Use of Reentry Support Services and Recidivism: a Field Experiment Varying Dosage

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Abstract

Many previously incarcerated individuals are rearrested in the months and years 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 compared to the control group, with those in the 3-visit treatment completing the most. Intent-to-treat effects on rearrest are null in the full sample, but Black participants who complete 3-4 visits are 21.8 percentage points less likely to be rearrested.

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

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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.

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, etc. 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 comply with the assigned treatment, and thus, treatment service programs are often not completed as designed.[4] Mixed findings

[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 (Beshears et al., 2012).

[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 (2022) note 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

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. Our analysis accounts for noncompliance and unordered treatments reflecting variation in dosage.

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. 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

part of these services, it implies a higher frequency of personal contact and potentially more scrutiny and monitoring. D'Amico and Kim (2018) find an 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.

proportion completing 3-4 visits (14%) is no different than the Control. We use differences and similarities across the three treatment distributions of visits to identify the causal effect of visit dosage on recidivism.

There are no significant treatment effects on rearrest rates in the full sample, but there is heterogeneity by race. For Black participants, the Easy treatment significantly reduces the rearrest rate by 21 percentage points relative to the Hard treatment, after correcting for multiple hypothesis testing. For Non-Black participants, there are no significant treatment effects.

We are interested in the effect of different visit dosages on recidivism. Because we have more than one treatment and the treatments result in unordered monotonicity in visits, the ITT estimates alone combine the effects of different visit dosages. To isolate the effect of particular visit dosages on rearrest, we derive the necessary conditions and assumptions needed for identification. Under these conditions, we estimate the causal effect of 3-4 and 5+ visits on rearrest. Consistent with the ITT results, we find no significant effects of dosage on rearrest in the full sample but do find significant heterogeneity by race. For Black participants, completing 3-4 visits results in a significant 21.8 percentage point reduction in the probability of rearrest. Completing 5+ visits has no significant effect on rearrest. For Non-Black participants, there are null effects of dosage on rearrest. All results adjust for multiple hypothesis testing.

Our findings suggest that a modest number of visits, in our setting and for certain groups, can be beneficial to reduce recidivism, while a larger number of visits has no effect. Services used during visits up to and including the fourth were mainly focused on satisfying short-run needs, i.e. clothing, housing, food, identification card, transportation. Only from the fifth visit onwards were participants more likely to seek out services for longer-run needs, i.e. employment, peer support and mentoring. This result emphasizes the importance of a modest dosage of service delivery that meets the short-run needs of the previously incarcerated to help reduce recidivism. Participants were allowed to choose which services they wanted to focus on, and those whose incentivized dosage was modest focused mainly on short-run needs. Those with larger incentivized dosages eventually focused on long-run needs, but there was no effect on rearrests rates for this group.

Black participants drive the dosage effects on recidivism. This is consistent with Black participants being more disadvantaged and thus in need of service provision. Pre-treatment measures support this: the group is older and has more children, lower education and less access to other support services. We also examine whether the significant dosage effects on recidivism are due to the dosage itself or selection into treatment. We find evidence consistent with selection, whereby those who are Black, unemployed, have less access to

other support services and have been arrested more recently are more likely to comply in the Easy treatment relative to the Hard treatment.

The findings suggest that a policymaker might want to encourage a few visits to reentry services, but not encourage a lot of visits. This would attract individuals who would benefit the most from the services and avoid harm for those who rely on them excessively. We note, however, that the optimal policy will depend on the composition of response types in the population under study, and experimental designs like ours can help identify optimal policies.

Our main contribution is to show the importance of heterogeneous and non-monotone response to treatments in the context of reentry services aimed to reduce recidivism. Rose and Shem-Tov (2021) find that incarceration reduces recidivism, but the effect diminishes with the dose of imprisonment. Our identification strategy and setting are different, and we find that treatment effects on recidivism vary by reentry service visit dosage.

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 document 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. In our experiment, the SUTVA condition can be violated if different participants choose different treatments (i.e. services) or due to interference. We find no evidence for this. Services used across treatments are not statistically different from each other. Using a synthetic control group not affected by the treatment at all, we test for interference and find no evidence. Estimates accounting for potential contamination due to timing of visits are identical to our main results.

Our experimental approach to increase use of support services is by providing visit goals tied to a monetary incentive upon achievement of the goal. 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 challenging, yet attainable, goals, and a certain goal that works well for one individual might not for another. By setting different visit dosage goals, our study design allows us to examine the effect of these goals and contribute to this literature.[5]

[5]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

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 also discuss heterogeneous treatment effects. Section 6 describes the conditions to identify dosage effects on recidivism and presents estimates. Section 7 discusses our findings, and Section 8 concludes.

2 Field experiment

The field experiment is designed to examine how encouragement of different dosages of reentry services affects recidivism.[6] The design employs encouragement of service usage via increased visits, rather than random assignment to service access. It also focuses on visits,

participants via visits to the service provider and includes monetary incentives.

[6]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.

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.

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.[7] Their reintegration program includes a variety of services, including material assistance (i.e. bus passes, use of computers and phones, clothing), informational resources, referrals, 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.[8]

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

[7] 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).

[8] 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.2.

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 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.[9]

Upon completion of the intake survey, participants are randomized into one of three groups: a control group and two treatment groups.[10] 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.[11]

[9]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.

[10]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.

[11]Most services provided were counted as valid (Table A.2). 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.[12] 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

[12]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).

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.1). 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.[13] 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 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

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

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.2 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.[14] 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.[15] 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

[14]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.

[15]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.

control from January 2011 through July 2023.

For our analysis, a criminal offense is defined as an encounter with law enforcement that 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.[16] 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% are Black, 5% are women and the average age is 44 years old.[17]

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

[16] There is balance across the Pure Control and Control, so we pool them. Table A.1 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.

