Chaire de recherche en **évaluation économique** des **programmes publics**

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

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wp eepp 2021 004

www.eepp.ulaval.ca

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Incarcerated offenders are offered a wide range of programs to encourage their chances of successful reintegration into society. Little is known, however, about the degree to which such programs improve prisoners' reentry. In this paper, I study the effects of a cognitive-behavioral program implemented in Quebec, Canada, with a rich micro-level dataset. To manage the econometric issue of inmates' self-selection into the program, I exploit inmates' random assignment to probation officers who exhibit varying propensities to recommend the rehabilitation measure. I find large, negative, and significant effects of the program on recidivism, as measured by an inmate's probability of serving a subsequent sentence: within one year following release, the program reduces recidivism by up to 18 percentage points. Moreover, the program is shown to decrease the number of future offenses. Further analyses indicate that the most plausible mechanism can be attributed to the program's success in altering offenders' preferences towards crime.

1 Introduction

In all jurisdictions, about one half of convicted offenders will recidivate. This statistic holds true in both the United States (Durose et al., 2014) and Canada (Bonta et al., 2003) despite marked differences in approaches to the mainstream criminal justice systems (Webster and Doob, 2007; Liebmann, 2010). Multiple factors have been advanced to explain an individual's propensity for criminality, including, but not limited to, substance abuse, early childhood trauma, and socioeconomic status (Doleac, 2019). Such factors can also determine whether an individual is likely to recidivate upon release. Improved social and economic conditions (Yang, 2017), work opportunities (Agan and Starr, 2018), gradual punishment (Mueller-Smith and Schnepel, 2019), and incarceration itself (Bhuller et al., 2020) are just some of the deterrent factors that have been examined in the recent economic literature.

One often overlooked aspect of criminal recidivism, however, is the extent to which prison-based programs, whose goals are to provide the participants with tools to better reintegrate society, can have long-lasting effects on one's criminal trajectory. Indeed, a number of recent studies (Kuziemko, 2013; Bhuller et al., 2020; Hjalmarsson and Lindquist, 2020; Macdonald, 2020) point to prison-based interventions as an essential factor in determining rehabilitation success.[1] Ranging from education training to behavioral therapy, prison-based programs require significant financial, material, and human resources to operate. The effectiveness of these programs on various inmate populations still remains contested.

In an effort to bridge the gap between research-based knowledge and practice, I seek to provide an answer to one critical question: can recidivism be prevented from behind bars? I estimate the effects of *Parcours*[2], a prison-based cognitive-behavioral program that seeks to deter criminal activities, by leveraging the unique selection process whereby inmates are enrolled in the program. A crucial complicating factor to assessing the effectiveness of this program on recidivism lies in the program being strictly voluntary: inmates are free to enroll at their own liberty. However, at the onset of incarceration, risk evaluators may formulate the recommendation to engage in *Parcours*. I thus exploit a so-called judges fixed effects design in order to identify the causal effect of participating. The present paper is, to my knowledge, the first attempt to derive causal estimates of the effects of a prison-based behavioral program on criminal recidivism. On analysis, I find that the program significantly reduces the likelihood of reoffending upon release.

Parcours was gradually launched in all Quebec's provincial prisons in 2007, and has since allowed for more than two thousand inmates to participate. It is aimed at inmates deemed to be *at risk* by a risk assessment tool. In this context, the risk tool is used to predict the likelihood that an individual will engage in criminal behavior following release. The program also targets individuals with supportive views of crime and those who demonstrate a lack of accountability for one's actions. The decision to participate rests with the inmates themselves. Indeed, like most, if not all prison-based programs, *Parcours* is voluntary, thus inducing a self-selection bias. Therefore, it would not be sufficient to compare the recidivism rates among participants with that of non-participants to infer the program's effect, as the resulting coefficients would plausibly be biased. I start my analysis by estimating a number of ordinary least squares regressions in which I naively compare participants with non-participants. I find small effects, if any, from the program.

In Quebec, inmates are randomly assigned to probation officers at the onset of their prison sentence. Probation officers are responsible for evaluating the inmates' risks and needs in order to complete

[1] This literature is reviewed at great length in Arbour et al. (2020).

[2] *Parcours* is the French word for journey, or path.

a personalized intervention plan. The probation officers display varying propensities to recommend the program. Hence, I leverage the random assignment of inmates to probation officers to create instrumental variables that exogenously affect participation. Such a design is commonly called a *judges fixed effects* design, since it usually consists in exploiting judges' propensities to take a decision of interest, or a *leniency* design. The primary threat to identification of the program's causal effect is the exclusion restriction, or in other words, the possibility that the probation officers could affect the inmates beyond their recommendation to enroll in *Parcours*.[3] I make the case that the inmates and their assigned probation officers have negligible interaction beyond a primary risk assessment. To support this argument, probation officers do not collaborate with, nor counsel inmates throughout their sentences, and thus interaction after the early stages of incarceration is limited. What is more, I show that probation officers' propensities do not affect inmates when the program is not in effect, thus providing solid evidence for the exclusion restriction. When using the instrumental variable strategy, I find that participation in the program reduces the likelihood of recidivism by around 18 percentage points within one year following the inmate's release. The results are robust to numerous specifications, and remain significant after I adjust the inference procedure with the method provided in Lee et al. (2020)[4]. The gaps between the OLS and IV estimators can be attributed either to negative selection, or to the subpopulation of compliers being especially responsive to the treatment. I show that the compliers are young inmates, which explains the magnitude of the findings considering that young offenders are more likely to reoffend. I discuss, in turn, all the assumptions for the instrument to be valid and robustly test these assumptions.

To better understand the mechanisms by which the program affects inmates' behavior, I examine other relevant outcomes. I show that the program significantly reduces the number of future offenses in the short-term, when repeat offenses are most likely. For inmates who do reoffend, I find little evidence of behavioral changes. For instance, the program does not significantly affect the probability of the next crime being violent in nature, such as an assault against a person. However, the program is shown to significantly postpone reoffenses, if any. Thus, I argue that reentry programs might be vital to ensuring continuity of intervention upon incarceration. Nor do I find evidence that the program increases one's likelihood of being granted parole, which could have directly affected the probability of recidivism. Therefore, I suggest that the causal channel on recidivism is how effective the program is at targeting complex, dynamic criminogenic factors that alter the participant's preferences for lawful activities. In the final portion of the paper, I explore heterogeneity in the results with the implementation of causal random forests (Athey et al., 2019). I find no compelling evidence of heterogeneity, suggesting that participants from all criminal backgrounds, and who have been prosecuted for a variety of crimes, appear to respond similarly to *Parcours*.

My paper endeavors to contribute to three major strands of the literature on this subject. Firstly, this paper is, as mentioned, one of the first to credibly identify the effects of a prison-based program on recidivism as the number of well-identified studies concerned with such programs is surprisingly limited. Additional research has, however, grappled with similar issues. For instance, Balafoutas et al. (2020) conducted an experiment whereby Greek inmates were encouraged to document and reflect on their prison terms; a seemingly simple activity that yielded positive prosocial behavior amongst randomly selected participants. Moreover, Zanella (2020) examines how the Italian prison job program affects a convict's rehabilitation. Leveraging an instrumental variable design and

[3]For instance, the effect of the program could be confounded with the effect of the quality of the probation officer.

[4]In Lee et al. (2020), the authors show that a valid inference procedure for an instrumental variable regression requires a F statistic of at least 104.7, departing from the popular belief that an F statistic greater than 10 is sufficient (Andrews et al., 2019).

a structural model, he finds that working in prison can either increase or decrease the chances of rehabilitation, depending on the sentence length to be served by the convict.[5] In this paper, I analyze the effects of participating in a prison-based intervention on an array of dimensions, including the probability to recidivate, which is the primary target of any program. The rich dataset studied throughout this paper enables me to consider the short- and long-term effects of *Parcours* on inmates' lives both during and after a prison term.

Secondly, my research highlights several psychological facets of a criminal's decision-making process. Unlike static factors, such as age or sex, that are strong predictors of criminal activity, the effects of dynamic factors, such as attitude towards authority, are more challenging to assess (Brown et al., 2009; Kroner and Yessine, 2013). In Maggioni et al. (2018), the authors study inmates from the State of California who were randomly assigned to a comprehensive accountability program focused on measuring trust amongst prison populations. Overall, the study finds that trust amongst participants significantly increased over time. Heller et al. (2017) study three behavioral interventions[6] targeted at juvenile offenders and at-risk youths. They find large behavioral responses: participants were less likely to be subsequently arrested and less likely to be readmitted in juvenile detention. In this paper, I show that an inmate's attitude towards authorities, accountability, and awareness of action are crucial factors to consider in an effort to understand recidivism among adult offenders. I present evidence that these factors can be strengthened in a behavioral program delivered during incarceration in a rather short and arguably low-cost intervention.

Thirdly, from an empirical perspective, my research contributes to the literature that exploits instrumental variables *à la* judges fixed effects; a strategy that consists of advancing random assignment to a decision-maker to arrive at plausible exogenous variations in the instruments (see Frandsen et al., 2019, for a recent review). I adapt the canonical design introduced by Kling (2006) by considering risk evaluators as the *judges*. I carry out recent econometric tests and perform novel 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 probation officers. I then 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. Perhaps most importantly, I explain how the inmates are assigned to probation officers: this will be crucial to understand the instrumental variable design. Finally,

[5] It is shown that inmates with longer sentences benefit more from working than their counterparts with shorter sentences. The advanced hypothesis is that the new skills formation compensates for capital depreciation, a fixed cost for any convict regardless of the sentence length. The instrument relies on the inmate's entry date, as earlier-sentenced inmates are more likely to work.

[6] The 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.

I present the data and some descriptive statistics.

2.1 *Parcours*: a Program for Short Sentences

In Canada, any offender sentenced to less than two years of prison time will serve their sentence in a provincial facility. Alternatively, when the sentence exceeds two years, the sentence will be served in a federal facility. In the province of Quebec, the 18 provincial prisons can also accommodate incarcerated individuals awaiting their sentence. Convicts 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. A number of provincial facilities offer specialized programs for the risks and needs of their prisoners. For 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. 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.

The Quebec's Ministry of Public Security oversees the Act Respecting the Quebec Correctional System (LSCQ, hereinafter) within all provincial facilities: it is the Ministry's responsibility to help offenders in their transition to becoming law-abiding citizens and to help facilitate their reintegration into the community. To do so, the Ministry has implemented a number of programs accessible to inmates while serving their sentences. 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, Serge Ménard, then Quebec's Minister of Public Security, commissioned an extensive independent examination of the entire provincial criminal justice system. After a collection of testimonies from frontline workers, Claude Corbo, a professor of political science, published a substantial report. Corbo (2001) provides specific recommendations to the Ministry, among which it is suggested that the LSCQ be amended to include a mandatory psychological evaluation of every offender under the responsibility of the provincial government (recommendations 22 and 23). Such psychological evaluations were recommended with a purpose of assessing the risks and needs of offenders in order to offer tailored programs (recommendation 46). 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 with sentences longer than six months (but still under two years) given that the likelihood of reintegration for such offenders is most uncertain.

Two significant reforms emerged from Corbo (2001): since 2007, every inmate with a sentence longer than six months is evaluated using the Level of Service/Case Management Inventory (Andrews et al. (2000); LS/CMI, hereinafter). Furthermore, *Parcours*, a program precisely designed for risky offenders, was developed and implemented in most facilities across Quebec.

The LS/CMI is an actuarial tool used to assess an inmate's risk (to match the level of service to the offender's risk) and needs (to target them in treatment) employed across Canada and in the United States. It comprises eight sections that gather essential information to provide an accurate portrait of an offender, from their criminal history, mental health disorders and substance abuse issues. Following the questionnaire, the convict receives a score out of a possible 43 points, which classifies them in one of five categories of risk: very high (30+), high (20-29), medium (11-19), low (5-10) and very low (0-4). Generally, an LS/CMI evaluation is completed at each sentence, however, if the probation agent 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.

At the request of the Ministry, *Parcours* was developed in 2007 by criminology professor Denis Lafortune (Lafortune and Blanchard, 2010). The program was specifically designed for high-risk individuals or for those who score at least 20 points as assessed by the LS/CMI, although individuals with lower scores can also participate. In a way, *Parcours* is intrinsically linked to Corbo (2001) as it follows directly from the report's recommendations. I refer to the appendix (Section A.2) for a further discussion of *Parcours*' key elements and curriculum.

2.2 Assignment Between an Inmate and a Probation Officer

When an inmate's sentence is between six months and two years, it is required by the law in the province of Quebec (LSCQ, articles 12 and 13) that a risk evaluation be conducted as quickly as possible following an official sentence. Offenders serving a sentence in a detention facility 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.

At the beginning of one's sentence, an inmate serving a six-month or greater term is matched with a probation officer. She[7] will evaluate the risks and needs of the inmate, and is responsible for formulating a tailored intervention plan unique to each inmate. After the plan is completed, the case is transferred to another agent, either another probation officer or a correctional counsellor. The probation officer, therefore, who was initially tasked with devising a plan does not follow the progress of the inmate she evaluated, nor does she accompany him when he is released from prison.

To create a valid instrument based on the assignment of an inmate to a probation officer, it is required that the allocation be random or, at least, random after controlling for observable characteristics. In provincial facilities, the allocation of cases depends on the status of the individual (whether they are sentenced or awaiting judgment), the length of the sentence (shorter or longer than six months), the type of crime (sexual offenses are deemed critical in the process) and whether the inmate is of Indigenous descent. These elements are not problematic: first, I only consider in-

[7] In the paper, I sometimes will use *he* and *his* to designate an inmate as the studied population of convicts consists of males only. In contrast, I will use *she* or *her* to refer to probation officers, who are central to the research design.

dividuals who were evaluated using the LS/CMI. Second, sexual offenders are not considered in the sample, although I still control for the type of crime in all baseline regressions. Lastly, I control for the Indigenous aspect by adding a dummy variable to all regressions.

Despite the fact that some probation officers are specialized in a variety of criminal backgrounds, they are trained to remain balanced and broad-based in the types of inmates they evaluate. Often, cases are assigned in a way that balances the workload among professionals on a team. Most notably, the inmates are not allocated based on their propensity to recidivate nor their likelihood to participate in correctional programs. Therefore, among a given group, for example, inmates convicted of a certain type of crime, the assignment to probation officers would be entirely random.

2.3 Data and Descriptive Statistics

Three datasets are necessary to carry out my analysis of the effects of the program on recidivism. First, the Ministry of Public Security provided me with the DACOR (Administrative Correctional Files) dictionary; an extensive computerized management system. The system contains information about any individual that receives a sentence 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. The most recent version of this file covers the period from 2007 to 2019.

From DACOR, I can determine if the 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 six possible categories: against a person, against property, weapons, gangs and explosives, traffic-related, drug-related and *other*. From this dataset, I also create the dependent variables (recidivism within a time window): 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 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.

Second, I was granted access to all the LS/CMI 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 probation officer who was responsible of the evaluation. Since the tool was implemented in 2007, I can observe one's entire experience with the LS/CMI.

Third, I requested the Parcours participation data, which are managed at the facility-level. In total, 11 prisons (out of 18) were able to provide me with these data. The authorities from the Ministry anonymized the data which I was able to merge with the panel dataset rather easily. 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 the 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 centers, are included in the 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.

