Measuring the Intergenerational Effects of Incarceration

Manudeep Bhuller
University of Oslo

Gordon B. Dahl
UC San Diego

Katrine V. Løken
Norwegian School of Economics

Magne Mogstad
University of Chicago

May 30, 2021

Abstract: Many children have parents who serve prison time. Various theories suggest either positive or negative intergenerational effect of incarceration, making this an empirical question. A large correlational literature generally finds negative criminal, behavioral, academic, and health effects for the child. These results are unlikely to capture causal effects due to correlated unobservables. An emerging literature using panel data and quasi-experimental methods finds mixed results, with some evidence that parental incarceration is actually beneficial for a child. Additional rigorous and compelling causal evidence is required to fully measure the intergenerational effects of incarceration.

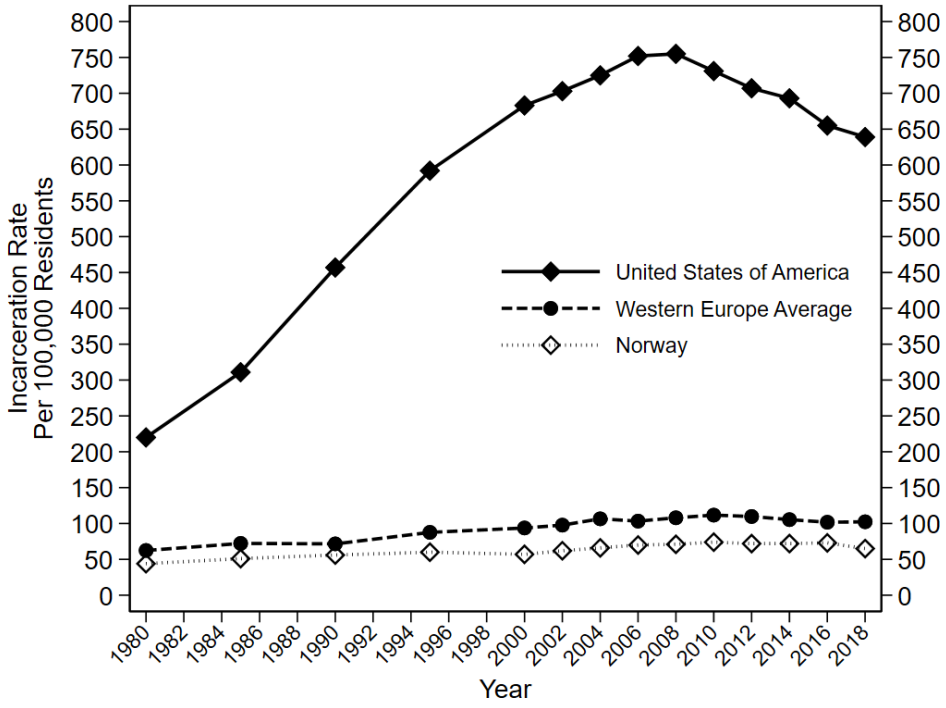
Keywords: Incarceration, Crime, Intergenerational Spillovers, Parental Influence

1. Introduction

A dramatic trend over time has been the increase in the number of incarcerated individuals (see Figure 1). Across European countries, for example, incarceration rates have risen by almost 65% between 1980 and 2018 (from 62 per 100,000 residents to 102). Yet nowhere have incarcerations reached such a high level as in the US, where rates rose from 200 per 100,000 in 1980, to a high of roughly 750 in the mid-2000s, before declining to 639 in 2018.

As Figure 2 shows, the current US incarceration rate is substantially larger than of any other nation. The graph plots incarceration rates in a country versus GDP for 160 countries with more than half a million residents. The next closed countries to the US rate are Rwanda, El Salvador, Turkmenistan, Thailand, Cuba, and Russia, but even these incarceration rates pale in comparison to the US.

Figure 1. Incarceration Rates in the US, Western Europe and Norway, 1980-2018.

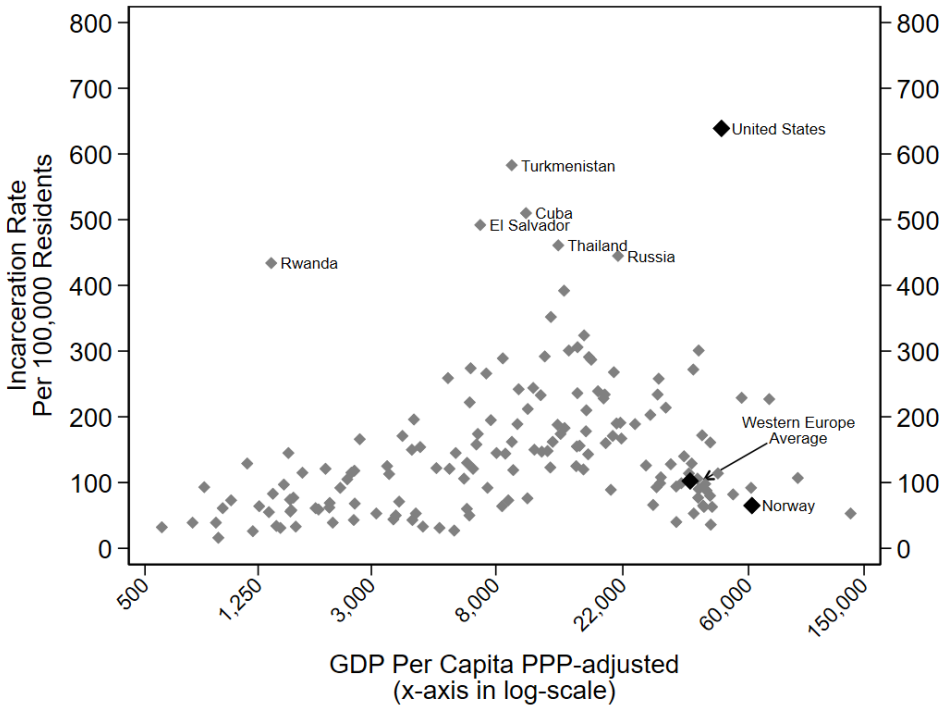


Notes: The Western European countries used to construct the population-weighted average include Austria, Belgium, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland and the UK.
Source: World Prison Brief, Institute for Criminal Policy Research.

