Financial Crime and Punishment *

Kristiina Huttunen,† Martti Kaila,‡ David Macdonald§ and Emily Nix¶

Abstract

Financial crimes impose significant costs on society. This paper investigates whether prison sentences reduce financial crimes. Using random assignment of judges to identify causal impacts of prison sentences from 2000 to 2018 in Finland, we show that prison reduces defendant reoffending by 42.9 percentage points in the three years following the crime. We also find that a prison sentence reduces the likelihood that a defendant's colleagues commit financial crimes in the future, suggesting important spillover effects of harsher punishments for financial misconduct.

Keywords: Financial misconduct, prison, colleague spillovers

JEL codes: K14, K42, G38, G50

*We thank Kenneth Ahern, Tom Chang, John Matsusaka, Kevin Murphy, Chris Parsons, and our discussant Julian Zhang along with seminar participants at USC Marshall and the LMU California Corporate Crime conference for helpful comments. This paper was supported by an Academy of Finland Grant.

†VATT Institute for Economic Research, Aalto University and IZA, kristiina.huttunen@aalto.fi

‡University of Helsinki, martti.kaila@helsinki.fi

§Aalto University

¶Corresponding Author: University of Southern California Marshall School of Business, enix@usc.edu

1 Introduction

Financial crimes, including transgressions like fraud and accounting offences, impose significant costs on society. The U.S. Department of Justice estimates that fraud alone costs 40-50 billion dollars annually (FBI, 2021) and almost 10% of United States residents have been victims of identity fraud (Piquero, 2018). 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).

However, despite the large costs and many victims of financial misconduct, those who commit financial crimes are sent to prison less often compared with those who commit other types of nonviolent crimes. We find that 11% of defendants who commit financial crimes are sentenced to prison in Finland, a lower rate compared with nonviolent property crimes (36% incarcerated) and nonviolent drug crimes (22% incarcerated). The fact that financial crimes are costly but often result in less severe repercussions compared with other nonviolent crimes is well known. For example, Taub (2020) states that financially based crime "costs victims an estimated $300 billion to $800 billion per year" and "street-level 'property' crimes, including burglary, larceny and theft, cost us far less — around $16 billion annually, according to the F.B.I.". Taub (2020) goes on to document the (relative) lack of consequences for financial crimes. This juxtaposition of large costs of financial crimes with lesser punishments prompts a natural question: Would harsher sanctions for financial crimes reduce their frequency?

In this paper we estimate the causal impact of a prison sentence on the likelihood a financial-crime defendant reoffends. Additionally, we show that a prison sentence has important implications for the criminal behavior of the defendant's colleagues. Reducing future charges amongst financial-crime defendants is particularly relevant since we show that almost half reoffend within five years, consistent with what Egan *et al.* (2019) find for financial advisers who commit misconduct. This implies that reducing criminality amongst those already caught for financial crimes could play an important role in preventing these crimes. However, whether prison reduces defendant reoffending is theoretically ambiguous. On the one hand, prison could break a defendant's ties to the labor market and society, leading them to commit more crimes. Alternatively, prison could rehabilitate or deter defendants, reducing reoffending. If prison sentences also reduce the criminality of colleagues, this would signal an important general deterrence effect of harsher punishments for financial crime defendants.

To complete our analysis, we construct unique population-level administrative data from Finland from 2000-2018 that allow us to identify defendants in financial-crime cases and link defendants to their labor market information and workplace at the time of the crime. We follow the European Financial and Economic Crime Centre's definition of financial and economic crimes when selecting crimes to include in the analysis. The most common types of crimes we study are fraud (60% of all cases), business offences (15%), forgery (9%), and money laundering (7%). We show that financial-crime defendants look very different from other nonviolent defendants on observable characteristics. They are more likely to be employed, have higher incomes, are six years older, are twice as likely to be college educated, and more than twice as likely to be in upper management.

To identify the causal impact of prison sentences on financial-crime defendants and their colleagues, we use an instrumental variable strategy that exploits the fact that in Finland cases are randomly assigned to judges by law, and these judges differ in how likely they are to send defendants to prison. To this end, we collected data on judges in conjunction with the National Court Registrar which we linked to the administrative defendant records. This instrument is necessary to identify the impact of prison given that we document extensive endogenous selection in who is sent to prison, along with differential trends in outcomes before sentencing, both of which will lead to bias in OLS or difference-in-difference estimates. This identification strategy to isolate causal impacts of punishments was originated in Kling (2006) and has since been used and further developed in a large literature

(Dobbie and Song, 2015; Aizer and Doyle, 2015; Bhuller *et al.*, 2020; Mueller-Smith, 2020; Cheng *et al.*, 2021; Chang and Schoar, 2022). We show support for randomization through balance checks. We also find a strong first stage: judge assignment is highly predictive of receiving a prison sentence.

Using our instrument we find that when a financial-crime defendant is sent to prison, the probability the defendant is charged with another crime in the three years post sentencing decreases by 42.9 percentage points. This is in sharp contrast to the OLS estimates which suggest that prison is associated with an increase in recidivism. We conclude that for financial-crime defendants, prison is effective at reducing recidivism. Turning to mechanisms, we rule out incapacitation and do not find strong evidence in favor of rehabilitation mediated through improved formal labor market outcomes, although these estimates are imprecise. This leaves us with specific deterrence[1] as a likely explanation, possibly combined with some other form of rehabilitation.

Next, we examine spillovers on peers' criminality as a possible broader deterrence effect of prison sentences. We find that sentencing an individual who has committed a financial crime to prison also reduces the probability their colleagues are charged with financial crimes. These effects are significant in the case of fraud, which make up almost 60% of our observations. We argue that these spillovers are most consistent with a general deterrence effect: Colleagues potentially update their beliefs of the likelihood of receiving a prison sentence and this deters them from committing crimes. We show that an alternative explanation, that colleagues are co-conspirators and so when the defendant is sent to prison this mechanically reduces their crimes as well, is not consistent with the data.

Our paper makes two main contributions. First, we contribute to the literature on financial misconduct by investigating how harsher sanctions affect defendants charged with such misconduct. This literature has largely focused on quantifying the extent and conse-

[1]Specific deterrence is when a defendant is deterred from committing future crimes by experiencing a punishment. For example, going to prison allows defendants to learn how unpleasant it is, and they reduce future offending to avoid returning.

quences of these acts for both individuals and firms. Egan *et al.* (2019) find that roughly half of financial advisers who commit financial misconduct are fired after being caught, consistent with our descriptive results on employment. They also find that after being fired for financial misconduct most financial advisers are easily rehired into new firms and commit more financial misconduct, suggesting that firing people is not sufficient to eliminate future misbehavior. Fich and Shivdasani (2007) and Gurun *et al.* (2018) show that financial fraud, one of the major categories we focus on in this paper, is quite costly to both firms and victims. We find that a financial crime is associated with a decrease in firm profits, providing suggestive evidence that there is a business case for reducing the amount of financial misconduct. Together, these papers demonstrate that financial crime is frequent and costly, motivating our interest in how to reduce these crimes. Our results provide the first rigorous empirical evidence on the role of prison to reduce financial misconduct.

As such, we also contribute to a smaller literature documenting other actions to reduce financial misconduct. Kowaleski *et al.* (2020) find that ethics exams for employees reduce financial misconduct, Honigsberg and Jacob (2021) estimate that removing records of misconduct via expungement increases reoffending, and Heese *et al.* (2021) show that a strong local press to document misconduct reduces its incidence. Relative to these papers, we examine a stronger and more direct consequence for financial misconduct: imprisonment. Moreover, in contrast to a number of papers in the prior literature that focus more narrowly on financial advisers (Honigsberg and Jacob, 2021; Egan *et al.*, 2019), we include anyone who commits any type of financial misconduct in our analysis.

Second, our paper is the first to investigate the causal effects of prison sentences on colleagues' criminal behavior. Previous studies investigating spillovers of prison sentences have focused on other family members or members of the same criminal networks (Norris *et al.*, 2021; Arteaga, 2020; Bhuller *et al.*, 2018; Billings, 2018; Dobbie *et al.*, 2018). More closely related to our paper is the literature documenting how financial criminality spills over to colleagues. Dimmock *et al.* (2018) show that there is contagion in perpetrating financial misconduct. They find that quasi-random exposure to those who commit financial misconduct increases financial misconduct of their colleagues. Battaglini *et al.* (2019) find that there are important spillovers in criminality between tax professionals and their clients, which could have large implications for government revenue (Artavanis *et al.*, 2016). These results help motivate and are consistent with our finding that there is also a spillover effect of observing a colleague receive a harsher punishment for financial misconduct.

This paper is organized as follows. Section 2 describes the institutional context, our data, and how we define financial crimes. This section also provides descriptive results on firm profits before and after an employee engages in financial misconduct. Section 3 provides descriptive results for individuals who commit financial crimes. Section 4 reviews our identification strategy and provides empirical support for our instrument. Section 5 reports the impacts of prison sentences on defendants and Section 6 examines effects of a prison sentence on colleagues' criminal behavior. In Section 7 we provide context for our estimates, including a discussion of external validity. Section 8 concludes.

2 Institutional Context and Data

2.1 Institutional Context

In Finland, most criminal cases begin once a police report has been filed. Upon completion of an initial investigation, the police refer the case to a prosecutor if there is significant evidence. The prosecutor then decides whether to formally charge the accused and proceed to a court trial. In order for a defendant to receive a prison sentence, he or she must appear before a judge in court, so in this paper we focus on defendants in court cases. Appendix Figure B.2 summarizes the criminal proceeding for cases that end up in district courts.

When a case arrives in a court, Finnish law mandates that it is randomly assigned to a judge or a panel of judges. This random assignment is key to our identification strategy. Because judges vary in their likelihood of assigning prison as a punishment, random assignment of cases provides exogenous variation in the punishment defendants receive. We use this to identify the causal impacts of prison.[2] We verified the randomization process through conversations with administrators in the courts, and we also provide empirical evidence that cases are assigned randomly in practice. A subset of judges might specialize in certain cases in larger courts, so the randomization occurs conditional on the type of crime committed, which we account for in the analysis.

A criminal case in court can be dealt with by either one judge or a panel of one professional judge and two to four lay judges.[3] In some very severe cases, a panel of three professional judges handle the case, but this almost never occurs for financial cases. When we use our judge stringency measure to identify effects of prison, we use the stringency of the professional judge.[4] In terms of choosing a sentence when there are lay judges, the professional judge first 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 found guilty. Last, they vote on the content of the punishment (e.g. length of prison sentence). The professional judge always votes first when there are lay judges.[5] Lay judges are also randomly assigned to cases.[6]

The Finnish criminal code determines the type of sentence and the minimum and maximum sentences the judge may consider. The most common sentence types are fines, probation, and prison. A prison sentence is only allowed if the criminal code specifies it as a

[2]Plea bargaining can cause problems in judge fixed-effect designs, since being assigned a stricter judge may cause defendants to take a plea bargain. In our setting this is not possible. Plea bargaining has only been allowed from 2015 onward in Finland, after our estimation sample ends.

[3]Lay judges are politically appointed "assistant judges". They must meet several requirements: between the ages 25-65 (only 25-63 prior to 2014) and cannot hold another position in the court. They cannot work for the police or as a lawyer. 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.

[4]Since October 2006 minor cases can be settled through a written procedure between one judge and the defendant (and their lawyer) if the maximum sentences is 2 years, the defendant has already confessed their guilt, and the defendant opts for this procedure. If relevant, the victim must also agree to the procedure. We include these cases in our main analysis as they are still decided by the judge.

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

[6]We do not use identities of lay judges, and instead rely on the professional judge's stringency. Given that lay judges are also randomly assigned, if in rare cases lay judges overrule the professional judge's opinion, this will just introduce measurement error and is not a threat to the validity of our instrument.

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 criminal code. Appendix Figure B.2 presents the shares of different kinds of punishments for financial crimes. Among all court cases (without yet imposing the sample restrictions), 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%).

Defendants serve their prison sentence in publicly funded prisons. Prisons in Finland focus on rehabilitation. All prisoners must either enter education programs or work, unless their health conditions make such participation impossible. Prisoners are able to stay in touch with family and friends through phone calls, visits, and approved leaves. After serving their prison sentences, the majority of defendants are released on parole.

We discuss external validity of our estimates to other contexts in Section 7.

2.2 Data

We use a combination of existing administrative data and administrative data we collected for the purposes of this project. Our main data set is Statistics Finland's district court data which covers every criminal case that took place in the Finnish criminal courts between 1992 and 2018. We collapse the data to the individual-case level (a single case can contain multiple crimes, for example fraud could be committed along with identity theft). When we present case-level statistics, we use the designated primary crime (generally the most severe crime committed) from the court records. The data contains information on the verdict, allowable punishment, the actual sentence, and individual level identifiers we use to link this data to other administrative data sets.

Statistic Finland's district court data does not contain judge information. Thus, we collected judge information from the national court registrar of Finland. We link the judge ID back to the district court data using unique individual-case level decisions numbers. The judge data are only available digitally from 2000 to 2016, so we focus on these dates for our main analysis.[7]

Finally, we link the data to the Finnish Linked Employer-Employee Data (FLEED) acquired from Statistics Finland, which contains administrative tax records that cover the whole population of Finland. FLEED provides information on defendant demographics, earnings and employment, including unique firm and plant identifiers for their workplaces.

