

Parental Incarceration and Children's Educational Attainment

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Abstract

This paper presents new evidence showing that parental incarceration increases children's educational attainment. I collect criminal records for 90,000 low-income parents who have been convicted of a crime in Colombia, and link them with administrative data on the educational attainment of their children. I exploit exogenous variation in incarceration resulting from the random assignment of defendants to judges, and extend the standard framework to incorporate both conviction and incarceration decisions. I show that the effect of incarceration for a given conviction threshold can be identified. My results indicate that parental incarceration increases educational attainment by 0.78 years for the children of convicted parents on the margin of incarceration.

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1 Introduction

Millions of children around the world are affected by the incarceration of their parents. In the United States, for example, approximately 2.7 million children have a parent in prison, while over one million children in EU countries do (Sykes and Pettit, 2014). This reality is potentially very concerning given that family environments during the early years, and parenting in particular, are known to be major determinants of human development (Heckman, 2013 and Almond et al., 2018). While it has been documented that parental incarceration is negatively associated with a host of important indicators for children’s well being, such as mental health, education, and crime (Wakefield, 2015), establishing the causal impact of parental incarceration raises a number of challenges. Households with incarcerated parents are typically disadvantaged along many dimensions – for instance, they are more likely to be poor and to experience domestic violence, even prior to the incarceration event (Arditti, 2005; Arditti et al., 2012).

Multiple mechanisms could explain a negative causal effect of parental incarceration on child outcomes. The incarceration of a parent is typically a shocking experience for a child (Parke and Clarke-Stewart, 2003). It is usually followed by financial hardship, disruptions in children’s daily lives, such as unstable childcare arrangements and moves among homes or schools, and growing up without a parent has been linked to adverse outcomes for children (McLanahan et al., 2013). Working in the opposite direction, there are reasons to believe that parental incarceration might be positive for some children. Parents in prison have very high rates of drug and alcohol abuse, are more likely to suffer from mental health disorders and to have experienced childhood trauma, and are also more likely to have engaged in intimate partner violence.¹ As a result, for some families, removing a violent parent or a negative role model from the household can create a safer environment for a child. Furthermore, a large literature documents the intergenerational transmission of violence, substance abuse and crime (Hjalmarsson and Lindquist, 2012), and incarceration can help to limit or break such transmission. Ultimately, the sign and size of such effects are empirical matters, motivating the current analysis.

In this paper, I study the causal effects of parental incarceration on children’s educational attainment in Colombia. I link sociodemographic data on households with children from the SISBEN, the country’s census of low-income populations, to criminal records for approximately 90,000 convicted parents for the years 2005 to 2016. I combine these data with anonymized individual-level records from the Attorney

¹In the US, Mumola (2000) documents that 60% of parents in prison reported that they used drugs in the month before their offense, 25% reported a history of alcohol dependence, and about 14% reported a mental illness. Western (2018) also documents that around 60% of parents in prison had experienced childhood trauma, such as domestic violence and sexual abuse. Western et al. (2004) documents that incarcerated men engage in domestic violence at a rate about four times higher than the rest of the population

General’s Office that provide information on the universe of criminal cases along with courtroom identifiers. I then link the educational outcomes of criminals’ children using administrative data on public school enrollment, and web-scrape the children’s criminal records after they turn eighteen years old.

To identify the causal effect of parental incarceration, I exploit exogenous variation resulting from the random assignment of cases to judges with different propensities to incarcerate defendants.² I extend the standard instrumental variable framework to incorporate the fact that judges make multiple decisions; notably, they decide both on conviction and incarceration, two decisions that I model separately. I use a general framework built around a multi-dimensional threshold model where treatment can take one of three possible outcomes: i) not convicted, ii) convicted and not incarcerated, and iii) convicted and incarcerated. Thus, a judge first decides on the basis of the available evidence whether it is enough to convict; then, for those convicted, the same judge decides whether to incarcerate by looking at the severity of the crime and seeing whether there are any attenuating or aggravating factors.

This approach improves on the previous empirical literature in three main directions. First, the multidimensional nature of the judge’s decision has raised concerns about the validity of the exclusion restriction; by explicitly modelling two decisions instead of one, the underlying exclusion restriction assumption is weaker.³ Second, it also relaxes the monotonicity assumption by allowing judges to evaluate two distinct attributes of defendants’ heterogeneity and have different propensities regarding each dimension. Third, when estimating treatment effects of incarceration relative to a combination of those who are convicted but not incarcerated and those not convicted, this estimate combines two distinct policy relevant causal effects: the causal effect of conviction and the causal effect of incarceration. Conviction concerns the burden of proof in prosecution and criminal investigation efforts, while incarceration is a matter of punishment and rehabilitation. My model provides a framework to estimate these two effects separately.

I estimate that, on average, parental incarceration increases education by 0.78 years for children of convicted parents who were on the margin of going to prison – namely, those whose incarceration sentence would have been different under a harsher or more lenient judge. Given that my instrument is continuous, this estimate is not the effect on a single margin, but the weighted average for the children of individuals whose

²See Kling (2006); Aizer and Doyle (2015); Di Tella and Schargrodsky (2013); Mueller-Smith (2017); Bhuller et al. (2018); and Dobbie et al. (2018a), among others.

³This has raised concerns in the literature about the validity of the standard exclusion restriction – that is, whether judges who are harsher in terms of incarcerating defendants are also harsher along other margins. In Mueller-Smith (2017), the data exhibit multidimensional and non-monotonic sentencing patterns (the dimensions including fines, community service, and probation among others), and he proposes an estimation procedure using LASSO to account for these features. Also, Bhuller et al. (2020) addressed concerns about possible violations of the exclusion restriction given multidimensional sentencing by augmenting the model to include other measures of trial outcomes. They find no evidence of such violations.

judge assignment could have resulted in a different incarceration outcome. With an average schooling of 7 years, my estimated effect corresponds to a 11.1 percent increase in the years of education.

Marginal treatment effect (MTE) estimates suggest that the benefit of parental incarceration is larger for children of parents who were incarcerated by more lenient judges. Intuitively, such parents have worse unobserved characteristics on average, and the benefits of removing them are larger than those of removing parents incarcerated by the most strict judges, who on average are more positively selected. In terms of observed heterogeneity, point estimates suggest that the benefit of parental incarceration is larger for boys than for girls and the difference is statistically significant. Along other dimensions, I find that incarceration for violent crimes and the incarceration of fathers yield larger increases in children's education, although these differences are not statistically distinguishable. I also find that treatment effects of incarceration in this application do not vary along the conviction margin.

Finally, while studying recidivism in the parent population would require a longer period of analysis, I document the changes after incarceration for a non-random sample of households, who are part of the two waves of SISBEN and who experienced a criminal conviction in between. For these households, I find that incarceration is associated with a 6.8 percentage point increase in labor force participation by the partner, a decline in the income score of the household, and an increase in the probability of living with grandparents.

This paper contributes to the literature on the intergenerational effects of incarceration. It is the first paper in a developing country setting, which is where the highest crime rates are usually found, and where poor and disadvantaged children receive limited government protection and support. Contemporaneous to my work, three other studies in developed country contexts exploit the random assignment of cases to judges to measure these causal effects, and provide different results. Bhuller et al. (2018) estimate imprecise null effects on academic achievement in Norway, and Dobbie et al. (2019) find that parental incarceration decreases educational attainment in Sweden. For Ohio, Norris et al. (2020) estimate null effects in test scores or grade repetition, but find that parental incarceration causes children to live in higher socioeconomic status neighborhoods as adults, and decreases the likelihood that a child is incarcerated. The effects of parental incarceration depend in systematic ways on factors that are likely to vary by context: the level of income, the incidence of crime, the severity of the penal system and the generosity of the welfare system, among others. Specifically, the higher crime rates, the fact that I focus on co-residing parents and not birth parents, and Colombian prison sentences being much longer and thus constituting a substantial shock to a household can all help explain why we observe

large positive effects in this context.^{4 5}

This paper also contributes to the literature studying identification in multivalued treatment settings along margin-specific treatment effects (see Heckman and Urzua (2010), Kirkeboen et al., (2016) Pinto (2019), and Mountjoy (2019)). I provide a new identification result using the framework developed in Lee and Salanie (2018) for the treatment assignment model described above, and establish which types of estimands can be used to recover interpretable and useful causal parameters in the presence multiple dimensions of essential heterogeneity. This treatment assignment model arises in a variety of other important settings. For example, in a context where school admissions are decided on the basis of academic excellence and financial aid is granted for those admitted based on need, my result provides a way to estimate the causal effect of financial aid for those with a specific level of academic achievement. It is natural to allow for the possibility that the effect of financial aid may differ for students who were marginally accepted relative to those with the highest grades; doing so provides valuable information for the design of education policy.⁶ This is also to the best of my knowledge the first empirical application of a choice model with multi-dimensional thresholds.

Finally, my paper contributes to the literature examining how parents affect their children’s outcomes. This includes a large body of papers studying the intergenerational effects of human capital (Black et al., 2005; Oreopoulos et al., 2006), wealth (Black et al., 2015), and welfare receipt (Dahl et al., 2014), among other outcomes. Specifically, my paper adds to the literature examining the relationship between household structure and children’s outcomes, and shows that living with a parent is not always better for children. Using incarceration as an instrument for the supply of eligible partners, Finlay and Neumark (2010) study whether marriage is good for children, and find that unobserved factors drive the negative relationship between never-married motherhood and child education. In addition, there is mixed evidence in terms of the effects of removing children from their parents and placing them in foster care; Roberts (2019) for South Carolina, and Gross (2020) for Michigan obtain positive effects on schooling, Bald et al., (2019) find mixed results across gender and

⁴In Colombia prison sentences are 4.4 years, compared to 2.9 years in the US and three and eight months in Sweden and Norway.

⁵Prior work has documented that only a fraction of incarcerated parents live with their children prior to incarceration (for example, 37% in the United States (Glaze and Maruschak, 2008)), which can limit the size of the treatment effects. Consistent with this view, other papers that focus on parents living with their children in the US, using a different identification strategy, find results similar to mine. Specifically, Cho (2009) finds that children in Chicago’s public schools whose mothers went to prison instead of jail for less than one week are less likely to experience grade retention. Using an event study design, Billings (2018) finds that incarceration improves end-of-grade exams and behavioral outcomes.

⁶A different set of applications are found in health practices where estimated treatment effects may differ along dimensions of both urgency and treatment fit. In the incarceration context, when estimating recidivism, the conviction margin may trace the criminal capital of the defendant and this may in turn affect the treatment effect of going to prison.

age for Rhode Island, and Doyle (2007, 2008) finds negative labor market and crime outcomes for Illinois. The results in my paper suggest that children may benefit from the absence of a convicted parent who is on the margin of incarceration.

The rest of the paper is structured as follows. Section 2 provides background on the judicial system in Colombia, and Section 3 describes the data sources and provides summary statistics. Section 4 sets out the model I develop to identify causal effects in my setting, Section 5 presents my estimation approach and results, and Section 6 discusses the results, the mechanism and external validity. Section 7 concludes.

2 Background: The Colombian Court System

In this section, I describe the criminal justice system in Colombia: how defendants are processed, how cases are assigned to judges, the types of crimes involved, and the stages of a standard trial.

Figure 1 illustrates how defendants are typically processed in Colombia’s criminal justice system.⁷ A criminal record is created when an arrest is made. Once this occurs, the police and a randomly assigned prosecutor must present the evidence that motivated the arrest in front of a judge within 36 hours. This judge, who is randomly assigned from the lowest tier of the judicial hierarchy, determines whether the arrest was legal and whether the defendant should await trial in prison.⁸ Next, the case is randomly assigned to another judge who will preside over the trial—this is the judge who provides the exogenous variation in conviction and incarceration I use in this paper. In practice, once the first judge decides to continue with the prosecution of a defendant, the case is entered immediately into a software program that assigns a judge at random among the judges in the judicial district and at the court level that the case is designated to; I refer to the district/court level as the “randomization unit.”

Colombia is divided into 33 judicial districts. In the largest cities, a district usually encompasses the city’s metropolitan area, and for the rest of the country, it usually corresponds to a state. Depending on the severity of the charge(s), a case will be randomized within one out of three possible court levels within the judicial district in which the crime was committed. The first level which correspond to municipal courts, receive simple cases such as misdemeanors, property crimes involving small amounts, and simple assault cases. These cases account for 38% of the data. More severe crimes, such as violent crimes, drug- or gun-related crimes, and large property crimes, are sent to circuit courts (56%). Lastly, the most severe types of crime, such as aggravated

⁷Acuerdo CSJ, 3329.

⁸A defendant will go to prison before trial when at least one of the following conditions holds: i) the defendant is a danger to society, ii) the defendant can interfere with the judicial investigation, or iii) there is reason to believe that the defendant will not appear in court for trial. Art 308. Criminal Proceedings Code.

homicide or terrorism, are assigned to a specialized judge (6%).⁹ On average, there are 20 judges per randomization unit, and the largest district—Bogota—has 55 judges.

