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Marco Castillo
Texas A&M University, IZA and CESifo

Sera Linardi
University of Pittsburgh

Ragan Petrie
Texas A&M University and CESifo

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Recidivism and Barriers to Reintegration: A Field Experiment Encouraging Use of Reentry Support*

Many previously incarcerated individuals are rearrested following release from prison. We investigate whether encouragement to use reentry support services reduces rearrest. Field experiment participants are offered a monetary incentive to complete different dosages of visits, either three or five, to a support service provider. The incentive groups increased visits, and one extra visit reduces rearrests three years after study enrollment by six percentage points. The results are driven by Black participants who are more likely to take up treatment and benefit the most from visits. The study speaks to the importance of considering first-stage heterogeneity and heterogeneous treatment effects.

JEL Classification: K42, C93

Keywords: recidivism, reentry support services, dosage effects, field experiment

Corresponding author:
Marco Castillo
Department of Economics
Texas A&M University
4228 TAMU
College Station, TX 77843
USA

E-mail: marco.castillo@tamu.edu

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1 Introduction

Evidence suggests the period after release from incarceration is critical. Individuals often attempt to re-integrate into society without basic needs secured (Roman and Travis, 2006; Geller and Curtis, 2011), i.e. housing, clothing or a cell phone, and the connection and social support of friends and family (Denney et al., 2014). This makes the transition challenging. Of the 400,000 individuals released from state prisons in 2005, almost half are re-arrested within the first year and one-third within the first six months (Durose et al., 2014). Reentry services in the United States are intended to provide support for this transitional period, however utilization is low. The average inmate completes just under 0.6 rehabilitation programs a year in jail (Kuziemko, 2013), and less than 50% of individuals released to parole with a referral to community treatment attended any session at all (Prendergast et al., 2003).[1] Many factors may contribute to this low usage, including sizeable nuisance costs and inertia.[2] Increased usage of reentry services might make the transition smoother, more likely to be successful and ultimately reduce recidivism. We investigate this possibility. With a partner aftercare service provider, we conduct a field experiment that encourages previously-incarcerated individuals to use support services by offering a monetary incentive when a goal of a certain number of visits is met.

Crucially, our experimental design does not require participants to use a predetermined set of services but rather those deemed most adequate by the participant. By allowing participants to choose service usage, we reduce the chance of mismatching treatments to participants. We find that the use of reentry services is effective at reducing arrests up to three years after study enrollment. This effect is largest among participants with higher resistance to treatment, consistent with negative selection. We also find significant hetero-

[1]Prisoner reentry services in the United States are made up of an informal assortment of government and nonprofit organizations, which makes it difficult to estimates the overall usage of reentry service (Nhan et al., 2017).

[2]Examples of not following through on actions that are challenging, but beneficial, can be found in applying for financial aid (Bettinger et al., 2012), saving for the future (Madrian and Shea, 2001) or getting prescription refills (Besheears et al., 2012).

geneity in the demand for reentry services and effectiveness: Black participants are more likely to visit the provider and experience the largest reduction in rearrest rates.

Reentry services are available through federal, state, nonprofit and privately-funded providers.[3] Services aim to help navigate post-incarceration life and can be comprehensive, i.e. providing food, clothing, identification cards, housing and job referrals, training, counseling and peer support. Several randomized controlled trials have evaluated these types of programs (Grommon et al., 2013; Cook et al., 2015; Wiegand and Sussell, 2016; D'Amico and Kim, 2018) by randomizing a treatment group of voluntary participants to receive specialized services. The estimated intent-to-treat effects are mixed, showing null, positive and negative effects on reducing recidivism. Many participants do not fully utilize all services offered or attend all sessions of a service program. Thus, treatment service programs are often not completed as designed, i.e. some participants do not "comply" with treatment.[4] Mixed findings could result from ineffective programs or incomplete treatment. Treatment dosage may be low, not by design but, because participation in the program was incomplete. Support services might be more effective at reducing recidivism if participants would use them more frequently or receive the intended dose. In our study, we fix the reentry services available and vary the dosage of services used by offering different incentivized visit goals. We then examine whether more service usage causally reduces recidivism.

The recent literature on optimal treatment assignment shows that there might be gains by improving the matching of treatments to different populations (see Kitagawa and Tetenov,

[3]The National Reentry Resource Center provides information for re-entrants and listings of all Second Chance Act grantees in the U.S. to help re-entrants connect with service providers in their communities.

[4]Grommon et al. (2013) report compliance rates decline during the year-long program of a substance abuse treatment program. In the first phase of the program, participants completed an average of 6.5 hours a week of the 10 hour planned treatment. In review papers, Doleac et al. (2019); Doleac (2023) notes several possible reasons for mixed ITT results. Individual programs cannot overcome the large barriers to successful reentry. Wrap-around services may be challenged to deliver multiple programs. If case management is part of these services, it implies a higher frequency of personal contact and potentially more scrutiny and monitoring. D'Amico and Kim (2018) find a slight increase in the total number of rearrests for those in the treatment group, likely due to more intensive case management. Increasing the level of supervision for probationers and parolees either has no impact on the likelihood of committing new offenses (Lane et al., 2005; Barnes et al., 2012; Boyle et al., 2013; Hyatt and Barnes, 2017) or increases recidivism (Lee, 2022). Prendergast et al. (2015) uses monetary incentive and finds no effects on attendance at a five-month long community substance abuse treatment among prisoner and parolees.

2018; Athey and Wager, 2021) and by allowing some participants to choose to be treated or not (Ida et al., 2022). Heterogeneity in selection into treatment and treatment heterogeneity in observables and unobservables might explain disparate effects of reentry policies. As shown by Abadie et al. (2024), heterogeneity in treatment participation might also be of consequence in estimating the treatment on the treated parameters. Our experimental design addresses the issue of heterogeneity and treatment selection by recruiting participants from diverse backgrounds and encouraging reentry services without limiting them to a predetermined set of options. The advantage of this approach is that it uncovers marked differences in demand for services by race and separately identifies the effect of different services on reducing recidivism.

The field experiment is implemented in partnership with a Pittsburgh, PA support service provider for the previously incarcerated. Individuals are recruited into the study and randomized into three groups that vary the number of required visits to the provider for a fixed monetary incentive. The Control group has no required visits or monetary incentive. Participants in the Easy treatment need to complete three visits to receive a \$50 incentive, and those in the Hard treatment must complete five visits to receive the \$50 incentive. The number of visits chosen for the Easy and Hard treatments were calibrated to historical data from our partner and were chosen to be attainable and potentially encourage enough service usage to facilitate a more successful transition to non-prison life. The design focuses on visits, rather than use of particular services, to allow participants to choose services that best meet their needs. We vary the dosage, rather than the incentive, because this gives us variation in visits and we can test the causal impact of service usage on recidivism.

The main outcomes we examine are number of visits to the provider within a year of study enrollment and probability of arrest up to three years after enrollment. We link data from our intake survey with the participants, administrative data on participants' visits from our partner and publicly-available, administrative data on arrests from the state of Pennsylvania. These data allow us to examine frequency and timing of visits, services used and frequency and timing of arrests by treatment and to explore heterogeneity in treatment response.

Our results show that the incentivized goal treatments did increase visits relative to the Control group. Those in the Easy group completed more visits than those in the Hard group, suggesting that the five visit goal was more difficult to achieve.[5] The Easy treatment resulted in 30% of participants completing 3-4 visits and 19% completing 5+ visits. The proportion completing 5+ visits in the Hard treatment is the same as the Easy treatment (19%), and the proportion completing 3-4 visits (14%) is no different than the Control.

We find a significant difference by race in visits completed. The number of visits in the treated conditions is 40% larger for Black participants than Non-Black participants. These differences are apparent in each treatment arm of the study. The number of visits by race in the Control group is virtually identical, thus discarding pre-existing differences between populations. In our sample, Black participants are more disadvantaged. They are slightly less educated, have more children and have fewer alternative places to go for support. But, they are no more likely to be unemployed, and thus have more free time. While it would seem that disadvantage might drive higher use of support services, no clear set of covariates predict differences in visit completion by race.

The treatment effects on rearrest in the full sample are not significant for either arm of the experiment. The Easy treatment, which increased visits the most, reduced arrests up to three years after study enrollment by 5.7 (s.e. 4.6) percentage points, but the estimate is noisy. To fully exploit the variation on visits generated by our treatments, we estimate the effect of visits on rearrests using 2SLS and treatment assignment as instruments. We follow Abadie et al. (2024)'s approach to address a heterogeneous first stage. In this case, in the full sample, we find that one extra visit reduces arrests by 6.2 (s.e. 3.6) percentage points. This effect is driven mainly by Black participants who experience a 29.6 (s.e. 12.4) reduction in arrest. These results reproduce if we use alternative measures of long-term outcomes, i.e.,

[5] Table A.3 shows that this result is not mechanical, i.e., due to the higher probability of incarceration as time passes.

criminal offense, summary, misdemeanor, or felony charges.

Our study suggests that heterogeneity in treatment take-up might translate into heterogeneity in treatment effects. Black participants are more likely to take up the treatment, and they explain the reduction in rearrests in our study. An advantage of our design is that participants could choose which services to use, which allows us to assess which services are more effective. While completing any visit is negatively associated with rearrest (except for legal services, which are in low demand), we find that visits that included counseling, mentoring or peer or job-related support are associated with the largest decline in arrests. Black participants are more likely to use these services than non-Black participants. While we cannot distinguish whether Black participants chose the most effective treatments or these treatments are more effective for Black participants only, these findings suggest more work is needed in improving treatment assignment.

To investigate whether those not currently using these services are more likely to benefit from them, we estimate marginal treatment effects following the approach of Brinch et al. (2017). We find that those with a higher resistance to treatment are more likely to benefit from our partner's services. In other words, small monetary incentives might provide the needed push that helps reluctant ex-inmates use the services they need to get back on their feet.

