Financial Crimes and the Role of Prison to Reduce Them *

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Abstract

We show that financial crimes are committed by a very different subset of the population compared with other crimes, have increased as a share of total crimes over time, but receive lighter punishments on average. Given these stylized facts, we investigate whether harsher sanctions could reduce the incidence of financial crimes. Using random assignment of judges to cases to identify causal impacts of harsher punishments and unique administrative data from Finland, we show that a prison sentence reduces recidivism by 43.2 percentage points in the three years following the crime. We additionally show that individuals quasi-randomly exposed to a colleague who is imprisoned for a financial crime are less likely to commit financial crimes in the future, suggesting important spillover effects of punishments.

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1 Introduction

Financial crimes, including transgressions like fraud and business offenses, impose significant costs on society every year. The United States Department of Justice estimates that fraud alone costs 40-50 billion dollars annually (FBI, 2021). In addition, the European Union Financial and Economic Crime Centre estimates that 98% of criminal assets from economic and financial crimes cannot be recovered (EFECC, 2021). Financial crimes have also grown in importance over the past two decades. In our context we find that the share of all crimes that are financial crimes have increased 14% from 1992 to 2016, and make up just over 16% of all crimes committed in 2016. Just under 10% of United States residents have been victims of identity fraud (Piquero, 2018). Thus financial crime is costly, is becoming more widespread, and claims a large number of victims.

However, despite the large costs of financially based crimes and their growing importance, those who commit financial crimes often receive lighter punishments compared with those who commit other types of crimes. Specifically, we show that 8% of defendants who commit financial crimes are sentenced to prison, a lower rate compared with non-financial crimes: 13% of property crime defendants and 15% of drug crime defendants were sent to prison over the same time period.

Whether financial crimes should lead to more jail time is hotly debated. Prison could reduce the incidence of financial crimes in two main ways. First, individuals sent to prison might be less likely to recidivate. This is a particularly relevant margin given the high rates of recidivism among financial crime defendants. We find that almost half of financial crime defendants commit an additional crime within 5 years of their first crime. This high rate of recidivism amongst financial crime defendants is consistent with the finding in Egan *et al.* (2019) who find that roughly one third of financial advisers who commit financial misconduct are repeat offenders. Second, sending a financial crime defendant to prison could serve as an effective deterrent for others. In particular, one way prison might deter criminality of others is by affecting the likelihood other individuals within the defendant's peer group commit crimes. If this happens, then sentencing an individual to prison could produce a general deterrence effect.

To date there is little empirical evidence on whether prison actually reduces financial crimes. While there exist papers looking at the impact of prison on all defendants that find mixed impacts of prison on recidivism, in practice the majority of the defendants in these previous studies have committed property, drug, and violent crimes.[1] This is a problem when trying to extrapolate these results to financial crimes because we show that financial crime defendants look very different from other defendants on observables. They are much more likely to be employed, have much higher incomes, are at least 6 years older, are twice as likely to be college educated, and are at least twice as likely to be in upper management. Thus, extrapolating from all defendants to financial crime defendants is not reasonable and the impact of harsher sanctions on financial crime defendants is an open empirical question.

In this paper we estimate the impacts of a prison sentence on financial crime defendants' rates of recidivism and their labor market outcomes. Additionally, we show that a prison sentence has important implications for the criminal behavior of the defendants' colleagues. To do so, we construct unique population level administrative data from Finland from 2000-2019 that allows us to identify defendants in financial crime cases, link defendants to their firms at the time of the financial crime, and link defendants to their randomly assigned judges during criminal proceedings. We collected data on judges in conjunction with the National Court Registrar for the purpose of this study. We broadly follow the European Financial and Economic Crime Centre and the FBI's definition of financial and economic crimes as our guide when selecting crimes that we include in our analysis and label "financial crimes". The most common types of financial crimes we study are fraud (60% of all cases), business offense (15%), forgery (9%), and money laundering (7%).

[1]In the United states Mueller-Smith (2014) finds prison increases recidivism. Kuziemko (2013) shows that longer prison sentences increase recidivism in the United States. In Norway Bhuller *et al.* (2020) find that prison decreases recidivism.

Descriptively we find that prison sentences are associated with large drops in earnings and employment, but may decrease recidvism. The drops in labor market outcomes are consistent with existing studies that show similarly large labor market consequences for subgroups of financial criminals, such as managers committing corporate misconduct (Karpoff *et al.*, 2008). However, we also document a large Ashenfelter dip, with drops in employment and earnings both preceding and accompanying the sentence. This could be due to firms suspecting misconduct and firing and reporting workers preceding the actual sentencing. Alternatively, a loss of employment could cause individuals to turn to financial misconduct to replace their earnings. When we estimate more formal event studies we still find large pre-trends. Due to these anticipation effects, event studies (and simple OLS estimates) will fail to identify the causal impact of a prison sentence on future outcomes for financial crime defendants. Thus while these descriptive results are interesting, an alternative approach is needed to identify the causal impact of prison on recidivism and labor market outcomes.

Motivated by this fact, we use random assignment to judges and variation in how likely a judge is to assign prison as a punishment to causally identify the impact of prison on defendant behavior. We provide both institutional details and balance checks that suggest the instrument satisfies the exclusion restriction. Using this approach, we find a strong first stage: those who are randomly assigned to a stricter judge are much more likely to go to prison. We then show that the placebo impact on defendants before the defendant is charged is zero, suggesting that unlike the event study approach, our instrumental variable of random assignment of judges is a valid identification strategy to recover causal effects.

Turning to the main results, we find that when a financial crime defendant is quasi-randomly assigned prison as a punishment, the probability the defendant is charged with another crime in the next 3 years post sentencing decreases by 43.2 percentage points. Moreover, despite the apparently large negative impacts on earnings and employment in the descriptive figures and in the OLS estimates, point estimates from the IV suggest zero or largely positive effects on future earnings and employment, although confidence intervals are not very precise and none of these results are statistically significant. We conclude that for financial crime defendants, prison is effective at reducing recidivism, and may do so without costly labor market consequences.

Next, we examine spillovers on peers' criminality as a possible broader deterrence effect of prison sentences for financial crimes. Peers may update their estimate of the likelihood of receiving more serious sanctions based on observing someone in their network go to prison, and as a result may change their propensity to commit crimes. While spillovers of parental incarceration on child outcomes has been well explored (Norris *et al.*, 2021; Arteaga, 2020; Billings, 2018; Dobbie *et al.*, 2018b), there is not evidence on the potential spillovers of criminal sanctions on colleagues, nor is there evidence on spillovers of punishments in the context of financial crimes.

We estimate the impact of quasi-randomly assigning an individual who has committed a financial crime to prison on their colleagues' likelihood to commit a financial crime in the future. We define individuals as colleagues if they worked in the same firm as the defendant at the time the crime was committed. We consistently find that sentencing an individual who has committed a financial crime also reduces the probability his colleagues commit future financial crimes. These effects are significant in the case of fraud, which make up almost 60% of our observations, and are also significant when we restrict to smaller firms of 70 or fewer or 50 or fewer employees.

Our paper is most closely related to the literature on financial crimes. This literature has largely focused on the firm's role in producing and reducing financial misconduct. For example, Egan *et al.* (2019) find that roughly half of financial advisers who commit financial misconduct are fired after being caught, similar to our finding of large drops in employment following an incident in our descriptive results. They also find that after being fired for financial misconduct, most financial advisers are easily rehired and go on to commit financial misconduct once again. They find that some firms appear to "specialize" in misconduct, and these firms are more likely to cater to lower educated and more elderly populations, which could have important equity implications. In a related paper, Egan *et al.* (2021) find that women who commit financial misconduct are more harshly punished compared with men, as they are more likely to lose their jobs and less likely to find new jobs. Fich and Shivdasani (2007) show that financial fraud, one of the major financial crime categories we focus on in this paper, lead to reputation costs amongst firms, in addition to the widely known valuation losses associated with these events. Additionally, Gurun *et al.* (2018) show large implications for victims of financial fraud that transmit through victim networks. More distant but still focused on financial crimes, Parsons *et al.* (2018) find that there is important geographic heterogeneity in the rates of financial misconduct, and explore possible explanations for why some cities in the United States exhibit higher rates of misconduct.

We contribute to this literature by focusing on defendants charged with financial misconduct and the role the criminal justice system might play to reduce this behavior. A number of papers look at possible preventative actions from requiring employees pass exams that focus on ethics (Kowaleski *et al.*, 2020) to having strong local press to document misconduct (Heese *et al.*, 2021). There also exists a literature focused on identifying those committing financial crimes or what factors predict or are associated with financial crimes, which is particularly relevant given that these individuals can be very hard to catch (Agrawal and Jaffe, 1995; Glaeser and Saks, 2006; Dyck *et al.*, 2010; Khanna *et al.*, 2015; Ali and Hirshleifer, 2017; Dimmock *et al.*, 2021; Dyck *et al.*, 2021; Li *et al.*, 2022). Rather than focusing on finding criminals or preventative measures as these papers, we focus on what reduces crime amongst those who have already been caught for financial crimes. We view both as important areas of research when considering how to reduce the amount of financial misconduct.

Given their high rates of recidivism, reducing crimes amongst existing financial criminals seems a reasonable starting point. We show that financial crime defendants are very different on observables compared to other types of criminal defendants and the criminal justice system treats these defendants more leniently. Our results suggest that harsher sanctions can achieve the aim of reducing recidivism amongst financial crime defendants, although this should only be one factor judges (and more broadly societies) should use when determining whether harsher sanctions are justified.

Closely related to our focus on the potential spillovers of observing a randomly assigned harsher sentence on a colleague on your own future financial misconduct, Dimmock *et al.* (2018) show that there is also contagion in perpetrating financial misconduct. They find that quasi-random exposure to colleagues who commit financial misconduct increases the rate of financial misconduct of a given individual. These results help motivate and are consistent with our finding that there is also a spillover effect of observing harsher punishments for financial misconduct of colleagues. The fact that we find that there are multiple colleagues within a firm who are on the margin of being influenced into or out of a financial crime also suggests potentially important firm roles in promoting or reducing crime, consistent with the literature that demonstrates a role for corporate culture as discussed in Liu (2016).