Once the judge is assigned, the prosecutor and defense present their arguments to the judge over the course of multiple hearings. The purpose of the first hearing is to formally press charges. In a second hearing, prosecution and defense present all relevant evidence. Next, based on the strength of the evidence, the judge decides on conviction at a third hearing. If the defendant is found guilty, the judge holds a final hearing to determine sentence length and incarceration considering the severity of the crime, potential future harm to society and any aggravating or mitigating factors. The Colombian Penal Code establishes minimum and maximum sentences for each crime, but there is significant discretion on the part of the judge. The general sentencing guidelines range is often quite broad. For example, prison time for possession of 100 grams of cocaine is between five and nine years (Penal Code, Art 376). The judge also determines the crime and severity of the charge the defendant will ultimately be sentenced for—for example, murder versus involuntary manslaughter.

The decision to send a defendant to prison is determined by the length of the sentence. To deal with prison overcrowding, those convicted only serve time in prison when the sentence is longer than a certain threshold.¹⁰ This threshold is set at the national level and has increased over time. Currently, a sentence equal to four years or less is not served in prison.¹¹ As a result, the population that faces a trial is divided into three groups: i) not convicted; ii) convicted and not incarcerated; and iii) convicted and incarcerated. The fact that a portion of the convicted population does not serve time in prison is not a special feature of the Colombian penal system; for example, it is comparable to a sentence of probation in the US.

In Colombia, judges are selected based on their performance on an exam from an open call of attorneys, with specific legal experience requirements for each category of judge. Appointments do not have term limits, and it is common that, over time, judges rise within the judicial hierarchy. The average tenure of a judge is six years, and on average, a judge presides over 344 cases.

While in prison, inmates can receive visits from adults once a week and from their children once a month. The government does not provide special welfare assistance to inmates' families. Unlike in the US, being convicted of a crime does not change one's eligibility for welfare benefits, and in the labor market, it is not common practice to ask about previous convictions, although this information is available online.

⁹Art 35-37, Criminal Proceedings Code.

¹⁰This feature is not unique to the Colombian setting (e.g. Italy) and can also be compared to a probation sentence.

¹¹In these cases, the only consequence of being convicted is that for the duration of the sentence, the judge must be notified of any change of address or if the convict plans to travel outside the country. Art 63 Penal Code, and Ley 1709 de 2014.

3 Data Construction

3.1 Data sources

I collect data from several sources. First, I use two waves of Colombia’s census of potential beneficiaries of welfare (SISBEN). These data are collected by the government to characterize the country’s poor population and to target social programs to them. SISBEN has information on national identification numbers (NINs), household structure, age, gender, education, labor force participation of each household member, and a large set of variables on characteristics and assets of each house (e.g., refrigerator, stove, and floor material, among others). With this information, the government creates a score for each household that summarizes its level of wealth. The score is used to determine eligibility for most public programs—for example, free health insurance, conditional cash transfers, nutrition programs, subsidized housing, and college loans, among many others (Bottia et al., 2012). The first wave, conducted from 2003 to 2005, has data on 31.9 million citizens; the second wave, conducted from 2008 to 2010, has data on 25.6 million citizens.

From this database, I obtain two key elements for my analysis. First, I observe parent and child links when they live in the same household. Second, I use parents’ NINs to scrape criminal records. Anecdotal evidence for Colombia suggests that a substantial share of children with an incarcerated parent were not living with the parent at the time of the crime, all of these cases will not be part of from my sample. My target population is, however, likely to be the most affected by parental incarceration.¹²

In Colombia, criminal records from defendants who are convicted are available online for 17 out of 33 judicial districts. These 17 districts represent 67% of the population, 69% of homicides, and 83% of property crimes; they include the largest cities in the country; and they are richer and more urban than the 16 districts without data online.¹³ Each criminal record includes the name and NIN of the defendant, crime, date of crime, sentence information, and the court type and number that handled the case. I collected data on court directories and court identifiers to link each record to a specific judge. There is only one judge per courtroom but judges change over time, I construct the tenure of each judge at each courtroom to assign cases to judges.

I complement these data with individual-level, anonymized records from the Attor-

¹²Given how my parent-to-child links are constructed, I focus on parents who are living with the children rather than the biological parents. This definition includes stepchildren when the parent identifies the child as his or her child instead of describing themselves as not being related to the child.

¹³The universe of judicial sentences is public; however, they are only available in the nation’s National Archives. Criminal records for Bogotá can be found at the following link: <http://procesos.ramajudicial.gov.co/jepms/bogotajepms/conectar.asp>

ney General’s Office. This database has information on the universe of criminal cases (including cases that did not result in a conviction), along with courtroom identifiers, date of trial, final verdict, and gender and age of the defendant. I use this information to construct a measure of conviction stringency at the judge level. Finally, I use administrative records of public school enrollment for 2005-2016 with names and NINs to construct a measure of educational attainment.¹⁴ Children’s educational attainment is capped at 11, which is the last year of high school in Colombia.

3.2 Sample

To construct my sample, I proceed as follows: From SISBEN, I take the NINs of all parents living with their children in the 17 districts that have information online and web-scrape their criminal records. This adds up to 17 million adults. For computational reasons, I only search for records in the district where the person was living at the time of the SISBEN survey. To assess the number of records I miss due to this restriction, I take a 5% random sample and look for their criminal records in all 17 districts. From this, I estimate that I miss 8.6% of the sample due to crimes committed in districts different from the one found in SISBEN. My sample, therefore, includes only poor parents who, at the time of the SISBEN survey, lived with their children, lived in the largest districts of the country, and committed crimes in the district in which they were living.

I find criminal records for 256,366 individuals. Of these 90,056 have missing fields in at least one of the key variables, such as court identifier, crime, year, or sentence. Half of these records with missing data correspond to Medellin, which is the second largest district after Bogota, and has missing court identifiers in all of their records. I keep only crimes committed after 2005 and after the year of the first SISBEN year records, which results in 135,832 records.¹⁵ Next, I drop all records from court levels for which there was only one judge (4,325 cases dropped), and also in cases in which the number of records per judge in a year is fewer than 15 (32,701). I also only keep courtrooms for which I have judge/year conviction rates from the Attorney General’s Office database. This leaves me with criminal records from 90,526 adults. I retain only the first conviction in my sample, and collect data on the crime, courtroom identifier, and decisions regarding sentence and incarceration. I merge the criminal records back into the SISBEN data and keep only the first parental conviction in the household.

I link these data to two outcome variables for these children: educational attainment and criminal records. I find school records for 74% of them, similar to the share of children between ages 12 and 17 who attend school (76%, 2005 Census). Table

¹⁴95% of children in SISBEN attend a public school (DANE-GEIH)

¹⁵In 2005, there was a reform in the judicial system that renders the two periods incomparable. In the previous system, a judge served as both prosecutor and judge at the same time, and he or she was anonymous to the defendant. Additionally, at the time of this reform, there were other changes put in place regarding sentencing guidelines.

C3 in the Appendix shows evidence that having a missing education record is not statistically related to parental incarceration. I also search for criminal records for all children of convicted parents who were 18 years of age by 2017. My final data set consists of 43,908 children born between 1990 and 2007, who experienced the conviction of a parent between ages 0 and 14, and for whom I observe their Sisben information prior to the conviction record. In the following section, I characterize the population of convicted and incarcerated individuals, as well as their households and children.

3.3 Summary statistics

The population in my sample is negatively selected along three margins: education, income and criminal activity. In Table 1, I present socioeconomic characteristics for adults in the overall population, for parents in SISBEN with and without a conviction, and for parents with a conviction, by incarceration status. By comparing column 1 and columns 2 and 3, we see that parents in the SISBEN have fewer years of education, are less likely to have a high school degree and live in larger households. Among parents in the SISBEN, individuals with a conviction are also negatively selected across a host of variables (column 3 relative to column 2). Convicted adults have fewer years of schooling, are less likely to have a high school degree or more (23% vs. 31%), and have lower income scores. They also live in larger households. Adults with criminal records are disproportionately male (83%), they are more likely to work and to be the head of the household than those without a criminal record.¹⁶ Among those convicted, incarcerated parents have lower education and lower income levels (columns 4 and 5). Gender differences in the probability of incarceration conditional on conviction are far smaller than those in conviction. Incarceration is associated with lower probabilities of working.

Property crimes are the most common type of offense (25%), followed closely by drug-trafficking crimes (24%). Violent crimes account for 20% of the records, and gun-related crimes and misdemeanor offenses account for 18% and 12%, respectively. Incarceration rates vary substantially by crime. Figure D1 ranks crimes by their incarceration rates for selected crimes. Serious crimes, such as kidnapping or rape, have the highest incarceration rates, whereas failure to pay child support, simple assault, and property damage have the lowest. In the middle of the distribution, we find crimes such as drug trafficking, domestic violence, counterfeit currency trafficking, theft, and smuggling, among others.

¹⁶In the US context, for example, 29% of parents in state prisons have a high school degree or more, 92% are male, and the median age is 32 (Mumola, 2000).

4 Identification

Children from households with incarcerated parents are disadvantaged along many dimensions. As a result, simple comparisons of outcomes involving children with and without incarcerated parents would lead to negatively biased estimates of the effects of incarceration. A common way to address this endogeneity is to exploit the random assignment of defendants to judges who differ in their leniency when deciding to incarcerate. The assumption underlying this identification approach is that selection into incarceration is decided upon crossing a threshold over a single dimension of unobserved heterogeneity.¹⁷ Departing from that literature, I account explicitly for both the selection into conviction and the selection into incarceration using a general framework built around a multi-dimensional threshold model. In doing so, I provide a new identification result using the methodology developed in Lee and Salanie (2018). Specifically, I consider a multivalued treatment model (not convicted, convicted and not incarcerated, and convicted and incarcerated), where selection into conviction and incarceration is determined by the crossing of two distinct thresholds. Section 4.1 presents a simplified framework to provide intuition behind the identification result, and in Section 4.2, I set out the model formally.

4.1 A simplified framework

Defendants are characterized by their attributes along two dimensions. The first dimension refers to the level of reasonable doubt surrounding the case and the second one refers to the severity of the crime. To fix ideas, suppose that along each dimension, the attribute can take on 3 values. Thus, regarding the strength of the evidence (“doubt”), we can divide defendants in three groups: those for whom there is no doubt about their responsibility in the crime (type 1), those for whom we have some direct evidence (type 2), and finally those for whom we have circumstantial evidence only (type 3). Similarly, along the “severity” dimension we can also divide defendants into 3 groups: mild (type A), medium (type B) and high severity (type C). As a result, we can categorize defendants in 9 types, as in Figure 2.

Judges make conviction and incarceration decisions by evaluating defendants’ attributes. When deciding on conviction C , a judge assesses the strength of the evidence in the case at hand. Let us assume a judge can be one of two types in conviction: harsh (H_C) or lenient (L_C). Harsh judges do not require much evidence to convict a defendant and they convict type 1 and type 2 defendants. In contrast, lenient judges require more evidence to convict a defendant, and they only convict type 1 defendants.

Next, if a defendant is convicted, the same judge then decides on incarceration I . The judge makes this decision based on an assessment of how harmful the convicted

¹⁷Ahn and Powell (1993) and Angrist (1995) establish identification results for a similar problem in the context without treatment effect heterogeneity.

defendant may be to society, and how much punishment the defendant deserves, which corresponds to our second attribute, severity. Again, regarding incarceration, a judge can be either lenient l_I or harsh h_I . A harsh judge incarcerates type B and type C defendants, whereas a lenient one only incarcerates type C defendants. Given it is the same judge making both decisions, a judge can be of one of four types.

Potentially, the treatment effects of conviction and incarceration for each type of defendant are different, and ideally, we would like to be able to identify each of those treatment effects. Following the structure of my data, in this paper I focus on providing a framework to identify incarceration effects. To provide intuition behind my identification approach, first note that after the conviction decision is made, the pool of defendants of harsh and lenient judges will be different. Harsh judges will decide on incarceration for type 1 and type 2 defendants, whereas lenient judge will make this decision only for type 1 defendants. However, we can make progress if we exploit the fact that within conviction type, there is variation in incarceration leniency. Specifically, let us focus on harsh judges at the conviction stage, they are: $[H_C, h_I]$ and $[H_C, l_I]$. They all make incarceration decisions for the same types of defendant: everyone who is type 1 and type 2. Note that C1 and C2 defendants will always be incarcerated no matter who the judge is (always takers), and likewise A1 and A2 defendants will avoid prison regardless of judge assignment (never takers). However, for B1 and B2 defendants, going to prison is a lottery, they are compliers: if they are assigned to a harsh incarceration judge ($[H_C, h_I]$), then they will go to prison, but if they are assigned to a lenient judge they will not ($[H_C, l_I]$). As a result, we can use judge leniency along the incarceration margin to identify the incarceration treatment effects for B1 and B2 type defendants. The same argument follows when considering lenient judges in conviction; in this case, I will be able to identify treatment effects for B1 defendants. In general, I can identify incarceration treatment effects for compliers along different margins of selection into conviction.¹⁸

4.2 Model

In this subsection, I formalize the previous intuition and extend it to the case of continuous instruments, thereby delivering a new identification result.