The steep trends and troubling levels of incarceration in both the US and the EU have led many to argue for prison reform. For instance, the UN Declaration on Crime Prevention and Criminal Justice explicitly recognizes the importance of prison reform (UNODC 2016). The American Civil Liberties Union has listed prison reform as a priority area, and actively advocates for lower incarceration rates, improvements in prison conditions, and more focus on rehabilitation and treatment programs and inmates' mental health (ACLU 2021). Others have argued that prison reform is essential to achieve the UN's 2030 Agenda for Sustainable

Development on peaceful and inclusive societies (PRI 2017). While recognizing an urgent need for prison reform in the US, Rosen and Rayart (2015) and Lehrer (2013) discuss possible challenges in implementing these reforms. Others have pointed to prison reforms in California, which led to sharp and persistent reductions in the state's incarceration rate, with only modest impacts on property crime and no impacts on violent crime (Lofstrom and Raphael 2016).

Figure 2. Incarceration Rates and GDP per Capita.



Notes: Sample consists of 160 countries with population greater than 0.5 million and with available data on incarceration and GDP. Incarceration rates and GDP are for the latest available year. GDP per capita is adjusted for purchasing power parity (PPP) and reported in 2010 US dollars. The Western European countries used to construct a population-weighted average are Austria, Belgium, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland and the UK. Sources: World Prison Brief, Institute for Criminal Policy Research; World Economic Outlook, International Monetary Fund; World Bank.

Most of the policy discussion has focused on the direct, negative effects on incarcerated individuals themselves. But individuals do not live in a vacuum, and any effects of prison reforms could cascade to others in their network of contacts and acquaintances. In particular, children could be strongly impacted by having a parent serve time in prison. Focusing on the US context, Wakefield and Wildeman (2018) present a number of policy recommendations to limit the collateral consequences of parental incarceration on their children. For instance, they

advocate that the presence of children and families should be accounted for in the criminal justice process and that alternatives to incarceration should be explored for primary caregivers.

Estimates are that roughly 2.7 million children in the US have a parent behind bars in any given year. Similarly, in the EU roughly 1 million children have a parent in prison. To emphasize the magnitude of these numbers, they mean that approximately 1 in 28 children in the US and 1 in 50 children in the EU have a parent behind bars at some point during a year (Glaze and Maruschak 2010, Philbrick, Ayre, and Lynn 2014). Given these statistics, it is imperative to know not only the effect of incarceration on the individual, but also their offspring.

In this chapter we first lay out the reasons why there might be negative or positive intergenerational spillovers arising from parent incarcerations. We then outline how to model these spillovers econometrically and discuss the difficulty of estimating causal effects with observational data. The main challenge is that families with a parent who is sent to prison are likely to be different from other families in ways which are not observable to the researcher. Another challenge encountered by researchers is the lack of large scale, linked intergenerational datasets. We then turn to a review of the correlational literature, which documents associations in criminal behavior and other outcomes for the child including behavior, academic achievement, and health. Finally, we review recent advances in the estimation of causal intergenerational effects. As a part of this discussion, we detail an example from our own work of how a credible research design can be used to estimate intergenerational spillover effects. A key takeaway from our chapter is that correlational studies can be misleading, and that we are only beginning to understand the causal effects of parental incarceration on children.

2. Theories of Intergenerational Incarceration Spillovers

The criminology literature provides a range of theoretical perspectives on how parental incarceration may impact child outcomes. The main theories in this literature focus on (i) the loss of parental socialization through role modeling, support, and supervision, (ii) the strains of economic deprivation, and (iii) the stigma and shame of societal labeling. Hagan and Dinovitzer (1999) refer to these as *socialization*, *strain*, and *stigma* perspectives, respectively. In the following, we briefly outline the key implications from these perspectives.

Families are a key unit in society, with parents being a potentially critical presence in their offspring's life. Parents spend large amounts of time with their children while they are still living at home, especially at younger ages, providing more cumulative exposure than almost any other single influence in most cases. The socialization perspective emphasizes that parents provide supervising and support in the childhood socialization process, besides serving as role models for their offspring, with children looking up to their parents as examples. A prominent example of this perspective is the control theory of crime and deviance, which maintains that parents can often steer their children in prosocial directions (Hirschi 1969).

The socialization perspective hypothesizes that parental incarceration can weaken a child's human and social capital. A father's absence while he is locked up (or less often, a mother) could create a vacuum for children in terms of a role model and parental guidance, leading to adverse impacts. Some of these impacts could be mitigated (or further enhanced) by increased reliance on the remaining parent and other (bad) social peers (McLanahan and Bumpass 1988). The literature further hypothesizes that intergenerational influences depend on the gender combination of parents and their children (for literature reviews, see Bush and Peterson 2013; McHale, Updegraff, and Whiteman 2013).

The absence of a parent could cause a child to grow up in a single parent household with lower family income. The impacts of economic deprivation on children are emphasized by the strain theories of crime and deviance (Merton 1938; Cloward and Ohlin 1960; Messner and

Rosenfeld 1993). Growing up in poverty could lead to a myriad of downstream challenges such as unsafe housing, food insecurity, and underperforming schools. Single parenthood could also increase stress in the household, creating a less nurturing home environment (McLanahan and Sandefur 1994). In some cases, incarceration could lead to a child not having a permanent home environment, but instead lead to a child being shuttled between a series of foster care or extended family situations.

The stigma perspective highlights the effects of societal labeling that incarceration attaches to individuals and the groups to which they belong. Such stigma effects are also hypothesized to deplete the social capital of children with incarcerated parents (Hagan and Palloni 1990; Hagan 1991). Stigma effects might also persist after a parent is released from prison. If incarceration leads to discrimination in hiring, then family income and stability could be harmed in the longer run (Agan and Starr 2018; Doleac and Hansen 2020). Moreover, incarceration could lead to future crime, and potentially even create a parent who is a more hardened criminal. These factors all argue that a parent's incarceration could harm a child.

Contrary to most of the theories above, incarceration could also have positive effects on a child. For example, if a father is abusive, then incarceration could remove a disruptive influence in a child's life. It could also facilitate a clean break from an abusive relationship in the longer run. In both of these cases, incarceration would improve the home environment. This view can be supported by an alternative strain theory, which underlines the importance of prior relationship between an offender and offspring that predates incarceration (Simon 1993). Another channel for positive spillover effects could occur if prison time is rehabilitative, instead of criminogenic (Bhuller, Dahl, Løken and Mogstad 2020). For example, if serving time in prison results in specific deterrence (where the parent does not want to repeat the prison experience), then a parent might commit less crime upon release. If prison offers the opportunity to learn new skills via job training or education programs, then this could result in

positive labor market effects. In the rehabilitative case, prison could help to create a more positive home environment in the longer run. Finally, a child could be deterred from committing crime after observing their parent's experience.

