LAW AND ECONOMICS SEMINAR
Fall 2025

Professor A. Mitchell Polinsky
Professor John J. Donohue III

Thursday, October 2, 2025
4:15 - 5:45 p.m.
Stanford Law School
Room 272

"Punishing Financial Crimes: The Impact of Prison Sentences on Defendants and Their Colleagues"

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Note: It is expected that you will have reviewed the speaker's paper before the seminar.
The author was asked to provide a "reader's guide" to her paper for readers who are not able to read the paper in its entirety. This is her response: "The most important parts of this paper are the introduction, and then the reader who is short on time can skip straight to Section 5, pages 20-25, and Section 6, which contain all of the core takeaways of the paper. The preceding sections discuss the identification strategy (random assignment of judges) and the institutional context, but given the methodological checks are all sound, these are not vital to read. With a little extra time, Section 7, and especially Figure 5 may be of interest as we are able to also estimate the impacts of financial crimes on victims using our unique data."

PUNISHING FINANCIAL CRIMES: THE IMPACT OF PRISON SENTENCES ON DEFENDANTS AND THEIR COLLEAGUES *

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Abstract

Financial crimes are costly to society but less severely punished than other nonviolent crimes. We investigate whether prison sentences reduce financial crimes. Using random assignment of judges in Finland to identify causal impacts, we find a prison sentence reduces defendant reoffending by 42.9 percentage points three years post-sentencing. Given prior evidence of financial misconduct "contagion," we also explore spillovers on colleagues. A prison sentence reduces the likelihood that a financial crime defendant's colleagues commit crimes by 27 percentage points, suggesting broader deterrent effects of harsher punishments, but only for fraud cases. Last, we show financial crimes are not victimless crimes.

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1 Introduction

Financial crimes, including transgressions like fraud and accounting offenses, impose significant costs on households and firms each year. In Finland, the context of this study, financial crimes cost an estimated €150 million per year (Tanttari and Alanko, 2017). While financial crimes are sometimes thought of as "victimless crimes," we estimate that they impose small but statistically significant labor market costs on victims.

Moreover, financial crimes are increasingly common across the world. The Federal Trade Commission reports that $8.8 billion was lost to fraud in 2022, a 30% increase compared with 2021. The number of fraud cases reported to police in Finland in our data doubled between 2010 and 2016 and doubled again from 2016 to 2023 according to official statistics. Yet despite the nontrivial costs and many victims of financial crimes, these defendants are sent to prison less often compared with those who commit other nonviolent crimes. 11% of financial crime defendants are sentenced to prison in Finland, a lower rate compared with nonviolent property crimes (36%) and nonviolent drug crimes (22%). Financial crime defendants are also less likely to be found guilty, with 87.9% found guilty compared with 93.7% of nonviolent property crime defendants and 97.7% of nonviolent drug crime defendants. Conditional on conviction, 10% of financial crime defendants are sentenced to prison, which is a much lower rate than the 33% (21%) of property (drug) crime defendants sentenced to prison conditional on conviction.

Given the costs of financial crimes and the lesser consequences for those who commit them, a key policy question is whether prison sentences decrease financial crimes. This paper studies two ways prison sentences might reduce financial misconduct. First, we estimate the causal impact of a prison sentence on recidivism of financial crime defendants. Reducing future crimes amongst existing defendants is of first-order importance, given that almost half of these defendants reoffend within five years. Whether prison reduces future financial misconduct 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. This could be especially relevant for financial crime defendants who are more likely to be employed, have higher incomes, are six years older, are twice as likely to be college-educated, and are more than twice as likely to be in upper management compared with other nonviolent offenders. Alternatively, prison could rehabilitate defendants or have a deterrence effect, reducing reoffending.

Second, we examine whether there is a broader "chilling" effect of prison sentences for financial crimes via reductions in the financial misconduct of workplace colleagues. To date, little is known about how observing a more severe sentence might change the behavior of connected individuals outside of the family. If prison sentences also reduce the criminality of colleagues, this would constitute an important general deterrence effect of harsher punishments for defendants.

To complete our analysis, we use population-level administrative data from Finland from 2000-2018. We identify defendants in financial crime cases and use unique identifiers to link them 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 financial crimes. The defendants we observe in the data primarily commit fraud (60% of all cases), business offenses (15%), forgery (9%), and money laundering (7%).

To identify the causal impact of prison sentences on financial crime defendants and their colleagues, we leverage the fact that cases are randomly assigned to judges by law in Finland, and judges differ in how likely they are to send defendants to prison. To implement this strategy we collected data on judges in conjunction with the National Court Registrar which we linked to the administrative defendant records. This identification strategy to isolate causal impacts of punishments 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; Chang and Schoar, 2022). We show support for randomization through balance checks. We also find a strong first stage, with judge assignment highly predictive of receiving a prison sentence.

We find that sentencing a financial crime defendant to prison decreases the probability they are charged with another offense within three years of sentencing by 42.9 percentage points. This causal estimate contrasts sharply with OLS estimates, suggesting prison is associated with an *increase* in reoffending. Turning to mechanisms, we rule out incapacitation as the marginal defendant is sentenced to 253 days, and our reoffending results only become significant 2 years after sentencing. We also 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 as a likely explanation.

Next, we examine spillovers on peers' criminality as a possible broader deterrence effect of prison sentences. For fraud defendants, we observe a 27 percentage point reduction in the likelihood of colleagues being charged with a crime in the next three years. While this is a large effect, it is smaller than the spillovers observed in Bhuller *et al.* (2018), which finds that criminal network members (brothers) of defendants sentenced to prison are 51 (32) percentage points less likely to be charged with a crime in the next four years. We caution that the standard error is large, so we cannot rule out that the reduction is only 3.7 percentage points at the upper bound of the 95% confidence interval. We also caution that while the point estimate when pooling all financial crime defendants together (as opposed to focusing on just fraud defendants) is still negative, it is smaller and not statistically significant.

In the last part of the paper, we explore the policy relevance of our results. First, we demonstrate an important reason why policymakers may be interested in reducing financial crimes. We find that despite often being viewed as victimless crimes, financial crimes impose small but significant negative labor market impacts on victims, with a 5.3% drop in earnings for victims in the year after the crime. We do so through an event study research design comparing those we observe were victims of financial crimes to observational similar individuals who were not. For this exercise, we use police data on reported financial crimes where we can observe victims and use their unique IDs to perfectly link to administrative data on their labor market outcomes.

Last, we show that it is difficult to justify the more lenient treatment of financial crime defendants relative to other non-violent property and drug crime defendants with "efficiency" arguments. These arguments generally hinge on the perception that incarceration will be less effective at reducing reoffending among financial crime defendants. To the contrary, we provide evidence that incarceration more effectively reduces recidivism for financial crime defendants relative to these other non-violent crime defendants.

Our paper makes several contributions. First, we contribute to the literature on financial misconduct. Egan *et al.* (2019) find that roughly half of financial advisers committing misconduct are fired after being caught, but they are easily rehired into new firms and continue to engage in financial misconduct. We complement this innovative study by showing their descriptive results on employment and recidivism apply to financial misconduct in general. More consequentially, we provide the first rigorous empirical evidence on the role of prison in reducing financial misconduct through its impacts on defendants and their colleagues. This contributes to a smaller group of papers documenting other actions to reduce financial misconduct. Honigsberg and Jacob (2021) estimate that removing records of misconduct increases reoffending and Kowaleski *et al.* (2020) find that ethics exams for employees reduce financial misconduct.

As such, we also contribute to a growing literature in economics examining the impacts of prison sentences on defendants. This literature is mixed. In the United States, Aizer and Doyle (2015) shows that incarcerating juveniles increases adult recidivism, and Mueller-Smith (2020) finds that incarcerating adults in Texas also increases recidivism. In contrast, Kuziemko (2013) and Rose and Shem-Tov (2021) find that increasing sentence lengths in the United States decreases recidivism. These results may be mixed in part because of heterogeneous treatment effects across different populations of defendants, as suggested by Eren and Mocan (2021) who find very different impacts for juveniles who commit drug versus property crimes, and Bhuller *et al.* (2020) who find very different effects depending on whether defendants are employed or not. This means that we cannot take a "one size fits all" approach to understanding the impacts of prison sentences on defendants. That our IV and OLS estimates are opposite-signed suggests that good identification is necessary to identify impacts on marginal defendants, who are a potential policy-relevant group when considering whether to increase or decrease prison sentences. Policies leading to incremental changes in judge behavior are most likely to impact this group.

Third and last, we contribute to the literature on the importance of corporate culture and spillovers in misconduct across colleagues within a workplace or across workplaces. For example, Johnson (2020) finds that citing one firm for OSHA violations causes other facilities to improve their compliance with existing rules. Dimmock *et al.* (2018) shows that there is contagion in perpetrating financial misconduct, with individuals quasi-randomly exposed to financial misconduct of colleagues more likely to commit misconduct themselves. Bayer *et al.* (2009) and Stevenson (2017) demonstrate contagion of criminal behavior among incarcerated juveniles, and Battaglini *et al.* (2019) document important spillovers in criminality between tax professionals and their clients. These findings build on a broader literature documenting important peer effects between colleagues (Ichino and Maggi, 2000; Jackson and Bruegmann, 2009; Mas and Moretti, 2009; Dahl *et al.*, 2014; Nix, 2020), and motivate our focus on the spillover effects of observing a colleague receive a harsher punishment for financial misconduct.[1] While ours is the first paper to estimate the spillover effects of prison sentences on colleague criminality, we contribute to a growing literature examining spillovers of prison sentences on family members (Norris *et al.*, 2021; Arteaga, 2020; Billings, 2018; Dobbie *et al.*, 2018) and members of criminal networks (Bhuller *et al.*, 2018).

