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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 negative associations have been documented between parental incarceration and a host of important indicators of children's wellbeing, such as mental health, education, and crime (Wakefield, 2014), 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 et al., 2005; Arditti, 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

[1] In 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) document that incarcerated men engage in domestic violence at a rate about four times higher than the rest of the population.

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 estimate the causal effect of parental incarceration on children's educational attainment in Colombia. I link sociodemographic data on households with children from SISBEN, the country's census of low-income populations, to criminal records for approximately 90,000 convicted parents for the years 2005 to 2016. In these data, I do not observe cases that did not lead to a conviction, so I cannot link unconvicted parents to their children. I am able, however, to combine these data with anonymized individual-level records from the Attorney General's Office that provide information on the universe of criminal cases along with judge and 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 convict and incarcerate defendants.[2] I extend the standard instrumentals variable framework to incorporate the fact that judges make multiple decisions: they decide both on conviction and incarceration —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 there are sufficient grounds to convict; then, for those convicted, the judge decides whether to incarcerate by evaluating the severity of the crime and whether there are any attenuating or aggravating factors. Effectively, I compare parents who were convicted under the same conviction stringency, but whose judges differed in their incarceration stringency.

This approach improves on the previous literature in three main respects. First, the multidimensional nature of the judge's decision (conviction, incarceration, sentences

[2]See 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.

length, etc.) has raised concerns about the validity of the exclusion restriction; that is, whether the estimated effect corresponds to the effect of incarceration or whether it is also the effect of the additional decisions the judge makes. By 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, although given the structure of my data in this application, I can only estimate the effect of incarceration.

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 judge assignment could have resulted in a different incarceration outcome. With an average schooling of 7.7 years, this effect corresponds to a 10 percent increase.

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 hetero-

[3] In Mueller-Smith (2017), the data exhibit multidimensional and non-monotonic sentencing patterns (the dimensions include 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.

geneity, differences in point estimates suggest that the benefit of parental incarceration is larger for boys than for girls, when incarceration is for a violent crime and when the incarcerated parent is the father, 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.

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 face higher risks of dropping out of school. Contemporaneous to my work, three other studies in developed countries 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. (2021) estimate null effects in test scores or grade repetition, but find that parental incarceration causes children to live in higher socio-economic 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 in Colombia and the fact that I focus on co-residing parents and not birth parents, and that prison sentences in my context are much longer and as a result constitute a much bigger shock for children, can all help explain why we observe large and 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,

[4]In Colombia prison sentences are 4.4 years, compared to 3.1 years in the US (Motivans, 2015) and three and eight months in Sweden and Norway, respectively (Bhuller et al. 2018; Dobbie et al. 2019).

[5]Prior 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.

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. In a context in which school admissions are decided based on 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 scores. In this context, the sample is first selected based on academic excellence, and the level of selection can be inferred from the GPA or test cutoff, and used in the regression in which financial need is estimated. The identification result is also useful in contexts in which, due to data entry burden, only a censored sample is fully entered into a system. For example, in domestic violence courts in Puerto Rico, complete case data are only entered into the system for cases in which an immediate temporary protection order is granted (APOS, 2020). If one is interested in using a judge instrument design on this sample to evaluate, for example, the effect of a final protection order or other court outcomes, one could do so by controlling for the level of selection in the dataset created by the judge's tendency to grant a temporary protection order, which can be recovered from the total case counts. A similar situation occurs when using administrative data to estimate the effect of foster care using an examiner design. In this case, researchers often only have access to data for cases that have been determined to be substantiated; if the social worker's overall caseload can be estimated, correction for the level of censoring can be applied (e.g. Bald et al., 2019 or Roberts, 2019).

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., 2005a; Oreopoulos et al., 2006), wealth (Black et al., 2005b), and welfare receipt (Dahl et al., 2014), among other outcomes. 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 and Baron (2020) for Michigan obtain positive effects on schooling, Bald et al., (2019) find mixed results for Rhode Island, and Doyle (2007, 2008) finds negative labor market and crime outcomes for Illinois. My results suggest that children may benefit from the absence of a convicted parent who is on the margin of incarceration.

In terms of policy implications, it is important to highlight that the result of this paper does not imply a recommendation to change the level of incarceration. Incarceration is a costly policy tool, and a cost-benefit analysis to estimate its optimal level is beyond the scope of this paper. Even when the average treatment effect of parental incarceration is positive, the MTE and heterogeneity analysis suggest that for important subsets of the population the effects are zero or negative. The result of this paper does imply that children of convicted parents who are marginally not incarcerated are in a vulnerable situation and the government could do more to protect them.

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 and the types of crimes involved.

Figure E1 illustrates how defendants are typically processed in Colombia's criminal justice system.[6] 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.[7] 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/year 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 corresponds to municipal courts, receives 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 homicide or terrorism, are assigned to a specialized judge (6%).[8] On average, there are 20 judges per

[6] Acuerdo CSJ, 3329.

[7] A 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.

[8] Art 35-37, Criminal Proceedings Code.

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. 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.[9] 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.

Judges are selected based on their performance in an exam from an open call of attorneys, with specific legal experience requirements for each rank. 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

[9] 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). See Table E5

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

[11] In 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.

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.

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. 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 people; the second wave, conducted from 2008 to 2010, has data on 25.6 million people.

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 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,

[12] Given 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.

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 in each courtroom to assign cases to judges.

I complement these data with individual-level, anonymized records from the Attorney 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 16 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.

[13] The 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

[14] 95% of children in SISBEN attend a public school (DANE-GEIH).

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 have missing court identifiers in all of their records. I keep only crimes committed after 2005 and after I observe individuals first in SISBEN, 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 for 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 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 and public-school enrollment records. In the following section, I characterize the population of convicted and incarcerated individuals, as well as their 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

[15]In 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.

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.

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 E2 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.

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 whether to incarcerate.

[16] In 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).

The assumption underlying this identification approach is that selection into incarceration is decided upon crossing a threshold of crime severity over a single dimension of unobserved heterogeneity. Departing from that literature, I account explicitly for the fact that there is selection across both conviction and incarceration, and allow this selection to be based on different dimensions of unobservables, and for heterogeneous treatment effects along these unobservables. Specifically, I consider a multivalued treatment model, where selection into conviction and incarceration is determined by the crossing of two distinct thresholds, which results in three possible treatment outcomes (not convicted, convicted but not incarcerated, and convicted and incarcerated), to provide a new identification result using the methodology developed in Lee and Salanie (2018).[17] 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 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 into 9 types, as in Figure E3.

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 terms of conviction: harsh (H_C) or lenient (L_C). Harsh judges do not require much evidence to convict a

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

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 judge then decides on incarceration I. The judge makes this decision based on an assessment of how harmful the convicted 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. So a judge can be of one of four types: $[H_C h_I, H_C l_I, L_C h_I, L_C l_I]$.

In this context, identification means comparing the same type of defendant across different treatment assignments. Following the structure of my data, I focus only 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 a lenient judge will make this decision only for type 1 defendants. So incarceration effects would also mask differences in selection, and as a result would not be identified.

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 denoted by: $[H_C, h_I]$ 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. In other words 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 ($[H_C, l_I]$) they will not. 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.

This intuition can be extended further to a continuous setting such as the one in Figure 1, where a defendant is characterized by their unobservables along the conviction and incarceration decision (u^C, u^I), a judge is characterized by a pair of thresholds (p_C, p_I) which split the space of defendants into those who are free, convicted but not incarcerated, and convicted and incarcerated. The treatment effect of incarceration relative to conviction is identified by comparing judges with the same conviction thresholds and different incarceration thresholds.

4.2 Model

In this subsection, I formalize the previous intuition to deliver 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. T_i denotes the assigned treatment of individual i, and takes values in $supp(T) = \{t_f, t_c, t_I\}$, where t_f stands for free or 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 $supp(\mathbf{V})$. 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:

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

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

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

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

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

$$\text{Exclusion Restriction}: \ Z \perp Y(t)|\mathbf{X} \text{ for all } t \in supp(T). \tag{4}$$

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, $\phi_I(.)$ 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:[18]

$$T = f_T(Z, \mathbf{V}) = \begin{cases} t_f & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) > \xi_c(Z)] \\ t_c & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) \leq \xi_c(Z)] \cdot \mathbf{1}[\phi_I(\mathbf{V}) > \xi_I(Z)] \\ t_I & \text{if } \mathbf{1}[\phi_c(\mathbf{V}) \leq \xi_c(Z)] \cdot \mathbf{1}[\phi_I(\mathbf{V}) \leq \xi_I(Z)] \end{cases} \tag{5}$$

This model relies on two separable threshold functions, which play the role of the monotonicity condition (Vytlacil, 2002).[19]

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

$$U^c = F_{\phi^c(\mathbf{V})}(\phi^c(\mathbf{V})) \sim Unif[0,1] \tag{6}$$

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

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:

$$P_c(z) = F_{\phi^c(\mathbf{V})}(\xi^c(Z)); z \in supp(Z) \tag{8}$$

$$P_I(z) = F_{\phi^I(\mathbf{V})}(\xi^I(Z)); z \in supp(Z). \tag{9}$$

Let $P_c(z)$ denote the probability of conviction when $Z = z$. The independence condition

[18] I assume the following standard regularity conditions: A1) $E(|Y(t)|) < \infty$ for all $t \in supp(T)$, A2) $P(T = t|Z = z) > 0$ for all $t \in supp(T)$ and all $z \in supp(Z)$ and, A3) $(\phi_c(\mathbf{V}), \phi_I(\mathbf{V}))$ are absolutely continuous with respect to Lebesgue measure in $\mathbb{R}^2$.

