INTERGENERATIONAL EFFECTS OF INCARCERATION

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ABSTRACT

An often overlooked population in discussions of prison reform is the children of inmates. How a child is affected depends both on what incarceration does to their parent and what they learn from their parent's experience. To overcome endogeneity concerns, we exploit the random assignment of judges who differ in their propensity to send defendants to prison. Using longitudinal data for Norway, we find that imprisonment has no effect on fathers' recidivism but reduces their employment by 20 percentage points. We find no evidence that paternal incarceration affects a child's criminal activity or school performance.

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There has been a dramatic rise in incarceration over the last 40 years in OECD countries, raising important questions about the accompanying societal consequences.[1] Arguments about the effects of incarceration on convicts' own recidivism and future employment are central to policy debates about the criminal justice system (e.g., Bhuller et al. 2016, Kling 2006, Mueller-Smith 2015). An often overlooked, but sizable, population which is also critical for policy is the children of these inmates. It is estimated that over 1 million minor children in EU countries and 2.7 million in the U.S. have a parent behind bars at some point during a year (Glaze and Maruschak 2010, Philbrick, Ayre, and Lynn 2014). Stated differently, roughly one in every 50 children in the EU and one in every 28 children in the U.S. has a parent in prison in a given year.

The experience of having a parent imprisoned could have important spillover effects on a child. Parental incarceration could create emotional trauma, increase stigma and social alienation, or impose financial hardship. However, it is also possible that imprisonment could serve as a salient deterrent to a child's own illegal behavior or temporarily remove a negative influence from a child's life. How children are affected will depend both on what prison does to their parent and what they learn from their parent's experience. If prison rehabilitates a parent so that they commit less crime and enter the labor market, this could have positive role model spillovers on the child and increase family income. On the other hand, if a parent's time behind bars is criminogenic, it could cause children to copy their parent's negative behavior or disrupt the stability of their home environment.

Despite the importance of the issue, data limitations and endogeneity concerns mean that we know little about the effects of incarceration on parents and the accompanying intergenerational spillovers. The first challenge is that the data requirements are high. One not only needs a panel dataset for parents' criminal behavior and labor market outcomes, but also the ability to link this to data for their children. The second challenge comes from omitted variable bias. Which parents are sent to prison is not random, and moreover, there

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[1]In Western Europe, the average incarceration rate per 100,000 residents rose from 62 in 1980 to 112 in 2010. The increase was even larger in the U.S., going from 220 to over 700 (World Prison Brief, 2016).

could be unobserved characteristics common to both parents and children. This short paper overcomes both the data and identification challenges in the context of Norway's criminal justice system. As far as we know, the only other quasi-experimental work on this topic is a contemporaneous working paper by Dobbie et al. (2017) using data from Sweden.

1 Research Design and Data

This section describes our research design and data, copying some of the most relevant information from Bhuller, Dahl, Løken, and Mogstad (2016, hereafter BDLM). While further details can be found there, here we highlight the richness of the data and how the random assignment of judges can be used to estimate the effects of incarceration on both a parent's own behavior, as well as that of their children.

1.1 Research Design. We deal with omitted variable bias by exploiting the random assignment of criminal cases to Norwegian judges who differ systematically in their stringency. Our measure of judge stringency is the average incarceration rate in all other cases a judge has handled. This variable serves as an instrument for parental incarceration since it is highly predictive of the judge's decision in the current case, but as we document, uncorrelated with a parent's observable case characteristics. This approach builds on our prior work (BDLM 2016), which uses a similar setting to estimate the causal effect of incarceration on inmates' future crime and employment, without regard to parental status.

The criminal justice system in Norway works as follows. If the police suspect an individual of a crime, they file a formal report. A public prosecutor then decides whether the individual should be charged with a crime as well as whether the case should proceed to a court trial. About half of police reports lead to a formal criminal charge. Of these charged cases, the public prosecutor advances approximately 40% of them to a trial. The other charged cases are either dismissed, directly assigned a fine, or sent to mediation by the public prosecutor.

Of the cases which proceed to trial, approximately 60% are non-confession cases, while the remaining are cases where the defendant has confessed to the charges filed by the public

prosecutor.[2] We focus on non-confession cases in this paper. Once a case proceeds to trial, it is assigned to a judge. If the judge finds the accused guilty, he or she can assign a combination of possible punishments which are not necessarily mutually exclusive. Slightly over half of cases result in incarceration, with probation, community service and fines combined accounting for 44% of outcomes. In a small fraction of cases (5%), the defendant is found not guilty.