The remainder of the paper is arranged as follows: Section 2 describes our data. Section 3 provides a detailed description of how we define financial crimes and provides descriptive statistics about individuals who commit these crimes. Section 4 presents descriptive results on the impacts of a prison sentence on defendant outcomes. Section 5 reviews our empirical strategy and provides empirical support for our instrument of quasi-random assignment of judges to cases to identify causal effects of prison sentences. Section 6 reports our main estimates on the impacts of prison sentences on defendants and Section 7 examines effects of a prison sentence on colleagues' criminal behavior. Section 8 focuses on the impacts of financial crime on firm outcomes [in progress]. Section 9 concludes.

2 Data and Institutional Context

We use a combination of existing administrative data and administrative data we collected for the purposes of this project. We use the FLEED/FOLK administrative tax records to obtain information on everyone in Finland's earnings, employment, and demographics. We link this data to police data covering every crime in Finland. For the main analysis where we use the defendant's randomly assigned judge as an instrument to identify the causal impacts of prison on defendant outcomes, we link this administrative data to the court data. Note that one case can contain multiple crimes (for example, fraud could be committed along with identity theft). When we present case level statistics we will use the designated primary crime from the police and/or court records, and use this primary crime code when deciding whether a crime is a financial crime or not.[2] For our identification strategy, we require data on the judge assigned to each case which was not available in the administrative data. Thus, we coordinated with the national court registrar of Finland to collect the data on every judge assigned to every criminal case in Finland. Given that the data is only available digitally from 2000 to 2016, we focus on these dates for our main analysis.[3]

For our main analysis using judge assignment to identify effects, some additional restrictions are necessary to ensure random assignment of judges. Thus, consistent with prior papers in this literature, we restrict the data to cases assigned to judges who try cases in courts with at least two active judges, since there must be at least two judges to have random assignment between judges. In addition, in our context for the very small number of cases where the defendant's first language is Swedish, by law the defendant is required to have access to a Swedish speaking judge.[4] This can violate random assignment in courts

[2]According to court officials, this primary crime code in the data is generally the most severe crime.

[3]The data are in paper form prior to 2000, which was prohibitively costly to collect and link.

[4]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.

that only have one active Swedish judge, so we drop these cases since we do not observe the language spoken by the judges. We also drop juvenile defendants as they are treated differently by the courts and not always randomly assigned to judges.[5] Last, we require judges to see a minimum of 100 randomly assigned cases between the years 2000 and 2015, to make sure we can get an accurate measure of stringency.[6] In Online Appendix Table C.5 we show how each of these restrictions decreases the number of judges, courts, and defendants in our sample.

2.1 Institutional Context

In this paper we focus on defendants who appear in court and for whom we can observe the judge to identify effects. It is useful, then, to define which financial misconduct cases will result in a court proceeding. Most cases will begin once a police report has been filed. After completing their initial investigation, the police will then refer the case to a prosecutor if there is sufficient evidence. The prosecutor then files charges. Conditional on proceeding to court we see that 9% of cases receive a not guilty verdict, 9% are sent to prison, and 82% receive some other punishment, generally fines (62%) and probation (17%). Figure B.1 in the Appendix summarizes this process.

If the case proceeds to a court trial, by law it is randomly assigned to a judge or a panel of judges. We will leverage this random assignment to identify causal impacts of prison in this paper. Later, we provide supportive evidence that this institutional feature is actually implemented randomly, as described by law.[7] Note that in larger courts a subset of judges might specialize in certain cases, so the randomization occurs conditional on the type of crime committed, which we account for in the analysis. A court session is held, and the judge(s) make a decision on guilt. If the judges determine that the defendant is guilty, then

[5] We require defendants be above age 23.

[6] While some version of this restriction is present in all judge fixed effects papers, some others require only 50 cases per judge. We were more cautious here, but requiring only 50 cases does not materially change the estimates.

[7] In addition, we have verified this process through conversations with administrators in the courts.

they decide on the proper sentence and sentence length.

A criminal case can be dealt with by either one judge or a panel of one professional judge and two to four lay judges.[8] In some very severe cases, a panel of three professional judges handle the case, but this almost never occurs for financial cases. Note that later when we use judge stringency to identify effects of prison, we use the stringency of the professional judge. Since October 2006 in minor cases it is possible to settle the case through a written procedure between one judge and the defendant (and his or her lawyer). This can be used only if the maximum sentences is 2 years, the defendant has already confessed his or her guilt, and the defendant opts for this procedure. If there is a relevant victim, the victim must also agree to the procedure. We include such cases in our main analysis as they are still decided by the judge.

In terms of choosing a sentence, the way judges make a decision when there are lay judges is that the professional judges explains the case and relevant points to the lay judges. All judges then vote on the verdict. First they vote if the defendant is guilty. Next they vote on whether to punish the defendant, if he or she is guilty. Last, they vote on the content of the punishment (i.e. length of prison sentence). The professional judge always votes first and when there are lay judges, they vote after.[9]

The type and length of the sentence is determined by the Finnish criminal code. This code specifies all possible punishments, from least severe to most severe. In the vast majority of cases, fines, probation, or prison is used. A prison sentence is only allowed if it is specified as a possible punishment for a given crime type. The maximum specified punishment is binding. However, judges can choose a more lenient punishment than the most lenient punishment allowed in the crime code, meaning that the lower end is not binding and is subject to judge discretion. This is to provide flexibility for the court to actively

[8] Lay judges are politically appointed "assistant judges". They must meet several requirements: 25-65 years old (up to only 63 prior to 2014) and cannot hold another position in the court. They also cannot work for the police or as a lawyer themselves. Prior to 2014, if the case required a panel of judges, then it consisted of one professional judge and 3 lay judges. After January 5, 2014 only 2 lay judges were required.

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

prevent overly harsh penalties.[10]

3 Defining Financial Crimes and Characterizing Who Commits Them

3.1 Defining Financial Crimes and Their Rising Importance

When defining financial crimes, we used the definitions from the European Financial and Economic Crime Centre and the FBI database for white-collar crimes as an initial guide.[11] Thus, we include things like fraud, falsification of financial information, identity theft, money laundering, embezzlement, accounting offenses, and so on as financial crimes. For a full list of all crime types included, see Appendix A. In Table 1 we report the top 5 broad crime categories and the share of all financial crimes within each category. The largest category we include is fraud which consists of 60% of all crimes in our estimation sample, followed by business offenses (15%), forgery (9%), and money laundering (7%). Other types of offenses make up the remaining 9% of cases.

In Figure 1 we graph the proportion of all crimes committed in Finland from 1992 to 2016 that were financial crimes, violent crimes, and property crimes, the three largest crime categories. We find that over time, the share of all crimes consisting of financial crimes has grown from just under 14% to over 16%. This represents a 14% increase in the share of all crimes that are financial crimes over this 24 year period, an important increase in the relative importance of financial crimes over time.

3.2 Who Commits Financial Crimes?

In Table 2 we compare the characteristics of defendants accused of a financial crime versus defendants accursed of violent, drug, or property crimes and find that financial crime defendants look very different. Specifically, on average they are around 10 years older at the time

[10]For more information, see the Criminal Code of 1889 and Hinkkanen and Lappi-Seppälä (2011).

[11]See https://www.europol.europa.eu/about-europol/european-financial-and-economic-crime-centre-efecc and https://www.fbi.gov/investigate/white-collar-crime for a reference.

of conviction compared with property and drug offenders (who we consider the most likely alternative crime types). In addition, they are almost twice is likely to be female, are three times as likely to have a college degree, and are more likely to have children. In terms of labor market outcomes before sentencing, financial crime defendants earn just under double the annual income, are half as likely to be unemployed, are twice as likely to be office workers, and are four times as likely to be found in upper management, with 16% serving in upper management. In summary, on every possible dimension, financial crime defendants are positively selected compared with property and drug defendants. This remains true even when comparing financial crime defendants to violent crime defendants and all other crime defendants (excluding financial, property, drug and violent crimes). These results make it clear why we might want to understand the impacts on financial crime defendants separately - they are a distinctively different type of defendant and as such, may respond to harsher sanctions differently.

Next, in Table 3, we turn to the differences in how these crime categories are punished. We show that 36% of those who commit property crimes and 21% of those who commit drug crimes are sent to prison, which is almost double the 11% of those who commit financial crimes who are sent to prison. Instead, those who commit financial crimes are much more likely to be given a probation sentence, and have almost double the likelihood to be found not guilty (12% of those who commit financial crimes compared with 6% of those who commit property crimes and 2% of those who commit drug crimes). Conditional on receiving a sentence, the length of the sentence (77 days) is lower for financial crimes compared with property crimes (100 days) and drug crimes (163 days). These statistics support the general assumption and discussion that financial crimes are punished less harshly compared with other types of crimes.

Last, in Figure 2 we present evidence on the rate of recidivism for the population of financial crime defendants. We find that by five years after committing a financial crime, approximately 45% of defendants have committed another financial crime. Within just the first year, 25% of financial crime defendants have already committed another crime. These very high rates of recidivism indicate the importance of investigating how to prevent future criminality within the population of those arrested for financial crimes. Based on these descriptive results, cutting down on recidivism of financial crime defendants could play an important role in reducing financial crimes overall.

4 Descriptive Impacts of Prison on Financial Crime Defendants

In the preceding section we established a number of important results. First, financial crimes have risen over time. Second, financial crime defendants look very different than other types of defendants, and are punished much more leniently. One possible reason for this leniency could be that financial crimes are often viewed as "victimless" crimes. Unlike property theft and drug sales, both also crimes that are primarily financial motivated, these crimes do not involve direct victims in the same way. However, as described in the introduction, these crimes are quite costly in aggregate. Thus, punishing these crimes more harshly might still be justified, but only if harsher punishments are effective at reducing the quantity of financial crimes committed. In this section we turn to our first descriptive evidence on the possible impact of harsher sanctions on defendants.

Figure 3 shows the raw impact of a prison sentence on whether the defendant is charged with a crime in the years around sentencing (Panel A) and the defendant's employment (Panel B) and earnings (Panel C) before and after sentencing. We see a drop in income and employment following the sentence which is much larger for those sent to prison compared with those who commit a financial crime but are not sent to prison. However, we also observe a large "Ashenfelter dip" in this sentencing context, i.e. a drop in earnings that precedes the sentence.