[19] Consider 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 $\phi_I(.)$ and the judges' incarceration decision.

(3) implies $P_c, P_I \perp U^c, U^I$. Using this notation, the model can be expressed as:

$$T = \begin{cases} t_f & \text{if } \mathbf{1}[U^c > P_c(z)] \\ t_c & \text{if } \mathbf{1}[U^c \leq P_c(z)] \cdot \mathbf{1}[U^I > P_I(z)] \\ t_I & \text{if } \mathbf{1}[U^c \leq P_c(z)] \cdot \mathbf{1}[U^I \leq P_I(z)] \end{cases} \tag{10}$$

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 . 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 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:

$$P_I^*(z) = Pr(U^I < P_I(z)|U^c < P_c(z)) \tag{11}$$

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*:

$$E(Y(t_I) - Y(t_c)|P_I^*(Z), U^c < p_c) = \tag{12}$$

$$\int_0^1 \frac{\partial E(Y \cdot \mathbf{1}[T \in \{t_c, t_I\}]|P_c(Z) = p_c, P_I^*(Z) = p_I^*)}{\partial p_I^*} dp_I^* \tag{13}$$

(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 (13) is useful in any setting where treatment assignment follows the design in equation (10). 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. In Appendix D, I provide Monte-Carlo simulations to the proposed estimation method. There, I show that the estimator proposed converges to the parameter of interest and that without this correction, the instrumental variables yields a biased estimator on the censored data.

5 Estimation

To apply the identification result, 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 leave-out means 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:

$$D_{itrz} = \gamma_{rt} + \gamma X_i + \epsilon_{itrz}$$

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

$$\widehat{P_{zi}} = \frac{1}{n_z - 1} \sum_{k \neq i} \widehat{\epsilon_{ktrz}},$$

where n_z is the number of cases of judge z, and $\widehat{\epsilon_{ktrz}}$ is the residual from a regression of the conviction/incarceration dummy on γ_{rt} and X_i.

Figure E4 shows the distribution of conviction and incarceration rates at the judge level, and $\widehat{P_{zi}}$ for both conviction and incarceration. From the graph, we can see that judge's fixed effects represent a sizable share of the variance in conviction and incarceration.

5.1 Instrument validity

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

$$D_{itorz} = \beta_0 + \gamma_{rt} + \gamma X_o + \beta_1 \widehat{P_{zi}} + \mathbf{X_i^T} \boldsymbol{\beta_2} + \epsilon_{itorz}.$$

As before, D_{itorz} corresponds to the conviction or incarceration dummy, and $\widehat{P_{zi}}$ is the leave-out mean of judge z assigned to person i in conviction or incarceration. I run this regression with and without controls, $\boldsymbol{X_i}$. 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.[20] In the incarceration regression, I control for schooling, income, occupation, gender, year of birth, and year in the survey.

Relevance

According to the results in Table E1, 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 assignment. Following Bhuller et al (2020), I report the Effective F-statistic of 122 and 256 for conviction and incarceration respectively, both of which are above the Montiel Pfluegger critical value of 23.1 for a worst case bias of 10% and also above 37.4 the

[20] These 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.

corresponding critical value for a bias of at most 5%. Figure 2 depicts this first-stage relationship for conviction (left panel) and incarceration (right panel).[21]

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 3 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.

Independence

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 2 checks the balance across defendants for my judge-stringency measures for conviction and incarceration. Across gender, age, number of charges and types of crime, 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 I control for the conviction level with a polynomial of P_c. In the second panel of Table 2, 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.

Exclusion Restriction

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

[21] Note 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. This can not be tested directly, but will hold if the monotonicity assumption is satisfied. Tables 3, Table E3 and Table E4 provide support for this monotonicity assumption. Additionally, according to a recent survey study by Sanchez Ruiz (2016), it is estimated that 91% of the prison population in Medellin is part of SISBEN. To the extent of my knowledge there is no other estimate of the share of SISBEN population in the criminal system.

Colombia, fines are rare and only associated with large property crimes, and because I model the conviction decision directly.[22] It is possible that strict judges are both more likely to incarcerate defendants and to give them longer sentences. If this is the case, the baseline estimates capture a linear combination of the extensive margin effect of being incarcerated and the intensive margin of longer sentences. To evaluate the importance of the judge's sentencing behavior, I check what happens if I control for a judge's sentence length stringency, defined as the average sentence length in the other cases a judge has handled. In Table E2, when I add a control for sentence length stringency, it has little effect on the IV estimates. There are, however, other soft dimensions of the judge's behavior that affect the outcomes of the trial, and that may be related to the judge's incarceration stringency such as how a judge treats a defendant, for which I cannot run a similar exercise as the one in Table E2.

Monotonicity

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, 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 3 shows these first-stage tests, in which I find positive estimates across all slices of the data. This, however 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 E3, I test pair-

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

wise monotonicity following Norris (2019) and find I cannot reject monotonicity across individuals characteristics, except for violent crimes.[23] Frandsen et al (2020) 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 E4, 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

Following the results in Section 4, my main specification takes the following form:
Second stage regression

$$Y_{itrz} = \alpha_0 + \phi_{rt} + \phi X_i + \alpha_1 \widehat{D_{itrz}} + \mathbf{W_i^T} \boldsymbol{\alpha_2} + \nu_{itrz}. \tag{14}$$

First stage regression

$$D_{itrz} = \beta_0 + \gamma_{rt} + \gamma X_i + \beta_1 \widehat{P_{zi}} + \mathbf{W_i^T} \boldsymbol{\beta_2} + \epsilon_{itrz}. \tag{15}$$

Y_{itrz} corresponds to years of education of child i, whose parent saw judge z, in year t and court-district r. Incarceration status D_{itrz} is instrumented using the judge's incarceration stringency. The controls in $\boldsymbol{W}$ include judge conviction stringency, gender, year of birth and SISBEN year. The regression also includes randomization unit fixed effects and offense-level incarceration rates.

I begin by discussing the OLS estimate of this design. Table 4 shows a regression of years of education on parental incarceration. 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

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

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, with an Effective F-statistic of 84.9.[24]

Next, Figure 4 provides a graphical representation of the reduced-form regression. It 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 exogenously increasing the probability of having a parent in prison, increases years of education. Column 4 of Table 4 shows the regression results for this reduced form: I estimate large and statistically significant improvements in years of educations. Finally, column 5 shows results from the IV; I estimate that having an incarcerated parent increases schooling by around 0.78 years on average for all conviction levels. These estimates are statistically different from zero.[25]

From a baseline level of education of 7.69 years of schooling, this effect corresponds to a 10% increase in educational attainment for this population. To put this in a historical context, from 1990 to 2010—a period that corresponds to the fastest increase in educational attainment in Colombia—average schooling increased by 2.96 years, from 5.99 to 8.96. The effect size estimated here corresponds to 26% of this historical increase.[26] Finally, the effect size estimated here is also of economic significance when compared with large policy interventions. For a reference, Jackson et al. (2016) estimate that a 10% increase in school spending across all 12 grades, as a result of school finance reforms that began in the 1970s, increased average completed schooling by 0.31 years.

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 E6 shows reduced-

[24] I follow Bhuller et al. (2020) Appendix D who in their Monte Carlo simulations, find that using the Montiel-Pflueger critical values and Effective F statistic works well for identifying issues with weak instruments in the context of the judge instrument.

[25] I find that the increase in years of education is mostly accrued through a higher graduation rate from middle school. Figure E5 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.

[26] Unesco, DNP-Unidad de Desarrollo Social and Ramirez and Tellez (2006).

form estimates of judge stringency on conviction probability; the effect is close to zero. However, the analysis is under-powered to detect 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 Robustness

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 E7 I instead order observations along P_c, and run multiple regressions on a rolling window over P_c, moving the window 800 observations each time. Figure E7 in the Appendix shows that for each sample, I find a positive effect of incarceration on education. In addition, in Table E6 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.

Table E7 in the Appendix explores alternative specifications, using different levels of clustering, sample restrictions varying the minimum case-load of judges in the sample and excluding covariates. The IV result is robust to all of these specification changes. Table E8 uses an alternative definition of the instrument, where I construct the instrument as the average conviction and incarceration rate for each judge, without residualizing randomization unit fixed effects. The result are very similar to my baseline estimation. Finally, 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. Columns 6-8 of Table 4 show that there is no supporting evidence that the positive effects I estimate are the result of pre-existing differences in educational attainment.

5.4 Parents & Children at the Margin

To derive policy implications, it is important to acknowledge the local nature 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.[27] 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, 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.[28]

I characterize compliers by observable characteristics in Table 5. As explained in Abadie (2003), these characteristics can be recovered by calculating the fraction of compliers in different subsamples. For most subgroups, their representation among compliers is similar as in the overall sample. The most distinctive feature of the compliers is their educational background: 53% of complier children have parents with high education, while their fraction in the entire sample is only 46%.[29] In addition, the type of charges in the complier population are less likely to be related to family affairs such as domestic violence or child support charges (82% are not family related in the complier population versus 72% overall). Along other characteristics such as age, sex, and other types of crime, the complier population is very similar to the overall population.