Our main contribution is to show the importance and impact of heterogeneous response to treatments in the context of reentry services aimed to reduce recidivism. We identify a heterogeneous first-stage demand for reentry services by race. This has not been examined before in previous experimental studies on reentry services (Grommon et al., 2013; Cook et al., 2015; Wiegand and Sussell, 2016; D'Amico and Kim, 2018) and only examined to a limited extent in the broader literature on crime.[6] Differences in demand for treatment by

[6]Differences in compliance by race are scant in a restorative justice program (Shem-Tov et al., 2024) and non existence in Michigan's IGNITE program (Alsan et al., 2024). Compliance is often addressed in the full sample, i.e. Heller et al. (2017)'s study on the effect of the Becoming a Man program, but not separated by race. Other literatures have examined compliance differences by race, i.e. Kling et al. (2007) find a larger proportion of Black families among compliers in a housing voucher program (see Table A1 in that paper).

race may be difficult to assess in some studies due to targeting[7] or because compliance is not an issue.[8] By uncovering differential first-stage response to treatment, and accounting for this in our main analysis of treatment on re-arrest, our study illustrates barriers some populations face to access beneficial services. Identifying the nature of these barriers is an important next task.

The second contribution of our paper is establishing the importance of dosage effects in reentry services on reducing recidivism. Our focus is on the critical reentry period after incarceration, and we use experimental variation to identify causal dosage effects. We directly address noncompliance through our experimental design and analysis. Previous randomized controlled trials on post-incarceration services focused on intent-to-treat effects and documented noncompliance but do not adjust treatment effect estimates (Grommon et al., 2013; Cook et al., 2015; Wiegand and Sussell, 2016; D'Amico and Kim, 2018). Our study advances this literature by explicitly incorporating dosage into our design, thus allowing a test of the causal effect of visit dosage on recidivism. We find those more likely to benefit from the services are more resistant to treatment. To our knowledge, no previous research has investigated whether there is negative selection into post-incarceration interventions.[9]

By setting different visit dosage goals, our study design allows us to examine the effect of these goals and contribute to the goal-setting literature.[10] Goal-setting theory (Locke and Latham, 1990) is based on the premise that conscious goals affect action (Ryan, 1970). Goals need to be challenging, but attainable, to motivate completion of a task (Zimmerman et al., 1992). If goals are too much of a stretch, they will not be achieved (Sitkin et al., 2017; Markovitz, 2012; Ordóñez et al., 2009), and monetary stakes can also influence goal achievement (Corgnet et al., 2015; Goerg and Kube, 2012). It is not clear-cut how to set

[7]For instance, participants in Bhatt et al. (2024) are ninety-seven percent Black.

[8]Many quasi-experimental studies using judge designs do not face a compliance problem.

[9]Evidence of negative selection is reported in the literature of early childhood interventions (e.g. Cornelissen et al., 2018).

[10]Some studies use light-touch and nudge-type interventions to address recidivism, including mental health outreach (Batistich et al., 2021) and reminders (Fishbane et al., 2020). Our approach requires effort from participants via visits to the service provider and includes monetary incentives.

challenging, yet attainable, goals, and a certain goal that works well for one individual might not for another. The heterogeneity we observe in the first-stage confirms the differential impact of a certain goal for ex-inmates in a reentry support services setting.

In terms of generalizability of our empirical results, we follow the List (2020) four SANS conditions in our reporting. First, in terms of selection, our sample is a subset of clients to a large aftercare service provider in Pittsburgh who visited the facility during an 18-month period. Almost all clients invited to enroll in the study did. In terms of attrition, our compliance rates on recorded visits are 100%, as we have administrative records. Compliance rates on arrest records are likely not as high because we have administrative records on criminal offenses and arrests from the state of Pennsylvania only, but not other states. While we cannot guarantee complete administrative records of arrests, we think that we have most records because evidence suggests our sample has limited geographical mobility. Considering naturalness of the outcome and setting, we use a framed field experiment (Harrison and List, 2004), thus our setting is one in which participants know they are in a study but are engaged in a natural task. Finally, in terms of scaling our findings, the effect of visit dosage on rearrest may change as our encouragement design is extended to other populations and settings. This is because, while aftercare service providers likely focus on similar services, aftercare services might differ in content across settings. We view the visit dosage effects as an initial WAVE1 insight, in the parlance of List (2020). Replications need to be completed to understand if the visit dosage effects apply to other previously-incarcerated populations and well as other service providers in other settings.

The paper proceeds as follows. Section 2 describes the field experiment design, our partner support service setting and field implementation. Section 3 describes the data sets used and linked for the analysis. Section 4 presents summary statistics of our participant sample and services used. Section 5 reports on intent-to-treat estimates on visits and probability of arrest. We discuss heterogeneous treatment effects, treatment on the treated estimates and marginal treatment effects. Section 6 discusses the results. Section 7 presents a benefit-cost analysis of the intervention, and Section 8 concludes.

2 Field experiment

The field experiment is designed to examine how encouragement of different dosages of reentry services affects recidivism.[11] The design employs encouragement of service usage via increased visits, rather than random assignment to service access. It also focuses on visits, rather than providing a particular service, to allow participants to use services that meet their needs. Our design fits squarely with the study setting and our partner's requirement that use of their services is not denied to any individual. Plus, it does not withhold potentially beneficial services to anyone.

Our aim is to understand the effect of service usage on the probability of arrest. As such, we fix the monetary incentive upon reaching the goal and vary the number of visits needed to reach the goal. An alternative approach would have been to fix the visit goal and vary the incentive to reach the goal. We do not use this latter design approach for two reasons. First, we do not know what would be the appropriate goal in this setting. Second, we want to understand how the number of visits affects rearrests. This requires that we have treatments that exogenously alter the required number of visits. Had we fixed the number of visits and altered the incentive, we would have a binary outcome, i.e. visit goal met or not. To understand dosage effects, we need variation in visits to explore treatment effect heterogeneity on how visit dosage impacts recidivism.

[11] Our design is similar to that used in the Moving to Opportunity experiment that encouraged families living in high-poverty areas to move to low-poverty areas (Katz et al., 2001). Our study examines the intensive margin effects of dosage as participants are recruited among those who arrive to use aftercare services, which most often are to pick up a bus pass. Implementation of a field experiment that also focused on changing the extensive margin was prohibitively costly and infeasible with our partner.

2.1 Aftercare services

We partnered with an aftercare service provider (ASP) in Pittsburgh, PA and employed research assistants to be on site to implement the field experiment. The ASP is a non-profit that provides comprehensive support services to previously incarcerated individuals.[12] Their reintegration program includes a variety of services, including material assistance (i.e. bus passes, use of computers and phones, clothing), informational resources, referrals, support services (i.e. peer support groups, mentoring) and guidance regarding employment, housing, other social services and obtaining an identification card. The ASP provides these services in-house and via referrals to other service providers, such as housing lists and mental health services, in the area.[13]

Our partner ASP is among the largest providers of comprehensive services to the previously incarcerated in Allegheny County, PA. They provide services on-site at their office and have a support program run within the Allegheny County Jail. Most clients come to know of our partner from referrals, the in-jail program and word of mouth. Based on our partner's records, in 2015 and 2016, prior to the field experiment, they served 811 individuals. The majority of their clients (67%) were most recently arrested in Allegheny County, and almost all in Pennsylvania. The top three services used were computer usage, bus cards, and ID assistance. Most clients (61%) came to the ASP only once, 28% came 2-4 times, and the remaining 11% visited 5 or more times. Clients who came more frequently were more likely to use the computer and obtain work-related services, whereas those who visited less frequently were unlikely to seek employment help.

Activities delivered by our partner center around servicing individuals' needs, not advocating for the use of particular services. During client intake, staff ask what the individual wants to work on that day. Staff may suggest that clients take toiletries or browse the

[12] Our setting is support services for post-incarceration. This differs from studies that explore programs and interventions aimed at at-risk youth and preventing criminal behavior (i.e. Heller, 2014; Blattman et al., 2017).

[13] A full list of services provided by the ASP, and whether the use of that service during a visit would count towards a "valid" visit (for the experiment), is in Table A.1.

clothing closet. One day a week is walk-in only, in which clients could use services without an appointment. On other days, clients would need an appointment to meet with a staff member and receive services.

2.2 Design

Individuals arrive at our partner ASP to use services and are invited to be part of a study on use of aftercare services and recidivism. Upon agreement, a research assistant orally completes the intake survey with the participant and records the responses. The survey includes questions on contact information, date of birth, most recent incarceration date and location, demographics and education.[14]

Upon completion of the intake survey, participants are randomized into one of three groups: a control group and two treatment groups.[15] All groups were presented with a business-size card, the content of which varied depending on treatment assignment. On the front of the card, there was the provider's logo, address and phone number. On the back of the card, a research assistant filled in the participant's name, an identification code and the survey date. This procedure ensures the card is unique to the participant and could not be shared.

For the two treatment groups, the front of the card also included 5 blank boxes. Each time a participant in the treatment groups visited the service provider and used at least one "valid" service, a research assistant or staff member would put their initials in the box.[16]

[14] Materials used in the field experiment and intake survey questions are in Appendix B. The study has IRB approval from University of Pittsburgh (PRO17020307) and Texas A&M University (IRB2018-0488D). The study is pre-registered at the American Economics Association RCT Registry (AEARCTR-0003375). The registration includes the study description and pre-analysis plan.

[15] Randomization was done by the research assistant shuffling 12 blank, opaque envelopes and allowing the participant to choose one. Each envelope contained a card for one of the three treatments. There were four envelopes for each treatment group, so each treatment had an equal probability of being assigned. The participant and research assistant were blind to which envelope contained which treatment.

[16] Most services provided were counted as valid (Table A.1). However, if a participant came in to pick up a bus pass, make a personal phone call or use the computer for personal activities (i.e. checking social media, online search for something unrelated to core provider services), those did not count as a valid service. Each visit is recorded in the ASP's database, including name of the client, date of the visit and services used. Clients are required to present identification to use services.

