The Longer-Term and Health Effects of an Experiment to Reduce Gun Violence

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Abstract

Community violence interventions aim to reduce shootings by providing services to those at highest risk of shooting or being shot. Our previous study of READI Chicago—a program that offered 18 months of supported work and cognitive behavioral therapy to men at high shooting risk—showed mixed results during the 20-month follow-up period, but large declines in shooting arrest and victimization among a subgroup referred by local outreach workers. This paper adds longer-term data through 40 months post-randomization, after programming ended for most participants. It also adds hospital records that provide both alternative violence measures and other health outcomes. The initial results persist: both data sources show large declines in serious violence through 40 months for outreach referrals, but potentially adverse effects for those referred by a predictive algorithm. There is also some indication of an increase in overdoses and mental health crises among outreach referrals, though we cannot rule out this being due to increased treatment-seeking. On net, the program's benefits still outweigh its costs almost 4 years later, with consistent lessons about how selection mechanisms affect treatment response.

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PLEASE NOTE: This is a very preliminary draft for the purposes of conference submission. The main results are complete, but other analysis is still in progress. By the time of the conference, we plan to have a full, finished working paper.

1 Introduction

Community violence interventions (CVIs) have gained popularity in recent years as a way to reduce gun violence, alongside traditional law enforcement approaches (Buggs, 2022). These CVIs try to interrupt cycles of violence by using interpersonal influence, mediation, and social services to change the behaviors of the people most likely to shoot or be shot. The most common CVI model is community-wide, with outreach organizations intervening in and mediating active disputes (e.g., Butts et al., 2015). To complement this, a new set of interventions has emerged that are more preventative in nature—attempting to identify the people at highest risk of being involved in a shooting and offering them economic and/or psychological support (e.g., Blattman et al., 2023; Swan et al., 2023).

In a 2024 paper, we reported the first experimental results for one such preventative intervention in Chicago: the Rapid Employment and Development Initiative, or READI Chicago (Bhatt et al., 2024). READI offered 18 months of subsidized, supported work combined with group cognitive behavioral therapy (CBT), and referrals to housing, substance abuse, mental health, and legal services as needed. The first paper reported 20-month impacts on serious violence. This paper reports 40-month impacts on the same set of violence outcomes, and adds new health outcomes over the same period.

At its inception, READI identified nearly 2,500 adult men in five Chicago neighborhoods with the highest levels and rates of gun violence. Three methods were used to identify men at the highest risk of gun violence involvement: (1) a machine learning algorithm based on administrative arrest and victimization records; (2) referrals from local outreach workers; and (3) screening among those leaving prison and jail. Half of the identified men were then randomized into being offered READI, with those not receiving the offer being free to pursue

any other services already available to them.

In our evaluation 20 months after randomization, we found that there was no statistically significant change in a pre-specified index combining three measures of serious violence from Chicago Police Department (CPD) arrest and victim records. Nonetheless, one of those measures—shooting and homicide arrests—declined by 45 percent among the treatment group ($p = 0.13$ after multiple testing adjustment).[1] Despite the wide confidence intervals, the magnitude of this impact suggested that READI was a highly cost-effective program: given the large social cost of a shooting, READI generated social benefits from reduced crime and violence of 4 to 18 times its cost. We also found large and statistically significant declines in both shooting and homicide arrests and victimizations among treatment men referred to READI via outreach workers—80 and 38 percent, respectively. In contrast, we found no impact of the program on men referred via the algorithm. While one must take impacts on index components and subgroups with caution, it is also the case that: the analysis was pre-specified; the impacts were extremely large; and the significant effects on outreach referrals survived formal multiple testing adjustments.

In this paper, we address two limitations of the 2024 study. First, and as described in our pre-analysis plan, we consider longer-term outcomes, tracing the impact of participating in READI on violence outcomes through 40 months after randomization.[2] This allows us to consider impacts not just while most participants were in the program, but also for more than a year and a half after their participation ended.

Second, our 40-month analysis adds a new data source: hospital visit records from the Illinois Department of Public Health (IDPH). These data address common concerns with and limitations of police data as a tool to measure violence involvement. For example, gun victimization should be relatively well-measured in police data, since healthcare workers in

[1] We focus here on intent-to-treat (ITT) estimates for the READI treatment group, rather than treatment-on-the-treated (TOT) estimates for READI participants as we did in Bhatt et al. (2024). We do this to facilitate comparisons with the new health outcomes for which we can only report ITT estimates, as described further in section 3.2.

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

Illinois are legally required to report gunshot wounds to the police.[3] But other kinds of non-gun serious violence are likely to be under-reported in police victimization records.[4] Additionally, our police data are limited to Chicago, while the hospital data are state-wide.

Using the same police data as in the first study, the 40-month impacts broadly resemble the 20-month ones. We see a small and statistically insignificant difference in our pre-specified index of serious violence. However, this overall null effect estimate hides two important sources of heterogeneity. One is that we continue to see substantively large reductions in the most costly forms of violence—shooting and homicide victimizations and arrests. Even though treatment effect estimates on these index components are imprecise, when crime and violence incidents are scaled by estimates of their social cost, we again estimate statistically significant benefit-cost ratios between 3.9 and 15.5 to 1.

We also continue to observe large differences by referral pathway. Among those referred by outreach workers, we see a large and statistically significant drop in the primary pre-specified serious violence index (-0.11 standard deviations), and in shooting and homicide victimization alone (-36 percent) over 40 months. There is no significant effect of READI among algorithm referrals, however. In fact, the point estimates for the algorithm referral group over 40 months generally point toward adverse effects, though they fall outside conventional significance bounds.

Turning to the hospital data, we again observe broadly similar results to those from the police data. Overall, there is no significant change in visits for either shooting or non-shooting violence over the 40-month period. But, as in the police data, the outreach referral group shows a statistically significant decline in shooting victimization (-30 percent). Point estimates for the algorithm group are also adverse in the hospital data, with a statistically significant increase in non-shooting violence (+46 percent, slightly larger than the imprecise 34 percent increase in serious non-shooting victimizations reported to the police). The similar

[3] 20 ILCS 2630/3.2

[4] This concern is why our primary pre-specified serious violence index does not include violent victimizations other than gunshot injuries and homicides.

subgroup findings in two independent data sources are reassuring and useful to future CVI research in that they suggest that police data are not systematically distorted by differential willingness to report victimizations.

Outside of violence, we find few changes in other kinds of hospital visits, though we are somewhat under-powered to detect changes here. Nonetheless, there is one unexpected result: the decline in violence among the outreach group is accompanied by a statistically significant and proportionally large increase in overdoses and mental health crises, albeit from a very low baseline (+323 percent from a control mean of 0.014). Given the number of hypotheses we test across outcomes and subgroups in the hospital data, this adverse effect is one that should be explored in other CVI settings.

2 Experimental sample and intervention

As in many U.S. cities, shootings in Chicago are highly concentrated in a few disadvantaged neighborhoods and among a small subset of people.[5] Such shootings have many proximate causes (Abt, 2019; Blattman, 2022). READI is a bundled intervention designed to address common proximate causes of gun violence and to prevent it from escalating (Bhatt et al., 2024). Getting the people at highest risk of engaging in shootings into such programs can pull them out of situations where gun violence is one of the main ways to enforce territory or contracts, monopolize markets, manage disagreements, and deter rivals from hostile acts. In addition, CBT and other aspects of group therapy teach important skills. Many shootings follow from automatic responses to perceived slights, or other mistakes and misperceptions related to fear, anger, and biased beliefs. CBT teaches people to recognize these biases, regulate their emotions, and slow down their thinking. It also helps them identify other, non-violent ways to manage rivalries and disputes without starting escalatory spirals of violence.

To be successful, however, CVIs must meet three challenges: identifying people at suf-

[5] See, e.g., Aspholm (2020); Green et al. (2017); Braga (2003); Farrington et al. (2006); Wolfgang and Tracy (1982); Abt (2019).

ficiently high *ex ante* risk of gun violence, engaging them, and reducing their risk. But prediction is challenging due to the complexity and transience of gun violence risk (Berk et al., 2009; Chandler et al., 2011; Heller et al., forthcoming; Wheeler et al., 2019), and high-risk people are often disconnected from institutions and face barriers such as housing instability, substance use, and safety concerns (Fagan and Wilkinson, 1998; Anderson, 1999). Thus, we designed the original READI study not just to estimate overall impacts, but also to test different mechanisms for identifying and engaging men at high risk of future gun violence.

2.1 Sample selection, referral pathways, and randomization

READI was a collaboration between a large non-profit (Heartland Alliance), local outreach and employment organizations, the University of Chicago Urban Labs, and the study investigators. The program targeted men aged 18 and over at highest risk of gun violence in the five Chicago neighborhoods with the highest shooting rates, grouped into three operational sites: Austin/West Garfield Park, North Lawndale, and Englewood/West Englewood (Appendix Figure A.1).

To foster variation in participants' risk profiles and responsiveness to intervention, men were recruited through three pathways. First, the *algorithm pathway* used administrative police data to generate a risk score predicting 18-month gun violence involvement (Heller et al., forthcoming). This pathway was useful for identifying high-risk men based on observables, especially those outside the networks of participating outreach and employment organizations. Only men with the highest risk scores were eligible for randomization. Second, the *outreach pathway* relied on local outreach workers' knowledge to identify men at high risk, capturing factors unobservable in police records or arising from fast-moving situations in the neighborhood. Third, the *re-entry pathway* recruited high-risk men exiting jail or prison who would be missed by the algorithm and outreach organizations, and who might be at a particularly sensitive transition period.

From August 2017 to March 2020, referrals were made on a rolling basis as program

capacity expanded, with randomization at the person level within strata defined by site, referral pathway, and date. Treatment probability was approximately 50 percent, and randomization generated the expected degree of balance on 17 observables (see Bhatt et al., 2024).[6]

2.2 Intervention components

READI had four main program components.

Outreach Initial engagement involved community outreach workers locating referred men—often a protracted process given their mobility and mistrust—and preparing them for enrollment. Formal participation began with orientation and job onboarding.

Subsidized work Participants were offered 18 months of supported work, five days per week (29.5 hours total). Jobs followed a structured "career pathway" model with opportunities for wage growth and greater independence over time. They typically began performing outdoor clean-up services or basic indoor tasks (such as food packing), but could eventually access subsidized placements with local employers in services and manufacturing.

CBT-informed group sessions Prior to supported work in the afternoons, participants were required to attend small-group CBT sessions, typically three mornings per week. Sessions focused on building awareness of automatic thoughts and emotions, practicing constructive behavioral responses, adapting to new non-violent social situations and identities, and dealing with traumatic stress. Additional professional development sessions were held twice weekly. Attendance at these daily sessions was also incentivized through stipends. The goal was to reinforce new cognitive and social skills before, at, and beyond the worksite.

Other support and services Recognizing participants' barriers to consistent engagement—including housing instability, substance use, legal troubles, family quarrels, and mental health struggles—outreach staff provided direct assistance and referrals to external services when

[6] Appendix Table A.4 reports the baseline characteristics and tests of balance originally reported in Bhatt et al. (2024).

needed, with an emphasis on crisis management.

A few other program aspects are useful to highlight. First, READI was designed and implemented extremely quickly to respond to a 60 percent spike in gun violence from 2015–16 (Kapustin et al., 2017). Thus, READI's model changed slightly over time as the organizations learned how to address the challenge and population. Nonetheless, the four components above were always present.

Second, in March 2020, the COVID-19 pandemic suspended in-person work and shifted CBT programming online. Participants received standby payments during the initial disruption, with limited in-person activities resuming in August 2020. Although services were affected, these disruptions did not compromise the internal validity of the study.

Finally, men in the study were free to access alternative programming, and some surely did, particularly men in the control group. To our knowledge, programming of READI's length and intensity was unique at the time in the neighborhoods where it operated. Similar programs appeared and grew in subsequent years, and it is possible that members of the treatment or control groups received services. This is nearly impossible to track, and implies our 40-month treatment effect estimates are likely a lower bound on READI's program impacts relative to no services.

