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Juvenile Incarceration and Adult Recidivism

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Juvenile Incarceration and Adult Recidivism*

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Abstract

Although there is debate about whether juvenile incarceration deters future crime, it is a common practice worldwide. We contribute to this debate by using Chilean data to assess the causal impact of different types of juvenile incarceration on recidivism in young adulthood (18-21 years old). To address the endogeneity issues, we use the quasi random assignment of detention judges as instrumental variable to estimate the effect of pretrial detention, and the quasi random assignment of public attorneys to estimate the effect of any type of incarceration. Considering a standard IV linear model, we find that pretrial detention increases the probability of recidivism by 61 percentage points (pp), and when we define the treatment as any type of incarceration, this impact is equal to 65 pp. When we estimate bivariate probit models – using a novel approach for estimating this model in the context of fixed effects – the impact of pretrial detention and incarceration on recidivism are equal to 12 pp and 15 pp, respectively. We also estimate the marginal treatment effect (MTE), finding that the magnitudes of the marginal effects are larger for those individuals with low treatment probabilities. If we use MTE estimates to calculate the average treatment effect (ATE), the impact of pretrial detention on recidivism is equal to 28 pp. If we define the treatment as any type of incarceration, this impact is equal to 36 pp. Finally, we find that an important mechanism behind these impacts is the effect of these different types of incarceration on high school graduation.

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1 Introduction

Although it is a common practice worldwide, often with the hope or assumption that it will impact positively future behavior, little is known about the impact of juvenile incarceration.[1] As Aizer and Doyle (2015) points out, in a life-cycle context, incarceration during adolescence may interrupt human and social capital accumulation at a critical time, leading to reduced future wages in the legal sector and greater future criminal activity.

This is a even more relevant concern when the incarceration is due to a pretrial detention, as we study in this paper, in which case the decision about incarceration is resolved in few minutes and without any serious and detailed discussion about any evidence of guilt or consideration of innocence.[2] In the case of Chile (2012), the empirical context of this paper, 21,274 teenagers were prosecuted (out of a population of about one million), of them, 7.9% experienced pretrial detention (1,678 individuals). To be really worried about, 27% of the teenagers who were imprisoned due to a pretrial detention were either declared non-guilty or their verdict set a non-custodial sanction.

To contribute to this debate, this paper uses two sources of exogenous variation – the quasi random assignment of detention judges and public attorneys – to estimate the causal impact of different types of juvenile incarceration on recidivism as young adults, between18 and 21 years-old. Using Chilean data, we use detention judges as instrumental variable to estimate the effect of pretrial detention and public attorneys to estimate the effect of any type of incarceration. We construct the instruments using a residualized, left-out judge leniency and public attorney quality measures that account for case selection, following Dahl et al. (2014).[3] As we discuss in the paper, our two instruments meet the conditions defined by the literature when interpreting the estimation results as a local average treatment effect (see for example Dobbie et al. (2018)).

In the main specification, following the most common approach in the literature,

[1]For a general review about the effects of incarcerations, its costs and benefits, see Schnepel (2016).

[2]As the Open Society Justice (UN) Initiative stated (see Open Society Fundations (2011)): "Pretrial detention is one of the worst things that can happen to a person: the detainee immediately loses his freedom, and can also lose his family, health, home, job, and community ties."

[3]Kling (2006) instruments the sentence length by using an index of each judge's sentencing severity, finding that incarceration has small positive effects on employment that fade over time. Di Tella and Schargrodsky (2013) and Green and Winik (2010) also used this strategy to estimate the effect of incarceration on recidivism.

we estimate the local treatment effect (LATE) by running a two-stage least squared estimation (2SLS). Given the binary nature of both the dependent and the endogenous/independent variables, we run a bivariate probit model as an alternative specification. Regarding this non-linear model, we contribute to the literature by proposing a simple approach to estimating the average marginal treatment effect in the presence of fixed effects. The two approaches give very similar results in terms of sign and statistical significance, but the differences are more salient regarding magnitudes, where the linear model shows point estimates that are between three and five times bigger respect to the non linear model. The latter is in line with the Montecarlo experiment results, presented in the Appendix A. To better understand the differences in the magnitudes of the marginal effects between these two approaches, we also estimate the marginal treatment effect (MTE), which allows us to study the heterogeneity in the estimated effects, by showing how the marginal effects vary across youths who are induced into treatment, as the probability of pretrial detention varies with the instrument.

This paper uses administrative data. In particular, we assemble administrative dataset from the Ministry of Education and the Public Defender's Office (*Defensoría Penal Pública*, DPP). We construct our estimation samples considering all Chilean youths, who were prosecuted between 2008 and 2012 when they were between 15 and 17 years old, and who had any educational record between 2002 and 2013. We further restrict this to have an estimation sample that is appropriate given our research questions. Specifically, we restrict the sample to the first relevant crime for each juvenile, defined as crimes with an incarceration rate above 5%. In addition to this, we restrict our attention to courts that have at least 3 judges and to cases whose judges (or attorneys, depending on the instrument considered) have at least 30 cases per year. The final estimation sample has – in the case of the pretrial estimation model – 4,386 juveniles, 986 of them were incarcerated pretrial. In the case of the conviction model, the estimation sample includes 7,730 juveniles, 1,826 of whom were incarcerated.

We find strong evidence of an impact from different types of incarceration on young adult recidivism (18-21 years old). In particular, the linear IV model shows that pretrial detention causes an increase of 61 pp in the probability of new penal prosecution as young adult, and similar magnitudes for the effects on other recidivism measures. When we define the treatment as any type of incarceration, this impact is equal to 65 pp. When we estimate bivariate probit models, these effects are equal to 12 pp and 15 pp, respectively. Finally, the MTE estimates show that the magnitudes of the marginal effects are larger for those individuals with low probabilities of treatment (either pretrial detention or incarceration). Furthermore, the average treatment effects which are calculated from these MTE estimations show magnitudes that fall in between the linear and non-linear IV models, namely, the impact of pretrial detention on the probability of a new penal prosecution as young adult is equal to 28 pp (not statistically significant). If we define the treatment as any type of incarceration, this impact is equal to 36 pp. Regarding mechanisms, our results show that an important mechanism behind these impacts is the effect these different types of incarceration have on high school completion.

This paper is related to two strands of the literature. Firstly, there are very few papers studying the effect of pretrial detention on individuals' future outcomes. In the most compiling paper, Dobbie et al. (2018) show that pretrial detention decreases formal sector employment and the receipt of employment and tax-related government benefits. However, they also show that pretrial detention has no net effect on future crime. When it comes to developing countries, there are a few papers that estimate the effect of pretrial detention on labor outcomes, which find results along the same lines of Dobbie et al. (2018), see Grau et al. (2019) and Ribeiro (2018).

Secondly, there is a literature that studies the effect incarceration on recidivism considering juvenile and adult populations. In the case of adult incarceration, the causal evidence is mixed, mainly depending on the source of exogenous variation. When the exogenous variation is given by a discontinuity in sentencing guidelines, as in the case of Estelle and Phillips (2018) and Franco et al. (2018), the evidence shows that harsher sentences created by sentencing guidelines reduce recidivism. In contrast, when the exogenous variation is given by a random or quasi random assignment of judges, the evidence shows that incarceration increases or has no effect on the probability of recidivism, see Green and Winik (2010); Mueller-Smith (2015); and Nagin and Snodgrass (2013).

In the case of juvenile incarceration, most studies attempt to identify the causal effects by controlling for observed individual characteristics (De Li (1999); Tanner et al. (1999); Sweeten (2006)), while others controlling by household fixed characteristics (Hjalmarsson (2008)). The most convincing causal evidence on this topic is mixed. For example, Hjalmarsson (2009) considers a regression-discontinuity approach, taking advantage of sentencing guideline cutoffs, finding that incarceration reduces the probability that juvenile offenders re-offend.[4] On the contrary, Aizer and Doyle (2015) take advantage of the random assignment of judges, as we do, and find that juvenile incarceration substantially reduces high school completion rates and increases adult incarceration rates including for violent crimes.[5]

This paper has three main contribution to the previous literature. To the best of our knowledge this is the first paper that studies the effect of juvenile pretrial detention – as a specific type of incarceration – on recidivism as adult. Additionally, we contribute by showing evidence of the impact of juvenile incarceration on recidivism for a developing country. Finally, we develop a simple approach to estimate bivariate probit models in the presence of high dimensional fixed effects. This approach works well to the extent that there are enough data points – as in our case – within each fixed effect group.

The rest of the paper is organized as follows. In Section 2 we describe the main features of the Chilean penal juvenile system. In Section 3 we describe our sources of information and the data base we construct, while Section 4 shows the validity of the two instrumental variables, and presents the main estimation strategy (2SLS) and the alternative one (biprobit). Section 5 presents our main results and discusses their robustness. Section 6 shows the estimations of the marginal treatment effect models. Finally, Section 7 concludes.

2 The juvenile penal system in Chile

The new law for the juvenile penal system was enacted in 2005, and this new system started in 2007. The reform was inspired by the Convention on the Rights of the Child, specifically that measures and sanctions are expected to be reintegrative and rehabilitative in their content. Among other aspects, this new law introduced three major changes regarding the previous system. First, it reduced the age of criminal responsibility from

[4]There are papers exploiting sharp changes in sentence severity that occur at age 18. Guarin et al. (2013) show that more severe penalties reduce recidivism, and Loeffler and Grunwald (2015) show that increasing the maximum age who are sent to juvenile court does not affect juvenile recidivism.

[5]In other related literature, there are papers studying the effect of pushing for harsher juvenile sentencing policies by using the juvenile waiver to criminal court. In a meta-analysis for this research question, Zane et al. (2016) find that juvenile transfer had a small but statistically nonsignificant increase on recidivism.

16 to 14 years old. Second, it ended the ambiguity of the previous system. Before, depending on the discernment test and judges' considerations,adolescents could be treated as adults or juveniles. Third, for those who are found guilty, the punishment is one grade less severe than what would be given to an adult.[6]

This new juvenile penal system was roughfly implemented at the same time that Chile reformed its Criminal Justice System using a gradual processes staring in 2000 (in some geographic regions), finishing in 2005. The reform replaced the former written, secret, and inquisitional system, which was in place for more than a century; with an oral, public and adversarial procedure.[7] As part of the reform, new institutions were created, including the Office of the Public Prosecutor (Ministerio Público), the Public Defender's Office (Defensoría Penal Pública, DPP), the Guarantee Court (Juzgados de Garantía-special courts to safeguard the rights of the defendant and the victim during the investigation process), and Oral Criminal Trial Courts (Tribunales Orales de Juicio Penal). The DPP provides free legal representation for almost all individuals who have been accused of committing a crime, and documents all defendants that use their services, either juveniles or adults, including detailed information on the crime.

In this new system the juvenile penal process has the following stages. It starts with the arrest of the individual, in most of the cases because he is caught by the police in *flagrante delicto* (i.e., in the commission of the crime); in other cases because the Public Prosecutor conducted an investigation and based on that the individual is accused. This stage ends in the detention hearing at the Guarantee Court, where the detention judge must choose among three possible outcomes: to begin a penal proceeding, an alternative ends (including compensation agreements and the conditional suspension of proceedings), or to dismiss the proceedings. It should be noticed that most of the cases are solved in this Guarantee Court either by the decision of alternative ends or by dismissing the proceedings. As a general matter, a penal proceeding is only for severe crimes.