[27] The interpretability of IV estimates as a weighted average of complier treatment effects relies on either a monotonicity assumption or restrictions on treatment effect heterogeneity (Norris et al. 2021). Tables 3, E3 and E4 provide evidence that support these monotonicity assumptions.

[28] Parental 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:

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

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.

[29] High education in this sample is measured as having more than primary education.

5.5 Heterogeneity

In this 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 defendants' quality, broadly defined. The intuition is as follows: Parents who are incarcerated under the most lenient judges have worse characteristics than those incarcerated under strict judges. This is because 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.[30] 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 5). 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. Specifically, I find that boys' schooling increases by 1.07 years, whereas girls' schooling increases by 0.46 years, although this difference is not statistically distinguishable. 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,

[30] I 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.

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.

5.6 Mechanisms

5.6.1 What explains the positive effect?

The results suggest that living with a convicted parent has negative consequences. There are many reasons to believe that this is plausible. First, criminals are more likely to exert psychological and physical violence at home, and this can often be detrimental to a child's well-being. In the US context, Western et al. (2004) find that incarcerated men engage in domestic violence at a rate about four times higher than the rest of the population. Further, psychology research documents that spending time with parents who engage in high levels of antisocial behavior is associated with more conduct problems for their children (Jaffee et al., 2003). This literature concludes that the positive effects of being raised by married biological parents depend on the quality of care the parents provide.

Second, Chimeli and Soares (2017) document that illegal business activities cause violence and crime. Taking this finding to the household, this could translate into additional stress and a dangerous family environment brought by the violence and threats of violence faced by the parent involved in illegal activities, and related to guaranteeing property rights or resolving disputes within the business. There is also literature on the intergenerational transmission of violence, substance abuse, and crime. Specifically, in the role-model theory, in which children directly observe and model their parents' behavior, incarcerating parents could be beneficial, as it removes bad role models from the house and forces children to update their beliefs about the consequences of criminal behavior (Hjalmarsson and Lindquist, 2012). Beyond intergenerational transmission, childhood exposure to negative behaviors is documented to have direct adverse effects on outcomes in both childhood and adulthood (Balsa, 2008; Chatterji and Markowitz, 2000).

5.6.2 How does the environment of the child change?

Identifying the causal effects of incarceration on household structure, mental health, and family relationships is key to understanding the results I have presented, but is outside the scope of the current study. Nevertheless, to begin characterizing the changes that households experience, I first estimate the effect of incarceration on parental recidivism. Table E9 shows instrumental variable estimates on the probability of future conviction and incarceration. I find no statistically significant effects on recidivism either in the short or medium term.

Next I look at the changes in the household environment after an episode of incarceration. I do this through two approaches: first, 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 (3- to 5-year window) and estimate how the household changed after the episode of parental incarceration. Appearing in both waves of the SISBEN is not random, and leaving the sample is generally associated with an improvement in living standards.[31] With this caveat, Table E10 shows OLS regressions that provide suggestive evidence that incarceration is associated with an increase in the labor force participation (LFP) of the spouse, a worsening of the income score of the household, a decrease in the probability of a male as the head of the household, and an increase in the education of the head of the household—mostly because mothers have more schooling than fathers. I also find that the probability of living with grandparents increases. These changes suggest that over a short period after a parent goes to prison, the child's environment goes through a big transformation in terms of who the child is living with, their role, and their income level. Ultimately, the incarceration of a parent allows the household

[31] By definition this population differs from the complier population that identifies the treatment effects in the IV estimation.

to re-optimize and transition to a new equilibrium that is on net beneficial to the child. In the second exercise, I take only outcome variables observed in the second wave and instrument parental incarceration with the judge incarceration stringency. This exercise shows results in the same direction as the first difference OLS regression; however, standard errors are substantially larger.

5.7 External Validity & Relation to the Literature

Three contemporaneous papers investigate the effects of parental incarceration with similar quasi-experimental designs. For Scandinavia, 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 the US, Norris et al. (2021) 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. Understanding what drives these differences improves our ability to make policy recommendations to improve the well-being of children of incarcerated parents. Here, I address the main differences across contexts with special emphasis on the differences in the complier population and the different counterfactuals faced by children. I also propose a simple framework that captures the core mechanism that drives the sign and the size of the effect.

Different from the other settings, Colombia has a much higher crime rate than the US and Scandinavia. According to the United Nations, in 2018 the homicide rate in Colombia was 25 per 100,000 compared with 5 in the US and 1 and 0.5 in Sweden and Norway, respectively. This is a critical distinction, since in the Colombian context and in many developing countries, crime is at the center of public policy and as a consequence, there is considerable urgency for research into this topic along with evidence-based policy recommendations. Second, the higher crime rates combined with lower incarceration rates in Colombia change the selection of incarcerated parents, specifically marginal defendants in Colombia are likely to engage in more serious criminal activity than those at the margin of incarceration in the other contexts. If criminal activity is negatively correlated with parenting quality, which is what the MTE suggests, the effect of incarceration should be more beneficial in Colombia.

Another important distinct feature is the strength of the incarceration treatment. Specifically, sentences in Colombia are dramatically longer than in the other contexts. Prison sentences in Colombia are on average 4.4 years, compared with 3.1 years in the US (Motivans, 2015) and three and 8 months in Sweden and Norway (Bhuller et al. (2018); Dobbie et al. (2019)). So the "treatment" children face is disproportionally large in Colombia. If on average the parent who is removed from the household has a negative effect, the longer the separation, the larger this benefit. Additionally, this longer separation can trigger permanent changes in the household that may also allow children and their families to settle into a new equilibrium that is not possible with shorter-term disruptions. Furthermore, given the structure of my data, my analysis studies only children who were co-residing with their parents prior to the conviction episode, whereas the other work studies birth parents. Prior 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 attenuate the size of the treatment effects in the other contexts.

Other important features are the differences in income levels, quality of public education, and generosity of the welfare system. Colombia fares worse along all these dimensions, and what that translates into is a greater risk of dropping out of school for poor and vulnerable children.[32] According to the World Bank (2020), in 2016 in Colombia only 47% of the population has secondary education completed, compared to 75% in Sweden, 78% in Norway and 89% in the US. As a result, a response in educational attainment is more likely in my context than in the others.

To assess these differences in a systematic way, I provide a framework motivated by the heterogeneity in results, which links parental quality to the treatment effect of parenting and to the probability of incarceration. Figure 6 summarizes this framework. The x-axis traces parental quality: as we move to the right, parental quality increases. The y-axis

[32] Svitaschi (2020) finds that cash transfers mitigate the negative effects of being exposed to illegal industries.

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 6).

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. 2021 and Dobbie et al. 2019) consider birth parents who may not necessarily co-reside with their children. In this framework we can hypothesize that this translates into smaller treatment effect of parents and as a result 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 & Policy Discussion

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, in contrast to what is observed in the correlational analysis, parental incarceration increases 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.

It is important to highlight that the result of this paper does not imply a recommendation to change the level of incarceration. First, incarceration is a costly policy tool, and a comprehensive cost-benefit analysis is beyond the scope of this paper. Second, even when the average effect for the complier population is positive, the MTE and heterogeneity analysis suggest that for a part of the population the effects are zero and negative; more work needs to be done to characterize the households in each of these groups to better assist them. Third, what the results of this paper do imply is that children of convicted parents who are marginally not incarcerated are in a vulnerable situation and the government can do more to protect them. In some cases, these children are exposed to a negative role model or an abusive parent at home, and visits and assistance from child protection social workers could help these families in the aftermath of a decision not to incarcerate to ensure the children are safe. Alternatively, foster care or a similar intervention could be appropriate in more extreme cases.

I finish with an invitation for future work on this topic. Further research is needed to understand the mechanisms that drive the effects of parental incarceration and their heterogeneity. This will help identify children and households who are at risk of following a path of socioeconomic vulnerability, which will yield more effectively tailored public policies. This paper and contemporaneous work provide new evidence on the causal effect of parental incarceration, and have changed previously held priors based mostly on correlation-type evidence, but more work is needed. An important gap in the literature is the lack of estimates on the effects of reunification after prison. Parents eventually leave prison; some will return to live with their children, and this situation constitutes a new set of challenges for the household that remain unexplored.