Boxes would be initialized for each visit, not each service. The enrollment visit does not count towards the visits goal. Any valid visit after enrollment is initialized on the card. A participant who came in to the ASP once and used three valid services would get one box initialed, just as a participant who came in once and used one valid service. Once all 5 boxes were initialed, the card could be traded in for a prepaid Visa debit card loaded with $50. One of the treatment groups already had two of the boxes initialed on the card and thus only had to complete 3 visits to get the Visa card (Easy treatment). The other group had no initials on the card and thus had to complete 5 visits to get the Visa card (Hard treatment). The treatment groups needed to complete the required visits within a year to get the $50 Visa card. Initials and visits were validated using the provider's visit records prior to issuing a participant the Visa card. The research project fully utilized electronic record keeping, and participants were presented with physical cards to increase saliency. Participants were informed that lost cards would be replaced and discrepancies in visits recorded on the card would be resolved in accordance with electronic visit records.

The Control group also received a card, but it did not have the 5 blank boxes. The front of the card included a statement that encouraged the holder to use at least 5 services within the year. Images of the cards used for the Control group, Easy treatment and Hard treatment are in Appendix B. Upon completion of the intake survey and random assignment to treatment, all participants are given a bus pass from the Port Authority of Allegheny County for one week of unlimited rides (valued at $24). A bus pass is one of the most common services that brings clients to the ASP.

The two treatments, Easy and Hard, keep the encouragement of the $50 Visa card constant and changed the cost to get the reward, i.e. by having to complete 3 or 5 visits. By encouraging repeated exposure to aftercare, the participant may develop a relationship with the provider staff and with a positive peer group of other clients using provider services. The card also provides a tangible way to keep track of service usage.

2.3 Implementation

Previous incarceration is a requirement to receive services at the ASP, thus all individuals who came to the office were eligible for invitation to be part of the study. Recruitment began at the beginning of October 2018 and continued until mid-March 2020 when in-person services at the provider were shut down due to lockdowns initiated by the emergence of COVID-19.[17] Research assistants were on site at the provider for partial days, four days a week, totaling 18 hours per week, and invited any individual who came into the office or called on the phone to be part of the study. There were very few refusals. Most individuals who were invited to be part of the study agreed to do so.

During the first month of recruitment, participants were placed only in the Control group. There was no mention of the monetary incentive and no randomization into the treatment groups. This was done to have a pure Control group that could not be influenced by knowledge of the monetary incentive in the treatment groups. The behavior of the pure Control group (n=41) is no different than the subsequent Control group (n=166), so these two groups are combined in the analysis (Table A.2). In total, 531 individuals were recruited to be part of our study: 207 in the Control group, 164 in the Easy treatment and 160 in the Hard treatment. Roughly 16% of the ASP's clients during our study period are treated.[18] The sample is further augmented with a random sample of an additional 200 contemporary ASP clients who serve as a synthetic control, bringing the sample to 731 (fully described in Section 3).

To determine what effect size we are powered to detect, we use the probability of being re-arrested within 20 months of release from incarceration based on a U.S. Department of Justice report on recidivism (Durose et al., 2014). The probability of re-arrest is 56%. In the analysis, we use a 36-month window as our outcome variable as this provides additional time

[17]The Governor of Pennsylvania closed all non-essential businesses and issued a stay-at-home order on March 19 and 23 (https://pittsburghpa.gov/mayor/covid-updates).

[18]According to the ASP reports, unduplicated clients were 361, 1442 and 1092 in 2018, 2019 and 2020 respectively.

for re-arrest that might have been disrupted due to the COVID-19 pandemic lockdowns. Under these assumptions, we are powered to detect a 15 percentage point reduction in re-arrest given our sample size (power=0.8, alpha=0.05). We note that our sample includes individuals who were released from incarceration within the previous year, as well as those who were released several years prior. Our power calculations use the arrest rate at 20 months after release, but our sample includes those who had been released more than 20 months prior. Thus, with our sample, the effect size we are powered to detect may be slightly smaller than 15 percentage points.

The lockdowns and business closures during the early phase of the COVID-19 pandemic affected participants' ability to visit the ASP for services for several months, in addition to likely affecting the ability to commit crimes. Between March-June 2020, the provider's office was closed, but staff called existing clients weekly to check on material needs and mental health. From June 2020 through April 2021, the provider returned to offering all of its services to new and existing clients through a combination of phone calls, video-chat and in-person appointments. During all periods of office closures, staff delivered basic necessities such as food, clothing, IDs, toiletries, and cleaning supplies to a large number of clients. Peer support group meetings and the mentor program transitioned from in-person meetings to video conferences. Since April 2021, the ASP has returned to providing all services in person. Given these disruptions, we control for month and year of enrollment in our analysis. All results are robust to the inclusion of these controls.

3 Data

There are three sources of data used in the analysis. The first is the data collected from the intake survey with our recruited sample and includes treatment assignment.

The second is from the ASP's administrative data on background characteristics of the client, i.e. date of birth, sex and race of the client, and detailed information on visits.

Each time a client visits the provider, the visit is recorded in their digital records, including the client's name, date of visit and services used. A visit is coded as "valid" if it was to use a provider service, such as housing search, food pantry, clothing, mentoring, support group, family services, employment services or obtaining an identification card (Table A.1 lists services and whether they counted towards a valid visit). Personal use of the phone or computer is not counted as valid. We broadly categorize services for the analysis into short-run necessities (i.e. food, housing, clothing, transportation, identification card) and longer-run needs (employment, family services, mentoring, peer group support).

To augment our sample, the ASP also provided visit and background data on a random sample of 200 clients who used services during our recruitment window of Oct 2018 to March 2020, so could have been recruited, but are not in our experiment sample. These clients visited the ASP outside of the time that the RA's were at the ASP doing recruitment. This group is pooled with the experimental sample and serves as a synthetic control in the analysis. We do not have intake survey data for these individuals.

The third is public data from the Unified Judicial System (UJS) of Pennsylvania.[19] This data set includes criminal offense and arrest records in Pennsylvania. Currently, there is no single data source that combines criminal offense and arrest records across all states. Finding these data in all states would require a state-by-state search. This is not done because of limited resources and it would likely yield few additional results.[20] Thus, our outcome variable is a lower bound on the total number of criminal offenses and arrest records a participant could have. We obtained records for our experimental sample and synthetic control from January 2011 through July 2023.

For our analysis, a criminal offense is defined as an encounter with law enforcement that

[19]We also searched federal crimes in the Public Access to Court Electronic Records (PACER) system. These data do not have birth dates, so we could not verify that a match on name was a valid match. Thus, we do not use these data in our analysis.

[20]Evidence suggests the previously incarcerated participants in our study have limited mobility. All participants, and all clients at the ASP, resided in western Pennsylvania. In 2020, most (82%) resided in 15 zip codes in Pittsburgh. The remaining 18% resided in 38 different zip codes throughout southwestern Pennsylvania.

resulted in a record entry in the UJS data, and an arrest is when the criminal offense produced an arrest. Not all criminal offenses end up in an arrest (roughly one in two offenses lead to an arrest). For example, violations, such as traffic infractions or failure to pay court fees that do not result in an arrest, do not count as an arrest. We do not distinguish between an arrest where charges were dropped or sustained. In the analysis, we use arrests that appear in the UJS data up to three years after the participant was recruited into our study.

4 Sample description

4.1 Characteristics

Table 1 provides summary statistics for our experimental sample, based on responses to the intake survey. The table combines the Pure Control with the Control to make one Control group.[21] Across all treatments, 29% of participants are female, 46% are classified by the ASP as Black, <1% as Hispanic/Multiple and 52% as White. The average age is 42.8 years, 6% are married or with a partner and have two children. The average education level is a high school diploma, 26% were employed at the time of intake and 31% knew of the ASP while they were incarcerated. The participants are a mix of recently arrested and those who have not been arrested for a while. Almost half have been arrested in the three years prior to the start of our study in 2018, with the average year of last arrest being 2015. The length of the most recent incarceration was 745 days (2 years). This means that, on average, our participants were within a year of release from incarceration when they enrolled in our study. Over half of our sample (55%) provided an institutional address at study enrollment, i.e. parole office, halfway house, rehabilitation facility, homeless shelter. Our sample is similar to the incarcerated population in Pennsylvania, but with more women, where 48%

[21] There is balance across the Pure Control and Control, so we pool them. Table A.2 reports sample descriptions for all subsamples used in the analysis, i.e. Pure Control, Control, Synthetic Control, Easy and Hard, and statistical tests for balancedness across the samples. There is balance across the Synthetic Control with the pooled Control so we pool those two groups for the analysis on re-arrests. Intake survey data is missing for the Synthetic Control, so they are not included in Table 1.

