ONLINE APPENDIX

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

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A.1 Research on policing and CVIs

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

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

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

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

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

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

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

A.2 Data details

A.2.1 Chicago Police Department

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

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

A.2.2 Incarceration data: jail and prison

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

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

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

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

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

A.2.3 Data from READI providers

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

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

A.2.4 Data linkage

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

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

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

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

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

A.2.5 Arrest data & bias

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

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

A.3 Randomization details

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

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

A.3.1 Algorithm referral pathway

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

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

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

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

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

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

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

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

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

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

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

A.3.2 Outreach referral pathway

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

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

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

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

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

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

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

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

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

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

A.3.3 Re-entry referral pathway

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

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

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

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

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

A.3.4 Variation in treatment probability within strata

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

A.4 Program details

A.4.1 Jobs and wages

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

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

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

A.4.2 CBT

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

A.4.3 Safety during READI programming

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

A.5 Additional results

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

A.5.1 Covariates and functional form

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

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

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

A.5.2 Scaling for incapacitation

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

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

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

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

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

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

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

A.5.3 COVID-19

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

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

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

A.5.4 Spillovers

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

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

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

As discussed in the main text, the evidence in Craig et al. (2199) provides some weak evidence that the ITT effect on serious violent-crime arrests that are not shootings or homicides may be an inflated measure of READI's direct effect (i.e., that the actual direct effect on these kinds of arrests could be negative despite the positive ITT estimate, due to differential spillovers onto treatment versus control peers of treated individuals). But the clearest evidence of a SUTVA violation is for drug-crime arrests, where it appears that READI actually decreases these arrests both among those directly treated and among controls who are exposed to treated peers.

A.5.5 Dynamic treatment effects

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

A.5.6 Incapacitation as mechanism

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

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

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

A.5.7 Other outcomes

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

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

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

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

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

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

A.5.8 Heterogeneity

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

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

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

A.5.9 Assessing role of observables in treatment heterogeneity

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

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

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

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

A.6 Details on benefit-cost comparison

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

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

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

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

A.6.1 Cost of crime estimates

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

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

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

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

A.6.2 Details on calculations

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

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

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

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

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

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

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

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

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

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

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

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

A.7 Appendix tables and figures

Table A.1: Baseline balance within pathways

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

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

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

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

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

Table A.3: Robustness to alternative specifications

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table A.12: Outreach pathway and reweighted ITT estimates