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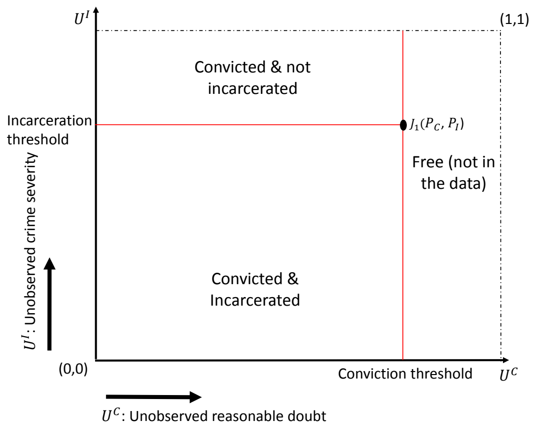
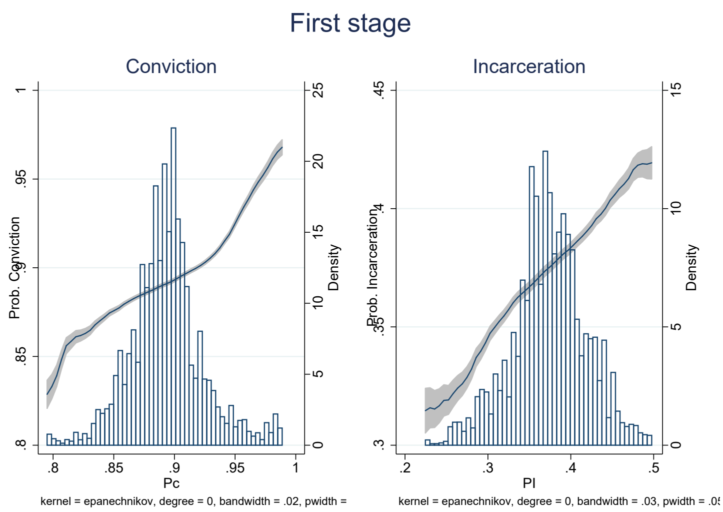
Figures

Figure 1: Identification: Defendant types space, judges' thresholds and treatment assignment



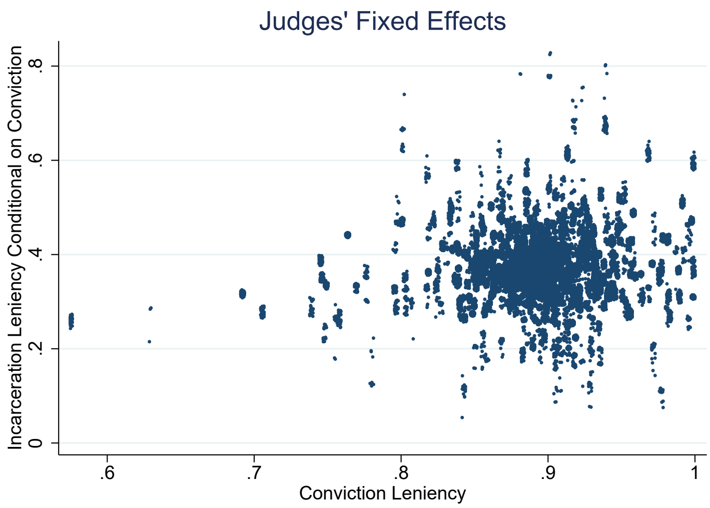
Notes: A defendant is characterized by a point in the unitary square. A judge is defined by a pair of thresholds along the two axes which determine treatment assignments. Defendants to the left of the conviction threshold are convicted, and those to the right are freed. Among the convicted, defendants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

Figure 2: First stage



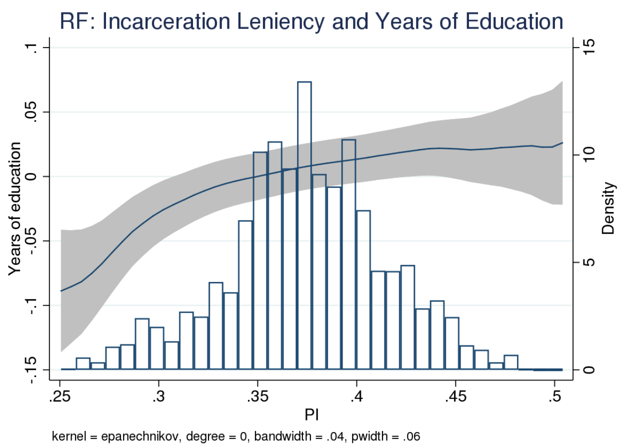
Source: Attorney General's office and criminal records. The histogram corresponds to the residualized leave-out means conviction and incarceration. Local polynomial regressions of conviction and incarceration on judge stringency.

Figure 3: Scatter plot: Judges' fixed effects



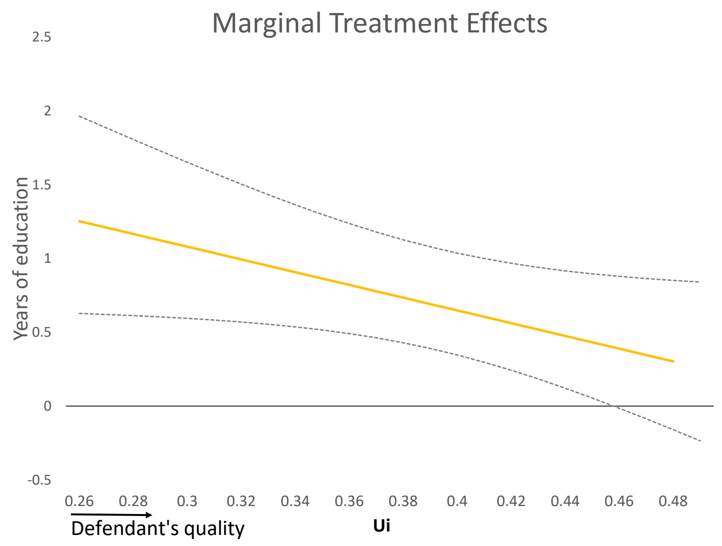
Source: Attorney General's office and criminal records. To construct the judge's fixed effect I take the residual at the judge level after regressing conviction/incarceration on (demeaned) randomization unit/year dummies, (demeaned) crime-level conviction/incarceration rates, without a constant.

Figure 4: Reduced form



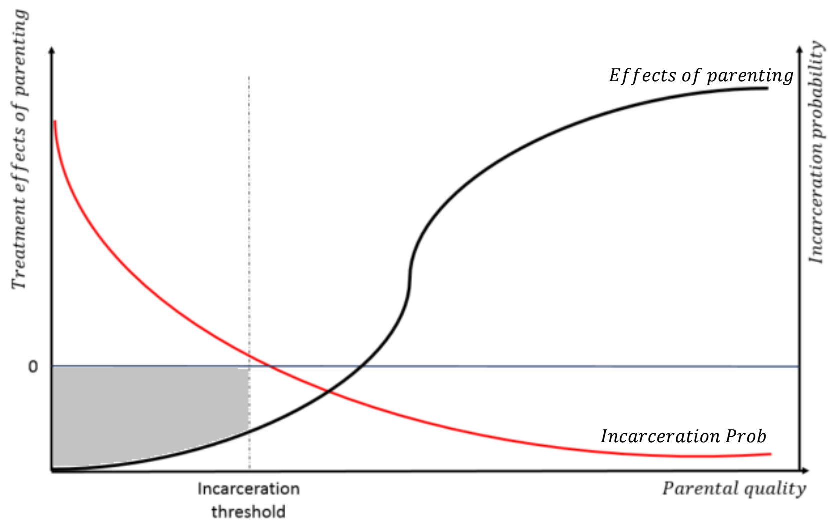
Notes: Histogram of parental incarceration judge leniency and the fitted value of local polynomial regressions of children's educational attainment on judge stringency.

Figure 5: MTE



Notes: Following the LIV approach in Heckman and Vytlacil (2005) I regress $Y_i = \alpha + \beta_1 P_I + \beta_2 P_I^2 + \beta_3 X$. P_I corresponds to the judge incarceration stringency. 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: $\beta_1 + 2\beta_2 P_I$. U_I on the x-axis is the underlying unobservable we are identifying the marginal treatment effect for.

Figure 6: 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.

Tables

Table 1: Population by conviction and incarceration

Sample:	Census: Adult population	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
Male	47.8%	47.6%	83.3%	83.8%	82.4%
# Household 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=1.	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

Column 1 corresponds to national level totals for the adult population from the 2005 Census. Columns 2 to 5 correspond to the population in Sisben in the analysis sample. D=1: Dummy variable corresponding to the stated category. 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 columns 4 and 5 are from school enrollment records.

Table 2: Balance test

Trial Sample			Convicted Sample	
Dep. Var: Conviction / Incarceration stringency	Judge: Conviction stringency	Judge: Incarceration stringency		Judge: Incarceration stringency
Gender	-0.0286	0.00159	Gender	-0.00847
	[0.0306]	[0.0220]		0.084
Age	0.383	1.253	Age	1.109
	[0.907]	[0.832]		[0.704]
Number of charges	0.0366	-0.0252	Income Score	0.902
	[0.0290]	[0.0297]		[1.193]
Violent crime	0.072	-0.0346	Education	0.275
	[0.0576]	[0.0282]		[0.219]
Property crime	0.0365	0.0219	Working: D=1	0.0115
	[0.0334]	[0.0267]		[0.0390]
Drugs related crime	-0.0806	0.00307	Studying: D=1	0.0139
	[0.0492]	[0.0293]		[0.0127]
Misdemeanor	-0.012	0.00474	Sisben year	-0.00185
	[0.0294]	[0.0169]		[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.* p<0.10, ** p<0.05, *** p<0.01.