2.3 Participant characteristics

The average age of study men at randomization was 25 years old and 97 percent were Black (Appendix Table A.7). Prior to randomization, study men had high levels of violence involvement: per 100 men, there were 7.8 prior arrests for shootings or homicides, 46 shooting victimizations, and 90 arrests for other serious violent crimes. The algorithm predicted that 11.4 percent of study men would be arrested for or the victim of gun violence in the 18 months after randomization (in practice that was under-predicted, with 16 percent of the control group involved in a gun crime).

General criminal legal system involvement prior to randomization among study men was

also significant: over 17 arrests and 3.4 reported victimizations per person, with an average of 177 days spent incarcerated in the prior 30 months. Ethnographic research and program observations suggest many study men were involved in Chicago's fragmented gang landscape (Aspholm, 2020).

We have limited educational and employment data from a select group of 535 surveyed treatment participants. Of these, 60 percent had not completed high school, and 31 percent held a diploma or GED. Before READI, 28–52 percent reported recent employment, depending on survey timing. Additional surveys of a smaller subgroup suggest that just prior to READI, 43 percent had formal employment, 38 percent informal legal work, and 59 percent engaged in risky informal work such as drug sales or theft (Kapustin et al., 2024).

The baseline characteristics of study men varied by referral pathway (Appendix Table A.7). Algorithm referrals were younger but had longer arrest and victimization histories than outreach referrals. Their predicted gun violence risk was higher (14 versus 9 percent), and they had double the number of prior victimizations. Re-entry referrals generally fell between those from the other two pathways.

Realized risk during the 40-month follow-up period confirms that referral pathways successfully identified high-risk men. About three-fourths of control group men were arrested and nearly one-half victimized, with 20 shooting or homicide victimizations per 100 men—48 times the citywide average. Algorithm referrals were more likely to be arrested or victimized overall. But rates of gun violence specifically were similar across pathways, despite algorithm referrals having a higher predicted risk based on observables at baseline. This suggests that humans making referrals leveraged unobservable factors predictive of gun violence.

2.4 Take-up

Despite the high-risk population, program take-up was substantial: 55 percent of men offered READI attended orientation, comparable to interventions with lower-risk groups (Heller

et al., 2017).[7] Take-up was highest among outreach referrals (79 percent), followed by re-entry (60 percent), and lowest among algorithm referrals (37 percent). Lower algorithm take-up partly reflected extreme difficulties in locating these men, plus a lack of pre-existing relationships and trust with outreach organizations.

Among those who took up, participants worked an average of 595 hours and attended 166 hours of CBT or training during the 40-month period, earning about $10,245 on average (Appendix Table A.1). Prior to COVID-19, participants worked about 29 percent of possible hours post-take-up.

Retention was initially high: 75 percent remained engaged after 20 weeks, and about half after one year. While algorithm participants had higher early attrition than outreach participants, long-term retention patterns were broadly similar once engaged.

3 Data and estimation

3.1 Data

Our main outcome of interest is a person's involvement in serious violence, either as a perpetrator or a victim. As true violence involvement is inherently unobservable, we pre-specified that we would proxy for perpetration and victimization using administrative arrest and victimization records, respectively, from the Chicago Police Department (CPD). This index, *Serious violence involvement*, standardizes and averages three component measures: (1) Shooting and homicide victimizations, (2) Shooting and homicide arrests, and (3) Other serious violent-crime arrests.[8] We pre-committed to reporting results after 20 and 40 months.

[7] These figures are slightly different than those reported in Bhatt et al. (2024) because, during months 21-40 after randomization included in this paper, 5 additional treatment group men started READI and 22 percent of participants worked at least some hours. Most participants finished their program participation close to the start of the new follow-up period, and 93 percent of the hours worked by READI participants came within the initial 20-month follow-up period. As such, one can roughly think of months 1-20 as the "program" period and months 21-40 as the "post-program" period, although some people were working during the latter period.

[8] Arrests are limited to incidents that occurred during the outcome period. Shooting arrests include any aggravated assault or aggravated battery that involves shooting a gun. Other serious violent-crime arrests include other violent offenses historically included in "Part I" of the Uniform Crime Reporting program: aggravated assault and aggravated battery (excluding homicide, manslaughter, and non-fatal shootings), robbery, and criminal sexual assault.

We now complement this police-based measure with data on hospital visits provided by the Illinois Department of Public Health (IDPH), covering all hospitals in the state. The data include both emergency department and inpatient admissions, where the latter represents about 10 percent of visits. The data are at the visit level, where each visit may contain multiple ICD-10 diagnosis codes of varying severity. When a condition is more severe, there may be less discretion in whether to seek care for it, so changes in its prevalence in the hospital data are likelier to reflect changes in the condition itself. In contrast, when a condition is less severe, it is less likely to be the "primary" reason for a visit and may only appear in these data during a visit for an unrelated matter.[9] For such a condition, a change in its prevalence in the hospital data may reflect changes in treatment-seeking rather than in the condition itself.

Our analysis of the hospital data is focused on whether READI affects violent victimization, followed by substance abuse, mental health, and other risky behaviors. We assign each diagnosis in a visit to one of several mutually exclusive categories based on these conditions, which we rank order to reflect their likely differing severity. Each visit is labeled with the category of its highest-ranked (i.e., most severe) diagnosis. The ranked diagnosis categories we use are:[10]

1. Violence. Initial encounters related to a shooting or non-shooting violent injury.[11]

2. Overdose or mental health crisis. This includes suicide attempts, "acute" or "severe" mental health conditions, or mental health conditions with a co-occurring diagnosis of

[9] The data do not provide a way to reliably determine the primary reason for a visit.

[10] One deviation from this ranking concerns car accidents, which we rank above other substance abuse or mental health crisis. We acknowledge that other physical health conditions, such as heart attacks, may be more severe than others ranked more highly here. Such conditions are relatively rare among study men during the outcome period and are not the focus of the hospital data analysis.

[11] Shootings are identified by diagnosis codes indicating an assault by firearm discharge (including accidental discharges) or that mention a puncture wound without a known non-gun cause, but exclude self-harm with a firearm. Non-shooting violent injuries are identified by diagnosis codes from IDPH (excluding those related to shootings), along with assault diagnosis codes X85-X99 and Y00-Y09: `https://dph.illinois.gov/topics-services/emergency-preparedness-response/ems/trauma-program/headspinal-cord-and-violent-injury-registry-hsvi/violent-injury-inclusion-codes.html`.

intentional self-harm.[12]

3. Other substance abuse (non-overdose) or mental health (non-crisis) disorder.[13]

4. Risky behavior and other injury. This includes car accidents, risky sexual behavior, or other injuries as identified by Downes et al. (2022).

5. Other. This includes all remaining diagnoses, including sequelae of violent injury.

Appendix A.1 provides further detail about this classification.

Due to confidentiality protections, the hospital data have several limitations worth noting. Most importantly, we could only link the hospital data to a limited set of binned covariates, such that no cell was smaller than 5 study men. We prioritized information necessary to ensure treatment assignment was conditionally random (i.e., randomization probabilities within strata), limited information on prior shooting victimizations from the police data at baseline (binned into 0, 1, or 2+) to be able to compare across data sources, and referral pathway given the importance of pathway heterogeneity in our initial results. To meet the 5-person cell requirement, we grouped 10 study men together who were either outreach or re-entry referrals; for the purposes of this analysis, we include them in outreach (only 3 were actually re-entry).

Another implication of the confidentiality protections is that we were unable to include information about shooting or violence outcomes from police records in the hospital dataset, as it could make subjects identifiable. This means we cannot directly compare which study men were shooting or violence victims during the outcome period across the hospital and police data. Another important limitation is that we were unable to include information on whether someone actually participated in the program. This means that we can only estimate intent-to-treat (ITT) effects using the hospital data; we cannot conduct an instrumental

[12] Overdoses include all diagnoses with the words "poisoning by."

[13] Other substance abuse excludes diagnoses related to uncomplicated nicotine and/or cannabis use, but includes diagnoses related to psychoactive substance use, alcohol abuse, and complications from alcohol abuse (e.g., alcoholic gastritis).

variables analysis without a participation indicator. To ease comparisons, we focus the entire paper, including the police data analysis, on ITT estimates.

3.2 Estimation

Our analysis in this paper focuses on estimating ITT effects with the following ordinary least squares regression:

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

where Y_i is an outcome like shooting and homicide arrest (in the police data) or a hospital visit for a shooting injury (in the hospital data); T_i indicates assignment to an offer of READI; $\mathbf{X}_i$ is a vector of pre-randomization characteristics; and γ_s is a vector of randomization strata fixed effects. The coefficient β provides the average outcome difference between those assigned to the treatment versus control groups. We estimate heteroskedasticity-robust standard errors.

For the police data analysis, the vector $\mathbf{X}_i$ includes the same set of baseline covariates included in our original analysis. For the hospital data analysis, where we have a more limited set of covariates, the vector $\mathbf{X}_i$ includes the number of prior shooting victimizations from the police data (binned as 1 and 2+, with 0 omitted) and the number of prior hospital visits within each mutually exclusive category. Likewise, for the hospital data analysis, γ_s is a vector of coarsened randomization strata fixed effects due to confidentiality protections.[14]

4 Results

4.1 Program impacts in police data

4.1.1 Average treatment effects

Panel A of Figure 1 displays ITT estimates for the prespecified primary outcome, an index of serious violence involvement, for three periods: months 1-20, 21-40, and 1-40 (cumulative). The 40-month results are similar to the initial 20-month results. There is no statistically

[14] This set includes the interaction of fixed effects for referral pathway, coarsened probability of randomization to treatment, and coarsened randomization period.

significant decline in violence involvement in the full sample.[15] That said, if we break down the 40-month index into its three components, we find that the most serious forms of violence saw decreases (Panel B of Figure 1). The point estimates imply magnitudes that are substantively important: a 7.6 percent decline in shooting and homicide victimizations; a 15 percent decline in shooting and homicide arrests; and a 7 percent increase in other serious violent-crime arrests. However, none of these estimates on the full sample are statistically significant after adjusting for multiple hypothesis testing by controlling for the family-wise error rate (FWER), with all three estimates yielding adjusted p-values of $p = 0.78$ (Anderson, 2008; Westfall and Young, 1993).

Social benefit-cost analysis To quantify READI's benefits, we also weight the crime and violence outcomes we measure in the police data by their social costs. As policymakers may value these outcomes differently, this social cost weighting provides an alternative to testing outcomes individually and adjusting the inference procedure accordingly (Viviano et al., 2021). It is also particularly important given READI's suggestively large impacts on the most serious (and socially costly) forms of violence, like shootings and homicides.

We limit our focus to the social costs of crime and violence. Given the considerable uncertainty in valuing these outcomes (Dominguez and Raphael, 2015), our goal is to provide a rough approximation of READI's potential benefits. We also do not consider READI's effects on broader economic or health outcomes.

Appendix Table A.11 presents our results, using less and more inclusive estimates of the costs of crime. The less inclusive approach applies low-end crime costs from Cohen and Piquero (2009) to directly observed arrests and victimizations. The more inclusive approach uses willingness-to-pay crime cost estimates from Cohen and Piquero (2009), adjusts for unobserved crime using clearance rates, and scales victimizations based on reporting rates. Program wages are treated as transfers, and net program costs reflect only administrative

[15] See Appendix Table A.9 for detailed statistics, including treatment-on-the-treated (TOT) effect estimates and standard errors adjusted for multiple testing.

expenses.

Without READI, the crime and violence in which study men are involved over 40 months costs society between $450,000 and $2.1 million *per person*, based on the control group means. Offering these men READI reduces these costs by between $120,000 and $480,000 per person ($p = 0.03$ and $p = 0.06$), or roughly 25 percent. If we consider TOT rather than ITT effects, participating in READI reduces social costs over 40 months by between $221,000 to $878,000 per participant ($p = 0.03$ and $p = 0.05$). Relative to the costs of offering or providing programming to these men, most of which are incurred during the first 20 months after randomization, the implied benefit-cost ratios range from 3.9:1 and 15.5:1.

