

# Incarceration, Recidivism, and Employment

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Using a random judge design and panel data from Norway, we estimate that imprisonment discourages further criminal behavior, with reoffense probabilities falling by 29 percentage points and criminal charges dropping by 11 over a 5-year period. Ordinary least squares mistakenly reaches the opposite conclusion. The decline is driven by individuals not working prior to incarceration; these individuals

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increase participation in employment programs and raise their future employment and earnings. Previously employed individuals experience lasting negative employment effects. These findings demonstrate that time spent in prison with a focus on rehabilitation can be preventive for a large segment of the criminal population.

## I. Introduction

Over the past several decades, incarceration rates have risen dramatically in many developed countries. In the United States, for example, the incarceration rate has increased from 220 per 100,000 residents in 1980 to more than 700 per 100,000 in 2012. In Europe, the increases (and levels) tend to be smaller but still substantial, with the average incarceration rate per 100,000 residents rising from 62 in 1980 to 112 in 2010 in Western European nations.<sup>1</sup> These increases raise important questions about how well ex-convicts reintegrate into society after incarceration and, in particular, whether they return to a life of crime. Prison time could convince offenders that crime does not pay or rehabilitate them by providing vocational and life skills training. Conversely, prison time could cause human capital to depreciate, expose offenders to hardened criminals, or limit opportunities due to employment discrimination or societal stigma. Indeed, the effects of incarceration could vary in magnitude and sign, depending on a prisoner's background (e.g., work history) as well as prison conditions (e.g., availability of prison programs and sentence lengths).

Understanding whether and in what situations time spent in prison is criminogenic or preventive has proven challenging for several reasons. One problem is data availability. The ideal data set would be a long and representative panel with individual-level information on criminal behavior and labor market outcomes. In many countries, however, the required data sources cannot be accessed and linked together. Another major challenge is the threat to identification from correlated unobservables. While ex-convicts have relatively high rates of criminal activity and weak labor market attachment, these correlations could be driven by their unobserved characteristics as opposed to the experience of being in prison.

Because of these challenges, evidence on the causal effects of incarceration is scarce. Nagin, Cullen, and Jonson (2009, 115), in their review article, summarize the state of the literature well: "Remarkably little is known about the effects of imprisonment on reoffending. The existing research is limited in size, in quality, [and] in its insights into why a prison

<sup>1</sup> These figures come from the World Prison Brief (Walmsley 2016). The Western European countries used to construct the population-weighted average include Austria, Belgium, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom.

term might be criminogenic or preventative.” Our paper overcomes both the data and the identification challenges in the context of Norway’s criminal justice system, offering new insights into how imprisonment affects subsequent criminal behavior.

Our work draws on two strengths of the Norwegian environment. First, by linking several administrative data sources, we are able to construct a panel data set containing complete records of the criminal behavior and labor market outcomes of every Norwegian. Second, we address threats to identification by exploiting the random assignment of criminal cases to Norwegian judges who differ systematically in their stringency. In our baseline specification, we measure judge stringency as the average incarceration rate in other cases a judge has handled. This serves as an instrument for incarceration since it is highly predictive of the judge’s decision in the current case but, as we document, is uncorrelated with observable case characteristics.

Our paper offers three sets of results. First, imprisonment discourages further criminal behavior. Using our measure of judge stringency as an instrument, we estimate that incarceration lowers the probability of reoffending within 5 years by 29 percentage points and reduces the corresponding number of criminal charges per individual by 11. These reductions are not simply due to an incapacitation effect. We find sizable decreases in reoffending probabilities and cumulative charged crimes even after defendants are released from prison.

Second, bias due to selection on unobservables, if ignored, leads to the erroneous conclusion that time spent in prison is criminogenic. Consistent with existing descriptive work, our ordinary least squares (OLS) estimates show positive associations between incarceration and subsequent criminal behavior. This is true even when we control for a rich set of demographic and crime category controls. Using the panel structure of our data reduces the estimates somewhat, but there are noticeable changes in crime and employment in the year prior to the court case, raising concerns about the validity of offender fixed effects or lagged dependent variable models. In contrast, our instrumental variables (IV) estimates, which address the issues of selection bias and reverse causality, find that incarceration is strongly preventive for many individuals on both the extensive and the intensive margins of crime.

Third, the reduction in crime is driven by individuals who were not working prior to incarceration. Among these individuals, imprisonment increases participation in programs directed at improving employability and reducing recidivism, and it ultimately raises employment and earnings while discouraging criminal behavior.<sup>2</sup> The effects of incarceration

<sup>2</sup> Since we observe charges and not actual crimes committed, it is in theory possible that ex-convicts do not, in fact, reduce their criminal activity but rather learn how to avoid being

for this group are large and economically important. Imprisonment causes a 35 percentage point increase in participation in job training programs for the previously nonemployed, and within 5 years, their employment rate increases by 36 percentage points. At the same time, the likelihood of reoffending within 5 years is cut in half (by 43 percentage points), and the average number of criminal charges falls by 18. A very different pattern emerges for individuals who were previously attached to the labor market. Among this group, which comprises roughly half of our sample, there is no significant effect of incarceration on either the probability of reoffending or the number of charged crimes. Moreover, they experience an immediate 30 percentage point drop in employment due to incarceration, and this effect continues out to 5 years. This drop is driven almost entirely by defendants losing their job with their previous employer while they are in prison. These heterogeneous effects based on prior employment status are important to keep in mind when interpreting our results.

Taken together, our findings have important implications for ongoing policy debates over the growth in incarceration rates and the nature of prison. A natural question is whether the positive effects from imprisonment found in Norway pass a cost-benefit test. While it is difficult to quantify both costs and benefits, rough calculations presented at the end of the paper suggest that the high rehabilitation expenditures in Norway are more than offset by the corresponding benefits to society.

Our estimates indicate that the high rates of recidivism among ex-convicts is due to selection and not a consequence of the experience of being in prison. Indeed, the Norwegian prison system is successful in discouraging crime and encouraging employment largely because of changes in the behavior of individuals who were not working prior to incarceration. These individuals had no job to lose and low levels of education and work experience. Norwegian prisons offer them access to rehabilitation programs, job training, and reentry support. Upon release, these previously unemployed individuals become more attached to the formal labor market and find crime relatively less attractive. In contrast, for individuals with some attachment to the labor market, many of them had an actual job to lose and human capital to depreciate by going to prison. These negative effects may well offset any positive impacts of rehabilitation and therefore help explain why incarceration does not seem to materially affect their criminal behavior or labor market outcomes.

Our paper contributes to a large literature across the social sciences on how incarceration affects both recidivism and future employment. Much of this literature focuses on incapacitation effects, finding reductions in crime

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caught while in prison. The fact that incarceration increases formal sector employment, which is a time substitute for criminal activity, suggests that this explanation is unlikely.

while offenders are in prison.<sup>3</sup> There is less evidence on longer-term recidivism, and the findings are mixed. In terms of labor market outcomes, OLS studies usually find either negative or no effect on earnings and employment.<sup>4</sup> More sophisticated work uses panel data and offender fixed effects to minimize selection issues. For recidivism, there are fewer studies using this approach and the evidence is mixed, while for labor market outcomes, a handful of studies find either no impact or a negative effect.<sup>5</sup>

More closely related to our paper, some recent work has relied on the quasi-random assignment of judges to study the effects of incarceration.<sup>6</sup> While each of these studies uses data from the United States, the findings are mixed. Kling (2006) presents results suggesting that time in prison improves labor market outcomes after release, although the IV estimates based on quasi-random assignment of judges are too imprecise to draw firm conclusions. Green and Winik (2010) and Loeffler (2013) report no detectable effects of incarceration on recidivism, whereas Aizer and Doyle (2015) find that juvenile incarceration results in lower high school completion rates and higher adult incarceration rates. Mueller-Smith (2015) uses data from Texas to investigate the impacts of adult incarceration and reports that incarceration increases recidivism rates and worsens labor market outcomes.

There are several possible reasons why no consensus has emerged as to how well ex-convicts reintegrate into society. While quasi-random assignment of judges can be useful to address concerns over correlated unobservables, there remain issues that could bias the estimates. In Green and Winik (2010), for instance, the estimation sample is small and the instrument is weak, which may lead to severe bias in the IV estimates. Mueller-Smith (2015) additionally explores the importance of two other issues.

<sup>3</sup> Recent studies in economics isolating incapacitation effects include those by Owens (2009), Buonanno and Raphael (2013), and Barbarino and Mastrobuoni (2014). We refer to Chalfin and McCrary (2017) for a recent review of the extensive literature on criminal deterrence.

<sup>4</sup> For example, Brennan and Mednick (1994), Gottfredson (1999), and Bernburg, Krohn, and Rivera (2006) all reach different conclusions for recidivism. For a summary of observational research on labor market outcomes, see Western, Kling, and Weiman (2001).

<sup>5</sup> See Freeman (1992) and Western and Beckett (1999) for early papers using panel data. Other evidence based on fixed effects or event study design include Waldfogel (1994), Grogger (1995), Kling (1999), and Skardhamar and Telle (2012).

<sup>6</sup> Similar designs in related contexts include studies by Dobbie, Goldin, and Yang (2018) and Stevenson (2018), who use the detention tendencies of quasi-randomly assigned bail judges to estimate the causal effects of pretrial detention, and Di Tella and Schargrodsky (2013), who investigate the use of electronic monitoring as an alternative to prison. For studies using quasi-random assignment of examiners or judges in contexts other than crime, see, e.g., Doyle (2007, 2008), Belloni et al. (2012), Doyle et al. (2012), Maestas, Mullen, and Strand (2013), Dahl, Kostøl, and Mogstad (2014), French and Song (2014), Dobbie and Song (2015), and Autor et al. (2019).

He argues in his setting that standard IV estimates could be biased because of violation of the exclusion and monotonicity assumptions. To assess the relevance and validity of our instrument, we therefore perform a number of checks, all of which suggest that our instrument is strong, is as good as randomly assigned, and satisfies exclusion and monotonicity.

Another possible explanation for the lack of consensus is that incarceration effects could vary depending on a prisoner's background or prison conditions. As documented later, prisoners in Norway have observable characteristics that are broadly similar to prisoners in many other countries. Instead, what is quite distinct, especially compared with the United States, is the prison system. In Scandinavian countries like Norway, the prison system focuses on rehabilitation, preparing inmates for life on the outside.<sup>7</sup> This is done in part by investing in education and training programs but also through extensive use of open prisons, in which prisoners are housed in low-security surroundings and allowed frequent visits to families while electronically monitored.<sup>8</sup> In comparison, in many other countries rehabilitation has taken a back seat in favor of prison policies emphasizing punishment and incapacitation. In the United States, a pivotal point was the 1974 Martinson report, concluding that nothing works in rehabilitating prisoners (Martinson 1974; Lipton, Martinson, and Wilks 1975). While influential, leading criminology scholars have questioned the evidence base for this conclusion (e.g., see the review in Cullen 2005). Our study serves as a proof of concept demonstrating that time spent in prison with a focus on rehabilitation can indeed be preventive.<sup>9</sup>

The remainder of the paper proceeds as follows. The next section provides background on the Norwegian court system, describes how criminal cases are assigned to judges, and outlines the baseline IV model. Section III presents our data. This section also describes similarities and differences in the criminal population and the criminal justice system of Norway versus other countries. In section IV, we discuss our instrument

<sup>7</sup> A recent New York Times article summarizes the system's rehabilitative aims: "The goal of the Norwegian penal system is to get inmates out of it . . . 'Better out than in' is an unofficial motto of the Norwegian Correctional Service . . . It works with other government agencies to secure a home, a job and access to a supportive social network for each inmate before release" (Benko 2015).

<sup>8</sup> Other countries are trying open prisons and finding positive results (Mastrobuoni and Terlizze 2014).

<sup>9</sup> The existing evidence base is scarce and does not answer our research question of whether and in what situations imprisonment as compared with not being incarcerated is preventive or criminogenic. Kuziemko (2013) uses data on inmates in Georgia and finds that access to parole boards increases participation in rehabilitation programs and reduces recidivism. There are also a few randomized controlled trials in the United States focusing primarily on postrelease training and education programs for ex-convicts. These studies have estimated zero or small (and often imprecise because of small samples) effects on long-term labor market and recidivism outcomes (see Visser, Winterfield, and Coggeshall 2005; Redcross et al. 2012; Cook et al. 2015).

and its validity. Section V presents our main results for recidivism, while section VI documents the important role of employment in reducing recidivism. Section VII concludes.

## II. Research Design

In this section, we describe our research design. We begin by reviewing key aspects of the criminal justice system in Norway, documenting how criminal court cases are randomly assigned to judges. We then describe how to use this randomization to estimate the effects of incarceration on subsequent criminal behavior and labor market outcomes.

### A. *The Norwegian Court System*

The court system in Norway consists of three levels: the district court, the court of appeals, and the supreme court. The vast majority of cases are settled at the district court level. In this paper, we focus on criminal cases tried in one of the 87 district courts in existence at one time or another in Norway during the period of our study. The largest district court is located in Oslo and has around 100 judges, while the smallest courts have only a few judges.