Even more closely related are a series of papers demonstrating important enforcement spillovers in the economics of crime literature. Corman and Mocan (2005) present evidence that sanctions for robbery and motor vehicle violations reduce crime beyond just the perpetrator. Rincke and Traxler (2011) show that when one household in a village is caught without a proper television license and forced to comply in Austria, this significantly increases compliance in nearby houses. Similarly, Drago, Mengel and Traxler (2020) show through an RCT that mailings sent to evaders increase compliance not only for the targeted household but also nearby households while Brollo *et al.* (2020) show spillovers on other students from enforcement actions for missing school.

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 enough evidence to prove a crime was committed. 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.

[1] Our spillover finding overlaps to some degree with Mohliver (2019) documenting how professional networks can extinguish a given practice once it is proven to be illegitimate or illegal.

Appendix Figure F1 summarizes the criminal proceedings for cases that end up in district courts.

When a case arrives in a court, 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.[2] We use this variation to identify the causal impacts of prison. 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.

The majority of criminal cases (66.72% of our sample) are dealt with by a panel of one professional judge and 2-4 lay judges, with most of the remaining cases (32.75% of our sample) assigned to a single professional judge.[3] In some very severe cases, a panel of three professional judges handles the case, but this almost never occurs for financial cases.[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. If the panel cannot reach a unanimous verdict through discussion, they will vote. First, they vote if the defendant is guilty. Next, they vote on how to punish the convicted defendant (i.e., with prison, probation, or fines). The professional judge always votes first when there are lay judges.[5] When we use our judge stringency measure to identify the effects of prison, we use the stringency of the professional judge, as in Bhuller *et al.* (2018) where the institutional context is similar. We do so for two reasons. First, we do not observe the identities of the lay judges. 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, given that lay judges are

[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 in Finland from 2015 onward after our estimation sample ends.

[3]Lay judges are politically appointed "assistant judges." They must be between the ages 25-65 (25-63 before 2014) and cannot hold another court position. They cannot work for the police or as a lawyer. Before 2014, cases requiring a judge panel comprised 1 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 sentence 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.

also randomly assigned. Second, while lay judges can, in theory, overrule the professional judge, this rarely happens in practice. Government documents state, "the number of voting decisions made in a jury panel can be considered marginal in both criminal and civil cases. In 1997, voting decisions accounted for less than half a percent of all criminal cases handled by a jury panel. In dispute and application cases, the mentioned percentage was 2 in both" (of Finland, 2007). In practice, the professional judge is the primary decision-maker in these cases (de Godzinsky and Ervasti, 1999). We provide much richer details on the decision-making process in Appendix B.

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 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 F1 presents the shares of different kinds of punishments for financial crimes. Among all criminal-court cases in Finland (i.e. before imposing any of the sample restrictions necessary to estimate the impacts of prison), 9% of cases receive a not guilty verdict. Conditional on receiving a guilty verdict, 10% are sent to prison, and 90% receive some other punishment, generally fines (68%) and probation (19%).

Defendants serve their prison sentence in publicly-funded prisons focused on rehabilitation. All prisoners enter education programs or work unless a health condition precludes participation. While almost every defendant assigned a prison sentence serves some time, most will serve only a subset of their sentence and will qualify for parole before the full sentence length is served.[6]

2.2 Data

We use a combination of existing administrative data and administrative data we collected for this project. Our main data set is Statistics Finland's district court data (Statistics Finland, 2025a), consisting of every criminal case in Finnish courts between 1992 and 2018 (we do not have access to data after 2018). We collapse the data to the individual-case level (a single case can contain

[6]Appendix Table G1 provides deterministic rules governing the length of sentences served in Finland. Appendix B provides details on punishments in Finland. We discuss external validity to other contexts in Appendix E.

multiple crimes; for example, fraud could be committed along with identity theft). When presenting 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 lacks judge information. Thus, we collected judge data from Finland's national court registrar, the legal registry centre, ORK (Legal Registre Centre, 2025). We link the judge ID back to the district court data using unique individual-case-level numbers. The judge data is only available digitally from 2000 to 2015.[7] Finally, we link the court data using unique defendant ids to the Finnish Linked Employer-Employee Data, FLEED (Statistics Finland, 2025b) available from 1988-2016, and FOLK basic (Statistics Finland, 2025c) and FOLK income datasets (Statistics Finland, 2025d). These data contain information on demographics, earnings, and employment, including unique firm and plant identifiers, for all Finnish residents.

Using these workplace identifiers, we link defendants to their colleagues. We identify every individual who had the same firm and plant IDs as the defendant in the year their offense was committed. We then link these colleagues to the court data using their unique person IDs and create outcomes measuring if these colleagues commit criminal offenses in the years after a defendant is sent to prison. Hereafter, "workplace" refers to the "plant/establishment."

We make a few additional restrictions to arrive at an analysis sample where the randomization of court cases to judges applies. We restrict the data to cases assigned to judges residing in courts with at least two active judges (there must be at least two judges to have random assignments). 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 Swedish-speaking judge.[8] We exclude juvenile defendants because they are

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

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

treated differently by the courts and not always randomly assigned to judges.[9] Last, we require each judge to see a minimum of 100 randomly assigned criminal (but not exclusively financial) cases during the years of analysis, 2000-2015, to ensure we obtain an accurate measure of judge stringency. Our results are robust to other cutoffs for the number of cases per judge, such as 50 cases per judge (see Appendix Table G3). In the main analysis, we restrict the sample to court decisions from 2000 to 2013 to ensure a long enough follow-up period after the court decisions. Appendix Table G2 shows how each of these restrictions decreases the number of judges, courts, and defendants in our sample. After imposing these restrictions, we are left with a final sample of 752 unique judges serving in 64 courts with 44,611 unique financial crime defendants.

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. We also estimate impacts on defendants' employment and total income. Employment is defined as whether the defendant's main activity is employment in the last week of December of the given calendar year. Income consists of income from work and other taxable income, such as taxable transfers like parental leave and unemployment benefits. It does not include capital income. To measure changes in colleagues' criminal behavior, 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. A colleague is defined as someone working in the same plant in the last week the year before the crime occurs.

2.3 Defining Financial Crimes

To define financial crimes, we use the definitions from the European Financial and Economic Crime Centre and the FBI database for white-collar crimes.[10] Table 1 reports the top 5 broad financial crime categories in our estimation sample. The largest crime category is fraud, which consists of 60% of all financial crimes in our estimation sample, followed by business offenses (15%), forgery (9%), and money laundering (7%). Other types of offenses make up the remaining

[9]Defendants below age 21 are treated as "young" defendants and treated differently by the law. We use the age 23 restriction to avoid all young defendants and because we focus on financial crime defendants who are generally older. Our results are robust to dropping the age restriction to age 21 (Appendix Table G4).

[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.

9% of cases. Previous papers have focused on subsets of these financial crimes. For example, Egan *et al.* (2019, 2021) focus only on financial adviser misconduct. [11]

Table 1: Types of Financial Crimes

Proportion:	Total Sample (1)	Convicted (2)	Prison (3)	Prison if Convicted (4)
Fraud	0.606	0.893	0.119	0.134
Business Offences	0.148	0.868	0.065	0.074
Forgery	0.092	0.946	0.181	0.191
Laundering	0.070	0.847	0.144	0.170
Political Corruption	0.009	0.405	0	0
Other	0.075	0.787	0.073	0.092
Observations	56583	56583	56583	49732

Notes: Column 1 reports the proportion of financial crime defendants made up of the 5 most prevalent sub-categories of financial crimes. Column 2 reports the share of defendants convicted for each sub-category, column 3 reports the share sent to prison out of all defendants, and column 4 reports the share sent to prison out of defendants who were convicted. The data consists of all district court cases with a financial crime defendant in Finland from 2000 - 2013, where defendants were 23 or older at the time of the offense, were not Swedish-speaking, and were sentenced in a courthouse with at least two active judges by a judge who handled at least 100 cases in the sample period. The unit of observation is at the defendant-case level.