Program impacts over time Generally speaking, READI's effects on violence appear to be larger during months 1-20 than 21-40. There are several reasons to be cautious with interpreting this as impact attenuation, however. First, none of the differences are statistically significant. Second, estimates during months 21-40 may be sensitive to the fact that arrest rates for serious violent crime during that period were even lower than before (Appendix Table A.8). Because we re-standardize the primary outcome index for each period, even small absolute changes in these arrest rates can have large effects on the index.[16]

Nonetheless, a moderation of treatment effects could be consistent with program impacts lessening somewhat over time, something longer term follow-up of the sample could help to answer. Evidence from months 1-20 suggest that any lessening of program impacts is unlikely to be the result of incapacitation (of being active in the program, and off the streets), as the largest reductions in shooting and homicide arrests involved incidents occurring on weekends (Bhatt et al., 2024).

[16] Re-standardizing within time period has two other implications worth noting. First, it is why the estimated effect on the index for outreach referrals in months 1-40 in Panel A of Figure 1 is not between the estimates for months 1-20 and 21-40. Second, it is why estimated effects for months 1-40 are generally more heavily weighted toward changes in months 1-20 than 21-40.

4.1.2 Heterogeneity analysis

By referral pathway One of our two pre-specified heterogeneity analyses was by referral pathway. As with the results from months 1-20, we see in Panel A of Figure 1 the largest and most significant decline in serious violence during months 1-40 among outreach referrals (-0.11 standard deviations, with an adjusted p-value of $p = 0.01$) and an adverse but not statistically significant estimated effect for algorithm referrals (+0.06 standard deviations, $p = 0.18$).

Looking at Panel B of Figure 1, all forms of serious violence decline among outreach referrals, though we can only be statistically confident in the shooting and homicide victimization decline. Adjusting for multiple outcomes, we observe a 35.5 percent decline in shooting and homicide victimizations ($p = 0.04$); a 52.1 percent decline in shooting and homicide arrests ($p = 0.16$); and a 28.9 percent decrease in other serious violent-crime arrests ($p = 0.23$).

The point estimates for algorithm referrals all point in an adverse direction, even if they are not statistically significant. As it is the largest pathway, this is what accounts for the moderation of impact estimates in the full sample, including the estimated overall adverse effect on other serious violent-crime arrests. We discuss possible reasons for the pathway difference, and possible adverse effects, below.

By baseline predicted risk Our second pre-specified heterogeneity analysis was by baseline predicted risk score. As with the results from months 1-20, we do not see significant heterogeneity by risk score in the full sample. However, in the subgroup with the most substantial estimated average impacts—outreach referrals—we do see significant heterogeneity by risk score.

Appendix Table A.10 examines the full sample, and reports ITT estimates for those with a baseline risk score over and under the median, as well as the 231 men with missing scores. None of the estimates are statistically significant, and we cannot reject the null that the three predicted risk groups have the same effect ($p = 0.44$). The point estimate on the primary

index is negative for the over median group, but it is larger and negative for those with a missing risk score. This can only happen for outreach and re-entry referrals, and could indicate lower baseline risk (or unobserved risk), since someone had too little recent police contact to meet the criteria for having an algorithm-generated risk score.[17]

As in our earlier analysis, we do see variation by predicted risk among outreach referrals. Panel A of Appendix Figure A.3 shows estimated effects on our primary outcome index by referral pathway and baseline risk score quartiles.[18] Declines in serious violence among outreach referrals are concentrated among those with above-median predicted risk. The 0.11 standard deviation decline in serious violence we observed among all outreach referrals is mainly driven by a roughly 0.4 standard deviation decline in serious violence among those in the two highest-risk quartiles. Baseline predicted risk has almost no relationship with treatment effects among algorithm referrals.

Interpreting heterogeneous effects There is some evidence to suggest that outreach workers were able to identify participants with high unobserved levels of risk and willingness to change (or that pre-existing outreach relationships predisposed participants to change).

Given READI's explicit goal of serving men at the highest risk of gun violence, it is important to note 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 $\hat{Y}(0)$, the predicted level of gun violence involvement absent treatment, is not necessarily the same as having a high β, the treatment effect of READI.

The two bottom panels of Appendix Figure A.3 examine realized risk ($\bar{Y}(0)$) and participation. Outreach workers did not prioritize referring people with the highest predicted risk: 75 percent of referrals had below-median or missing risk scores. Outreach workers appeared instead to select based on unobservable risk factors and likelihood of take-up. We conclude

[17] The criteria included having at least one arrest or two victimizations in the last 50 months. Consistent with the idea that less recent police contact indicates lower risk, the control means for the missing group are considerably lower.

[18] Risk quartiles are defined over the full sample; "lower" risk still corresponds to high absolute levels of risk. See Appendix Figure A.4.

this largely because outreach referrals had higher realized gun violence rates than algorithm referrals at similar predicted risk levels, and were about 42 percentage points more likely to participate. Our qualitative interviews also suggest outreach workers emphasized "readiness" for programming (Bhatt et al., 2024).

However, neither factor fully explains treatment heterogeneity. Realized risk gaps between pathways are similar across risk quartiles, yet large effects appear only in quartiles 3 and 4. Take-up rates are also fairly flat across quartiles, and intensive participation (hours worked) does not consistently differ across pathways. While outreach workers leveraged unobserved risk factors and influenced take-up, these do not fully account for the stronger effects among outreach referrals. The precise drivers of heterogeneity remain unclear, highlighting a need for future research.

Addressing potentially adverse effects Finally, what should we make of the possible increase in serious violence involvement among algorithm referrals?

In qualitative interviews with program workers, the lower level of readiness of algorithm participants was a common theme (Bhatt et al., 2024). Nonetheless, there were few signs that the intervention might have adverse effects on algorithm referrals. In principle, participation in READI could pose risks, as it increased intermingling with people from rival groups and required traveling across neighborhood boundaries. This may have changed participants' presence on the street and tendencies to seek violent solutions or retribution even after programming ended. On balance, however, READI was designed to mitigate risks far more than it raised them. Importantly, the evidence is not consistent with active program participation endangering algorithm participants specifically, since the estimated effect on their serious violence involvement is not higher during months 1-20 than 21-40. Furthermore, the largest estimated adverse effect among algorithm referrals—a 208 percent increase in shooting and homicide arrests in months 21-40 ($p = 0.15$, Panel B of Appendix Figure A.2)—is driven entirely by arrests of non-participants.

Turning back to Appendix Figure A.3, the absence of a relationship between predicted

risk and treatment effects among algorithm referrals is potentially important. We would be especially worried if the highest-risk men consistently had a worse response, but we see no clear pattern.

4.2 Hospital data descriptive statistics

Table 1 presents 12-month baseline characteristics and realized risk among the control group, as measured in the hospital data, by referral pathway.[19] In the calendar year prior to randomization, 48 percent of study men had at least one hospital visit, ranging from 34 percent among re-entry referrals to 57 percent among algorithm referrals. Of those men who had at least one visit, men visited hospitals 2.7 times on average, yielding approximately 1.3 visits per person across the sample. This represents a rate of hospital use 2.8 times the national average of 0.47 visits per person.

In the year prior to randomization, for every 100 study men, there were 17 visits due to violence—either shooting (9 visits on average) or non-shooting related (8 visits on average). Another 29 visits were due to other forms of risky behavior or injury. Approximately 3 visits were due to either overdose or acute mental health crises, with other less severe forms of substance abuse or mental health issues appearing in another 20 visits. We see significantly different hospital visitation rates overall and for almost all categories by pathway, with algorithm referrals having the highest rates. This is notable given that the algorithm was trained on police data to predict gun violence risk and not on hospital records or to predict broader health risks, indicating a consistency in risk across outcomes in the two administrative data sources.

We are also interested in how hospital visitations change in the year following randomization, even in the absence of treatment, in part to assess how violence and other health risks evolve over time. The bottom of panel of Table 1 shows mean hospital visit counts in the control group during the first post-program year. This represents the counterfactual of

[19] Approximately 5 percent of the sample only have 10 or 11 months of baseline hospital data because of when they were randomized. We show that these outcomes are balanced across treatment groups in Appendix Table A.2, and across groups within pathways in Appendix Table A.3.

what would have happened if READI did not exist. We focus on the first year to capture the same amount of time as in the baseline panel.

The average number of visits does not change relative to the year prior to randomization, with 1.3 visits per person. Relative to the baseline year, usage decreases slightly for algorithm referrals (1.5 visits per person vs. 1.6 in the prior year), but increases for both outreach referrals (1.3 vs. 0.9) and re-entry referrals (1.1 vs. 0.8). These changes over time are consistent with the way men were selected: When we identified men at the very highest shooting risk at baseline with the algorithm, there was some reversion to the mean. When men were identified by outreach or re-entry workers, they seemed to anticipate more of the unobservables that would lead to increased risk in the future. As a result of the different selection patterns, the referral pathways are less different in terms of their hospital usage in the post-randomization year than they were at baseline (see Bhatt et al. (2024) for a robust discussion on selection by outreach workers).

Because it is very rare to have both police and hospital data on violence outcomes for the same population, Table 2 compares counts of different types of violence in both datasets.[20] Given that the measurement of violent victimization always depends on someone's willingness to either report to the police or seek hospital treatment, this table helps assess what is measured in each dataset. We show these counts separately for the treatment and control groups, because if one was worried that treatment could generate differential willingness to report victimizations to the police or to seek hospital treatment, then we might expect to see differential gaps in counts across these groups.

Three main lessons come from Table 2. First, while the overall number of shooting victimizations captured in police and hospital data are very similar (1 percent difference), they reflect different types of shootings. The hospital data are capturing 73 percent fewer fatal shootings, despite covering all of Illinois rather than only Chicago. Because fatal shootings are among the best measured kinds of crime, the difference here is almost certainly

[20] As a reminder, we can only make these comparisons in the aggregate, because confidentiality protections prevent us from linking the two datasets at the individual level.

driven by victims who expire before reaching the hospital. The police data include these victims, while the hospital data do not.

The hospital data, on the other hand, capture 16 percent more non-fatal shootings than the police data. Given that hospitals are legally required to report shooting injuries to the police, this likely reflects shootings that happen outside Chicago and hence CPD's jurisdiction. Indeed, the proportion of non-fatal shootings that appear in the hospital data but not the police data is consistent with the proportion of arrests of these men that happen outside Chicago (18 percent).[21] This suggests that the READI population is spending around 18 percent of its time outside CPD's jurisdiction, roughly consistent with the hospital data capturing 16 percent more non-fatal shootings. As such, the hospital data likely provide a more complete view into non-fatal shootings by including non-Chicago incidents.

Second, there are more than twice as many non-shooting violent victimizations in the police data than in the hospital data. This is perhaps surprising, given that READI recruited a population that tends to be quite skeptical of the police and that spends some time outside Chicago; under-reporting of violent incidents is likely to be a major issue in police victim records. It appears, then, that a relatively small proportion of non-shooting violent victimizations result in injuries that require hospital treatment. We should therefore expect that the non-shooting violence outcomes may be capturing somewhat different types of violence across data sources: the hospital data likely capture some of the more serious injuries due to non-shooting violent victimization.

Third, the differences across data sources are fairly similar across the treatment and control groups. Indeed, none of the differences across data sources is statistically distinguishable across the two groups. This pattern, along with the outcome results in the next section, suggest that the theoretical concern that treatment could change detection rates in the police data or treatment-seeking in the hospital data does not bear out in practice, at least for

[21] This calculation comes from state-wide arrest data that we have for our sample. However, due to limitations in these state-wide data that we describe in an addendum to our pre-analysis plan, we do not use them to construct our arrest-related outcome measures.

these serious violence outcomes.