A breakdown of the data is provided in Table 1. In total, the sample represents 1809 unique

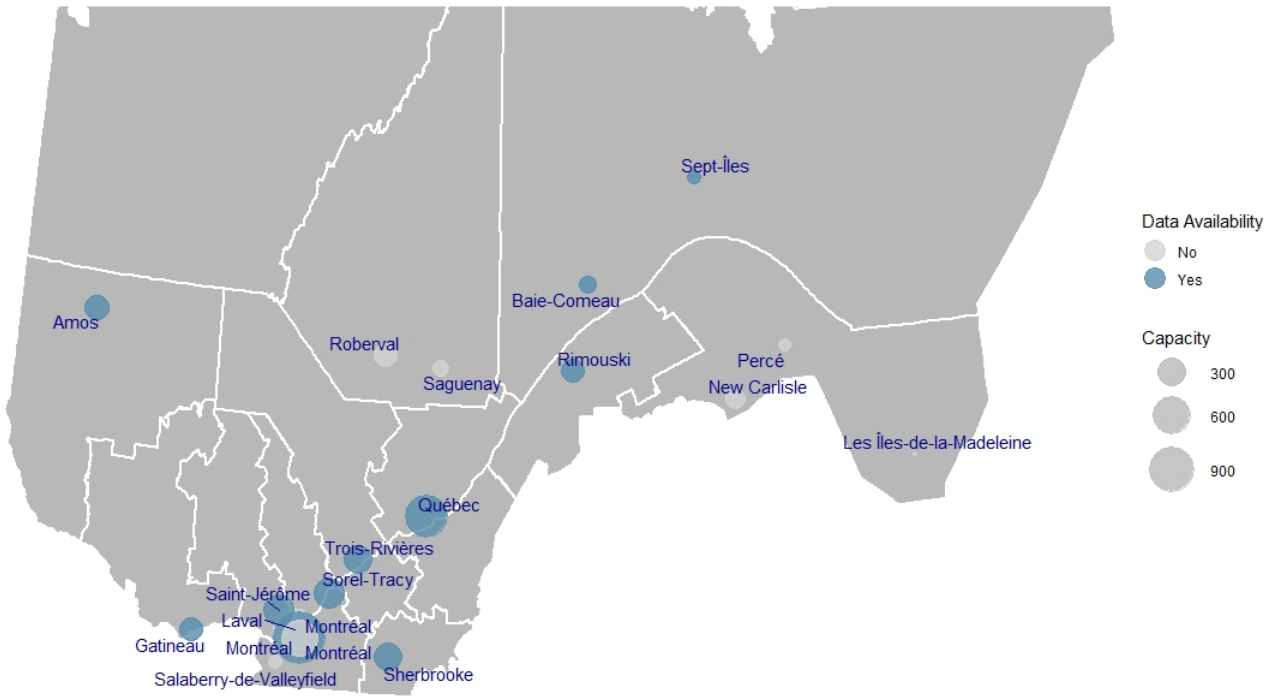


Figure 1: Correctional Facilities in the Province of Quebec

Notes. This is 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. *Takeaway.* The largest prisons are included in the final dataset. Only male prisoners are studied.

Table 1: Breakdown of the Participation Data by Facility

Facility	Number of Participants	Years Covered
Amos	57	2012-2014
Baie-Comeau	134	2007-2016
Hull	55	2008-2013
Montreal	287	2009-2016
Quebec	462	2007-2019
Rimouski	25	2013-2014
Sept-Iles	69	2010-2016
Sherbrooke	408	2007-2016
St-Jerome	84	2007-2015
Sorel-Tracy	71	2007-2013
Trois-Rivieres	157	2009-2016

Notes. This table reports the number of *Parcours* participants by facility and the years covered by each available file.

participants. Among those, some had a sentence of less than six months[8], and thus, an LS/CMI evaluation was not available for such inmates. I make one exception: I include participants with shorter sentences only if an LS/CMI evaluation was completed less than two years prior to the participation. Furthermore, there is potential for errors in the information collection process since, in some prisons, the information is collected by hand. In total, I was able to match 995 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 offered[9], a few participants (consisting of around five in most facilities) need to show interest for the program. Hence, I need to be cautious with whom I record as a non-participant. In this paper, a non-participant is 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.[10] 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.

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, offenders who committed a crime again property are overrepresented, as is for crimes involving drugs. In contrast, the average age in both groups is statistically the same. The key point here is to emphasize that participants are intrinsically different from non-participants on almost all observable variables. We can therefore expect that these differences be as large or starker on unobservable characteristics,

[8]Although the program was essentially for inmates with sentences longer than six months, convicts with shorter sentences can also participate.

[9]The 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.

[10]The 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.

such as motivation and remorse. A straight comparison of the recidivism rates between the two groups is likely to be biased - but in which direction?

On the one hand, highly motivated and remorseful inmates with an earnest desire to improve their lives could be naturally more willing to participate in rehabilitation measures. This is positive selection, in which case a naive average treatment effect would be downwardly biased. On the other hand, however, certain convicted inmates may regard *Parcours* as little more than an opportunity to take advantage of the system, thereby securing an early release. This is negative selection. For instance, since parole board members base their decision, among other things, on the correctional measures taken by the prisoner, participating in a program could ensure an easy way out. I find that participants have a overall lower recidivism rate than non-participants. Within one year, 31% of non-participants will reoffend, whereas this proportion drops to 25% among participants. This difference is also detectable when considering the rate of recidivism within six months and two years. Although this discrepancy could be explained by positive selection, it appears that participants are negatively selected, on average. Indeed, the risk score from the LS/CMI evaluation of participants is three points higher (out of 43) than that from non-participants. This measure highly correlates with recidivism (Arbour et al., 2020).

I then examine whether participants are more likely to request and subsequently, be granted, parole. Interestingly, participants are twice as much likely to seek parole than non-participants, however, on average, they are granted parole at the same rate as non-participants. I further discern some differences in recidivists' outcomes.[11] For instance, participating reoffenders will commit a smaller number of offenses within one year upon release, and the next offense is delayed by around three months. In contrast, the probability that the next offense leads to an incarceration spell, as opposed to a sentence within the community, is alike.

Notwithstanding the apparent adverse selection, the ultimate question remains: how much of the short-term differences are due to unobserved self-selection and what portion is caused by the program? To tackle such an issue, I discuss, in the next section, how I can leverage the random assignment to probation officers to construct an instrumental variable.

3 Identification Strategy

In this section, I introduce a simple econometric framework to which I will refer throughout the remaining of the paper. I then discuss the proposed instrumental variable strategy and the validity of the identifying assumptions.

3.1 Econometric Framework and Instrument

Consider a set of N inmates, indexed by $i = 1, \ldots, N$, each with a vector of observable characteristics X_i'. The researcher observes whether the inmate recidivates within a certain period of time, in which case $Y_i = 1$ and $Y_i = 0$ otherwise. In the paper, I vary the follow-up period from six months to two years. We also observe whether he participated in the program, in which case $D_i = 1$ and $D_i = 0$ otherwise.

[11] Here, *recidivists* does not refer to offenders with prior convictions, but to offenders that *will* recidivate during the follow-up period. For them, the outcomes of interest are the number of reoffenses, whether the next offense leads to an incarceration spell, and the delay before the next offense.

Table 2: Descriptive Statistics

	Participants	Non-Participants	p-value
Number of Observations			
N	995	5147	
Demographics/Crime			
Age	35.9 (11.71)	36.5 (12.04)	0.19
Indigenous	0.03 (0.18)	0.01 (0.09)	0.00
Prior Convictions	1.32 (1.99)	1.44 (2.35)	0.11
Crime: Against a Person	0.15 (0.35)	0.21 (0.41)	0.00
Crime: Against Property	0.36 (0.48)	0.28 (0.45)	0.00
Crime: Weapons, Gangs and Explosives	0.11 (0.31)	0.16 (0.36)	0.00
Crime: Traffic	0.07 (0.26)	0.10 (0.30)	0.02
Crime: Drugs	0.30 (0.46)	0.22 (0.42)	0.00
Crime: Other	0.01 (0.09)	0.02 (0.15)	0.00
Violent Crime	0.15 (0.36)	0.16 (0.36)	0.81
Parole			
Seeks Parole	0.16 (0.37)	0.08 (0.27)	0.00
Granted Parole	0.03 (0.18)	0.02 (0.15)	0.18
Recidivism			
Within 6 Months	0.14 (0.35)	0.20 (0.40)	0.00
Within 1 Year	0.25 (0.43)	0.31 (0.46)	0.00
Within 2 Years	0.39 (0.49)	0.42 (0.49)	0.03
Number of Reoffenses, Within 1 Year	0.32 (0.64)	0.44 (0.77)	0.00
Recidivists' Outcomes			
Number of Reoffenses, Within 1 Year	1.29 (0.61)	1.40 (0.76)	0.04
Next Sentence: Incarceration	0.75 (0.43)	0.77 (0.42)	0.23
Days Before Next Offense	625 (627)	512 (584)	0.00
Risk Score			
Risk Score	27 (6.95)	24 (8.75)	0.00

Notes. This table reports descriptive statistics (mean and standard deviation) of the final dataset. The sample is split based on the treatment status. I run multiple t-tests to compare the distribution of observable characteristics between the two samples and report the corresponding p-values. *Takeaway.* Participants are different from non-participants, even on observable characteristics. Thus we can expect that they are also different on unobservables. Participants tend to recidivate at a lower rate than non-participants, however, the portion of the difference owing to the program itself is unclear.

Consider then an inmate i, who serves a sentence in prison p after having committed a crime c in year t. In a regression setting, the *naive* approach is to directly estimate

$$Y_{iptc} = \alpha + \lambda_p + \lambda_t + \lambda_c + \delta D_i + X_i'\beta + \epsilon_{iptc},$$

where λ's are fixed effects and where X_i' is a vector of predetermined controls. These controls will include the inmate's age, age^2, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. This regression assumes that the covariance between D_i and ϵ_{iptc} given X_i is null or, in other words, it assumes the conditional independence assumption. However, several unobservable variables could be correlated with the treatment, thus violating the assumption, since inmates self-select into the program. For instance, in this setting, the researcher does not observe individual traits such as motivation, ability, remorse or awareness of consequences. Similarly, we do not observe potentially important factors from the criminal justice procedure, for example, the overall attitude of the judge during the trial. It should be stressed that judges can indeed prescribe participation to a rehabilitation measure, although programs remain entirely voluntary.

The self-selection bias is alleviated if the econometrician uses an instrument; a variable correlated with participation to the program but uncorrelated with the inmate's potential outcomes. Imbens and Angrist (1994) demonstrate how under certain conditions, which are to be discussed later, the instrument Z can be used to estimate a local average treatment effect - an average treatment effect on the compliers, or inmates whose treatment status was affected by the instrument.

Given the insights from the institutional setting and the specifications of the program *Parcours*, I propose a simple instrument, relying on the random allocation of prisoners between probation officers at the beginning of their sentence. Consider a set of J probation officers, indexed by $j = 1, \ldots, J$. I denote the match between the inmate and the probation officer by $j(i)$ - in other words, $j(i) = 1$ if inmate i is matched to probation officer j. Finally, throughout her career, the probation officer j has completed $n_j + 1$ unique risk evaluations of prisoners.

Consider now the participation rate of an evaluator; that is, over all the risk evaluations completed by officer j, the fraction of inmates who decided to enroll in the program. Mathematically, for an inmate such that $j(i) = 1$, this can be expressed as:

$$Z_i^c = \frac{1}{n_j} \sum_{\substack{k:j(k)=1 \\ k \neq i}} \mathrm{D}_k,$$

where D_k is the participation decision of inmate k. This measure is continuous. Hence, Z^c can be seen as a proxy of the evaluator's propensity to recommend the program. Notice that observation i is not considered when computing Z_i^c - this is commonly called a leave-one out instrument. To illustrate how this setting reproduces that of a natural experiment, consider the top panel of Figure 2. Two identical inmates, with respect to both observable and unobservable characteristics, enter a prison. Inmate i's risk is assessed by probation agent j. Agent j is, naturally, likely to advise participation. Given a high value of Z_i^c, inmate i decides to participate following the recommendation. On the contrary, inmate k happens to be evaluated by a probation officer who has a low propensity to recommend *Parcours*. Inmate k does not participate, perhaps, because he is unaware of the program and its effectiveness, or determines that he is unfit given that *Parcours* was not recommended from the onset. In this way, inmate k can be used as a counterfactual for

inmate i: they share the same potential outcomes, and their treatment decision was only affected through their probation agent's channel. Recall that I do not directly observe the recommendation, hence, Z^c defined above can be regarded as a proxy for a recommendation inclination.

At this stage, one might wonder where the variation arises from, or why some probation officers do *not* recommend the program. On the bottom panel of Figure 2, I plot each evaluator's average given risk score and their *Parcours* participation rate. We discern no clear relationship between these characteristics. That is, for a given average score, evaluators exhibit varying participation rates. Their propensities are neither correlated with their experience[12], as shown by the area of each circle. Therefore, I would support the hypothesis that evaluators naturally have varying propensities to advise participation in *Parcours*.

I further define a binary version of Z^c: I classify the probation officers into two groups whether they are of high-propensity or low-propensity. A probation officer from the high-propensity group has a program participation rate higher than the median participation rate of all her colleagues. That is,

$$Z_i^b = \begin{cases} 1 \text{ if } Z_i^c > \text{median}\{Z_k^c, \forall k \neq i\} \\ 0 \text{ otherwise.} \end{cases}$$

Given that the instruments are valid, which I verify in Section (3.2), the local average treatment effect, can be consistently estimated via an instrumental variable regression (IV, hereinafter). Again, consider an inmate i who enters a prison p at year t after committing a crime c. The first step of the estimation procedure is to obtain the predicted values of the following regression:

$$D_{iptc} = \alpha + \lambda_p + \lambda_t + \lambda_c + \rho Z_i + X_i'\beta + \eta_{iptc},$$

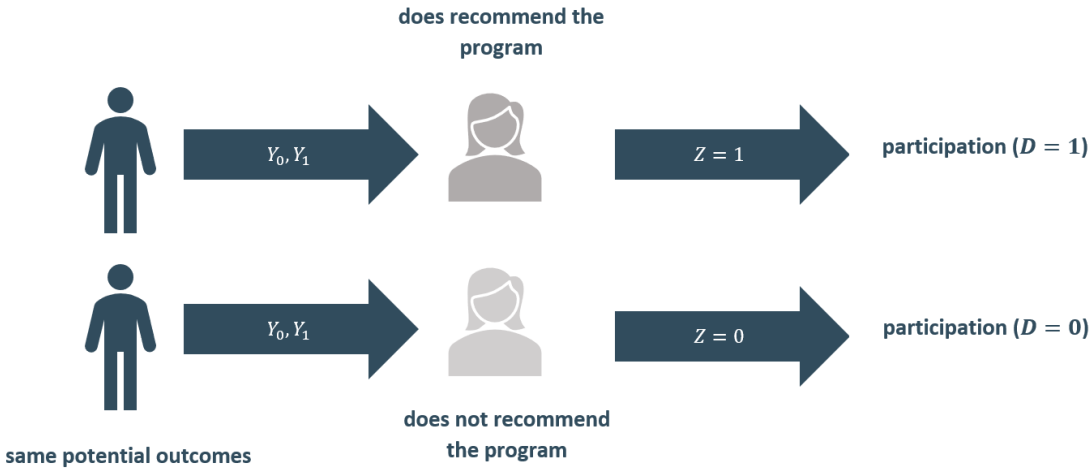
where D is the participation decision and ρ is the effect of the instrument on the participation - for the estimation to be valid, it is required that $\rho \neq 0$, an hypothesis that is easily verified with the F statistic of the first-stage regression. Z_i is any of the instruments defined previously, namely Z_i^c or Z_i^b. $\hat{D}_i$ can then be used to consistently estimate τ from the second-stage regression:

$$Y_{iptc} = \zeta + \lambda_p + \lambda_t + \lambda_c + \tau \hat{D}_i + X_i'\beta + \epsilon_{iptc},$$

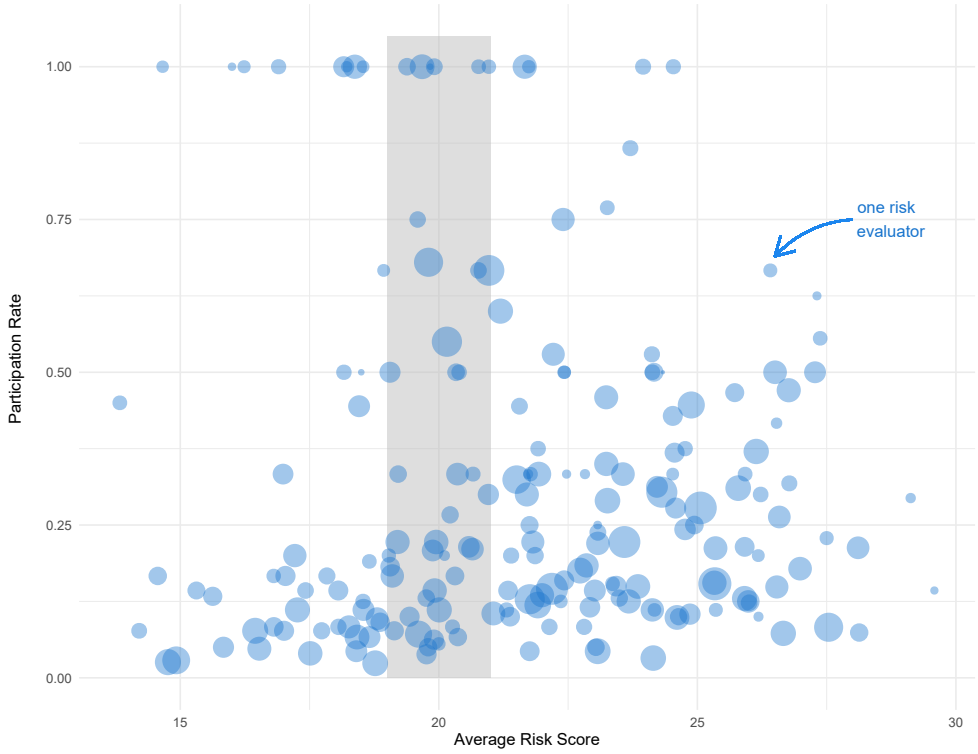
where Y_{iptc} equals one if the inmate i recidivates within a fixed period. I use alternative measures of Y according to the delay before the reoffense. In some specifications, Y_i is equal to one if the offender i recidivates within six months following his release while, in other specifications, this window is extended by up to two years. Y_{iptc} can also be other outcomes of interest, such as the number of reoffenses during a specific period of time, or the parole decision. Lastly, notice that the risk score itself cannot serve as an instrument since it correlates both with recidivism and treatment participation.[13]

[12] Here, the experience is defined as the total number of completed evaluations.