When the detention judge decides to begin a penal proceeding, she must decide the length of the trial (including the time for preparation), and the application of any precautionary measure (with none as a possible outcome). Pretrial detention is the most severe precautionary measure. This precautionary measure is requested by the

[6]See Couso and Duce (2013) for a detailed description of this reform.

[7]See Blanco et al. (2004) for a detailed description of the reform.

prosecutor, and it is made in a decision where the defense attorney participates. The legal arguments that may be invoked by the prosecutor to request such a measure are the clear danger of escape by the prosecuted, the situation in which the defendant represents a danger to society, or that the imprisonment of the prosecuted favors investigation of the criminal case (see Riego and Duce (2011)). At least in theory, the prosecuter must make a very strong argument when they are discussing the pretrial detention of a minor. In 2012, the last year of pretrial detention in our estimation sample, pretrial detention happened in 6.36% of the cases involving juvenile offenders.

There are several outcomes from a trial: a custodial sentence (either full or partial); a non-custodial sanction, which includes probation, community service, and reparation for damage caused; an alternative sanction, as in the case of drug treatment programs; and being judged as non-guilty. An aspect of trials that is very relevant for the exclusion restriction of one of the instrumental variables considered in this paper, is detention judges do not have a role in the trial outcome. This decision is made by the (three) judges of the oral proceedings court.

The institution that is in charge of any juvenile incarceration – including pretrial detention – is the National Service for Minors (SENAME), which is dependent on the Ministry of Justice. Alternative measures that involve non-custodial sanctions, are overseen by private institutions, who are supervised by SENAME.

In this context, the definition of the first treatment whose effect is studied in this paper is to have been incarcerated as juvenile because of pretrial detention. In this case, the control group is juveniles who were accused of a severe crime, who either weren't sent to penal procedures or if they did, were not required to be incarcerated pretrial. In turn the second treatment whose effect is studied in this paper, is to have been incarcerated as juvenile, either because of a pretrial detention or because a custodial sentence (either full or partial). In this case, the control group is given by juveniles who were accused of a severe crime, who were either sentenced with non-custodial or alternative sanctions or found innocent.

Finally, and even though this is something that will be formally tested in the empirical strategy section, it is relevant to stress that both the assignment of detention judges and of public attorneys do not depend on the characteristics of the prosecuted youth or of the criminal case. Indeed, in any particular court, detention judges and public attorneys are allocated across days and cases depending on the different workloads, trying to equalize them. Hence, conditional on court and year, these assignments can be considered random.

3 Data

3.1 Data sources

We assemble an administrative dataset from the Ministry of Education and the Public Defender's Office (*Defensoría Penal Pública*, DPP). The DPP provides free legal representation for almost all individuals (including youths) who have been accused of committing a crime. For those youths who are not legally represented by a DPP attorney, *i.e.,* who have a private attorney, we can observe the alleged crime but not the final verdict. That said, in our dataset only 1.1% of prosecuted youths have a private attorney. The DPP data is a administrative panel dataset that – among other things – contains information on the alleged crime, verdict, an ID for the public attorney, if there was pretrial detention, and the time in jail. On top of this, we added the name of the detention judges for each prosecution.

The information collected from the Ministry of Education is an administrative panel dataset from 2002 to 2018, which – for every student in the country – includes the school attended every year, the grade level (and whether the student has repeated the grade), the student's attendance rate, GPA, some basic demographic information.

3.2 Sample construction

The starting point of building our estimation samples is a dataset of 33,266 youths, who were prosecuted between 2008 and 2012 when they were between 15 and 17 years old, and who had any educational record between 2002 and 2013. This information comes from administrative data, therefore it includes all individuals who meet the previous conditions. These conditions are in place because the administrative data on youths only becomes available after 2007, we have information until 2018, and we define adult recidivism as any prosecution between 18 and 21 years old. [8] Thus, 2012 is the oldest

[8] We have other definitions of recidivism, but this is the main one. For example, we also define recidivism as conviction at these ages.

year in which we can consider individuals who were 15 years old in 2015, allowing us to observe them until they are 21 years-old.

We use the described database to estimate the leniency of judges' pretrial decisions and a measure of the DPP attorneys' quality, the two instrumental variables considered in this paper. However, we need to make further restrictions to have an appropriate estimation sample given our research questions. To start with, we restrict the sample to the first relevant crime for each juvenile. We define the first relevant crime as one where the incarceration rate is above 5%.[9] We do this because there are many types of crimes in which – regardless the assigned judge or the defence attorney – the probabilities of incarceration are very low due to the nature of the crime. We also restrict our attention to courts with at least 3 judges and to cases whose judges (or attorneys, depending on the instrument considered) have at least 30 cases per year. These restriction are due to the fact that the empirical strategy requires that the instrumental variables have different values conditioning on court and year.

After these restrictions, our final estimation sample has –in the case of the pretrial estimation model– 4,386 juveniles, 986 of them who were incarcerated pretrial. In the case of the conviction model, the estimation sample includes 7,730 juveniles, 1,826 who were incarcerated.

Tables 1 and 2 show to what extent the estimation samples are different in respect to the population. Overall, in both cases the samples are very similar in terms of defendant characteristics and in terms of the outcomes considered in our estimations. However the individuals in the estimation sample are prosecuted for more severe crimes in respect to the population. Indeed, homicides and violent robbery are about three times more frequent in the estimation samples comparatively. This difference is not problematic, since it is exactly what we want to produce with the restrictions that we impose to construct the estimation sample, i.e., focusing on more severe crimes.

The tables are informative concerning the individual characteristics of the samples considered in this paper. Namely, the average age in our estimation sample is little above 16 years-old, most of them are male, and they tend to low performance high school students: almost 60% repeated at least one grade, their GPA is about 4.5 (grades are between 1 and 7), and only between 22 and 31% of them graduate from high school.

[9]We use a classification from the DPP, which has 180 types of crimes.

As a benchmark, these values – for all high school students in Chile – are the following: 24% repeated at least one grade in high school,[10] the average GPA in 2012 was 5.4 and the 10th percentile was 4.6, and 88% of youths graduate from high school.

As it will become clear in following sections, to obtain causal estimates in the context of our empirical strategy only requires the DPP database, yet the inclusion of the educational data makes our estimates more convincing and allows us to study a potential mechanism behind the results. Thus, our main specification considers the estimation sample described above, but we also present the results of the estimations (in the cases where it is possible) considering the bigger sample.

[10] In Chile high school goes from 9th to 12th. This calculation was made for the cohort of students who were in 10th in 2008.

Table 1: The estimation sample when pretrial detention is the idenpendent variable of interet, and its comparison with population

	Full Sample		Estimation Sample	
	Pretrial Detention		Pretrial Detention	
	Yes	No	Yes	No
Panel A. Defendant Characteristics				
Age at Offense	16.19	16.15	16.19	16.11
	(0.80)	(0.80)	(0.79)	(0.80)
Any Grade Retention (%)	60.8	57.5	59.3	57.8
	(48.8)	(49.4)	(49.2)	(49.4)
Latest GPA	4.47	4.47	4.54	4.46
	(1.08)	(1.19)	(0.99)	(1.11)
Latest Attendance	81.46	82.47	81.76	82.06
	(19.76)	(17.32)	(20.45)	(18.02)
Male (%)	94.9	84.6	94.2	93.8
	(22.1)	(36.0)	(23.4)	(24.2)
Panel B. Charge Characteristics				
Homicide (%)	5.5	0.5	5.0	1.4
	(22.9)	(6.9)	(21.7)	(11.7)
Violent Robbery (%)	67.7	18.4	72.2	56.1
	(46.8)	(38.8)	(44.8)	(49.6)
Non-Violent Robbery (%)	18.5	13.5	17.8	16.7
	(38.8)	(34.2)	(38.3)	(37.3)
Other Crime (%)	8.2	67.6	5.0	25.8
	(27.5)	(46.8)	(21.7)	(43.8)
Panel C. Outcomes				
Penal Prosecution (%)	79.2	60.0	79.5	66.8
	(40.6)	(49.0)	(40.4)	(47.1)
Conviction (%)	65.5	40.1	65.8	48.0
	(47.6)	(49.0)	(47.5)	(50.0)
Prosecution (Violent Crime) (%)	41.3	23.5	40.6	30.6
	(49.2)	(42.4)	(49.1)	(46.1)
Graduate from Highschool (%)	23.9	35.0	24.0	30.7
	(42.7)	(47.7)	(42.8)	(46.1)
Takes Admission Test for Selective Universities (%)	15.4	19.7	15.8	16.3
	(36.1)	(39.8)	(36.5)	(36.9)
Convicted (%)	72.3	26.9	73.1	51.2
	(44.7)	(44.4)	(44.4)	(50.0)
Observations	2,528	30,738	986	3,390

Notes: This table shows the descriptive statistics for the full sample and the estimation sample, dividing both groups between pretrial detained and those who were free during trial. The full sample is a dataset of 33,266 youths, who were prosecuted between 2008 and 2012 when they were between 15 and 17 years old, and who had any educational record between 2002 and 2013. The estimation sample, used to estimate the impact of pretrial detention on recidivism, further restrict the full sample to the first relevant crime for each juvenile (defined as one where the incarceration rate is above 5%) and only considering courts with at least 3 judges and to cases whose judges have at least 30 cases per year. Standard errors are in parenthesis.

Table 2: The estimation sample when conviction is the idenpendent variable of interet, and its comparison with population

	Full Sample		Estimation Sample	
	Incarceration		Incarceration	
	Yes	No	Yes	No
Panel A. Defendant Characteristics				
Age at Offense	16.19	16.15	16.18	16.11
	(0.79)	(0.80)	(0.79)	(0.79)
Any Grade Retention (%)	60.6	57.5	59.3	59.2
	(48.9)	(49.4)	(49.1)	(49.1)
Latest GPA	4.47	4.47	4.47	4.42
	(1.07)	(1.19)	(1.04)	(1.17)
Latest Attendance	81.40	82.48	81.07	81.84
	(19.95)	(17.29)	(20.20)	(17.81)
Male (%)	95.0	84.6	94.7	93.9
	(21.8)	(36.1)	(22.3)	(24.0)
Panel B. Charge Characteristics				
Homicide (%)	5.5	0.5	4.5	1.6
	(22.8)	(6.8)	(20.8)	(12.4)
Violent Robbery (%)	66.9	18.3	71.1	54.8
	(47.1)	(38.7)	(45.3)	(49.8)
Non-Violent Robbery (%)	19.1	13.5	17.5	16.8
	(39.4)	(34.1)	(38.0)	(37.4)
Other Crime (%)	8.5	67.8	6.8	26.8
	(27.9)	(46.7)	(25.2)	(44.3)
Panel C. Outcomes				
Penal Prosecution (%)	79.6	59.9	78.9	66.9
	(40.3)	(49.0)	(40.8)	(47.1)
Conviction (%)	66.1	40.0	65.5	47.9
	(47.3)	(49.0)	(47.6)	(50.0)
Prosecution (Violent Crime) (%)	41.7	23.4	42.1	30.5
	(49.3)	(42.3)	(49.4)	(46.1)
Graduate from Highschool (%)	24.0	35.0	22.9	31.6
	(42.7)	(47.7)	(42.0)	(46.5)
Takes Admission Test for Selective Universities (%)	15.6	19.7	15.4	16.6
	(36.3)	(39.8)	(36.1)	(37.2)
Observations	2,653	30,613	1,826	5,904

Notes: This table shows the descriptive statistics for the full sample and the estimation sample, dividing both groups between those who were incarcerated (either before or after verdict) and those who were not incarcerated. The full sample is a dataset of 33,266 youths, who were prosecuted between 2008 and 2012 when they were between 15 and 17 years old, and who had any educational record between 2002 and 2013. The estimation sample, used to estimate the impact of any type of incarceration on recidivism, further restrict the full sample to the first relevant crime for each juvenile (defined as one where the incarceration rate is above 5%) and only considering courts with at least 3 judges and to cases whose public attorneys have at least 30 cases per year. Standard errors are in parenthesis.

