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

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Abstract

Most criminal justice systems use a "ladder of punishments" that starts with less severe punishments and progresses to more severe punishments according to crime severity and criminal history. Using random assignment to judges, we estimate causal impacts of three common punishments on the ladder–fines, probation, and prison–on defendants' criminal and labor market outcomes. We find that fines increase recidivism. However, this increase is concentrated among those committing less severe crimes. Probation decreases recidivism for those committing less severe crimes and first offenders. Neither fines nor probation affect earnings. Prison has a mixed impact, decreasing future charges but also decreasing earnings.

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Every country in the world must decide how to punish those who break the law. In their response to criminal activity, most criminal justice systems use a "ladder of punishments", which starts with less severe punishments (e.g., fines), and gradually progresses to more severe punishments (e.g. probation or incarceration) for defendants who commit more crimes or more severe crimes (Lappi-Seppälä, 2016; Hinkkanen and Lappi-Seppälä, 2011). Knowing the effects on defendants of different types of punishments is vital in order to determine how to implement such a ladder of punishments to reduce recidivism and promote rehabilitation. While several papers have examined the effectiveness of the highest rung on the ladder[1], prison sentences (e.g., Kling, 2006; Green and Winik, 2010; Aizer and Doyle, 2015; Eren and Mocan, 2017; Bhuller *et al.*, 2020), we know relatively little about the effectiveness of less severe punishments. Understanding their impact is particularly important given that prison is the punishment of last resort in most countries and makes up a minority of punishments in almost all European countries.

In this paper, we estimate the impacts of the ladder's three most common types of punishments–fines, probation, and incarceration–on defendants' future criminal and labor market outcomes.[2] This is challenging for three reasons. First, rich data on criminal defendants and their outcomes is required. Second, observed and unobserved characteristics of defendants may be correlated with both the punishment type and the defendants' outcomes. Without a source of exogenous variation in assigned punishment, estimates may be biased. Third, all three punishments must be used frequently enough to estimate causal impacts. We overcome these challenges and present the first evidence on the impact of all three punishments in one context. We collected data on every criminal court case and associated judge in Finland from 2000 to 2015, and we link the criminal and judge data to

[1]Of course, in a few countries there is yet a more severe punishment, the death penalty. We omit this punishment in this paper both because it is not applicable in our setting and because it is not applicable in many other countries.

[2]We do not provide a theoretical model in the paper, but such a model can be found in Doleac (2019). She presents a model of how punishments might affect future criminal behavior, focusing on those who have already committed at least one crime.

administrative data. This allows us to look at a rich set of observable characteristics and outcomes. We identify the causal effects of each punishment by using the fact that cases are randomly assigned to judges and judges vary in their likelihood to sentence defendants to fines, probation, or prison. We measure each judge's fine, probation, and prison stringency and use these measures as instruments to examine the criminal and labor market effects of these respective punishments. We show that these stringency measures are highly predictive of a defendant's receipt of a given punishment, but not correlated with defendant characteristics. Finland is a particularly good context for this analysis since all three punishments are used extensively there.

We present three main sets of results. In the first set of results, we present descriptive evidence that the ladder of punishments approach to crime is salient. Punishments grow harsher, moving from fines to probation to prison, as defendants commit either more severe crimes or a greater number of crimes.

In the second set of results, we estimate the effect of fines, probation, and prison on defendant outcomes. First, we focus on the least severe punishment, fines. Using the judge instrument we find that sentencing defendants to a fine leads to a significant increase in the probability of future criminal charges in the first few years after sentencing. These causal estimates are the opposite of the ordinary least squares (OLS) regression results, which suggest that fines are associated with a smaller probability of future criminal charges. In addition, while a given punishment's impact on the likelihood of any future charges is a standard outcome of interest when considering sentencing, the punishment's impact on the severity of future crimes is also of interest. We thus propose a new measure to capture crime severity, which could easily be used in other contexts. Using this measure, we find that fines increase the likelihood not only of future charges, but also of increased severity of future crimes. These negative outcomes in terms of criminal activity shortly after sentencing are accompanied by no significant impacts on labor market activity, as neither employment nor earnings are significantly effected by fines.

Second, we focus on the most severe punishment, prison sentences. We find that sentencing defendants to prison substantially decreases the number of future criminal charges a defendant commits, although the effects are concentrated in the first few years after sentencing, which could be consistent with the incapacitation effect of prison. In addition to the decrease in short-term future criminal charges, we find a decrease in the severity of future crimes. These causal estimates are again in marked contrast to the OLS estimates which suggest that prison is associated with large increases in future criminal charges, even in the short run. In terms of labor market outcomes, we find that prison has little impact on employment, but leads to substantially lower earnings. The IV point estimates are either of similar magnitudes or more negative than the OLS estimates, suggesting that the impact of prison on labor market outcomes is likely more negative than naive OLS would suggest.

Third and last, turning to the middle of the ladder, we find no significant effects of probation on criminal outcomes, in contrast to OLS. We additionally find that the negative and significant impacts of probation on earnings and employment suggested by OLS estimates are no longer significant when we turn to the IV estimates. Thus, the aggregate analysis suggests that probation is not particularly effective as a crime-reducing punishment, but that it also does not increase the probability of future crimes. These main estimates already expand upon the rich literature estimating the impact of prison on defendant outcomes, which we discuss in more detail below.

However, the analysis that pools all defendants together may hide important heterogeneity which is partly due to different counterfactuals. For example, for probation defendants will have committed both less severe and more severe crimes, and may also be first-time offenders or recidivists. For less severe crimes and first-time offenders, the counterfactual punishment is more likely to be fines, while for more severe crimes and recidivists, the counterfactual punishment is more likely to be prison. Thus, the causal effect of probation in the main analysis captures the impact of probation relative to a weighted average of multiple different punishments. These effects may go in opposite directions, resulting in a zero aggregate effect.

Hence, to get a fuller picture of the effect of each punishment, in the third set of results we restrict our analysis to subsamples in which the number of counterfactual punishments is likely to be smaller than in the total sample. We estimate the heterogeneous impact of fines, probation, and prison for less severe crimes versus more severe crimes, and on first-time offenders versus recidivists. We find that the net neutral effect of probation in the main results masks important heterogeneity. Probation causes substantial reductions in future charges for low-severity crimes and for first-time offenders. Additionally, we find that the criminogenic effect of fines in terms of future charges is only true for less severe crimes, and is stronger for recidivists when compared to the effect of receiving a fine for a first crime. The charge-reducing effects of prison are significant for those committing more severe crimes. The point estimates still suggest reductions in charges due to prison for less severe crimes, but are generally not significant.

Together, these results imply that there are distinct and complex trade-offs when setting the thresholds for each punishment type. The thresholds policy makers ultimately choose will depend on how they value reducing the number of future charges, severity of future crimes, and labor market outcomes of defendants. Additionally, our results show that considering how those punishment thresholds should differ in conjunction with the severity or number of crimes a defendant has committed is an important exercise. There is not a "one size fits all crimes" when it comes to the choice of punishment.

Our paper contributes to a large literature focused on the final rung on the ladder of punishments, estimating the impact of incarceration on defendant outcomes at both the extensive and intensive margins (e.g., Kling, 2006; Owens, 2009; Green and Winik, 2010; Kuziemko, 2013; Barbarino and Mastrobuoni, 2014; Aizer and Doyle, 2015; Eren and Mocan, 2017; Harding *et al.*, 2018; Dobbie *et al.*, 2018a). The papers find mixed results. Most closely related to our paper which focuses on the extensive margin of punishments for adult men, Mueller-Smith (2014) finds large negative effects of incarceration in Texas, showing that incarceration increases future criminal activity and reduces labor market outcomes of the marginal prisoner. In contrast, the results from Bhuller *et al.* (2020) suggest that prison may be rehabilitative in some circumstances. They show that in Norway, incarceration decreases future criminal charges, especially among defendants who were not working before the sentence. Furthermore, prison improves labor market outcomes of these previously non-employed defendants. As these papers demonstrate, the literature on the impacts of prison is not fully resolved. Our results lie in the middle of current findings, as we show that prison reduces future charges but also negatively impacts future earnings.

While we know quite a bit about the impact of prison on defendants from these papers, it is infrequently used in many countries relative to other punishments.[3] Thus, much more information is needed on other punishments, and the lack of existing evidence has been noted by policy makers. For example, a 2016 report to the president of the United States on the criminal justice system stated that "more research is needed to understand the impact of other criminal sanctions [besides prison], including monetary sanctions and probation." (Executive Office to the President of the United States (2016), pg. 38). In this paper, we address this important gap in the evidence.

As such, this paper is also related to a much smaller literature that looks at the impact of other punishments on defendant outcomes. A few important recent papers have looked at the impact of financial penalties on speeding. For example, Goncalves and Mello (2017) find that receiving a larger speeding ticket decreases the probability of speeding in the future. Mello (2018) finds that small fines associated with speeding tickets have large impacts on financially fragile individuals. Hansen (2015) looks at the impact of harsher penalties in response to drunk driving using a regression discontinuity design, with harsher penalties including larger fines, but also prison as a possible outcome. He finds that harsher penalties reduce recidivism.

A few other papers have looked at probation and electronic monitoring. Mueller-Smith

[3]See Section 1 for a detailed discussion.

and Schnepel (2020) look at the impact of diversion in the United States, which consists of both a probationary period and also the omission of a formal criminal conviction. Using a regression discontinuity design, they find that diversion increases the defendants' employment rates and reduces recidivism. Rose (2020) shows that elimination of prison as a punishment for technical violations while on probation in North Carolina led to lower future incarceration probabilities but also increased re-offending. Arguably most closely related to our paper, Di Tella and Schargrodsky (2013) estimate the impact of electronic monitoring versus prison in Argentina using random assignment to judges (as is our strategy) and find that electronic monitoring reduces recidivism by 11 to 16 percentage points when compared to prison. We add to this literature by estimating the causal impacts of all three punishments (fines, probation, and prison) on the population of defendants in a single setting.[4] We additionally show there is important heterogeneity in the impact of punishments depending on the severity of the crime and whether the defendant is a first time offender as opposed to a recidivist.

The remainder of the paper is arranged as follows: Section 1 provides an overview of the institutional context and describes the data. Section 2 presents and discusses descriptive results showing that the ladder approach to criminal punishments is salient. Section 3 reviews our empirical specification, and Section 4 reports our main estimates. Section 5 presents heterogeneous effects by severity of crime and number of previous crimes, and Section 6 concludes.

[4] Bhuller *et al.* (2020) consider multiple punishments, but primarily as a potential violation of the exclusion restriction in their setting when estimating the effect of prison on defendants. When describing this exercise, the authors state that "the key challenge to instrument exogeneity is that trial decisions are multidimensional, with the judge deciding on incarceration, fines, community service, probation, and guilt. In Section 5.5, we examine this threat to the exclusion restriction, showing that our estimates do not change appreciably when we augment our baseline model to either control for judge stringency in other dimensions or include an instrument for other trial sentencing decisions."

1 Institutional Context and Data

1.1 Institutional Context

To better understand the Finnish context, Figure 1 presents the structure of criminal investigations and trial outcomes in Finland.[5] A criminal investigation starts in one of two ways: either the police receive a report that a crime has been committed or the authorities find out through surveillance that there is reason to suspect a crime has taken place. If warranted by the information acquired from the report or surveillance, the police start a preliminary investigation.[6]

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. In this paper we focus only on cases that result in a court trial, since these are the cases for which we have data and causal identification.[7]

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. A court session is held, and the judge(s) then decide whether the defendant is guilty or not, and if the defendant is found guilty what the sentence should be. Random assignment to judges is a longstanding institutional feature that has also been legally codified into the constitution of Finland.[8] We use this fact in our analysis, but later in the paper also provide supportive evidence consistent with the institutional description of random assignment of judges to cases.

[5]Note that Figure 1 reports the probability of each punishment type across all crimes in Finland, and does not include the restrictions we place on the sample we analyze in Section 3 (e.g., standard study restrictions such as requiring judges to see a minimum number of cases and that courts have at least 2 judges to randomize across).

[6]See the Criminal Investigation Act of 1987 1:2 and 1:13, and the Criminal Investigation Act of 2011 2:1 and 3:1.

[7]In offenses where a maximum sentence is six months of imprisonment and the defendant has confessed, the prosecutor may use a penal proceeding and order a fine without a trial. However, a penal order is possible only if the police have issued a request for a fine according to the Criminal Procedure Act of 1997 (https://www.finlex.fi/fi/laki/ajantasa/1997/19970689) and Rikosoikeus (Criminal law)

[8]We have also confirmed the random assignment of cases to judges with multiple officials in the Finnish judicial system.

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 two to four lay judges.[9] The most severe cases are handled by a panel of three professional judges. When assigning judge stringency to a defendant's case, we assess either the professional judge or, in the few cases where there are multiple professional judges, the primary judge listed in the data provided by the court registrar.[10] After the court session, the judge or panel decides on the verdict and sentence. When the panel includes lay judges, the professional judge first explains to the lay judges the essential questions in the case and 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.[11][12]

In Finland, the criminal code defines a range of possible penalties for each crime. The principal punishments are fines, probation, and incarceration, which as Figure 1 shows comprise 85% of all punishments. For this reason, as well as the wide external validity of these three punishments to many other contexts, we focus on these punishments in this paper. For defendants under 18 years of age, there are separate instructions. Because younger defendants are treated differently, we do not include them in this paper. A prison sentence is only possible when it is indicated in the Finnish criminal code. Within sentencing

[9]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 (before 2014 the maximum age was 63) and cannot hold a position in a court or work as a prosecutor, or lawyer, or for the police. This rule is according to the Code of Judicial Procedure of 1734. Note that prior to 2014, the standard panel using lay judges was composed of one professional judge and three lay judges. However, the amendment which came into force on January 5, 2014, reduced the number of lay judges to two.