There are two types of professional judges in district courts: regular judges and deputy judges. Regular judges are appointed civil servants and can be dismissed only for malfeasance. One of the regular judges is appointed as chief judge to oversee the administration of the local court. In 2010 there were 370 full-time regular judges (including chief judges); their average age was 53, and 62% were male. Deputy judges, like regular judges, are also law school graduates but are appointed to a court for a limited period of time, which cannot exceed 3 years (5 years in Oslo). Deputy judges have a somewhat different caseload compared with regular judges, as discussed in section II.B. Not all deputy judges become regular judges, and those who do typically need several of years of experience in other legal settings before applying for and being appointed as a regular judge.

Criminal cases are classified into two broad types: confession and non-confession cases. Both types are settled by trial (as opposed to the United States, which has plea bargains). In confession cases, the accused has confessed to the police/prosecutor before his case is assigned to a judge. The confession is entered into evidence, but the prosecution is not absolved of the duty to present a full case, and the judge may still decide that the defendant is innocent.<sup>10</sup> In practice, most confession cases are relatively

<sup>10</sup> These rules apply to most civil law systems, in contrast to common law systems, where a majority of criminal cases are settled by confession and plea bargain rather than by a trial.

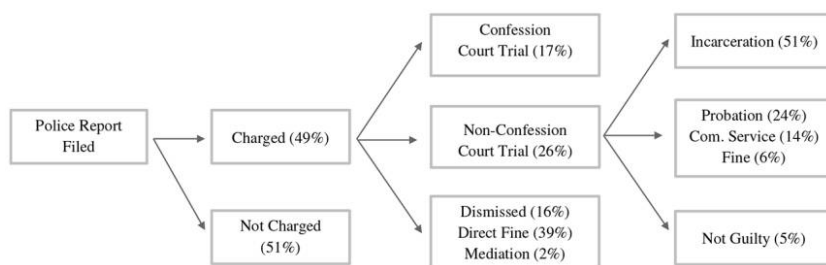


FIG. 1.—Processing of suspected crimes in Norway's criminal justice system. The sample consists of all criminal cases reported to the police in Norway between 2005 and 2009.

straightforward. To save on time and costs, they are therefore heard by a single professional judge who decides on sentencing. Nonconfession cases are heard by a panel of one professional and two lay judges or, in the case of extremely serious crimes, by two professional judges and three lay judges. The lay judges are individuals chosen from the general population to serve for a limited 4-year term. The professional judge presides over the case, while the lay judges participate on the questions of guilt and sentencing. As opposed to professional judges, lay judges hear only a few cases a year.<sup>11</sup>

One advantage of the Norwegian criminal justice system compared with some other countries is that it has no plea bargaining. For example, in the United States, criminal defendants often know their assigned judge before deciding whether to plead guilty in exchange for a reduced sentence. The fact that these pretrial strategies are not taking place in our setting makes the interpretation of our IV estimates easier to interpret (see Dobbie, Goldin, and Yang 2018). Moreover, in Norway, the judge handling the criminal court case is not necessarily the same as the pretrial custody judge, with random reassignment of judges for the court case.<sup>12</sup>

Figure 1 charts how suspected crimes are processed in Norway's criminal justice system. The figure reports percentages for the period 2005–9. If the police suspect an individual of a crime, they file a formal report. A public prosecutor then decides whether the individual should be charged with a crime as well as whether the case should proceed to a court trial. As

<sup>11</sup> Lay judges must satisfy certain requirements, such as not having a criminal record and not working in certain occupations (e.g., police officer). In a municipal district, the pool of lay judges is usually between 30–60 individuals. Lay judges are partially compensated for days absent from work if not covered by their employer. We do not observe the identity of the lay judges in our data, but since they are randomly assigned to judges within a court, they should not create any bias in our estimates.

<sup>12</sup> We verified the random reassignment of judges by comparing the actual probability of receiving the same judge in both the court case and the custody case relative to the counterfactual probability from random assignment. The difference was close to zero and not statistically significant.



reported in the figure, about half of police reports lead to a formal criminal charge. Of these charged cases, the public prosecutor advances 43% of them to a trial. The other charged cases are dismissed, directly assigned a fine, or sent to mediation by the public prosecutor. Around 60% of the cases that proceed to trial are nonconfession cases. Once a case proceeds to trial, it is assigned to a judge. If the judge finds the accused guilty, he or she can assign a combination of possible punishments that are not necessarily mutually exclusive. In the figure, we show percentages based on the strictest penalty received, so that the percentages add up to 100%. Just over half of cases result in incarceration, with probation, community service, and fines combined accounting for 44% of outcomes. In a small fraction of cases (5%), the defendant is found not guilty.

### *B. Assignment of Cases to Judges*

In Norway, the law dictates that cases be assigned to judges according to the principle of randomization (Bohn 2000; NOU 2002). The goal is to treat all cases *ex ante* equally and prevent outsiders from influencing the process of the criminal justice system. In practice, cases are assigned by the chief judge to other judges on a mechanical, rotating basis based on the date a case is received. Each time a new case arrives, it is assigned to the next judge on the list, with judges rotating between criminal and civil cases.<sup>13</sup>

There are some special instances where the assignment of cases does not follow the principle of randomization. These include cases involving juvenile offenders, extremely serious cases that require two professional judges, and complex cases expected to take a longer time to process, all of which can be assigned to more experienced judges. The Norwegian Department of Justice provides guidelines on the types of cases that can be nonrandomly assigned, and the Norwegian Courts Administration has flagged such cases in our data set. While all other cases are randomly assigned, some case types can be assigned to only regular judges, and deputy judges are assigned relatively more confession cases. This means that randomization occurs within judge type but not necessarily across judge types. Therefore, to have a sample of randomly assigned cases to the same pool of judges, we (1) exclude the special cases described above and (2) focus on regular judges handling nonconfession cases.

A key to our design is that not only are judges randomly assigned but also they differ in terms of their propensity to incarcerate defendants. In

<sup>13</sup> Baard Marstrand at the Norwegian Courts Administration verified that district courts are required to randomly assign cases to judges, except in a few instances, which we discuss in the text. We also checked with both the Bergen District Court (the second largest court, behind Oslo) and the Nedre Telemark District Court (a medium-sized court) that they follow the principle of randomization.

our baseline specification, we measure the strictness of a judge on the basis of their incarceration rate for other randomly assigned cases they have handled, including both past and future confession and nonconfession cases and not just those cases that appear in our estimation sample. Our estimation sample has 500 judges, each of whom have presided over an average of 258 randomly assigned court cases. In our baseline specification, our measure of judge stringency is calculated as the leave-out mean judge incarceration rate. When using this measure, we always condition on fully interacted court and year fixed effects to account for the fact that randomization occurs within the pool of available judges. This controls for any differences over time or across judicial districts in the types of criminals or the strictness of judges. In a number of specification checks, we show robustness of the results to how we measure judge strictness (see sec. V.C).

Table 1 verifies that judges in our baseline sample are randomly assigned to cases. The first column regresses incarceration on a variety of variables measured before the court decision. It reveals that demographic, type of crime, and past work and criminal history variables are highly predictive of whether a defendant will be incarcerated, with most being individually significant. In column 3, we examine whether our measure of judge stringency can be predicted by this same set of characteristics. This is the same type of test that would be done to verify random assignment in a randomized controlled trial. There is no statistically significant relationship between the judge stringency variable and the various demographic, crime type, and labor market variables. The estimates are all close to zero, with none of them being statistically significant at the 5% level. The variables are not jointly significant either ( $p = .920$ ). This provides strong evidence that criminal court cases are randomly assigned to judges in our sample, conditional on fully interacted court and year fixed effects.

It is natural to ask why some judges are more likely to incarcerate than others. While we do not observe personal characteristics of judges in our data for privacy reasons, we can measure how many cases they have handled. Using an OLS regression with the same controls as in table 1, we find no relationship between the number of cases handled and judge stringency in our baseline sample. While there may be a variety of other reasons a judge is more or less likely to incarcerate, it is important to keep in mind that as long as judges are randomly assigned, the underlying reasons should not matter for our analysis.

### C. IV Model

We are interested in the causal effects of incarceration on subsequent criminal behavior and labor market outcomes. This can be captured by the regression model

TABLE 1  
TESTING FOR RANDOM ASSIGNMENT OF CRIMINAL CASES TO JUDGES ( $N = 33,548$ )

	DEPENDENT VARIABLE				EXPLANATORY VARIABLE	
	Pr(Incarcerated)		Judge Stringency		Mean	Standard Deviation
	Coefficient Estimate (1)	Standard Error (2)	Coefficient Estimate (3)	Standard Error (4)		
Demographics and type of crime:						
Age	.0036***	.0004	-.0000	.0000	32.65	11.36
Female	-.0520***	.0071	-.0011	.0007	.106	.308
Foreign born	.0035	.0062	.0007	.0007	.135	.342
Married, year $t - 1$	-.0234***	.0117	-.0017	.0012	.111	.314
Number of children, year $t - 1$	-.0011	.0032	.0002	.0004	.783	1.244
High school degree, year $t - 1$	.0109	.0083	.0004	.0009	.172	.377
Some college, year $t - 1$	-.0532***	.0130	-.0013	.0015	.046	.209
Violent crime	.0843***	.0085	.0015	.0011	.256	.437
Property crime	-.0357***	.0109	.0011	.0012	.139	.346
Economic crime	-.0401***	.0116	.0018	.0015	.113	.316
Drug related	-.0484***	.0112	-.0000	.0013	.119	.324
Drunk driving	.0745***	.0128	.0002	.0014	.071	.257
Other traffic	-.0453***	.0127	.0003	.0012	.087	.281
Missing demographic information	-.2971**	.1386	-.0088	.0150	.030	.170
Past work and criminal history:						
Employed, year $t - 1$	.0284***	.0082	.0002	.0008	.352	.478
Ever employed, years $t - 2$ to $t - 5$	-.0016	.0083	.0001	.0009	.470	.499
Charged, year $t - 1$	.0498***	.0074	.0003	.0008	.459	.498
Ever charged, years $t - 2$ to $t - 5$	.0447***	.0078	-.0008	.0010	.627	.483
Incarcerated, year $t - 1$	.1423***	.0105	.0002	.0013	.139	.346
Ever incarcerated, years $t - 2$ to $t - 5$	.1690***	.0095	.0009	.0010	.279	.448
F-statistic for joint test	94.99		.593			
$p$ -value	.000		.920			

NOTE.—Shown is the baseline sample of nonconfession criminal cases processed in 2005–9. All estimations include controls for court  $\times$  court entry year fixed effects. Reported  $F$ -statistic refers to a joint test of the null hypothesis for all variables. The omitted category for education is “Less than high school, year  $t - 1$ ,” and the omitted category for type of crime is “Other crimes.” Standard errors are two-way clustered at the judge and defendant level.

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

$$Y_{i,t} = \beta_i I_{i,0} + X_i' \theta_t + \eta_{i,t}, \quad (1)$$

where  $\beta_i$  is the parameter of interest,  $I_{i,0}$  is an indicator variable equal to 1 if defendant  $i$  is sentenced to prison in period 0 (normalized to be the time of the court decision),  $X_i$  is a vector of control variables, and  $Y_{i,t}$  is the dependent variable of interest measured at some point  $t$  after individual  $i$ 's court decision (e.g., cumulative criminal charges 5 years after the court decision). As demonstrated in table 1, the incarcerated and non-incarcerated groups are far from comparable. This raises concerns of selection bias in OLS estimation of  $\beta_i$ . Our research design addresses this concern by exploiting that cases are randomly assigned to judges (conditional on year and court fixed effects) and that some judges are systematically more lenient than others. Taken together, this leads to random variation in the probability that an individual will be incarcerated depending on which judge they are assigned to. We utilize this exogenous variation in  $I_{i,0}$  to draw inference about the causal effects of incarceration.

Our main analysis is based on two-stage least squares (2SLS) estimation of  $\beta_i$  with equation (1) as the second-stage equation and a first-stage equation specified as

$$I_{i,0} = \gamma Z_{j(i)} + X_i' \delta + \nu_{i,0}, \quad (2)$$

where the scalar variable  $Z_{j(i)}$  denotes the stringency of judge  $j$  assigned to defendant  $i$ 's case. Under the assumptions of instrument exogeneity and monotonicity, the 2SLS estimand can be interpreted as a positive weighted average of the causal effect of incarceration among the subgroup of defendants who could have received a different incarceration decision had their case been assigned to a different judge.