These results suggest a few things. First, there are interesting dynamics in the labor market outcomes of financial crime defendants both before and after the sentence. This could be because losing a job leads to financial crimes, but could also be because firms discover an employee committing financial crimes, report the crime to the police, and then fire the employee and so the drop in employment (and earnings) corresponds to the period between when the crime was committed and when the sentence was decided. To better understand the latter possible explanation, in Figure 4 we present the dynamics of recidivism, earnings, and employment around the time the crime was committed, as opposed to the time of sentencing. We see that this somewhat mitigates the Ashenfelter dip, but does not eliminate it.

Next, we can estimate a simply event study style regression as follows:

$$Y_{ibt} = \alpha_i + \sum_{j=-3}^{6} \delta_j D_{b,t-j} + \pi_b + \gamma_t + \epsilon_{ibt}, \tag{1}$$

where we estimate the impact of a prison sentence relative to defendants who commit a financial crime but are not sent to prison. The coefficients of interest are the year dummies, ($\delta_j D_{b,t-j}$), with the year before sentencing omitted, so that results are relative to the year before sentencing. Additionally, we control for individual fixed effects (α_{ib}), sentencing year fixed effects (π_b), and year fixed effects (γ_t).

In Figure 5 we present estimates from this specification. Consistent with the descriptive graphs, we again find a decrease in the propensity to commit a crime post prison sentence, but also find evidence that even after controlling for individual fixed effects and time fixed effects that the clear pre-trends still appear. For employment and earnings these event studies if anything indicate that prison reverses the downward trend that occurs prior to sentencing, suggesting a positive effect of prison on employment and earnings. However, we still see extremely pronounced pre-trends for both employment and earnings. Thus, these results show that the assumptions necessary for event studies on the impact of prison to be interpreted causally clearly do not hold. This fact is indicative of interesting earnings dynamics around sentencing, but also suggests potentially important selection into the treatment of a prison sentence. This helps motivate our empirical strategy, using random assignment of judges to identify causal effects of prison sentences.

Also consistent with significant selection into prison sentences are the descriptive results in Table C.4 where we compare those who committed financial crimes and are sent to prison versus those who committed financial crimes and are given some other punishment or who receive a not guilty sentence. We find that those who are sent to prison are strongly negatively selected compared with those who commit financial crimes and are not sent to prison. They have more than half the income and wages, are 9 percentage points more likely to be unemployed, and are much more likely to have a previous criminal charge. The last fact may be mechanical given that multiple charges can make judges more inclined to assign a prison sentence, or can cause a probation sentence to become a prison sentence.

5 Main Empirical Specification: Using Random Assignment of Judges to Identify the Impact of Prison Sentences

In this section, we specify our research design to identify the causal impact of prison. Formally, the relationship between prison and defendant outcomes can be captured with the following equation:

$$Y_{ict} = \beta_0 + \beta_1 P_{ict} + \beta_2 \boldsymbol{X}_{ict} + \varepsilon_{ict}. \tag{2}$$

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 is given a prison sentence for 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 address the potential endogeneity of punishments, we use the fact that judges are randomly assigned to defendants. Thus, we estimate a two-stage least squares (2SLS) model where we instrument prison sentences P_{ict} with the judge j's propensity to assign defendants to prison, which we denote as Z_{icjt} . We construct our instrument using the residualized, leave-out judge stringency measure for each case, Z_{icjt}, consistent with the recent literature. To calculate this residualized stringency measure, we regress the punishment indicator on fully interacted court, year, and crime type fixed effects, and then estimate the residualized prison probability, P^*_{ict}. We do this using all available years from 2000 to 2016. Formally, the equation for our leave-out residual prison stringency can be written 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 incarceration proclivity over all judge j's cases. This gives us our instrument, Z_{icjt}, the residualized leave out mean of 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 prison sentence P_{ict} can be expressed by the following equation:

$$P_{ict} = \alpha_0 + \alpha_1 Z_{icjt} + \alpha_2 \boldsymbol{X}_{ict} + \epsilon_{ict}. \tag{3}$$

The second-stage relationship is given by Equation 2. 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 the defendant's 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 as 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). Note that this implies that we are estimating the local average treatment effect (LATE) for the compliers. In this context, this is the policy relevant parameter of interest.

5.1 Validity of the Judge Instrument

For the instrument to work in our setting, it must satisfy the exclusion restriction, have a strong enough first stage, and meet the monotonicity assumption. First, we present suggestive evidence that the exclusion restriction holds by checking balance in Table 4. We find that even though the characteristics we present in this table are highly correlated with whether a given financial crime defendant is sent to prison (with a joint F statistics of 432.11, see Column 1), they are almost all uncorrelated with the judge's incarceration stringency (as shown in Column 2). In addition, the joint F test to assess whether the variables are jointly significant is 1.28, indicating that they are not jointly predictive of judge stringency. Together, this evidence suggests that the exclusion restriction is met in our setting, with the stringency of the assigned judge unrelated to observables, although there is no way to check for correlation between assigned judge stringency and unobservable characteristics of the defendant. This implies that the institutionally mandated random assignment works in practice.

Second, there must be a strong first stage. This requires variability in judge stringency that is strongly predictive of the probability a defendant is sent to prison. We present the first stage estimates in Table 5 and find that a 10 percentage point increase in the stringency of the judge corresponds to a 5.6% increase in the probability the defendant is sent to prison, which is significant at the .001 level, indicating a strong first stage. In Figure 6 we provide a visual representation of the first stage. The histogram depicts the variation in judge stringency in our sample, and we find that there is quite a bit of variability across all judges. We overlay a nonparamtric regression line of the effect of judge stringency on the likelihood of receiving a prison sentence, and consistent with Table 5, we find a strong relationship.

Third, the instrument must pass monotonicity tests. What this means in practice is that any individual who is incarcerated by a lenient judge would also be incarcerated by a stricter judge. Additionally, the ranking of judge stringency should not depend on the characteristics of the case. To test this, in Appendix Table C.6 we use the approach from Bhuller *et al.* (2020) and show that the instrument is strongly correlated even when we calculate it for subsamples of the data. This indicates that judges behave consistently across cases. More recently, Frandsen *et al.* (2019) have developed a a test for the stricter monotonicity assumption, although they show that this assumptions rarely holds and when it fails IV estimates can still be interpreted as a weighted average of treatment effects. The results of this test indicate [in progress].

6 The Impact of Prison on the Defendant's Outcomes

Figure 7 Panel A shows the impacts of a prison sentence on defendant charges before and after the sentence. The 3 years prior to the sentence serve as a placebo check. If the IV works as it should, we expect point estimates capturing the impact of the randomly assigned future judge on charges in the past to be approximately zero and insignificant. This is precisely what we find, with all point estimates being close to zero and insignificant.

Turning to the post sentencing estimates, we see that in the first year there is a decrease in charges but it is not statistically significant. However, in the 2 years post sentencing, the 3 years post sentencing, and the 4 years post sentencing we see a statistically significant decline in whether the defendant is charged with a new crime. For those marginal financial crime defendants who are quasi- randomly assigned to prison, these results indicate that prison sentence causes a decrease in re-offending in the future. Point estimates indicate that in the 3 years post sentencing, a prison sentence reduces recidivism by 43 percentage points (See Table 6).

In Panel B of Figure 7 we analyze whether conditional on committing a crime the severity of crime increases. As introduced in Huttunen *et al.* (2020), we measure severity of a given crime by estimating the percent sent to prison within each crime code. We find that a randomly assigned prison sentence causes an increase in the severity of crime for those who go on to commit more crimes. This result could be because prison itself leads to more severe crimes, but could also be due to the changing composition of criminals, since the previous result suggests that some (potentially those least inclined to crime) reduce their criminal activity on the extensive margin.

In Figure 8 we turn to the labor market outcomes of defendants who committed financial crimes and are quasi-randomly sent to prison. We find that both employment and earnings point estimates are very close to zero. This is in contrast to both the event study estimates from Section 3 and OLS estimates shown in Table 7, both of which show that prison is associated with large decreases in earnings and employment. Specifically, OLS estimates with controls suggest that in the first 3 years after being sent to prison earnings fall by approximately €1200 each year which is always statistically significant and corresponds to a 4% drop in earnings each year compared with the pre-sentence mean earnings (€28,318 according to Table C.1). In contrast, IV estimates suggest a fall in earnings of €3,304 that is not significant in the first year post sentencing, and in the second and third year earnings rise by a statistically insignificant €551-€2909 corresponding to a statistically insignificant 1.9%-10% increase in earnings in the second and third year post sentencing relative to the pre-sentencing mean earnings. However, results are somewhat imprecise so while point estimates suggest small increases in earnings post sentencing, we can only rule out decreases in earnings larger than -15,000 in the first four years post sentencing.

The overall take-away from our main results is nicely summarized in Table 8, which presents the cumulative 3 year impacts of prison on recidivism, earnings, and employment. Starting with rows 1 and 2, the OLS estimates without and with controls, we see that descriptive evidence suggests a large negative impact of prison on labor market outcomes in columns 2 and 3, and prison is associated with an increase in recidivism as shown in column 1. Thus, naive OLS results, even including the very rich controls we have in the administrative data, would suggest that prison does not reduce recidivism and is costly in terms of defendant labor market outcomes.

However, when we turn to the IV estimates in the fourth row, we see that the sign flips for recidivism and in fact the LATE estimates imply a statistically significant decrease in recidivism, with prison leading to a 43.2% decline in the probability the defendant is charged with another financial crime in the following three years. [Note that the full sample mean reported in the table is not the relevant comparison mean. We are working on estimates for the complier means which will almost certainly be much larger, and will report these in future iterations of the draft, but these estimates are still in progress.] The point estimate for employment is positive, although not statistically significant. The point estimate on earnings is extremely small, moving from a statistically significant €25,441 cumulative reduction in earnings in the OLS without controls to a statistically insignificant €589 reduction in earnings in the IV. Relative to mean annual earnings of €28,318 in the sample prior to the sentence, this suggest a large reduction in earnings in the OLS, but causal estimates from the IV suggest a minuscule drop in earnings that is not significant.