Hagan and Dinovitzer (1999) further emphasize that incarcerated parents and their children might be different from non-incarcerated parents and their children, even prior to the sentencing. Intergenerational associations could thus be the result of predispositions – genetic or social – that are shared by parents and children, without necessarily being a consequence or a causal effect of parental incarceration. They refer to this as the *selection* perspective. If the intergenerational associations in crime result mainly from common social factors between children and parents, then the removal of a troublesome parent could also counteract such factors.[1] Other theories posit opposite-signed recoil effects due to children's desire to form their own identity or to rebel against parental expectations, thus mitigating the role of shared factors.

This discussion makes clear that the intergenerational effects of incarceration are a priori ambiguous. The effect will depend both on what the child learns from his parent's experience, whether the parent is a positive or negative influence on family life, and whether prison is rehabilitative or criminogenic. We return to this theme at the end of this chapter, when we discuss why intergenerational effects might be different in countries with more punitive versus rehabilitative prison systems.

3. Modeling Intergenerational Spillovers

Spillover effects, and the related idea of peer effects, both capture the idea that the experiences of one individual can directly impact another. In our setting, a parent's incarceration influences their child's decisions and well-being. While the idea of family spillovers is a natural one, it is less clear how large, or even what the sign of these effects are.

[1] More broadly, Foster and Hagan (2015) discuss how state and school policies can interact with parental incarceration. Also see Elder (1998) for a life course perspective on parental incarceration.

This is because it is empirically difficult to isolate the effect of a parental incarceration from the other factors affecting a child. To help fix ideas, and to explain how most researchers think about spillover and peer effects, it is helpful to write down a simple regression model.

Consider the case where there are just two members in the family, a parent and a child. We are interested in how a parent's incarceration affects some type of child outcome. Labeling the parent with the superscript *p*, and the child with the superscript *c*, the parental incarceration I_f^p and child outcomes y_f^c can be written as the following set of equations:

$$(1) \qquad y_f^c = \alpha^c + \beta^c I_f^p + \gamma_1^c x_f^c + \gamma_2^c x_f^p + \gamma_3^c w_f + \epsilon_f^c$$

$$(2) \qquad I_f^p = \alpha^p + \beta^p y_f^c + \gamma_1^p x_f^c + \gamma_2^p x_f^p + \gamma_3^p w_f + \epsilon_f^p$$

where x_f^c and x_f^c are the observable characteristics of the child and parent in family *f*, w_f are family level characteristics, and the error terms $(\epsilon_f^c, \epsilon_f^p)$ contains unobservable factors.

This simple two equation model is written in a very general form. It allows a child's outcomes to depend on their own and family-level characteristics, as well as their parent's characteristics and whether their parent has been incarcerated. It likewise allows a parent's incarceration to depend on their own and family-level characteristics, as well as the outcomes and characteristics of the child.

The main coefficient of interest is β^c, which captures the effect of parental incarceration on the relevant outcome of their child. There are many possible child outcomes which could be affected by parental incarceration. For example, a child's own criminal activity could be influenced by having a parent serve time. A child's performance in school or probability of engaging in risky behavior, such as the use of drugs, could likewise be affected. The system of equations as written above allows these outcomes to influence whether a parent is incarcerated, although this need not be the case in a more restrictive model.

The system of equations captures the idea that a child's choices, behavior, and success are not determined in isolation, but can depend on their parent. Specifically, the model allows

for the child's outcomes to depend on two types of parental spillovers: the characteristics of her parent and her parent's incarceration status. The first spillover is often referred to in the literature as a contextual effect, while the second is sometimes referred to as the peer effect if the outcome is similar (e.g., the child's own criminal activity or incarceration status). This chapter focuses on critically evaluating which types of statistical approaches can identify and estimate the second type of spillovers.

4. Challenges in Identifying Spillover Effects

The identification and estimation of spillover effects, as modeled in the prior section, is challenging due to three problems emphasized by Manski (1993). The three issues are reflection, endogenous group membership, and correlated unobservables. The reflection problem arises when both parents and children affect each other's decision. Reflection is not a concern if parents' influence children, but not the other way around. This is likely to be the case, although it is possible that a child could introduce a parent to a criminal network, or have behavioral problems which cause a parent to be less involved at home and engage in activities which lead to incarceration.

The second issue of endogenous group membership is also unlikely to be a major concern, as children do not choose their parents. Whether a parent chooses to be an integral part of the family, however, is likely to be endogenous.

In contrast, the third issue of correlated unobservables is likely to create large biases. The idea is that there are omitted variables which the researcher does not observe, but which play a role in a child's outcomes. For example, suppose family income is not observed, but that lower family income impacts both whether a parent becomes incarcerated and whether a child chooses to engage in criminal activity. The resulting omitted variable bias will make it

appear that parental incarceration matters, when in fact, the effect could simply be due to the omitted factor of family income.

This discussion emphasizes the difficulties in using observational data to make inferences about the effects of parental incarceration on their children. In the absence of some type of quasi-experimental variation, it is difficult to eliminate the bias from correlated omitted variables, as it is rare that all relevant factors are observed.

5. Correlational Evidence

The idea that children's criminal activity could be influenced by their parent's criminal activity is an old one. Early criminologists pointed out that parents who had been arrested or incarcerated had children who were also much more likely to become criminals as well (Dugdale 1877, Goddard 1912). These earliest studies were based on observations by criminologists of the correlation in criminal activity across generations.

Throughout the 20th century, these observed patterns were further developed using larger and more targeted datasets, including both administrative datasets and self-reported surveys. Researchers also expanded the correlational results for a broader set of outcomes affected by parental crime and incarceration, including anti-social behavior more generally and performance in school. We review this literature briefly in this section.

While correlational analysis does not necessarily provide causal evidence, it provides a useful starting point because it tells us what types of hypotheses one might like to explore with more rigorous causal analyses. Moreover, in many settings the best evidence available is from correlational studies, and researchers often attempt to account for confounding risk factors associated with parental crime or incarceration and child outcomes.

5.1 Studies of Intergenerational Associations in Crime

A large body of literature in criminology, and more recently some studies by economists, have documented substantial intergenerational associations in crime. Extensive surveys of this literature already exist, including a recent review by Wildeman (2020) and a meta-analysis by Besemer et al. (2017). In the following, we discuss some prominent examples of this work. Our objective is to provide a brief overview of the different stages that this literature has been through, and not to present a complete survey of this large literature.