Using these workplace identifiers we link defendants to their colleagues. We pull from the FLEED data every individual who had the same firm and plant IDs as the defendant in the year their offence was committed. We then link these colleagues to the court data using their unique person IDs. From this we create outcomes measuring if these colleagues commit criminal offences in the years after a defendant is sent to prison. Hereafter "workplace" refers to the "plant/establishment" as colleagues are collected at this level. We do not use the broader firm because it would likely include individuals who never interact (e.g. employees at different Nokia sites may never interact).

To ensure that the data we use for the analysis is consistent with the randomization of court cases to judges, we make a few additional restrictions. Consistent with prior papers in this literature, we restrict the data to cases assigned to judges who reside in courts with at least two active judges, since there must be at least two judges to have random assignment between them. In addition, Finnish law requires that any Swedish speaking defendant have access to a Swedish speaking judge upon request. We drop these cases from the estimation sample as we do not have information on the language spoken by judges and there would not be random assignment in courts with only one active Swedish speaking Judge.[8] We exclude juvenile defendants because they are treated differently by the courts and not always randomly assigned to judges.[9] Last, we require each judge to see a minimum of

[7]Data are in paper form prior to 2000, which was prohibitively costly to collect and link.

[8]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.

[9]Defendants below age 21 are treated as "young" defendants and treated differently by the law. We use the age 23 restriction because it avoids all young defendants and those on the cusp of juvenile defendants. We find that our results are robust to dropping the age restriction down to age 21, see Table C.6.

100 randomly assigned cases between the years 2000 and 2015, to make sure we can get an accurate measure of judge stringency. Our results are robust to other cutoffs for number of cases per judge, such as 50 cases per judge. In Appendix Table C.1 we show how each of these restrictions decreases the number of judges, courts, and defendants in our sample.

Our main outcome of interest for defendants is recidivism. We measure recidivism as the occurrence of any crime in the next year, the next two years, and so on after the year a defendant is sentenced. Additionally, we also estimate impacts on the defendant's employment and earnings. Employment is defined as whether the defendant has a job in December of that year. Earnings are the full taxable income each year. For colleagues, we estimate impacts on whether the colleague commits a crime in the first year, the first two years, or the first three years after the year the defendant they worked with was sentenced.

2.3 Defining Financial Crimes

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.[10] Table 1 reports the top 5 broad financial crime categories in our data and the share of all financial crimes that they encompass. The largest category we include is fraud which consists of 60% of all financial crimes in our estimation sample, followed by business offences (15%), forgery (9%), and money laundering (7%). Other types of offences make up the remaining 9% of cases. For a full list of all crimes included, see Appendix A.

In Figure 1 we graph the proportion of all crimes committed in Finland and decided in courts from 1992 to 2018 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

[10] 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.

increase in the relative importance of financial crimes over time. Moreover, the number of fraud cases reported to the police in Finland has nearly doubled between 2010 (20,380) and 2016 (40,416) and continues to grow.[11]

2.4 The Firm Case for Prevention: Descriptive Evidence on Profits

In Finland, financial crimes cost an estimated €150 million per year (Tanttari and Alanko, 2017), equivalent to 1.5% of GDP. Beyond this, financial crimes may cost firms directly. Figure 2 depicts average profits for firms 2 years before and 5 years after an employees commits a financial crime. This data is collected from firm financial statements and is only available at the firm level (not the plant level) for firms with 20 or more employees. The year 0, indicated by the dashed line, is the year in which the employee commits a financially based crime. In the 2 years prior to the incident, profits on average are approximately 6,500,000 Euros per year. After an employee commits a financial crime, average profits decrease by approximately 500,000 Euros, corresponding to on average an 8% decrease in profits just after the crime is committed which persists for 5 years. The calculation of profits is conditional on firm survival in the post period.[12] We cannot give this pattern a causal interpretation. However, this descriptive result suggests a potential firm-based financial case for prevention of these types of crimes.

3 Descriptive Results for Defendants

3.1 Who Commits Financial Crimes?

Table 2 documents that financial-crime defendants look very different in terms of observed characteristics compared to defendants of other types of crimes who have been the focus

[11]Substantially more financial crimes are reported to police than end up in court, since not all crimes are prosecuted.

[12]Figure B.1 depicts the percent of firms that survive who employ someone who commits a financial crime. By five years post event almost 25% of firms have exited.

of previous papers on the impacts of prison on defendant outcomes.[13] They are 5-7 years older, twice as likely to be female, five times as likely to have a tertiary degree, more likely to have children, and have much better labor market outcomes compared to other types of nonviolent-crime defendants. In summary, financial-crime defendants are on average different (and generally better off) across every dimension compared with other nonviolent defendants. These results clarify why we want to understand the impacts on financial-crime defendants separately: they are a distinctively different type of defendant and as such, may respond differently to harsher sanctions.

In Table 3, we turn to how different crime categories are punished. 11% of financial-crime defendants are sent to prison, which is nearly half the rate that drug-crime defendants (21%) and more than 3 times less than the rate that property-crime defendants (36%) 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). Thus, financial-crime defendants receive fewer and shorter prison sentences compared with other types of nonviolent crimes. One possible explanation for these discrepancies across nonviolent crimes is that policy makers believe prison is ineffective for financial-crime defendants, which we explore in this paper.

In Figure 3 we present evidence on the rate of recidivism for the population of financial-crime defendants. We find that five years after being sentenced for committing a financial crime, approximately 45% of defendants were caught committing another crime. Moreover, 25% of financial-crime defendants commit another crime within a year of sentencing. This high rate of recidivism underscores the importance of investigating how to prevent future

[13] In the United States, Mueller-Smith (2020) finds prison increases recidivism, yet Kuziemko (2013) shows longer prison sentences decrease recidivism. In Norway Bhuller *et al.* (2020) find that prison decreases recidivism. The majority of defendants in these studies are charged with property, drug, and violent crimes.

criminality within the population of financial offenders. Based on these descriptive results, reducing recidivism for financial-crime defendants could play an important role in reducing financial crimes overall.

Table 4 reports significant differences in who is given a prison sentence. In the table we compare those who commit a financial crime and are sent to prison to those who commit a financial crime and are given some other punishment or who receive a not guilty sentence. We find that those who are sent to prison have a third the income in the year before sentencing, are 32 percentage points more likely to be employed, and are much more likely to have a previous criminal charge compared with those who commit financial crimes and are not sent to prison.

3.2 Outcomes Around Prison Sentences

Figure 4 shows the raw dynamics of defendant offending in Panel (a), employment in Panel (c) and earnings in Panel (e) before and after sentencing for those who are sent to prison versus those who are not. In all panels, the year of sentencing is indicated with a dashed vertical line. Across all panels, a decline in the outcome of interest begins in the 2-3 years prior to sentencing. In Panel (a), the reduction in the proportion of offenders charged with a new offence continues to fall after sentencing, with a much larger reduction for those who are sentenced to prison.

For employment, Panel (c) shows that the bulk of the reduction occurs before sentencing, with employment dropping more for those sent to prison. Interestingly, employment seems to recover somewhat for defendants sent to prison in the 2-3 years after sentencing while it remains on a downward trajectory for those not incarcerated. Panel (e) shows a quite steep decline in earnings occurring 2 years prior to sentencing for defendants sentenced to prison, with more muted declines for those not sentenced to prison. Earnings continue to fall 1 year after sentencing for those sent to prison, but by a smaller amount than in the years preceding sentencing. Earnings begin to trend upwards for non-prison defendants directly after sentencing.

The results in Figure 4 suggest a few things. First, there are interesting dynamics in the labor market outcomes of financial-crime defendants both before and after the sentence. We document large drops in charges, employment, and earnings both preceding and accompanying the sentence. These drops in income and employment preceding sentencing could be due to a job loss that makes financial crime more attractive, consistent with negative financial shocks increasing financial misconduct (Dimmock *et al.*, 2021). Alternatively, these drops in earnings and employment prior to sentencing could be due to individuals losing their jobs after their crime is detected and they are arrested but before they are convicted and sentenced (Egan *et al.*, 2019; Karpoff *et al.*, 2008).

Appendix Figure B.4 depicts the dynamics of recidivism, earnings, and employment around the time the crime was committed, as opposed to the time of sentencing. These show similar, but slightly smaller, dips compared with the previous figures. The smaller dips before the crime occurs suggest that at least part of the deterioration in outcomes documented prior to sentencing were due to individuals losing their jobs after being caught for the crime but before sentencing.

We can also estimate an event study.[14] When we do so and report results in Panels (b) (d) and (e) we find similar dynamics but sill pervasive pre-trends for all outcomes. Due to these pre-trends, difference-in-difference or event study approaches 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

[14]The estimating equation for these results is

$$Y_{ibt} = \alpha_{ib} + \sum_{j=-8}^{8} \delta_j D_{b,t-j} + \pi_b + \gamma_t + \epsilon_{ibt}, \tag{1}$$

where we estimate the impact of a prison sentence relative to defendants who commit a financial crime but are not sent to prison. Of interest are the deltas (δ_j), which are the coefficients on indicator variables that take a value of 1 if it is period $t - j$ relative to sentencing year t and individual i is sentenced to prison. As is standard in the literature, the dummy for the year before sentencing is omitted, making the results relative to that year. Additionally, we control for individual fixed effects (α_{ib}), sentencing year fixed effects (π_b), and year fixed effects (γ_t).

the causal impact of prison on recidivism. This helps motivate our empirical strategy, using random assignment of judges to identify causal effects of prison sentences.

4 Research Design

4.1 Specification

Formally, the relationship between prison and defendant outcomes can be captured with the following equation:

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

where 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 their 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. We cluster standard errors by judge and defendant, the typical approach to clustering standard errors in this literature.

Estimates obtained using 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 of no prison (primarily a fine or probation in our context). This implies that we are estimating the standard parameter in an IV framework, namely the local average treatment effect (LATE) for the compliers. In this context, compliers are defendants who would not be sent to prison by a more lenient judge but are sent to prison by a stricter judge. The impact of prison on these marginal defendants is the relevant policy parameter of interest, as it captures those who would be most impacted by an incremental change in the use of prison.

4.2 Validity of the Judge Instrument

The IV estimate identifies the causal impacts of prison if four assumptions hold. First, the instrument has to be as good as randomly assigned. This assumption implies that the instrument should not correlate with the defendant's observed or unobserved pre-determined characteristics. Table 5 provides evidence that this assumption holds in our setting, at least based on observable characteristics of defendants. Column 2 reports results from a balance test in which we regress our instrument, judge stringency, on a set of pre-determined observables. We find that almost all coefficients are very small and not statistically significant. Furthermore, we fail to reject that the coefficients are jointly significant, with a joint F statistic of 1.216. This suggests that observable characteristics are not correlated with judge assignment. In contrast, column 1 shows result from a similar exercise as the balance check, but now our dependent variable is whether the defendant received a prison sentence, and not the judge stringency. We find that the same variables that do not correlate with our instrument are strong predictors of a prison sentence, with a joint F-statistic of 569.582. To summarize, Table 5 provides robust evidence that cases are randomly assigned, and the first LATE assumption holds in our context.

Second, we must have a strong first-stage relationship between our instrument and whether the defendant receives a prison sentence. In other words, we require variability in the judge stringency measure that predicts whether a defendant is sent to prison. Column 1 of Table 6 presents the first-stage estimate that we obtain using equation 3. We find that a 10 percentage point increase in the stringency of the judge corresponds to a 5.6 percentage point increase in the probability of the defendant being sent to prison, which is significant at the .001 level, indicating a strong first stage. Figure 5 provides a visual representation of the first stage. The histogram depicts the variation in judge stringency in our sample. We find that there is quite a bit of variability across all judges. We overlay a nonparametric regression line of the effect of judge stringency on the likelihood of receiving a prison sentence. Consistent with Table 6, we find a strong relationship.

Third, the monotonicity assumption must hold. In our context the monotonicity assumption means that the incarceration probability must be an increasing function of the instrument. What this means in practice is that any individual who is incarcerated by a lenient judge would also be incarcerated by a stricter judge. Appendix Table C.2 provides evidence that this assumption holds in our setting. We use the approach from Bhuller *et al.* (2020) and show that: 1. We have a strong first stage in the different sub-samples of the data, and 2. Our setting passes the so-called reverse-sample instrument test. In the reverse-sample test, we first calculate the instrument for a sub-sample, for example using only highly educated defendants. Next, we run the first-stage analysis within the low educated defendants' sub-sample but using the instrument that we created with the highly educated sample. If monotonicity holds, the first-stage coefficient should be positive, which is what we find across all reverse-sample tests.

The final assumption we need for our identification strategy to be valid is the exclusion restriction, which implies that our instrument influences the outcomes for defendants only through the prison sentence. For example, if more stringent judges also speak more harshly to defendants, and this "stern talking to" impacts reoffending, this would be an exclusion restriction violation. We assume this is not driving our main results, but this is an untestable assumption as we do not observe everything that happens in the courtroom. Another critical exclusion restriction concern is the potential of multidimensional sentencing. Specifically, if judges who assign more prison sentences are also more likely to combine prison sentences with fines, then we could be identifying the joint impact of prison and fines. In Section 7.2 we describe the challenge of multidimensional sentencing and show that our results remain after implementing robustness checks addressing this issue.