4 Empirical Strategy

4.1 The Estimation Problem

Our main interest is to address the causal impact of both pretrial detention and incarceration on outcomes such as recidivism and high school completion. To that end, consider the following model that ties the outcome Y_i with an indicator variable for pretrial detention (PreTrial_i) or incarceration (Incar_i) for juvenile i:

$$Y_i = \beta_0 + \beta_1 \text{PreTrial}_i + \beta_2 X_i + \varepsilon_i \quad (1)$$

$$Y_i = \theta_0 + \theta_1 \text{Incar}_i + \theta_2 X_i + \epsilon_i, \quad (2)$$

where X_i is a vector of individual and case controls, and both ε_i and ϵ_i are error terms.

The problem of estimating (1) and (2) via OLS is that our main variables of interest (namely pretrial detention and incarceration) are likely to be correlated with unobserved juvenile characteristics that also affect the outcome variable, biasing the estimation. For example, youths whose parents and siblings have a criminal history record may have a positive correlation with both ending up with a custodial sentence and future recidivism.

To overcome the endogeneity issue, we use a measure of the leniency of a randomly assigned detention judge as an instrument for pretrial detention, and a measure of the quality of a randomly assigned attorney as an instrument for incarceration. The causal chain is as follows. Youths are randomly matched with judges (attorneys) with varying degrees of leniency (quality), which changes their pretrial detention (incarceration) status due to chance. Therefore, we interpret any difference in outcomes among youths as the causal effect of the change in the probability of pretrial detention (incarceration) associated with judge (attorney) assignment. As we will argue later in this section, the results from these estimations identifies a local average treatment effect (LATE) for the youth that is at the margin of a custodial conviction, be it pretrial detention or any kind of incarceration.

In addition to the instrumental variable estimations, we also estimate the marginal treatment effect by using a bivariate probit (biprobit) estimator as a robustness check. This specification allows us to drop the linearity assumption of the 2SLS estimator, at the expense of stronger distributional assumptions. This is a relevant robustness check, given the large marginal effects that we obtain by estimating the 2SLS model and the discrete nature of the dependent and endogenous variables in our settings. Unfortunately, these two approaches, the 2SLS and biprobit, not only differ in the mentioned aspects but also in the interpretation of their estimates. While the 2SLS identifies a LATE, the biprobit design finds the average treatment effect (ATE). The latter makes more complicated to have a clear picture about what is really driving the differences between the two marginal effects in a specific context.

4.2 IV Construction

The two instrumental variables considered in this paper are essentially leave-out means that capture how the detention judge or the public attorney assignments impact the probability of having a pretrial detention or been incarcerated for any reason respectively.

Recall from section 3 that while constructing our instrumental variables we use a dataset that includes all criminal records during the youth's adolescence. From this starting point, we only used judges/attorneys with more than 10 cases a year to construct the instruments. For youth i matched with judge j, we estimate the average pretrial detention rate using every other case handled by judge j, after adjusting for court-by-year fixed effects. Formally, we first estimate the residual from the following regression:

$$\text{PreTrial}_{jc} = \alpha_0 + \alpha_1' \text{court}_c \times \text{year}_{jc} + \xi_{jc} \tag{3}$$

We then proceed by calculating the judge leniency variable, denoted by $Z_{j(i)}^{\text{judge}}$:

$$Z_{j(i)}^{\text{judge}} = \frac{1}{N_j - 1} \sum_{k \neq i}^{N_j - 1} \hat{\xi}_{kc}.$$

Notice that for constructing the attorneys' quality measure, $Z_{a(i)}^{\text{attorney}}$, it suffices to replace PreTrial_i with Incar_i in (3), where a is now indexing the attorney assignment for youth i.

It should be noted that the two leave-out means are customary as to avoid having an artificial strong identification, given by the direct linkage between the youth's own endogenous outcome and the instrument.

To be clear, both instrumental variables capture the same underlying idea – the probability of ending incarcerated – but they do so through different channels. Judge leniency measures the propensity that a given detention judge has of giving pretrial detention to any given juvenile, while the attorney's measure provides a ratio of "fails" among the cases seen by a given attorney, where "fails" are defined as his juvenile client ending up in any kind of incarceration either pretrial detention or custodial sentence.

As previous research has noted, this procedure is numerically equivalent to the judge fixed effect in a jackknife regression of pretrial detention (or incarceration) estimated over all years. As a result, our two-stage least squares estimators are essentially jackknife instrumental variables estimators (JIVE), which are recommended when fixed effects are used to construct the instrument (see Stock et al. (2002) and Kolesár et al. (2015)).

IV Variation

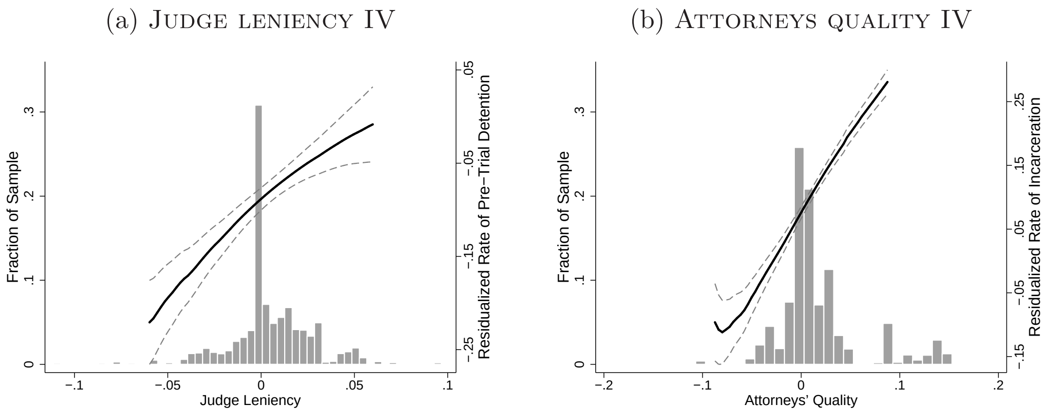
In figure 1 we present the distribution of the two instrumental variables used in this paper. The sample used to construct the instruments consists of 545 judges and 553 attorneys.[11] The average judge handle 118 cases, whereas the average attorney defends 113 cases. In the estimation sample, the mean of the leniency variable is 0.0048 with a standard deviation of 0.02, whereas the mean of the attorney's measure is 0.018 with a standard deviation of 0.043. The leniency measure ranges from -0.09 to 0.11, which in turn implies that moving from a more lenient to a less lenient judge is associated with a 20 pp. increase in getting pretrial detention, an 25% increase from the mean. Similarly, the attorneys' measure ranges from -0.14 to 0.22, which means that moving from the most qualified attorney to the worst is associated with a 36 pp. increase in the likelihood of ending with any kind of incarceration, a 47% increase from the mean.

4.3 IV Validity

In order to interpret the 2SLS estimates as a LATE, four conditions need to be met: (i) a non-trivial first stage, (ii) independence of the instrument, (iii) exclusion of the

[11] That said, in the estimation sample we only consider 332 attorneys who are those who work at the DPP. The others work for private firms that are hired by the DPP.

Figure 1: Distribution of the instruments and first stage



Notes: The figure of **panel (a)** reports the distribution of the judge leniency measure that is estimated following the procedure described in Subsection 4.2. It also shows the nonparametric estimation of the relationship between judge leniency and the residualized rate of pretrial detention. The figure of **panel (b)** reports the distribution of the attorneys quality measure that is estimated following the procedure described in Subsection 4.2. It also shows the nonparametric estimation of the relationship between attorneys quality and the residualized rate of incarceration.

instrument, and (iv) monotonicity. We now turn to discuss each of these conditions in our research.

Non-trivial First Stage

The right hand side of figure 1 shows the effect of the judge leniency on a youth's pretrial detention status, estimated via a local linear regression of the former against the latter after adjusting for court by year fixed effects, i.e. the residualized rate. The pretrial detention status varies monotonically along judge leniency in a fairly linear fashion, although it seems that the slope is steeper at the beginning. This suggest that moving away from the least lenient judge towards a more neutral one diminishes the chances of ending up in pretrial detention more aggressively than moving from a neutral one to a fairly lenient judge. The left hand side of figure 1 displays the analogous plot for the effect of the attorneys' quality on any kind of incarceration. As before, the incarceration rate varies monotonically across the attorney measure, while the graphical analysis suggests that this relationship is generally linear.

The first stage estimations are presented in tables 3 and 4. For both, pretrial detention and incarceration, we find that our instrumental variables are highly predictive of whether the youth ends with a custodial sentence. The magnitudes suggests that if a given youth is facing a judge who is 10 pp. more likely to give pretrial detention, then he is 17 pp.

more likely to get pretrial detention. Similarly, if a given youth is assigned an attorney who is 10 pp more likely to end up with his client facing any kind of incarceration, then the youth is 19 pp. more likely to end up incarcerated.

Table 3: First Stage: Judge Leniency

	Pretrial Detention
Judge IV	1.705***
	(0.356)
Age at Offense	0.023***
	(0.007)
Any Grade Retention	0.015
	(0.014)
Latest GPA	0.006
	(0.005)
Latest Attendance	0.001*
	(0.000)
Male	-0.019
	(0.025)
Homicide	0.477***
	(0.054)
Violent Robbery	0.251***
	(0.020)
Non-Violent Robbery	0.184***
	(0.023)
Observations	4,255
F Test	22.92

Notes: This table reports first-stage results for the linear IV model that estimates the effect of pretrial detention on recidivism. Thus, the regression is estimated on the sample as described in the notes to table 1. Regression includes year interacted with court fixed effects. Robust standard errors clustered at the judge level in parentheses. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

Independence

A key condition to be met is that these instruments are as good as random assignment. In order to verify that this assumption is true in our context, we present in tables 5 and 6 the same kind of analysis that would be performed in an actual experiment to assess the compliance and the randomization of the experiment. The first column displays the coefficients of a regression of the endogeneous variable against the covariates described in the rows, while the second column shows the same regression but now having the instrumental variables as the dependent variable. In table 5 we note that the age at the first relevant crime along with the crime-type dummies are highly predictive of

Table 4: First Stage: Attorneys' Quality

	Incarceration
Attorney IV	1.983***
	(0.311)
Age at Offense	0.023***
	(0.005)
Any Grade Retention	-0.007
	(0.011)
Latest GPA	0.003
	(0.005)
Latest Attendance	0.000
	(0.000)
Male	-0.013
	(0.020)
Homicide	0.381***
	(0.053)
Violent Robbery	0.227***
	(0.021)
Non-Violent Robbery	0.172***
	(0.021)
Observations	7,557
F Test	40.59

Notes: This table reports first-stage results for the linear IV model that estimates the effect of incarceration on recidivism. Thus, the regression is estimated on the sample as described in the notes to table 2. Regression includes year interacted with court fixed effects. Regression includes year interacted with court fixed effects. Robust standard errors clustered at the attorney level in parentheses. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

getting pretrial detention, whereas none of the baseline characteristics nor the crime-type dummies seem to predict the leniency of the assigned judge. This is further corroborated with the p-value of the joint significance test, which isn't able to reject the null hypothesis that neither of the coefficients is distinct from zero. The same is true in table 6, with the exception that here some variables do seem to predict the attorneys' quality (but they are modest in size). However, the p-value for the joint significance test is 0.3875, so again we can't reject the null.