The law in Norway dictates that cases are assigned to judges according to the principle of randomization. There are a few exceptions, such as for especially severe crimes or cases involving juveniles, which we exclude from our sample. To have a sample of randomly assigned cases for the same pool of judges, we limit our sample to regular judges handling non-confession cases. Regular judges are permanent civil servants (versus deputy judges who generally serve for a limited 3 year term).[3]

We measure the strictness of a judge based on their incarceration rate for all other cases they have handled, including both past and future confession and non-confession cases between 2005 and 2014, and not just those cases which appear in our estimation sample. There are 597 judges, each of whom have presided over an average of 238 randomly assigned court cases. To construct our judge stringency measure, we calculate the leave-out mean judge incarceration rate and regress it on fully interacted court and year fixed effects to account for the fact that randomization occurs within the pool of available judges. The residual from this regression is our measure of judge strictness.

Table 1 verifies that judges in both the entire sample, as well as the sample of fathers we will be focusing on, are randomly assigned to cases.[4] The first column regresses incarceration on a variety of variables measured before the court decision for the sample of male defendants with children. It reveals that demographic, type of crime, and past work and criminal history variables are highly predictive of incarceration, with most coefficients being individually

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[2]A defendant chooses whether to confess prior to knowing who their assigned judge will be. The absence of plea bargaining makes the interpretation of our IV estimates easier to interpret (see Dobbie et al. 2016).

[3]We further restrict the dataset to judges who handle at least 50 randomly assigned cases and to courts which have at least two regular judges in a given year. Our regression samples are limited to cases between 2005 and 2013 so that each defendant can be followed for three years.

[4]While it would be interesting to also study the effects for mothers, there are too few female inmates.

Table 1. Testing for Random Assignment of Criminal Cases to Judges.

	A. Male Defendants with Children			B. All Defendants		
	Incarcerated	Stringency	Mean	Incarcerated	Stringency	Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Age	-0.002***	0.000	41.36	0.005***	0.000	33.01
	(0.001)	(0.000)		(0.000)	(0.000)	
Female	-	-	-	-0.073***	-0.001**	0.112
				(0.006)	(0.001)	
Foreign born	-0.008	-0.003*	0.136	0.008*	0.001	0.129
	(0.013)	(0.001)		(0.005)	(0.001)	
Married, year t-1	-0.051***	0.000	0.106	-0.040***	-0.001	0.229
	(0.012)	(0.001)		(0.009)	(0.001)	
N children, year t-1	0.015***	-0.000	0.792	-0.005*	0.000	1.90
	(0.004)	(0.000)		(0.003)	(0.000)	
High school, t-1	-0.029***	0.001	0.205	-0.007	0.001	0.174
	(0.011)	(0.001)		(0.006)	(0.001)	
Some college, t-1	-0.052***	0.001	0.075	-0.063***	-0.000	0.048
	(0.017)	(0.002)		(0.009)	(0.001)	
Missing Xs	-0.703***	0.021	0.021	-0.553***	0.007	0.025
	(0.204)	(0.021)		(0.098)	(0.012)	
Violent crime	0.113***	0.000	0.230	0.085***	0.000	0.270
	(0.011)	(0.001)		(0.007)	(0.001)	
Property crime	-0.002	-0.001	0.123	-0.014	0.001	0.133
	(0.015)	(0.002)		(0.009)	(0.001)	
Economic crime	0.001	-0.000	0.160	-0.063***	0.001	0.108
	(0.013)	(0.002)		(0.009)	(0.001)	
Drug related	-0.033**	-0.001	0.113	-0.053***	-0.001	0.128
	(0.015)	(0.002)		(0.008)	(0.001)	
Drunk driving	0.057***	-0.003*	0.082	0.058***	-0.000	0.073
	(0.016)	(0.002)		(0.010)	(0.001)	
Other traffic	-0.079***	-0.003*	0.082	-0.056***	-0.000	0.075
	(0.017)	(0.002)		(0.010)	(0.001)	
Charged, t-1	0.098***	-0.001	0.418	0.094***	-0.000	0.464
	(0.009)	(0.001)		(0.005)	(0.001)	
Charged, t-2 to t-5	0.030***	0.000	0.615	0.091***	0.000	0.642
	(0.010)	(0.001)		(0.006)	(0.001)	
Employed, t-1	-0.026**	-0.001	0.401	-0.001	-0.000	0.353
	(0.010)	(0.001)		(0.006)	(0.001)	
Employed, t-2 to t-5	-0.033***	-0.000	0.581	0.007	-0.001	0.474
	(0.010)	(0.001)		(0.007)	(0.001)	
Dependent mean	0.565	0.460		0.524	0.461	
Joint F-statisic	38.89	1.005		116.69	1.016	
[p-value]	[0.001]	[0.450]		[0.001]	[0.437]	
N	20,191			59,506		