Table 1: Sample description and balance across treatment groups

	All	s.d.	Control	s.d.	5 visits	s.d.	3 visits	s.d.	F-test	p-val.
Female	0.29	0.46	0.28	0.45	0.30	0.46	0.25	0.43	0.51	0.60
Black	0.46	0.50	0.47	0.50	0.49	0.50	0.44	0.50	0.49	0.61
Age	42.76	11.30	42.48	10.85	42.02	11.06	43.19	11.28	0.46	0.63
Married/Partnered	0.06	0.25	0.07	0.25	0.06	0.23	0.07	0.25	0.09	0.91
Education (years)	12.21	1.61	12.18	1.60	12.15	1.64	12.30	1.59	0.39	0.68
Employed	0.26	0.44	0.27	0.45	0.20	0.40	0.30	0.46	2.37	0.09
Knew the aftercare	0.31	0.46	0.29	0.45	0.33	0.47	0.32	0.47	0.34	0.71
Number of children	2.06	2.17	2.18	2.17	2.01	2.09	1.98	2.27	0.48	0.62
Has other support	0.43	0.50	0.46	0.50	0.42	0.49	0.41	0.49	0.57	0.57
Year of last arrest	2,015.47	5.43	2,015.20	5.88	2,015.41	5.77	2,015.88	4.43	0.70	0.50
Duration of last incarceration	744.74	1,321.83	714.72	1,317.96	747.58	1,301.81	777.16	1,352.32	0.09	0.91
Arrested 36m prior to study	0.49	0.50	0.47	0.50	0.49	0.50	0.48	0.50	0.06	0.94
Institutional housing	0.55	0.50	0.54	0.50	0.56	0.50	0.57	0.50	0.25	0.78
Observations	731		207		160		164			

Notes: The Control group pools the pure Control and the subsequent Control groups together. A sample description of all four treatment groups is reported in Table A.2. The last two columns report the F-test statistic and corresponding p-value of a joint test of equality across the three treatment groups (Control, Hard, Easy). Numbers are average, and s.d. is standard deviation. Female is a dummy variable for female. Black is a dummy variable for black participant. Age is in years. Married/Partnered is a dummy variable for being in a relationship. Education is in years. Employed is a dummy variable for being currently employed at the time of enrollment into the study. Knew the aftercare is a dummy variable for having heard of our partner ASP while incarcerated. Number of children is number of children. Has other support is a dummy variable for having access to other support services. Year of last arrest is year. Duration of last incarceration is length of most previous incarceration in days. Arrested 36m prior is a dummy variable for having been arrested at least once during the 36 months prior to enrollment into the study. Institutional housing is a dummy variable for providing an institutional address at study enrollment, i.e. parole office, halfway house, rehabilitation facility, homeless shelter.

are Black, 5% are women and the average age is 44 years old.[22]

The table also reports summary statistics separately for the Control, Hard and Easy groups. The groups are balanced on all characteristics, with the exception of being employed at the time of intake. The final table column reports the p-value of an F-test of equality of coefficients across the three groups.

4.2 Services used

Participants used a variety of services at the ASP during visits, and the composition changes over time. Figure 1 illustrates the types of services used during the first through fifth+ visit for those who completed 1, 2, 3, 4 or 5+ valid visits within one year of study enrollment.[23]

[22] See Pennsylvania Department of Corrections Dashboard, https://dashboard.cor.pa.gov/us-pa/narratives/prison/2

[23] Participants complete a different number of visits. If we fix the visit and look at services used during that visit, we confound the composition of participant visit types. To address this, Figure 1 fixes the participant visit type by number of visits completed, i.e. 1, 2, 3, 4, or 5+, and shows the services used by that group at the first, second, third, fourth and fifth or more visit. We only use valid visits in the figure. If a participant

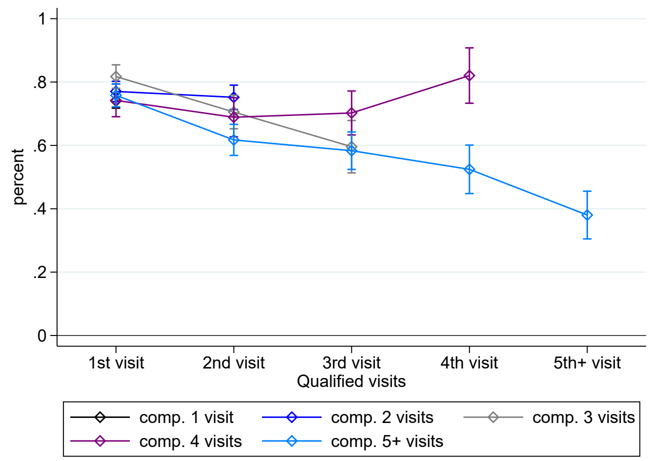


Figure 1: PERCENT OF SERVICES USED FOR SHORT-RUN NECESSITIES (I.E. CLOTHING, HOUSING, FOOD, ID, TRANSPORTATION) BY VISIT NUMBER

Notes: The figure shows usage of services for short-run necessities during the first, second, third, fourth and fifth+ visit. Each line shows usage for those who completed 1, 2, 3, 4 or 5+ valid visits within one year of study enrollment. Error bars denote standard errors. Sample used for the figure are those who completed 5+ visits (n=85), 4 visits (n=55), 3 visits (n=76), 2 visits (n=140) and 1 visit (n=303).

Services are grouped into short-run necessities (i.e. food, housing, clothing, transportation, identification card) and longer-run needs (i.e. employment, family services, mentoring, peer group support). The figure shows that, on the first visit, 70-80% of services used are for short-run necessities. The main pattern is that this proportion tends to drop each subsequent visit and is especially pronounced for those who completed 5+ visits.

For those who go for 5+ visits, it is only from the fifth visit onward does demand for services that address longer-run needs, i.e. employment, become more prominent. These findings suggest that programs that focus exclusively on employment and training of the previously incarcerated need to address short-run necessities first.[24]

came to pick up a bus pass or gift card, that is not a valid visit. Services used by Black and non-Black participants are similar (Figure A.2).

[24]Studies focusing on employment assistance for the previously incarcerated find employment referrals to be ineffective (Farabee et al., 2014), provide benefits in the short run (Cook et al., 2015) and offer no significant effect on labor market outcomes (D'Amico and Kim, 2018).

5 Treatment effects

We examine treatment effects on the number of visits within one year of study enrollment and the probability of arrest within three years. We confirm that our experimental design worked as intended to increase visits and show that more visits decrease recidivism. To examine treatment effects on rearrest, we present intent to treat (ITT) estimates of the Easy and Hard treatments and treatment on the treated (TOT) estimates, accounting for first-stage heterogeneity as in Abadie et al. (2024), using total number of visits.

5.1 Visits

Participants had one year to complete the required number of visits to the ASP for the $50 incentive in the Easy and Hard treatments. The average number of visits completed after one year in the Easy treatment is 3.07, in the Hard treatment is 2.51 and in the Control is 1.98. These are significantly different from one another.[25] This confirms that our encouragement design worked as intended and increased the number of visits for those offered the $50 incentive compared to those who were not offered an incentive. Panel (a) of Figure 2 tracks how the average number of visits changed over the 12 months following enrollment. We confirm that visits are significantly the highest in Easy, then Hard and then Control at every month since enrollment.[26]

The treatments also reduced the time taken to complete the required visits for the $50 incentive. Participants in the Easy treatment completed 3-4 visits in half the time or less than those in the Control and Hard treatments. Those in the Hard treatment completed 5 visits in two-thirds the time or less than those in the Control and Easy treatments.[27]

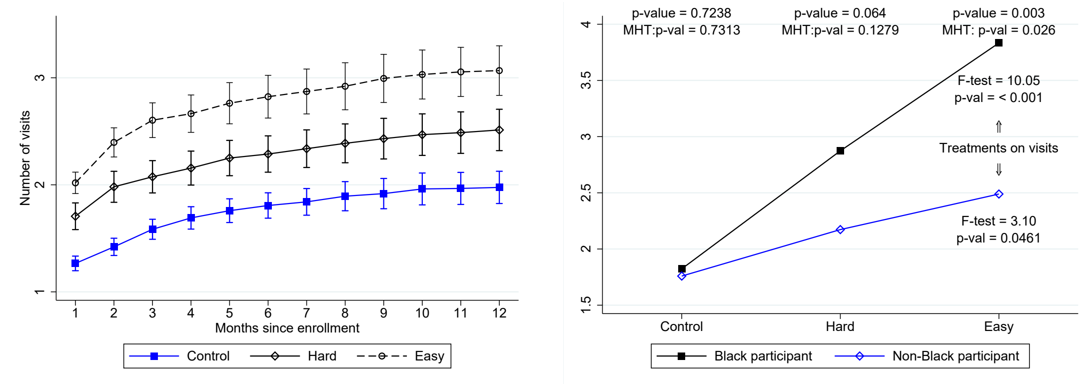
We find heterogeneous treatment effects on visits by race.[28] Panel (b) in Figure 2 shows

[25] Hard v. Control t-test = -2.22 (p-value = 0.0267). Easy v. Control t-test = -4.09 (p-value = 0.0001). Hard v. Easy t-test = -1.83 (p-value = 0.0676).

[26] Figure A.1 shows the cumulative distribution functions of visits across treatments. First-order stochastic dominance tests show that Easy FOSD Hard FOSD Control.

[27] The median number of days to complete 3-4 visits is 36 in the Control, 42 in Hard and 17 in Easy. The median number of days to complete 5 visits is 87 in Control, 33 in Hard and 52 in Easy.

[28] Comparison by race is included in the study pre-registration and pre-analysis plan. We do not explore



(a) Average visits

(b) Total visits by race

Figure 2: Treatment effects on number of visits

Notes: Both panels include visits completed within one year of study enrollment. Error bars denote standard errors. Panel (a) shows the average cumulative number of visits per treatment group at each month following study enrollment. Panel (b) shows the total number of visits a year after recruitment by treatment split by Black and Non-Black participants. P-values for difference in means tests between Black and Non-Black participants, both t-tests and adjusted for multiple hypotheses testing (MHT), are reported on the top of the figure. F-tests for differences across treatments, for each group, are reported on the right side of the figure.

Table 2: ITT effects on number of valid visits within a year of enrollment

	(1)	(2)	(3)
	All	Non-Black	Black
Hard	0.512	0.175	0.978
	(0.224)	(0.332)	(0.265)
Easy	1.161	0.487	1.884
	(0.307)	(0.215)	(0.663)

Notes: Number of observations is 731 for All, 338 for Black participants and 394 for non-Black participants. Standard errors in parentheses. ITT effects are calculated using Stata `multe` command, which implements Goldsmith-Pinkham et al. (2022) multiple treatment contamination bias robust estimates. We interact treatment and race to expand the number of effective treatments. Heterogeneous treatment effects are estimated jointly but presented in separate columns. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment.

total number of visits completed one year after recruitment by race and treatment condition. The total number of visits for Black and non-Black participants is no different in the control condition, but they differ significantly in the treatment conditions. We provide within-

heterogeneity by sex, as we originally specified, as there is insufficient variation (see Table 1).

treatment comparisons and the overall effect of treatments on visits. For the latter, we run separate regressions of the impact of treatments on the number of visits by race. Table 2 presents these estimates.[29] Treatment assignment has a larger and more significant effect on Black participants than non-Black participants.[30] When we estimate the effect of visits on arrests in Section 5.2, we follow Abadie et al. (2024) to account for first-stage heterogeneity.