Table 3: Monotonicity test: Out-of-sample First stage

	Women	Men	Violent	Not violent	Young	Old
Conviction:	0.767***	0.185***	0.260***	0.135***	0.302***	0.340***
out of sample FE	[0.0978]	[0.0309]	[0.0472]	[0.0325]	[0.0446]	[0.0546]
Obs	20,665	147,066	77,011	147,195	50,267	70,042
Incarceration:	0.564***	0.144***	0.146***	0.0888**	0.398***	0.326***
out of sample FE	[0.0927]	[0.0266]	[0.0450]	[0.0408]	[0.0572]	[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.* p<0.10, ** p<0.05, *** p<0.01.

Table 4: Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var: Years of education*	**OLS**	**OLS**	**First Stage**	**Reduced form**	**IV**	**OLS**	**Reduced form**	**IV**
Parental incarceration	-0.455***	-0.0587**			0.782**	-0.00867		0.0632
	[0.0789]	[0.0286]			[0.365]	[0.0140]		[0.193]
Judge Stringency			0.667***	0.521**			0.0396	
			[0.0719]	[0.238]			[0.121]	
F stat					96.68			
Effective F stat					84.86			
Obs	43,914	43,908	43,908	43,908	43,908	16,949	16,918	16,918
Clusters: Rand. Units	610	604	604	604	604	560	538	538
R squared	0.006	0.372	0.374	0.372		0.71	-0.032	

Two-way clustered standard errors clustered at the randomization unit level and household level. Columns 2 to 8 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. Columns 6-8 are placebo regressions. Different from the main specification here I restrict to cases where the initial schooling year is observed before the incarceration episode.* p<0.10, ** p<0.05, *** p<0.01.

Table 5: Characteristics of Marginal Cases

Parental characteristic	First Stage (1)	P[X=x] (2)	P[X=x|Complier] (3)	P[X=x|Complier]/P[X=x] (4)
Mother	0.721***	0.224	0.212	0.945
	[0.135]		[0.0571]	
Father	0.659***	0.776	0.788	1.016
	[0.0810]		[0.0571]	
Older (>33yo)	0.716***	0.583	0.588	1.008
	[0.0836]		[0.0702]	
Younger(<33yo)	0.638***	0.417	0.412	0.988
	[0.0987]		[0.0702]	
Only primary	0.634***	0.537	0.471	0.877
	[0.102]		[0.0760]	
Some secondary or more	0.712***	0.463	0.529	1.142
	[0.0856]		[0.0760]	
Violent crime	0.560***	0.387	0.402	1.038
	[0.0958]		[0.0815]	
Not Violent crime	0.705***	0.613	0.598	0.976
	[0.0984]		[0.0815]	
Not Drug related	0.625***	0.754	0.815	1.081
	[0.0748]		[0.0653]	
Not Family-crime related	0.648***	0.723	0.82	1.134
	[0.0763]		[0.0658]	

Column 1 corresponds to the first stage regression for each specific group. Column 2 is the frequency of the group in the estimation sample. Column 3 corresponds to the estimation of the characteristic in the complier sample, following Abadie (2003) and corresponds to a 2sls regression where the dependent variable corresponds to the endogenous variable multiplied by the indicator of the group. Column 4 divides column 3 by column 2 and corresponds to the complier relative likelihood.* p<0.10, ** p<0.05, *** p<0.01.

Table 6: Heterogeneous effects

IV	**Girls**	**Boys**	**Mother**	**Father**	**Long Sent.**
Dep var: Years of education	(1)	(2)	(3)	(4)	(5)
Parental Incarceration	0.455	1.071**	0.372	0.840**	1.085*
	[0.441]	[0.495]	[0.650]	[0.418]	[0.594]
P-value diff	[0.209]		[0.623]		[0.865]
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**	**Older**	**Short**
	Violent	Not violent	**child**	**child**	**Sent.**
Parental Incarceration	1.238*	0.489	0.843*	0.677	0.81
	[0.727]	[0.478]	[0.446]	[0.564]	[0.522]
P-value diff	[0.212]		[0.815]		
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. Young child is younger than 8 years, and older is 8 to 14 years old. Long sentence is defined as sentences longer than 64 months.* p<0.10, ** p<0.05, *** p<0.01.

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.[33] 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:

$$\begin{aligned}
&E(Y \cdot \mathbf{1}[T = t_c]|P_c(Z) = p_c, P_I(Z) = p_I) = && (16)\\
&\quad = E(Y(t_c) \cdot \mathbf{1}[T = t_c]|P_c(Z) = p_c, P_I(Z) = p_I) && (17)\\
&\quad = E(Y(t_c) \cdot \mathbf{1}[U^c \leq p_c] \cdot \mathbf{1}[U^I > p_I]|P_c(Z) = p_c, P_I(Z) = p_I) && (18)\\
&\quad = E(Y(t_c) \cdot \mathbf{1}[U^c \leq p_c] \cdot \mathbf{1}[U^I > p_I]) && (19)\\
&\quad = \int_0^{p_c} \int_{p_I}^1 E(Y(t_c)|U^c = u^c, U^I = u^I) f_{u^c u^I}(u^c, u^I)\, du^c\, du^I && (20)\\
& && (21)
\end{aligned}$$

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

Equation (16) is an expectation observed in the data. Equality (17) comes from the definition of observed outcomes. Equality (18) expresses the indicator $\mathbf{1}[T = t_c]$ in terms of the inequalities of the choice model. Equality (19) uses the independence relation $Z \perp (U^c, U^I)$. Equality (20) 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 (21) modifies the integration region. This change is useful to apply the Lebesgue differentiation theorem next;

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

Equality (22) 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. 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:

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

In the language of Heckman and Vytlacil (2005), Equation (23) 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

[33] Appendix B provides the intuition for the identification result under the independence assumption.

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 (23) 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:

$$E(Y(t_I) - Y(t_c)) = \int_0^1 \int_0^1 \frac{\partial^2 E(Y \cdot \mathbf{1}[T \in \{t_c, t_I\}] | P_c(Z) = p_c, P_I(Z) = p_I)}{\partial p_c \partial p_I} dp_c dp_I \tag{24}$$

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:

$$\begin{aligned} & E(Y \cdot \mathbf{1}[T = t_c] | P_c(Z) = p_c, P_I(Z) = p_I, U^c < p_c) = && (25) \\ & = E(Y(t_c) \cdot \mathbf{1}[T = t_c] | P_c(Z) = p_c, P_I(Z) = p_I, U^c < p_c) && (26) \\ & = E(Y(t_c) \cdot \mathbf{1}[U^I > p_I] | P_c(Z) = p_c, P_I(Z) = p_I, U^c < p_c) && (27) \\ & = E(Y(t_c) \cdot \mathbf{1}[U^I > p_I] | U^c < p_c) && (28) \end{aligned}$$

where I followed the same steps as before. Let:

$$P_I^* = Pr[U^I < P_I | U^c < P_c] = G(P_I) \tag{29}$$

P_I^* is the object I observe so I will define the observed expectations in terms of this variable:[34]

$$E(Y(t_c) \cdot \mathbf{1}[U^I > G^{-1}(p_I^* | U^c < p_c] | U^c < p_c) \tag{30}$$

$$\int_{P_I^*}^1 E(Y(t_c) | U^I = u^I, U^c < p_c) f_{u^{I*}|U^c < p_c}(p_I^*) du^I \tag{31}$$

applying the Lebesgue differentiation theorem, this results in:

$$\frac{\partial E(Y \cdot \mathbf{1}[T \in \{t_c\}] | p_c, p_I, U^c < p_c)}{\partial p_{I^*}} = -E(Y(t_c) | U^I = p_I, U^c < p_c) f_{u^I|U^c < p_c}(p_I^*) \tag{32}$$

And ultimately;

$$E(Y(t_I) - Y(t_c) | U^c < p_c) = \int_0^1 \frac{\partial E(Y \cdot \mathbf{1}[T \in \{t_c, t_I\}] | P_c(Z) = p_c, P_I^*(Z) = p_I^*, U^c < p_c)}{\partial p_I^*} dp_I^* \tag{33}$$

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.

[34]Where $f_{u^{I*}|U^c<p_c}(p_I^*)$ in eq. (31) corresponds to:$f_{u^I|U^c<p_c}(p_I) \frac{\partial P_I((p_I^*)}{(p_I^*}$

B Appendix: Intuition for the 2 dimension LATE

In this Appendix I go over the intuition of the results in Equations (22) to (24). 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 $\{P_c^2,P_I^2\}$.[35]

For notation purposes, let:

$$f(p_c, p_I) = E(Y\mathbf{1}[T \in \{t_c\}]|P_c(Z) = p_c, P_I(Z) = p_I) \tag{34}$$

and

$$g(p_c, p_I) = E(Y\mathbf{1}[T \in \{t_I\}]|P_c(Z) = p_c, P_I(Z) = p_I) \tag{35}$$

Next, I can rewrite, in discrete terms, the identification result in Equation (23) as:

$$\begin{aligned}\frac{\Delta f(p_c, p_I)}{\Delta p_c \Delta p_I} + \frac{\Delta g(p_c, p_I)}{\Delta p_c \Delta p_I} = \\ [f(p_c^2, p_I^2) - f(p_c^1, p_I^2)] - [f(p_c^2, p_I^1) - f(p_c^1, p_I^1)] + \\ [g(p_c^2, p_I^2) - g(p_c^1, p_I^2)] - [g(p_c^2, p_I^1) - g(p_c^1, p_I^1)] = E(Y(t_I) - Y(t_c)|u^c = p_c, u^I = p_I)\end{aligned} \tag{36}$$

Now, let us go over each term in (36). 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 E8 shades the area in the u^c, u^I square that identifies these individuals. The next panels in Figure E8 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 E9).