4.3 Program impacts in hospital data on violence

Table 3 shows that READI's estimated impacts on violent victimization at 40 months post-randomization are broadly similar in the police and hospital data.[22] As in the police data, we cannot detect a difference in the hospital data on shooting visits in the full sample. However, for every 100 outreach referrals, those offered READI had 5.6 fewer shooting visits, on average, a 30 percent decline ($p = 0.07$). By comparison, in the police data, those offered READI had 7 fewer reported shooting victimizations, a 37 percent decline ($p = 0.01$). Point estimates for shootings among algorithm referrals are positive, albeit not statistically significant, in both the hospital and police data, suggesting adverse effects. But in the hospital data, which as discussed above likely captures different non-shooting violent incidents, there are 10 additional such visits for every 100 algorithm referrals, a 46 percent increase that is statistically significant ($p = 0.02$). Overall, neither the differences in measurement error across data sources nor the longer follow-up period change the conclusions from the first study: READI dramatically reduced shooting victimization for outreach referrals but not for algorithm referrals, with potentially adverse effects for the latter on non-shooting violent victimization.

4.4 Program impacts in hospital data on other health outcomes

Finally, we assess whether being offered READI affected hospital visits for reasons other than violent victimization. These results are reported in Table 4, again spanning 40 months post-randomization. Focusing first on the full sample (top left panel), we find no statistically significant differences across treatment arms in the number of hospital visits with overdose or mental health crises, less serious substance abuse or mental health conditions, risky behavior and other non-violent injury, or other health conditions. The lack of statistical significance, however, hides some quantitatively meaningful differences, such as 20 percent more visits

[22] Tables A.5 and A.6 break down the 40-month period into the 1-20 and 21-40 month periods.

with less serious substance abuse or mental health conditions among men offered READI. As we noted in our pre-analysis plan, these are exploratory analyses, and for acute incidents such as overdoses and mental health crises, we lack the statistical power to rule out substantively meaningful effects for these outcomes.

Looking at heterogeneity by referral pathway in the remaining panels of Table 4, we see some marked contrasts. As mentioned earlier, interviews with outreach workers suggest they selected referrals partly on readiness for READI. We see evidence of this in the control means of outreach referrals for hospital visits with substance abuse or mental health conditions, which are much lower than those of algorithm referrals. Unexpectedly, being offered READI appears to increase these visits substantially for outreach referrals: by more than 200 percent for overdose or mental health crises ($p = 0.02$), albeit relative to a low control mean of 0.03 visits per person, and by more than 70 percent for less serious substance abuse or mental health conditions ($p = 0.15$). In contrast, we see no similar pattern of increased hospital visits for substance abuse or mental health among algorithm and re-entry referrals, with three of the four point estimates being negative, though none are statistically significant.

An increase in hospital visits with less serious substance abuse or mental health conditions need not indicate an increase in the conditions themselves, but could instead reflect more treatment-seeking behavior. Since these conditions are often noted in diagnosis codes when people visit the hospital for other reasons, an increase in their prevalence could result from increased treatment-seeking for other medical issues. But this seems less plausible for the increase in visits with—and, given their severity, likely *for*—overdose or mental health crises. These are medical emergencies for which there is much less discretion in seeking treatment, relative to less serious substance abuse or mental health conditions. Our intention in separating these more serious diagnosis codes was to limit the role for discretion in whether to seek medical help.

It is theoretically possible that, despite our efforts to isolate visits that occurred with relatively little discretion, some of the increase in visits for acute conditions still reflects

changes in willingness to seek treatment. Or it is possible that men offered READI experienced fewer fatal overdoses and so were more likely to make it to the hospital. But it seems more likely that the increase in serious drug and mental health incidents reflects an adverse outcome.

There are mechanisms through which READI might have increased these incidents. For example, outreach workers often worked with men to reduce their substance use prior to referring them to READI, possibly contributing to their lower control mean relative to algorithm referrals. But being newly recovering from a substance or mental health issue might have put treatment group men—who, if they participated in READI, could be exposed to negative incidents from the increased workload, a new peer group, and increased income—in a more precarious position. Whatever the reason, given that few previous CVI studies have included these kinds of health outcomes, the results highlight the importance of expanding the way we study violence interventions to include additional outcomes.

5 Discussion

This study revisits one of the largest-scale preventative community violence interventions (CVIs) nearly three-and-a-half years after randomization. While the pre-specified index of serious violence remains statistically unchanged in the full sample, our extended 40-month analysis confirms that READI produced large, persistent reductions in serious violence for one subgroup: high-risk men referred by outreach workers. Meanwhile, effects remain null—or possibly adverse—for men referred by an algorithm.

These results hold across both police and hospital data. Since we must always take subgroup analysis with caution, this consistency across time and datasets is reassuring. The consistency of violence effects across administrative sources also suggests that police data, while imperfect, may not be systematically biased when tallying serious violence events.

The clearest takeaway is that how individuals are selected into a CVI critically shapes program impacts. Outreach workers appeared able to identify men who were not only at high

risk, but also more responsive to treatment. In contrast, the algorithm successfully predicted future gun violence risk, but its referrals did not benefit from program participation. This difference underscores the importance of both unobservable factors and personal relationships in targeting. Our results suggest that combining human and machine referral methods may outperform either alone. At the same time, these results underscore how little we understand about the mechanisms driving individual behavior change in CVIs, and the conditions under which it occurs.

The results also suggest that something more than short-term incapacitation is at work. Previously, during the program period, we found that the largest reductions in one measure of serious violence—shooting and homicide arrests—occurred outside of normal program hours. The persistence of effects for men referred by outreach workers in this paper further suggests that impacts go beyond incapacitation. It may be that early therapeutic and relational investments have lasting impacts on decision-making and peer dynamics. We see some evidence that treatment impacts diminished over time, but the decline is too imprecise to be certain. It also occurs against a backdrop of declining violence in the experimental sample as a whole. Longer term follow-ups will help answer this question.

Regardless of whether the effects are persistent or not, READI's benefits due to reduced crime and violence involvement appear to outweigh its costs by a significant amount. The benefit-cost ratio remains above 3.9:1, and potentially much higher, depending on the assumptions used to calculate the social cost of crime. These gains stem largely from reduced shootings and homicides—a small share of incidents, but ones that carry high social costs. Thus, impacts on the social cost-weighted index are statistically significant, even if those on the unweighted (but pre-specified) index are not. That said, the gains are concentrated: benefits accrue primarily to a subset of outreach-referred men with high predicted risk. Improving precision in targeting such individuals could significantly increase average treatment effects and program efficiency.

The new health outcomes complicate the picture somewhat, suggesting potential adverse

effects on outreach referrals even as their likelihood of being shot or killed declined. We observe a significant rise in overdose and mental health crisis visits among these men—a 200 percent increase from a low baseline. Future analysis will attempt to incorporate the costs of overdoses and mental health crises into our benefit-cost calculations. In the meantime, without significant changes in mortality due to these outcomes, we have no reason to expect this to change our conclusions about READI's cost-effectiveness.

From a policy perspective, READI continues to demonstrate that high-cost interventions can be justified for very high-risk groups. It also illustrates the limits of purely data-driven targeting, and the importance of blending predictive models with on-the-ground intelligence. Cities looking to scale CVIs should treat recruitment strategy not as a logistical detail, but as a core design choice.

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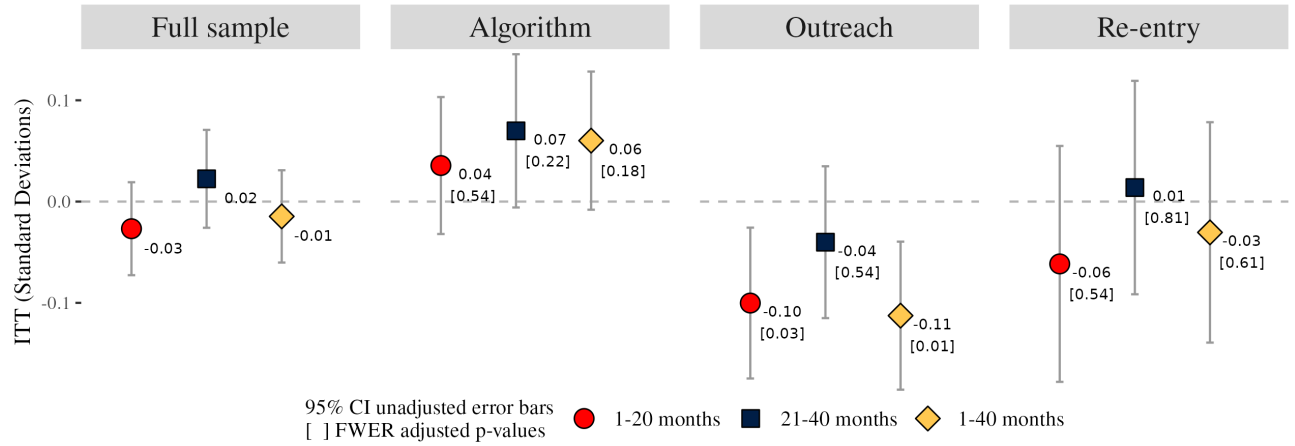
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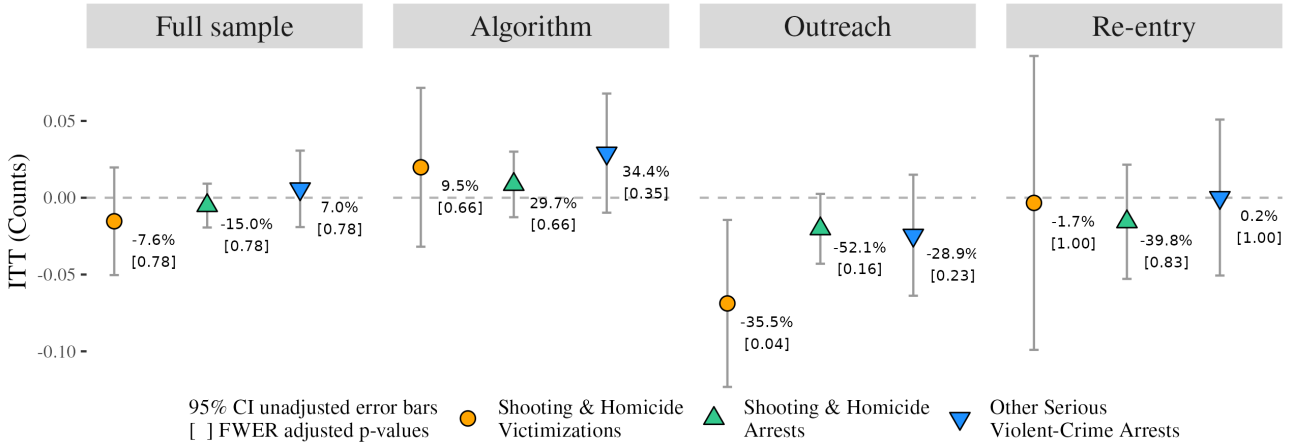
6 Figures and Tables

Figure 1: READI's estimated effects on serious violence involvement

Panel A: Primary index of serious violence involvement over time



Panel B: Primary outcome components, 1-40 months



Notes: Figure shows intent-to-treat (ITT) estimates from regressions of the indicated outcome on an indicator for assignment to an offer of READI, baseline covariates, and randomization strata fixed effects (equation 1). For heterogeneity by referral pathway, estimates for are from a single regression that interacts pathway indicators with treatment assignment. In Panel A, the dependent variable is the primary index of serious violence involvement, which standardizes each of the three components shown in Panel B based on the control group's distribution and takes their unweighted average. Each time period is standardized separately. Months 1-20 roughly correspond with the program period; months 21-40 roughly correspond to the post-program period; and months 1-40 are the cumulative total. In Panel B, the dependent variables are the primary index components for months 1-40. Family-wise error rate (FWER) p-values, reported in brackets, control for the family-wise error rate using the Westfall and Young (1993) method for step-down resampling. For multiple hypothesis testing adjustments within the primary index (Panel A), we define the three different referral pathways as a family. For multiple hypothesis testing adjustments within a referral pathway (Panel B), we define the three index components within each pathway as a family.