Given the quasi-random assignment of cases to judges, the key challenge to instrument exogeneity is that trial decisions are multidimensional, with the judge deciding on incarceration, fines, community service, probation, and guilt. In section V.E, we examine this threat to the exclusion restriction, showing that our estimates do not change appreciably when we augment our baseline model to either control for judge stringency in other dimensions or include an instrument for other trial sentencing decisions. In the presence of heterogeneous effects, one may also be worried about the monotonicity assumption; that is, defendants who are incarcerated by a lenient judge would also need to be incarcerated by a stricter judge, and vice versa for nonincarceration. In section IV.B, we implement two sets of tests, both of which indicate that monotonicity is likely to hold. On top of these challenges to identification, one may also be worried about exactly how to measure judge stringency  $Z_{j(i)}$  and perform statistical inference. For our main specifications, we measure  $Z_{j(i)}$  as the leave-out mean incarceration rate, which omits case  $i$ , that is, the average incarceration rate in other cases a judge has handled. In section V.C, we

show robustness to alternative measures of  $Z_{j(i)}$ , including a split sample approach. We also make sure the conclusions do not change materially if we exclude judges with relatively few cases or if we use confidence intervals that remain valid whether or not instruments are weak. In appendix D (apps. A–D are available online), we discuss potential challenges to estimation and inference in the random judge setting and perform a series of Monte Carlo simulations to assess the finite sample performance of the 2SLS estimator depending on how one measures  $Z_{j(i)}$ . These simulations lend support to the reliability of the statistical inference we perform when measuring  $Z_{j(i)}$  as the leave-out mean incarceration rate.

In most of our analysis, we perform 2SLS estimation of equations (1) and (2) using the entire sample of all defendants in nonconfession, randomly assigned cases. However, to interpret the results and inform policy, it would be useful to move beyond the resulting average causal effect and estimate the heterogeneous effect of incarceration along a variety of dimensions. One common approach to explore heterogeneity in effects would be to estimate the 2SLS model separately by subgroups. Ideally, we would want to split the sample by case characteristics (e.g., crime type, first-time vs. repeated offender), demographics (e.g., age, ethnicity, prior employment status) or both. However, for reasons of sample size and power, we cannot cut the data too finely. Instead, we focus attention on how effects differ by prior employment status, as the question of whether incarceration is criminogenic or preventive is likely to depend strongly on whether a defendant has an actual job to lose and human capital to depreciate by going to prison (see sec. VI). In addition to this subsample estimation, we explore heterogeneity in effects according to unobservables. To do so, we first estimate the marginal treatment effects (MTEs) and then use these estimates to learn about the average treatment effect (ATE), the average treatment effect on the treated (ATT), and the average treatment effect on the untreated (ATUT). The results from the subsample estimation and MTE analysis are reported in section V.D.

### III. Data and Background

#### A. Data and Sample Selection

Our analysis employs several data sources that we can link through unique identifiers for each individual. Information on the court cases comes from the Norwegian Courts Administration. The data set contains information for all court cases over the period 2005–14. We observe the start and end dates of every trial, various case characteristics, the verdict, and unique identifiers for both judges, defendants, and district courts. We link this information with administrative data that contain complete records for all criminal charges, including the type of crime, when it took place, and

suspected offenders. These data can be additionally linked to the prison register with information on actual time spent in prison. We merge these data sets with administrative registers provided by Statistics Norway, using a rich longitudinal database that covers every resident from 1967 to 2016. For each year, it contains individual demographic information (including sex, age, and number of children), socioeconomic data (such as years of education, earnings, employment), as well as geographical and firm identifiers.

To construct our baseline sample, we exclude the nonrandomly assigned cases described in section II.B and focus on regular judges handling nonconfession cases.<sup>14</sup> This yields a sample of randomly assigned cases to the same pool of judges. Excluding the nonrandomly assigned cases is straightforward, as these cases are flagged in our data set. Our baseline sample further restricts the data set to judges who handle at least 50 randomly assigned confession or nonconfession cases between the years 2005 and 2014 (i.e., at least 50 of the cases used to construct our judge stringency instrument). Since we will be including court  $\times$  year of case registration fixed effects in all our estimates, we also limit the data set to courts that have at least two regular judges in a given year. Our main estimation sample uses cases decided between 2005 and 2009 so that each defendant can be followed for up to 5 years after decision, while the judge stringency instrument is based on the entire period from 2005 to 2014. Table A1 (tables A1, A2, B1–B16, C1–C3, D1–D5 are available online) shows how the various restrictions affect the number of cases, defendants, judges, and courts in our sample. After applying our restrictions, the baseline estimation sample includes 33,548 cases, 23,373 unique defendants, and 500 judges.

### *B. Descriptive Statistics*

We now provide some summary statistics for defendants, crime types, and judges. Panel A in table A2 shows that defendants are relatively likely to be young, single men. They also have little education, low earnings, and high unemployment prior to the charge, with less than 40% of defendants working in the prior year. Serial offenders are common, with 38% of defendants having been charged for a different crime in the prior year. Panel B reports the fraction of cases by primary crime category. Around one-fourth of cases involve violent crime, while property, economic, and drug crime each comprise a little more than 10% of crimes. Drunk driving, other traffic offenses, and miscellaneous crime make up the remainder.

<sup>14</sup> In comparison, judges are fairly similar in their incarceration rates for confession cases. Replicating the IV specification of col. 3 of table 4 using only confession cases, we estimate an effect of  $-0.333$  (standard error of  $0.311$ ). While the magnitude of the coefficient is similar, the standard error is more than three times larger.

In figure 2, we document the typical employment and crime levels for our sample over time. Panel A plots the probability a defendant has any paid employment in a given month during the 10-year period surrounding their court decision. There are separate lines for defendants who are sentenced to incarceration versus not sentenced to incarceration. The first fact that emerges is that prior to the court decision, labor market participation is low for both groups, with less than 30% of defendants working in any month. Employment rates for the incarcerated group are a few percentage points lower; to ease comparison of changes over time, the graph also adjusts the nonincarcerated group's employment line to be the same as the incarcerated group's at the beginning of the sample period. Both groups have monthly employment rates that increase over time, reflecting the fact that employment rises as individuals become older.

The most striking pattern in the graph is the divergence in employment between the incarcerated and the nonincarcerated defendants around the time of the court decision. The positively sloped pretrends for both groups are fairly similar up until about 1 year before the court decision date. However, around 12 months prior to the decision, the incarcerated line trends sharply downward. This could be the result of incarcerated individuals being more likely to lose their jobs and turn to crime prior to the court's decision or, alternatively, incarcerated individuals being more likely to commit crime and lose their jobs as a result. Either way, the divergent trends prior to treatment suggest that the two groups are not comparable. The downward trend continues until about 6 months after the decision, at which point it resumes its upward trend. Comparing the two lines reveals a sizable and stubbornly persistent drop in employment for the incarcerated group relative to the nonincarcerated.<sup>15</sup> Similar patterns are found for earnings and hours worked (see fig. B1; figs. A1, A2, B1–B7 are available online).

In panel B of figure 2, we plot the probability an individual is charged with at least one crime in a month over time. The figure reveals that both types of defendants have a high propensity to commit a crime. Five years before the court decision, defendants who will be incarcerated have a 10% chance of committing a crime in a month compared with 7% for those who will not be incarcerated. Examining the pretrends, there is a large jump around the court decision for both groups, since in order to have a court decision an individual must first be charged with a crime. While the two groups have similar trends for much of the preperiod, they begin

<sup>15</sup> There are several reasons why employment does not drop to zero after the court decision for those sentenced to prison. First, the average waiting time after a court decision before being sent to prison is around 5 months, and many prison stays are short. Second, the receipt of employment-related payments while in prison, such as vacation pay, shows up as working for pay in our data set. Third, a small number of individuals are allowed to work outside of prison while incarcerated.

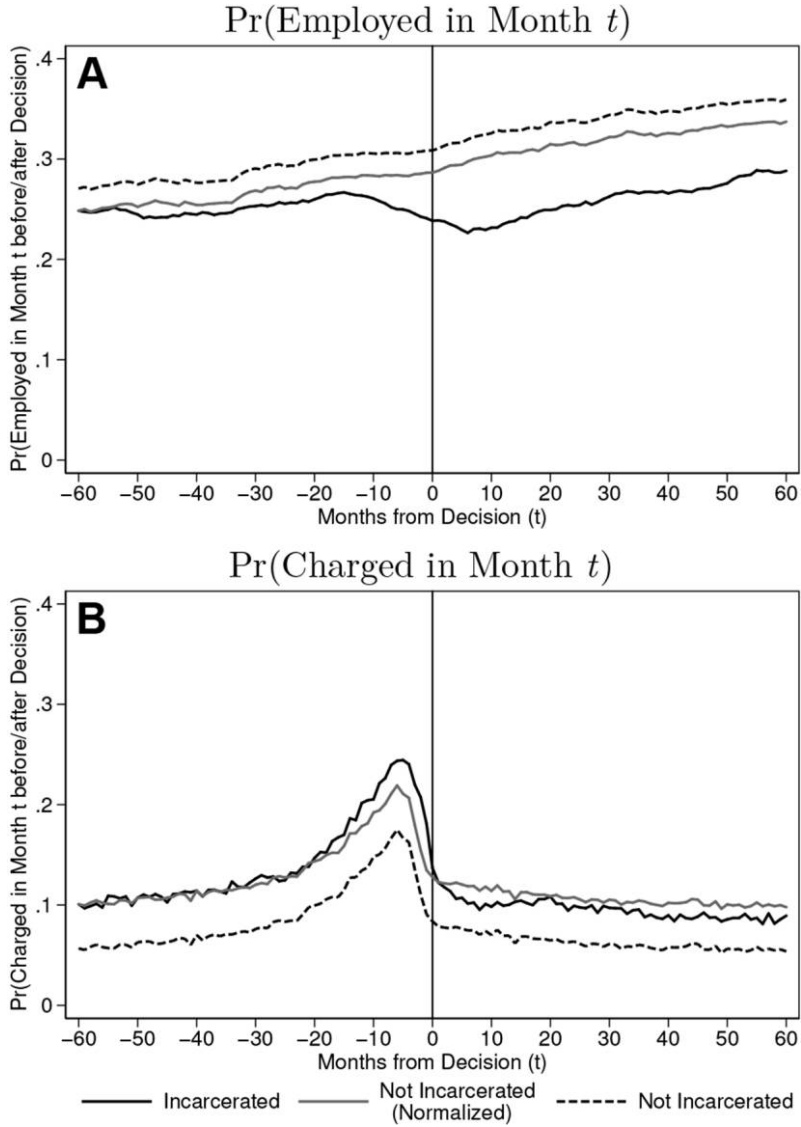


FIG. 2.—Employment and criminal charges before and after month of court decision. The baseline sample consists of 33,548 nonconfession criminal cases processed in 2005–9. Defendants are categorized into two groups, either incarcerated (solid black line) or not incarcerated (dashed black line). To ease the comparison of trends, in each panel we normalize the level of the not incarcerated group's outcomes to the level of the incarcerated group's outcome in month  $t = -60$ . Outcomes for this normalized not incarcerated group are shown by the gray solid line. In both panels, the X-axis denotes months since court decision (normalized to period 0).



to diverge a little more than a year before the court decision, with the incarcerated group exceeding the nonincarcerated group by around 10%. That is, the incarcerated defendants get into more trouble with the police in the months leading up to their court decision. After the court decision, the probability of being charged with a crime returns to around 10% for both groups.<sup>16</sup>

In addition to describing our data, the graphs presented in figure 2 highlight the hazards of using OLS or difference-in-differences to estimate the effects of incarceration. The incarcerated and nonincarcerated groups are not comparable in their preincarceration levels. Moreover, the trends in employment and criminal activity diverge before the court decision in ways that indicate that there is an Ashenfelter dip prior to incarceration. These patterns motivate our quasi-experimental approach using the random assignment of judges.<sup>17</sup>

### *C. What Does It Mean to Be Incarcerated in Norway?*

To help with interpretation, we briefly describe prison conditions in Norway (see <http://kriminalomsorgen.no>). Prisons emphasize rehabilitation and follow the principle of normality set forth by the Directorate of Norwegian Correctional Services. The principle dictates that “life inside will resemble life outside as much as possible” and that “offenders shall be placed in the lowest possible security regime.” This means that low-level offenders go directly to open prisons, which have minimal security as well as more freedoms and responsibilities. Physically, these open prisons resemble dormitories rather than rows of cells with bars. More serious offenders who are at risk of violent or disruptive behaviors are sent to closed prisons, which have heightened security. The two types of prisons create a separation between minor and more hardened criminals, at least until the hardened criminals have demonstrated good behavior.<sup>18</sup> While more serious offenders serve the majority of their sentence in closed prisons, they are usually transferred to open prisons for resocialization and further rehabilitation before release. Overall, one-third of prison beds are in open prisons, and the rest are in closed prisons.

<sup>16</sup> There are two reasons why both types of defendants can be charged with crimes in the months immediately following a court decision. First, we measure when an individual was charged, not when the crime was committed. Second, individuals can commit additional crimes after their court decision before they have been imprisoned (5-month waiting time on average) as well as additional crimes while in prison.

<sup>17</sup> While one could omit the 12 months on either side of treatment in an attempt to avoid the Ashenfelter dip, this would assume that the pretreatment changes are caused by transitory shocks rather than a trend break (see the discussion in Ashenfelter and Card 1985).

<sup>18</sup> This separation could be important, as Bayer, Hjalmarsson, and Pozen 2009 find that inmates build criminal capital through interactions with other criminals.