Notice that it is the combination of these two regressions, in these two tables, that makes this test a convincing approach. Because while the first column shows that this covariates are very relevant in predicting the endogenous variable, the second column shows that these relevant covariates are not correlated with the instrumental variable, in a similar fashion to how one would test the validity of a RCT.

Table 5: Randomization test for judges leniency

	Pretrial Detention (1)	*Judge IV* (2)
Age at Offense	0.022***	-0.001
	(0.007)	(0.000)
Any Grade Retention	0.013	-0.001
	(0.014)	(0.001)
Latest GPA	0.007	0.000
	(0.005)	(0.000)
Latest Attendance	0.001	-0.000*
	(0.000)	(0.000)
Male	-0.021	-0.001
	(0.024)	(0.001)
Homicide	0.476***	-0.000
	(0.053)	(0.002)
Violent Robbery	0.251***	-0.000
	(0.020)	(0.001)
Non-Violent Robbery	0.184***	-0.000
	(0.023)	(0.001)
Joint Test	0.0000	0.3749
Observations	4,255	4,255

Notes: This table reports reduced form results testing the random assignment of cases to detention judges. Judge leniency measure is estimated following the procedure described in Subsection 4.2. Column 1 presents estimates from an OLS regression of pretrial detention on the variables listed and year interacted with court fixed effects. Column 2 reports estimates from an OLS regression of judge leniency IV on the variables listed and year interacted with court fixed effects. The p-value reported at the bottom of columns 1 and 2 (named *Joint Test*) is for an F-test of the joint significance of the variables listed in the rows with the standard errors clustered at the judge level. Robust standard errors clustered at the judge level in parentheses. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

Exclusion & Monotonicity

It is well known that neither the exclusion nor the monotonicity assumptions are directly testable. Instead, we argue that in our setting these conditions are plausibly met.

The exclusion restriction for the judge leniency IV requires that detention judge assignment only impacts youth's outcomes through the probability of getting pretrial detention. This is likely to be the case, because, conditioning on deciding to begin a penal proceeding, the only role of these judges is to prescribe precautionary measures. Recall that the verdict is determined by three different judges from an oral proceedings court.[12] For the attorney measure, it requites that attorney assignment only impacts

[12] A detention judge may also sentence the case if it is *simple* enough, this occurs in an abbreviated trial process only for non severe crimes. In general it is defined during the detention hearing at the Guarantee Court. We drop this handful of cases.

Table 6: Randomization test for attorneys quality

	Incarceration (1)	*Attorney IV* (2)
Age at Offense	0.022***	-0.000
	(0.005)	(0.000)
Any Grade Retention	-0.009	-0.001
	(0.011)	(0.001)
Latest GPA	0.003	0.000
	(0.004)	(0.000)
Latest Attendance	0.000	-0.000
	(0.000)	(0.000)
Male	-0.009	0.002*
	(0.020)	(0.001)
Homicide	0.409***	0.014**
	(0.050)	(0.006)
Violent Robbery	0.237***	0.005**
	(0.020)	(0.002)
Non-Violent Robbery	0.181***	0.004**
	(0.020)	(0.002)
Joint Test	0.0000	0.3875
Observations	7,557	7,557

Notes: This table reports reduced form results testing the random assignment of cases to public attorneys. Attorneys quality measure is estimated following the procedure described in Subsection 4.2. Column 1 presents estimates from an OLS regression of incarceration on the variables listed and year interacted with court fixed effects. Column 2 reports estimates from an OLS regression of attorneys quality IV on the variables listed and year interacted with court fixed effects. The p-value reported at the bottom of columns 1 and 2 (named *Joint Test*) is for an F-test of the joint significance of the variables listed in the rows with the standard errors clustered at the attorney level. Robust standard errors clustered at the attorney level in parentheses. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

youth's outcomes through the probability of ending up with any kind of incarceration. In general, in the Chilean juvenile penal system, public attorneys assist their defendants throughout the entire trial duration, and the only relevant role they play in the future life of their defendants is through the sentences those clients might face.[13] That said, even in the case that any of these exclusion restrictions was not met, which we think is not the case, we still can estimate the reduced form specifications and interpreting the sign and statistical significance of the estimates as evidence of causal effect.

The monotonicity assumption requires that juveniles sent to pretrial detention with a lenient judge would have also faced pretrial detention with a more severe one and vice versa. In the attorney context, the assumption requires that juveniles incarcerated with

[13]Ideally, we would like to estimate the effect of incarceration due to conviction independently from pretrial detention, but in that case the attorney quality IV would not meet the exclusion restriction, because it would affect the probability of recidivism not only through the effect of incarceration due to conviction, but also through the effect of pretrial detention.

a high quality attorney would also incarcerated if they had a low quality one and vice versa.

4.4 Bivariate Probit

Since in our empirical setting both endogeneous and outcomes variables are binary, we take advantage by presenting biprobit estimates as a robustness check. With these estimations we are able to present causal results under both linear and non-linear models.

One caveat with the biprobit model is that it is difficult to estimate it under the presence of high dimensional fixed effects as is in our case. To circumvent this technical complication, we propose a novel estimation algorithm that doesn't require the simultaneous estimation of the whole fixed effects. The approach is very simple and it should work well to the extent that there are enough data points – as in our case – within court-by-year group. In particular, we estimate a biprobit model for each court-by-year group, then calculate the marginal effect for each of these groups. Finally, we take the weighted average of the marginal effects as the estimator of quantity of interest. Notice that by doing this procedure, we identify the average marginal effect across individuals, not the marginal effect of the average individual.[14]

The biprobit estimations are a key element to our robustness analysis. As it clear below, the biprobit results confirm what we find using the 2SLS model for the impact of pretrial detention and incarceration on recidivism, but the magnitudes are much smaller under this approach. Presenting the results of this alternative specification is very relevant in light of the results of a Montecarlo exercise we present in Appendix A.2, where we show that when the bivarate probit is the actual data generating process (with parameters that are very similar to our context), our approach of dealing with fixed effects works very well, and the 2SLS overestimates the average marginal effects in a relevant way. Although this is the best scenario for the biprobit model, since its critical assumption about normality is met, the results of this Montecarlo exercise are not obvious because in this conterfactual there is no violation to any assumption of the 2SLS in delivering a LATE estimated parameter. That said, when comparing these two approaches we should keep in mind that while the 2SLS identifies a LATE, our biprobit design recovers the average

[14]It is important to make this clarification because, in general, marginal effects in non-linear model can be calculated using both approaches, and in our case we can only identify one approach.

marginal treatment effect.

5 Results

In this section, we examine the effects of juvenile (15-17 years old) pretrial detention, and also the effects of any type of juvenile incarceration – pretrial detention or conviction – on different measures of recidivism as adult (18-21 years old), using the judge and public attorney IV strategies described above. To study possible mechanisms, we also show how these different types of incarcerations affect educational outcomes: high school graduation and whether the individual took the national admission test for selective universities. This test, the PSU (*prueba de selección universitaria*) is very similar to the American SAT test, and it is a requirement to apply for some selective universities. Thus, to take this test can be considered a measure of a successful high school educational experience and graduation, especially for low performance students like those in our estimation sample.

We present the results for all the different specifications in tables with the same structure. Specifically, the first column in all them provides the mean and the standard deviation (in brackets) of the dependent variables, calculated for the control group. The next columns report the point estimates of the parameters of interest and their standard deviations for different specifications (in columns) and dependent variables (in rows). The second presents the OLS result, which is included as a benchmark. The third to fifth columns show the IV estimations, which are in general the 2SLS model, but can be the bivariate probit in some cases of the robustness check subsection. These IV models are different across columns in terms of the set of covariates that are included in the estimation, where the last column presents the preferred specification, i.e., the one that considers a more complete set of covariates.

The effect of juvenile pretrial detention

We first discuss the effect of pretrial detention. Table 7 presents the 2SLS results for the impact of pretrial detention on conviction, recidivism, and educational outcomes. Regarding conviction, although the point estimates have relevant magnitudes, they are not statistically significant. We do find strong evidence of an impact on recidivism, regardless of how it is measured. In particular, the table shows that pretrial detention causes an increase of 61 pp in the probability of additional penal prosecution as adult, and similar magnitudes in the effects on other recidivism measures. We also find an impact of pretrial detention on educational outcomes; this type of incarceration reduces the probability of high school graduation by 55 pp and the probability of taking the university admission test by 33 pp. One remarkable element of this table, also present in all the other 2SLS tables, is that the point estimates are very stable as we add more covariates to the model, which reinforces the idea that the judge leniency is a appropriate instrumental variable in this context.

Table 7: Effect of juvenile pretrial detention on recidivism and educational outcomes

	Control Mean	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Panel A. Trial Outcomes					
Convicted	0.512	0.148***	-0.261	-0.262	-0.258
	[0.500]	[0.020]	[0.266]	[0.268]	[0.250]
Panel B. Recidivism as Adult Outcomes					
Penal Prosecution	0.668	0.135***	0.583**	0.615**	0.614**
	[0.471]	[0.019]	[0.253]	[0.258]	[0.250]
Conviction	0.480	0.162***	0.638**	0.618**	0.618**
	[0.500]	[0.021]	[0.266]	[0.266]	[0.257]
Prosecution (Violent Crime)	0.306	0.108***	0.656***	0.635***	0.632***
	[0.461]	[0.019]	[0.230]	[0.231]	[0.226]
Panel C. Educational Outcomes					
Graduate from Highschool	0.307	-0.063***	-0.514**	-0.539**	-0.554**
	[0.461]	[0.021]	[0.249]	[0.256]	[0.254]
Takes Admission Test for Selective Universities	0.163	-0.012	-0.308*	-0.327*	-0.326*
	[0.369]	[0.014]	[0.166]	[0.174]	[0.174]
CourtxYear FE	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	3,310	4,255	4,255	4,255	4,255

Notes: This table presents the OLS and two stage least squared estimations for the impact of juvenile pretrial detention on different outcomes: conviction, different measures of recidivism, and educational outcomes. All regressions include year interacted with court fixed effects. Robust standard errors clustered at the judge level in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

The effect of any type of juvenile incarceration

Table 8 presents the 2SLS results for the impact of any type of juvenile incarceration on recidivism and educational outcomes.[15] In particular, we find that incarceration causes an increase of 65 pp in the probability of penal prosecution as adult, and we find

[15] Because conviction is part of the definition of this treatment, it does not make any sense to study the impact of the treatment on conviction.

a bigger effect if we measure recidivism as conviction (86 pp) and a smaller effect if we measure recidivism as prosecution for a violent crime (46 pp). Regarding mechanisms, we find that incarceration reduces the probability of high school graduation by 35 pp; there is no effect on the probability of taking the admission test to college. As in the case of 2SLS for pretrial detention, the point estimates are stable, and if anything the magnitude of the effect seems to increase as we add more covariates.

