

Can Recidivism Be Prevented From Behind Bars? Evidence From a Behavioral Program*

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Abstract

Incarcerated offenders are offered a wide range of programs to encourage their reintegration into society. Little is known, however, about the success of such programs. In this paper, I study the effects of a cognitive-behavioral intervention implemented in Canada using novel micro data. To address inmates' self-selection into the program, I exploit the inmates' random assignments to evaluators with varying propensities to recommend the program. I find large and significant reductions in recidivism: within six months upon release, the program reduces recidivism by up to 16 percentage points. Moreover, the program is shown to decrease the number of days of incarceration in the future, saving around \$3,800 per participant in incarceration costs alone. Finally, I explore heterogeneity in treatment effects and find that the group composition and the timing of participation relative to the release date play a role in both the magnitude and persistence of the treatment effects.

Keywords: incarceration, recidivism, cognitive-behavioral

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1 Introduction

Even with substantial investments in rehabilitation assistance, it is estimated that half of the inmates released from prisons in the United States and Canada will eventually reoffend (Durose et al., 2014; Bonta et al., 2003; Department of Justice Canada, 2020). Until recently, many prison-based interventions focused on basic skills development or job training but did not explicitly target the psychological factors involved in the decision to commit crimes. In contrast, cognitive-behavioral therapy (CBT) programs, which are increasingly available in prisons, seek to break the cycle of reoffenses by addressing ways of thinking and behavioral patterns underlying criminal activities. Yet, our understanding of the effectiveness of these treatments on incarcerated offenders—and of the mechanisms that could be driving successful therapy—remains limited.

CBT effectively reduces crime and violence among at-risk juveniles and men, and probationers (Heller et al., 2017; Blattman et al., 2017; Barnes et al., 2017). However, whether these results translate to incarcerated offenders is unclear. Although incarceration may provide a unique opportunity to ensure constant engagement from the participants, it may also prove too late for those who have already carved their paths in crime. This paper is the first to derive causal estimates of the effects of prison-based cognitive-behavioral therapy on inmates’ disciplinary infractions, the likelihood of being granted parole, and recidivism. Any assessment of the effectiveness of a CBT program—administered in prison or elsewhere—must address the issue of voluntary participation. I adopt an *examiner design* and exploit exogenous variations in the propensity to recommend enrollment to identify the causal effect of participation. While I discern no immediate impact on behavior during incarceration, the program significantly reduces the likelihood of reoffending for at least a year upon release, and I find suggestive evidence that it lasts well beyond this horizon.

The program *Parcours*—the French word for *journey*—was gradually launched in all the provincial prisons of Quebec (Canada) in 2007, becoming the province’s only standardized program across all facilities. The program, which encompasses a series of activities, guided discussions and homework spread over several weeks, aims at correcting cognitive biases by targeting inmates who lack accountability for their actions and have supporting views of crime. In addition to being trained extensively, *Parcours* instructors must follow a detailed manual of instructions. Like most CBT programs, *Parcours* operates in small groups of two

to twelve participants.

I construct a unique dataset containing information on every individual admitted to a provincial prison between 2007 and 2019. In addition to details on the crime and sociodemographic characteristics, these data record disciplinary infractions throughout individual sentences, and whether inmates are granted an early release. Quebec’s provincial prisons—where *Parcours* is implemented—host individuals sentenced for two years or less, whereas those with longer sentences are sent to federal facilities. Nevertheless, *all* offenders initially await for trial in a provincial facility, and every single sentence is recorded in the data. This feature allows constructing an exhaustive recidivism measure—regardless of the sentence length—based on a personal offender identifier. The data about program participation are managed within facilities. Each prison was contacted individually, and 11 of the 18 prisons were able to share data on participation.¹

As with most prison-based programs, *Parcours* is voluntary, thus inducing a self-selection bias. In Quebec, inmates are assigned to risk evaluators at the onset of their prison sentence so as to balance the workload between available evaluators. Evaluators, also uniquely identified in the data, assess the inmates’ risks and criminogenic needs in order to elaborate a personalized intervention plan. Hence, I leverage the as-good-as-random assignment of inmates to the evaluators to create an instrumental variable that exogenously affects participation, but does not correlate with inmates’ characteristics. Using this approach, I find that participation in the CBT program has no significant impact on disciplinary infractions and on being granted parole. On the other hand, it does result in significant reductions in recidivism in the short term: within six months after release, I estimate the likelihood of recidivism declines by 16 percentage points. When considering follow-up periods longer than a year, point estimates seem to get closer to zero but they are less precisely estimated, suggesting the effects may partly dissipate over time. These results are robust to several specifications and alternative definitions of the instrument.

The primary threat to identification in this setting is through the exclusion restriction—the possibility that the evaluators could affect inmates beyond their recommendation to join the program. Inmates and their assigned evaluators have little interaction beyond an initial assessment. For instance, evaluators do not collaborate with or counsel inmates throughout

¹The largest prisons are included in the sample. The 11 prisons cover around 70% of all prison admissions over the sample period.

their sentences; thus, interaction after the early stages of incarceration is limited. Once the recommendation is formulated, the case is assigned to a case manager, who then closely monitors the inmate’s activities for the remainder of the incarceration period. Because the program operates in groups and requires trained counselors, *Parcours* is not always available. I show that the evaluators’ propensities to recommend participation are unrelated to any of the inmates’ outcomes when the program is unavailable, providing strong support for the exclusion restriction.

I next look at other relevant outcomes. Consistent with the main results, I find that participants commit a smaller number of offenses in the short-term and spend less time in incarceration upon release. The program causes participants to spend 18 fewer days in prison over a year. Given the high cost of incarceration in Quebec—around \$215 per day per inmate in 2019²—the program saves around \$3,800 in incarceration costs alone, not accounting for the social costs of crimes. I explore individual heterogeneity in the treatment effects using various techniques, including causal random forests (Athey et al., 2019) and marginal treatment effects (Heckman et al., 2006). I find evidence that drug and first-time offenders are remarkably responsive to the therapy. Moreover, I find these characteristics are overrepresented in the *complying* group—inmates who follow recommendations. Hence, targeting drug and first-time offenders may yield the most productive effects.

In the final section of the paper, I investigate how the group composition and the timing of the program affect the persistence and the size of the treatment effects. Because *Parcours* only operates one group at a time at a given facility, inmates have no latitude over who else participates and when the program begins relative to their release date. I can show that a participant’s own characteristics do not correlate with other participants’ characteristics, and that the timing of the program is also as good as random. From many personal discussions with *Parcours* instructors, I learned that the main challenge they experience is disorderly behavior by some participants during the sessions. In theory, by concentrating high-risk inmates in the same group, the program could yield adverse or null effects through criminogenic peers. In line with this, I obtain the most prominent effects when the group comprises first-time and older offenders. Lastly, I test whether the timing of participation matters, as has long been suggested in the criminology literature (Zamble and Porporino,

²Based on the number from Statistics Canada (2022a), the average inmate cost in Quebec is \$290 (CAD). I use an exchange rate of 1 CAD = 0.75 USD. At the federal level, the average cost is \$318.

1990; Wexler et al., 1990). I find suggestive evidence that participation closer to the release date deters recidivism more than when participation occurs at the early stages of incarceration. These results provide prison counselors and program administrators with low-cost ways to optimize their programs to encourage longer-lasting rehabilitation.

My paper contributes to three major strands of the literature on this subject. Firstly, this paper is one of the first to credibly identify the effects of a prison-based behavioral program on recidivism amongst a population of incarcerated adult men with most of the literature focusing on other populations, namely at-risk youths and probationers. However, understanding the effectiveness of CBT on incarcerated offenders has unique implications for incarceration costs and prison overcrowding given the severe nature of their crimes, and likelihood of being reincarcerated. Heller et al. (2017) study three behavioral interventions³ targeted at juvenile offenders and at-risk youths in Chicago. They find significant behavioral responses: participants were less likely to be subsequently arrested and less likely to be readmitted in juvenile detention.

The cognitive-behavioral program *Thinking for a Change* (Bush et al., 1997) is well implemented in jails and prisons in the United States. The program, similar to *Parcours* in its approach and length, focuses on fostering interpersonal problem-solving skills (Golden et al., 2006). However, it has yet to be studied in a prison setting. Lowenkamp et al. (2009) evaluate the effect of *Thinking for a Change* in the context of Indiana with a population of probationers—individuals serving within the community—and find a decrease in recidivism.⁴ Looking at the results from an RCT in Pennsylvania, Barnes et al. (2017) find that *Thinking for a Change* decreases the likelihood of violent reoffenses among probationers. In Liberia, Blattman et al. (2017) randomly assigned about 1,000 criminally-engaged men to three treatment arms: cognitive-behavioral therapy, a cash transfer, or both. They find large decreases in antisocial behavior (e.g., violence, arrests, street fights, etc.) for individuals receiving both CBT and the cash transfer. The effects were even perceived ten years after the treatment (Blattman et al., 2022). My paper adds to this body of evidence by showing that CBT interventions are effective with adult offenders when used in a correctional setting

³The studied interventions included a bundle of activities, such as positive behavior rewards and introspective reflection. The authors argue that the primary mechanism through which future crimes were deterred was automaticity; when confronted with belligerent situations, participants were made able to reflect on their actions before committing them.

⁴More specifically, the authors compare participants who were referred to the program to probationers who were not. Referrals based on potential gains could partly explain the results.

in that it reduces the likelihood of recidivism and costly incarceration.

Secondly, a number of recent studies (Bhuller et al., 2020; Hjalmarsson and Lindquist, 2020; Mastrobuoni and Terlizzese, 2019; Norris et al., 2022; Rose and Shem-Tov, 2021) find positive effects of incarceration on rehabilitation and health outcomes. These effects are mostly found in facilities with a heavy focus on rehabilitation instead of punishment (Lotti, 2022; Loeffler and Nagin, 2022). Many point to prison-based interventions as an essential factor in determining rehabilitation success. Lee (2019) studies the impacts of an array of prison-based interventions—including cognitive treatments—by exploiting constraints in the availability of such programs in Iowa. Surprisingly, he finds that no program consistently reduces recidivism. Arbour et al. (2021), in the same setting as the present paper, find that participation in programming reduces the likelihood of reoffending for those whose risk and needs are thoroughly evaluated at the sentence’s onset, hinting that targeting is an essential part of programs’ success. The present paper shows that therapy behind bars may explain, in part, the beneficial effects of rehabilitative prisons, and my results can help understand the heterogeneity in the literature. My analysis suggests that CBT is most helpful when groups are composed of first-time offenders and older inmates, a result that echoes a large literature of peer effects in prisons (Bayer et al., 2009; Stevenson, 2017; Damm and Gorinas, 2020). The timing of the program could also matter: programs delivered right at the onset may result in improved behavior for the whole sentence duration. However, newly-acquired skills could depreciate until the release and leave recidivism outcomes unchanged (Papp et al., 2021). My findings suggest that participation in the later stages of incarceration results in more persistent effects upon release.

Thirdly, from an empirical perspective, my research contributes to the literature that exploits *judge fixed effects designs*. I adapt the canonical design introduced by Kling (2006) that has since been used to evaluate several policies related to crime (e.g., Arteaga, 2020; Dobbie et al., 2018; Mueller-Smith, 2015; Bhuller et al., 2020). More precisely, I consider risk evaluators as the *judges* and I carry out placebo checks that take advantage of the setting’s unique features to validate the design. In addition, this paper emphasizes the critical role of risk assessors in prison settings and, more generally, in criminal justice systems.

The content of the paper is structured as follows. In Sections 2 and 3, I describe the program *Parcours*, the data and the random assignment between inmates and evaluators. I

explain the identification strategy and test for the identifying assumptions. In Section 4, I report and interpret the results. Finally, I briefly summarize the findings, propose avenues for further research and consider policy implications in Section 5.

2 Context and Data

In this section, I briefly introduce the criminal justice system in the province of Quebec. I then describe the *Parcours* program with an emphasis on its structure. I explain how the inmates are assigned to risk evaluators as this will be a key to understand the instrumental variable design. Finally, I present the data and some descriptive statistics.

2.1 Incarceration in Quebec

In Canada, any offender sentenced to less than two years of prison serve their sentence in a provincial facility. Alternatively, when the sentence exceeds two years, the sentence is served in a federal facility. In the province of Quebec, the 18 provincial prisons also accommodate incarcerated individuals awaiting their sentence regardless of the crime’s severity. Inmates are typically incarcerated at the prison nearest to their primary residence, but can nevertheless be transferred several times during the course of their sentence to adjust for the inflow of new prisoners and defendants. At the court, judges can also punish offenders with sentences to be served within the community. Offenders, in this case, have to abide by a number of conditions or fulfill community services. Community sentences are by far the most common form of punishment, with only 33% of sentences resulting in incarceration ([Statistics Canada, 2022b](#)). All provincial prisons offer a comprehensive set of programs and therapy to inmates, and some facilities further provide specialized programs for the risks and needs of their prisoners.⁵

The Quebec’s Ministry of Public Security oversees the Act Respecting the Quebec Correctional System (LSCQ) within all provincial facilities: it is the Ministry’s responsibility to help offenders to transition in becoming law-abiding citizens and help facilitate their reintegration into the community. To do so, the Ministry has implemented a number of programs

⁵For instance, the Percé detention center detains only sexual offenders, while the St-Jerome and the Amos prisons have reserved sections for offenders from Indigenous populations. Female offenders, for their part, are detained in either the Quebec or the Leclerc (Laval) prisons. All male offenders, excluding those with Indigenous backgrounds, are incarcerated in one of the other prisons available in the province.

accessible to inmates while serving their sentences. Such programs range from anger management to programs aimed at developing job skills. For instance, inmates can work in the kitchen and laundry facilities. Article 21 from the LSCQ reads:

“ The Minister shall develop and offer programs and services to encourage offenders to develop an awareness of the consequences of their behaviour and initiate a personal process focusing on developing their sense of responsibility. The programs and services offered shall make special allowance for the specific needs of women and Native persons. ”

In 2000, the Ministry commissioned an extensive independent examination of the entire provincial criminal justice system. After a collection of testimonies from frontline workers, a substantial report was published with specific recommendations ([Corbo, 2001](#)). Among a number of recommendations, it is suggested that the LSCQ be amended to include a mandatory psychological evaluation of every offender under the provincial government’s responsibility. Such psychological evaluations were recommended with the purpose of assessing the risks and needs of offenders in order to offer tailored programs. It is advocated that new programs, focused on high-needs individuals and the most severe cases, be developed. In other words, it was suggested that the Ministry develops programs designed explicitly for offenders who lack awareness of their crimes’ consequences, given that the likelihood of reintegration for such offenders is most uncertain.

In Quebec, all inmates serving for more than six months are automatically eligible for parole starting at the third of their sentence, although no release will occur prior to clearance by the governing parole board. Indeed, the parole board interviews the inmate, analyze the police reports and program participation records to determine whether parole should be granted, and if so, the conditions by which the paroled inmate must abide. Inmates who are denied of parole or who opt out from the process are released at the two-thirds of their sentence. The specific features of parole in Quebec are summarized at great lengths in [Arbour and Marchand \(2022\)](#).

2.2 *Parcours*: a Program for Short Sentences

Following the publication of the report, *Parcours*, a program precisely designed for risky offenders serving for less than two years, was developed and implemented in most facilities across Quebec (Lafortune and Blanchard, 2010). The following paragraphs highlight three important aspects of the program: its content and objectives, the targeted participants, and the formation of counselors.

Content and objectives. *Parcours* consists of a series of activities, homework and semi-guided discussions between a group of two to twelve participants. There are two versions of the program: one for incarcerated individuals and one for individuals serving their sentence in the community. For incarcerated offenders, the full curriculum plans for 24 hours of intervention, in addition to the extra homework between the sessions and the interviews before and after the program. The full program is generally completed in three months, and is available both in French and English. *Parcours* is divided into three modules of four sessions, each lasting two hours. The entire curriculum spans several weeks. In Module 1, *Time to Make Changes*, the participant is shown how changes arise from personal decisions and how to balance the pros and the cons of criminal activities. In Module 2, *A Matter of Values*, the counselor addresses how the inmate’s beliefs could cloud his judgment, including how cognitive distortions alter rational decision-making. Finally, in the third module, *Avoiding Pitfalls*, the participant reflects on the motivations behind his criminal behavior in an attempt to avoid repeating the same or similar judgment calls after release.

Each session is organized in accordance to detailed counselor guides⁶ that outline the activities of each session. The instructors’ guides further explain the underlying theory behind each module. The sessions begin with a review of the last meeting’s content and a follow-up on the last homework. Participants will usually take turn in explaining their responses, and the instructor draws parallels between similar answers. The rest of the session consists of a series of questions and semi-guided discussions between the instructor and the participants—and between the participants themselves—around a specific topic. Participants are invited to reflect on their own behavior and on the consequences of their actions. For instance, in the session *A Good Life With or Without Crime* in Module 1, the counselor inquires about the importance of crime in the participants’ lives, underlining that some participants will be

⁶Each module is covered by one user guide that contains around 150 pages.

career criminals, while others are simply going through a particularly difficult time. Participants are then asked to identify the benefits and costs of the most recent offenses they committed. The counselor's guide would include a proposed plan for this activity with the main idea to convey during the hour as well as questions and exercises. For instance, the following would be included in the guide regarding this activity:

The symbol ✓ indicates a message to be transmitted to the participants
The symbol ? indicates a question to be asked to the participants
The symbol ✎ indicates an exercise or a homework
Passages *in italics* indicate talking points that should follow the guide closely

✓ *Let's look at the kind of benefits one can get from crime.*

✎ In the participants' booklet, go to Advantages and disadvantages from the last offense.