5 The Impact of Prison on Defendant Reoffending

Figure 6 shows the impacts of a prison sentence on defendant charges before and after the sentence. The effects are obtained from separate regressions where the outcome in the years before sentencing is an indicator for being charged in a given year, while in the post-sentence years (from 0 to 5) the outcome is an indicator for being charged by that year. The three years prior to the sentence serve as a placebo check. If the IV works as it should, we expect to find no significant impact of the randomly assigned future judge on charges in the past. This is precisely what we find, with all point estimates being insignificant.

Turning to post-sentencing estimates, we see that in the first year there is a marked drop in charges against the defendant who was quasi-randomly sent to prison, but it is not statistically significant. By the second, third, and fourth years post sentencing there are statistically significant declines in whether the defendant is charged with a new crime by this time. Point estimates indicate that three years post sentencing, a prison sentence reduces recidivism by 43 percentage points (See Table 7). These decreases in reoffending in the IV estimates are in stark contrast to OLS estimates, which suggest that prison *increases* reoffending by 44 percentage points without controls, and by 9.1 percentage points when we include the large set of possible controls available in the administrative data.

A 43 percentage point drop in reoffending at first glance seems impossibly large, particularly when considering that only 39% of offenders in the full sample of financial-crime defendants return to prison within three years of conviction. However, the average reoffending rate in the full sample does not tell the whole story. In Table 7 we report the average reoffending rate in the 1 to 3 years post-sentencing for the entire sample, as well as for the sub-sample of those who are sentenced to prison. 72.8% (78.8%) of those who are sentenced to prison are charged with a new crime within two (three) years, both of which are nearly double the reoffending rates in the full sample. These differences in average reoffending between the full sample and those sent to prison indicates that those who go to prison in Finland are a highly selected group who are much more likely to reoffend. Given our IV estimates measure the effect of imprisonment for those on the margin of being sent to prison, it is likely that their potential recidivism rate is much larger than the overall reported recidivism rate in the sample, and likely much closer to the recidivism rate for those who are sent to prison. When this is taken into consideration, the estimated 43 percentage point decline in the recidivism rate may be closer to reducing recidivism by half, a more reasonable but still large effect.

Our main identification strategy identifies the impact of prison on the compliers: those for whom judges may disagree on sentencing and so are thus on the margin of receiving a prison sentence versus not. We cannot identify who the compliers are precisely, but we can recover their share in the sample and average characteristics using the approach from Abadie (2003) and Bhuller *et al.* (2020). In Appendix Tables C.3 for crime types and C.4 for defendant characteristics we estimate first stages separately for the relevant sub-groups and estimate the relative complier share for each subgroup.[15] The most interesting implication from this exercise is that compliers appear to be negatively selected from the population of all financial-crime defendants based on the over-representation of those without degrees, with previous charges, and the slight under-representation of those who are married.

Next, we focus on the contrast between the naive OLS estimates in Table 7 which suggest that prison leads to increases in recidivism and the IV estimates which find the opposite. The differences between the OLS and IV estimates may arise for two reasons. First, OLS estimates may suffer from selection bias which the IV corrects for using random assignment of cases. Alternatively, OLS and IV estimates may both identify causal effects of prison, but for different populations whose responses to prison sentences differ. We can

[15] The relative complier share is interpreted as the percent of the given group in the complier analysis relative to their share of all financial-crime defendants. Those sub-groups for whom the instrument of judge assignment has a stronger correlation with prison will be more heavily represented in the analysis.

test whether the second story is plausible by running a weighted OLS regression using the complier weights. We report results from this exercise in Table 7 under "OLS: Reweighted". We find that OLS estimates and reweighted OLS estimates are very similar, suggesting that OLS and IV estimates differ because of selection bias that is addressed by the random assignment to judges instrument.

5.1 Mechanisms

Having shown that prison reduces future charges for financial-crime defendants, it is worth discussing why this might be the case. We focus on the four most likely explanations.[16] First, prison could play an incapacitation role. In other words, those sent to prison are unable to commit new offences while incarcerated. We can largely rule this explanation out since the average prison sentence for financial-crime defendants is only 77 days and we only observe significant impacts on recidivism 2 years after sentencing, when most defendants would have already been released.

Second, prison may build criminal capital. As a result, defendants might learn how to better avoid detection during their prison sentence, and thus we are finding a decline in detection, and not a decline in offences committed.[17] Alternatively, building criminal capital behind bars can lead offenders to become more prolific. If time in prison causes defendants to commit more crimes, then charges would likely increase post-sentencing. The broader crime literature generally finds support for the latter results, namely that building criminal capital behind bars leads to increased recidivism (Bayer *et al.*, 2009; Damm and Gorinas,

[16] A fifth possible explanation is that receiving a prison sentence today makes it more likely that the judicial system assigns prison sentences in the future. Such a mechanical increase in future likelihood of a prison sentence could cause defendants who receive a prison sentence today to avoid crime in the future. This is unlikely to drive our main effects since for many defendants the alternative sentence to prison is probation. In Finland, probation is more accurately described as a "conditional prison sentence". If a defendant on probation commits another crime, then their original prison sentence is activated and they additionally can receive more time for the new crime. In contrast those who receive an unconditional prison sentence and serve their time are not at risk for additional time tacked on from their previous crime. Thus, probation likely has a sharper bite in terms of mechanically increasing future prison time.

[17] For discussion and interesting analyses on the detection of corporate fraud see Dyck *et al.* (2021) and Wang *et al.* (2010).

2020). This is inconsistent with our main finding of a reduction in charges after a prison sentence, so we view this explanation as unlikely.

Third, prison could play a rehabilitative role. One way rehabilitation may manifest is in improved labor market outcomes after sentencing. In Figure 7 we report IV estimates of the impact of prison on labor market outcomes of financial-crime defendants quasi-randomly sent to prison. We find that employment point estimates are mostly positive, but none of these results are significant. Earnings estimates are similarly noisy.[18] The overall take-away is summarized in Table 8 which presents the cumulative three year impacts of prison on earnings and employment (and also includes the reoffending impacts for completeness). The IV estimates in columns 2 and 3 find no significant impact of prison on labor market outcomes, despite the fact that OLS estimates suggest large and significant negative association between prison and these outcomes. In sum, we do not find strong support for rehabilitation through future labor market outcomes, although these estimates are noisy so we cannot rule out this explanation.

Fourth, there could be a specific deterrent effect. That is, being sent to prison may lead defendants to update their beliefs about either the probability of being sent to prison or prison conditions. As a result, defendants may choose to reduce criminality in the future to avoid returning to prison. We view this as a likely mechanism, given that we can rule out incapacitation and the literature suggests criminal capital formation works in the opposite direction of our results. That said, it is difficult to disentangle specific deterrence from rehabilitation. Although we do not find compelling evidence that rehabilitation is mediated through labor market outcomes, Finnish prisons do focus a great deal on rehabilitation, which may show up in ways we do not observe. Despite this, prison in Finland is likely still unpleasant and experiencing it may motivate defendants to reduce offending to avoid it in the future. We conclude that the large reduction in reoffending we find is likely due to some combination of specific deterrence and rehabilitation.

[18]Detailed OLS and IV estimates are found in Appendix Table C.7.

6 Impact of Sending a Defendant to Prison on Their Colleagues

In this section we explore if sending financial-crime defendants to prison also causes their colleagues to reduce the number of financial crimes they commit. We define colleagues as those employed in the same workplace as the defendant in the year their offence was committed. We use the year the offence was committed to define colleagues because we found in Section 3.2 that many financial-crime defendants separate from their firms between their offence and conviction. We also restrict to establishments with 50 or fewer employees, since in larger establishments it becomes less likely defendants have interacted with all their coworkers.[19]

To estimate the impact of imprisoning a financial-crime defendant on their colleagues we use a similar 2SLS strategy as described in Section 4. In this case, the dependent variable is an indicator for if a colleague commits a financial crime in the years after a defendant they worked with is sentenced. To recover causal effects, we use the same judge stringency IV to instrument for having worked with a financial-crime defendant sent to prison.

Table 9 reports the impact of having worked with a defendant quasi-randomly assigned prison on whether a colleague commits financial crimes. In Panel A we consider the coworkers of all financial-crimes defendants. OLS estimates suggest a positive correlation between defendants sent to prison and their colleagues' criminality. On the other hand, the IV estimates show a consistent negative effect. These results suggest that there is selection in the OLS estimates, and that sending a defendant to prison reduces the probability that their colleagues commit financial crimes in the years after sentencing. However, the IV estimates are not statistically significant.

Next, we examine results for subcategories of financial-crime defendants. First, in Panel B we focus on the colleagues of fraud defendants, which make up 60% of all cases in the

[19]We find similar results when we use alternative establishment size cutoffs to estimate impacts on colleagues.

court data. In this case the IV estimates remain consistently negative, but are also larger in absolute magnitude and statistically significant. The estimate in Column 1 suggests that colleagues of fraud defendants were 19 percentage points less likely to commit a financial crime in the year after the defendant was sentenced to prison, and this effect slightly strengthens over time.

In Panels C and D, we turn to the colleagues of defendants who committed business offences and other financial crimes respectively. In both cases the IV estimates are much smaller than in the fraud cause and not statistically significant. It should be noted that these results are difficult to interpret due to the lack of a strong first stage relationship in both cases. That said, we find no evidence that incarcerating these two categories of defendants impacts the offending of their colleagues.

These results suggest that the overall reduction in offending found for colleagues of a defendant randomly sent to prison is driven by those who worked with fraud defendants. Our results indicate there is a broader general deterrence effect of sending fraud defendants to prison, beyond the impact on the defendant's own likelihood of reoffending.

6.1 Mechanisms

We consider three possible explanations for why we observe impacts of sending defendants to prison on their colleagues. First, observing a colleague sent to prison may cause an individual to become a savvier or more careful criminal. Because we only observe detected crimes this would appear as a reduction in offending in our estimates. It is not possible to observe if this is occurring, but it seems implausible that this effect could be large enough to drive the decline in offending among colleagues we estimate.

Second, if defendants and colleagues are co-conspirators (i.e. they committed crimes together) then sending a defendant to prison reduces opportunities for their colleagues to commit crimes with them. In the data we find that in 17% of all financial-crime cases, there is at least one other co-conspirator (i.e. more than one defendant). However, we find that only 0.9% of our colleagues sample were a co-conspirator with the defendant they worked with, which is the relevant margin for our IV estimates. Thus, while we cannot fully rule out the second possible mechanism, it seems unlikely it would drive our results.

Third and last, observing a colleague sent to prison may have a deterrent effect. Specifically, individuals may revise their beliefs upwards about the likelihood they might be sent to prison for committing a financial crime. This would increase the expected cost of committing an offence and lead those on the margin to reduce offending. Given the other possibilities discussed above are unlikely to be strong enough to produce the estimated effects we find, we view this third explanation as the most likely one.

7 Discussion

7.1 Interpretation of Our Estimates

Our estimates capture the effect of prison on defendants who would be sent to prison by a harsh judge, but not by a more lenient judge (the "compliers"). This has important policy implications. Prison may not reduce offending for the most severe offenders whom all judges would incarcerate (the "always takers") nor for more minor offenders for whom no judge would recommend prison (the "never takers") . As such, we cannot extrapolate our estimates to these groups. However, when considering whether to marginally increase the incarceration rate for financial-crime defendants our identified estimate is the policy relevant parameter of interest. Our results indicate that judges could send more financial-crime defendants on the margin to prison and reduce recidivism.

When considering the estimates of spillovers on colleagues, if colleagues of defendants on the margin of prison are similar to colleagues of defendants who are not, our colleague results could hold quite broadly. On the other hand, if defendants on the margin of being sent to prison work with colleagues more prone to criminal activity, then our colleague results may not be relevant for the colleagues of never takers or always takers.

7.2 Multidimensional Sentencing Robustness

In this section we explore the extent to which "multidimensional sentencing" is an issue for our estimates. Multidimensional sentencing is an exclusion restriction violation which can arise when judges make multiple punishment decisions at the same time. In our context, judges make two relevant decisions for defendants, whether they are guilty and whether to sentence them to prison. If judges more likely to assign prison are also more likely to find defendants guilty, then our estimates may capture the bundled effect of these decisions, rather than just the impact of sending a defendant to prison. Multidimensional sentencing issues can also arise in some settings with judicial decisions involving fines and probation. This is less of a concern in our setting, as in Finland probation and prison are never assigned together, and only 0.4% of all cases in our data were assigned a fine and prison.

To explore if multidimensional sentencing is an issue with our estimates we follow the approach in Bhuller *et al.* (2020), and control for the "guilty-verdict stringency" of judges in the first and second stage equations. This measure is constructed identically to our main judge stringency instrument but using the judge's tendency to declare a defendant guilty as opposed to give a prison sentence. Formally, it is the residiualized leave out mean of guilty verdicts for each judge. We then augment the main first and second stage equations as shown in the following equations:

$$Y_{ict} = \beta_0 + \beta_1 P_{ict} + \beta_2 \boldsymbol{X}_{ict} + \beta_3 Z^G_{ict} + \varepsilon_{ict}. \tag{4}$$

$$P_{ict} = \alpha_0 + \alpha_1 Z_{icjt} + \alpha_2 Z^G_{icjt} + \alpha_3 \boldsymbol{X}_{ict} + \epsilon_{ict}. \tag{5}$$

where all variables are as previously defined in Section 4, but we additionally control for judge guilty-verdict stringency, denoted Z^G_{icjt}. The intuition for this approach is that we control for the fact that judges more likely to assign prison sentence are also more likely to find a defendant guilty, thereby isolating the impact of a prison sentence on outcomes given by β_3.