Table 8: Effect of juvenile incarceration on recidivism and educational outcomes

	Control Mean	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Panel A. Recidivism as Adult Outcomes					
Penal Prosecution	0.669	0.128***	0.587***	0.587***	0.650***
	[0.471]	[0.013]	[0.174]	[0.174]	[0.198]
Conviction	0.479	0.162***	0.777***	0.785***	0.863***
	[0.500]	[0.017]	[0.201]	[0.203]	[0.226]
Prosecution (Violent Crime)	0.305	0.130***	0.438***	0.428***	0.475***
	[0.461]	[0.014]	[0.127]	[0.127]	[0.142]
Panel B. Educational Outcomes					
Graduate from Highschool	0.316	-0.100***	-0.266**	-0.300**	-0.353**
	[0.465]	[0.017]	[0.135]	[0.129]	[0.141]
Takes Admission Test for Selective Universities	0.166	-0.019*	-0.051	-0.047	-0.048
	[0.372]	[0.010]	[0.113]	[0.106]	[0.124]
CourtxYear FE	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	5,786	7,557	7,557	7,557	7,557

Notes: This table presents the OLS and two stage least squared estimations for the impact of juvenile incarceration on different measures of recidivism and educational outcomes. All regressions include year interacted with court fixed effects. Robust standard errors clustered at the attorney level in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

Overall, there are two important results regarding our research question. Firstly, both juvenile pretrial detention and any type of juvenile incarceration have a very important effect on different measures of recidivism. Secondly, their impacts on high school graduation seem to be a relevant mechanism that helps to explain the effect of these types on juvenile imprisonment on recidivism as adult.

5.1 Robustness Checks

We present a set of alternative specifications as a robustness check. We do so for both the impact of pretrial detention and the impact of any type of incarceration.

In the first robustness check exercise, mainly motivated by the relevant magnitudes we obtain from linear models and the dichotomatic nature of the dependent and independent variables, we estimate the treatment effects using the bivariate probit model. Table 9 shows this exercise for the impact of juvenile pretrial detention. Concerning conviction, we find positive effects and mixed results regarding statistical significance. In keeping with the linear model, but with smaller magnitudes, we find that pretrial detention causes an increase of 12 pp in the probability of a new penal prosecution as adult, and again we find similar magnitudes for the other recidivism measures. Regarding mechanisms, similar to the linear model, we find an impact of pretrial detention on high school graduation, but now of 12 pp. However, in this specification we do not find an effect on the probability of taking the university admission test.

Table 10 shows the estimations for the impact of juvenile incarceration using a bivariate probit model. Specifically, we find that incarceration causes an increase of 15 pp in the probability of a new penal prosecution as adult, and – as in the case of the linear model – we find a bigger effect by measuring recidivism as any conviction (20 pp) and a small effect if we measure recidivism only as prosecution for a violent crime (13 pp). About mechanisms, we find an impact of pretrial detention on high school graduation, but now 12 pp. Surprisingly, in this specification we find a positive effect of 6 pp on the probability of taking the admission test to college, a result that is only found in this alternative specification.

There are two elements to highlight given the comparison between the linear and non-linear models. First, we observe very similar results in all the outcomes in terms of the direction of the effect and statistical significance, except for the probability of taking the admission test for selective universities. One possible explanation for this difference is that the approach used in this paper to estimate the bivariate probit with fixed effects does not work well when the outcome occurs very rarely, as in the case of taking the university test. Second, the differences are more salient regarding magnitudes, where the linear model shows point estimates that are between three and five times larger in respect to the non linear model. As stated, the latter is in line with the Montecarlo experiment results, presented in the Appendix A.

From a theoretical point of view, as we discussed in the empirical strategy section, there are two important reasons why these linear and non linear IV models may deliver different estimates for marginal effects. It may be due to the fact their identification of causal effects rest on different assumptions; in the case of bivaraite probit an assumption about the normality in the distribution of the unobserved component in the two equations. It may also be due to the fact that in the case of the linear model, the identified marginal effects is local (i.e. a LATE), while in the case of the non linear model it is identified the average marginal effect across individuals.

Table 9: Effect of juvenile pretrial detention on recidivism and educational outcomes (nonlinear model)

	Control Mean	OLS	biprobit	biprobit	biprobit
	(1)	(2)	(3)	(4)	(5)
Panel A. Trial Outcomes					
Convicted	0.512	0.148***	0.200***	0.153***	0.058
	[0.500]	[0.020]	[0.051]	[0.052]	[0.051]
Panel B. Recidivism as Adult Outcomes					
Penal Prosecution	0.668	0.135***	0.141***	0.094*	0.121**
	[0.471]	[0.019]	[0.045]	[0.048]	[0.047]
Conviction	0.480	0.162***	0.151***	0.120**	0.100*
	[0.500]	[0.021]	[0.055]	[0.059]	[0.055]
Prosecution (Violent Crime)	0.306	0.108***	0.160***	0.106**	0.128**
	[0.461]	[0.019]	[0.054]	[0.052]	[0.056]
Panel C. Educational Outcomes					
Graduate from Highschool	0.307	-0.063***	-0.067	-0.114**	-0.115***
	[0.461]	[0.021]	[0.056]	[0.049]	[0.044]
Takes Admission Test for Selective Universities	0.163	-0.012	0.019	0.004	-0.003
	[0.369]	[0.014]	[0.045]	[0.036]	[0.034]
CourtxYear FE	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	2,440	3,009	3,009	3,009	3,009

Notes: This table presents the OLS and bivariate probit estimations for the impact of juvenile pretrial detention on different outcomes: conviction, different measures of recidivism, and educational outcomes. The bivariate probit model, that in these cases include year interacted with court fixed effects, is estimated following the procedure described in subsection 4.4. The standard errors are calculated following the procedure described in appendix A.3. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

In the second robustness check exercise, we replicate the previous linear model estimations but now controlling by courts' covariates by year as opposed to controlling by courts' fixed effects. Those include the number of cases, the number of judges (a proxy of size), the fraction of cases where the pretrial detention was enforced, the fraction of cases with conviction, and the fraction of cases with severe crime implications (defined as crimes with the probability of ending in imprisonment being above the 75th percentile). These estimation results are presented in Appendix B.1. The first aspect to stress is that only the judge leniency IV passes the test of randomness when we control for courts' co-

Table 10: Effect of juvenile incarceration on recidivism and educational outcomes (nonlinear model)

	Control Mean	OLS	biprobit	biprobit	biprobit
	(1)	(2)	(3)	(4)	(5)
Panel A. Recidivism as Adult Outcomes					
Penal Prosecution	0.669	0.128***	0.087**	0.133***	0.145***
	[0.471]	[0.013]	[0.038]	[0.033]	[0.029]
Conviction	0.479	0.162***	0.139***	0.197***	0.196***
	[0.500]	[0.017]	[0.043]	[0.033]	[0.032]
Prosecution (Violent Crime)	0.305	0.130***	0.171***	0.155***	0.127***
	[0.461]	[0.014]	[0.038]	[0.034]	[0.033]
Panel B. Educational Outcomes					
Graduate from Highschool	0.316	-0.100***	-0.046	-0.071**	-0.059*
	[0.465]	[0.017]	[0.039]	[0.033]	[0.032]
Takes Admission Test for Selective Universities	0.166	-0.019*	0.061*	0.059**	0.042*
	[0.372]	[0.010]	[0.036]	[0.028]	[0.024]
CourtxYear FE	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	4,146	5,358	5,358	5,358	5,358

Notes: This table presents the OLS and bivariate probit estimations for the impact of juvenile incarceration on different measures of recidivism and educational outcomes. The bivariate probit model, that in these cases include year interacted with court fixed effects, is estimated following the procedure described in subsection 4.4. The standard errors are calculated following the procedure described in appendix A.3. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

variates (table 13), and public attorney quality IV does not pass the randomness test, since the null hypothesis is rejected at a 5% significance level (table 14). This means that only in the case of pretrial detention controlling by courts' covariates is as good as controlling by fixed effects to ensure random assignment of the instrument (judge leniency or public attorney quality respectively). Therefore, we focus our attention on the estimation results for pretrial detention.

Table 15 presents these results for the 2SLS model. As can be observed, compared with table 7, the estimates are very similar between the two approaches – with and without fixed effects – even in terms of magnitudes.

In the third robustness check exercise, we estimate the same 2SLS models but with a new the estimation sample. In particular, we now consider a larger sample size by only using the information from DPP, as we lost data when merging with the educational database. By doing so, we take advantage of the fact that our statistical tests show that the instrumental variables are uncorrelated to educational covariates, thus not considering these covariates is not a concern for identification of the causal effects. Obviously, in this case we cannot estimate the impacts on educational outcomes.

We present these exercises in Appendix B.2. Tables 17 and 18 show the impact of pretrial detention and incarceration for this alternative estimation sample respectively. The first element to notice, by comparing these tables with tables 7 and 8, is that by non dropping from our sample the individuals who do not have educational records in this period, we increase our sample sizes by about 1,000 individuals. The second, and more important element to highlight is that these estimations present very similar results compared to those in the main specification.

Overall, the robustness check analysis allows us to conclude that there is a very strong evidence for the effect of juvenile pretrial detention and any type of juvenile incarceration on recidivism as adult; and also on the relevance of high school graduation as a mechanism for this result. That said, and given the differences in the magnitudes of the marginal effects between the linear and non linear IV models, we need to be careful about the conclusion regarding magnitudes.

6 Heterogenity and Marginal Treatment Effects

To better understand why we have very different magnitudes of the marginal effects between the linear and non linear IV models (2SLS and biprobit, respectively), we complement the previous analysis by estimating the marginal treatment effects (MTE), following Heckman and Vytlacil (2005). This approach allows us to study to what extent the impact of juvenile imprisonment on recidivism vary across the youths in our sample.

As previously discussed, besides that their identification of causal effects rest on different assumptions, linear and non linear IV models may deliver different estimates for marginal effects because the former estimates the LATE and the latter estimates the ATE, which can be very different when the effect is heterogenous. Moreover, as Heckman et al. (2006) emphasize, LATE estimates are not only local in the sense that they represesnt the average treatment effect for the compliers, they are also weighted averages of the effects across compliers, where those wieghts do not have any relevant policy evaluation interpretation. In this context, the estimation of the MTE is useful because, as Heckman and Vytlacil (1999) show, any LATE estimate can be obtained as a weighted average of the MTE.

To describe this approach, we follow Doyle (2007), using the same model and notation introduced in Section 4, with the only difference being that the effect of pretrial detention on recidivism (Y) is heterogeneous (β_1^i).[16]

$$Y_i = \beta_0 + \beta_1^i \text{PreTrial}_i + \beta_2 X_i + \varepsilon_i \tag{4}$$

Let $\bar{\beta}_1$ be the average treatment effect, thus we can rewrite equation 4 as:

$$Y_i = \beta_0 + \bar{\beta}_1 \text{PreTrial}_i + \beta_2 X_i + (\beta_1^i - \bar{\beta}_1)\text{PreTrial}_i + \varepsilon_i \tag{5}$$

In this context, there are two potential econometric problems in estimating the causal effect of pretrial detention on recidivism. First, $PreTrial$ can be correlated with ε. Second, $PreTrial$ can be correlated with β_1^i. The latter occurs when judges make their decisions considering how pretrial detention could affect the probability of recidivism in the case of the given individual. To address these issues simultaneously, we need a model that describes how judges make their decisions and an instrument (Z) that impacts the probability of pretrial detention but do not directly affect recidivism, i.e., an exclusion restriction.