[13] In all regressions, I do not control for the risk score, since it could be correlated with the evaluator's unobserved characteristics, for instance. In a previous version of this paper, I created another instrument, namely the evaluator's average score. This measure was also found to be correlated with the treatment participation, but was removed for violations of some IV assumptions. In the main IV regressions, controlling for both the individual's risk score and the evaluator's average given risk score produces virtually identical results. Nonetheless, I use the risk score for descriptive purposes. This risk score is central to the strategy used in Arbour et al. (2020).



(a) Intuition



(b) Variation

Figure 2: Intuition for the Instrumental Variable Strategy

Notes. The intuition of the identification strategy is the following: imagine two identical inmates; they have the same observable and unobservable characteristics. The first one is randomly assigned to a probation officer who has a high propensity to recommend the program. The second one is evaluated by another agent, less prone to advise participation in the program. These two inmates are counterfactuals for each other thanks to the random assignment. In the bottom panel, I plot each evaluator's average risk score and participation rate. For a restricted window around a specific score, as the grey area on the graph, officers exhibit varying propensities to recommend the program.

3.2 Identifying Assumptions

For the instrumental regression estimation to be valid, the chosen instrument has to satisfy three central criteria. In the following section, I endeavor to succinctly summarize each of the three criteria and perform timely econometric tests. In all cases, I do not reject the validity of the instrument. I then further test the validity of $Z \in \{Z^c, Z^b\}$ with a joint hypothesis test, and with placebo checks. While it does not mean that the instrument is in fact correct, its validity is not refuted.

Random assignment. The random assignment assumption ensures that the potential outcomes (based on observable and unobservable characteristics) are not correlated with the evaluator that the inmate was assigned to. Mathematically, following the notation of Angrist et al. (1996), the triple $(Y_{0i}, Y_{1i}, D_i(z))$ must be independent from any instrument derived from j when $j(i) = 1$. This condition rules out the possibility that confounding factors coming from the probation agent are affecting the outcomes directly. In the appendix (see Section B.1), I test the random assignment assumption with a balance test across the evaluators.

I do not find compelling evidence against the random assignment assumption as no significant correlation is detected. However, to further validate the assumption, I conducted interviews with workers on the field. Following a series of discussions with an experienced probation officer, as well as with authorities from the Ministry of Public Security, a common theme emerged: whilst some evaluators specialize in certain types of offender behavior, they remain relatively impartial in their duties. Therefore, the allocation is not purely random on observable characteristics. This is not entirely surprising nor a primary concern. Assuming these characteristics are controlled for, the assignation within a group appears to be as good as random. Importantly, all variables that could drive the selection are observed in this context.

Exclusion restriction. The exclusion restriction guarantees that the only channel through which the instrument affects the outcome is via the program. For instance, with a binary instrument, it is required that

$$Y_{di}(Z_i^b = 1) = Y_{di}(Z_i^b = 0),\ \forall d = \{0, 1\}.$$

For instance, a non-participant has the same potential outcomes whether or he was initially recommended to the program ($Y_{0i}(Z_i^b = 1) = Y_{0i}(Z_i^b = 0)$). This prevents other decisions or behaviors from the evaluator that could be both correlated with the instrument and the outcomes. This could occur if the probation officer was to track the progress of the offender throughout his sentence and, even, following his release. As mentioned previously, once the evaluation is made, the probation officer transfers the case to one of her colleagues without following-up. In addition, the inmate hardly interacts with the officer responsible of the evaluation as only between two and three hours are necessary to fill out the questionnaire. It seems implausible 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 quality of the evaluator given they have minimal contact.

I formally test the exclusion restriction with a newly developed test by Frandsen et al. (2019). The intuition for the test as well as the results are presented in the appendix (see Section B.2). Overall, I fail to reject the null hypothesis that the exclusion restriction hypothesis is violated. Nevertheless, there are some exceptions: in the prisons of Montreal, St-Jerome and Trois-Rivieres, the evaluators appear to have a direct impact on outcomes beyond their *Parcours* recommendation. It is important to note, however, that these prisons are among the largest in Quebec, and therefore

employ a large number of evaluators compared to smaller incarceration centers. Thus, it might only be statistical noise at this point since this is a very strict test. Further, I will show that the main results hold even when excluding all the observations coming from these prisons.

However, another challenge remains: the probation officer also recommends other programs and correctional measures to the inmate. This is problematic if an evaluator's propensity to recommend *Parcours* is correlated to her propensity to recommend other programs to which I do not observe the participation records. If this is the case, then, I may attribute to *Parcours* the benefits from other programs. In the appendix (see Appendix B.5), I present suggestive evidence that the estimates do not change after controlling for the probation agent's propensity to recommend other programs[14] nor are they affected when controlling for the agents' experience. Finally, I leverage the fact that the program is not offered on a continuous basis, as a minimum number of participants is required. Intuitively, inmates who are incarcerated while the program is not in effect should not be influenced by the instrument.[15] This is exactly what I further find in Appendix B.5.

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 introduced to the program. To illustrate, consider two probation officers (j and w) who have different propensities to suggest the program. j recommends the program 100% of the time, while w recommends it in only 50% of her cases. Hence, the monotonicity assumption tells us that if $w(i) = 1$ and $D_i = 1$, then $D_i = 1$ if $j(i) = 1$. More generally: $\forall(j, w)$ and $\forall i$, either $D_i(j) \geq D_i(w)$ or $D_i(j) \leq D_i(w)$, where $D_i(k)$ is the decision of inmate i to participate or not following his evaluation with probation officer k. The monotonicity assumption ensures that the local average treatment effect can be interpreted as *local*.

The monotonicity assumption cannot be formally tested on its own given that it is impossible to distinguish between defiers and never-takers or between defiers and always-takers. However, it has testable implications, namely (1) evaluators' propensities have a monotonic effect on the inmates' participation decision, and, (2) the first stage estimates (say, from a regression of D on Z) must be positive for every slice of the data. To test the twofold assumption, I employ similar approaches as Frandsen et al. (2019), Arteaga (2019) and Bhuller et al. (2020). The results are presented in the appendix (see Section B.3). Overall, I find no evidence against the monotonicity assumption. All the first stage estimates are positive, and the relationship between the treatment status and the instrument is weakly monotonic and increasing.

Joint hypotheses. Interestingly, econometricians have recently devised with ways to jointly test the three hypotheses for the instrument validity (random assignment, exclusion restriction and monotonicity). In particular, Kitagawa (2015) and Mourifié and Wan (2017) developed tests built on testable implications of the instrument's validity. I have chosen to implement Kitagawa's test since it is well suited for discrete outcomes and can conveniently be extended to the case with additional covariates. I present an intuition for the test followed by the results in the appendix (see Section B.4).

When testing Z^b, I do not reject the null that the instrument is valid, despite no controls being taken into account. On the contrary, when testing Z^c, controlling for covariates appears primordial. Finally, it is worth noting that Kitagawa's procedure does not test for the relevance of the instru-

[14] Participation data for other programs than *Parcours* is only available for three prisons.

[15] I thank Professor Kevin Schnepel for suggesting this test.

Table 3: OLS - Effect of Participation on Recidivism

	Dependent variable: recidivism within...					
	6 months		1 year		2 years	
	(3.1)	(3.2)	(3.3)	(3.4)	(3.5)	(3.6)
Program ($\hat{\delta}$)	-0.0565	-0.0593	-0.0612	-0.0583	-0.0389	-0.0362
(s.e)	(0.0122)	(0.0120)	(0.0162)	(0.0159)	(0.0197)	(0.0194)
[95% CI]	[-0.0805, -0.0325]	[-0.0828, -0.0358]	[-0.0931, -0.0294]	[-0.0895, -0.0272]	[-0.0773, -0.0004]	[-0.0741, 0.0018]
Outcome Mean	0.19	0.19	0.30	0.30	0.42	0.42
Controls		✓		✓		✓
Clusters	✓	✓	✓	✓	✓	✓
N	6,012	6,012	5969	5969	5747	5747

Notes. This table reports the results from the naive OLS regressions. The robust standard errors are in parentheses and the confidence intervals, in brackets. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. Standard errors are clustered at the prison-year-evaluator level. In columns (3.1) and (3.2), the dependent variable is an indicator for recidivism within six months. In columns (3.3) and (3.4), the dependent variable is an indicator for recidivism within one year. In columns (3.5) and (3.6), the dependent variable is an indicator for recidivism within two years. *Takeaway.* At face value, participants recidivate at a lower rate than non-participants, however, the difference could stem from the program itself or from confounding factors. These confounding factors were not taken into account in this estimation.

ment (the effect of Z on D). This condition is generally empirically verified using the rule of thumb that the F-statistic from the first stage regression be more than 10 (Staiger and Stock (1994)), although recent evidence shows that the F statistic should in fact be larger than 104. When the F statistic is lower than 104, I implement the correction procedure proposed in Lee et al. (2020).

4 Results

In this section, I present the results from various specifications of the primary regression. To start, I investigate the program's effect on the probability of recidivating within a fixed period of time. I then study other outcomes of interest, such as the number of reoffenses and the probability of being granted an early release. I begin with naive regressions, in which the selection bias is not accounted for, and then use the instrumental variable strategy to derive causal estimates.

4.1 The Effect of Participation on Recidivism

4.1.1 Naive Regressions

I estimate by ordinary least squares (OLS, hereinafter) the following linear probability regression:

$$Y_{iptc} = \alpha + \lambda_p + \lambda_t + \lambda_c + \delta D_i + X_i'\beta + \epsilon_{iptc},$$

where Y_{iptc} is equal to one if inmate i recidivates within a fixed period of time; a period that accounts for 6 months to two years following release. λ's are fixed effects for the prison, the year, and the type of crime. The vector X_i' contains the age (and the age squared) of the individual, the number of prior convictions, a dummy for being part of an Indigenous population, and an indicator for a violent crime. D_i is the variable of interest and is an indicator of participation in *Parcours*. The estimates of δ are reported in Table 3.

In columns (3.1) and (3.2), I consider recidivism within a six-month time frame. With or without controlling for several covariates, it is estimated that the participants' recidivism rate is 6 percentage

points (or 32%) lower than that of non-participants. This difference is significant at the 0.1% level. The standard errors are clustered at the prison-year-evaluator level. Similar conclusions are reached when considering recidivism within one year: the numbers in columns (3.3) and (3.4) reveal that the recidivism rate of participants is, again, 6 percentage points (or 20%) lower than that of non-participants; a difference that is still significant at the 0.1% level. The effect drops by 2 percentage point when increasing the observation window to two years, and the effect becomes only marginally significant.

The results could hint at *positive* selection. In the case of positive selection, offenders that are, on average, less inclined to recidivate are those who integrate into the program. I would argue that the selection's direction remains unclear: the negative point estimates could stem from the program being beneficial to most inmates and do not necessarily imply positive selection. For instance, in column (3.6), I discern no significant effect of the program, which could very well be explained by *negative* selection. The observed difference of 6 percentage points could result from the program itself or from confounding factors, such as motivation, ability or remorse. The program's causal effects will be isolated from the self-selection bias in the further sections.

4.1.2 Instrumental Variables Regressions

In the following section, I use the instrumental variable design to arrive at causal estimates of the impact of the program on recidivism. For convenience, I recall the estimation steps here:

$$\begin{aligned} \text{(first stage)} \quad & D_{iptc} = \alpha + \lambda_p + \lambda_t + \lambda_c + \rho Z_i + X_i'\beta + \eta_{iptc} \\ \text{(second stage)} \quad & Y_{iptc} = \zeta + \lambda_p + \lambda_t + \lambda_c + \tau \hat{D}_i + X_i'\beta + \epsilon_{iptc}. \end{aligned}$$

I will also present the results from the *reduced form* regressions, that is, regressions of the outcome (Y_{iptc}) on the instrument (Z_i):

$$\text{(reduced form)} \quad Y_{iptc} = \gamma + \lambda_p + \lambda_t + \lambda_c + \omega Z_i + X_i'\beta + \upsilon_{iptc}.$$

Z_i is representative any of the instruments defined previously, specifically either the participation rate per evaluator, Z_i^{c}, or the indicator of whether an evaluator is of low or high-propensity, Z_i^{b}. The first set of results is presented in Table 4, where I use Z_i^{c} to instrument participation.

In column (4.1), I estimate, without any controls, a marginally significant effect of -8 percentage points. However, when adjusting the estimation with the set of controls in column (4.2), I find a precisely estimated effect: participating in *Parcours* decreases the probability to reoffend by 15 percentage points within six months following release. In an effort to broaden this study, I extend the recidivism window by up to two years following an inmate's release. I observe very similar patterns across all specifications. I find that participation in the program decreases the likelihood of recidivism by 19 and 17 percentage points within one and two years following the release, respectively. Therefore, the program's effects materialize in the short term, and last for at least two years. It is important to further evaluate three key takeaways from these results.

Firstly, the numbers in Panel A from Table 4 show the reduced form estimates and suggest that evaluators' recommendation propensities have a sound effect on recidivism. In most specifications,

Table 4: IV - Effect of Participation on Recidivism

	Dependent variable: recidivism within...					
	6 months		1 year		2 years	
	(4.1)	(4.2)	(4.3)	(4.4)	(4.5)	(4.6)
Panel A: Reduced Form Estimates						
Instrument ($\hat{\omega}$)	-0.0590	-0.0785	-0.1035	-0.1024	-0.0913	-0.08916
(s.e)	(0.0327)	(0.0345)	(0.0377)	(0.0395)	(0.0421)	(0.0445)
[95% CI]	[-0.1232, 0.0052]	[-0.1462, -0.0108]	[-0.1775, -0.0295]	[-0.1799, -0.0249]	[-0.1737, -0.0088]	[-0.1765, -0.0018]
Panel B: Second Stage Estimates						
Program ($\hat{\tau}$)	-0.0798	-0.1453	-0.1394	-0.1885	-0.1223	-0.1651
(s.e.)	(0.0441)	(0.0639)	(0.0507)	(0.0736)	(0.0560)	(0.0824)
[95% CI]	[-0.1662, 0.0065]	[-0.2705, -0.0201]	[-0.2388, -0.0399]	[-0.3329, -0.0442]	[-0.2321, -0.0125]	[-0.3265, -0.0037]
Panel C: First Stage Statistics						
KP-F [c.v. = 104.7]	455.77	154.16	459.49	154.68	434.95	141.21
tF 95% CI	†	†	†	†	†	†
Outcome Mean	0.19	0.19	0.30	0.30	0.42	0.42
Controls		✓		✓		✓
Clusters	✓	✓	✓	✓	✓	✓
N	5929	5929	5886	5886	5669	5669

Notes. This table reports the results from the IV regressions of the probability to recidivate on program participation. The robust standard errors are in parentheses and the confidence intervals, in brackets. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. Standard errors are clustered at the prison-year-evaluator level. In columns (4.1) and (4.2), the dependent variable is an indicator for recidivism within six months. In columns (4.3) and (4.4), the dependent variable is an indicator for recidivism within one year. In columns (4.5) and (4.6), the dependent variable is an indicator for recidivism within two years. KP-F reports the Kleinergen-Paap F statistics. †: the tF procedure is not necessary since the F statistic is larger than the critical value. *Takeaway.* I detect large, significant and negative effects of the program on recidivism for all the periods considered. The high F statistics indicate that the estimation does not suffer from the weak IV bias.