The model is described by the standard IV framework, which consists of five main random variables: $T, Z, Y, \mathbf{V}, \mathbf{X}$. Those variables lie in the probability space (Ω, F, P) , where individuals are represented by elements $i \in \Omega$ of the sample space Ω . The variables are defined as follows:

- T_i denotes the assigned treatment of individual i , and takes values in $\text{supp}(T) =$

¹⁸Note that the identification challenge arises as result of the potential different treatment effects for the different types of defendants, and it is independent of whether it is the same judge making both decisions or not.

$\{t_f, t_c, t_I\}$. Where t_f stands for not convicted, t_c for convicted but not incarcerated, and t_I for convicted and incarcerated.

- Z_i is the instrumental variable in this analysis and takes values in the support of Z , representing judge assignment.
- Y_i denotes the outcome of interest for individual i , —e.g., years of education of the child.
- \mathbf{X}_i represents the exogenous characteristics of individual i .
- \mathbf{V}_i stands for the random vector of unobserved characteristics of individual i , and takes values in $\text{supp}(\mathbf{V})$.

The random vector \mathbf{V} is the source of selection bias in this model: it causes both the treatment T and outcome Y . The standard IV model is defined by two functions and an independence condition, as follows:

$$\text{Outcome Equation: } Y = f_Y(T, \mathbf{X}, \mathbf{V}, \epsilon_Y) \quad (1)$$

$$\text{Treatment Equation: } T = f_T(Z, \mathbf{X}, \mathbf{V}) \quad (2)$$

$$\text{Independence: } Z \perp (\mathbf{V}, \epsilon_Y) | \mathbf{X} \quad (3)$$

where ϵ_Y is an unobserved zero-mean error term associated with the outcome equation that is independent of \mathbf{V} .

In this notation, a counterfactual outcome is defined by fixing T to a value $t \in \text{supp}(T)$ in the outcome equation. That is, $Y(t) = f_Y(t, \mathbf{V}, \mathbf{X}, \epsilon_Y)$. The observed outcome for individual i is given by:

$$Y = Y(T) = \sum_{t \in \{t_f, t_c, t_I\}} Y(t) \cdot \mathbf{1}[T = t]. \quad (4)$$

The independence condition (3) implies the following exclusion restriction:

$$\text{Exclusion Restriction : } Z \perp Y(t) | \mathbf{X} \text{ for all } t \in \text{supp}(T). \quad (5)$$

For notation simplicity, I suppress exogenous variables \mathbf{X} henceforth. All of the analysis can be understood as conditional on pre-treatment variables.

I assume that the treatment equation is governed by a combination of two threshold-crossing inequalities. First, there is a conviction stage in which the defendant is:

$$\begin{cases} \text{Free} & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) > \xi_c(Z)] \\ \text{Convicted} & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) \leq \xi_c(Z)], \end{cases}$$

where $\mathbf{1}[\cdot]$ denotes a binary indicator and $\phi_c(\cdot)$ and $\xi_c(\cdot)$ are real-valued functions. Function $\phi_c(\cdot)$ measures the degree of culpability assessed by the judicial system. This function maps variables and information that are not observed by the econometrician but that are observed by the judge, such as the amount of evidence, and the effort of the defense and prosecuting lawyers, into a single dimensional index. The function $\xi_c(\cdot)$ assesses judge leniency on conviction. This function can be understood as a threshold of reasonable doubt beyond which the defendant is not convicted by the judge. Judges differ in their leniency and may set different thresholds of evidence. The judge convicts defendant i whenever $\phi_c(V_i) \leq \xi_c(Z_j)$. If that is the case, we move to the second stage where the judge makes a decision regarding incarceration:

$$\begin{cases} \text{Not incarcerated} & \text{if } \mathbf{1}[\phi_I(\mathbf{V}) > \xi_I(Z)] \\ \text{Incarcerated} & \text{if } \mathbf{1}[\phi_I(\mathbf{V}) \leq \xi_I(Z)] \end{cases}$$

Similarly, $\phi_I(\cdot)$ is a function whose arguments are the case and defendant's characteristics relevant for an assessment of the punishment level, such as crime severity and the defendants risk to society. As before, the judge compares $\phi_I(\mathbf{V})$ to her/his threshold to incarcerate $\xi_I(Z)$.

Treatment assignment can be summarized as follows by combining the two threshold rules:¹⁹

$$T = f_T(Z, \mathbf{V}) = \begin{cases} t_f & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) > \xi_c(Z)] \\ t_c & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) \leq \xi_c(Z)] \cdot \mathbf{1}[\phi_I(\mathbf{V}) > \xi_I(Z)] \\ t_I & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) \leq \xi_c(Z)] \cdot \mathbf{1}[\phi_I(\mathbf{V}) \leq \xi_I(Z)] \end{cases} \quad (6)$$

This model relies on two separable threshold functions these play the role of the

¹⁹I assume the following standard regularity conditions: A1) $E(|Y(t)|) < \infty$ for all $t \in \text{supp}(T)$, A2) $P(T = t|Z = z) > 0$ for all $t \in \text{supp}(T)$ and all $z \in \text{supp}(Z)$ and, A3) $(\phi_c(\mathbf{V}), \phi_I(\mathbf{V}))$ are absolutely continuous with respect to Lebesgue measure in \mathbb{R}^2 . The first assumption guarantees the existence of the expectation, the second one assures that there is a share of the population assigned to each treatment group for every judge, and the third one allows me to apply the Lebesgue differentiation theorem.

monotonicity condition (Vytlačil, 2002).²⁰

Without loss of generality, it is useful to express treatment assignment using the following variable transformations:

$$U^c = F_{\phi^c(\mathbf{V})}(\phi^c(\mathbf{V})) \sim \text{Unif}[0, 1] \quad (7)$$

$$U^I = F_{\phi^I(\mathbf{V})}(\phi^I(\mathbf{V})) \sim \text{Unif}[0, 1], \quad (8)$$

where $F_K(\cdot)$ denotes the cumulative distribution function of a random variable K . U^c, U^I are uniformly distributed random variables in $[0, 1]$, and there is no restriction on the joint distribution of U^I and U^c . Likewise, we can define two propensity scores as follows:

$$P_c(z) = F_{\phi^c(\mathbf{V})}(\xi^c(Z)); z \in \text{supp}(Z) \quad (9)$$

$$P_I(z) = F_{\phi^I(\mathbf{V})}(\xi^I(Z)); z \in \text{supp}(Z). \quad (10)$$

Let $P_c(z)$ denote the probability of conviction when $Z = z$. Moreover, independence condition (3) implies $P_c, P_I \perp U^c, U^I$. Using this notation, the model can be expressed as:

$$T = \begin{cases} t_f & \text{if } \mathbf{1}[U^c > P_c(z)] \\ t_c & \text{if } \mathbf{1}[U^c \leq P_c(z)] \cdot \mathbf{1}[U^I > P_I(z)] \\ t_I & \text{if } \mathbf{1}[U^c \leq P_c(z)] \cdot \mathbf{1}[U^I \leq P_I(z)] \end{cases} \quad (11)$$

In the model, U^c and U^I have the same interpretation as in the previous section, and P_c is interpreted as the share convicted by judge z (see Figure 3). Without the assumption of independence between U^c and U^I , variation in incarceration leniency is only identified once I fix the conviction threshold. Thus, the counterfactuals of interest are $Y(t_I)$ and $Y(t_c)$ for those who were convicted under $P_c = p_c$. This means

²⁰Consider two judges, j and j' , who see defendants i and i' , who differ in their level of culpability. Say i' has more evidence against him than i ; namely $\phi_c(i') < \phi_c(i)$. Suppose that judge j convicts defendant i' but not i . Then the threshold function implies that it cannot be the case that judge j' convicts defendant i , but not i' . More generally, let $D_i(j) = \mathbf{1}[T_i(j) = t_c]$ denote the binary indicator that judge j convicts defendant i . Thus if judge j convicts i' but not i , it implies:

$$D_i(j) > D_{i'}(j)$$

Then it cannot be the case that judge j' convicts defendant i , but not i' . In turn this means:

$$D_i(j) > D_{i'}(j) \rightarrow D_i(j') \geq D_{i'}(j'),$$

which is equivalent to stating that:

$$D_i(j) > D_i(j') \rightarrow D_{i'}(j) \geq D_{i'}(j').$$

We can generalize this to all individuals to arrive at the standard monotonicity assumption of Imbens and Angrist (1994). Similarly, the assumption is the same for $\phi_I(\cdot)$ and the judges' incarceration decision.

that the objective is to identify causal effects of the form: $E(Y(t_I) - Y(t_c)|U^c < p_c)$, which is analogous to the exercise described in Section 4.1. Let:

$$P_I^*(z) = Pr(U^I < P_I(z)|U^c < P_c(z)) \quad (12)$$

where P_I^* is the judge's incarceration probability conditional on conviction.

Proposition: *The difference in counterfactual outcomes $E(Y(t_I) - Y(t_c)|P_I^*(Z), U^c < p_c)$ is identified from the data as follows:*

$$E(Y(t_I) - Y(t_c)|P_I^*(Z), U^c < p_c) = \quad (13)$$

$$\int_0^1 \frac{\partial E(Y \cdot \mathbf{1}[T \in \{t_c, t_I\}]|P_c(Z) = p_c, P_I^*(Z) = p_I^*)}{\partial p_I^*} dp_I^* \quad (14)$$

(See Appendix A for the proof.)

What this result says is that we can trace the treatment effect of incarceration once we fix a threshold for conviction. We do this by evaluating changes in the outcome variable when we change the judge's incarceration probability: P_I^* . This delivers the MTE along the unobservable dimension $U^I|U^c < p_c$. The integral over the support of the instrument gives the LATE, or alternatively the ATE when the instrument has full support.

The identification result in equation (14) is useful in any setting where treatment assignment follows the design in equation (11). In the context of criminal policy where judges decide on both conviction and incarceration, the researcher has two instruments to identify two policy relevant treatment effects. The first one, conviction, takes the form of the traditional LATE in the literature, given that treatment is decided upon crossing a single threshold. The second one, the effect of incarceration, is only identified as function of the crossing of the first threshold.

5 Estimation

To apply the identification result of the previous section, I start by estimating the sample analogs of the conviction ($P_c(Z)$) and incarceration ($P_I^*(Z)$) instruments in the model. These variables can be interpreted as the probability of being convicted and incarcerated respectively, given the assignment to a specific judge. Following the literature, these are estimated as judge fixed effects from regressions after parsing out variation at the level at which the randomization of judges occurred and specific case characteristics. That is, the conviction/incarceration decision can be decomposed into a portion that is related to the individual, the judge, the offense, and the randomization unit/year. I do this as follows:

$$D_{itorz} = \gamma_{rt} + \gamma_o + \epsilon_{itorz}$$

where D_{itorz} corresponds to a conviction or incarceration dummy, i indexes individuals, t the year, r court-level/judicial district, o offense and z the judge. The parameter γ_{rt} corresponds to randomization-level fixed effects, which are court-level/judicial-district by year-level fixed effect, γ_o is crime level conviction/incarceration rates and ϵ_{itorz} is a mean zero error term. Following the literature, I estimate the judge instrument \widehat{p}_{z-i} for defendant i to be the following leave-out estimator:

$$\widehat{p}_{z-i} = \frac{1}{n_z - 1} \sum_{k \neq i} \widehat{res}_{z,k},$$

where n_z is the number of cases of judge z , and res_{zk} is the residual from a regression of the conviction/incarceration dummy on γ_{rt} and γ_o .

Figure 4 shows the distribution of conviction and incarceration rates at the judge level, and \widehat{p}_z for both conviction and incarceration. From the graph, we can see that although court-level/year and crime-level fixed effects explain most of the variation, the judge's fixed effects still represent a sizable share of the variance in conviction and incarceration.

5.1 Instrument validity

Next, I examine how much judge fixed effects predict individual-level decisions by estimating a first-stage regression, as follows:

$$D_{itrz} = \beta_0 + \gamma_{rt} + \gamma_o + \widehat{p}_{zi} + \beta_1 X_i + \epsilon_{itrz}.$$

As before, D_{itrz} corresponds to the conviction or incarceration dummy, and p_z is the leave-out mean of judge z assigned to person i in conviction or incarceration. I run this regression with and without controls, X_i . In the conviction regression, where I use anonymized data from the Attorney General's Office, I can only control for age, gender, and number of crimes charged.²¹ In the incarceration regression, I control for schooling, income, occupation, gender, year of birth, and year in the survey. According to the results in Table 2, judges have a strong influence on conviction and incarceration decisions. The estimates are highly significant and indicate that being assigned to a judge with a ten percentage point higher conviction/incarceration rate increases the defendant's probability of conviction and incarceration by seven percentage points. This relationship is robust to the inclusion of controls, as expected given random

²¹These extra case variables are included in the system at the discretion of the (randomly assigned) prosecutor and are missing for a considerable share of the cases.

assignment. Figure 5 depicts this first-stage relationship for conviction (left panel) and incarceration (right panel).²²

Recall from the previous section that the variation in incarceration stringency for a given level of conviction stringency is what identifies treatment effects in this context. Figure 6 shows a scatter plot of both conviction and incarceration fixed effects. From the graph, we can see that there is substantial variation along the incarceration axis for each conviction rate.

