Shared Punishment? The Impact of Criminal Sentences of Parents on Child and Partner Outcomes*

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PRELIMINARY DRAFT - PLEASE DO NOT CITE OR CIRCULATE WITHOUT PERMISSION FROM THE AUTHORS

Abstract

Around the world, the parents of millions of children interact with the criminal justice system each year. What are the trade-offs of different punishments in terms of the potential spillovers on defendant's families? In this paper, we link rich data in Finland on parental criminal activity to child and partner outcomes. We use the data, along with randomized assignment to judges as an instrument, to provide causal evidence on the impact of enacting different punishments on the families of defendants. For children, we analyze the impact from birth through early adulthood on a variety of outcomes, including schooling, wages, and criminal activity of the children. We find that OLS estimates, including rich controls suggest negative impacts of prison on children and positive effects of fines on children. Causal estimates, on the other hand, suggest no statistically significant impact of either fines or incarceration on child outcomes, although the estimates are too imprecise to draw strong conclusions.

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Introduction

Millions of children around the world have parents who commit crimes each year. These parents then interact with the criminal justice system and face a wide range of possible punishments, from more lenient punishments like fines to harsher penalties such as prison. To provide some context on the extent of interactions children have with the criminal justice system, in the United States the Bureau of Justice reports that the number of children with a parent in prison has increased from just under 1 million in 1991 to just under 1.75 million in 2007 (Glaze and Maruschak, 2008). In 1999, 10% of all minor children in the United States had a parent in prison, jail, on probation, or on parole (Travis *et al.*, 2006). In the United Kingdom, the BBC reported in 2014 that an estimated 200,000 children had a parent in prison (Powell, 2014). In Finland, the context of these paper, we find that over 6% of children aged 0-14 have a parent who faces a court trial in their lifetimes.[1]

Yet despite the large number of children impacted by the criminal justice system each year, we know very little about the relative trade-offs of different punishments in terms of their impacts on defendant's families, with the bulk of the evidence focused on the impact of parental incarceration on child outcomes. Using unique data from Finland, in this paper we estimate the impact of two of the most frequently used punishments, fines and prison, on a broad set of child and partner outcomes.

A large literature demonstrates that parental time and material investments are pivotal for child development. A natural hypothesis based on this literature is that punishments levied on parents, which may cause reductions in some of these investments, harms children. On the other hand, it may be that parents who engage in criminal activity are on average more absent or particularly bad parents, and punishing them or removing them from the home either has no effect or may benefit children. Moreover, the longer term impact will surely depend on how different punishments of parents affect the parent's future criminal activity and labor market participation. The recent evidence on the direct impact of incarceration (for example) on prisoners is mixed and may be context specific, with Mueller-Smith (2014) finding negative effects in the U.S. and Bhuller

[1]This is for the main sample of analysis in this paper, which will be downward biased as it excludes traffic cases, young defendants, and a few others as described in 3.

et al. (2016) finding positive effects in Norway. Huttunen *et al.* (2019), which estimates impacts of fines and prison using the same data as in this paper, finds that there are distinctive trade-offs between fines and prison, with fines marginally increasing future criminal activity but having no effect on labor market outcomes while prison decreases future criminal activity but with negative impacts on labor market outcomes.

The lack of evidence on the effects of criminal justice system punishments of parents on children may be explained by three challenges. First, it is difficult to find rich data linking parents to children with comprehensive information on parental criminal activity and child outcomes. Second, even with such data, it is difficult to separately identify the causal effect of criminal justice punishments of parents from the effect of unobservable characteristics of parents and children that are correlated with both parental criminal activity and child outcomes. There are a number of reasons why OLS estimates might not give the causal impact of the punishment on the parent on child outcomes. Conditions that lead to increased criminal activity among parents might also be conditions that lead to worse child outcomes. For example, we find that parents who engage in crime are poorer, have larger families, and are more likely to be members of marginalized groups compared to parents who do not engage in crime. In addition, characteristics of parents that make them more likely to engage in crime may also make them worse parents. They may be more violent, younger, have lower cognitive ability, and have less impulse control.[2] In addition to potentially having parents with fewer resources, less capacity to take advantage of those resources, and worse characteristics, it might also be the case that children of criminals differ from children of non-criminals. If characteristics that cause crime and other negative outcomes have a genetic component, then these characteristics may be more prevalent among children of criminal parents. In the descriptive section we show that many of these issues are salient, and discuss how failing to address these issues may lead to significant bias in estimates.

Third, child development and its interaction with parental investments is complex. It is unlikely that punishing parents has the same impact across all ages of the child at the time of punish-

[2]We can't directly measure violent tendencies or impulse control, but we do find that on average criminal parents are younger, and we will also be able to look at the cognitive and noncognitive skills at age 18 [data acquisition in progress].

ment, particularly given the rich literature showing how investments impact child development differently depending on the age of the child. We also show that the sample of parents who commit crimes is very different at different child ages. Parents who commit crimes when their children are older are much more likely to be serial offenders, and the impact of the first punishment of the parent that the child experiences might be different than the impact of a second, third, or later prison sentence. Last, different punishments might have different impacts on the different dimensions of human capital, which in turn interact to differentially impact adult outcomes. For these reasons, while a simple local average treatment effect across all children whose parents experience fines, probation and/or incarceration on a small number of outcomes at a specific point in time is informative, it leaves a great deal left to be learned about the impact of parental interactions with the criminal justice system on children.

In this paper, we address these challenges. To address the first challenge we construct a unique panel data set for all children born in Finland between 1988-2016 and their parents. We link administrative tax, school, and crime data. We collected detailed data from every criminal court case in Finland from 2000-2016, and also collected data on annual health care visits of children aged 0-6 from 2000-2016.

To address the second challenge, we use the randomization of judge assignment to cases combined with the variation in sentencing by different judges as an instrument for punishments of parents. Using this instrument we causally estimate the effect of different punishments of parents on child outcomes, but only for the sample of parents on the margin of being sentenced to either fines or prison. This instrument was first developed in Kling (2006) and has also been used in Di Tella and Schargrodsky (2013), Green and Winik (2010), Aizer and Doyle (2015), Dobbie and Song (2015) and many others, although to our knowledge it has not been used to estimate the impact of fines on parent and/or child outcomes. We use these papers, and in particular Mueller-Smith (2014), Dobbie *et al.* (2018a), and Bhuller *et al.* (2016), as models to construct and evaluate our judge stringency measures.

To address the third challenge, we estimate the impact of punishments at three stages of childhood: early childhood (ages 0 to 4), middle childhood (ages 5 to 10) and late childhood (ages

11 to 14). We also occasionally include a fourth period, just before birth (ages -1 to 0). We analyze the impact of different punishments occurring across the full sample of children, as well as these subcategories, on a number of outcomes throughout childhood and into adulthood. In the short run, we look at the impact of different punishments of parents on child cognitive skills and health using comprehensive health center exams at ages 4 and 5 [health in progress]. In terms of medium run outcomes, we include GPA at age 16, cognitive and noncognitive skills at age 18 for boys[3] [in progress], and criminal activity in adolescence. Last, we look at completed schooling, labor market participation and income when the children are in their late teens and early twenties.

We find that while there are strong correlations between incarceration of parents and negative outcomes for children, the causal evidence is much less certain. The causal estimates are generally imprecise, and while we do not find significant impacts of parental incarceration on child outcomes, we cannot rule out the negative impacts from the OLS estimates. In terms of fines, OLS estimates indicate largely small but positive spillovers on children of parents being fined (as opposed to being sentenced to probation or prison). However, as with incarceration, the causal estimates using the judge instrument show no significant spillovers of fines on child outcomes, but are too imprecise to draw strong conclusions either way.

Our paper is most closely related to a very recent literature analyzing the causal impact of incarceration on children.[4] Dobbie *et al.* (2018b) identify the impact of incarceration of fathers when children are aged 11-14 on juvenile crime, teenage pregnancy, and school completion in Sweden. They find an increase in juvenile crime and teenage pregnancy, and reductions in youth employment. Bhuller *et al.* (2018) do a similar analysis in Norway and find no impact on children. Billings (2018) looks at the impact of arrests and incarceration of parents on their children in

[3]These test scores are collected by the military for almost all men in Finland.

[4]This literature builds on descriptive evidence from a variety of disciplines that on the impact of parental prison time on children. Geller *et al.* (2011) describe drops in financial contributions fathers make to children after prison (where father's contributions consist of child support for nonresident parents and 25% of earnings for resident parents). Wildeman (2009) shows that at least in the U.S., the risk of parental imprisonment falls primarily on minority children with less educated parents, which suggests that parental imprisonment practices might contribute to inequality. Turney and Haskins (2014) find that paternal incarceration is associated with an increased likelihood that young children do not progress to the next grade. Murray *et al.* (2012) provide a review of existing research and report a consensus on the possible negative effects of parental prison time on child outcomes, but point out that "firm causal conclusions cannot be drawn".

the United States. He finds that while arrests of parents coincide with negative child outcomes, incarceration of parents is associated with positive outcomes, suggesting that incarceration of parents may benefit children in the United States. Norris *et al.* (2018) also examine incarceration of fathers and mothers in the United States and find a similar result, namely that incarceration of mothers leads to a reduction in later criminal activity by the children, although incarceration of parents in general leads to a reduction in graduation rates and an increase in teenage pregnancy for girls. Last, Artega (2018) looks at the impact of incarceration in Colombia and finds that incarceration improves educational outcomes of children. While we also look at the impact of incarceration that occurs when children are older or when combining all ages on the same outcomes, our extensive data collection efforts allow us to separately analyze the impact of incarceration in different stages of childhood. We are also able to look at the impact on a richer set of outcomes, including birth, childhood, adolescence, and adult outcomes. Our largest departure from this literature is to add the effect of other type of punishments, specifically fines, on child outcomes.

Our paper also contributes to a smaller literature examining intergenerational linkages in crime. Hjalmarsson and Lindquist (2012) use rich data in Sweden to conduct a thorough descriptive analysis, and find that "sons (daughters) with criminal fathers have 2.06 (2.66) times higher odds of having a criminal conviction than those with noncriminal fathers" and that "at the intensive margin, the intergenerational crime relationship is as strong as those for earnings and years of schooling". Hjalmarsson and Lindquist (2013) use Swedish data and information on adopted versus biological children to examine the relative contribution of pre and post-birth factors to child criminal activity. They find that while pre-birth factors matter, post-birth factors are more important. We contribute to their analysis by providing causal estimates of the intergenerational crime relationship, although unfortunately these results are quite imprecise.

Last, this paper contributes to the growing but still scarce literature in economics on parental inputs and their impacts on child outcomes across different ages (see, for example, Hoynes *et al.* (2016), Cunha *et al.* (2010), and Attanasio *et al.* (2015)). Many papers in this literature have shown that parental time and material investments are crucial inputs for child human capital development, particularly at the youngest ages. Our paper focuses on a potentially more extreme negative shock to parental time and resources, and how this might differentially impact children early versus later in childhood.

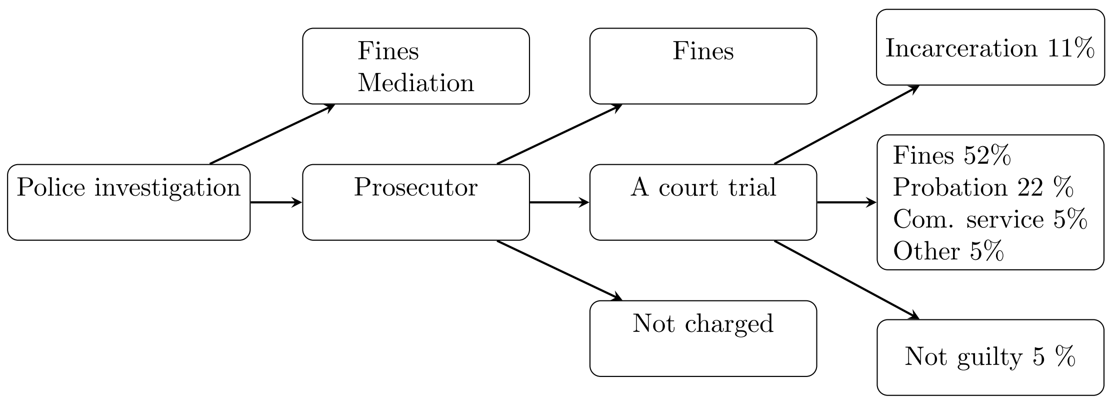
The remainder of the paper is arranged as follows. In Section 1 we provide an overview of the institutional context. In Section 2 we describe the data and provide descriptive evidence on child exposure to crime. In Section 3 we discuss our empirical specification and assess the validity of our instrument. We report our main estimates in Section 4 and conclude in Section 5.

1 Institutional context

For a more detailed description of the institutional context of criminal proceedings in Finland we refer the reader to Huttunen *et al.* (2019). The institutional context described in that paper is identical to this paper. However, we also briefly summarize the main components of the Finnish court system in this section to help the reader understand our research design and the context of this paper.

We focus on Figure 1 which outlines the sentencing procedure in Finland. As the figure shows, a criminal case starts with a police case. Then either the defendant immediately receives a fines or mediation (for example, in minor traffic violations), or is assigned a prosecutor. The prosecutor can decide to simply give fines to the case, bring the case to court, or not charge the defendant. Our data comes from the courts so starts at that stage. Conditional on reaching court, we see that the majority of sentences are fines, at 52 percent. The second most common sentence is probation, and the third most common sentence is incarceration. Together, these three sentences make up the vast majority of all sentences, so we will focus primarily on them in what follows. Conditional on a guilty verdict, fines are considered to be the lowest level punishment while prison is the harshest available punishment (there is no capital punishment in Finland). As discussed in detail in Huttunen *et al.* (2019), while there is a progression of punishments, all three punishments (fines, probation, and prison) are often used for different cases within the same 6 digit crime code. Thus, for defendants who commit the same crime, while assignment to a particularly lenient judge might lead to a punishment of fines, a middle of the road judge might assign the defendant to probation, and a strict judge may assign the defendant to prison.

Figure 1: Sentencing Process in Finland



2 Data

The main source of data for this paper is criminal data provided by the courts in Finland.[5] The court data includes every crime committed by individuals 15 or older and brought to the court from 1977-2015. Some key variables include the category of crime (ranging from treason and murder to tax evasion to traffic violations), the date the crime was committed, the date of the court decision, and the sentence imposed by the court. Often, multiple crimes are committed at once, and are combined into one court case.[6] While our data includes information on every crime committed, for our final data set we count court cases rather than individual crimes.[7][8]

We link parents in the court data to their children using administrative data on all parent-child pairs in Finland.[9] We then link to the the registry data which includes basic demographic

[5]Since we use administrative data, it is not subject to the under-reporting of illegal activity that occurs in survey data.

[6]For example, a drunk driving incident might include a property crime (for damage caused by the accident) as well as the crime of driving under the influence. When describing types of crimes, we will use the designated primary crime from the records.

[7]In most cases, crime incidents consist of one or more criminal acts committed in the same day. However, in some cases criminal incidents consist of many crimes committed over longer periods, but all of which are part of one court case.

[8]For example the drunk driving example above, while appearing twice in the raw data, will enter our data as one incident, although we also retain information on the number and types of crimes committed in a given court case.

[9]The analysis is restricted to men who have children. Note that this is potentially selective. Fertility is endogenous, and might differ among parents who interact with courts and/or are imprisoned versus those who are not.

variables, income, labor market activity, and school completion for parents and children. From school records we obtain the children's GPA at age 16. We link to military records at age 18 which include non-cognitive test scores as well as cognitive test scores (only for men) [in progress]. We collected early childhood outcomes from health centers in 55 municipalities, including Helsinki, from 2000-2014. The data comes from health center check-ups that are conducted annually from birth to age 6. We use the results from development tests at age 4 or 5 that are designed to detect any slow development in speech, neurological, physiological or psychological/emotional areas. The tests are conducted by nurses and medical doctors as part of the extensive health checks at these ages. The cognitive development test includes tasks and remarks such as "Draw a cross", "Asks questions", "Able to explain details from a specific picture", and "Recognizes colours". We construct a dummy taking value one if the child fails at least one of the tests and zero otherwise for the extensive health check that occurs at age four or five (depending on the year the test is conducted). We complement the +/- information for failing an individual test with information from the open text remarks connected to the individual test in question. Last, we worked with the court registrar in Finland to collect the judge id for every court case from 2000-2016.[10]

In Figure 2, we present descriptive results on the profile of childhood exposure to parent criminal activity. We select all children aged 18 in 2006-2013. For these children, we have data on every court incident involving their parents from birth to age 18. Approximately 20% of all 18 year olds have had a parent interact with the courts over the course of their childhood. Age of the child is given on the x axis. In the top graph, we plot the number of court cases by age of child. Within each age, we also document the cumulative number of years in which the parent has had a court case. For example, at age 1 we find that the majority of parents with a court case did not appear in court when the child was age 0. The bottom graphs are similar, except instead of number of court cases we plot number of fine sentences (graph on the bottom left) and prison sentences (graph on the bottom right) over child ages. A striking result is the decrease in crimes as the child ages,

Moreover, mortality may differ across criminal and non criminal populations. Analyzing the impact of criminal activity on mortality and the selection into parenthood and how it relates to crime is interesting, but beyond the scope of this paper.

[10]Financial and time constraints limited our ability to collect data prior to 2000, as this data has not been digitized so would need to be collected and inputted manually.

