Benjamini and Hochberg (1995) method. CM shows control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses. F-tests of the null hypothesis that all three treatment effects are equal across neighborhoods are as follows: Primary Index: $p = 0.0881$; Shooting & Homicide Victimizations: $p = 0.596$; Shootings & Homicide Arrests: $p = 0.256$; Other Serious Violent-Crime Arrests: $p = 0.369$

Table A.11: READI's estimated effects on serious violence involvement, by age group

	CM	ITT	CCM	TOT	Observed ITT	FWER	FDR-q
		Estimates				P-values	
Primary Index of Serious Violence by Age							
Over Median	-0.0619	-0.0155	-0.0682	-0.0280	0.585	0.580	0.585
		(0.0284)		(0.0502)			
Under Median	0.0627	-0.0394	0.0992	-0.0733	0.293	0.503	0.585
		(0.0374)		(0.0673)			
Primary Outcome Components by Age, Counts							
Over Median							
(N = 1228)							
Shootings & Homicides Victimizations	0.0919	-0.0097	0.0948	-0.0176	0.590	0.873	0.769
		(0.0179)		(0.0317)			
Shootings & Homicides Arrests	0.0145	0.0021	0.0047	0.0042	0.769	0.873	0.769
		(0.0073)		(0.0129)			
Other Serious Violent-Crime Arrests	0.0419	-0.0078	0.0501	-0.0145	0.505	0.873	0.769
		(0.0116)		(0.0206)			
Under Median							
(N = 1228)							
Shootings & Homicides Victimizations	0.1373	-0.0119	0.1399	-0.0221	0.562	0.595	0.562
		(0.0206)		(0.0369)			
Shootings & Homicides Arrests	0.0376	-0.0238	0.0596	-0.0445	0.013	0.038	0.040
		(0.0096)		(0.0173)			
Other Serious Violent-Crime Arrests	0.0670	0.0152	0.0590	0.0286	0.364	0.595	0.546
		(0.0168)		(0.0301)			

Notes: N = 2,456. Estimates for each outcome are from a single regression that interacts age group with treatment. Median age at time of randomization is 24.7. Primary index standardizes each of the three components shown in the bottom panel based on the control group's distribution and takes their unweighted average. Other serious violent-crime arrests include criminal sexual assault, robbery, and aggravated assault and battery. For multiple hypothesis testing adjustments within the primary index, we define the two age groups as a family. For the component adjustments, we define the three different outcomes within each age group as a family. FWER p-values control for the family-wise error rate using the Westfall and Young (1993) method for step-down resampling. FDR-q values control for the false discovery rate using the Benjamini and Hochberg (1995) method. CM shows control means; ITT shows the intent-to-treat estimates; CCM shows the control complier mean; and TOT shows the treatment-on-the-treated estimates from an IV regression. Regressions include baseline covariates and randomization strata fixed effects. Heteroskedasticity-robust standard errors reported in parentheses. F-tests of the null hypothesis that all both treatment effects are equal across age groups are as follows: Primary Index: $p = 0.613$; Shooting & Homicide Victimizations: $p = 0.934$; Shootings & Homicide Arrests: $p = 0.0344$; Other Serious Violent-Crime Arrests: $p = 0.266$

Table A.12: Outreach pathway and reweighted ITT estimates

| | Algorithm Exclusion | | | Re-entry Exclusion | | |
| | Outreach | Algorithm | | Outreach | Re-entry | |
	OLS	OLS	AO	OLS	OLS	AO
Primary Index of Serious Violence	-0.205	0.045	0.078	-0.108	-0.070	0.074
Shooting & Homicide Victimizations	-0.067	0.022	0.014	-0.045	-0.032	0.004
Shooting & Homicide Arrests	-0.044	-0.004	0.010	-0.025	-0.006	0.021
Other Serious Violent-Crime Arrests	-0.037	0.023	0.030	-0.010	-0.020	-0.001

Notes: For the entire sample, we estimate predicted probabilities, $\hat{p}$, of being an outreach referral using a logit on neighborhood fixed effects, age at randomization, and pre-randomization characteristics including prior arrest, victimization, and incarceration. Sample is restricted to enforce common support, see text for details. Resulting N = 1,672 for the algorithm exclusion and N = 1,122 for the re-entry exclusion. The AO ITT column implements the reweighting method described in Andrews and Oster (2017), estimating what the ITT would be among referrals in each pathway if they had the mean observables of outreach referrals.