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

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.1. 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.

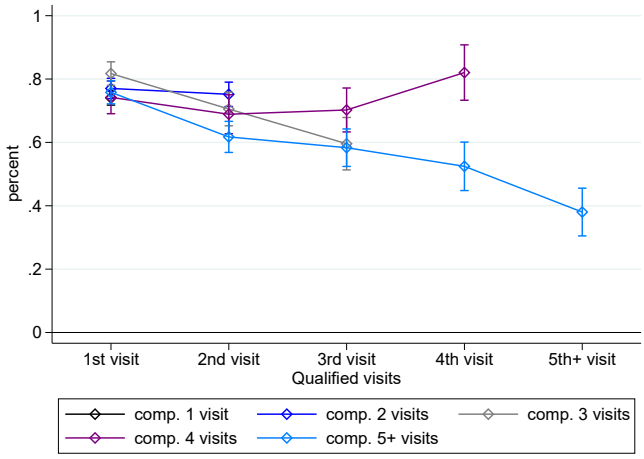
coefficients across the three groups.

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+ visits within one year of study enrollment.[18] Services are grouped into short-run necessities (food, housing, clothing, transportation, identification card) and longer-run needs (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

[18]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.



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 (n=85), 4 visits (n=55), 3 visits (n=76), 2 visits (n=140) and 1 visit (n=303).

Figure 1: Percent of services used for short-run necessities (i.e. clothing, housing, food, ID, transportation) by visit number

previously incarcerated need to address short-run necessities first.[19]

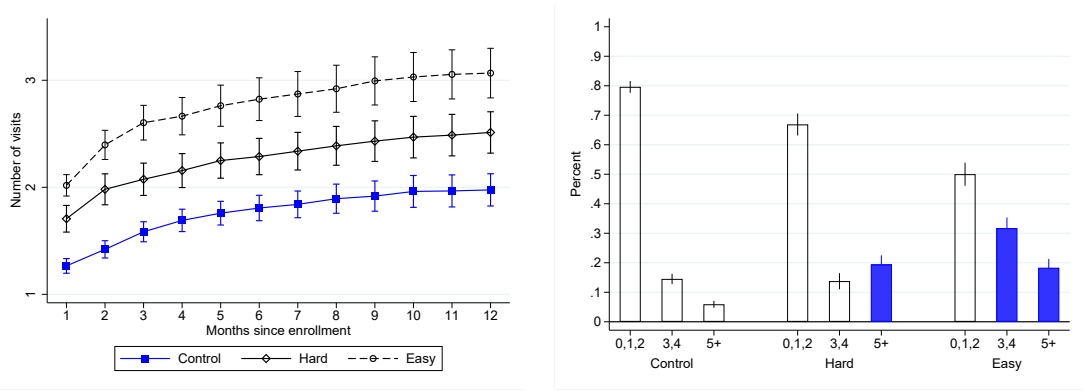
5 Treatment effects

We examine treatment effects on number of visits and the probability of arrest within three years of study enrollment. We confirm that our experimental design worked as intended to increase visits. We then examine the effect of treatment dosage on recidivism in the full sample and separately for Black and Non-Black participants, as specified in our pre-registration and pre-analysis plan. We do not explore heterogeneity by sex, as we originally intended, as there is insufficient variation (see Table 1). Heterogeneity analysis is adjusted for multiple hypothesis testing.

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

[19]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).



(a) Average visits (b) Total 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) show the distribution of total visits per treatment group. Blue indicates the proportion in Easy and Hard that completed the required number of visits for the $50 incentive.

Figure 2: Treatment effects on number of visits

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.[20] 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 highest in Easy, then Hard and then Control at every month since enrollment.[21]

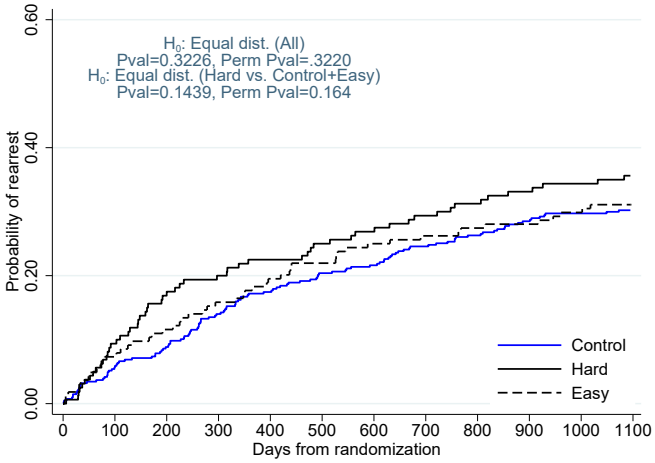
The treatments also reduced the time taken to complete visits. Participants in the Easy treatment completed 3-4 visits in half the time or less than those in the Control and Hard treatments. Those in the Hard treatment completed 5 visits in two-thirds the time or less than those in the Control and Easy treatments.[22]

Compliance was attained in the Hard treatment if visits equaled or exceeded five and in the Easy treatment if visits equaled or exceeded three. The distribution of the final number of visits by treatment is shown in Panel (b) of Figure 2. We combine 0-2, 3-4 and 5+ visits together in the figure to ease comparison of compliance across treatments. The figure illustrates that the Easy treatment has a higher level of compliance (50%) than the Hard treatment (19%). The Easy treatment significantly increased the percent of participants

[20] 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).

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

[22] 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.



Notes: The figures show failure functions for arrests by treatment over the three years after study enrollment.

Figure 3: Rearrest rates

who completed 3-4 and 5+ visits compared to the Control, and it significantly increased the number of 3-4 visits compared to the Hard treatment. Compared to the Control group, the Hard treatment only increased the proportion of participants who completed 5+ visits. The proportion of participants who completed 5+ visits is the same in the Easy and Hard treatments. Compliance with treatment by Black and Non-Black participants is similar to each other and the full sample (Table A.2).