For the instrument to be valid, the judge’s fixed effects must be orthogonal to the defendant’s characteristics. I test this in the anonymized data from the Attorney General’s Office, where the universe of cases the judge has heard is available. Table 3 checks the balance across defendants for my judge-stringency measures for conviction and incarceration. Across gender, age, number of charges and types of crime—which are the only variables available in these data, I find no individual or joint statistical significance. In addition, the identification result is supported by the observation that once P_c is fixed, the pool of convicted defendants is balanced across judges. I test whether covariates are associated with incarceration stringency for the convicted sample once control for the conviction level with a polynomial of P_c . In second panel of Table 3, I test the individual and joint significance of variables associated with education, income, and occupation status, and find no evidence of a relationship with judge stringency.

To interpret the results of the IV as the causal effect of incarceration, judge stringency must only affect child’s outcomes through incarceration. This may not be the case if the judge fixed effects capture other dimensions of trial decisions, such as fines or guilt (Mueller-Smith, 2017). In my setting, this is less of a concern because in the case of Colombia, fines are rare and only associated with large property crimes, and because I model the conviction decision directly.²³

Finally, the monotonicity assumption requires that conviction or incarceration decisions made by a lenient judge would also have been made by a stricter judge. One testable implication of monotonicity is that first-stage estimates should be non-negative for all sub-samples (Bhuller et al, 2020). That is, if a judge is lenient for example, he or she is going to be lenient for both women and men, and for both violent crimes and nonviolent crimes. To test this assumption, I construct judge fixed effects for just one group in the population, (for example, for men) and use this fixed effect in a first-stage regression to predict individual conviction and incarceration for women. I do this for gender, type of crime, and age group. Table 4 shows these first-stage tests, in which I find positive estimates across all slices of the data. This, however

²²Note that there is an implicit assumption about the estimated judge conviction stringency in the trial sample being a good predictor of conviction stringency in the sub-sample of adults from the poverty census.

²³In addition, the failure to pay these fines does not entail any consequence in terms of incarceration.

only tests for a weak form of monotonicity, which is enough to interpret IV estimates as a convex combination of treatment effects of compliers, but it is not sufficient for the identification of marginal effects along the entire distribution of judge propensities. The weaker assumptions rely on averaging across the entire set of judges, while identification of marginal effects throughout the distribution requires assumptions to hold judge by judge (Norris, 2019). In Table D1 I test pairwise monotonicity following Norris (2019) and find I can not reject monotonicity across individuals characteristics, except for violent crimes.²⁴ Frandsen et al (2019) show that under the usual assumptions, average outcomes by judge will be a continuous function with bounded slope of judge propensities to incarcerate. Intuitively, if this is not the case, it implies that either judges influence outcomes beyond their propensity to assign treatment, or judges disagree on their implicit ordering of which defendants should be treated. In Table D2 I implement Frandsen et al (2019) joint monotonicity and exclusion test and I find there is no evidence of violation of these assumptions.

5.2 Results

I begin by discussing the OLS estimate of this design. Table 5 shows a regression of parental incarceration on years of education. Following Abadie et al. (2017), standard errors are two-way clustered at the randomization-unit level and the household level. Without controls (column 1), a child whose parent went to prison has around 0.45 fewer years of schooling than a child whose parent did not. Once I add controls (column 2), this difference is reduced drastically to less than 0.06 years. Still, we expect that incarcerated parents are negatively selected on unobservables that cannot be accounted for. Column 3 shows the first stage regression for the sample of children which confirms the strong positive relationship between judge stringency and parental incarceration. Following Bhuller et al (2020), I report the Motiel Pfluegger Effective F-statistic of 84.86, which is above the critical value of 23.1 for a worst case bias of 10% and also above 37.4 the corresponding critical value for a bias of at most 5%.

Next, Figure 7 provides a graphical representation of the reduced-form regression. This graph plots the distribution of judges' incarceration fixed effects against the predicted years of education from a local polynomial regression. From the graph, we can see that there is a strong positive relationship between judge stringency in incarceration and years of education. That is, moving to the right, and thus increasing the probability of having a parent in prison exogenously, I estimate that the years of education also increase. Column 4 of Table 5 shows the regression results for this reduced form: I estimate large increases in years of education for all specifications. Finally, column 5 shows results from the IV; I estimate that having an incarcerated

²⁴However, I split judge leniency based on this characteristic and find very similar point estimates.

parent increases years of schooling by around 0.78 years on average for all conviction levels. These estimates are statistically different from zero.²⁵

To derive policy implications, it is important to acknowledge the local feature of my results. My estimate is a weighted average of the effect of incarceration of parents for whom judge assignment could have resulted in a different incarceration outcome. This group, will not include parents convicted—for example, of murder or rape—since they are likely to be incarcerated regardless of judge assignment, or defendants convicted of minor crimes who will also avoid prison, regardless of judge assignment. Defendants convicted of drug- or gun-trafficking, domestic violence, and medium-sized property crimes compose the complier group in my estimation, and they are the group my estimates apply to. This marginal population, is particularly relevant because it is the population that is more likely to be affected by policy interventions to the criminal justice system. Following Dahl et al. (2014), I find that compliers make up approximately 29.8% of the sample.²⁶

I also study how parental incarceration affects the chance that the child is later convicted of a crime. For this exercise, I restrict the data to children who had turned 18 years old by 2017, so that their criminal records would be public. Figure D5 shows reduced-form estimates of judge stringency on conviction probability; the effect is close to zero. However, the analysis is under-powered to detect to estimate reasonably sized treatment effects. This is not surprising, since conviction is a low incidence event; only 1.6% of children had a criminal record, and the difference in the OLS is only 0.1 percentage points.

5.3 Heterogeneity

In this section, I examine the heterogeneity of the results along observables and unobservables. In my context, marginal treatment effects (MTE) are particularly interesting, because they trace the causal effect of incarceration along parents' unobserved characteristics (U^I) that matter for incarceration and that are correlated with defen-

²⁵I find that the increase in years of education is mostly accrued through a higher graduation rate from middle school. Figure D4 in the Appendix plots the treatment effect of parental incarceration on grade completed from 6th grade to 11th grade. There are positive treatment effects for all grades, but the effect is larger for 9th grade which corresponds to the last grade of middle school.

²⁶Parental compliers are defendants who would have received a different incarceration decision had their case been assigned to the most lenient judge instead of the strictest judge. We can define the size of this group (π_c) as follows:

$$\pi_c = \text{Prob}(\text{Incarceration} = 1 | z_j = \bar{z}) - \text{Prob}(\text{Incarceration} = 1 | z_j = \underline{z})$$

where \bar{z} and \underline{z} correspond to the incarceration rates of a judge at the 99th and 1st percentiles, respectively. Because of monotonicity, the share of parents who would go to prison regardless of the judge assigned to their case—always takers—is given by the incarceration rate for the most lenient judge and is equal to 22.5%. On the other hand, 47.7% of the sample are children of never takers who would not go to prison no matter which judge was assigned to their case. I estimate that children of compliers make up approximately 29.8% of the sample.

dants’ quality, broadly defined. What this exercise does is to evaluate the possibility of different effects of parental incarceration given the type of defendant who is going to prison, which is characterized by his or her location along the y-axis of Figure 3. The intuition is as follows: Parents who are incarcerated under the most lenient judges have worse characteristics than those incarcerated under strict judges. In other words, a strict judge incarcerates almost everyone, but a lenient judge incarcerates only the worst defendants, so that those incarcerated under relatively lenient judges are more negatively selected.²⁷ I follow Heckman and Vytlacil (2005) in estimating this MTE, and find that at the 5% level, there are heterogeneous treatment effects along parental quality (Figure D2). Specifically, I find that the positive effects of incarceration on schooling accrue when the worst defendants go to prison.

The magnitude of the effect of parental incarceration on children’s education is a function of several factors: the nature of the relationship between the parent and the child prior to the incarceration episode, the type or quality of this parent, and the role of the child in the household. To document this heterogeneity, I estimate the IV regression for different subgroups in the data. Following prior research in economics as well as in psychology and sociology, I estimate different regressions by gender of the child, gender of the parent, and the nature of the offense—violent, or non violent, age of the child and sentence length. Table 6 shows IV results for these different groups in the data.

According to the estimates, the benefits of parental incarceration are larger for boys than girls, and this difference is statistically significant. Specifically, I find that boys’ schooling increases by 1.07 years, whereas girls’ schooling increases by 0.46 years. This result is consistent with previous research in psychology and economics, which documents that boys are more vulnerable than girls to negative experiences in the household (Bertrand and Pan (2013); Autor et al. (2016); Parke and Clarke-Stewart (2003); Hetherington et al., 1998). Specifically, Autor et al. find that relative to their sisters, boys have higher rates of disciplinary problems, lower achievement scores, and fewer high school completions when growing up in disadvantaged environments. On the other hand, point estimates for children exposed young (0-7 years old) versus old (8 to 14 years), are very similar.

I split the sample by gender of the parent and find that incarceration is more beneficial in cases in which the father is the one going to prison. A source of heterogeneity associated with the type of parent going to prison is the nature of the crime they committed. I find larger benefits in cases where the crime is violent versus not. Finally I also find larger point estimates from longer sentences -above median. However, these differences are not statistically significant.

²⁷I look at this empirically and find that among incarcerated defendants, those incarcerated under stricter judges tend to have fewer and less severe charges. This follows almost directly from the definition of leniency, but also helps to illustrate the ways in which these defendants are better.

Identifying the causal effects of incarceration in household structure, mental health and family relationships is key to understand the results in this paper. This, however, is outside of the scope of this work and would require substantial additional data. Nevertheless, to start a characterization of the changes that households and children experience after an episode of incarceration, I take households for which I have two observations in the SISBEN (44% of cases), in which the parent was convicted of a crime between observations. Appearing in both waves of the SISBEN is not random; and generally leaving the sample is associated with an improvement in living standards. With this caveat, Table D6 shows suggestive evidence that incarceration is associated with an increase in labor force participation (LFP) of the spouse, a worsening of the income score of the household, and a decrease in the probability of a male as the head of the household. I also find that the probability of living with grandparents increases.

5.4 Robustness & External validity

In the results section, I presented my preferred specifications for the estimates of the effect of parental incarceration on educational attainment. To assess the robustness of the results to this choice, in Figure D6 I instead order observations along P_c , and run multiple regressions on a rolling over P_c , moving the window 800 observations each time. Figure D6 in the Appendix shows that for each sample, I find a positive effect of incarceration on education. In addition, in Table D4 I split the sample in low and high levels of P_c and compute the instrumental variable estimate, I find there is no difference across samples or with respect to the baseline results.

As a placebo check, I evaluate whether there are differences in schooling for children of incarcerated versus non-incarcerated parents before the date of the sentence. Table D5 in Appendix D shows that there is no supporting evidence that the positive effects I estimate are the result of preexisting differences in educational attainment.

To assess the external validity of my results, I provide a framework motivated by my heterogeneity analysis, which links parental quality to parenting treatment effects and the probability of incarceration. Figure D3 summarizes this framework. The x-axis traces parental quality: as we move to the right, parental quality increases. The y-axis measures the treatment effect of parenting: having better parents is better for children. Most importantly, however, there is a segment on the support of parental quality for which parents are detrimental for children. The secondary y-axis measures incarceration probability: In the model, the probability of being incarcerated decreases when parental quality increases. Each society chooses a level of incarceration, which is characterized by a threshold in the support of parental quality. This threshold determines the average effect of incarcerating parents (the gray area in Figure D3).

To determine the extent to which the results in this paper apply to other settings, we need to think about the location of the incarceration threshold along the parental quality axis and the shape of the function of the treatment effects of parents in each

country. Countries with higher incarceration rates will incarcerate, on average, better parents than those with lower rates, and as a result we should expect lower benefits or even costs from parental incarceration. We can also expect a much flatter function of treatment effects of parenting in generous welfare states, such as the Nordic countries, in which children’s education and health vary less with parental characteristics. As a consequence, we would find smaller treatment effects of parental incarceration (both positive and negative). Similarly, some of the estimates in the literature (Norris et al. 2020 and Dobbie et al. 2019) consider birth parents who may not necessarily co-reside with their children. In this framework we can hypothesized that it translates to smaller treatment effect of parents and as a result into a smaller effect of parental incarceration. Finally, the slope of this function will also depend on whether the child experiences a short or long term separation from the parent. Median sentences vary from months to years across the different contexts and households may react differently when facing a transitory change, compared to a long-term shock.