Table A.13: Inputs to social cost of crime estimates

Crime Type	Bottom-Up Components			Total Bottom-Up Costs	Willingness-to-Pay Costs	Estimated Clearance Rate	Estimated Reporting Rate
	Victim Costs	Legal System Costs	Offender Productivity Costs				
Murder	5,438,106	354,659	165,508	5,910,985	13,429,758	0.37	1.00
Rape	159,597	9,812	5,320	177,330	327,705	0.28	0.32
Armed Robbery	34,284	17,378	9,458	59,110	304,179	0.09	0.52
Robbery	14,186	8,748	4,729	27,191	32,629	0.09	0.52
Aggravated Assault	43,741	15,960	7,566	65,021	76,961	0.15	0.56
Simple Assault	5,320	5,911	1,537	13,004	15,014	0.11	0.37
Burglary	2,364	2,719	1,182	5,911	37,476	0.09	0.48
Motor Vehicle Theft	6,502	3,428	1,182	10,640	15,487	0.12	0.78
Larceny	532	2,010	828	3,310	1,892	0.11	0.28
Drunk Driving Crash	33,102	2,010	828	35,466	68,095	0.09	1.00
Arson	67,385	2,010	828	70,932	133,115	0.09	1.00
Vandalism	437	745	0	1,182	1,620	0.09	0.28
Fraud	1,300	2,010	828	4,138	3,665	0.09	0.28
Other	0	591	0	591	591	0.09	0.28

Notes: Crime types, bottom-up components, and willingness-to-pay costs are based on estimates from Cohen and Piquero (2009). We inflate to 2017 dollars and subtract legal system and offender productivity costs from willingness-to-pay estimates to avoid double counting. Total bottom-up costs are the result of summing up the bottom-up component costs. Estimated clearance rate for murder from CPD. Estimated clearance rates for other offenses are the midpoints of the ranges reported in Belfield et al. (2006). Rates of reporting victimization to the police are derived from Morgan and Thompson (2021).

Figure A.1: READI job stage progression

Stage 1: Safety

Neighborhood Revitalization, Chicago Parks District, CPS, Aldermanic Wards, etc.

Duration: At least 3 months
Wage: $12.00

To Advance to Stage 2:
• Complete 30 hours of CBT
• Be on time and have monthly attendance of 75%
• Be safe at the work site
• Demonstrate positive workplace behavior
• Complete a career interest assessment

Stage 2: Connections

Animal Care and Control, Growing Power, Cook County Housing Authority, Chicago Food Depository, etc.

Duration: At least 3 months
Wage: $12.50

To Advance to Stage 3:
• Complete 80-100 hours of CBT
• Be on time and have monthly attendance of 90%
• Demonstrate work-readiness skills
• Positive engagement in CBT
• Create a Transition Plan with job coach

Stage 3: Exploration

Sectors: Hospitality, Manufacturing, Warehousing & Logistics, Continuing Education, Transportation, and Government

Duration: Up to 12 months
Wage: $13.50

To Advance to Stage 4:
• Complete 160-200 hours of CBT
• Have over 90% attendance with positive participation
• Travel to and from the work site independently
• Demonstrate advanced job skills
• Finalize Transition Plan with job coach

Stage 4: Transition

Unsubsidized employment within one of the Stage 3 sectors or paid training program, such as union apprenticeships

Duration: At least 6 months (Total program duration should not exceed 18 months)
Timeframe: Start of unsubsidized work or 18th month up to 24th month
Wage: At least $13.50