One interesting question is whether the flip in the sign of the estimate moving from the OLS estimates with controls to the IV estimates is because the sign does actually flip (i.e. the OLS estimates get things wrong). Alternatively, the change could be purely due to selection into which observations are on the margin of treatment and thus comprise the LATE identified and estimated by the IV. If we were able to identify the compliers to treatment of a prison sentence and estimate OLS for this subsample, would we get the same results as the IV? To address this question, in the third row of results we estimate the OLS with controls, but additionally reweight the observation to mimic the observations used in the IV. While we cannot identify the exact subset of compliers, we can estimate the share of compliers and their average backgrounds using the approach applied by Abadie (2003) and Bhuller *et al.* (2020). This allows us to better compare similar samples across IV and OLS estimates. We estimate the complier weights that are used to reweight the OLS analysis similarly to Bhuller *et al.* (2020) and Dobbie *et al.* (2018a). We find that even in this OLS reweighted sample estimates suggest that prison increases recidivism, which is consistent with OLS getting the sign wrong.

7 Impact of Prison on the Defendant's Colleagues' Financial Crimes

Next, we examine if a prison sentence may also cause peers to reduce the number of financial crimes they commit. Given the nature of these crimes, we focus on colleagues as the peer group of interest. There are a number of reasons why observing a colleague go to prison might change your behavior. If one of my colleagues is sentenced to prison for a financial crime, I might update the likelihood I believe I will be sent to prison for a financial crime upwards. If I was on the margin of committing a crime, this could be enough for me to choose not to do so.

To estimate the impact of an individual being sent to prison on his colleagues' criminal activity, we estimate similar regressions to the previous section, but instead of the defendant's own recidivism as the outcome of interest, we instead look at whether each of his or her colleagues commit a crime. In other words, we treat each colleague as an individual observation and examine whether they commit a financial crime. We continue to use the random assignment to judges as an instrument to identify causal effects. We use the colleagues at the time the crime was committed, since the descriptive evidence suggests that many individuals separate from their firms following a financial crime (potentially because they are fired after the firm discovers the crime). We report results of this exercises in Panel A of Table 9. When we consider all crime categories, we see a consistent negative effect, suggesting that sending a defendant to prison not only reduces his own probability of going to prison as shown in the previous section, but also reduces the probability that his colleagues commit financial crimes. However, the estimates are very noisy and are generally not significant.

Next, in Panels B-D we examine results for subcategories of crimes. We focus on the two largest sets of crimes: fraud which makes up 60% of all cases and business offences which makes up 15% of all cases, and group the remaining 25% in an "Other Financial Crimes" category in Panel D. First, it is worth noting that of these groups, we only have a significant first stage for Fraud. This is not surprising given the much smaller sample sizes reported in column 1, consisting of the number of defendants that can be used in the analysis to assess impacts on colleagues. While we report all results for Business Offences (Panel C) and Other Financial Crimes (Panel D) for completeness, given the lack of a significant first stage these estimates are impossible to interpret. Turning to Fraud in Panel B, we see that the negative overall effect appears to be largely driven by fraud cases. For fraud cases we consistently see a negative and significant effect of observing one's colleague being sent to prison on the likelihood an individual commits financial crimes. The effects are about half the size of the own effects on defendants, but still suggest large potential spillovers of criminal sentences on peer behavior. This is an important finding as it suggests a general deterrence role of a prison sentence beyond just its impact on the defendant.

Next, we estimate the effects of sentencing a defendant to prison on his colleague's outcomes by the size of the firm. We split the sample into firms with 30 employees or less, 50 employees or less, and 70 employees or less. We focus on these small and medium sized firms, as it is much more likely in these cases that the defendant knew the other employees in the firm. In much larger firms, defendants may not interact with most other employees.

This could be why we do not have precise estimates in the main exercise (although they are consistently negative), since in that case we are including many observations for other employees in the firm with whom the defendant never interacted. Results are found in Table 10. We find negative effects in all cases. These effects are largely significant for firms with 50 or fewer and 70 or fewer employees. This suggest that a prison sentence can cause a general deterrence effect on colleagues who interact with the defendant.

8 Impacts of Financial Crimes on Firms

[This work is in progress as we only just received more detailed data on firm outcomes. Our data consists of the firm's balance sheet each year. We are in the process of estimating the impact of a financial crime on relevant firm outcomes when the crime happens in an event study framework.

In addition, we will look at the causal impact of sending a defendant to prison on firm outcomes. If sending a defendant to prison causes colleagues to commit less financial crimes (as we showed in the previous section), this could potentially increase firm profits if financial crimes are costly to firms. Alternatively, it could also decrease firm profits if some of these crimes are committed to increase profits and improve outcomes for individuals associated with the firm. If we have the power for the analysis, we will use the judge instrument to estimate these effects and resolve this question. However, power may be an issue for this second exercise as we only have firm data for firms with 20 or more employees.]

9 Conclusion

In this paper we showed that financial crimes have grown 14% as a proportion of all crimes from 1992-2016, yet despite this growth in the importance of financial crimes, these defendants are punished much more leniently compared with defendants who commit other types of crimes. We also show that these defendants have a very high rate of recidivism, with just under half going on to commit an additional crime in the five years post sentencing, but they also look very different than other types of defendants. It is thus important to understand if harsher sanctions might play a role in stemming the rise in financial crimes. Motivated by these facts, we estimated the impact of harsher sanctions, specifically a prison sentence, on the likelihood defendants recidivate.

Using random assignment to judges as an instrument to identify the causal impact of prison sentences on financial crime defendants, we find that the probability financial crime defendants re-offend decreases by 43.2 percentage points, without significant (or negative) impacts on labor market outcomes. We additionally find that there are important spillovers on colleagues, as a prison sentence also reduces the probability that a colleague commits a financial crime in the future. This suggests scope for policy makers to potentially use prison as one possible tool to reduced recidivism among financial crime defendants and reduce financial crimes through a broader deterrence effect, although much more research is needed to see if these results generalize to other contexts.

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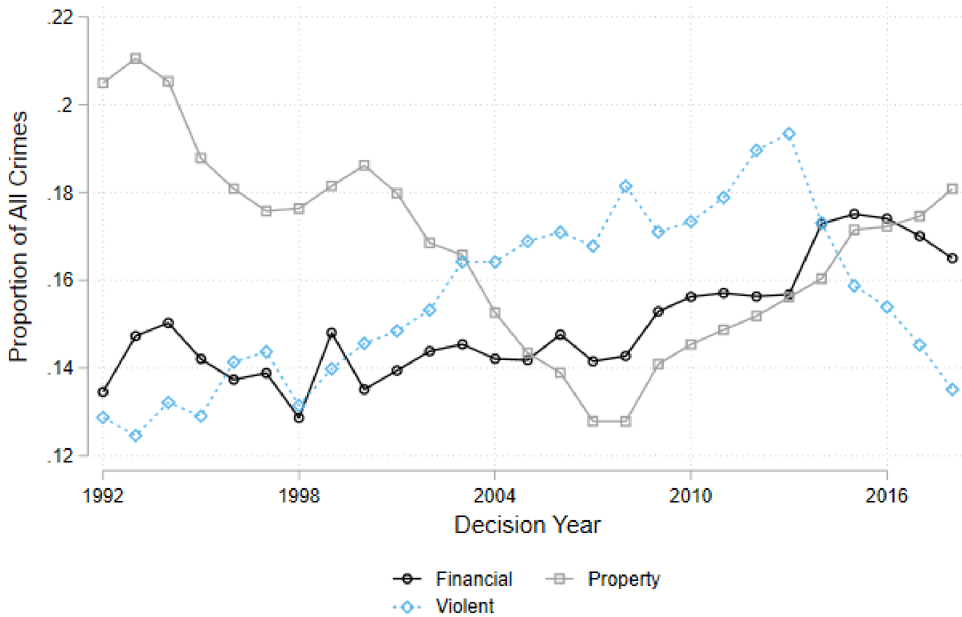
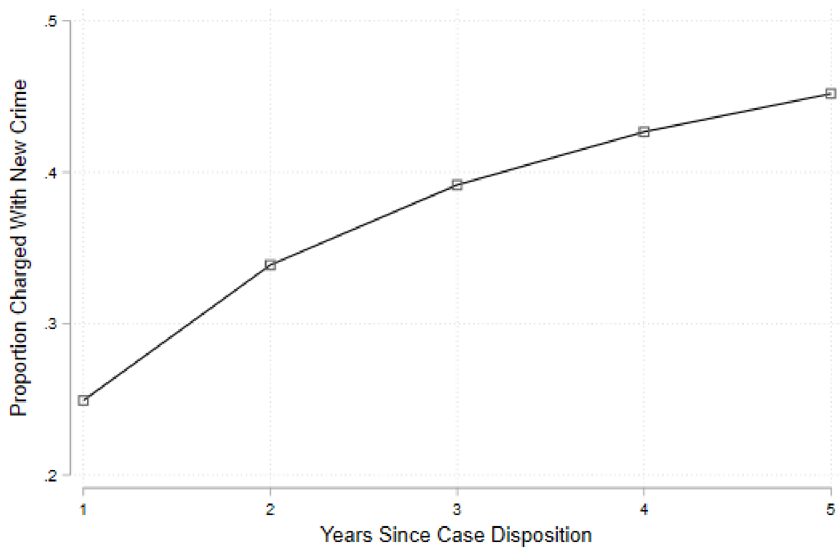
Figure 1: Proportion of Financial and Other Crime Types, 1992-2016



Note: Figure shows that share of all crimes that are financial crimes has increased over this 24 year interval. Data as defined in Section 2.