Alternatively, we can also instrument the guilty sentence using the guilty-verdict stringency of judges. This involves estimating the following equations:

$$Y_{ict} = \beta_0 + \beta_1 P_{ict} + \beta_2 G_{ictict} + \beta_3 \boldsymbol{X}_{ict} + \varepsilon_{ict}. \tag{6}$$

$$P_{ict} = \alpha_0 + \alpha_1 Z_{icjt} + \alpha_2 Z^G_{icjt} + \alpha_3 \boldsymbol{X}_{ict} + \epsilon_{ict} \tag{7}$$

$$G_{ict} = \alpha_0 + \alpha_1 Z_{icjt} + \alpha_2 Z^G_{icjt} + \alpha_3 \boldsymbol{X}_{ict} + \epsilon_{ict}. \tag{8}$$

where we instrument both prison and guilty sentences with the relevant leave-out-mean stringency variable in the second two equations, and then estimate the second stage equation using the instrumented prison and guilty variables, as shown in equation 6.

We find that the results remain and are similar in magnitude with these robustness checks, see Appendix Table C.5. Panel A shows the results where we simply include guilty-verdict stringency as a control and we find slightly larger (but statistically indistinguishable) negative effects on recidivism. This indicates that our effects are likely driven by the prison sentence, and not confounded by the judge's decision on guilt. Interestingly, in Panel B when we instrument for guilty sentences, we find that guilty sentences appear to increase recidivism by 6.2-12.7 percentage points, and this increase is significant in the 2 years after sentencing. Thus, it appears that prison and a guilty verdict impact recidivism in opposite directions, with being found guilty leading to an increase in recidivism.[20]

It could also be the case that stricter judges act in other ways we can't observe that may confound our estimates. For example, stricter judges could also behave more harshly in the courtroom, yelling at defendants or lecturing them on the consequences of their criminality, which could impact recidivism. It is not possible to rule out these effects as

[20]With multiple instruments we must assume constant treatment effects for these estimates to recover the causal effects. See Mountjoy (2021) for more discussion.

we do not observe these behaviors. That said, it is unlikely that these less tangible judge behaviors are as important for reoffending as a prison sentence.

7.3 External Validity

This paper provides prima facie evidence that it is possible for prison to reduce financial crimes through direct effects on defendants as well as via spillovers on colleagues' criminality. This does not mean that prison will always reduce financial crimes. In addition to being unable to extrapolate our results to always takers or never takers, our complier defendants might not reduce recidivism if they were incarcerated in a different context with different prison conditions. Moreover, prison may not reduce criminal charges for colleagues in other contexts.

Ideally, this study could be replicated in many other contexts to assess external validity. This is challenging given the extraordinary data requirements and the fact that judges must be randomly assigned to financial cases to identify causal impacts of prison. Instead, in this section we discuss similarities and differences between the criminal justice system for financial-crime defendants in Finland relative to the United States. While the United States is not the only country of interest when it comes to external validity, judicial systems across Europe are much more similar to each other, making external validity more likely.

To better understand external validity of our estimates to the United States we use data from North Carolina from 2000-2015. This allows us to at least compare descriptive statistics.[21] The results suggest more similarity than one might expect, particularly given the large and well known differences for other crime types.

In Appendix Table C.9, we show that 9% of all court cases in North Carolina involve financial crimes. Much like Finland, financial-crime cases in North Carolina have the lowest

[21] Rose and Shem-Tov (2021) study the effect of longer versus shorter prison sentences on financial crime recidivism in North Carolina. However, the identification strategy of that paper would only apply for longer sentences than we observe among the vast majority of financial-crime defendants, so it is unlikely to work for financial-crime defendants. Moreover, we would not be able to replicate the analysis for spillovers on colleagues.

incarceration rate (15%) and result in the shortest prison sentences (43 days) when compared to other crime types.[22] When it comes to recidivism we find that 65% of financial-crime defendants in North Carolina reoffend within the first five years.[23] This rate of reoffending is nearly identical to that of property offenders, and over 10 percentage points larger than that for drug offenders. Therefore, much like in Finland, financial-crime defendants are sentenced more leniently, despite having comparable reoffending rates to other nonviolent crime defendants. The descriptive statistics on sentencing in North Carolina are also of largely similar magnitudes to those in Finland, indicating that financial-crime defendants are treated similarly in both countries.

Differences in prison conditions while incarcerated might also affect the external validity of our results.[24] Finland spends between €150-€214 per day per inmate, depending on the type of prison in which the defendant is housed.[25] There is large variability in prison spending within the United States. For example, North Carolina spends $103 per day to house an inmate and California spends $291 per day, and Finland's per inmate spending is between these two.

Of course, it matters how this money is spent, and more spending does not necessarily indicate better prison conditions. Prisons in the Nordic countries, Finland included, are often held up as exceptional for placing a great deal of emphasis on rehabilitation and treating inmates humanely. In contrast, prison conditions are notoriously bad for general inmates in the United States. However, financial-crime defendants in the United States are often

[22]These calculations of sentence length exclude lifetime sentences. Lifetime sentences almost never occur for financial crimes, but make up 1% of sentences for violent crimes.

[23]The data used in this section is limited to cases in North Carolina so recidivism is limited to reoffending within the state.

[24]Katz *et al.* (2003) find that there is a robust negative correlation between prison conditions–proxied for by the death rate in prison–and crime. On the other hand, several other studies show that higher security level housing units, which generally have worse conditions, are associated with increases in recidivism (Shapiro and Chen, 2007; Drago *et al.*, 2011; Mastrobuoni and Terlizzese, 2014). Di Tella and Schargrodsky (2013) finds that offenders in Argentina–where prison conditions are generally poor–assigned to home arrest with electronic monitoring had lower recidivism rates.

[25]See the government report found at the following link: https://www.rikosseuraamus.fi/fi/index/ajankohtaista/julkaisut/monisteetjaraportit/rikosseuraamuslaitoksentilinpaatosjatoimintakertomus2018.html.

sent to minimum-security prisons, which are more similar to Finnish prisons than those where other defendants are housed. The United States Bureau of Prisons states that "minimum security institutions, also known as Federal Prison Camps (FPCs), have dormitory housing, a relatively low staff-to-inmate ratio, and limited or no perimeter fencing. These institutions are work- and program-oriented."

We close with two major takeaways. First, financial-crime defendants are an important crime group, are less likely to receive a prison sentence compared with other nonviolent crimes, and have high rates of recidivism across countries. As such, evidence to better understand how the criminal justice system might reduce these crimes is important. This paper provides such evidence, filling a hole in the current literature. Second, there is still much to be learned from our analysis, whether our estimates hold exactly in the United States context or not. We provide the first rigorous evidence on the impacts of prison for financial-crime defendants. These results could be informative for policy discussions in other countries not only on how to approach financial-crime defendants, but also on the broader question of how to reform prison systems in general.

8 Conclusion

In this paper we show that despite the growing importance of financial crimes, these defendants are less likely to be sent to prison compared with defendants who commit other types of nonviolent crimes. We also show that these defendants look very different than other types of defendant but still have high rates of recidivism, with just under half going on to commit an additional crime in the five years post sentencing. 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 estimate the impact of harsher sanctions, specifically a prison sentence, on the likelihood defendants reoffend and the likelihood their colleagues commit financial crimes.

Using random assignment to judges as an instrument to identify the causal impact of prison on financial-crime defendants, we find that the probability financial-crime defendants reoffend after a prison sentence decreases by 42.9 percentage points. 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. Together, these results suggest 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.

However, individual recidivism and broader deterrence effects are not the only things to consider when a judge, or more generally the public, decides whether to punish someone who commits a financial crime with a prison sentence. While it is important to understand if prison is effective in reducing financial crimes, there are many other reasons why a society might choose not to send individuals to prison. Thus, the results from this paper should not be interpreted as an endorsement of increased prison sentences for financial-crime defendants. Rather, this study provides rigorous evidence on some of the effects of prison sentences in the context of financial crimes. The potential reductions in these crimes must be weighed carefully against the costs of prison sentences and the impacts of alternative policies in order to arrive at an equitable resolution to these crimes.

References

ABADIE, A. (2003). Semiparametric Instrumental Variable Estimation of Treatment Response Models. *Journal of Econometrics*, **113** (2), 231–263.

AIZER, A. and DOYLE, J. J. (2015). Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges. *The Quarterly Journal of Economics*.

ARTAVANIS, N., MORSE, A. and TSOUTSOURA, M. (2016). Measuring Income Tax Evasion Using Bank Credit: Evidence from Greece. *The Quarterly Journal of Economics*, **131** (2), 739–798.

ARTEAGA, C. (2020). Parental Incarceration and Children's Educational Attainment. *The Review of Economics and Statistics*, pp. 1–45.

BATTAGLINI, M., GUISO, L., LACAVA, C. and PATACCHINI, E. (2019). *Tax Professionals: Tax-Evasion Facilitators or Information Hubs?* Working Paper 25745, National Bureau of Economic Research.

BAYER, P., HJALMARSSON, R. and POZEN, D. (2009). Building Criminal Capital Behind Bars: Peer Effects in Juvenile Corrections. *The Quarterly Journal of Economics*, **124** (1), 105–147.

BHULLER, M., DAHL, G. B., LØKEN, K. V. and MOGSTAD, M. (2018). *Incarceration Spillovers in Criminal and Family Networks*. Working Paper 24878, National Bureau of Economic Research.

—, —, — and MOGSTAD, M. (2020). Incarceration, Recidivism, and Employment. *Journal of Political Economy*, **128** (4), 1269–1324.

BILLINGS, S. B. (2018). Parental Arrest and Incarceration: How Does it Impact the Children? *Working Paper*.

Chang, T. and Schoar, A. (2022). Judge Specific Differences in Chapter 11 and Firm Outcomes. In *AFA 2007 Chicago Meetings Paper*.

Cheng, I.-H., Severino, F. and Townsend, R. R. (2021). How Do Consumers Fare When Dealing with Debt Collectors? Evidence from Out-of-Court Settlements. *The Review of Financial Studies*, **34** (4), 1617–1660.

Damm, A. P. and Gorinas, C. (2020). Prison as a Criminal School: Peer Effects and Criminal Learning Behind Bars. *The Journal of Law and Economics*, **63** (1), 149–180.

Di Tella, R. and Schargrodsky, E. (2013). Criminal Recidivism after Prison and Electronic Monitoring. *Journal of Political Economy*, **121** (1), 28–73.

Dimmock, S. G., Gerken, W. C. and Graham, N. P. (2018). Is Fraud Contagious? Coworker Influence on Misconduct by Financial Advisors. *The Journal of Finance*, **73** (3), 1417–1450.

—, — and Van Alfen, T. (2021). Real Estate Shocks and Financial Advisor Misconduct. *The Journal of Finance*, **76** (6), 3309–3346.

Dobbie, W., Grönqvist, H., Niknami, S., Palme, M. and Priks, M. (2018). *The Intergenerational Effects of Parental Incarceration*. Working Paper 24186, National Bureau of Economic Research.

— and Song, J. (2015). Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection. *American Economic Review*, **105** (3), 1272–1311.

Drago, F., Galbiati, R. and Vertova, P. (2011). Prison Conditions and Recidivism. *American Law and Economics Review*, **13** (1), 103–130.

Dyck, I., Morse, A. and Zingales, L. (2021). How Pervasive Is Corporate Fraud? *Rotman School of Management Working Paper*, (2222608).

EFECC (2021). Financial and Economic Crime.

Egan, M., Matvos, G. and Seru, A. (2019). The Market for Financial Adviser Misconduct. *Journal of Political Economy*, **127** (1), 233–295.

FBI (2021). White Collar Crime.

Fich, E. M. and Shivdasani, A. (2007). Financial Fraud, Director Reputation, and Shareholder Wealth. *Journal of Financial Economics*, **86** (2), 306–336.

Gurun, U. G., Stoffman, N. and Yonker, S. E. (2018). Trust Busting: The Effect of Fraud on Investor Behavior. *The Review of Financial Studies*, **31** (4), 1341–1376.

Heese, J., Pérez-Cavazos, G. and Peter, C. D. (2021). When the Local Newspaper Leaves Town: The Effects of Local Newspaper Closures on Corporate Misconduct. *Journal of Financial Economics*.

Honigsberg, C. and Jacob, M. (2021). Deleting Misconduct: The Expungement of BrokerCheck Records. *Journal of Financial Economics*, **139** (3), 800–831.

Karpoff, J. M., Lee, D. S. and Martin, G. S. (2008). The Consequences to Managers for Cooking the Books. *Journal of Financial Economics*, **88** (88), 193–215.

Katz, L., Levitt, S. D. and Shustorovich, E. (2003). Prison Conditions, Capital Punishment, and Deterrence. *American Law and Economics Review*, **5** (2), 318–343.

Kling, J. R. (2006). Incarceration Length, Employment, and Earnings. *American Economic Review*, **96** (3), 863–876.

Kowaleski, Z. T., Sutherland, A. G. and Vetter, F. W. (2020). Can Ethics Be Taught? Evidence from Securities Exams and Investment Adviser Misconduct. *Journal of Financial Economics*, **138** (1), 159–175.