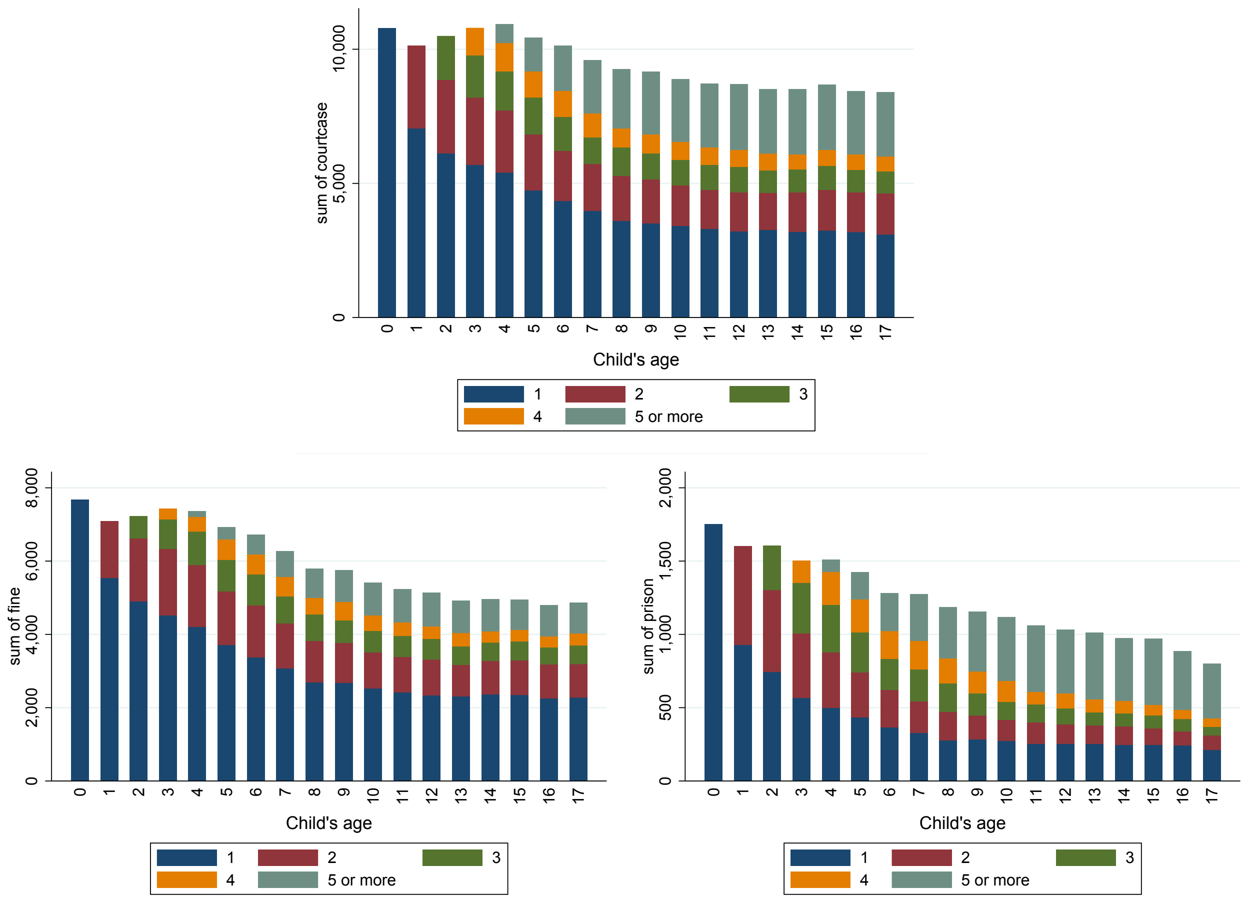
and the mass of court appearances (and to a lesser extent prison sentences) when the child is born.[11] This result reflect the fact that propensity to commit any crime decreases with age. It is also consistent with findings that parenthood might cause parents to commit fewer crimes. The fact that exposure to parent criminal activity is more frequent when children are young might be particularly costly, given the literature suggesting that the early years are pivotal for child development.

These graphs also show that serial offenders are common. Moreover, serial prison sentences are even more common. While this result is consistent with results in other countries in previous papers, it will be important when interpreting our estimates. Specifically, these graphs suggest that parents who commit crimes when children are older are different than parents who commit crimes when children are younger. The former consist of more repeat offenders, and a natural hypothesis is that these parents are worse than the population of parents who commit crimes when their children are younger. As a result, while using judge assignment as an instrument does give the causal effect of sending parents to prison, it does so conditional on the population of criminal parents at a given child age. In light of this descriptive result, we will estimate the causal effects of prison sentences for the full sample but also separately by child stages: early childhood (ages 0-4), middle childhood (ages 5-10), and late childhood (ages 11-14). Our estimated causal effects of incarcerating parents may differ across ages not only because the impact of inputs into child development differ across ages, but also because the population of parents committing crimes varies across ages. It is also useful to understand the specific characteristics of criminal proceedings in Finland. We turn to these next.

In Table 1 we present population means for all children born in 1988-2015 (i.e. all the children who we will be able to observe outcomes using the judge IV) and their parents for a subset of the variables in our data. We present means for the full population, those who appear in court but are not incarcerated while the child is 0-18, and those who appear in court and are incarcerated while the child is 0-18. From the table we can see that the characteristics of parents who appear before court are clearly worse than those of the full population, and the subset of parents who

[11] Court appearances when the child is 0 will include crimes committed just before the child as born as well as in the year the child is born.

Figure 2: Childhood exposure to crime



Note: The top graph plots the number of cases that the child has experienced previously, given the child is experiencing a court case at a given age. The bottom graphs repeat this exercise but for fines (left panel) and prison (right panel) sentence of their parent at the given age.

Table 1: Descriptive statistics

	Full Population		Court Population					
			All Court		Fined		Incarcerated	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parent Characteristics (when child is born)								
Age	31.1	5.9	29.3	7.0	29.4	6.6	27.5	6.3
Income thousands euros	18.0	23.5	12.01	19.3	12.75	17.7	6.1	15.71
Employed	0.716	0.451	0.412	0.492	0.449	0.497	0.185	0.388
Primary degree	0.180	0.383	0.571	0.494	0.535	0.495	0.772	0.420
Secondary degree	0.446	0.497	0.344	0.475	0.371	0.483	0.211	0.408
Tertiary degree	0.374	0.483	0.084	0.278	0.094	0.292	0.017	0.131
Marital status	0.671	0.469	0.395	0.488	0.406	0.491	0.279	0.444
Number of children	1.824	1.177	1.839	1.916	1.921	1.002	1.781	1.041
Court incident t-1	0.016	0.125	0.326	0.460	0.270	0.444	0.645	0.478
Prison sentence t-1	0.001	0.042	0.100	0.299	0.056	0.230	0.363	0.481
Observations	3,357,553		180,328		85,124		28,482	
Child Characteristics								
Income age 19	6.2	5.6	5.6	5.7	5.8	5.8	4.9	5.4
NEET 19	0.206	0.404	0.323	0.467	0.309	0.462	0.398	0.489
Any degree age 19	0.677	0.471	0.408	0.491	0.424	0.495	0.301	0.459
GPA	7.629	0.922	7.082	1.018	7.095	1.017	6.881	0.952
Court incidents age 15-19	0.027	0.162	0.066	0.249	0.063	0.244	0.101	0.301
Prison sentences age 15-19	0.001	0.025	0.005	0.072	0.004	0.065	0.012	0.109

go to prison while their children are younger than 18 exhibit even more negative characteristics. Parents who interact with the courts are younger, less likely to be married, earn less when the child is born, and have lower educational attainment. Parents who in addition serve a prison sentence do even worse than the parents who experience one or more court cases but do not go to prison. Descriptively, the children of criminal parents also appear to do worse, as shown in the bottom panel of 1. They have lower income, lower educational attainment, more children, lower GPA at age 16, are slightly less likely to be married, and are much more likely to have interacted with the court and gone to prison themselves. However, these descriptive results could be driven entirely by selection. In the next section we review our empirical strategy to isolate the causal effect of different punishments of parents on child outcomes.

3 Empirical specification

To study the effect of parental incarceration on child outcomes we estimate the following two-equation system

$$Y_{icft} = \beta_0 + \beta_1 P_{cft} + \beta_2 \boldsymbol{X}_{icft} + \varepsilon_{icft} \tag{1}$$

$$P_{cft} = \alpha_0 + \alpha_1 Z_{jf} + \alpha_2 \boldsymbol{X}_{icft} + \epsilon_{icft} \tag{2}$$

Where Y_{icft} is the outcome for child i of parent f who had a court case c in year t. P_{cft} is a dummy variable equal to one if the parent f has a prison sentence or fine sentence associated with his court case c in year t. $\boldsymbol{X}_{icft}$ is a vector of case, parent, and child control variables (including court by year by crime type fixed effects) and ϵ_{icft} is the error term. OLS estimates of β_1 will be biased if unobserved characteristics of the parent, child, or family are correlated with having a prison or fine sentence. Based on Table 1, we have reason to expect that there might be significant selection in the OLS estimates that could lead to this type of bias.

To address this issue we use random assignment of cases to judges within courts to create exogenous variation in probability of prison sentence which is captured via the instrument Z_{jf}, the leave out residualized incarceration or fine rate for each judge. We calculate Z_{jf} using a similar approach to Dobbie *et al.* (2018b):

$$P^*_{fct} = P_{fct} - \kappa \boldsymbol{X}_{ct}$$

$$Z_{cf} = \left(\frac{1}{n_j - n_{jf}}\right)\left(\sum_{k=0}^{n_j} P^*_{fk} - \sum_{c=0}^{n_{jf}} P^*_{fc}\right),$$

where $\kappa \boldsymbol{X}_{ct}$ represents court-by-year-by-crime fixed effects. In the first equation, we remove the court by year by crime type fixed effects to obtain P^*_{fct}. In the second equation we take the average of this residual incarceration or fine proclivity, but for each defendant we remove the defendant's own cases from the average incarceration or fine rate to create the leave out mean residual incarceration or fine rate for each defendant.

This strategy works if judges vary in their sentencing severity, and the assignment of parents to judges is not correlated with unobserved characteristics of parents or their children associated with both likelihood of fines or incarceration and child outcomes. Under the principal of randomization of cases to judges within year, court, and crime type, which is a legal requirement in Finland, the latter condition should be met, although we also provide evidence supporting this exclusion restriction in the next subsection.

Similarly to Bhuller *et al.* (2016), to construct our judge stringency instrument we restrict our sample of judges to those for whom we observe at least 100 randomly assigned cases between the years 2000-2015. We also restrict the judges to those for whom we observe at least two judges in the same court. In Appendix Table 13, we show how each of these restrictions decreases the number of judges, courts, defendants, and parent-children pairs in our sample. Note that we use all court cases to estimate judge fixed effects, not just those where the defendant has a child under the age of 14.[12] If judge sentencing varies with whether the defendant has a child, this would not be appropriate. In Appendix Figure 7, we show that this does not appear to be an issue - judge stringency measures constructed using only parents are highly correlated with judge stringency measures using the entire sample (we show these results for the incarceration stringency, the correlation for fine stringency is similar). While we construct the judge instruments the same way for all estimates, the final sample used to estimate the causal impacts on the various outcomes used will vary across outcomes, depending on the number of birth cohorts we use. We report sample sizes below every model estimated.[13]

[12]For this reason, the judge stringency graphs reported in this paper are identical to the stringency graphs reported in Huttunen *et al.* (2019).

[13]Note that we also drop all traffic cases in the current draft. Traffic cases have some non-random assignment which we are currently working with the court registrar to address. We also drop judges in training, as these judges are not given full slates of cases and so would also violate randomization. In a very small minority of cases where the defendent's first language is Swedish, the defendant is required by law to have access to a Swedish speaking judge. This will also violate random assignment so we drop these cases as well. Last, we require the defendant's age to be above 22 as younger defendants are treated differently. With the exception of traffic cases, the amount of the other categories of cases is negligible.

3.1 Judge instrument

In Figure 3 we present a graphical representation of the judge stringency measure for fines, and in Figure 4 we report the same figure but for the judge stringency measure for prison. As the histograms in the figures show, there is a large amount of variation in judge stringency in terms of giving fines, and a smaller but still decent amount of variation in judge stringency in terms of incarceration. Moreover, the fitted lines suggest that the first stage is very strong. We also report the first stage estimates from equation 2 using the full sample and each sub-sample (early childhood, middle childhood, and late childhood) in Table 2. The coefficients are all large and significant. In Panel A we report the estimates without controls, and then add demographic controls in Panel B. If our instrument is valid, we would not expect to see the addition of demographic controls to significantly change our estimates, so the estimates in Panels A and B should be similar. This is indeed what we find. In terms of the first stage estimates, we find that being assigned to a judge who is 10 percentage points more likely to incarcerate leads to an increase in the probability of incarceration of approximately 6 percentage points. In terms of fines, being assigned to a judge who is 10 percentage points more likely to fine leads to an increase in the probability of receiving a fine of approximately 10 percentage points.

Note that the sample size for our first stage will vary depending on the outcome of interest. In Appendix Table 14 we report the first stage separately for each outcome, in order to show the strength (or weakness) of the instrument for the relevant samples for whom we estimate each child outcome of interest. We find that the instrument is always significant and strong with one exception: we do not have a strong first stage for the impact of parents being incarcerated in early childhood (ages 0-4) on age 19 outcomes. This is not particularly surprising given that we have very low sample size for this group. We will not observe late life outcomes for most of the younger children because they will not yet be old enough (i.e. in order for us to identify causal effects in early childhood, the children must be in early childhood in 2000 or later). Thus, while we present those results in the main analysis, we would recommend not putting too much stock in those particular estimates.

Having established that the instrument has sufficient variation and a strong first stage, we

Figure 3: Judge stringency variation - Fines

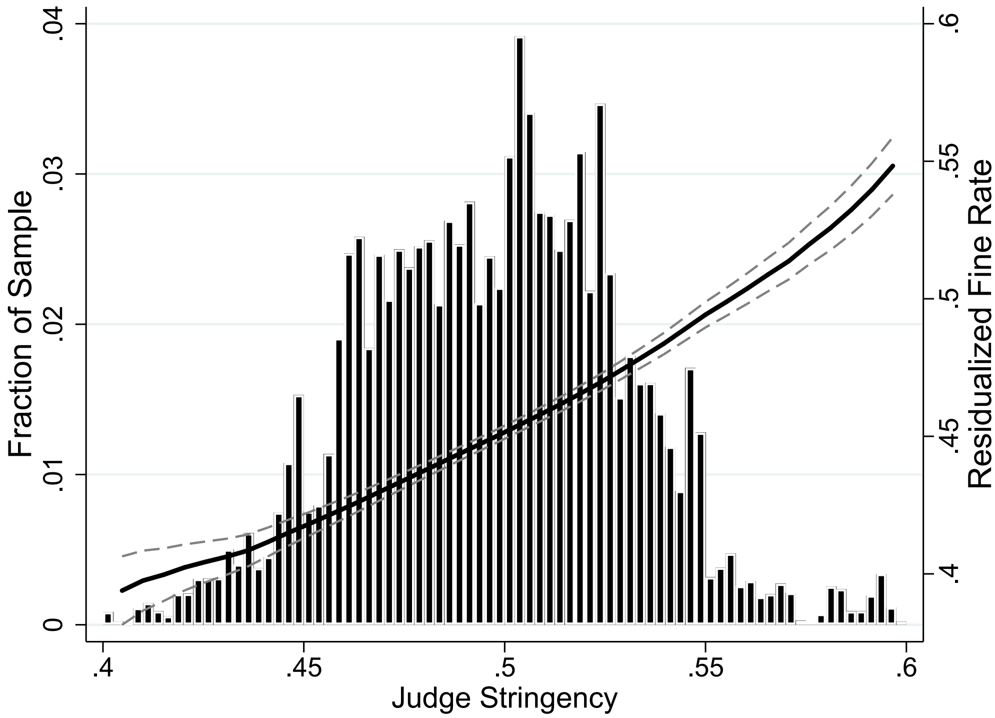


Figure 4: Judge stringency variation - Prison

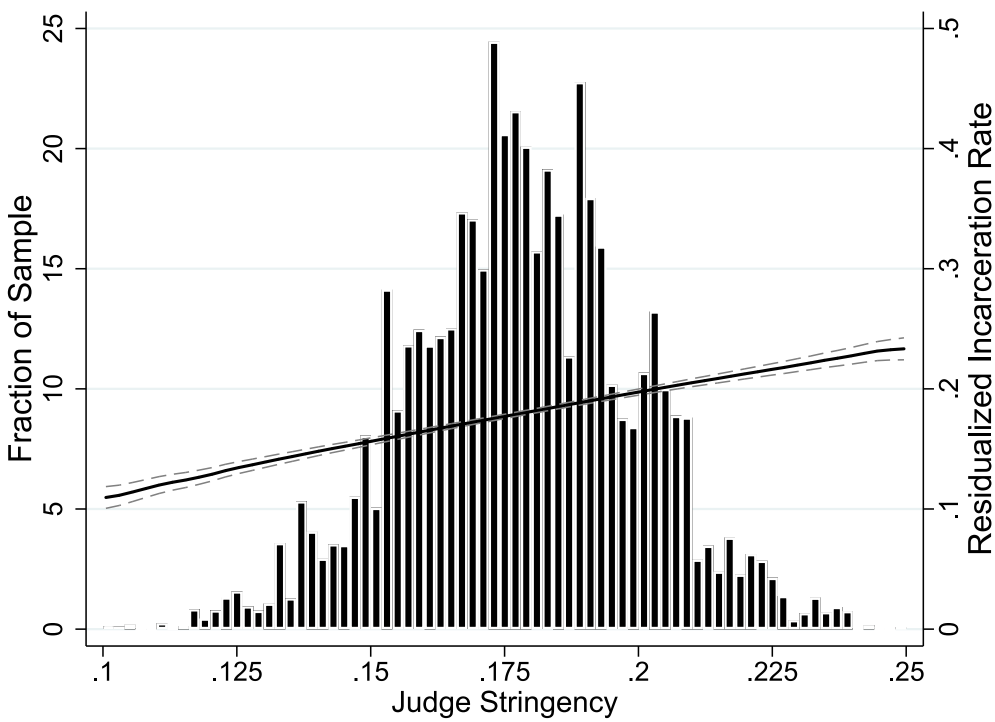


Table 2: First stage

Estimation Samples:	All (1)	Early (2)	Middle (3)	Late (4)
Dependent Variable		***Pr(Fined)***		
A. Court by Year by Crime type fixed effects				
Judge stringency	1.022***	1.010***	0.920***	1.151***
	(0.076)	(0.094)	(0.090)	(0.092))
B. Add controls for demographics Judge stringency	1.032***	1.037***	0.936***	1.135***
	(0.075)	(0.094)	(0.089)	(0.092)
Dependent mean	0.472	0.474	0.471	0.472
F-stat	133.037	82.736	92.030	74.121
N	187,049	57,597	68,165	61,278
Dependent Variable		***Pr(Incarcerated)***		
A. Court by Year by Crime type fixed effects				
Judge stringency	0.603***	0.609***	0.612***	0.604***
	(0.070)	(0.089)	(0.092)	(0.093)
B. Add controls for demographics Judge stringency	0.609***	0.631***	0.650***	0.566***
	(0.066)	(0.087)	(0.087)	(0.085)
Dependent mean	0.158	0.162	0.162	0.148
F-stat	75.352	46.602	44.054	41.940
N	187,049	57,597	68,165	61,278

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

now turn to tests of the validity of the instrument. Beyond the institutional characteristics of the Finnish court system that support the exclusion restriction, we also report balance test results in Table 3. In column (2) we report the estimates from a regression of defendant characteristics on judge stringency for fines. We find that none of the coefficients are significant, and the joint test for significance of these coefficients has an F test statistic of 0.626 and a p-value of 0.833 for fines. The results for prison in column (4) are similarly reassuring, with an F test statistic of 1.053 and a p-value of 0.398. Thus, defendants do appear to be randomly assigned to judges. The balance test passes despite the fact that these characteristics are highly correlated with fines and incarceration, as shown in columns (1) and (3). Almost every single variable is significantly associated with fines and incarceration and the p-value is zero for both cases.