I find large and significant reduced form estimates. Panel C from Table 4 provides evidence that the instrument is a strong predictor for participation in *Parcours*. I first report the Kleinbergen-Paap Wald F statistics from the first stage regressions. This statistic is robust to heteroskedasticity and to serial correlation (Kleibergen and Paap, 2006; Kleibergen, 2007). It is equivalent to the effective F statistic developed in Olea and Pflueger (2013), who derived critical values for a weak instrument test. All then F statistics in Table 4 are larger than these critical values.[16] One must be prudent with the treatment effect's inference, even if the estimation does not appear to suffer from the weak instrument bias. Lee et al. (2020) argue that valid inference around the treatment effects necessitates an F statistic larger than 104.7, which is the case here. Therefore, the *tF* procedure developed in Lee et al. (2020) is not deemed necessary for this set of results.

Secondly, the estimated treatment effects may seem implausibly high for a program that lasts a total of 24 hours, albeit similar-in-magnitude estimates can be found in the recent literature (see Zanella, 2020). The large estimates could be explained by the fact that the program was developed for inmates serving short sentences and that a specific population was targeted by its creators. I would argue that the rehabilitation success for inmates with such short sentences is highly plausible. Given that the program is aimed at curbing one's criminal trajectory, including positively impacting an inmate's decision-making process and accountability, it is plausible that inmates will alter their tendencies towards crime to some degree both during and after program participation. An alternative explanation for the large estimates stems from the variance in the estimates: although the confidence intervals are narrow enough, so as not to include zero, and hence provide statistically significant estimators, they still cover a wide range of possible true treatment effects.

Thirdly, it is worth addressing the substantial gaps between the OLS and the IV results. The OLS regressions delivered small treatment effects, between 3 and 6 percentage points, and, when the observation window was set to two years, the effect was not significantly different from zero. In contrast, the IV regressions yielded large and significant effects for all considered outcomes. One explanation for this gap can be attributed to the selection bias. The results above are consistent with *negative* selection - inmates with a higher propensity to recidivate enroll in the program - resulting in a small or null treatment effect when using OLS. However, once the selection bias is accounted for in the IV setting, participation in the program significantly reduces recidivism. This implies that the OLS results can be seen as upper bounds of the true treatment effect, and can be interpreted as such. Still, it can be argued that the IV regressions are of particular importance to identify the results from rehabilitation efforts at such a large scale. A further possible reason to explain the gaps can be ascribed to dissimilar parameters being identified by both strategies. While OLS regressions estimate an average treatment effect (biased, in this context), IV regressions estimate a local average treatment effect on the subpopulation of compliers, that is, inmates who are influenced by the instrument. Overall, the populations considered might not be the same. A combination of both effects - the selection and the compliers effects - is conceivable. The question remains, however, *who* are the compliers?

4.1.3 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

[16] Olea and Pflueger (2013) show that the F statistic should be larger to 37.42 for a worst case bias of 5%, and larger than 23.11 for a worst case bias of 10%.

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 easily identified. I do so using the recent technology developed in Marbach and Hangartner (2020) and Kennedy et al. (2020). I endeavour to provide an overview of the methodology, but I frequently refer to the original papers for detailed explanations.

Marbach and Hangartner (2020) observe that the group of always-takers can be divided into two groups: observable nonencouraged always-takers and unobservable always-takers. Whilst one can estimate the mean of a covariate X for observable nonencouraged always-takers, $\mathbb{E}(X|D=1, Z^{\text{b}}=0)$, the covariate mean for encouraged always-takers, $\mathbb{E}(X|D=1, Z^{\text{b}}=1)$, is not directly identified since it is confounded with that from treated compliers. However, when the instrument is random, the distribution of X is the same across observable and unobservable always-takers. The same principle applies to never-takers. Marbach and Hangartner (2020) demonstrate how the mean of X for the population of compliers is identified from the data:

$$\mathbb{E}(X|\text{compliers}) = \frac{\overbrace{\mathbb{E}(X)}^{\text{all sample}} - \overbrace{\mathbb{E}(X|D=1, Z^{\text{b}}=0)\pi_{\text{at}}}^{\text{obs. always-takers}} - \overbrace{\mathbb{E}(X|D=0, Z^{\text{b}}=1)\pi_{\text{nt}}}^{\text{obs. never-takers}}}{\underbrace{\mathbb{E}(D|Z^{\text{b}}=1) - \mathbb{E}(D|Z^{\text{b}}=0)}_{\text{first stage}}}.$$

I present the results of this exercise in Figure 3. I use Z^{b} as an instrument and compute the standard errors with 1000 bootstrap replications. I measure the sample mean of various characteristics[17] for compliers, always-takers and never-takers. The results suggest that compliers are young, have a low-medium risk score and are more likely to have committed a crime against a person. Such profiling is crucial to understand the gap between the OLS and IV regressions: younger inmates, as well as offenders who committed a crime against a person, are more prone to recidivate. Thus, by complying with the program recommendation, these groups whittle the overall program's effect down, hence implying a stronger treatment effect.

This exercise has an important caveat: observable characteristics are considered separately.[18] To estimate compliance scores in a robust fashion, I employ the insights of Kennedy et al. (2020). Denote the decision to participate under the instrument Z as $D_i(Z)$, and define a latent indicator C_i that is equal to 1 if inmate i is a complier and equal to 0 otherwise, i.e. $C_i = \mathbb{1}\{D_i(1) > D_i(0)\}$. The goal is to identify the compliance score function, $\mathbb{P}(C=1|X=x)$. The authors represent the equation of interest in the following fashion, and propose a nonparametric estimation method.

$$\begin{aligned}\gamma(x) &= \mathbb{P}(C=1|X=x) \\ &= \mathbb{P}(D=1|X, Z^{\text{b}}=1) - \mathbb{P}(D=1|X, Z^{\text{b}}=0).\end{aligned}$$

I calculate the fraction of compliers for each age in the sample. More precisely, I define a complier as an individual with an estimated individual compliance score ($\hat{\gamma}$) greater than the median compliance score, which is approximately 0.15. The results of this exercise are shown in Figure 4. The fraction

[17] Here, I include the risk score as a covariate, the reason being that it could potentially serve a quickly available compliance measure.

[18] It could as well be that young offenders are more likely to commit crimes against a person, for instance.

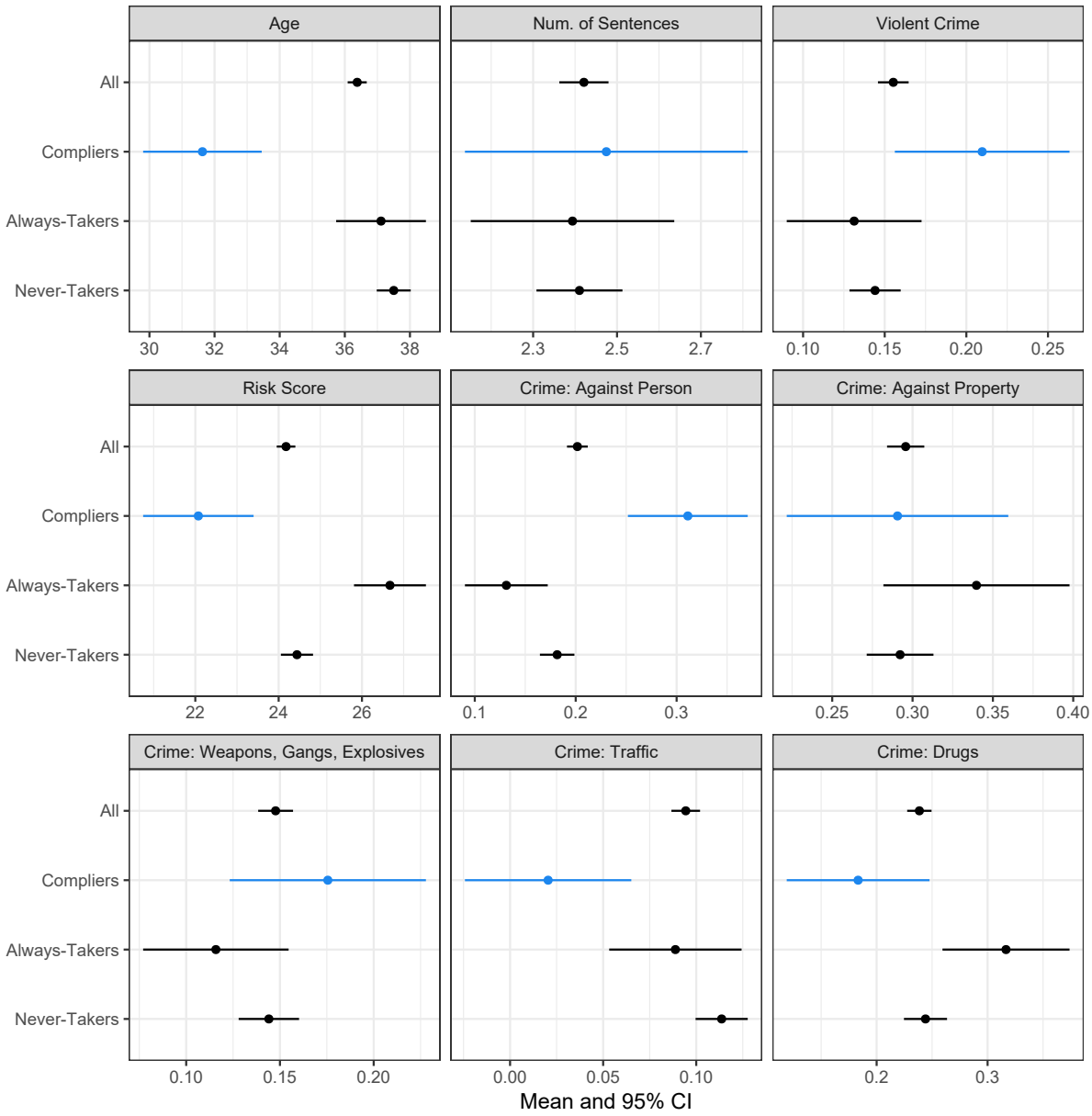


Figure 3: Complier and Noncomplier Populations

Notes. This figure implements the compliers profiling strategy developed in Marbach and Hangartner (2020). *Takeaway.* I find that compliers are most likely young and have a medium risk score. They are also more likely to be convicted of a crime against a person.

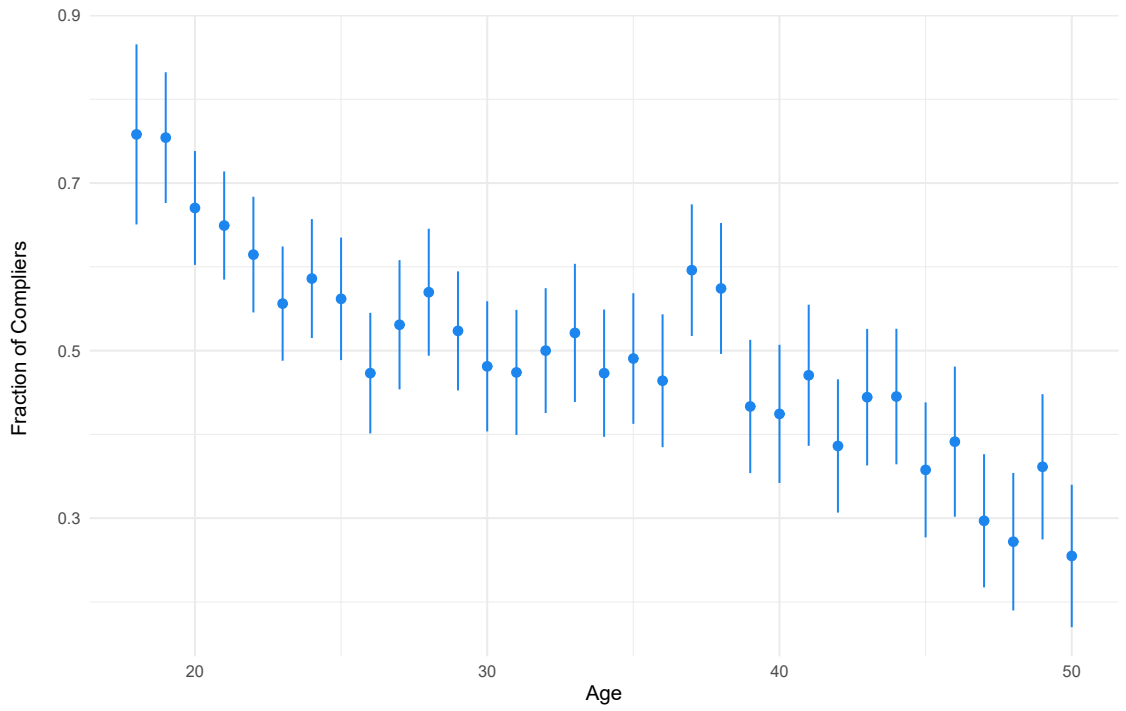


Figure 4: Fraction of Compliers, by Age

Notes. This figure implements the compliers profiling strategy developed in Kennedy et al. (2020). For visualization purposes, I only display age groups with at least 30 observations, and thus cut the sample at age 50. *Takeaway.* I find that the fraction of compliers if decreasing with age, hinting at the young population being most likely to comply with the evaluators' recommendation.

of compliers is large for young inmates, but gradually decreases as age increases. Next, I show the distribution of $\hat{\gamma}(\text{Crime}, \text{Age})$ and $\hat{\gamma}(\text{Crime}, \text{Risk Score})$ on the bottom and top panels of Figure 5 respectively. Each inmate is represented by one dot. A light grey dot is an inmate with a low compliance score, whereas inmates represented by a blue dot have a higher compliance score; in other words, they are likely to be affected by the instrument. The results are twofold: first, young inmates have higher compliance scores for all types of crime especially for crimes against property, the only exception being traffic-related crimes that prove less common across young inmate populations. Second, compliance scores are relatively heterogeneous with respect to the risk scores as there is no clear pattern. Thus, this exercise partially validates the results obtained above: compliers are most likely young inmates regardless of the type of crime committed. However, the instrument is be *sharp* enough - in the sense defined in Kennedy et al. (2020) - to predict compliers based on the risk score.

4.1.4 Robustness

The results presented in Section 4.1.2 are robust to many alterations of the baseline specifications, including whether I include demographics (X_i') and fixed effects (λ's), and whether I alter the clustering level. I run further robustness checks and present the main results in Table 5. For all specifications, I now focus on recidivism within one year. The full set of controls is included in each regression.