In Norway, there are a total of 61 prisons. The largest prison (in Oslo) has 392 cells, while the smallest has 13. Norway has a strict policy of one prisoner per cell and tries to place prisoners close to home so that they can maintain links with the families. This means that there is often a waiting list for nonviolent individuals before they can serve their prison time. Sentenced individuals are released after their trial and receive a letter informing them when a cell opens up; in our data, we calculate an average wait time of 5 months.

To help with rehabilitation, all prisons offer education, mental health, and training programs. In 2014, 38% and 33% of inmates in open and closed prisons, respectively, participated in some type of educational or training program. The most common programs are for high school and work-related training, although inmates can also take miscellaneous courses. All inmates are involved in some type of regular daily activity, unless they have a serious mental or physical disability. If they are not enrolled in an educational or training program, they must work within prison.<sup>19</sup>

All inmates have the right to daily physical exercise and access to a library and newspapers. By law, all prisoners have the same rights to health care services as the rest of the population. The Norwegian Directorate of Health is responsible for managing health programs for inmates. Most notably, 18% of inmates participate in a drug-related program while in prison. After release, there is an emphasis on helping offenders reintegrate into society, with access to programs set up to help ex-convicts find a job and access social services, like housing support.<sup>20</sup>

#### *D. Comparison to Other Countries*

There are both similarities and differences in the criminal population and the criminal justice system of Norway versus the rest of the world. Along most dimensions, Norway looks broadly similar to many other Western European countries. Also, while it shares some commonalities with the United States, the United States is an international outlier in some respects.

##### 1. Incarceration Rates

Figure A1 graphs Norway's incarceration rate over time. In 1980, there were an estimated 44 incarcerated individuals per 100,000 in Norway. This rate has increased gradually over time, with a rate of 72 per 100,000 in

<sup>19</sup> All prisoners, whether working or participating in training or education programs, receive a small stipend while in prison (around \$8 per day in 2015). This stipend is not included in any of our earnings measures.

<sup>20</sup> It is important to realize that the initial judge assigned to a case does not determine which prison a defendant is sent to; the type of training, educational, or work program a defendant participates in; or when a defendant is eligible for parole.

2012. This 64% increase is not merely due to more crime being committed over time, as there has been a more modest 25% increase in crime over the same period (Lappi-Seppälä 2012). Norway's gradual increase is mirrored in other Western European countries as well, although Norway's rate is slightly lower. In comparison, the US incarceration rate has shot up dramatically, so much so that a separate scale is needed in the figure for the United States. Not only did the United States start at a higher rate of 220 in 1980, but also this rate reached more than 700 by 2012.<sup>21</sup>

Comparing Norway and the United States with a broader set of countries, the United States remains an outlier. This can be seen in figure A2, which plots incarceration rates versus gross domestic product (GDP) for 160 countries with a population of greater than half a million. No other country comes close to the US rate of roughly 700 per 100,000, and only the six countries of Rwanda, El Salvador, Turkmenistan, Thailand, Cuba, and Russia have more than 400 per 100,000. In contrast, the figure shows that Norway's incarceration rate is similar to the average for other Western European countries (102 per 100,000). The United States is particularly an outlier after controlling for GDP per capita; relative to other countries with high GDP per capita (purchasing power adjusted), the US incarceration rate is several multiples higher.<sup>22</sup>

## 2. Inmate Characteristics

Along many dimensions, the prison populations in Norway, Western Europe, and the United States are similar.<sup>23</sup> Across all these countries, roughly three-fourths of inmates have not completed the equivalent of high school. Five percent of prisoners in Norway are female compared with 5% in Western Europe and 7% in the United States. In all these countries, inmates are in their early or midthirties on average.

The types of offenses committed by inmates differ across countries but perhaps less than one might expect. In terms of the fraction of prisoners who have committed a drug offense, the rates are surprisingly similar, with 24% in Norway, 22% in Western Europe, and 20% in the United States. By

<sup>21</sup> Neal and Rick (2016) show that most of the growth in incarceration rates in the United States can be explained by changes in sentencing policy as opposed to higher crime and arrest rates.

<sup>22</sup> It is more difficult to compare measures of criminal activity across countries because of differences in reporting. With this caveat in mind, the United States has more than double the number of reported assaults than either Norway or the rest of Western Europe, according to the United Nations Survey on Crime Trends (Harrendorf, Heiskanen, and Malby 2010). Such differences cannot fully explain the large incarceration gap, however, with at least part of the difference being due to longer mandatory sentencing policies for minor crimes (see Raphael and Stoll 2013).

<sup>23</sup> For details on the US criminal population, see Raphael and Stoll (2013) and Bureau of Justice Statistics (2015). For Scandinavia and other European countries, see Kristoffersen (2014) and Aebi, Tiago, and Burkhardt (2015).

comparison, 14% are serving a sentence for assault/battery and 4% for rape/sexual assault in Norway, respectively, compared with 11% and 7% in Western Europe and 9% and 11% in the United States. Of course, these comparisons need to be understood in the context of a much higher incarceration rate in the United States. But they point to a considerable overlap in the types of crimes committed by inmates across countries.<sup>24</sup>

### 3. Prison Expenditures, Sentence Lengths, and Postrelease Support

One difference across countries is the amount of money spent on prisoners. Western European countries spend an average of \$66,000 per inmate per year, which is roughly double the average of \$31,000 for the United States. But these averages mask substantial heterogeneity in part due to differences in labor costs, which in Norway account for two-thirds of the prison budget. For example, in Norway the yearly total cost is \$118,000 (similar to Sweden, Denmark, and the Netherlands), in Italy \$61,000, and in Portugal \$19,000. In the United States, the state of New York spends \$60,000 per prisoner, Iowa \$33,000, and Alabama \$17,000. And in New York City, the annual cost per inmate reaches \$167,000.<sup>25</sup>

Norway is able to maintain the type of prison conditions summarized in section III.C in part due to its larger prison budget. In particular, more resources can be devoted to education and training programs, and overcrowding is not an issue. In contrast, while most state prison systems in the United States aim to provide General Educational Development test preparation, adult basic education, and vocational skills training, a recent RAND (2014) report finds that funding for such initiatives is scarce. The United States also faces serious overcrowding issues, with federal prisons being 39% over capacity (GAO 2012) and more than half of states at or above their operational capacity (Bureau of Justice Statistics 2014).

Another difference between Norway (and Western Europe) versus the United States is sentence length. The average time spent in prison using our judge stringency instrument is estimated to be 184 days, or 6 months, for our Norwegian sample. Almost 90% of spells are less than 1 year. This is considerably shorter compared with the average prison time of 2.9 years

<sup>24</sup> These numbers for Norway differ from our estimation sample for two reasons: we do not have illegal immigrants in our data set, and our sample is restricted to nonconfession cases, which are randomly assigned. The numbers for the United States are the weighted average of inmates in federal and state prisons.

<sup>25</sup> Cost estimates are calculated by dividing total prison budgets by number of prisoners. The numbers for Western Europe (sans Belgium and Switzerland) are for 2013 and are purchasing power parity adjusted (Aebi, Tiago, and Burkhardt 2015). The data for 40 US states with available data are for 2010 (Henrichson and Delaney 2012). New York City data are for 2012 (NYC Independent Budget Office 2013).

for the United States (Pew Center 2011) and fairly similar to the median of 6.8 months in other Western European countries (Aebi, Tiago, and Burkhardt 2015). Because of this disparity in sentence lengths, the average cost per prisoner spell in Norway and Europe is smaller compared with the United States, even though the cost per prisoner per year is generally higher.

Norway has been a leader in reforming its penal system to help integrate inmates back into society upon release. While offenders in Norway may lose their job when going to prison, they are usually not asked or required to disclose their criminal record on most job applications. Moreover, while gaps will still appear on employment resumes, these will often span months rather than years due to shorter prison spells. Upon release, all inmates have access to support from the Norwegian work and welfare services. This includes work training programs and help searching for a job as well as access to a variety of social support programs, such as unemployment benefits, disability insurance, and social assistance.

#### IV. Assessing the Instrument

##### A. *Instrument Relevance*

Figure 3 shows the identifying variation in our data, providing a graphical representation of the first stage. In the background of this figure is a histogram that shows the distribution of our instrument (controlling for fully interacted year and court dummies). Our instrument is the average judge incarceration rate in other cases a judge has handled, including the judge's past and future cases that may fall outside of our estimation sample. The mean of the instrument is 0.45, with a standard deviation of 0.08. The histogram reveals a wide spread in a judge's tendency to incarcerate. For example, a judge at the 90th percentile incarcerates about 54% of cases as compared with approximately 37% for a judge at the 10th percentile.

Figure 3 also plots the probability a defendant is sent to prison in the current case as a function of whether he is assigned to a strict or lenient judge. The graph is a flexible analog to the first stage in equation (2), plotting estimates from a local linear regression. The likelihood of receiving a prison sentence is monotonically increasing in the judge stringency instrument and is close to linear. Table 2 reports first-stage estimates where we regress a dummy for whether a defendant is incarcerated in the current case on our stringency instrument. In panel A, we include fully interacted court and year dummies but otherwise no other controls. The first column reports the first-stage estimate at the time of the court decision, whereas the other columns report first-stage estimates in each of the five subsequent years. These columns are identical except for the

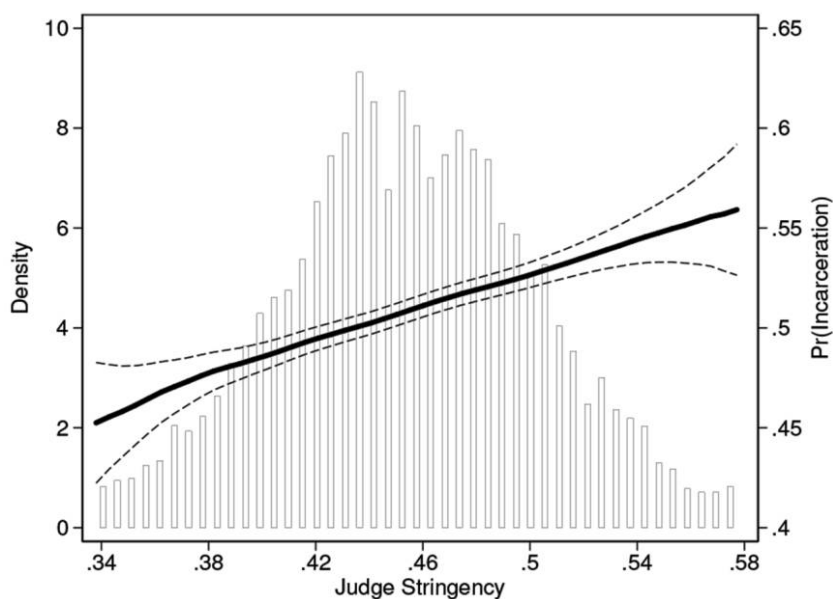


FIG. 3.—First-stage graph of incarceration on judge stringency. The baseline sample consists of 33,548 nonconfession criminal cases processed in 2005–9. The probability of incarceration is plotted on the right Y-axis against leave-out mean judge stringency of the assigned judge, shown along the X-axis. The plotted values are mean-standardized residuals from regressions on court  $\times$  court entry year interacted fixed effects, and all variables are listed in table 1. The solid line shows a local linear regression of incarceration on judge stringency. Dashed lines show 90% confidence intervals. The histogram shows the density of judge stringency along the left Y-axis (top and bottom 2% excluded).

very modest impact of sample attrition (around 6% over 5 years) stemming from death or emigration of defendants.<sup>26</sup> The point estimate of nearly 0.5 barely moves across columns, indicating that attrition exerts a negligible impact on the first-stage relationship. The estimates are highly significant, suggesting that being assigned to a judge with a 10 percentage point higher overall incarceration rate increases the probability of receiving a prison sentence by roughly 5 percentage points.<sup>27</sup>

<sup>26</sup> Another test for selective attrition is to regress the probability of attriting on the judge stringency instrument. Performing this test, we find no evidence of a significant relationship (see table B1).

<sup>27</sup> Note that the number of instruments is determined by the number of moment conditions (and not the number of values the instrument takes). Even though there are many judges, our 2SLS model has one moment condition and therefore a single instrument. Note also that the first-stage coefficient need not be 1, unless the following conditions hold: (1) the sample of cases used to calculate the stringency measure is exactly the same as the estimation sample, (2) there are no covariates, and (3) there are a large number of cases per judge. In our setting, there is no reason to expect a coefficient of 1. In particular, the full set of court times year dummies breaks this mechanical relationship. In sec. V, we perform specification checks for the instrument, including a split-sample approach.