The modern criminology literature considering intergenerational associations in crime dates back to Glueck and Glueck (1950), who compared background characteristics of 500 delinquent white boys and an equal number of nondelinquent white boys raised in poor communities in Boston, Massachusetts. Another early work from this period is Fergusson (1952), who did a similar comparative analysis of boys growing up in Glasgow, Scotland. These early studies triggered a subsequent literature that documented associations between parental background and child crime. In an important and extensive study of this kind, Sampson and Laub (1993) reexamined Glueck and Glueck's (1950) original data and their eighteen-year follow-up of the Boston boys. In Chapter 4 of their book, Sampson and Laub (1993) provided evidence from a multivariate analysis, where they correlated child delinquency to a series of family background variables, including indicators for arrest and conviction of the mother or the father. For instance, they found significant associations between parental crime and indicators of the parent's presence (e.g., mother's supervision) and style of parenting (e.g., harshness).

Later studies have taken advantage of similar and more conventional data collections to document the associations between arrests of children, parents and other relatives. A well-cited study is Farrington et al. (2001), who used data from the Pittsburgh Youth Study (PYS), where parents of 1,395 boys aged 8, 11 or 14 were interviewed and asked to report incidences of arrests across three generations of relatives, including fathers, mothers, sons, daughters, uncles, aunts, grandfathers and grandmothers. The authors reported evidence that offenders were highly

concentrated in families, with a father's arrest being a strong predictor for a son's arrest or delinquency, also after controlling for the arrests of other relatives. Another study is Farrington et al. (2006), who relied on the Cambridge Study in Delinquent Development (CSDD), and again found that having a convicted parent was a strong predictor of various negative outcomes. Using the CSDD data from England, the Transfive data from the Netherlands and the Rochester Intergenerational Study (RIGS) data from the US, van de Weijer, Augustyn and Besemer (2017) likewise documented intergenerational correlations in crime for all three countries.

Other recent studies by criminologists have made attempts to estimate more specifically the association between parental criminal justice contact (e.g., incarceration) and child crime, accounting for various risk factors that tend to be correlated with parental and child crime. For instance, using the National Longitudinal Study of Adolescent Health (Add Health), Roettger and Swisher (2011) found a strong association of child delinquency and paternal incarceration after controlling for a series of background variables. Re-examining data from the PYS, Murray, Loeber and Pardini (2012) found that parental arrest and conviction without incarceration did not predict youth problem behavior. However, they did find an association between parental incarceration and child's theft probability, as compared to a control group matched on propensity scores. Similarly, controlling for parental convictions and other childhood risk factors, Besemer et al. (2011) found an association between parental imprisonment and child offenses in England, but not in the Netherlands.

In other recent studies, criminologists have made attempts to link particular samples or complete birth cohorts to administrative crime records covering multiple generations rather than relying solely on survey data. For instance, Bijleveld and Mijkman (2009) linked Dutch administrative data to 198 high-risk adolescent males placed in a special school between 1911 and 1914, including crime records for their parents and offspring, spanning a total of five generations with a total 6,322 individuals. Similarly, Junger et al. (2013) linked a complete

cohort of the families in which a child was born in a Dutch city to arrest records for mothers, fathers, grandparents and siblings. These studies also find a strong concentration of criminal behavior within families.

An early attempt by economists to study intergenerational associations in crime is Case and Katz (1991), who used the 1989 Boston Inner City Youth Survey to assess whether parents passed on specific behaviors to their children. Two such behavioral traits were (i) having a family member who ever served time in jail and (ii) having one or more family members with drug or alcohol problems, which they found correlated strongly with child crime and child illegal drug use, respectively. Following this line of research, Duncan et al. (2005) used data from the National Longitudinal Survey of Youth (NLSY), and documented intergenerational associations along a series of specific parental traits and behaviors, including having been ever convicted.

Economists have more recently started using comprehensive administrative data sources to study intergenerational associations in crime. For instance, Hjalmarsson and Lindquist (2012) used data from the Stockholm Birth Cohort Study, comprising a cohort of around 15,000 individuals born in 1953, and linked this to administrative crime records for both individuals in this cohort and their parents. They found that children with criminal fathers had more than twice the probability of being convicted of a crime themselves, but that much of the effect could be accounted for by a parent's education and other behaviors. In another study, Hjalmarsson and Lindquist (2013) compared intergenerational associations in crime across children and their adoptive and biological parents. Building on these comparisons, they were able to assess the relative importance of pre-birth and post-birth factors, finding both to be important at the extensive margin of crime and post-birth factors to dominate at the intensive margin.

To summarize, much of the existing correlational literature thus confirms the early observations made by criminologists that children's criminal activity tends to be correlated to

their parent's criminal activity or criminal justice contact. These correlations seem to hold across studies from different countries, contexts and time periods, and across evidence based on retrospective surveys and intergenerationally-linked administrative crime records. Despite these empirical regularities, there remain concerns related to correlated unobservables and other problems discussed in Section 4, which limit the interpretation of such evidence as causal.

5.2 Studies of Associations Between Parental Crime and Child Outcomes

A number of social scientists as well as public health researchers have been interested in the broader question of how parental arrests, convictions or incarcerations associate with other outcomes of children, including their health, education and other indicators of social behavior.[2] A recent review of the US literature that is focused on the consequences of parental incarceration for child health and well-being is Wildeman, Goldman and Turney (2018), who examined 62 studies published between 2000 and 2017, which according to the authors applied extensive covariate adjustments and other rigorous designs. Earlier, Murray, Farrington and Sekol (2012) performed a meta-analysis of 40 studies that looked at associations between parental incarceration and child mental health, drug use, educational performance or measures of anti-social behaviors.

Wildeman, Goldman and Turney (2018) summarized the correlational literature as indicating that paternal incarceration is negatively associated with a range of child health and well-being indicators, however, the evidence on an association between maternal incarceration and child health was more mixed (e.g., Trice and Brewster, 2004; Wildeman and Turney, 2014). Murray, Farrington and Sekol (2012) summarized that among the 40 studies they surveyed, the most rigorous studies showed that parental incarceration was associated with higher risk for children's antisocial behavior, but not for mental health problems, drug use, or poor educational performance. They also pointed out that the methodological quality of many of the studies they

[2] Pattilo, Weiman and Western (2004) and Travis, Western and Redburn (2014) provide broad overviews of this topic. Hagan and Dinovitzer (1999) and Foster and Hagan (2009, 2015) discuss related theoretical perspectives.

surveyed was poor. Besides these two comprehensive reviews, Johnson and Easterling (2012) summarized and compared ten studies from the US documenting associations between parental incarceration and child well-being, highlighting a number of methodological challenges.