[10]Note that since October of 2006 it has been possible to settle very minor criminal cases through a written procedure with one judge without a court trial. The written procedure can be applied if the maximum sentence for a given crime is 2 years, the defendant has confessed to the crime and is willing to use the written procedure, and any possible victim(s) also agree to the written procedure (see the Criminal Procedure Act of 1997). These cases are also included in our main analysis, although the results are robust if we exclude them.

[11]The voting proceeds as follows: First, the panel votes on the verdict. Next, if the defendant is found guilty, a second vote is held to determine whether the convicted is to be 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 the 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.

[12]See the Code of Judicial Procedure 1734 and the Criminal Procedure Act of 1997.

ranges, only the stated maximum punishments are binding. 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 probation or fines. In contrast, if the maximum sentence is fines, a judge cannot send the defendant to prison.[13]

In Online Appendix A we provide detailed descriptions for each punishment type analyzed in this paper. However, some important things to note are the following: First, Finland uses a day fine system in which the monetary value of the fine punishment is a product of the severity of an offense and the offender's income. There is no upper bound for a day fine unit, but the minimum is 6 euros. If the defendant defaults on the fine, he can be sent to prison. As we will show in the next section, these day fines are used quite broadly, even for more severe crimes. Second, just over 90% of prison sentences in this period are below a year, and the average sentence length is 188 days. 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; Bhuller *et al.*, 2020).

The distribution of punishments in Finland is broadly similar to that of a number of other developed countries. For example, the share incarcerated among those who were convicted is similar to England (7%) France (17%) and Germany (5%).[14] What these statistics demonstrate is that prison is used infrequently in many countries. In the United States where prison is used relatively more frequently,[15] if policy makers wish to reduce the rate of incarceration then other types of punishments will have to be used much more. These

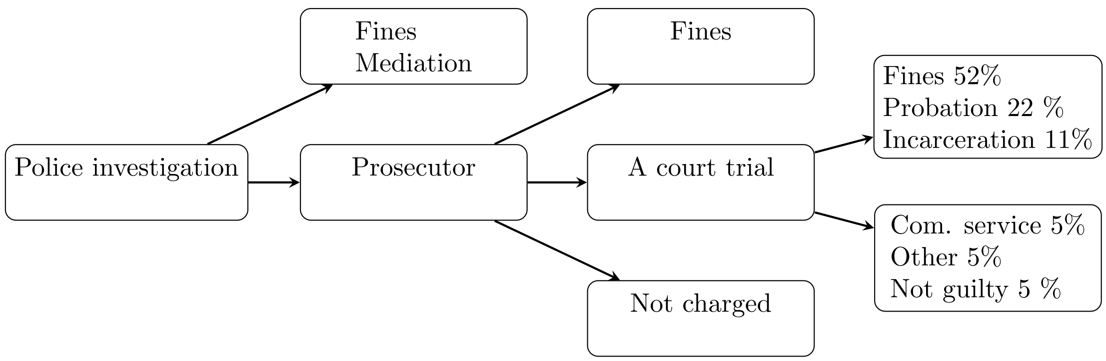
[13]The reason why the lower limits are flexible is to allow the court to actively prevent overly harsh penalties, with this goal taking precedence over preventing overly lenient punishments. For information, see the Criminal Code of 1889 and Hinkkanen and Lappi-Seppälä (2011).

[14]We aggregated these numbers based on a number of different sources. For England, we obtained data on punishments from the statistics of the Ministry of Justice; for France from the website of the Ministry of Justice; for Germany from the Federal Ministry of Justice and Consumer Protection.

[15]However, it is likely the case that even in the United States, prison is not the majority punishment. While we were unable to find good aggregate statistics on the share of defendants sent to prison, Rose (2020) states, "less attention has been paid to the impact of probation, the most common way criminals are punished in the United States."

facts underscore why it is so important to extend previous analysis of the effect of prison on defendant outcomes to the effect of other punishments on these outcomes, as we do in this paper.

Figure 1: Sentencing Process and Trial Outcomes in Finland



Notes: The figure describes each stage of sentencing in Finland. On the far right, we report the percentage of each punishment given for the entire population of court cases.

1.2 Data

Using administrative data from Finland, we obtained data on each decision given in Finnish district courts from 1977 to 2015. Variables of particular interest include the category of crime, the date it was committed, 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 use the designated primary crime from the records.[16] The crime data we initially obtained lacked information on judges, so we coordinated with the court registrar to collect data on every judge assigned to every criminal case in Finland. This data is only available electronically from 2000 to 2015, so we focus on these dates for our main analy-

[16]This is generally the most severe crime.

sis.[17][18] We link the crime data to the registry data which include every defendant's basic demographic characteristics such as income, labor market activity, and school completion. Since at the time of our analysis the registry data were only available until up to 2015, and our analysis required that we follow individuals for 5 years post sentencing, we restricted our sample to those sentenced between 2000 and 2010, although we use the entire period (2000 to 2015) when constructing the judge stringency variable.

Similarly to previous papers using judge fixed effects as an instrument, we must make some restrictions to the data to ensure the random assignment of cases to judges. We restrict the sample to cases assigned to judges who try cases in courts with at least two active judges (within whom randomization can occur). We make a few additional restrictions to our sample that that are specific to our setting, again to respect random assignment. First, in a very small minority of cases where the defendant's first language is Swedish, he is required by law to have access to a Swedish speaking judge.[19] This can violate random assignment in courts that only have one active Swedish judge, so we drop these cases. Second, similarly to Dobbie *et al.* (2018b) and others we omit all traffic cases as these are sometimes not subject to randomization. Third, we drop juvenile defendants as they are treated differently.[20] Additionally, to get sufficient precision with the judge stringency measure we restrict the sample of judges to those who see at least 100 randomly assigned cases between the years 2000 and 2015.[21] In Online Appendix Table C1, we show how these restrictions decreases the number of judges, courts, and defendants in our sample.

In Table 1 we present descriptive statistics for the sample used in the main analysis of all defendants tried in Finnish district courts from 2000 to 2010. In the first column we report

[17]The data are in hard copy prior to 2000. Due to cost constraints, we focused on collecting and linking the 2000 to 2015 electronic data on judges.

[18]The judge data also provides us with some additional information on criminal cases not included in the crime data. For example, information on the date when a case entered the court comes from the judge data.

[19]The share of Swedish speakers in Finnish population was 5.4% in 2010, but the share of those who a) commit crime and b) request a Swedish judge is even lower, 2.5% of cases.

[20]We require defendants be above age 22.

[21]Some papers require only 50 cases per judge. We were more cautious here, but requiring only 50 cases does not materially change the estimates.

statistics for all individuals who appear in court, and in the next three columns statistics for our relevant subsamples of defendants sentenced to a fine, probation or prison.[22] All means are taken at the time of sentencing 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 descriptive statistics also suggest substantial selection in terms of those who commit crimes and are sent to prison versus those who receive a fine or probation. For example, Table 1 shows that those who are sentenced to prison are are less educated, less likely to be employed, and have much lower incomes compared to those who are sentenced to probation or a fine. These selection patterns suggest there might be unobserved differences between these groups, which is why it is so important to go beyond simple OLS and identify the causal impact of different punishments. As we will show, identifying causal effects changes our estimates dramatically.

[22]Of these 169,602 cases, 27,558 did not receive a fine, probation, or prison. Of these, 6,599 were sentenced to community service and 7,274 some other punishment, and 13,685 were found not guilty.

Table 1: Descriptive statistics

	Full Court Sample	Sub-samples		
		Fine	Probation	Prison
Defendant characteristics	(1)	(2)	(3)	(4)
Age	36.54	36.73	37.25	33.74
	(10.44)	(10.58)	(10.51)	(8.673)
Have children	0.383	0.423	0.403	0.159
	(0.885)	(0.927)	(0.904)	(0.583)
Married	0.228	0.231	0.233	0.147
	(0.420)	(0.422)	(0.423)	(0.354)
Secondary degree	0.389	0.418	0.425	0.266
	(0.487)	(0.493)	(0.494)	(0.442)
Tertiary degree	0.0939	0.100	0.0860	0.0210
	(0.292)	(0.301)	(0.280)	(0.144)
Employed	0.442	0.496	0.497	0.157
	(0.497)	(0.500)	(0.500)	(0.363)
Income	13303.3	14518.8	14037.5	5431.8
	(16790.7)	(16690.2)	(14120.2)	(9171.1)
Native born	0.945	0.942	0.934	0.971
	(0.229)	(0.234)	(0.249)	(0.168)
Female	0.155	0.169	0.172	0.0696
	(0.361)	(0.374)	(0.377)	(0.254)
Prior prison sentence at time t-1	0.124	0.0643	0.00500	0.478
	(0.329)	(0.245)	(0.0705)	(0.500)
Prior charge at time t-1	0.353	0.283	0.246	0.722
	(0.478)	(0.450)	(0.431)	(0.448)
Prior prison sentence at time t-2,t-3	0.158	0.0886	0.00967	0.571
	(0.365)	(0.284)	(0.0979)	(0.495)
Prior charge at time t-2,t-3	0.452	0.378	0.324	0.851
	(0.498)	(0.485)	(0.468)	(0.356)
Number of cases	169602	82299	31424	28321

Notes: The table contains descriptive statistics for the sample used in the main analysis: Adults tried in Finnish district court from 2000 to 2010 subject to the restrictions specified in Section 1.2. Means for all variables specified are reported for the subsamples. Standard deviations in parentheses.

2 The Ladder of Punishments

The ladder model of punishments follows from the principle of proportionality[23]: the punishment should fit the crime. More specifically, the severity of the punishment should increase either as the seriousness of the offense increases or as the defendant commits more crimes. On the lowest rung of the ladder, the prosecutor does not even bring a case to court because the offense is so petty, and on the highest run of the ladder, an unconditional prison sentence is the most severe punishment. In this paper, we focus on the three most common types of punishments in Finland and in most other countries, namely fines, probation, and prison. In Online Appendix A we provide detailed descriptions of how these punishments are applied in Finland.

In the remainder of this section we present descriptive evidence on the ladder approach to punishments. Figure 2 plots the share of different kinds of punishment over different crimes,[24] with the x-axis ranked in order from less severe to more severe using our measure of severity, the share of prison sentences given for each offense. For each offense in the criminal code on the x-axis, the share of fines, probation and prison sentences judges mete out to punish it are represented by blue, green, and yellow dots, respectively. The sizes of these dots reflect the frequency of each offense and punishment combination. The graph in Panel A shows all crime codes, while Panel B focuses on the more frequently committed crimes. Note that for some crime codes, prison is not allowed as a punishment. For more details on minimum and maximum punishments for each crime code see Online Appendix Figure B2.

Figure 2 shows that lower-severity crimes are more likely to be punished with fines. As the crimes become more severe, punishments on average move next to probation and last

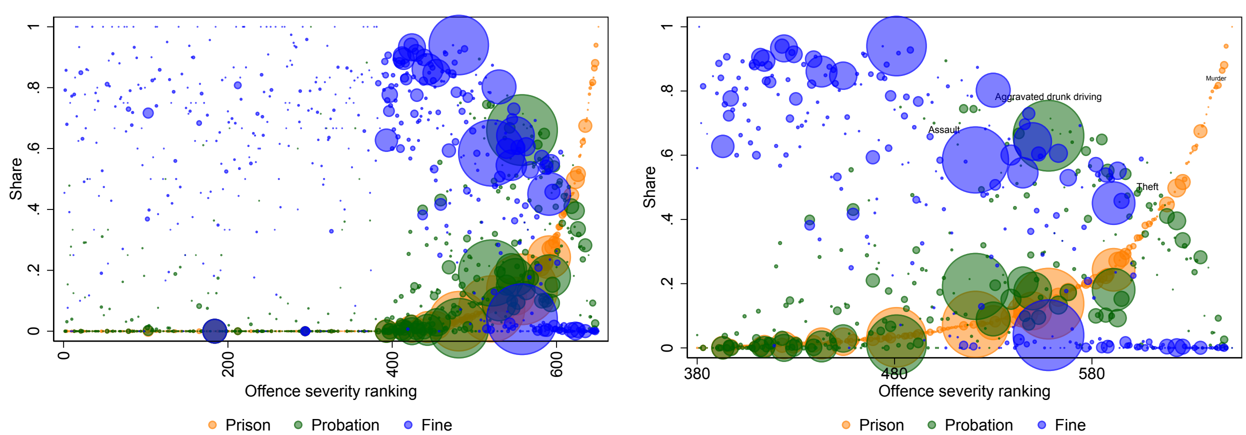
[23]The principle implies that the criminal code should rank different punishments according to their severity and also define the maximum punishment for each penalty. In Finland in particular, it is considered better to prevent too stringent punishments than too lenient punishments. (Hinkkanen and Lappi-Seppälä, 2011).

[24]The Finish criminal code divides crimes into categories using detailed six-digit crime codes. We use this six-digit categorization to divide all crimes into different types in this section.

to prison. However, the figure also shows that all three punishments are given for many crime codes, meaning 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. We will use the fact that not all judges agree on the correct punishment for all defendants as a way to identify the causal impacts of the different punishments.