TABLE 2  
FIRST-STAGE ESTIMATES OF INCARCERATION ON JUDGE STRINGENCY  
(Dependent Variable: Pr(Incarcerated))

Estimation Sample	Time of Decision (1)	Month 12 after Decision (2)	Month 24 after Decision (3)	Month 36 after Decision (4)	Month 48 after Decision (5)	Month 60 after Decision (6)
	A. Court $\times$ Year of Court Case Registration Interacted Fixed Effects					
Judge stringency	.4897*** (.0665)	.4922*** (.0661)	.4887*** (.0662)	.4818*** (.0659)	.4795*** (.0661)	.4699*** (.0669)
F-statistic (instrument)	53.56	54.67	53.69	52.79	51.89	48.61
	B. Add Controls for Demographics and Type of Crime					
Judge stringency	.4793*** (.0666)	.4811*** (.0662)	.4755*** (.0662)	.4694*** (.0659)	.4680*** (.0661)	.4587*** (.0670)
F-statistic (instrument)	51.11	52.07	50.82	50.09	49.41	46.20
	C. Add Controls for Demographics, Type of Crime, Past Work, and Criminal History					
Judge stringency	.4705*** (.0632)	.4723*** (.0627)	.4667*** (.0624)	.4622*** (.0622)	.4606*** (.0627)	.4525*** (.0634)
F-statistic (instrument)	54.67	55.95	55.09	54.38	53.18	50.24
Dependent mean	.5083	.5077	.5066	.5055	.5047	.5045
Number of cases	33,548	33,275	32,786	32,341	31,870	31,428

NOTE.—Shown is the baseline sample of nonconfession criminal cases processed in 2005–9. Standard errors are two-way clustered at the judge and defendant level.  
\*\*\*  $p < .01$ .

## B. *Instrument Validity*

### 1. Conditional Independence

For our instrument to be valid, the stringency of a judge must be uncorrelated with both defendant and case characteristics that could affect a defendant's future outcomes (controlling for fully interacted court and year dummies). As discussed in section II.B, table 1 provides strong empirical support for the claim that the criminal justice system in Norway randomly assigns cases to judges within each court in a given time period.

As a second test, panels B and C of table 2 explore what happens if a large set of control variables are added to the first-stage regressions. If judges are randomly assigned, predetermined variables should not significantly change the estimates, as they should be uncorrelated with the instrument. As expected, the coefficient does not change appreciably when demographic and crime type controls are added in panel B. As shown in panel C, this coefficient stability continues to hold when we additionally condition on lagged dependent variables capturing a defendant's prior work and criminal history.

### 2. Exclusion

Conditional random assignment of cases to judges is sufficient for a causal interpretation of the reduced form (RF) impact of being assigned to a stricter judge. However, interpreting the IV estimates as measuring the causal effect of incarceration requires an exclusion restriction: the incarceration rate of the judge should affect the defendant's outcomes only through the incarceration sentencing channel and not directly in any other way. The key challenge here is that trial decisions are multidimensional, with the judge deciding on incarceration, fines, community service, probation, and guilt. After discussing our main results, we will present empirical evidence that the exclusion restriction holds (see sec. V.E). In particular, we will show that our estimates do not change appreciably when we augment our baseline model to either control for judge stringency in other dimensions or include an instrument for other trial sentencing decisions.

### 3. Monotonicity

If the causal effect of incarceration is constant across defendants, then the instrument needs to satisfy only the conditional independence and exclusion assumptions. With heterogeneous effects, however, monotonicity must also be assumed. In our setting, the monotonicity assumption requires that defendants incarcerated by a lenient judge would also be incarcerated by a stricter judge, and vice versa for nonincarceration. This



assumption ensures that the 2SLS estimand can be given a local ATE interpretation; that is, it is an average causal effect among the subgroup of defendants who could have received a different incarceration decision had their case been assigned to a different judge.

One testable implication of the monotonicity assumption is that the first-stage estimates should be nonnegative for any subsample. For this test, we continue to construct the judge stringency variable using the full sample of available cases but estimate the first stage on the specified subsample. Results are reported in column 1 of table B2. In panel A, we construct a composite index of all the characteristics found in table 1, namely, predicted probability of incarceration, using the coefficients from an OLS regression of the probability of incarceration on these variables (while conditioning on fully interacted court and year dummies). We then estimate separate first-stage estimates for the four quartiles of predicted incarceration. Panel B breaks the data into six crime types. Panels C and D split the data by previous labor market attachment and by whether the defendant has previously been incarcerated, respectively. Panels E–G split the samples by age, education, and number of children. For all these subsamples, the first-stage estimates are large, positive, and statistically different from zero, consistent with the monotonicity assumption.

A second implication of monotonicity is that judges should be stricter for a specific case type (e.g., violent crimes) if they are stricter in other case types (e.g., all crimes except for violent crimes). To test this implication, we break the data into the same subsamples as we did for the first test but redefine the instrument for each subsample to be the judge's incarceration rate for cases outside of the subsample. For example, for the violent crime subsample, we use a judge's incarceration rate constructed from all cases except violent crime cases. Column 2 of table B2 lists the first-stage estimates using this reverse-sample instrument, which excludes own-type cases. The first-stage estimates are all positive and statistically different from zero, suggesting that judges who are stricter for one type of case are also stricter for other case types.

## **V. Effects of Incarceration on Recidivism**

In this section, we present our main findings, showing that (1) incarceration causes a large reduction in the probability of reoffending; (2) the drop is not due to only incapacitation, with further reductions in criminal charges after release; and (3) the total number of charged crimes falls over time, with many individuals being diverted from a future life of crime. We then contrast these IV estimates to OLS, learning that the high rates of recidivism among ex-convicts is due to selection and not a consequence of the experience of being in prison. Several robustness and heterogeneity checks follow.

## A. *Main Results*

### 1. Reoffense Probabilities

Panel A of figure 4 graphically presents IV estimates of the effect of incarceration on the probability of reoffending. We define reoffending as the probability of being charged with at least one crime by the end of a given time period.

The graph presents a series of cumulative monthly estimates from 1 month to 60 months after the court decision. For example, the estimate at month 6 uses the probability an individual has been charged with at least one crime by 6 months after the decision as the dependent variable in the second stage of the IV model. As expected, there is little effect on reoffending in the first few months after the court decision, since not much time has elapsed for the committing of new crimes. But the estimate becomes more negative over time, and at around 18 months there is a large and statistically significant reduction of over 25 percentage points in recidivism for those previously sentenced to incarceration. This negative effect persists at roughly the same level all the way to 60 months.

### 2. Incapacitation versus Postrelease Effects

The recidivism effect found in panel A of figure 4 could simply be due to incapacitation, as individuals sentenced to prison time will be locked up and therefore have few criminal opportunities.<sup>28</sup> To better understand the role of incapacitation, table 3 presents IV estimates of the effects of incarceration on prison time.

We find that, on average, being incarcerated leads to a sentence of 231 days in prison. But this is sentencing time (i.e., potential prison time), not actual time served. Using the IV model, we estimate that being incarcerated leads to 184 days, or approximately 6 months, in actual prison time served. This smaller number makes sense, as Norway allows individuals to be released on parole after serving about two-thirds of their prison sentence for good behavior.<sup>29</sup> In column 2 of table 3, we also estimate the average wait time between the court decision and when individuals start serving their prison sentence. The average wait time is estimated to be around 5 months.

In panel B of figure 4, we plot a series of IV estimates for the probability of being in prison, 1–60 months after the court decision. The figure is

<sup>28</sup> Individuals may be charged with a crime while serving prison time, as they can commit crimes while in prison. They may also have other cases working their way through the system while in prison.

<sup>29</sup> The IV estimate suggests that a majority of individuals receive parole in our data set. If an inmate commits a new offense while on parole, this counts as a new charge in our data set.

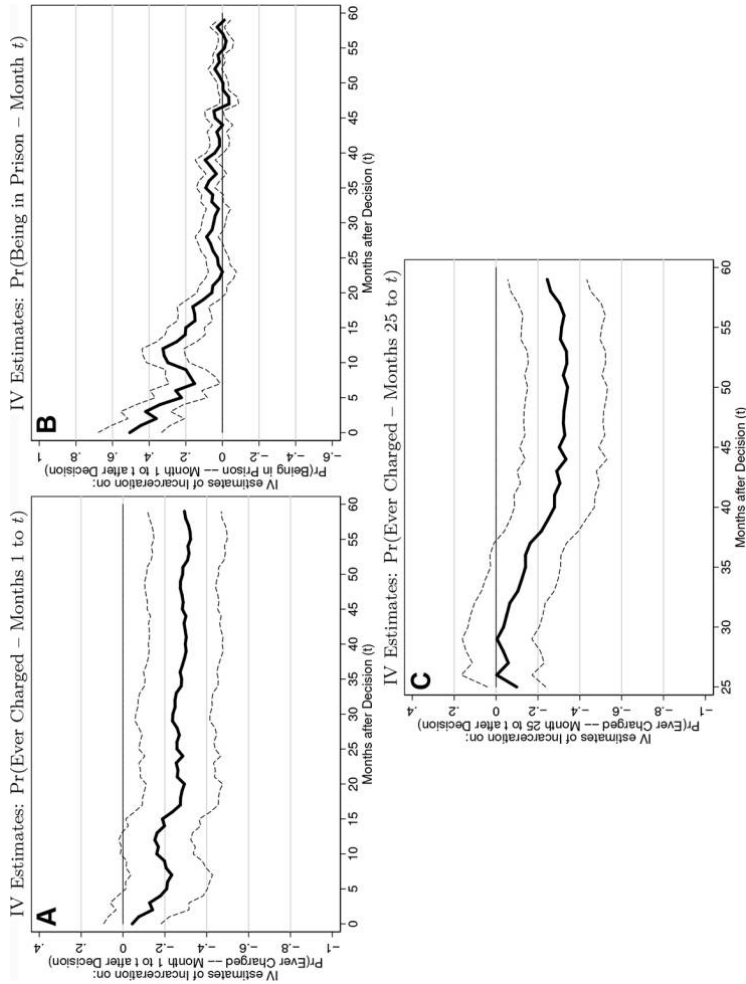


FIG. 4.—Effect of incarceration on recidivism and probability of being in prison. Shown is the baseline sample of nonconfession criminal cases processed in 2005–9 ( $N = 33,548$  at time of decision and  $N = 31,428$  in month 60 after decision). Panel *B* plots prison probabilities related to only the original sentence. Dashed lines show 90% confidence intervals.

TABLE 3  
EFFECT OF INCARCERATION ON PRISON TIME ( $N = 31,428$ )

	DEPENDENT VARIABLE		
	Days of Prison Sentence (Potential Prison Time) (1)	Days Spent outside Prison before Serving Sentence (Waiting Time) (2)	Days of Prison Sentence Served (Actual Prison Time) (3)
RF: judge stringency	104.57** (49.03)	67.89*** (19.47)	83.19*** (25.15)
2SLS: incarcerated	231.088** (91.72)	150.02*** (38.12)	183.83*** (49.78)
Dependent mean	153.75	69.92	69.20

NOTE.—Shown is the baseline sample of nonconfession criminal cases processed in 2005–9. Controls include all variables listed in table 1. Days spent in pretrial custody are included in actual prison time in col. 3. Standard errors are two-way clustered at the judge and defendant level.

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

similar to a survival function, in that if all treated individuals (i.e., those sentenced to prison) started out in prison in month 1, the estimates would map out 1 minus the probability of exit from prison. It is not exactly a survival function though, since not all individuals sentenced to prison begin serving their sentences immediately because of waiting times for an open space. As expected, the probability of being in prison if an individual is sentenced to prison starts out high. This probability falls rapidly, with fewer than 30% of incarcerated individuals being in prison for the original criminal charge 6 months after the court decision. By month 18, only around 5% of these individuals are still in prison, and by month 24 almost none are still in prison.

The main point to take away from panel B of figure 4 is that any incapacitation effect from being incarcerated at time 0 can operate in only the first 2 years. Using this insight, we now graph the probability of ever being charged with a crime between months 25 and 60 in panel C of figure 4. By ignoring crimes committed within the first 2 years after the decision, we are estimating incarceration effects that cannot be attributed to the original incapacitation spell. As in panel A of figure 4, it takes a few months for individuals to start being charged with a crime in this window. But by 15 months after the start of this new window (i.e., 39 months after the court decision), there is a strong and statistically significant reduction in crimes for individuals previously sentenced to prison. The effect is a sizable 25 percentage point reduction in reoffending at least once between months 25 and 60.

In table B3, we provide further granularity by running year-by-year models for crimes committed in a particular year. The table documents

a negative recidivism effect in the first year (when most individuals are in prison for the current case), the second year (after the majority are already released from prison), and in each of years 3–5 (when virtually all are out of prison). The individual point estimates are somewhat noisy, but the estimates all go in the same direction. In table 4, we group the first 2 years together and years 3–5 together for increased precision. That table reveals sizable reductions in recidivism both in years 1 and 2 as well as in years 3–5, consistent with a reduction in crime that is separate from an incapacitation effect.