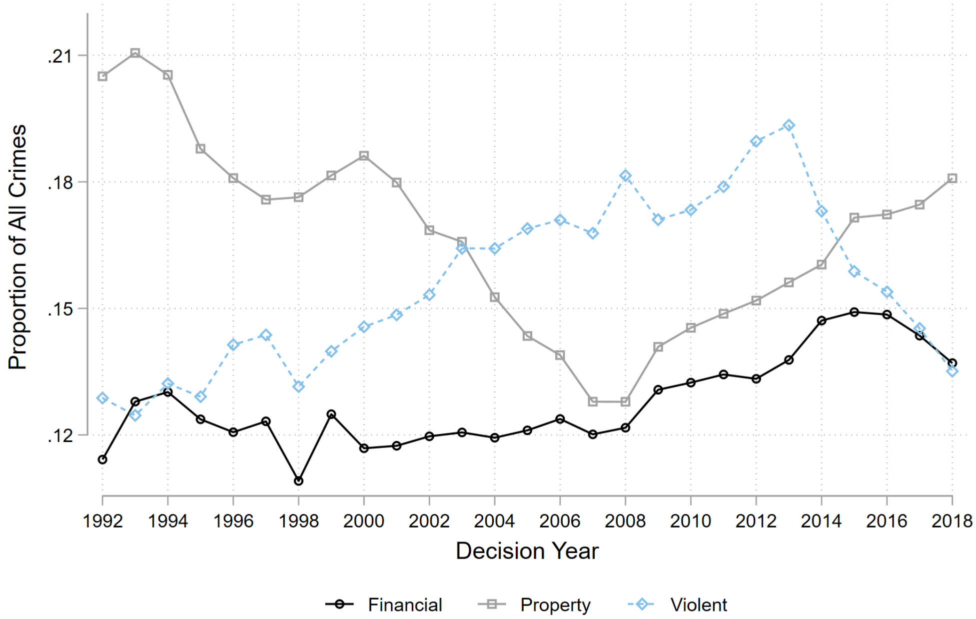
Figure 1 reports the share of all criminal cases decided in Finnish courts from 1992 to 2018 which were financial crimes, violent crimes, or property crimes, the three largest crime categories excluding traffic crimes. Since traffic crimes are excluded, the shares do not add to 1.[12] We find that over time, the share of all crimes consisting of financial crimes has grown from just under 12% to over 14%. This represents a 16% increase in the share of all crimes that are financial crimes over these 26 years. 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.[13]

[11]For a full list of all crimes included, see Appendix A.

[12]Figure 1 collapses the data at the defendant-case level. Each case is assigned a crime type based on the most severe crime. Appendix Figure F2 replicates Figure 1 but also includes traffic crimes, so the shares add to 1.

[13]Substantially more financial crimes are reported than end up in court since not all crimes are prosecuted.

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



Note: This figure plots the share of all district court cases containing a financial, property, or violent crime in Finland between 1992-2018, before applying the sample restrictions described in Section 2 and 4.

3 Descriptive Statistics for Defendants and Their Colleagues

3.1 Who Commits Financial Crimes?

Panel A of 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 largely been the focus of previous papers on the impacts of prison on defendant outcomes. 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 than other nonviolent-crime defendants. In summary, financial crime defendants are, on average, different (and generally better off) across every dimension compared with other nonviolent defendants.

Table 2 Panel B reveals how different crime categories are punished in our estimation sample. 11% of financial crime defendants are sent to prison, which is nearly half the rate that drug-crime defendants (21%) and less than a third of 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 of being 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 than defendants for other nonviolent crimes. At first glance, the fact that there are over twice as many financial crimes as property crimes may be surprising, but crime statistics often lump fraud in with property crimes, and fraud makes up a large share of all financial crimes.

Financial crime defendants recidivate often. In the five years after being sentenced for a financial crime, approximately 45% of the defendants were caught committing another crime (Appendix Figure F3). This high rate of recidivism underscores the importance of investigating how to prevent future 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. Comparing those who commit financial crimes and are sent to prison versus those who are not sent to prison, we find that those who are sent to prison have a third of the income in the year before sentencing, are 32 percentage points less 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 (Appendix Table G5).

Appendix Table G6 reports summary statistics separately for the main sub-categories of financial crimes: fraud, business offenses, and other financial crimes. Fraud and other financial crimes differ from business offenses in several ways: The offenders are younger, more likely to be female, have lower earnings, and are less likely to be employed.

3.2 Firms and Colleagues

Appendix Table G7 Panel A provides summary statistics on the establishments where financial crime defendants are employed compared to all other establishments in Finland. We restrict to establishments with 50 or fewer employees to be consistent with our analysis of the spillovers to colleagues later in the paper. For establishments where financial crime defendants are employed, we report statistics from the year before the crime. For all other establishments, we report aver-

Table 2: Defendant Sample Means By Crime Type

	Financial (1)	Property (2)	Drug (3)	Violent (4)	Other (5)
Panel A: Labour Market and Demographic Variables					
Age at Conviction	38	33	31	36	40
	(10)	(9)	(8)	(10)	(11)
Female	0.249	0.144	0.137	0.126	0.119
	(0.433)	(0.351)	(0.344)	(0.332)	(0.323)
Earned Income (€)	14175	5885	7326	13532	14745
	(16373)	(7467)	(8882)	(13291)	(15171)
Employed	0.424	0.133	0.209	0.411	0.413
	(0.494)	(0.340)	(0.407)	(0.492)	(0.492)
Share Upper Management	0.052	0.006	0.010	0.023	0.041
	(0.223)	(0.079)	(0.103)	(0.153)	(0.200)
Share Tertiary Degree	0.158	0.027	0.033	0.084	0.137
	(0.364)	(0.163)	(0.179)	(0.278)	(0.344)
Num. of Children	0.544	0.202	0.169	0.410	0.385
	(1.044)	(0.647)	(0.582)	(0.913)	(0.897)
Observations	56,583	37,199	22,444	80,599	34,286
Panel B: Court Outcomes					
Prison	0.114	0.356	0.216	0.134	0.101
	(0.318)	(0.479)	(0.412)	(0.340)	(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	0.549	0.612
	(0.500)	(0.492)	(0.495)	(0.498)	(0.487)
Sentence in Days	77	100	163	151	66
	(403)	(639)	(563)	(1422)	(355)
Not Guilty	0.121	0.063	0.022	0.081	0.075
	(0.326)	(0.243)	(0.150)	(0.273)	(0.265)
Previous Charge	0.357	0.715	0.575	0.396	0.344
	(0.479)	(0.451)	(0.494)	(0.489)	(0.475)
Observations	56,583	37,199	22,444	80,599	34,286

Notes: This table presents summary statistics for criminal defendants in Finland between 2000-2015 by the type of primary crime they were charged with. Panel A reports sample means of labor market and demographic characteristics. Income and employment information are measured in December of each year. Panel B reports sample means of court outcomes. Previous charge is an indicator if the defendant had been charged with a crime within the three years before their current case. Means are reported with standard deviations in parentheses below. The sample is restricted to defendants who were 23 or older at the time of the offense, were not Swedish-speaking, and were sentenced in a courthouse with at least two active judges by a judge who handled at least 100 cases in the sample period.

ages across all years. Panel A shows that establishments employing financial crime defendants are slightly larger, pay 14% less and are more likely to be in real estate and construction compared with all other establishments in Finland.

Panel B reports descriptive statistics for defendants employed the year before the crime, their colleagues, and workers in all other establishments in Finland employing 50 or fewer workers. These statistics show that defendants and their colleagues are 2-3 years younger, earn slightly less, and are less likely to be college-educated and more likely to be high school dropouts than all other workers in similarly sized establishments.

4 Research Design

4.1 Specification

We capture the relationship between prison and defendant outcomes as follows:

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

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 and 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. To calculate this residualized, leave-out judge stringency measure, Z_{icjt}, we regress a prison 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. We use all cases when calculating judicial stringency, not just financial cases. Formally, the equation for our leave-out residual prison stringency instrument can be written as:

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

where n_j is number of cases seen by judge j and n_{ij} is the number of defendant i's cases seen by judge j. After we remove the defendant's 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 $Z_{j(i)ct}$ and the prison sentence P_{ict} is:

$$P_{ict} = \alpha_0 + \alpha_1 Z_{j(i)ct} + \alpha_2 \boldsymbol{X}_{ict} + \epsilon_{ict}, \tag{2}$$

and the second-stage relationship is given by Equation 1. This 2SLS strategy works if judges vary in their sentencing severity and the assignment of defendants to judges is not correlated with unobserved defendant characteristics associated with both the likelihood of a given punishment and the defendant's outcomes. Given randomization of cases to judges within year, court, and crime type (a legal requirement in Finland), the latter condition is 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). In other words, we estimate 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 a relevant policy parameter of interest, as it captures those who would be most impacted by an incremental change in the use of prison. For example, in a policy change where judges were instructed to incarcerate a larger share of financial-crime defendants, we expect that the compliers would be precisely those incarcerated.

Following several papers that use a judge stringency IV design, we include those found not guilty in our estimation (e.g., Bhuller *et al.* (2018); Dobbie *et al.* (2018)). At first glance, these defendants seem irrelevant to the first-stage incarceration decision, but Arteaga (2020) highlights that judges may strategically make conviction and incarceration decisions jointly.[14] Thus, omitting cases found not guilty would threaten identification in the judge stringency IV design. Section 5 accounts for potential multidimensional sentencing issues with the conviction decision. We also show that our results are robust to not including those found not guilty (Appendix Table G8).

4.2 Validity of the Judge Instrument

This identification strategy recovers the causal impacts of prison if four assumptions hold. First, the instrument has to be as good as randomly assigned. A testable implication of this assumption is that the instrument should not correlate with the defendant's observed pre-determined characteristics. Table 3 Column 2 reports results from a balance test in which we regress our judge-stringency instrument on a set of pre-determined observables. We find that almost all coefficients are very small and not statistically significant. Only three variables are significant, but only at the 10% level (compared to almost all variables being significant at the 99% level in Column 1). Moreover, they are almost zero, so they are not economically meaningful. Any significance is likely due to chance. 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 the 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's 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 3 provides robust evidence that cases are randomly assigned.