Kuziemko, I. (2013). How Should Inmates Be Released From Prison? An Assessment of Parole Versus Fixed-Sentence Regimes. *The Quarterly Journal of Economics*, **128** (1), 371–424.

Mastrobuoni, G. and Terlizzese, D. (2014). Rehabilitating Rehabilitation - Prison Conditions and Recidivism.

Mountjoy, J. (2021). *Community Colleges and Upward Mobility*. Working Paper 29254, National Bureau of Economic Research.

Mueller-Smith, M. (2020). The Criminal and Labor Market Impacts of Incarceration. *Working Paper*.

Norris, S., Pecenco, M. and Weaver, J. (2021). The Effect of Parental and Sibling Incarceration: Evidence from Ohio and Pennsylvania. *American Economic Review*.

Piquero, N. L. (2018). White-Collar Crime Is Crime: Victims Hurt Just the Same. *Criminology and Public Policy*, **17**, 595.

Rose, E. K. and Shem-Tov, Y. (2021). How Does Incarceration Affect Reoffending? Estimating the Dose-Response Function. *Journal of Political Economy*, **129** (12), 3302–3356.

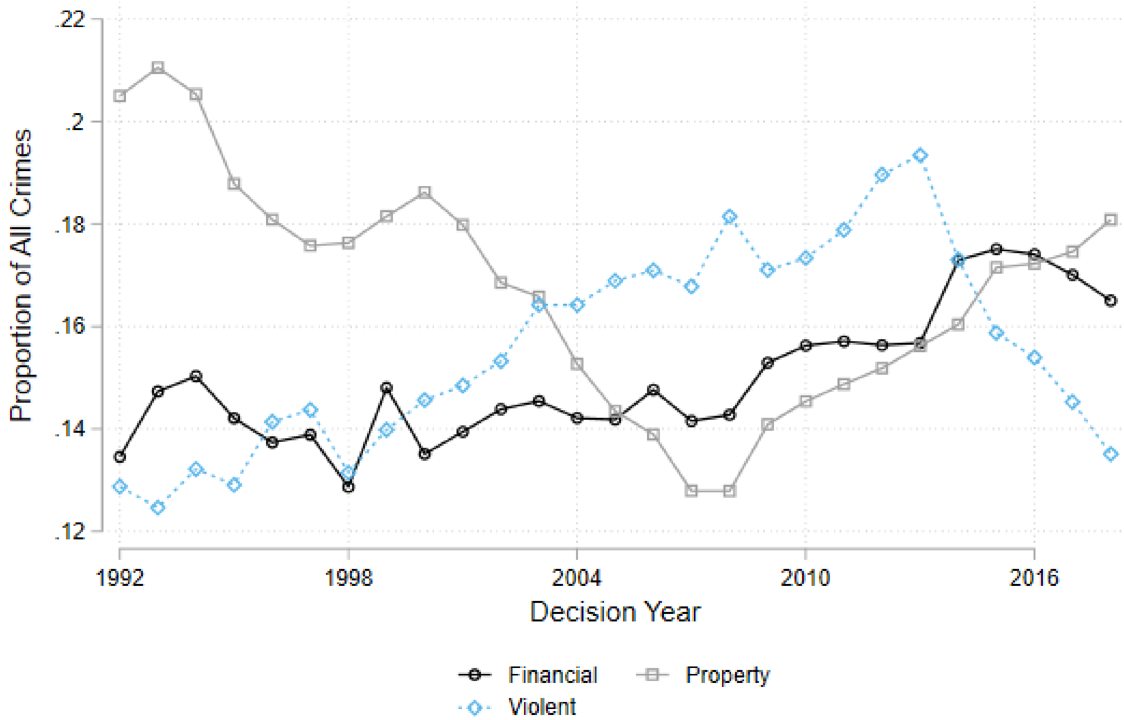
Shapiro, J. M. and Chen, M. K. (2007). Do Harsher Prison Conditions Reduce Recidivism? A Discontinuity-based Approach. *American Law and Economics Review*, **9** (1), 1–29.

Tanttari, S. and Alanko, M. (2017). *Petosrikollisuus ja sen ehkäisy Rikoksentorjuntakatsaus 2017*. Tech. rep., Oikeusministeriö.

Taub, J. (2020). *Big Dirty Money: The Shocking Injustice and Unseen Cost of White Collar Crime*. Penguin.

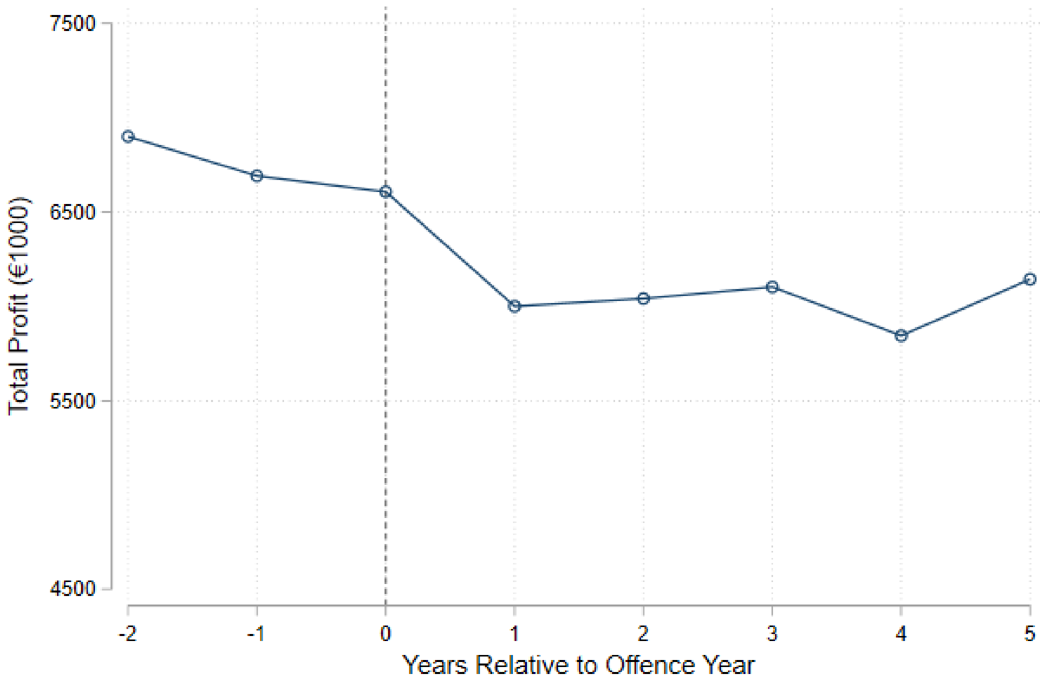
Wang, T. Y., Winton, A. and Yu, X. (2010). Corporate Fraud and Business Conditions: Evidence from IPOs. *The Journal of Finance*, **65** (6), 2255–2292.

Figure 1: Proportion of Financial and Other Crime Types, 1992-2018



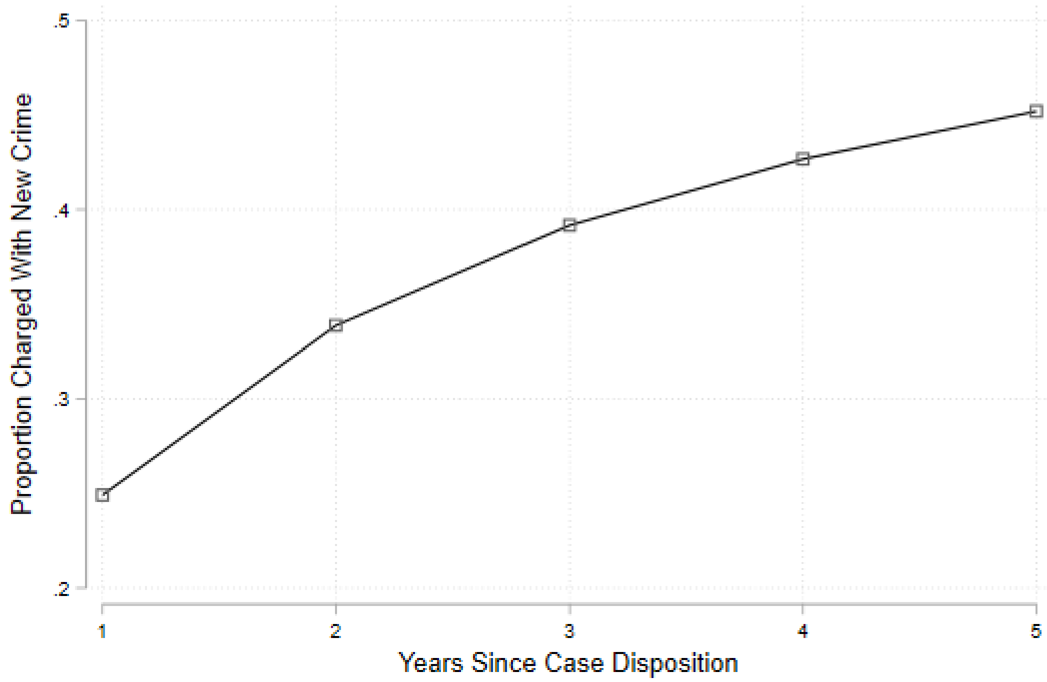
Note: Figure shows the share of financial, property, and violent crimes of all district court cases in Finland in 1992-2018, without applying sample restrictions described in Section 2 and 4.

Figure 2: Average Profits Before and After an Employee Commits a Financial Crime



Note: Figure shows the average profits of firms where an employee commits a financial crime before and after the crime. Dashed line at time 0 indicates the time the crime was committed. Profits only available for firms with more than 20 employees and is at the firm, not plant level.

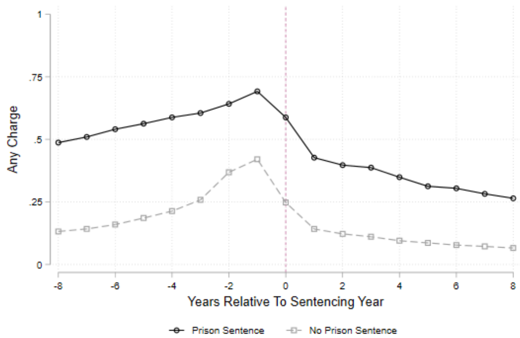
Figure 3: Recidivism for Financial-Crime Defendants



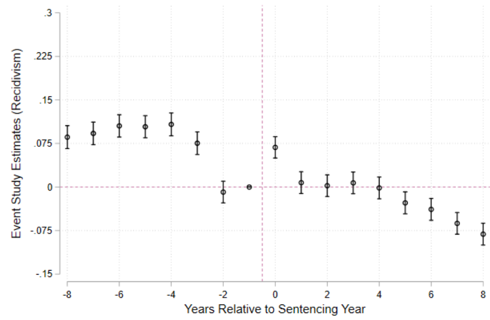
Note: Figure shows the proportion of financial-crime defendants who reoffend (reappear in the court with a new crime) by the year since the initial sentence in our analysis sample. The sample construction is defined in Sections 2 and 4.

Figure 4: Raw and Event Study Patterns of Criminality, Employment, and Income Around the Time of Sentencing