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Table 1: Hospital baseline characteristics and realized risk by pathway

	All	Algorithm	Outreach	Re-entry	P-value, Test of Pathway Difference
Baseline (12-month pre-randomization)					
N	2456	1232	881	343	
All Visits	1.3	1.6	0.925	0.802	<.001
% with Any Visit	0.477	0.571	0.398	0.344	<.001
Visit Category, Counts					
Violence	0.168	0.243	0.091	0.099	<.001
Shooting	0.087	0.132	0.039	0.047	<.001
Non-Shooting Violence	0.081	0.110	0.052	0.052	<.001
Overdose or Mental Health Crisis	0.029	0.037	0.019	0.023	0.198
Other Substance Abuse or Mental Health	0.203	0.267	0.109	0.213	0.002
Risky Behavior and Other Injury	0.286	0.359	0.235	0.155	<.001
Other	0.581	0.734	0.471	0.312	<.001
Realized Risk Among Controls (12-month post-randomization)					
N	1232	616	441	175	
All Visits	1.3	1.5	1.3	1.1	0.104
% with Any Visit	0.498	0.524	0.476	0.457	0.156
Visit Category, Counts					
Violence	0.146	0.151	0.127	0.177	0.462
Shooting	0.064	0.065	0.061	0.069	0.951
Non-Shooting Violence	0.082	0.086	0.066	0.109	0.412
Overdose or Mental Health Crisis	0.025	0.044	0.002	0.017	0.001
Other Substance Abuse or Mental Health	0.192	0.224	0.150	0.189	0.491
Risky Behavior and Other Injury	0.332	0.357	0.329	0.251	0.143
Other	0.649	0.693	0.658	0.474	0.039

Notes: Top panel shows 12-month baseline characteristics. Approximately 5 percent of the sample only have 10 or 11 months of baseline hospital data because of when they were randomized. Hospital visits are assigned to mutually exclusive categories based on diagnosis codes as described in section 3. Bottom panel shows control means during the 12 months post-randomization. P-value from joint test of the null that referral pathway means are equal using heteroskedasticity-robust standard errors.

Table 2: Hospital visits and victimizations reported to the police for violence, 1-40 months

	Hospital Data, Illinois	Police Data, Chicago	Difference %
Total Counts			
All Shootings	466	470	-1%
Fatal Shootings	25	91	-73%
Non-Fatal Shootings	441	379	16%
Non-Shooting Violence	561	1258	-55%
Control Counts			
All Shootings	239	243	-2%
Fatal Shootings	12	51	-76%
Non-Fatal Shootings	227	192	18%
Non-Shooting Violence	253	624	-59%
Treatment Counts			
All Shootings	227	227	0%
Fatal Shootings	13	40	-68%
Non-Fatal Shootings	214	187	14%
Non-Shooting Violence	308	634	-51%

Notes: Table shows counts of violent victimizations among study men in the hospital (IDPH) and police (CPD) data. Geographic coverage and type of incident varies across data sources; see discussion in section 3. Violent victimization outcomes in the hospital data include any visit with the relevant injury diagnosis code excluding those marked as a sequelae of a previous event. The diagnosis codes used to define "Non-Shooting Violence" are those related to violent injury identified by IDPH, excluding those related to shootings (see section 3). "Difference %" column reports the percent difference in incident counts between the hospital and police data.

Table 3: READI's estimated effects on violent victimization in hospital and police data, 1-40 months

	Hospital Data, Illinois			Police Data, Chicago		
	CM	ITT	ITT P-value	CM	ITT	ITT P-value
Full Sample (N = 2456)						
All Shootings	0.1940	-0.0076	0.714	0.1972	-0.0130	0.467
		(0.0208)			(0.0178)	
Non-Shooting Violence	0.2054	0.0371	0.189	0.5065	0.0287	0.618
		(0.0283)			(0.0576)	
Algorithm (N = 1232)						
All Shootings	0.2094	0.0138	0.662	0.2013	0.0258	0.325
		(0.0315)			(0.0263)	
Non-Shooting Violence	0.2273	0.1048	0.024	0.7143	-0.0341	0.746
		(0.0462)			(0.1052)	
Outreach (N = 881)						
All Shootings	0.1859	-0.0564	0.065	0.1905	-0.0701	0.011
		(0.0306)			(0.0276)	
Non-Shooting Violence	0.1701	-0.0186	0.628	0.3084	0.0866	0.131
		(0.0385)			(0.0573)	
Re-entry (N = 343)						
All Shootings	0.1600	0.0416	0.459	0.2000	-0.0087	0.859
		(0.0562)			(0.0492)	
Non-Shooting Violence	0.2171	-0.0653	0.289	0.2743	0.1108	0.200
		(0.0616)			(0.0865)	

Notes: Table reports intent-to-treat (ITT) estimates and p-values from regressions of the indicated outcome on an indicator for assignment to an offer of READI, baseline covariates, and randomization strata fixed effects (equation 1). Heteroskedasticity-robust standard errors are reported in parentheses. Violent victimization outcomes in the hospital data include any visit with the relevant injury diagnosis code excluding those marked as a sequelae of a previous event. The diagnosis codes used to define "Non-Shooting Violence" are those related to violent injury identified by IDPH, excluding those related to shootings (see section 3). For the police data, "All Shootings" includes any record of a shooting or gun-related homicide victimization, and "Non-Shooting Violence" includes any other incidence of violent victimization reported to the police. CM is the control mean. 7 outreach and 3 re-entry referrals who were grouped together due to confidentiality protections are counted as outreach.

Table 4: READI's estimated effects on hospital visits not due to violent victimization, 1-40 months by pathway

	CM	ITT	P-value	% Change	CM	ITT	P-value	% Change
	Full sample (N = 2456)				**Algorithm (N = 1232)**			
All Visits	3.9	0.191	0.469	4.9%	4.6	-0.097	0.815	-2.1%
		(0.264)				(0.413)		
% with Any Visit	0.736	0.025	0.139	3.4%	0.792	0.025	0.264	3.2%
		(0.017)				(0.022)		
Visit Category, Counts								
Overdose or Mental Health Crisis	0.102	-0.008	0.791	-7.8%	0.162	-0.055	0.313	-34.1%
		(0.030)				(0.055)		
Other Substance Abuse or Mental Health	0.592	0.120	0.345	20.3%	0.765	0.099	0.643	13.0%
		(0.127)				(0.214)		
Risky Behavior and Other Injury	0.936	0.079	0.198	8.4%	1.1	-0.039	0.656	-3.7%
		(0.061)				(0.088)		
Other	1.8	-0.030	0.814	-1.6%	2.1	-0.220	0.243	-10.3%
		(0.126)				(0.188)		
	Outreach (N = 881)				**Re-entry (N = 343)**			
All Visits	3.3	0.586	0.181	17.6%	2.7	0.213	0.585	7.8%
		(0.437)				(0.391)		
% with Any Visit	0.683	0.017	0.571	2.5%	0.674	0.047	0.335	7.0%
		(0.030)				(0.049)		
Visit Category, Counts								
Overdose or Mental Health Crisis	0.034	0.070	0.018	204.7%	0.063	-0.037	0.303	-58.5%
		(0.029)				(0.036)		
Other Substance Abuse or Mental Health	0.392	0.291	0.149	74.2%	0.486	-0.250	0.129	-51.5%
		(0.202)				(0.165)		
Risky Behavior and Other Injury	0.880	0.137	0.193	15.5%	0.600	0.363	0.007	60.5%
		(0.105)				(0.135)		
Other	1.7	0.164	0.453	9.9%	1.2	0.161	0.429	13.3%
		(0.218)				(0.204)		

Notes: Table reports intent-to-treat (ITT) estimates and p-values from regressions of the indicated outcome on an indicator for assignment to an offer of READI, baseline covariates, and randomization strata fixed effects (equation 1). Hospital visits are assigned to mutually exclusive categories based on diagnosis codes as described in section 3. CM is the control mean. "% Change" reports the proportional change of the coefficient relative to the CM. 7 outreach and 3 re-entry referrals who were grouped together due to confidentiality protections are counted as outreach.

Appendix

A.1 Additional details on visit and diagnosis categorization in hospital data

As noted in section 3, our process for categorizing hospital visits involves first assigning each diagnosis within a given visit to one of the following mutually exclusive categories: shooting injury, non-shooting violent injury, overdose or mental health crisis, other substance abuse or mental health disorder, risky behavior and other injury, and a final category encompassing all other diagnoses. We rank these categories in order to reflect their likely differing severity. Each visit is labeled with the category of its highest-ranked (i.e., most severe) diagnosis.[1a] Below we outline the specific criteria used assign a diagnosis to each of these mutually exclusive categories, from most-severe to least-severe:

Shooting injury: A diagnosis is considered a shooting if it meets any of the following criteria:[2a]

- Gun assault: any diagnosis code beginning with X93, X94, or X95
- Legal intervention involving firearm discharge: any diagnosis code beginning with Y350 that has a diagnosis description containing "suspect injured"
- Accidental discharge: any diagnosis code beginning with W32, W33, or W34
- Other or unspecified firearm discharge: any diagnosis code beginning with Y22, Y23, or Y24
- Any diagnosis description containing "puncture wound," when the visit does not have an additional known non-shooting diagnosis associated with it[3a]

Non-shooting violent injury: A diagnosis is considered a non-shooting violent injury if it has not already been classified as a shooting, and meets either of the following criteria:

- Any diagnosis code included in IDPH's list of violent injury inclusion codes from `https://dph.illinois.gov/topics-services/emergency-preparedness-response/ems/trauma-program/headspinal-cord-and-violent-injury-registry-hsvi/violent-injury-inclusion-codes.html`
- Any of the following assault diagnosis codes: X85-X99 or Y00-Y09

[1a] In some cases a visit might be classified only when multiple diagnoses are present in the same visit.

[2a] Any visit that also includes a diagnosis categorized as a suicide attempt (diagnosis codes beginning with T1491) is automatically categorized as a mental health crisis visit.

[3a] Known non-shooting diagnoses include any diagnosis code beginning with V, W0, W1, W2, W30, W31, W35, W36, W37, W38, W39, W4, W5, W6, W7, W8, W9, X0, X1, X3, X5, X71, X75, X76, X77, X78, X79, X8, X92, X96, X97, X98, X99, Y01, Y02, Y03, Y04, Y08, Y21, Y25, Y26, Y27, Y28, Y29, Y30, Y31, Y32, Y33, T74, T76, or Z04

Overdose: Any diagnosis code beginning with T36-T50, where the diagnosis description includes "poisoning by"

Mental health crisis: Any diagnosis with the following criteria:

- Suicide attempt: diagnoses beginning with T1491
- Severe or acute mental health diagnosis: any diagnosis code beginning with F, excluding F10-F19, where the diagnosis description also contains "severe" or "acute"
- Any mental health diagnosis when an additional intentional self-harm diagnosis is present within the same visit:
  - Mental health diagnosis: diagnosis code beginning with F (excluding F10-F19) or diagnosis codes R45851, R45850 (suicidal ideations)
  - Intentional self-harm diagnosis: any diagnosis description that contains "self-harm," excluding cases that contain "history"

Other substance abuse (non-overdose) or mental health (non-crisis) disorder: Other substance abuse (non-overdose) includes all diagnoses beginning with F10-F19, excluding those related to uncomplicated nicotine use[4a] or uncomplicated cannabis use[5a], as well as diagnoses beginning with K2920, K70, K852, K860, Q860, or R89

Mental health (non-crisis) includes all remaining diagnoses beginning with F (excluding F10-F19) that have not already been classified as mental health crisis above, as well as diagnosis codes R45851 and R45850 (suicidal ideations)

Risky behavior and other injury: This category includes the following:

- Car accidents: any diagnosis beginning with V0-V8[6a]
- Risky sexual behavior: any diagnosis beginning with A5, A60-A64, or Z202 (all STD related); Z725 (risk sexual behavior), Z1144 (HIV related), or R300 (dysuria)
- Other injuries as identified by Downes et al. (2022): diagnoses beginning with V, W, X, Y00-Y34, Y85-Y87, Y89

Other: all other diagnoses that do not fall into any of the above categories, including sequelae of shooting and non-shooting violent injury

[4a] Uncomplicated nicotine use includes any diagnosis beginning with F17200, F17201, F17210, F17211, F17220, F17221, F17290, F17291, Z0283, Z720, Z7722, or Z87891

[5a] Uncomplicated cannabis use includes any diagnosis beginning with F1210, F1211, F1220, F1290, or F1291

[6a] Given that car accidents are potentially more likely to be the primary reason for a given hospital visit, we rank these above other substance abuse or mental health crisis.