In addition to the traditional balance tests, we also run a placebo test to further establish the validity of our instrument. In Table 4 we report OLS and IV estimates of the impact of fines and incarceration on the child's birth weight when the fine or incarceration occurs after the child is born. While the OLS results are significant and negative, indicating that fines and incarceration are correlated with low birth weight, the IV results are very small (for fines) and not significantly different from zero (for both cases). This is reassuring since our approach requires that the only difference between defendants is that one parent is randomly assigned to a strict judge. If this random assignment to judge occurs after birth, we would expect to see no significant difference in outcomes for the children before the randomization occurs.

The balance test and placebo results presented in this section support the institutional claims of instrument validity. The instrument also appears to have strong predictive power in terms of sentencing as demonstrated by the first stage estimates. However, there are two additional concerns that must be addressed before we proceed with the main results: multi-dimensional sentencing and monotonicity. We briefly summarize these concerns below and provide evidence that this is not an issue in our setting.

Table 3: Balance tests

	Pr(fined) (1)	Judge Fined (2)	Pr(prison)	Judge Prison
Demographics (1 year before sentencing)				
Age	0.0004	0.0000	-0.0001	0.0000
	(0.0003)	(0.0000)	(0.0002)	(0.0000)
Kids	0.0055**	0.0000	-0.0076***	0.0000
	(0.0018)	(0.0001)	(0.0009)	(0.0001)
Married	-0.0243***	-0.0003	0.0076***	0.0001
	(0.0044)	(0.0002)	(0.0027)	(0.0002)
Post compulsory degree	0.0129***	-0.0003	-0.0157***	0.0003
	(0.0040)	(0.0002)	(0.0026)	(0.0002)
College	-0.00466	-0.0005	-0.0140***	0.0004
	(0.0068)	(0.0004)	(0.0031)	(0.0003)
Employed	0.0343***	0.0001	-0.0427***	-0.0002
	(0.0044)	(0.0002)	(0.0028)	(0.0002)
Income	0.0000*	0.0000	-0.0000***	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native born	0.0012	0.0005	0.0260***	-0.0002
	(0.0070)	(0.0004)	(0.0039)	(0.0003)
Female	0.0523***	-0.0004	-0.0450***	0.0001
	(0.0054)	(0.0003)	(0.0029)	(0.0001)
Past Criminal History				
Ever incarcerated, t-1	-0.1080***	-0.0004	0.2750***	0.0006*
	(0.0068)	(0.0004)	(0.0072)	(0.0003)
Ever charged, t-1	-0.0378***	0.0000	0.0389***	-0.0003
	(0.0046)	(0.0003)	(0.0033)	(0.0002)
Ever incarcerated, t-2 to t-3	-0.1061***	0.0000	0.2760***	-0.0002
	(0.0062)	(0.0004)	(0.0067)	(0.0002)
Ever charged, t-2 to t-3	-0.0430***	0.0002	0.0466***	0.0001
	(0.0047)	(0.0003)	(0.0028)	(0.0002)
F Test	232.3	0.626	881.1	1.053
P Value	0.000	0.833	0.000	0.398
N	187049	187049	187049	187049

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients. *p<0.1, **p<0.05, ***p<0.01

Table 4: Placebo tests

	OLS	**IV**
	(1)	(2)
Fines: Child birth weight	21.4***	18.50
	(2.828)	(44.59)
Prison: Child birth weight	-73.58***	-174.5
	(7.91)	(90.22)
N	174982	174982
Dependent mean	3415.6	3415.6
Controls	No	No

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

Multi-dimensional sentencing and monotonicity assumptions

As discussed in Mueller-Smith (2014), two important assumptions that should always be checked in these settings are: no multidimensional sentencing (to avoid violating the exclusion restriction) and monotonicity. The former assumption is a legal requirement in Finland. Judges in Finland are only allowed to assign a single punishment. However, to confirm that this legal condition holds in practice, we also checked across our sample and found no cases of multidimensional sentencing. Of course, the judge might impact the defendant in ways other than punishment, for example in his or her treatment of the defendant. We have no information on how judges treat defendants in the courtroom, but our hypothesis is that such violations, if they occur, do not strongly impact defendants.

To check if our instrument is consistent with the monotonicity assumption, we take a similar approach as in Bhuller *et al.* (2016), and do two things. First, we show that the first stage is similarly strong and positive across a number of sub samples. Next, we perform a "reverse sample instrument test". Specifically, for a series of different variables we take a subset of the sample to construct the judge instrument, and then estimate the first stage using the other part of the sample (which was not used to construct the judge instrument). We present these results in the

Appendix. None of the results from these two exercises indicate a violation of monotonicity which is reassuring. Note, though, that if we had encountered violations, Mueller-Smith (2014) suggests solutions to both of these problems. Given that we do not have any obvious issues, we now proceed to the main results.

4 Main results

4.1 Impact of punishing parents with a fine on child outcomes

We present the main results for the spillover effects of fines on defendant's children in Table 5 (for OLS results) and Table 6 (for IV results, i.e. the causal impact of the child's parent receiving a fine, as opposed to probation or prison). The OLS results show very little impact of fines on child outcomes. Note that OLS estimates include a rich set of controls. We find significant negative correlations between getting a fine and GPA which suggest negative spillovers on children, but we also find that slightly later life outcomes appear to be improved by one's parent receiving a fine (rather than probation or prison). Specifically, there is a small and significant increase in the likelihood the child has obtained a degree by age 19, and a small but significant decrease in the probability that the child is not involved in productive activity (i.e. does not have a job and is not in education) at age 19. Otherwise, most of the outcomes are very close to zero. Thus, OLS estimates largely suggest either no impact or a very small positive impact of a child's parent receiving a fine.

Next, we turn to the IV estimates. Unfortunately the results are not very informative. While we find no statistically significant impacts of fines on child outcomes, the estimates are so imprecise that we cannot rule out even larger effects of fines on child outcomes compared to OLS.

One reason why the estimates might be so imprecise is that the impact of receiving fine might depend on the amount of fine the parent has to pay. Just as we have seen impacts of income shocks to parents on child outcomes in previous papers, conditional on receiving a fine we might expect a larger fine to have more negative impacts on the child. While we do not have results on the impact of fine amount on child outcome, those estimates are in progress and could bring additional nuance to our interpretation of the trade-offs of different punishments.

4.2 Impact of incarcerating parents on child outcomes

The OLS results giving the association between incarcerations of defendants and their children's outcomes are reported in Table 7. While we find no significant impacts on whether the child fails the cognitive skills test at age 5 and criminal activity between the ages of 15 and 17, in every other case we find that incarceration is associated with worse outcomes for children. Note that this is true even though we have included a rich set of controls when estimating the OLS estimates. We find that having a parent incarcerated is associated with a decrease in GPA of 4.1 percentage points and is highest (although not significant) when the incarceration occurs in early childhood. Incarceration of the parent is associated with increases the likelihood that a child is not in school or in the labor market at age 19 by 3.5 percentage points, and the effect increases the later in the child life's the parent is incarcerated. Last, having a parent incarcerated during childhood is associated with a 4.7 percentage point decrease in the likelihood of a school degree by age 19, with the largest impact if the parent is incarcerated in early childhood. Thus, overall the OLS results suggest negative impacts of incarceration on child outcomes, although no period of childhood stands out as having particularly negative effects.

We now turn to the IV results in Table 8. We find no statistically significant impacts of prison sentences on any of the child outcomes. The lack of significance is true when looking at the impact of incarceration on all outcomes, irregardless of when the prison sentence occurred, as well as prison sentence that occurred during specific stages of childhood. However, the lack of significant effects should not be confused with no effect of incarcerating parents on children. The results are very imprecise and the point estimates are often far from zero.

In summary, while the OLS estimates suggest large negative effects of incarceration, the causal estimates paint a much less clear picture. When comparing outcomes of children of marginal prisoners who are effectively randomly assigned to prison due to random assignment to a stricter judge, we find no statistically significant results on children. However, unfortunately the results are so imprecise that we cannot rule out the negative effects we see in the OLS estimates.

Table 5: OLS Outcomes - Fines

	All	Early	Middle	Late
	(1)	(2)	(3)	(4)
A: Failure of Cognition Test Age 5				
Estimate	0.0073	0.0073	-	-
SE	(0.0067)	(0.0067)	-	-
Mean of dep	0.319	0.319	-	-
Observations	25525	25525	-	-
B: GPA Age 16				
Estimate	-0.020*	-0.018	-0.035*	-0.014
Se	(0.010)	(0.034)	(0.016)	(0.011)
Mean of dep	7.082	7.142	7.081	7.077
Observations	71065	4376	24980	41681
C: Crime Ages 15-17				
Estimate	0.002	0.000	0.003	0.002
Se	(0.002)	(0.003)	(0.002)	(0.002)
Mean of dep	0.038	0.013	0.0032	0.046
Observations	91317	8407	32526	50374
D: No employment or schooling age 19				
Estimate	-0.011*	-0.000	-0.010	-0.013*
Se	(0.005)	(0.029)	(0.008)	(0.006)
Mean of dep	0.323	0.355	0.339	0.314
Observations	56183	1246	18011	36919
E: Degree by age 19				
Estimate	0.008*	0.028	0.006	0.007
Se	(0.005)	(0.022)	(0.008)	(0.005)
Mean of dep	0.408	0.408	0.408	0.408
Observations	66590	2833	22582	41166

Note. All estimations include controls for court by court entry year by crime type fixed effects, as well as demographic and labor market controls. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

Table 6: IV Outcomes - Fines

	All	Early	Middle	Late
	(1)	(2)	(3)	(4)
A: Failure of Cognition Test Age 5				
Estimate	-0.057	-0.057	-	-
SE	(0.098)	(0.311)	-	-
Mean of dep	0.319	0.319	-	-
Observations	25525	25525	-	-
B: GPA Age 16				
Estimate	-0.150	-1.492	-0.381	0.064
Se	(0.161)	(1.098)	(0.316)	(0.191)
Mean of dep	7.082	7.142	7.081	7.077
Observations	71065	4376	24980	41681
C: Crime Ages 15-17				
Estimate	-0.016	0.049	-0.116	0.020
Se	(0.028)	(0.097)	(0.061)	(0.033)
Mean of dep	0.038	0.013	0.032	0.046
Observations	91317	8407	32526	50374
D: No employment or schooling age 19				
Estimate	-0.146	0.356	-0.074	-0.209
Se	(0.094)	(0.885)	(0.171)	(0.115)
Mean of dep	0.323	0.355	0.339	0.314
Observations	56183	1246	18011	36919
E: Degree by age 19				
Estimate	-0.101	-1.078	-0.071	-0.069
Se	(0.085)	(1.151)	(0.149)	(0.102)
Mean of dep	0.408	0.408	0.408	0.408
Observations	66590	2833	22582	41166

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

Table 7: OLS Outcomes - Prison

	All	Early	Middle	Late
	(1)	(2)	(3)	(4)
A: Failure of Cognition Test Age 5				
Estimate	-0.0063	-0.0063	-	-
SE	(0.0123)	(0.0123)	-	-
Mean of dep	0.319	0.319	-	-
Observations	25525	25525	-	-
B: GPA Age 16				
Estimate	-0.041*	-0.065	-0.022	-0.051*
Se	(0.018)	(0.052)	(0.025)	(0.021)
Mean of dep	7.082	7.142	7.081	7.077
Observations	71065	4376	24980	41681
C: Crime Ages 15-17				
Estimate	0.003	-0.002	0.002	0.004
Se	(0.004)	(0.004)	(0.004)	(0.005)
Mean of dep	0.038	0.013	0.032	0.046
Observations	91317	8407	32526	50374
D: No employment or schooling age 19				
Estimate	0.035***	0.020	0.032**	0.037***
Se	(0.010)	(0.051)	(0.014)	(0.011)
Mean of dep	0.323	0.355	0.339	0.314
Observations	56183	1246	18011	36919
E: Degree by age 19				
Estimate	-0.047***	-0.063**	-0.039**	-0.049***
Se	(0.009)	(0.031)	(0.013)	(0.009)
Mean of dep	0.408	0.408	0.408	0.408
Observations	66590	2833	22582	41166

Note. All estimations include controls for court by court entry year by crime type fixed effects, as well as demographic and labor market controls. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

Table 8: IV Outcomes - Prison

	All	Early	Middle	Late
	(1)	(2)	(3)	(4)
A: Failure of Cognition Test Age 5				
Estimate	0.063	0.063	-	-
SE	(0.233)	(0.233)	-	-
Mean of dep	0.319	0.319	-	-
Observations	25525	25525	-	-
B: GPA Age 16				
Estimate	-0.364	1.052	-0.233	-0.587
Se	(0.365)	(1.380)	(0.478)	(0.539)
Mean of dep	7.082	7.142	7.081	7.077
Observations	71065	4376	24980	41681
C: Crime Ages 15-17				
Estimate	-0.024	-0.143	0.072	-0.071
Se	(0.058)	(0.106)	(0.076)	(0.097)
Mean of dep	0.038	0.013	0.032	0.046
Observations	91317	8407	32526	50374
D: No employment or schooling age 19				
Estimate	-0.013	0.469	-0.244	0.181
Se	(0.198)	(1.164)	(0.270)	(0.284)
Mean of dep	0.323	0.355	0.339	0.314
Observations	56183	1246	18011	36919
E: Degree by age 19				
Estimate	0.166	-0.263	0.169	0.202
Se	(0.191)	(0.574)	(0.244)	(0.292)
Mean of dep	0.408	0.408	0.408	0.408
Observations	66590	2833	22582	41166

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

4.3 Impact of fines and incarceration for resident parents

Thus far, when considering the causal impacts of fines and prison sentences of parents on children, we have failed to find significant impacts. One reason why this might be the case is that the vast majority of parents who are fined or incarcerated are men and a number of them may no longer reside with the child. We would expect the impact of sentences levied on resident parents to be more salient for their children compared to non-resident parents. In this section, we look only at resident parents. Note that we define resident parents as all mothers and fathers who have lived with the mother at least once during the five years before the sentence. While children may not always reside with mothers, it is rare for them not to do so and since we do not observe residency for children, this is the best approximation for residency we have.[14] We report OLS results for fines in Appendix Table 15, IV results for fines in Appendix Table 16, OLS results for prison in Appendix Table 17, and IV results for prison in Appendix Table 18. Against the hypothesis that effects will be stronger for resident parents, the IV results indicate no clear effects of prison sentence or fines on child outcomes. For the resident sample, OLS results indicate stronger correlations between prison sentence and long-term child outcomes at age 19 than when including non-resident fathers. Having a parent incarcerated is associated with 5.5 percentage point higher likelihood that a child is not in school or in employment at age 19 (this corresponds to 17% decrease when compare to the baseline that is 0.315), and 6.3 percentage smaller likelihood that child has obtained a high school degree by age 19 (14% decrease). As with the sample that includes non-resident parents, the OLS results indicate that fines are generally associated with more positive outcomes for children, with the exception of GPA at age 16, although most impacts are small.

4.4 Impact of criminal punishments on partners

We have shown that despite strong OLS estimates, there do not appear to be significant causal impacts of either fines or prison on children. In this subsection, we move to the possible spillovers

[14]We are able to establish biological parents through an alternative data set that collects this information upon the birth of the child.

on defendant's partners. We focus on three outcomes of interest. First, we look at whether couples are more likely to separate following either a punishment of fines or a punishment of prison. We report the estimates in Table 9. The results for fines are in the top panel and the results for incarceration are in the bottom panel. OLS estimates for fines suggest that having a partner who is punished with a fine, as opposed to probation or prison, is associated with a decrease in the probability that the partnership dissolves. However, IV estimates are not significant, and the point estimates suggest no impact of fines on whether the couple remains together. Turning to the impact of prison, OLS estimates suggest that prison leads to a large jump in becoming single. IV estimates are also large and positive for the first and third year after the sentence, but similar to the results for children, are very imprecise. Next, we look at the impacts on the partner's employment and earnings. The estimates suggest that both fines and incarceration have negative spillovers on spouse's earnings, although the effects are only significant for the case of the impact of fines on the spouse's earnings in the first two years after punishment.

Table 9: Impact on Partner Separations

Dep. variable	Pr(single)		
	1 year after	2 years after	3 years after
	(1)	(2)	(3)
OLS: Fines	-0.047***	-0.048***	-0.043***
No controls	(0.004)	(0.004)	(0.004)
OLS: Fines	-0.003	-0.003	-0.001
Controls	(0.003)	(0.003)	(0.003)
IV: Fines	0.072	0.045	-0.036
No controls	(0.056)	(0.058)	(0.057)
OLS: Incarceration	0.189***	0.184***	0.177***
No controls	(0.006)	(0.005)	(0.006)
OLS: Incarceration	0.040***	0.033***	0.032***
Controls	(0.005)	(0.005)	(0.005)
IV: Incarceration	0.113	-0.019	0.169
No controls	(0.153)	(0.148)	(0.144)
Dep. mean	0.588	0.590	0.588
Number of cases	92334	91357	90367

Table 10: Impact on Spouse's charges

Dep. variable	Pr(Spouse Charged)		
	1 year after	2 years after	3 years after
	(1)	(2)	(3)
OLS: Fines	-0.023***	-0.021***	-0.017***
No controls	(0.003)	(0.003)	(0.002)
OLS: Fines	0.003	0.002	0.002
Controls	(0.002)	(0.002)	(0.002)
IV: Fines	0.042	0.035	0.055
No controls	(0.040)	(0.039)	(0.036)
OLS: Incarceration	0.159***	0.134***	0.118***
No controls	(0.008)	(0.007)	(0.007)
OLS: Incarceration	0.034***	0.024***	0.028***
Controls	(0.007)	(0.006)	(0.006)
IV: Incarceration	0.085	0.062	-0.009
No controls	(0.123)	(0.122)	(0.112)
ymean	0.088	0.077	0.070
N	68800.000	68800.000	68800.000

Table 11: Impact on Spouse's Employment

Dep. variable	Pr(Spouse Employed)		
	1 year after (1)	2 years after (2)	3 years after (3)
OLS: Fines	0.047***	0.049***	0.048***
No controls	(0.004)	(0.004)	(0.004)
OLS: Fines	-0.002	0.000	0.002
Controls	(0.004)	(0.004)	(0.004)
IV: Fines	-0.073	-0.082	-0.062
No controls	(0.058)	(0.058)	(0.062)
OLS: Incarceration	-0.266***	-0.263***	-0.243***
No controls	(0.008)	(0.008)	(0.009)
OLS: Incarceration	-0.023***	-0.027***	-0.023***
Controls	(0.008)	(0.008)	(0.008)
IV: Incarceration	0.059	-0.153	-0.212
No controls	(0.204)	(0.190)	(0.194)
Dep. mean	0.571	0.575	0.580
Number of cases	73291	72778	71715

Table 12: Impact on Spouse's Earnings

Dep. variable	Spouse's Earnings		
	1 year after (1)	2 years after (2)	3 years after (3)
OLS: Fines	1243.492***	1265.625***	1231.915 ***
No controls	(169.984)	(163.575)	(174.748)
OLS: Fines	-173.129	-165.135	-177.245
Controls	(152.520)	(145.039)	(157.250)
IV: Fines	-4933.019*	-4910.871*	-2868.663
No controls	(2720.613)	(2648.413)	(2925.956)
OLS: Incarceration	-9075.957***	-9023.303***	-8899.448***
No controls	(207.597)	(225.592)	(234.718)
OLS: Incarceration	-623.382***	-585.498***	-722.775
Controls	(211.704)	(225.321)	(228.647)
IV: Incarceration	-5357.400	-3290.092	-9357.119
No controls	(8605.734)	(8440.101)	(9101.428)
Dep. mean	15492	15879	
Number of cases	72778	72275	71715

4.5 Complier analysis

While we cannot identify specific compliers in the data, we can extend the judge fixed effects analysis to analyze the set of compliers in the data.[15] In this subsection we do this by splitting our data into subsamples, using the subsamples to calculate probability of incarceration, and then using these estimates to calculate compliance weights for each subsample. The intuition is that a subset with a stronger first stage relative to other subsets contains more compliers. We report the results in Figure 5 for fines and in Figure 6 for prison. Fine compliers are more likely to have a tertiary degree and more likely to be involved in property crimes. For prison the results show that those without a degree, with previous charges, and who commit violet and non-property crimes are more likely to be compliers while those who are employed and accused of property

[15]This is described in more detail in Bhuller *et al.* (2016), Dobbie *et al.* (2018a), Dobbie *et al.* (2018b), and Abadie (2003).

crimes are less likely to be compliers.