Second, we must have a strong first-stage relationship between our instrument (judge stringency) and whether the defendant receives a prison sentence. Column 1 of Table 4 presents first-stage estimates from equation 2. A 10 percentage point increase in the stringency of the judge corresponds to a 5.6 percentage point increase in the probability the defendant is sent to

[14] A large literature shows judges and other criminal justice officials alter sanctions strategically based on their preferences (e.g., Macdonald (2024); Bushway *et al.* (2012); Rehavi and Starr (2014))

Table 3: Balance Check

	Prison (1)	Judge Strictness (2)
Age	0.0000335	-0.0000112
	(0.000129)	(0.0000101)
Female	-0.022***	0.000195
	(0.002)	(0.000203)
Children	-0.004***	-0.0000158
	(0.0009)	(0.0000813)
Married	0.004*	-0.000251
	(0.002)	(0.000224)
Secondary Degree	-0.008***	-0.0000443
	(0.003)	(0.000197)
Post Secondary Degree	-0.008**	-0.000500*
	(0.003)	(0.000301)
Employed	-0.019***	-0.000384*
	(0.002)	(0.000210)
Income	-0.000000204***	1.23e-08*
	(7.46e-08)	(6.49e-09)
Native Born	0.025***	0.0000936
	(0.003)	(0.000370)
Prison at time t-1	0.316***	-0.0000659
	(0.009)	(0.000352)
Prison at time t-2,t-3	0.295***	0.000268
	(0.009)	(0.000374)
Charge at time t-2,t-3	0.055***	0.000376
	(0.003)	(0.000235)
P-Value	0.000	0.267
F-Statistic	569.796	1.216
Observations	56,582	56,582

Notes: This table reports estimates from regressing a prison sentence indicator (column 1) and the judge stringency instrument (column 2) on defendant characteristics. These characteristics are highly predictive of a prison sentence but not of judge stringency. All estimates include controls for court-by-year fixed effects. Standard errors clustered two-way at the judge and defendant level are in parentheses. The reported F-statistic tests the joint hypothesis that the coefficients on all included variables are zero. *p<0.1, **p<0.05, ***p<0.01

prison, which is significant at the .001 level, indicating a strong first stage. Column 2 adds controls consisting of the variables from Table 3. The point estimate changes from 5.6 percentage points to 4.5 percentage points, but this difference is not statistically significant. When presenting IV estimates later, we will include these same controls, but results are statistically indistinguishable if we do not. Figure 2 provides a visual representation of the first stage. The histogram depicts variation in judge stringency. 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 4, we find a strong relationship.

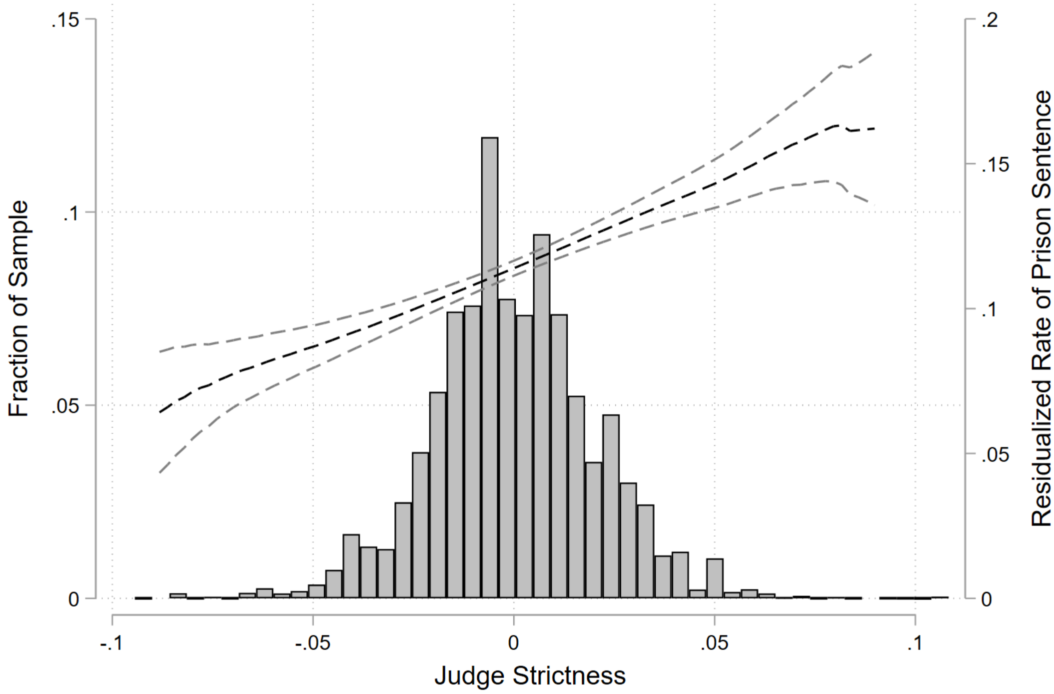
Table 4: First Stage

Dependent Variable: P(Prison sentence)		
	(1)	(2)
Judge Stringency	0.565^{***}	0.457^{***}
	(0.084)	(0.062)
Outcome Mean	0.114	0.114
Court $\times$ Year FEs	Y	Y
F-Statistic	44.982	53.695
Controls	N	Y
Observations	56,582	56,582

Notes: This table presents first-stage estimates without (column 1) and with (column 2) additional controls. Estimates are obtained using equation (2). Both columns include court-by-year fixed effects. Standard errors clustered two-way at the judge and defendant level appear in parentheses. The reported F-statistic is for the joint hypothesis that the coefficients on all included regressors are zero. *p<0.1, **p<0.05, ***p<0.01

Third, the monotonicity assumption must hold. In our context, this assumption means the incarceration probability must be an increasing function of the instrument. In practice, this means that a stricter judge would also incarcerate any individual who a lenient judge incarcerates. Appendix Table G9 provides evidence this assumption holds. Using the approach from Bhuller *et al.* (2020), we show a strong first stage in the different sub-samples of the data and that 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,

Figure 2: Variation in Judge Stringency and First Stage



Notes: This figure provides a graphical representation of the judge stringency IV. The histogram represents the distribution of individual judges' stringency measures, which capture how strict each judge is after residualizing out court by year by crime type fixed effects. The black dashed line plots the fitted values from a nonparametric regression of the effect of judge stringency on the likelihood a given defendant receives a prison sentence (the right-hand axis), with its 95% confidence interval shown by the grey dashed lines. The sample consists of criminal court cases in Finland between 2000 and 2015, restricted to defendants who were 23 or older at the time of the offense, are not Swedish-speaking, and were sentenced in a courthouse with at least two active judges by a judge who handled at least 100 cases in the sample period.

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, as we find across all reverse-sample tests. A recent test proposed by Frandsen *et al.* (2023) checks for more subtle violations of monotonicity. As with other papers, such as Norris *et al.* (2021) and the other examples from Frandsen *et al.* (2023)), we fail this stricter test of monotonicity. However, as Frandsen *et al.* (2023) point out, this failure simply means that our results should be interpreted as weighted averages of treatment effects.[15]

[15]The test's failure technically means that the strict monotonicity assumptions fail or the exclusion restriction fails. However, given the rich and abundant evidence we provided for the exclusion restriction in our setting, we interpret the failure of the test as a failure of strict monotonicity.

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. We describe this challenge in more detail and present robustness checks in Section 5.

5 The Impact of Prison on Defendant Reoffending

Figure 3 shows the impacts of a prison sentence on reoffending, i.e., a dummy variable equal to one if the defendant appears in a court case for any crime in the years before and after the sentence. The effects are obtained from separate regressions where the outcome in the years before sentencing is an indicator of being charged in a given year, while in the post-sentence years (from 0 to 5) the outcome is an indicator of 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 whether a defendant committed a crime in the past. This is precisely what we find.

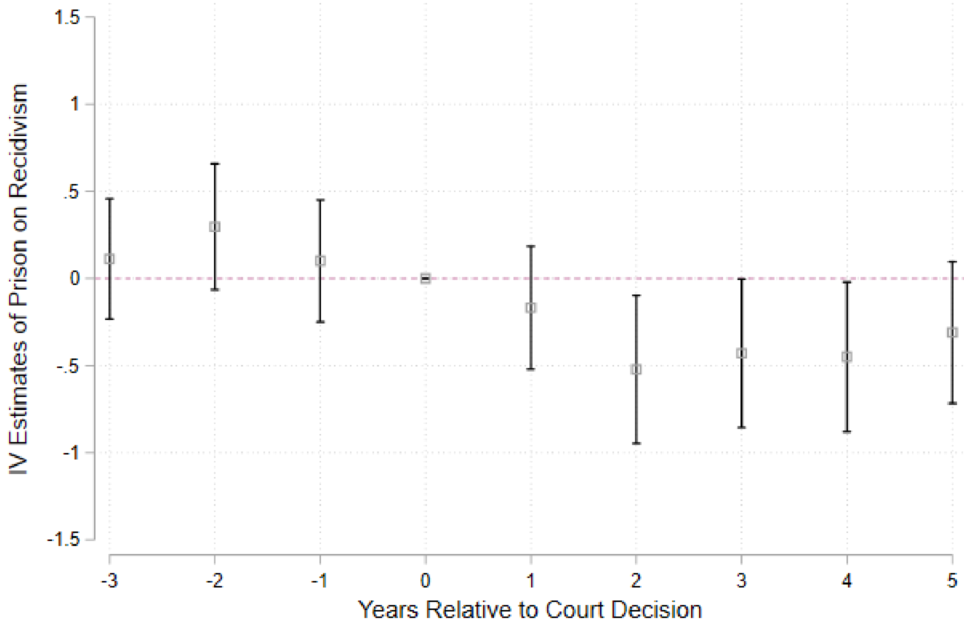
Turning to post-sentencing estimates, we see that in the first year there is a marked drop in reoffending for the defendant quasi-randomly sent to prison, but it is not statistically significant. By the second, third, and fourth years post-sentencing, there are statistically significant declines in whether the defendant commits a new crime. Point estimates indicate that three years post-sentencing, a prison sentence reduces recidivism by 43 percentage points (See Table 5). 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. Appendix Table G11 focuses instead on total charges (intensive margin criminality) as opposed to recidivism (extensive margin criminality) and shows that sending a financial crime defendant to prison results in a statistically significant reduction of 14 charges in the three years afterward.