✎ *During your last offense (or last three offenses), which advantages were you seeking to obtain?*

Examples: pleasant lifestyle, respect, power, relationship with peers, excitement from "beating" the system, drugs, etc.

✎ *Which disadvantages were associated with your last offense?*

Examples: incarceration, losing friends, not being able to see your family, etc.

✎ Ask the participants to fill out columns (1) and (3) [in the participants' booklet] and review the responses.

✓ *The advantages you have identified are hurdles to bringing changes in your life. You will have to overcome them in order to desist from crime.*

? *Can someone volunteer to name one advantage? How many of you identified this advantage as well?*

Adapted from the instructor's guide for Module 1, and reproduced with the author's authorization.

Each session concludes with an explanation of the homework that must be completed prior to the next meeting. The counselor completes a report for each participant after each of the three modules. The report includes the number of completed sessions, an appreciation of his involvement in the discussions, and an assessment of his progress and growth. The participant is provided with a certificate at the program's completion and with copies of the

reports.

Targeted participants. There are very few restrictions to participation. First, the participants are required to know how to write and read in order to complete the homework. Second, there is no restriction regarding the type of crime that was committed, nor about its severity. Third, and most importantly, convicts are recommended to participate in *Parcours* based on their lack of awareness of consequences and absence of accountability. [Lafortune and Blanchard \(2010\)](#) propose several criteria to identify such a candidate. The participant, for instance, valorizes or favors criminal activities to satisfy his needs, believes in criminal values, or considers himself as a victim. Targeted participants are deemed to have a low level of receptivity to treatment.

Formation of instructors. *Parcours* instructors are prison counselors, and usually have completed an undergraduate degree in either psychology, criminology or social work. In addition, they receive training in order to be certified as *Parcours* instructors. They are then provided with all the material to sufficiently implement *Parcours*, including, as mentioned, an extensive manual which accompanies each module, as well as detailed descriptions of the activities and the participants' booklets.

2.3 Assignment Between an Inmate and an Evaluator

A risk evaluation is conducted quickly after an official sentence is pronounced. The evaluation is conducted with the widely used Level of Service/Case Management Inventory (LS/CMI, [Andrews et al., 2000](#)). The evaluation is conducted quickly after the inmate's arrival at the facility because incarcerated offenders can benefit from various community release measures after the first sixth of their sentence is completed, and again after they are one third completed. For instance, they can request temporary absences for the purpose of participating in a spiritual activity, be involved in an activity to encourage their social reintegration (work, school, etc.) or request to leave the premises of the detention facility for personal, familial reasons (e.g., to attend a funeral). Additionally, offenders serving a prison sentence of six months or more may benefit, between the sixth and the third of their sentence, from a temporary absence in preparation for parole, and for parole itself from the third of their sentence. Hence, the evaluation must be submitted before the authorities can grant these privileges. The law requires that the LS/CMI evaluation be completed either seven days before the

sixth of the sentence or 45 days following the confirmation of the sentence, whichever comes first.

The evaluation contains 43 questions about the criminal history of the inmate, and other criminogenic needs and risks, such as substance abuse, problematic relationships with peers and mental health disorders. Generally, an LS/CMI evaluation is completed at each sentence, however, if the evaluator estimates that a previous evaluation is still valid (conditional on it being completed less than two years prior), then no new evaluation is deemed necessary. The evaluation takes between two and three hours to complete. It includes an interview with the inmate, and an examination of prison and health records.

At the beginning of the sentence, the inmate is matched with an evaluator in order to balance the workload across all evaluators at the given place of incarceration. Evaluators are prison counselors who are employed by the prison and do not move across facilities. Because the evaluation must be completed swiftly at the sentence's onset, prison managers have little to no flexibility over who will be in charge of conducting the evaluation. Importantly, all evaluators have received training prior to using the LS/CMI, and are qualified to be broad-based in the types of inmates they evaluate. Following a series of discussions with evaluators and authorities at the Ministry of Public Security, a common theme emerged: while evaluators are mostly impartial in their duties, some severe cases are usually handled by the most experienced evaluators. This is not a primary concern since the data contain the type of crime committed by the inmate and *all* the evaluations completed since the implementation of the LS/CMI in 2007.⁷

Following the assessment, the evaluator produces a customized intervention plan for the inmate. The plan acknowledges the inmate's criminogenic needs and further includes recommendations to participate in programs. If the evaluator believes the offender meets the criteria, notably a low level of responsiveness to treatment, they can recommend participation in *Parcours*. After the plan is completed, the case is transferred to another agent, who will act as the case manager and be responsible of tracking the inmate's progress during his sentence. Following the evaluator's recommendation, the assigned case manager introduces the program although inmates can also formulate the desire to participate in the program

⁷This allows to construct a proxy for the evaluator's experience, namely the total number of evaluations completed before the current case. My instrumental variable design will account for this, however, the results are identical when I do not proxy for the evaluators' experience. Although I do not precisely know the cases that were not randomly assigned, this suggests the number of such cases is relatively small.

even if it is not noted in their intervention plan. The case manager reviews the content of *Parcours* with the candidate, and answers their questions, if any. If the inmate wants to go through with participation, they sign a consent form and are provided with the schedule of meetings. Therefore, the evaluator, who was initially tasked with devising a plan, does not follow-up on the inmate's progress, nor do they interact beyond the early stages of incarceration. Moreover, to avoid conflicts of interest, the evaluator cannot recommend participation in *Parcours* if they are set to be the *Parcours* instructor.

2.4 Data and Descriptive Statistics

Three datasets are necessary to carry out my analysis on the effects of the program on inmates' behavior and recidivism. First, the Ministry of Public Security provided me with the DACOR (Administrative Correctional Files) dataset; an extensive computerized management system of the provincial prisons. The system contains information about any individual who receives a sentence or awaits a trial in the province of Quebec: the sociodemographic characteristics, the types of crime committed and the details of the sentence. It also precisely informs about the incarceration process: the dates of arrival and departure from prison and the transfers, if any. Each individual receives a unique anonymous identifier, which allows me to track them over time. These data run from 2007 to 2019.

From DACOR, I can determine if any individual has any prior convictions and for which crimes he was sentenced. For each sentence, I record the most serious offense and categorize it in one of four possible categories: assault, theft & property, drugs, and *other*. From this dataset, I also create the dependent variables of interest: the number of disciplinary infractions, whether the inmate is granted parole, and recidivism within a defined time window. Although I do not observe the date of the crime, the data allow to calculate the precise time elapsed between the end of a sentence and the date of the new sentence. From this, I create dummy variables for recidivism. Recidivism, in this study, is thus defined as a reoffense that can lead to reincarceration, but not necessarily, as the new sentence could be served in the community. Breaches to parole conditions are not considered as reoffenses as they are linked with the previous crime. Disciplinary infractions are also recorded with the date of the infraction and its nature. Because infractions could happen before the initial evaluation, I start counting the number of infractions at the sixth of the sentence, that is,

after the evaluation.

Second, I was granted access to all the risk evaluations from 2007 to 2017. Each evaluation is labeled with the same anonymous identifier from the DACOR dictionary, providing sufficient information to merge the two datasets to form a panel. Finally, each observation is marked with the anonymous identifier of the evaluator who was responsible for the evaluation. Since the tool was implemented in 2007, I can observe all evaluators' entire experience with the LS/CMI.

Third, I collected the *Parcours* participation data, which are managed at the facility-level. In total, 11 prisons (out of 18) were able to provide me with the participation data. The authorities from the Ministry anonymized the data which I was able to merge with the panel dataset from DACOR. On Figure 1, I provide a map of the province of Quebec and its detention centers. The area of each circle corresponds to the capacity of each facility. Blue circles represent the facilities where I was provided with the *Parcours* participation data. The largest prisons, including the Montreal, Quebec, St-Jerome and Sherbrooke prisons, are included in the final dataset. From the *Parcours* data and the LS/CMI evaluators' identifiers, I can back out the participation rate per evaluator, which, as I will detail later, is the instrumental variable to predict one's participation. In the final sample, I observe 399 unique evaluators, and the average evaluator has completed 15 evaluations.

A breakdown of the participation data is provided in Table 1. In total, I was able to match 952 participants to their correctional records. All 11 prisons that shared their data are male-only prisons: therefore, only male convicts appear in the final sample. *Parcours* is not offered on a continuous basis. For the group version of the program to be offered⁸, a few participants (consisting of around five or six in most facilities) need to show interest for the program. In this paper, a non-participant is thus an inmate who was evaluated by the LS/CMI, and who stayed in prison for at least 30 days while the program was offered in his facility.⁹ Additionally, to be considered as a non-participant, an inmate must not have been transferred during his sentence, thus precluding the possibility of participation in the program in a facility where I do not have the participation data.

⁸The creators of the program designed two formats; an individual and a group version covering essentially the same material. In this paper, I only consider participants who engaged in the group version. The individual format is mostly reserved for offenders who serve their sentence within the community.

⁹The final sample is virtually the same whether I increase the time restriction to 45 or 60 days. Recall that the maximal time spent in prison is 24 months, although all convicts are released at the two thirds of their sentence.

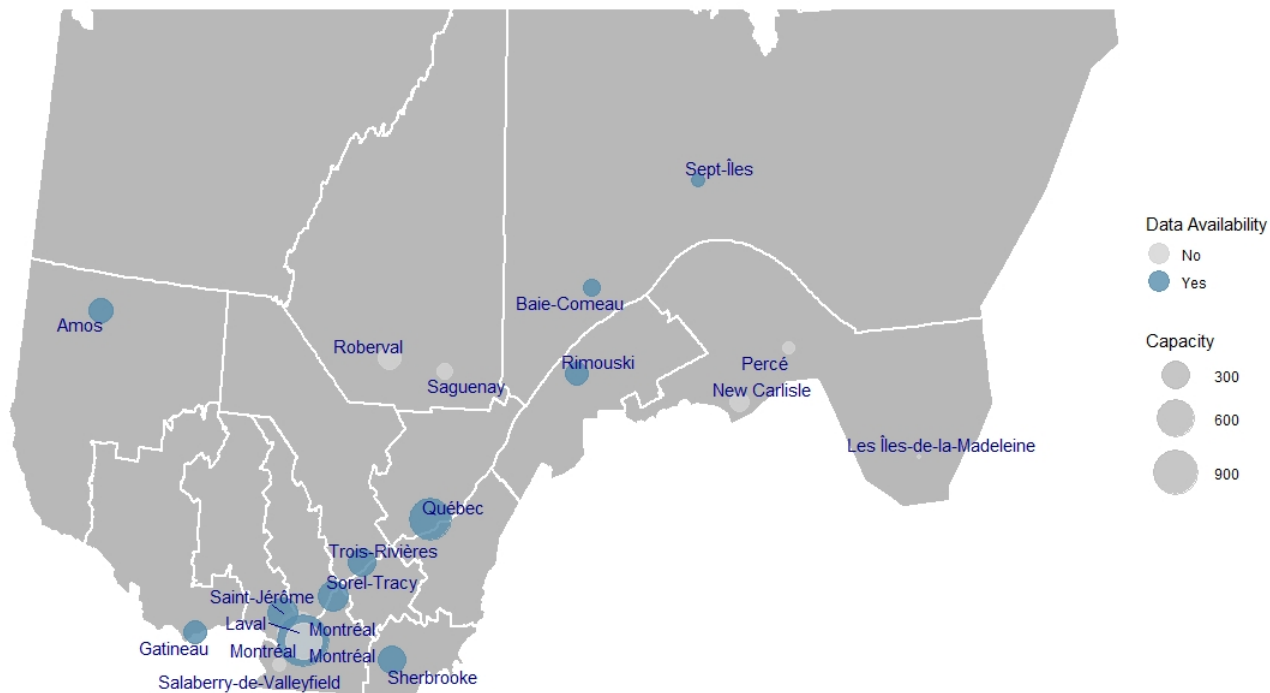


Figure 1: Detention Facilities in the Province of Quebec

Notes. This figure shows a map of the province of Quebec, Canada. Each circle represents a provincial detention center. The area of each circle is proportional to the capacity of the prison. I was granted the access to the *Parcours* participation data for each prison represented by a blue circle. The largest prisons are included in the final dataset and only male prisoners are included in the sample.

Table 1: Breakdown of the Participation Data by Facility

Facility	Number of Participants	Years Covered
Amos	28	2012-2014
Baie-Comeau	77	2007-2016
Hull	29	2008-2013
Montreal	145	2009-2016
Quebec	201	2007-2019
Rimouski	18	2013-2014
Sept-Iles	47	2010-2016
Sherbrooke	247	2007-2016
St-Jerome	43	2007-2015
Sorel-Tracy	29	2007-2013
Trois-Rivieres	88	2009-2016
Total	952	

Notes. This table reports the number of *Parcours* participants by facility and the years covered by each available file. A total of 952 participants are included in the sample.

Table 2: Descriptive Statistics—by Treatment Status

	(1) Participants	(2) Non-Participants	(3) <i>p-value: (1) = (2)</i>
Crime: Other	0.181	0.258	0.000***
Crime: Assault	0.155	0.206	0.000***
Crime: Burglary&Theft	0.294	0.228	0.000***
Crime: Drugs	0.370	0.307	0.000***
Age: [18-24]	0.180	0.191	0.428
Age: [25-30]	0.194	0.177	0.201
Age: [31-38]	0.245	0.216	0.053
Age: [39-46]	0.185	0.193	0.567
Age: [47-83]	0.196	0.223	0.067
1st sentence	0.460	0.486	0.149
2nd sentence	0.247	0.208	0.007**
3rd sentence	0.126	0.111	0.178
4th sentence	0.054	0.067	0.123
5+ sentence	0.113	0.129	0.189
Indigenous	0.035	0.009	0.000***
Violent crime	0.155	0.156	0.974
Observations	952	4 895	5 847

Notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) reports the characteristics' means among participants. Column (2) reports the characteristics' means among non-participants. Column (3) shows the p-values for testing the equality of the means.

In Table 2, I report the descriptive statistics of the final sample based on the treatment status. For each characteristic, I run a t-test to assess the difference in the means in both subsamples and I report the associated p-value. On most demographic characteristics, participants prove statistically different from non-participants. For instance, within the group of participants, drug offenders and those who committed burglary or theft are overrepresented. I categorize the inmate's age in one of five equally-sized categories. In contrast to crime, the distribution of age in both groups is statistically the same. The numbering of the sentences records whether the inmate is a first-time offender (1st sentence) or a repeat offender (second sentence or more, top-coded to five). I finally observe whether the inmate is of Indigenous backgrounds and have an indicator for the crime being violent in nature.

The program targets high-risk individuals with low levels of receptivity to treatment, and this selection process translates into worse in-prison behavior among participants. The left panel of Figure 2 shows the difference in the number of disciplinary infractions. Examples of such infractions include possession of a prohibited object, alteration of goods and property,

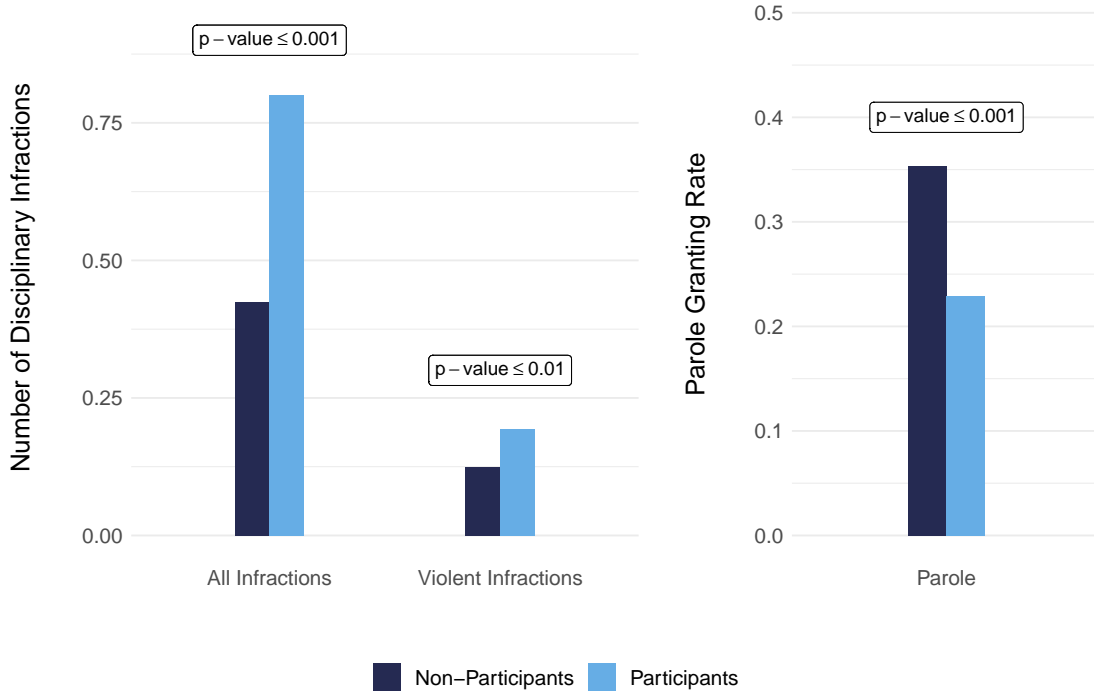


Figure 2: Differences in Disciplinary Infractions and Parole

Notes. The bar plot on the left shows the number of disciplinary infractions, broadly defined, and the subset of violent infractions, committed by non-participants (dark blue) and participants (light blue). The bar plot on the right reports the parole granting rate for non-participants (dark blue) and for participants (light blue). The p-values test for the equality of means across the two groups.

and unwillingness to follow rules. The figure shows that non-participants (dark blue) commit, on average, more violations than participants (light blue), and the difference is significant. When concentrating on the subset of physically-violent disciplinary infractions, the graph shows participants commit more of such infractions, too.¹⁰ Participants are also less likely to be granted parole. Non-participants have a 35% chance of being granted an early release, whereas this probability drops by more than 10 percentage points among participants.¹¹

Despite the apparent selection into the treatment, participants are less likely to reoffend, as shown on Figure 3. I consider recidivism from within six months up to within five years after release. I find that participants have a overall lower recidivism rate than non-participants. Within six months, 20% of non-participants reoffend, whereas this proportion drops to 14% among participants. This difference is also detectable when considering the

¹⁰In Section 4, I assess whether these differences hold true when including covariates. I still find that participants commit more disciplinary infractions, but the coefficient is only marginally significant. The coefficient on the number of violent infractions is close to zero, and not significant.