6 Final Remarks

In this paper, I estimate the causal effects of parental incarceration on children’s educational attainment in Colombia. I exploit exogenous variation resulting from the random assignment of judges with different propensities to convict and incarcerate defendants. I find that, opposite to what is observed in correlation analysis, parental incarceration increases children’s educational attainment. I finish with an invitation for further work on this topic. The present paper and the contemporaneous literature considers only short and medium-term effects, however, as parents eventually leave prison, some will return to live with their children, and this situation constitute a new set of challenges to the household that remain unexplored. If incarceration decreases one’s human capital and social and emotional skills, the type of parent who returns after incarceration can be much worse than the one who left. In that case, the long-term effects may be different from those I estimate here. Finally, my paper only offers suggestive evidence on the mechanisms that explain the positive effects of parental incarceration on children’s educational attainment. Further research is required to characterize the family environments children face in these households before and after parental incarceration. This work will help design policies to improve the well-being of some of the most vulnerable members of our society.

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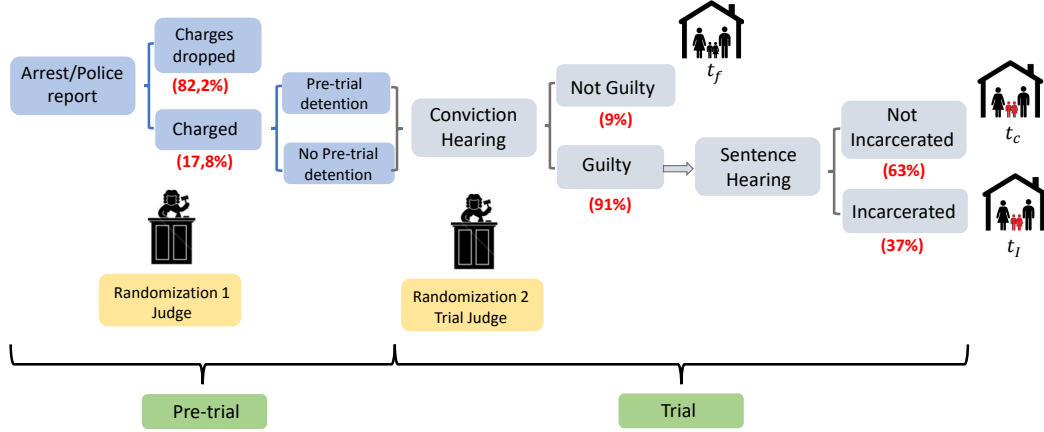
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Figures

Figure 1: Prosecution and trial stages



Source: Colombian Penal proceedings code, Informe de la Comision Asesora de Politica Criminal (2012), SPOA and Criminal records. The treatment status studied in this paper corresponds to t_f , which refers to parents who are not convicted or free, t_c those convicted but not incarcerated, and t_I those convicted and incarcerated. Incarceration is a function of sentence length. Currently, a sentence equal to four years or less is not served in prison.

Figure 2: Identification

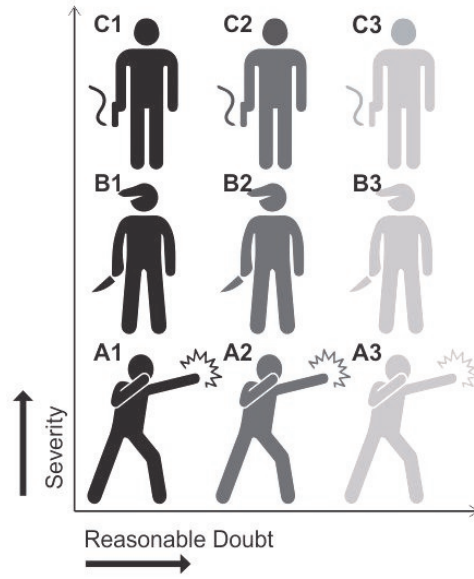
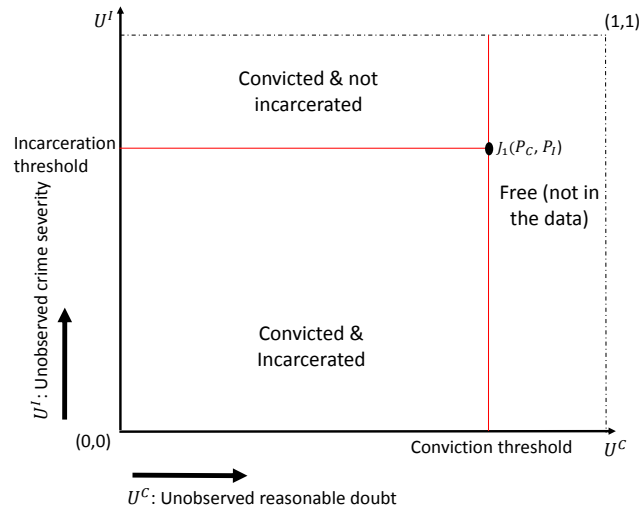
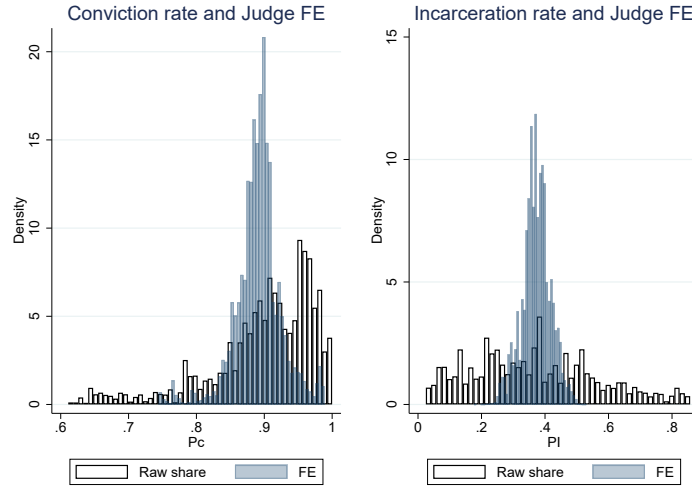


Figure 3: Identification: Defendant types space, judges' thresholds and treatment assignment



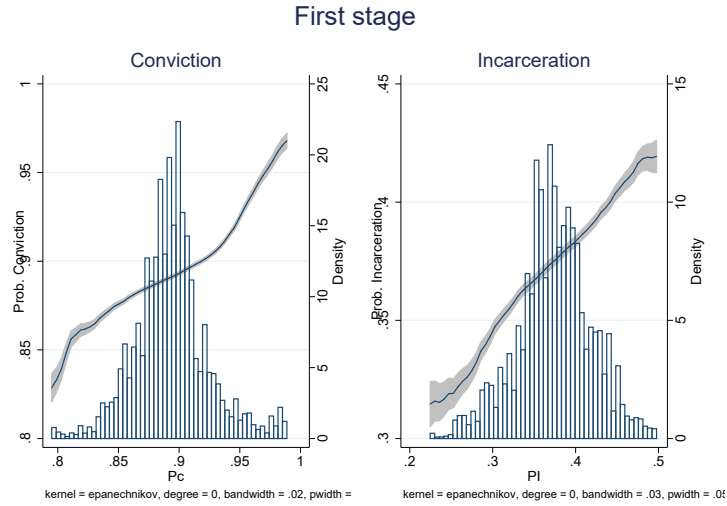
A defendant is characterized by a point in the unitary square. A judge is defined by a pair of thresholds along the two axes which determine treatment assignments. Defendants to the left of the conviction threshold are convicted, and those to the right are freed. Among the convicted, defendants below the incarceration threshold go to prison, and those above do not.

Figure 4: Judges' fixed effects



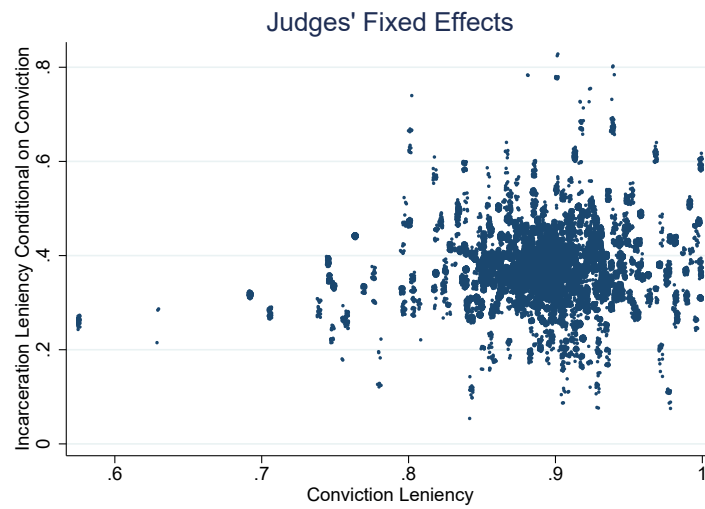
Source: Attorney General's office and criminal records. Raw rates are conviction/incarceration averages-by-judge. To construct the judge's fixed effect I take the residual at the judge level after regressing conviction/incarceration on (demeaned) randomization unit/year dummies, (demeaned) crime-level conviction/incarceration rates, without a constant.

Figure 5: First stage



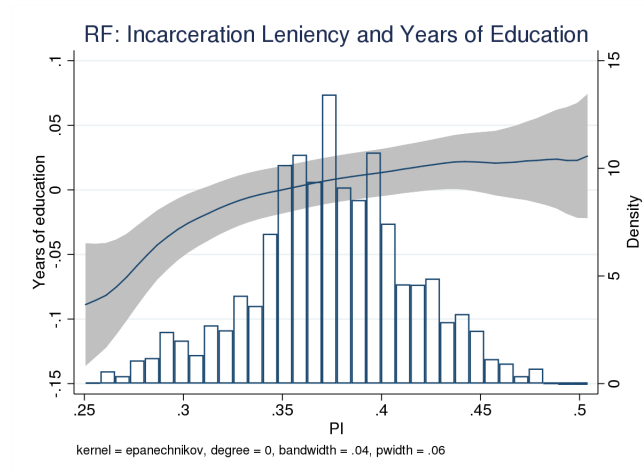
Source: Attorney General's office and criminal records. To construct the judge's fixed effect I take the residual at the judge level after regressing conviction/incarceration on (demeaned) randomization unit/year dummies, (demeaned) crime-level conviction/incarceration rates, without a constant. Local polynomial regressions of conviction and incarceration on judge stringency.

Figure 6: Scatter plot: Judges' fixed effects



Source: Attorney General's office and criminal records. To construct the judge's fixed effect I take the residual at the judge level after regressing conviction/incarceration on (demeaned) randomization unit/year dummies, (demeaned) crime-level conviction/incarceration rates, without a constant.

Figure 7: Reduced form



Notes: Histogram of parental incarceration judge leniency and the fitted value of local polynomial regressions of children's educational attainment on judge stringency.

Tables

Table 1: Population by conviction and incarceration

Sample:	Census: Adult popu- lation	SISBEN Criminal record		SISBEN w/ conviction By incarceration	
		No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)
Years of education	7.50	6.82	6.68	6.85	6.41
Finished High School D=1	35.9%	31.2%	22.8%	24.1%	20.6%
Income score		34.01	30.90	31.75	29.46
Gender (Male=1)	47.8%	47.6%	83.3%	83.8%	82.4%
# HH members	3.90	4.28	4.47	4.45	4.51
Occupation: Working D=1	49.9%	47.3%	65.4%	66.6%	63.5%
Head of the household D=1	42.6%	41.2%	47.1%	46.4%	48.3%
Year of birth	1964.8	1966.9	1974.8	1975.1	1974.4
Marital status: Single D.	44.9%	34.7%	40.7%	41.4%	39.7%
Obs	24,790,810	16,195,178	89,637	56,262	33,375
Years of education for Young Pop (15-19)*	8.41	7.69	7.03	6.77	6.41
Notes: Columns 1-5 are group means. HHH: Head of the household, HS: High School. D: Dummy. Income Score: Score from 0 to 100, calculated using variables on income and education of the members of the household, size and characteristics of the house. Adult population=20 years and older. Source: 2005 Census, SISBEN and criminal records.*Estimates for column 4 and 5 are from school enrollment records.					

Table 2: First stage

Dep var: Decision Dummy	(1)	(2)	(3)	(4)
	Conviction	Conviction	Incarceration	Incarceration
Judge Stringency	0.690*** [0.0627]	0.689*** [0.0622]	0.688*** [0.0485]	0.687*** [0.0486]
Controls	X		X	
F stat	121.2	122.7	263.0	256.1
Obs	116,062	116,062	71,950	71,950
Clusters	820	820	616	616
R-sq	0.136	0.136	0.374	0.376
adj. R-sq	0.13	0.13	0.369	0.37

Controls column 1: randomization unit fixed effects and an offense conviction index, column 2 adds gender, age, number of crimes, and crime category. Controls column 3: Randomization unit fixed effects, an offense incarceration index, Pc and Pc squared. Column 4 adds: Years of education, gender, income score, age at the time of th crime, occupation, and year of survey. Standard errors clustered at the randomization unit level. Sources: Attorney General's Office, criminal records and poverty census.