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

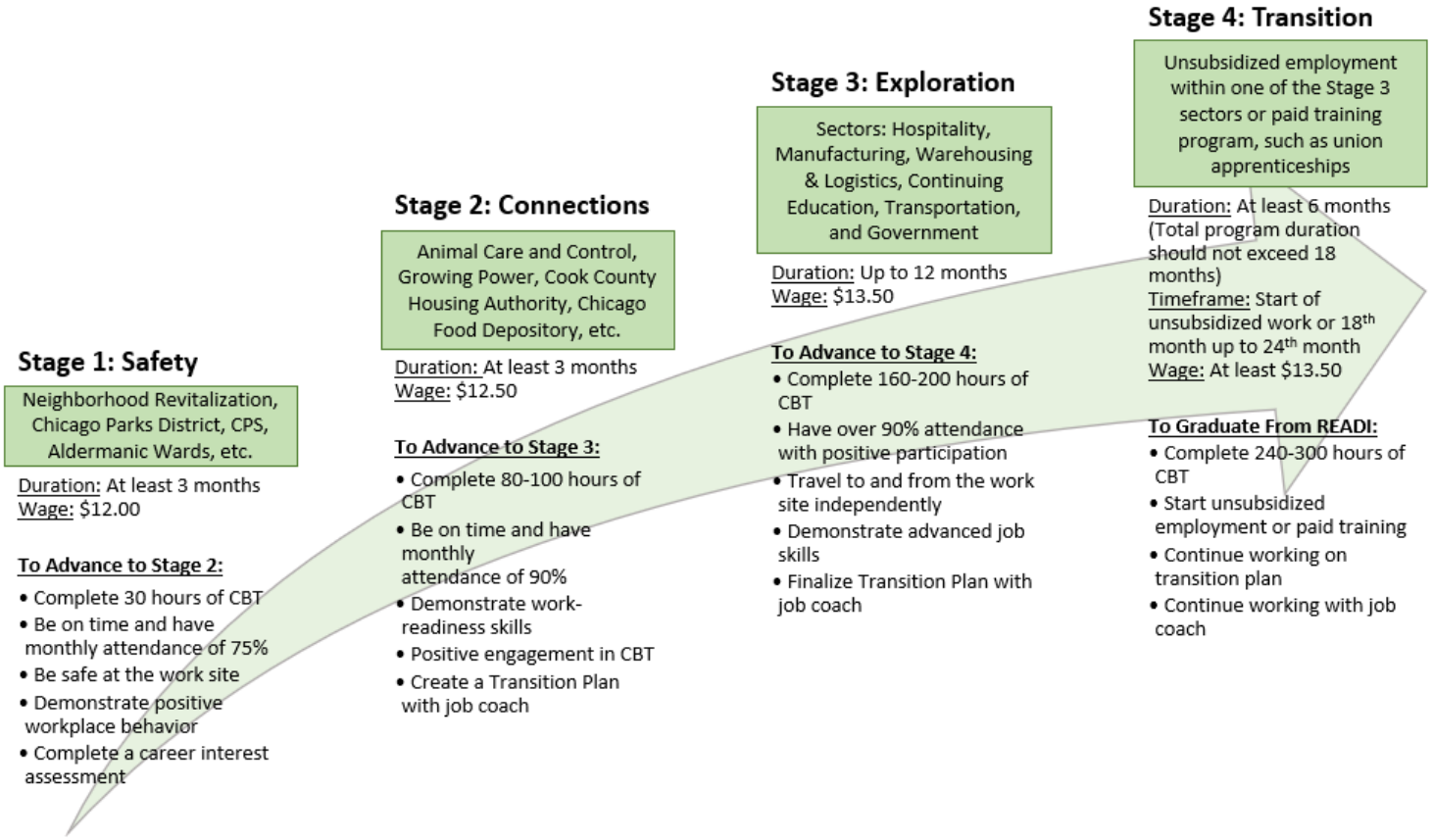
Notes: For the entire sample, we estimate predicted probabilities, $\hat{p}$, of being an outreach referral using a logit on neighborhood fixed effects, age at randomization, and pre-randomization characteristics including prior arrest, victimization, and incarceration. Sample is restricted to enforce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

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

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

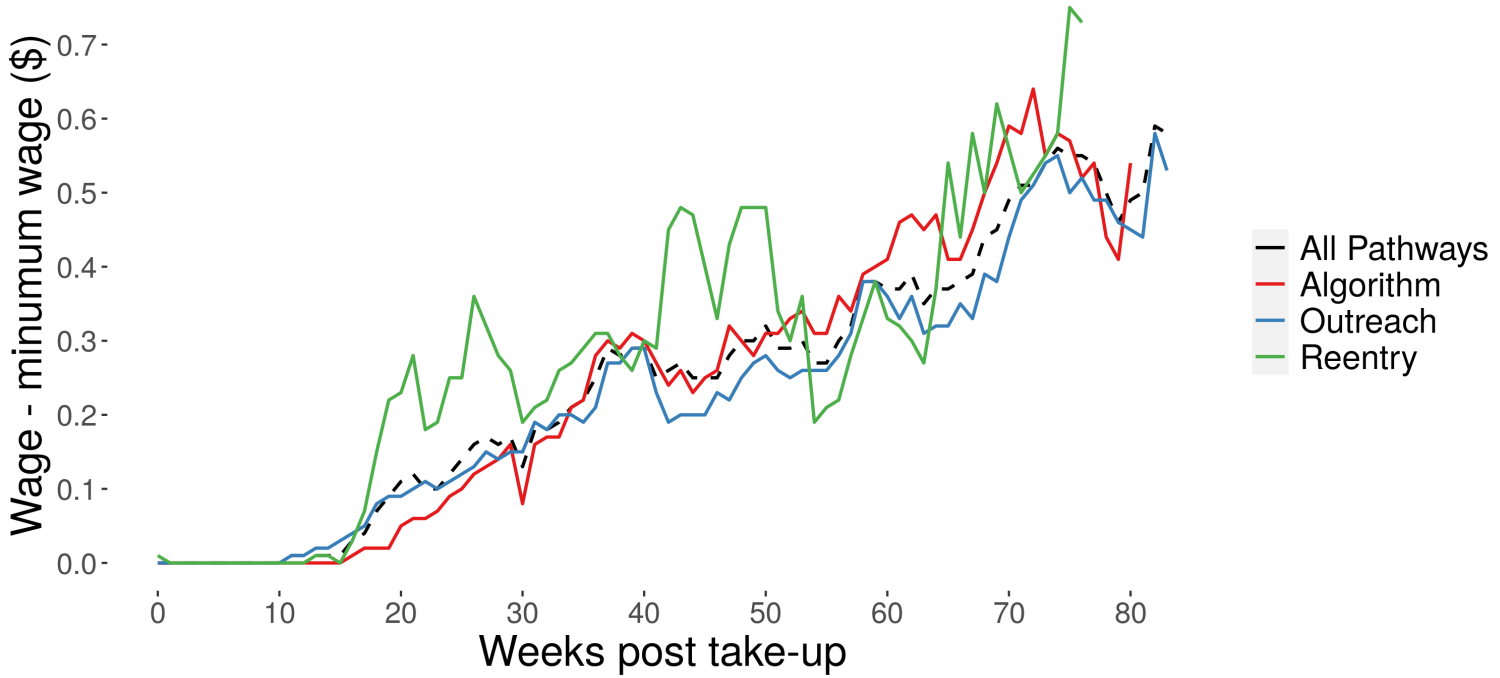
Notes: Crime types, bottom-up components, and willingness-to-pay costs are based on estimates from Cohen and Piquero (2009). We inflate to 2017 dollars and subtract legal system and offender productivity costs from willingness-to-pay estimates to avoid double counting. Total bottom-up costs are the result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