There are several possible explanations for first-stage heterogeneity by race. For example, Black participants might be more cash-constrained or have a lower opportunity cost of their time and respond more strongly to cash incentives. Table A.5 shows that while Black participants have fewer years of education, more children, and fewer support alternatives, they are not more likely to be unemployed and thus have more free time.[31] Also, Black participants might be more disadvantaged, lack access to alternative reentry programs or have poor social networks. While there is evidence consistent with this hypothesis (Table A.5), we would then expect Blacks to have more visits in the control group. We do not and conclude there is not support for this explanation.

Heterogeneous first-stage response to treatment based on observable characteristics suggests that there might be selection on unobservables. That is, Black participants might select into treatment because they expect to benefit from it. We discuss sample selection and selection on gains based on estimates of marginal treatment effects (Heckman and Vytlacil, 2007) that use the visit variation produced by our experimental design in Section 5.3. Before we turn to that, in the next section, we discuss treatment effects on arrests.

5.2 Arrests

Our main results are presented in Table 3. This table reports intention-to-treat effects of the Easy and Hard treatments and the effect of visits on the probability of arrest within

[29]The differential effect of the Hard and Easy condition on visits is not mechanical, i.e., failing to complete five visits can be due to arrests, which is more likely as time elapses. Table A.3 shows that the results are similar if we restrict the sample to those not arrested in the first six months after recruitment.

[30]We find similar estimates if we exclude the synthetic control (see Table A.4).

[31]It is also not a question of proximity. There is no significant difference in distance from place of residence to the ASP facilities for Blacks and non-Blacks (p-value = 0.3069).

three years of study enrollment. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment. We follow Goldsmith-Pinkham et al. (2022) suggested approach to estimate the effect of multiple treatments when covariates are included. That is, the regressions include the interaction of covariates and treatment variables. To estimate the treatment on the treated (TOT) based on visits, we follow Abadie et al. (2024)'s approach to account for the first-stage heterogeneity established in the previous section. This approach is more efficient when treatment effects are heterogeneous by group. Thus we allow heterogeneous response to treatment in the first stage of the 2SLS regressions.

The first two columns show estimates for all participants, and the last four columns present estimates by race. Estimates by race are estimated jointly but presented separately for clarity. Columns 1, 3 and 5 report ITT estimates of the Easy and Hard conditions. The Hard condition estimates are both positive and negative and small in magnitude, the Easy condition estimates are negative and slightly larger in magnitude, however, both estimates are noisy. Column 2 shows the TOT effect in the full sample of an extra visit is a six percentage point decrease in the probability of rearrest in the three years since recruitment.[32] In other words, those completing two extra visits (the intended goal in the Easy treatment given the pre-intervention median of one visit per year) experience an average 12 percentage point decrease in the probability of arrest in the three years since enrollment. This is close to a 40% decrease from the baseline probability of arrest of 30%.

Columns 4 and 6 show that the negative treatment effect of visits on arrest rates are present for Black participants only. Non-Black participants do not exhibit this effect. We are slightly underpowered to detect heterogeneous effects on rearrests by race, however this is not the case for visits.[33] Given the non-Black subpopulation was less responsive to treatment, these estimates should be interpreted with more caution. With these caveats, we conclude that the findings in Table 3 suggest that a potential reason for Black participants to be more

[32]Estimates are similar when synthetic controls are excluded (Table A.6).

[33]Our RCT was interrupted due to COVID-19, thus preventing us from reaching our target population.

Table 3: Treatment effects on 3-year re-arrest rate

	All		Non-Black		Black	
	(1) ITT	(2) TOT	(3) ITT	(4) TOT	(5) ITT	(6) TOT
Hard	0.029		0.019		-0.011	
	(0.059)		(0.096)		(0.047)	
Easy	-0.057		-0.004		-0.093	
	(0.046)		(0.053)		(0.074)	
Visits		-0.062		0.196		-0.296
		(0.036)		(0.116)		(0.124)

Notes: Number of observations is 731 for All, 338 for Black participants and 394 for non-Black participants. Standard errors in parentheses. ITT effects are calculated using Stata `multe` command, which implements Goldsmith-Pinkham et al. (2022) multiple treatment contamination bias robust estimates. We interact treatment and race to expand the number of effective treatments. Heterogeneous treatment effects are estimated jointly but presented in separate columns. TOT estimates are the effect of the number of visits on the probability of being arrested up to 36 after enrollment. The estimates implement Abadie et al. (2024) approach to deal with first-stage heterogeneity. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment. Missing information is handled by replacing missing items with a zero and adding a dummy for missing data.

likely to comply with treatment is that the treatment was beneficial.

5.3 Marginal treatment effects

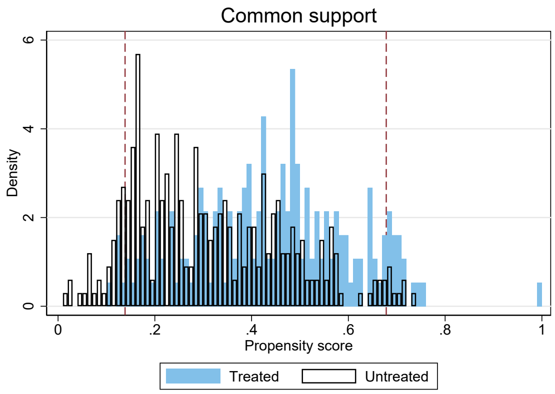
We exploit heterogeneous response to treatment to investigate whether selection into treatment is positive or negative. Variation in the propensity to complete a certain number of visits is used to estimate marginal treatment effects (Heckman and Vytlacil, 2007). Marginal treatment effects (MTE) are the derivative of the probability of being arrested within three years of enrollment with respect to the predicted probability of exceeding a threshold of visits. These can be interpreted as the treatment effect for individuals with different resistance to treatment (Heckman and Vytlacil, 2007).

We follow (Brinch et al., 2017) to estimate MTE. The authors show that, if potential outcomes are separable functions of observable and unobservable variables, discrete instruments interacted with covariates can be used to estimate treatment effects and the marginal response function over a larger support. To implement this approach, we create a dummy variable that equals one if at least three valid visits are completed within a year of enrollment. We use this definition of treatment completion to avoid potential exclusion restriction violations. This can happen if there are extensive margin effects on visits below such a defined threshold (Andresen and Huber, 2021).

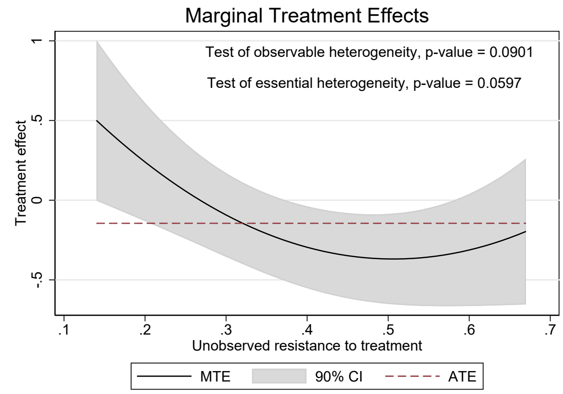
Figure 3 shows the estimation results. Panel (a) shows the propensity score estimated using a Logit regression on covariates interacted with treatment indicators. The probability of completing at least three visits is twenty percent in the control condition, thirty-three percent in the Hard condition, and fifty percent in the Easy condition. To estimate MTE, we trimmed the support by 2.5% on each tail and estimated the marginal response functions separately using a second-order polynomial. We use a parametric specification due to the small size of our sample. The main takeaway from Panel (a) is that being assigned to the Easy or Hard treatment increases the predicted probability of completing three or more visits.

There is evidence of heterogeneous selection into treatment. Panel (b) shows that treatment effects are decreasing in resistance to treatment (or the predicted probability of exceeding three visits). Both observed and unobserved heterogeneity are significant at the ten percent level. These results are consistent with participants facing constraints that prevent them from taking full advantage of these services. The heterogeneous treatment effects by resistance levels can explain the mixed evidence on average treatment effects found in previous studies on reentry support services. Estimates using a threshold of four or more visits are similar and more precise (Figure A.3). As discussed in the next section, most participants strictly exceeded three visits; thus, estimates using a threshold of four or more visits are not likely to violate the exclusion restriction.

In sum, these findings are consistent with negative selection into treatment. Participants who might benefit the most from re-entry services are those who use them the least absent incentives. Encouraging the use of services brings in participants with more to gain, and



(a) Propensity score



(b) Marginal treatment effects

Figure 3: Marginal treatment effects of completing 3 or more visits

Note: Number of observations 531, only experimental sample. In Panel (a), the dashed vertical lines represent the upper and lower bounds on the common support of the propensity score (based on 2.5% trimming) used to estimate the MTEs. Propensity scores are predicted with a Logit regression. "Treated" means completing three or more visits, and "Untreated" means completing fewer than three visits. In Panel (b), the MTE estimation is based on a quadratic polynomial specification in the sample with common support. The x-axis is the resistance to treatment (or the predicted probability of exceeding three visits). The y-axis is the estimated treatment effect, and the dotted horizontal red line is the average treatment effect. We follow Brinch et al. (2017) in assuming separability between observed and unobserved heterogeneity. We estimate marginal treatment response functions separately and calculate marginal treatment effects as the difference between them (Heckman and Vytlacil, 2007). All estimations were done via `mtefe` in Stata (Andresen, 2018).

marginal treatment effects are the largest for this group.

6 Discussion

Our study shows that one additional valid visit reduces rearrest by 6 percentage points. The effect is large, especially compared to other studies on reentry service programs (D'Amico and Kim, 2018; Grommon et al., 2013; Cook et al., 2015; Wiegand and Sussell, 2016) which find mixed and small effects on recidivism. We explore possible reasons for our findings.