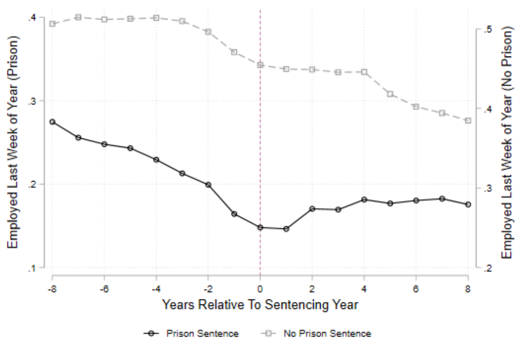
(a) Any Offence Charged



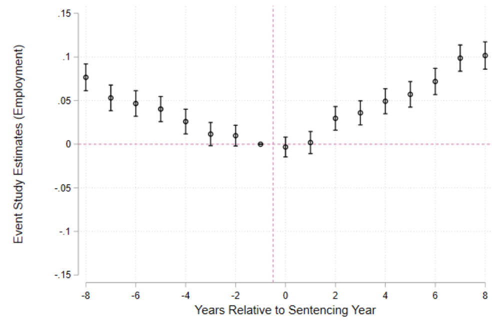
(b) Any Offence Charged



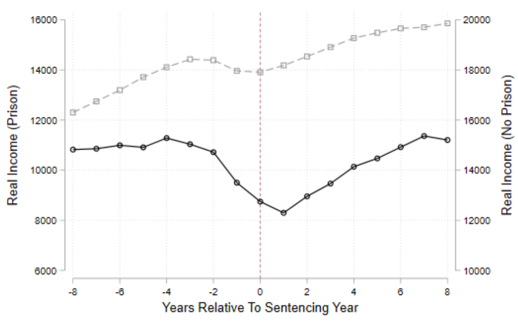
(c) Employment



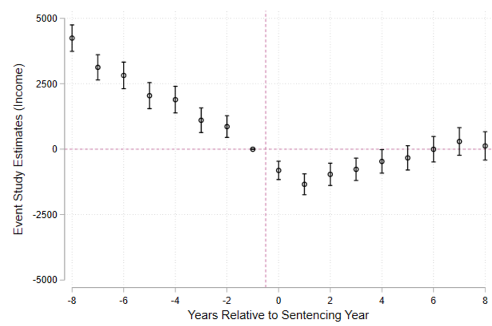
(d) Employment



(e) Income

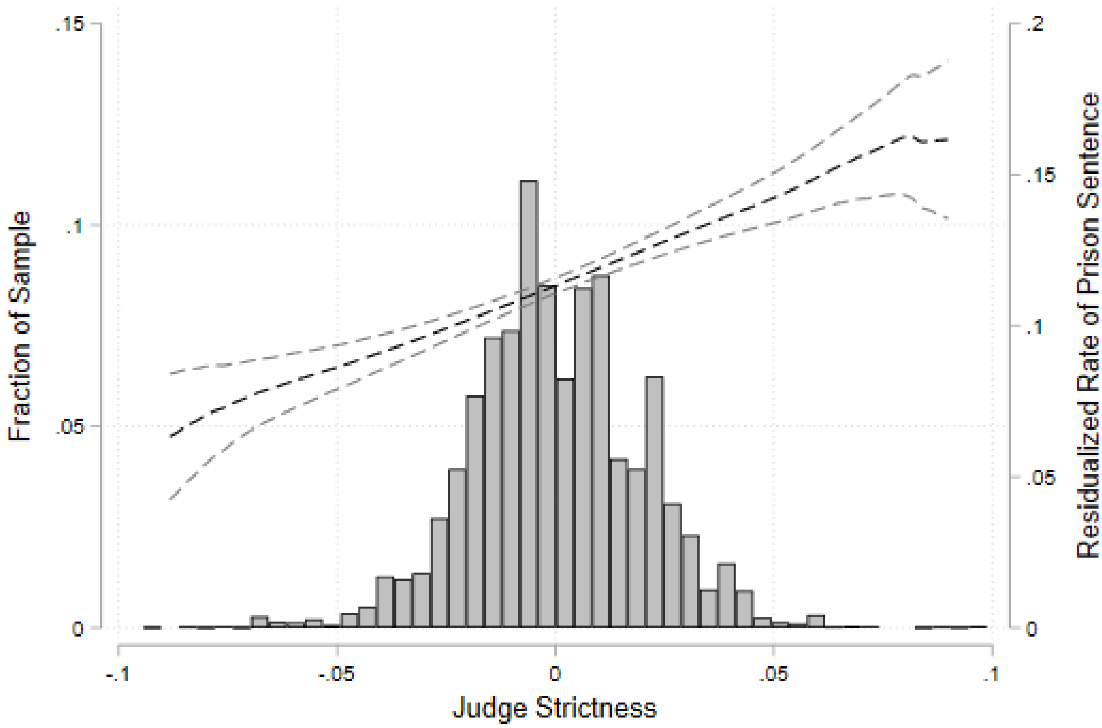


(f) Income



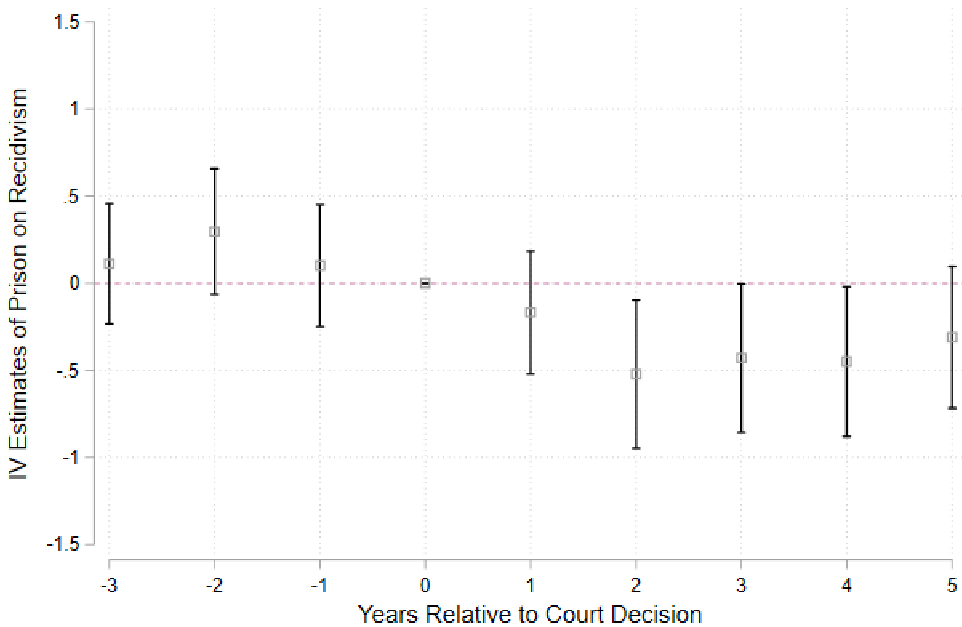
Note: Panel A shows raw dynamics for whether an offence is charged 8 years before and 8 years after sentencing separately for those who are sent to prison (black line) versus those who are not sent to prison (grey line). Panel C (E) shows employment (income) of defendants 8 years before and 8 years after sentencing separately for those sent to prison as well as those who commit a financial but are not sent to prison (grey line). Employment and income are measured at the end of the year. On the right-hand panels, event study estimates from equation 1 as described in Section 3.2 are shown for charges (Panel B), employment (Panel D) and income (Panel F). Sample construction as defined in Sections 2 and 4.

Figure 5: Variation in Judge Stringency and First Stage



Notes: Figure is a graphical representation 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 black dashed line is a nonparametric regression of the effect of judge stringency on the likelihood a given defendant receives a prison sentence (the right-hand axis). The grey dashed lines represent 95% confidence intervals.

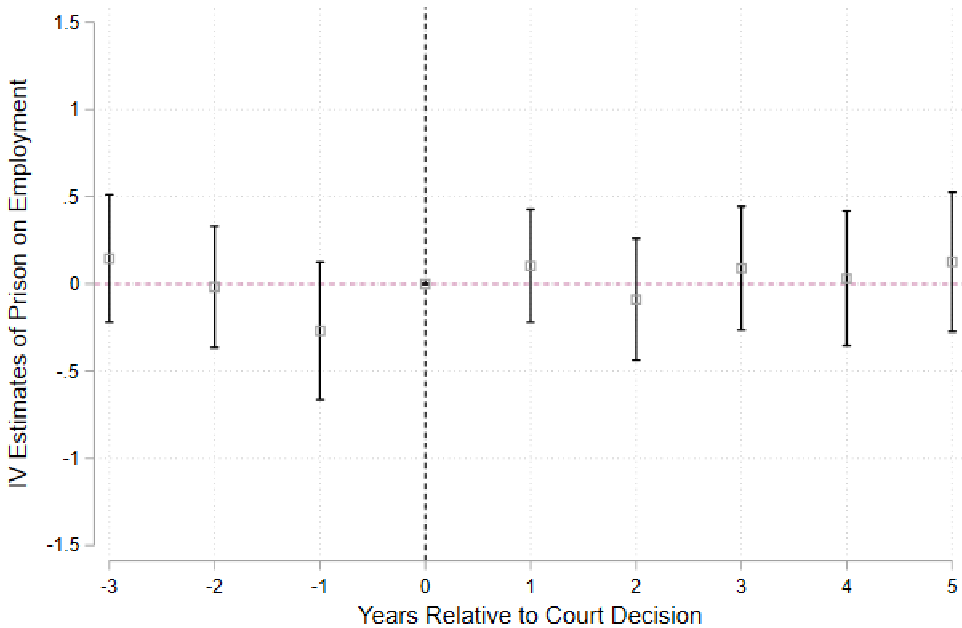
Figure 6: Impact of Prison on Defendant Criminal Charges



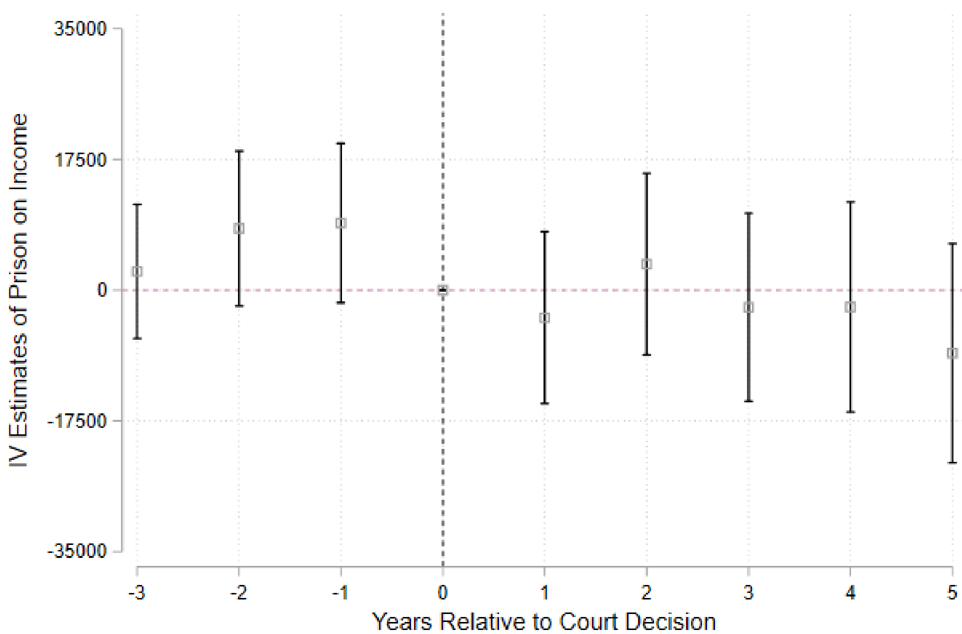
Note: Figure plots the IV estimates of the impact of incarceration on whether the defendant is charged using the random assignment to judges and variation in judge leniency as the instrument, as described in Section 4. The estimates are obtained from separate IV regressions, where the outcome in the years -3 to -1 is whether the defendant is charged in a given year. In the years 1-5 the outcome is cumulative (i.e. charged 1 year after, charged within 2 years after, charged within 3 years after, etc.). 95% confidence intervals depicted. Sample construction as defined in Sections 2 and 4.

Figure 7: Impact of Prison on Defendant Labor Market Outcomes

(a) Employment



(b) Income



Note: Panel A (B) shows the IV estimates of the impact of incarceration on employment (income) of defendants 3 years before and 5 years after the sentencing using the identification approach of random assignment to judges as described in Section 4. Employment and income are measured at the end of the year, and the figures show the annual effects. 95% confidence intervals depicted. Sample construction and data as defined in Sections 2 and 4.

Table 1: Types of Financial Crimes

	Proportion of Sample (1)	Proportion Sent to Prison (2)
Fraud	0.606	0.119
Business Offences	0.148	0.065
Forgery	0.092	0.181
Laundering	0.070	0.144
Political Corruption	0.009	0.000
Other	0.075	0.073

Notes: The data consists of all district court cases in Finland in 2000 - 2013. Unit of observation is individual- case level. Table shows the proportion of total financial crimes of relevant sub-categories, including all sub-categories that make up 5% or more of the data.

Table 2: Defendant Sample Means By Crime Type

	Financial (1)	Property (2)	Drug (3)	Violent (4)	Other (5)
Age at Conviction	38.61	33.28	31.85	36.93	40.23
	(10.72)	(9.004)	(8.027)	(10.43)	(11.68)
Share Female	0.249	0.144	0.137	0.126	0.119
	(0.433)	(0.351)	(0.344)	(0.332)	(0.323)
Earned Income (€)	14064.8	5818.1	7260.4	13433.4	14609.8
	(16356.6)	(7451.0)	(8869.0)	(13297.6)	(15166.5)
Wages (€)	9393.0	2490.2	3989.0	9179.1	9443.6
	(15720.4)	(6665.4)	(8493.4)	(13646.9)	(15290.1)
Share Employed	0.420	0.132	0.207	0.408	0.409
	(0.494)	(0.338)	(0.405)	(0.491)	(0.492)
Share Student	0.0320	0.0397	0.0585	0.0336	0.0304
	(0.176)	(0.195)	(0.235)	(0.180)	(0.172)
Share White Collar Worker	0.0901	0.0226	0.0340	0.0590	0.0645
	(0.286)	(0.149)	(0.181)	(0.236)	(0.246)
Share Upper Management	0.0526	0.00629	0.0107	0.0240	0.0415
	(0.223)	(0.0791)	(0.103)	(0.153)	(0.200)
Share Tertiary Degree	0.158	0.0273	0.0333	0.0842	0.137
	(0.364)	(0.163)	(0.179)	(0.278)	(0.344)
Num. of Children	0.544	0.202	0.169	0.411	0.385
	(1.044)	(0.647)	(0.582)	(0.914)	(0.897)
Observations	56583	37199	22444	80455	34286

Notes: Unit of observation is individual-case level. These summary statistics are for the estimation sample. The sample construction is defined in Sections 2 and 4. Income and employment measured at the end of the year. All variables measured the year before the crime

Table 3: Punishment by Crime Type

	Financial (1)	Property (2)	Drug (3)	Violent (4)	Other (5)
Prison	0.114	0.356	0.216	0.132	0.101
	(0.318)	(0.479)	(0.412)	(0.339)	(0.301)
Probation	0.255	0.137	0.170	0.193	0.163
	(0.436)	(0.343)	(0.375)	(0.395)	(0.369)
Fine	0.483	0.409	0.573	0.550	0.612
	(0.500)	(0.492)	(0.495)	(0.498)	(0.487)
Sentence	77.85	100.3	163.5	104.0	66.37
	(403.4)	(639.0)	(563.3)	(427.4)	(355.1)
Not guilty	0.121	0.0630	0.0229	0.0811	0.0758
	(0.326)	(0.243)	(0.150)	(0.273)	(0.265)
Prev. Prison Spells	1.151	4.566	1.944	1.005	0.841
	(4.670)	(8.778)	(5.658)	(3.766)	(3.789)
Observations	56583	37199	22444	80455	34286

Notes: Table shows statistics on the severity of punishment (percent sent to prison, probation, or fines, length of prison sentence, percent not guilty) for financial crimes (column 1) as compared with drug crimes, property crimes, and violent crimes (columns 2-4) with all other crimes in column 5. Unit of observation is individual-case level. Summary statistics reported for the estimation sample. The sample construction is defined in Sections 2 and 4.