Figure A.1: READI job stage progression



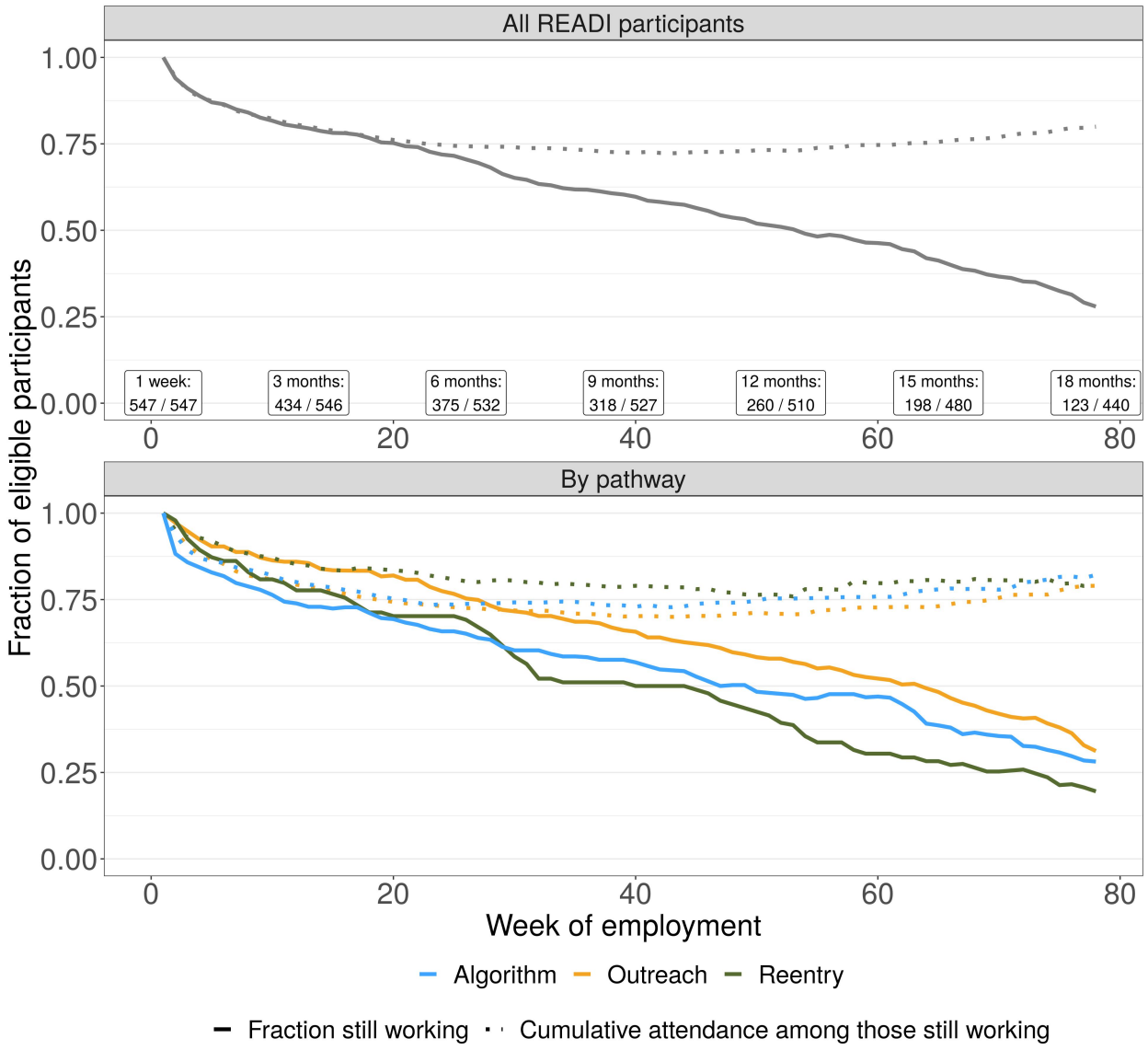
Notes: The duration and advancement requirements for READI's job stages were subject to change based on a participant's needs and progress. Program staff exercised discretion in deciding which participants were ready to advance job stages. The diagram shows READI's initial design; the details of implementation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