5.2 Arrests

Treatment effects on the probability of arrest within three years of study enrollment are illustrated in Figure 3 using Kaplan-Meier failure rates (Kaplan and Meier, 1958). These are estimated for each treatment group using public data on arrests from the Unified Judicial System of Pennsylvania's criminal records.[23]

Rates are highest for the Hard treatment followed by the Easy and Control treatments. These are not significantly different from one another in the full sample, but they are more pronounced and significant in the subsample of Black participants (Figure A.3).[24]

Our main results are presented in Table 2. This table reports intention-to-treat effects of the Easy and Hard treatments on the probability of arrest within three years of study enrollment. The first three columns report treatment effects for All, Non-Black and Black

[23]We do not examine treatment effects on types of crimes committed because of lack of power and such analysis was not preregistered.

[24]The hypothesis of equal distance of failure rates for the full sample across treatments cannot be rejected (p-value=0.3226). For Black participants, this hypothesis is rejected (p-value=0.0321).

Table 2: Treatment effects on 3-year re-arrest rate

	All	Non-Black	Black	All	Non-Black	Black
Easy	0.003	0.102	-0.125	0.005	0.078	-0.112
s.e.	(0.040)	(0.055)	(0.057)	(0.041)	(0.058)	(0.056)
MHT p-value		[0.292]	[0.179]		[0.428]	[0.168]
Hard	0.055	0.022	0.085	0.058	0.003	0.084
s.e.	(0.043)	(0.059)	(0.062)	(0.043)	(0.062)	(0.062)
MHT p-value		[0.711]	[0.412]		[0.956]	[0.428]
Constant	0.201	0.191	0.212	0.301	0.289	0.326
s.e.	(0.031)	(0.036)	(0.042)	(0.022)	(0.031)	(0.033)
Month/year controls				x	x	x
Obs	731	393	338	728	390	336
Diff Easy-Hard	-0.052	0.080	-0.210	-0.053	0.075	-0.196
H0: Easy-Hard=0, F-test	1.102	1.359	8.886	1.129	1.140	8.318
p-value	(0.294)	(0.244)	(0.003)	(0.288)	(0.286)	(0.004)
MHT p-value		[0.328]	[0.000]		[0.263]	[0.014]

Notes: The table presents the intention to treat estimates for arrests within three years of study enrollment. Regressions control for sex, race, and whether the participant had an arrest in the three years prior to study enrollment. The last three columns control for the interaction of month and year of enrollment. In the top panel, standard errors are in parentheses and p-values adjusted for multiple hypothesis testing are in square brackets. The bottom panel reports the difference in coefficients between the Easy and Hard treatments and the F-statistic and p-value for the null hypothesis that these coefficients are equal. The last row reports p-values for the F-test adjusting for multiple hypothesis testing.

participants. All regressions control for the participant's sex, race and whether an arrest occurred in the three years prior to study enrollment. To account for differential timing of participation, especially given shutdowns due to COVID-19, the last three columns use the same specification as the first three with the inclusion of controls for month and year of enrollment.[25]

We find no significant effects of the Easy or Hard treatments, compared to the Control, on rearrest. This holds in the full sample and subsamples of Black and Non-Black participants, for which results are corrected for multiple hypothesis testing. However, we do find significant differences between the two treatments for Black participants. As reported in the bottom panel of the table, the Easy treatment reduces the rate of rearrest by 20-21 percentage points compared to the Hard treatment. This difference is not significant in the full sample or for

[25]There were no constraints on when participants in the Control, Easy and Hard treatment groups could visit the ASP for services. We test for interference using the pooled experimental and synthetic control groups and find no evidence (Table A.3).

Non-Black participants.

6 Dosage effects

In this section, we discuss compliance with treatment and conditions under which we can obtain the causal effect of different visit dosages on recidivism.

We define compliance as taking values 0, 1, or 2 that correspond to 0-2 visits (0), 3-4 visits (1), and 5 or more visits (2). Treatment assignment is defined by variable $Z \in \{0, 1, 2\}$ for Control, Hard and Easy treatment conditions. The corresponding counterfactual outcomes are $Y_i(0, z)$, $Y_i(1, z)$, and $Y_i(2, z)$. Each participant responds to treatment assignment according to $D_i : \{0, 1, 2\} \to \{0, 1, 2\}$. The observed outcome is determined as follows:

$$Y_i = \sum_j \mathbf{1}\{Z_i = j\} Y_i(D_i(j)) \tag{1}$$

Assumption 1: i. $Y_i(D, Z) = Y_i(D)$ for all D and Z. ii. $(Y_i(D), D_i(Z))$ are independent of Z for all D and Z.

Assumption 1.i is an exclusion restriction, and Assumption 1.ii is an independence assumption. They establish that treatment assignments are valid instruments.[26] In our experiment, there are 27 different ways to respond to the incentives offered.[27] To discipline our analysis, we start by making a monotonicity assumption.

Assumption 2 (Monotonicity): $D_i(j) \geq D_i(0)$ for all i and $j = 1, 2$.

The assumption says that visits are increasing in incentives. The assumption does not claim that either the Easy or Hard treatment elicits higher compliance. Evidence of monotonicity can be seen in Figures 2 and A.1.

Monotonicity implies that the ITT estimates for the Easy treatment capture the effect on participants who increased visits from 0-2 to 3-4, 0-2 to 5+ or 3-4 to 5+ with respect to the Control group. The ITT estimates for the Hard treatment capture the effect on participants who increased visits from 0-2 to 3-4, 0-2 to 5+ or 3-4 to 5+ with respect to the Control group.[28]

[26] We follow Lee and Salanié (2020)'s presentation of the necessary conditions for valid instruments.