Let δ_i be the probability of future misconduct of youth i during the trial, where this misconduct is the behavior that pretrial detention is trying to prevent. This information is observed by judges. Given this probability δ each judge has a specific threshold ($-Z_i\gamma$), such that the judge decides to apply pretrial detention when $\delta_i \geq Z_i\gamma$.

Given this framework, the MTE and the ATE that can be derived from the MTE are identified under the following conditions: $E(Z\delta) = 0$; $E(Z\epsilon) = 0$; $E(Z(\beta_1^i - \bar{\beta}_1)) = 0$; and $\gamma \neq 0$. In our case, given that judges (whose leniency is equal to Z) are quasi-randomly assigned at the court-by-time level, it is reasonable to think that the first three conditions are met. The last condition is met given that we already showed that judges' leniency does impact the probability of receiving pretrial detention.[17] Then, letting $P(z)$ equal

[16] In this section we describe the MTE approach focusing on pretrial detention as the endogenous variable, but, as before, we estimated this model considering both juvenile pretrial detention and juvenile incarceration as the endogenous variable.

[17] Table 3 in the case of judge leniency and Table 4 in the case of attorney quality.

$P(PreTrial = 1|Z = z)$, the MTE is given by:

$$\beta_1^{mte} = \frac{\partial E(Y)}{\partial P(z)}.$$

We obtain these estimates using a semiparamteric approach, where the treatment model is estimated using a probit model and the MTE parameters are obtained using the local instrumental variable approach (LIV).[18] Figure 3, in Appendix B.3, shows how the different outcomes presented during this paper (conviction, different measures of recidivism, and educational outcomes) vary across youths who are induced into pretrial detention condition as the probability of pretrial detention varies with the instrument. From these plots it is clear that the marginal effect is very heterogeneous across individuals. Specifically, they show that the magnitudes of the marginal effects are larger for those individuals who have low probabilities of pretrial detention. Thus, in the case of positive impacts (the three measures of recidivism), the marginal effects are larger for those with a low probability of treatment, and in the case of negative impacts (high school graduation and takes the PSU), the marginal effects are smaller for those with a low probability.[19] Notice, however, that the standard errors are too wide to statistically reject a slope of zero. Given these results, it is arguable that part of the reason why the 2SLS estimations (LATE) deliver very relevant magnitudes is because the compliers with more weights are those with a low probability of treatment.[20]

To have a better comparison among the 2SLS, biprobit and MTE estimates, in Tables 11 and 12 we present the ATE calculated from the MTE estimates for the two treatment discussed in this paper, considering the same sets of outcomes presented in Section 5. In general, in this case we get estimates that are in between the marginal effects coming

[18]We do so by using the STATA command *margte*, see Brave and Walstrum (2014) for details. Notice that the identification in this semiparametric approach depends crucially on the common support assumption for the propensity score, which requires that there are positive frequencies of $\widehat{P(z)}$ in the range of (0,1) for both individuals who are pretrial detained and for those who are not. Figures 5 and 6 in Appendix B.3, show the common support in the case of judge instrument and attorney instrument, respectively.

[19]Notice that figure 5 (Appendix B.3) shows that the semiparametric estimations of MTE should not consider estimates for propensity scores beyond 0.8. The same is true in the case of Figure 6, where the IV is attorney quality.

[20]The analysis and the conclusions are very similar if we consider the MTE of the juvenile incarceration and the attorney quality as the sources of exogenous variation in the probability of treatment (figure 4).

from the 2SLS and the biprobit,[21] with few exceptions and with some point estimates that are not statistically significant. In particular, the impact of pretrial detention on recidivism, measured as a new penal prosecution as a young adult, is equal to 28 pp, which is not statistically significant (Table 11). Meanwhile that marginal effect is equal to 61 pp when it is estimated by 2SLS and 12 pp using a bivariate probit model. A similar phenomenon occurs when we compare the marginal effects of juvenile incarceration on recidivism (measured as a new penal prosecution as a young adult), where the ATE (from MTE) is equal to 36 pp, while that estimate is equal to 65 pp in the case of 2SLS and 15 pp in the case of the bivariate probit model. These differences reinforce the concern about the local nature of the linear IV models and the specificsweights that produce the LATE estimates.

Table 11: Marginal treatment effect of juvenile pretrial detention on recidivism and educational outcomes

	Control Mean	OLS	MTE
	(1)	(2)	(3)
Panel A. Trial Outcomes			
Convicted	0.512	0.147***	-0.103
	[0.500]	[0.020]	[0.248]
Panel B. Recidivism as Adult Outcomes			
Penal Prosecution	0.668	0.130***	0.280
	[0.471]	[0.016]	[0.350]
Conviction	0.480	0.158***	0.268
	[0.500]	[0.023]	[0.284]
Prosecution (Violent Crime)	0.306	0.104***	0.642***
	[0.461]	[0.019]	[0.248]
Panel C. Educational Outcomes			
Graduate from Highschool	0.307	-0.068***	-0.654**
	[0.461]	[0.020]	[0.332]
Takes Admission Test for Selective Universities	0.163	-0.015	-0.187
	[0.369]	[0.018]	[0.190]
CourtxYear Controls	–	Yes	Yes
Individual Controls	–	Yes	Yes
Case Controls	–	Yes	Yes
Observations	3,303	4,242	4,242

Notes: This table presents the OLS and the marginal treatment estimations for the impact of juvenile pretrial detention on different outcomes: conviction, different measures of recidivism, and educational outcomes. Column (3) reports the average treatment effect (ATE), which is calculated from the marginal treatment effects estimated following the nonparametric procedure described in the current section. All regression control for courts' covariates. Standard errors in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

[21]Notice that all these estimated ATE should be viewed with caution since the calculation of ATE parameters from semiparateric MTE estimates requires that the common support is met across all the possible values, from 0 to 1.

Table 12: Marginal treatment effect of juvenile incarceration on recidivism and educational outcomes

	Control Mean	OLS	MTE
	(1)	(2)	(3)
Panel A. Recidivism as Adult Outcomes			
Penal Prosecution	0.669	0.132***	0.359*
	[0.471]	[0.013]	[0.203]
Conviction	0.479	0.168***	0.592**
	[0.500]	[0.017]	[0.247]
Prosecution (Violent Crime)	0.305	0.122***	0.292
	[0.461]	[0.014]	[0.270]
Panel B. Educational Outcomes			
Graduate from Highschool	0.316	-0.098***	-0.136
	[0.465]	[0.016]	[0.163]
Takes Admission Test for Selective Universities	0.166	-0.017	0.228
	[0.372]	[0.011]	[0.215]
CourtxYear Controls	–	Yes	Yes
Individual Controls	–	Yes	Yes
Case Controls	–	Yes	Yes
Observations	5,723	7,457	7,457

Notes: This table presents the OLS and the marginal treatment estimations for the impact of juvenile incarceration on different measures of recidivism and educational outcomes. Column (3) reports the average treatment effect (ATE), which is calculated from the marginal treatment effects estimated following the nonparametric procedure described in the current section. All regression control for courts' covariates. Standard errors in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

7 Conclusion

This paper studies the effect of juvenile incarceration on young adult recidivism. One novelty of this paper is that it also studies the effect of pretrial detention as a specific type of incarceration. For both pretrial detention and incarceration, the paper shows very relevant impacts of those on adult recidivism. A possible explanation for these results can be found in Bayer et al. (2009) and Stevenson (2017), who find evidence of peer effects, such that juvenile offenders serving time in the same correctional facility influence each other's subsequent criminal behavior. Furthermore, the results of this paper show that an important mechanism behind the impact of juvenile incarceration on recidivism is the effect of incarceration on high school graduation.

The results are qualitative robust to different specifications. The MTE estimates show that the effect of juvenile incarceration on recidivism is very heterogeneous, with magnitudes that are larger for those individuals who have lower probabilities of treatment. This heterogeneity can partially explain why the linear and non- linear IV models deliver very different results in terms of magnitudes. That said, it should be stressed that all our empirical strategies (2SLS, biprobit, and MTE) deliver marginal effects that are relevant from a public policy point of view; because in all these cases a change in juvenile incarceration policy would imply a significant reduction in young adult crime.

This causal evidence calls into question the appropriateness of juvenile incarceration as a public policy, in light of the long-term effects that it may have on individuals' lives. A concern that is even more relevant is the case of pretrial detention, given that this precautionary measure is made by a detention judge in few minutes and without any serious and detailed discussion about if such a measure is needed. This is also particularly troubling when it comes to the principle of presumed innocence.

It is well known that the teen years are a critical developmental period, featuring major physical, psychological and attitudinal transitions. Thus these results are particularly troubling, as the juvenile penal system should be very careful to not causing damage to juveniles, their futures, and therefore society during this critical time. That said, it should be noted that the findings of this paper do not preclude the possibility that juvenile incarceration has an effect of deterrence on crime, as it is shown in Drago et al. (2009) and Levitt (1998).

Finally, it should be noted that to have a better understanding about what good alternatives to incarceration measures might be, we would need better data on the programs given to our control group. As stated in the paper, when the treatment is defined as any type of juvenile incarceration, the control group is juveniles who were accused of a severe crime who either were sentenced with non-custodial or alternative sanctions or found innocent and recieved no punishment at all. Thus our causal results are given by the effect of incarceration relative to these alternative measures (including no punishment). Therefore, we would like to have better data on these alternatives programs to learn which programs are having the best success since probably not punishing any juvenile crime of any type is not an effective strategy, as in the case of Di Tella and Schargrodsky (2013).[22]

[22]Smith and Akers (1993) and Spohn and Holleran (2002) are other examples along these lines, although the evidence presented in those papers are not necessary causal.

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Appendix

A Montecarlo exercise to compare linear and non linear IV model when the dependent and endogenous variables are discrete

We now turn to discuss the Montecarlo simulation mentioned in section (??). This appendix starts with the description of the simulation, then we discuss the results of this simulation, and finally end with the bootstrap method used to obtain the standard errors.

A.1 The Montecarlo

Preliminaries.

Consider the following biprobit model:

$$PP_i = \mathbb{1}\{\alpha_g + \delta_0 Z^{\text{judge}}_{j(i)} + \delta_1' X_i + \varepsilon_i > 0\} \tag{6}$$

$$R_i = \mathbb{1}\{\theta_g + \beta_0 PP_i + \beta_1' X_i + \nu_i > 0\}. \tag{7}$$

Where g indexes the court-by-year group, PP_i denotes a binary variable for pretrial detention status of youth i, R_i stands for recidivism as an adult, X_i represents a vector of controls, and (ε_i, ν_i) are normal bivariate random variables distributed with a correlation coefficient ρ.

In order to simulate the model, we need to estimate the parameters in (6) and (7), along with ρ. We do so as follows:

1. We estimate equations (6) and (7) for each court-by-year group with more than 30 observations, and proceed to estimate $\{\delta_0, \delta_1, \beta_1, \rho\}$ by taking the average of these parameters across groups. In a similar fashion, we consider each pair of $\{\alpha, \theta\}$ estimated for group g as the estimator of the fixed effect for this group.

2. Set $\hat{\beta}_0$ such that the weighted average marginal effect matches the one estimated by our algorithm.

A key problem with simulating the aforementioned models is the need to generate enough variation within each group. Since the minimum observations required for a group are 30, this means that the variation needs to be high. Thus we scaled the random component variance up to 20 times, and also truncated $\hat{\alpha}_g$ to lie within the $[-5, 5]$ interval and set θ_g equal to -3.5. In an attempt to further increase the variation, we subtract 100 from $\hat{\alpha}_g$, which helps to avoid having too many values equal to one in the simulated pretrial detention variable ($\tilde{PP}_i$) below.