Figure A.2: READI wage growth by pathway



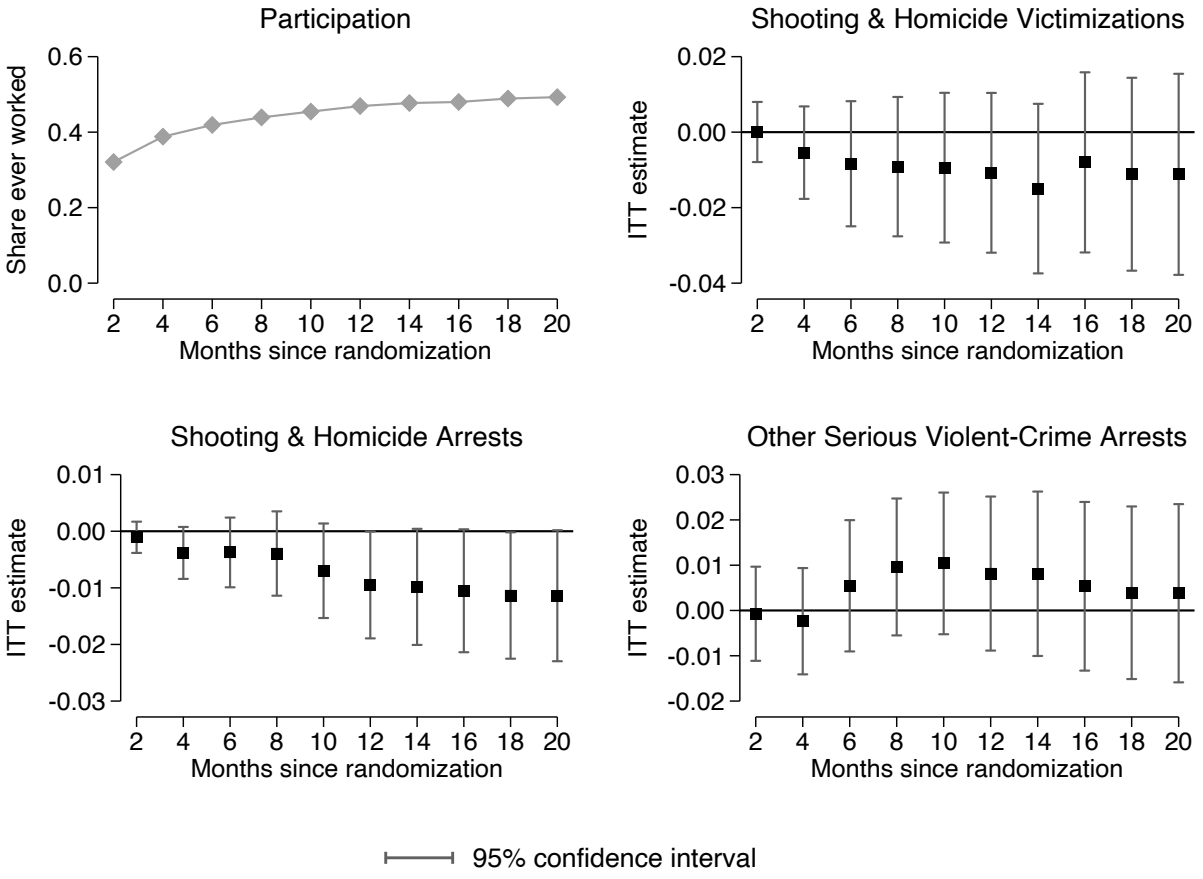
Notes: READI's starting wage was $11 at its launch in August 2017, increased to $12 in July 2018, and to $13 in July 2019. Average wage is calculated using only participants who report to work during a given week.