In theory, it is possible that future pretrial detentions or prison spells could induce an incapacitation effect even after the original prison spell is completed. This could happen if a prior prison sentence flags an individual as higher risk so that in a future case they are either remanded to

TABLE 4  
EFFECTS OF INCARCERATION ON RECIDIVISM ( $N = 31,428$ )

	DEPENDENT VARIABLE			
	Pr(Ever Charged)			Number of Charges
	Months 1–24 after Decision (1)	Months 25–60 after Decision (2)	Months 1–60 after Decision (3)	Months 1–60 after Decision (4)
OLS: incarcerated:				
No controls	.130*** (.007)	.115*** (.007)	.113*** (.006)	5.275*** (.321)
Demographics and type of crime	.126*** (.007)	.109*** (.007)	.105*** (.006)	5.369*** (.310)
All controls	.068*** (.006)	.050*** (.007)	.052*** (.006)	2.917*** (.278)
Complier reweighted	.057*** (.007)	.042*** (.007)	.049*** (.006)	1.595*** (.251)
RF: judge stringency:				
All controls	-.108** (.047)	-.111** (.048)	-.133*** (.045)	-5.196** (2.452)
IV: incarcerated:				
All controls	-.239** (.113)	-.245** (.113)	-.293*** (.106)	-11.482** (5.705)
Dependent mean	.57	.57	.70	10.21
Complier mean if not incarcerated	.56	.57	.73	13.62

NOTE.—Shown is the baseline sample of nonconfession criminal cases processed in 2005–9. Controls include all variables listed in table 1. In addition, RF and IV also control for court  $\times$  court entry year fixed effects. OLS standard errors are clustered at the defendant level, while RF and IV standard errors are two-way clustered at the judge and defendant level.

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

custody while awaiting trial or have an increased chance of being sent to prison. To explore this possibility, in table B4 we examine whether judge stringency in the current case affects time spent in prison for new charges unrelated to the current case. We first estimate how an incarceration sentence in the current case affects the probability of being sent to prison in the future as a result of either pretrial detention or a new incarceration sentence. We find only a small insignificant effect (a 1 percentage point increase relative to the mean of 42%). This small impact likely reflects two opposing forces. Incarceration reduces the likelihood of recidivism, thus lowering the chances of being charged with a future crime. At the same time, we find evidence suggesting that the probability of being incarcerated in the future conditional on an individual being charged with a future crime is higher, consistent with the notion that judges are tougher on repeat offenders (see table B4).

The small effect on future incarceration helps interpret the mechanisms behind our main estimates. In particular, they suggest that incapacitation effects due to future prison spells do not explain the large and persistent reduction in recidivism. For example, estimates for the cumulative number of days spent in prison for new cases is just 4.5 days. This increase is small compared with the direct increase of 184 days of prison time served reported in table 3.

### 3. Number of Crimes

A comparison of panels A and C in figure 4 suggests that incarceration not only prevents an individual from ever committing a crime (the extensive margin) but also prevents individuals from committing a series of future crimes (the intensive margin). In panel A, after month 18, the probability of ever being charged with a crime is flat, suggesting that additional individuals are not being prevented from committing a crime after that time. But in panel C, we see that the probability an individual will commit a crime between 25 and 60 months is affected by an incarceration decision at time 0. This means that many of the individuals who were prevented from committing a crime in panel A are also being prevented from committing another crime in panel C.

To further explore the intensive margin, panel A of figure 5 plots IV estimates for the cumulative number of charges in the months after the court decision. The estimated effects become more negative over time. After 1 year, the estimated effect of an incarceration decision is around three fewer crimes per individual, whereas after 2 years, the effect is seven fewer crimes. By 4 years, the effect is 11 fewer crimes per individual (see also table 4).

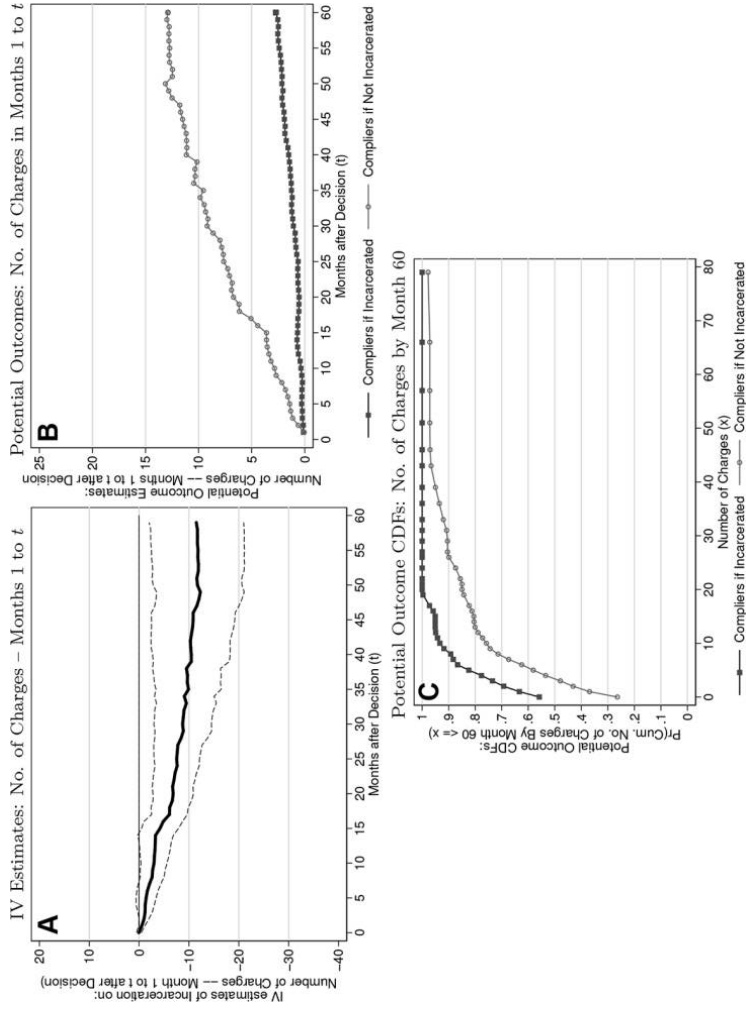


FIG. 5.—Effect of incarceration on number of charges. Shown is the baseline sample of nonconfession criminal cases processed in 2005–9 ( $N = 33,548$  at time of decision and  $N = 31,428$  in month 60 after decision). Dashed lines show 90% confidence intervals.

#### 4. Potential Crimes

Our IV estimates represent the average causal effects for compliers who could have received a different court decision had their case been assigned to a different judge. To better understand this LATE, we follow Imbens and Rubin (1997) and Dahl, Kostøl, and Mogstad (2014) in decomposing the IV estimates into the average potential outcomes if the compliers would have been incarcerated and if they would not have been incarcerated. The top line in panel B of figure 5 is the number of potential charges if the compliers would not have been incarcerated. The line trends upward in close to a linear fashion, with approximately two to three extra criminal charges per year and around 13 crimes on average after 5 years. In sharp contrast, the compliers would have been charged with far fewer crimes if incarcerated; even by month 60, they would have been charged with only two crimes on average.

Panel C plots the distribution functions for cumulative potential charges as of year 5 and for compliers if they would have been incarcerated and if they would not have been incarcerated. The difference between the two cumulative distribution functions (CDFs) when the number of charges is one is around 30 percentage points, which mirrors the IV effect graphed in panel A of figure 4 at 5 years out. Comparing the CDFs further to the right (i.e., for a larger number of charges) makes clear that incarceration is not simply preventing low-crime individuals from committing future crime. To see this, suppose that incarceration caused individuals who would have been charged with five crimes or fewer (or some similarly small number of crimes) from being charged with any crimes but that more hardened criminals (those charged with more than five crimes) were unaffected. In this case, the two lines in panel C would lie on top of each other starting at five charges. But, in fact, the two lines diverge at one charge, remain fairly parallel until around 18 charges, and do not get close to each other until around 45 charges. For instance, 12% of compliers would have been charged with more than 18 crimes if they were not incarcerated, whereas few, if any, compliers would have been charged with this many crimes if incarcerated. Taken together, the results suggest that incarceration must be preventing some individuals from being charged with a large number of crimes and stopping some individuals from a life of crime entirely.<sup>30</sup>

<sup>30</sup> From the graph, one cannot infer whether an individual charged with 35 crimes reduces their charges to zero vs. whether an individual charged with 35 crimes reduces their crime to 15 while the individual charged with 15 reduces their crime to zero. But the shapes of the CDFs do imply that high-volume criminals must reduce their number of charged crimes.



*B. Comparison to OLS*

With few exceptions, the bulk of the research on recidivism is based on OLS regressions with controls for observable confounding factors. In table 4, we present OLS estimates of equation (1) with and without a rich set of controls. The first OLS specification in table 4 regresses whether an individual has reoffended (i.e., been charged with a new crime after the court decision) on whether the defendant was sentenced to prison but includes no other control variables. The OLS estimates 0–2 years after the decision, 2–5 years after the decision, and 0–5 years after the decision are all positive and significant; for example, individuals sent to prison are 11 percentage points more likely to reoffend at least once over the next 5 years.

In the next specification of table 4, we add a host of defendant characteristics, including demographic variables and the type of crime they are being charged with. These controls affect the estimates only slightly. In the third specification, we additionally add lagged variables for whether defendants have previously been charged with a crime, whether they have previously been incarcerated for a previous crime, and whether they have worked in the prior year (i.e., including all the variables listed in table 2 as controls). This brings the coefficient down to 5 percentage points.

The divergence between the OLS estimates and the IV estimates in table 4 is stark. The OLS estimates always remain positive, while the IV estimates are negative and large. One possible explanation is that the OLS estimates suffer from selection bias due to correlated unobservables. If this is the case, we can conclude that the high rates of recidivism among ex-convicts is due to selection and not a consequence of the experience of being in prison.

Another possible explanation for the differences between the IV and OLS estimates is effect heterogeneity, so that the average causal effects for the compliers differ in sign compared with the mean impacts for the entire population. To explore this possibility, it is useful to characterize compliers by their observable characteristics. We begin by splitting our sample into eight mutually exclusive and collectively exhaustive subgroups based on prior labor market attachment and the predicted probability of incarceration (see table B5). The predicted probability of incarceration is a composite index of all the observable characteristics, while prior employment is the key source of heterogeneity in effects, as discussed in the next section. Next, we estimate the first-stage equation (2) separately for each subsample, allowing us to calculate the proportion of compliers by subgroup. We then reweight the estimation sample so that the proportion of compliers in a given subgroup matches the share of the estimation sample for that subgroup. The fourth row of table 4 presents OLS estimates based on this reweighted sample. The results suggest

that the differences between the IV and the OLS estimates cannot be accounted for by heterogeneity in effects, at least due to observables.

### *C. Specification Checks*

Before exploring the results further, we present specification checks related to the construction of the instrument and the procedure for inference (see also app. D). The first column of table B6 presents our baseline results for comparison. In this specification, we include any defendant whose judge handled at least 50 cases. In the next three specifications, we instead require judges to handle at least 25 cases, at least 75 cases, or at least 100 cases, respectively. These changes do not materially affect the estimated effects. This is reassuring, as one might be worried that the statistical inference becomes unreliable if the number of cases per judge is too small.

The next two specification checks examine sensitivity to changing how the instrument is constructed. In column 5, we randomly split our sample in half and use one half of the sample to calculate the average incarceration rate of each judge. We next use these measures of judge leniency as an instrument for incarceration in the other half of the sample. The resulting estimates (and standard errors) do not materially change. The last column shows that our findings are not sensitive to whether we calculate judge stringency based on nonconfession cases only or if we include all randomly assigned cases (both confession and nonconfession cases) in these calculations.

As a final robustness check, panel D in table B6 reports Anderson-Rubin (AR) confidence intervals. The confidence intervals remain valid whether or not the instrument is weak, in the sense that their probability of incorrectly rejecting the null hypothesis and covering the true parameter value remains well controlled. Since IV estimates are nonnormally distributed when an instrument is weak, the AR procedure does not rely on point estimates and standard errors but instead uses test inversion.<sup>31</sup> We find that the confidence intervals do not materially change. Consistent with this finding, we can strongly reject the null hypothesis of weak instrument using the test proposed by Montiel Olea and Pflueger (2013).

### *D. Heterogeneous Effects*

#### *1. First-Time Offenders*

We now examine whether there are heterogeneous effects in the recidivism result. We first limit the sample to first-time offenders, defined as

<sup>31</sup> For details on the procedure, see the review article by Andrews, Stock, and Sun (2019).

defendants who have not previously served time in prison and for whom this is the first court case observed in our sample. Table C2 reports results analogous to table 4 for this subsample. The 5-year cumulative estimates in column 3 are somewhat larger for first-time offenders, with the probability of recidivism dropping by 43 percentage points. Interestingly, the effect is concentrated in the months 25–60 after their court decision, with less evidence for a drop in crime during the period that includes their imprisonment (months 1–24).

Looking at first-time offenders is useful not only for exploring heterogeneous effects but also for ease of interpretation. In our baseline sample, individuals can appear more than once in our data set if they are brought to trial for multiple crimes over time. Individuals appearing multiple times could be in the incarcerated group in one year and the non-incarcerated group in another year. While judges are randomly assigned for each case and hence the baseline estimate is still causal, the interpretation is more nuanced. With first-time offenders, each individual appears only once in the sample. The cost of looking only at an individual's first criminal case is that the sample drops in half, from more than 30,000 observations to fewer than 15,000. Given that the results are qualitatively similar but with less precision for the smaller sample, we focus on results using the more comprehensive data set, which contains all cases with random assignment. A more complete set of results for first-time offenders, which mirror those found for the full sample in what follows, can be found in tables C1–C3.