¹¹When controlling for covariates, I still find a significant difference of 13 percentage points.

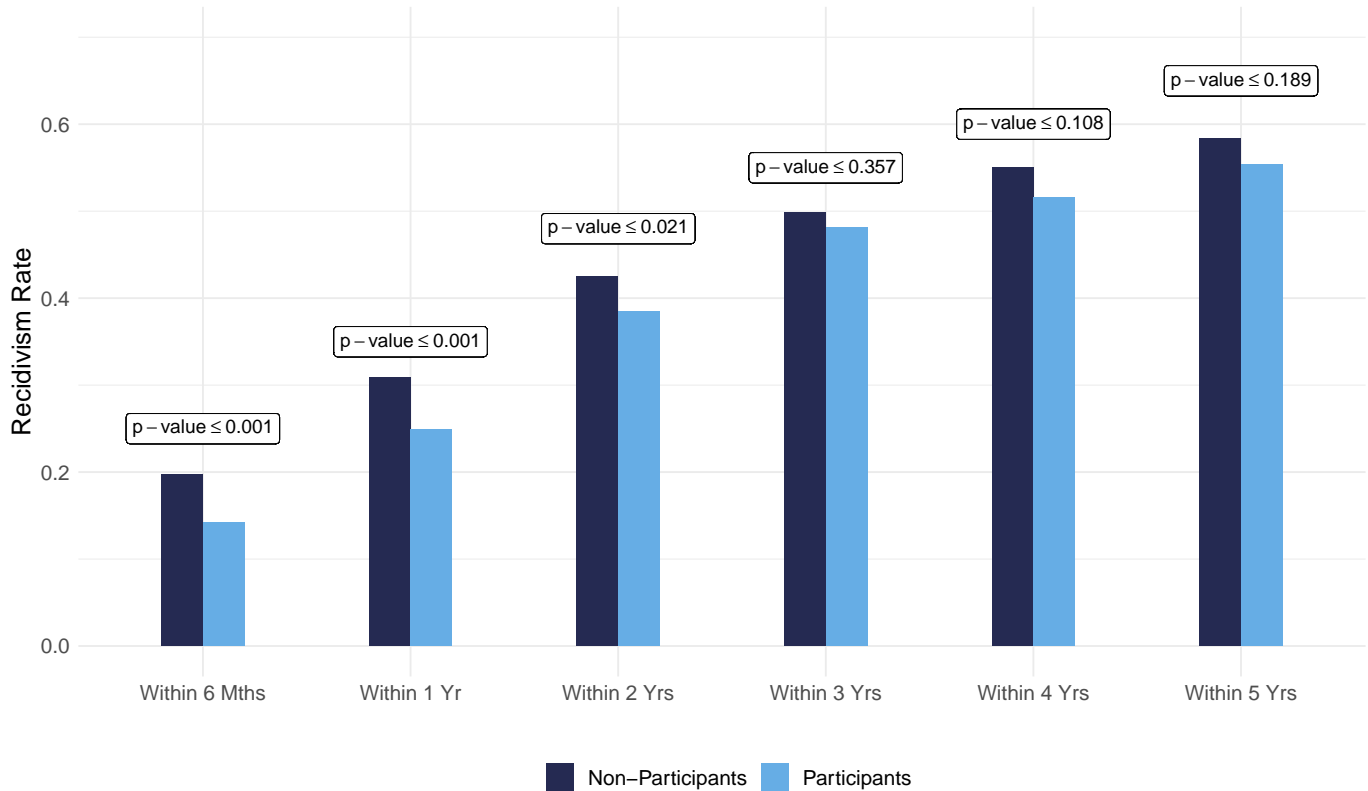


Figure 3: Differences in Recidivism Rates

Notes. The bar plot shows the recidivism rates among non-participants (dark blue) and participants (light blue) within six months upon release up to five years. The p-values test for the equality of means across the two groups.

rate of recidivism within one and two years, but becomes indistinguishable from zero after three years. Because of the apparent negative selection in prison behavior, these differences in recidivism rates can likely be interpreted as the lower bounds of the treatment effects. The degree of the bias remains unclear as it could reflect selection from the evaluators and self-selection from the participants themselves. On the one hand, evaluators are instructed to target inmates with supportive values of crime for *Parcours*. On the other hand, evaluators may choose to recommend participation for those who would benefit from the therapy, and select those with the highest potential for gains. In addition, highly motivated and remorseful inmates with an earnest desire to improve their lives could be naturally more willing to participate in therapy. However, certain inmates may regard *Parcours* as little more than an opportunity to secure an early release. I discuss, in the next section, how I can leverage the random assignment to evaluators to construct an instrumental variable to get a causal effect that is not contaminated by selection biases.

3 Identification Strategy

In this section, I introduce the research design and discuss the proposed instrumental variable strategy. I provide evidence for the validity of the identifying assumptions.

3.1 Research Design

We wish to estimate the treatment effect β in the following regression:

$$y_i = \alpha + \beta T_i + \mathbf{x}_i \lambda + \epsilon_i, \quad (1)$$

where y_i is an outcome for inmate i —either the number of disciplinary infractions, parole or recidivism—and T_i is a participation indicator. \mathbf{x}_i is a vector of individual characteristics. Using ordinary least-squared to estimate Equation (1) is not sufficient to get a causal estimate of β as several unobservable variables—stemming from selection from the evaluators and self-selection from inmates—could be correlated with the treatment. Such variables could include motivation, ability, awareness of consequences, or the desire to get an early release, all potentially resulting in a biased estimation of β . As mentioned before, the direction of the bias is ambiguous as the participants could enroll for various reasons, or be selected based on their potential gains from therapy.

To get a causal estimate of the treatment effect, I exploit the as-good-as-random assignment of inmates to risk evaluators. Consider an inmate i who is assigned to evaluator j . To account for most experienced evaluators being matched with inmates who committed the most severe crimes in some cases, I first regress a participation dummy on an interacted set of prison and year fixed effects, as well as a crime fixed effect ($crime_i$). I further include the number of evaluations completed by evaluator j prior to evaluating inmate i , a proxy for their experience ($neval_{ij}$). More precisely, for an inmate i who matched with evaluator j , I estimate

$$T_{ij} = \delta_0 + \delta_1 year_i \times prison_i + \delta_2 crime_i + \delta_3 neval_{ij} + \epsilon_{ij}, \quad (2)$$

and I denote by $\hat{\epsilon}_{ij}$ the residual from this regression. Let \mathcal{N}_j represent the set of inmates evaluated by evaluator j , and let $\dot{\epsilon}_j$ be the sum of residuals at the evaluator level: $\dot{\epsilon}_j = \sum_{i \in \mathcal{N}_j} \hat{\epsilon}_{ij}$. The instrument for inmate i is given by

$$z_{ij} = \frac{\dot{\epsilon}_j - \hat{\epsilon}_{ij}}{|\mathcal{N}_j| - 1},$$

where $|\mathcal{N}_j|$ is the total number of inmates assessed by evaluator j .

Intuitively, by including controls in Equation (2), z_{ij} is a relative participation rate per evaluator compared to other evaluators in the same prison at the same year with a similar experience, when matched with inmates who committed the same crime. Importantly, I remove inmate i when measuring their instrument to avoid any small sample biases. I test the robustness of the findings in many different ways by altering the way I construct z_{ij} , for instance, by not including any controls in Equation (2). In practice, including the crime fixed effect and the experience proxy to account for institutional details do not affect the results. Alternatively, one could use the evaluators dummies as separate instruments, and I present results using these strategies as well.

To understand how this setting reproduces a quasi-natural experiment, consider inmate i and his counterfactual i' . The only difference between the two is the evaluator they match with: let $z_{ij} > z_{i'k}$. That is, inmate i is matched with an evaluator with a higher relative participation rate than in the counterfactual scenario. Therefore, inmate i is more likely to be pushed into the treatment for exogenous reasons, and one can compare the outcomes of i and i' . This strategy thus estimates the treatment effect for the subpopulation of compliers—inmates whose treatment status are affected by the instrument (Angrist et al., 1996). I estimate various regressions of the form:

$$\begin{aligned} \text{(first stage)} \quad T_{ij} &= \alpha_0 + \alpha_1 z_{ij} + \mathbf{x}_i \boldsymbol{\alpha} + \epsilon_{ij} \\ \text{(second stage)} \quad y_{ij} &= \beta_0 + \beta_1 T_{ij} + \mathbf{x}_i \boldsymbol{\beta} + \eta_{ij}, \end{aligned}$$

where \mathbf{x}_i contains up to all the variables included in Table 2 in addition to year and prison fixed effects, and the experience variable ($neval_{ij}$ above). I also present the results from the *reduced form* regressions, that is, regressions of the outcome on the instrument:

$$\text{(reduced form)} \quad y_{ij} = \delta_0 + \delta_1 z_{ij} + \mathbf{x}_i \boldsymbol{\delta} + v_{ij}.$$

The reduced form regressions are interesting in and of themselves, as they measure the

effects of being matched with evaluators with higher participation rates, and they rely on fewer identifying assumptions. Nevertheless, the usual 2SLS assumptions appear to be met in this setting.

3.2 Testing the Identifying Assumptions

I now test the identifying assumptions that allow for the treatment effect of the CBT program— $\hat{\beta}_1$ in the second stage regression—to be interpreted causally.

Variation in the instrument and first stage. The top panel of Figure B.1 shows the distribution of z_{ij} —the residualized participation rate. The plot displays wide variation in the participation rates, but is the instrument correlated with actual participation? The bottom panel shows a non-parametric version of the first stage equation. The figure shows that going from the evaluator with the lowest participation rate to the highest increases the likelihood of participating by around 50 percentage points. This relationship holds with and without controls, as shown in Table A.1. Column (1) shows a simple regression of T_{ij} on z_{ij} , and I find a strong correlation between the two variables, with a F-statistic of 59. [Olea and Pflueger \(2013\)](#) show that the F-statistic should be larger than 37.42 for a worst case bias of 5%, and larger than 23.11 for a worst case bias of 10%. I obtain even larger F-statistics when I include either the randomization controls (all the variables in Equation (2)) or the full set of controls (adding all the variables from Table 2). Column (4) shows the results of the first stage regression including only the sample of offenders whom I observe in the earlier years of the dataset. For these inmates, I can track recidivism for up to five years and I find a strong first stage coefficient for them as well despite the smaller sample size. Throughout the paper, I follow the literature (e.g., [Agan et al., 2022](#)) and cluster the standard errors at the prisoner and evaluator level. The precision is not affected when using non-clustered, robust standard errors, as I will show in the robustness checks section.

Random assignment. Inmates are assigned with evaluators in a way to balance the workload between available evaluators within a facility. I conducted interviews with prison managers and evaluators. Following a series of discussions, a common theme emerged: whilst some evaluators specialize in certain types of offender behavior—the most serious cases could be assigned to the most experienced evaluators—, they remain relatively impartial in their duties and are trained to evaluate all types of offenders. I formally test for balance

of inmates' characteristics across evaluators. The first column of Table A.2 shows that individual characteristics predict the treatment status, but the second column shows that the same characteristics do not correlate with the participation rate of their evaluator. In other words, conditional on the randomization controls, inmates appear to be randomly matched with evaluators, as the institutional details would suggest.

Exclusion restriction. The exclusion restriction guarantees that the only channel through which the instrument affects the outcome is by the inmate's decision to enroll in the program. This prevents other decisions or behaviors from the evaluator that could be correlated with the decision to get the treatment and the outcomes. This could occur if the evaluator was to track the progress of the offender throughout his sentence and, even, monitor his activities upon release. If this were the case, the evaluator could have other channels of influence that would be captured in their residualized participation rate, z_{ij} . As mentioned previously, once the evaluation is made, the evaluator transfers the case to a case manager without following-up on the case. In addition, the inmate hardly interacts with the evaluator as only between two and three hours are necessary to fill out the questionnaire within days after arriving at the facility. It seems unlikely that the future criminal behavior of the inmate (i.e., recidivism) be determined by such an interaction at the beginning of the sentence or by the overall behavior of the evaluator given they have minimal contact. Another possibility is that the evaluator could recommend other programs in addition to *Parcours*, which would be a violation of the exclusion restriction. I use placebo checks to provide evidence that there is no such violation.

Parcours operates on a group basis and instructors are required to go through specialized training. Therefore, the program is not always available. A reason may be that there are too few volunteers to form a group, or that there are no trained instructors at the time and place of incarceration. For inmates incarcerated when the program is not available, the evaluators' participation rates should *not* affect *any* of their outcomes, since they have no choice regarding participation.¹² For the particular set of inmates in the placebo sample, I estimate

¹²To conduct this test, I create the instrument with the main sample—when the program is available—but predict the residual $\hat{\epsilon}_{ij}$ for individuals in the placebo sample. I then calculate $\hat{\epsilon}_j$ using these residuals, and divide by the total number of cases by evaluator j . An inmate is considered in the placebo sample if he was sentenced while the program was not available during the timeframe for which I have the participation data at the given prison. For instance, in the Montreal Detention Center, the participation data is available from 2009 to 2016, however, the program could have been offered before 2009 and after 2016, and thus, the sample must be restricted to this window. An inmate incarcerated in this prison will be in the placebo sample if he

reduced form regressions¹³ and, as shown on Table A.3, I find no effect on their immediate behavior outcomes: the instrument does not affect the inmates’ number of disciplinary infractions or their likelihood of being granted parole. As displayed on Table A.4, I find coefficients that are close to zero for the recidivism outcomes, or even positive coefficients, although they have wide confidence intervals. I further show on Table A.5 that individual characteristics do not predict the availability of the program, with an overall F-statistic of 0.40. All considered, these tests provide strong evidence in favor of the exclusion restriction in the setting.

Monotonicity. The monotonicity assumption, or the no-defier hypothesis, verifies that all the inmates are affected in the same direction by the instrument. In my setting, it rules out the situation in which an inmate decides not to participate solely because he receives a recommendation to participate. Similarly, an inmate cannot decide to participate because he was not recommended for the program. The non-parametric first stage regression on the bottom panel of Figure B.1 shows a monotonic relationship between the instrument and the probability to participate. Following the literature (Dobbie et al., 2018; Bhuller et al., 2020; Arteaga, 2020), I also show that the first stage is strong across all the subsamples ensuring the average monotonicity condition across evaluators (Frandsen et al., 2019). Column (1) of Table A.9 shows positive and significant coefficients for all subsamples, except for inmates with Indigenous backgrounds, who represent about 1% of the sample, where the coefficient is positive but imprecise.

4 Results

I first assess differences in outcomes between participants and non-participants. I then use the instrument to derive causal estimates of participation on immediate behavior behind bars and recidivism. I further explore heterogeneity in the treatment effects and present robustness checks.

was sentenced between 2009 and 2016, for at least three months, while no other inmate participated.

¹³Inmates in the placebo sample have no choice regarding participation. Thus, the best way to conduct the placebo test is by regressing their outcomes on the instrument. Mechanically, $T = 0$ for all inmates in the placebo sample.