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.[36] 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 data-set

[35] Equivalent to {HL}, {HH}, {LH}, and {LL} in Section 4.

[36] Judicial districts with online data: Armenia, Barranquilla, Bogota, Bucaramanga, Buga, Cali, Ibague, Florencia, Manizales, Medellin, Neiva, Palmira, Pasto, Pereira, Popayan, Tunja, and Villavicencio.

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 immates 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.

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%).[37] 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 becomes 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

[37] Five 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	0.00118
	[0.00150]	[0.00157]
Income score	0.00118***	0.000837***
	[0.0000822]	[0.0000879]
Male D.	-0.0400***	-0.0209***
	[0.00279]	[0.00290]
Head HH D.	0.00877**	0.00771**
	[0.00370]	[0.00389]
Single	-0.0298***	-0.0213***
	[0.00222]	[0.00239]
Years edu. HHH	0.0004	0.000919
	[0.00150]	[0.00157]
D: Studying	0.0264***	-0.00653
	[0.00490]	[0.00486]
D: Working	0.0177***	0.0154***
	[0.00209]	[0.00226]
Yob	-0.00708***	-0.00312***
	[0.0000877]	[0.0000956]
Constant	14.55***	6.55E+00
	[0.173]	[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 with sentence data.

Table C3: Sample selection

Dep var: Missing Education records.	(1)	(2)	(3)	(4)
Parental incarceration	0.00506	0.0101	-0.084	0.0154
	[0.00599]	[0.00636]	[0.0581]	[0.0679]
	OLS	OLS	IV	IV
Obs	58,873	31,172	58,872	31,152
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: Monte Carlo Simulation

The model is adapted from the standard IV framework, which consists of four main random variables: $T, Z, Y, \mathbf{V}$. The variables are defined as follows:

- T_i denotes the assigned treatment of individual i, and takes values in $supp(T) = \{t_f, t_c, t_I\}$. 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.
- $\mathbf{V}_i$ stands for the random vector of unobserved characteristics of individual i. We assume $\mathbf{V}$ is two dimensional, and specifically it equals to $(U_c,\, U_I)$

U_c, U_I are the source of selection bias in this model: it causes both the treatment T and outcome Y. U_C is distributed Beta [2,2] and $U_I = aU_C +$ $\boldsymbol{N}$ [0,1], where $a > 0$.

In this notation, a counterfactual outcome is defined by fixing T to a value $t \in supp(T)$ in the outcome equation as follows:

$$Y = Y(F) = T_F + \beta_I U_I + \beta_c U_c + \epsilon_y$$

$$Y = Y(C) = T_C + \beta_I U_I + \beta_c U_c + \epsilon_y$$

$$Y = Y(I) = T_I + \beta_I U_I + \beta_c U_c + \epsilon_y$$

For the simulation exercise I set $\beta_C = 2.1$ and $\beta_I = 2.3$, $T_F = 0.4$, $T_c = 0.8$ and $T_I = 1.4$. The object of interest here is $\Delta_{IC} = T_I - T_C = 0.6$.

Treatment is assigned as follows: Parents are randomly assigned to one of a 100 judges who are characterized by two thresholds: Z_c which is drawn from a uniform [0.6,1], and Z_I drawn from a uniform distribution [0,1]. Once this random assignment occurs, treatment follows this rule:

$$T = \begin{cases} T_F & \text{if } \mathbf{1}[U_C > Z_c] \\ T_C & \text{if } \mathbf{1}[U_C \leq Z_C] \cdot \mathbf{1}[U_I > Z_I] \\ T_I & \text{if } \mathbf{1}[U_C \leq Z_C] \cdot \mathbf{1}[U_I \leq Z_I] \end{cases} \tag{37}$$

With this setting I run a baseline regression for a sample of 50,000 observation which can be found in Table D1.[38] The model replicates the bias in the OLS where the coefficient for incarceration is negative (-3.46). This bias disappears if we could observe the confounders of this model U_c and U_I (column 2). In the absence of the censoring from the conviction stage, the IV estimate (0.68) is very close to the true parameter (0.6). The next columns refer to the censored data, where we only observe cases with convictions; 88% of the data. This level of censoring is similar to the one I face in my empirical application. Columns 4 and 5 replicates the results from the full sample exercise: i) a very large bias in the OLS and ii) an unbiased estimate of incarceration when U_C and U_I are observed. More importantly, column 6 estimates the IV without any correction and column 7 shows my proposed strategy. For this simulation my proposed strategy yields an estimate (0.61) that is much closer to the true parameter than the IV approach without any correction (0.55). Furthermore this difference is systematic and the bias from my estimate approaches zero as the sample size increases, but this is not true for the IV estimate without correction as is clear from Figure D1. The bias also converges to zero for the split sample approach.[39]

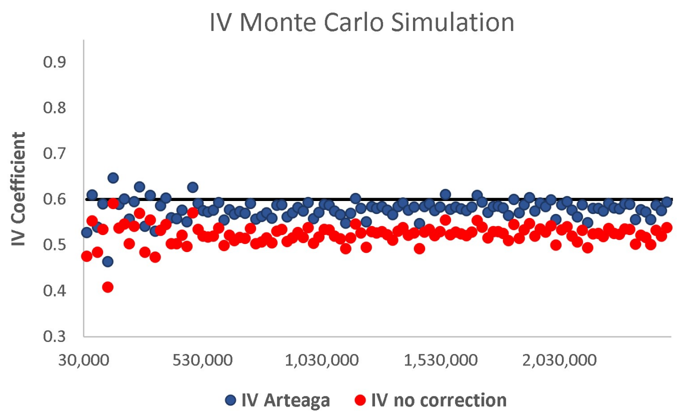
[38] I use set seed 2038947.

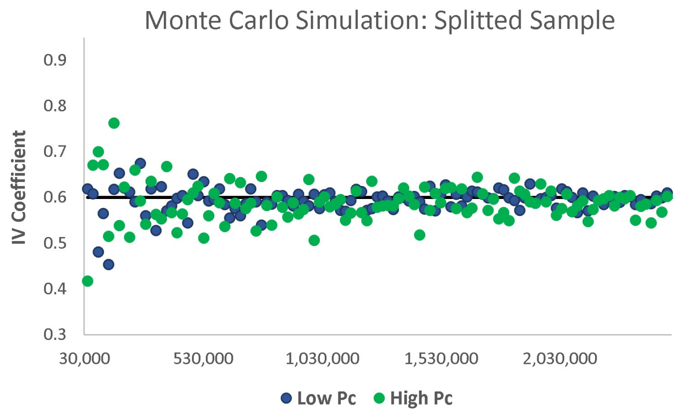
[39] In this model where the unobservables U_C and U_I are positively correlated the uncorrected IV has a bias downward. In the case where U_C and U_I are modelled to have a negative correlation the bias from the simulation is positive.

Table D1: Simulated OLS and IV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	Full	Full	Full	Censored	Censored	Censored	Censored
Model	OLS	OLS+Unobs	IV	OLS	OLS+Unobs	IV	IV Arteaga
Incarceration	-3.460***	0.626***	0.684***	-3.223***	0.618***	0.554***	0.610***
	[0.0262]	[0.0116]	[0.0750]	[0.0260]	[0.0118]	[0.0672]	[0.0674]
UI		2.300***			2.297***		
		[0.00412]			[0.00442]		
Uc		1.850***			2.138***		
		[0.0190]			[0.0216]		
Zc							2.879***
							[0.103]
Obs	75,000	75,000	75,000	66,478	66,478	66,478	66,478

Figure D1: Monte Carlo Simulation results





E Appendix: Extra tables and figures

Table E1: First stage

Dep var: Decision Dummy	(1)	(2)	(3)	(4)
	Conviction	Conviction	Incarceration	Incarceration
Judge Stringency	0.690***	0.689***	0.688***	0.687***
	[0.0627]	[0.0622]	[0.0485]	[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 the crime, occupation, and year of survey. Standard errors clustered at the randomization unit level. Sources: Attorney General's Office, criminal records and poverty census. * p<0.10, ** p<0.05, *** p<0.01.

Table E2: Controlling for Judge Stringency in Sentence Length

Dep var: Years of education	(1)	(2)
Parental Incarceration	0.782**	0.760**
	[0.365]	[0.364]
Judge Sentence length FE		0.000683
		[0.000906]
Obs	43,908	43,785
F stat	96.68	81.85
Effective F stat	86.05	87.96

Column 1 corresponds to the baseline regression. Column 2 adds as control judge stringency in sentence length.* p<0.10, ** p<0.05, *** p<0.01.