In the following, we briefly discuss some examples of studies that have provided evidence on associations between parental crime or incarceration and children's anti-social behavior, educational or health outcomes. A well-cited study of this kind is Murray and Farrington (2005), who used longitudinal data from the CSDD. Their data included eleven measures of anti-social personality and delinquency for 411 Inner London males measured at the ages of 14, 18, 32 and 40, as well as information on parental imprisonment. Comparing boys separated by parental imprisonment during their first 10 years of life to (i) boys who did not experience separation, (ii) boys separated by hospital or death, (iii) boys separated for other reasons (usually disharmony), and (iv) boys whose parents were only imprisoned before their birth, the authors found that parental imprisonment predicted all anti-social outcomes and concluded that this appeared to affect children beyond separation and associated risks.

In the US context, many researchers have documented that children of incarcerated parents have more behavioral problems, are less successful in school, and complete less education. For instance, Johnson (2009) provided associations between parental incarceration and a number of child outcomes, such as a behavioral problems index, whether the child was expelled or suspended from school, disruptive behavior problems in school, school absenteeism, grade repetition, etc. An interesting feature of this study is that it is based on the Panel Survey of Income Dynamics (PSID), which is a close to a representative sample of the US population. This differs from much of the previous research on intergenerational associations that was based on particular samples. For the various child behavior outcomes, the author relied on PSID's Child Development Supplement (CDS), and also provided results by the age of child when a parent was incarcerated. More recently, Haskins (2014) used the Fragile

Families and Child Wellbeing Study (FFS) to study associations between paternal incarceration and child's cognitive and non-cognitive school readiness scores at age five. The FFS is a longitudinal dataset that follows 4,898 children and their parents who were living in 20 large US cities between 1998 and 2000. An interesting feature of Haskins' study is that she used propensity score matching (PSM) to account for observed differences across children with incarcerated and non-incarcerated fathers.

There also some examples of studies from other countries which have gone beyond standard regression methods to assess associations between parental incarceration and child outcomes. For instance, Rud et al. (2014) applied matching techniques and found that having criminally involved parents is associated with a higher probability of finishing primary education as the highest education level attained and a lower probability of attaining higher education using the Netherlands' Kinship Panel Study (NKPS). Andersen (2016) linked Danish crime registers to children born in 1991 to study how educational outcomes and criminality up to age 20 associated with not only the frequency, but also the total duration of parental incarceration. Studying the same cohort of Danish children, Wildeman et al. (2014) applied discrete-time survival analysis to estimate the association of paternal and maternal incarceration with child mortality, while controlling for parental socio-demographic characteristics. These studies again found that child outcomes were adversely related to parental incarceration.

6. Causal Evidence Using Panel Data

Most researchers now recognize that correlational methods, such as ordinary least squares (OLS), are useful descriptive tools for documenting associations but not for identifying causal effects. This is because of the problems identified in Section 4 of reflection, endogenous group membership, and especially correlated unobservables. Only recently have researchers begun to use more advanced statistical techniques to isolate the causal link between generations.

This section discusses the progress made in estimating the causal intergenerational effects of incarceration using panel data. The next section will discuss an alternative approach – the use of random judge designs.

Observational studies often attempt to control for as many confounding factors as possible, including characteristics of parents, children, and the family environment. But it is unlikely that all confounding factors are included in any dataset. Recently, researchers have taken advantage of panel data to address this challenge. Importantly, panel data methods allow researchers to account for unobserved factors that differ across parents yet remain unchanged over time. This allows estimating intergenerational causal effects under milder assumptions than in observational studies. Studies using panel data approaches are listed in Table 1.

One way to take advantage of panel data is to use a fixed-effect regression. The idea is to compare child outcomes before and after the incarceration of a parent or before and after an exogenous event. This approach differences out any factors which are time invariant. Using the same notation as equation 1, the pre-post difference in a child's outcomes can be written as:

$$\Delta y_f^c = \beta^c \Delta I_f^p + \gamma_1^c \Delta x_f^c + \gamma_2^c \Delta x_f^p + \gamma_3^c \Delta w_f + \Delta \epsilon_f^c \quad (3)$$

To the extent that the characteristics of the child (x_f^c), the parent (x_f^p), and the family (w_f) do not vary over time, the equation simplifies to:

$$\Delta y_f^c = \beta^c \Delta I_f^p + \Delta \epsilon_f^c \quad (4)$$

The fixed effect approach has the advantage of eliminating bias from unobserved confounding factors which do not change over time. A limitation of this approach is that it cannot get rid of bias from unobserved confounding factors which do change over time. For example, if a parent loses their job, this may both worsen the home environment as well as lead to parental crime and incarceration. To the extent that these time varying factors can be controlled for, this should mitigate such concerns. Another limitation is that standard panel data methods are usually best suited to analyze the effects of exogenous or singular events

rather than a sequence of correlated events (Baltagi and Griffin 1984).[3] In practice, this often limits the set of questions that can be answered using these methods.

Billings (2018) uses this fixed effect approach in a paper studying the effect of parental incarceration versus an arrest which does not lead to incarceration. His setting is North Carolina, where he has data all incarcerations of individuals older that age 16 in local jails and all incarcerations in North Carolina's prison system. He matches this data with a rich set of public-school variables for each child (e.g., absences, test scores, school behavioral problems, suspensions, and grade repetition). Given the nature of his data, his paper focuses on children who reside with their parents.

The study includes individual fixed effects to get rid of any time invariant factors, such as the child's home environment, which might create omitted variable bias. On top of this, the study also includes information on the type of crime, prior arrest history, and whether the crime occurred at home or involved the child as a victim. These additional factors are included as controls because they could influence whether a parent will be sent to prison or jail after an arrest.

Billings (2018) finds positive effects on children for several outcomes after a parent is incarcerated. A student's behavior at school improves by 0.07 standard deviations and their test scores rise by 0.02 standard deviations. While the paper cannot estimate the effects of arrests causally, they appear to have the opposite effect of negatively impacting a child. In terms of other outcomes, there is limited evidence that parental incarceration affects grade retention, high school dropout rates, or criminal behavior as an adult.