Figure 2: Ladder of punishments - Share of sentence type as a function of the severity of crime

(a) All Crimes (b) More Frequent Crimes

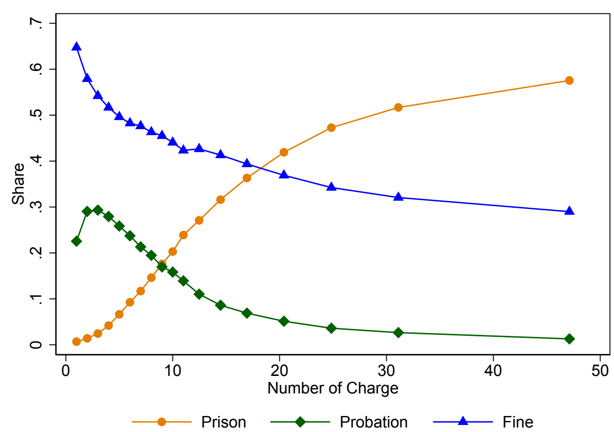


Notes: Figure 2 shows the share of each type of punishment meted out as a function of the severity of a crime. Crimes are divided into different types of offenses using the six-digit Finnish crime code. Different types of offenses are ordered from less severe to more severe using our measure of severity, the share of prison sentences meted out for the offense. The size of each dot reflects the frequency of each offense and punishment combination. The sample consists of all cases that appeared in Finnish district courts between 2000 and 2015. We only use offences observed at least four times in our sample. Panel A shows all crime codes, while Panel B focuses on the more frequently committed crimes.

In Figure 3, we demonstrate the second feature of the ladder model, namely that the severity of the punishment should increase as the defendant commits more crimes. The figure plots the share of different kinds of punishments (on the y-axis) relative to the number of crimes the defendant has committed (on the x-axis). This figure shows that while prison is almost never used for the defendant's first case, as the defendant commits more crimes, the severity of the punishment increases. Among individuals without an extensive criminal history, fines dominate. However, as a defendant commits more crimes, the probability that he will receive probation increases, and then as the defendant continues to commit crimes, the probability that he will receive a prison term increases.

In Online Appendix Figures B1 we replicate Figures 2 and 3 but restrict the sample to include only serial criminals (those who commit three or more crimes). Serial criminals are of particular interest since most crimes are committed by a small subset of the population. The results are identical and suggest that individuals who go on to commit multiple crimes do not generally start off at serious crimes that are likely to send them to prison. Instead, they begin their criminal careers with minimal crimes that end in lower level punishments. As such, understanding the efficacy of early punishments could also help prevent potential serial criminals from continuing their criminal activity.

Figure 3: Ladder of punishments - Number of prior crimes



Notes: Figure 3 plots the share of fines, probation, and prison sentences (on the y-axis) against the number of previous charges for a given defendant. To derive the correct number of previous cases for each defendant, we checked each defendant's criminal history from 1992 to 2015. However, the graph only depicts cases that appeared in Finnish district courts between 2000 and 2015.

These results also suggest an additional outcome of interest. Specifically, to capture crime escalation, we calculate the leave-out mean of prison probability for each crime code (where we calculate this measure separately for each defendant, leaving out his own cases). We argue that the percentage of defendants sent to prison for each six-digit crime code serves as a good proxy for the severity of that type of crime. We will estimate the impact of fines, probation, and prison on the severity of crime measured in this way to understand if crime escalation is more or less likely in response to each punishment.

3 Empirical Specification

Our aim is to identify the causal effect of fines, probation, and prison on defendant criminal and labor market outcomes up to 5 years after the punishment. We focus on court cases that occurred in the years 2000 to 2010 so that we can follow each defendant for at least 5 years after sentencing. The relationship between punishment and defendant outcomes can be captured with the following equation that we estimate separately for each punishment type:

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

Y_{ict} is the outcome for defendant i who had a court case c in year t. P_{ict} is a dummy variable equal to 1 if the defendant i has a given punishment (either fine, probation, or prison sentence) associated with his court case c in year t (and 0 otherwise). $\boldsymbol{X}_{ict}$ is a vector of case and defendant control variables (including court by year 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.

To overcome this challenge, we use the fact that in our context, judges are randomly assigned to defendants. More formally, we use a two-stage least squares (2SLS) approach where we instrument the punishment P_{ict} with the judge j's stringency measure Z_{icjt} within that punishment category. Following the recent literature we construct our instrument using the residualized, leave-out judge stringency measure for each case, Z_{icjt}. To calculate this residualized stringency measure, we regress the punishment indicator on fully interacted court, year, and crime type fixed effects[25] to get a residualized variation of each punishment probability, P^*_{ict}. We do this using all available years from 2000 to 2015. We then construct our leave-out residual stringency as:

$$Z_{icjt} = \left(\frac{1}{n_j - n_{ij}}\right)\left(\sum_{k=0}^{n_j} P^*_{ikt} - \sum_{c=0}^{n_{ij}} P^*_{ict}\right),$$

where n_j is the number of cases seen by judge j and n_{ij} is the number of cases of defendant i seen by judge j. After we remove the defendant's own cases, we take the average of this residual fine, probation, or incarceration proclivity over all judge j's cases. This gives us our instrument, Z_{icjt}, the residualized leave out mean of fine, probation, or incarceration stringency for each defendant i whose case c is assigned to judge j.

The first stage relationship between our instrument Z_{icjt} and the defendant's punishment P_{ict} can be expressed by the following equation:

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

The second-stage relationship is given by Equation 1. This 2SLS strategy works if judges vary in their sentencing severity and the assignment of defendants to judges is not correlated with unobserved defendant characteristics associated with both the likelihood of a given punishment and of given defendant 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 below. We cluster standard errors by judge and defendant.

Our prison stringency instrument can be interpreted in the same way as the rest of

[25]Note that we can use either two digit or six digit Finnish crime type codes and the results are similar. We also checked to ensure that there is a large number of cases within each cell.

the literature i.e., the effect of receiving a prison sentence (due to random assignment to a stricter judge) relative to a counterfactual lighter punishment (primarily a fine or probation in our context). Our fine stringency instrument can similarly be interpreted as the effect of being randomly assigned (through a lenient judge assignment) to a fine as opposed to a counterfactual harsher punishment. Last, our probation stringency measure will allow us to identify the effect of receiving probation as a punishment as opposed to the other possible punishments. Note that in all three cases we are estimating the local average treatment effect (LATE) for the compliers.

3.1 Validity of the Judge Instrument

There are three necessary assumptions underlying the judge instrument approach that we use in this paper. First, the instrument must satisfy the exclusion restriction. Second, the instrument must have a strong first stage. Third and last, the instrument must satisfy the monotonicity assumption. In this section we present results in support of each of these assumptions.[26]

We start with suggestive evidence that the exclusion restriction is satisfied. Beyond the institutional characteristics of the Finnish court system that support the exclusion restriction (described in Section 1), we also report balance test results in Table 2. Table 2 provides evidence that the randomization of cases holds in our setting. If cases are randomly assigned, then predetermined observed and unobserved characteristics of defendants should be similar. To test this, we run a regression where the variable on the left-hand-side is the judge's punishment stringency measure, and on the right-hand side, various predetermined case and defendant characteristics. If random assignment holds, then none of these predetermined characteristics should be associated with the strictness of the defendant's assigned judge. As columns 2, 4, and 6 demonstrate, this is exactly what we find. None of

[26]For a detailed discussion of the implications of these conditions in the judge IV framework, see Mueller-Smith (2014).

the variables can predict the stringency measure. They are also not jointly significant, as reported in the P-values and F-statistics. In columns 1, 3, and 5 we do the same exercise, but now our dependent variable is each one of the punishments. Now the results reveal a strong correlation between case characteristics and punishments. Thus, the balance test passes despite the fact that these variables are highly correlated with whether the defendant receives a sentence of a fine or probation or prison, as shown in columns 1, 3, and 5.

Multidimensional sentencing is a particular violation of the exclusion restriction that could occur in judge IV settings even if the standard balance test passes. The judge might impact the defendant in ways other than the assigned punishment–for example, a more lenient judge might also be kinder when speaking to defendants. We do not observe anything about judges' behavior aside from the punishment they mete out, but as with other papers we assume that such violations, if they occur, do not strongly impact defendants.[27]

Next, we present evidence that there is a strong first stage. We report the standard judge stringency graphs for each punishment in Figure 4. The histograms show the distribution of a specific punishment's stringency measure, while the solid lines plot regression results from a non-parametric version of Equation 2. The figures demonstrate that there is substantial variation in judge stringency in all three punishments. The fitted lines suggest that there is a strong first stage: as the judge stringency increases, the residualized fine, probation, and incarceration rates also increase.

Table 3 reports the first-stage coefficients we obtain using Equation 2. We report the results separately for fines, probation, and prison. In Panel A, we show the estimates without controls, and in Panel B with controls. The table again shows evidence of a strong first stage, similar to the regression lines in the figures. All the first stage coefficients are large, which indicates there is a strong relationship between each specific punishment stringency

[27]One other possible type of multidimensional sentencing is assigning multiple punishments to the same crime. Based on the institutional context and our examination of the data, this does not appear to be an issue in our setting.

Table 2: Balance tests for Fines, Probation, and Prison

	P(Fine) (1)	**Fine IV** (2)	**P(Prob)** (3)	**Prob. IV** (4)	**P(Prison)** (5)	**Prison IV** (6)
Demographics						
Age	-0.0010***	0.0000	0.0002	-0.0000	-0.0004***	-0.0000
	(0.0002)	(0.0000)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
Number of kids	0.0076***	-0.0000	-0.0046***	0.0001	-0.0070***	0.0000
	(0.0016)	(0.0001)	(0.0012)	(0.0001)	(0.0008)	(0.0001)
Married	-0.0325***	-0.0000	-0.0081**	-0.0002	0.0071***	0.0000
	(0.0035)	(0.0002)	(0.0027)	(0.0001)	(0.0020)	(0.0001)
Secondary degree	0.0168***	-0.0002	-0.0043*	0.0001	-0.0170***	0.0001
	(0.0027)	(0.0002)	(0.0019)	(0.0001)	(0.0019)	(0.0001)
Tertiary degree	-0.0158**	-0.0001	-0.0513***	-0.0002	-0.0149***	-0.0002
	(0.0053)	(0.0003)	(0.0042)	(0.0002)	(0.0025)	(0.0002)
Employed	0.0311***	-0.0001	-0.0015	-0.0001	-0.0398***	-0.0001
	(0.0032)	(0.0002)	(0.0027)	(0.0002)	(0.0022)	(0.0001)
Income	0.0000	-0.0000	-0.0000***	0.0000	-0.0000***	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Native born	0.0012	0.0002	-0.0052	-0.0005	0.0194***	0.0000
	(0.0069)	(0.0003)	(0.0052)	(0.0003)	(0.0034)	(0.0002)
Female	0.0159***	-0.0000	-0.0070*	-0.0001	-0.0356***	-0.0001
	(0.0040)	(0.0002)	(0.0032)	(0.0002)	(0.0020)	(0.0001)
Past criminal history						
Prison at time t-1	-0.1140***	-0.0000	-0.1063***	0.0001	0.2764***	-0.0002
	(0.0048)	(0.0003)	(0.0025)	(0.0002)	(0.0051)	(0.0002)
Charged at time t-1	-0.0500***	-0.0002	-0.0097***	0.0001	0.0446***	-0.0000
	(0.0035)	(0.0002)	(0.0028)	(0.0002)	(0.0025)	(0.0001)
Prison at time t-2,t-3	-0.1335***	-0.0001	-0.1467***	0.0002	0.2946***	0.0000
	(0.0043)	(0.0002)	(0.0024)	(0.0002)	(0.0049)	(0.0002)
Charged at time t-2,t-3	-0.0454***	0.0002	-0.0309***	-0.0001	0.0520***	-0.0001
	(0.0033)	(0.0002)	(0.0029)	(0.0001)	(0.0021)	(0.0001)
P-value	0.0000	0.8841	0.0000	0.7589	0.0000	0.7994
F-statistic	464.6603	0.5625	667.3610	0.7054	1420.2643	0.6635
Number of cases	169602	169602	169602	169602	169602	169602

Notes: All estimations include controls for court by year fixed effects. Standard errors clustered two-way at judge and defendant level. Standard errors appear in parentheses. *p<0.05, **p<0.01, ***p<0.001

measure and the probability that the punishment occurs. Furthermore, when we add the control variables reported in Table 1, the coefficients barely move, which supports the validity of our setting. With all controls added, the interpretation of the first-stage coefficients is the following: We find that being assigned to a judge who is 10 percentage points more likely to give a defendant a fine leads to an increase in the probability of receiving a fine of approximately 7.2 percentage points. For probation, we find that being assigned to a judge who is 10 percentage points more likely to assign probation leads to an increase in the probability of probation of 5.1 percentage points. For 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 5.4 percentage points.[28]

Finally, to interpret our main findings as local average treatment effects, we need to assume monotonicity.[29] In the judge fixed effects setting, the strict version of the monotonicity assumption implies that each individual who is incarcerated by a lenient judge should also be incarcerated by a stricter judge. Furthermore, according to the monotonicity assumption, the judge's punishment stringency relative to other judges should not vary depending on the characteristics of a case or defendant. Recent work points out that the stricter version of the monotonicity assumption is unlikely to hold in judge fixed-effect designs but proposes a weaker, average monotonicity condition, under which IV estimates may still be interpreted as weighted averages of treatment effects (see Frandsen *et al.*, 2019). The average monotonicity condition requires that covariance between the individual's judge-specific treatment status and judge overall treatment propensities is weakly positive. In other words, as long as an individual satisfies monotonicity with enough judges

[28]Note that as in other papers that use judge fixed effects (Bhuller *et al.*, 2020; Dobbie *et al.*, 2018a; Mueller-Smith, 2014), the probability of a specific punishment does not increase one-for-one with the specific stringency measure. This is due to the measurement error that attenuates the effect toward zero. The measurement error may arise, for instance, from the use of lay judges. In addition, Bhuller *et al.* (2020), state that the first-stage coefficient does not have to be 1 unless the following hold: a) the number of cases per judge is large; b) there are no covariates; and c) the sample that is used to construct the stringency measure is identical to the estimation sample.