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Table A.1: Program take-up rates and participation conditional on take-up, by pathway

		Conditional on Take-up						
		Work Hours				CBT/Training Hours	Total Hours	Total Earnings
	Take-up Rate	25th Percentile	50th Percentile	75th Percentile	Average	Average	Average	Average
All Participants	55%	156	480	941	595	166	761	$10,245
Algorithm	37%	99	430	960	574	152	726	$9,766
Outreach	79%	199	564	993	645	178	822	$10,957
Re-entry	60%	128	441	792	511	162	673	$9,325

Note: Take-up defined as attending the first day of READI orientation. Hours and earnings are averages over the entire 40-month outcome period among those who took up. Work hours correspond to time spent at a worksite. CBT/training hours correspond to time spent in group CBT sessions, professional development sessions, and online trainings. Hours and earnings are limited to men who took up and appear in the payroll data. Participation records for CBT and training before April 2020 are incomplete; hours shown are extrapolated based on available data.

Table A.2: Baseline characteristics, hospital data, 12 months prior to randomization

	Control Mean	Treatment Mean	Pairwise p-value
N	1232	1224	
All Visits	1.2	1.3	0.740
% with Any Visit	0.470	0.484	0.432
Visit Category, Counts			
Violence	0.173	0.163	0.788
Shooting	0.091	0.083	0.688
Non-Shooting Violence	0.082	0.081	0.995
Overdose or Mental Health Crisis	0.024	0.033	0.361
Other Substance Abuse or Mental Health	0.176	0.230	0.365
Risky Behavior and Other Injury	0.272	0.300	0.336
Other	0.602	0.559	0.457
Joint Test			
p-value of F-test			0.655

Notes: Pairwise p-value from test of treatment-control difference using heteroskedasticity-robust standard errors and controlling for randomization strata fixed effects. Approximately 5 percent of the sample only have 10 or 11 months of baseline hospital data because of when they were randomized. Hospital visits are assigned to mutually exclusive categories based on diagnosis codes as described in section 3. Joint test includes coarsened randomization strata fixed effects (interaction of fixed effects for referral pathway, coarsened probability of randomization to treatment, and coarsened randomization period) and the covariates listed here.

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Table A.3: Baseline characteristics by pathway, hospital data, 12 months prior to randomization

	Algorithm pathway			Outreach pathway			Re-entry pathway		
	Control Mean	Treatment Mean	Pairwise p-value	Control Mean	Treatment Mean	Pairwise p-value	Control Mean	Treatment Mean	Pairwise p-value
N	616	616		441	440		175	168	
All Visits	1.6	1.6	0.974	0.934	0.916	0.851	0.663	0.946	0.293
% with Any Visit	0.575	0.567	0.773	0.381	0.416	0.294	0.326	0.363	0.305
Visit Category, Counts									
Violence	0.245	0.240	0.874	0.098	0.084	0.567	0.109	0.089	0.718
Shooting	0.133	0.131	0.897	0.045	0.032	0.351	0.057	0.036	0.769
Non-Shooting Violence	0.112	0.109	0.921	0.052	0.052	0.994	0.051	0.054	0.806
Overdose or Mental Health Crisis	0.037	0.037	0.997	0.011	0.027	0.287	0.011	0.036	0.244
Other Substance Abuse or Mental Health	0.248	0.286	0.637	0.102	0.116	0.769	0.109	0.321	0.429
Risky Behavior and Other Injury	0.344	0.373	0.493	0.218	0.252	0.422	0.154	0.155	0.940
Other	0.763	0.705	0.538	0.506	0.436	0.427	0.280	0.345	0.281
Joint Test									
p-value of F-test			0.934			0.384			0.445

Note: Pairwise p-value from test of treatment-control difference using heteroskedasticity-robust standard errors and controlling for randomization strata fixed effects. Approximately 5 percent of the sample only have 10 or 11 months of baseline hospital data because of when they were randomized. Hospital visits are assigned to mutually exclusive categories based on diagnosis codes as described in section 3. Joint test includes coarsened randomization strata fixed effects (interaction of fixed effects for referral pathway, coarsened probability of randomization to treatment, and coarsened randomization period) and the covariates listed here.

Table A.4: Baseline characteristics, police and incarceration data

	Control Mean	Treatment Mean	Pairwise p-value
N	1232	1224	
Demographics			
Black	0.969	0.971	0.864
Age	25.3	25.1	0.431
Primary Outcome Components, Counts			
Shooting Victimizations	0.479	0.436	0.119
Shooting & Homicide Arrests	0.081	0.074	0.526
Other Serious Violent-Crime Arrests	0.912	0.878	0.439
Risk Prediction			
Predicted Involvement in a Violent Gun Crime (Risk Score)	0.115	0.114	0.798
Missing Risk Score	0.108	0.080	0.008
Arrest Counts			
All Arrests	17.1	17.7	0.192
Less Serious Violent-Crime Arrests	1.5	1.5	0.921
Drug Crime Arrests	4.9	5.3	0.057
Property Crime Arrests	1.6	1.7	0.606
Other Crime Arrests	8.1	8.3	0.380
Victimization Counts			
All Victimizations	3.5	3.3	0.299
Other Violent Victimizations	2.4	2.3	0.491
Non-Violent Victimizations	0.592	0.571	0.644
Incarceration Measures			
Days Incarcerated	177.6	175.7	0.807
Incarcerated at Randomization	0.041	0.040	0.909
Joint Test			
p-value on F-test			0.328

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^{th} percentile. Other serious violent-crime arrests include criminal sexual assault, robbery, and non-shooting aggravated assault and battery. Less serious violent-crime arrests include non-Part I violent-crime arrests, such as simple assault and battery and domestic violence. 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 includes randomization strata fixed effects and the covariates listed here, excluding all arrests and all victimizations since they are linear combinations of other variables.

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Table A.5: READI's estimated effects on non-violent hospital visits, 1-20 months, by pathway

	CM	ITT	P-value	% Change	CM	ITT	P-value	% Change
	Full sample (N = 2456)				**Algorithm (N = 1232)**			
All Visits	2.1	0.137	0.352	6.4%	2.4	0.123	0.596	5.1%
		(0.147)				(0.231)		
% with Any Visit	0.614	0.014	0.474	2.2%	0.651	0.032	0.233	4.8%
		(0.019)				(0.026)		
Visit Category, Counts								
Overdose or Mental Health Crisis	0.042	0.003	0.799	7.6%	0.067	-0.014	0.478	-21.5%
		(0.013)				(0.020)		
Other Substance Abuse or Mental Health	0.306	0.084	0.268	27.6%	0.378	0.084	0.526	22.2%
		(0.076)				(0.132)		
Risky Behavior and Other Injury	0.521	0.041	0.317	7.8%	0.573	-0.017	0.766	-3.0%
		(0.041)				(0.058)		
Other	1.0	0.003	0.963	0.3%	1.1	-0.001	0.995	-0.1%
		(0.073)				(0.106)		
	Outreach (N = 881)				**Re-entry (N = 343)**			
All Visits	1.9	0.227	0.351	11.7%	1.7	-0.046	0.857	-2.7%
		(0.244)				(0.258)		
% with Any Visit	0.578	-0.010	0.757	-1.7%	0.577	0.009	0.857	1.6%
		(0.032)				(0.053)		
Visit Category, Counts								
Overdose or Mental Health Crisis	0.014	0.044	0.033	323.5%	0.029	-0.039	0.020	-137.5%
		(0.021)				(0.017)		
Other Substance Abuse or Mental Health	0.215	0.185	0.119	85.8%	0.280	-0.177	0.078	-63.1%
		(0.119)				(0.100)		
Risky Behavior and Other Injury	0.508	0.070	0.323	13.8%	0.371	0.178	0.060	47.9%
		(0.071)				(0.095)		
Other	1.0	-0.008	0.951	-0.8%	0.783	0.047	0.745	6.1%
		(0.128)				(0.146)		

Notes: Table reports intent-to-treat (ITT) estimates and p-values from regressions of the indicated outcome on an indicator for assignment to an offer of READI, baseline covariates, and randomization strata fixed effects (equation 1). Hospital visits are assigned to mutually exclusive categories based on diagnosis codes as described in section 3. CM is the control mean. "% Change" reports the proportional change of the coefficient relative to the CM. 7 outreach and 3 re-entry referrals who were grouped together due to confidentiality protections are counted as outreach.

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Table A.6: READI's estimated effects on non-violent hospital visits, 21-40 months, by pathway

	CM	ITT	P-value	% Change	CM	ITT	P-value	% Change
	Full sample (N = 2456)				**Algorithm (N = 1232)**			
All Visits	1.7	0.054	0.726	3.1%	2.2	-0.219	0.373	-10.1%
		(0.154)				(0.246)		
% with Any Visit	0.514	0.032	0.103	6.2%	0.588	0.009	0.738	1.6%
		(0.020)				(0.028)		
Visit Category, Counts								
Overdose or Mental Health Crisis	0.060	-0.011	0.636	-18.6%	0.096	-0.041	0.364	-42.9%
		(0.024)				(0.045)		
Other Substance Abuse or Mental Health	0.286	0.036	0.587	12.5%	0.386	0.015	0.888	4.0%
		(0.066)				(0.109)		
Risky Behavior and Other Injury	0.415	0.038	0.286	9.2%	0.498	-0.022	0.662	-4.5%
		(0.036)				(0.051)		
Other	0.794	-0.033	0.676	-4.2%	0.998	-0.219	0.072	-21.9%
		(0.079)				(0.122)		
	Outreach (N = 881)				**Re-entry (N = 343)**			
All Visits	1.4	0.358	0.135	26.1%	1.0	0.260	0.225	25.5%
		(0.240)				(0.214)		
% with Any Visit	0.456	0.051	0.121	11.2%	0.400	0.066	0.212	16.6%
		(0.033)				(0.053)		
Visit Category, Counts								
Overdose or Mental Health Crisis	0.020	0.026	0.097	125.5%	0.034	0.003	0.920	7.4%
		(0.015)				(0.025)		
Other Substance Abuse or Mental Health	0.177	0.106	0.274	60.0%	0.206	-0.074	0.433	-35.8%
		(0.097)				(0.094)		
Risky Behavior and Other Injury	0.372	0.067	0.289	18.0%	0.229	0.185	0.017	80.9%
		(0.063)				(0.077)		
Other	0.653	0.171	0.192	26.3%	0.429	0.114	0.288	26.5%
		(0.131)				(0.107)		

Notes: Table reports intent-to-treat (ITT) estimates and p-values from regressions of the indicated outcome on an indicator for assignment to an offer of READI, baseline covariates, and randomization strata fixed effects (equation 1). Hospital visits are assigned to mutually exclusive categories based on diagnosis codes as described in section 3. CM is the control mean. "% Change" reports the proportional change of the coefficient relative to the CM. 7 outreach and 3 re-entry referrals who were grouped together due to confidentiality protections are counted as outreach.