4.1 Main Results

I start by estimating simple OLS regressions. I first regress the number of disciplinary infractions, and the number of violent infractions, on a treatment indicator and other covariates. Table 3 shows that participants, on average, commit more disciplinary infractions over the course of their sentence, although the effect is not precisely estimated. This result is not surprising given the program targets high-risk offenders with supportive views of crime. However, the number of violent infractions is virtually the same regardless of the treatment status. The negative selection into the treatment converts into a smaller likelihood of being granted parole for participants. In column (3), I use an indicator for being granted parole as the dependent variable. Even when adjusting for the full set of covariates, I estimate a difference of 13 percentage points between participants and non-participants.

Table 3: OLS estimation—Disciplinary Infractions and Parole

<i>Dep. Var. :</i>	(1) Disciplinary Infrac.	(2) Violent Disciplinary Infrac.	(3) Parole
Program	0.104 (0.070) [-0.033,0.241]	-0.004 (0.025) [-0.054,0.045]	-0.132*** (0.018) [-0.168,-0.096]
Average of dep. var.	0.389	0.108	0.319
Full controls	✓	✓	✓
Observations	5847	5847	3362

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) reports an OLS regression of the number of disciplinary infractions on a participation indicator. Column (2) reports an OLS regression of the number of violent disciplinary infractions on a participation indicator. Column (3) reports an OLS regression of an indicator for being granted parole on a participation indicator. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

When looking at the recidivism outcomes, I discern different patterns between participants and non-participants. On Table 4, I consider recidivism within several time windows, and find differences hovering around 5 percentage points in the short-term. However, past the two-year mark, the differences in recidivism rates are close to zero, and not significant at any conventional levels. The observed differences in the short-term could result from the program itself or from confounding factors that are unobserved in the data, such as

motivation, ability or remorse—even if observable characteristics are accounted for.

Table 4: OLS estimation—Recidivism

<i>Recidivism within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Program	-0.061*** (0.015) [-0.090,-0.033]	-0.069*** (0.019) [-0.105,-0.032]	-0.049*** (0.018) [-0.084,-0.014]	-0.029 (0.020) [-0.068,0.010]	-0.021 (0.020) [-0.061,0.019]	-0.010 (0.023) [-0.055,0.036]
Average of dep. var.	0.188	0.299	0.418	0.496	0.544	0.579
Full controls	✓	✓	✓	✓	✓	✓
Observations	5847	5804	5587	4923	4146	3343

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report OLS regressions of recidivism indicators within six months upon release up to five years on a participation indicator. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Table 5 shows the results on the number of disciplinary infractions and the likelihood of being granted parole when using the instrumental variable approach. Panel A shows the reduced form effect—the coefficient from a regression of the dependent variable on the instrument—and I detect a decrease in the number of violent infractions, but no change in the number of disciplinary infractions as a whole and in the likelihood of being granted parole. In the same table, Panel B shows the 2SLS treatment effect: the coefficients are in line with improvements in behavior on all accounts as a result of the program, although the effects are imprecisely estimated or only marginally significant. Importantly, most offenders do not commit infractions while behind bars: in the data, 80% of inmates have zero infractions to their record, while 93% have not committed any violent infractions. To account for the number of infractions consisting of mostly zeros, I scale the variables using the inverse hyperbolic sine transformation. In Table A.6, I reestimate the 2SLS regressions with the scaled variables and find no significant effects on both the number of disciplinary infractions and the number of violent infractions.

Turning to the recidivism outcomes on Table 6, I find strong decreases in recidivism in the short-term. Within one year following the release, I estimate a reduction of 18 percentage points in recidivism, a decrease of 60% from the baseline mean. After two years, the coefficients remain negative and of substantial magnitude, however, the confidence intervals become wider. On the one hand, the sample size becomes smaller when considering longer time windows since some individuals are not observed for that long, which makes it difficult

Table 5: 2SLS estimation—Effect on Disciplinary Infractions and Parole

<i>Dep. Var.</i>	(1) Disciplinary Infrac.	(2) Violent Disciplinary Infrac.	(3) Parole
Panel A: Reduced Form			
Instrument	-0.093 (0.170) [-0.426,0.240]	-0.141** (0.071) [-0.280,-0.003]	0.075 (0.089) [-0.100,0.250]
Panel B: 2SLS			
Program	-0.296 (0.564) [-1.403,0.810]	-0.451* (0.253) [-0.948,0.046]	0.179 (0.233) [-0.277,0.636]
Average of dep. var.	0.389	0.108	0.319
Full controls	✓	✓	✓
Observations	5847	5847	3362

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) reports the reduced form coefficient (panel A) and 2SLS effect (panel B) on the number of disciplinary infractions. Column (2) reports the reduced form coefficient (panel A) and 2SLS effect (panel B) on the number of violent disciplinary infractions. Column (3) reports the reduced form coefficient (panel A) and 2SLS effect (panel B) on the likelihood of being granted parole using only the sample of inmates eligible for parole. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

to detect longer-term effects. On the other hand, the pattern may suggest the effects depreciate over time as the coefficients associated with longer-term effects are smaller than the short-term effects.

Although I cannot exactly distinguish between the two channels, previous research has shown depreciation of CBT effects with time (Blattman et al., 2017). In column (3) of Table 5, the sample is nearly identical to that in column (1), but the coefficient is somehow imprecise. This is despite the sample mean becoming larger when longer time windows are considered. Nonetheless, the point estimates suggest the effects do not depreciate completely, and, perhaps, continuation in care upon release could anchor the skills honed by CBT (Kouyoumdjian et al., 2015). Later in the analysis, I will provide suggestive evidence that delaying the start of the program relative to the release date improves the effects' persistence, indicating that follow-up interventions upon release have the potential to minimize the depreciation of skills.

Table 6: 2SLS estimation—Recidivism

<i>Recidivism within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Panel A: Reduced Form						
Instrument	-0.097** (0.047) [-0.188,-0.005]	-0.117** (0.052) [-0.220,-0.014]	-0.086 (0.058) [-0.199,0.027]	-0.046 (0.062) [-0.168,0.077]	-0.069 (0.068) [-0.201,0.064]	-0.086 (0.071) [-0.227,0.054]
Panel B: 2SLS						
Program	-0.164** (0.079) [-0.319,-0.009]	-0.176** (0.087) [-0.348,-0.005]	-0.151 (0.099) [-0.345,0.043]	-0.075 (0.112) [-0.295,0.145]	-0.105 (0.115) [-0.330,0.119]	-0.111 (0.105) [-0.317,0.095]
Average of dep. var.	0.188	0.299	0.418	0.496	0.544	0.579
Full controls	✓	✓	✓	✓	✓	✓
Observations	5847	5804	5587	4923	4146	3343

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the reduced form coefficients (panel A) and 2SLS effects (panel B) on recidivism from within six months upon release up to five years. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

To put the results in perspective, I follow the recent literature ([Agan et al., 2022](#); [Baron and Gross, 2022](#)) and use the insights from [Abadie \(2003\)](#) and [Dahl et al. \(2014\)](#) to compute the rates of recidivism for the group of *complying non-participants*. This allows for a credible benchmark since the 2SLS results above apply to the specific group of compliers only. I can calculate these means by first estimating the share of compliers in the sample by comparing participation rates across evaluators at both extremes of the distribution.¹⁴ By measuring the recidivism rates among *complying non-participants*, we are able to get an estimate of the recidivism rates for a credible group of counterfactuals.¹⁵ The results from this exercise are shown in Table 7. Column (1) shows the recidivism rate in the whole sample, while columns (2) and (3) show these rates within the group of non-participants and the group of complying non-participants. Column (4) report the 2SLS effects in percentage points; in column (5), I measure the effect relative to the mean of complying non-participants. Consistent with the previous results, the program reduces recidivism by roughly 60% in the short-term, with the

¹⁴Consider the first stage equation: $T_{ij} = \alpha_0 + \alpha_1 z_{ij} + \mathbf{x}_i \boldsymbol{\alpha} + \epsilon_{ij}$ and let z_p be the p -th percentile of z_{ij} . The share of compliers is given by $s_c = \hat{\alpha}_1 \times (z_{99} - z_1)$ and the share of always-takers is $s_a = \hat{\alpha}_0 + \hat{\alpha}_1 \times z_1$. Finally, let the share of never-takers be $s_n = 1 - s_c - s_a$. The rate of recidivism among complying non-participants is given by $\frac{s_c + s_n}{s_c} P(y_{ij} = 1 | T_{ij} = 0, z_{ij} = z_1) - \frac{s_n}{s_c} P(y_{ij} = 1 | T_{ij} = 0, z_{ij} = z_{99})$. See [Agan et al. \(2022\)](#) for the detailed derivation.

¹⁵I will later characterize the compliers.

Table 7: Comparison with Control and Compliers Mean

<i>Recidivism within...</i>	(1) $P(y = 1)$	(2) $P(y = 1 T = 0)$	(3) $P(y = 1 \text{Complier}, T = 0)$	(4) Effect (p.p.)	(5) Effect (%)
6 Months	0.188	0.197	0.250	-0.164	65.600
1 Year	0.299	0.309	0.298	-0.176	59.043
2 Years	0.418	0.425	0.500	-0.151	30.200
3 Years	0.496	0.499	0.713	-0.075	10.517
4 Years	0.544	0.550	0.625	-0.105	16.800
5 Years	0.579	0.584	0.790	-0.111	14.053

Notes. Columns (1) and (2) show the recidivism rates in the entire sample and in the group of non-participants, respectively. Column (3) shows the recidivism rates among the group of complying non-participants, using the method described in the main text and controlling for the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator. Column (4) reports the main effects found in Table 6, and column (5) shows the effect size in percentages by dividing column (4) by column (3).

effects becoming more modest when considering longer time windows.

It is finally worth addressing the substantial gaps between the OLS and the 2SLS results. The OLS regressions delivered small differences in recidivism rates—between 3 and 6 percentage points. One explanation for this gap can be attributed to the apparent negative selection bias, with participants committing, for instance, more disciplinary infractions while the program is still ongoing. This implies that the OLS results can be seen as lower bounds (in absolute terms) of the true treatment effects, and can be interpreted as such. Still, it can be argued that the 2SLS regressions are of particular importance to identify the results from rehabilitation efforts as one could, in more extreme cases, find only null effects. A further possible reason to explain the gaps can be ascribed to different parameters being identified by both strategies. I found that the recidivism rates of complying non-participants are higher than that from the whole sample, hinting that compliers in this setting may be more prone to recidivism. I will tackle this question in Section 4.2.

I next turn my attention to further outcomes of interest, specifically the number of re-offenses and the time spent incarcerated in the future. On Table A.7, I use the number of reoffenses within specified time frames as the outcome, and find similar results to what I obtained when using indicators for recidivism instead. The effects also translate into fewer days of incarceration in the future. I estimate that participating in the CBT program decreases by roughly 18 days the number of days spent incarcerated within one year after release as shown on Table 8. While this number may appear small in magnitude, it is important to

note that sentences requiring incarceration are quite seldom in Quebec. Community sentences are usually prioritized and represent 67% of all sentences, whereas 33% of sentenced individuals are incarcerated ([Statistics Canada, 2022b](#)).

Incarceration in Quebec is costly with an average inmate cost of \$215 per day in 2019 ([Statistics Canada, 2022a](#)). Thus, a simple back-of-the-envelope calculation indicates that a program like *Parcours* generates savings of about \$3,870 ($18 \text{ days} \times \$215/\text{day}$) per inmate in incarceration costs alone. This calculation certainly omits other significant costs such as the social costs of the crimes themselves and costs associated with victimization, and thus can be interpreted as a conservative bound of the treatment’s benefits. The program is not free either: a qualified CBT instructor must be hired by the prison. If each prison hires such an instructor in a way of providing CBT on a continuous basis, say by offering the program every three months, then about 48 inmates could participate every year by assuming an average group size of 12 participants. This yields savings of about \$185,760 in incarceration costs ($48 \text{ inmates} \times \$3,870 \text{ per inmate}$). However, if 96 hours are required to operate the program once (to prepare, deliver the program, writing reports and conduct pre- and post-interviews) then 384 hours per year are allocated to administering the CBT program. Assuming a conservative hourly wage of \$40 per hour, the program costs \$15,360 per year in human resources, far behind the savings due to the decrease in incarceration. In addition to implications for incarcerations costs, these results are meaningful when thinking about prison overcrowding, as most of Quebec prisons have an occupancy rate near 100% and often higher ([Ministère de la Sécurité publique, 2016](#)).

On Panel B of Table 8, I explore the extensive margin of incarceration time by considering individuals who *do* reoffend. For this subsample, I do not detect any significant effect on incarceration time. I focus even more on this sample in Table A.8 by investigating if recidivists display changes in behavior after receiving CBT. In column (1), I estimate that repeat offenders delay their next offense by about 192 days, although this measure is relatively noisy. Columns (2) to (5) use as dependant variables dummies for the next crime being committed—none of these coefficients are significant. Together, these findings are coherent with some participants being entirely discouraged from committing further crimes after their treatment. However, inmates who reoffend display a similar pattern of behavior regardless of participation in CBT.

Table 8: 2SLS—Number of Days Incarcerated

<i>Days incarcerated within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Panel A: Intensive and extensive margins						
Program	-16.837* (9.543) [-35.540,1.867]	-18.204* (9.368) [-36.566,0.157]	-13.408 (9.341) [-31.716,4.901]	-12.122 (8.688) [-29.150,4.905]	-2.279 (7.694) [-17.359,12.802]	-6.652 (7.272) [-20.906,7.601]
Average of dep. var.	11.388	10.118	7.806	5.436	3.992	3.058
Observations	5847	5810	5599	4937	4170	3374
Full controls	✓	✓	✓	✓	✓	✓
Panel B: Extensive margin only						
Program	51.615 (61.560) [-69.040,172.271]	20.129 (37.249) [-52.878,93.136]	16.145 (23.441) [-29.798,62.089]	-1.861 (19.527) [-40.132,36.411]	14.161 (17.464) [-20.068,48.390]	1.128 (12.425) [-23.225,25.480]
Average of dep. var.	59.306	61.134	65.401	67.235	72.673	80.699
Observations	1098	1737	2338	2441	2257	1935
Full controls	✓	✓	✓	✓	✓	✓

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the 2SLS effects at the intensive and extensive margins (panel A) and extensive margin only (panel B) on the number of days of incarceration from within six months upon release up to five years. Panel B focuses on the subset of inmates who recidivate during the given timeframe. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

4.2 Who Are the Compliers?

In the instrumental variable regressions previously described, the estimated treatment effects were on the subpopulation of compliers. Although such methods are widespread, they provide limited insights to policy-makers in circumstances where programs are being considered for expansion since the population that reacts to the instrument remains unknown. For instance, suggesting the program to noncompliers might produce unproductive results as their participation decision will not be affected by the instrument.

In this brief section, my aim is to classify the inmates based on their compliance level such that the subgroup for which the treatment effect is estimated can be more easily identified.¹⁶ To do so, I follow [Dobbie et al. \(2018\)](#) and calculate the share of compliers in each subsample by using the method outlined in footnote 14. Using Bayes' rule, for a given characteristic $X_i = x$, we have

$$P(X_i = x | \text{Complier}) = \frac{P(\text{Complier} | X_i = x) \times P(X_i = x)}{P(\text{Complier})},$$

where $P(\text{Complier} | X_i = x)$ is the share of compliers among the subsample with $X_i = x$.

¹⁶I thank Jason Baron for providing the code to run these calculations.

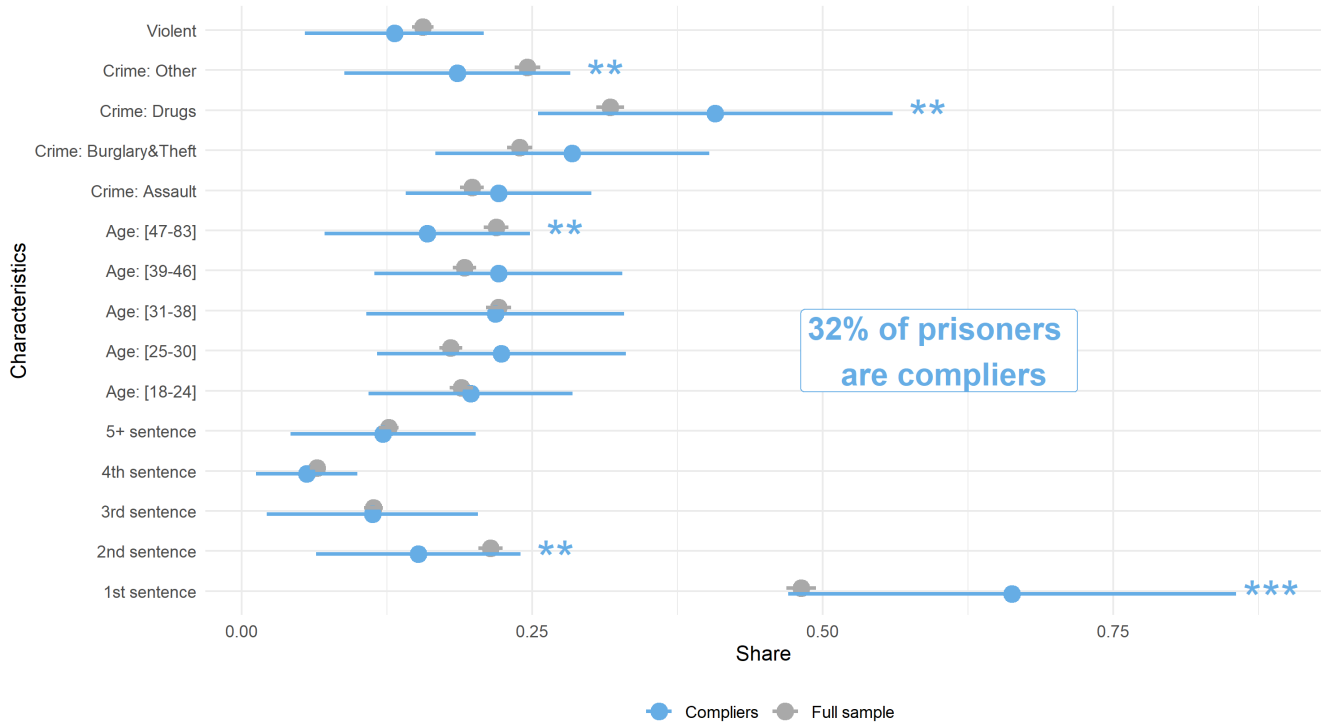


Figure 4: Characterization of Compliers

Notes. This figure shows each characteristic's proportion in the whole sample (in grey) and in the group of compliers (in blue). The confidence intervals around the compliers' means are calculated using 250 bootstrap replications. I estimate that the share of compliers in the entire sample is 32%.