A 43 percentage point drop in reoffending at first glance seems impossibly large, particularly when considering that only 39% of financial crime defendants commit a new crime within three years of conviction. However, the average reoffending rate in the full sample does not tell the whole story. In Table 5, we report the average reoffending rate in the 1 to 3 years post-sentencing for the entire sample and for the sub-sample of those who are sentenced to prison. 72.8% (78.8%) of those sentenced to prison are charged with a new crime within two (three) years, nearly double the reoffending rates in the full sample. These differences in average reoffending between the full sample and those sent to prison indicate 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 incarceration for those on the margin of being sent to prison, their potential recidivism rate is likely larger than the overall reported recidivism rate in the sample. Compared to the mean recidivism rate for those sent to prison, 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.

The main analysis uses all cases to estimate impacts. However, judges who act as part of a panel may behave differently than those who decide cases on their own (Kastellec, 2013; Holden *et al.*, 2021). To address this concern, we replicate our results using only those cases decided by a single judge. While this leaves us underpowered since only 32.75% of cases are decided by just one judge, we still find a negative 35 percentage point impact on recidivism in the three years post-sentencing from being randomly assigned a stricter judge (Appendix Table G12).

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). That is, we estimate the first-stage regression separately by crime types and by defendant characteristics in Appendix Tables G13 and G14 to understand whether some

Figure 3: Impact of Prison on Defendant Reoffending



Note: This figure plots IV estimates of the impact of incarceration on whether a defendant appears in a court case in the three years before and five years after sentencing. Estimates are obtained by estimating equation (1), with the prison indictor instrumented by the judge stringency IV. The estimate for each year is obtained by a separate IV regression. The outcome in years -3 to -1 measures whether the defendant is charged in that given year, and in years 1-5, the outcome is cumulative, measuring whether the defendant is charged within that many years since sentencing. Standard errors are clustered two-way at the judge and defendant level. 95% confidence intervals are shown. The sample is constructed as defined in Sections 2 and 4.

subgroups are over-represented among the compliers.[16] 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 5, which suggests that prison increases 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, given that defendants sentenced to prison are likely to have higher recidivism risk on average. This difference likely arises due to two factors: first, those sent to prison

[16]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.

will, on average, have committed more severe crimes and have more extensive criminal histories, especially since first-time offenders are rarely incarcerated in Finland; second, even among offenders similar along these two dimensions, judges who wish to prioritize public safety will likely choose who to incarcerate based on other characteristics that correlate to recidivism risk, not all of which we observe in our data. The IV can correct this bias by exploiting the random assignment of cases to judges with different levels of sentencing leniency. Alternatively, OLS and IV estimates may both identify causal effects of prison, but for different populations whose responses to prison sentences differ. We can 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 5 under "OLS: Reweighted". We find that OLS estimates after controlling for observable characteristics and reweighted OLS estimates are very similar, suggesting that OLS and IV estimates differ because of selection bias in the OLS estimates.

Our main estimate captures 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"). 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 prison rates 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.

Why does prison reduce future criminal activity for financial crime defendants? First, prison could play an incapacitation role. In other words, those sent to prison cannot commit new offenses while incarcerated. We can largely rule out this explanation since the average prison sentence for financial crime defendants is only 77 days, and 85% of all defendants receive a sentence length of a year or less. That said, sentence length for the marginal defendants sent to prison is likely more relevant. We find that the marginal defendant is sentenced to 253 days and is expected to serve 142 days in prison (Appendix Table G1). Given our reoffending results only become significant 2 years after sentencing, incapacitation seems unlikely to explain our results.[17]

Table 5: Disaggregate Impact of Prison on Reoffending 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	0.248	0.338	0.391
Outcome Mean if Prison	0.592	0.728	0.788
Complier Mean	0.603	1.004	1.021
Court $\times$Year FE	Y	Y	Y
Observations	56,582	56,582	56,582

Notes: This table reports OLS and IV estimates of the impact of prison on the probability of appearing in court for a new crime within the specified time after sentencing. Estimates are obtained by estimating equation (1). In IV estimates the prison indicator is instrumented by the judge stringency IV. All estimates include controls for court-by-year fixed effects. IV estimates include the additional controls included in the OLS: Controls and OLS: Reweighted results. Standard errors clustered two-way at the judge and defendant level appear in parentheses. *p<0.1, **p<0.05, ***p<0.01

Second, prison may build criminal capital, with incarcerated defendants learning to avoid detection in the future. If this is true, we are finding a decline in detection and not offenses committed. Alternatively, building criminal capital behind bars can make offenders more prolific. If time in prison causes defendants to commit more crimes, then reoffending would likely increase post-sentencing. The broader crime literature generally supports the latter hypothesis, namely that building criminal capital behind bars leads to increased recidivism (Bayer *et al.*, 2009; Damm and Gorinas, 2020). This is inconsistent with our estimates of a reduction in reoffending after a prison sentence, so we view this explanation as unlikely.

[17]Moreover, when looking at the data, it appears that those who have committed many more previous crimes and those who commit much more severe crimes are much more likely to receive much longer sentences. These individuals would generally be "always-takers" and thus would not drive our main estimates.

Third, prison could play a rehabilitative role. One way rehabilitation may manifest is in improved labor market outcomes after sentencing. In Appendix Figure F4, 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 them are significant. Earnings estimates are similarly noisy.[18] The overall takeaway is summarized in Appendix Table G16, 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 OLS estimates suggesting a 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 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 is likely due to some combination of specific deterrence and rehabilitation.[19]

[18]Detailed OLS and IV estimates are found in Appendix Table G15.

[19]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 the 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, their original prison sentence is activated, and they can also 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 mechanically increasing future prison time.

Heterogeneity by Income and Financial Crime Type There are two reasons to be interested in the heterogeneity of the impacts of a prison sentence by income. First, the descriptive results in Table 2 suggest that while financial crime defendants are positively selected on income, employment, and education compared with other non-violent defendants, they are still negatively selected relative to the general population, with the majority committing fraud (Table 1). Second, Bhuller *et al.* (2018) shows that there is important heterogeneity in the impact of punishments based on economic background when pooling all crimes together.

Appendix Table G17 reports results separately for those who are above- versus below-median income (first-stage estimates can be found in Appendix Table G18). While we find strong negative effects for both groups, the effects are more precisely estimated for the below-median income group. Turning to heterogeneity by type of financial crime, Appendix Table G22 reports estimates for sub-categories of financial crimes, while Appendix Table G23 reports the first stage. The results indicate that prison is particularly effective at reducing the likelihood of future offending for individuals who have committed fraud.

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 that can arise when judges make multiple punishment decisions simultaneously. In our context, judges make three relevant decisions for defendants: whether they are guilty, whether to impose a lesser punishment than prison (fine or probation), and whether to sentence them to prison. If judges more likely to assign prison are also more likely to find defendants guilty, our estimates may capture the bundled effect of these decisions rather than just the impact of sending a defendant to 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 the stringency of judges along other decision margins in the first and second stage equations. These stringency measures are constructed identically to our main judge stringency instrument but use the judge's tendency to find a defendant guilty or impose a fine or probation, as opposed to giving a prison sentence. Formally, the two alternative stringency measures we compute are the residualized leave-out mean of guilty verdicts and the residualized leave-out mean of imposing a fine or probation for each judge. We then augment the main first and second-stage equations in the following way:

$$Y_{ict} = \beta_0 + \beta_1 P_{ict} + \beta_2 \boldsymbol{X}_{ict} + \beta_3 Z^D_{j(i)ct} + \varepsilon_{ict}. \tag{3}$$

$$P_{ict} = \alpha_0 + \alpha_1 Z_{j(i)ct} + \alpha_2 Z^D_{j(i)ct} + \alpha_3 \boldsymbol{X}_{ict} + \epsilon_{ict}. \tag{4}$$

where all variables are as previously defined in Section 4, but we additionally control for the stringency measures along different decision margins, denoted by Z^D_{icjt}. The intuition for this approach is to control for the possibility that judges more likely to assign a prison sentence may also be more likely to find a defendant guilty or impose a fine or probation, thereby removing the potential exclusion restriction violation.