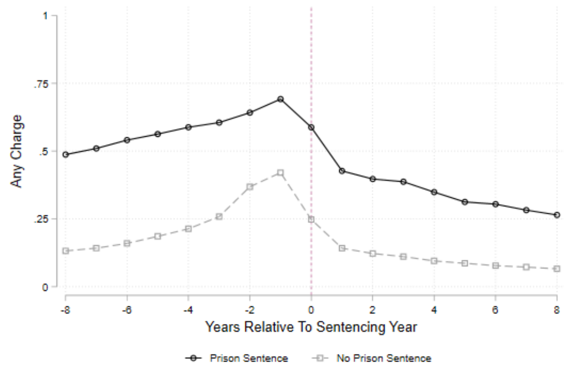
Figure 2: Recidivism for Financial Crime Defendants



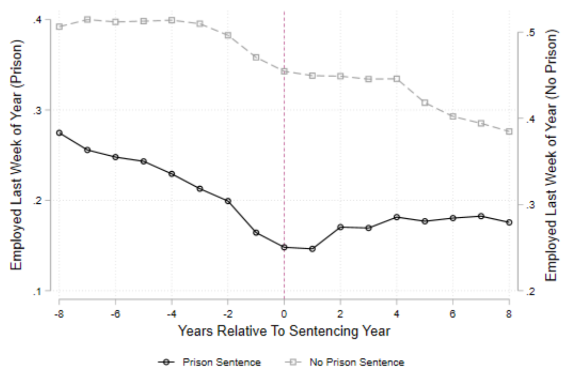
Note: Figure shows the proportion of financial crime defendants who re-offend the first year after sentencing, the first two years after sentencing, and so on. Data as defined in Section 2.

Figure 3: Raw Patterns of Employment and Earnings Around the Time of Sentencing

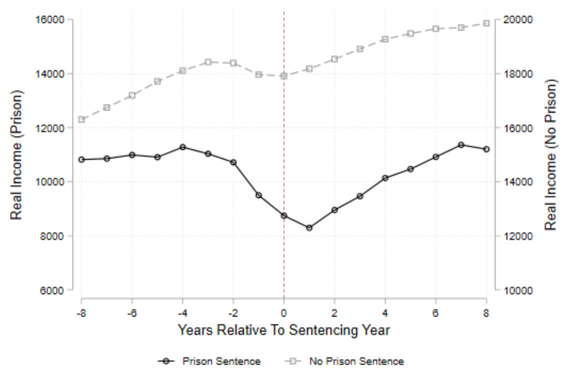
(a) Any Charges



(b) Employment



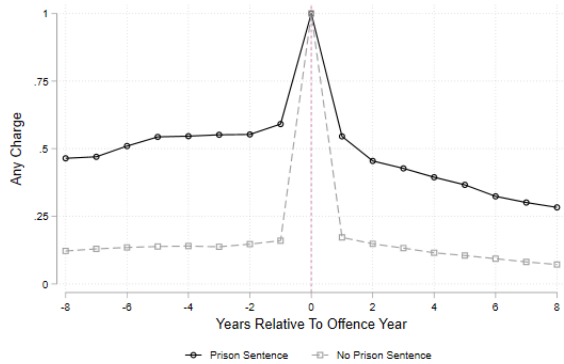
(c) Earnings



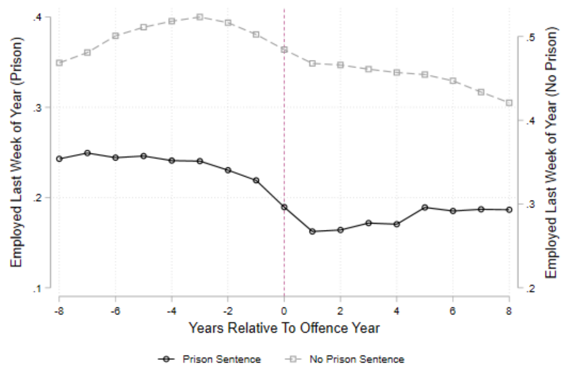
Note: Panel A (B) shows employment (earnings) of defendants 8 years before and 8 years after sentencing separately for prison as well as the other two most common punishments (probation and fines). Employment and earnings are measured at the end of the year. Sample construction and data as defined in Section2.

Figure 4: Raw Patterns of Employment and Earnings Around the Time of the Crime

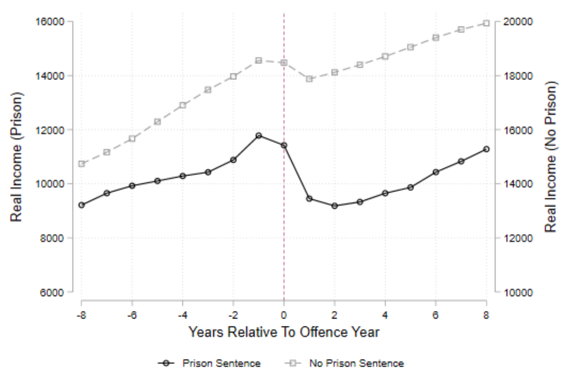
(a) Any Charges



(b) Employment



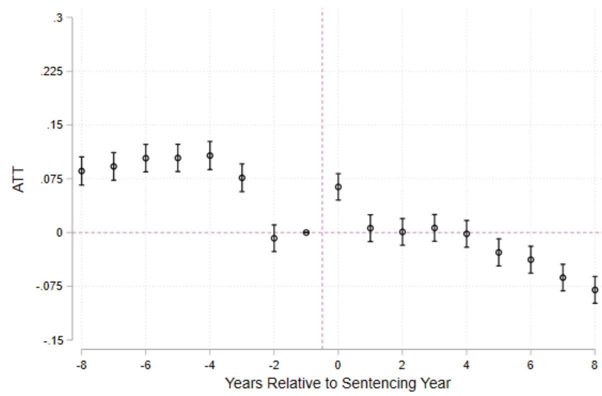
(c) Earnings



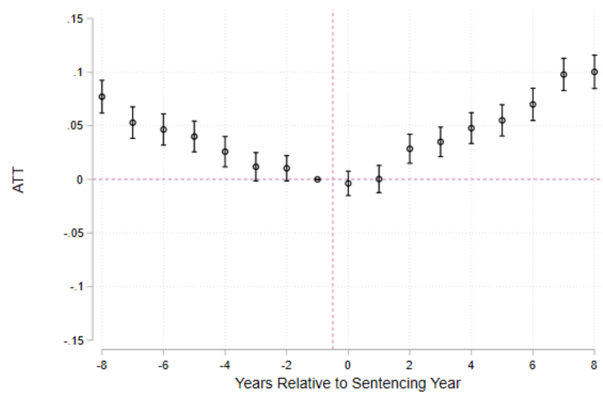
Note: Panel A (B) shows employment (earnings) of defendants 8 years before and 8 years after the crime separately for prison as well as the other two most common punishments (probation and fines). Employment and earnings are measured at the end of the year. Sample construction and data as defined in Section2.

Figure 5: Event Study Estimates of Recidivism, Employment, and Earnings Around the Time of Sentencing

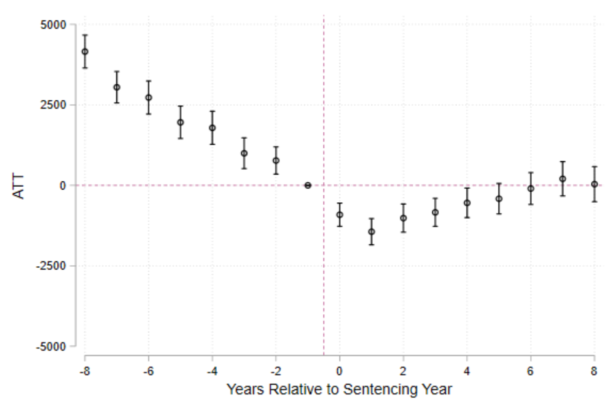
(a) Any Charges



(b) Employment

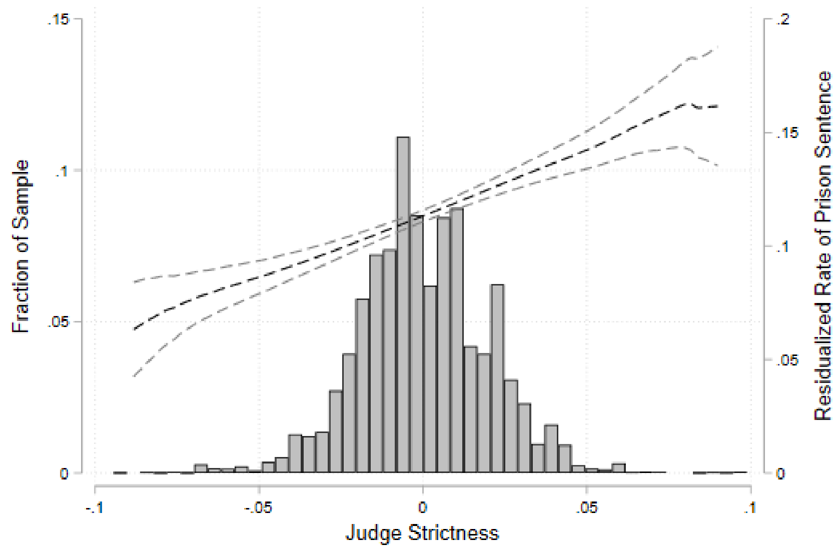


(c) Earnings



Note: Panel A shows re-offending, Panel B shows employment and Panel C shows earnings of defendants 8 years before and 8 years after the sentencing separately for prison as well as the other two most common punishments (probation and fines), estimated in an event study framework as described in Section 4. Re-offending takes the value 1 each year if the individual commits a financial crime in a given year. Employment and earnings are measured at the end of the year. Sample construction and data as defined in Section 2.