The Simulation.
For each observation, we draw two random variables from:

$$\begin{bmatrix} \tilde{\varepsilon} \\ \tilde{\nu} \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 20 & 20\hat{\rho} \\ 20\hat{\rho} & 20 \end{bmatrix} \right) \tag{8}$$

Then we construct $\tilde{PP}_i, \tilde{R}_i$ from

$$\tilde{PP}_i = \mathbb{1}\{\hat{\alpha}_g + \hat{\delta}_0 Z^{\text{judge}}_{j(i)} + \hat{\delta}_1' X_i + \tilde{\varepsilon}_i > 0\} \tag{9}$$

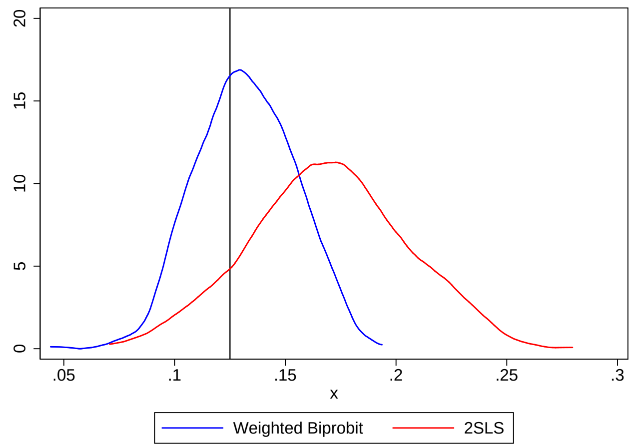
$$\tilde{R}_i = \mathbb{1}\{\hat{\theta}_g + \hat{\beta}_0 PP_i + \hat{\beta}_1' X_i + \tilde{\nu}_i > 0\}. \tag{10}$$

From here, we calculate the average marginal effect using our algorithm on the simulated data and fit the 2SLS model. We then repeat the process 700 times.

A.2 Results

The results of the Montecarlo simulation are presented in figure (2). The vertical line is placed at the average marginal effect estimated with the non-simulated data. From this figure, it becomes clear that 2SLS overestimates the ATE, whereas the biprobit average marginal effects (estimated with our algorithm) are almost centered with the one obtained with the non-simulated data.

Figure 2: Biprobit vs 2SLS Density



Notes: This figure shows the result of the Montecarlo exercise that compares the distribution of biprobit and 2SLS estimates, following the procedure described in section A.1. It should be noted that this exercise assumes that the data generating process meets the assumptions of biprobit.

A.3 Bootstrapping the Standard Errors

The standard errors displayed on the biprobit tables were computed by using bootstrap. In order to account for the court-by-year unit, we used a block-bootstrap with the following structure:

1. Fit our biprobit estimator and store the coefficient for each court-by-year group.

2. Draw a random number g from $1, 2, \ldots, N_g$, where N_g denotes the total number of court-by-year groups. Then store the coefficient corresponding to group g.

3. Repeat step (2) for a total of N_g times and take the weighted average of these coefficients, where the weights are given by the fraction of observations in group g relative to the total.

4. Repeat steps (2) and (3) 1000 times and proceed to compute the standard error as follows:

$$\text{Std. Err} = \sqrt{\frac{1}{999}\sum_{i=1}^{N_g}(\hat{\beta}_i - \overline{\hat{\beta}})}$$

B Robustness Checks: results for alternative specifications

B.1 Estimations controlling by courts characteristics instead of using fixed effects

Table 13: Randomization test for judge leniency (controlling for courts' covariates)

	Pretrial Detention (1)	*Judge IV* (2)
Age at Offense	0.023***	-0.000
	(0.007)	(0.000)
Any Grade Retention	0.013	-0.001
	(0.013)	(0.001)
Latest GPA	0.006	0.000
	(0.005)	(0.000)
Latest Attendance	0.001	-0.000
	(0.000)	(0.000)
Male	-0.015	-0.000
	(0.023)	(0.002)
Homicide	0.462***	0.001
	(0.053)	(0.002)
Violent Robbery	0.245***	0.000
	(0.020)	(0.001)
Non-Violent Robbery	0.177***	0.000
	(0.021)	(0.001)
Joint Test	0.0000	0.8394
Observations	4,242	4,242

Notes: This table reports reduced form results testing the random assignment of cases to detention judges. Judge leniency measure is estimated following the procedure described in Subsection 4.2. Column 1 presents estimates from an OLS regression of pretrial detention on the variables listed and also controlling for courts' covariates. Column 2 reports estimates from an OLS regression of judge leniency IV on the variables listed and also controlling for courts' covariates. The p-value reported at the bottom of columns 1 and 2 (named *Joint Test*) is for an F-test of the joint significance of the variables listed in the rows with the standard errors clustered at the judge level. Robust standard errors clustered at the judge level in parentheses. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

Table 14: Test of randomization for public attorney quality (controlling for courts' covariates)

	Incarceration (1)	*Lawyer IV* (2)
Age at Offense	0.021***	-0.000
	(0.005)	(0.001)
Any Grade Retention	-0.006	-0.002
	(0.011)	(0.002)
Latest GPA	0.003	-0.000
	(0.004)	(0.001)
Latest Attendance	0.000	-0.000*
	(0.000)	(0.000)
Male	-0.006	0.000
	(0.021)	(0.002)
Homicide	0.400***	0.017**
	(0.050)	(0.008)
Violent Robbery	0.234***	0.007**
	(0.019)	(0.003)
Non-Violent Robbery	0.182***	0.009***
	(0.018)	(0.003)
Joint Test	0.0000	0.0126
Observations	7,457	7,457

Notes: This table reports reduced form results testing the random assignment of cases to public attorneys. Attorneys quality measure is estimated following the procedure described in Subsection 4.2. Column 1 presents estimates from an OLS regression of incarceration on the variables listed and also controlling for courts' covariates. Column 2 reports estimates from an OLS regression of attorneys quality IV on the variables listed and also controlling for courts' covariates. The p-value reported at the bottom of columns 1 and 2 (named *Joint Test*) is for an F-test of the joint significance of the variables listed in the rows with the standard errors clustered at the attorney level. Robust standard errors clustered at the attorney level in parentheses. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively..

Table 15: Effect of juvenile pretrial detention on recidivism and educational outcomes (controlling by courts characteristics)

	Control Mean	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Panel A. Trial Outcomes					
Convicted	0.512	0.147***	-0.208	-0.216	-0.232
	[0.500]	[0.020]	[0.232]	[0.234]	[0.225]
Panel B. Recidivism as Adult Outcomes					
Penal Prosecution	0.668	0.130***	0.550**	0.568**	0.572***
	[0.471]	[0.019]	[0.224]	[0.225]	[0.219]
Conviction	0.480	0.158***	0.666***	0.663***	0.668***
	[0.500]	[0.021]	[0.256]	[0.257]	[0.242]
Prosecution (Violent Crime)	0.306	0.104***	0.676***	0.668***	0.676***
	[0.461]	[0.019]	[0.211]	[0.218]	[0.225]
Panel C. Educational Outcomes					
Graduate from Highschool	0.307	-0.068***	-0.490*	-0.460*	-0.458**
	[0.461]	[0.021]	[0.253]	[0.241]	[0.228]
Takes Admission Test for Selective Universities	0.163	-0.015	-0.241	-0.262*	-0.265*
	[0.369]	[0.014]	[0.154]	[0.150]	[0.153]
CourtxYear Controls	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	3,303	4,242	4,242	4,242	4,242

Notes: This table presents the OLS and two stage least squared estimations for the impact of juvenile pretrial detention on different outcomes: conviction, different measures of recidivism, and educational outcomes. All regressions control for courts' covariates. Robust standard errors clustered at the judge level in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

Table 16: Effect of juvenile pretrial detention on recidivism and educational outcomes (nonlinear model controlling by courts characteristics)

	Control Mean	OLS	biprobit	biprobit	biprobit
	(1)	(2)	(3)	(4)	(5)
Panel A. Trial Outcomes					
Convicted	0.512	0.147***	-0.060	-0.032	-0.200***
	[0.500]	[0.020]	[0.281]	[0.288]	[0.183]
Panel B. Recidivism as Adult Outcomes					
Penal Prosecution	0.668	0.130***	0.140	0.076	0.021
	[0.471]	[0.019]	[0.540]	[0.509]	[0.282]
Conviction	0.480	0.158***	0.271**	0.269**	0.076
	[0.500]	[0.021]	[0.342]	[0.337]	[0.254]
Prosecution (Violent Crime)	0.306	0.104***	0.305***	0.281***	0.190**
	[0.461]	[0.019]	[0.273]	[0.273]	[0.238]
Panel C. Educational Outcomes					
Graduate from Highschool	0.307	-0.068***	-0.109	-0.045	0.045
	[0.461]	[0.021]	[0.554]	[0.574]	[0.322]
Takes Admission Test for Selective Universities	0.163	-0.015	-0.206**	-0.209**	-0.071
	[0.369]	[0.014]	[0.333]	[0.390]	[0.306]
CourtxYear Controls	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	3,303	4,242	4,242	4,242	4,242

Notes: This table presents the OLS and bivariate probit estimations for the impact of juvenile pretrial detention on different outcomes: conviction, different measures of recidivism, and educational outcomes. All regressions control for courts' covariates. Robust standard errors clustered at the judge level in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

B.2 Estimation sample without information on education

Table 17: Effect of juvenile pretrial detention on recidivism and educational outcomes (without educational covariates)

	Control Mean	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Panel A. Trial Outcomes					
Convicted	0.508	0.157***	-0.108	-0.109	-0.176
	[0.500]	[0.018]	[0.222]	[0.220]	[0.219]
Panel B. Recidivism as Adult Outcomes					
Penal Prosecution	0.665	0.127***	0.756***	0.759***	0.754***
	[0.472]	[0.016]	[0.218]	[0.217]	[0.217]
Conviction	0.479	0.155***	0.804***	0.803***	0.787***
	[0.500]	[0.018]	[0.231]	[0.229]	[0.228]
Prosecution (Violent Crime)	0.302	0.118***	0.752***	0.761***	0.773***
	[0.459]	[0.017]	[0.221]	[0.220]	[0.224]
CourtxYear FE	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	4,139	5,327	5,327	5,327	5,327

Notes: This table presents the OLS and two stage least squared estimations for the impact of juvenile pretrial detention on conviction and different measures of recidivism. All regressions include year interacted with court fixed effects. These estimations are different from those reported in table 7 because educational covariates are excluded in this case and due to that the sample size is larger. Robust standard errors clustered at the judge level in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

Table 18: Effect of juvenile incarceration on recidivism and educational outcomes (without educational covariates)

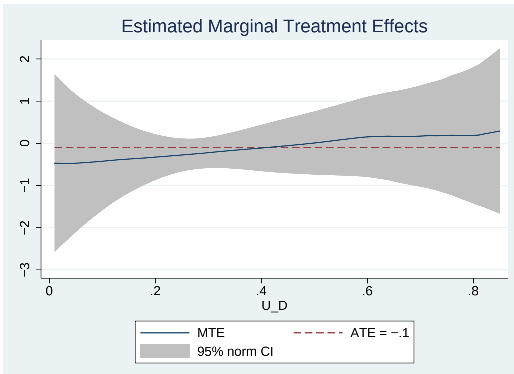
	Control Mean	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Panel A. Recidivism as Adult Outcomes					
Penal Prosecution	0.667	0.124***	0.580***	0.560***	0.616***
	[0.471]	[0.012]	[0.164]	[0.161]	[0.181]
Conviction	0.480	0.161***	0.785***	0.765***	0.838***
	[0.500]	[0.016]	[0.189]	[0.186]	[0.205]
Prosecution (Violent Crime)	0.304	0.127***	0.456***	0.440***	0.486***
	[0.460]	[0.013]	[0.122]	[0.120]	[0.132]
CourtxYear FE	–	Yes	Yes	Yes	Yes
Individual Controls	–	Yes	No	Yes	Yes
Case Controls	–	Yes	No	No	Yes
Observations	6,532	8,542	8,542	8,542	8,542

Notes: This table presents the OLS and two stage least squared estimations for the impact of juvenile incarceration on different measures of recidivism. All regressions include year interacted with court fixed effects. These estimations are different from those reported in table 8 because educational covariates are excluded in this case and due to that the sample size is larger. Robust standard errors clustered at the attorney level in bracket. ***, ** and * indicate statistical significance at the 99%, 95% and 90%, respectively.