Perhaps our finding is due to the choice of our outcome variable. If instead of using arrests, we use the probability of a criminal offense within three years of recruitment, we find a similar, but noisier, estimate (see Table A.7). Similar patterns are obtained if we concentrate on arrests involving a summary, misdemeanor or felony charge.[34] The test of

[34] We use the UJS classification for offense type. We construct dummy variables that equal one if any counts are a summary, misdemeanor or felony.

the joint hypothesis that arrests, criminal offenses, felonies, misdemeanors, and summary are jointly insignificant in the full sample is $\chi^2(5) = 8.96$, p-value = 0.1106. Estimates in the experiment subsample, which contains additional information, are more precise. The joint test in this sample is $\chi^2(5) = 10.56$, p-value = 0.0608. While results using alternative outcomes are noisy, they point in the same direction as arrests. We conclude that arrests provide a good summary statistic for treatment effects.

Other reasons for our large effect are sample selection and treatment endogeneity. Our sample is a subset of the population of previously arrested individuals. While records from the ASP show that the modal number of visits per person is one, our participants might be more sensitive to treatment since they find it useful to get help with basic needs. We cannot discard this channel. However, our experiment does provide causal estimates of additional services used by this population. In other words, a potential benefit of the ASP might be their ability to attract those who have the potential to benefit from their services. Additional studies can help elucidate whether these gains extend to a broader set of ex-inmates. From a policy perspective, allowing participants to select the services they need might be more advantageous to reduce program costs and generate beneficial outcomes.

Some types of visits may be associated with larger reductions in arrests. Table A.8 presents TOT estimates of rearrest across six categories of visits instead of the total number of visits. While completing any type of visit is negatively associated with rearrest (except for legal services which are in low demand), we find that visits that included counseling, mentoring and peer or job-related support are associated with the largest declines in arrests.[35] Black participants are more likely to use these types of services than non-Black participants (Figure A.4).

Finally, participants may have learned that additional services were useful, i.e., the final dosage was larger than planned in the experimental design. To assess this hypothesis, we

[35]The ASP describes support groups as providing a safe and welcoming forum for reentrants and their supporters to share resources, network, and address social, intellectual, vocational, spiritual, emotional, environmental and physical needs.

calculate the increase in demand for services above what was required to claim the rewards. We compare the control condition and the Easy condition since this condition made it easier to achieve visit goals. We find that 9% of participants in the control and 14% in the Easy condition completed three valid visits. However, 11% of participants completed strictly more than three visits in the control, while 36% completed more than three visits in the Easy condition. While we cannot discard the hypothesis that participants completed more visits as an insurance policy, this is also consistent with participants finding the services beneficial. This analysis also shows that the marginal participant is more likely to complete four instead of three visits. As discussed in Section 5.3, these participants experienced the largest gains.

7 Benefit-Cost Analysis

Using back-of-the-envelope calculations, we assess the dollar value of services used for those who completed 3-4 visits in the Easy treatment and 5+ visits in the Hard treatment. We combine services into five broad categories: bus pass, clothing/food, legal assistance to obtain an ID, housing search assistance, job search assistance and peer support and mentoring, and we calibrate a value for each service.[36] The average value to a participant of services used when completing 3-4 visits is $257.81 and when completing 5+ visits is $500.80.[37] These values are sizeable and exceed the value of the $50 gift card received upon visit goal completion. An alternative measure of value per visit can be obtained using the financial reports from the ASP. Using reports from 2021 and assuming two visits per year and 1092

[36]A bus pass costs $24. We assume a clothing/food visit is worth $20, given that the cost of obtaining used clothing at Goodwill and donated food. A legal assistance visit to obtain an ID is assumed to be $60 for labor (3 hours of assistance * $20/hour) since obtaining an ID for low-income, homeless individuals is free in Pennsylvania. A housing search visit is assumed to be $80 for labor (4 hours of assistance * $20/hour) since assistance includes counseling, referrals, search on in-house computers and help filling out applications. A job search visit is assumed to be $80 for labor (4 hours of assistance * $20/hour) since assistance includes resume development, counseling, identifying employment opportunities and online search. A peer support/mentoring visit is assumed to be $50 to account for the opportunity cost of the peer or mentor's time.

[37]The average number of units/visits for bus pass, clothing/food, legal, housing, job and peer support are: 2.0, 1.65, 0.38, 0.69, 0.90 and 0.52 for 3-4 visits and 3.03, 2.87, 0.53, 1.50, 1.57 and 1.87 for 5+ visits.

visitors per year, we find that the average value of services used per visit is $603.

We follow Alsan et al. (2024)'s approach to calculate the costs to society that were avoided due to the reduction in crime induced by our treatment. The UJS dataset provides a gradation of each type of crime and a description. Summary counts are not disaggregated; misdemeanors and felonies are grouped into four categories. To measure cost, we first find the crime description that captures at least fifty percent of the counts in each category. We then use Miller et al. (2021)'s cost calculations for various crimes to provide an average cost for each count, using a simple average of the crimes we find. We then calculate the sum or the max over all of these costs and counts associated with an arrest three years since study enrollment. These costs are calculated only if the counts are associated with an arrest. Table A.7 shows the average causal effect of visits on the cost of crime (last two columns). We find that one extra visit is associated with a $3,586.60 reduction in the societal cost of crime. Our estimates, while noisy, suggest the additional services used due to the experimental treatments pass the benefit-cost analysis.

8 Conclusion

We investigate whether increasing the number of visits to use support services for previously-incarcerated individuals can decrease recidivism. Given the heterogeneity in services needed by these individuals, we implement an encouragement design that experimentally varies the dosage of visits, with no restrictions on the types of services used. We find that one extra visit decreases the probability of arrest three years after enrollment by six percentage points. These findings from the full sample hide a large degree of heterogeneity. Black participants are more likely to increase visits than non-Black participants when encouraged to do so (3.8 v. 2.5 completed visits) and explain the overall effects of visits on rearrests. Consistent with negative selection into treatment, the benefits of the intervention are largest among those who are less likely to comply with the treatment. Not all services are equally effective at reducing rearrest, with peer support, mentoring and job-related services having the largest impacts, and Black participants are more likely to choose these more effective services.

Our experiment provides a possible explanation for the existing mixed evidence on the effectiveness of reentry services in reducing recidivism. The amount and composition of reentry services might be miscalibrated. Our experimental design uncovers these features by allowing self-selection into types of service and encouraging different service levels. Our analysis shows that heterogeneity is important in all stages of the intervention: who selects into treatment, what services participants select, and who benefits from it. The study speaks to the importance of considering first-stage heterogeneity and heterogeneous treatment effects. A fruitful avenue to explore would be deploying research designs to help uncover the most effective aspects of reentry support services to aid those seeking help and embed those in program design.

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APPENDICES INTENDED FOR ONLINE PUBLICATION

A Extra figures and tables

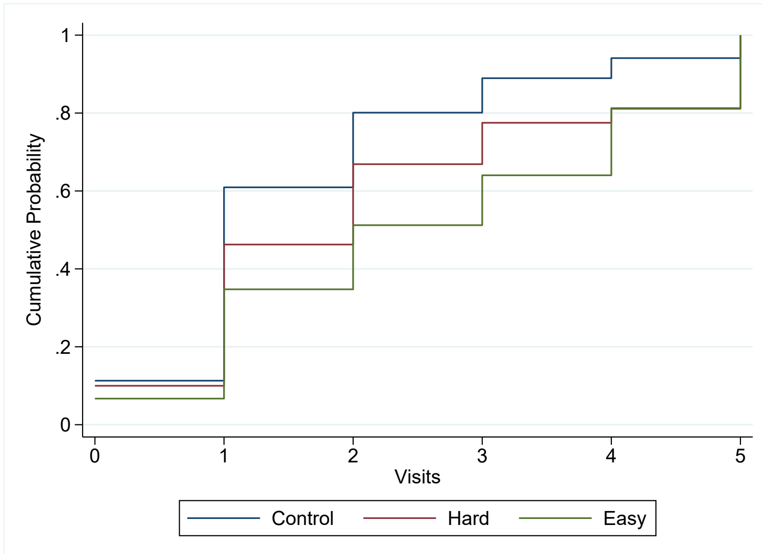

Figure A.1: Distribution of number of visits by treatment

Notes: The figure shows the distribution of number of visits by treatment. The treatments are first-order stochastically ordered by Easy, Hard and Control.

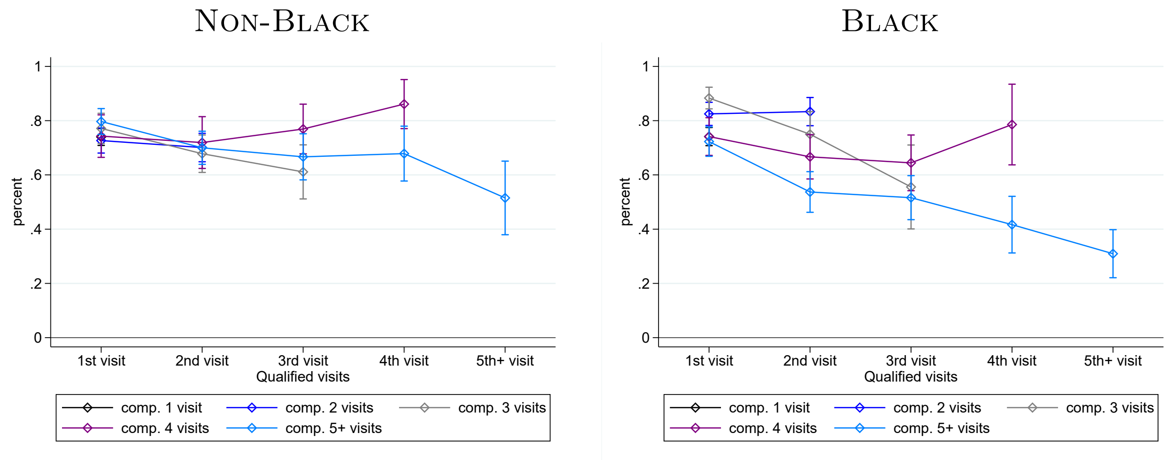

Figure A.2: Percent of services used for short-run necessities (i.e. clothing, housing, food, ID, transportation) by visit number - Non-Black and Black participants

Notes: The figure shows usage of services for short-run necessities during the first, second, third, fourth and fifth+ visit. Each line shows usage for those who completed 1, 2, 3, 4 or 5+ visits within one year of study enrollment. Error bars denote standard errors. Sample used for the figure are those who completed 5+ visits, 4 visits, 3 visits, 2 visits and 1 visit.