[3] For instance, when events can occur multiple times and can be mutually correlated (as is often the case with criminal activity), then it becomes challenging to isolate the short-run and long-run effects even using panel data methods. Baltagi and Griffin (1984) consider two polar cases: i) if recurring events are independent over time, then one can easily recover the short-run effects associated with each event, and ii) if an event can occur only once and the researcher has a long enough observation window, then a long-run effect can be recovered.

These findings are notable because the effects are opposite most of the correlational literature. In interpreting the effects, it is important to recognize the paper estimates short-term effects. Hence, the interpretation is that removing a negative influence from the family environment (via incarceration), helps children in the short run. The paper does not answer the question of what happens once the parent is released from prison.

Wildeman and Anderson (2017) take advantage of panel data using difference-in-differences design, which has a similar logic to a fixed effect design. They study the effects of a reform in Denmark which decreased the probability of incarceration for certain crimes. In lieu of incarceration, the reform increased the chance a parent would receive a sentence which did not involve jail time (i.e., a noncustodial sentence).

The paper compares children age 12-18 whose father was eligible for a noncustodial sentence due to the reform versus a control group of children whose fathers committed similar crimes but were not eligible in a four-year window surrounding the reform. Assuming the families only differ in terms of whether the father was eligible for a noncustodial sentence, this comparison should estimate a causal effect. They find that a male child's probability of committing a crime between the ages of 22-28 falls by roughly 15 percent if their father was eligible for a noncustodial sentence. No effect was found for girls.

Cho has two papers which leverage panel data. Her first paper (Cho 2009a) uses a fixed effects approach to study the effect of maternal incarceration on educational achievement. Using data from Chicago Public Schools, she finds no effect for either math or reading test scores. Her second paper (Cho 2009b) uses a difference-in-difference design combined with propensity score matching to create a control group. Using a similar data, she finds that maternal incarceration lowers the chances a child will repeat a grade.

A final study is Porter and King (2015). They also use a difference-in-difference design, but for the control group use children whose parents will be treated in the future. In

other words, they take advantage of the timing of incarceration. The idea is that the treatment group will be more comparable to the control group, since both families with experience a parental incarceration at some time. The assumption for this design is that when a parent is incarcerated is as good as random.

They use wave I of a survey of students interviewed in grades 7-12 in the US combined with a follow-up survey (wave IV) when these same individuals were ages 24-34. The treatment is whether a father is incarcerated prior to the wave I interview, but after the respondent is born. The comparison group is fathers who were never incarcerated before the wave I interview, but who would be incarcerated by wave IV.

The paper finds no evidence of intergenerational effects for a variety of crimes the authors classify as "instrumental": there is no effect on shoplifting, theft, burglary, robbery, or drug dealing. In contrast, the paper finds some evidence that "expressive" crime increases (e.g., violent, destructive, or aggressive behavior).

7 Causal Evidence Using Random Judge Designs

A second recent approach to identifying causal intergenerational effects has been the use of random judge designs. Since we have used this approach in our own work, we use it as a case study to illustrate the method. We then report the results from four other studies which have used this approach. A summary of studies using a random judge design can be found in Table 2.

7.1 Case Study from Norway

In Bhuller, Dahl, Løken, and Mogstad (2018, 2020) we utilize a random judge design to estimate the effect of incarceration on adults in general, as well as on their children, in Norway. We discuss some of the most relevant information from these two papers to illustrate the advantages of this approach. The logic of a random judge design is to exploit the random

assignment of criminal cases to judges who differ systematically in their propensity to send a criminal defendant to prison.

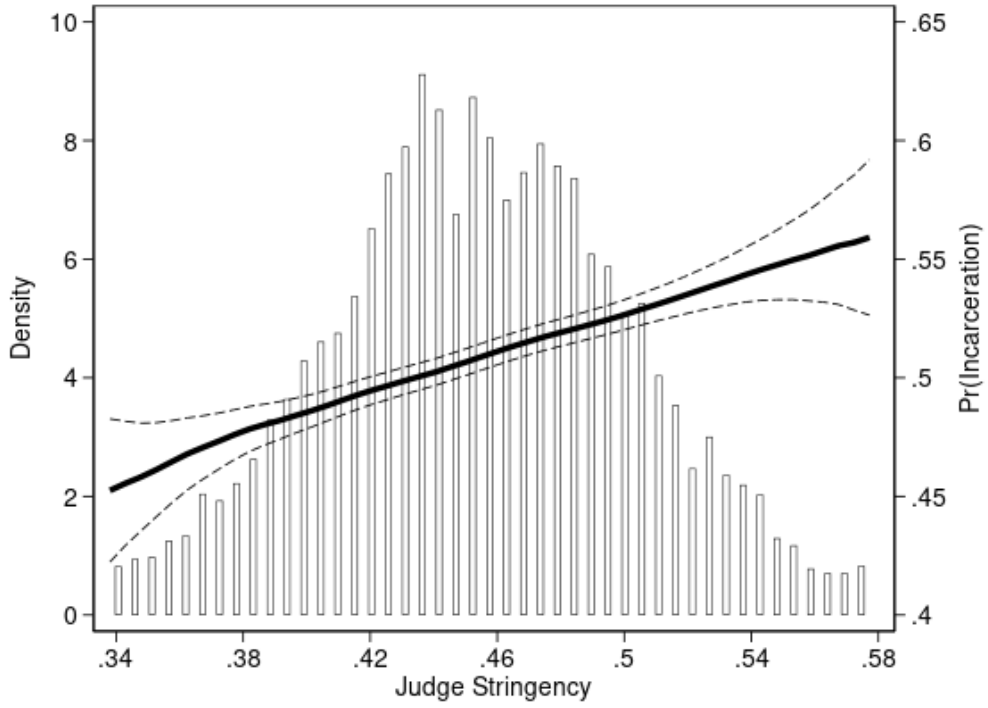
In Norway, the law specifies that most criminal cases be assigned to judges using the "principle of randomization." The stated goal is to treat all individuals accused of a crime equally, at least in an ex-ante sense. Especially severe crimes or juvenile cases are exempted from this requirement, and so cannot be studied with a random judge design. An advantage of the Norwegian setting is that there is no plea bargaining. This contrasts with the US, where defendant might decide to plead guilty in exchange for a lower sentence if they are assigned to a harsh judge.

A random judge design requires not only that judges are randomly assigned, but also that they have different incarceration tendencies. The alternative to incarceration is either probation, community service, a fine, or in rare instances, acquittal. This is important to keep in mind when interpreting results. We define a variable for the strictness of a judge using their incarceration rate in all of the other cases they have handled. In our 2018 paper, we have 597 judges, each of whom handled an average of 238 randomly assigned criminal cases. To create a measure of judge stringency, we take the leave-out mean judge incarceration rate and regress it on fully interacted court and year fixed effects. The residual is used as our measure of judge strictness. This residualization is necessary because randomization occurs within the pool of available judges in a given court and year.