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Table A.7: Baseline characteristics and 40-month realized risk by pathway, police and incarceration data

	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	0.963	0.986	0.953	<.001
Shooting Victimizations	0.458	0.631	0.287	0.275	<.001
Shooting & Homicide Arrests	0.078	0.083	0.064	0.095	0.109
Other Serious Violent-Crime Arrests	0.895	1.0	0.667	1.0	<.001
Ever Shot	0.347	0.463	0.233	0.225	<.001
Ever Arrested	0.977	1.0	0.945	0.974	<.001
All Arrests	17.4	20.4	13.6	16.1	<.001
Ever Victimized	0.835	0.908	0.768	0.746	<.001
All Victimizations	3.4	4.5	2.3	2.3	<.001
Days Incarcerated	176.6	130.3	156.3	393.1	<.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	0.201	0.208	0.194	0.197	0.877
Shooting & Homicide Arrests	0.034	0.029	0.039	0.039	0.654
Other Serious Violent-Crime Arrests	0.083	0.084	0.084	0.073	0.888
Ever Shot or Killed	0.182	0.201	0.160	0.169	0.200
Ever Arrested	0.725	0.773	0.678	0.674	0.001
All Arrests	2.4	2.7	2.1	1.9	<.001
Ever Victimized	0.464	0.531	0.400	0.393	<.001
All Victimizations	0.933	1.2	0.662	0.624	<.001
Days Incarcerated	157.0	161.5	137.3	190.1	0.104
Involved in a Violent Gun Crime	0.159	0.174	0.148	0.135	0.335

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 99^{th} 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 for the control group. With the exception of that 18-month outcome period, the rest of the bottom panel shows control means during the 40-month outcome period. P-value from joint test of the null that referral pathway means are equal using heteroskedasticity-robust standard errors. Other serious violent-crime arrests include criminal sexual assault, robbery, and non-shooting aggravated assault and battery

Table A.8: Control means of primary index components, by outcome period

	All	Algorithm	Outreach	Re-entry
N Controls	1232	616	438	178
1 - 20 month control means				
Shooting & Homicide Victimizations	0.110	0.106	0.114	0.118
Shooting & Homicide Arrests	0.027	0.023	0.032	0.028
Other Serious Violent-Crime Arrests	0.054	0.057	0.050	0.056
21 - 40 month control means				
Shooting & Homicide Victimizations	0.090	0.102	0.078	0.079
Shooting & Homicide Arrests	0.006	0.005	0.007	0.011
Other Serious Violent-Crime Arrests	0.028	0.028	0.034	0.017
1 - 40 month control means				
Shooting & Homicide Victimizations	0.200	0.208	0.192	0.197
Shooting & Homicide Arrests	0.033	0.028	0.039	0.039
Other Serious Violent-Crime Arrests	0.083	0.084	0.084	0.073

Notes: Other serious violent-crime arrests include criminal sexual assault, robbery, and non-shooting aggravated assault and battery

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Table A.9: READI's estimated effects on serious violence involvement, 1-40 months

	Estimates				P-values		
	CM	ITT	CCM	TOT	Observed ITT	FWER	FDR-q
Primary Index of Serious Violence							
	0	-0.0146	-0.0098	-0.0267	0.529		
		(0.0233)		(0.0408)			
Primary Outcome Components, Counts							
Shooting & Homicide Victimizations	0.2013	-0.0153	0.2024	-0.0280	0.391	0.779	0.649
		(0.0179)		(0.0314)			
Shooting & Homicide Arrests	0.0341	-0.0051	0.0287	-0.0093	0.483	0.779	0.649
		(0.0073)		(0.0128)			
Other Serious Violent-Crime Arrests	0.0828	0.0058	0.0818	0.0106	0.649	0.779	0.649
		(0.0127)		(0.0223)			

Notes: 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 non-shooting 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 intent-to-treat estimates; CCM shows control complier means; and TOT shows 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.

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Table A.10: READI's estimated effects on serious violence involvement, by baseline risk level, 1-40 months

	Estimates				P-values		
	CM	ITT	CCM	TOT	Observed ITT	FWER	FDR-q
Primary Index of Serious Violence by Risk Level							
Over Median (N = 1112)	0.0898	-0.0272	0.0931	-0.0602	0.492	0.739	0.737
		(0.0396)		(0.0840)			
Under Median (N = 1113)	-0.0556	0.0091	-0.0644	0.0149	0.775	0.777	0.775
		(0.0319)		(0.0502)			
Missing (N = 231)	-0.1508	-0.0696	-0.1329	-0.1023	0.191	0.466	0.574
		(0.0533)		(0.0756)			
Primary Outcome Components by Risk Level, Counts							
Over Median							
Shooting & Homicide Victimizations	0.2527	-0.0121	0.2472	-0.0267	0.682	0.897	0.957
		(0.0295)		(0.0626)			
Shooting & Homicide Arrests	0.0466	-0.0108	0.0436	-0.0239	0.386	0.763	0.957
		(0.0125)		(0.0265)			
Other Serious Violent-Crime Arrests	0.1111	0.0012	0.1235	0.0025	0.957	0.954	0.957
		(0.0220)		(0.0467)			
Under Median							
Shooting & Homicide Victimizations	0.1664	-0.0031	0.1705	-0.0052	0.900	0.990	0.917
		(0.0249)		(0.0393)			
Shooting & Homicide Arrests	0.0259	-0.0010	0.0216	-0.0016	0.917	0.990	0.917
		(0.0097)		(0.0153)			
Other Serious Violent-Crime Arrests	0.0684	0.0129	0.0643	0.0211	0.465	0.850	0.917
		(0.0176)		(0.0278)			
Missing							
Shooting & Homicide Victimizations	0.1278	-0.0910	0.1792	-0.1337	0.082	0.196	0.245
		(0.0522)		(0.0745)			
Shooting & Homicide Arrests	0.0150	0.0028	0.0109	0.0043	0.856	0.939	0.856
		(0.0154)		(0.0218)			
Other Serious Violent-Crime Arrests	0.0226	-0.0066	0.0100	-0.0100	0.756	0.939	0.856
		(0.0214)		(0.0302)			

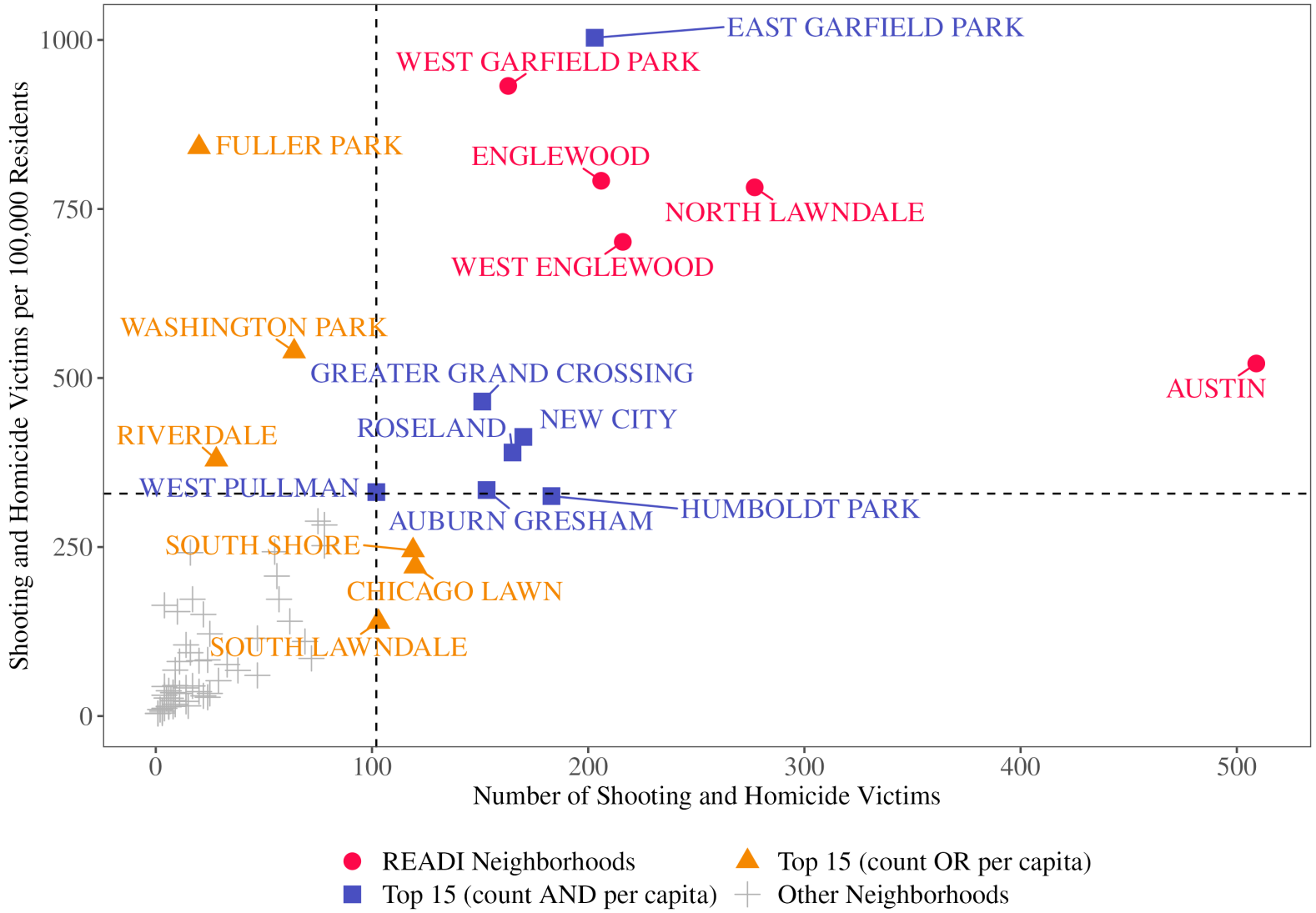
Notes: Estimates for each outcome are from a single regression that interacts indicators for above/below-median risk score and missing risk score with treatment. The median risk score in the study sample, the predicted probability at baseline of being arrested for or the victim of a violent gun crime 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 non-shooting 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 intent-to-treat estimates; CCM shows control complier means; and TOT shows 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.11: READI's estimated effect on the social costs of crime, 1-40 months

	Less Inclusive Estimates				More Inclusive Estimates			
	CM	ITT	CCM	TOT	CM	ITT	CCM	TOT
Social Cost of Victimization								
READI Sample Victims	$265,108	-$52,126	$267,985	-$95,178	$699,145	-$129,558	$713,395	-$236,563
		($43,193)		($75,798)		($106,687)		($187,241)
READI Sample Offenders	$164,076	-$63,042*	$194,307	-$115,110*	$1,379,964	-$343,533	$1,509,840	-$627,267
		($34,493)		($60,591)		($236,582)		($415,450)
Social Cost of Punishment								
Legal System Costs	$14,400	-$3,856*	$16,308	-$7,041*	$21,400	-$5,116**	$23,433	-$9,342**
		($2,269)		($3,987)		($2,450)		($4,303)
Productivity Loss from Incarceration	$5,940	-$1,814*	$6,802	-$3,313*	$9,185	-$2,406**	$10,107	-$4,393**
		($1,059)		($1,860)		($1,143)		($2,008)
Total Social Cost of Crime	$449,524	-$120,838**	$485,403	-$220,642**	$2,109,694	-$480,613*	$2,256,774	-$877,565*
		($56,059)		($98,416)		($257,904)		($452,863)
Social Cost of Program								
Administrative Costs		$36,669		$66,955		$36,669		$66,955
Transfer to Participants		-$5,611		-$10,245		-$5,611		-$10,245
Net READI Costs		$31,058		$56,710		$31,058		$56,710
Benefit-Cost Ratio		3.9:1		3.9:1		15.5:1		15.5:1

Notes: 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 40-month outcome period. CM shows control means; ITT shows intent-to-treat estimates; CCM shows control complier means; and TOT shows 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 A.1: Shooting victims per 100,000 residents (2016), by neighborhood



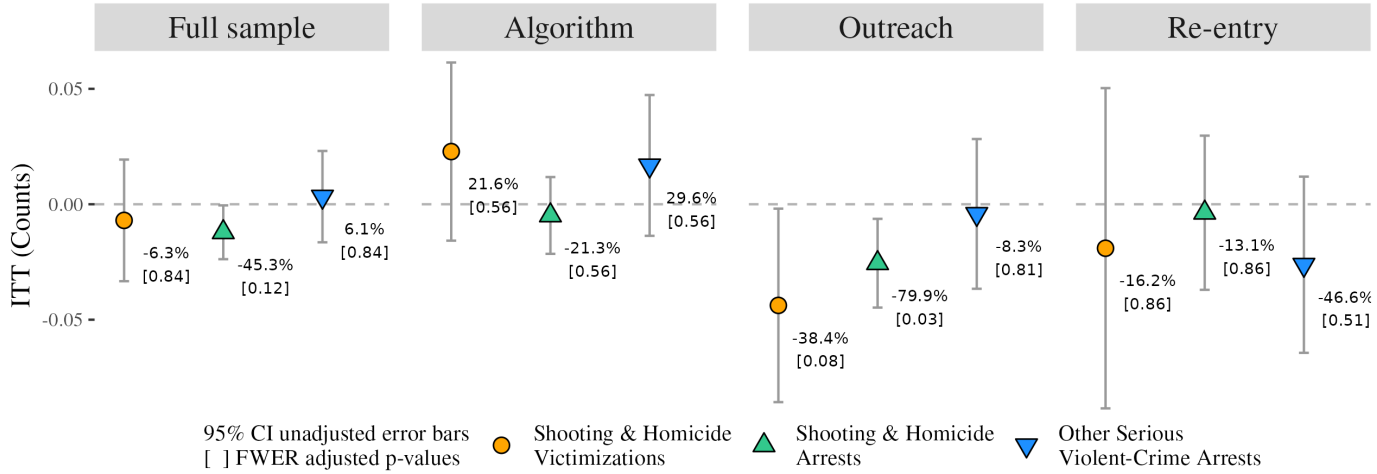
Notes: Plot shows counts and rates of shooting and homicide victims in each of Chicago's 77 neighborhoods in 2016. Dashed lines represent top 15 neighborhoods for each dimension.