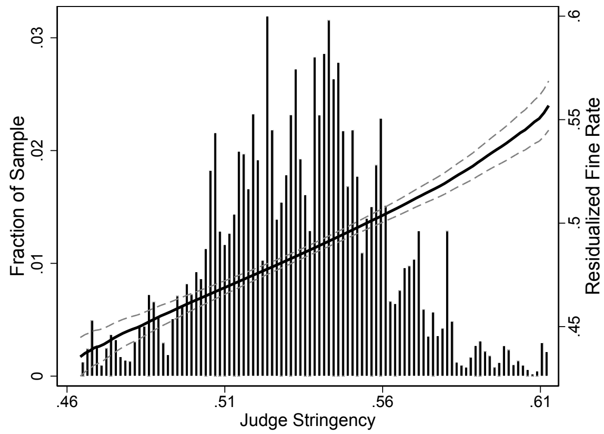
[29]Note that if the causal effect is constant, then the instrument only needs to satisfy conditional independence and the exclusion assumptions.

implying that the total covariance stays weakly positive, the monotonicity may fail with some judge individual pairs. The average monotonicity assumption also generates testable implications: overall judge propensities should be positively correlated with group specific propensities. In Online Appendix Tables D1–D3, we provide extensive evidence that the implications of the average version of monotonicity holds in our setting using the same approach as in Bhuller *et al.* (2020) and Dobbie *et al.* (2018b).[30] See Online Appendix D for a more detailed discussion.

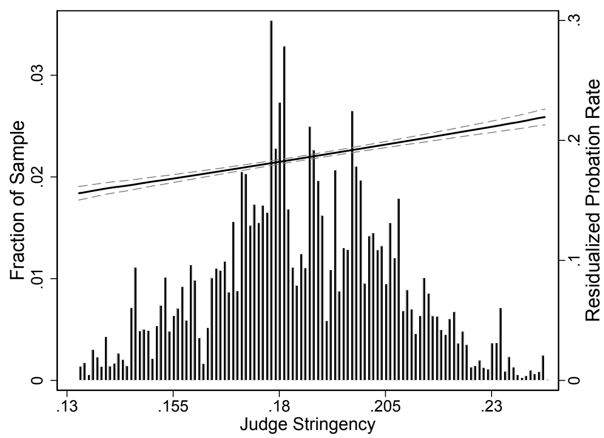
[30] We also perform the joint test of strict monotonicity and exclusion restriction provided by Frandsen *et al.* (2019). As in previous papers (e.g., Norris *et al.*, 2018; Dobbie *et al.*, 2018b) We reject the null hypothesis that the exclusion restriction and monotonicity assumption hold. Due to the institutional knowledge and the evidence shown in Table 2 that backs the exclusion restriction, we argue that this is due to failure of the strict monotonicity assumption.

Figure 4: Judge Stringency Variation for Each Punishment

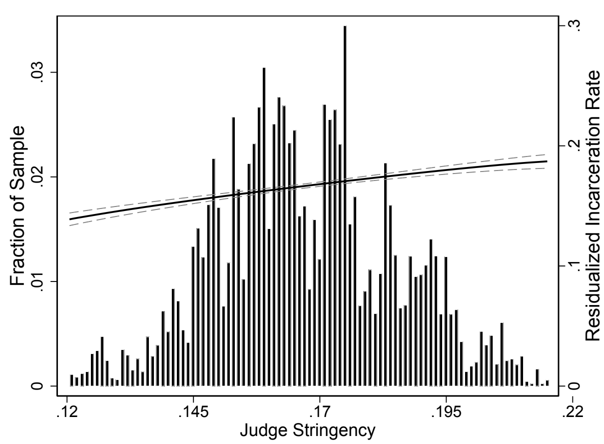
(a) Fines



(b) Probation



(c) Prison



Notes: The figures are graphical representations of the instrument for each punishment. The histogram represents the distribution of individual judges' stringency measures, which capture how strict each judge is after removing court by year by crime type fixed effects. The solid line is a nonparametric regression of the effect of judge stringency on the likelihood a given defendant receives each punishment (the right-hand axis). The dashed lines represent 95% confidence intervals.

Table 3: First stage

Dependent variable	P(Fine)	P(Probation)	P(Prison)
	(1)	(2)	(3)
A. Court by year by crime fixed effects			
Judge stringency	0.714***	0.500***	0.498***
	(0.0554)	(0.0527)	(0.065)
B. Add controls			
Judge stringency	0.720***	0.512***	0.539***
	(0.0553)	(0.0513)	(0.057)
Dependent mean	0.485	0.185	0.167
F-statistic	165.7	90.07	58.299
Number of cases	169602	169602	169602

Notes: This table reports estimates from Equation 2. In Panel A we control for court by year fixed effects. In Panel B we add controls listed in Table 1. Standard errors clustered two-way at judge and defendant level appear in parentheses. *p<0.05, **p<0.01, ***p<0.001.

3.2 Complier Analysis

As we will show, the IV estimates in this paper differ dramatically when compared to OLS estimates. It is thus important to understand two reasons why these estimates might differ so much from each other. The first possibility is selection, which we have already discussed in detail and is the motivation for carefully estimating causal impacts. The second is that both OLS and IV are causal estimates, but there are heterogeneous effects and OLS and IV are estimating effects on different samples. To try and disentangle these explanations for the main results, we re-estimate the OLS using complier weights calculated separately for each punishment. In addition to the OLS and IV results, we also report the reweighted OLS results. In Online Appendix E we briefly describe how we do this analysis and who the compliers are for each punishment type.

4 Main Results

Criminal Activity. We first present the impact of different punishments on future criminal charges. In Table 4 we present the impact of fines, probation, and prison on whether the defendant is charged with another crime in the first year after sentencing, the first 2 years after sentencing, the first 5 years after sentencing, or 3–5 years after sentencing. OLS results with controls suggest that fines are associated with decreased probability of future charges. However, IV estimates show the opposite: fines cause a small increase in the probability of future criminal charges after sentencing, and the effect within 2 years is significant at the 5% level.

We also find a change in sign when we turn to prison. OLS results suggest that prison is associated with higher likelihood of future charges, but in the IV estimates the sign flips and we find that a prison sentence decreases the probability of future charges. This result is significant in the first year and the first two years after charging. The reweighted OLS results look very similar to the OLS results with controls. Together, these estimates suggest that the OLS evidence is misleading and likely due to selection–prison significant decreases the likelihood of future charges, while fines increase the likelihood in the short run. For probation, we find that while OLS estimates suggest that probation is associated with a higher probability of future charges, there is no significant effect of probation on future charges when we turn to the IV estimates.

Figure 5 shows the cumulative impact of each punishment on the probability of future charges over time. Along with the estimates in column 4 of Table 4, the results suggest that the impacts of prison on charges may be driven by the incapacitation effect. The decrease in future charges caused by prison is concentrated in the first 2 years after sentencing. Moreover, when we estimate the effect of fines on future charges 3–5 years post sentencing, we find that fines lead to a significant reduction in the probability that a defendant will be re-charged.

Thus far we have seen that at least in the short run prison causes a large decrease in future charges, fines causes a smaller but still meaningful increase in future charges, and probation leads to a small (but always insignificant) decrease in charges. However, it is also interesting to know if the changes in probability of future charges are also accompanied by changes in the severity of the crimes charged. To measure this outcome, we propose two measures of crime severity. First, is the defendant sent to prison on a new charge. Table 5 uses the same approach as Table 4, but now examines the effect on whether the defendant is sentenced to prison on a subsequent charge within the 5 years following sentencing. The IV analysis shows no significant impact of any of the three punishments on the probability of future prison sentences.

However, it is important to realize that prison is an imperfect proxy for crime severity, since as is the case in many other countries, in Finland probation and prison sentences can mechanically lead to future prison sentences based on the law. Given this issue, we suggest an additional measure of crime severity that does not suffer from this mechanical link, namely the leave-out mean of prison sentences for each crime code as discussed in Section 2. Note that when an individual does not commit a crime in a given year, we assign his crime escalation variable the value zero. This will mean that if we find that a given punishment reduces severity of future crimes charged, this could be due to a reduction in the severity of crimes committed by individuals given a specific punishment, but it could also capture a reduction in the number of future charges. We argue that this is precisely what we wish to capture, as a crime that is not committed can be thought of as the lowest possible level of severity.

We report results in Table 6. Note that for this result we simply report the effect on later severity of crimes charged in the first, second, and third year after sentencing. In cases where a defendant commits two crimes in the same year, we use the most severe crime to assign him a severity measure for that year. We find that while OLS results suggest that prison increases future crime severity and fines decrease future crime severity, IV results suggest the opposite–that fines increase the severity of future crimes in the first 2 years after treatment, and significantly so in the second year. Prison decreases the severity of future crimes charged in the 3 years after the initial sentence, and significantly so for the second year. Probation has no significant effects on the severity of future crimes, but the point estimates suggest a reduction in severity of future crimes.

Overall, our IV results suggest that prison, and not fines or probation, decreases the probability of future charges in the short run, perhaps due to the incapacitation effect. Similarly, prison decreases severity of future crimes while fines increase severity of future crimes in the short run. However, future criminal activity may not be the only outcome of interest. We turn next to the impacts of these different punishments on labor market outcomes of defendants.

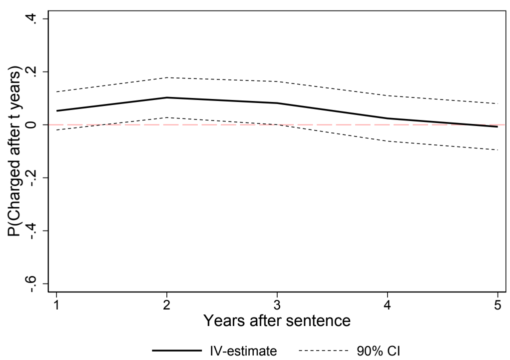
Table 4: Impact of Different Punishments on Probability of Future Charges Post Sentencing

	Pr(Charged)			
	1 year after	1–2 years after	1–5 years after	3–5 years after
	(1)	(2)	(3)	(4)
Panel A. Fine				
OLS: No controls	-0.093***	-0.105***	-0.105***	-0.103***
	(0.003)	(0.003)	(0.003)	(0.003)
OLS: Controls	-0.010***	-0.010***	-0.013***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)
OLS: Reweighted	-0.009***	-0.010***	-0.013***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)
IV	0.053	0.103*	-0.007	-0.121*
	(0.044)	(0.046)	(0.053)	(0.055)
Panel B. Probation				
OLS: No Controls	-0.085***	-0.084***	-0.071***	-0.088***
	(0.003)	(0.004)	(0.004)	(0.004)
OLS: Controls	0.001	0.011***	0.018***	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
OLS: Reweighted	0.007*	0.016***	0.023***	0.007*
	(0.003)	(0.003)	(0.003)	(0.003)
IV	-0.082	-0.122	-0.003	-0.031
	(0.090)	(0.093)	(0.098)	(0.084)
Panel C. Incarceration				
OLS: No controls	0.329***	0.364***	0.361***	0.361***
	(0.005)	(0.004)	(0.003)	(0.004)
OLS: Controls	0.054***	0.050***	0.058***	0.074***
	(0.004)	(0.004)	(0.003)	(0.004)
OLS: Reweighted	0.047***	0.039***	0.045***	0.061***
	(0.005)	(0.004)	(0.003)	(0.004)
IV	-0.198*	-0.291**	-0.120	0.033
	(0.099)	(0.104)	(0.106)	(0.096)
Dependent mean	0.324	0.435	0.565	0.424
Number of cases	169602	169602	169602	169602

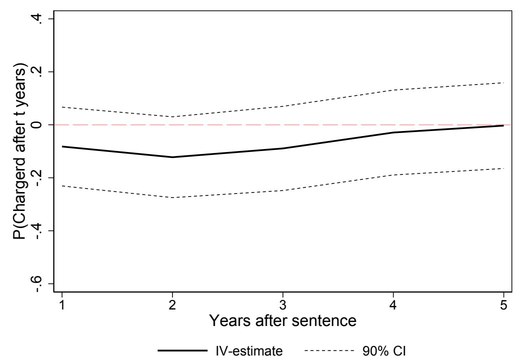
Notes: The table reports OLS and IV estimates of the impact of fines, probation, and incarceration on the probability of being charged with a crime within specified time periods after sentencing. All estimates include controls for court by year fixed effects. IV estimates include controls. Standard errors clustered two-way at judge and defendant level appear in parentheses. *p<0.05, **p<0.01, ***p<0.001.

Figure 5: Cumulative Impact of Different Punishments on Future Charges

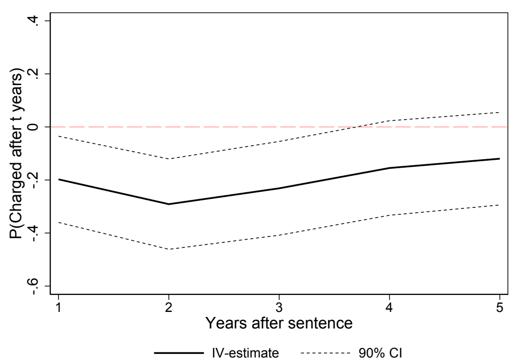
(a) Fines



(b) Probation



(c) Prison



Notes: The figure panels show the cumulative impact of each punishment type on the probability that the defendant will be charged for a new crime in the following years.