## 2. Open versus Closed Prisons

A second type of heterogeneity is the type of prison an individual is sent to. As a reminder, there are two types of prisons in Norway: open and closed. As described in section III.C, open prisons have minimal security as well as more freedoms and responsibilities compared with closed prisons. The two types of prisons not only result in different day-to-day activities but also create a separation between minor and more hardened criminals. Whether a convicted defendant is initially sent to an open or closed prison depends on both the severity of the crime as well as geographical proximity and available space at open versus closed prisons. Judges do not directly determine whether individuals are sent to open versus closed prisons. Moreover, when we run a multinomial regression with three outcomes (incarcerated in open prison, incarcerated in closed prison, not incarcerated), we find that a judge's stringency does not differentially affect whether an individual is sent to an open versus a closed prison.<sup>32</sup>

<sup>32</sup> In a multinomial logit regression, which includes the same controls as in panel C of table 2, judge stringency has an average marginal effect of 0.24 (standard error is 0.04) for

To explore whether the types of individuals sent to open versus closed prisons experience different outcomes, we first predict whether an individual will be sent to an open versus closed prison on the basis of the predetermined characteristics in table 2. We then create dummy variables for whether an individual's probability of being sent to an open prison is above or below the median. Finally, we interact these dummy variables with our judge stringency measure and create two analogous instruments. In table B7, we reestimate our main IV specification but with two separate endogenous variables and instruments based on the interactions. We find remarkably similar effects of incarceration on recidivism for those individuals with below and above median probabilities of being sent to an open versus a closed prison (see col. 4). However, it is important not to overinterpret these results, since the two groups could experience heterogeneous effects from incarceration if prison type were held fixed.

### 3. Marginal Treatment Effects

Finally, we explore heterogeneity by examining MTEs. Ignoring subscripts for simplicity, we model the observed outcome as  $Y = I \times Y(1) + (1 - I) \times Y(0)$ , where  $I$  is an indicator for treatment (being incarcerated) and  $Y(1)$  and  $Y(0)$  are the associated potential outcomes, which are a linear function of both observable ( $X$ ) and unobservable factors. The choice of treatment by a judge is given by  $I = 1[v(X, Z) - U]$ , where  $v$  is an unknown function,  $U$  is an unobserved continuous random variable, and  $Z$  is our judge stringency instrument.<sup>33</sup> One can normalize the distribution of  $U|X = x$  to be uniformly distributed over  $[0, 1]$  for every value of  $X$ . Under this normalization, it is straightforward to show that  $v(X, Z)$  is equal to the propensity score  $p(X, Z) \equiv P[D = 1|X = x, Z = z]$ .

The MTE is defined as  $E[Y(1) - Y(0)|U = u, X = x]$ . The dependence of the MTE on  $U$  for a fixed  $X$  reflects unobserved heterogeneity in treatment effects, as indexed by a judge's latent propensity to choose incarceration for a defendant (where  $U$  captures unobserved characteristics of the defendant, which influence the judge's choice). The choice equation implies that, given  $X$ , defendants with lower values of  $U$  are more likely to take treatment, regardless of their realization of  $Z$ . Following Brinch, Mogstad, and Wiswall (2017), we assume separability between observed and unobserved heterogeneity in the treatment effects. Together with the assumption of an exogenous instrument that satisfies monotonicity, this restriction on the potential outcomes is sufficient to allow point identification of MTE over the unconditional support of the propensity

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open prison versus 0.22 (standard error is 0.04) for closed prison, with not incarcerated being the omitted category.

<sup>33</sup> The weakly separable choice equation is equivalent to assuming monotonicity (Imbens and Angrist 1994).

score  $p(X, Z)$ .<sup>34</sup> We probe the stability of MTE estimates to various specifications of the empirical MTE model. Reassuringly, the estimates based on a linear, quadratic, cubic, or quartic specification all yield similar estimates, as does a semiparametric specification based on local linear regressions.

Panel A in figure 6 graphs the propensity score distributions for the treated and untreated samples. The dashed lines indicate the upper and the lower points of the propensity score with common support (after trimming 1% of the sample with overlap in the distributions of propensity scores). Panel B of figure 6 plots MTE estimates by the unobserved resistance to treatment (i.e., the latent variable  $U$ ) based on a local IV approach using a global cubic polynomial specification. The MTE estimates are most negative for those with a low unobserved resistance to treatment and rise as unobserved resistance to treatment increases. This implies that incarceration reduces recidivism the most for defendants whose unobservables would make them very likely to go to prison regardless of the stringency of their judge. In contrast, defendants whose unobservables would make them very unlikely to go to prison experience, if anything, an increase in recidivism due to treatment, with the caveat that the estimates are noisy.

As shown by Heckman and Vytlačil (1999, 2005, 2007), all conventional treatment parameters can be expressed as different weighted averages of the MTE. Recovering these treatment parameters for the entire population, however, requires full support of the propensity score  $p(X, Z)$  on the unit interval. Since we do not have full support, we follow Carneiro, Heckman, and Vytlačil (2011) in rescaling the weights so that they integrate to 1 over the region of common support. Table B8 uses the MTE estimates to construct such rescaled estimates of the ATT, ATE, and ATUT. These weighted averages are obtained by integrating the MTE over the propensity score for the relevant sample. The ATT estimates reveal that the recidivism effects of imprisonment are especially large for the treated; for example, the linear specification yields an estimate of  $-0.42$ , which is more negative than either the LATE or the ATE. By comparison, the estimated ATE (also plotted as the horizontal line in fig. 6) is similar to the LATE. The ATUT, in contrast, is closer to zero and not statistically significant.

#### *E. Threats to Exclusion Restriction*

As discussed in section IV.B, interpreting the IV estimates as average causal effects of incarceration requires the judge stringency instrument to affect

<sup>34</sup> Separability between observed and unobserved heterogeneity in the treatment effect is weaker than additive separability between  $I$  and  $X$ , which is a standard auxiliary assumption in applied work using IV. Furthermore, it is implied by (but does not imply) full independence ( $Z, X \perp Y(1), Y(0), U$ ), a common assumption in applied work estimating MTEs. See Mogstad and Torgovitsky (2018).

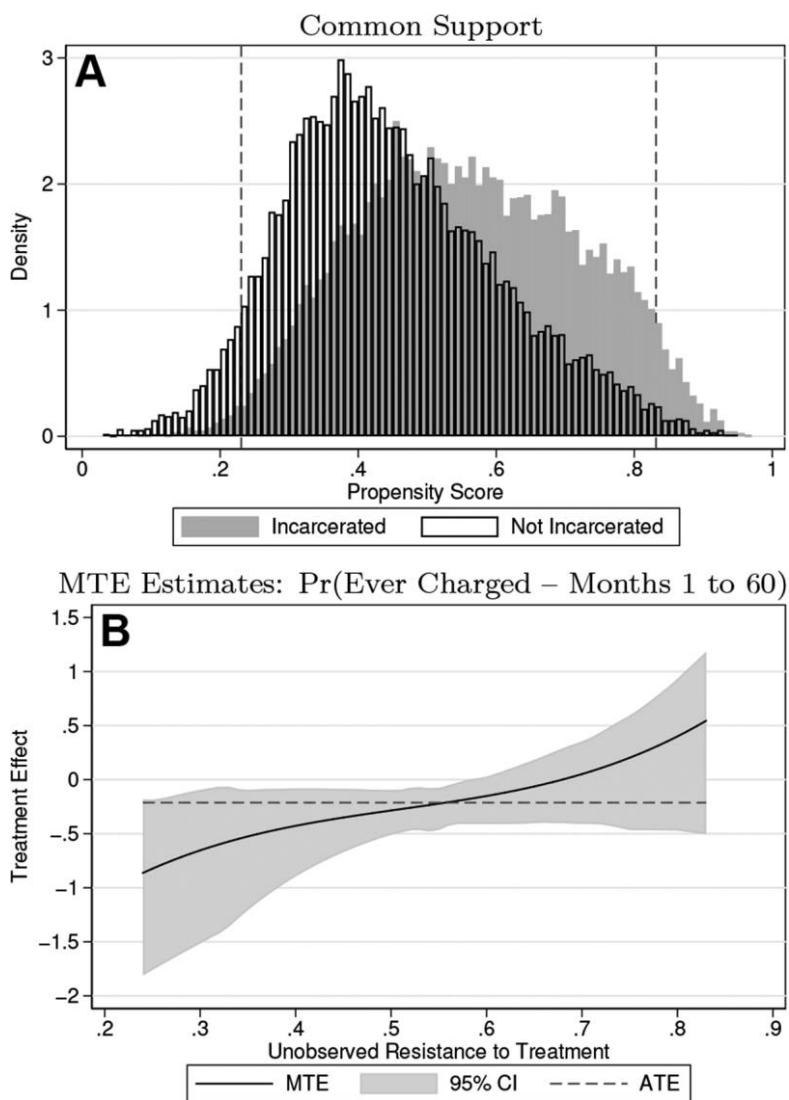


FIG. 6.—Common support and MTEs. Shown is the baseline sample of nonconfession criminal cases processed in 2005–9 ( $N = 31,428$  in month 60 after decision). In A, dashed lines indicate the upper and the lower points of the propensity score with common support (based on 1% trimming). In B, the MTE estimates are based on a local IV approach using a global cubic polynomial specification for the trimmed sample with common support ( $N = 28,275$ ). Standard errors are constructed on the basis of 100 bootstrap replications. A color version of this figure is available online.

the defendant's outcomes only through the prison sentencing channel. A potential issue is that trial decisions are multidimensional, with judges deciding on incarceration, fines, community service, probation, and guilt (where the penalties are not mutually exclusive).

To make this issue precise, it is useful to extend the baseline IV model given by equations (1) and (2), distinguishing between the incarceration decision and other trial decisions:

$$I_{i,0}^{Incar} = \alpha Z_{j(i)}^{Incar} + \gamma Z_{j(i)}^{Other} + X_i' \delta + v_{i,0}, \quad (3)$$

$$I_{i,0}^{Other} = \zeta Z_{j(i)}^{Incar} + \lambda Z_{j(i)}^{Other} + X_i' \psi + u_{i,0}, \quad (4)$$

$$Y_{i,t} = \beta_t I_{i,0}^{Other} + \theta_t I_{i,0}^{Incar} + X_i' \omega_t + \eta_{i,t}, \quad (5)$$

where  $j$  denotes the judge that handles defendant  $i$ 's case;  $I_{i,0}^{Incar}$  is an indicator variable equal to 1 if defendant  $i$  is sentenced to prison in period 0;  $I_{i,0}^{Other}$  is an indicator variable equal to 1 if defendant  $i$  is sentenced to fines, community service, or probation;  $Z_{j(i)}^{Incar}$  denotes the judge stringency instrument for the incarceration decision;  $Z_{j(i)}^{Other}$  denotes the judge stringency instrument for trial decisions other than incarceration; and  $X_i$  is a vector of control variables that includes a full set of case year  $\times$  court dummy variables. The omitted reference category is not guilty. As in the baseline model, we measure  $Z_{j(i)}^{Incar}$  and  $Z_{j(i)}^{Other}$  as leave-out means.

There are two cases in which the baseline IV estimates based on equations (1) and (2) are biased because they abstract from trial decisions other than incarceration. The first case is if  $Z_{j(i)}^{Incar}$  correlates with  $Z_{j(i)}^{Other}$ , and  $Z_{j(i)}^{Other}$  directly affects  $Y_{i,t}$  (conditional on  $X_i$ ). This would violate the exclusion restriction in the baseline IV model because  $Z_{j(i)}^{Incar}$  affects  $Y_{i,t}$  not only through  $I_{i,0}^{Incar}$  but also through its correlation with  $Z_{j(i)}^{Other}$ . However, controlling for  $Z_{j(i)}^{Other}$  in both equations (1) and (2) will eliminate this source of bias. The second case is if  $Z_{j(i)}^{Incar}$  correlates with  $I_{i,0}^{Other}$  conditional on  $Z_{j(i)}^{Other}$ , and  $I_{i,0}^{Other}$  affects  $Y_{i,t}$  holding  $I_{i,0}^{Incar}$  fixed (conditional on  $X_i$ ). In the baseline IV model, this would violate the exclusion restriction because  $Z_{j(i)}^{Incar}$  affects  $Y_{i,t}$  not only through  $I_{i,0}^{Incar}$  but also through its influence on  $I_{i,0}^{Other}$ . The augmented IV model given by equations (3)–(5) addresses this issue by including  $I_{i,0}^{Other}$  as an additional endogenous regressor and  $Z_{j(i)}^{Other}$  as an extra instrument.<sup>35</sup>

In tables B9 and B10, we examine these two cases, finding support for the exclusion restriction. To start, we first calculate a judge's tendencies

<sup>35</sup> Note that a causal interpretation of the IV estimates based on eqq. (3)–(5) requires assumptions in addition to the instrument exogeneity and monotonicity conditions discussed in sec. IV.B. As shown by Kirkeboen, Leuven, and Mogstad (2016), one may either assume that the effects of each treatment are the same across individuals or invoke additional restrictions on individuals' choice behavior.

on trial decisions other than incarceration.<sup>36</sup> For example, we measure a judge's probation stringency as the average probation rate in the other cases a judge has handled. Panel A of table B9 repeats our baseline specification for comparison. In panel B, we add a judge's probation stringency, community service stringency, and fine stringency as three additional controls in both the first and the second stages. A decision of not guilty is the omitted category. The IV estimates for both recidivism outcomes are similar to our baseline, albeit with standard errors that are larger. To increase precision, panel C combines these three control variables into a single "probation, community service, or fine" stringency variable. Again, the IV estimates for recidivism are similar to the baseline in panel A, but the standard errors are considerably larger.