Figure 5: Complier Weights - Fines

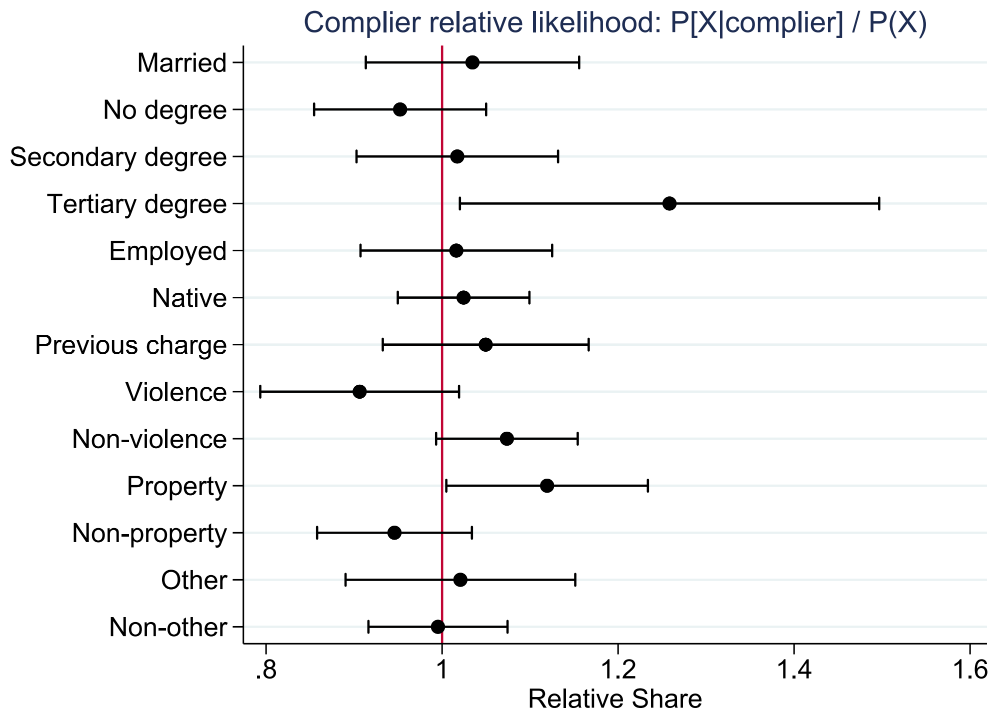
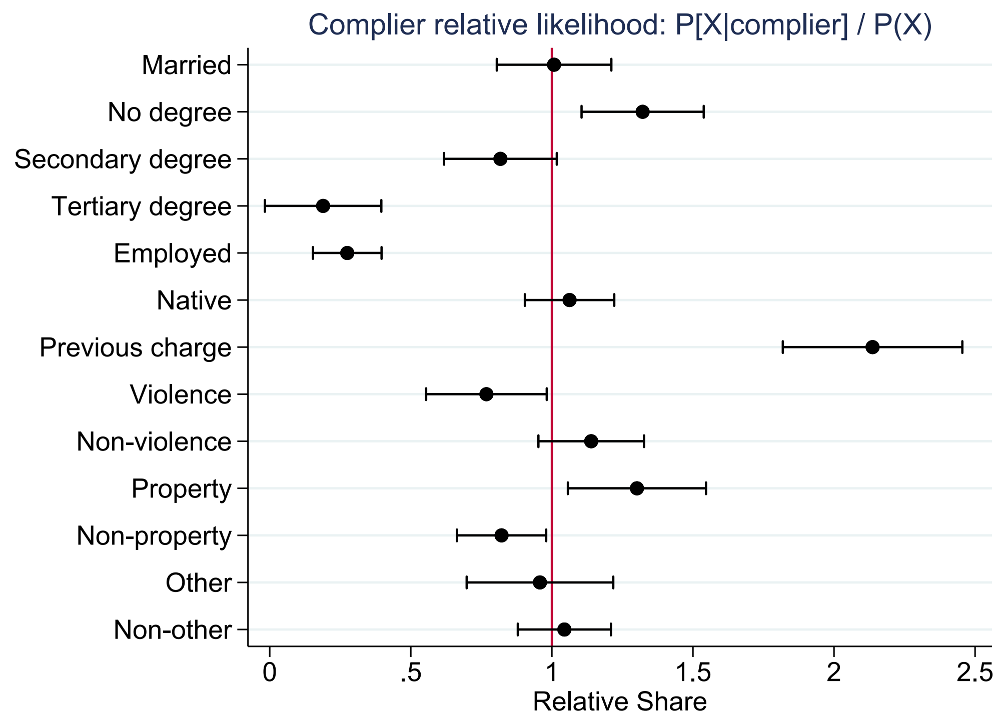


Figure 6: Complier Weights - Prison



We can also use the weights calculated in the previous step to re-estimate a complier weighted OLS. This re-weighted sample uses the random variation in assignment of judges to get closer to the causal impact of incarceration on parent and child outcomes, in a similar way to the IV results. [These estimates are still in progress.]

5 Conclusion

In this paper, we provide evidence on the impact of assessing a fine on a child's parent, or sending a child's parent to prison, on a variety of parent and child outcomes in Finland. We find that fines are associated with marginally better or no impact on child outcomes. Causal estimates show no significant impact of fines on child outcomes, although these estimates are very imprecise. Compared to punishing parents with fines, we might expect prison sentences of parents to have a more negative impact on children. While the OLS estimates of the impact of prison are consistent with this hypothesis, our IV results are unfortunately too imprecise to draw strong conclusions.

If the OLS results are accurate, and prison has more negative impacts than fines, this alone might not be problematic for policy makers provided they can counteract the harms done to children, who should not be punished for their parent's mistakes. Fortunately, a number of studies have demonstrated that well designed interventions can have large impacts on child development across ages. While it would be useful to have more precise causal evidence in order to either confirm or rule out the OLS results, if the negative impacts of prison are confirmed, one way to counterract these effects is for policy makers to offer interventions for children affected by the criminal justice system.

Moreover, as our descriptive statistics suggest, irregardless of the causal effects children of parents who interact with the criminal justice system are clearly at risk for worse outcomes. In order to reach the most vulnerable children, policy makers might wish to target children of defendants for interventions whether or not the government causes additional harm to the children by punishing their parents.

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A Appendix

Table 13: Sample restrictions for judges from 2000-2015

	Sample size after each restriction (in each row)				
	A. Judge Stringency Panel				
Number of	Cases	Defendants	Judges	Courts	Children
No restrictions	388829	202408	3361	65	-
Drop training judges	304326	168882	1035	65	-
Swedish speaking	296245	163688	1034	65	-
Drop judges < 100 over career	282135	157644	680	65	-
Drop courts <2 judges	282119	157637	680	65	-
	B. Panel of Analysis for cases decided between 2000-2013				
Number of	Cases	Defendants	Judges	Courts	Children
Merging with kids	187049	60373	677	65	100477

	Baseline instrument	Reverse-sample Instrument
Sub-sample:	First stage P(Incarcerated)	First stage P(Incarcerated)
Any post compulsory education		
Estimate	0.389	0.331
(se)	(0.064)	(0.051)
Observations	53993	56488
No post compulsory education		
Estimate	0.581	0.598
(se)	(0.079)	(0.078)
Observations	69731	72886
Previously Employed		
Estimate	0.150	0.116
(se)	(0.052)	(0.041)
Observations	48096	50297
Previously non-Employed		
Estimate	0.729	0.456
(se)	(0.080)	(0.107)
Observations	75703	79134
Married		
Estimate	0.379	0.431
(se)	(0.089)	(0.083)
Observations	41074	42825
Not married		
Estimate	0.610	0.391
(se)	(0.064)	(0.046)
Observations	82913	86820
Over 30 years old		
Estimate	0.497	0.411
(se)	(0.006)	(0.057)
Observations	80863	84386
Less than 30 years old		
Estimate	0.667	0.555
(se)	(0.095)	(0.077)
Observations	42953	45094
Violence crimes		
Estimate	0.363	0.285
(se)	(0.075)	(0.062)
Observations	45637	47779
Property crimes		
Estimate	0.563	0.489
(se)	(0.0986)	(0.099)
Observations	43298	45138
Other crimes		
Estimate	0.398	0.422
(se)	(0.099)	(0.100)
Observations	24074	25351

Figure 7: Robustness to using only parents to construct instrument

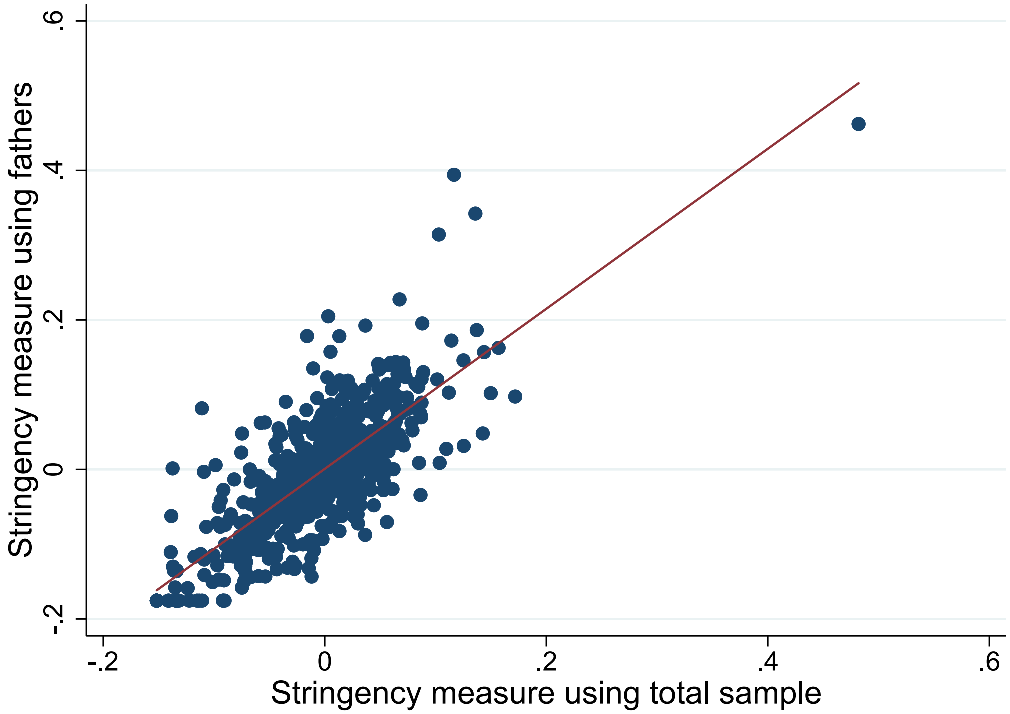

Table 14: First Stage for Outcome Subsamples - Prison

	All (1)	Early (2)	Middle (3)	Late (4)
Cognition Age 5	0.383***	0.383***		
	(0.104)	(0.104)		
GPA Age 16	0.517***	0.840**	0.535***	0.467***
	(0.0873)	(0.257)	(0.138)	(0.106)
Crime Ages 15-17	0.536***	0.712***	0.520***	0.486***
	(0.0782)	(0.177)	(0.124)	(0.0944)
Age 19 outcomes	0.543***	0.304	0.583***	0.530***
	(0.106)	(0.885)	(0.163)	(0.123)

Table 15: OLS Outcomes - Fines, Resident Parents

	All (1)	Early (2)	Middle (3)	Late (4)
A: Failure of Cognition Test Age 5				
Estimate	0.0059	0.0059	-	-
SE	(0.0079)	(0.0079)	-	-
Mean of dep	0.308	0.308	-	-
Observations	18289	18289	-	-
B: GPA Age 16				
Estimate	-0.038**	-0.055	-0.041	-0.034**
Se	(0.013)	(0.041)	(0.021)	(0.016)
Mean of dep	7.148	7.165	7.142	7.148
Observations	36871	3122	13946	19765
C: Crime Ages 15-17				
Estimate	0.002	0.001	0.000	0.003
Se	(0.002)	(0.003)	(0.003)	(0.003)
Mean of dep	0.031	0.010	0.028	0.039
Observations	48310	6089	18316	23878
D: No employment or schooling age 19				
Estimate	-0.013**	-0.006	-0.019	-0.012*
Se	(0.007)	(0.035)	(0.011)	(0.008)
Mean of dep	0.315	0.329	0.326	0.308
Observations	28221	873	9855	17474
E: Degree by age 19				
Estimate	0.002	0.015	0.007	-0.002
Se	(0.006)	(0.027)	(0.010)	(0.008)
Mean of dep	0.433	0.412	0.432	0.436
Observations	34080	2035	12464	19560

Note. All estimations include controls for court by court entry year by crime type fixed effects, as well as demographic and labor market controls. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

Table 16: IV Outcomes - Fines, Resident Parents

	All (1)	Early (2)	Middle (3)	Late (4)
A: Failure of Cognition Test Age 5				
Estimate	0.051	0.051	-	-
SE	(0.114)	(0.114)	-	-
Mean of dep	0.308	0.308	-	-
Observations	18289	18289	-	-
B: GPA Age 16				
Estimate	0.063	-1.982	0.142	0.349
Se	(0.267)	(1.366)	(0.416)	(0.334)
Mean of dep	7.148	7.165	7.142	7.148
Observations	36871	3122	13946	19765
C: Crime Ages 15-17				
Estimate	-0.054	0.135	-0.184	-0.007
Se	(0.036)	(0.132)	(0.097)	(0.044)
Mean of dep	0.031	0.010	0.028	0.039
Observations	48310	6089	18316	23878
D: No employment or schooling age 19				
Estimate	-0.256	1.871	-0.273	-0.361
Se	(0.166)	(5.441)	(0.235)	(0.216)
Mean of dep	0.315	0.329	0.326	0.308
Observations	28221	873	9855	17474
E: Degree by age 19				
Estimate	-0.105	-4.411	-0.205	0.110
Se	(0.159)	(11.859)	(0.220)	(0.194)
Mean of dep	0.433	0.412	0.432	0.436
Observations	34080	2035	12464	19560

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

Table 17: OLS Outcomes - Prison, Resident Parents

	All	Early	Middle	Late
	(1)	(2)	(3)	(4)
A: Failure of Cognition Test Age 5				
Estimate	-0.0062	-0.0062	-	-
SE	(0.0157)	(0.0157)	-	-
Mean of dep	0.308	0.308	-	-
Observations	18289	18289	-	-
B: GPA Age 16				
Estimate	-0.033	-0.050	-0.023	-0.060
Se	(0.028)	(0.064)	(0.038)	(0.037)
Mean of dep	7.148	7.165	7.142	7.148
Observations	36871	3122	13946	19765
C: Crime Ages 15-17				
Estimate	0.005	0.000	0.006	0.013
Se	(0.006)	(0.005)	(0.007)	(0.010)
Mean of dep	0.031	0.010	0.028	0.039
Observations	48310	6089	18316	23878
D: No employment or schooling age 19				
Estimate	0.055**	0.054	0.044	0.062**
Se	(0.017)	(0.068)	(0.023)	(0.021)
Mean of dep	0.315	0.329	0.326	0.308
Observations	28221	873	9855	17474
E: Degree by age 19				
Estimate	-0.063***	-0.078*	-0.076***	-0.057***
Se	(0.013)	(0.036)	(0.020)	(0.017)
Mean of dep	0.433	0.412	0.432	0.436
Observations	34080	2035	12464	19560

Note. All estimations include controls for court by court entry year by crime type fixed effects, as well as demographic and labor market controls. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

Table 18: IV Outcomes - Prison, Resident Parents

	All	Early	Middle	Late
	(1)	(2)	(3)	(4)
A: Failure of Cognition Test Age 5				
Estimate	-0.034	-0.034	-	-
SE	(0.229)	(0.229)	-	-
Mean of dep	0.308	0.308	-	-
Observations	18289	18289	-	-
B: GPA Age 16				
Estimate	-0.569	1.367	-0.449	-1.358
Se	(0.760)	(1.318)	(0.773)	(1.398)
Mean of dep	7.148	7.165	7.142	7.148
Observations	36871	3122	13946	19765
C: Crime Ages 15-17				
Estimate	0.004	-0.083	0.021	0.062
Se	(0.070)	(0.077)	(0.096)	(0.159)
Mean of dep	0.031	0.010	0.028	0.039
Observations	48310	6089	18316	23878
D: No employment or schooling age 19				
Estimate	0.439	0.611	0.238	0.628
Se	(0.303)	(1.825)	(0.339)	(0.480)
Mean of dep	0.315	0.329	0.326	0.308
Observations	28221	873	9855	17474
E: Degree by age 19				
Estimate	0.052	-0.390	0.247	-0.132
Se	(0.335)	(0.589)	(0.357)	(0.559)
Mean of dep	0.433	0.412	0.432	0.436
Observations	34080	2035	12464	19560

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

The Crime Ladder: Estimating the Impact of Different Punishments on Defendant Outcomes*

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PRELIMINARY DRAFT - PLEASE DO NOT CITE OR CIRCULATE WITHOUT PERMISSION FROM THE AUTHORS

Abstract

Criminologists advocate a "ladder" of punishments in response to criminal activity, starting with less severe punishments such as fines, and gradually progressing to more severe punishments such as incarceration either as a defendant commits more crimes or more severe crimes. Understanding the trade-offs between different punishments in terms of future criminal and labor market outcomes of defendants is vital in order to determine how to optimally implement such a ladder of punishments. In this paper we analyze the impacts of two common punishments, fines and prison, on defendant's future criminal and labor market outcomes. We find that prison has a mixed impact, decreasing the number of future criminals charges but also decreasing employment and earnings. Fines, on the other hand, have no impact on labor market outcomes, but slightly increase future criminal activity, although fines do not lead to a statistically significant increase in the probability of prison. Our results suggest that neither punishment is entirely better than the other, and policy makers will need to decide what outcomes are more important when choosing appropriate punishment thresholds.