Table 5: Impact of Different Punishments on Probability of Future Prison Sentences Post Sentencing

	Pr(Prison sentence)			
	1 year after	1–2 years after	1–5 years after	3–5 years after
	(1)	(2)	(3)	(4)
Panel A. Fine				
OLS: No controls	-0.119***	-0.155***	-0.137***	-0.187***
	(0.002)	(0.002)	(0.002)	(0.002)
OLS: Controls	-0.044***	-0.059***	-0.052***	-0.075***
	(0.001)	(0.002)	(0.002)	(0.002)
OLS: Reweighted	-0.043***	-0.058***	-0.051***	-0.075***
	(0.001)	(0.002)	(0.002)	(0.002)
IV	0.006	0.016	-0.040	0.015
	(0.035)	(0.037)	(0.038)	(0.040)
Panel B. Probation				
OLS: No controls	-0.110***	-0.134***	-0.111***	-0.147***
	(0.002)	(0.002)	(0.002)	(0.003)
OLS: Controls	-0.020***	-0.020***	-0.011***	-0.017***
	(0.002)	(0.002)	(0.002)	(0.002)
OLS: Reweighted	-0.012***	-0.011***	-0.004*	-0.005**
	(0.001)	(0.002)	(0.002)	(0.002)
IV	0.008	-0.006	0.014	0.000
	(0.062)	(0.067)	(0.068)	(0.071)
Panel C. Incarceration				
OLS: No controls	0.384***	0.482***	0.413***	0.557***
	(0.002)	(0.002)	(0.002)	(0.002)
OLS: Controls	0.163***	0.197***	0.155***	0.226***
	(0.002)	(0.002)	(0.003)	(0.003)
OLS: Reweighted	0.160***	0.185***	0.146***	0.201***
	(0.002)	(0.002)	(0.003)	(0.002)
IV	0.019	-0.047	0.063	-0.047
	(0.070)	(0.076)	(0.076)	(0.080)
Dependent mean	0.137	0.188	0.175	0.246
Number of cases	169602	169602	169602	169602

Notes: The table reports OLS and IV estimates of fines, probation, and incarceration on the probability of receiving a prison sentence within subsequent time periods. All estimates include controls for court by year fixed effects. IV estimates include controls. Standard errors clustered two-way at judge and defendant level appear in parentheses. *p<0.05, **p<0.01, ***p<0.001.

Table 6: Impact of Different Punishments on Severity of Subsequent Crime Charges

	Severity of Subsequent Crime		
	1 year after (1)	2 years after (2)	3 years after (3)
Panel A. Fine			
OLS: No controls	-0.029***	-0.023***	-0.024***
	(0.001)	(0.001)	(0.001)
OLS: Controls	-0.007***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.001)
OLS: Reweighted	-0.007***	-0.004***	-0.006***
	(0.001)	(0.001)	(0.001)
IV: Controls	0.015	0.028*	-0.003
	(0.015)	(0.013)	(0.011)
Panel B. Probation			
OLS: No controls	-0.025***	-0.021***	-0.021***
	(0.001)	(0.001)	(0.001)
OLS: Controls	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.001)
OLS: Reweighted	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)
IV: Controls	-0.065	-0.145	-0.093
	(0.088)	(0.088)	(0.079)
Panel C. Prison			
OLS: No controls	0.096***	0.079***	0.078***
	(0.002)	(0.002)	(0.002)
OLS: Controls	0.027***	0.018***	0.022***
	(0.001)	(0.001)	(0.001)
OLS: Reweighted	-0.043	-0.071	-0.050
	(0.041)	(0.039)	(0.038)
IV: Controls	-0.023	-0.052*	-0.033
	(0.030)	(0.026)	(0.026)
Dependent Mean	0.061	0.052	0.047
Number of cases	169602	169602	169602

Notes: The table reports OLS and IV estimates of the impact of fines, probation, and incarceration on the severity of crimes charged within specified time periods after sentencing. All estimates include controls for court by year fixed effects. IV estimates include controls. Standard errors clustered two-way at judge and defendant level appear in parentheses. *p<0.05, **p<0.01, ***p<0.001.

Labor Market Outcomes. Table 7 shows the impact of fines, probation, and prison on whether the defendant was employed post sentencing. The OLS estimates suggest that fines increase the probability of employment, while probation and prison decrease the probability of employment when including a rich set of controls, and even in the reweighted OLS estimates. However, the IV results suggest no significant impact of these punishments on employment. Employment is an indicator that takes the value of 1 if the individual's main activity in that year was employment, and 0 otherwise. Since this measure does not fully capture the quantity or quality of work during the year, we view earnings as a richer indicator of the effects of the different punishments on labor market outcomes. We turn to this outcome next.

Table 8 shows the impact of fines, probation, and prison on cumulative earnings in the first year, the first 2 years, the first 5 years, and 3–5 years after sentencing. We find that the OLS estimates suggest positive impacts of fines on earnings, and negative impacts of probation and incarceration. In this case the IV estimates suggest similar results. We find that the negative impacts of prison on earnings are as large as or even larger than what OLS estimates with controls would suggest, and are only slightly smaller than naive OLS estimates without controls in the first year and first 2 years after sentencing. In contrast, probation no longer has a significant impact on earnings, and the point estimates, while mostly negative, are small. Fines have a small but not significant positive impact on earnings in every column. These results suggest that prison causes negative labor market outcomes for defendants, while fines and probation do not. In Figure 6 we graphically represent the cumulative IV impacts over time on earnings.

Table 7: Impact of Different Punishments on Probability of Employment Post Sentencing

	Pr(Employed)			
	1 year after	1-2 years after	1-5 years after	3-5 years after
	(1)	(2)	(3)	(4)
Panel A. Fine				
OLS: No controls	0.107***	0.107***	0.099***	0.095***
	(0.003)	(0.003)	(0.003)	(0.003)
OLS: Controls	0.024***	0.021***	0.014***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)
OLS: Reweighted	0.024***	0.021***	0.014***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)
IV	0.006	-0.072	-0.010	0.062
	(0.043)	(0.046)	(0.045)	(0.047)
Panel B. Probation				
OLS: No controls	0.034***	0.044***	0.049***	0.040***
	(0.004)	(0.004)	(0.004)	(0.004)
OLS: Controls	-0.020***	-0.014***	-0.009**	-0.014***
	(0.003)	(0.003)	(0.003)	(0.003)
OLS: Reweighted	-0.020***	-0.015***	-0.010***	-0.015***
	(0.003)	(0.003)	(0.003)	(0.003)
IV	-0.043	0.091	0.025	-0.113
	(0.071)	(0.074)	(0.076)	(0.079)
Panel C. Incarceration				
OLS: No Controls	-0.318***	-0.325***	-0.306***	-0.288***
	(0.003)	(0.004)	(0.005)	(0.004)
OLS: Controls	-0.074***	-0.070***	-0.057***	-0.049***
	(0.003)	(0.003)	(0.004)	(0.004)
OLS: Reweighted	-0.045***	-0.043***	-0.038***	-0.031***
	(0.003)	(0.003)	(0.004)	(0.004)
IV	-0.098	-0.013	-0.064	-0.117
	(0.083)	(0.090)	(0.095)	(0.091)
Dependent mean	0.355	0.419	0.518	0.448
Number of cases	169602	169602	169602	169602

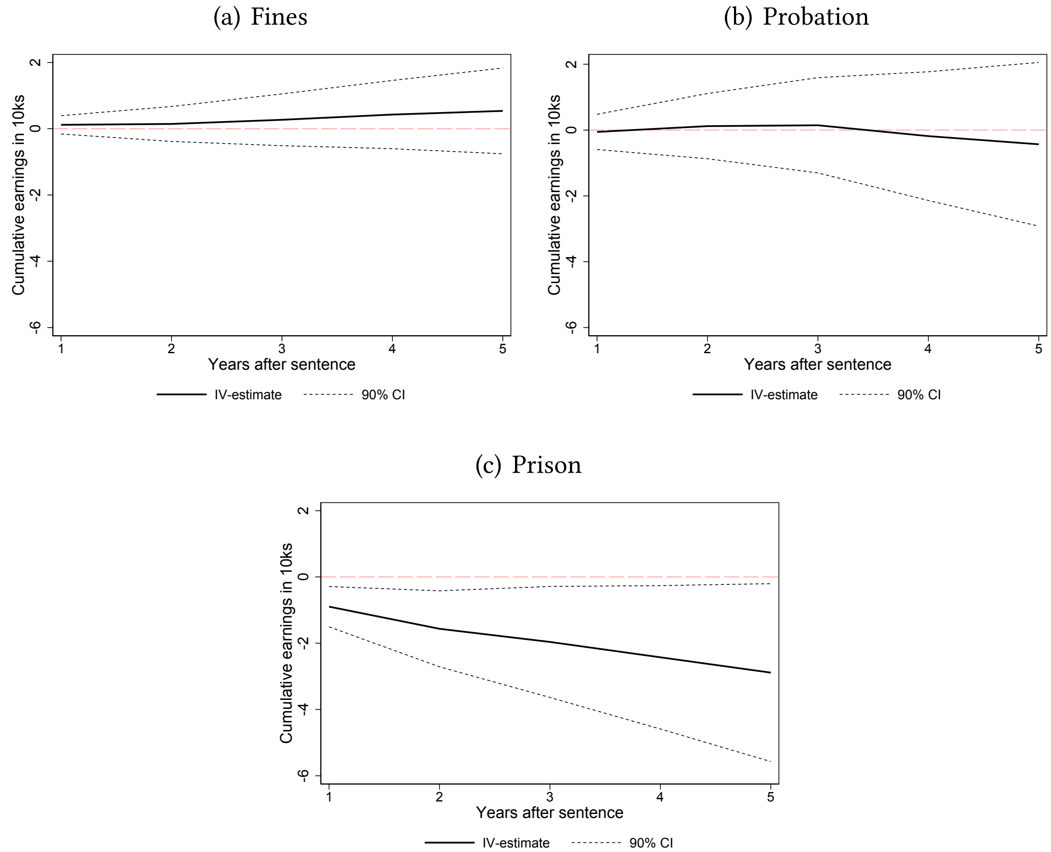
Notes: The table reports OLS and IV estimates of the impact of fines, probation, and incarceration on employment in the specified time periods after sentencing. All estimates include controls for court by year fixed effects. IV estimates include controls. Standard errors clustered two-way at judge and defendant level appear in parentheses. *p<0.05, **p<0.01, ***p<0.001.

Table 8: Impact of Different Punishments on Earnings (in Euros) Post Sentencing

	Earnings			
	1 year after (1)	1-2 years after (2)	1-5 years after (3)	3-5 years after (4)
Panel A. Fine				
OLS: No controls	3048.010***	5969.114***	14560.677***	8591.563***
	(123.150)	(243.578)	(598.173)	(368.334)
OLS: Controls	537.445***	939.972***	2052.101***	1112.129***
	(75.036)	(150.730)	(363.826)	(236.048)
OLS: Reweighted	542.913***	943.329***	2055.949***	1112.620***
	(75.599)	(151.286)	(365.529)	(237.439)
IV	1176.174	1441.236	5380.823	3939.587
	(1680.426)	(3203.852)	(7870.718)	(5089.904)
Panel B. Probation				
OLS: No controls	-453.609**	-882.462**	-2187.919**	-1305.457**
	(143.149)	(276.603)	(668.422)	(408.573)
OLS: Controls	-1318.196***	-2624.054***	-6577.408***	-3953.354***
	(93.576)	(173.713)	(411.724)	(276.729)
OLS: Reweighted	-871.934***	-1777.341***	-4605.558***	-2828.217***
	(82.275)	(150.342)	(354.473)	(236.296)
IV	-557.237	1197.172	-4324.465	-5521.636
	(3255.258)	(6026.044)	(15117.122)	(9811.856)
Panel C. Incarceration				
OLS: No controls	-9689.025***	-19161.001***	-46588.815***	-27427.814***
	(110.490)	(219.726)	(534.460)	(330.169)
OLS: Controls	-1355.222***	-2459.351***	-5430.960***	-2971.609***
	(95.516)	(181.913)	(426.112)	(284.261)
OLS: Reweighted	-712.617***	-1252.136***	-2918.330***	-1666.194***
	(68.307)	(133.495)	(303.862)	(199.766)
IV	-8967.871*	-15649.736*	-28879.477	-13229.741
	(3696.596)	(6970.660)	(16319.371)	(10132.314)
Dependent mean	9974	20101	50729	30627
Number of cases	169602	169602	169602	169602

Notes: The table reports OLS and IV estimates of the impact of fines, probation, and incarceration on annual earnings (Euros) in specified time periods after sentencing. All estimates include controls for court by year fixed effects. IV estimates include controls. Standard errors clustered two-way at judge and defendant level appear in parentheses. *p<0.05, **p<0.01, ***p<0.001.

Figure 6: Cumulative Impacts of Different Punishments on Earnings (in 10,000 Euros)



Note: The figures show the cumulative effects of each punishment on earnings (in 10,000 Euros).

5 Heterogeneity by Crime Severity and Number of Previous Crimes

In Section 2 we showed that punishments tend to grow more severe as a defendant commits more serious crimes and as a defendant commits a greater number of crimes. This has two implications. First, given that both severity of crime and number of previous crimes are important determinants of punishment outcomes, it is interesting to examine potential heterogeneity in the impact of punishments on these two dimensions.