We verify that judges are randomly assigned using the same types of tests that are used to verify random assignment in randomized controlled trials. Specifically, we regress our measure of judge stringency on a large set of variables which are highly predictive of whether an individual will be incarcerated (e.g., demographic characteristics of the defendant, crime type, work history, criminal background). We find no statistically significant relationship between these variables and our judge stringency measure. Few of the individual variables are

statistically significant (no more than would be expected by chance), and they are not jointly significant either (see Bhuller et al. 2018, Table 1). This provides strong support for conditional randomization.

Figure 3. First-Stage Graph of Incarceration on Judge Stringency.



Notes: This figure is taken from Bhuller et al. (2020), page 1290. The probability of incarceration is plotted on the right Y-axis against leave-out mean judge stringency of the assigned judge, shown along the X-axis. The plotted values are mean-standardized residuals from regressions on court by year interacted fixed effects, and a set of demographic and crime type control variables. The solid line shows a local linear regression of incarceration on judge stringency. Dashed lines show 90% confidence intervals. The histogram shows the density of judge stringency along the left Y-axis (top and bottom 2% excluded).

In Figure 3 we graph the histogram of the judge stringency measure, where the residualized values have been centered around the mean. It is clear that judges differ dramatically in their propensity to send a defendant to prison. A judge at the 90th percentile of our judge stringency measure incarcerates roughly 54% of cases compared to only 37% at the 10th percentile. This wide variation can be used as an exogenous shock to the probability a parent is incarcerated.

We use this variation in judge stringency as an instrumental variable (IV) in a two-stage least squares regression framework. Continuing with the notation used in equations 1 and 2, the first stage equation is:

$$(4) \qquad I_f^p = \alpha^p + \theta^p z_j^p + \gamma_1^p x_f^c + \gamma_2^p x_f^p + \gamma_3^p w_f + \epsilon_f^p$$

where z_j^p is the judge stringency instrument associated with the judge *j* assigned to a parent's case. Since the instrument is conditionally exogeneous to all child, parental, and family characteristics, it should not matter whether these covariates are included in the regression or not (one does need to include court-year fixed effects, which is the level of randomization).

The first stage is visually represented as the solid black line in Figure 3. As expected, the probability a defendant will be incarcerated is an increasing function of the assigned judge's incarceration stringency. The second stage equation is given by equation 1. This research design hinges on judge stringency being a valid IV for the assigned judge's incarceration decision, which requires the assumptions of relevance and exclusion. That judge stringency satisfies the relevance condition is readily confirmed by Figure 3. The exclusion condition requires that judge stringency can only affect child outcomes through its impact on the assigned judge's incarceration decision, which is harder to test empirically (see discussions in Bhuller et al. 2020, Section V.E). Under these assumptions, this design allows estimating short-run and long-run causal effects of parental incarceration on child outcomes.[4]

Turning to our empirical findings, we first report OLS estimates. We find that children of incarcerated fathers are 1 percentage point more likely to be charged with a crime themselves, relative to a mean of 13%. We find no significant effect on test scores and grades using OLS. These estimates are based on outcomes up to 10 years after a father's court date.

[4] With heterogeneous treatment effects, an additional monotonicity assumption is required, which implies that this IV approach identifies the effects of parental incarceration for children whose parents were sentenced to prison as a result of being assigned a strict judge relative to children whose parents were assigned to a lenient judge and thus did not receive a prison sentence. Imbens and Angrist (1994) refer to this group as compliers.

Hence, they capture both the short run effect while a father is in prison, and the long run effect after he is released. Turning to our IV estimates, we find no statistically significant effect of a father's incarceration on a child's future criminal activity or performance in school. Given the imprecision in our estimates, we are unfortunately unable to rule out modestly-sized effects.

7.2 Other Studies Using a Random Judge Design

Three other studies have used a random judge design to study the intergenerational effects of incarceration. The first is a contemporaneous paper by Dobbie et al. (2019), which uses high quality register data from Sweden, similar to the data we use from Norway. They find that parental incarceration causes an increase in teen crime, and decreases in educational attainment and adult employment. The effects are driven by children in disadvantaged families, with criminal convictions rising by 10 percentage points, high school graduation decreasing by 25 percentage points, and adult employment decreasing by 29 percentage points. There are no noticeable effects for children in advantaged families. They conclude that the incarceration of parents increases intergenerational poverty and leads to higher criminality of children, despite the extensive safety net in Sweden.

A second study is by Norris, Pecenco and Weaver (2020), which uses administrative data for 30 years from the state of Ohio. They match court records to other outcomes using name and date of birth. In contrast to most OLS studies, they find that parental incarceration has beneficial effects on children. They report that children of incarcerated fathers and mothers are less likely to be incarcerated themselves (a 4.9 percentage point drop) and that they live in better neighborhoods as adults. For school performance and teen parenthood, they find no general effects.

A third study is by Arteaga (2020) which studies evidence from Colombia. She links criminal records for 90,000 low-income parents who have been convicted of a crime,

combined with information on the educational attainment of their children. She extends the standard random judge design to include both the conviction and incarceration decisions of judges. The paper finds a 0.78 year increase in the years of education of children whose parents are incarcerated versus the counterfactual of conviction.

A final study is by Huttunen et al. (2020), which uses rich Finnish data on parental criminal punishments and child outcomes. They look at a broad set of outcomes, such as schooling, wages, and criminal activity. Using OLS, they find evidence that along many dimensions, children are negatively impacted by parental incarceration and positively impacted by fines. In sharp contrast, they find no statistically significant effects when using a random judge design, either from parental incarceration or fines, although the estimates are somewhat imprecise.

8. Conclusion

Given the high incarceration rates in many countries, a key policy question is what effect parental incarceration has on the next generation. Correlational studies reveal a strong intergenerational association between parent and child criminality. This correlational literature also documents either negative or no effects on a broad set of behavioral, educational, and health outcomes. Based on this observational literature, one might be tempted to conclude that incarceration is harmful for children. But as we argue above, these correlations are unlikely to capture causal effects.[5]

The literature using panel data or quasi experimental methods is much smaller and more recent. The evidence from these attempts to provide causal evidence is more mixed.