Table 4: Sample Means for Defendants With Versus Without Prison Sentences

	All (1)	Prison (2)	Not Prison (3)
Age at Offence	38.61	35.52	39.01
	(10.72)	(9.446)	(10.81)
Share Female	0.249	0.116	0.267
	(0.433)	(0.321)	(0.442)
Earned Income (€)	14064.8	5729.6	15137.5
	(16356.6)	(9766.7)	(16721.4)
Wages (€)	9393.0	2129.1	10327.9
	(15720.4)	(7048.2)	(16275.1)
Share Employed	0.420	0.134	0.457
	(0.494)	(0.340)	(0.498)
Share Student	0.0320	0.0366	0.0314
	(0.176)	(0.188)	(0.174)
Share White Collar Worker	0.0901	0.0327	0.0974
	(0.286)	(0.178)	(0.297)
Share Upper Management	0.0526	0.0122	0.0578
	(0.223)	(0.110)	(0.233)
Share Tertiary Degree	0.158	0.0513	0.171
	(0.364)	(0.221)	(0.377)
Num. of Children	0.544	0.228	0.584
	(1.044)	(0.696)	(1.074)
Share with Criminal Charge 2 or 3 Years Prior	0.357	0.830	0.296
	(0.479)	(0.375)	(0.456)
Observations	56583	6452	50131

Notes: Table shows descriptive statistics for the full sample of individuals who commit financial crimes (column 1) and those sent to prison (column 2) versus those who commit financial crimes but who are not sent to prison (column 3). All statistics are for the year before sentencing, and those who are not employed are included as zeros in the earnings and wage means. Summary statistics reported for the estimation sample. The sample construction is defined in Sections 2 and 4.

Table 5: Balance Check

	Prison (1)	Judge Strictness (2)
Age	0.0000335	-0.0000112
	(0.000129)	(0.0000101)
Female	-0.0220***	0.000195
	(0.00256)	(0.000203)
Children	-0.00462***	-0.0000158
	(0.000995)	(0.0000813)
Married	0.00461*	-0.000251
	(0.00279)	(0.000224)
Secondary Degree	-0.00886***	-0.0000443
	(0.00301)	(0.000197)
Post Secondary Degree	-0.00858**	-0.000500*
	(0.00380)	(0.000301)
Employed	-0.0196***	-0.000384*
	(0.00278)	(0.000210)
Income	-0.000000204***	1.23e-08*
	(7.46e-08)	(6.49e-09)
Native Born	0.0258***	0.0000936
	(0.00384)	(0.000370)
Prison at time t-1	0.316***	-0.0000659
	(0.00936)	(0.000352)
Prison at time t-2,t-3	0.295***	0.000268
	(0.00907)	(0.000374)
Charge at time t-2,t-3	0.0559***	0.000376
	(0.00308)	(0.000235)
P-Value	0.000	0.267
F-Statistic	569.796	1.216
Observations	56582	56582

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 estimates include controls for court by year fixed effects. Standard errors clustered two-way at judge and defendant level, the level of treatment. Standard errors appear in parentheses. *p<0.1, **p<0.05, ***p<0.01

Table 6: First Stage

Dependent Variable: Prison Dummy		
	(1)	(2)
Judge Stringency	0.565^{***}	0.457^{***}
	(0.0842)	(0.0624)
Outcome Mean	.114	.114
CourtXYear FEs	Y	Y
F-Statistic	44.982	53.695
Controls	N	Y
Observations	56582	56582

Notes: Table shows first stage estimates with (column 1) and without (column 2) additional controls. Both columns include court by year fixed effects. Standard errors appear in parentheses. *p<0.1, **p<0.05, ***p<0.01

Table 7: Disaggregate Impact of Prison on Charges Post Sentencing

	1 year after (1)	1-2 years after (2)	1-3 Years after (3)
OLS: No Controls	0.385***	0.436***	0.444***
	(0.008)	(0.007)	(0.007)
OLS: Controls	0.091***	0.095***	0.091***
	(0.008)	(0.007)	(0.007)
OLS: Reweighted	0.087***	0.087***	0.079***
	(0.010)	(0.008)	(0.008)
IV	-0.168	-0.522**	-0.429**
	(0.180)	(0.216)	(0.217)
Outcome Mean	.248	.338	.391
Outcome Mean if Prison	.592	.728	.788
CourtXYear FE	Y	Y	Y
Observations	56582	56582	56582

Notes: Table reports OLS and IV estimates of the impact of prison 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 additional controls, as with the OLS:Controls and OLS:Reweighted results. Standard errors clustered two-way at judge and defendant level appear in parentheses. *p<0.1, **p<0.05, ***p<0.01

Table 8: Cumulative 3 year Impact of Prison on Charges, Employment and Income Post Sentencing

	Charged (1)	Employed (2)	Income (3)
OLS	0.444***	-0.305***	-31933.805***
	(0.007)	(0.009)	(661.557)
OLS: Controls	0.091***	-0.038***	-6552.153***
	(0.007)	(0.007)	(598.480)
OLS: Reweighted/Controls	0.079***	-0.025***	-3417.294***
	(0.008)	(0.008)	(422.755)
IV	-0.429**	0.122	-4429.982
	(0.217)	(0.176)	(16802.779)
Outcome Mean	.391	.509	47636
CourtXYear FE	Y	Y	Y
Observations	56582	56582	56582

Notes: The table reports OLS and IV estimates of the impact of prison on the probability of being charged with a crime, employment, and income in the three years after sentencing. IV estimates include controls. Standard errors clustered two-way at judge and defendant level appear in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 9: Spillover Impact of Prison on Coworkers of Financial Criminals

	First Stage		Collegue Financial Crime Within:		
	Defendants	All Obs	1 Year	1-2 Years	1-3 Years
	(1)	(2)	(3)	(4)	(5)
Panel A: All Financial Crimes					
OLS with Controls	0.396***	0.483***	0.015**	0.017**	0.017**
	(0.119)	(0.113)	(0.007)	(0.007)	(0.007)
IV Estimate			-0.047	-0.054	-0.057
			(0.083)	(0.099)	(0.104)
Observations	10164	133946	100253	100253	100253
Panel B: Fraud					
OLS with Controls	0.402***	0.506***	0.006	0.006	0.005
	(0.130)	(0.133)	(0.006)	(0.007)	(0.007)
IV Estimate			-0.190**	-0.228**	-0.272**
			(0.085)	(0.106)	(0.120)
Observations	5862	74607	55359	55359	55359
Panel C: Business Offences					
OLS with Controls	0.401	0.499*	0.049***	0.059***	0.067***
	(0.331)	(0.293)	(0.018)	(0.018)	(0.018)
IV Estimate			-0.049	-0.027	0.077
			(0.234)	(0.243)	(0.240)
Observations	2024	27953	21238	21238	21238
Panel D: Other Financial Crimes					
OLS with Controls	0.354*	0.565***	0.011	0.005	0.005
	(0.199)	(0.207)	(0.013)	(0.012)	(0.012)
IV Estimate			0.056	0.066	-0.001
			(0.122)	(0.145)	(0.159)
Observations	2051	31364	23619	23619	23619

Notes: Data restricted to plants with 50 or fewer employees as described in Section 6. Column 1 reports first stage estimates for defendants only, the relevant sample for identification. Column 2 reports first stage estimates for the colleagues sample. Standard errors in parentheses clustered at the defendant-judge level to account for the level of treatment. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Internet 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

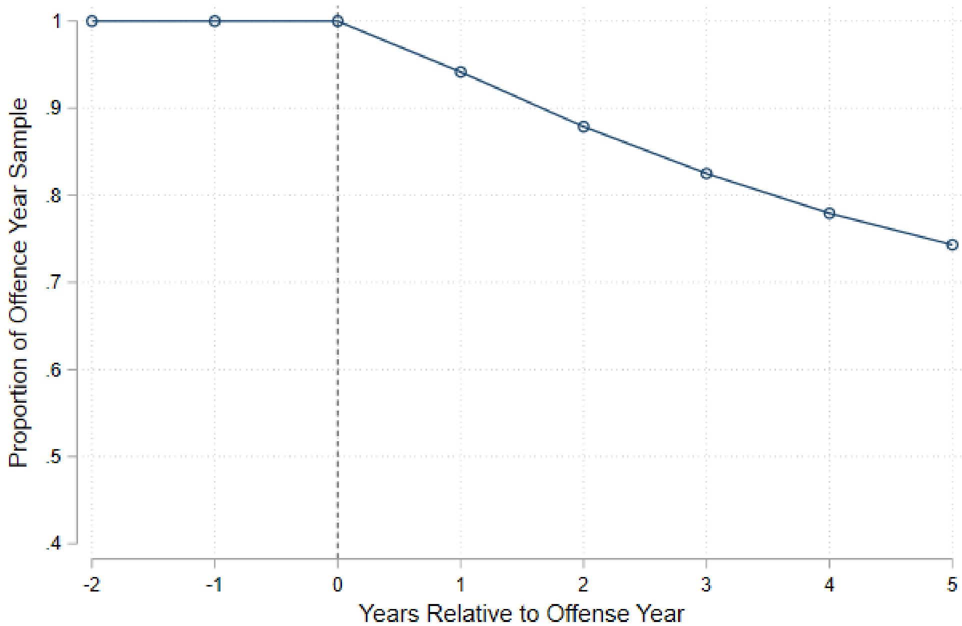
38 - Data and communications offences

48 - Environmental offences

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

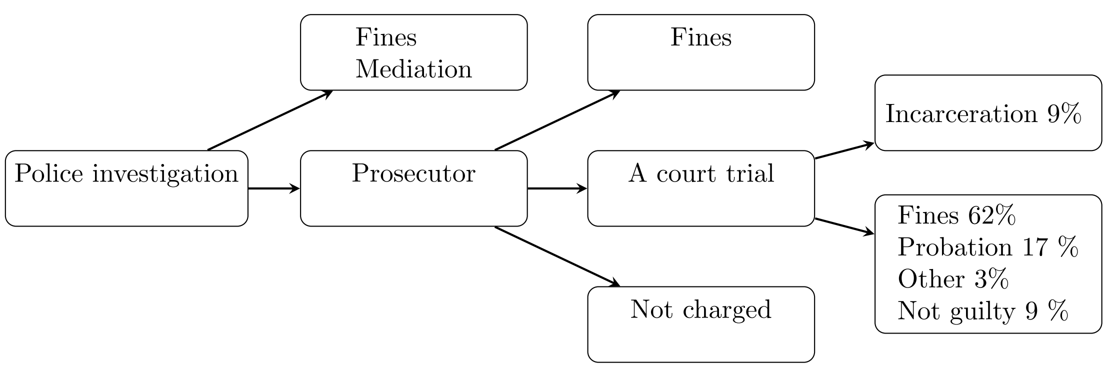
B Additional Figures

Figure B.1: Firm Survival Before and After an Employee Commits a Financial Crime



Note: Figure shows the exit rate of firms where an employee commits a financial crime before and after the crime. Dashed line at time 0 indicates the time the crime was committed.

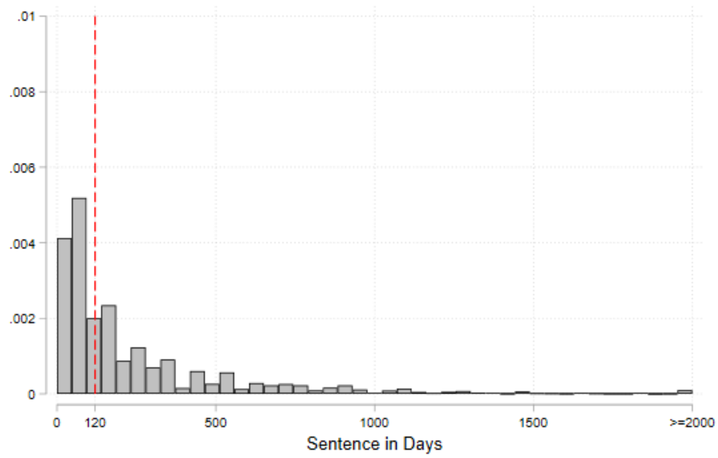
Figure B.2: Sentencing Process and Court Outcomes in Finland



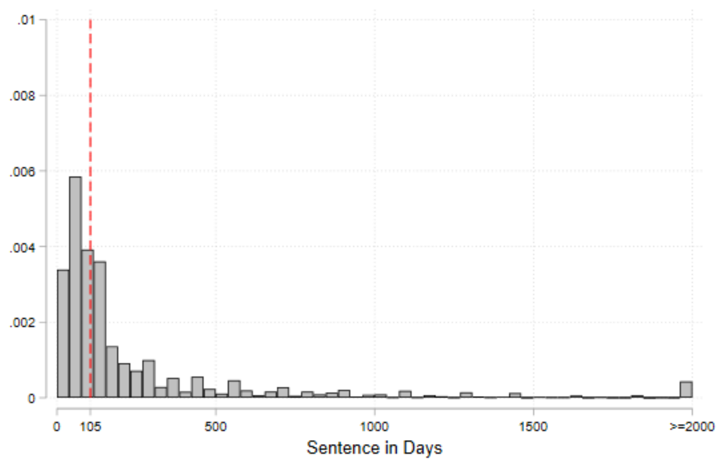
Notes: The figure provides a visual representation of the sentencing process in Finland, and provides information for final sentences for financial crimes.