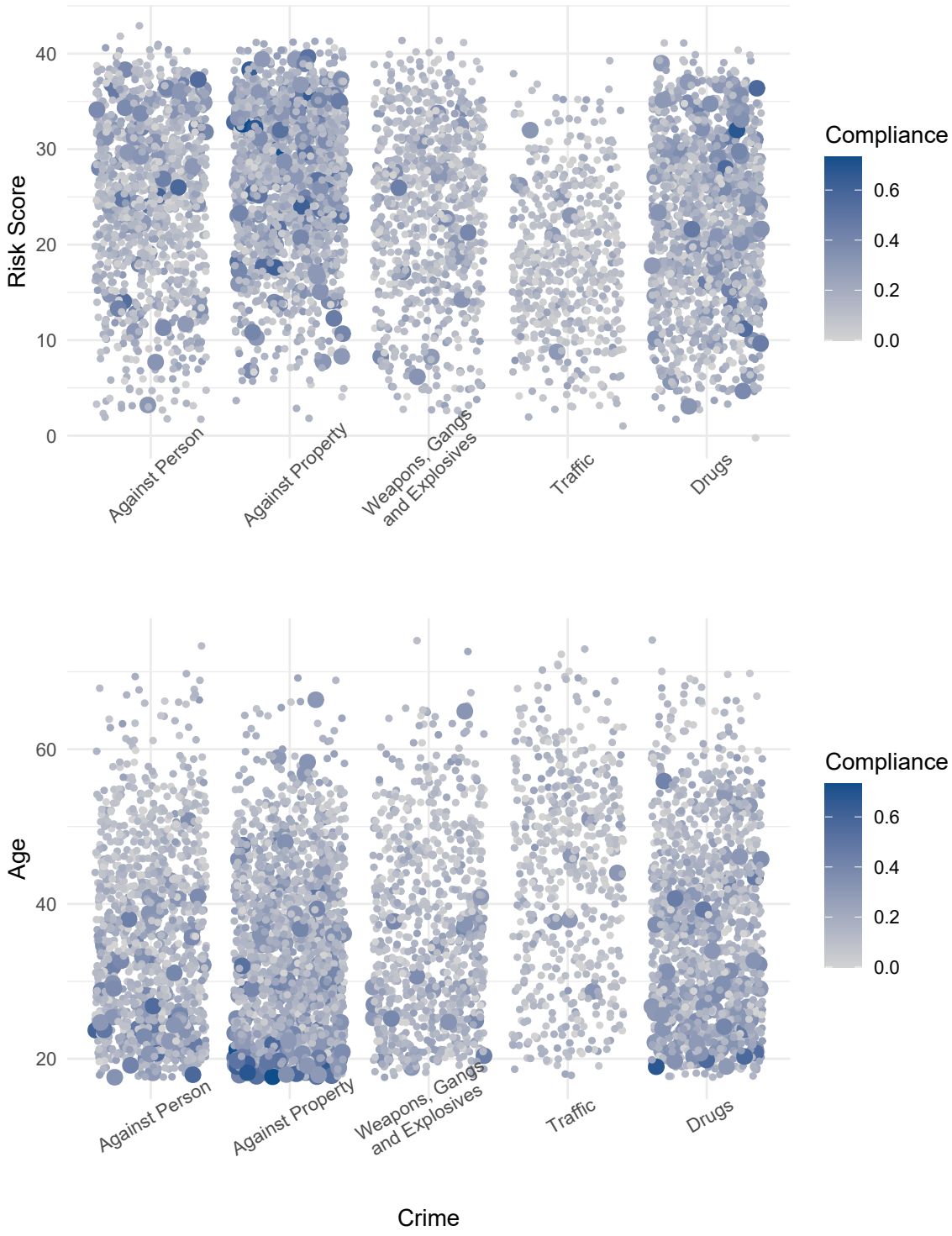


Figure 5: Compliance Level by Crime, Age and Risk Score

Notes. This figure implements the compliers profiling strategy developed in Kennedy et al. (2020). *Takeaway.* I find that compliers are most likely young irrespective of the crime committed.

Table 5: IV: Robustness Checks

	Dependent variable: recidivism within 1 year			
	Instrument: Z^c			Instrument: Z^b
	(5.1)	(5.2)	(5.3)	(5.4)
Panel A: Second Stage Estimates				
Program ($\hat{\tau}$)	-0.3152	-0.3607	-0.2505	-0.1842
(s.e)	(0.1105)	(0.1462)	(0.0972)	(0.0973)
[95% CI]	[-0.5318, -0.0986]	[-0.6472, -0.0742]	[-0.4413, -0.0597]	[-0.3662, -0.0023]
Panel B: First Stage Statistics				
KP-F [c.v. = 104.7]	69.12	25.31	69.33	76.67
tF 95% CI	[-0.5428, -0.0876]	[-0.7101, -0.0113]	[-0.4510, -0.0500]	[-0.3727, 0.0042]
Subset	$0 < Z^c < 1$	Sentence > 6 months	8 prisons	
Outcome Mean	0.3	0.22	0.32	0.3
Controls	✓	✓	✓	✓
Clusters	✓	✓	✓	✓
N	4407	3390	4090	5886

Notes. This table reports the results from numerous robustness checks. The robust standard errors are in parentheses and the confidence intervals, in brackets. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. Standard errors are clustered at the prison-year-evaluator level. In all specifications, the dependent variable is an indicator for recidivism within one year following release. In column (5.1), I restrict the domain of Z^c between 0 and 1 excluded. In column (5.2), I only consider inmates with sentences exceeding six months. In column (5.3), I remove the observations from three facilities, Montreal, St-Jerome and Trois-Rivieres, for which the Frandsen et al. test suggested violations of the exclusion restriction. In column (5.4), I instrument participation with Z^b. KP-F reports the Kleinergen-Paap F statistics. Finally, I report the 95% confidence interval obtained with the tF procedure. *Takeaway.* By all accounts, the results appear to be robust to several specification choices although they are not as precise as with the baseline regressions.

In column (5.1), I estimate the baseline regression, but I remove the observations for which the instrument is either 0 or 1. In doing so, I remove evaluators who never or always recommend participation in the program. I find that the point estimate is even larger, in absolute value, than that from the baseline regression and remains highly significant. However, the F statistic drops to 69.12 and, thus, inference might not be valid as evidenced by Lee et al. (2020). I implement their *tF* procedure to compute a valid confidence interval and find a slightly wider tutorial that does not include zero.[19]

I then estimate the main equations on diverse subsets of the data. In column (5.2), I limit my estimation to the inmates with a sentence longer than six months, in other words, those whom the program was initially developed for. The result is larger than that obtained from the entire sample and is significant at the 5% level. In column (5.3), I remove the observations from the prisons of Montreal, St-Jerome and Trois-Rivieres. In earlier segments of this paper, evidence suggested (using Frandsen et al.'s test for the exclusion restriction) that probation officers from these three facilities affected the outcome directly, that is, through channels beyond their propensity to recommend the program. I find virtually unchanged results: participation to *Parcours* decreases recidivism by 23 percentage points, a result that is also significant at the 5% level. I find comparable results when I use Z^{b}, the binary version of Z^{c}, as an instrument in column (5.4). However, the estimation is less precise and the confidence interval computed with the *tF* procedure does not preclude zero.

4.2 The Effect of Participation on the Number of Reoffenses

Thus far, the results have shown that the program is successful at reducing the likelihood of recidivism within two years following an inmate's release. One might wonder if participating in the program reduces the number of repeated offenses after the inmate exits prison. Consider a new dependent variable, Y_i^R, which is equal to the number of sentences within one year following the current conviction of inmate i. In the data, Y^R ranges from 0 (if the inmate does not recidivate) to 7. Recall that the prescribed sentences under study are within the time frame of two years, and thus, it is not uncommon for an individual to serve more than a handful of sentences during a certain period of time.

Firstly, I estimate linear regressions of the following form:

$$Y_{iptc}^R = \zeta + \lambda_p + \lambda_t + \lambda_c + \tau D_i + X_i'\beta + \epsilon_{iptc},$$

with and without an instrument for D_i. Secondly, to account for Y^R being a discrete variable, I also estimate Poisson regressions.

In the first two columns of Table 6, I do not account for the selection bias. I find that participation in the program reduces the number of reoffenses by between 0.12 and 0.11.[20] The results are all significant at the 0.1% level. When I instrument for participation, I find even larger effects. In column (6.3), I estimate a decrease in the number of repeated crimes by 0.35. In column (6.4), the effect jumps -0.55 crimes per year. The gaps between the naive and the IV regressions estimates mirror my previous findings with the linear probability models - larger effects are found after controlling for selection. Aside from negative selection, these gaps could reflect that compliers are most likely young offenders; those who the literature defines as most at-risk, and who tend to reoffend more frequently than their older counterparts (Doleac, 2019).

[19] I refer to Lee et al. (2020) for the intuition and procedure.

[20] For the Poisson regressions, I report the raw coefficients. To interpret such numbers, it is useful to multiply them by the sample mean. For instance, the average partial effect for the model in column (6.2) is $-0.2580 \times 0.42 = -0.1084$.

Table 6: Effect of Participation on the Number of Reoffenses

	Dependent variable: number of reoffenses within 1 year			
	No Instrument		Instrument: Z^c	
	(6.1)	(6.2)	(6.3)	(6.4)
Model	OLS	Poisson	IV	Poisson
Panel A: Estimates				
Program ($\hat{\tau}$)	-0.1218	-0.2580	-0.3502	-1.3103
(s.e.)	(0.0236)	(0.0670)	(0.1252)	(0.3158)
[95% CI]	[-0.1680, -0.0756]	[-0.3893, -0.1267]	[-0.5955, -0.1049]	[-1.9293, -0.6914]
APE		-0.1084		-0.5503
Panel B: First Stage Statistics				
KP-F [c.v. = 104.7]			153.91	
tF 95% CI			†	
Outcome Mean	0.42	0.42	0.42	0.42
Controls	✓	✓	✓	✓
Clusters	✓	✓	✓	✓
N	5986	5986	5903	5903

Notes. This table reports the results from various regressions in which the dependent variable is the number of reoffenses following the current sentence. The robust standard errors are in parentheses and the confidence intervals, in brackets. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. Standard errors are clustered at the prison-year-evaluator level. In the first two columns, I do not take into account the selection bias. In column (6.1), I estimate a standard OLS regression. In columns (6.2), I estimate a Poisson regression. In the last two columns, I instrument participation with Z^c. In column (6.3), I run a standard instrumental variable regression. Finally, in column (6.4), I estimate an instrumental variable Poisson regression. KP-F reports the Kleinergen-Paap F statistics. †: the tF procedure is not necessary since the F statistic is larger than the critical value. *Takeaway.* The results show that the program decreases the number of subsequent offenses. The results are stronger when the selection bias is corrected.

4.3 The Effect of Participation on Parole Decision

As we reflect on the intricacies of Parcours, the question remains, *why* is program effective? In other words, what are the ways and channels in which the program directly impacts the likelihood of recidivism? As previously stated, the primary, ultimate goal of the program is to reduce the rate of recidivism, although it is worth exploring further potential beneficial outcomes as a useful indicator of the program's mechanism. For instance, the program could improve the behavior of the participant while he is incarcerated or it could strengthen positive relationships both inside and outside the prison setting. In the following section, I examine whether participants are more likely to request and to subsequently be granted parole after they have participated in *Parcours*.

Parole, derived from the French word *parole* (promise), grants an inmate an early release from incarceration under the assumption that the remainder of their sentence can be served under supervision in the community. Emerging research on public safety and corrections suggests that parole contributes to public safety by favouring a gradual, supported, and controlled transition between a highly organized and restrictive setting (prison) to a more complex, active and unrestricted environment (society). This transition is facilitated by way of conditions that the parole board imposes, most often ranging from the restriction of drugs and alcohol, to positive stipulations that require inmates to seek employment or meetings with parole officers. These conditions guide and ease the transition of the former inmate by managing their social reintegration into society.

In Quebec, inmates are required to formally seek parole, although no release will occur prior to clearance by the governing parole board. Indeed, the parole board must then determine whether parole should be granted, and if so, the conditions on which an inmate must abide. Similar to the previous regressions, I estimate linear probability models in which the dependent variable is a dummy variable that is equal to one if the inmate requests parole. In a second specification, the dependent variable is an indicator for parole being granted. The results are outlined in Table 7.

Unsurprisingly, when I estimate the regression by ordinary least squares, I find that participants are more likely to request parole. Once the selection is accounted for, the effect is imprecisely estimated. Similarly, I find no difference in the probability of being granted parole between participants and non-participants regardless of whether I use the instrument or not. The imprecision could stem from a lack of variation in the dependent variable since parole is sought and granted in few cases. Nevertheless, these findings speak to the causal mechanism of the program on recidivism: it would appear that the program does not improve one's chances to ensure an early release, at least, not convincingly. Arguably, the program's transmission channel lies mainly in the dynamic criminogenic factors that it aims to remedy. By altering the attitude of the offender towards crime or by positively contributing to his ability to handle day-to-day situations, *Parcours* offers tangible solutions to convicts and probation officers seeking to reduce and prevent criminal behavior. In contrast, the program's content does not seem wholly sufficient to convince the parole board members that the participant is prepared for an early release. Finally, the lack of effect could be purely mechanical: the effects of *Parcours* were not demonstrated until now. It appears unlikely that parole board members would base their decision on a program whose effects were uncertain at the time.

4.4 Do Recidivists Change Behavior?

The results, thus far, have demonstrated that the program significantly reduces the probability of recidivating, among other outcomes. Nevertheless, a fraction of participants *do* recidivate, but could be affected in other ways, such as a change in the type of crimes committed or their severity.

Table 7: Effect of Participation on Seeking and Being Granted Parole

	Dependent variable: seek parole		*Dependent variable: granted parole*	
	(7.1)	(7.2)	(7.3)	(7.4)
PANEL A: ESTIMATES				
Program	0.0849	0.0510	0.0081	0.0411
(s.e.)	(0.0142)	(0.1164)	(0.0067)	(0.0608)
[95% CI]	[0.0571, 0.1127]	[-0.1771, 0.2791]	[-0.0049, 0.0211]	[-0.0781, 0.1603]
PANEL B: FIRST STAGE STATISTICS				
KP-F [c.v. = 104.7]		26.51		26.51
tF 95% CI		[-0.2318, 0.3338]		[-0.1067, 0.1889]
Outcome Mean	0.10	0.10	0.03	0.03
Controls	✓	✓	✓	✓
Clusters	✓	✓	✓	✓
N	3451	3451	3451	3451

Notes. This table reports the results from naive (columns 7.1 and 7.3), and IV (columns 7.2 and 7.4) regressions where the dependent variable is either an indicator that equals one if the inmate sought parole (and zero otherwise) or if he was granted parole (and zero otherwise). The robust standard errors are in parentheses and the confidence intervals, in brackets. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. Standard errors are clustered at the prison-year-evaluator level. KP-F reports the Kleinergen-Paap F statistics. Finally, I report the 95% confidence interval obtained with the tF procedure. *Takeaway. Parcours* does not seem to impact the probability that an individual applies for parole or that he is granted parole. This suggests that parole is not the main mechanism through which the program impacts the recidivism likelihood.

Table 8: IV - Effect on Recidivists

Dependent variables:	*Next Crime: Assault*	*Next Crime: Violent*	*Next Crime: Prison*	*Delay Before Reoffense*
	(8.1)	(8.2)	(8.3)	(8.4)
Panel A: Second Stage Estimates				
Program	0.0467	-0.0188	-0.0198	164.18
(s.e)	(0.0776)	(0.0559)	(0.0930)	(122.15)
[95% CI]	[-0.1054, 0.1988]	[-0.1285, 0.0908]	[-0.2021, 0.1626]	[-75.22, 403.59]
Panel B: First Stage Statistics				
KP-F [c.v. = 104.7]	95.58	95.58	93.31	95.58
tF 95% CI	[-0.1070, 0.2004]	[-0.1296, 0.0919]	[-0.2049, 0.1654]	[-77.78, 406.03]
Outcome Mean	0.16	0.11	0.77	530.69
Controls	✓	✓	✓	✓
Clusters	✓	✓	✓	✓
N	3313	3313	3241	3313

Notes. This table reports regressions studying behavioral changes among recidivists, inmates who will reoffend during the follow-up period. The robust standard errors are in parentheses and the confidence intervals, in brackets. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. Standard errors are clustered at the prison-year-evaluator level. In column (8.1), the dependent variable is an indicator for the next crime being an assault. In column (8.2), the dependent variable is an indicator for the next crime being violent in nature. In column (8.3), the dependent variable is an indicator for the next crime leading to an incarceration sentence. In column (8.4), the dependent variable is the number of days elapsed before the next offense. KP-F reports the Kleinergen-Paap F statistics. Finally, I report the 95% confidence interval obtained with the tF procedure. *Takeaway.* I discern no evidence of recidivists changing their behavior. However, the time elapsed before a reoffense appears longer, although it is not really precise.

With the ensuing results, I closely examine whether the program had any lasting and substantial effects on repeat offenders' outcomes. I estimate a number of linear probability and OLS regressions in which only repeat offenders are considered. The findings are presented in Table 8.