$P(\text{Complier})$ is the share of compliers in the full sample. I repeat this process with 250 bootstrap replications to obtain standard errors around the compliers' means. On Figure 4, I show the prevalence of each characteristics among the full sample (in grey) and the sample of compliers (in blue). On a number of characteristics, compliers are similar to the whole sample of inmates. I indeed obtain that 32% of all inmates are compliers, suggesting that a significant proportion of inmates are complying with the recommendation. This helps thinking about the external validity of *Parcours* as well as having implications for targeting. Compliers are more likely to be first-time offenders, perhaps individuals with less prior knowledge about prison activities, and more likely to be incarcerated for offenses associated with drugs. However, second-time offenders and older individuals, aged 47 and more, possibly who have more experience behind bars and may rely less on their intervention plan, are less prevalent in the group of compliers. Table A.9 shows the proportion of each characteristic in the whole sample and in the sample of compliers along with standard errors.

4.3 Heterogeneity

I explore heterogeneity in treatment effects in three complementary ways: by running 2SLS regressions on various subsamples, estimating marginal treatment effects and with instrumental random forests to get individual treatment effects.

Each row of Table 9 estimates the 2SLS regressions on a subsample of the data. Recognizing that most estimates are imprecisely estimated—this exercise involves splitting the data into small samples—some interesting patterns appear. It seems CBT is particularly effective with drug offenders, who experience significant treatment effects for at least two years. The most striking finding is when splitting the sample on whether the inmates are first-time offenders (first sentence) or repeat offenders (second sentence or more). For first-time offenders, I find large and persistent effect up to three years after the treatment. For them, there appears to be little to no depreciation in skills: even passed the four-year mark, coefficients are of appreciable magnitude and the confidence intervals cover largely negative values.

Although the local average treatment effect (LATE) is in itself a policy-relevant parameter, it hides important features of the entire population. The LATE is the average treatment effect on the very specific population of compliers, which I have shown are mostly drugs offenders and first-time offenders. That being, it does not inform on the treatment effects for people who are not on the edge of receiving the treatment. This raises a crucial question: does a decision-maker responsible of managing a large-scale program in prison only care about the treatment effects on the compliers? Decision-makers have to deal with strict budgets and scarce human, financial and material resources: a better allocation of inmates across all programs requires an understanding of the overall carceral population as opposed to those who comply to recommendations.

The marginal treatment effect framework draws on the Roy model (Roy, 1951) and its generalized version (Eisenhauer et al., 2015) to model selection into treatment (Heckman and Vytlacil, 2007; Andresen, 2018; Zhou and Xie, 2019; Cornelissen et al., 2016). The key addition from the MTE literature is to have a latent (unobservable) index variable describing one’s resistance (or negative preference) to treatment. Consider an inmate i with potential outcomes $y_i(1)$ when treated and $y_i(0)$ when untreated, and let y_i be the observed outcome. Selection into the program goes as follow. Inmate i will participate in the program ($T_{ij} = 1$)

Table 9: Heterogeneity by Individual Characteristics

<i>Recidivism within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Panel A: Type of Crime						
Crime: Other	0.071 (0.267) [-0.452,0.594]	0.033 (0.263) [-0.482,0.549]	0.210 (0.297) [-0.372,0.791]	0.283 (0.363) [-0.429,0.994]	-0.021 (0.403) [-0.811,0.770]	0.098 (0.374) [-0.636,0.831]
Crime: Assault	-0.136 (0.162) [-0.453,0.182]	-0.228 (0.205) [-0.630,0.174]	-0.230 (0.217) [-0.655,0.196]	-0.201 (0.227) [-0.646,0.244]	-0.210 (0.291) [-0.781,0.361]	-0.083 (0.217) [-0.507,0.342]
Crime: Burglary&Theft	-0.170 (0.147) [-0.458,0.118]	-0.143 (0.156) [-0.449,0.162]	-0.173 (0.179) [-0.524,0.178]	-0.039 (0.180) [-0.392,0.315]	-0.156 (0.149) [-0.448,0.136]	-0.253** (0.128) [-0.503,-0.002]
Crime: Drugs	-0.337*** (0.110) [-0.553,-0.121]	-0.352*** (0.125) [-0.598,-0.107]	-0.290** (0.133) [-0.551,-0.029]	-0.190 (0.157) [-0.497,0.118]	-0.088 (0.173) [-0.428,0.252]	-0.143 (0.192) [-0.519,0.232]
Panel B: Age						
Age: [18-24]	-0.274* (0.141) [-0.551,0.002]	-0.185 (0.167) [-0.512,0.141]	-0.250 (0.199) [-0.639,0.140]	-0.331 (0.222) [-0.766,0.105]	-0.400* (0.220) [-0.832,0.032]	-0.460** (0.213) [-0.878,-0.042]
Age: [25-30]	-0.133 (0.154) [-0.435,0.169]	-0.294 (0.198) [-0.683,0.094]	-0.106 (0.241) [-0.578,0.366]	0.060 (0.267) [-0.463,0.583]	0.090 (0.264) [-0.426,0.607]	0.174 (0.199) [-0.216,0.564]
Age: [31-38]	-0.002 (0.199) [-0.392,0.388]	-0.066 (0.216) [-0.489,0.358]	-0.081 (0.215) [-0.503,0.341]	0.039 (0.227) [-0.405,0.483]	0.047 (0.232) [-0.407,0.502]	0.046 (0.210) [-0.366,0.458]
Age: [39-46]	-0.342** (0.166) [-0.668,-0.016]	-0.315* (0.171) [-0.650,0.019]	-0.269 (0.227) [-0.713,0.176]	-0.147 (0.228) [-0.595,0.300]	-0.139 (0.227) [-0.583,0.306]	-0.130 (0.214) [-0.549,0.289]
Age: [47-83]	-0.137 (0.221) [-0.570,0.295]	-0.021 (0.253) [-0.516,0.475]	0.079 (0.273) [-0.456,0.613]	0.018 (0.333) [-0.634,0.670]	-0.150 (0.366) [-0.866,0.567]	-0.191 (0.322) [-0.822,0.441]
Panel C: Number of Sentences						
First-time offenders	-0.199*** (0.068) [-0.332,-0.065]	-0.214*** (0.082) [-0.375,-0.053]	-0.240** (0.109) [-0.454,-0.026]	-0.248** (0.122) [-0.487,-0.010]	-0.189 (0.121) [-0.427,0.049]	-0.228 (0.146) [-0.514,0.057]
Repeat offenders	-0.111 (0.164) [-0.432,0.210]	-0.090 (0.170) [-0.423,0.242]	0.003 (0.186) [-0.362,0.369]	0.247 (0.213) [-0.171,0.664]	0.092 (0.237) [-0.373,0.557]	0.098 (0.188) [-0.272,0.467]
Full controls	✓	✓	✓	✓	✓	✓

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the 2SLS effects on recidivism from within six months upon release up to five years. Each row characterizes the subset of inmates used in the regression. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

depending on their observed characteristics (\mathbf{x}_i), the participation rate of their evaluator (z_{ij}) and their distaste for treatment (u_i): $T_{ij} = \mathbb{1}\{P(\mathbf{x}_i, z_{ij}) > u_i\}$, where $P(\mathbf{x}_i, z_{ij})$ is the propensity score function. The marginal treatment effect (MTE) is defined as the effect of participation for a defined measure of distaste for treatment:

$$\text{MTE}(\mathbf{X}_i = \mathbf{x}, U_i = u) = E(y_i(1) - y_i(0) | \mathbf{X}_i = \mathbf{x}, U_i = u).$$

The MTE curve thus shows how the treatment effects behave for inmates who are pushed into the program as the propensity score increases. For low levels of distaste for treatment, inmates at the margin are similar to always-takers. For higher values of resistance, inmates at the margin of participation resemble never-takers.

Figure 5a shows the distribution of the propensity scores for treated (in blue) and untreated (in white) inmates estimated using probit regressions.¹⁷ The MTE curve is identified only when the two distribution of propensity scores overlap—following the literature, I trim the extremes (1% of each side as shown by the dashed red lines) to ensure a common support. Figure 5b shows the MTE curve when using a quadratic polynomial specification and when considering recidivism within 6 months as the outcome. I construct the confidence interval with clustered bootstrap with 100 replications and estimate the model using the separate approach (Brinch et al., 2017).

I obtain an upward-sloping curve indicating that inmates at the margin whose unobserved characteristics would make them likely to enroll in the program (i.e., with a low level of resistance to treatment) are those who benefit the most from CBT. As unobserved resistance increases, inmates on the margin have characteristics similar to those of never-takers (i.e., inmates with high levels of resistance to treatment). For those, the treatment effects are closer to zero, suggesting little to no impact past a certain level of resistance. Figure B.2 shows similar upward-sloping MTE curves for other specification choices. This result resonates with a large body of research in medicine and psychology on the effects of compulsory versus voluntary treatments (Hachtel et al., 2019). Most conversations in the criminal justice field on this revolve upon drug treatment courts, which are specialized courts that mandate substance abuse treatments as an alternative to incarceration (Gottfredson et al., 2003). Parole boards may also mandate treatments as a condition for early release (Ar-

¹⁷These calculations were run using the Stata package *mtfe* (Andresen, 2018).

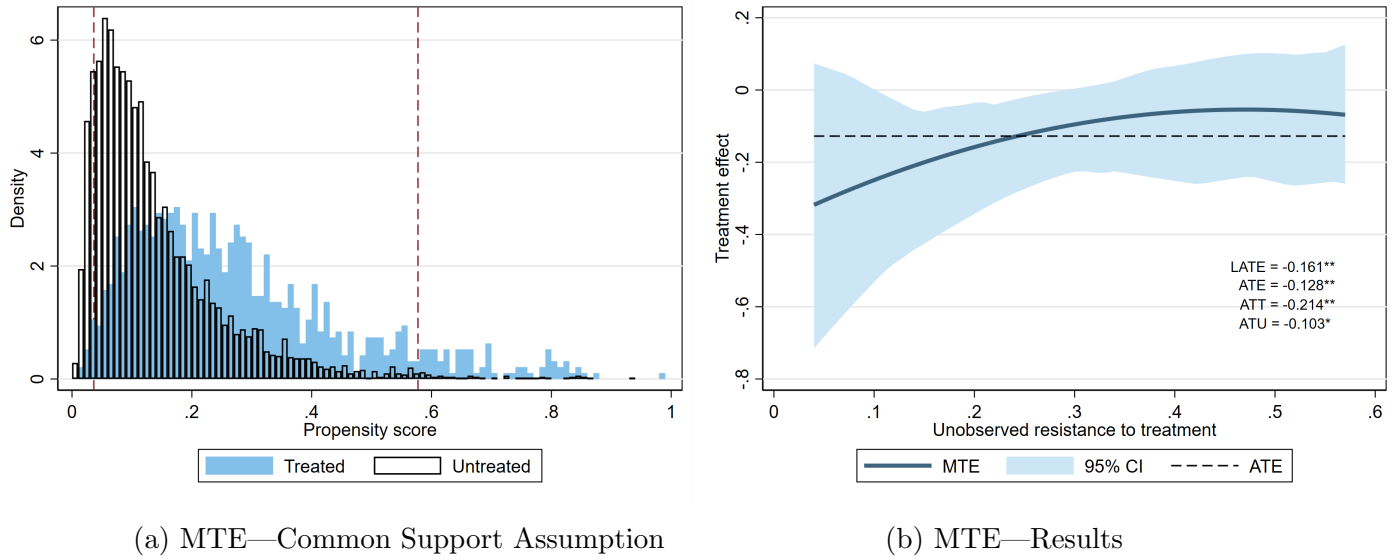


Figure 5: Marginal Treatment Effects

Notes. The figure on the left shows the distribution of the propensity scores among participants (blue) and non-participants (white), calculated using a probit regression. The dashed red lines show the 1st and 99th percentiles of the distribution, and observations at the extremes are trimmed. The figure on the right shows the MTE curve with recidivism within six months as the dependent variable. The confidence interval is obtained using 250 bootstrap replications. The MTE curve is estimated using the separate approach and uses a quadratic polynomial specification. The MTE estimation uses the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator. Estimations are made with the Stata package developed by [Andresen \(2018\)](#).

[bour and Marchand, 2022](#)). While most CBT programs are already voluntary, my analysis suggests that the most intrinsically motivated inmates benefit the most from the treatment.

Lastly, I explore how recent machine learning techniques can perform at estimating individual treatment effects along with their standard errors. A series of papers ([Athey and Imbens, 2016](#); [Wager and Athey, 2018](#); [Athey and Wager, 2019](#); [Biewen and Kugler, 2021](#)) have developed a reliably unbiased, robust and *honest* method to perform these calculations. The authors, in short, use random forests to assess the presence of heterogeneous treatment effects. In recent work, [Athey et al. \(2019\)](#) generalized the causal forest framework to incorporate common estimation methods, such as regressions with instrumental variables. [Athey et al. \(2019\)](#) show that $\beta(x)$ —a conditional average treatment effect—is identified from the

data with an instrument, z_{ij} here, under the usual moment conditions:

$$\begin{aligned}\mathbb{E}\{z_{ij}(y_{ij} - \beta(x)T_{ij} - \alpha(x))|X_i = x\} &= 0 \\ \mathbb{E}\{y_{ij} - \beta(x)T_{ij} - \alpha(x)|X_i = x\} &= 0.\end{aligned}$$

$\beta(x)$ can be estimated locally on different subpopulations by slicing x over an array of values. It is worth noting, however, the possibility of overestimating spurious effects and in doing so, confound statistical noise with true heterogeneity. To mitigate this risk, [Wager and Athey \(2018\)](#) introduced *honest* random forests that aim at capturing true heterogeneity. Each tree is grown using one half of the sample, whereby the other half of the sample is marked as the prediction sample. The algorithm seeks the variable that maximizes heterogeneity in the treatment effect by splitting the growing sample into all possible ways. Other nodes are sought with the same procedure until a certain stopping criterion is met, for instance, a required minimum of observations to split the sample. The predicted treatment effects are then calculated using the observations in the prediction sample, thus circumventing any spurious effects mentioned previously. This algorithm is repeated and replicated multiple times, turning a large number of trees into a forest. In order to best predict an individual treatment effect, as well as the standard error for each observation in the dataset, the estimation samples are bootstrapped at each iteration. In brief, predictions for individual i are made only using the trees in which i was not in the bootstrapped sample. Such predictions are called out-of-bag predictions.

Again using recidivism within six months as the outcome variable and all control variables, I first estimate individual treatment effects *without* using the instrument—an exercise similar to using OLS for estimating a regression of a recidivism dummy on a participation indicator. The top panel of Figure 6 shows the distribution of the “treatment effects”. The distribution is centered around -0.05 , similar to the coefficient obtained by OLS (Table 4). However, the causal forest algorithm uncovers a somewhat wider distribution, with some large effects detected in the neighborhood of -0.18 .