Table E3: 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 E4: 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 (2020) test for Monotonicty. 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 E5: Sentencing guidelines

Sentencing guidelines Crime	Prison time 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 E6: IV by P_c level group

Instrumental Variables	(1)	(2)
Dep var: Years of education	Low Pc	High Pc
Parental incarceration	0.845**	0.802
	[0.399]	[0.508]
Montiel-Pflueger Effective F stat	79.53	45.24
Obs	21,925.00	21,989.00
Clusters: Rand. Units	508	492

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

Table E7: Alternative IV specifications

Dep var: Years of education	(1)	(2)	(3)	(4)	(5)	(6)
Parental incarceration	0.782**	0.782**	0.782**	0.629*	0.816*	0.654*
	[0.365]	[0.362]	[0.365]	[0.378]	[0.478]	[0.346]
Model	Baseline	Cluster: Judge level	Cluster: Rand. Unit	Total cases>25	Total cases>50	No controls
Obs	43,908	43,914	43,908	38,255	25,813	43,908
Clusters: Rand. Units	604	764	604	451	218	604

Column 1:Two-way clustered standard errors clustered at the randomization unit level and household level. Columns 2 clusters at the judge level. Column 3 clusters (one-way) at the randomization unit level. Column 4 includes only judges that saw over 25 cases a year. Column 5 includes only judges that saw over 50 cases a year. Coulmn 6 exludes Sisben covariates. * p<0.10, ** p<0.05, *** p<0.01.

Table E8: Raw Judge Stringency Instrument

	(1)	(2)	(3)
Dep var: Years of education	**First Stage**	**Reduced form**	**IV**
Parental incarceration		0.317**	0.852**
		[0.144]	[0.386]
Judge Stringency raw	0.370***		
	[0.0411]		
F stat	81.44		
Obs	43,908	43,908	43,908
Clusters: Rand. Units	604	604	604
R squared	0.365	0.363	

This exercise uses raw judge means on conviction and incarceration as instruments. Specifically, different from the baseline estimation I do not residualize randomization level fixed effects.* p<0.10, ** p<0.05, *** p<0.01.

Table E9: Parental recidivism

Dep var: Recidivism	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Criminal record recidivism				Prison recidivism			
Incarceration	-0.0499***	-0.0112***	0.0579	0.0457	-0.0139***	-0.00421**	0.000491	-0.0173
	[0.00425]	[0.00256]	[0.0723]	[0.0529]	[0.00303]	[0.00214]	[0.0360]	[0.0272]
Model	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Time frame	Any	Post 3 years	Any	Post 3 years	Any	Post 3 years	Any	Post 3 years
Obs	69,511	69,511	69,496	69,496	69,496	69,496	69,496	69,496
Clusters	580	580	580	580	580	580	580	580

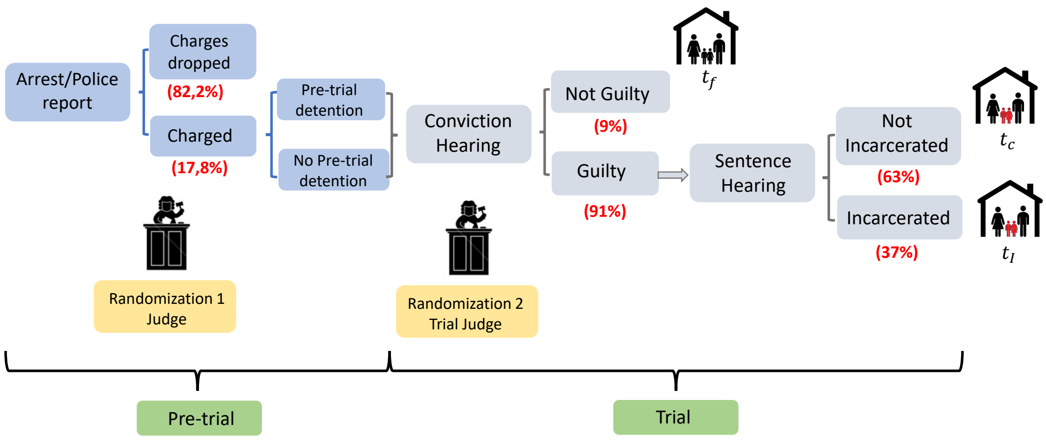
Controls: randomization unit fixed effects, Years of education, gender, age, Pc and Pc squared and offense incarceration rate. SE in brakets, clustered at the randomization unit level. * p<0.10, ** p<0.05, *** p<0.01.

Table E10: Changes after incarceration

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var: Incarceration	LFP	Income score	Education head of HH	Male head of HH	People in the HH	Three Gen.HH
OLS PANEL difference	0.0684***	-2.366***	0.0914***	-0.0788***	-0.0969***	0.0214*
	[0.0187]	[0.193]	[0.0300]	[0.00604]	[0.0303]	[0.0111]
IV 2010 Data	0.336	-3.745	0.427	-0.177	-0.553	0.0525
	[0.466]	[7.770]	[0.938]	[0.180]	[1.083]	[0.201]
Mean Dep. Var	0.399	26.41	5.099	0.595	4.658	0.215

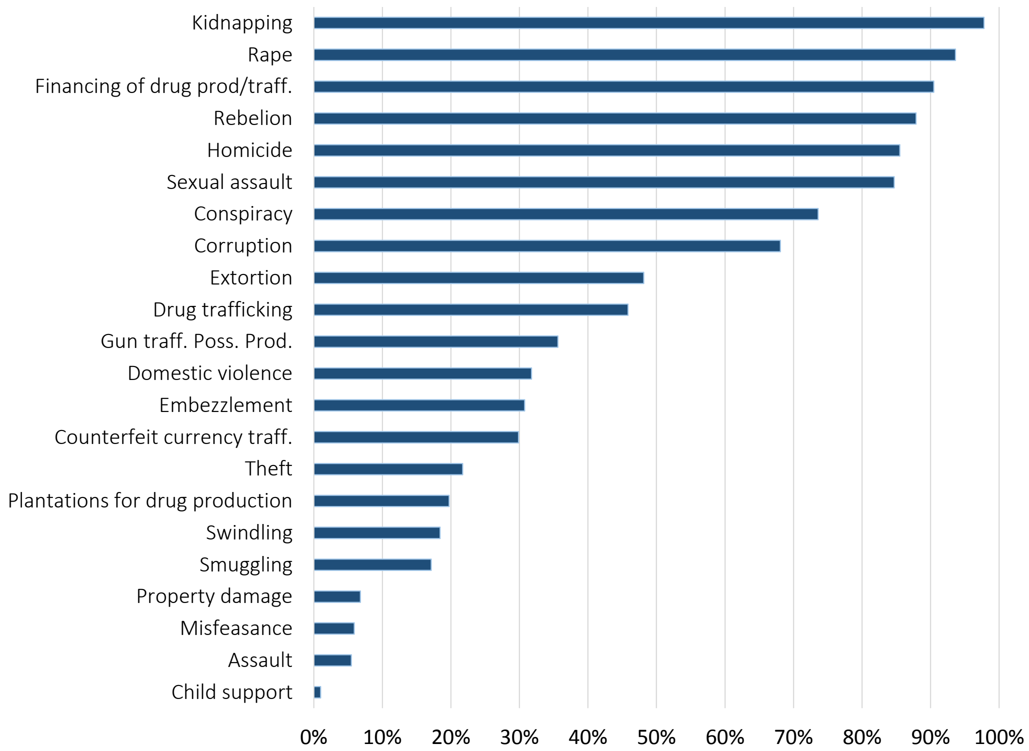
HH stands for household. Panel difference regressions corresponds to first difference regressions for those who appear in both SISBEN waves and who experience family member incarceration in between waves. IV 2010 Data corresponds to an instrumental variable regression where the dependent variable is observed in the last wave of SISBEN.* p<0.10, ** p<0.05, *** p<0.01.

Figure E1: 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 E2: Incarceration rates