Alternatively, we can also instrument the other decision margins using the alternative stringency measures. This involves estimating the following equations:

$$Y_{ict} = \beta_0 + \beta_1 P_{ict} + \beta_2 D_{ict} + \beta_3 \boldsymbol{X}_{ict} + \varepsilon_{ict}. \tag{5}$$

$$P_{ict} = \alpha_0 + \alpha_1 Z_{j(i)ct} + \alpha_2 Z^D_{j(i)ct} + \alpha_3 \boldsymbol{X}_{ict} + \epsilon_{ict} \tag{6}$$

$$D_{ict} = \alpha_0 + \alpha_1 Z_{j(i)ct} + \alpha_2 Z^D_{j(i)ct} + \alpha_3 \boldsymbol{X}_{ict} + \epsilon_{ict}. \tag{7}$$

where we instrument both prison and an alternative decision margin (D_{ict}) with the relevant leave-out-mean stringency variable in equations 6 and 7, then estimate the second stage equation 5 using both the instrumented prison variable and alternative decision margin variable.

Humphries *et al.* (2024) show that when covariates are present, controlling for judge conviction stringency is sufficient to identify point estimates along the incarceration versus non-incarceration punishment margin when judges make decisions over guilt and incarceration sequentially, matching the sequential nature of the Finnish court system, as described in the Appendix.[20] Humphries *et al.* (2024) also show that issues can arise with the identification of the incarceration margin if these choices are made jointly. This could be an issue with plea bargaining, where judges exert significant influence over and often explicitly bundle the conviction and incarceration decisions. This is likely a non-issue in Finland, where little plea bargaining occurs, and when it does, its features are very different than the United States. Given this and the presence of covariates in our IV specification, if there are issues with multidimensional sentencing, controlling for judge stringency on the conviction margin should be sufficient to identify effects on the incarceration margin. Regardless, we show that our results are robust to both controlling for other decision margins and instrumenting for these margins.

Our results are largely unchanged when we control for alternative stringency measures (Appendix Table G24). When we control for guilty stringency in Columns 3 and 4, we find little change in the first stage and slightly larger (but statistically indistinguishable) negative effects on recidivism. The first stage is slightly smaller when controlling for probation or fine stringency in Columns 5 and 6, but the IV estimate is nearly identical.

Appendix Table G25 shows results when we instrument for alternative decision margins. In Panel B, IV estimates of the effect of prison on 3-year recidivism when including the instrumented guilty verdicts in Column 2 and instrumented fine or probation in Column 3 are both statistically indistinguishable from the baseline specification. We further find that guilty verdicts appear to increase recidivism by 6.2 percentage points, and probation and fines increased recidivism by 5.8 percentage points, though neither is statistically significant. This suggests that being found guilty and being assigned probation or a fine both impact recidivism in the opposite direction than being assigned a prison sentence.[21]

It could also be the case that stricter judges act in other ways we can't observe, which 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 impossible to rule out these effects as we do not observe these

[20] Kamat *et al.* (2024) provide an alternative approach that bounds the effect in the sequential model when covariates aren't present.

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

behaviors. That said, it is unlikely that these less tangible judge behaviors are as important for reoffending as a prison sentence.

6 Impact of Sending a Defendant to Prison on Their Colleagues

Next, we examine if sending a financial crime defendant 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 offense was committed. We select colleagues from the year the offense was committed because many financial crime defendants separate from their firms between their offense and conviction (see Appendix C). 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.[22]

To estimate the impact of imprisoning a financial crime defendant on their colleagues, we use a similar 2SLS strategy as described in Section 4. The dependent variable is an indicator equal to 1 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 working with a financial crime defendant sent to prison.

Table 6 reports the impact of a defendant being quasi-randomly incarcerated on whether their work colleagues commit financial crimes. Columns 1 and 2 in each panel report first-stage estimates, and the last three columns report OLS and IV estimates for 1 to 3 year offending rates for colleagues. The first-stage estimate in Column 1 restricts the sample to a single observation per defendant. Column 2 contains repeat defendant observations for each coworker. We view the first-stage estimates in the first column as more reflective of the instrument's variation.

Panel A considers coworkers of all financial crimes defendants. OLS estimates suggest a positive correlation between defendants sent to prison and their colleagues' criminality. In contrast, the IV estimates show a consistent negative effect. These results suggest that 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 overall impact, while nega-

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

Table 6: Spillover Impact of Prison on Coworkers of Financial Criminals

	First Stage		Colleague Financial Crime Within:		
	Defendants (1)	All Obs (2)	1 Year (3)	1-2 Years (4)	1-3 Years (5)
Panel A: All Financial Crimes					
First Stage	0.396***	0.483***			
	(0.119)	(0.113)			
OLS with Controls			0.015**	0.017**	0.017**
			(0.007)	(0.007)	(0.007)
IV Estimate			-0.047	-0.054	-0.057
			(0.083)	(0.099)	(0.104)
Observations	10,164	133,946	100,253	100,253	100,253
Panel B: Fraud					
First Stage	0.402***	0.506***			
	(0.130)	(0.133)			
OLS with Controls			0.006	0.006	0.005
			(0.006)	(0.007)	(0.007)
IV Estimate			-0.190**	-0.228**	-0.272**
			(0.085)	(0.106)	(0.120)
Observations	5,862	74,607	55,359	55,359	55,359
Panel C: Business Offences					
First Stage	0.401	0.499*			
	(0.331)	(0.293)			
OLS with Controls	0.401	0.499*	0.049***	0.059***	0.067***
			(0.018)	(0.018)	(0.018)
IV Estimate			-0.049	-0.027	0.077
			(0.234)	(0.243)	(0.240)
Observations	2,024	27,953	21,238	21,238	21,238
Panel D: Other Financial Crimes					
First Stage	0.354*	0.565***			
	(0.199)	(0.207)			
OLS with Controls			0.011	0.005	0.005
			(0.013)	(0.012)	(0.012)
IV Estimate			0.056	0.066	-0.001
			(0.122)	(0.145)	(0.159)
Observations	2,051	31,364	23,619	23,619	23,619

Notes: This table reports estimates of the impact of sending financial-crime defendants to prison on the offending of their work colleagues. The sample is restricted to defendants who are employed in 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 sample of colleagues. Columns 3-5 report OLS and IV estimates of the impact of sentencing defendants to prison on the offending of their work colleagues within 1 to 3 years after sentencing. Panel A reports results for the colleagues of all financial crime defendants, and panels B through D report results restricting to the colleagues of fraud, business offense, and other financial crime defendants respectively. Standard errors clustered two-way at the judge and defendant level appear in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

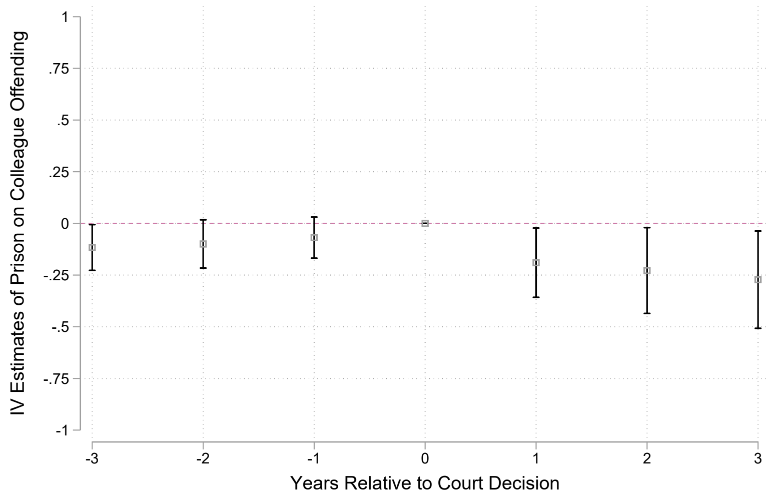
tive, is small and not statistically significant.

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 court data. The IV estimates remain consistently negative but are larger in absolute magnitude and statistically significant at the 5 percent level. The estimate in Column 3 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. We also report results in Figure 4, which summarizes the IV effect over time and reassuringly shows no evidence of pre-trends. When we collapse the colleague outcomes to the average number of colleagues re-offending, which reweights the estimates to give equal weight to each defendant, we find similarly negative spillover effects (Appendix Table G30). In Panels C and D, we turn to the colleagues of defendants who committed business offenses and other financial crimes. In both cases, the IV estimates are much smaller than in the fraud cause and not statistically significant. We conclude that sending fraud defendants to prison has a broader general deterrence effect beyond the impact on the defendant's own likelihood of reoffending.

There are several possible explanations for these spillovers on colleagues. First, defendants and colleagues could be co-conspirators, such that sending a defendant to prison reduces the opportunity for their colleagues to commit crimes with them in the future. We find that 17% of all financial crime cases include at least one other co-conspirator. However, we find that only 0.9% of the colleagues we examine were co-conspirators with the defendant they worked with, which is the relevant margin for our IV estimates. Thus, while we cannot fully rule out this possible mechanism, it seems unlikely it would drive our results.

More likely are a series of other possible explanations. 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, so we can't test this possibility. A related explanation is that firms might change their operating practices in response to having an employee sent to prison in ways that deter others in the firm from committing crimes. Last, observing a colleague sent to prison may have a deterrent effect, with colleagues revising their beliefs about the likelihood they might be sent to prison for committing a financial crime. This would increase the expected cost of committing an offense and lead those on the margin to reduce offending.