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Introduction

Criminologists advocate a "ladder" of punishments in response to criminal activity, starting with less severe punishments such as fines, and gradually progressing to more severe punishments such as incarceration either as a defendant commits more crimes or more severe crimes (Lappi-Seppälä (2016), Hinkkanen and Lappi-Seppälä (2011)). Understanding the trade offs between different punishments in terms of future criminal and labor market outcomes of defendants is vital in order to determine how to optimally implement such a ladder of punishments. In a growing literature, economists have primarily focused on the final rung in the ladder of possible punishments, estimating the impact of incarceration on defendant outcomes.[1] However, incarceration is not the only punishment available to the criminal justice system. Moreover, outside of the United States, prison is less commonly used compared to other possible punishments like probation and fines. As Becker stated in his seminal paper on crime, if fines are effective at deterring crime then "social welfare is increased if fines are used *whenever possible*" (Becker (1968), pg. 28). Yet despite the frequent use of punishments other than prison and these early statements by Becker at the start of the economics of crime literature, we still know relatively little about the impact of other punishments, such as fines, and their relative trade-offs compared to prison.[2]

Just as was the case with incarceration, the two major challenges to analyzing the relative impacts of different punishments is a lack of rich data on prisoners and their outcomes and a source of plausible causal identification. In this paper we overcome both challenges by collecting and linking rich administrative data in Finland and using random assignment of judges to identify the causal impacts of both incarceration and fines. The data allows us to observe the criminal and labor market trajectories of every individual in Finland. Random assignment of judges has been used often in this literature, and as such is a well developed instrument to obtain causal identification.

[1] For example, consider Kling (2006), Di Tella and Schargrodsky (2013), Green and Winik (2010), Aizer and Doyle (2015), Mueller-Smith (2014), Dobbie *et al.* (2018a), and Bhuller *et al.* (2016), which we discuss in more detail below.

[2] The need for more evidence on additional punishments has not gone unnoticed in the policy world. A 2016 report to the president of the United States on incarceration and the criminal justice system stated "more research is needed to understand the impact of other criminal sanctions, including monetary sanctions and probation." (to the President of the United States (2016), pg. 38).

Our unique panel data set includes data we collected from every criminal court case in Finland from 1977-2015. We have collected information on judges from 2000-2015, so our main analysis focuses on these years.[3] We link the criminal and judge data to administrative tax, and school data. This allows us to look at a rich set of observable characteristics of all defendants, as well as a number of outcomes including labor market outcomes and future criminal activity. Information on judges, along with an institutional context that requires random assignment of judges to cases, allows us to use the judge instrument. This instrument was first developed in Kling (2006) and has also been used in Di Tella and Schargrodsky (2013), Green and Winik (2010), Aizer and Doyle (2015), Dobbie and Song (2015) and many others. We use these papers, and in particular Mueller-Smith (2014), Dobbie *et al.* (2018a), and Bhuller *et al.* (2016), as models to construct and evaluate our judge stringency measures. With this data set and instrument in hand, we are able to present evidence on the question of how best to use different punishments in order to address crime.

We find that being randomly assigned to prison (as opposed to receiving probation or a fine) decreases the number of future criminal charges, and has no impact on the probability of future prison sentences. These causal estimates are in marked contrast to the OLS estimates which suggest that prison strongly increases future criminal charges and also increases prison sentences. Our causal estimates indicate how important selection is in this context, even when including a rich set of controls. In terms of labor market outcomes, we find that prison leads to lower employment and lower earnings.

In contrast, we find almost the exact opposite with fines. Randomly assigning individuals to fines as a punishment (as opposed to probation or prison) leads to an increase in future criminal charges, although the result is only significant in the second year after sentencing. These results again are very different when compared to the OLS results which suggested that fines reduce future criminal charges. There is no significant impact of fines on prison sentences. The lack of impact on prison sentences could be evidence that while crime increased, there is not an escalation in criminal behavior. This is an important point to understand, so we suggest a proxy for severity of crime and estimate the impact of fines and prison sentences on this crime escalation variable

[3] Prior to 2000 the judge data was not digitized, which makes it much more costly to collect, so for this paper we focus only on 2000-2015.

[these results are in progress, we describe the variable in the text]. However, these somewhat negative outcomes in terms of criminal activity are accompanied by no significant impacts on labor market activity, as neither employment nor earnings are effected by fines (in contrast to the impacts of prison sentences).

In sum, our results suggest that while prison appears to be marginally more effective in terms of reducing crime, it comes at the cost of reducing labor market outcomes of defendants. It is not clear which outcomes policy makers should favor. While the marginal defendants are certainly not identical when estimating the causal impact of fines versus prison, we present evidence in the paper that there is overlap between marginal cases, which makes our conclusions particularly stark. In Huttunen *et al.* (2019), we also look at the spillovers on children and partners of fines and prison. We find that neither type of punishment appears to have significant spillovers on children, although we do find some evidence of negative spillovers on partners.

Our paper is most closely related to a recent literature that presents mixed results on the impact of incarceration on prisoners. Mueller-Smith (2014) finds large negative effects of incarceration in Texas, showing that incarceration increases future criminal activity and reduces labor market income of the marginal prisoner. In contrast, Bhuller *et al.* (2016) find positive impacts of incarceration on the labor market and future criminal outcomes of marginal prisoners in Norway, with this result driven almost entirely by men who were unemployed at the time of the crime. The results from Bhuller *et al.* (2016) suggest that prison might in some circumstances by rehabilitative. A third addition to this literature is Rose and Shev-Tov (2019) which uses a regression discontinuity design to estimate the impact of prison for defendants in North Carolina. They find that prison reduces crime post sentencing both via incapacitation effects and with smaller impacts post prison, although the benefits of longer sentences are outweighed by the costs of prison. Thus, the literature on the impacts of prison is not fully resolved, and our analysis adds directly to the understanding of the impacts of prison. In terms of the literature, our results lie in the middle of current findings, as we show that prison reduces future charges but also negatively impacts future labor market outcomes. In addition, we add depth to this discussion by explicitly looking at one of the alternative punishments that is generally in the broader counterfactual in these papers, i.e. fines. Separately estimating the impact of both fines and prison allows for a comparison between the two punishment types.

We are also related to a a much smaller literature that looks at the impact of other punishments. For example, Mello (2018) finds that small fines associated with speeding tickets have large impacts on financially fragile individuals, lowering their employment probability by 8%. Di Tella and Schargrodsky (2013) estimate the impact of electronic monitoring versus prison in Argentina and find that electronic monitoring has a negative effect on recidivism compared to prison.

The remainder of the paper is arranged as follows. In Section 1 we provide an overview of the institutional context. In Section 2 we describe the data, present descriptive statistics on crime in Finland, and show that the ladder approach to criminal punishments is salient. We discuss our empirical specification in Section 3 and report our main estimates in Section 4. Section 5 concludes.

1 Institutional context

1.1 The Finnish Court System

The Finnish general court system is divided into three levels: the district court, the regional appellate court, and the supreme court.[4] Our study focuses on criminal cases settled in district courts. During the time span of this paper, the number of courts has varied. In 2000 there were 66 district courts, but the number of district courts was reduced to 54 in 2007 and then to 27 in 2010.[5] Altogether there are around 500 judges working in the district courts, Approximately 100 of these judges work in the District court of Helsinki, which is the largest court.[6]

District courts are often divided into departments such that some deal with criminal matters and some with civil matters (see figure 1). The divisions are specified in the standing order of the

[4]In accordance with the constitution of Finland (https://www.finlex.fi/fi/laki/ajantasa/1999/19990731).

[5]The number of courts and locations are stated in the district court act (https://www.finlex.fi/fi/laki/ajantasa/kumotut/1993/19930581).

[6]Sources: "Statistics on workload in Courts during 2016" ("Tuomioistuinten tyotilastoja vuodelta 2016") and "Helsinki District courts annual report 2016".

court and is confirmed by a chief judge (*Laamanni*) who is the director of financial and administrative matters of the court.[7] Furthermore, each division may have a director who organizes work and ensures that the legal interpretations are uniform within the department.[8]

In addition to the posts of chief judge and department directors, there are two kinds of judicial positions in the district courts: district court judges and trainee judges. District court judges (*karajatuomari*) hold a degree in law and have life-time tenure. In addition to the law degree, a candidate must be a Finnish citizen and have demonstrated either through activities in the court or by some other way that she is adequately informed and possesses the necessary qualities. Judge appointments are made by the president of Finland on the proposition of the Finnish Government, assisted by a Judicial Appointment Board.[9] Trainee judges (*karajanotaari*) are law graduates who are performing a year long judicial apprenticeship. The tasks of the trainees depends on the phase of apprenticeship. At the beginning, trainee judges mostly handle civil matters, but at later stages they may be member of a panel of judges which handles criminal cases. However, the trainee judge cannot be the chairman of the panel in a criminal case where a possible maximum sentence is over two years.[10]

Finally, every district court has several non-judicial positions, and maintains a pool of lay judges (*lautamies*). Lay judges are politically appointed "assistant judges" who are part of the judge panel in some criminal cases. A lay judge must meet several requirements; for example, they must be at least 25 but not over 65 years old and cannot hold a position in a court or work as a prosecutor, police or lawyer.[11] Lay judges are paid based on judicial days, and they are entitled to reimbursement of costs incurred while performing their duties, as a compensation for lost earnings. According to the law, cases are allocated to lay judges based on the rotation principle

[7]Source: District court act 1993 https://www.finlex.fi/fi/laki/ajantasa/kumotut/1993/19930581.

[8]Source: District court act 1993 (https://www.finlex.fi/fi/laki/ajantasa/kumotut/1993/19930581). Note that before 2011, the district court act stipulated that a court may be divided into sections but did not mention anything about the directors of divisions.

[9]Legislation on judges has varied a bit during the time span of our study. The act on Judicial Appointments came into force in 2000 (https://www.finlex.fi/fi/laki/ajantasa/kumotut/2000/20000205) and before that the requirements for judges were set by the District court act. As a result of the changes, there may be considerable differences between old and young judges. For example, nowadays judges must have a higher degree in law, while before 2015 a lower level degree was enough.

[10]Source: District court act 1993 and Court act 2016.

[11]Before 2014, maximum age was 63.

which guarantees that the allocation is random.

1.2 Criminal proceedings

Figure 2 presents the structure of the criminal investigation process in Finland.[12] A criminal investigation may start in two ways: either the police receives a report that a crime has been committed or the authorities find out through surveillance that there is a reason to suspect a crime has taken place. Based on the information acquired from the report or surveillance, police then decide whether to start a preliminary investigation.[13]

After the police complete a preliminary investigation, the case moves to a prosecutor who must file charges when probable grounds exist to support the guilt of the suspect. There are a few reasons why not all of the cases result in a court trial. In some cases, a prosecutor does not bring charges on a procedural basis, for example, when the prosecutor considers that there is a lack of evidence. Also, the prosecutor may decide not to file charges when a crime is considered minor and the expected punishment is fines. Lastly, in offenses where a maximum sentence is six months of imprisonment, the prosecutor may use a penal proceeding and order a fine without a court trial. However, a penal order is possible only if a defendant has confessed the offense and the police have issued a request for a fine.[14]

If the prosecutor decides to bring charges, the case is moved to a court trial and randomly assigned to a judge or a panel of judges, who then decide whether the defendant is guilty or not and what is the sentence based on a court session. The composition of the panel of judges depends on the severity of the crime. A typical criminal case is dealt with by either one judge or a panel of one professional judge and 3-4 lay judges. The most severe cases are handled by a panel of three professional judges.[15] Notice that starting from October of 2006, it has been possible to

[12]Note that Figure 2 reports probability of each punishment type across all crimes in Finland, and does not include the restrictions we place on the sample we analyze (such as including cases from judges with at least 100 cases, courts with at least 2 judges, etc.) in this paper, so the proportion of punishments will not align perfectly with the descriptive results we report later in the draft.

[13]Sources: Criminal investigation act 1987 1:2 and 1:13, Criminal investigation act 2011 2:1 and 3:1.

[14]Source: Criminal procedure act 1997 (https://www.finlex.fi/fi/laki/ajantasa/1997/19970689) and Rikosoikeus (Criminal law) - Lappi-Seppala et al. (2016).

[15]Source: Code of Judicial Procedure 1734. Note that prior to 2014, the standard lay judge line up was one professional judge and three lay judges. However, the amendment which came into force in 01.05.2014, reduced the

settle minor confession cases through a written procedure with one judge and without a court trial. The written procedure can be applied if a maximum sentence for a given crime is 2 years, the defendant has confessed the crime and is willing to use the written procedure, and finally, a possible victim also agrees to a written procedure.[16] After the court session, the judge or the panel decides on the verdict and sentence. When the panel has a lay judge member, the professional judge first explains to the lay judges the essential questions in the case and what are the relevant points of law to be considered. If the panel cannot reach a unanimous decision, the verdict and sentence are decided by a vote. The voting proceeds as follows. First, the panel votes on the verdict. Then if the defendant is found guilty, a second vote is held to determine whether the convicted is punished. Finally, if the panel decides to give a sentence, the content of the sentence is decided by a vote. The professional judges always vote first and then lay judges vote in age order starting from the youngest. The side with the majority of votes wins. If the result is a tie, the least severe option from the point of view of the defendant is chosen regardless of which side the professional judge is on.[17]

1.2.1 Sentencing and punishments

In Finland, the criminal code defines a range of penalties for each crime. The principal punishments are fines, probation, and incarceration. For defendants under 18 years old, there is also a specific juvenile punishment. A prison sentence is only possible when it is indicated in the Finnish criminal code. Within theses ranges, only stated maximum punishments are binding, while lower limits are not compulsory. In principle this means that although the criminal code stipulates in some cases that the minimum punishment is a prison sentence, a judge may use discretion and impose only fines. In contrast, if the maximum sentence is fines, a judge cannot send the defendant to prison. The reason why the lower limits are flexible is to allow the court to actively prevent overly harsh penalties, with this goal taking precedence over preventing overly lenient punishments.[18] As we show in Figure 12 in the Appendix, it is very rare for defendants

number of lay judges to two.

[16]Source: Criminal Procedure Act 1997

[17]Sources: Code of Judicial Procedure 1734 and Criminal Procedure Act 1997.

[18]Source: Criminal code 1889 and Hinkkanen and Lappi seppala (2011).

to be incarcerated on their first offense, consistent with the ladder approach to crime. Generally, incarceration only occurs after multiple offenses. Fines are the most commonly assessed punishment in Finland. However, there is a great deal of overlap of punishments in certain crime codes, which we discuss in more detail in the next section.

Figure 1: Layout of Helsinki District Court

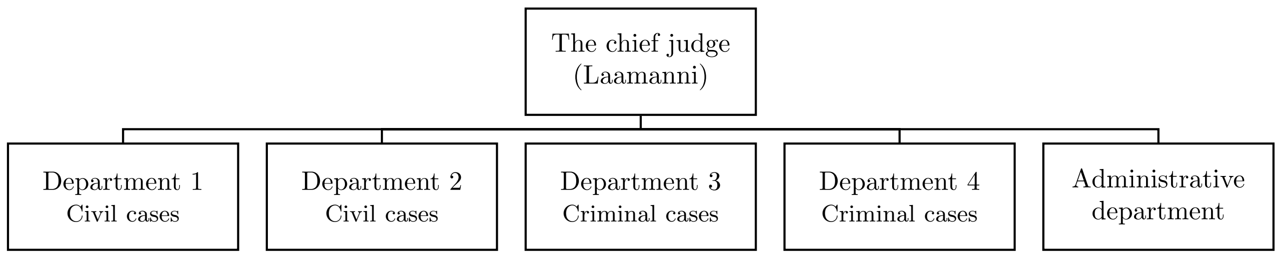
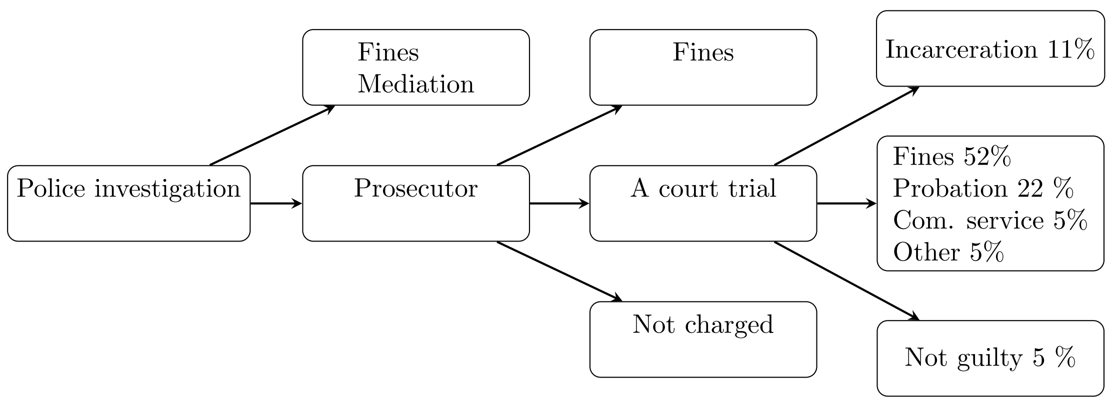


Figure 2: Sentencing Process in Finland



2 Data

We use administrative data from Finland. We obtained data on every crime committed above age 15 for every individual in Finland from 1977-2015. Variables of particular interest include the category of crime (six digit level), the date the crime was committed, the dates when the case entered the court, the court decision date, and the sentence imposed by the judge. Note that it is possible for one case to include multiple crimes. When describing types of crimes, we will use the designated primary crime from the records. The crime data initially lacked information on judges, so we coordinated with the court register to collect the data on every judge assigned to every criminal case in Finland. This data is only available electronically from 2000-2015, so we focus on these dates for our main analysis.[19]

We link the crime data to the registry data which includes basic demographic variables, income, labor market activity, and school completion for every defendant. We also have data on the GPA at age 16 and military records at age 18 which include non-cognitive test scores as well as cognitive test scores (only for men) [military data is in progress].