Second, while the LATE for prison we estimate in the previous section is consistent with previous papers (and we extend the analysis to also examine fines and probation), there is a question of interpretation that is particularly relevant for our paper. Specifically, the aggregate analysis in Section 4 pooled all defendants together to evaluate the effect of each punishment. The LATE effect can thus be interpreted as the effect of a given punishment, relative to a weighted combination of the possible alternative punishments. For example, in the previous section we showed that at least in the short run fines increase the probability of future criminal charges while prison decreases the probability. A naive interpretation would thus suggest that fines are less effective at reducing future criminal activity compared to prison in the short run. However, given the results from Section 2, a natural hypothesis is that the compliers for the LATE for prison on average have committed more serious crimes and more previous crimes compared to the compliers for the LATE for fines. To some extent we are able to confirm that this guess is largely accurate in our complier analysis presented in Online Appendix Figure E1.[31] The difference in complier populations means that the LATEs for prison, fines, and probation in the previous section are not directly comparable, unless there are homogeneous treatment effects. While these LATEs are still interesting in their own right, as previous papers estimating the LATE for prison have demonstrated,[32] by estimating the heterogeneous effects in this section we narrow down the comparisons. We find that this adds important nuance to the overall interpretation of our results.[33]

To divide crimes by severity, we use the measure of crime severity introduced in Section 2 and divide the sample into below-median-severity (lower-severity) and above-median-severity (higher-severity) crimes. To estimate the heterogeneity by number of crimes, we

[31] These figures show, for example, that compliers for the fine punishment are significantly less likely to have committed violent crimes, while those for the prison punishment are significantly more likely to commit violent crimes. Compliers for probation are less likely to have previous charges, while compliers for prison are more likely to have previous charges. Together, the graphs show that the complier populations look somewhat different across punishments.

[32] For example, Mueller-Smith (2014) and Bhuller *et al.* (2020) estimate the same LATE for prison that we report in this paper, using the same empirical approach of judge stringency, but in Texas and Norway, respectively. In the previous section we extended that analysis to also estimate the LATEs for probation and fines.

[33] Note that this does not fully resolve the issue of direct comparability but does narrow down the set of counterfactuals, and we show this alone has important implications. Unfortunately the assumptions required to implement alternative approaches to get exactly at the comparison between, for example, prison and fines do not hold in our setting. For example, the approach in Mountjoy (2019) is promising, but the stronger assumptions required to implement his approach do not hold in our context.

divide the sample into defendants for whom this is the first crime and defendants who have committed previous crimes. We present causal estimates of the impact of different punishments on future charges by crime severity in Figure 7.[34][35]

These estimates show that there are clear differences in the results by crime severity. For lower-severity crimes, fines increase the probability of future charges and probation decreases the probability. The mirror image of fines and probation for lower-severity crimes reported in Figure 7 Panels A and C may reflect the fact that probation is the more likely counterfactual to fines for such crimes. We see no significant effects of fines or probation for the group of defendants who have committed higher-severity crimes. Note that these results suggest that probation is quite effective at reducing future charges for lower-severity crimes, despite having no significant impact in the full sample LATE presented in the previous section, while the criminogenic effect of fines is concentrated in lower-severity crimes. Prison causes statistically significant reductions in future charges for higher-severity crimes. Together, these results suggest that for severe crimes, prison appears to decrease future charges quite effectively. For less severe crimes, probation causes the largest reduction in future charges.

Figure 8 shows the heterogeneous impacts of punishments by offender status (first offenders vs. recidivists). This is useful for us since it narrows down the trade-off to be almost entirely between fines versus probation for first offenders since prison is almost never used for the first offense (see Figure 3), allowing us to get an almost direct comparison between these two punishments on the important dimension of the first crime a defendant commits. What we find is that while fines appear to have close to zero effects on future charges, probation decreases the probability of future charges. This result is striking as it suggests that it is possible to reduce future criminal activity substantially by using probation more aggressively in the first sentence. It is also consistent with the results from Figure 7. We

[34]Tables for these figures are available upon request.

[35]Note that the crime severity measure is being used to split the sample, so we do not estimate impacts on crime severity.

find that when it comes to those who have previously committed crimes, fines increase the probability of future charges, while prison decreases the probability.

Next we turn to the heterogeneous impacts on labor market outcomes of defendants. We focus on earnings, since this is where we found significant effects in the previous section. Figure 9 presents heterogeneity in the effects by crime severity, and Figure 10, results for first versus later crimes. We largely don't find significant effects, and point estimates suggest zero or close to zero effect of fines on earnings in all cases. Point estimates for probation suggest that for lower-severity crimes and first offenders, probation causes an increase in earnings, although the effects are not significant. For those who have previously committed crimes or committed higher-severity crimes the effect of probation is negative and close to significant. Just as in the full sample, prison reduces future earnings.

6 Conclusion

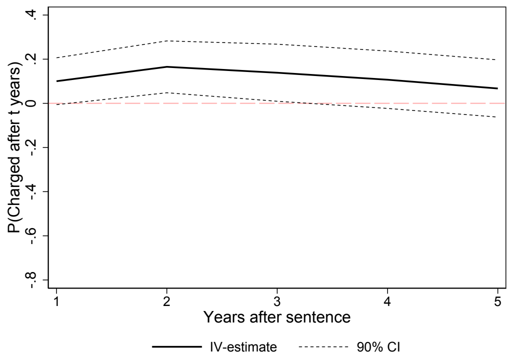
As Becker stated in his seminal paper on crime, if fines are effective at deterring crime then "social welfare is increased if fines are used *whenever possible*" (Becker (1968), p. 28). Yet, despite statements like this one, and the frequent use of punishments other than prison around the world, we know relatively little about the impacts of these other punishments, such as fines and probation, on defendant outcomes. Moreover, the causal impacts of all punishments on the punishment ladder have not been estimated in a single context.

In this paper we have filled this gap in the literature and estimate the impacts of the three most common punishments on the punishment ladder–fines, probation and prison–on defendants' criminal and labor market outcomes. We show that while sentencing defendants to prison lowers the number and the severity of future charges in the short run, it also lowers their future labor market outcomes. However, both crime-reducing effects appear to be concentrated in the first few years after sentencing, which may be consistent with an incapacitation effect. In contrast, sentencing defendants to fines increases future

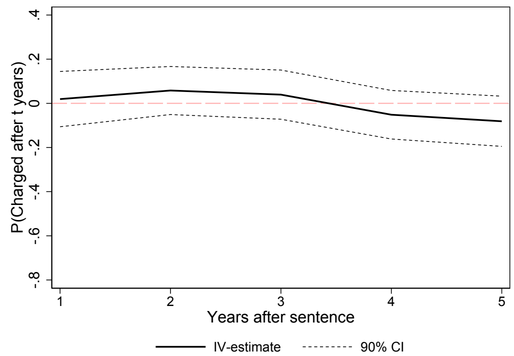
Figure 7: Cumulative Impacts of Punishments on Charges by Severity

Fines

(a) Lower Severity

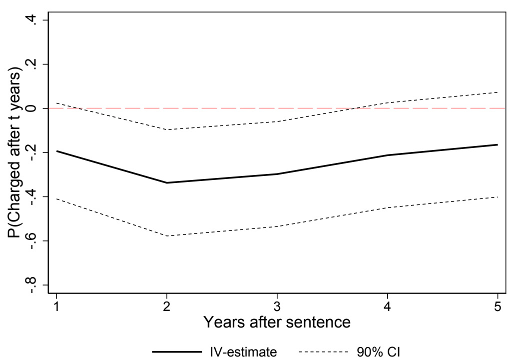


(b) Higher Severity

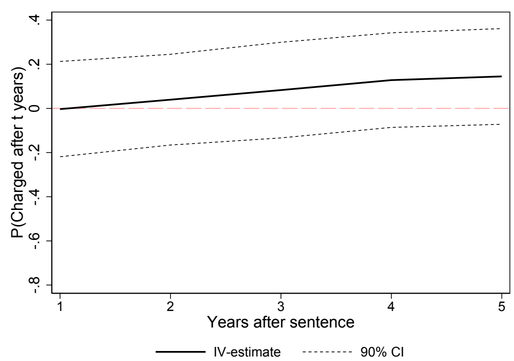


Probation

(c) Lower Severity

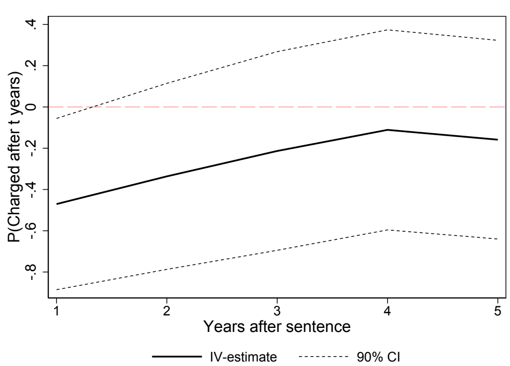


(d) Higher Severity

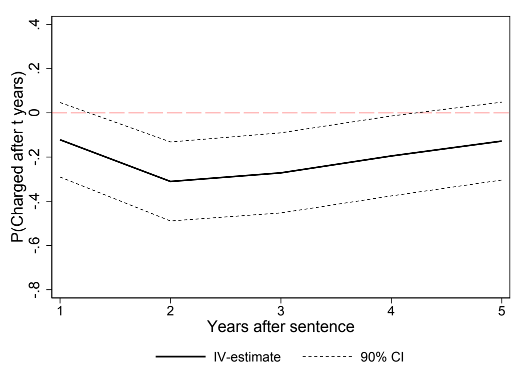


Prison

(e) Lower Severity



(f) Higher Severity

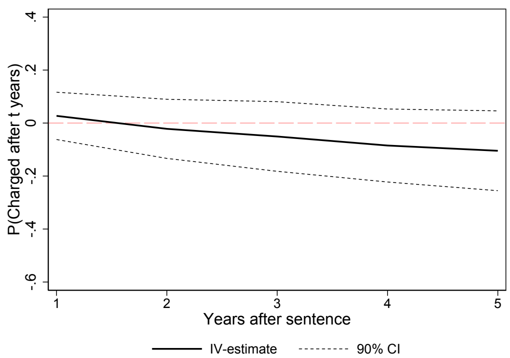


Note: The graph shows the cumulative effects of each punishment type on charges separately for less severe (left panel) and more severe (right panel) crimes.

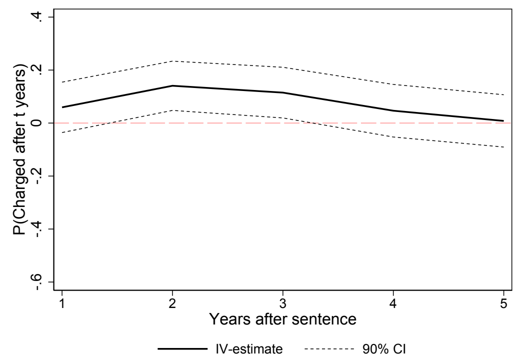
Figure 8: Cumulative Impacts of Punishments on Charges by Offender Status

Fines

(a) First Offenders

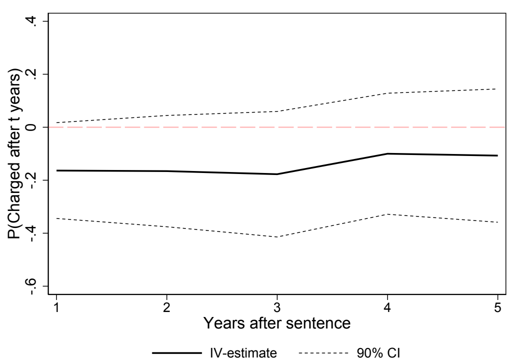


(b) Recidivists

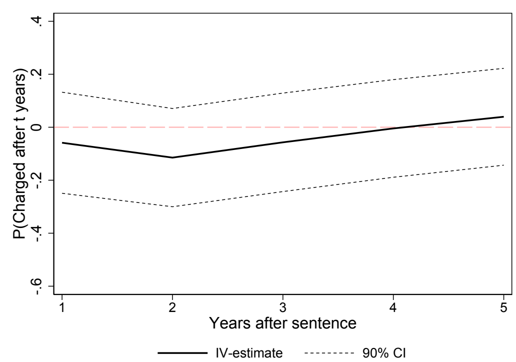


Probation

(c) First Offenders

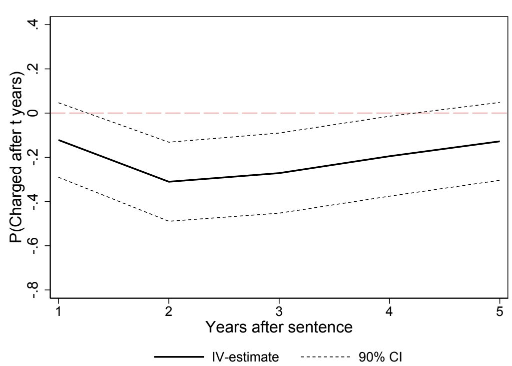


(d) Recidivists



Prison

(e) Recidivists

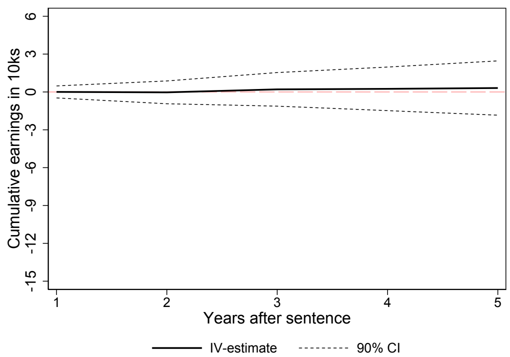


Notes: The graphs shows the cumulative effects of each punishment type on charges for first offenders and recidivists. We omit the effect of prison for first offenders because as shown in Figure 3, close to zero defendants are sent to prison for their first crime. (The graph can be provided upon request, but because there are close to zero such observations the standard errors are extremely large.)