Figure 4: Impact of Prison on Colleague Reoffending



Note: This figure plots IV estimates of the impact of incarcerating fraud defendants on the criminal offending of their work colleagues. Criminal offending is defined as appearing in a criminal court case. Estimates are obtained by estimating equation (1), where the outcome is a colleague offending indicator, and the dependent variable of interest is a defendant incarceration indicator instrumented by the judge stringency IV. The outcome in years -3 to -1 measures whether a colleague offends in that given year, and in years 1-5, the outcome is cumulative, measuring whether a colleague is charged within that many years since the defendant was sentenced. Standard errors are clustered two-way at the judge and defendant level and 95% confidence intervals are shown for each estimate. Sample construction as defined in Sections 2 and 4.

7 Impacts of Financial Crime on Victims

When considering when and why prison sentences might be justified for financial crimes, it is informative to understand what impacts these crimes might have on victims. Anecdotal accounts, such as those summarized in (Lopez, 2024), indicate that fraud can upend victims' lives and lead to significant mental distress, with potentially important economic consequences for victims. However, rigorously estimating economic impacts on victims beyond these anecdotes is historically challenging due to data constraints: a researcher must be able to identify the victims in the data and link them to relevant outcomes. We leverage police administrative data (Statistics Finland, 2025e) containing all arrests in Finland between 2006 to 2018 to overcome these challenges. This data contains unique victim identifiers, which allow us to identify victims of financial crimes and then link them to their labor market information in the Statistics Finland FLEED modules.

To understand the impact of experiencing a financial crime on the victim's labor market outcomes, we estimate a matched difference-in-differences design. This approach allows us to carefully compare the outcomes of individuals who are observationally identical before victimization, but where one experiences a financial crime, and the other does not, similar to the approach from Adams-Prassl, Huttunen, Nix and Zhang (2024). Formally, we construct a matched control group for financial crime victims using a two-step matching procedure. First, we select all individuals between the ages of 20 and 60 from the Finnish register data and find the pool of exact matches for each victim based on their broad education level, employment status, income grouping, age grouping, and gender before the crime occurs. Second, we use propensity score matching within these groups of exact matches to identify the best control for each victim. Specifically, we estimate a propensity score using age, number of children, yearly earnings, average employment, marital status, municipality, precise education level, and industry in the 1-3 years preceding the victimization shock. We then select a single comparison individual without replacement with the closest propensity score for each victim.

With matched controls and victims identified, we estimate the following regression model:

$$Y_{ibt} = \sum_{j=-5, j\neq -1}^{5} \delta_j D_{ib,t-j} + \alpha_{ib} + \gamma_t + \omega_j + \epsilon_{ibt}. \tag{8}$$

Y_{ibt} denotes victim i's earning or employment in base-year sample b at year t. b is the victimization year. $D_{ib,t-j}$ is an indicator variable for the treatment (experiencing a financial crime) separately for each year j since the event. δ_j are the coefficients of interest, identifying the effects of being a victim of a financial crime relative to the matched control. We omit the year before the event ($j = -1$), so all estimates of δ_j are relative to the year before the incident. We include individual-incident-year fixed effects, α_{ib}, year fixed effects, γ_t, and time since crime fixed effects, ω_j.[23]

[23]We omit covariates (except individual fixed effects), since our two-step matching procedure selects an observationally similar control observation for each victim and then we include this control observation in the regression. We avoid staggered treatment concerns as controls are never victimized (Callaway and Sant'Anna, 2021).

Figure 5 reports raw means along with estimates of equation 8. Focusing first on employment in Panels (a) and (c), we find significant negative impacts in both the raw means and the matched difference-in-difference estimates. However, these negative impacts are very small compared to violent crimes (Bindler and Ketel, 2022; Adams-Prassl, Huttunen, Nix and Zhang, 2024). This is unsurprising since it is unlikely that being a victim of fraud, for example, would cause as many individuals to drop out of the labor force as violent crime victimization. Turning to earnings in Panels (b) and (d), we again find a clear drop in earnings for the victims relative to their matched controls (point estimates reported in Appendix Table G31). Panel (d) estimates that victims lose 1,141 Euros the year after the crime occurred relative to their matched controls. Compared to the average earnings before the crime occurred, this translates to a 5.3% decline in earnings, a small but meaningful economic impact on victims. To demonstrate our results are robust, Appendix Figure F5 reports estimates where we instead use future victims as the counterfactual observation in a difference-in-differences design. For this exercise, we do not match observables before the event, but we do include age-fixed effects in the regression to account for age-earnings profiles. We observe flat pre-trends and a statistically significant drop in labor market outcomes for victims compared with individuals who are also financial crime victims but at a later date.

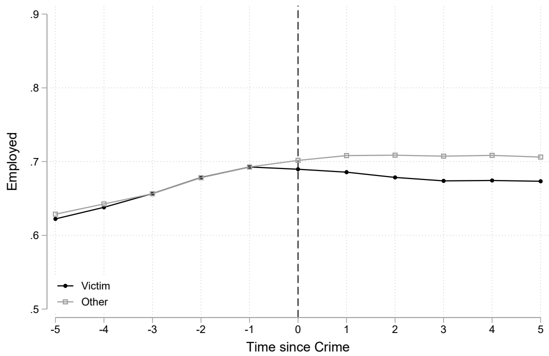
We show that our results are driven by both intensive and extensive margin effects in Appendix Figure F6, which finds negative impacts even for those who remain employed after victimization. We also find significant effects for employment for both wage-earners and the self-employed, but the earnings impact is largely driven by wage earners (Appendix Figure F7).

Last, victims in our sample can be victimized by a financial crime and some other (possibly violent) crime in the same year. If this is common, our main estimates could conflate the financial crime victimization costs with other victimization costs. If the other victimization costs are larger, this could bias our cost estimates upward. To address this concern, we separately estimate impacts for those who only experience financial crime victimization shocks in a given year versus those who experience financial crime victimization and some other crime victimization in the same year. Appendix Figure F8 shows that victimization effects are slightly smaller than our main estimates when we restrict to victims who only experience financial crime victimization. While

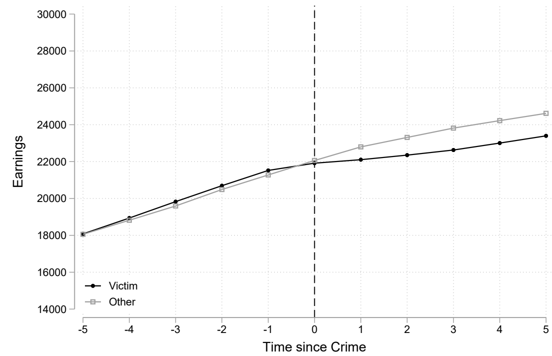
Figure 5: Impact of Financial Crimes on Victim's Employment and Income

Panel I: Raw Means

(a) Employment

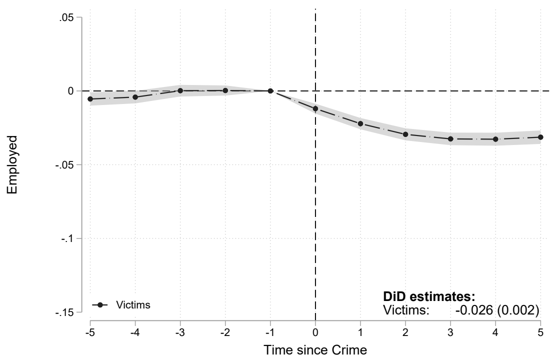


(b) Earnings

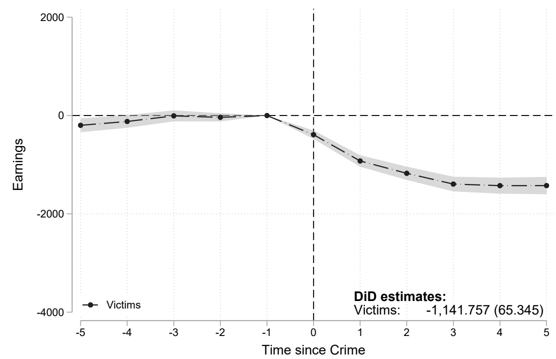


Panel II: Event Study Estimates

(c) Employment



(d) Earnings



Notes: Panel I reports raw means of employment and earnings around the time of victimization for both the victim and their matched control. Panel II reports event-study estimates of victimization impacts obtained using equation (8) with the matched control. Event time 0 runs from 5 years before to 5 years after the year of the victimization event (year 0). Employment indicates whether the individual was employed in the last week of the year. Earnings consist of total taxable labor market income (the sum of salary, self-employment earnings, and taxable benefits). Standard errors are clustered at the individual level.

multiple victimization shocks do lead to much larger negative effects on earnings and employment, they are less common, with only 24% of financial crime victims experiencing other types of victimization in the same year. Our main estimates combine the two groups to be as hands-off as possible.

These results negate the common misconception that financial crimes are "victimless crimes." We caution that financial crimes reported to police are likely a selected sample of the more serious cases. However, this is precisely the policy-relevant group when considering the societal costs imposed by defendants who end up in court for committing financial crimes.