[27] Possible patterns of the three binned visit behaviors across the three treatments are $3^3 = 27$.

[28] The classification we adopt is not without a potential cost. Angrist and Imbens (1995) show that binarizing a continuous treatment can lead to a violation of the exclusion restriction if the chosen thresholds do not fully capture changes in the intensive margin across treatments. To examine potential intensive margin variation within our defined categories, we test if the number of visits of those completing less than three visits is different between treatments and control and if the number of visits of those completing at least five visits is different between treatments and controls. We find no evidence consistent with changes in the intensive margin of visits. This suggests our discretization does not generate a violation of the exclusion restriction.

Let β_E^{ITT} and β_H^{ITT} denote the ITT estimate of the Easy and Hard treatments. Let $i_z \to j_{z'}$ denote that a participant switches from compliance level i when $Z = z$ to j when $Z = z'$. Let $P(i_z \to j_{z'})$ represent the proportion of participants in the population who follow this pattern of behavior. Under Assumption 1 and Assumption 2, we have that: $\beta_E^{ITT} = P(0_C \to 1_E)E[Y(1) - Y(0)|0_C \to 1_E] + P(0_C \to 2_E)E[Y(2) - Y(0)|0_C \to 2_E] + P(1_C \to 2_E)E[Y(2) - Y(1)|1_C \to 2_E]$ and $\beta_H^{ITT} = P(0_C \to 1_H)E[Y(1) - Y(0)|0_C \to 1_H] + P(0_C \to 2_H)E[Y(2) - Y(0)|0_C \to 2_H] + P(1_C \to 2_H)E[Y(2) - Y(1)|1_C \to 2_H]$.

This exercise illustrates that the ITT estimators combine the effects of different treatment dosages. We cannot rely on ITT estimates by themselves to identify the effect of specific visit dosage on recidivism. Nonetheless, we can use the expressions to determine the conditions needed to identify the causal effect of specific visit dosages. For instance, economic rationality suggests that a participant would not switch from 0-2 visits if assigned to the Control to 3-4 visits if assigned to the Hard condition.[29] Such behavior would not secure rewards. If the participant does not need the reward to complete 3-4 visits, they should complete them in the Control as well. We should then not expect a statistically significant increase in 3-4 visits between the Control and Hard treatments. Figure 2, Panel (b), shows behavior consistent with this condition. The proportion of participants completing 3-4 visits in Control and Hard are the same.[30] The main implication of this observation is that we can identify the effect of 5+ visits on rearrest by estimating the effect of being assigned to the Hard treatment compared to the Control, excluding the Easy condition.

Having excluded the possibility that 3-4 visits increase in the Hard treatment, we explore conditions to identify the effect of 3-4 visits on rearrest. The formulas above suggest we can determine the effect of 3-4 visits by comparing the Easy and Hard treatments, excluding the Control, if two conditions hold. First, the proportion of participants who switch to 5+ visits is similar in the Easy and Hard treatments, and second, those who switch to 5+ visits in the Easy and Hard conditions are the same participants. The first condition is testable since we observe visits, and Figure 2, Panel (b), validates it. The proportion of 5+ visits in Easy and Hard are the same ($\sim 19\%$).[31] We assess the validity of the second condition by comparing the composition of the first five visits for those who completed at least five visits in the Easy

[29]Both 0-2 visits and 3-4 visits are available in Control and Hard at the same latent price, i.e. the non-observed incentives are fixed. Switching across treatments would reveal that both options are mutually revealed preferred to each other. Such behavior could be consistent with someone who planned to do more but failed, however, we assume this behavioral pattern is absent.

[30]There is no significant difference in the proportion of 3-4 visits between the Control and Hard treatments for the whole sample (p-value = 0.8196) and the subsamples of Black (p-value = 0.7607) and Non-Black (p-value = 0.5010) participants.

[31]There is no significant difference in the proportion of 5+ visits between the Easy and Hard treatments for the full sample (p-value = 0.8040) and the subsamples of Black (p-value = 0.7604) and Non-Black (p-value = 0.5711) participants.

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

	3-4 visits dosage effect			5+ visits dosage effect		
	All	Non-Black	Black	All	Non-Black	Black
Easy	-0.057	0.080	-0.218			
s.e.	(0.050)	(0.069)	(0.073)			
MHT p-value		[0.248]	[0.018]			
Hard				0.058	0.007	0.076
s.e.				(0.044)	(0.063)	(0.063)
MHT p-value					[0.875]	[0.430]
Constant	0.272	0.204	0.299	0.301	0.283	0.330
s.e.	(0.054)	(0.062)	(0.069)	(0.022)	(0.031)	(0.033)
Obs	324	173	151	564	298	264

Notes: The table presents the intention to treat estimates for arrests within three years of study enrollment. Regression control for sex, race, and whether the participant had an arrest in the three years before study enrollment. The first three columns compare the Easy and Hard treatments to identify the effect of 3-4 visits. The last three columns compare the Control and Hard treatments to identify the effects of 5+ visits. Standard errors are reported in parentheses, and p-values adjusted for multiple hypothesis testing are reported in square brackets.

and Hard treatments. We find no statistical difference in the composition of these visits, providing evidence that the maintained identification assumption is not violated.[32]

Dosage effects are presented in Table 3 under the conditions outlined above. The first three columns report treatment effects using the Easy and Hard treatments for All, Non-Black and Black participants. These estimates identify the effect of 3-4 visits on the probability of being arrested within three years of study enrollment. The last three columns report treatment effects using the Control and Hard treatments. These estimates identify the effect of 5+ visits on the probability of arrest. All regressions control for the participant's sex, race and whether an arrest in the last three years before enrollment in the study occurred.[33]