Overall, I find no convincing evidence of reoffenders changing their behavior. Namely, I do not find changes in the probability to commit an assault (column (8.1)), to commit a less violent crime (column (8.2)) or that the subsequent crime leads to an incarceration sentence (column (8.3)), as opposed to a sentence served within the community. There appears to be an effect, however, in the delay prior to the reoffense. I estimate that the program delays the next offense by roughly 165 days, although this measure is relatively noisy.

In light of this result, I augment my analysis by estimating the *contemporaneous* effect of the program; estimating the effect of the program on the number of reoffenses during time intervals following an inmate's release. The estimates are plotted in Figure 6. The first point estimate shows a large decrease in the number of reoffenses directly following release. The number of reoffenses, however, appears to increase between the second and the third year post-release with a comparable magnitude. This indicates that the program shifts the timing of reoffenses by around two years. For instance, it could be that the program provides the participants with the necessary tools to find work, but not necessarily the tools to secure employment in the long term.

4.5 Heterogeneity

The following section endeavors to examine the data in an effort to better understand the role of heterogeneity in the treatment effects. A series of papers (Athey and Imbens, 2016; Wager

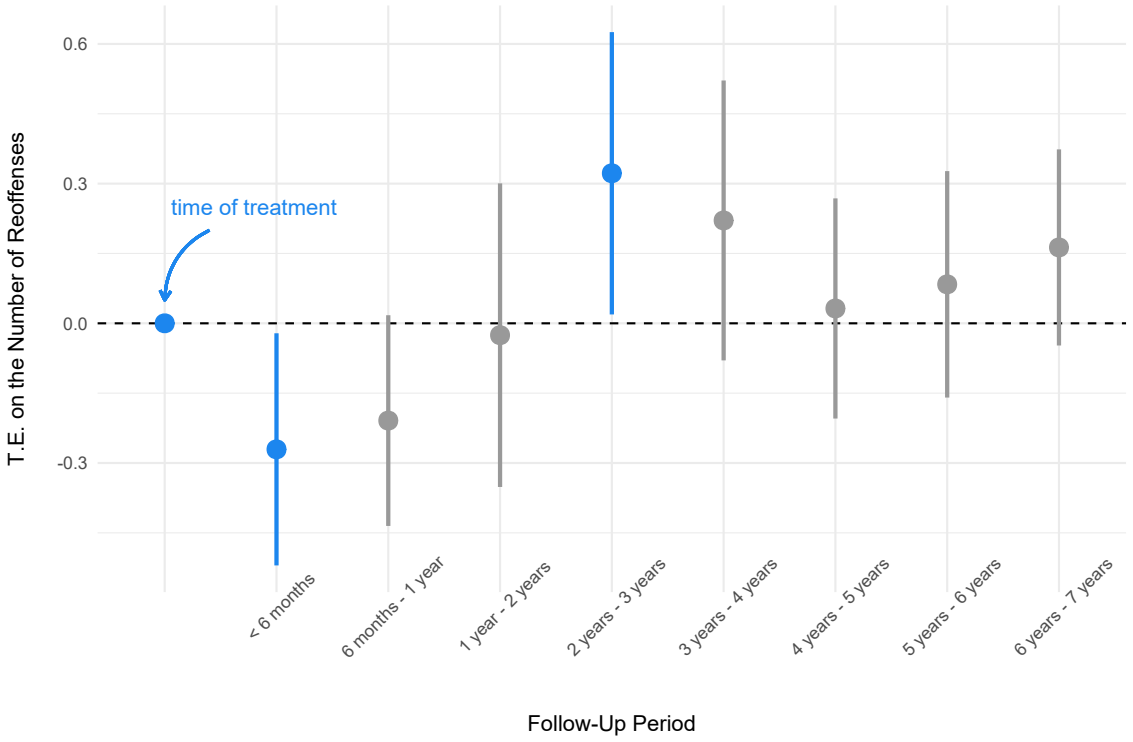


Figure 6: Contemporaneous Effects on the Number of Reoffenses, Recidivists Only

Notes. This figure reports the contemporaneous effects on the number of reoffenses, that is, the number of reoffenses committed during a given interval of time upon release. Only recidivists are considered. *Takeaway.* The program decreases the number of reoffenses directly following release, specifically in the subsequent six months. The effect is reversed between the second and third year. Thus, the program delays the reoffenses by several months.

and Athey, 2018; Athey and Wager, 2019) 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 works, Athey et al. (2019) generalized the causal forest framework to incorporate common estimation methods, such as the quantile regression and the instrumental variable regression. I hereby provide a brief intuition for the method.[21]

Consider the following structural equation put forth by Athey et al. (2019):

$$Y_i = \alpha(x) + \tau(x)D_i + \epsilon_i,$$

where $\tau(x)$ is understood to be a *conditional* average treatment effect, that is, an average treatment effect conditional on predetermined characteristics $X_i = x$. Athey et al. (2019) show that $\tau(x)$ is identified from the data with an instrument, say Z_i^c, under the usual moment conditions:

$$\begin{aligned}\mathbb{E}\{Z_i^c(Y_i - \tau(x)D_i - \alpha(x))|X_i = x\} = 0\\ \mathbb{E}\{Y_i - \tau(x)D_i - \alpha(x)|X_i = x\} = 0.\end{aligned}$$

$\tau(x)$ 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. According to this method, each regression tree is grown using one half of the sample, whereby the other half of the sample is marked as the estimation sample. The algorithm seeks the variable (and its cutoff, if the variable is continuous) that maximizes heterogeneity in the treatment effect by splitting the growing sample into all possible ways. Other nodes are sought recursively 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 estimation sample, thus circumventing any spurious effects mentioned previously. This algorithm is repeated and replicated multiple times, turning a large number of regression 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.

I run this procedure with the full set of controls and I instrument participation with Z^c. The dependent variable is an indicator for recidivism within one year following release. In Figure 7a, I present the distribution of all individual treatment effects in the population. In sum, I would argue that the heterogeneity in the treatment effects is limited, as the distribution largely lies between -0.20 and -0.16. Moreover, the distribution is centered around -0.18 which supports the robustness of the estimates obtained with the regular IV regressions. In Figure 7b, I plot the predicted treatment effects, ordered by age, along with individual confidence intervals.[22] Once again, I observe very little variation in the treatment effects' magnitude and variance.

Perhaps unsurprisingly, I discern no noticeable differences in the treatment effects' distribution for other observable characteristics, as displayed in Figure 8. Namely, the distribution of the treatment

[21] The detailed algorithm and tuning parameters are left to the appendix, where I also provide an illustrative example.

[22] To do this, I randomly picked 42 individuals from the sample and randomly assign a vector of ages ranging from 18 to 60. Then, for each, I predict the treatment effect using the random forest estimated on the real data and compute the variance of each estimate.

effects appears to be the same whether the crime was violent, regardless of the type of crime and seems independent of the number of prior convictions. The absence of discrepancies across all these distributions can be explained by the fact that the program was not developed for a definite type of offender. Indeed, the program's content, which focuses on accountability, easily adapts to all types of crime and offenders. Recall, too, that the convictions in this setting are associated with low-level crimes and mostly misdemeanors. It is important to stress that the absence of heterogeneity could arise from a lack of power resulting from a low sample size. More data are required to detect significant heterogeneity.

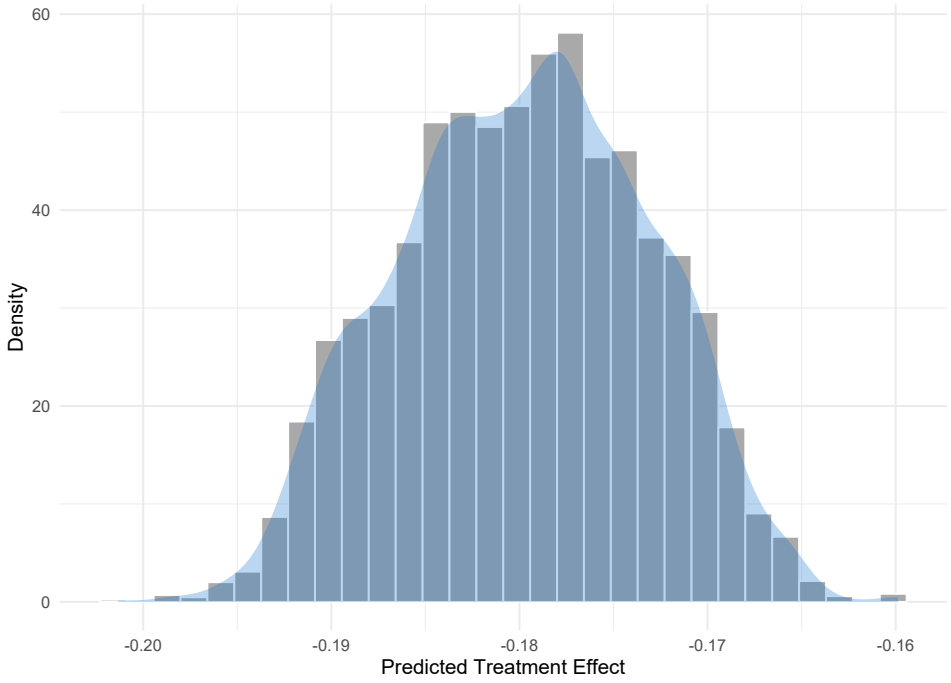
5 Conclusion

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. 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 and positive activities offered in some prison settings may provide a safer, more stable environment for inmates. Thereby, forced detention 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.

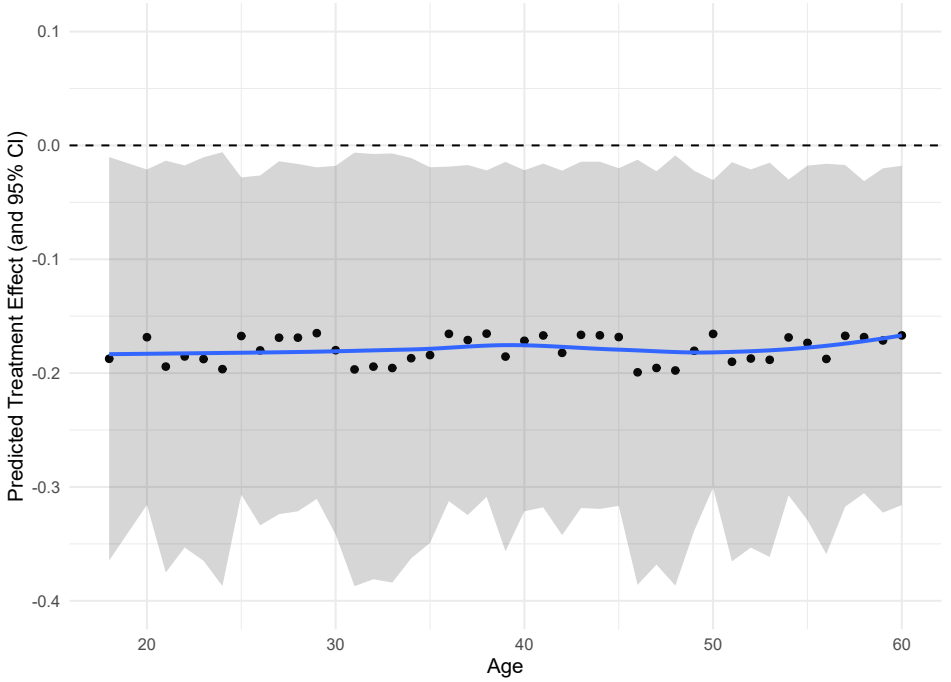
In this paper, I evaluated the effects of *Parcours*, a behavioral program implemented in the prisons in Quebec, Canada. By leveraging randomness in the assignment of inmates to probation officers, I was able to derive causal estimates of the effect of the program on recidivism. I found large, negative and significant effects on recidivism. This paper further finds that young inmates are most likely to comply with the program recommendation. This result is meaningful since young inmates are widely considered most-at-risk (Doleac, 2019). Thus, targeting young inmates has the potential of accelerating the positive effects of such a program by, namely, reducing the likelihood of costly incarceration. The results suggest that the criminogenic factors targeted by the program (accountability, attitudes towards criminality and victimization) were the primary channel of causality. This study demonstrates that reinforcing these decision-making traits could, almost entirely, deter some offenders from committing further crimes. In circumstances where participants do reoffend, the subsequent offense has been shown to be delayed by several months. Further research is required to determine whether reentry programs, in other words, programs delivered upon detention, hold promise during the reintegration process. Reentry programs might be key to ensure continuity in the acquisition of behavioral skills.

Going forward, more data 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 offenders can benefit from participating. 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.



(a) Distribution of the Predicted Treatment Effects



(b) Predicted Treatment Effects by Age

Figure 7: Heterogeneous Treatment Effects

Notes. The figure in the top panel plots the distribution of the treatment effects. These individual treatment effects were computed from out-of-bag predictions. The figure on the bottom panel reports the predicted average treatment effect with respect to age, along with individual confidence intervals. *Takeaway.* The presence of heterogeneity appears limited, as the distribution lies between -0.20 and -0.16. Similarly, inmates from all ages appear to benefit from the program.

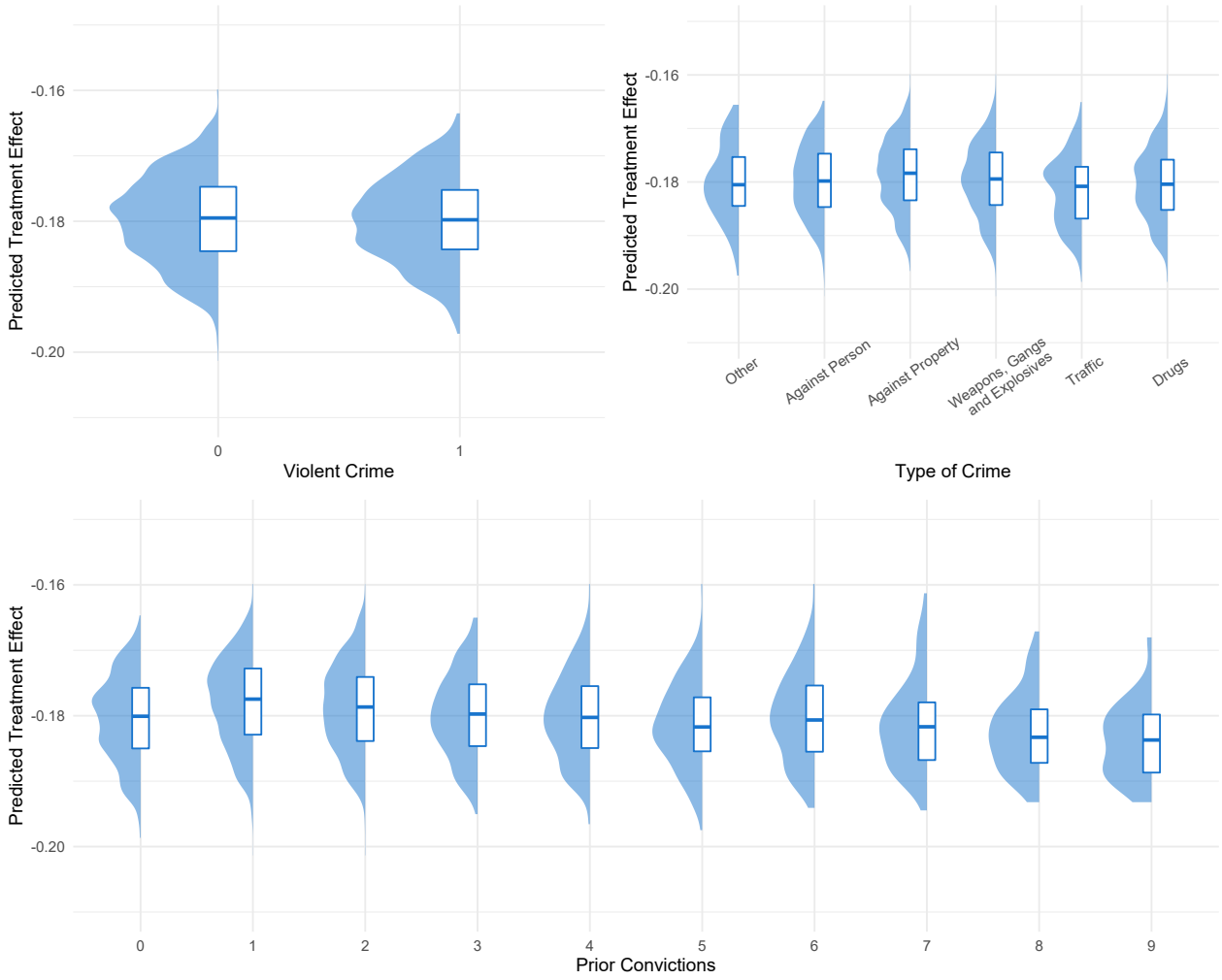


Figure 8: Heterogeneity With Respect to Observable Characteristics

Notes. These figures plot the distribution of the treatment effects with respect to whether the crime is violent, the type of crime and the number of prior convictions respectively. *Takeaway.* The presence of treatment effect heterogeneity appears limited. Inmates from various criminal backgrounds respond similarly to *Parcours*.