Figure B.3: Distribution of Sentence Length for Financial Crimes versus Other Crimes

(a) Financial Crimes



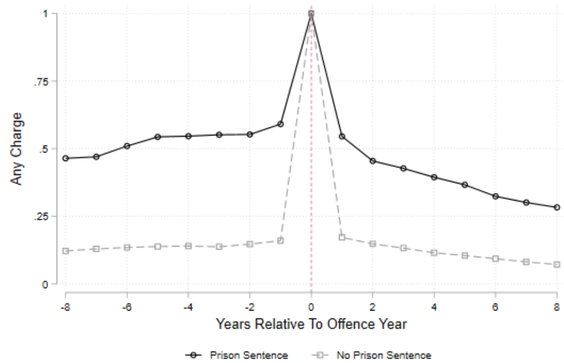
(b) Other Crimes



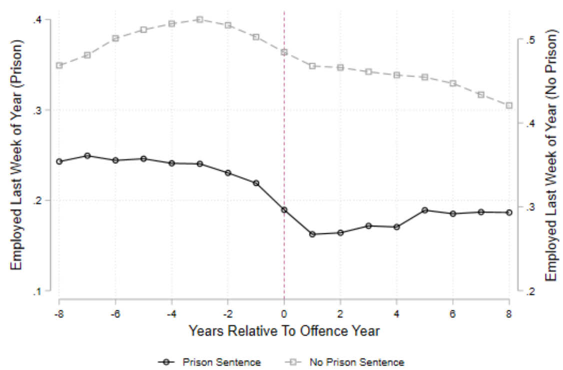
Note: Panel A (B) shows histogram of sentence length conditional on being sent to prison for financial crimes (all other crimes). For these histograms we use all court data without imposing the restrictions from our main analysis. Median sentence length shown via the red dotted line.

Figure B.4: Raw Patterns of Criminality, Employment, and Income Around the Time of Offence

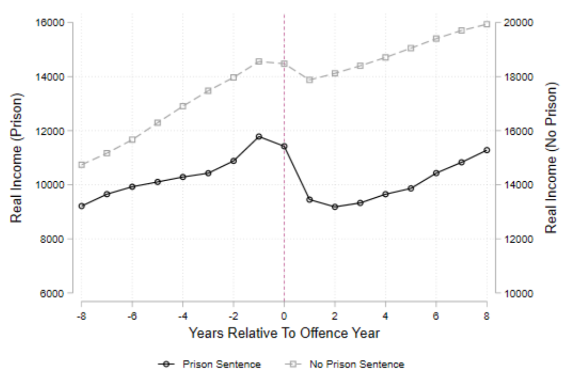
(a) Any Offence Charged



(b) Employment



(c) Income



Note: Panel A shows whether defendants are charged with an offence years before and 8 years after the crime was committed (which can be different than the sentencing year as shown in Figure B.4) separately for those who are sent to prison versus those who are not sent to prison. Panel B (C) shows employment (income) of defendants 8 years before and 8 years after the crime was committed. Employment and income are measured at the end of the year. Income include zeros for those who are not employed. Sample construction and data as defined in Sections 2.

C Additional Tables

Table C.1: Sample Size After Restrictions

	Cases (1)	Defendants (2)	Judges (3)	Courts (4)
All Cases (age>=23)	95314	67797	2862	75
Assigned a Judge	80347	59556	2862	65
Drop training Judges	68376	52458	915	65
Drop Swedish Speaking	66389	50874	911	65
Drop Judges with < 100 Cases	65288	50159	752	65
Drop Courts with < 2 Judges	65285	50158	752	65
Drop if Singleton in Cell	59179	45677	752	64
Drop if Missing Covariate Values	57632	44611	752	64

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.

Table C.2: Monotonicity of the Instrument

	Baseline instrument	Reverse-sample instrument
sub-sample:	First Stage P(Incarcerated) (1)	First Stage P(Incarcerated) (2)
Main Estimation Sample		
Estimate	0.457***	0.374***
	(0.062)	(0.056)
Observations	56582	56582
Over 30 years old		
Estimate	0.479***	0.127***
	(0.066)	(0.038)
Observations	44688	44688
Under 30 years old		
Estimate	0.394***	0.412***
	(0.134)	(0.125)
Observations	11855	11855
Any post-compulsary education		
Estimate	0.337***	0.180***
	(0.067)	(0.047)
Observations	29738	29738
No post-compulsary education		
Estimate	0.594***	0.431***
	(0.106)	(0.105)
Observations	26816	26816
Marrried		
Estimate	0.373***	0.307***
	(0.096)	(0.084)
Observations	18488	18488
Not married		
Estimate	0.484***	0.180***
	(0.073)	(0.063)
Observations	38068	38068
Previously not employed		
Estimate	0.668***	0.338**
	(0.093)	(0.150)
Observations	32701	32701

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.3: First Stage for Subgroups: Types of Crimes

	All (1)	Fraud (2)	Business (3)	Forge (4)	Laundering (5)
Judge IV	0.565***	0.457***	0.296	1.016***	1.005***
	(0.084)	(0.101)	(0.186)	(0.310)	(0.293)
Relative complier share		.779	.473	1.835	1.502
		(.138)	(.249)	(.432)	(.466)
F-Statistic	44.982	20.258	2.518	10.726	11.756
Observations	56582	34307	8392	5190	3946

Notes: Table shows first stage estimates for the full sample (column 1) and then explores the first stage for subsamples for types of crimes, including estimates of the relative complier share to better understand who is among the compliers. *p<0.1, **p<0.05, ***p<0.01

Table C.4: First Stage Complier Analysis: Defendant Characteristics

	All (1)	Charge (n-1) (2)	No Degree (3)	Married (4)
Judge IV	0.565***	0.694***	1.095***	0.416***
	(0.084)	(0.140)	(0.200)	(0.122)
Relative complier share	1.502	1.878	1.327	0.807
	(0.466)	(0.278)	(0.183)	(0.164)
F-Statistic	44.982	24.407	30.023	11.675
Observations	56582	26816	15760	18488

Notes: Table shows first stage estimates for the full sample (column 1) and then explores the first stage for subsamples for types of crimes, including estimates of the relative complier share to better understand who is among the compliers. *p<0.1, **p<0.05, ***p<0.01

Table C.5: Impact of Prison on Recidivism Post Sentencing Correcting for Multidimensional Sentencing

	First Stages		Recidivate Within		
	Pr(Incarcerated)	Pr(Guilty)	1 year after	1-2 years after	1-3 Years after
	(1)	(2)	(3)	(4)	(5)
Panel A: Control for Guilty Verdict Stringency					
IV: Incarcerated	0.557***		-0.231	-0.615**	-0.474**
	(0.087)		(0.196)	(0.239)	(0.236)
Panel B: Instrument Guilty With Guilty Verdict Stringency					
IV: Incarcerated	0.557***	-0.032	-0.219	-0.597**	-0.466**
	(0.087)	(0.090)	(0.192)	(0.234)	(0.231)
IV: Guilty Verdict	0.035	1.470***	0.085	0.127*	0.062
	(0.075)	(0.091)	(0.067)	(0.077)	(0.074)
Observations	56582	56582	56582	56582	56582

Notes: Table shows robustness to adding a control for the judge's guilty stringency measures (Panel A) as well as instrumenting guilty and prison separately in the same specification (Panel B). First stage estimates are shown in the left hand side while the impacts on recidivism of defendants is shown on the right hand side. *p<0.1, **p<0.05, ***p<0.01

Table C.6: Impact of Prison on Recidivism: Age Robustness

	1 year after (1)	1-2 years after (2)	1-3 Years after (3)
OLS: No Controls	0.380***	0.428***	0.435***
	(0.008)	(0.007)	(0.006)
OLS: Controls	0.092***	0.093***	0.089***
	(0.008)	(0.007)	(0.006)
OLS: Reweighted	0.083***	0.083***	0.074***
	(0.009)	(0.008)	(0.007)
IV	-0.004	-0.409**	-0.290
	(0.162)	(0.197)	(0.196)
Outcome Mean	.259	.35	.404
CourtXYear FE	Y	Y	Y
Observations	61451	61451	61451

Notes: The table reports OLS and IV estimates of the impact of prison on the probability of being charged with a crime within the specified time periods after sentencing, equivalent to Table 7, except here we include defendants 21 and above as opposed to defendants 23 and above. 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<0.1, **p<0.05, ***p<0.01

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

	1st year after (1)	2nd year after (2)	3rd Year after (3)	4th year after (4)	5th year after (5)
Panel A: Employment					
OLS: No Controls	-0.302***	-0.281***	-0.274***	-0.262***	-0.252***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
OLS: Controls	-0.052***	-0.036***	-0.040***	-0.029***	-0.028***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
OLS: Reweighted	-0.029***	-0.019***	-0.029***	-0.018***	-0.016**
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
IV	0.104	-0.089	0.089	0.032	0.126
	(0.165)	(0.178)	(0.180)	(0.197)	(0.204)
Panel B: Income					
OLS: No Controls	-10616.918***	-10640.523***	-10676.364***	-10862.515***	-10855.065***
	(214.508)	(235.805)	(247.572)	(246.602)	(259.737)
OLS: Controls	-2328.787***	-2192.730***	-2030.636***	-1992.761***	-1871.059***
	(206.994)	(220.930)	(231.600)	(231.590)	(230.710)
OLS: Reweighted	-1275.481***	-1090.005***	-1051.809***	-1021.059***	-797.659***
	(148.597)	(171.137)	(195.553)	(176.928)	(185.251)
IV	-4325.748	2786.095	-2890.329	-2811.982	-8981.969
	(5777.284)	(6152.252)	(6401.521)	(7085.444)	(7389.590)
Earnings Mean	15177.642	15877.711	16581.585	17234.358	17889.358
Employment Mean	.397	.392	.384	.379	.374
CourtXYear FE	Y	Y	Y	Y	Y
Observations	56582	56582	56582	56582	56582

Notes: The table reports OLS and IV estimates of the impact of prison on on the probability of employment (Panel A) and the impact on income (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<0.1, **p<0.05, ***p<0.01

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

	<= 50 Employees (1)	> 50 Employees (2)
Age	39.14	41.27
	(12.03)	(11.35)
Female	0.427	0.529
	(0.495)	(0.499)
Swedish Speaking	0.0252	0.0304
	(0.157)	(0.172)
Earned Income	25350.5	31467.8
	(14896.5)	(17477.6)
Wages	23709.1	30159.5
	(15178.4)	(17905.6)
Upper Management	0.0964	0.162
	(0.295)	(0.368)
College Degree	0.279	0.429
	(0.448)	(0.495)
Num. of Children	0.902	0.907
	(1.100)	(1.084)
Previous Prison t-3	0.00182	0.000238
	(0.0426)	(0.0154)
Previoust Charge t-3	0.0464	0.0173
	(0.210)	(0.131)
Number of Establishments	9546	3108
Number of Workers	132643	1705605

Notes: This table presents sample means for those who worked at the same plant as an employed defendants at the time of their crime (sample construction is defined in Section 2). Column 1 restricts to plants with 50 employees or fewer and column 2 restricts to firms with more than 50 employees. All statistics are for the year the defendant's crime was committed.

Table C.9: Summary Statistics for Crimes in North Carolina

	Financial (1)	Property (2)	Drug (3)	Violent (4)	Other (5)
Prison	0.15	0.23	0.19	0.31	0.19
	(0.36)	(0.42)	(0.39)	(0.46)	(0.39)
Probation	0.85	0.77	0.81	0.69	0.81
	(0.36)	(0.42)	(0.39)	(0.46)	(0.39)
Sentence in Days Excluding Life	43.10	61.06	99.56	311.07	126.44
	(142.59)	(207.47)	(375.44)	(934.15)	(603.39)
Reoffend within 5 Years	0.65	0.63	0.52	0.50	0.51
	(0.48)	(0.48)	(0.50)	(0.50)	(0.50)
Observations	145,176	342,317	322,141	243,612	967,355

Notes: The table shows summary statistics that replicate Table 3 to get a better understanding of conditions in the United States. Results are for 2000-2015 in North Carolina, where we obtained data sufficient for summary statistics. We exclude all life sentences when computing the average sentence length (this makes little difference for financial crimes, but does increase sentence length for other crime types). Note that recidivism we only include any reoffending that within North Carolina. Charges outside of North Carolina are not available.