Notes: The outcome variable for columns 1 and 4 is a dummy for whether the defendant is incarcerated, and the outcome for columns 2 and 5 is the judge stringency measure described in the text. All regressions include controls for court x court entry year fixed effects. The omitted category for education is "Less than high school, year t-1" with a mean of 0.720 for male defendants with children and 0.778 for all defendants and the omitted category for type of crime is "Other crimes" with a mean of 0.212 for male defendants with children and 0.215 for all defendants. Standard errors are two-way clustered on judge ID and defendant ID. *p<0.10, **p<0.05, ***p<0.01.

significant. In column 4, we repeat this exercise for all defendants, regardless of whether they have children, and likewise find these variables significantly predict incarceration. There are a few differences in which variables are most predictive across the two samples; for example, past employment history matters more for male defendants with children.

In columns 2 and 5, we examine whether judge stringency can be predicted by this same set of variables, and find no statistically significant relationship. This is true for both the sample of all defendants, as well as the subsample of male defendants with children. The estimates are all close to zero, and the number of significant coefficients is not more than would reasonably be expected due to chance. The coefficients are also not jointly significant, providing strong evidence for conditional randomization.

1.2 Data. We use several administrative datasets which can be linked using individual identifiers. Information on all court cases between 2005 and 2014 comes from the Norwegian Courts Administration. We link this information with administrative data that contain complete records up to 2016 for all criminal charges, including the type of crime and date of a crime. We merge these datasets with administrative registers containing demographic information from Statistics Norway for every resident. A key advantage of the Norwegian registry data is that we can link children to their parents.

Columns 3 and 6 of Table 1 report descriptive statistics for individuals accused of a crime and brought to trial. It documents means for the sample of fathers, and contrasts it with the overall sample. Regardless of parental status, individuals accused of a crime are a disadvantaged group: they have little education, low earnings, and high unemployment. But the table also reveals that incarcerated fathers differ from the overall sample in several salient dimensions. First, while the average age of all defendants is 33, the father subsample is over 41 years old. Fathers are also slightly more educated, and commit relatively more property crimes and fewer violent crimes. Additionally, fathers are significantly more likely to be employed prior to incarceration. These differences will be important to keep in mind when comparing the effects of imprisonment for fathers versus inmates in general.

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2 The Effect of Incarceration on Fathers

Remarkably little is known about the causal effects of incarceration for convicts, let alone the consequences of incarceration for convicts who are fathers. So we begin our analysis by estimating the effects of prison on fathers, and compare these estimates to those for all inmates. This is an important first step, as how children are affected will depend on what prison does to their father and what they learn from his experience.

One factor which could negatively affect children is an increase in a father's criminal activity after release from prison. A child who observes their father getting into trouble with law enforcement could copy this role model. Continuing criminal behavior could also contribute to an unstable home environment. While an emerging literature explores the causal effects of incarceration on recidivism, there is little quasi-experimental evidence focusing specifically on parents.

Another factor which has been hypothesized to negatively impact children is a drop in income due to imprisonment. Johnson (2009) uses the Panel Study of Income Dynamics and its Child Development Supplement to document that family income drops when a parent is incarcerated, and that income does not fully rebound after release. While this is some of the first panel evidence on family income and incarceration, its observational nature means it could still suffer from omitted variable bias.[5]

To obtain causal estimates for an incarcerated father's recidivism and earnings, we use instrumental variables. In the first stage, we regress the probability a defendant is sent to prison on our judge stringency measure, along with a set of fully interacted court and year dummies and the controls listed in Table 1. As expected, the instrument is highly relevant. While not reported in a table, for fathers the estimated first stage coefficient on judge stringency is 0.498 (s.e. = 0.078). A similarly strong first stage is found for the entire sample of defendants.[6] The validity of this instrument is discussed in BDLM (2016).