To better understand the Finnish context, in Figure 3 we present data on criminal activity and the use of punishments in Finland. In the top graph of Figure 3, we plot the total number of court cases involving defendants each year by principal crime category from 2000-2015.[20][21] In the bottom panel we repeat the exercise but restrict to defendants who received a fine (the figure on the left) and defendants who received a prison sentence (figure on the right). These figures show that although Finland is a small country, we have a large number of cases each year. The majority of cases are property or violent crimes. The number of prison sentences has gone down over time, reflecting Finland's push toward more lenient sentences. This push has been a long term and concerted policy endeavor, as in the 1960s Finland was an outlier in terms of the frequency of prison sentences, even compared to the United States. This long term push has been largely successful, and during the period we study in this paper Finland has similar incarceration rates per capita as its European neighbors.

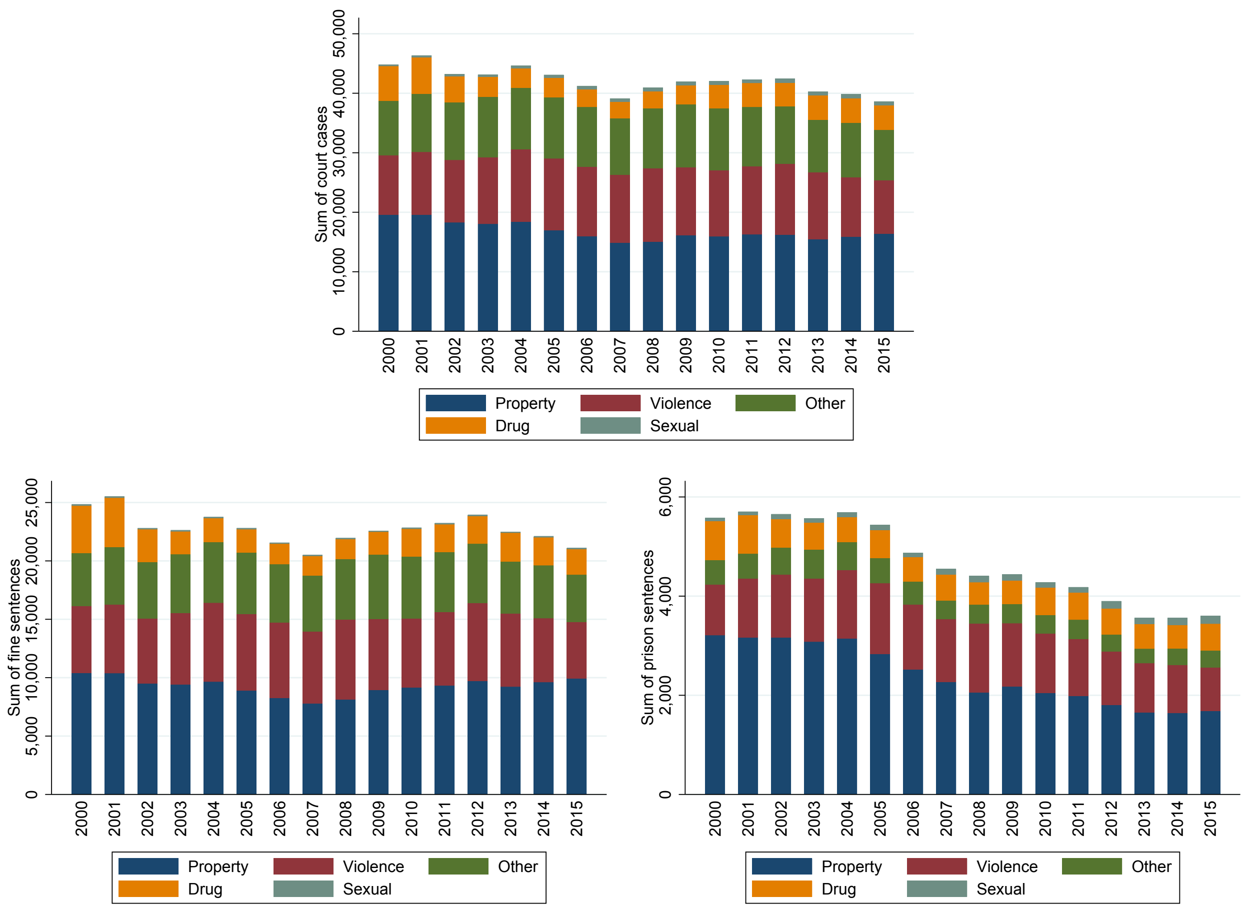
Note that just over 90% of prison sentences in this period are below a year and the average sentence length is 188 days, or approximately 6 months. These sentence lengths are consistent with other European countries, but are shorter than sentences in the U.S., an outlier where the average sentence length is 2.9 years (see Aebi *et al.* (2015) and Bhuller *et al.* (2016)). Certain categories of fines are pegged to the defendant's income. This progressive nature of fines is an

[19]The data is available in hard copy prior to 2000. Due to cost constraints, we have focused on collecting and linking the 2000-2015 data on judges.

[20]We restrict to 2000-2015 because this is our sample of analysis for the paper, based on availability of digitized judge data. For completeness, we also include a figure documenting crime and prison sentences from 1977-2015 in the appendix.

[21]In the case of multiple crimes for a given court case, the court designates a primary crime, and that crime in general is most closely linked with the court ruling.

Figure 3: Court cases, prison, and fines over time in Finland



Note: The top panel plots court cases for all defendants from 2000-2015. The bottom left panel plots court cases that result in a fine while the right panel plots court cases that result in prison.

interesting topic of analysis, but is not the focus of this paper so we do not explicitly model the progressive nature of the fine schedule. So long as defendants are randomly assigned, this does not impact our main analysis. In Section 3.1, we present evidence that suggests this assumption holds.

In Table 1 we present descriptive statistics for all defendants in Finland from 2000-2015. In the first column we report statistics for all individuals who appear in court, and in the second two columns we present statistics for our two relevant subsamples: those who appear in court and receive a fine, and those who appear in court and receive a prison sentence. All means are taken at the time of the court case unless otherwise specified.

From the table we can see that defendants who end up in prison are clearly worse off at the time of sentencing compared to the entire sample. Those who receive fines, on the other hand, appear to be positively selected from the population of defendants. This is consistent with the ladder approach to crime with earlier and less severe criminals receiving lighter sentences such as fines, while more severe cases receive harsher punishments like prison. These results also indicate substantial selection in terms of those who commit crimes that are sent to prison, which is why it is important to go beyond simple OLS and identify the causal impact of different punishments; as we will show in what follows, this changes our estimates of some of the impacts of fines and prison dramatically.

2.1 The Ladder of Punishments

Before presenting evidence on the causal impacts of different punishments, we present evidence in this section that the ladder approach to punishments is salient. We start with Figure 4. On the x-axis of this figure are the crime codes, ordered by the percent of cases in each crime that are sent to prison. On the y-axis is the share of each crime code that receives a specific type of punishment. The points are weighted by the number of crimes committed during our period in each crime code - punishment type. The left hand figure shows all crime codes, while the figure on the right hand side zooms in on the right side of the figure on the left. As can be seen in the figure, punishments with the lowest proportion of prison sentences instead experience

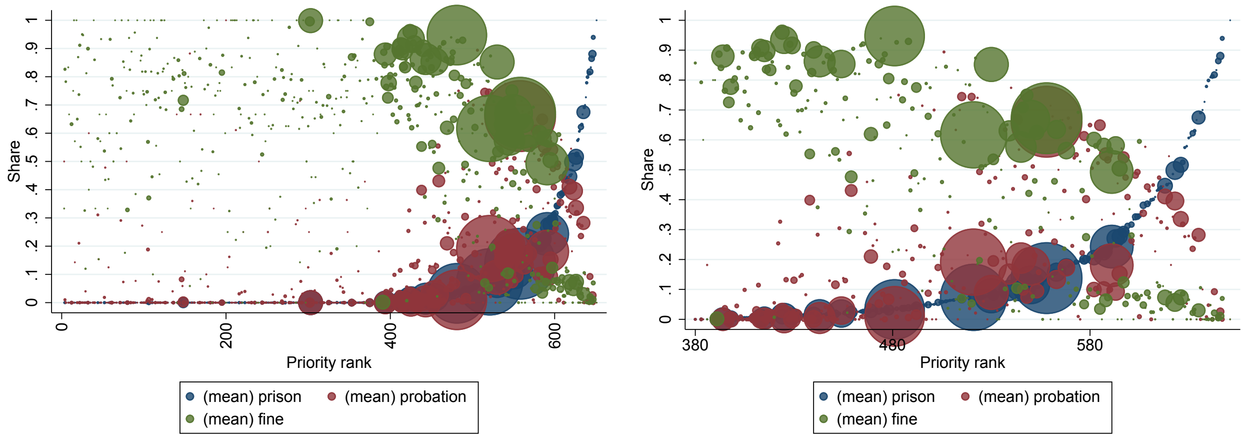
Table 1: Descriptive statistics

	Full Court Sample		Sub-samples			
			Fined		Incarcerated	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Defendant Characteristics						
Age	36.74	10.56	36.93	10.71	33.94	8.761
Income in Euors	13865.3	17335.9	15072.7	17098.3	5580.0	9123.9
Employed	0.440	0.496	0.491	0.500	0.157	0.364
Secondary degree	0.393	0.488	0.422	0.494	0.267	0.442
Tertiary degree	0.0965	0.295	0.103	0.304	0.0211	0.144
Marital status	0.233	0.422	0.234	0.423	0.154	0.361
Number of children	1.826	1.184	1.839	1.168	1.708	1.002
Court incident t-1	0.346	0.476	0.279	0.449	0.714	0.452
Court incident t-2, t-3	0.446	0.497	0.375	0.484	0.846	0.361
Prison sentence t-1	0.117	0.322	0.0614	0.240	0.467	0.499
Prison sentence t-2, t-3	0.152	0.359	0.0869	0.282	0.562	0.496
Observations	220677		106082		35671	

a high proportion of fines. As the use of prison increases, the use of fines decreases. As fines decrease, the proportion of each crime type that are assigned to probation increase. Finally, the use of probation decreases as the use of prison continues to increase. The takeaway from these figures is that just as the Finnish crime codes describes, lower level crimes start with fines as punishment and then as the crime becomes more severe, punishments move next to probation and last to prison. However, as we can also see from the figure, most crime codes receive all three punishment types to some degree. This is important as it means that the counterfactual for fines may not always be probation. If a defendant receives a particularly harsh judge compared to a particularly lenient judge, he may receive a prison sentence as opposed to a fine punishment.

While the ladder of sentencing is interesting in the whole population, it may be that fines are only used for defendants who will only commit a few crimes and so are not relevant for serial criminals. Using punishments to optimally reduce the activity of serial criminals is particularly important given that serial criminals commit the vast majority of all crimes in Finland. If serial criminals start by immediately committing more serious crimes that always result in either probation or prison our analysis of fines and their impacts on later criminal activity and labor market outcomes would be much less relevant. In light of these questions, in Figure 5 we focus only on

Figure 4: Ladder of punishments - Serial criminals

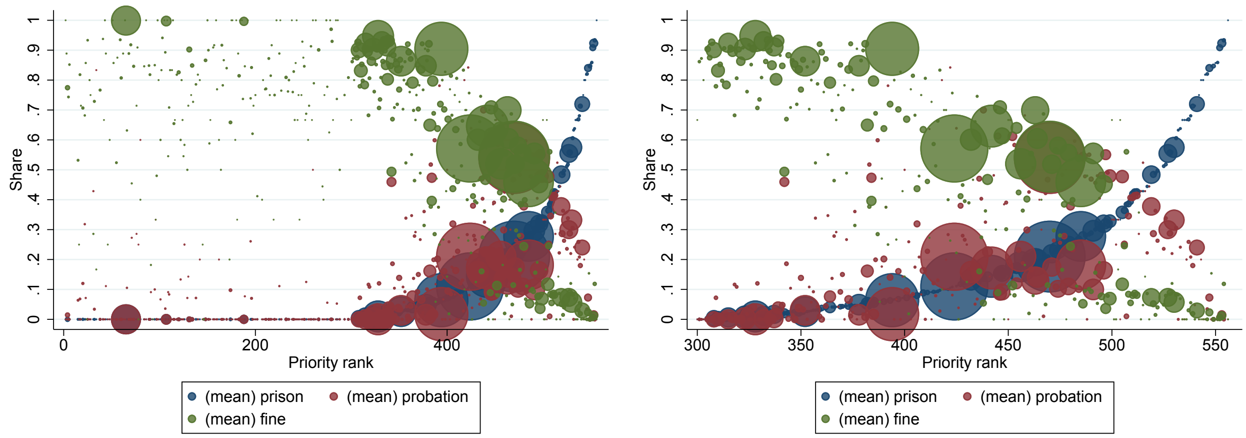


Note: The figures plot cases according to the crime code (x-axis) and share of crime code in each punishment type (y-axis). Each point is weighted by the number of cases. The crime code is in order of share of crime code sent to prison. In the left panel are all cases, the right panel zooms in on the right hand side of the left figure.

serial criminals, i.e. defendants who commit more than 3 crimes (results are similar for different cutoffs). The figures show that serial criminals also appear to commit a number of lower level crimes as well as more severe crimes. The results demonstrate that lower level punishments are not only relevant overall, but may also be important stepping stones for future serial criminals. Individuals who go on to commit multiple crimes do not generally start off at serious crimes that are likely sent to prison. Instead, they begin their criminal careers with minimal crimes and lower level punishments. As such, understanding the efficacy of early punishments, specifically fines, also sheds light on how to prevent potential serial criminals from continuing their criminal activity.

These figures also suggest an outcome we will look at in addition to number of crimes, probability of going to prison, employment, and earnings. Specifically, as in these figures, we calculate the percent of each crime code that is sent to prison. This serves as a proxy for how severe each 6 digit crime code is. In Section 4 we will estimate the impact of fines and probation on the severity of crime measured in this way to measure whether there is crime escalation in response to prison and/or fines [these estimates are in progress].

Figure 5: Ladder of punishments - Serial criminals



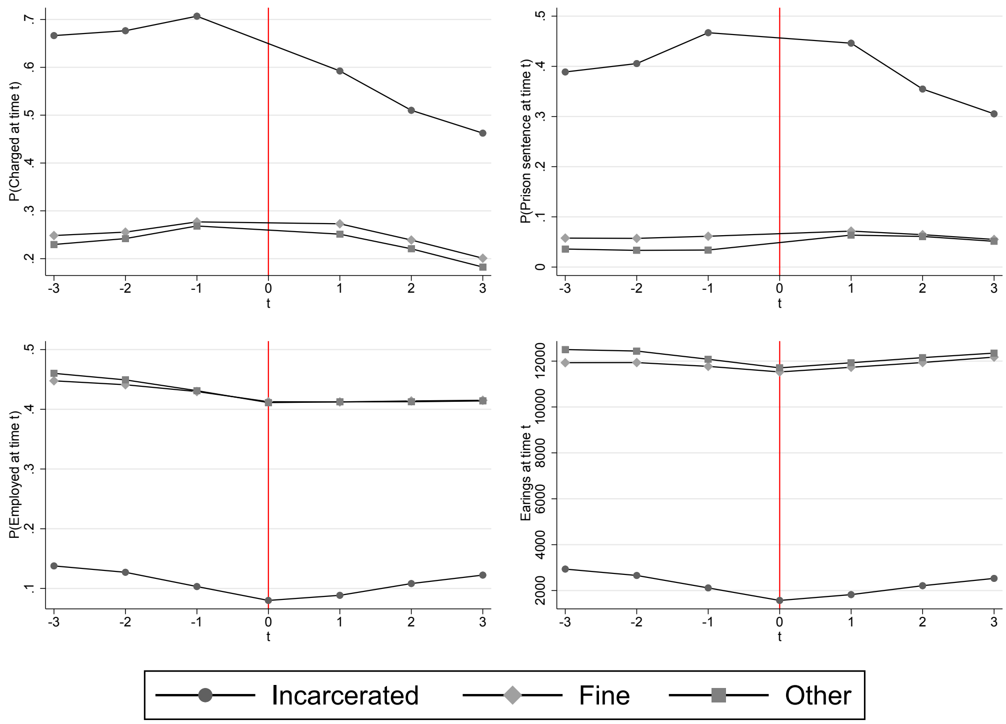
Note: These figures restrict the sample to individuals who commit more than 3 crimes in their lifetime. The figures plot cases according to the crime code (x-axis) and share of crime code in each punishment type (y-axis). Each point is weighted by the number of cases. The crime code is in order of share of crime code sent to prison. In the left panel are all cases, the right panel zooms in on the right hand side of the left figure.

2.2 Descriptive Results

Next we present some descriptive results on the impacts of fines and prison on defendant outcomes via event study graphs in Figure 6, where 0 indicates the date of sentencing. The darkest line with circles represents defendants who are sentenced to prison, and the lightest grey line with triangles represents those who are sentenced to fines. The figures clearly show the selection of defendants who are sentenced to prison. Even before sentencing, these defendants are much less likely to have a job, have lower earnings, and have more charges and previous prison sentences.

The graphs also show strikingly different patterns in what happens after sentencing. Charges drop after every type of punishment, as shown in the top left graph, but most dramatically for those who are sent to prison. In terms of future prison sentences, there is no discernible change at time of sentencing for fines and other punishments, but a decrease in probability of prison for those who are punished with a prison sentence. These descriptive results suggest a potentially positive impact of prison sentences on later criminal activity. Last, in terms of change to labor market activities after sentencing, we see that while there does not appear to be much of a pattern for those who are fined or given some other sentence, those who are given a prison sentence do

Figure 6: Charges, Prison, Employment, and Earnings Before and After Sentencing



Note: Includes all cases from 2000-2015, except those that are excluded from the main analysis as described in 3 and captured in Table 9. The darkest line represents cases that are sentenced to prison, the lightest gray line represents those who are sentenced to fines and the middle grey line represents those sentenced to other punishments (primarily probation).

seem to have a slight discontinuity moving toward marginally higher employment and earnings after prison.

Of course, all of these results are merely descriptive. There is clearly significant selection in terms of who receives which type of punishment, which makes it difficult to interpret these event studies. In light of these issues, we next turn to our identification strategy to get at the causal impacts of prison and fines.

3 Empirical specification

To identify the casual effect of fines and prison on defendant outcomes we estimate the following two-equation system for punishment P where P stands for either fine or prison.

$$Y_{ict} = \beta_0 + \beta_1 P_{ict} + \beta_2 \boldsymbol{X}_{ict} + \varepsilon_{ict} \tag{1}$$

$$P_{ict} = \alpha_0 + \alpha_1 Z_{ij} + \alpha_2 \boldsymbol{X}_{ict} + \epsilon_{ict} \tag{2}$$

Y_{icft} is the outcome for defendant i who had a court case c in year t. P_{ict} is a dummy variable equal to one if the defendant i has a given punishment (either fine or prison sentence) associated with his court case c in year t. $\boldsymbol{X}_{ict}$ is a vector of case and defendant control variables (including court by year by crime type fixed effects) and ϵ_{ict} is the error term. OLS estimates of β_1 will be biased if unobserved characteristics of the defendant are correlated with receiving a given sentence. Recall that the descriptive statistics presented in Table 1 suggest selection that could lead to such bias in the OLS estimates.