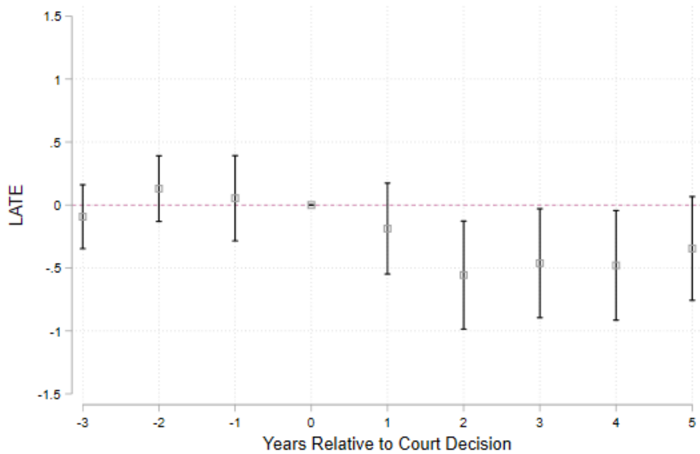
Figure 6: Variation in Judge Stringency and First Stage



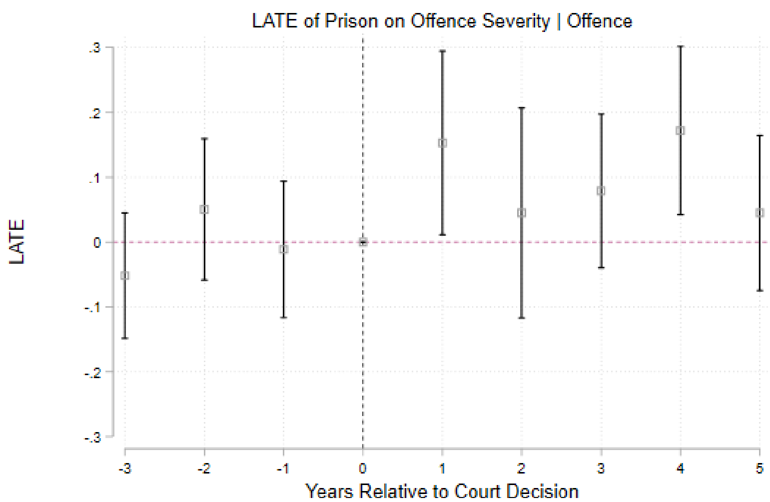
Notes: Figure is a graphical representations of the instrument of randomized judge assignment. The histogram represents the distribution of individual judges' stringency measures, which capture how strict each judge is after removing court by year by crime type fixed effects. The solid line is a nonparametric 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.

Figure 7: IV Estimates of the Impact of Prison on Defendant Recidivism and Crime Severity

(a) Recidivism



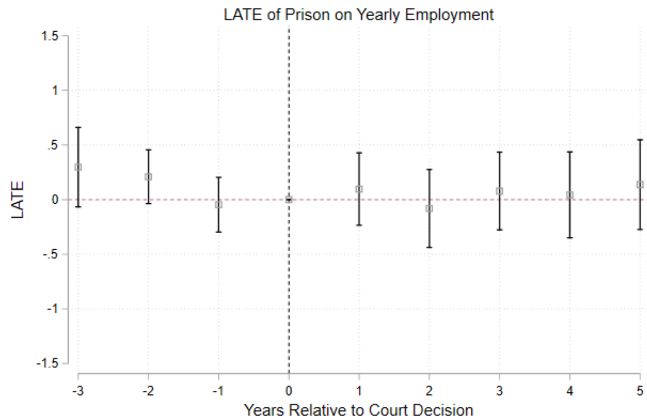
(b) Severity of Crime Conditional on Committing a Crime



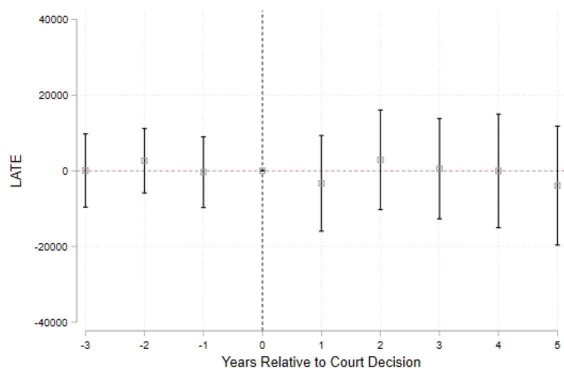
Note: Figure Panel A shows the impact of being quasi-randomly assigned to prison on recidivism of defendants 3 years before (these years are a placebo check) and 5 years after the sentencing using the identification approach of random assignment to judges as described in Section 5. In the 5 years after the effects are cumulative (i.e. charged 1 year after, charged within 2 years after, and so on). Panel B depicts the impact of being sent to prison on severity of crime, but only for those who commit a crime. Severity of crime is measured as the leave-out mean of those sent to prison within the crime category, as described in the text. 90% confidence intervals depicted. Sample construction and data as defined in Section 2.

Figure 8: IV Estimates of the Impact of Prison on Defendant Labor Market Outcomes

(a) Employment



(b) Earnings



Note: Panel A (B) shows the impact of being quasi-randomly assigned to prison on employment (earnings) of defendants 3 years before and 5 years after the sentencing using the identification approach of random assignment to judges as described in Section 5. Employment and earnings are measured at the end of the year, and the figures show the annual effects. Sample construction and data as defined in Section2.

Table 1: Financial Crimes Sub-Categories

	Proportion of Sample	Proportion Sent to Prison
Fraud	0.599	0.119
Business Offences	0.147	0.065
Forgery	0.091	0.181
Laundering	0.069	0.144
Political Corruption	0.008	0.000
Other	0.086	0.070

Notes: Unit of observation is individual/case level. All cases 2000 - 2016. Table shows the proportion of total financial crimes of relevant sub-categories, including all sub-categories that make up 5% or more or the data.

Table 2: Summary Statistics for Individuals Committing Different Types of Crimes

	Financial (1)	Propety (2)	Drug (3)	Violent (4)	Other (5)
Age at Conviction	43.03	35.55	31.74	37.73	41.77
	(10.13)	(9.496)	(7.459)	(9.576)	(10.79)
Female	0.298	0.218	0.182	0.157	0.148
	(0.457)	(0.413)	(0.386)	(0.364)	(0.355)
Swedish Speaking	0.000536	0.000502	0.00120	0.000410	0.000307
	(0.0232)	(0.0224)	(0.0346)	(0.0202)	(0.0175)
Earned Income	30851.3	18713.3	18601.7	27898.2	31135.5
	(23628.9)	(14455.5)	(14149.8)	(17689.4)	(21338.8)
Wages	26570.7	15112.6	15160.5	24783.3	26978.0
	(23820.0)	(15008.3)	(14258.9)	(18585.7)	(22414.0)
Unemployed	0.0966	0.215	0.202	0.115	0.0989
	(0.295)	(0.411)	(0.401)	(0.319)	(0.299)
Student	0.0214	0.0296	0.0444	0.0215	0.0203
	(0.145)	(0.170)	(0.206)	(0.145)	(0.141)
Office Workers	0.224	0.114	0.120	0.161	0.185
	(0.417)	(0.318)	(0.325)	(0.367)	(0.389)
Upper Management	0.160	0.0417	0.0342	0.0798	0.139
	(0.367)	(0.200)	(0.182)	(0.271)	(0.346)
College Degree	0.305	0.123	0.0997	0.168	0.257
	(0.461)	(0.329)	(0.300)	(0.374)	(0.437)
Num. of Children	1.036	0.706	0.493	0.948	0.870
	(1.175)	(0.989)	(0.846)	(1.131)	(1.110)
N	57253	37199	22444	80455	33616

Notes: Unit of observation is individual/case level. All cases 2000 - 2013, our estimation sample for this paper for the IV estimation. Earnings and employment measured at the end of the year. All variables measured the year before the crime

Table 3: Summary Statistics on Types of Punishment for Different Types of Crimes

	Financial (1)	Propety (2)	Drug (3)	Violent (4)	Other (5)
Prison	0.113	0.356	0.216	0.132	0.102
	(0.317)	(0.479)	(0.412)	(0.339)	(0.303)
Probation	0.253	0.137	0.170	0.193	0.164
	(0.435)	(0.343)	(0.375)	(0.395)	(0.370)
Fine	0.487	0.409	0.573	0.550	0.608
	(0.500)	(0.492)	(0.495)	(0.498)	(0.488)
Sentence	77.74	100.3	163.5	104.0	66.33
	(401.9)	(639.0)	(563.3)	(427.4)	(356.9)
Not Guilty	0.120	0.0630	0.0229	0.0811	0.0766
	(0.325)	(0.243)	(0.150)	(0.273)	(0.266)
Prev. Prison Spells	1.139	4.566	1.943	1.006	0.856
	(4.645)	(8.777)	(5.657)	(3.768)	(3.821)
N	57253	37199	22444	80455	33616

Notes: Table shows statistics on the severity of punishment (percent sent to prison, probation, or fines, length of prison sentence, percent not guilty) for the four major crime categories: Financial crimes (column 1) as compared with drug crimes, property crimes, and violent crimes. Unit of observation is individual/case level. All cases 2000 - 2016.

Table 4: Balance Check

	Prison (1)	Judge Strictness (2)
Age	0.000225	-0.00000943
	(0.000138)	(0.0000100)
Female	-0.0238***	0.000211
	(0.00279)	(0.000203)
Children	-0.00388***	-0.0000137
	(0.00106)	(0.0000813)
Married	0.00660*	-0.000254
	(0.00299)	(0.000222)
Secondary Degree	-0.0106***	-0.0000742
	(0.00318)	(0.000194)
Post Secondary Degree	-0.00624	-0.000516
	(0.00392)	(0.000301)
Employed	-0.0196***	-0.000350
	(0.00291)	(0.000208)
Income	-0.000000184*	1.29e-08*
	(7.67e-08)	(6.57e-09)
Native Born	0.0224***	0.0000823
	(0.00410)	(0.000365)
Prison at time t-1	0.105***	0.0000599
	(0.00424)	(0.000224)
Prison at time t-2,t-3	0.403***	0.000277
	(0.00847)	(0.000339)
Charge at time t-2,t-3	0.0458***	0.000365
	(0.00315)	(0.000231)
P-Value	0.000	0.221
F-Statistic	432.113	1.288
N	57252	57252

Notes: Table shows that a variety of characteristics are highly predictive of a prison sentence (column 1) but not predictive of judge stringency (column 2). 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

Table 5: First Stage

	(1)	(2)
Judge Stringency	0.563***	0.449***
	(0.0832)	(0.0643)
Outcome Mean	.109	.109
Court by Year FEs	Y	Y
Controls	N	Y
Observations	57252	57252

Notes: Table shows first stage estimates with (column 1) and without (column 2) additional controls (both columns include courty by year fixed effects as controls). Standard errors appear in parentheses. *p<0.05, **p<0.01, ***p<0.001