The table shows no significant dosage effects for the full sample or Non-Blacks. For Black participants, completing 3-4 aftercare service visits significantly decreases recidivism by 21.8

[32]To test the hypothesis, we average the dummy variables indicating a visit was for a particular purpose (e.g., clothing). We compare these averages across treatments. The statistical tests all suggest no differences in behavior (jobs (p-value = 0.851), bus pass (p-value = 0.438), legal advice (p-value = 0.938), clothing (p-value = 0.357), housing (p-value = 0.464), social and mentoring (p-value = 0.783))

[33]Estimates are identical if we include controls for month and year of study enrollment. As a robustness check for potential interference, we adjust estimates in Table 3 with an adjacency matrix that clusters standard errors for participants visiting the ASP on the same date. Results are identical (Table A.4). We find no evidence of treatment effects spillovers in the pure and synthetic control.

percentage points. Completing 5+ visits increases recidivism by 7.6 percentage points, but this is not significant. These results suggest that dosage effects are nonlinear, with a lower dosage of visits, i.e. 3-4 in our sample and setting, reducing recidivism and a higher dosage of visits, i.e. 5+ in our sample, having no significant effect.

7 Discussion

Participants could choose which services they wanted to use. For those who completed 3-4 visits, most usage ($70 - 80\%$) is focused on short-run necessities (Figure 1). This pattern is consistent for Black and Non-Black participants (Figure A.4). The significant effect of a smaller dosage of visits on reducing recidivism is consistent with the importance of taking care of short-run needs over the course of several visits. More visits, at least in our setting, did not reveal effects on re-arrest. Those who completed 5+ visits within a year of study enrollment tended to use fewer services that address short-run needs from the first visit, compared to those who completed 3-4 visits, and significantly so from the fourth visit onwards. These patterns hold for All, Non-Black and Black participants. We find no evidence that the composition of services used for short-run necessities (food, housing, clothing, transportation, ID) differed across Non-Black and Black participants.

Pre-treatment variables suggest that Black participants are more disadvantaged and have less support than Non-Black participants (Table A.5). It would then make sense that the ASP services are more effective for them. Black participants are more likely to be male (78% v. 65%), are older (44.1 v. 41.6 years), have more children (2.37 v. 1.8 children), have lower education (12.0 v. 12.4 years) than Non-Black participants, less likely to be in institutional housing (44% v. 68%) but also less likely to be in a rehabilitation program (5.2% v. 9.5%). They also are 10 percentage points less likely to participate in other support programs outside of the ASP than Non-Black participants, i.e., they may have fewer program substitutes (see Kline and Walters, 2016).

The significant effect of visit dosage on recidivism (Table 3) could be due to the dosage itself or the types of people who select into treatment. We can identify the characteristics of those who switched to 3-4 visits in the Easy treatment and those who switched to 5+ visits in the Hard treatment using the same assumptions that allow identification of dosage effects (see Section 6). Estimation of the characteristics of different response types are reported in Table A.6. While complier characteristics across Easy and Hard treatments are similar in several aspects, there are differences. Those who comply in the Easy treatment, compared to the Hard treatment, are more likely to be Black, less likely to be employed, less likely to have access to other support services, more likely to have been arrested recently, and less likely to

have provided an institutional address at study enrollment.[34] These differences suggest that the significant effect of 3-4 visits on reducing recidivism, compared to the null effect of 5+ visits, might be due to the type of individual the Easy and Hard treatments attract, rather than dosage itself.

Finally, 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.[35] 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.[36] These values are sizeable and exceed the $50 gift card received upon visit goal completion.

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. In the full sample, we find null intent-to-treat effects on the probability of rearrest, but this hides important heterogeneity. Black participants are roughly 21 percentage points less likely to be rearrested if they complete a modest number of visit to the service providers. A larger number of visits is ineffective. Participants choose the types of services they need, and the focus during initial visits is meeting short-run needs, such as procuring clothing, housing, food, transportation and an identification card. Services, such as employment, only come into focus in later visits.

Our field experiment provides two different levels of visit dosages, and participants can respond by completing any number of visits. This complexity, coupled with unordered mono-

[34] We are cautious in interpreting the differences in employment too strongly as this variable was not strongly balanced across treatments (see Table 1).

[35] 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.

[36] 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.

tonic response to treatment, yields many different compliance types. To estimate the causal effect of visit dosage on the probability of rearrest, unconfounded by complier types, we discuss conditions for identification of dosage effects. We note that the ability to identify dosage effects is highly context-dependent (see Heckman and Pinto, 2018; Pinto, 2021). Alternative identifying assumptions than those used in our study might be required to identify treatment effects (see Rose and Shem-Tov, 2021; Lee and Salanié, 2020).

Our experiment provides a possible explanation for the existing mixed evidence on the effectiveness of reentry services in reducing recidivism. The effect of service dosage is nonlinear, and examination of intent-to-treat effects of a binary treatment misses this important nuance. Our study also speaks to the design and interpretation of experiments in the presence of dosage effects. Had we implemented either the Easy or Hard treatment in isolation, and only looked at the full sample, we might have concluded that reentry services are ineffective.

By observing behavior in both the Easy and Hard conditions and examining heterogeneous treatment effects of dosages, we can conclude that a small dosage is effective. The lower dosage treatment may be more effective for Black participants because they are more disadvantaged and lack support. Characterization of complier types show that the treatments attract different types of people, suggesting that selection into treatment maybe important in understanding the significant effect of small visit doses on reducing recidivism.

Overall, our study illustrates the benefits of exploring non-monotone responses to treatment to determine policy effectiveness. A second look at support services for the previously incarcerated or other similar programs for which the current evidence on average treatment effects of effectiveness is mixed may be warranted. A fruitful avenue to explore would be deploying research designs to help uncover the most effective aspects to aid those seeking help and embed those in program design.