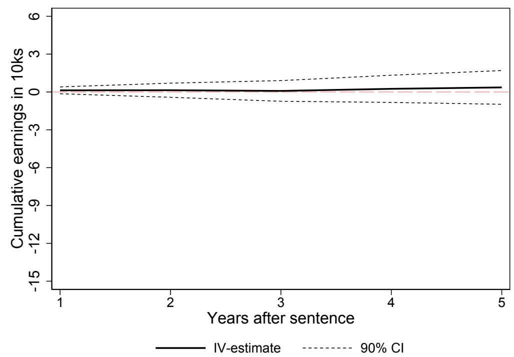
Figure 9: Cumulative Impacts of Punishments on Earnings (in 10,000 Euros) by Severity

Fines

(a) Lower Severity

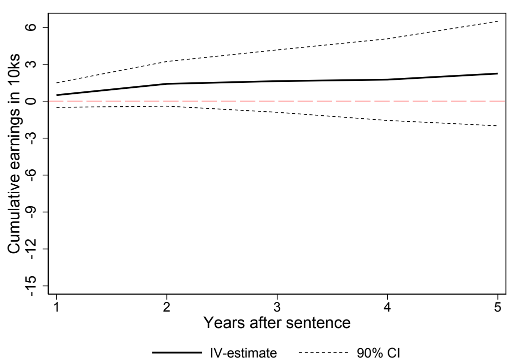


(b) Higher Severity

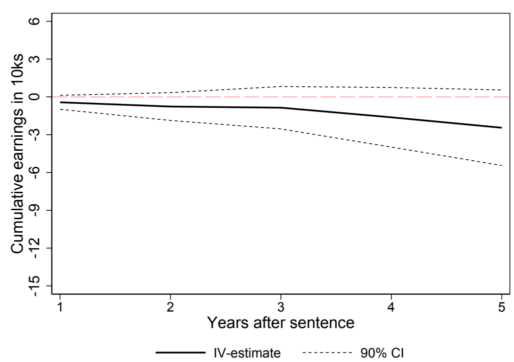


Probation

(c) Lower Severity

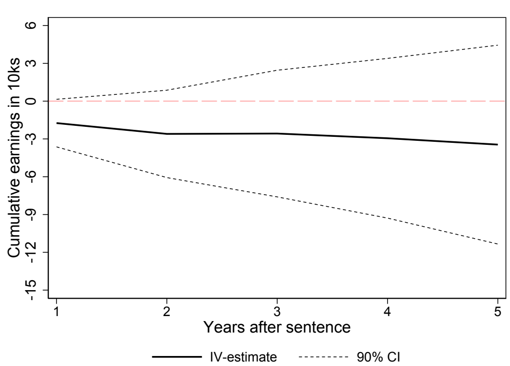


(d) Higher Severity

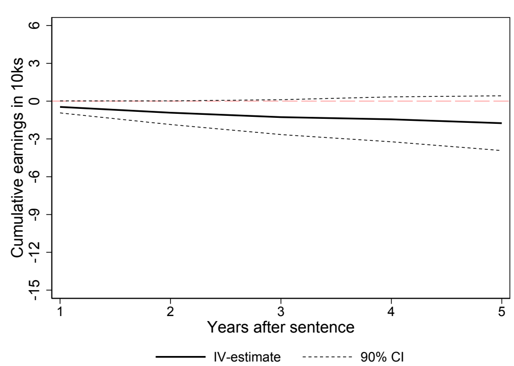


Prison

(e) Lower Severity

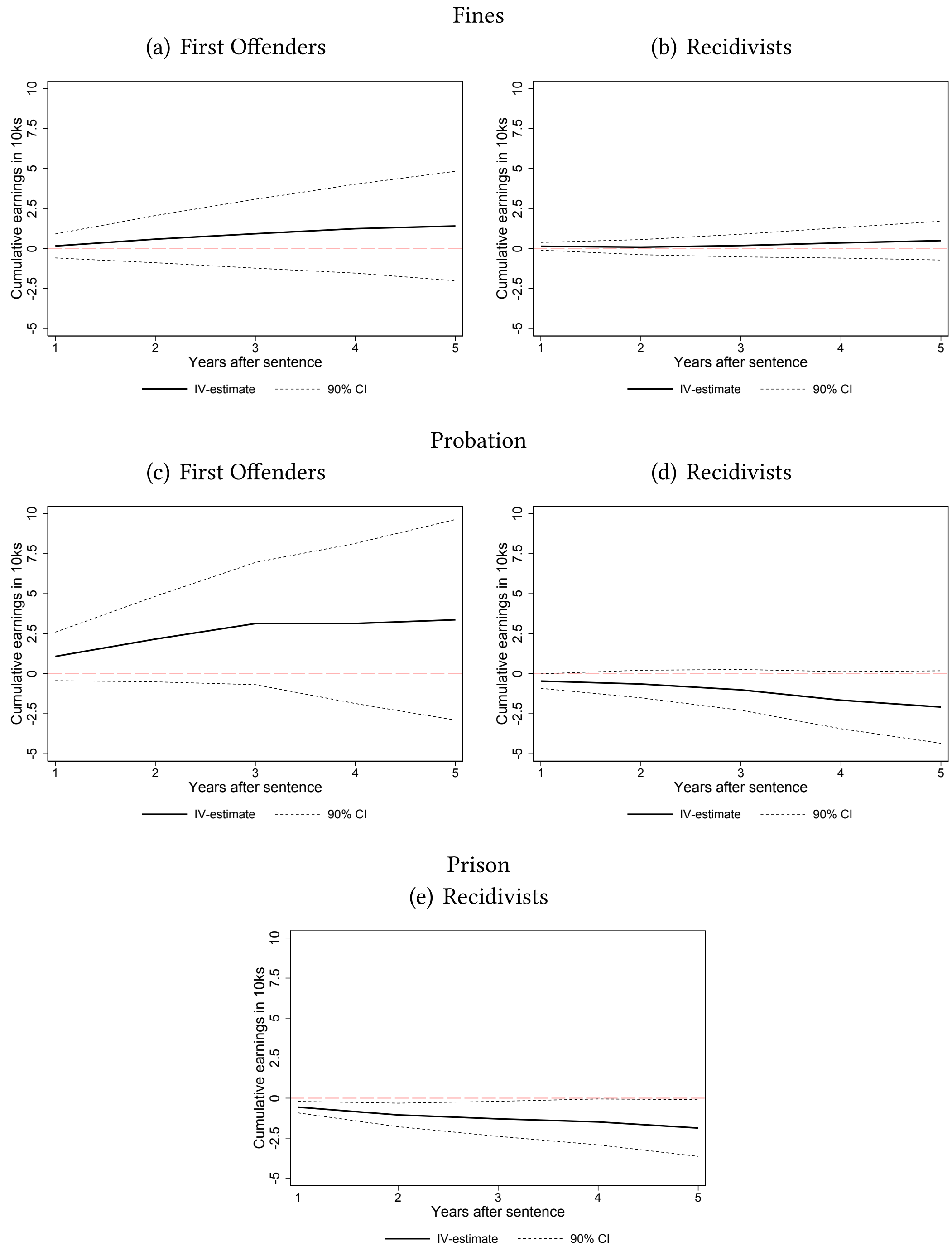


(f) Higher Severity



Note: The graph shows the cumulative effects of each punishment type on earnings for less severe (left panel) and more severe (right panel) crimes.

Figure 10: Cumulative Impacts of Punishments on Earnings (in 10,000 Euros) by Offender Status



Notes: The graphs shows the cumulative effects of each punishment type on earnings separately for defendants who are first time offenders and recidivists. We omit the effect of prison for those who have not committed crimes previously because there are close to zero such cases.

criminal charges and escalates the severity of criminal activity, at least in the first few years after sentencing. Fines do not have the negative impacts on later labor market outcomes of defendants that prison does. We find no significant impact of probation on either future criminal or labor market activities of defendants when we estimate the LATE for the full population.

However, when we turn to the heterogeneous effects of fines, probation, and prison on two dimensions of the criminal punishments ladder–severity of crime and whether the defendant is a first offender or recidivist–we find that the increase in charges caused by fines is concentrated in the lower-severity crimes, while the decrease in charges from prison is largely concentrated in the higher-severity crimes. We also see that the zero overall impact of probation on future criminal activity masks important heterogeneity. Probation causes a reduction in future charges for lower-severity crimes and first offenders. Our results suggest that probation is quite effective at deterring future criminal activity for defendants who have committed lower-severity crimes and first offenders, while prison is very effective at reducing future criminal activity for defendants who have committed higher-severity crimes.

The direct impact of punishments on defendants is not the only outcome to consider. As shown in previous papers on incarceration (see, e.g., Cox and Wallace, 2016; Norris *et al.*, 2018; Dobbie *et al.*, 2018b; Artega, 2018; Bhuller *et al.*, 2018; Billings, 2018), there may be important externalities to families to consider when optimally assigning punishments. Extending the previous analysis on the possible spillovers of prison on the defendant's family members to also consider spillovers of other punishments is an important avenue for future research. Additionally, punishments might differentially deter criminal activity of peers, another area for future research.[36] While this paper provides a first step, understanding

[36]There is a rich literature on peer effects in criminal activity (Bayer *et al.*, 2009; Billings *et al.*, 2019; Deming, 2011; Kling *et al.*, 2005). Given this, it seems possible that different punishments could affect peers' propensities to commit crimes differently. For a good overview of some of the existing literature on the role of punishments and deterrence, see Chalfin and McCrary (2017).

the full impacts of different punishments in even more detail and in additional contexts is clearly important in order to optimally implement a ladder of criminal punishments.

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For Online Publication: Appendix

A Additional Details on the Institutional Context

In this section, we review the three punishments and provide more detail on how the punishments are implemented in our context.

Fines

The first step on the ladder of punishments is a fine, the most lenient and common form of punishment that district courts in Finland impose. Finland uses a day fine[37] system in which the monetary value of the fine is the a product of the severity of an offense and the offender's income. More severe crimes will generally receive a higher number of day fines from a judge. For example, the average number of day fine units for petty theft has historically been around 17, while the average number for aggravated theft has historically been around 47. The Finnish crime code defines the minimum and maximum number of day fines that can be assigned for each offense. Usually, the maximum per offense is 120, but if the same case involves multiple crimes, the number may increase to 240. The value of a single day fine is approximately half of the offender's daily net income.[38] The minimum monetary value of a single day fine is 6 euros, but there is no maximum. If a convicted defendant defaults on payment, the punishment may be converted into imprisonment.[39]

Probation

Next on the ladder of punishments is probation. A judge may impose a probation sentence if the unconditional prison sentence would be at most 2 years, the offense is not too severe, and the offender does not possess an extensive criminal history. Probation is a conditional

[37]The name originates from the idea that the size of a single fine should be equal to the income loss a criminal would suffer if he were to instead spend a day in prison

[38]The exact formulation is $\frac{\text{Monthly net income - 255 euros}}{60}$

[39]For additional information see Lappi-Seppälä (2009).

prison sentence, which means that while a specific prison sentence is handed down, it is suspended for a probationary period, which can last from 1 to 3 years. If the sentenced person commits another offense punishable by unconditional imprisonment during the probationary period, the original sentence of conditional imprisonment can be enforced by imposing a joint sentence of unconditional imprisonment to cover both the previous sentence that was given probation, and the sentence for the new offense. It is important to emphasize that the suspended sentence is enforced only when the severity of the new offense leads to imprisonment. Hence, not just any later crime is enough to automatically convert probation into prison time. A joint sentence may be more lenient than the simple addition of the new and old sentences. In addition, if a judge considers that probation alone is not sufficient punishment for a crime, the judge may bundle probation with unconditional fines.

Prison

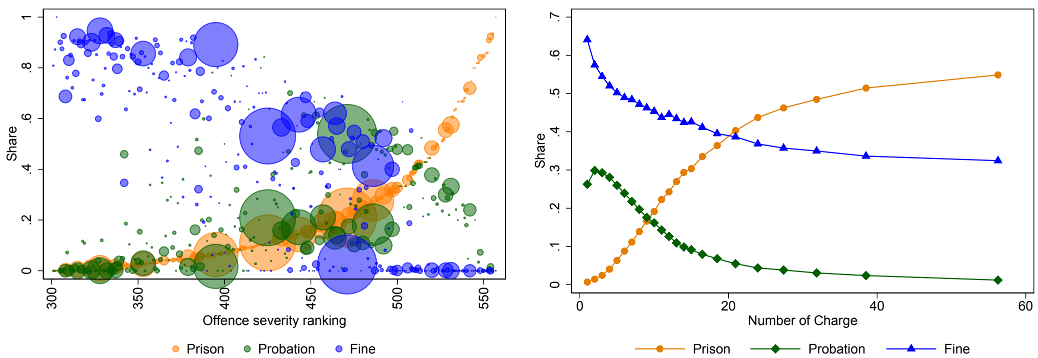
An unconditional prison sentence is the most severe punishment in the Finnish criminal justice system, and hence, the final rung of the punishment ladder. Judges do not generally impose unconditional prison sentences unless the offense committed is particularly severe or the offender has an extensive criminal history. For each offense, the Finnish criminal code specifies when a prison sentence is allowed and the minimum and maximum terms. Except in exceptional circumstances, offenders below the age of 18 years are almost never sentenced to prison.

Convicted defendants serve their sentences in publicly funded open or closed prisons which arrange rehabilitative activities for them, along with educational or work opportunities. For prisoners, it is mandatory to take part in either educational or work opportunities unless they are exempted for health reasons. Prisoners have the right to be in touch with the outside world through phone calls, visits, and approved leaves.

In Finland, almost all prisoners are released on parole. According to the guidelines, first-time prisoners are eligible for parole after serving half of their sentence, while recidivists are eligible for parole after two-thirds of their sentence.