Figure A.3: READI job retention, overall and by pathway, including COVID period



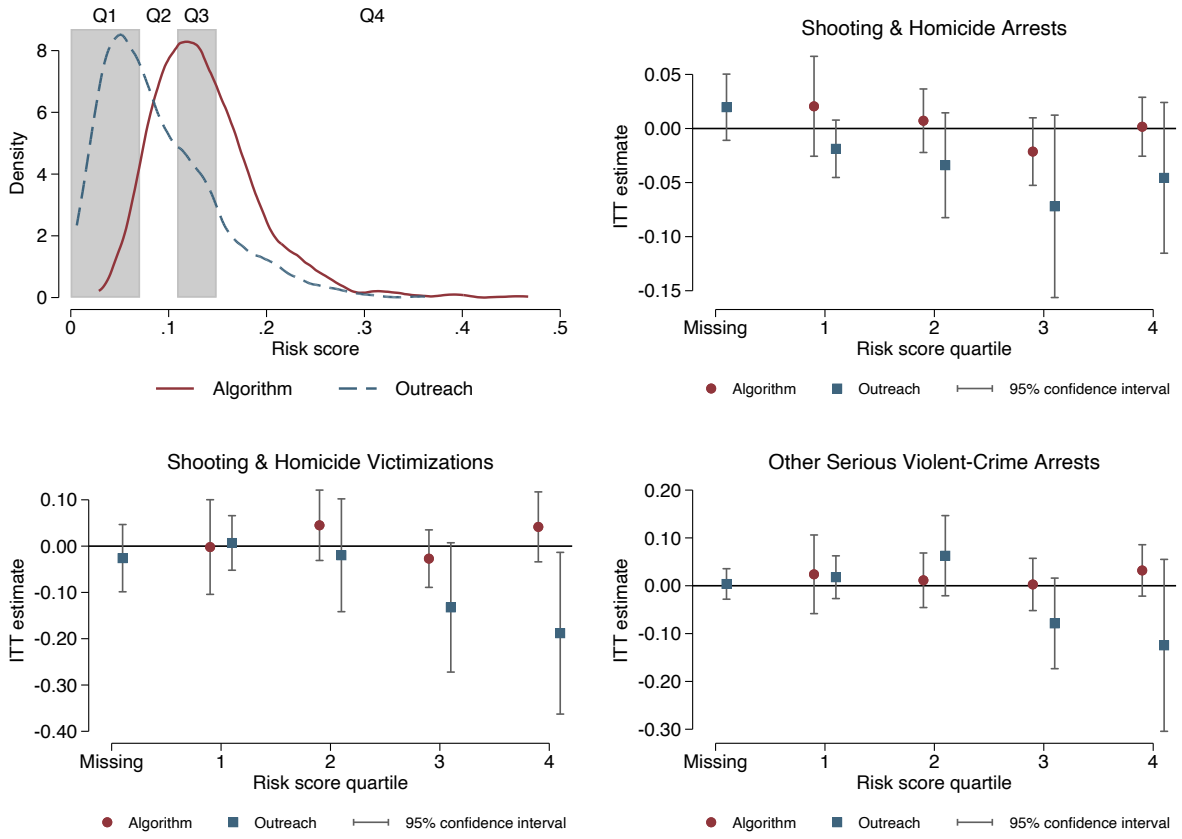
Notes: Figure shows two measures of job retention for men who started READI measured from payroll data. The solid line shows the proportion of participants who work at least once after the time shown on the x-axis conditional on observing them for that long. The boxes show the number of workers contributing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

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



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

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



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.