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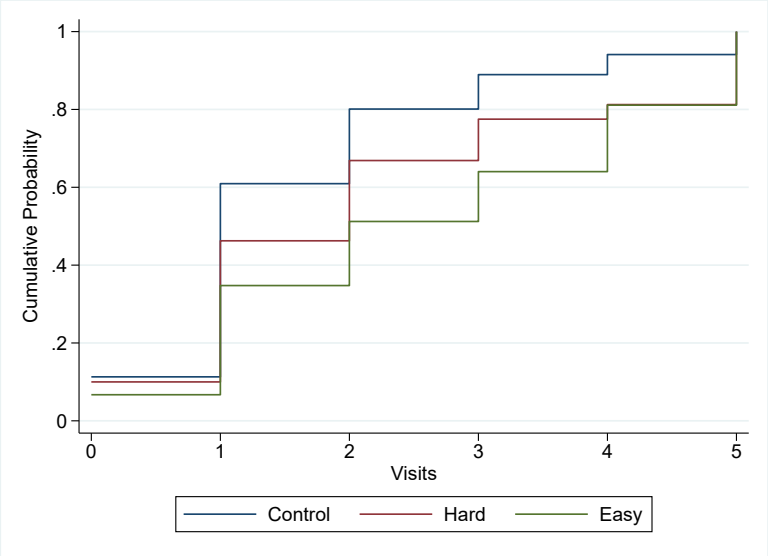
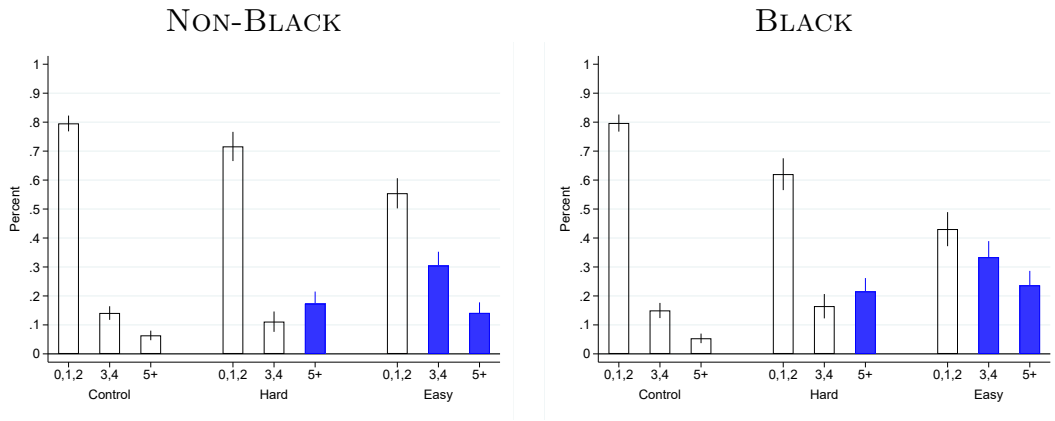
APPENDICES INTENDED FOR ONLINE PUBLICATION

A Extra figures and tables



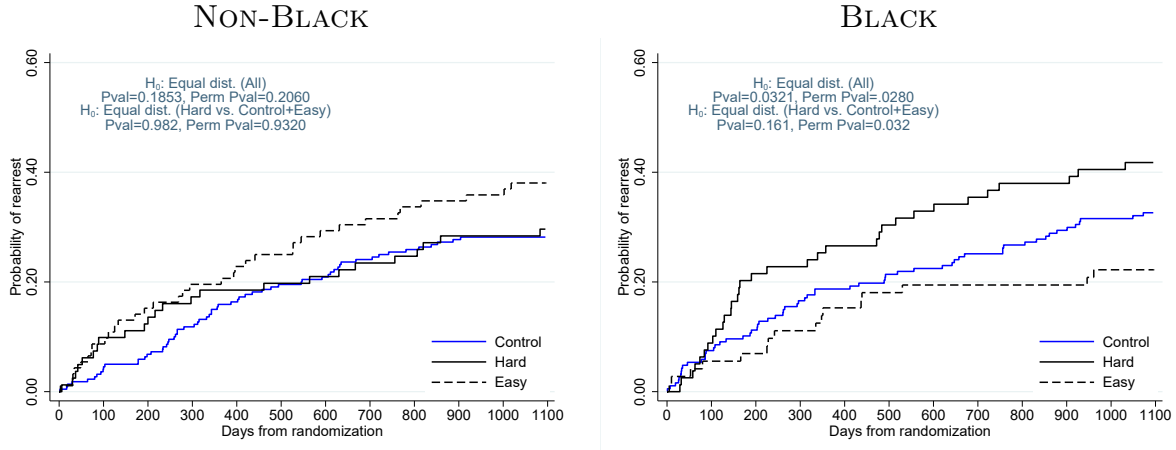
Notes: The figure shows the distribution of number of visits by treatment. The treatments are first-order stochastically ordered by Easy, Hard and Control.