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[5]Our prior work (BDLM, 2016) documents an Ashenfelter dip in earnings prior to incarceration, illustrating the limitations of an event study design for this question.

[6]The estimate for the entire sample is 0.434 (s.e. = 0.060). Note the first stage coefficient need not be one

Table 2 presents results for the direct effects of incarceration on fathers. In these regressions, we include the same set of control variables used in the first stage, including the full set of court times year dummies. Our first outcome is whether the father was ever charged with at least one crime within 36 months of his original court date; 58% of fathers in our defendant sample re-offend based on this measure. OLS estimates reveal a positive correlation between incarceration and future crime, with a 7 percentage point increase in the probability of being charged with another crime. In contrast, the IV estimate has the opposite sign, and is close to zero. While the standard error is large, there is no evidence of a recidivism effect.

Table 2. The Effects of Incarceration on the Defendant.

	A. Male Defendants with Children		B. All Defendants	
	Ever Charged	Ever Employed	Ever Charged	Ever Employed
OLS: Incarcerated	0.070***	-0.059***	0.074***	-0.053***
	(0.008)	(0.008)	(0.005)	(0.005)
RF: Judge Stringency	-0.016	-0.099*	-0.080**	-0.012
	(0.063)	(0.057)	(0.035)	(0.038)
IV: Incarcerated	-0.032	-0.202*	-0.198**	-0.027
	(0.130)	(0.118)	(0.092)	(0.092)
Dependent Mean	0.584	0.428	0.634	0.487
N	19,322	19,322	57,169	57,169

Notes: Regressions include all variables listed in Table 1 as controls, plus court x court entry year fixed effects. Standard errors are two-way clustered on judge ID and defendant ID. *p<0.10, **p<0.05, ***p<0.01.

We next turn to employment. Our outcome variable is whether the father is ever employed in the 36 months after his court date. Only 43 percent of defendant fathers find employment, highlighting their weak labor market attachment. The OLS estimates in Table 2 suggest a modest drop in employment of 6 percentage points. The IV results, on the other hand, indicate a far more dramatic employment effect. Incarceration reduces a father's employment probability by a statistically significant 20 percentage points. The contrasts between the OLS and IV results, both for this outcome and recidivism, are striking, and imply that the wrong conclusions would be reached with a correlational analysis.

To enable a comparison, we also present estimates for the sample of all defendants. While

since the sample of cases used to calculate the stringency measure differs from the estimation sample, and because there are covariates (e.g., a full set of court times year dummies).

the OLS estimates are similar to those for the father sample, the IV estimates diverge sharply. For all defendants, we find a strong reduction in recidivism, with re-offending being cut by almost one third. We also estimate a small, and statistically insignificant, effect on employment for all defendants. This is the opposite pattern of results compared to panel A, which showed results for fathers. This contrast is important, because it implies that results for all defendants, even if causal, do not carry over to the subsample of fathers. For example, policy interventions focused on helping families of inmates should arguably focus more on the employment margin compared to the re-offense margin based on our IV estimates.

The heterogeneous effects for fathers dovetails well with the results we found in our prior work (BDLM 2016). For all defendants, we found that imprisonment discourages further criminal behavior, with a reduction extending beyond incapacitation. We showed the estimated decline in crime was driven by individuals who were not working prior to incarceration. In contrast, for individuals working prior to incarceration, there was no reduction in future crime, but earnings dropped substantially both during imprisonment and post release. Our sample of fathers is more similar to individuals who were working prior to incarceration, including in prior employment status, gender, and age. Hence, it is not surprising the effects for the two subsamples are similar. Our results for parents are also broadly consistent with those found in the working paper by Dobbie et al. (2017), which finds that incarceration has little effect on recidivism for parents, but negatively impacts participation in the labor market and family structure.

3   The Effect of Paternal Incarceration on Children

We now turn to the effect of a father's incarceration on his children. In the U.S., researchers have documented that children of incarcerated parents have more behavioral problems, are less successful in school, and complete less education (e.g., Johnson 2009, Haskins 2014, and Rud et al. 2014). Looking at intergenerational correlations in crime, Hjalmarsson and Lindquist (2012) document that children with criminal fathers have more than twice the probability of being convicted of a crime themselves, but that much of the effect can be

accounted for by a parent's education and other behaviors. Weijer, Augustyn, and Besemer (2017) likewise document intergenerational correlations in crime for the Netherlands, England, and the U.S. Contemporaneous research by Dobbie et al. (2017) uses a similar identification strategy as we do, and finds large increases in teen crime and pregnancy, and a reduction in youth employment in Sweden.