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Appendix

A Prison-Based Programs and *Parcours*

A.1 Studying Prison-Based Programs

There are several reasons as to why prison-based programs are understudied; in the subsequent paragraphs, I outline some of the challenges and explain how I overcome them in this study.

Selection bias. Since program participation in prison is on a voluntary basis, inmates self-select into the programs offered. The direction of the bias is ambiguous: while some inmates may be more inclined to enroll in certain programs because they are motivated to improve their lives and want to better reintegrate into society, others might participate to increase their chances of an early release and will resume criminal activities following their release. A naive comparison between participants and non-participants yields biased estimates of the program effect and can merely serve as an upper or lower bound since the direction of the bias is unknown. *Solution.* To correctly assess the program's impacts, I develop a quasi-natural experiment relying on the probation agents' propensities to recommend the program. The setting reproduces that of a natural experiment and circumvents the econometric issue related to inmates' self-selection.

Lack of standardization. In the United States and in Canada, some programs are managed at the facility-level, whereas others are conducted in numerous facilities concurrently, thus possibly inducing differences in the ways the activities of a program are organized. From a research perspective, it is concerning that potentially few inmates will have participated in the same program. Researchers are thus forced to pool together participants with varying curriculum. In this respect, it can be difficult to assess which programs are most effective and to provide evidence of external validity. *Solution. Parcours* is a delineated set of guidelines surrounding the intervention, the interviews and the activities, thus making it possible to compare participants from independent prisons.

Which outcomes? A researcher faces the challenge of determining which outcomes to study among a host of possibilities: a program can have a direct effect on the inmate's behavior in prison, can qualify him for an early release or parole, can improve his relationships with his peers outside of prison, or increase his ability to find a suitable job. Overall, the ultimate goal of any program is to reduce the likelihood of recidivism by such mechanisms. Even when considering recidivism, the outcome of interest is unclear. In fact, the very definition of recidivism is ambiguous as some researchers consider it to be a reoffense (that could even be left unreported), a rearrest, a reconviction or a reincarceration. Furthermore, it is unclear as to whether the researcher should focus on the short or the long-term effects of the program. *Solution.* I define recidivism as a subsequent sentence following the current incarceration. With detailed administrative data, I am able to consider recidivism in the short-, medium- and long-term. I also study additional outcomes that might be of interest, such as the number of reoffenses and the time elapsed before an additional sentence.

Prison is a black box. The criminal justice system is a complex network of agents and actions. The court process, the administrative proceedings, and the daily operations of a prison present a host of subtleties and intricacies that the researcher must consider before analyzing any data. In prison, inmates will inevitably have to report to a number of agents and interact with their fellow peers; all of which could affect the inmates' outcomes beyond the scope of the researcher. The

complexity of the system often yields imprecise data (e.g. some prisons do not record participation in their programs) and restricted data access. *Solution.* Across the province of Quebec, 11 prisons cautiously documented the participation records of *Parcours*. Relying on a rich administrative dataset, I am able to map the trajectory of an inmate from the beginning stages of their evaluation to the end stages of their release. Interviews were conducted to understand the tasks and responsibilities of the probation officers with the aim of validating the identification strategy.

Randomized controlled trials are challenging. Regardless as to whether a program was randomized across inmates or across prisons, inference of the treatment effect would not be eased. The balance of characteristics between the groups is not guaranteed, and external validity would be questionable. This proves increasingly true when programs are voluntary. *Solution.* The use of anonymous data poses little threat to security and ethics. The quasi-natural research design I am proposing allows accounting for the fact that the program is wholly voluntary. The results are thus policy-relevant.

A.2 Description of *Parcours*

The following paragraphs will highlight three important aspects of the program: its content and objectives, the targeted participants, and the formation of counsellors.

Content and objectives. *Parcours* consists of a series of activities, homework and interviews. It can be offered on a one-on-one basis or in group environments. Furthermore, it is offered in both detention centers as well as for those who are serving their sentence in the community. For convicts who are serving sentences in a detention center, that I will focus my research on, the full program takes around 24 hours. The 24 hours comprise the total time spent with a counsellor, but do not account for the homework between sessions nor the interviews before and after the program. *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*[23], 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, *Values and Perceptions*, the counsellor 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.

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. The inmate must be willing to deeply reflect on his criminal acts in an effort to make a lasting change. Second, there is no restriction regarding the type of crime that was committed, nor about its severity. Selection is made solely on the level of risk. Third, convicts are recommended to participate in the *Parcours* program 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...

1. valorizes or favors criminal activities to satisfy his needs;
2. is unable to imagine himself in the shoes of victims;
3. believes in criminal values;

[23] Translations are my own.

4. exhibits hostility towards the criminal justice system;
5. denies or minimizes any responsibility for his actions;
6. rationalizes his criminal behavior;
7. puts the blame on others or on external circumstances;
8. considers himself as a victim.

It should be noted that most facilities have not implemented *Parcours* entirely: most prisons only offer the first two modules. Numerous *Parcours* counsellors have expressed the opinion that the third module was the most difficult to carry as participants had serious problems in understanding the content.

Formation of counsellors. *Parcours* counsellors are trained probation officers or carceral counsellors. They are provided with all the material to sufficiently implement *Parcours*, including an extensive manual which accompanies each module, as well as detailed descriptions of the activities. Following each module, counsellors complete an evaluation report for each participant. In the first year of the implementation, more than 500 practitioners received the training.

Since the onset of the program in 2007, there has been no study regarding the impact of *Parcours* on recidivism.[24]

B Testing the Identifying Assumptions

B.1 Random Assignment

I test the random assignment of inmates to probation officers with a usual balance test. Under random assignment, we would expect the exogenous characteristics to be uncorrelated with the instrument. I first partial out the instrument from a fully interacted set of the prison and year dummies to account for the set of available evaluators varying across time and facility. I then regress the partialled-out instrument on the observable characteristics. The results are presented in Table B.1. All the coefficients are not statistically significant.

B.2 Exclusion Restriction

I test for the exclusion restriction with the newly developed test from Frandsen et al. (2019). The intuition for the test is the following: the only channel or decision from officer j affecting Y should come from Z, and nothing else. Thus, the test proceeds in two steps. First, I regress Y (recidivism within one year) on Z^c and compute the residuals. Intuitively, the residuals from the regression, say r, contain everything correlated with the outcome (purged from the effect of the instrument), including other channels through which the evaluator could affect the offender's outcome. If the exclusion restriction is respected, the residuals should not be correlated with the evaluators' fixed effects. I run the following regression for each prison separately:

[24] A preliminary analysis of the program was run a few months after the implementation. It did not take into account the selection bias. Some minor aspects of the program were modified following this, namely how the post-program interviews are conducted.

Table B.1: Balance Tests

	Dependent variable: Z^c	
	(B.1.1)	(B.1.2)
Type of Crime		
Against Person	-0.0193	(0.0140)
Against Property	0.0067	(0.0136)
Weapons, Gangs and Explosives	-0.0107	(0.0141)
Traffic	-0.0074	(0.0144)
Drugs	0.0167	(0.0140)
Age	0.0013	(0.0008)
Age Squared	0.0000	(0.0000)
Prior Convictions	0.0008	(0.0010)
Indigenous	-0.0018	(0.0194)
Violent Crime	-0.0069	(0.0051)
Clusters	✓	
N	5945	

Notes. This table reports the results of balance tests. Under random assignment, we would expect no correlation between the instruments and the observable characteristics. I first regress the instrument on a fully interacted set of prison and year dummies and compute the residuals. I then regress the residuals on the set of characteristics. I report the coefficient of each characteristic and the associated standard errors, in parentheses. *Takeaway.* The allocation between probation agents and inmates appears to be mostly random. Some probation officers are specialized with certain profiles of inmates: adding fixed effects guarantees randomization within a group.

Table B.2: Frendsen et al.'s test for the exclusion restriction

Prison	F	p-value	Prison	F	p-value
Amos	0.81	0.5101	Sept-Iles	1.20	0.3264
Baie-Comeau	0.67	0.7648	Sherbrooke	1.29	0.0719
Hull	1.32	0.1623	St-Jerome	2.40	0.0000
Montreal	1.46	0.0043	Sorel	0.67	0.7301
Quebec	1.14	0.1877	Trois-Rivieres	1.47	0.0453
Rimouski	1.02	0.4405			

Notes. This table reports the F statistics of the Frendsen et al.'s test for the instrument Z^c. I only consider the subset of evaluators who performed at least 100 evaluations (more than 75% of the sample). *Takeaway.* The main channel through which evaluators affect the outcome is via their propensity to recommend the program. There are some exceptions: the prison of Montreal, St-Jerome and Trois-Rivieres. All the results hold when excluding these facilities.

$$r_i = \alpha + \sum_{k=1}^{J} \beta_k k(i) + \epsilon_i$$

and I report the F statistic. The results are displayed in Table B.2.

B.3 Monotonicity

The monotonicity assumption has two testable implications, namely (1) evaluators' propensities have a monotonic effect on the inmates' participation decision, and, (2) the first stage estimates (say, from a regression of D on Z) must be positive for every slice of the data. To test the first implication of monotonicity, I cut the dataset into two equally-sized groups based on the values of Z^c. Intuitively, the effect of Z^c on the treatment status, i.e. the first stage estimates, should be positive for all the intervals considered. The results are presented in Table B.3. Furthermore, I plot in Figure B.1 the density of Z^c in the top panel. In the bottom panel, I plot a nonparametric regression of the treatment status on Z^c. The relationship is monotonic and increasing on all the domain.

Next, the first stage estimates must be positive for every subset of the data. It is easily verifiable by running a number of regressions, each conditioning on a particular subset. The results are shown in Table B.4. The coefficients are positive and significant for all regressions, one exception being when I consider only inmates with Indigenous backgrounds and another if the crime is in the *other* category. However, the lack of precision probably stems from a very small sample size.

B.4 Joint Assumptions

I briefly describe the intuition behind the Kitagawa's test here. Consider a Borel set B in $[0, 1]$, the domain of Y. Imbens and Rubin (1997) show that under random assignment, the following conditions must hold for all B if the exclusion restriction and the monotonicity of the instrument criteria are not violated:

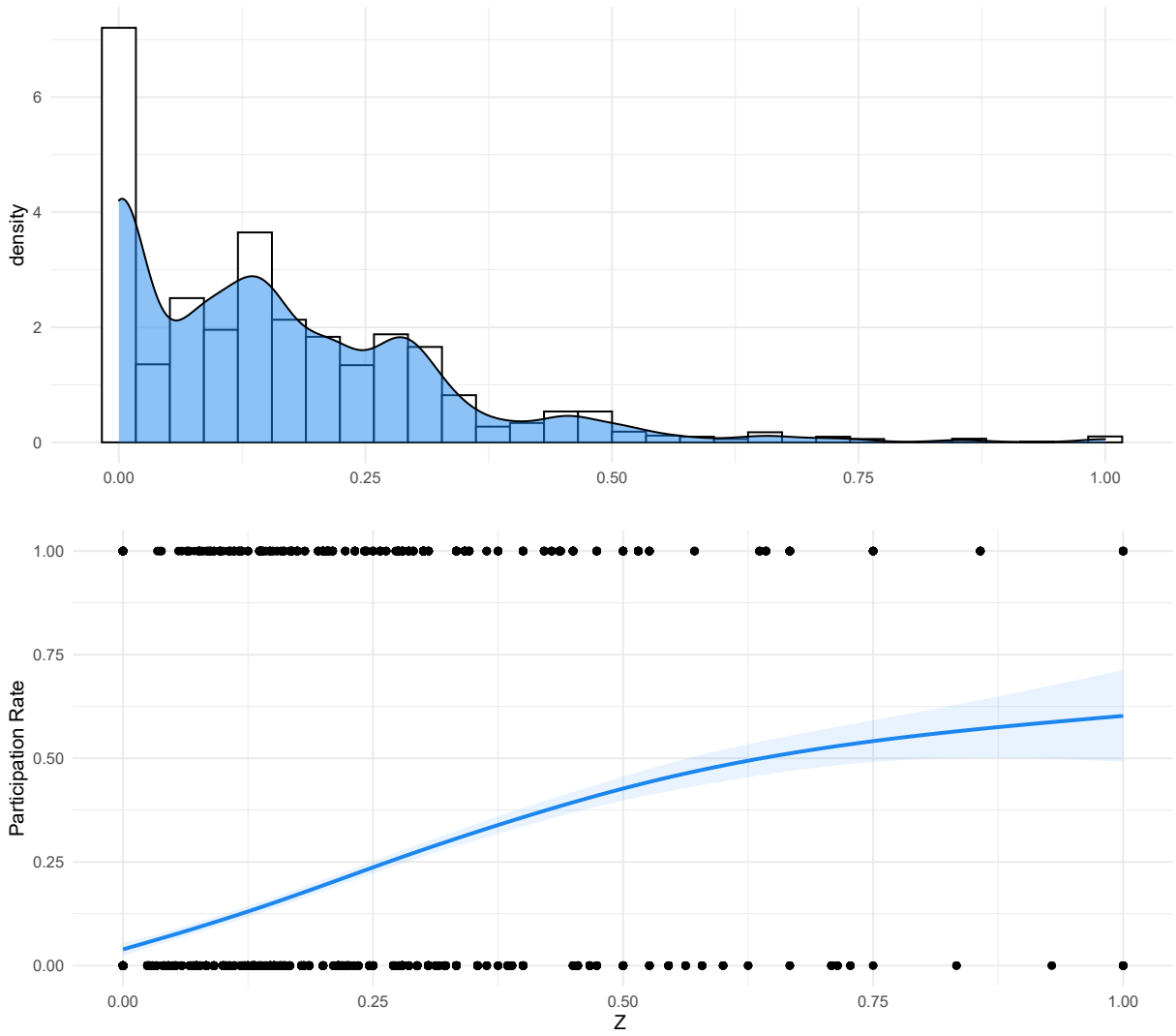

Figure B.1: Density and Monotonicity of Z^c

Notes. The figure in the top panel plots the distribution of the instrument, Z^c. The figure in the bottom panel plots the results from a nonparametric regression of the participation rate on the instrument. *Takeaway.* Several evaluators have never recommended the program, however, the main results hold even if removing such observations. The likelihood of participation is monotonic with respect to the instrument.

Table B.3: Monotonicity Assumption: Part 1

	Dependent variable : participation decision		
	Pooled	$0 \leq Z^c < 0.1304$	$0.1304 \leq Z^c \leq 1$
	(B.3.1)	(B.3.2)	(B.3.3)
Effect of Z^c on Participation	0.5403	0.4366	0.3969
	(0.000)	(0.001)	(0.000)
Controls	✓	✓	✓
Clusters	✓	✓	✓

Notes. This table reports the results from the first testable implication of the monotonicity assumption. I split the sample into two equally-sized groups based on the values of Z^c and run first stage regressions with a full set of controls and clustered standard errors. I report the estimated coefficients and the corresponding p-values. *Takeaway.* All the point estimates are positive and significant.

$$\mathbb{P}(Y_1 \in B, D_1 > D_0) \geq 0$$
$$\mathbb{P}(Y_0 \in B, D_1 > D_0) \geq 0,$$

where Y_1 and Y_0 are the outcomes of participants and non-participants respectively. Such nonnegativity conditions are testable under the null hypothesis that the instrument is valid. The results are presented in Table B.5.