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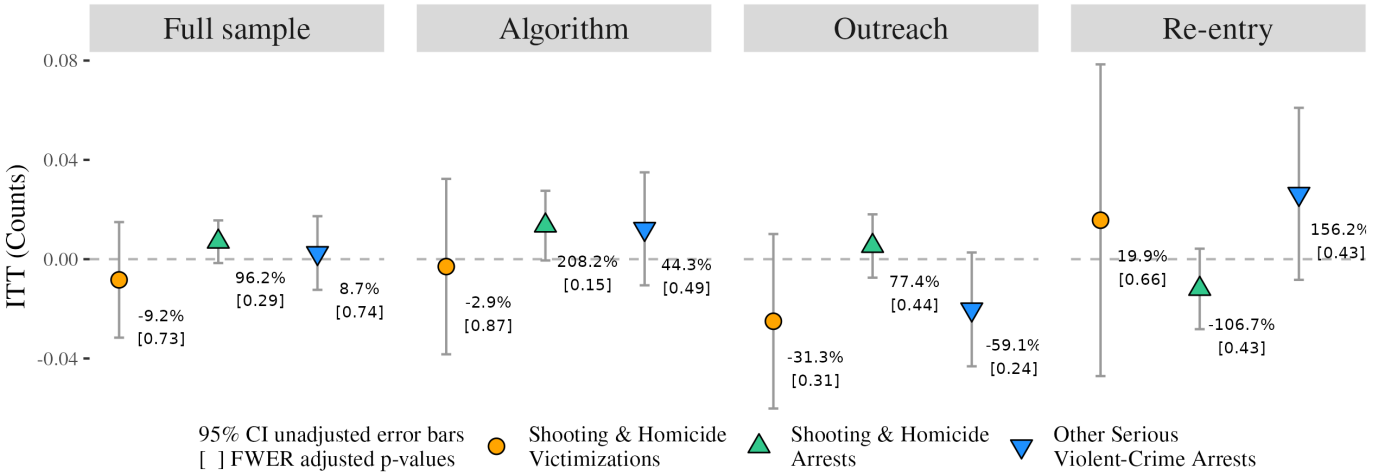
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Figure A.2: READI's estimate effects on serious violence involvement, by time period

Panel A: Primary outcome components, 1-20 months

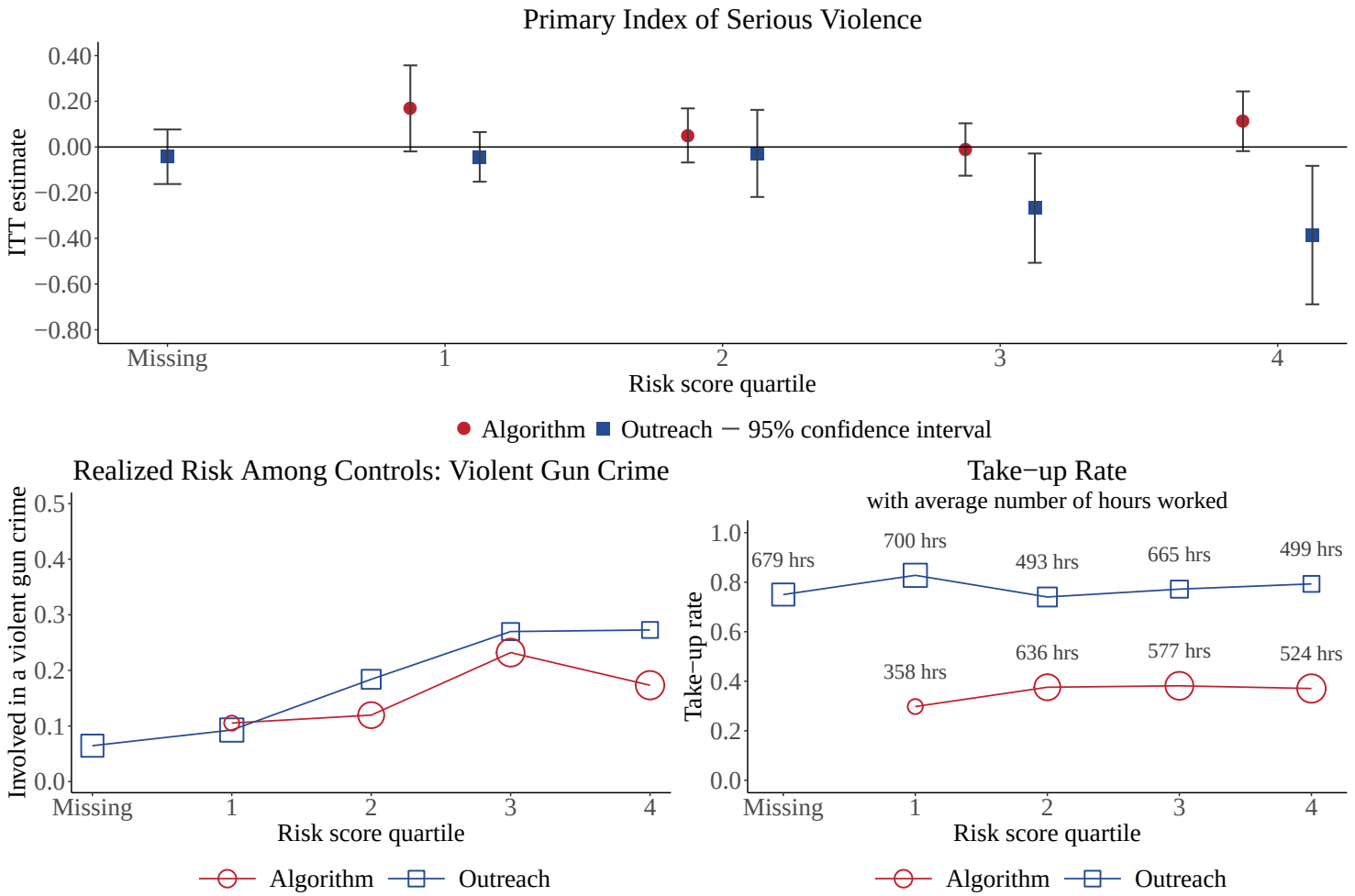


Panel B: Primary outcome components, 21-40 months



Notes: See notes for Figure 1, Panel B. Months 1-20 roughly correspond with the program period, and months 21-40 roughly correspond to the post-program period.

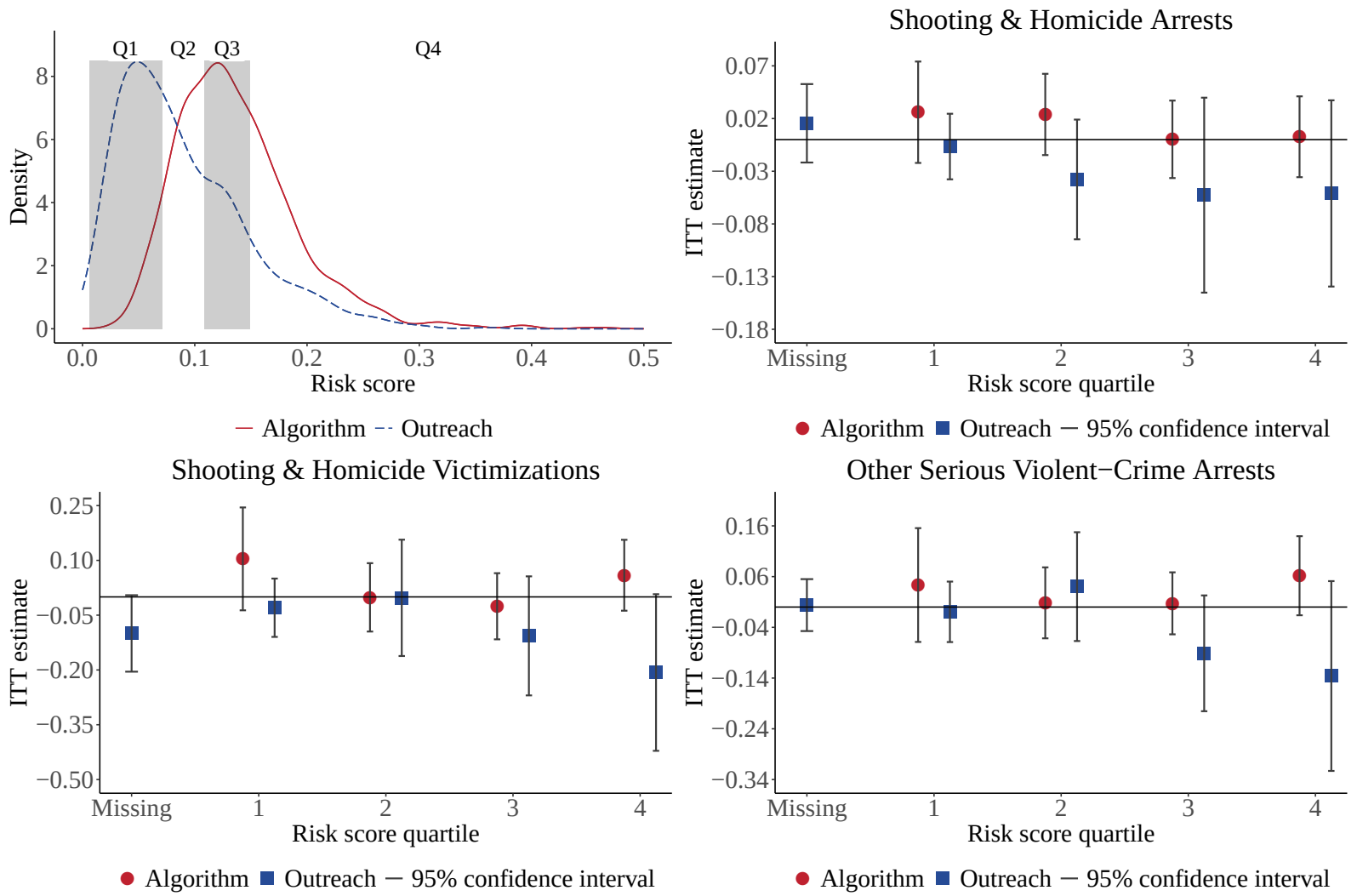
Figure A.3: READI's estimated effects, realized risk, and take-up by pathway and predicted risk, 1-40 months



Notes: The top panel shows coefficient estimates and 95 percent confidence intervals (using heteroskedasticity-robust standard errors) on three-way interactions of pathway indicators, risk score 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 score quartile indicators, and an indicator for being randomized to receive a READI offer. Bottom 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. Bottom 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 bottom panels are weighted to reflect the share of algorithm and outreach referrals in each risk score quartile. N = 231 for the missing risk score group (of whom 161 are outreach referrals). For ITT estimates of each individual index component by pathway and risk score quartile, see Appendix Figure A.4.

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Figure A.4: Distribution of risk scores by pathway and estimated effects on index components by pathway and risk level



Notes: Top left panel shows distributions of the risk score, the predicted probability at baseline of being a victim or an arrestee in a violent gun crime during the next 18 months, by pathway. Shaded areas denote quartiles of the risk score. The remaining panels show 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 regressions of the primary index components 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.

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