To address this issue we use random assignment of cases to judges within courts to create exogenous variation in probability of prison or fine sentences which is captured via the instrument Z_{ij}, the leave out residualized incarceration rate for each judge. We calculate Z_{ij} using a similar approach to Dobbie *et al.* (2018b):

$$\begin{aligned} P^*_{ict} &= P_{ict} - \kappa \boldsymbol{X}_{ct} \\ Z_{ic} &= \left(\frac{1}{n_j - n_{ij}}\right)\left(\sum_{k=0}^{n_j} P^*_{ik} - \sum_{c=0}^{n_{ij}} P^*_{ic}\right), \end{aligned}$$

where $\kappa \boldsymbol{X}_{ct}$ represents court-by-year-by-crime fixed effects. In the first equation, we remove the court by year by crime type fixed effects to obtain P^*_{ict}. In the second equation we take the average of this residual incarceration or fine proclivity, but for each defendant we remove the defendant's own cases from the average incarceration or fine rate to create the leave out mean residual incarceration or fine rate for each defendant.

This strategy works if judges vary in their sentencing severity, and the assignment of fathers to judges is not correlated with unobserved characteristics of fathers or their children associated with both likelihood of incarceration and child outcomes. Under the principal of randomization of cases to judges within year, court, and crime type[22], which is a legal requirement in Finland, the latter condition should be met, although we also provide evidence supporting this exclusion restriction in the next subsection.

As in Bhuller *et al.* (2016), to construct our judge stringency instrument we restrict our sample of judges to those for whom we observe at least 100 randomly assigned cases between the years 2000-2015. We also restrict the judges to those for whom we observe at least two judges in the same court. In Appendix Table 9, we show how each of these restrictions decreases the number of judges, courts, and defendants in our sample.[23]

While it would be interesting to be able to look separately at probation as a punishment, this measure is not straightforward in the same way as fines and prison. Judges in Finland are supposed to start by giving fines, then move on to probation, and then move on to prison. Thus, a judge who has a high fine stringency measure will tend to be a more lenient judge, with judge leniency decreasing as the judge's fine stringency measure decreases and we move to judges who tend to give more severe punishments such as probation or prison. Similarly, a judge with a high prison stringency measure will be a stricter judge, and judges will grow more lenient as the associated prison stringency measure decreases, representing the fact that more lenient judges give fewer prison sentences conditional on court by year by crime type fixed effects. We show that this leads to a strong negative correlation between calculated judge fine stringency and judge prison stringency in Figure 13. In contrast, judges with a low probation stringency will include both more lenient judges who are more likely to punish defendants with fines and stricter judges

[22]Note that we can use either 2 digit or 6 digit crime type codes and the results are similar. We also checked that there is a large number of cases within each cell and found this to be the case.

[23]Note that we also drop all traffic cases in the current draft. Traffic cases have some non-random assignment which we are currently working with the court registrar to address. We also drop judges in training, as these judges are not given full slates of cases and so would also violate randomization. In a very small minority of cases where the defendent's first language is Swedish, the defendant is required by law to have access to a Swedish speaking judge. This will also violate random assignment so we drop these cases as well. Last, we require the defendant's age to be above 22 as younger defendants are treated differently. With the exception of traffic cases, the amount of the other categories of cases is negligible.

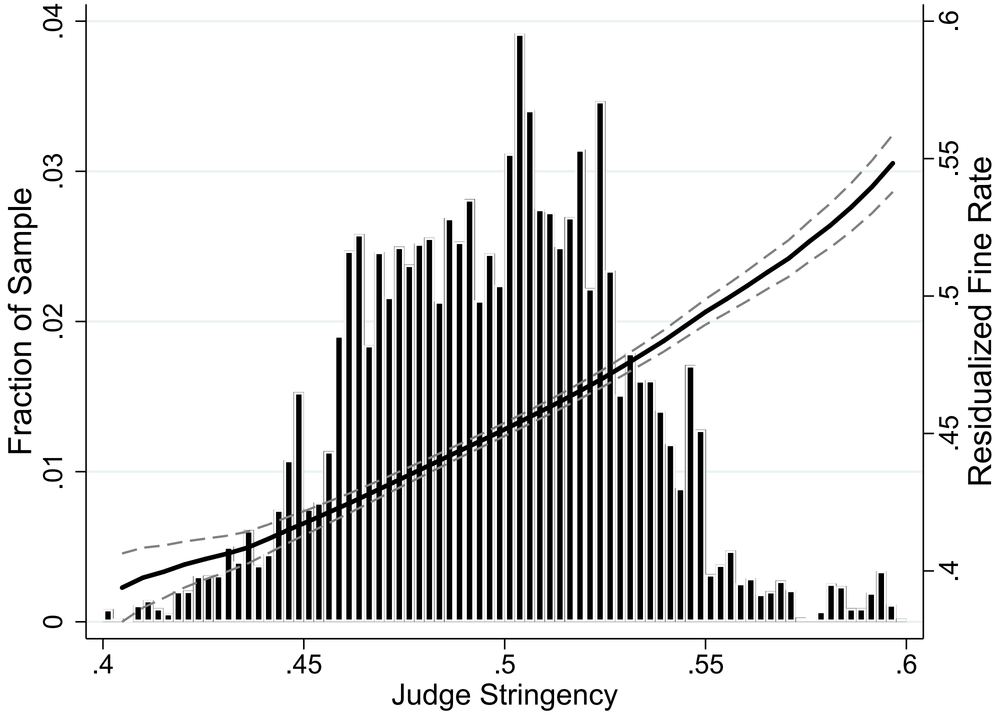
who are more likely to punish defendants with prison. Thus, the leniency of the judge is no longer monotonically related to the probation stringency measure, making it difficult to interpret the impact of probation stringency in the same way as with fines and prison.

Our prison stringency instrument can be interpreted in much the same way as the rest of the literature, i.e. the effect of being randomly assigned a harsher judge who is more likely to punish the defendant to prison relative to the counterfactual punishments of fine or probation. Our fine stringency instrument can be interpreted similarly, as the effect of being randomly assigned a more lenient judge who is more likely to punish the defendant with fines as opposed to the counterfactual harsher (at least by law) punishments of probation or prison. As we showed in Subsection 2.1, there are crime categories where all three punishments are used, so we cannot assume that the counterfactual to fines is always probation, the next step on the ladder. For the same crime code, a very lenient judge might give a defendant a fine, a middle of the road judge might give a defendant probation, and a harsh judge might give a defendant prison.

3.1 Judge instrument

We start by reporting the standard judge stringency graph for fines in Figure 7 and incarceration in Figure 8. The figures show that there is substantial variation in judge stringency in both punishments. The fitted line suggests that there is a strong first stage - as the judge stringency increases, the residualized fine and incarceration rates also increase. We also report the first stage estimates from equation 2 separately for fines and prison in Table 2. The coefficients are all large and significant. In Panel A we report the estimates without controls, and then add demographic controls in Panel B. If our instrument is valid, we would not expect to see the addition of demographic controls to significantly change our estimates, so the estimates in Panels A and B should be similar. This is indeed what we find. In terms of the first stage estimates, we find that being assigned to a judge who is 10 percentage points more likely to fine leads to an increase in the probability of receiving a fine of approximately 9.2 percentage points. In terms of incarceration, we find that being assigned to a judge who is 10 percentage points more likely to incarcerate leads to an increase in the probability of incarceration of approximately 5.5 percentage points.

Figure 7: Judge stringency variation - Fines



Both of these estimates suggest that the instrument is relatively strong in predicting the type of punishment of interest.

Having established that the instrument has sufficient variation and a strong first stage, we now turn to tests of the validity of the instrument. Beyond the institutional characteristics of the Finnish court system that support the exclusion restriction, we also report balance test results in Table 3. In column (2) we report the estimates from a regression of defendant characteristics on judge stringency for fines. We find that none of the coefficients are significant, and the joint test for significance has an F test statistic of 1.152 and a p-value of 0.314. Thus, defendants do appear to be randomly assigned to judges. The balance test passes despite the fact that these characteristics are highly correlated with fines, as shown in column (1). Every single variable is significantly associated with incarceration and the p-value is zero. The same is true when we turn to prison. Again, none of the characteristics are significant in predicting the judge stringency for prison, with the joint test for significance having an F test statistic of 0.359 with a p-value of 0.977. Again, this is despite the fact that these characteristics are highly correlated with whether

Figure 8: Judge stringency variation - Prison

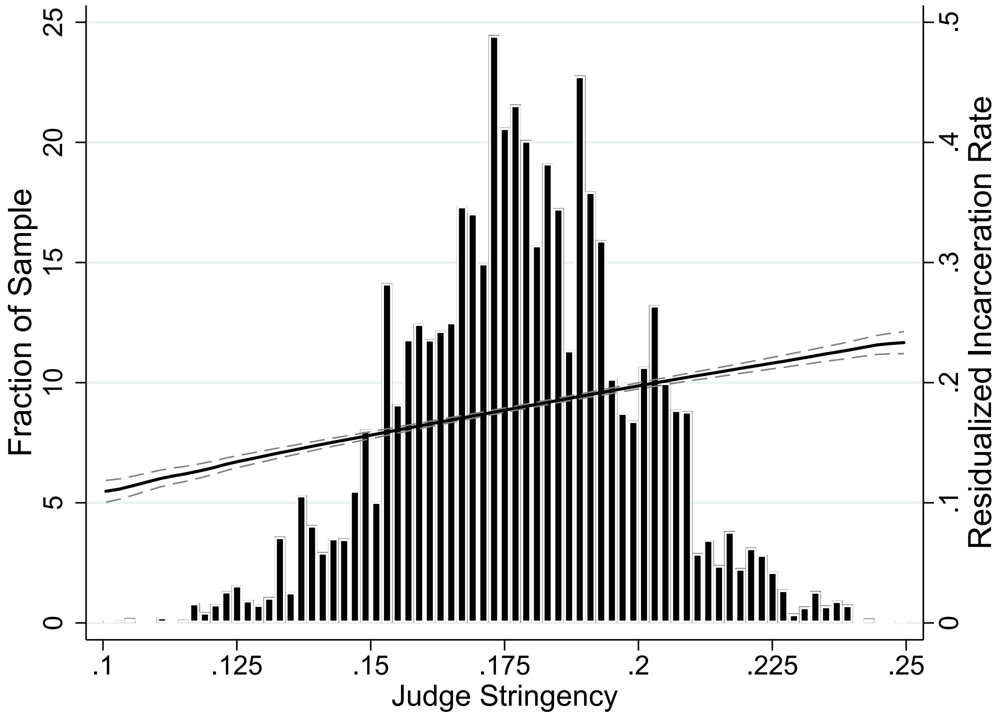


Table 2: First stage

Dependant Variable:	Pr(Fined) (1)	Pr(Prison) (2)
A. Court by Year by Crime type fixed effects		
Judge stringency	0.946***	0.551***
	(0.0633)	(0.0543)
F-stat	209	102.9
B. Add controls for demographics		
Judge stringency	0.943***	0.538***
	(0.0650)	(0.0487)
F-stat	540.7	1424.9
Dependent mean	0.489	0.171
N	220,677	220,677

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients in parenthesis. *p<0.1, **p<0.05, ***p<0.01

the defendant receives a prison sentence, as shown in column (3).

In Table 4, we show the trade-offs made by judges when deciding on punishments. The table reports the estimates from equation (2) where the outcome of interest is each of three different punishments: fines, probation, and prison. We find that being randomly assigned a judge with higher fine stringency (i.e. a more lenient judge) is associated with a decrease in the probability that a defendant receives probation or prison (we also repeat the first stage increase in fines estimates for completeness). Being randomly assigned a judge with a higher prison stringency measure (i.e. a harsher judge) is associated with a decrease in the probability that the defendant receives fines, but an increase in the probability the defendant receives probation. The impact of fine stringency on prison and the impact of prison stringency on fines are both straightforward and consistent with predictions of the model, i.e. more lenient judges should be more likely to give fines and stricter judges more likely to give prison. The effect on probation is less straightforward and at first glance the negative impact of prison stringency on probation may appear counterintuitive - if, as we show in the first stage estimates reported in Table 2, higher prison stringency results in higher probability of prison, shouldn't this also coincide with fewer probation cases? The problem with this naive prediction is as follows. We can think of the prison stringency measure as a proxy for stricter judges. If all cases involved a decision between probation and prison, then we would expect that stricter judges would tend to choose prison, resulting in a negative coefficient on probation. However, as shown in Figure 2, the majority of cases end in fines. The problem is that we observe all cases but cannot distinguish between cases where no judge would give prison, and instead judges are deliberating between fines and probation versus cases where the judge is deliberating between probation and prison. Given the preponderance of fines, it is likely true that in most cases the judge is deciding between fines and probation, not between probation and prison. When deciding between fines and probation, we would expect stricter judges to tend to choose probation. Thus, if the majority of marginal cases are cases between fines and probation (as opposed to probation and prison) we would expect to see a positive association between prison stringency and probation and a negative association between fine stringency and probation, which is precisely what we find in column (2). In sum, the results in

Table 3: Balance tests

	Pr(fined)	Judge Fined	Pr(Prison)	Judge Prison
	(1)	(2)	(3)	(4)
Demographics (1 year before sentencing)				
Age	-0.0008***	0.0000	-0.0003***	0.0000
	(0.00013)	(0.0000)	(0.00008)	(0.0000)
Kids	-0.0078***	-0.0001	-0.0083***	0.0000
	(0.0013)	(0.0000)	(0.0006)	(0.0000)
Married	- 0.0322***	0.0000	0.0073***	0.0000
	(0.0030)	(0.0001)	(0.0017)	(0.0001)
Secondary	0.0148***	-0.0002	-0.0163***	0.0000
	(0.0023)	(0.0001)	(0.0016)	(0.0000)
College	-0.0146**	-0.0004	-0.0185***	-0.0001
	(0.0045)	(0.0002)	(0.0021)	(0.0001)
Employed	0.0268***	-0.0000	-0.0353***	0.0000
	(0.0029)	(0.0001)	(0.0018)	(0.0001)
Income	0.0000***	0.0000	-0.0000***	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native born	0.0043	0.0001	0.0186***	-0.0000
	(0.0056)	(0.0003)	(0.0029)	(0.0002)
Past Criminal History				
Ever incarcerated, t-1	-0.116***	-0.0001	0.280***	0.0000
	(0.0044)	(0.0002)	(0.0046)	(0.0001)
Ever charged, t-1	-0.0476***	-0.0003	-0.0445	0.0000
	(0.0030)	(0.0001)	(0.0021)	(0.0001)
Ever incarcerated, t-3 to t-2	-0.0466***	0.0001	0.0538***	0.0000
	(0.0040)	(0.0002)	(0.0044)	(0.001)
Ever charged, t-3 to t-2	-0.0466***	0.0001	0.0538	0.0000
	(0.0029)	(0.0001)	(0.0018)	(0.0000)
F Test	543.3	1.152	1623.3	0.359
P Value	0.000	0.314	0.000	0.977
N	220,677	220,677	220,677	220,677

Note. All estimations include controls for court by court entry year by crime type fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors are below the coefficients. *p<0.1, **p<0.05, ***p<0.01

Table 4 show that a) judges are trading off between different punishment types, b) reinforce the challenges with looking at probation separately, which is why we focus on the easier to interpret punishments of fines and prison, and c) make it clear that the counterfactual for fines is both probation and prison and the counterfactual for prison is both fines and probation.

Table 4: Impact on other Punishments

Dep. variable	Fines	Probation	Prison
	(1)	(2)	(3)
IV: Fines	0.946***	-0.382***	-0.252***
(No controls)	(0.063)	(0.027)	(0.025)
IV: Incarceration	-0.525***	0.261***	0.551***
(No controls)	(0.054)	(0.043)	(0.054)
Dep. mean	0.489	0.230	0.171
Number of cases	220677	220677	220677

Multi-dimensional sentencing and monotonicity assumptions

As discussed in Mueller-Smith (2014), two important assumptions that should always be checked in these settings are: no multidimensional sentencing (to avoid violating the exclusion restriction) and monotonicity. The former assumption is a legal requirement in Finland. Judges in Finland are only allowed to assign a single punishment. However, to confirm that this legal condition holds in practice, we also checked across our sample and found no cases of multidimensional sentencing.[24] Of course, the judge might impact the defendant in ways other than punishment, for example a more lenient judge might also be kinder when speaking to defendants. We do not observe anything about the judge behavior aside from the punishment, but our hypothesis is that such violations, if they occur, do not strongly impact defendants.

To check if our instrument is consistent with the monotonicity assumption, we take a similar

[24]There is one separate category of punishment, probation and fines, which is considered a more severe punishment than probation alone in the context of the criminal code. For the purposes of the current paper we categorize these cases as probation sentences.

approach as in Bhuller *et al.* (2016), and do two things. First, we show that the first stage is similarly strong and positive across a number of sub samples. Next, we perform a "reverse sample instrument test". Specifically, for a series of different variables we take a subset of the sample to construct the judge instrument, and then estimate the first stage using the other part of the sample (which was not used to construct the judge instrument). We present these results in the Appendix for prison[25] and the tests for fines are in progress. Thus far, none of the results indicate a violation of monotonicity which is reassuring. Note, though, that if we had encountered violations, Mueller-Smith (2014) suggests useful solutions to both of these problems. Given that we do not have any obvious issues, we now proceed to the main results.

4 Main results

Criminal Activity. We start with the results on charges. In Table 5 we present the impact of fines (in the top panel) and prison (in the bottom panel) on whether the defendant is charged with another crime. OLS results suggest that fines decrease the number of subsequent charges while prison sentences are associated with an increase in subsequent charges. However, when we turn to the IV estimates we find the opposite: fines cause a small increase in criminal charges after sentencing, and the effect is significant at the 10% level 2 years after the sentence. The opposite is true for prison. In the IV the sign flips and we find that prison causes a decrease in the number of charges after sentencing. This result is significant in the second year after charging. These results suggest that the OLS evidence is misleading and likely due to selection - prison appears to cause a decrease in charges while fines cause an increse in charges.

In Table 6 we repeat the same exercise but now with the outcome of whether the defendant is sent to prison in the following three years. While OLS results again suggest that punishing defendants with fines decreases the probability of future prison sentences and punishing defendants with a prison sentence increases the probability of later prison sentences, these results go away in the IV, where we find no significant impact of either punishment on future prison sentences in the three years following the sentence. Last, we report the impact of prison and fines on crime

[25]Note that these are older results, updated results are in progress.

escalation. Recall that this variable is the percent of each crime code that is sent to prison, which we argue is a good proxy for severity of crime in Section 2.1. [These results are in progress.]

Overall, our IV results suggest that prison is better at lowering criminal activity, primarily by reducing the number of charges, when compared to fines. We turn now to the impacts of these different punishments on labor market outcomes.