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



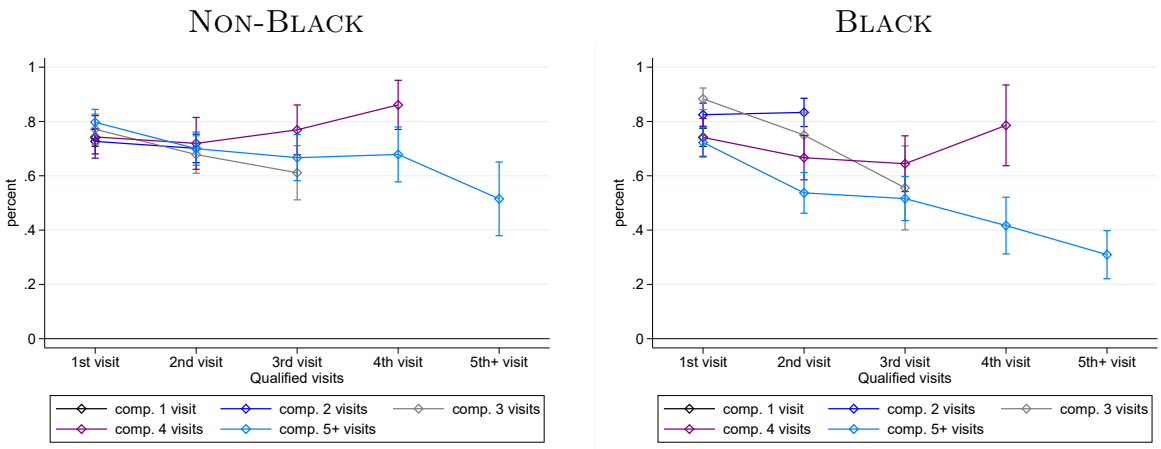
Notes: Blue indicates the proportion in Easy and Hard that completed the required number of visits for the $50 incentive. Error bars denote standard errors.

Figure A.2: Total number of visits completed within one year of study enrollment - Non-Black and Black participants



Notes: The figure shows failure functions for arrests by treatment over the three years after enrollment.

Figure A.3: Rearrest rates for 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.4: Percent of services used for short-run necessities (i.e. clothing, housing, food, ID, transportation) by visit number - Non-Black and Black participants

Table A.1: 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 labelled s.d. which is 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 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.

Table A.2: 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.3: Testing for interference using the Control group

	All	Non-Black	Black
Proportion in network assigned to Hard	0.055	-0.073	0.257
	(0.099)	(0.126)	(0.162)
Proportion in network assigned to Easy	-0.081	-0.018	-0.179
	(0.110)	(0.143)	(0.171)
Constant	0.197***	0.216***	0.207**
	(0.049)	(0.058)	(0.063)
Obs	407	220	187
R2	0.059	0.044	0.086
H0: no interference	0.664	0.838	0.189

Notes: Dependent variable is probability of re-arrest within 3 years of study enrollment. Controls include sex, race and whether the participant had an arrest in the three years prior to study enrollment. We test if arrest rates in the control group are affected by the assignment to treatment of other ASP clients in the participants' network. We represent a participant's network by an adjacency matrix. The adjacency matrix equals 1 if two participants visited the ASP on the same date at least once during the study and 0 otherwise. We use both qualified and non-qualified visits to create this matrix. Participants in the control group visited on the same date with 4.95 (3.82) participants during the study. The null hypothesis (H0) reports the joint significance test of the proportion of people assigned to both Easy and Hard treatments. While there is significant overlap on visit dates, we cannot reject the null hypothesis of no interference.

Table A.4: Dosage Effects on 3-year rearrest rate (Table 3) - accounting for spatial correlation

	3-4 visits dosage effect			5+ visits dosage effect		
	All	Non-Black	Black	All	Non-Black	Black
Easy	-0.057	0.080	-0.218***			
	(0.049)	(0.068)	(0.071)			
Hard				0.053	0.020	0.085
				(0.042)	(0.058)	(0.061)
Constant	0.272***	0.204***	0.299***	0.187***	0.208***	0.212***
	(0.052)	(0.061)	(0.065)	(0.035)	(0.041)	(0.046)
Obs	324	173	151	567	301	266

Notes: The table reproduces Table 3 and allows for interference. Dependent variable is probability of re-arrest within 3 years of study enrollment. Controls include sex, race and whether the participant had an arrest in the three years prior to study enrollment. We create an adjacency matrix that equals 1 if two participants visited the ASP on the same date at least once during the study and 0 otherwise. We use both qualified and non-qualified visits to create this matrix. The Moran test for spatial dependence on the whole sample is $\chi^2(1) = 3.53$ (p-value = 0.0604). We use this matrix to adjust standard errors for spatial correlation. Estimates are obtained using Stata' spregress.

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: Complier characteristics

	Switched to 3-4 visits				Switched to 5+ visits			
	All	Complier	NT	AT	All	Complier	NT	AT
Female	0.27	0.44	0.25	0.18	0.29	0.47	0.25	0.44
	(0.02)	(0.21)	(0.04)	(0.09)	(0.02)	(0.33)	(0.04)	(0.13)
Black	0.47	0.51	0.43	0.59	0.48	0.42	0.48	0.56
	(0.03)	(0.18)	(0.05)	(0.11)	(0.03)	(0.33)	(0.04)	(0.13)
Age	42.61	40.89	42.88	43.50	42.28	42.70	42.01	44.50
	(0.62)	(4.06)	(1.03)	(2.47)	(0.58)	(25.50)	(0.96)	(2.15)
Married/Partnered	0.06	0.04	0.08	0.00	0.06	0.02	0.06	0.13
	(0.01)	(0.08)	(0.03)	(0.00)	(0.01)	(0.15)	(0.02)	(0.09)
Education(years)	12.23	11.93	12.34	12.05	12.17	12.27	12.12	12.56
	(0.09)	(1.46)	(0.15)	(0.25)	(0.09)	(1.14)	(0.14)	(0.33)
Employed	0.25	0.06	0.33	0.09	0.24	0.81	0.16	0.19
	(0.02)	(0.22)	(0.04)	(0.06)	(0.02)	(0.59)	(0.03)	(0.10)
Knew the aftercare	0.32	0.10	0.35	0.50	0.31	0.11	0.33	0.31
	(0.03)	(0.19)	(0.05)	(0.11)	(0.02)	(0.55)	(0.04)	(0.12)
Number of children	1.99	2.11	1.94	2.10	2.10	2.11	2.07	2.47
	(0.12)	(0.74)	(0.20)	(0.61)	(0.11)	(1.34)	(0.19)	(0.32)
Has other support	0.42	0.42	0.46	0.20	0.44	0.57	0.41	0.60
	(0.03)	(0.16)	(0.05)	(0.09)	(0.03)	(0.29)	(0.04)	(0.13)
Year of last arrest	2,015.64	2,015.76	2,015.71	2,015.18	2,015.29	2,010.99	2,015.92	2,015.25
	(0.29)	(4.71)	(0.45)	(1.17)	(0.31)	(8.46)	(0.40)	(1.06)
Duration of last incarceration	762.99	254.55	857.26	1,174.24	729.21	215.38	790.83	959.87
	(78.40)	(410.35)	(142.99)	(463.48)	(74.66)	(3,341.49)	(127.56)	(387.35)
Arrested 36m prior to study	0.48	0.60	0.48	0.32	0.48	0.24	0.50	0.63
	(0.03)	(0.21)	(0.05)	(0.10)	(0.03)	(0.81)	(0.04)	(0.12)
Institutional housing	0.57	0.39	0.59	0.70	0.55	0.65	0.54	0.40
	(0.03)	(0.18)	(0.05)	(0.11)	(0.03)	(0.50)	(0.04)	(0.13)