Table 3: Balance test

Trial Sample	Convicted Sample			
Dep. Var: Conviction / Incarceration stringency	Judge: Conviction stringency	Judge: Incarceration stringency		Judge: Incarceration stringency
Gender	-0.0286 [0.0306]	0.00159 [0.0220]	Gender	-0.00847 0.084
Age	0.383 [0.907]	1.253 [0.832]	Age	1.109 [0.704]
Number of charges	0.0366 [0.0290]	-0.0252 [0.0297]	Income Score	0.902 [1.193]
Violent crime	0.072 [0.0576]	-0.0346 [0.0282]	Education	0.275 [0.219]
Property crime	0.0365 [0.0334]	0.0219 [0.0267]	D: Working	0.0115 [0.0390]
Drugs related crime	-0.0806 [0.0492]	0.00307 [0.0293]	D: Studying	0.0139 [0.0127]
Misdemeanor	-0.012 [0.0294]	0.00474 [0.0169]	Sisben year	-0.00185 [0.0316]
Obs	116,062	101,638		71,950
Clusters	820	796		616
P value F-test	0.44	0.33		0.38

Standard errors clustered at the randomization unit level. Each row corresponds to a different regression of judge leniency and defendant characteristics controlling for randomization unit fixed effects and offense level conviction or incarceration rates. The F-test corresponds to a regression where I include all the variables at the same time. Source Attorney General's office, criminal records and Sisben. When testing balance across crime categories I construct an alternative measure of conviction stringency that doesn't parse-out crime level conviction rates.

Table 4: Monotonicity test: Out-of-sample First stage

	Women	Men	Violent	Not violent	Young	Old
Conviction: out of sample FE	0.767*** [0.0978]	0.185*** [0.0309]	0.260*** [0.0472]	0.135*** [0.0325]	0.302*** [0.0446]	0.340*** [0.0546]
Obs	20,665	147,066	77,011	147,195	50,267	70,042
Incarceration: out of sample FE	0.564*** [0.0927]	0.144*** [0.0266]	0.146*** [0.0450]	0.0888** [0.0408]	0.398*** [0.0572]	0.326*** [0.0449]
Obs	21,472	100,912	47,147	74,395	48,113	72,406

First stage regressions. Controls for randomization unit and crime conv/inc rate. Standard errors clustered at the randomization unit. I compute the judge conv/incarceration rate for the complement of each group and use it to estimate the first stage.

Table 5: Results

	(1)	(2)	(3)	(4)	(5)
Dep var: Years of education*	OLS	OLS	First Stage	Reduced form	IV
Parental incarceration	-0.455*** [0.0789]	-0.0587** [0.0286]		0.521** [0.238]	0.782** [0.365]
Judge Stringnecy			0.667*** [0.0719]		
Montiel-Pflueger Effective F stat			84.86		
Obs	43914	43908	43908	43908	43908
Clusters: Rand. Units	610	604	604	604	604
R squared	0.006	0.372	0.374	0.372	

Two-way clustered standard errors clustered at the randomization unit level and household level. Columns 2 to 5 control for randomization unit fixed effects, offense incarceration rate, Pc and Pc squared, year of birth, gender and survey year. *For column 3 the dependent variable corresponds to parental incarceration.

Table 6: Heterogeneous effects

IV	Girls	Boys	Mother	Father	Long Sent.
Dep var: Years of education	(1)	(2)	(3)	(4)	(5)
Parental Inc.	0.455 [0.441]	1.071** [0.495]	0.372 [0.650]	0.840** [0.418]	1.085* [0.594]
Montiel-Pflueger Effective F stat	79.05	46.87	27.10	65.01	48.66
Obs	21,620	22,294	9,855	34,059	35,500
	Type of crime		Young child	Older child	Short Sent.
	Violent	Not violent			
Parental Inc.	1.238* [0.727]	0.489 [0.478]	0.843* [0.446]	0.677 [0.564]	0.81 [0.522]
Montiel-Pflueger Effective F stat	33.02	84.86	75.26	54.08	58.24
Obs	17,005	26,909	25,376	25,376	35,418

Two-way clustered standard errors clustered at the randomization unit level and household level. Columns 2 to 5 control for randomization unit fixed effects, offense incarceration rate, Pc and Pc squared, year of birth, gender and survey year. Young child is younger than 8 years, and older is 8 to 14 years old.

A For Online Publication Appendix: Model and proofs

This Appendix continues with the discussion of Section 4.2. For ease of exposition, I will first explore identification under the assumption that $U^c \perp U^I$ and then I will go over the results without it.²⁸ Under the independence assumption we can identify $P_I(z)$ from the data. That is:

$$P(U^I < P_I(z)|U^c \leq P_c(z)) = P(U^I < P_I(z)) = P_I(Z)$$

The left hand side is observed from the data, the first equality follows directly from the independence assumption, and the last one from the uniform distribution of U^I . P_I is interpreted as the share incarcerated.

The goal is to identify and evaluate the treatment effect: $E(Y(t_I) - Y(t_c))$, which is a function of counterfactual variables $Y(t_I)$ and $Y(t_c)$. To achieve this goal, it is useful to express the observed expectations in terms of the variables that define the model:

$$E(Y \cdot \mathbf{1}[T = t_c]|P_c(Z) = p_c, P_I(Z) = p_I) = \tag{15}$$

$$= E(Y(t_c) \cdot \mathbf{1}[T = t_c]|P_c(Z) = p_c, P_I(Z) = p_I) \tag{16}$$

$$= E(Y(t_c) \cdot \mathbf{1}[U^c \leq p_c] \cdot \mathbf{1}[U^I > p_I]|P_c(Z) = p_c, P_I(Z) = p_I) \tag{17}$$

$$= E(Y(t_c) \cdot \mathbf{1}[U^c \leq p_c] \cdot \mathbf{1}[U^I > p_I]) \tag{18}$$

$$= \int_0^{p_c} \int_{p_I}^1 E(Y(t_c)|U^c = u^c, U^I = u^I) f_{u^c, u^I}(u^c, u^I) du^c du^I \tag{19}$$

$$\tag{20}$$

$$\begin{aligned} &= - \int_0^{p_c} \int_0^{p_I} E(Y(t_c)|U^c = u^c, U^I = u^I) f_{u^c, u^I}(u^c, u^I) du^c du^I + \\ &\quad \int_0^{p_c} E(Y(t_c)|U^c = u^c) f_{u^c}(u^c) du^c \end{aligned}$$

Equation (15) is an expectation observed in the data. Equality (16) comes from the definition of observed outcomes. Equality (17) expresses the indicator $\mathbf{1}[T = t_c]$ in terms of the inequalities of the choice model. Equality (18) uses the independence relation $Z \perp (U^c, U^I)$. Equality (19) expresses the expectation as the integral over the distribution of U^c, U^I where $f_{U^c, U^I}(u^c, u^I)$ stands for the probability density function of U^c, U^I at the point (u^c, u^I) , and is equal to one. Equality (20) modifies the integration region. This change is useful to apply the Lebesgue differentiation theorem next;

$$\frac{\partial^2 E(Y \cdot \mathbf{1}[T = t_c]|P_c(Z) = p_c, P_I(Z) = p_I)}{\partial p_c \partial p_I} = -E(Y(t_c)|U^c = p_c, U^I = p_I) \tag{21}$$

Equality (21) arises as a direct application of the Lebesgue differentiation theorem. What this result provides is a connection between the observed outcomes and the targeted counterfactual outcome.

²⁸Appendix B provides the intuition for the identification result under the independence assumption.

We can use the same steps applied to counterfactual $Y(t_c)$ to obtain the counterfactual for $Y(t_I)$. Combining these two I obtain:

$$\frac{\partial^2 E(Y \cdot \mathbf{1}[T \in \{t_c, t_I\}] | P_c(Z) = p_c, P_I(Z) = p_I)}{\partial p_c \partial p_I} = E(Y(t_I) - Y(t_c) | U^c = p_c, U^I = p_I) \quad (22)$$

In the language of Heckman and Vytlacil (2005), Equation (22) defines the marginal treatment effect (MTE) of outcome Y with respect to treatment assignment t_c and t_I . It is interpreted as the causal effect of incarceration versus conviction only, for the share of defendants whose culpability and punishment assessments, U^c and U^I respectively, are set at quantiles p_c and p_I . The derivative in Equation (22) traces the MTE of incarceration relative to conviction throughout the unitary square of U^c, U^I . This result is an application of Lee and Salanie (2018) and extends the result of Heckman and Vytlacil (1999). In Appendix B I explain graphically the intuition of this result. The main idea is that changes in P_c and P_I affect treatment assignment exogenously. Then, by examining the derivative of the outcome variables with respect to P_c and P_I , we capture how the outcome variable changes when treatment changes at each point in the space of the unobservable confounding variables.

The average treatment effect (ATE) is the causal effect of t_c and t_I on Y in the population, and it corresponds to the integral of the MTE over the support of U^c and U^I :

$$E(Y(t_I) - Y(t_c)) = \int_0^1 \int_0^1 \frac{\partial^2 E(Y \cdot \mathbf{1}[T \in \{t_c, t_I\}] | P_c(Z) = p_c, P_I(Z) = p_I)}{\partial p_c \partial p_I} dp_c dp_I \quad (23)$$

Without the assumption of independence between U^c and U^I , variation in P_I is only identified once the conviction threshold has been fixed. Thus, the counterfactual of interest is now: $Y(t_I)$ and $Y(t_c)$ for those who were convicted under $P_c = p_c$. This means the objective is to identify causal effects of the form: $E(Y(t_I) - Y(t_c) | U^c < p_c)$. Let:

$$E(Y \cdot \mathbf{1}[T = t_c] | P_c(Z) = p_c, P_I(Z) = p_I, U^c < p_c) = \quad (24)$$

$$= E(Y(t_c) \cdot \mathbf{1}[T = t_c] | P_c(Z) = p_c, P_I(Z) = p_I, U^c < p_c) \quad (25)$$

$$= E(Y(t_c) \cdot \mathbf{1}[U^I > p_I] | P_c(Z) = p_c, P_I(Z) = p_I, U^c < p_c) \quad (26)$$

$$= E(Y(t_c) \cdot \mathbf{1}[U^I > p_I] | U^c < p_c) \quad (27)$$

where I followed the same steps as before. Let:

$$P_I^* = Pr[U^I < P_I | U^c < p_c] = G(P_I) \quad (28)$$

P_I^* is the object I observe so I will define the observed expectations in terms of this variable:²⁹

$$E(Y(t_c) \cdot \mathbf{1}[U^I > G^{-1}(p_I^* | U^c < p_c)] | U^c < p_c) \quad (29)$$

$$\int_{P_I^*}^1 E(Y(t_c) | U^I = u^I, U^c < p_c) f_{u^I | U^c < p_c}(p_I^*) du^I \quad (30)$$

²⁹Where $f_{u^I | U^c < p_c}(p_I^*)$ in eq. (39) corresponds to: $f_{u^I | U^c < p_c}(p_I) \frac{\partial P_I((p_I^*))}{(p_I^*)}$

applying the Lebesgue differentiation theorem, this results in:

$$\frac{\partial E(Y \cdot \mathbf{1}[T \in \{t_c\}] | p_c, p_I, U^c < p_c)}{\partial p_I^*} = -E(Y(t_c) | U^I = p_I, U^c < p_c) f_{u^I | U^c < p_c}(p_I^*) \quad (31)$$

And ultimately;

$$E(Y(t_I) - Y(t_c) | U^c < p_c) = \int_0^1 \frac{\partial E(Y \cdot \mathbf{1}[T \in \{t_c, t_I\}] | P_c(Z) = p_c, P_I^*(Z) = p_I^*, U^c < p_c)}{\partial p_I^*} dp_I^* \quad (32)$$

What this result says is that we can trace the treatment effect of incarceration relative to conviction once we fix a threshold for conviction. We do this by evaluating the changes in the outcome variable when we change P_I^* . This delivers the MTE along the unobservable dimension $U^I | U^c < P_c$. The integral over the support of the instrument gives the LATE, or the ATE when the instrument has full support.

B Appendix: Intuition for the 2 dimension LATE

In this Appendix I go over the intuition of the results in Equations (20) to (22). This result extends the intuition behind LATE to a two-dimensional space. To make this point clear, let us think in discrete terms and use an example with four judges with threshold levels $\{P_c^1, P_I^1\}$, $\{P_c^1, P_I^2\}$, $\{P_c^2, P_I^1\}$, and $\{P_c^2, P_I^2\}$.³⁰

For notation purposes, let:

$$f(p_c, p_I) = E(Y \mathbf{1}[T \in \{t_c\}] | P_c(Z) = p_c, P_I(Z) = p_I) \quad (33)$$

and

$$g(p_c, p_I) = E(Y \mathbf{1}[T \in \{t_I\}] | P_c(Z) = p_c, P_I(Z) = p_I) \quad (34)$$

Next, I can rewrite, in discrete terms, the identification result in Equation (22) as:

$$\begin{aligned} \frac{\Delta f(p_c, p_I)}{\Delta p_c \Delta p_I} + \frac{\Delta g(p_c, p_I)}{\Delta p_c \Delta p_I} = \\ [f(p_c^2, p_I^2) - f(p_c^1, p_I^2)] - [f(p_c^2, p_I^1) - f(p_c^1, p_I^1)] + \\ [g(p_c^2, p_I^2) - g(p_c^1, p_I^2)] - [g(p_c^2, p_I^1) - g(p_c^1, p_I^1)] = E(Y(t_I) - Y(t_c) | u^c = p_c, u^I = p_I) \end{aligned} \quad (35)$$

Now, let us go over each term in (35). First, $f(p_c^2, p_I^2)$ represents the outcomes of convicted but not incarcerated individuals who had a judge with thresholds $\{P_c^2, P_I^2\}$. Panel a in Figure D7 shades the area in the u^c, u^I square that identifies these individuals. The next panels in Figure D7 highlight the following terms in Equation 35 and their differences. Ultimately, what Equation (22) is doing is identifying the complier range in a two-dimensional space, which instead of an interval is a rectangle (Figure D8).