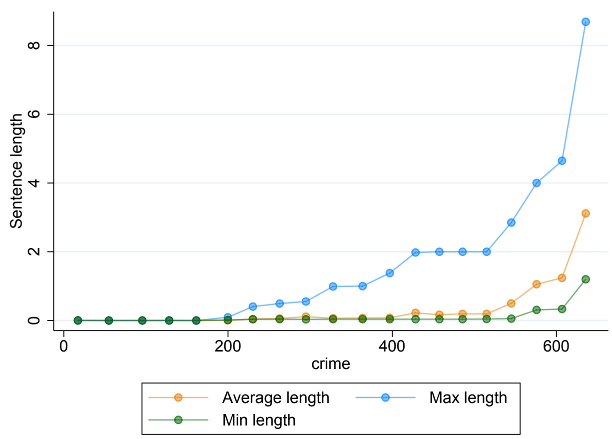
B Additional Evidence on the Ladder of Punishments

Figure B1: Ladder of punishments - Serial criminals



Note: These figures restrict the sample to individuals who commit more than three crimes in their lifetime.

Figure B2: Maximum and minimum prison sentence length by crime code



Note: The figure depicts the maximum and minimum possible prison sentence length for each Finnish crime code.

C Sample Restrictions

Table C1: Sample size after restrictions

A. Judge Stringency Panel 2000-2015

Number of:	Cases (1)	Defendants (2)	Judges (3)	Courts (4)
No restrictions	388829	202408	3361	65
Drop training judges	304326	168882	1035	65
Swedish speaking	296245	163688	1034	65
Drop judges with < 100 cases	282135	157644	680	65
Drop courts with <2 judges	282119	157637	680	65

B. Panel of Analysis for Cases Decided Between 2000-2010

Number of:	Cases (1)	Defendants (2)	Judges (3)	Courts (4)
Analysis data	169602	101359	598	65

Notes: The table reports the sample size of cases, defendants, judges, and courts after imposing each restriction specified in each row. We start with the sample that has already removed juvenile defendants and traffic cases. Panel A represents the restrictions and observations used to construct the judge instrument. When we analyze impacts on defendants we need to follow them for at least 5 years, so Panel B additionally restricts the data to include only defendants whom we can follow for a full 5 years, which is why there are fewer observations.

D Evidence for the Monotonicity Assumption

In Tables D1, D2, and D3, we provide evidence that the monotonicity assumption holds in our setting. In the first column of each table, we use the stringency measures that we construct using the total sample and run the first-stage analysis separately in various subsamples. As the results show, the first stage is strictly positive in each sample. The second column of each table shows results from the reverse sample test. Again we run the first stage separately in various subsamples, but in contrast to the first column, for each judge and subsample, we construct a subsample-specific stringency measure using only cases that do not belong in that specific subsample. All the first-stage coefficients are again strictly positive.

In addition, a new test proposed by Frandsen *et al.* (2019) checks for more subtle violations of monotonicity. We implement this test. As with other papers (e.g. see Norris *et al.* (2018) and also the examples from Frandsen *et al.* (2019)), we fail this stricter test of monotonicity. However, as we discuss in the main text (see Section 3.1), and as Frandsen *et al.* (2019) point out, this simply means that our results can be interpreted as weighted averages of treatment effects.[40]

[40]The failure of the test either means that the exclusion restriction fails or the strict monotonicity assumptions fails. We interpret the results as evidence of the latter, given the rich and abundant evidence we have provided for the exclusion restriction in our setting.

Table D1: Monotonicity Fine

	Baseline instrument	Reverse-sample Instrument
Sub-sample:	First stage P(Fine)	First stage P(Fine)
Over 30 years old		
Estimate	0.746	0.322
	(0.054)	(0.038)
Observations	108418	108418
Less than 30 years old		
Estimate	0.679	0.511
	(0.071)	(0.063)
Observations	61184	61184
Any post compulsory education		
Estimate	0.751	0.575
	(0.062)	(0.054)
Observations	81583	81583
No post compulsory education		
Estimate	0.694	0.513
	(0.059)	(0.046)
Observations	88016	88016
Married		
Estimate	0.826	0.747
	(0.090)	(0.086)
Observations	38730	38730
Not married		
Estimate	0.683	0.214
	(0.049)	(0.029)
Observations	130869	130869
Previously Employed		
Estimate	0.714	0.539
	(0.071)	(0.063)
Observations	59888	59888
Previously non-Employed		
Estimate	0.722	0.289
	(0.053)	(0.037)
Observations	109712	109712
Violent crimes		
Estimate	0.638	0.316
	(0.073)	(0.068)
Observations	59881	59881
Non-Violent crimes		
Estimate	0.742	0.190
	(0.053)	(0.032)
Observations	109720	109720
Property crimes		
Estimate	0.764	0.381
	(0.070)	(0.060)
Observations	60684	60684
Non-Property crimes		
Estimate	0.686	0.238
	(0.053)	(0.038)
Observations	108917	108917
Other crimes		
Estimate	0.765	0.658
	(0.100)	(0.092)
Observations	30680	30680
Non-Other crimes		
Estimate	0.702	0.241
	(0.047)	(0.030)
Observations	138920	138920

Notes: In the first column, we estimate the first-stage Equation 2 separately for different subgroups. Our dependent variable is an indicator for fines. The independent variable is the baseline fine stringency measure we use in the main analysis. In the second column, we run the first-stage Equation 2 in different subsamples, but construct the stringency measure using cases that do not belong in that specific subgroup. Standard errors are two-way clustered at the judge and defendant level and appear in parentheses.

Table D2: Monotonicity Probation

	Baseline instrument	Reverse-sample Instrument
Sub-sample:	First stage P(Probation)	First stage P(Probation)
Over 30 years old		
Estimate	0.484	0.299
	(0.054)	(0.038)
Observations	108418	108418
Less than 30 years old		
Estimate	0.133	0.065
	(0.077)	(0.068)
Observations	61184	61184
Any post compulsory education		
Estimate	0.524	0.425
	(0.063)	(0.052)
Observations	81583	81583
No post compulsory education		
Estimate	0.475	0.379
	(0.058)	(0.044)
Observations	88016	88016
Married		
Estimate	0.430	0.401
	(0.089)	(0.081)
Observations	38730	38730
Not married		
Estimate	0.526	0.208
	(0.049)	(0.030)
Observations	130869	130869
Previously Employed		
Estimate	0.541	0.501
	(0.073)	(0.068)
Observations	59888	59888
Previously non-Employed		
Estimate	0.475	0.313
	(0.052)	(0.036)
Observations	109712	109712
Violent crimes		
Estimate	0.557	0.292
	(0.074)	(0.070)
Observations	59881	59881
Non-Violent crimes		
Estimate	0.467	0.165
	(0.052)	(0.029)
Observations	109720	109720
Property crimes		
Estimate	0.527	0.330
	(0.074)	(0.062)
Observations	60684	60684
Non-Property crimes		
Estimate	0.490	0.151
	(0.052)	(0.035)
Observations	108917	108917
Other crimes		
Estimate	0.462	0.423
	(0.088)	(0.077)
Observations	30680	30680
Non-Other crimes		
Estimate	0.502	0.201
	(0.048)	(0.035)
Observations	138920	138920

Notes: In the first column, we estimate the first-stage Equation 2 separately for different subgroups. Our dependent variable is an indicator for probation. The independent variable is the baseline fine stringency measure we use in the main analysis. In the second column, we run the first-stage Equation 2 in different subsamples, but construct the stringency measure using cases that do not belong in that specific subgroup. Standard errors are two-way clustered at the judge and defendant level and appear in parentheses.

Table D3: Monotonicity Prison

	Baseline instrument	Reverse-sample Instrument
Sub-sample:	First stage P(Incarcerated)	First stage P(Incarcerated)
Over 30 years old		
Estimate	0.495	0.236
	(0.061)	(0.039)
Observations	108418	108418
Less than 30 years old		
Estimate	0.513	0.521
	(0.091)	(0.087)
Observations	61184	61184
Any post compulsory education		
Estimate	0.339	0.246
	(0.060)	(0.041)
Observations	81583	81583
No post compulsory education		
Estimate	0.666	0.554
	(0.079)	(0.079)
Observations	88016	88016
Married		
Estimate	0.494	0.477
	(0.089)	(0.077)
Observations	38730	38730
Not married		
Estimate	0.504	0.271
	(0.061)	(0.047)
Observations	130869	130869
Previously Employed		
Estimate	0.114	0.096
	(0.047)	(0.033)
Observations	59888	59888
Previously non-Employed		
Estimate	0.701	0.393
	(0.071)	(0.107)
Observations	109712	109712
Violent crimes		
Estimate	0.388	0.417
	(0.078)	(0.068)
Observations	59881	59881
Non-Violent crimes		
Estimate	0.556	0.315
	(0.066)	(0.047)
Observations	109720	109720
Property crimes		
Estimate	0.683	0.683
	(0.094)	(0.092)
Observations	60684	60684
Non-Property crimes		
Estimate	0.383	0.224
	(0.057)	(0.034)
Observations	108917	108917
Other crimes		
Estimate	0.182	0.221
	(0.082)	(0.071)
Observations	30680	30680
Non-Other crimes		
Estimate	0.569	0.198
	(0.059)	(0.045)
Observations	138920	138920

Notes: In the first column, we estimate the first-stage Equation 2 separately for different subgroups. Our dependent variable is an indicator for prison. The independent variable is the baseline fine stringency measure we use in the main analysis. In the second column, we run the first-stage Equation 2 in different subsamples, but construct the stringency measure using cases that do not belong in that specific subgroup. Standard errors are two-way clustered at the judge and defendant level and appear in parentheses.

E Complier Analysis

Although we cannot identify compliers from the data per se, we can recover the share of compliers and their average backgrounds using the approach applied by Abadie (2003), Dahl *et al.* (2014), Bhuller *et al.* (2020) and Bhuller *et al.* (2020). In short, the idea is to define compliers as the individuals whose punishment would be different if their case were handled by the strictest judge instead of the most lenient judge. Then under the assumptions of the LATE, and if we consider prison compliers, it follows that the most stringent judge incarcerates always-takers and compliers. In contrast, the most lenient judge only imprisons always-takers. Thus, the difference in the incarceration rates of the most lenient and the strictest judges reveals the share of compliers. The intuition is the same for the other punishments.

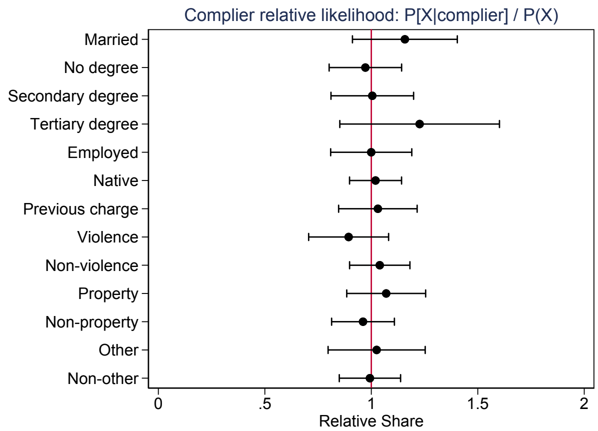
In Figure E1 we show the relative likelihoods that compliers belong to specific subgroups.[41] To construct these figures, we first recover the share of compliers in the total sample and different subsamples, then use these estimates to calculate complier relative likelihoods. Based on the figures, compliers for fines are not very strongly selected, but do appear to be less likely to commit violent crimes, and more likely to commit property crimes and to be married. Compliers for prison are less likely to have a degree, and more likely to have faced previous charges, be unemployed, and to be accused of violent crimes.

We estimate the complier weights that are used to reweight the OLS analysis similarly to Bhuller *et al.* (2020) and Dobbie *et al.* (2018a). First, we split our estimation sample into eight mutually exclusive and collectively exhaustive groups based on prior criminal history and the predicted probability of incarceration. We then recover the relative likelihood of compliers in each group using the approach described in the paragraph above.

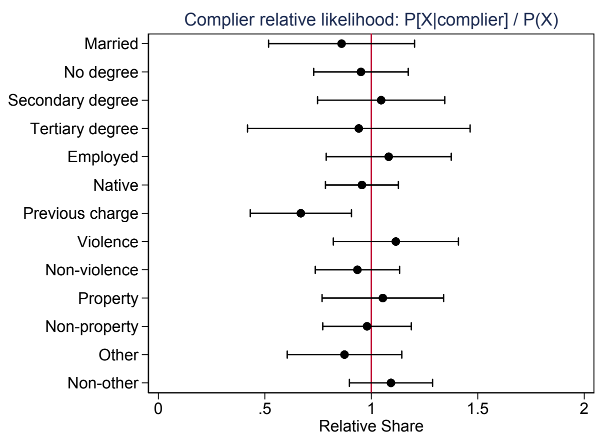
[41]Doyle (2008) states in footnote 18 of his paper, that in the case of a binary instrument, the complier relative likelihood is equal to the ratio of the first stage coefficients in the subsample relative to the total sample.

Figure E1: Complier Relative Likelihoods

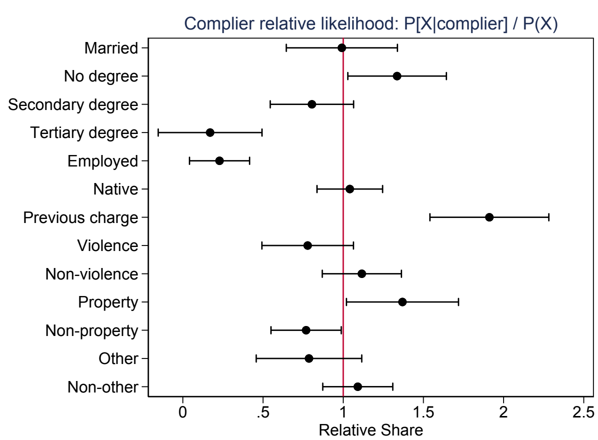
(a) Fines



(b) Probation



(c) Prison



Notes:Following the approach of Bhuller *et al.* (2020), the figures report the relative likelihood of being a complier for each punishment within each subgroup. We construct the confidence intervals using standard errors we obtain using bootstrapping with 500 replications.