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

Figure E3: Identification

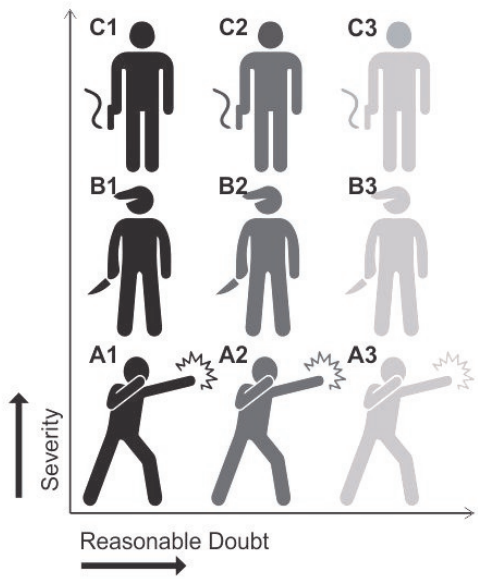
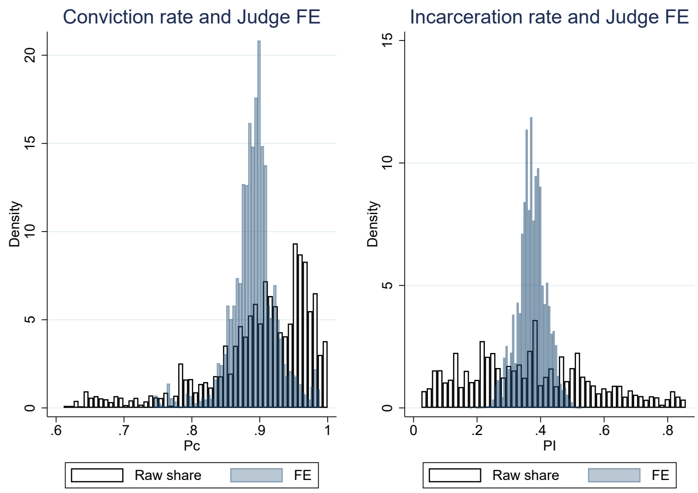
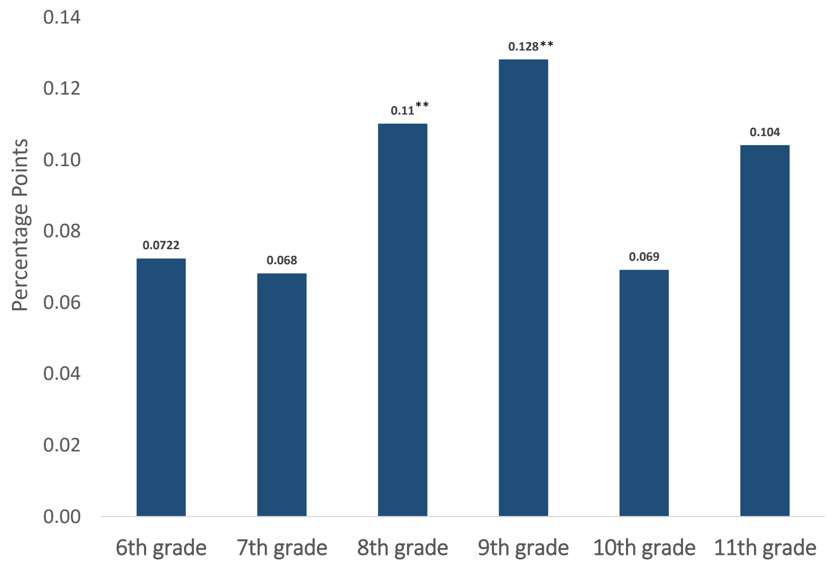


Figure E4: Judges' fixed effects



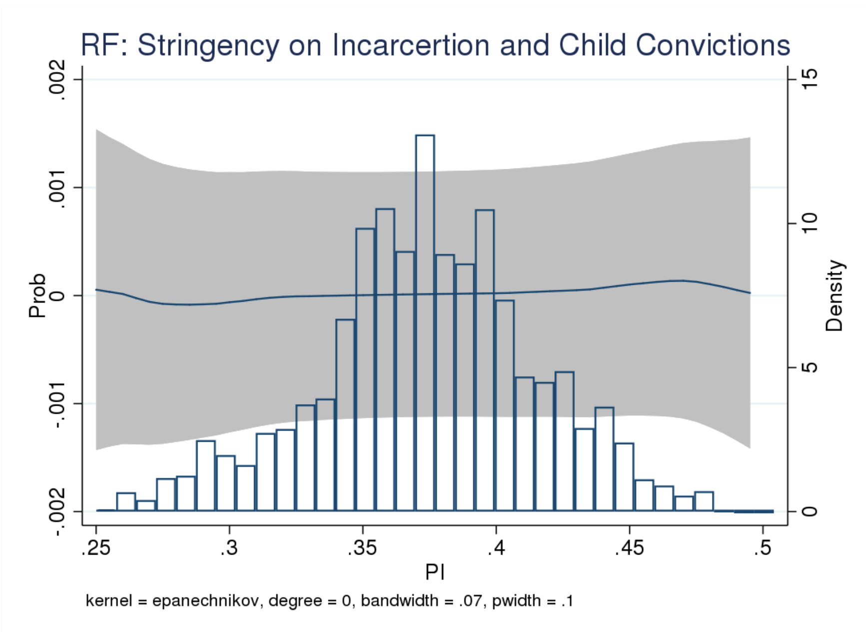
Source: Attorney General's office and criminal records. Raw rates are conviction/incarceration averages-by-judge. To construct the judge's fixed effect I take the residuals after regressing conviction/incarceration on (demeaned) randomization unit/year dummies, (demeaned) crime-level conviction/incarceration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

Figure E5: Treatment effects by grade



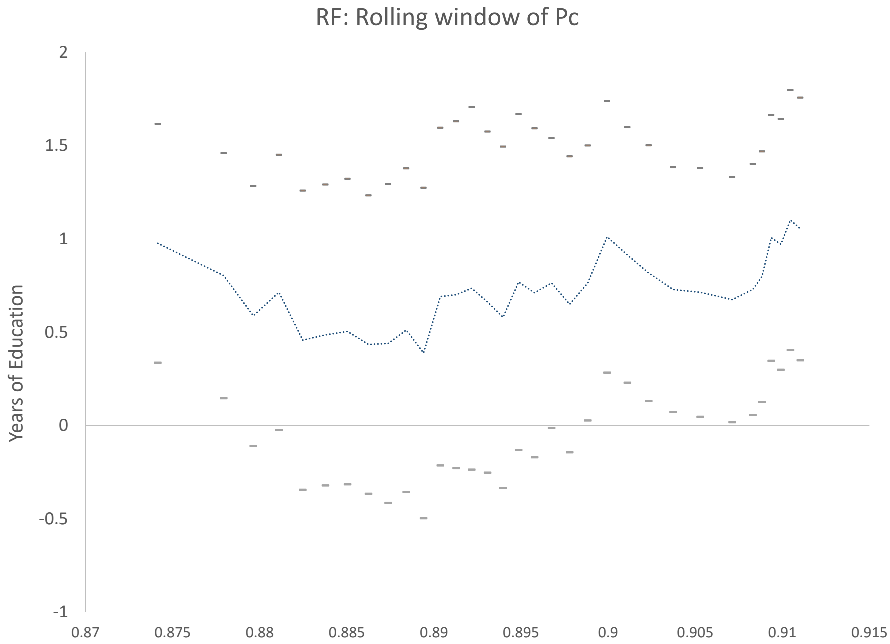
Notes: Two-way clustered standard errors clustered at the randomization unit level and household level. Controls: randomization unit fixed effects, offense incarceration rate, Pc and Pc squared, year of birth, gender and survey year.

Figure E6: 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 E7: 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 E8: Identification in two dimensions

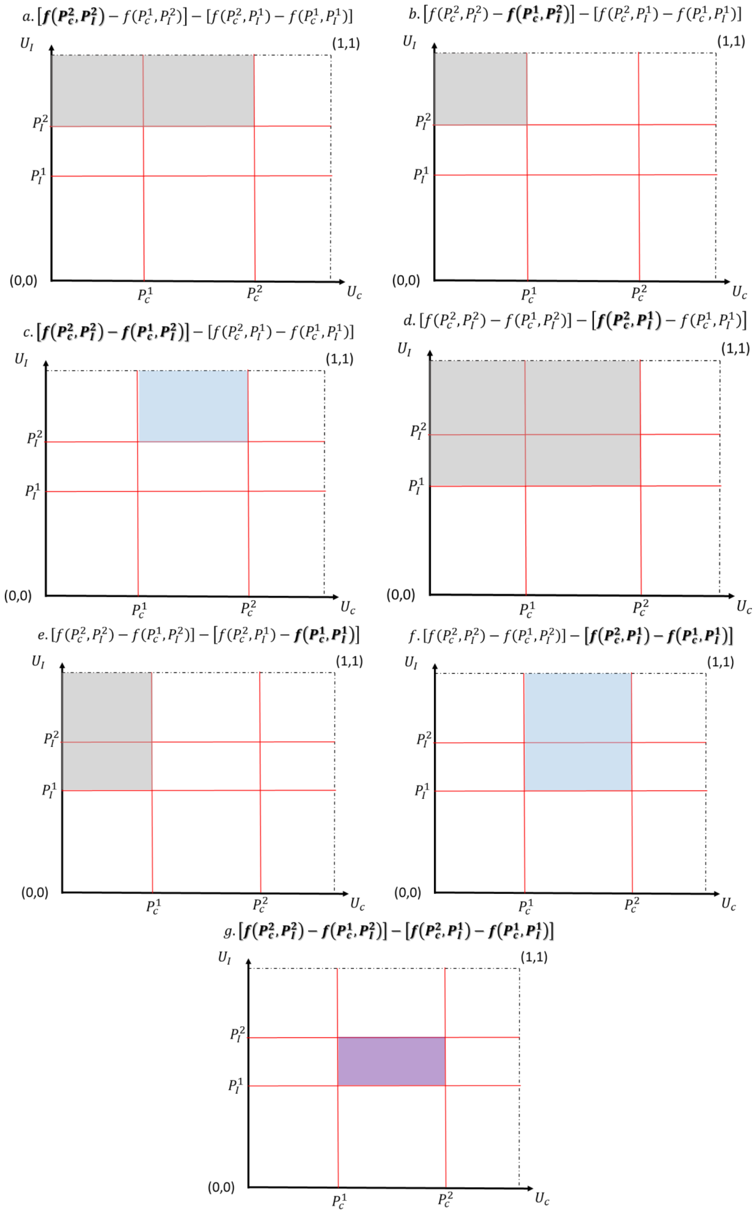


Figure E9: Compliers rectangle