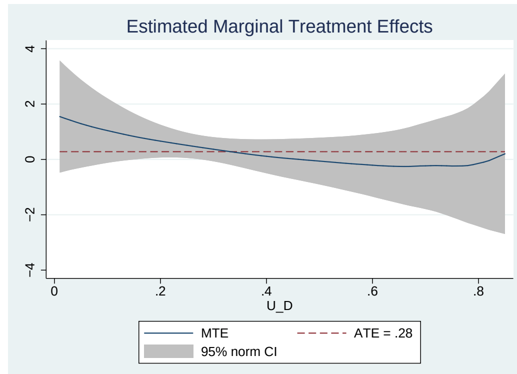
B.3 Marginal Treatment effect

Figure 3: MTE of juvenile pretrial detention on recidivism and other outcomes

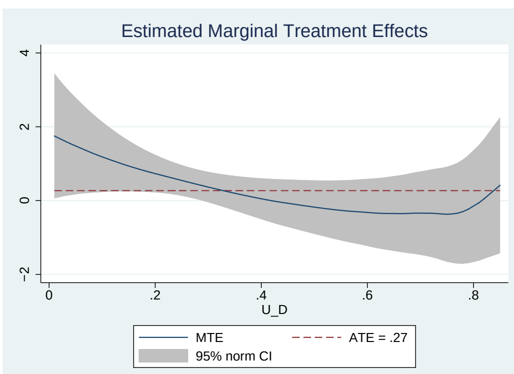
(a) Trial outcome: convicted



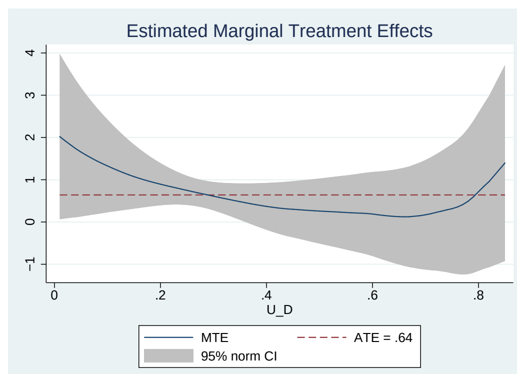
(b) Recidivism: penal prosecution



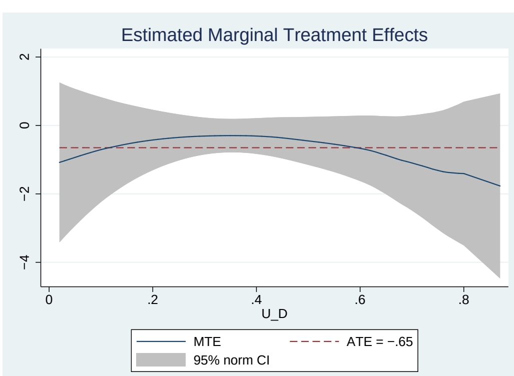
(c) Recidivism: conviction



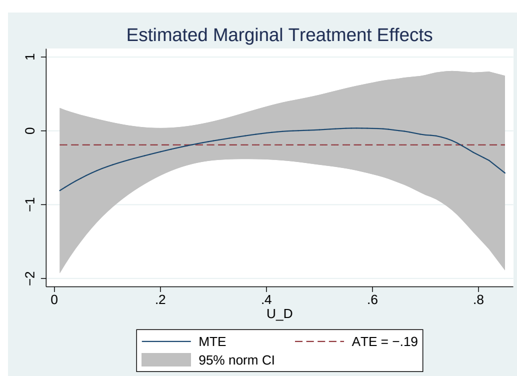
(d) Recidivism: prosecution (violent crime)



(e) Education: high school graduation



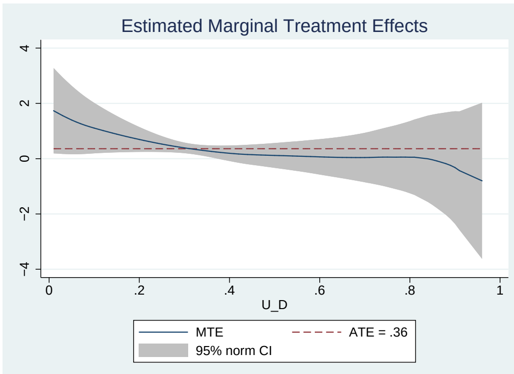
(f) Education: Takes PSU



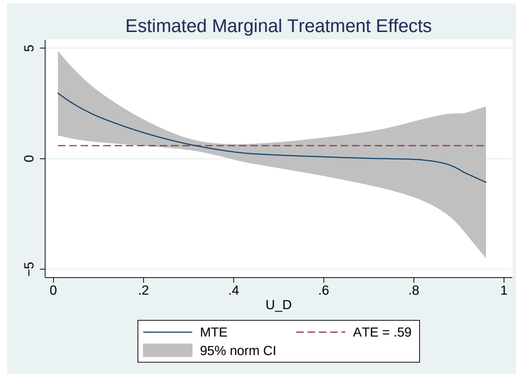
Notes: These figures present the marginal treatment estimations for the impact of juvenile pretrial detention on different outcomes, following the nonparametric procedure described in Section 6.

Figure 4: MTE of juvenile incarceration on recidivism and educational outcomes

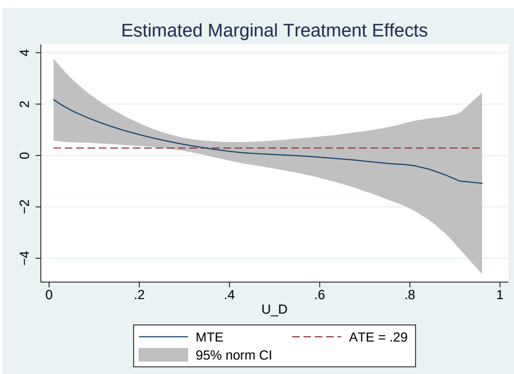
(a) Recidivism: penal prosecution



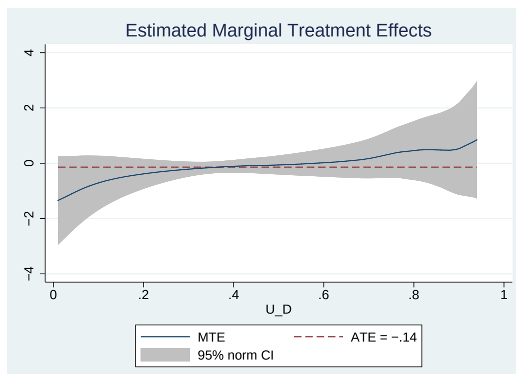
(b) Recidivism: conviction



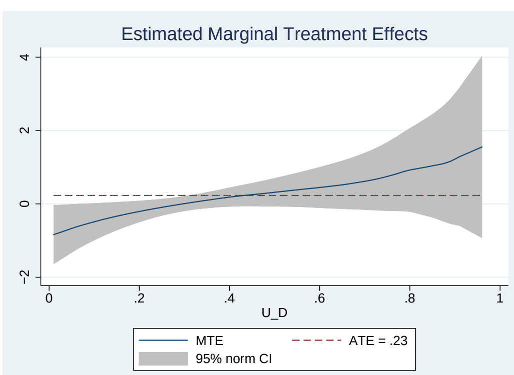
(c) Recidivism: prosecution (violent crime)



(d) Education: high school graduation

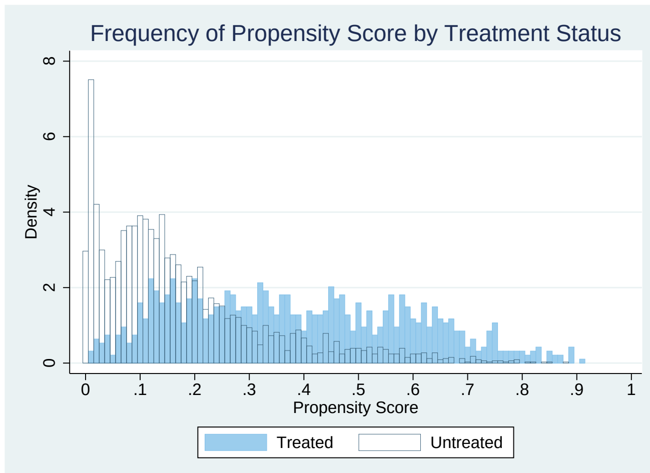


(e) Education: Takes PSU



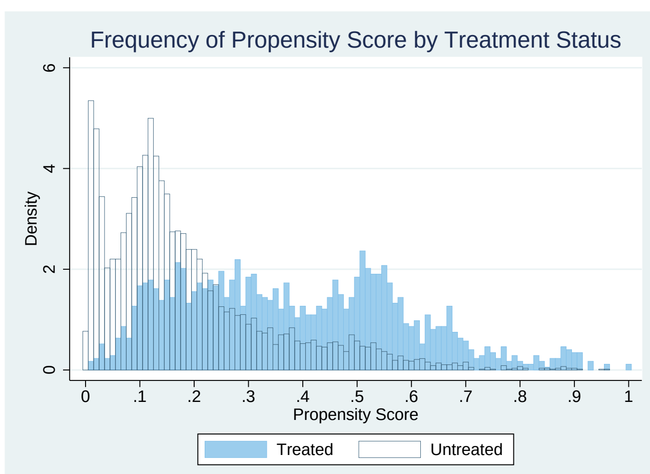
Notes: These figures present the marginal treatment estimations for the impact of juvenile incarceration on different outcomes, following the nonparametric procedure described in Section 6.

Figure 5: Common support when judge leniency is the instrumental variable



Notes: This figure shows the common support of the first-stage estimates of the propensity score, estimated using a logit model, between treated and untreated groups. The sample considered in this estimation is the one that is used to estimate the MTE of pretrial detention on recidivism, which is described in table 1.

Figure 6: Common support when attroney quality is the instrumental variable



Notes: This figure shows the common support of the first-stage estimates of the propensity score, estimated using a logit model, between treated and untreated groups. The sample considered in this estimation is the one that is used to estimate the MTE of incarceration on recidivism, which is described in table 2.