Labor Market Outcomes. In Table 7 we report the impact of fines (top half of the table) and prison (bottom half of the table) on whether the defendant was employed in the three years following the sentence. The OLS estimates suggest that fines increases the probability of employment while prison decreases the probability of employment, even when including a rich set of controls. However, the IV results suggest no impact of fines on employment, although the point estimates are marginally negative in the second and third year after sentencing, and large negative impacts of prison in the first and third year after sentencing. Next, in Table 8 we report the impacts on the defendant's earnings in the three years after the punishment. We find no statistically or economically significant effect of fines on wages. For prison, however, we find large negative point estimates, and the effect is significant in the year after prison. These results suggest that prison is worse for defendants in terms of later labor market activity.

Table 5: Impact on Charges

Dep. variable	Pr(Charged)		
	1 year after	2 years after	3 years after
	(1)	(2)	(3)
OLS: Fines	-0.088***	-0.075***	-0.078***
No controls	(0.002)	(0.002)	(0.002)
OLS: Fines	-0.008***	-0.005**	-0.010***
Controls	(0.002)	(0.002)	(0.002)
IV: Fines	0.0773	0.0919*	0.0393
No controls	(0.0399)	(0.0385)	(0.0371)
OLS: Incarceration	0.326***	0.277***	0.282***
No controls	(0.003)	(0.003)	(0.003)
OLS: Incarceration	0.053***	0.031***	0.052***
Controls	(0.003)	(0.003)	(0.003)
IV: Incarceration	-0.164	-0.241*	-0.081
No controls	(0.106)	(0.104)	(0.0968)
Dep. mean	0.319	0.278	0.254
Number of cases	205,602	205,602	205,602

Table 6: Impact on Prison

Dep. variable	Pr(Prison)		
	1 year after (1)	2 years after (2)	3 years after (3)
OLS: Fines	-0.114***	-0.090***	-0.082***
No controls	(0.001)	(0.001)	(0.001)
OLS: Fines	-0.093***	-0.073***	-0.067***
Controls	(0.001)	(0.001)	(0.001)
IV: Fines	0.014	0.008	-0.006
No controls	(0.029)	(0.027)	(0.026)
OLS: Incarceration	0.377***	0.292***	0.265***
No controls	(0.002)	(0.002)	(0.002)
OLS: Incarceration	0.324***	0.244***	0.222***
Controls	(0.002)	(0.002)	(0.002)
IV: Incarceration	-0.006	-0.037	-0.045
No controls	(0.074)	(0.070)	(0.067)
Dep. mean	0.131	0.112	0.101
Number of cases	205,602	205,602	205,602

Table 7: Impact on Employment

Dep. variable	Pr(Employed)		
	1 year after (1)	2 years after (2)	3 years after (3)
OLS: Fines	0.102***	0.097***	0.092 ***
No controls	(0.002)	(0.002)	(0.002)
OLS: Fines	0.038***	0.036***	0.033***
Controls	(0.002)	(0.002)	(0.002)
IV: Fines	0.002	-0.042	-0.026
No controls	(0.036)	(0.036)	(0.036)
OLS: Incarceration	-0.322***	-0.305***	-0.292***
No controls	(0.003)	(0.003)	(0.003)
OLS: Incarceration	-0.109***	-0.101***	-0.096
Controls	(0.002)	(0.002)	(0.002)
IV: Incarceration	-0.178	-0.038	-0.121
No controls	(0.095)	(0.099)	(0.098)
Dep. mean	0.360	0.364	0.368
Number of cases	217,250	213,966	210,664

Table 8: Impact on Earnings

Dep. variable	Earnings		
	1 year after (1)	2 years after (2)	3 years after (3)
OLS: Fines	3024.83***	2937.44***	2919.73***
No controls	(82.14)	(87.65)	(87.63)
OLS: Fines	700.57***	654.93***	659.72***
Controls	(58.44)	(66.32)	(67.23)
IV: Fines	-49.21	-693.36	-86.05
No controls	(1420.42)	(1522.21)	(1524.81)
OLS: Incarceration	-9918***	-9755***	-9670***
No controls	(109.78)	(117.62)	(117.78)
OLS: Incarceration	-1305.73 ***	-1296.97***	-1397.95***
Controls	(82.75)	(94.11)	(95.56)
IV: Incarceration	-8643.89*	-6009.12	-4132.46
No controls	(3801.20)	(4118.85)	(4107.93)
Dep. mean	10204.80	10455.15	10695.28
Number of cases	217,250	217,250	217,250

4.1 Complier Analysis

As Bhuller *et al.* (2016), Dobbie *et al.* (2018a), Dobbie *et al.* (2018b), and Abadie (2003) discuss, while we cannot identify specific compliers in the data set, it is possible to extend the judge fixed effects analysis in order to analyze the set of compliers in the data. In this subsection we do this by splitting our data into subsamples, using the subsamples to calculate probability of incarceration or fines, and then using these estimates to calculate compliance weights for each subsample. The intuition is that a subset with a stronger first stage relative to other subsets contains more compliers. We report the results for Fines in Figure 9 and for prison in Figure 10. The results show that for fines, compliers are not very strongly selected, but do appear to be less likely to commit violent crimes, more likely to commit property crimes, and more likely to be married. For prison, those without a degree and with previous charges are more likely to be compliers while those who are employed and accused of violent crimes are less likely to be compliers.

Figure 9: Complier Weights - Fines

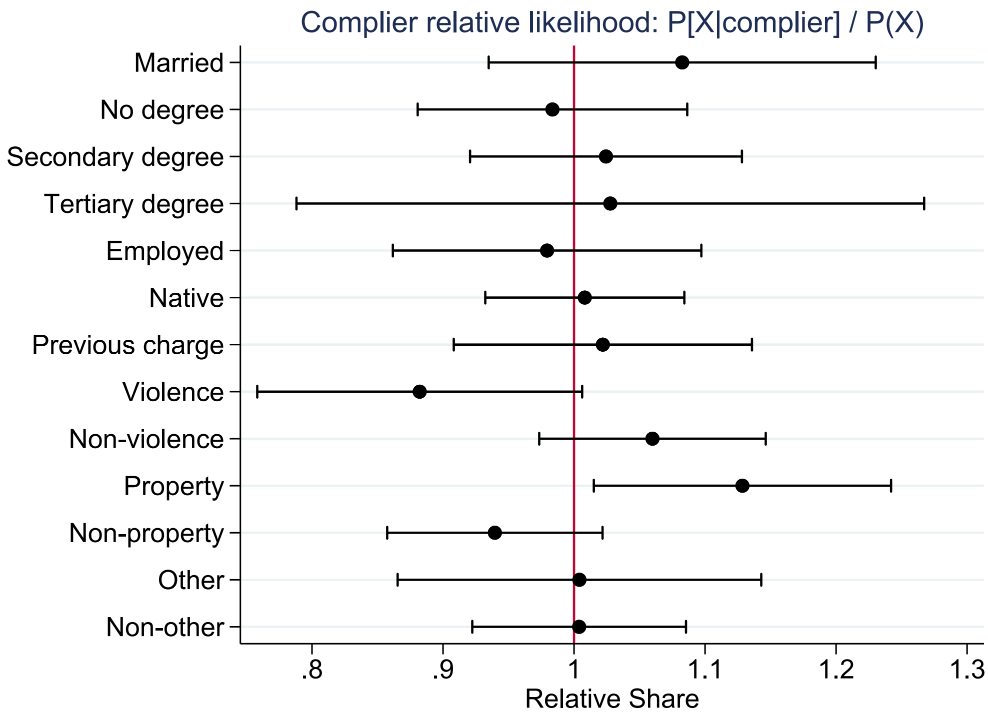
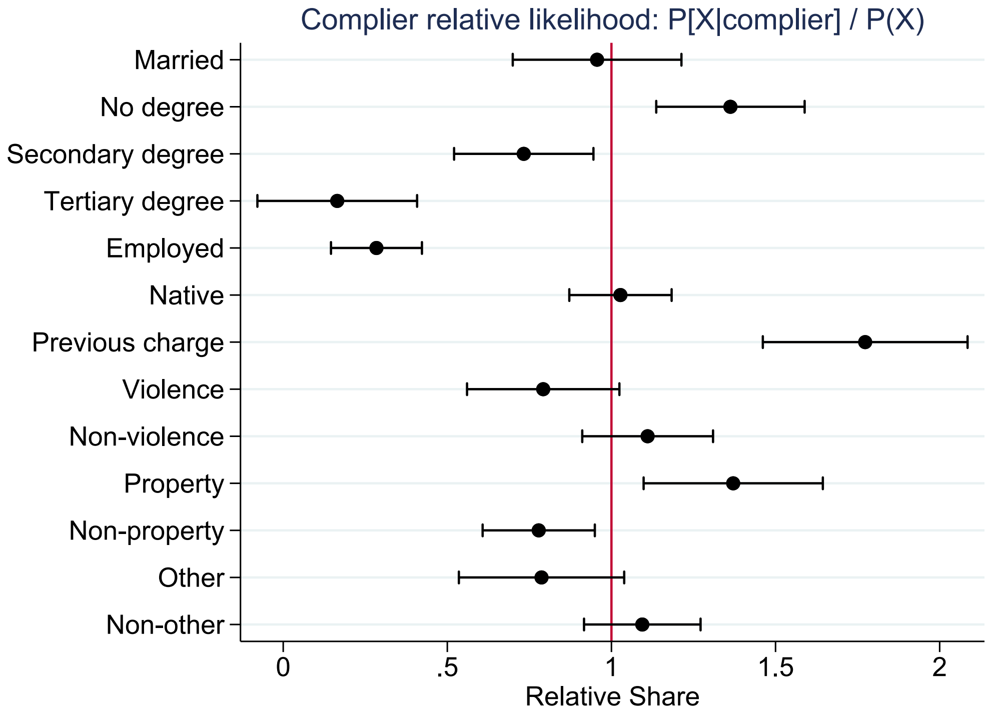


Figure 10: Complier Weights - Prison



We can also use the weights calculated in the previous step to re-estimate a complier weighted OLS. This re-weighted sample uses the random variation in assignment of judges to get closer to the causal impact of fines and incarceration on defendant outcomes, in a similar way to the IV results. [These estimates are still in progress.]

5 Conclusion

In this paper, we have shown that while sentencing defendants to prison lowers the number of future charges, it also lowers future labor market outcomes of defendants. In contrast, sentencing defendants to fines increases future criminal activity, although it does not appear to escalate criminal activity by leading to more prison sentences. In addition, fines do not have the negative impacts on later labor market outcomes of defendants that prison does.

Our results suggest that the original Becker suggestion, to use fines whenever possible, is the right approach if one wants to minimize labor market impacts to defendants, but it is not the right approach in order to minimize criminal activity. Note, however, that these mixed results are drawn from the Finnish context. Finland is a country that uses fines much more frequently than most other countries, which means that the marginal criminal case assigned to fines in Finland is likely a much more sever criminal case than marginal cases assigned to fines in other contexts. The fact that we still do not find dramatic differences in the criminal activity of defendants assigned to fines versus prison in this context, where prison is much more rarely used than fines, suggests that there may be scope for greater use of fines without sacrificing decreases in future criminal activity, in other contexts, and additional evidence on this point would be useful. We also point out that the impacts of fines on future criminal activity are not particularly severe, so even in Finland the criminal justice system could probably use fines even more frequently without large negative consequences.

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A Appendix

Figure 11: Court and prison cases in Finland 1977-2015

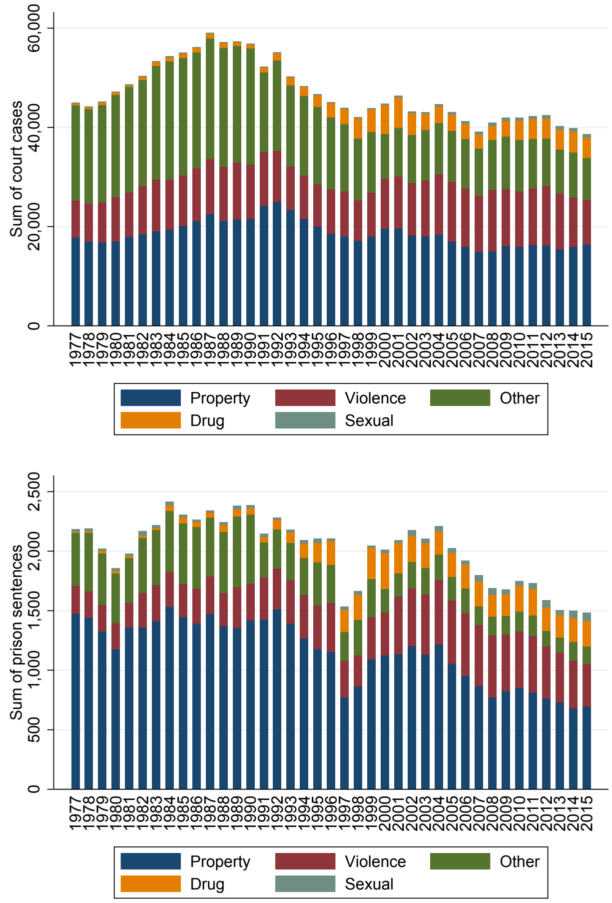

Figure 12: Court and prison cases in Finland 1977-2015

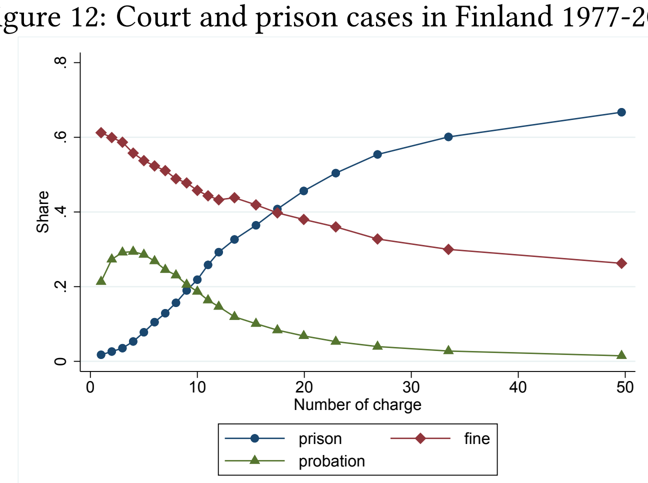

Figure 13: Judge stringency variation - Correlation between Fines and Prison

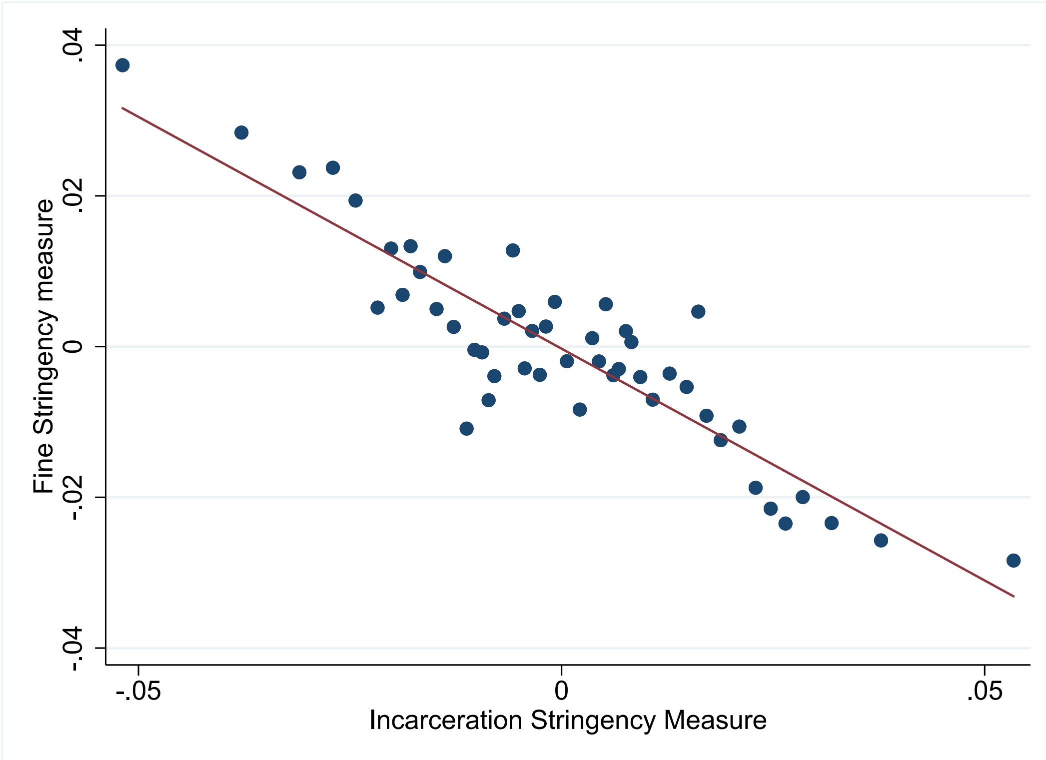

Table 9: Sample restrictions for judges from 2000-2015

	Sample size after each restriction (in each row)			
	A. Judge Stringency Panel			
Number of	Cases	Defendants	Judges	Courts
No restrictions	388829	202408	3361	65
Drop training judges	304326	168882	1035	65
Swedish speaking	296245	163688	1034	65
Drop judges < 100 over career	282135	157644	680	65
Drop courts <2 judges	282119	157637	680	65
	B. Panel of Analysis for cases decided between 2000-2013			
Number of	Cases	Defendants	Judges	Courts
Analysis data	220677	126760	668	65

Sub-sample:	Baseline instrument First stage P(Incarcerated)	Reverse-sample Instrument First stage P(Incarcerated)
Any post compulsory education		
Estimate	0.389	0.331
(se)	(0.064)	(0.051)
Observations	53993	56488
No post compulsory education		
Estimate	0.581	0.598
(se)	(0.079)	(0.078)
Observations	69731	72886
Previously Employed		
Estimate	0.150	0.116
(se)	(0.052)	(0.041)
Observations	48096	50297
Previously non-Employed		
Estimate	0.729	0.456
(se)	(0.080)	(0.107)
Observations	75703	79134
Married		
Estimate	0.379	0.431
(se)	(0.089)	(0.083)
Observations	41074	42825
Not married		
Estimate	0.610	0.391
(se)	(0.064)	(0.046)
Observations	82913	86820
Over 30 years old		
Estimate	0.497	0.411
(se)	(0.006)	(0.057)
Observations	80863	84386
Less than 30 years old		
Estimate	0.667	0.555
(se)	(0.095)	(0.077)
Observations	42953	45094
Violence crimes		
Estimate	0.363	0.285
(se)	(0.075)	(0.062)
Observations	45637	47779
Property crimes		
Estimate	0.563	0.489
(se)	(0.0986)	(0.099)
Observations	43298	45138
Other crimes		
Estimate	0.398	0.422
(se)	(0.099)	(0.100)
Observations	24074	25351