Table 6: Disaggregate Impact of Prison on Recidivism Post Sentencing

	1 year after	1-2 years after	1-3 Years after
OLS: No Controls	0.385***	0.436***	0.444***
	(0.008)	(0.007)	(0.007)
OLS: Conrols	0.092***	0.093***	0.086***
	(0.008)	(0.007)	(0.007)
OLS: Reweighted	0.089***	0.087***	0.075***
	(0.010)	(0.008)	(0.008)
IV	-0.171	-0.526**	-0.432**
	(0.181)	(0.218)	(0.219)
Outcome Mean	.248	.338	.391
CourtXYear FE	Y	Y	Y
N	56582	56582	56582

Notes: The table reports OLS and IV estimates of the impact of prison on 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

Table 7: Disaggregate Impact of Prison on Labor Market Outcomes Post Sentencing

	1st year (1)	2nd year (2)	3rd Year (3)	4th year (4)	5th year (5)
Panel A: Earnings					
OLS: No Controls	-8497.002***	-8546.288***	-8747.859***	-8856.531***	-8749.257***
	(189.637)	(190.768)	(203.416)	(211.719)	(224.807)
OLS: Conrols	-1225.879***	-1125.779***	-1176.425***	-1164.366***	-980.542***
	(184.428)	(188.273)	(197.468)	(205.262)	(218.521)
OLS: Reweighted	-364.281**	-298.388*	-426.694**	-492.632**	-250.912
	(110.604)	(127.151)	(134.839)	(151.172)	(171.433)
IV	-3304.939	2909.816	551.154	-26.039	-3914.329
	(6459.314)	(6714.974)	(6769.471)	(7663.065)	(8020.959)
Panel B: Employment					
OLS: No Controls	-0.302***	-0.280***	-0.273***	-0.262***	-0.252***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
OLS: Controls	-0.048***	-0.033***	-0.037***	-0.026***	-0.027***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
OLS: Reweighted	-0.025***	-0.016*	-0.025***	-0.014*	-0.015*
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
IV	0.101	-0.077	0.083	0.048	0.142
	(0.169)	(0.182)	(0.182)	(0.201)	(0.209)
Earnings Mean	10009.179	10389.563	10740.334	11066.419	11409.081
Earnings Mean	.398	.393	.385	.381	.375
Court by Year FE	Y	Y	Y	Y	Y
N	56799	56799	56799	56799	56799

Notes: The table reports OLS and IV estimates of the impact of prison on on the probability employment (Panel A) and the impact on earnings (Panel B) 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

Table 8: Cumulative 3 year Impact of Prison on Recidivism, Employment and Earnings Post Sentencing

	Recidivism (1)	Employment (2)	Earnings (3)
OLS: No Controls	0.437***	-0.305***	-25441.536***
	(0.006)	(0.008)	(510.204)
OLS: Conrols	0.083***	-0.038***	-3405.883***
	(0.006)	(0.007)	(501.424)
OLS: Reweighted	0.069***	-0.024**	-1049.805***
	(0.007)	(0.008)	(293.230)
IV	-0.432*	0.103	-589.397
	(0.219)	(0.177)	(18596.772)
Outcome Mean	.386	.508	30527.338
CourtXYear FE	Y	Y	Y
N	57252	56799	57252

Notes: The table reports OLS and IV estimates of the impact of prison on on the probability of being charged with a crime, employment, and earnings in the three years after sentencing. IV estimates include controls. Standard errors clustered two-way at judge and defendant level appear in parentheses.

Table 9: Spillover Impact of Prison on Co-workers of Financial Criminals

		Colleague Financial Crime Within		
	First Stage	1 Year	1-2 Years	1-3 Years
	(1)	(2)	(3)	(4)
Panel A: All Financial Crimes				
IV Estimate	0.453***	-0.046	-0.037	-0.040
	(0.119)	(0.082)	(0.097)	(0.103)
N	10050	101059	101059	101059
Panel B: Fraud				
IV Estimate	0.442***	-0.181**	-0.204**	-0.247**
	(0.132)	(0.082)	(0.101)	(0.114)
N	5707	55359	55359	55359
Panel C: Business Offences				
IV Estimate	0.455	-0.058	-0.005	0.115
	(0.337)	(0.226)	(0.231)	(0.233)
N	2010	21238	21238	21238
Panel D: Other Financial Crimes				
IV Estimate	0.333	0.073	0.070	-0.011
	(0.235)	(0.141)	(0.167)	(0.183)
N	2098	24425	24425	24425

Notes: Column 1 reports first stage on defendant outcome for each category. Note that the number of observations in this column is smaller than for the others, as it includes only the defendants (the relevant observations for the first stage), whereas the other columns consist of all colleagues. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Spillover Impact of Prison on Coworkers of Financial Criminals by Frim Size

	1 Year	1-2 Years	1-3 Years
	(1)	(2)	(3)
Panel A: 30 Employees or Fewer			
IV Estimate	-0.121	-0.124	-0.192
	(0.138)	(0.160)	(0.179)
N	34496	34496	34496
Panel B: 50 Employees or Fewer			
IV Estimate	-0.181**	-0.204**	-0.247**
	(0.082)	(0.101)	(0.114)
N	55359	55359	55359
Panel C: 70 Employees or Fewer			
IV Estimate	-0.192*	-0.185	-0.233*
	(0.101)	(0.113)	(0.132)
N	73202	73202	73202

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix

A Details on Classification of Financial Crime

Below is a list of every crime category in the Finnish judicial system that we categorize as financial crimes and thus include in our analysis.

Category 1: Fraud (60%)

16 - offence against public authorities

28 - embezzlement

29 - offences against public finances (tax fraud)

36 - fraud

37 - counterfeiting/means of payment fraud

39 - offences by a debtor

44 - unlicensed medical practice

61 - unlicensed traffic offences (bus/tax)

Category 2: Business Offences (15%)

30 - accounting offences

46 - smuggling/import offences

47, 73, 78 - workplace/employment offences

69 - business infractions

49, 65 - copyright issues

51 - securities offences

Category 3: Forgery (9%)

33 - forgery offences

Category 4: Laundering (7%)

32 - Receiving and money laundering offences

Category 5: Political Corruption (<1%)

40 - Offences in office

Category 6: Other (<9%)

15 - False Statement

17 - Lottery offences

24 - Defamation

31 - extortion

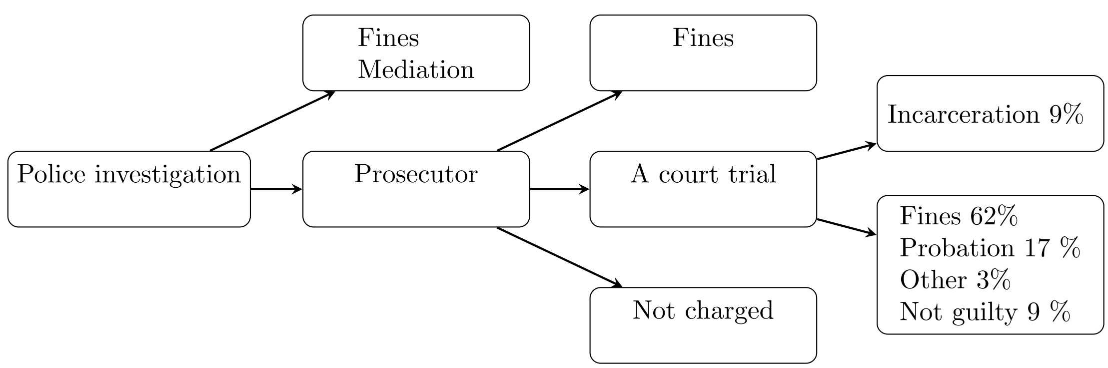
38 - Data and communications offences

48 - Environmental offences

67,70,82 - mixed bag (.3%)

B Additional Figures

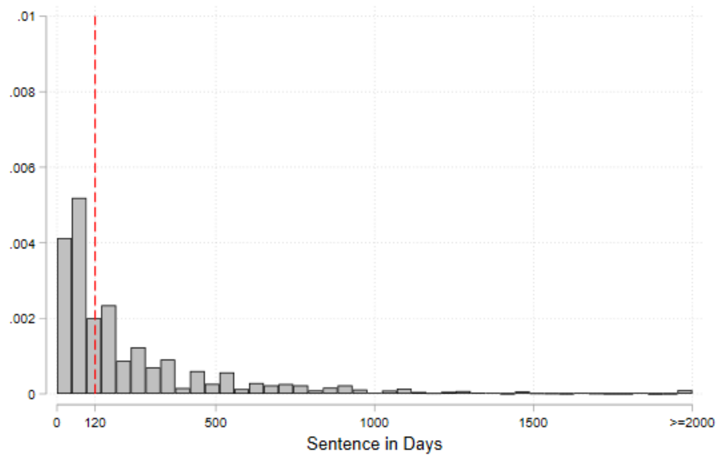
Figure B.1: Sentencing Process and Trial Outcomes in Finland



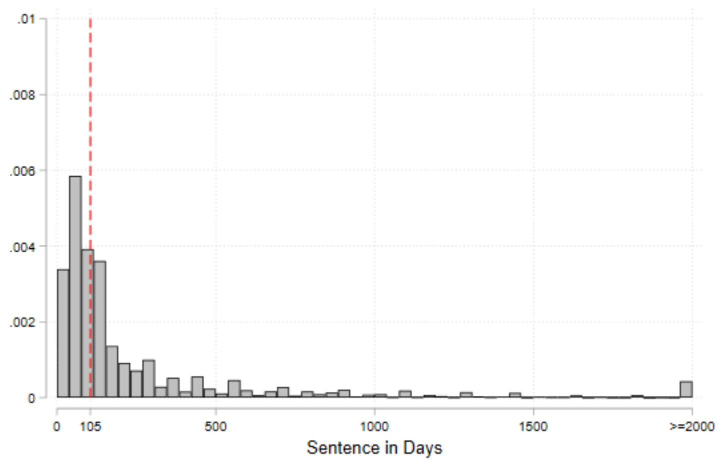
Notes: The figure provides a visual representation of the sentencing process in Finland, and provides information for final sentences for financial crimes.