The bottom panel shows the distribution of treatment effects when using the instrument. The distribution is centered around -0.13 , which is close to what was obtained when using the standard 2SLS approach. I can reject that the predictions from the causal forest and the

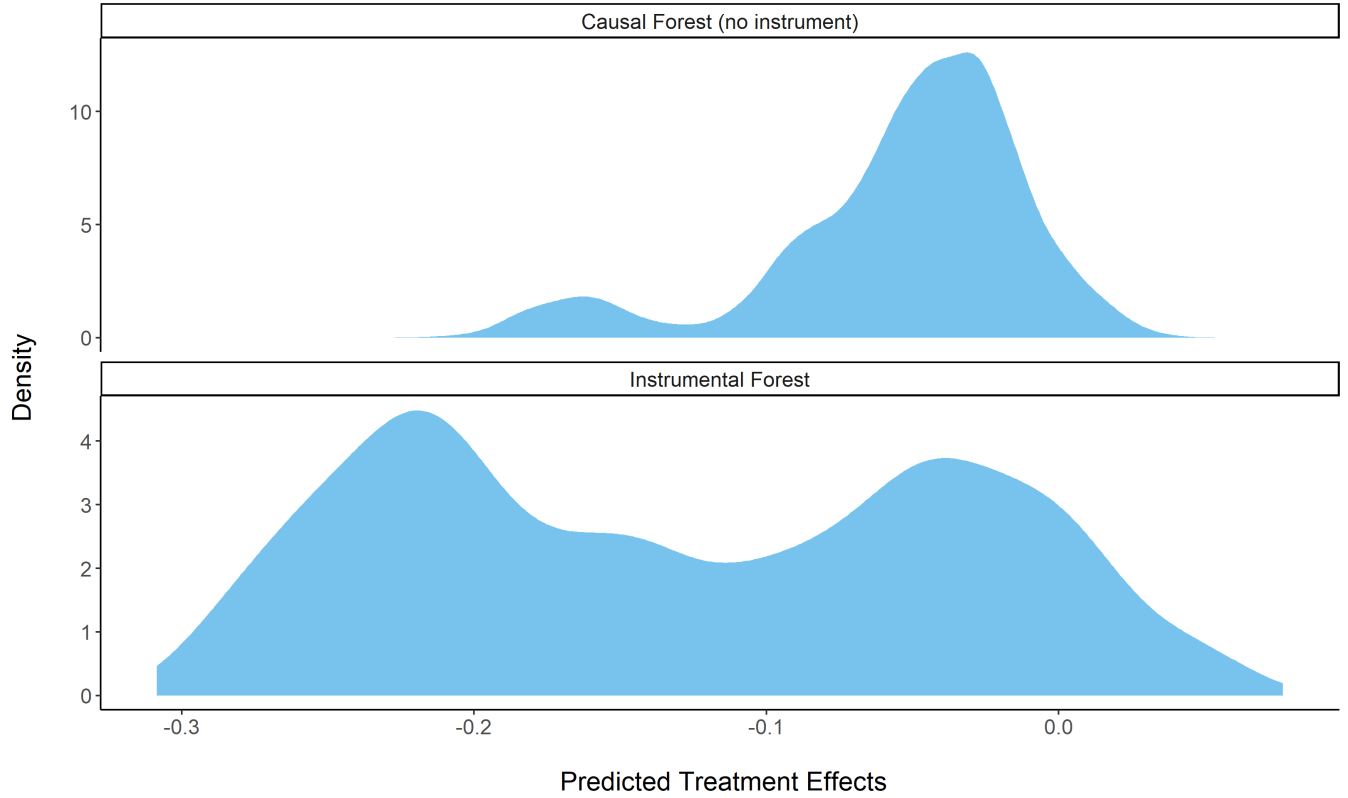


Figure 6: Densities of Treatment Effects, Without and With the Instrument

Notes. The plot at the top shows the distribution of out-of-bag predicted treatment effects given by the causal forest algorithm, without instrumenting for participation. The plot at the bottom shows the distribution of out-of-bag predicted treatment effects given by the instrumental forest algorithm, in which the residualized participation rate, z_{ij} , is used to instrument for participation. All trees are grown with the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

instrumental forest were drawn from the same distribution ($p \leq 0.0001$). A recent literature on instrumental variables regressions shows that, in most cases, uncovered treatment effects are a weighted average of treatment effects among compliers and always- or never-takers. Some treatments effects will receive negative weights, hence will fail to provide a convex combination of weights. [Blandhol et al. \(2022\)](#) propose to control for covariates in a non-parametric manner. Using the instrumental forest approach not only confirm the main results, but shows that controlling for the covariates non-parametrically produces very similar results.

Figure 7 ranks all inmates based on their predicted (out-of-bag) treatment effects and plots the confidence interval around each point estimate. To obtain the confidence intervals, I grew 50,000 trees using bootstrap. Like with the MTE curve, all treatment effects are

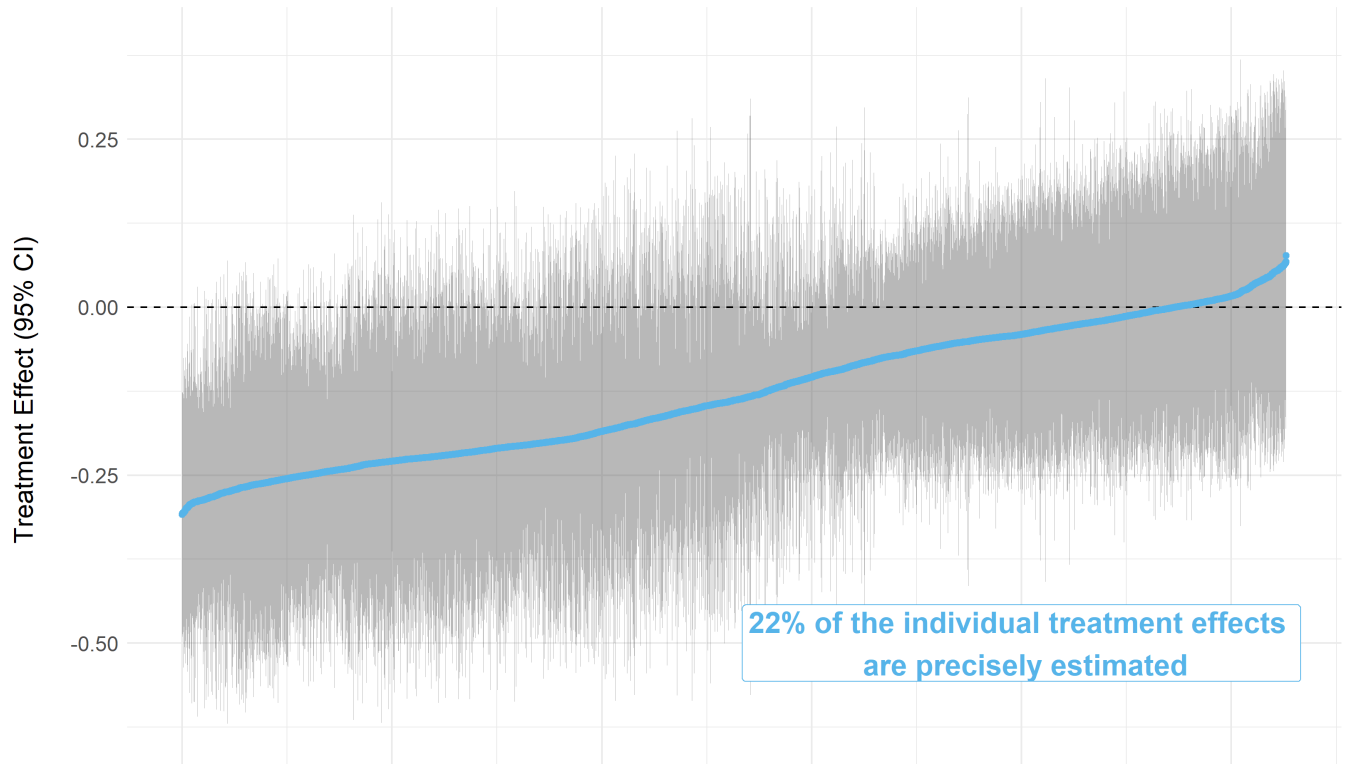


Figure 7: Individual Treatment Effects

Notes. The plot shows each out-of-bag predicted treatment effect—one for each inmate, ranked along the x-axis—along with individual 95% confidence intervals computed with the instrumental forest algorithm, in which the residualized participation rate, z_{ij} , is used to instrument for participation. 50,000 trees are grown with the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator. 22% of individual treatment effects are significant.

negative—meaning that the likelihood of recidivism decreases for all inmates—but some treatment effects are more precise than others. Roughly 22% of individual treatment effects are significant at the 5% level. The treatment effects for inmates at the very end of the distribution (on the right) are rather noisy and indistinguishable from zero.

4.4 Group Composition and Timing

The current setting offers a unique opportunity to test the features of CBT that yield the most beneficial effects. Because *Parcours* is given to only one group at a time in a given prison, participants have no influence over *who* else participates or *when* the program starts. In theory, concentrating high-risk prisoners or inmates prone to disorderly behavior could negate any positive effects from the program, even more so when the program asks par-

ticipants to interact with each other. For each session group p , I calculate the group size (n_p), the average number of sentences, average age and the rate of assault offenders of the participants. In each case—except for the group size—I remove i ’s own observation to avoid small sample biases. For instance, for a given characteristic x , I compute the group average for inmate i as

$$\bar{x}_{(-i)} = \frac{\sum_{k \in \mathcal{P}_i} x_k - x_i}{n_{p_i} - 1},$$

where \mathcal{P}_i denotes the set of inmates in the group of i . Columns (1) to (4) of Table A.13 show that a participant’s own characteristics do not predict the composition of their group. I do this by estimating various regressions of the form $\bar{x}_{(-i)} = \alpha_0 + \mathbf{x}_i \boldsymbol{\alpha} + \epsilon_i$. I find that most coefficients are not significant, with non-significant F-statistics as well. There is one exception with the group’s average age, where coefficients associated with serving for a third or more time are associated with a lower average age among their group. The only condition for group formation for *Parcours* is that participants should not know each other prior to their current sentence—for instance, they cannot be members of the same street gang. This condition could explain the result because offenders serving their third or more sentence are bound to know more people in the facility.

To better understand if the group composition plays a role in the persistence and magnitude of the treatment effects, for each characteristic, I split the treatment sample into two based on the median of the variable. I estimate a 2SLS regression on each subsample. The results are presented in Table 10. I consider once again recidivism up to within five year. Most coefficients are imprecisely estimated since the sample size of each regression shrinks by half mechanically. Nonetheless, I find suggestive evidence that when the group consists of older individual (aged 36 and more), the treatment effects are larger and more persistent than if the group is composed of younger inmates. This result resonates with a large body of literature of the age-crime profile, showing that older individuals are less likely to engage in criminal activities ([Bell et al., 2022](#)). My results suggest that such inmates can have positive influence in a group-based CBT program. Similarly, I find larger effects when first- or second-time offenders compose the majority of the group. I do not find any evidence of heterogeneity based on the size of the group or rate of assault offenders among the participants.

Table 10: Heterogeneity by Group Composition

<i>Recidivism within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Panel A: Group Size						
Group size ≤ 5	-0.208 (0.128) [-0.459,0.043]	-0.184 (0.142) [-0.461,0.094]	-0.105 (0.165) [-0.429,0.218]	0.042 (0.190) [-0.330,0.414]	-0.107 (0.198) [-0.495,0.282]	-0.139 (0.170) [-0.472,0.193]
Group size > 5	-0.210 (0.144) [-0.492,0.072]	-0.255* (0.152) [-0.554,0.044]	-0.218 (0.160) [-0.531,0.095]	-0.069 (0.180) [-0.422,0.284]	-0.060 (0.195) [-0.441,0.322]	-0.172 (0.196) [-0.557,0.213]
Panel B: Age						
Ave. Age ≤ 36	-0.171 (0.130) [-0.426,0.085]	-0.169 (0.145) [-0.453,0.115]	-0.150 (0.165) [-0.474,0.174]	-0.026 (0.191) [-0.400,0.348]	-0.055 (0.204) [-0.455,0.345]	-0.087 (0.175) [-0.430,0.255]
Ave. Age > 36	-0.241* (0.138) [-0.512,0.030]	-0.262* (0.145) [-0.547,0.023]	-0.166 (0.153) [-0.465,0.133]	0.008 (0.172) [-0.328,0.345]	-0.096 (0.185) [-0.460,0.267]	-0.225 (0.177) [-0.571,0.121]
Panel C: Number of Sentences						
Ave. Num. Sentences ≤ 2	-0.247** (0.125) [-0.493,-0.001]	-0.289** (0.131) [-0.545,-0.032]	-0.203 (0.141) [-0.479,0.072]	-0.036 (0.151) [-0.333,0.260]	-0.080 (0.141) [-0.357,0.197]	-0.089 (0.131) [-0.346,0.168]
Ave. Num. Sentences > 2	-0.171 (0.149) [-0.463,0.120]	-0.143 (0.165) [-0.467,0.181]	-0.113 (0.188) [-0.483,0.256]	0.020 (0.240) [-0.450,0.490]	-0.103 (0.307) [-0.705,0.498]	-0.328 (0.271) [-0.859,0.203]
Panel D: Assault Offenders						
Assault Offenders Rate = 0	-0.230* (0.135) [-0.495,0.035]	-0.222 (0.148) [-0.511,0.067]	-0.101 (0.167) [-0.429,0.226]	0.037 (0.196) [-0.346,0.421]	-0.102 (0.206) [-0.505,0.300]	-0.182 (0.195) [-0.563,0.200]
Assault Offenders Rate > 0	-0.184 (0.131) [-0.440,0.072]	-0.211 (0.137) [-0.480,0.059]	-0.221 (0.151) [-0.517,0.076]	-0.063 (0.168) [-0.392,0.266]	-0.073 (0.177) [-0.421,0.274]	-0.134 (0.161) [-0.450,0.181]
Full controls	✓	✓	✓	✓	✓	✓

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the 2SLS effects on recidivism from within six months upon release up to five years. Each row characterizes the subset of participants included in each regression based on the composition of their group. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Another interesting margin to analyze is the program's starting point relative to one's planned release date. For each participant, I calculate the fraction of time served, say s_i , at the date of the first session. For example, if $s_i = 0.10$, then participant i completed 10% of his sentence before the program started. The histogram on Figure B.3 shows the distribution of s across the sample of participants: about half the participants start before the third of their sentence, while 75% of participants start before 37% of their sentence has been completed. Column (5) of Table A.13 shows the results of the following regression:

$$s_i = \alpha_0 + \mathbf{x}_i \boldsymbol{\alpha} + p_i + \epsilon_i,$$

where p_i is a group fixed-effect. Intuitively, this regression compares the timing of the program for participants in the same group. The results show that one’s characteristics do not predict when the program starts—this suggests that, within a group, the fraction of time served prior to the program’s onset, can be viewed as random across participants.

I estimate several new 2SLS regressions in which I define the treatment based on when the program started relative to the fraction of time served before the first session. In this section, an inmate i is defined as treated if $s_i \geq k$, and I vary k from 0 to 0.40 with increments of 0.01. That is, $k = \{0, 0.01, 0.02, \dots, 0.40\}$. Participant i is dropped if $s_i < k$. For each dependent variable, I thus estimate 41 2SLS regressions, each time dropping participants who started the program too early to be considered. I denote each treatment effect coefficient by $\hat{\beta}_k$. For instance, $\hat{\beta}_0$ is the treatment effect when considering participants who completed at least 0% of their sentence before the program started—this is simply all participants. Thus $\hat{\beta}_0$ corresponds to the main 2SLS effect found previously. $\hat{\beta}_{0.20}$ corresponds to the treatment effect when the participant sample is composed of participants who completed at least 20% of their sentence before the first session.

If the skills learned through CBT depreciate during the sentence, we should expect the $\hat{\beta}_k$ ’s to get *more negative* as k increases, as this would mean that participants who completed a larger portion of their sentence prior to the program—when the program starts closer to their release date—would experience larger treatment effects. In contrast, learning could persist—and expands—beyond the completion of the program. If the time served between program completion and release is used as an opportunity to put in practice the newly-acquired skills, we would expect the $\hat{\beta}_k$ ’s to converge to zero. This is because participants who started early will have more time to put into practice their new skills, whereas participants who start closer to their release do not get this opportunity and perhaps quickly go back to their previous habits upon release. Finally, if the program yields the same effects regardless of its timing, the $\hat{\beta}_k$ ’s would be constant.