I estimate (35) by fitting a polynomial on p_I and p_c and evaluating the cross-derivative on the support of the instruments. Figure D9 shows the MTE in the relevant segment of the (u^c, u^I) square.

³⁰Equivalent to $\{\text{HL}\}$, $\{\text{HH}\}$, $\{\text{LH}\}$, and $\{\text{LL}\}$ in Section 4.

There are some interesting features of these results; first, as before, as we increase u^I (defendants' quality), the effect on years of schooling decreases, confirming that this positive effect is accrued when incarceration removes a bad parent from the household. What is new in Figure D9 is that now we can also move along the u^c margin, or the "strength of the evidence" margin. The data also show that as evidence becomes weaker, the positive effects also decrease. Ultimately, what this exercise shows is that the effect on children is very sensitive to the type of case a judge is deciding on. In the case of Colombia, marginal incarcerations are of defendants still very negatively selected and with sufficient evidence against them, so that their children are better off without that parent. How this result extends to other settings is a function of the location of the marginal cases in the u^c , u^I square.

C Appendix : Data construction

In this appendix, I explain in detail the construction of the sample and variables I use throughout the paper. The starting point for my data construction are the two SISBEN surveys. These data are collected by the government to target social programs for the poor. The survey is conducted at the household level, and consists of two modules. In the first, it asks about the characteristics of the house (flooring material, number of bedrooms, etc), access to utilities, and assets in the households (TV, refrigerator, car, etc.). In the second part, all members of the household are listed with names and national identification numbers, and their relationship to the head of the household is specified. The questionnaire then asks about gender, age, education level, marital status, disability status, and occupation. This survey is applied to everyone living in a municipality with a population of 30,000 or less, and in larger municipalities local authorities target households who could be potential beneficiaries of welfare programs. If a household is not targeted by local authorities and wishes to be surveyed, it can easily request to be included. The government uses this information to create a formula that measures the household's ability to provide resources for its members, and computes a score for each household that determines eligibility for different social programs. These data provide me with i) identification numbers with municipality location to web-scrape criminal records and, ii) parent-to-child links.

I select the population of adults who lived in the 17 out of 33 municipalities that have criminal records online. These districts represent 67% of the population, and 69% of homicide and 83% of property crimes.³¹ I then web-scrape criminal records (from <http://procesos.ramajudicial.gov.co/consultaprocesos/>) by selecting the district and then searching individually for records with the ID numbers.

I find criminal records for 256,366 individuals. The top panel of Table C1 describes the sample restrictions. Table C2 shows differences between the characteristics of individuals in the final dataset and those who were dropped. For the set of observations that have sentence data, I find that there is no evidence of differential incarceration rates across samples.

To assess how representative my sample is of the prison population, I compare counts of individuals sentenced by year from my data with counts of new inmates from official records of the Prison Authority (INPEC). I only have information available for 2015; according to INPEC, there were 27,287 new inmates that year, from my data, I find that 5,932 defendants were sent to prison, which would suggest that I have data on 22% of the prison population. This number, however, should be taken with caution, because INPEC data include flows of inmates across prisons, and I don't have data on the size of these flows.

³¹Judicial districts with online data: Armenia, Barranquilla, Bogota, Bucaramanga, Buga, Cali, Ibagu , Florencia, Manizales, Medell n, Neiva, Palmira, Pasto, Pereira, Popayan, Tunja, and Villavieja.

Next, I link these convicts to the 518,765 individuals living in their households, of whom 192,842 are in the relevant cohort years (1990-2007), 92,301 experienced parental incarceration between ages 0 and 14 and the episode is observed after the first sisben survey; of these 59,370 are the child of a convict. Finally, I have education data for 74% of these children. This rate is close to the share of children between ages 12 and 17 who attend school, according to the census (76%).³² Table C3 shows regressions of missing education record on parental incarceration. I perform two exercises: the first on the whole sample and a second only on a sample of educational records that had yet to exist at the time of the criminal record. OLS estimates are close to zero, once I instrument for incarceration the estimate become negative but statistically equal to zero.

Table C1: Sample Construction

Criminal records data	
	Individuals
Initial sample	256,366
Non missing year, court, crime or district	166,310
Record post 2005	135,832
More than 15 cases per year/judge	103,131
Districts with more than 1 judge	98,806
Matches with spoa	90,526
Sisben: Poverty Census and Public School Data	
	Individuals
Initial sample	518,765
Cohort 1990 to 2007	192,842
Exposure window	92,301
Child of the convicted person	59,370
Non missing controls	58,739
Non missing education	43,908

³²Five percent of children in the poverty census attend private school which is another reason to have a missing record in the public school enrollment dataset.

Table C2: Sample selection-Defendants

Dep var: Out of sample D.	(1)	(2)
Incarceration		0.00141 [0.00204]
Years edu.	0.0018 [0.00150]	0.00118 [0.00157]
Income score	0.00118*** [0.0000822]	0.000837*** [0.0000879]
Male D.	-0.0400*** [0.00279]	-0.0209*** [0.00290]
Head HH D.	0.00877** [0.00370]	0.00771** [0.00389]
Single	-0.0298*** [0.00222]	-0.0213*** [0.00239]
Years edu. HHH	0.0004 [0.00150]	0.000919 [0.00157]
D: Studying	0.0264*** [0.00490]	-0.00653 [0.00486]
D: Working	0.0177*** [0.00209]	0.0154*** [0.00226]
Yob	-0.00708*** [0.0000877]	-0.00312*** [0.0000956]
Constant	14.55*** [0.173]	6.55E+00 [3279.3]
Obs	260,968	196,314
R-sq	0.14	0.306

Additional controls: Municipality FE and survey year FE. The first column includes all criminal records and the second restricts to the ones sentence data.

Table C3: Sample selection

Dep var: Missing Education records.	(1)	(2)	(3)	(4)
Parental incarceration	0.00506 [0.00599]	0.0101 [0.00636]	-0.084 [0.0581]	0.0154 [0.0679]
	OLS	OLS	IV	IV
Obs	58873	31172	58872	31152
R-sq	0.135	0.35		

Two-way clustered standard errors clustered at the randomization unit level and household level. Controls: randomization unit fixed effects, offense incarceration rate, Pc and Pc squared, year of birth, gender and survey year. Columns 1 and 3 correspond to the whole sample and columns 2 and 4 restrict to cases yet to appear at the time of the sentence.

D Appendix: Extra tables and figures

Table D1: Monotonicity test: Norris

Category	P-value
Gender	0.243
Income	0.997
Age	0.995
Working	0.447
Education	0.782
Violent crime	0.007
Gender#Age	0.922
Gender#Education	0.554
Gender#Income	0.907
Income#Education	0.445

Norris (2019) test for pairwise monotonicity.

Table D2: Monotonicity Test: Frandsen et al

Randomization Unit	Critical value	P-value
1	143.433	0.137
2	19.55	0.358
3	17.304	0.186
4	14.413	0.072
5	7.368	0.195
6	6.773	0.238
7	3.271	0.514
8	2.8	0.592
9	4.085	0.395
10	1.584	0.663
11	0.746	0.862
12	0.007	0.997
13	0.016	0.992
14	3.071	0.08
15	0.05	0.822
Joint test	224.471	0.104

Frandsen et al (2019) test for Monotonicity. I run the test in the randomization units where there are 4 or more judges and more than 800 cases. This corresponds to 68% of my sample.

Table D3: Sentencing guidelines

Sentencing guidelines		Prison time	
Crime	Colombia	US NY	
Possession of cocaine: 14 grams -100 grams	5 to 9 years	1 to 9 years	
Assault			
Simple/third degree	1 to 3 years	Up to 1 year	
2nd degree	2 to 7 years	3 to 7 years	
Theft			
Simple	2 to 9 years	Up to 1 year	
Aggravated theft	6 to 14 years	2-7 years	
Domestic violence	4 to 8 years	Less than a year to 25 years	

Source: Colombia articles 376, 112, 239, 240 of the penal code, respectively. For New York: 220.16, 120.00, 120.00, 155.25 or 165.40, 155.30 and 120.00 to 120.12 sections of New York penal law code, respectively.

Table D4: IV by P_c level group

Instrumental Variables	(1)	(2)
Dep var: Years of education	Low P_c	High P_c
Parental incarceration	0.845** [0.399]	0.802 [0.508]
Montiel-Pflueger Effective F stat	79.53	45.24
Obs	21925	21989
Clusters: Rand. Units	0.373	0.358
Two-way clustered standard errors clustered at the randomization unit level and household level. Controls for randomization unit fixed effects, offense incarceration rate, P_c and P_c squared, year of birth, gender and survey year.		

Table D5: Placebo check

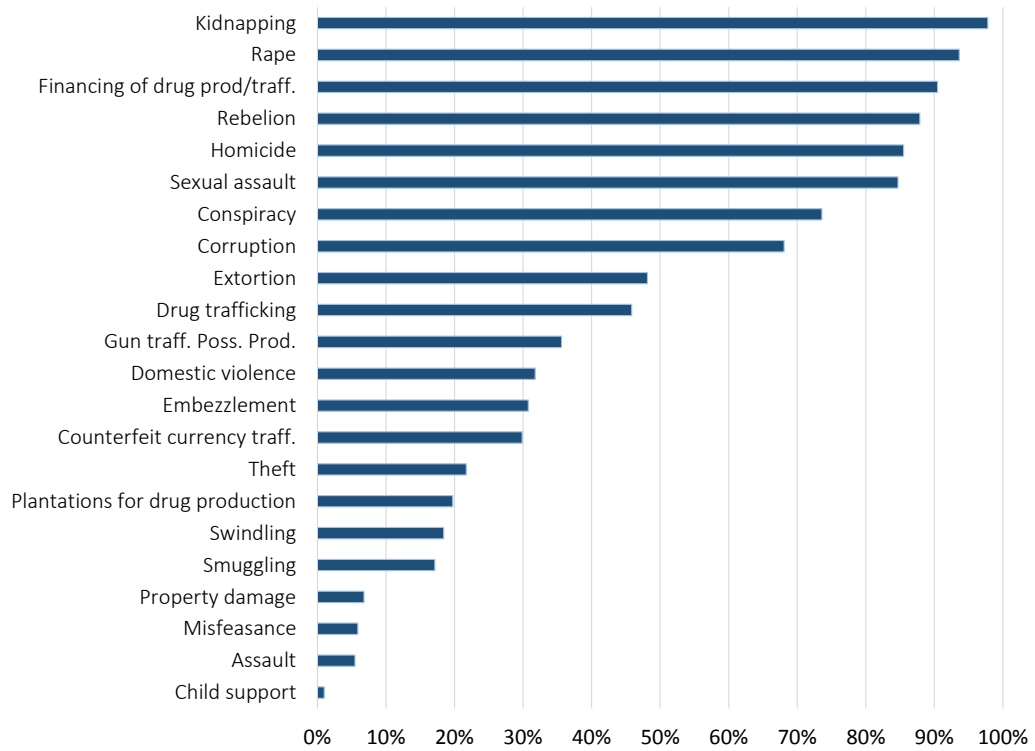
Placebo test

Dep var: Years of education	OLS	RF	IV
Parental inc.	-0.00867 [0.0140]		0.0632 [0.193]
Judge leniency		0.0396 [0.121]	
Obs	16,949	16,918	16,918
Two-way clustered standard errors clustered at the randomization unit level and household level. Controls for randomization unit fixed effects, offense incarceration rate, P_c and P_c squared, year of birth, gender and survey year. Different from the main specification here I restrict to cases where the initial schooling year is observed before the incarceration episode.			