[5] In related work (Bhuller et al. 2020), we document how correlational evidence can lead to misleading conclusions. Using a random judge design, we find that when an individual is incarcerated, it strongly discourages subsequent criminal behavior. In contrast, OLS finds positive associations between incarceration and recidivism, even after controlling for a rich set of characteristics. In other words, bias due to selection on unobservables, if ignored, leads to the erroneous conclusion that time spent in prison is criminogenic rather than deterrent.

Some studies find positive effects on child outcomes, others negative, and still others no statistically significant effect due to a lack of precision.

One possible explanation for divergence across the various causal studies is that incarceration means different things in different countries. For example, prisons in many European countries emphasize rehabilitation and are relatively humane compared to the US. And while the average prison sentence in Europe is approximately 8 months, it is closer to 3 years in the US (Bhuller et al. 2020). These differences could lead to not only different incarceration experiences for a parent, but also to different spillover effects on their children. But we note that the differences across causal studies are not always easily explained by these factors.

Our conclusion is that there is still much to be learned about the intergenerational effects of incarceration, and that it is premature to draw firm conclusions. Given the importance of the question, more rigorous and compelling research using quasi-experimental designs needs to be used to improve our understanding. On a positive note, this is exactly the direction that recent research is heading.

Table 1. Studies estimating the causal effects of parental incarceration using panel data designs.

Article	*Publication*	*Design*	*Data and Setting*	*Treatment*	*Comparison*	*Child Outcomes*
Cho (2009a)	Journal of Human Resources	Difference-in-differences with child fixed effects	Illinois Department of Corrections, Cook County Jail, and Chicago Public Schools N=26,134	Mother imprisoned	Mother jailed for three days or less	Reading and math standardized test scores
Cho (2009b)	Journal of Urban Economics	Difference-in-differences with propensity score matching	Illinois Department of Corrections, Cook County Jail, and Chicago Public Schools N=42,488	Mother imprisoned	Mother jailed for one week or less	Grade retention
Porter and King (2015)	Journal of Research in Crime and Delinquency	Difference-in-differences, using timing of incarceration	NLSAH – US students interviewed in grades 7-12 in 1993/1994 (wave I) and retrospective interview at ages 24-34 (wave IV) N=12,172	Father incarcerated (jail or prison) prior to wave I interview, but after the respondent was born	Father never incarcerated prior to wave I interview, but will be incarcerated by wave IV	Self-reported criminal activity in the past year (wave I), by type of crime: - Violent, destructive, or aggressive behavior ("expressive" crime) - Shoplifting, theft, burglary, robbery, and selling drugs ("instrumental" crime)
Wildeman and Andersen (2017)	Criminology	Difference-in-differences, using a legal reform	Danish register data on children aged 12-18 with convicted fathers, two years before and after April/July 2000 N=3,398	Father eligible for community service; committed simple assault, DUI, traffic offense or misdemeanor	Father ineligible for noncustodial sentence; committed crimes that had otherwise similar severity	Ever charged with a crime by 22-28 years of age, i.e., 10 years after the father's conviction, by child gender
Billings (2018)	Journal of Labor Economics[†]	Panel-data regressions with child fixed effects	Correctional records for individuals aged 16+ in North Carolina Prisons and Mecklenburg County Jail N=120,629	Parent incarcerated	Parent arrested	Current year or within five years: - Test scores - Grade repetition - Behavioral index; days absent, days suspended and school crimes - Child mobility; moved residence or school

Notes: NLSAH—National Longitudinal Study of Adolescent Health (Add Health).

[†] Currently undergoing revision for the listed journal. The final publication year may differ from the year first publicly available working paper indicated here if the article is accepted for publication.

Table 2. Studies estimating the causal effects of parental incarceration using random judge designs.

Article	*Publication*	*Data and Setting*	*Treatment*	*Counterfactual*	*Parent Outcomes*	*Child Outcomes*
Bhuller, Dahl, Løken and Mogstad (2018)	AEA Papers and Proceedings	Norwegian register data linked to court records N=35,027	Father incarcerated	Father not incarcerated, i.e., received either probation, community service, fine or acquittal after trial	3 years post decision: - Ever charged - Ever employed	- Ever charged in the 10 years post decision - Scores on national tests in 5/8/9th grade and GPA in 10th grade
Dobbie, Grönqvist, Niknami, Palme and Priks (2019)	Working paper	Swedish register data linked to court records N=174,338	Parent incarcerated ‡	Parent not incarcerated, i.e., received either probation, fine, other sentence or acquittal after trial	6 years post decision: - Ever convicted - Employed - Earnings - Single adult household	- Teen conviction by type of crime - Teen parenthood - Teen/adult education outcomes: GPA percentile, enrollment at age 16, high school degree by age 25 - Adult labor market outcomes: employment and earnings at age 25
Norris, Pecenco and Weaver (2020)	American Economic Review†	Administrative data from Ohio matched to court records based on name and date of birth N=83,532	Parent incarcerated ‡	Parent not incarcerated, i.e., received either probation, fine or acquittal after trial	- New charges - Cumulative births - Eviction and mobility of non-defendant parent - Charges against the co-parent	- Ever charged, ever convicted and ever incarcerated by age 25 - Number of charges, convictions and incarcerations by age 25 - Academic performance: math, reading, GPA, absence, grade repetition - Teen parenthood
Arteaga (2020)	Review of Economics and Statistics†	Colombian court records linked to the SISBEN census N=91,317	Parent incarcerated	Parent convicted (faced probation)	- Labor force participation of non-incarcerated spouse - Income score - Education and gender of household head -Size of household	- Years of education - Heterogeneity by child gender and age, type of crime, paternal or maternal incarceration, long or short sentence
Huttunen, Kaila, Kosenen and Nix (2020)	Working paper	Finnish register data linked to court records N=43,908	a. Parent fined‡ b. Parent incarcerated‡	a. Parent faced probation or incarceration b. Parent faced probation or fined	- Partner separation - Charges against spouse - Spouse employment and earnings	- Cognitive test at age 5 - GPA at age 16 - Crime at ages 15-17 - Employed or schooling at age 19 - Degree at age 19

Notes: SISBEN—Colombian Census of Potential Beneficiaries of Welfare/Low-Income Population.

† Currently undergoing revision for the listed journal. The final publication year may differ from the year of first publicly available working paper indicated here if the article is accepted for publication.

‡ Dobbie et al. (2020) and Norris et al. (2020) also provide results by the gender of the parent. Huttunen et al. (2020) provide results by the age of child (early, middle or late) when the parent was sentenced.

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