Figure 8 shows the $\hat{\beta}_k$ ’s as a function of k . I estimate the treatment effects using recidivism within six months up to within three years after release. The patterns are quite striking, when considering recidivism in the short-term, inmates who begin the program later experience larger treatment effects. These effects appear to be more persistent too, with sizable effects even two years after release. Changing the timing of the program does not appear to make

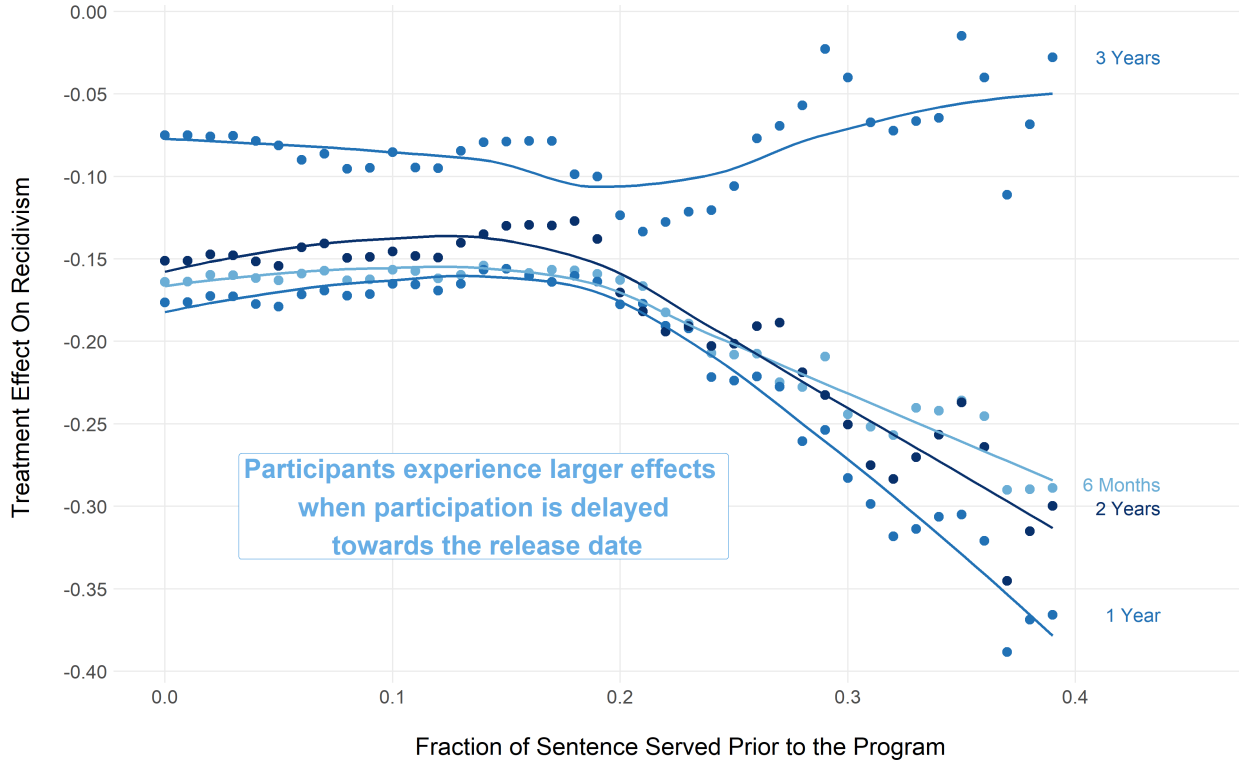


Figure 8: Treatment Effect By Fraction of Sentence Served

Notes. The plot shows the 2SLS treatment effects by conditioning on $s_i \geq k$, where increments in k are shown on the x-axis. Each point represents a different 2SLS regression. For example, when considering recidivism within six months upon release (light blue), I first include all participants in the treatment sample (first point starting from the left), then all participants who completed at least 1% of their sentence before the program started (second point starting from the left), and so on, up to 40%. I consider recidivism from within six months upon release up to three years. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

the effects permanent, as the treatment effects for recidivism within three years are rather constant and close to zero. However, the fact that skills acquired through CBT depreciate overtime implies that follow-up interventions upon release may be necessary to ensure a persistent desistance from crime, or, at least, longer-lasting treatment effects.

4.5 Robustness Checks

I test the robustness of the main findings in several ways. In Table A.10, I show the results from regressions using only the randomization controls—that is, these regressions do not control for the inmate’s age category, Indigenous backgrounds, whether the crime was violent and the number of sentences. The point estimates are virtually the same which is expected

when the instrument does not correlate with characteristics, however, adding more controls helps with precision.

The results are also robust to alterations to the instrument. When constructing the instrument, I accounted for more experienced evaluators being assigned to more serious offenders in some cases. Because these cases are rare, removing the crimes fixed effects ($crime_i$) and the evaluator's experience ($neval_{ij}$) from Equation (2) should not affect the results. I reconstruct the instrument using the following first-step equation instead:

$$T_{ij} = \delta_0 + \delta_1 year_i \times prison_i + \epsilon_{ij}.$$

I use this alternative instrument to predict participation in CBT in the same 2SLS framework. I present the results in Table A.11, and find unchanged results when doing so.

Another approach is to use the jackknife IV estimator (JIVE) proposed by Angrist et al. (1999). Instead of relying on a precalculated participation rate, this approach uses the actual evaluators' dummies in an overidentified model and omits observation i when calculating the first stage coefficient. Let \mathbf{G} be the $N \times K$ matrix of evaluators' indicators, where N is the number of inmates and K is the number of evaluators, and let \mathbf{T} represents the vector of treatment indicators. An element of the instruments matrix, say g_{ij} , equals 1 if inmate i is matched with evaluator j , and zero otherwise. Following the notation of Poi (2006), let $\mathbf{G}_{(-i)}$ be the same matrix without the i -th row, and let $\mathbf{T}_{(-i)}$ be similarly defined. The JIVE approach calculates a predicted treatment probability for all inmates, removing one row of \mathbf{G} at a time. The first stage coefficient for i is thus given by $\left(\mathbf{G}'_{(-i)}\mathbf{G}_{(-i)}\right)^{-1}\mathbf{G}'_{(-i)}\mathbf{T}_{(-i)}$. This procedure mimics the *leave-one-out* technique when precalculating the residualized participation rate, but accounts for the model being overidentified.

The Panel A of Table A.12 compares different estimators using recidivism within six months at the outcome variable. Columns (1) and (2) repeat the main specification using the precalculated participation rate, z_{ij} , as the instrument. In column (1), I show the effect without including any controls in the regressions, while column (2) adds the full set of controls. For this exercise, I do not cluster the standard errors, and use robust standard errors instead. The significance level remains unaffected. Columns (3) uses all the evaluators dummies as instruments, without *any* controls, and I find a significant reduction of 8 percentage points. Column (4) adds the full set of controls and shows an effect similar in magnitude to

the main coefficients. In a recent paper, [Goldsmith-Pinkham et al. \(2022\)](#) show that first stage regressions with multiple IVs may suffer from *contamination* and yield estimates that do not satisfy the monotonicity assumption when more controls (e.g., prison or year fixed effects) are needed. The fact that using the evaluators dummies without any controls renders a negative and significant effect suggests there is a negligible degree of contamination in this setting.

The Panel B of Table A.12 shows the treatment effects when using LASSO to select the control variables (column (1)) and the instruments among all the evaluators' dummies (column (2)). When letting LASSO selecting the most correlated instruments, the algorithm selects five indicators out of 399. The resulting treatment effect is noisier, yet still negative. With a p-value of 0.103, the confidence interval suggests the treatment effect is likely negative even when using this restrictive approach.

5 Preventing Recidivism from Behind Bars

While research on every aspect of criminal behavior is growing, there is still much researchers have yet to learn about criminal psychology and the human mind, including its motivations to commit crimes. Experts in the field of criminal psychology are making great strides in understanding the rationale of crime, and yet the effects of incarceration on inmates, one of the most common methods of punishment, remains unclear and understudied. Crime and criminal convictions are highly circumstantial, and thus inmates will almost undoubtedly experience incarceration differently, for better or worse. Despite a host of possible negative consequences of institutionalization, including negative influence from criminal peers, prosocial activities offered in some prison settings may provide a safer, more stable environment for inmates. Thereby, incarceration can present the opportunity to take part in programs that enable one to sharpen and acquire new skills, to receive group support or individual therapy, and to undertake an internal process of reflection.

Parcours is one such example of meaningful programming in which inmates can engage while incarcerated. In this paper, I evaluated the effects of participating in the behavioral program implemented in the prisons in Quebec, Canada. By leveraging randomness in the assignment of inmates to risk evaluators who can recommend the program, this paper is

the first to derive causal estimates of CBT on the specific population of incarcerated men. I found large, negative and significant effects on recidivism in the short-term, while the behavior of participants while incarcerated remains unchanged. This is likely because the program is still ongoing at that time. This paper further finds that first-time offenders, as well as drug offenders, are most likely to comply with the program recommendation in addition to benefiting greatly from therapy. Thus, targeting first-time offenders and drug offenders has the potential of accelerating the positive effects of such a program by, namely, reducing the likelihood of costly incarceration. Further, this paper finds that

- (1) Around \$3,800 are saved in incarceration costs alone since participants are less likely to be reincarcerated in the future;
- (2) Participants with intrinsic motivation to improve benefit the most from the CBT program;
- (3) Composition and timing matter: CBT is most beneficial when the group is composed of first-time offenders and older inmates. Inmates who participate at a later stage of incarceration experience larger treatment effects.

Further research is required to determine whether reentry programs, in other words, programs delivered upon detention, hold promise during the reintegration process. Brief follow-up interventions might be key to ensure continuity in the acquisition of behavioral skills. Going forward, more data and experiments are needed to determine heterogeneity in the treatment effects. For instance, it remains unclear how Indigenous offenders' specific issues are tackled by the program, as well as whether female inmates can benefit from CBT. Other types of measures, such as educational training or mental health therapy, would also gain credibility from further research.

There appears to be a large gap in the criminal research field regarding not only crime prevention, but in the treatment of criminals both during detention and in aftercare. Further evidence for other types of programs, settings, and profiles of participants is required in an effort to improve policies encouraging successful reintegration. For the time being, the great advantage of programs like *Parcours*, as demonstrated by this paper, is that it brings us one step closer to preventing recidivism from behind bars.

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Appendix

A Additional Tables

Table A.1: First Stage Statistics

	(1) Program	(2) Program	(3) Program	(4) Program
Instrument	0.619*** (0.081)	0.549*** (0.060)	0.549*** (0.061)	0.641*** (0.075)
N	5 847	5 847	5 847	3 343
F-stat (excl. inst.)	58.83***	83.01***	81.56***	72.67***
Rand. controls		✓		
Full controls			✓	✓
5-year sample				✓

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (4) report the first stage coefficients from regressions of participation indicators on the instrument, the residualized participation rate, z_{ij} . Column (1) contains no controls. Column (2) includes the set of **randomization** controls: prison and year fixed effects, type of crime and the experience of the evaluator. Column (3) includes the **full** set of controls and adds: age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime. Column (4) reproduces column (3) but with the subset of inmates who are observed for at least five years. The F-statistics test the significance of the excluded instrument.

Table A.2: Balance Test

	(1) Program	(2) Instrument
Age: [25-30]	0.018 (0.013)	0.013** (0.005)
Age: [31-38]	0.021 (0.013)	0.010** (0.004)
Age: [39-46]	0.001 (0.015)	0.013** (0.005)
Age: [47-83]	-0.013 (0.015)	0.015*** (0.005)
2nd sentence	0.038*** (0.012)	0.002 (0.004)
3rd sentence	0.048*** (0.015)	0.005 (0.005)
4th sentence	-0.004 (0.018)	0.001 (0.006)
5+ sentence	0.034** (0.016)	0.003 (0.006)
Violent crime	0.019 (0.016)	-0.007 (0.005)
Indigenous	-0.067 (0.062)	-0.008 (0.015)
Constant	0.138*** (0.012)	-0.013** (0.006)
N	5847	5847
Rand. controls	✓	✓
F-stat	2.68***	1.25

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) regresses the indicator for participation on the set of characteristics. Column (2) regresses the instrument, the residualized participation rate z_{ij} , on the same characteristics. Both regressions absorb the set of **randomization** controls: prison and year fixed effects, type of crime and the experience of the evaluator. The F-statistics test the significance of the short regressions—the coefficients displayed on the table. .

Table A.3: Placebo Check—Disciplinary Infractions and Parole

<i>Dep. Var.</i>	(1) Disciplinary Infrac.	(2) Violent Disciplinary Infrac.	(3) Parole
Instrument	0.009 (0.142) [-0.270,0.287]	-0.014 (0.044) [-0.100,0.071]	0.054 (0.073) [-0.089,0.197]
Average of dep. var.	0.244	0.078	0.343
Full controls	✓	✓	✓
Observations	9501	9501	4973

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) reports the reduced form effect on the number of disciplinary infractions in the placebo sample. Column (2) reports the reduced form effect on the number of violent disciplinary infractions in the placebo sample. Column (3) reports the reduced form effect on the likelihood of being granted parole using only the sample of inmates eligible for parole in the placebo sample. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator. The construction of the placebo sample is explained in footnote 12.

Table A.4: Placebo Check—Recidivism

<i>Recidivism within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Instrument	-0.011 (0.044) [-0.098,0.075]	-0.034 (0.054) [-0.141,0.073]	0.039 (0.050) [-0.060,0.138]	0.078 (0.055) [-0.029,0.185]	0.079 (0.055) [-0.029,0.187]	0.101* (0.061) [-0.019,0.220]
Average of dep. var.	0.201	0.317	0.431	0.502	0.552	0.589
Full controls	✓	✓	✓	✓	✓	✓
Observations	9501	9339	8754	7517	6182	4961

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the reduced form coefficients from regressions of recidivism indicators within six months upon release up to five years on the instrument, the residualized participation rate z_{ij} , in the placebo sample. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator. The construction of the placebo sample is explained in footnote 12.

Table A.5: Balance Test—Program Availability

<i>Dep. Var. :</i>	Program Available
Age: [25-30]	0.005 (0.012)
Age: [31-38]	0.012 (0.011)
Age: [39-46]	0.006 (0.012)
Age: [47-83]	−0.002 (0.011)
2nd sentence	−0.001 (0.009)
3rd sentence	−0.014 (0.013)
4th sentence	−0.015 (0.015)
5+ sentence	−0.011 (0.012)
Violent crime	−0.009 (0.012)
Indigenous	0.003 (0.026)
Constant	0.411*** (0.011)
N	16 415
Rand. controls	✓
F-stat	0.40

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows the results from a regression of a program availability indicator on the set of characteristics. The regression absorbs the set of **randomization** controls: prison and year fixed effects, type of crime and the experience of the evaluator. The F-statistic tests the significance of the short regression—the coefficients displayed on the table.

Table A.6: 2SLS estimation—Effect on Disciplinary Infractions (inv. hyperbolic sine)

<i>Dep. Var:</i>	(1) asinh(Disciplinary Infrac.)	(2) asinh(Violent Disciplinary Infrac.)
Panel A: Reduced Form		
Instrument	0.023 (0.071) [-0.116,0.162]	-0.056 (0.039) [-0.132,0.020]
Panel B: 2SLS		
Program	0.074 (0.221) [-0.360,0.508]	-0.178 (0.134) [-0.440,0.085]
Average of dep. var. (untransformed)	0.389	0.108
Full controls	✓	✓
Observations	5847	5847

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table reproduces the first two columns of Table 5 but applies the inverse hyperbolic sine transformation to the dependent variables. Column (1) reports the reduced form coefficient and 2SLS effect on the number of disciplinary infractions (transformed). Column (2) reports the reduced form coefficient and 2SLS effect on the number of violent disciplinary infractions (transformed). All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Table A.7: 2SLS estimation—Number of Reoffenses

<i>Num. of reoffenses within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Program	-0.196* (0.102) [-0.395,0.004]	-0.274* (0.145) [-0.559,0.011]	-0.074 (0.239) [-0.541,0.394]	0.284 (0.335) [-0.372,0.940]	0.351 (0.445) [-0.521,1.223]	0.163 (0.463) [-0.743,1.070]
Average of dep. var.	0.219	0.413	0.739	1.053	1.349	1.580
Full controls	✓	✓	✓	✓	✓	✓
Observations	5847	5804	5587	4923	4146	3343

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the 2SLS effects on the number of reoffenses within six months upon release up to five years. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Table A.8: 2SLS estimation—Outcomes for Recidivists Only

<i>Dep. Var. :</i>	(1) Num. days elapsed	(2) Crime: Violent	(3) Crime: Assault	(4) Crime: Burglary	(5) Crime: Drugs
Program	192.717 (188.967) [-177.652,563.085]	-0.010 (0.084) [-0.176,0.155]	0.040 (0.108) [-0.171,0.251]	0.161 (0.115) [-0.064,0.386]	0.190 (0.120) [-0.046,0.426]
Average of dep. var.	530.921	0.109	0.156	0.237	0.237
Full controls	✓	✓	✓	✓	✓
Observations	3259	3259	3259	3259	3259

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) reports the 2SLS effect on the number of days elapsed before the next reoffense for the sample of recidivists. Columns (2) to (4) report the 2SLS effects on the next crime committed for the sample of recidivists. Column (2) considers the next crime being violent, while columns (3) to (5) consider the next crime being an assault, burglary or related to drugs, respectively. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Table A.9: Characterization of the Compliers