Table D6: Changes after incarceration

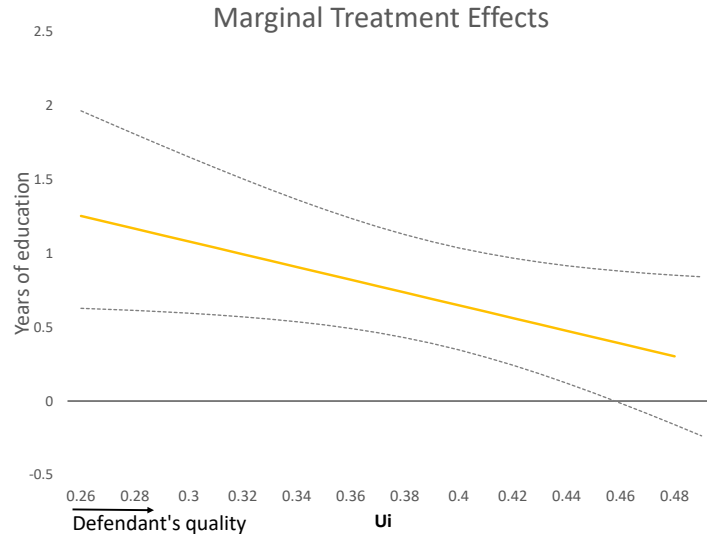
	(1)	(2)	(3)	(4)	(5)	(6)
Dep var:	LFP	Income score	Education head of HH	Male head of HH	People in the HH	Three Gen.HH
Incarceration	0.0687*** [0.0187]	-2.336*** [0.193]	0.0939*** [0.0299]	-0.0779*** [0.00604]	-0.0938*** [0.0303]	0.0201* [0.0110]
Mean Dep. Var	0.40	26.41	5.10	0.60	4.66	0.22
Obs	9,673	82,779	82,779	82,779	81,612	16,372
R-sq	0.223	0.745	0.203	0.187	0.33	0.1
Household fixed effect, poverty score, years of education of HHH, year of survey FE. Households members with data on both cross-sections of the poverty census and who had an incarceration episode in between surveys. Source: SISBEN and criminal records.						

Figure D1: Incarceration rates



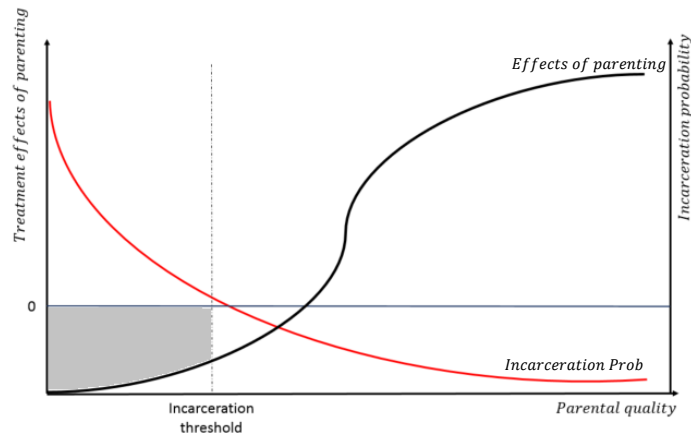
Source: Criminal records. Selected crimes, where I restrict to crimes with at least 100 cases.

Figure D2: MTE



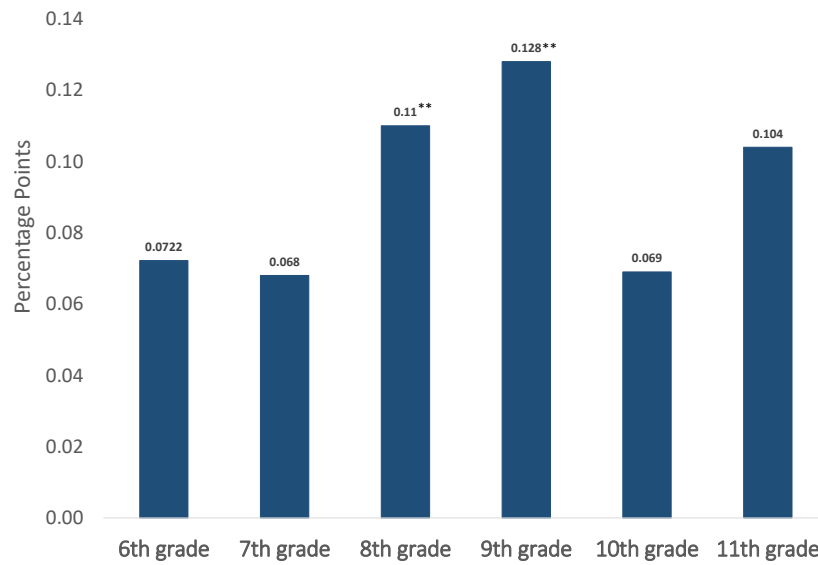
Notes: Following the LIV approach in Heckman and Vytlačil (2005) I regress $Y_{educ} = \alpha + \beta_1 P_i + \beta_2 P_i^2 + \beta_3 X$. Controls included: Randomization unit fixed effects, offense incarceration rate, Pc and Pc squared, year of birth, gender and survey year. Two-way clustered standard errors clustered at the randomization unit level and household level. I take the derivative of the equation with respect to P_i and plot the function. This graphs plots: $\beta_1 + 2\beta_2 P_i$.

Figure D3: Model of parenting and incarceration



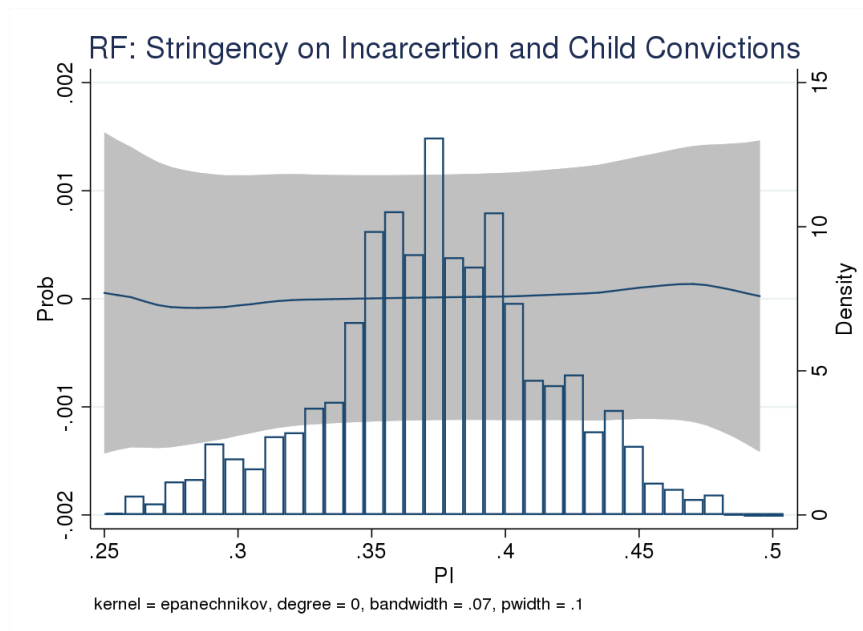
Notes: The x-axis traces parental quality: as we move to the right, parental quality increases. The y-axis measures the treatment effect of parenting: having better parents is better for children. The secondary y-axis measures incarceration probability.

Figure D4: Treatment effects by grade



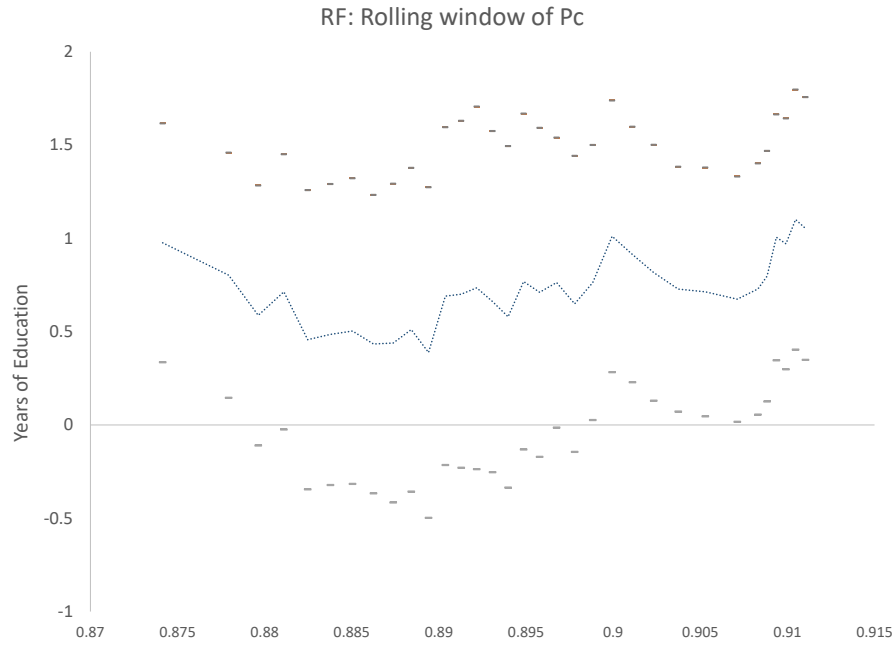
Notes: Two-way clustered standard errors clustered at the randomization unit level and household level. Controls: randomization unit fixed effects, offense incarceration rate, Pc and Pc squared, year of birth, gender and survey year.

Figure D5: Reduced form



Notes: Histograms of parental incarceration judge stringency and the fitted value of local polynomial regressions of children's criminal records on judge stringency.

Figure D6: Rolling reduced form



Notes: Two-way clustered standard errors clustered at the randomization unit level and household level. Reduced form estimates of a sample size of 26,000, with a rolling window of 800 on P_c . Grey lines represent 90% confidence intervals.

Figure D7: Identification in two dimensions

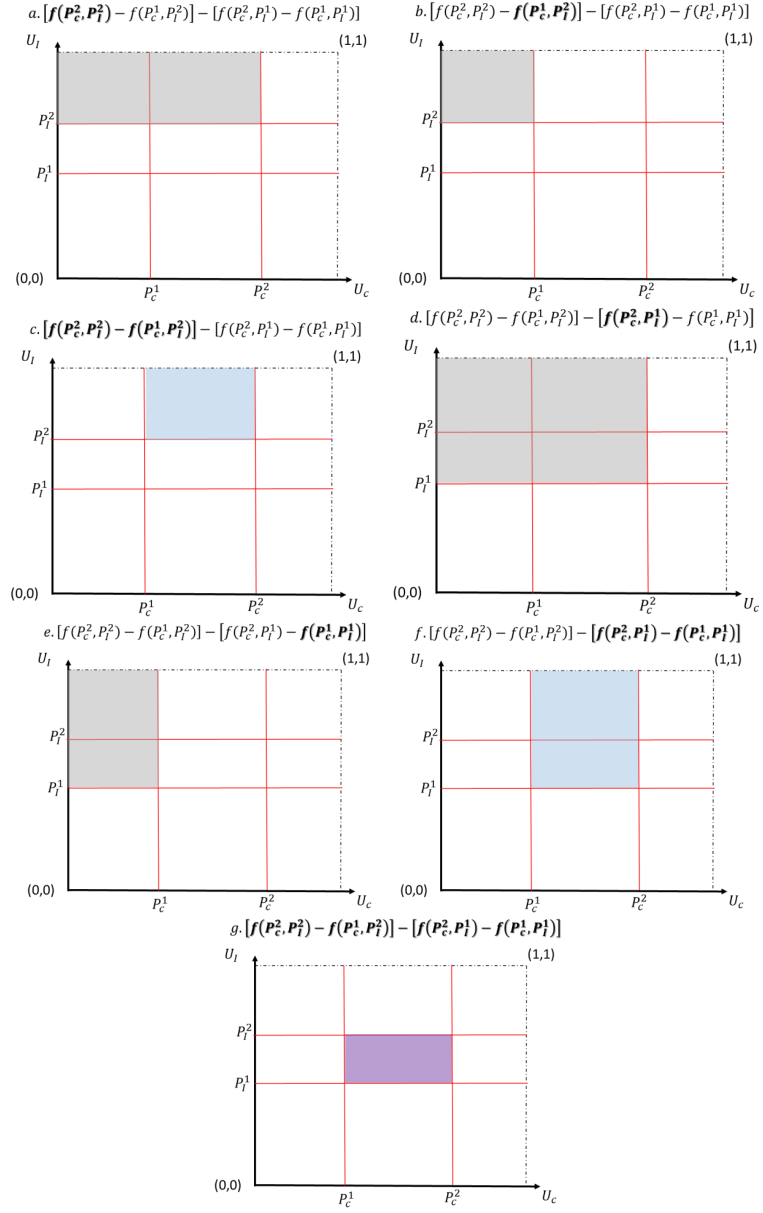


Figure D8: Compliers rectangle

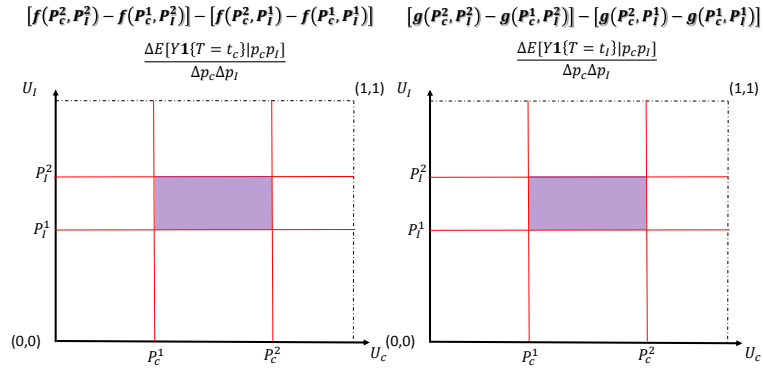


Figure D9: Unconditional MTE