We start our analysis of children by presenting OLS estimates in Table 3. These add to the limited set of correlational papers on intergenerational effects. To maximize the sample size, we follow children for as long as possible (up to 10 years after their father's court date). The results we show capture both a short run effect, while a father is still in prison, and a longer run effect, after a father is released; this is because there is not enough precision to separately estimate the two effects. We find that children whose father serves prison time are 1 percentage point more likely to themselves be charged with a crime, relative to a mean of 13%. We also report OLS estimates for school grades, and find no significant effect.

Table 3. The Effects of Incarceration on Male Defendants' Children.

	Ever Charged	School Grades
OLS: Incarcerated	0.010**	-0.228
	(0.005)	(0.205)
RF: Judge Stringency	-0.015	-0.983
	(0.042)	(1.527)
IV: Incarcerated	-0.035	-2.285
	(0.096)	(3.561)
Dependent Mean	0.132	44.71
N	35,027	19,546

Notes: Ever charged is a dummy for whether a child is charged with at least one crime in the ten year period (up to 2016) after the father's court date. School grades are points on national tests (mean 50, std. dev. 10) in 5th, 8th, and 9th grade and GPA points on a similar scale in 10th grade, and are averaged over all scores available in the ten year period (up to 2016) after the father's court date. Due to institutional constraints and data availability, we restrict the ever charged sample to children over age 5 as of the parent's court date (age 15 is the minimum age of criminal responsibility) and the school grades sample to children age 1–15 as of the parent's court date. Regressions include all variables listed in Table 1 as controls, plus court x court entry year fixed effects and child age at time of parent's court appearance. Standard errors are two-way clustered on judge ID and defendant ID. *p<0.10, **p<0.05, ***p<0.01.

Of course, these OLS results could simply reflect unobservables which are correlated across generations, rather than the experience of having a father imprisoned. This motivates our analysis which uses judge stringency as an instrumental variable. As shown in Table 3, we

find no statistical evidence that having a father incarcerated affects a child's own criminal activity or performance in school, although we are not able to rule out modestly-sized effects.[7] Many researchers have hypothesized that child gender could matter (e.g., Philbrick, Ayre, and Lynn 2014), so we also separately estimated the effects for sons versus daughters. While the OLS estimates go in the predicted direction (with sons being more negatively affected), the IV estimates are too imprecise to be informative.

4 Discussion

Little is known about the impact of incarceration on inmates who are parents, and even less is known about the impact on their children. Various theories predict children could be either harmed or helped by having a father imprisoned, but the evidence base is scarce. These results are some of the first causal evidence on the effect of prison for fathers and the intergenerational spillovers on their children.

We find that incarcerated fathers experience a large drop in employment but no change in the probability of re-offending. Understanding that fathers are differentially affected by prison time is useful for predicting and interpreting the intergenerational effects on children. We find no causal evidence for an intergenerational link in crime or for an effect on school grades. We are quick to point out that our IV estimates for children are imprecise enough that we cannot rule out modest spillover effects. Hence further research using quasi-experimental designs could help provide a better understanding of the collateral costs of parental incarceration on children. As more data become available which can follow children for a longer time period, it would be especially interesting to study longer-term outcomes like children's adult crime and employment.

It is important to recognize our findings may not extrapolate to other countries, where the experience of being in prison for a parent could be quite different. For example, prisons in Norway (as in many European countries) emphasize rehabilitation and are relatively humane

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[7]One possible reason our results diverge somewhat from Dobbie et al. (2017) is that both their estimates and our estimates are imprecisely estimated, and so the differences between the two studies are due to statistical noise.

compared to the U.S. And while prison sentences in Norway and the rest of Europe average around 8 months, they are closer to 3 years in the U.S. These divergent sentence lengths contribute to the fact that the U.S. is an outlier in incarceration rates, with a rate which is roughly 7 times the European average.[8] For these reasons, our findings are probably more applicable to Europe than the U.S.

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[8]Despite these differences, the observable characteristics of inmates and the distribution of crime types (with the exception of homicides), are fairly similar in Norway, Europe, and the U.S. (see BDLM 2016).

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