To Graduate From READI:
• Complete 240-300 hours of CBT
• Start unsubsidized employment or paid training
• Continue working on transition plan
• Continue working with job coach

Notes: The duration and advancement requirements for READI's job stages were subject to change based on a participant's needs and progress. Program staff exercised discretion in deciding which participants were ready to advance job stages. The diagram shows READI's initial design; the details of implementation varied somewhat in practice as the model developed over time.
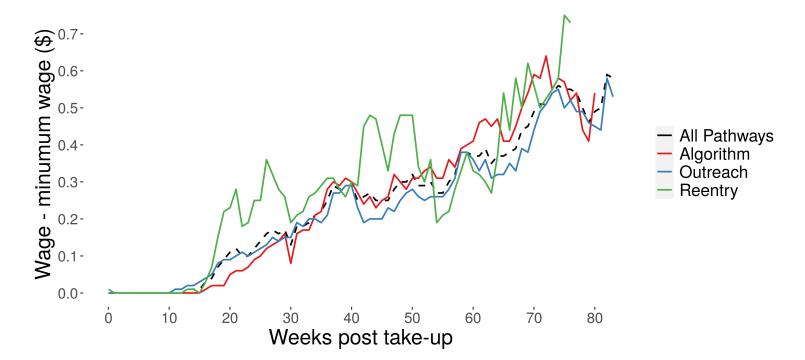
Figure A.2: READI wage growth by pathway

Notes: READI's starting wage was $11 at its launch in August 2017, increased to $12 in July 2018, and to $13 in July 2019. Average wage is calculated using only participants who report to work during a given week.
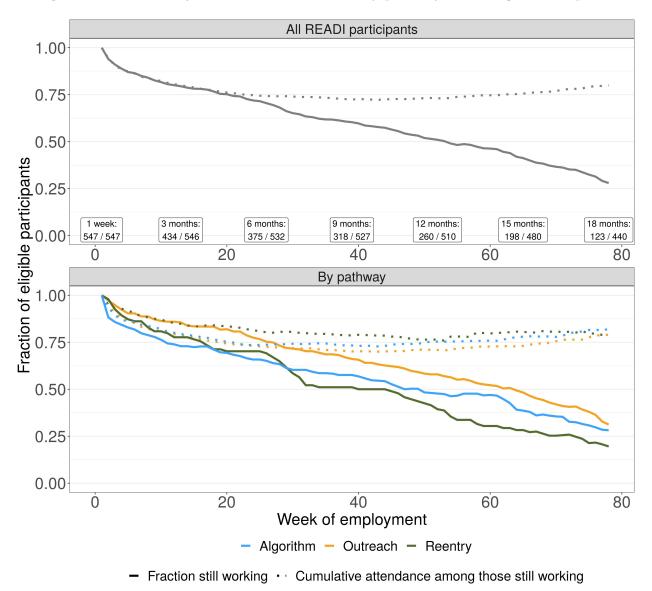
Figure A.3: READI job retention, overall and by pathway, including COVID period

Notes: Figure shows two measures of job retention for men who started READI measured from payroll data. The solid line shows the proportion of participants who work at least once after the time shown on the x-axis conditional on observing them for that long. The boxes show the number of workers contributing to each point. The dotted line shows the average proportion of possible weeks worked among those still working at each point in time. At 18 months after first taking up, N = 38 algorithm referrals, N = 68 outreach referrals, and N = 17 re-entry referrals are still observed working.

Figure A.4: Cumulative first stage and ITT effects over time

Participation

Shooting & Homicide Victimizations

Shooting & Homicide Arrests

Other Serious Violent-Crime Arrests

⊢——⊣ 95% confidence interval

Notes: Figures show cumulative treatment effects up to the time shown on the x-axis, inclusive. Top left panel shows indicators for any participation; other panels show the three main components of the primary index. Regressions include baseline covariates and randomization strata fixed effects, and 95 percent confidence intervals are constructed using heteroskedasticity-robust standard errors.

Figure A.5: Distribution of risk scores by pathway and estimated effects on index components by pathway and risk level