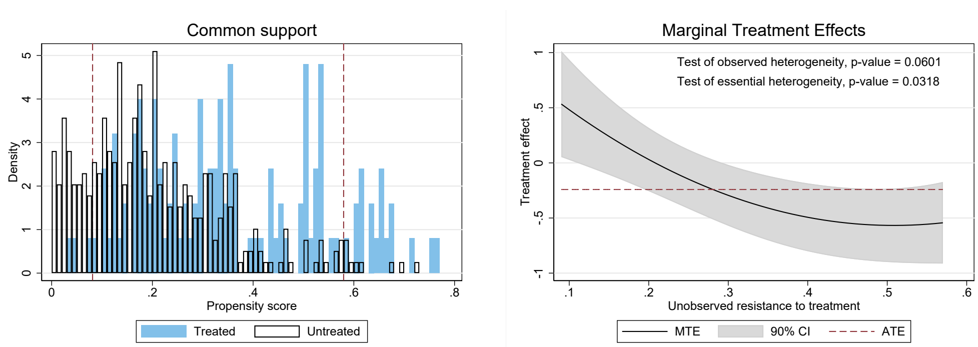

Figure A.3: Marginal treatment effects of completing 4 or more visits

Note: In the top left-hand figure, the dashed lines represent the upper and lower bounds on the common support of the propensity score (based on 2.5% trimming) used to estimate the MTEs. Propensity scores are predicted via a logit regression. The MTE estimation in the top right-hand figure is based on a quadratic polynomial specification in the sample with common support. The x-axis in that figure is the predicted probability of completing four or more visits in the year following recruitment. The shaded area has a 90% confidence interval. The outcome of interest is the probability of being rearrested in the three years following recruitment. We follow Brinch et al. (2017) in assuming separability between observed and unobserved heterogeneity. We estimate marginal treatment response functions separately and calculate marginal treatment effects as the difference between them (Heckman and Vytlacil, 2007). All estimations were done via `mtefe` in Stata (Andresen, 2018).

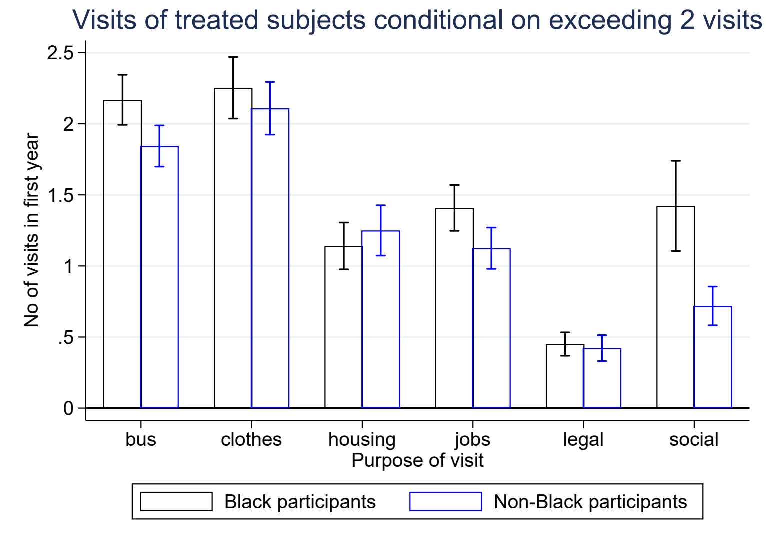

Figure A.4: Type of visit completed by race

Notes: The figure shows the number of visits by type and by race among treated participants who completed at least 3 visits.

Table A.1: Services offered by the Aftercare Service Provider (ASP) and whether use of the service during a visit would count as a valid visit in the Easy and Hard treatments

	Services offer by Aftercare Service Provider	Counted as valid visit
1	Housing - search for options	yes
2	Food pantry and food voucher	yes
3	Clothing closet and clothing voucher	yes
4	Mentoring resources	yes
5	Family services	yes
6	Peer support group program	yes
7	Employment - resume, job applications, training and education searches	yes
8	Obtaining a state identification card	yes
9	Computer and phone use for housing or employment search	yes
10	Pick up bus pass (only reason for visit)	no
11	Personal use of phone or computer	no

Table A.2: Sample description and balance across treatment groups - all subsamples used in analysis

					Control					Treated				
	All	s.d.	Impure	s.d.	Pure	s.d.	Synthetic	s.d.	Hard	s.d.	Easy	s.d.	F-test	p-val.
Female	0.28	0.45	0.29	0.45	0.24	0.43	0.34	0.47	0.30	0.46	0.25	0.43	0.92	0.45
Black	0.47	0.50	0.45	0.50	0.56	0.50	0.45	0.50	0.49	0.50	0.44	0.50	0.72	0.58
Age	42.56	11.03	42.59	10.57	42.05	12.04	43.30	11.98	42.02	11.06	43.19	11.28	0.39	0.81
Married/Partnered	0.06	0.25	0.08	0.27	0.02	0.16			0.06	0.23	0.07	0.25	0.60	0.61
Education (years)	12.21	1.61	12.25	1.73	11.90	0.90			12.15	1.64	12.30	1.59	0.78	0.51
Employed	0.26	0.44	0.26	0.44	0.32	0.47			0.20	0.40	0.30	0.46	1.77	0.15
Knew the aftercare	0.31	0.46	0.28	0.45	0.32	0.47			0.33	0.47	0.32	0.47	0.29	0.83
Number of children	2.06	2.17	2.13	2.02	2.37	2.69			2.01	2.09	1.98	2.27	0.45	0.72
Has other support	0.43	0.50	0.48	0.50	0.41	0.50			0.42	0.49	0.41	0.49	0.54	0.65
Year of last arrest	2,015.47	5.43	2,014.95	6.34	2,016.26	3.19			2,015.41	5.77	2,015.88	4.43	1.07	0.36
Duration of last incarceration	744.74	1,321.83	697.21	1,337.48	784.77	1,252.96			747.58	1,301.81	777.16	1,352.32	0.10	0.96
Arrested 36m prior to study	0.48	0.50	0.48	0.50	0.41	0.50	0.52	0.50	0.49	0.50	0.48	0.50	0.40	0.81
Institutional housing	0.55	0.50	0.55	0.50	0.49	0.51			0.56	0.50	0.57	0.50	0.33	0.80
Observations	531		166		41		200		160		164			

Notes: Numbers in columns are averages, except for columns labeled s.d., the standard deviation. The last two columns report the F-statistic and p-value for a joint test of equality of the row variable across all treatments. Female is a dummy variable for females. Black is a dummy variable for black participants. Age is in years. Married/Partnered is a dummy variable for being in a relationship. Education is in years. Employed is a dummy variable for currently employed at the study's enrollment time. Knew the aftercare is a dummy variable for having heard of our partner ASP while incarcerated. Number of children is number of children. Has other support is a dummy variable for having access to other support services. Year of last arrest is year. Duration of last incarceration is length of most previous incarceration in days. Arrested 36m prior is a dummy variable for having been arrested at least once during the 36 months prior to enrollment into the study. Institutional housing is a dummy variable for providing an institutional address at study enrollment, i.e. parole office, halfway house, rehabilitation facility, homeless shelter.

Table A.3: ITT estimates of the number of valid visits within a year of enrollment conditional on no arrest six months after enrollment

	All	Non-Black	Black
Hard	0.558	0.035	1.174
	(0.234)	(0.316)	(0.317)
Easy	1.165	0.502	1.860
	(0.335)	(0.227)	(0.728)

Notes: Number of observations is 731 for All, 338 for Black participants and 394 for non-Black participants. Standard errors in parentheses. ITTs are calculated using Stata `multe` command, which implements Goldsmith-Pinkham et al. (2024) multiple treatment contamination bias robust estimates. We interact treatment and race to expand the number of effective treatments. Heterogeneous treatment effects are estimated jointly but presented in separate columns. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment.

Table A.4: ITT estimates of the number of valid visits within a year of enrollment - Experimental sample

	All	Non-Black	Black
Hard	0.495	0.170	0.927
	(0.225)	(0.291)	(0.312)
Easy	1.267	0.487	2.017
	(0.308)	(0.235)	(0.616)

Notes: Number of observations is 531 for All, 248 for Black participants and 283 for non-Black participants. Standard errors in parentheses. ITTs are calculated using Stata `multe` command, which implements Goldsmith-Pinkham et al. (2022) multiple treatment contamination bias robust estimates. We interact treatment and race to expand the number of effective treatments. Heterogeneous treatment effects are estimated jointly but presented in separate columns. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment.