Characteristics	(1) First Stage	(2) $P(X = x)$	(3) $P(X = x \text{Complier})$	(4) Ratio (3)/(2)	(5) $p\text{-value: } (2) = (3)$
Crime: Other	0.377*** (0.101)	0.246 (0.006)	0.186 (0.050)	0.755	0.044**
Crime: Assault	0.654*** (0.122)	0.198 (0.005)	0.221 (0.041)	1.116	0.356
Crime: Burglary&Theft	0.678*** (0.129)	0.239 (0.006)	0.284 (0.060)	1.189	0.202
Crime: Drugs	0.610*** (0.102)	0.317 (0.006)	0.407 (0.078)	1.285	0.047**
Age: [18-24]	0.645*** (0.119)	0.189 (0.005)	0.197 (0.045)	1.043	0.764
Age: [25-30]	0.584*** (0.131)	0.180 (0.005)	0.223 (0.055)	1.243	0.174
Age: [31-38]	0.561*** (0.161)	0.221 (0.005)	0.218 (0.057)	0.987	0.933
Age: [39-46]	0.598*** (0.118)	0.192 (0.005)	0.221 (0.054)	1.153	0.364
Age: [47-83]	0.358*** (0.103)	0.219 (0.005)	0.160 (0.045)	0.730	0.030**
1st sentence	0.695*** (0.085)	0.481 (0.007)	0.663 (0.098)	1.376	0.001***
2nd sentence	0.409*** (0.136)	0.214 (0.005)	0.152 (0.045)	0.710	0.022**
3rd sentence	0.428** (0.178)	0.113 (0.004)	0.112 (0.046)	0.992	0.972
4th sentence	0.522*** (0.195)	0.065 (0.003)	0.056 (0.022)	0.862	0.516
5+ sentence	0.561*** (0.153)	0.126 (0.004)	0.122 (0.041)	0.962	0.844
Indigenous	0.417 (0.598)	0.014 (0.002)	0.014 (0.173)	1.057	0.994
Violent	0.528*** (0.155)	0.156 (0.005)	0.131 (0.039)	0.843	0.302
Share of the sample		1.000	0.321		

Notes. Standard errors in parentheses in column (1) are two-way clustered at the prisoner and evaluator level; Standard errors in parentheses in column (3) are obtained by bootstrap with 250 replications; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) reports the first stage coefficient for all subsamples. Columns (2) and (3) show the mean of each characteristic in the full sample and among compliers, respectively. Column (4) takes the ratio between column (3) and (2). Column (5) shows the p-value for a test of equality of means between columns (2) and (3). The share of compliers is calculated using the method described in footnote 14 and the means of characteristics among compliers are calculated using the method described in the text. All intermediary regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Table A.10: 2SLS estimation—Recidivism (no additional controls)

<i>Recidivism within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Panel A: Reduced Form						
Instrument	-0.090* (0.051) [-0.191,0.011]	-0.115* (0.059) [-0.231,0.002]	-0.082 (0.065) [-0.209,0.046]	-0.037 (0.069) [-0.173,0.099]	-0.059 (0.078) [-0.212,0.094]	-0.065 (0.083) [-0.229,0.098]
Panel B: 2SLS						
Program	-0.154* (0.088) [-0.327,0.019]	-0.174* (0.101) [-0.372,0.024]	-0.147 (0.112) [-0.366,0.073]	-0.062 (0.126) [-0.308,0.185]	-0.088 (0.133) [-0.350,0.173]	-0.082 (0.122) [-0.322,0.157]
Average of dep. var.	0.188	0.299	0.418	0.496	0.544	0.579
Rand. controls	✓	✓	✓	✓	✓	✓
Observations	5847	5804	5587	4923	4146	3343

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the reduced form coefficients (panel A) and 2SLS effects (panel B) on recidivism from within six months upon release up to five years. All regressions use the residualized participation rate, z_{ij} , to instrument for participation. All regressions include the set of **randomization** controls: prison and year fixed effects, type of crime, and the experience of the evaluator.

Table A.11: 2SLS estimation—Recidivism (alternative instrument definition)

<i>Recidivism within...</i>	(1) 6 Months	(2) 1 Year	(3) 2 Years	(4) 3 Years	(5) 4 Years	(6) 5 Years
Panel A: Reduced Form						
Instrument	-0.100** (0.046) [-0.191,-0.009]	-0.118** (0.052) [-0.220,-0.015]	-0.090 (0.057) [-0.202,0.022]	-0.052 (0.062) [-0.174,0.070]	-0.075 (0.067) [-0.207,0.058]	-0.089 (0.071) [-0.230,0.051]
Panel B: 2SLS						
Program	-0.174** (0.079) [-0.329,-0.019]	-0.184** (0.088) [-0.356,-0.012]	-0.159 (0.098) [-0.352,0.034]	-0.087 (0.112) [-0.306,0.132]	-0.118 (0.114) [-0.341,0.105]	-0.116 (0.105) [-0.323,0.090]
Average of dep. var.	0.188	0.299	0.418	0.496	0.544	0.579
Full controls	✓	✓	✓	✓	✓	✓
Observations	5847	5804	5587	4923	4146	3343

Notes. Standard errors in parentheses are two-way clustered at the prisoner and evaluator level; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) to (6) report the reduced form coefficients (panel A) and 2SLS effects (panel B) on recidivism from within six months upon release up to five years. All regressions use the residualized participation rate constructed using only prison and year fixed effects to instrument for participation. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Table A.12: IV estimation—Recidivism (alternative IV strategies)

Panel A: JIVE estimator				
<i>Specification</i>	(1) Main	(2) Main	(3) JIVE	(4) JIVE
Program	-0.175** (0.074) [-0.319,-0.030]	-0.164** (0.079) [-0.319,-0.009]	-0.079* (0.044) [-0.165,0.006]	-0.191** (0.080) [-0.348,-0.035]
Average of dep. var.	0.188	0.188	0.188	0.188
Full controls		✓		✓
Observations	5847	5847	5847	5847
Panel B: LASSO				
<i>Specification</i>	(1) LASSO (controls)	(2) LASSO (instruments)		
Program	-0.154** (0.065) [-0.281,-0.028]	-0.211 (0.129) [-0.464,0.043]		
Average of dep. var.	0.188	0.188		
Full controls	✓	✓		
Observations	5847	5847		

Notes. Robust standard errors in parentheses; 95% confidence intervals in square brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) and (2) from Panel A report the main 2SLS effects on recidivism within six months upon release using no controls and the full set of controls, respectively, and using the residualized participation rate, z_{ij} , to instrument for participation. Columns (3) and (4) use the evaluators' dummies as instruments using no controls and the full set of controls, respectively. Columns (1) from Panel B reports the 2SLS effects on recidivism within six months upon release using the residualized participation rate, z_{ij} , to instrument for participation and using LASSO to select the controls among the full set of controls. Columns (2) from Panel B reports the 2SLS effects on recidivism within six months upon release using the evaluators' dummies as instruments and using LASSO to select the instruments that are most correlated with participation. The **full** set of controls includes the type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator.

Table A.13: Balance Tests on Group Composition

	(1) Size	(2) Ave. Num. Sentences	(3) Ave. Age	(4) Assault Rate	(5) Starting Point
Age: [25-30]	-0.319 (0.336)	-0.070 (0.110)	1.322* (0.712)	-0.021 (0.022)	-0.029 (0.018)
Age: [31-38]	0.219 (0.349)	-0.140 (0.104)	0.582 (0.704)	-0.006 (0.024)	-0.054*** (0.018)
Age: [39-46]	0.031 (0.358)	-0.118 (0.102)	0.795 (0.754)	-0.003 (0.026)	-0.027 (0.020)
Age: [47-83]	-0.139 (0.349)	-0.234** (0.102)	-0.082 (0.779)	0.021 (0.025)	-0.024 (0.017)
2nd sentence	-0.273 (0.243)	0.042 (0.083)	-0.738 (0.595)	-0.005 (0.018)	0.020 (0.014)
3rd sentence	0.389 (0.326)	-0.030 (0.111)	-1.912** (0.748)	0.008 (0.024)	-0.004 (0.019)
4th sentence	-0.144 (0.396)	-0.187 (0.152)	-2.635** (1.055)	-0.019 (0.036)	0.031 (0.028)
5+ sentence	0.015 (0.343)	-0.230* (0.124)	-2.589*** (0.861)	0.007 (0.027)	0.024 (0.022)
Indigenous	0.434 (0.494)	0.406 (0.259)	-0.380 (1.366)	0.098 (0.063)	-0.025 (0.044)
Violent crime	-0.106 (0.360)	0.017 (0.093)	-0.076 (0.713)	0.011 (0.021)	0.014 (0.018)
Constant	5.682*** (0.299)	2.091*** (0.084)	36.637*** (0.568)	0.138*** (0.020)	0.309*** (0.014)
Rand. controls	✓	✓	✓	✓	✓
Group FE					✓
N	952	952	952	952	952
F-stat	1.02	1.39	1.95**	0.67	1.48

Notes. Standard errors in parentheses are clustered at the prisoner level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) regresses the group size of participant i on i ' characteristics. Columns (2) to (4) regress a leave-out mean characteristic, namely the average number of sentences, age and assault rate, on the participant's characteristics. Column (5) regresses the program's starting point, defined as the fraction of sentence time elapsed prior to the program's first session, on the participant's characteristics. All regressions absorb the set of **randomization** controls: prison and year fixed effects, type of crime and the experience of the evaluator. In addition, the regression in Column (5) absorb a group fixed effect. The F-statistics test the significance of the short regressions—the coefficients displayed on the table.

B Additional Figures

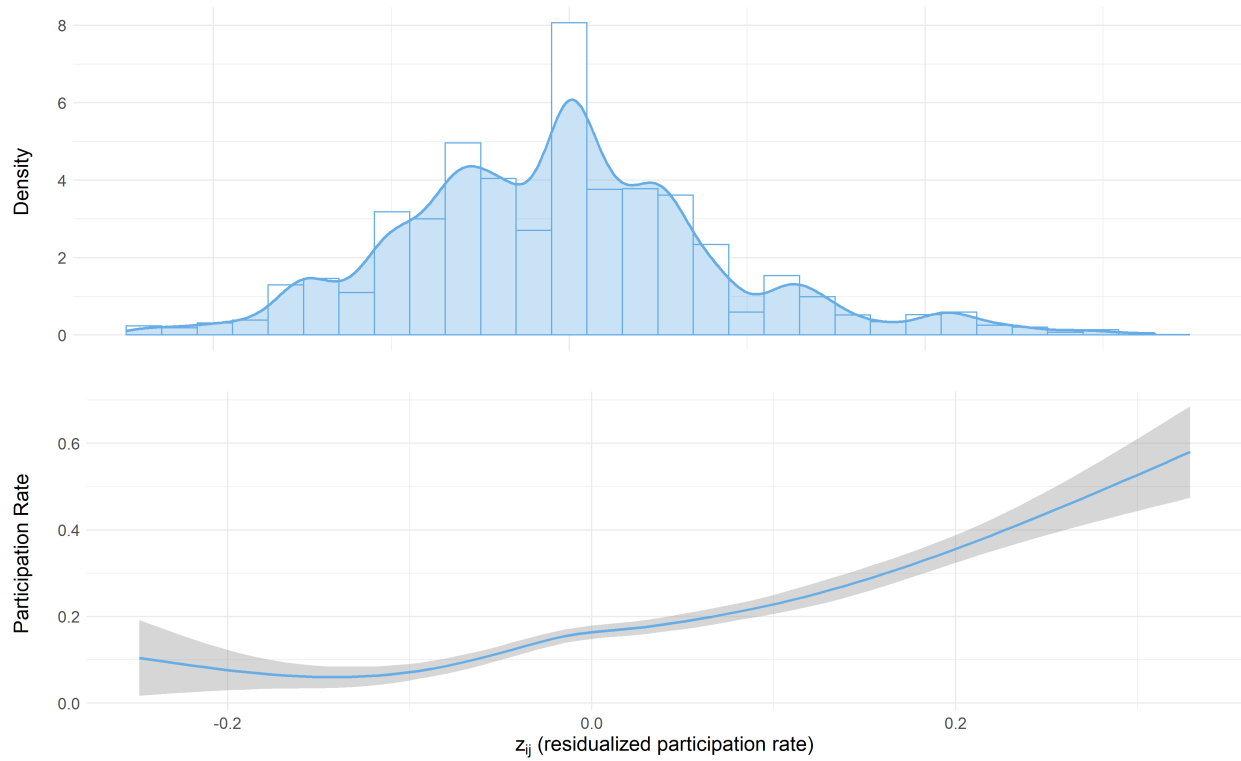


Figure B.1: Density of z_{ij} and Non-Parametric First Stage

Notes. The plot at the top shows the distribution of z_{ij} , the residualized participation rate. The plot at the bottom shows the non-parametric first-stage relationship between the actual participation decision and the instrument (without other controls). The non-parametric curve is obtained through the generalized additive model (GAM) smoothing method.

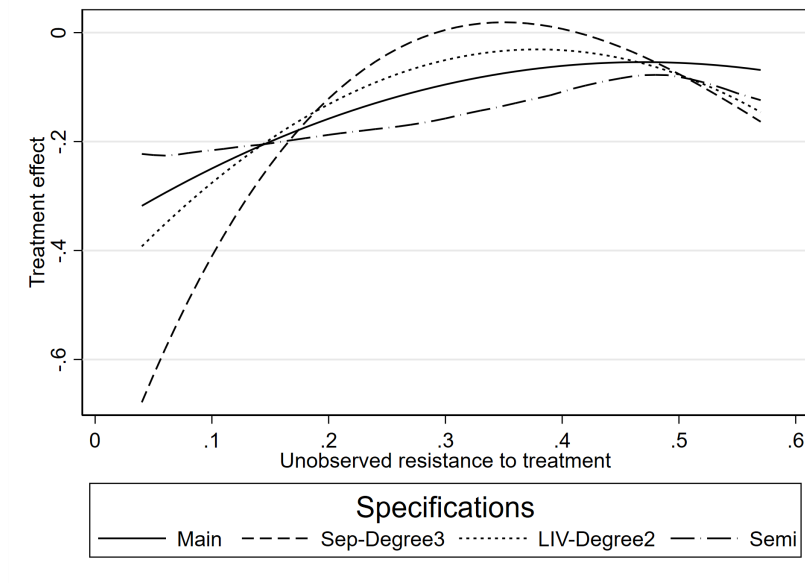


Figure B.2: MTE—Alternative Specifications

Notes. The figure shows MTE curves obtained with different specifications. The full dark line shows the main specification (separate approach, quadratic polynomial). The other specifications try a cubic polynomial, the local IVs approach with a quadratic polynomial and the semi-parametric approach. All regressions include the **full** set of controls: type of crime, age category, number of sentences, an indicator for being Indigenous, an indicator for a violent crime, prison and year fixed effects, and the experience of the evaluator. Estimations are made with the Stata package developed by [Andresen \(2018\)](#).

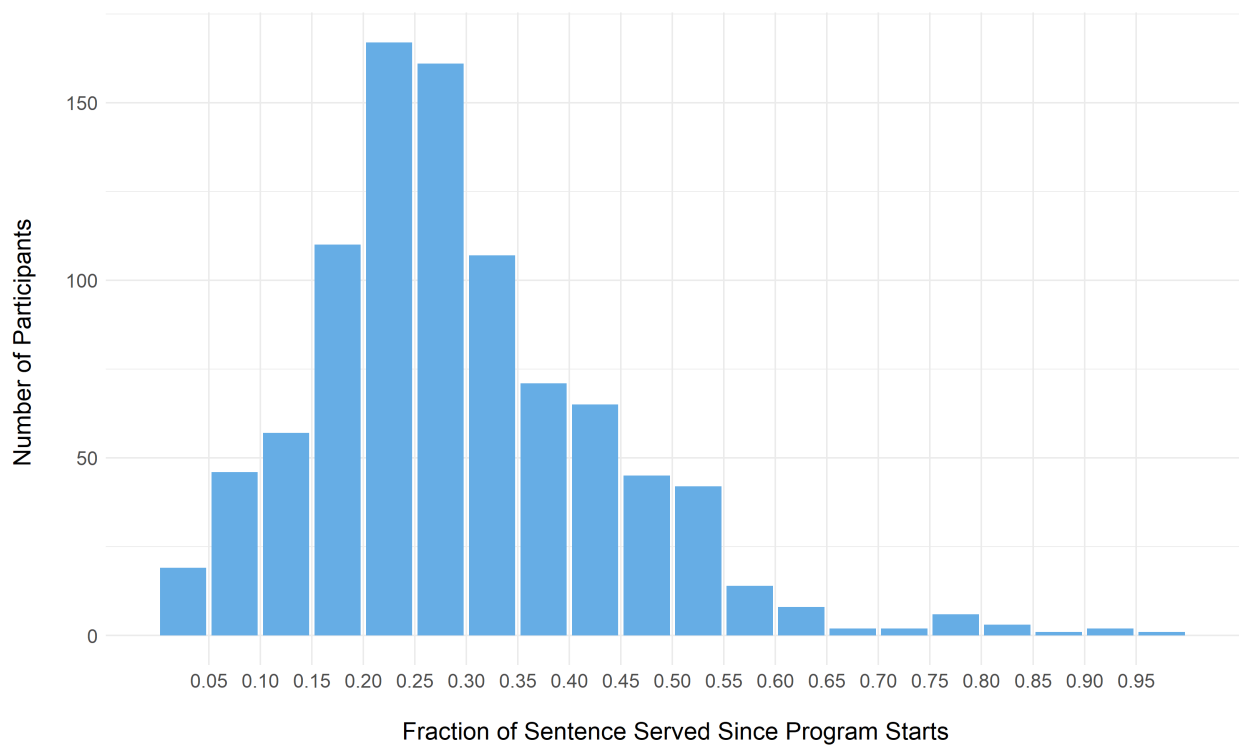


Figure B.3: Timing of the Program

Notes. The figure shows the distribution of s_i , the fraction of time served before the program's first session.