8 Broader Implications of Our Estimates for Policy

8.1 Financial Crimes Versus Other Nonviolent Crimes

We motivated this paper in part by showing financial crime defendants are treated more leniently than other defendants (see Table 2). One way to justify differences in treatment is on efficiency grounds, i.e., if prison is more effective at reducing future recidivism for other types of nonviolent crimes, this could explain harsher punishments. To explore this possibility, Table 7 reports the impact of sending defendants who commit other types of nonviolent crimes to prison on reoffending. Column 1 repeats the estimates for financial crime defendants for convenience. Columns 2-3 report the results for nonviolent property and drug crimes. We report first-stage estimates for each alternative crime group in Appendix Table G32, and in each case, we observe a strong first stage. Strikingly, we find in Table 7 that the impact is largest and only significantly negative for financial crime defendants. The next largest estimate is less than half the size, a 14 percentage point decline in reoffending for property defendants, but is insignificant. Therefore, it is hard to justify the more lenient treatment of financial crime defendants on efficiency grounds. Table 7 suggests that reoffending could be more effectively reduced with more prison sentences for financial crime defendants compared with other nonviolent drug and property crimes.

Another possible justification is that financial crime defendants are less likely to turn to more severe crimes. To assess this, Table 7 Panel B estimates the impact of a prison sentence on whether the defendant commits a violent crime. Not only does sending financial crime defendants to prison reduce recidivism broadly, but it also reduces violent crimes. This is true for nonviolent property crimes as well, but we find no significant effect for nonviolent drug crimes.

A third possible justification could be that these crimes are less costly to the victims. To assess this, we compare financial crime victimization costs to other crimes that commonly include victims in Appendix Figure D1, using the same approach from Section 7. Unsurprisingly, we find that financial crimes impose smaller labor market costs on victims compared with violent crimes, a fact that may justify harsher punishments for violent crimes. As a point of comparison, McCollister *et al.* (2010) in the criminology literature estimate aggravated assault is 3.9 times

Table 7: Impact of Prison on Reoffending Post Sentencing Across All Crime Types

	Financial (1)	Property (2)	Drug (3)
Panel A: Impact on Reoffending			
OLS: No Controls	0.444***	0.228***	0.149***
	(0.007)	(0.005)	(0.010)
OLS: Controls	0.091***	0.073***	0.007
	(0.007)	(0.005)	(0.009)
OLS: Reweighted	0.075***	0.061***	0.001
	(0.007)	(0.005)	(0.009)
IV	-0.429**	0.024	-0.018
	(0.217)	(0.110)	(0.142)
Outcome Mean	0.391	0.749	0.631
Outcome Mean if Prison	0.788	0.902	0.807
Complier Mean	1.021	0.801	0.763
Court $\times$Year FE	Y	Y	Y
Observations	56582	37199	22443
Panel B: Impact on Violent Reoffending			
OLS: No Controls	0.116***	0.077***	0.034***
	(0.007)	(0.006)	(0.007)
OLS: Controls	0.002	0.006	-0.028***
	(0.007)	(0.007)	(0.007)
OLS: Reweighted	-0.007	-0.000	-0.045***
	(0.008)	(0.007)	(0.009)
IV	-0.328**	-0.208**	-0.059
	(0.131)	(0.101)	(0.108)
Outcome Mean	0.083	0.229	0.143
Court $\times$Year FE	Y	Y	Y
Observations	56582	37199	22443

Notes: This table reports OLS and IV estimates of the impact of prison on the probability of appearing in court for any crime (Panel A) or appearing in court for a violent crime (Panel B) within three years after initial sentencing for financial, property, and drug crime defendants. Estimates are obtained by estimating equation (1), with the prison indictor instrumented by judge stringency in the IV estimates. All estimates include court-by-year fixed effects. IV estimates include the additional controls used in the OLS: Controls and OLS: Reweighted results. Standard errors clustered two-way at the judge and defendant level appear in parentheses. *p<0.1, **p<0.05, ***p<0.01

costlier than financial crimes (see McCollister *et al.* (2010) Table 3), while our estimates indicate aggravated assault is 2.3 times costlier than financial crimes (see Appendix Figures D1 and F9). Comparisons would be different if we instead used Table 1 from McCollister *et al.* (2010), which includes various other possible costs, such as jury awards to reflect pain and suffering. Our estimates include economic costs for financial crime victims, while McCollister *et al.* (2010) omit such costs for financial crimes, claiming in the table notes that financial crimes "are generally considered 'victimless' crimes." Our estimates demonstrate that it is incorrect to assume financial crimes are victimless. We find similar victimization costs for victims of non-violent property crimes, even though property crimes are punished much more harshly than financial crimes.

Last, financial crime defendants differ on several dimensions, as shown in Table 2. For example, they are more likely to have children. Perhaps some judges attempt to balance a more complex set of objectives other than minimizing recidivism and providing justice for victims when sentencing criminal defendants and treat defendants with children more leniently. This could raise concerns about the monotonicity assumption required for identification with our judge leniency design. We explore this possibility through a reverse-sample monotonicity check and find no evidence this assumption is violated for defendants with children (Appendix Table G33). However, the overall difference in the characteristics of financial crime defendants may have some effect on which defendants are on the margin of incarceration. The size of our estimate may be affected by the fact that financial crime defendants are more likely to have children, as well as other factors such as having higher employment and income, which all could tend to strengthen the specific deterrent effect of prison as they can provide extra incentives to not return to prison. This possibility is consistent with the overall takeaway that the use of incarceration for financial crime defendants appears to be an effective way to lower recidivism.

8.2 Cost-Benefit Calculations

Our cost-benefit calculation compares the direct costs of incarcerating a marginal financial crime defendant to the benefits of reduced reoffending. The latter consists of two components: the reduction in future court and policing costs and the reduction in future victim costs. In a method similar to Bhuller *et al.* (2018), we create an outcome variable for each component in our estimation sample. We then use these outcomes in our main IV specification to estimate these costs for the marginal financial crime defendant. For full details, see Appendix Section D. To be consistent with Bhuller *et al.* (2020) this exercise uses the number of charges to calculate the reduction in future court and policing costs. Appendix Table G11 reports reductions in future charges.

Table 8 presents our estimates. Column 1 reports the direct incarceration cost is €46,918. Column 2 shows this is matched by a €46,416 reduction in future court and policing costs, and column 3 finds a reduction in victim costs of €5287. These estimates suggest that the direct prison cost of incarcerating the marginal financial crime defendant is effectively offset by reductions in future policing and court costs. Adding in the reduction in costs to victims tips the scales towards incarceration being a cost-effective method to reduce future offending for financial crime defendants, even before accounting for the impacts on fraud defendants' colleagues. While our estimates improve on some of the existing literature by explicitly incorporating economic costs to victims, we still fail to capture the full range of impacts that researchers and society might consider when calculating the social costs of crime and incarceration. For example, our analysis does not capture the mental anguish costs to victims, nor does our analysis capture the potential costs of incarceration to families, which may be particularly salient for financial crime defendants who are more likely to have children on average.

Table 8: Cost-Benefit Calculation

	Direct Cost of Prison	Indirect Future Costs	Victimization Costs
	(1)	(2)	(3)
	46918	-46416	-5282
	(8038)	(23029)	(2683)
Observations	56499	56499	56499

Notes: Table depicts estimated costs and benefits of incarcerating financial-crime defendants on the margin of incarceration. Column 1 presents the direct prison cost of incarcerating a defendant. Column 3 presents the impact on future criminal justice system-related costs, including policing, court, and incarceration costs. Policing costs are calculated as the total police budget divided by police incidents, and court costs are calculated by dividing the total court budget by the number of criminal cases. Column 3 presents reductions in victim costs, estimated using the earnings costs to victims of financial crimes (see Section 7). Estimates are obtained by estimating equation (1), with the associated cost per-defendant as the outcome and the prison indictor instrumented by the judge stringency IV. Standard errors clustered two-way at the judge and defendant level appear in parentheses. *p<0.1, **p<0.05, ***p<0.01

9 Conclusion

In this paper, we show that despite the growing importance of financial crimes, these defendants are less likely to be sent to prison than defendants who commit other nonviolent crimes. We also find that these defendants look very different than other types of defendants but still have high rates of recidivism. 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 that defendants reoffend and that their colleagues commit financial crimes.

Using random assignment to judges to identify the causal impacts of prison, we find that financial crime defendants are 42.9 percentage points less likely to reoffend after a prison sentence. We also document important spillovers on colleagues, with a prison sentence also reducing the probability that a colleague commits a crime in the future. Together, these results suggest scope for policymakers to potentially use prison as one possible tool to reduce recidivism among financial crime defendants and reduce financial crimes through a broader deterrence effect. Last, we show these crimes result in small direct costs to victims.

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. Still, we show in Appendix E that the tendency for financial crimes to have a lower incarceration rate than other crimes and some of the other descriptive patterns for financial crimes also hold in other settings.

We close with two main takeaways. First, financial crime defendants are an important crime group, are less likely to receive a prison sentence compared with other nonviolent crimes, have high rates of recidivism across countries, and impose small but significant negative costs on victims. As such, evidence to better understand how the criminal justice system might reduce financial crimes is important. This paper provides such evidence, filling a hole in the current literature.

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, such as negative health effects for defendants (Hjalmarsson and Lindquist, 2022) and spillovers on families (Billings, 2018; Norris *et al.*, 2021). 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 to arrive at an equitable resolution to these crimes.

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