Notes: The table reports average characteristics for each response type by compliance in the Easy (Switched to 3-4 visits) and Hard (Switched to 5+ visits) treatments. Standard errors in parentheses. Response types are Compliers, Never Takers (NT) and Always Takers (AT). 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.

B Field experiment materials

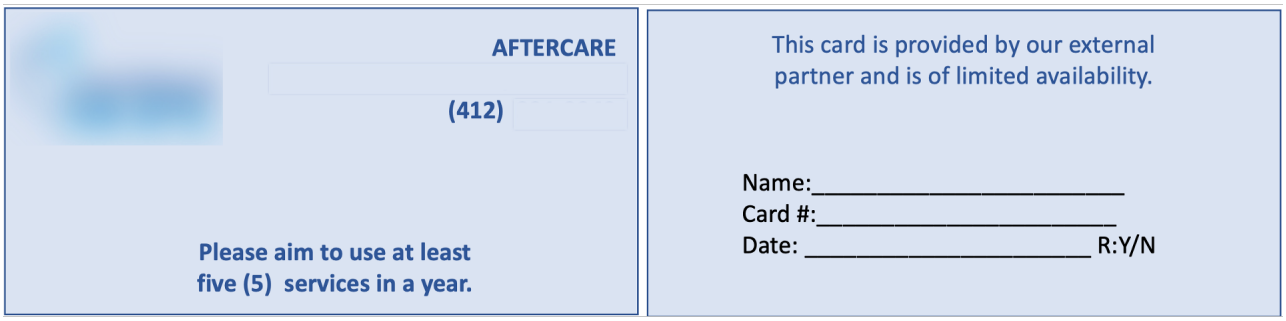


Figure B.1: Card for the Control group

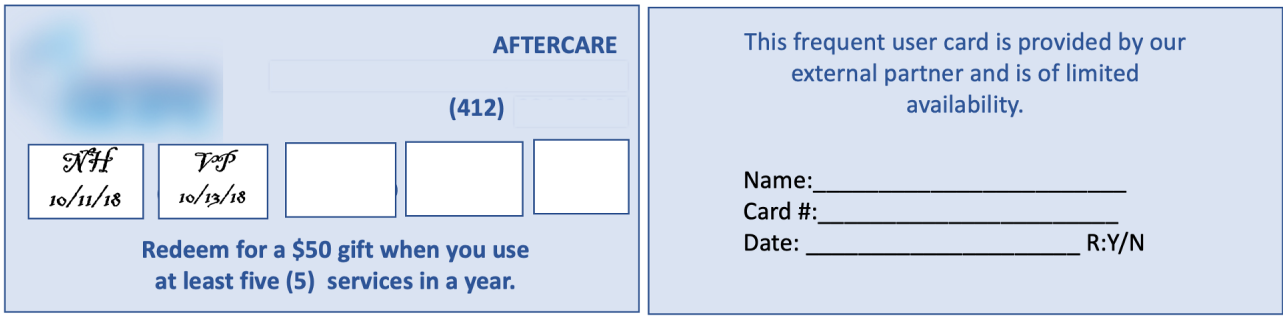


Figure B.2: Card for the Easy Treatment group

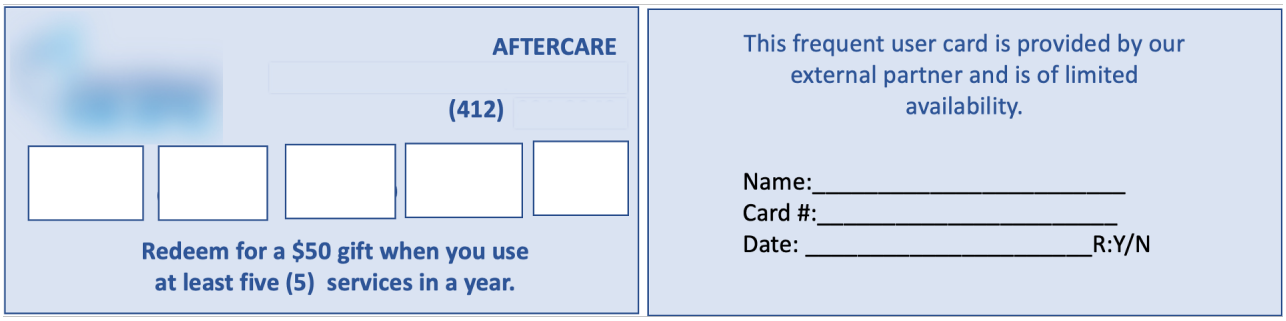


Figure B.3: Card for the Hard Treatment group

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?