Figure B.2: Distribution of Sentence Length for Financial Crimes versus Other Crimes

(a) Financial Crimes

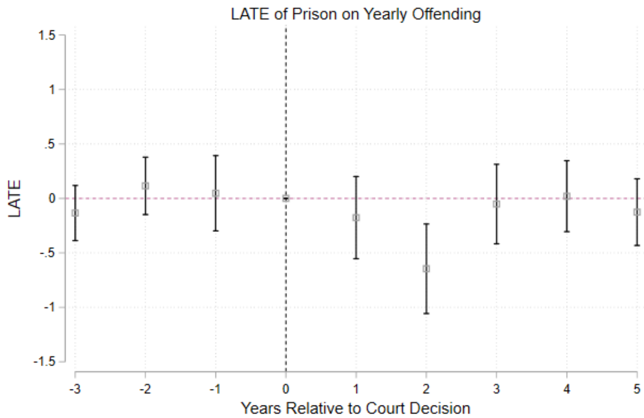


(b) Other Crimes



Note: Panel A (B) shows histogram of sentence length conditional on being sent to prison for financial crimes (all other crimes). Sample construction and data as defined in Section2.

Figure B.3: Yearly IV Estimates of the Impact of Prison on Recidivism



Note: Figure shows the impact of being quasi-randomly assigned to prison on recidivism of defendants 3 years before and 5 years after the sentencing using the identification approach of random assignment to judges as described in Section 5. Figure show the annual effects, as opposed to the cumulative effects shown in the main results. Sample construction and data as defined in Section2.

C Additional Tables

Table C.1: Summary Statistics for Individuals Committing Different Types of Crimes (All Cases, Without Estimate Sample Restrictions)

	Financial (1)	Propety (2)	Drug (3)	Violent (4)	Other (5)
Age at Conviction	40.92	29.90	27.83	34.02	38.34
	(11.35)	(10.44)	(7.741)	(11.00)	(12.74)
Female	0.288	0.158	0.164	0.146	0.142
	(0.453)	(0.365)	(0.370)	(0.354)	(0.349)
Swedish Speaking	0.0396	0.0342	0.0542	0.0365	0.0469
	(0.195)	(0.182)	(0.226)	(0.188)	(0.211)
Earned Income	28318.4	15744.2	15531.4	24570.4	27423.4
	(22673.5)	(12919.0)	(12417.0)	(16915.1)	(20362.3)
Wages	24233.3	12892.6	12818.5	21834.0	23979.9
	(22655.1)	(13060.2)	(12513.0)	(17476.3)	(20736.6)
Unemployed	0.101	0.183	0.181	0.111	0.109
	(0.301)	(0.387)	(0.385)	(0.315)	(0.312)
Student	0.0244	0.0465	0.0578	0.0303	0.0263
	(0.154)	(0.210)	(0.233)	(0.171)	(0.160)
Office Workers	0.202	0.0950	0.102	0.137	0.140
	(0.402)	(0.293)	(0.303)	(0.344)	(0.347)
Upper Management	0.136	0.0322	0.0267	0.0628	0.105
	(0.343)	(0.176)	(0.161)	(0.243)	(0.306)
College Degree	0.271	0.0778	0.0645	0.135	0.201
	(0.444)	(0.268)	(0.246)	(0.342)	(0.401)
Num. of Children	0.972	0.612	0.447	0.829	0.755
	(1.151)	(0.947)	(0.804)	(1.079)	(1.047)
N	138366	143807	54070	158845	447295

Notes: Unit of observation is individual/case level. All cases 2000 - 2013, our estimation sample for this paper for the IV estimation. Earnings and employment measured at the end of the year. All variables measured the year before the crime

Table C.2: Summary Statistics on Types of Punishment for Different Types of Crimes (All Cases, Without Estimate Sample Restrictions)

	Financial (1)	Propety (2)	Drug (3)	Violent (4)	Other (5)
Prison	0.0851	0.190	0.152	0.109	0.0840
	(0.279)	(0.392)	(0.359)	(0.312)	(0.277)
Probation	0.174	0.133	0.148	0.189	0.265
	(0.380)	(0.340)	(0.356)	(0.391)	(0.441)
Fine	0.620	0.591	0.656	0.583	0.773
	(0.485)	(0.492)	(0.475)	(0.493)	(0.419)
Sentence	53.69	62.17	111.7	93.74	38.06
	(312.6)	(429.8)	(417.5)	(474.6)	(205.8)
Not Guilty	0.0920	0.0511	0.0208	0.0759	0.0261
	(0.289)	(0.220)	(0.143)	(0.265)	(0.160)
Prev. Prison Spells	0.783	2.035	1.234	0.739	0.710
	(3.742)	(5.972)	(4.430)	(3.157)	(3.502)
N	138366	143807	54070	158845	447295

Notes: Table shows statistics on the severity of punishment (percent sent to prison, probation, or fines, length of prison sentence, percent not guilty) for the four major crime categories: Financial crimes (column 1) as compared with drug crimes, property crimes, and violent crimes. Unit of observation is individual/case level. All cases 2000 - 2016.

Table C.4: Summary Stats: Financial Crime by Punishment Type

	All	Prison	No Prison
Age at Conviction	43.36	42.59	43.39
	(10.55)	(10.27)	(10.56)
Female	0.302	0.106	0.312
	(0.459)	(0.308)	(0.463)
Swedish Speaking	0.000729	0.00127	0.000704
	(0.0270)	(0.0357)	(0.0265)
Earned Income	23035.0	10739.0	23601.6
	(21437.1)	(14356.2)	(21539.6)
Wages	17468.4	6082.0	17993.2
	(21581.9)	(12563.9)	(21765.3)
Unemployed	0.164	0.237	0.161
	(0.370)	(0.425)	(0.367)
Student	0.0317	0.0394	0.0313
	(0.175)	(0.195)	(0.174)
White Collar Worker	0.174	0.118	0.177
	(0.379)	(0.323)	(0.381)
Upper Management	0.121	0.0471	0.124
	(0.326)	(0.212)	(0.330)
College Degree	0.274	0.169	0.279
	(0.446)	(0.375)	(0.448)
Num. of Children	0.982	0.654	0.997
	(1.177)	(1.003)	(1.183)
Criminal Charge t-2,t-3	0.180	0.627	0.159
	(0.384)	(0.484)	(0.366)
N	56583	6452	50131

Notes: Table shows descriptive statistics for those sent to prison (column 1) versus those who commit financial crimes but who are not sent to prison (column 2).

Table C.5: Sample size after restrictions

	Cases	Defendants	Judges	Courts
All court cases	-	138366	NA	NA
Assigned a Judge	94865	69066	3178	65
Drop training Judges	79983	60544	1008	65
Drop Swedish Speaking	77623	58665	1004	65
Drop Judges with < 100 Cases	53762	42731	308	60
Drop Courts with < 2 Judges	53753	42724	308	60

Notes: The table reports the sample size of cases, defendants, judges, and courts after imposing each restriction specified in each row. In all rows we have already removed traffic cases and juveniles as described in the main text. 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.

Table C.6: Monotonicity of the Instrument

	Baseline instrument	Reverse-sample instrument
	First Stage	First Stage
sub-sample:	P(Incarcerated)	P(Incarcerated)
Main Estimation Sample		
Estimate	0.449***	0.366***
	(0.064)	(0.057)
Observations	57252	57252
Over 30 years old		
Estimate	0.477***	0.110**
	(0.069)	(0.037)
Observations	47149	47149
Under 30 years old		
Estimate	0.323*	0.280*
	(0.145)	(0.138)
Observations	10054	10054
Any post-compulsary education		
Estimate	0.341***	0.187***
	(0.068)	(0.048)
Observations	30044	30044
No post-compulsary education		
Estimate	0.574***	0.457***
	(0.109)	(0.109)
Observations	27183	27183
Marrried		
Estimate	0.407***	0.332***
	(0.099)	(0.087)
Observations	18660	18660
Not married		
Estimate	0.465***	0.181**
	(0.077)	(0.064)
Observations	38566	38566
Previously employed		
Estimate	0.174**	0.115**
	(0.059)	(0.042)
Observations	24031	24031
Previously not employed		
Estimate	0.661***	0.381*
	(0.096)	(0.153)
Observations	33192	33192

Notes: Column 1 estimates the first-stage Equation 3 separately for different subgroups. Our dependent variable is an indicator for prison. The independent variable is the prison stringency measure we use in the main analysis. Column 2 estimates the first-stage Equation 3 in different subsamples, but constructs 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Summary Stats: Colleagues of Financial Offenders (All)

	<= 50 Employees	> 50 Employees
Age	39.12	41.26
	(12.03)	(11.35)
Female	0.427	0.528
	(0.495)	(0.499)
Swedish Speaking	0.0252	0.0305
	(0.157)	(0.172)
Earned Income	25322.0	31472.8
	(14885.4)	(17476.4)
Wages	23678.3	30165.8
	(15168.3)	(17904.0)
Upper Management	0.0962	0.161
	(0.295)	(0.367)
College Degree	0.278	0.429
	(0.448)	(0.495)
Num. of Children	0.901	0.907
	(1.100)	(1.084)
Previous Prison t-3	0.00187	0.000240
	(0.0432)	(0.0155)
Previous Charge t-3	0.0464	0.0173
	(0.210)	(0.131)
Number of Firms	9657	3141
Number of Workers	134167	1718893

Table C.10: Summary Stats: Colleagues of Financial Offenders (Fraud)

	<= 50 Employees	> 50 Employees
Age	38.54	40.78
	(12.16)	(11.62)
Female	0.456	0.564
	(0.498)	(0.496)
Swedish Speaking	0.0245	0.0328
	(0.154)	(0.178)
Earned Income	24088.3	29718.0
	(14589.1)	(17267.6)
Wages	22375.7	28382.3
	(14836.6)	(17675.0)
Upper Management	0.0850	0.147
	(0.279)	(0.354)
College Degree	0.257	0.404
	(0.437)	(0.491)
Num. of Children	0.888	0.892
	(1.090)	(1.079)
Previous Prison t-3	0.00192	0.000256
	(0.0438)	(0.0160)
Previous Charge t-3	0.0464	0.0184
	(0.210)	(0.134)
Number of Firms	5619	1766
Number of Workers	72164	832360