Table A.5: Characteristics of Non-Black and Black participants

	Non-Black	s.d.	Black	s.d.	F-test	p-val.
Female	0.35	0.48	0.22	0.42	12.00	0.00
Age	41.63	10.16	44.08	12.37	9.60	0.00
Married/Partnered	0.05	0.23	0.08	0.27	1.32	0.25
Education (years)	12.39	1.78	12.00	1.35	7.71	0.01
Employed	0.25	0.43	0.27	0.45	0.49	0.48
Knew the aftercare	0.28	0.45	0.35	0.48	3.50	0.06
Number of children	1.80	1.94	2.37	2.38	8.85	0.00
Has other support	0.48	0.50	0.38	0.49	5.66	0.02
Year of last arrest	2,015.92	4.87	2,014.95	5.99	4.14	0.04
Duration of last incarceration	727.40	1,213.80	764.75	1,439.02	0.09	0.76
Arrested 36m prior to study	0.47	0.50	0.51	0.50	0.24	0.62
Institutional housing	0.66	0.48	0.44	0.50	26.71	0.00
Observations	283		248			

Notes: First and third columns are averages. Columns labelled s.d. are the standard deviation of the average. The last two columns report the F-statistic and p-value for a joint test of equality of the row variable across Non-Black and Black participants. Female is a dummy variable for female. Age is in years. Married/Partnered is a dummy variable for being in a relationship. Education is in years. Employed is a dummy variable for being currently employed at the time of enrollment into the study. Knew the aftercare is a dummy variable for having heard of our partner ASP while incarcerated. Number of children is number of children. Has other support is a dummy variable for having access to other support services. Year of last arrest is year. Duration of last incarceration is length of most previous incarceration in days. Arrested 36m prior is a dummy variable for having been arrested at least once during the 36 months prior to enrollment into the study. Institutional housing is a dummy variable for providing an institutional address at study enrollment, i.e. parole office, halfway house, rehabilitation facility, homeless shelter.

Table A.6: Treatment effects on 3-year re-arrest rate - Experimental sample

	All		Non-Black		Black	
	ITT	TOT	ITT	TOT	ITT	TOT
Hard	0.026		-0.040		0.045	
	(0.046)		(0.056)		(0.058)	
Easy	-0.008		0.095		-0.103	
	(0.046)		(0.056)		(0.062)	
Visits		-0.066		0.162		-0.260
		(0.036)		(0.111)		(0.119)

Notes: Number of observations is 531 for All, 248 for Black participants and 283 for non-Black participants. The experimental sample does not include the synthetic control (n=200). Standard errors in parentheses. ITT are calculated using Stata `multe` command, which implements Goldsmith-Pinkham et al. (2022) multiple treatment contamination bias robust estimates. We interact treatment and race to expand the number of effective treatments. Heterogeneous treatment effects are estimated jointly but presented in separate columns. [2]2SLS of the effect of the number of visits on the probability of being arrested up to 36 after enrollment. It implements Abadie et al. (2024) approach to deal with first-stage heterogeneity. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment. Missing information is handled by replacing missing items with a zero and adding a dummy for missing data.

Table A.7: Treatment effect of one extra visit on criminal offenses, type and cost of crime

	Crim. Offenses	Arrests			Cost of crime	
	All	Felony	Misdemeanor	Summary	Total	Max
Visits	-0.049	-0.019	-0.044	-0.046	-3,586.6	-707.8
	(0.038)	(0.029)	(0.033)	(0.031)	(3,460.9)	(515.9)

Notes: Number of observations is 731. Standard errors in parentheses. UJS data set classifies offenses into summary, misdemeanors, and felonies. Misdemeanors and felonies have three subcategories each. To calculate costs, we attribute to each one of these categories the cost per crime estimated by Miller et al. (2021). In particular, we take the average of the crimes that account for at least fifty percent of offenses in such category. We recode criminal offenses without a category as zero. The total cost is calculated by adding all criminal counts listed thee years after recruitment. We winsorize the sum at the upper 1% due to the existence of extreme observations. Similarly, the maximum is calculated as the maximum cost over all criminal counts during the three years since recruitment. Regressions implement Abadie et al. (2024)'s approach to deal with first-stage heterogeneity. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment. Missing information is handled by replacing missing items with a zero and adding a dummy for missing data.

Table A.8: Treatment effect of one extra visit by type of visit on arrests 3 years after recruitment

	Bus	Clothes	Housing	Jobs	Legal	Social
Visits	-0.081	-0.119	-0.115	-0.322	0.586	-0.201
	(0.095)	(0.093)	(0.126)	(0.224)	(0.557)	(0.119)

Notes: Number of observations is 731. Standard errors in parentheses. Each column estimates the TOT using different types of visits. TOT estimates are the effect of the number of visits on the probability of being arrested up to 36 after enrollment. The estimates implement Abadie et al. (2024) approach to deal with first-stage heterogeneity. Regressions control for sex, race, residence status (temporary housing, rehab center, homeless) and whether the participant had an arrest in the three years before study enrollment. Missing information is handled by replacing missing items with a zero and adding a dummy for missing data.

B Field experiment materials

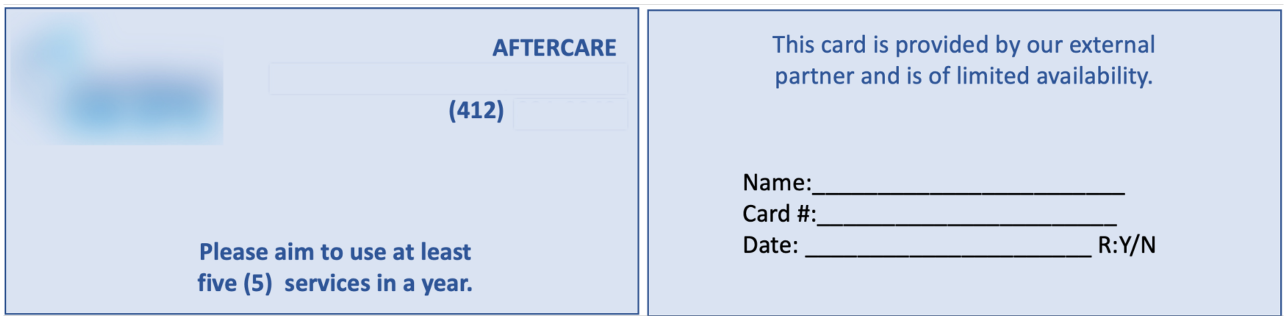

Figure B.1: Card for the Control group

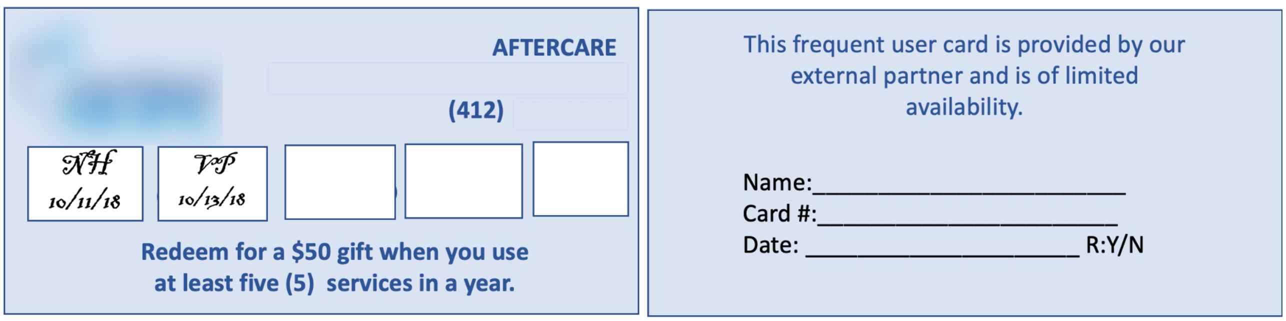

Figure B.2: Card for the Easy Treatment group

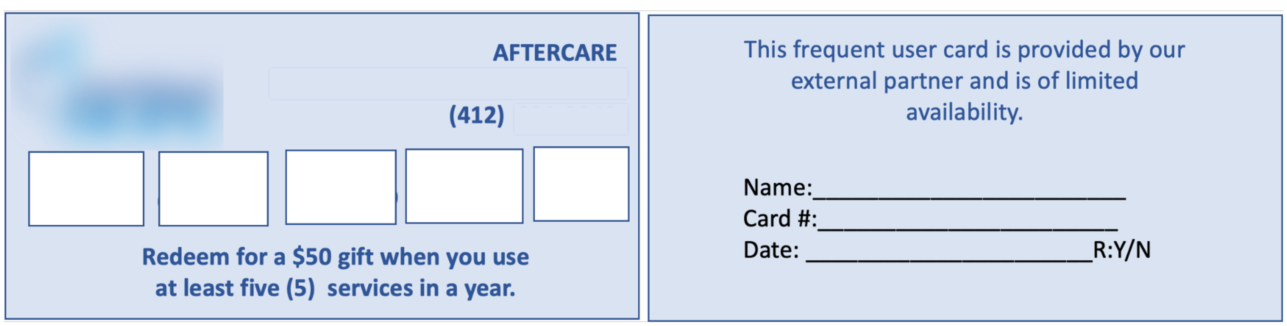

Figure B.3: Card for the Hard Treatment group

Intake survey questions

1. Name (first, last)
2. Date of birth
3. Address and zip code
4. Do you have a cell phone? If yes, what is phone number?
5. Do you have another contact person in case we cannot reach you? (name, phone number, relationship)
6. Highest level of education
7. Currently employed? Number of hours work per week
8. Marital status
9. Number of children
10. How did you get to our office today? (bus, drive, got a ride, took taxi/uber, walk, bike, other)
11. How long did it take you and how much did it cost?
12. Is this your first time coming to our office? If not, how long have you been coming to our office?
13. Did you know about us while you were still in jail? If so, how did you hear of us? what services did you think we provided?
14. Where were you last arrested (county)? What year?
15. How long was your last incarceration?
16. Was that your first arrest? If not, where was your first arrest (county) and year?
17. How long was your first incarceration?
18. Have you ever been arrested in other states outside of Pennsylvania?
19. Do you participate in other support programs outside of our services? If so, list them. How satisfied are you with them?

20. How many people can you think of that would help you out in the following situations? Name the top three persons (their relationship to you)? If you need cash, a job, life advice?
21. In a typical day, when do you get up, when do you go to sleep, how many hours do you spend outside the house?
22. How often do you attend church, chapel or other places of worship?
23. What is the biggest challenge you experienced after being released?
24. What service do you wish we provide to help you overcome the challenge?
25. Do you feel that our office can help you avoid future rearrests? Why or why not?