B.5 Further Evidence for the Exclusion Restriction

The exclusion restriction is unlikely to hold if the decision-maker takes multidimensional decisions. For example, a court judge decides whether to convict an individual and the length of the sentence to serve. In my setting, the probation officers responsible for the evaluation can recommend Parcours but can also suggest other programs ranging from education training to mental health workshops. Besides the Parcours data available for 11 prisons, I have access to the complete program participation record in the prisons of Quebec (from 2010 to 2016), Montreal (from 2007 to 2012) and St-Jerome (from 2011 to 2015). From these data, I can compute the propensity to recommend any correctional measure for each probation officer.

More precisely, I compute

$$p_j = \frac{1}{n_j} \sum_{k=1}^{n_j} D_k,$$

where D_k is equal to one if inmate k participates to at least one program and where n_j is the number of completed evaluations by officer j. Then, p_j is a proxy for the probation officer j's propensity to recommend other programs than Parcours while n_j can be thought of as a proxy for the evaluator's experience. I run a set of IV regressions while controlling for p_j and n_j both in the first and second stage, an approach recently applied in Cohen (2020). The results are shown in Table B.6.

Table B.4: Monotonicity Assumption: Part 2

Dependent variable: participation decision		
Subset	Coefficient	p-value
Type of Crime		
Against Person	0.586	0.000
Against Property	0.733	0.000
Weapons, Gangs and Explosives	0.450	0.000
Traffic	0.2171	0.088
Drugs	0.5294	0.000
Other	0.1457	0.294
Age		
Age ≤ 35	0.6304	0.000
Age > 35	0.4415	0.000
Prior Convictions		
0	0.5514	0.000
1	0.6215	0.000
≥ 2	0.5104	0.000
Indigenous		
Yes	0.3023	0.468
No	0.5489	0.000
Crime is Violent		
Yes	0.6237	0.000
No	0.5219	0.000
Controls	✓	
Clusters	✓	

Notes. This table reports the results from the second testable implication of the monotonicity assumption. I split the sample according to any observable characteristic and run first stage regressions with a full set of the remaining controls and clustered standard errors. I report the estimated coefficients and the corresponding p-values. *Takeaway.* All the point estimates are positive, and most of them are significant. The only exceptions are when the individual is Indigenous and when the crime is in the category *other*. The test provides suggestive evidence that inmates with all characteristics react similarly to the instrument.

Table B.5: Kitagawa's Test (for Z^b and Z^c)

	Binary Instrument	Continuous Instrument	
		without covariates	with covariates
$\xi = 0.05$	-12.34 (p = 1)	134.41 (p = 0)	16.32 (p = 1)
$\xi = 0.10$	-12.34 (p = 1)	67.20 (p = 0)	8.16 (p = 1)
$\xi = 0.15$	-12.34 (p =1)	44.80 (p = 0)	5.44 (p = 1)

Notes. This table reports the test statistics of the Kitagawa's test. These are drawn from a bootstrap distribution, hence, p-values can be exactly zero or one. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. ξ's are trimming constants. I thank Professor Kitagawa for providing the code to implement the test and Professor Ismael Mourifié for pointing out this test to me. *Takeaway.* Z^b and Z^c appear to be valid instruments (their validity is not refuted) when additional covariates are controlled for.

Table B.6: Further Tests for the Exclusion Restriction

	OLS		Instrument: Z^c			
	(B.6.1)	(B.6.2)	(B.6.3)	(B.6.4)	(B.6.5)	(B.6.6)
Program	-0.0918	-0.0857	-0.3159	-0.2972	-0.3142	-0.2964
	(0.0440)	(0.0446)	(0.1774)	(0.1706)	(0.1570)	(0.1455)
Control for p_j	No	Yes	No	Yes	No	Yes
Control for n_j	No	No	No	No	Yes	Yes
F (first stage)			31.64	9.45	32.63	9.14
Controls	✓	✓	✓	✓	✓	✓
Cluster	✓	✓	✓	✓	✓	✓
N	1532	1532	1515	1515	1515	1515

Notes. In this table, I report further robustness checks to validate the exclusion restriction. The dependent variable is an indicator for recidivism within two years following the release. The standard errors are reported in parentheses and are clustered at the prison-year-evaluator level. In the first two columns, the selection bias is not accounted for. In the last four columns, Z^c is used to instrument participation. p_j is the probation agent's propensity to recommend other programs besides *Parcours*, while n_j is the total number of completed evaluations. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. *Takeaway.* While p_j and n_j could be correlated with Z^c and thus induce omitted variables biases in the main specifications, it appears that omitting them does not change the magnitude nor the significance level of the parameter of interest. Although this test is not perfect, it indicates that the main results are probably slightly negatively-biased.

In columns (B.6.1) and (B.6.2), I run standard OLS regressions both with a full set of controls. In the second specification, I include p_j. The estimated coefficients have a similar magnitude and are significant at the 4 and 6% level respectively. In column (B.6.3), I run the benchmark regression with Z^c as an instrument. This regression uses the same specification as the principal regressions in the paper. Luckily, when adding a control for p_j, in column (B.6.4), or for n_j, in column (B.6.5), I find similar coefficients with comparable levels of precision. In the last column, I control for both p_j and n_j, and the resulting coefficient is similar to that from the benchmark regression.

Some remarks are in order. Firstly, although most estimated coefficients are statistically significant, the estimation yields wide confidence intervals. This results from a lack of statistical power since the sample size is rather small. Secondly, when I control for p_j, the F statistics become lower than 10, which could bias the estimations. It indicates that adding p_j in the model reduces the prediction power of Z^c on the treatment status. I would argue that not including p_j in the main regressions of the paper matter, but not that much; as demonstrated by this exercise, the magnitude and significance levels remain virtually unchanged whether p_j is included. It implies, however, that the estimated coefficients might be slightly downward-biased as Parcours participants potentially participated in other programs as well. It is important to stress that Parcours is the most thorough, extensive and complete program offered. Most other programs last only a few hours.

Lastly, I run placebo checks. Since Parcours requires a certain number of participants to operate (around 10, although this number varies across facilities), some offenders are incarcerated while the program is not available. Intuitively, under the exclusion restriction, the instrument should not have any effect on these inmates' outcomes as they did not have the choice of whether to participate. I run the reduced form regressions on these inmates and present the results in Table B.7. None of the reduced form estimates is found to be significant.

C Random Forests: Algorithms and Parameters

Random Forest Algorithm

- Goal: estimate $Y_i = \mu(X_i) + \tau(X_i)W_i + \epsilon_i$, where W_i and ϵ_i are correlated
- Consider a binary instrument $Z_i \in \{0, 1\}$
- $\tau(x)$ is identified from $\mathbb{E}[Z_i(Y_i - W_i\tau(x)) - \mu(x)|X_i = x] = 0$

Grow a single tree

1. Select a fraction η of the sample, and $k \in K$ covariates
2. Compute $\hat{\tau}_P$ in the $1 - \eta$ fraction of the sample
3. In the parent node, compute the pseudo outcomes

$$\rho_i = \left(Z_i - \bar{Z}_P\right)\left(\left(Y_i - \bar{Y}_P\right) - \left(W_i - \bar{W}_P\right)\hat{\tau}_P\right)$$

4. Choose two children C_1 and C_2 to maximize

Table B.7: Placebo Test

	Dependent variable: recidivism within...		
	6 months	1 year	2 years
	(B.7.1)	(B.7.2)	(B.7.3)
Instrument	-0.2086	-0.1627	-0.1804
(s.e)	(0.1457)	(0.1584)	(0.291)
[95% CI]	[-0.4943, 0.0767]	[-0.4732, 0.1479]	[-0.5156, 0.1548]
Outcome Mean	0.19	0.30	0.42
Controls	✓	✓	✓
Clusters	✓	✓	✓
N	10,216	10,039	9,397

Notes. In this table, I report placebo checks to validate the exclusion restriction. The dependent variable is an indicator for recidivism within six months, one year and two years following the release. The standard errors are reported in parentheses and are clustered at the prison-year-evaluator level. Controls include age, age^2, time-, prison- and crime fixed effects, the number of prior convictions, a dummy for belonging to an Indigenous group, and an indicator for a violent crime. I regress the outcome variable on Z^c for the set of inmates who had a prison spell while the program was not available. These regressions can be thought of as placebo reduced form regressions. *Takeaway.* The instrument does not have any significant effect on this set of inmates. Therefore, this demonstrates how the instrument only correlates with recidivism when the program is available; in other words, the exclusion restriction is valid.

$$\widetilde{\Delta}\left(C_1, C_2\right) = \sum_{j=1}^{2} \frac{1}{|\{i : X_i \in C_j\}|} \left(\sum_{\{i:X_i \in C_j\}} \rho_i \right)^2$$

5. Repeat until the minimum node size is attained

Grow a forest

1. Grow a total of B trees
2. For $\forall b \in B$ and $\forall i$, compute the weights

$$\alpha_{bi}(x) = \frac{\mathbb{1}\left(\{X_i \in L_b(x)\}\right)}{|L_b(x)|},$$

   where $L_b(x)$ is the set of individuals with characteristics x.

3. Average them out

$$\alpha_i(x) = \frac{1}{B} \sum_{b=1}^{B} \alpha_{bi}(x)$$

4. Compute the individual treatment effects

$$\hat{\tau}(x) \in \text{argmin}_{\tau} \left\{ \left\| \sum \alpha_i(x) \psi_{\tau} \left(Y_i, W_i, Z_i \right) \right\|^2 \right\}$$

Tuning Parameters

- Define $k(x) = \mathbb{E}[K_i | X = x]$ and $\tilde{K}_i = K_i - \hat{k}^{(-i)}(X_i)$.
- Estimate $\tilde{Y}_i, \tilde{W}_i$ and $\tilde{Z}_i$ with $B = 3000$
- Train a first random forest will all covariates, $B = 4000$
- Keep the most *important* covariates
- Estimate a random forest using only these variables with $B = 5000$
- Compute $\tau(\hat{X})$
- All other parameters are entirely data-driven

Illustrative Example

An example of a tree is displayed on Figure C.1. A tree is constructed from the top to the bottom: the algorithm first selects a variable (a first node) that discriminates best between the different outcomes based on some measure of entropy. The node generates two branches, one if the condition given by the node is respected and another one if it is not. The algorithm is then subsequently applied again to find other nodes until a stopping condition is met, whether this is the depth of the tree reaching its maximum or the number of observations in the leaves (the terminal nodes) reaching the value given by the user.

Consider the following example along with Figure C.1. Adam (A), Benjamin (B) and Charles (C) are identical triplets with the same potential outcomes. They are all 25 years of age, they do not have a previous criminal record, and they recently committed a crime against a person. When the first tree is grown (e.g. the tree on Figure C.1), only A is part of the bootstrapped sample. He ends up in the second leaf from the left based on his exogenous characteristics. In this leaf, the recidivism rate is 20% while the participation rate is 13%. Had B and C been sampled, they would have ultimately fallen in the same leaf as A. Thus, they can be used to compute the treatment effect within this leaf. This is done with the instrumental variable since B and C are credible counterfactuals for each other. A treatment effect is estimated for each leaf of the tree. This is repeated a large number of times and in turn, trees grow into a forest. The result is an average treatment effect for every inmate in the sample.

Properties

As previously mentioned, forests are a aggregation of many trees and are employed to reduce the variance of the estimator. The novel techniques developed in Athey et al. (2019) allow researchers to draw causal inference from random forests as they were originally used to predict (not explain) outcomes. I succinctly describe some features of the method below.

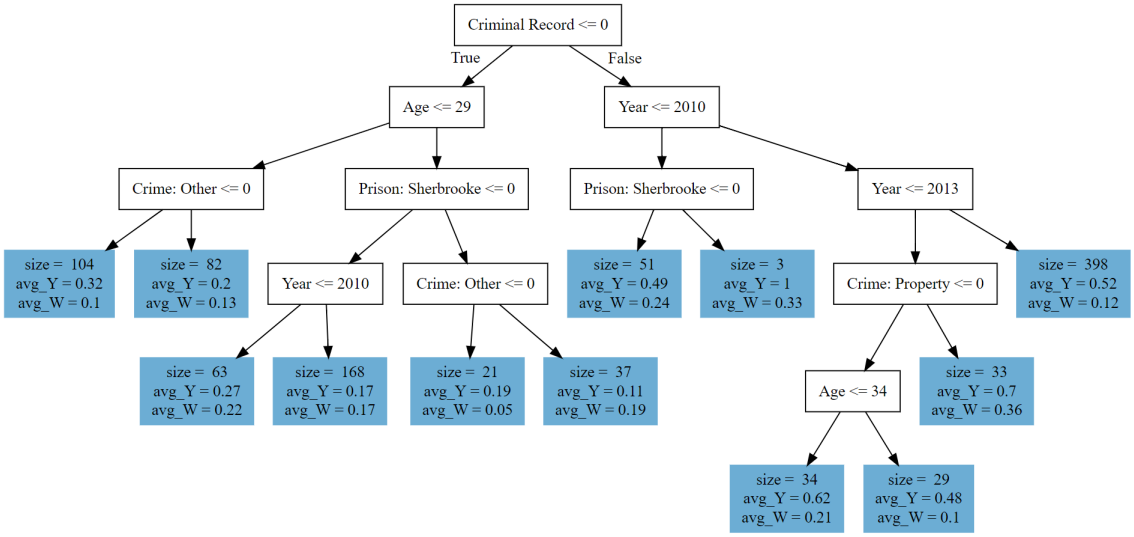

Figure C.1: Example of a Regression Tree

Notes. This is an example of a regression tree. In this iteration, having a prior conviction was determined to be the most discriminating variables between those who experience large treatment effects from those who do not. The leaves in blue are the terminal nodes, where treatment effects are computed using the instrumental variable. In the first leaf starting from the left, on a total of 104 inmates, the recidivism rate is 32% while the program participation rate is 10%.

Bootstrap aggregating. Bootstrap aggregating (or bagging) improves classification by aggregating several trees. In each iteration, a different sample of the original data is used to grow the tree. In addition, the algorithm uses random split selection: at each node, a different subset of m exogenous variables, with $m < K$, are used to split the node into two branches. This step produces less correlated trees and thus reduces the variance. See Ishwaran (2015).

Honesty. In Wager and Athey (2018), the authors introduced honest random forests. An honest random forest grows trees using one half of the bootstrapped sample, while the other half is used to estimate the treatment effects of interest.

Partitioning. Causal random forests are well suited to explore heterogeneity since the splitting process is optimized to capture heterogeneous treatment effects. Athey et al. (2019) exploit recursive partitioning by defining a new criterion that increases the heterogeneity in the treatment effects as fast as possible.

Local centering. Estimating the treatment effect of the program necessitates precise estimates of the marginal expectations of Y_i and D_i : $y(x) = \mathbb{E}(Y_i|X = x)$ and $d(x) = \mathbb{E}(D_i|X = x)$. Moreover, let $\tilde{K}_i = K_i - \hat{y}^{(-i)}(X_i)$, the orthogonalized leave-one-out estimator of the marginal expectation. The forest is grown using the centered outcomes $\tilde{Y}$ and $\tilde{D}$ for more robustness. See Chernozhukov et al. (2017).

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Acknowledgement:

I am thankful to David Price for his unwavering support and keen suggestions. I wish to thank Victor Aguirregabiria, Carolina Arteaga, Luc Bissonnette, Vincent Boucher, Loren Brandt, Bernard Fortin, Philip Oreopoulos, Kevin Schnepel and Clémentine Van Effenterre for providing generous comments at various stages of this project. I am indebted to Guy Lacroix for granting me access to the data and for his constant support. I wish to thank Steeve Marchand for his invaluable collaboration. I have greatly benefited from discussions with Bernard Chéné, Guy Giguère and Isabelle Paquet, who helped demystify the criminal justice system's subtleties. I want to express my gratitude to my PhD colleagues, most notably Alex Hempel, Alexandre Lehoux, Poli Natama, Xuemin Song and Natalia Vigezzi for their constructive comments. Finally, I would like to thank the seminars participants at Université Laval, University of Toronto, the online crime seminar and at the Graduate Students in Economics of Education Zoom (GEEZ) group. The views expressed here are from the author only and not necessarily those from Quebec's Ministry of Public Security. The author declares no conflict of interest.

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