We next estimate the augmented IV model given by equations (3)–(5). Table B10 presents the first stage, RF, and IV estimates. To make sure we have enough precision and avoid problems associated with weak instruments, we use a specification with three decision margins: "incarceration," "probation, community service, or fine," and "not guilty." For the incarceration first stage, the judge stringency instrument for the incarceration decision has a similar coefficient as before. For the other first stage, the judge stringency instrument for the incarceration decision matters little, if anything, but the other instrument is strongly significant. To formally evaluate the overall strength of the instruments, we report the Sanderson-Windmeijer *F*-statistics, indicating that weak instruments are not an issue. Looking at the RF estimates, the coefficients on the judge stringency instrument for the incarceration decision are virtually unchanged compared with the baseline IV model. In contrast, we find that the judge stringency instrument for the other decisions has almost no effect on recidivism in the RF. Likewise, the IV estimates for incarceration in the final columns of table B10 are similar to those from the baseline IV model, which does not include an instrument for the other decision margins.

A useful by-product of examining the threats to exclusion from trial decisions other than incarceration is that it helps with interpretation. The baseline IV model compares the potential outcomes if incarcerated with the outcomes that would have been realized if not incarcerated. The augmented IV model helps to clarify what is meant by not incarcerated, distinguishing between not guilty as opposed to alternative sentences to imprisonment. The IV estimates in table B10 suggest significant effects of being sentenced to prison compared with being found not guilty, whereas the "probation, community service, or fine" category does not have a statistically different effect compared with not guilty.

<sup>36</sup> While not a trial decision per se, judges could also differ in how quickly they process cases. Creating a second instrument on the basis of a judge's average processing time in other cases they have handled and redoing the empirical tests reported below with processing time as an additional covariate yields similar conclusions.



*F. Sentence Length*

It is possible that strict judges (as measured by our judge stringency instrument) are more likely both to incarcerate defendants and to give them longer sentences. If this is the case, our baseline estimates capture a linear combination of the extensive margin effect of being incarcerated and the intensive margin of longer sentences. However, most of the sentences observed in the data are short, so there is limited variation along the intensive dimension. As shown in figure B2, the median sentence length is 3 months in our sample, with roughly 80% of sentences being less than 1 year. Empirically, there is little difference in sentence lengths across judges, holding incarceration rates fixed. This is consistent with judges having discretion on the incarceration decision but using mandatory rules or guidelines for sentence lengths.

Keeping these caveats in mind, we now explore various models that use sentence length. To provide context, panel a of figure B3 graphs sentence length in days (including zeros) as a function of our judge incarceration stringency instrument. The upward slope largely reflects the fact that stricter judges send more defendants to prison. Panel b illustrates how sentence length is affected by our instrument. It plots estimates of the probability a sentence length will exceed a given number of days (including zeros) as a function of the judge stringency instrument and reveals that most of the action is for relatively short sentences.<sup>37</sup> As shown in table 3, using our judge stringency instrument results in an increase of roughly 6 months spent in prison, which helps in interpreting our main estimates in table 4.

A complementary analysis is to replace the endogenous variable of incarceration with sentence length but still use our judge incarceration stringency variable as the instrument. As shown by Imbens and Angrist (1994), 2SLS applied to an IV model with variable treatment intensity (such as days in prison) captures a weighted average of causal responses to a unit change in treatment for those whose treatment status is affected by the instrument. The weight attached to the  $j$ th unit of treatment is proportional to the number of people who, because of the instrument, change their treatment from less than  $j$  to  $j$  or more. In our setting, this means that coding the endogenous regressor as days in prison (instead of incarceration) permits identification of a weighted average of the effect of another day in prison. Thus, this parameter captures a convex combination of the extensive margin effect of going to prison and the intensive margin effects of longer sentencing. When estimating this model with days in prison as the endogenous regressor, the results are consistent with those

<sup>37</sup> To calculate these estimates, we use a specification similar to eq. (2) but replace the dependent variable for incarceration with indicators for a sentence length exceeding a given number of days.

using the binary incarceration measure. The effect of increasing sentence length by 250 days (roughly the average sentence length) yields estimates that are similar in size to our estimates based on the binary endogenous variable of incarceration but with standard errors that are 75% larger (see table B11).

Finally, we consider models that include both incarceration and sentence length simultaneously. Our first exploration is what happens if we control for a judge's sentence length stringency, defined as the average sentence length in the other cases a judge has handled. In panel D in table B9, when we add in controls for sentence length stringency, it has little effect on our IV estimates. When we try to go a step further, treating both incarceration and sentence length as endogenous variables and using two instruments, the standard errors for IV blow up due to the multicollinearity of incarceration stringency and sentence length stringency (see table B12).<sup>38</sup> This means that we cannot separately identify the intensive and extensive margin effects. But it is worth noting that the RF regression, which includes both instruments, finds a similar estimate for incarceration stringency compared with baseline and no significant effect for sentence length stringency.

## VI. Employment and Recidivism

This section explores factors that may explain the preventive effect of incarceration, showing that the decline in crime is driven by individuals who were not working prior to incarceration. Among these individuals, imprisonment increases participation in programs directed at improving employability and reducing recidivism and ultimately raises employment and earnings while discouraging further criminal behavior.

### A. *Recidivism as a Function of Prior Employment*

To examine heterogeneity in effects by labor market attachment, we assign defendants to two similarly sized groups on the basis of whether they were employed before the crime for which they are in court occurred. We classify people as previously employed if they were working in at least one of the past 5 years; the other individuals are defined as previously nonemployed.<sup>39</sup> We then reestimate the IV model separately for each subgroup.

<sup>38</sup> These patterns are similar whether we also add controls for "probation, community service, or fine" stringency. The first-stage graph of sentence length stringency on sentence length can be found in panel c of fig. B3.

<sup>39</sup> As in the study of Kostøl and Mogstad (2014), an individual is defined as employed in a given year if his annual earnings exceed the yearly substantial gainful activity threshold (used to determine eligibility to government programs, such as unemployment insurance). In 2010, this amount was approximately NOK 72,900 (\$12,500). Our results are not sensitive to exactly how we define employment.

Figure 7 presents the IV estimates for the two subsamples of the effect of incarceration on the probability of reoffending. The results show that the effect is concentrated among the previously nonemployed. The effects of incarceration for this group are large and economically important. In particular, the likelihood of reoffending within 5 years is cut in half because of incarceration, from 96% to 50%. Examining the results in figure B5 reveals that incarceration not only reduces the probability of reoffending among the previously nonemployed but also the number of crimes they commit. Five years out, this group is estimated to commit 22 fewer crimes per individual if incarcerated. By comparison, previously employed individuals experience no significant change in recidivism due to incarceration.

A natural question is whether the heterogeneous effects are due to labor market attachment per se or to variables correlated with prior employment. To explore this, we first compare the characteristics of the previously employed and nonemployed subsamples. As seen in table A2, the two

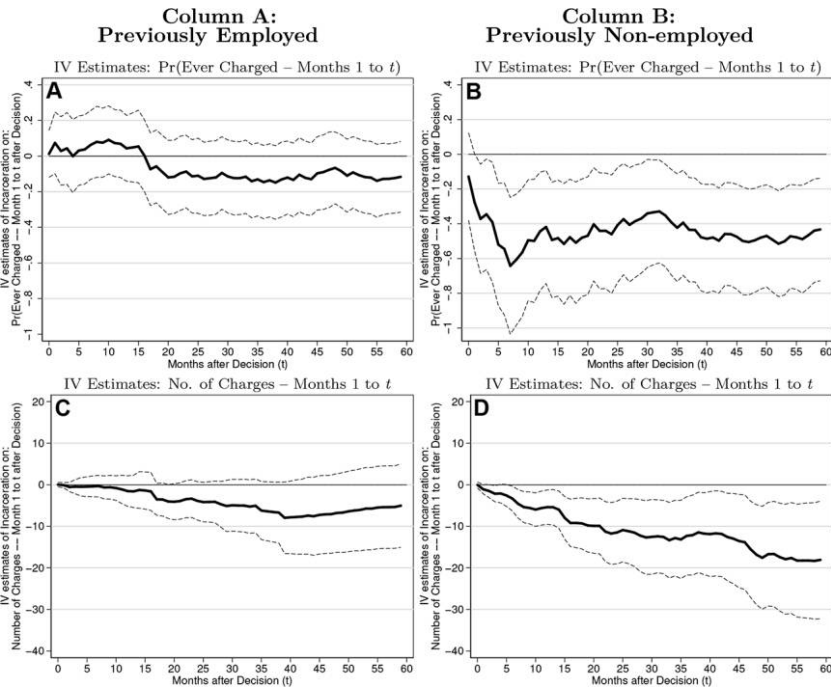


FIG. 7.—Effect of incarceration on recidivism by previous labor market attachment. Shown is the baseline sample consisting of nonconfession criminal cases processed in 2005–9 ( $N = 33,548$  at time of decision and  $N = 31,428$  in month 60 after decision). Dashed lines show 90% confidence intervals.

subsamples differ in characteristics other than prior employment.<sup>40</sup> The nonemployed group is about 2 years younger, less likely to be married, and have lower education. They are more likely to commit property and drug-related crimes instead of economic and traffic-related offenses, including drunk driving. Both groups are charged with an equal number of violent crimes. The nonemployed individuals are also 50% more likely to have been charged with a crime in the year before their court case.

These comparisons make clear that the previously employed and nonemployed have different characteristics. To find out whether these differences can explain the contrasting recidivism effects, we reweight the subsamples so that they are similar on the basis of observables. To do this, we estimate the probability of being in the previously employed group using all the control variables listed in table 1 (excluding the variables on past work history). Figure B4 plots the estimated propensity scores for both the previously employed and the previously nonemployed groups. There is substantial overlap for the entire range of employment probabilities. Using these propensity scores, we weight each subsample so that they have the same distribution as the opposite subsample.

Table 5 reports estimates in columns 1 and 3 for the baseline balanced sample 5 years after the court decision, without any reweighting. Consistent with the figures discussed above, prison time dramatically reduces the extensive and intensive recidivism margins for the previously nonemployed defendants. The same is not true for the previously employed, where the effects are much smaller in absolute value and not statistically significant. The table then reports the weighted results in columns 2 and 4. This has little effect on the estimates, indicating that differences in observable characteristics are not driving the contrasting results. Instead, it appears that the differential effects are driven by labor market attachment per se or correlated unobservable characteristics.

### *B. The Effect of Incarceration on Future Employment*

Why are the reductions in recidivism concentrated among the group of defendants with no prior employment? To shed light on this question, we turn to an examination of the labor market consequences of incarceration depending on prior employment.

Using the previously nonemployed sample, panel B of figure 8 plots the IV estimates for the probability of being ever employed by a given time period. Two years after the court decision, previously nonemployed

<sup>40</sup> A few of the nonemployed will have earned more than the minimum threshold in the year before their court case, even though by definition the 5 years before their crime they were nonemployed. This is because the date of a court case does not line up precisely with the date of a crime.

TABLE 5  
EFFECT OF INCARCERATION ON RECIDIVISM BY PREVIOUS LABOR MARKET ATTACHMENT

	SUBSAMPLE			
	Previously Employed (N = 16,547)		Previously Nonemployed (N = 14,881)	
	(1)	(2)	(3)	(4)
A. Dependent Variable: Pr(Ever Charged)				
Months 1–60 after decision	Baseline	Reweightd	Baseline	Reweightd
RF: judge stringency, all controls	–.062 (.063)	–.079 (.068)	–.183*** (.060)	–.157*** (.069)
IV: incarcerated, all controls	–.117 (.119)	–.146 (.126)	–.433** (.177)	–.365* (.192)
Dependent mean	.62	.58	.79	.76
Complier mean if not incarcerated	.55	.60	.96	.86
B. Dependent Variable: Number of Charges				
Months 1–60 after decision	Baseline	Reweightd	Baseline	Reweightd
RF: judge stringency, all controls	–2.686 (3.134)	–2.304 (2.953)	–7.637** (3.167)	–8.448*** (3.046)
IV: incarcerated, all controls	–5.042 (5.983)	–4.280 (5.584)	–18.085** (8.452)	–19.688** (8.672)
Dependent mean	7.29	6.10	13.45	11.92
Complier mean if not incarcerated	3.61	5.16	24.01	21.97

NOTE.—Shown is the baseline sample of nonconfession criminal cases processed in 2005–9. Controls include all variables listed in table 1 plus controls for court × court entry year fixed effects. Standard errors are two-way clustered at the judge and defendant level. In cols. 2 and 4, we use propensity score reweighting to adjust for differences in observable characteristics across subsamples; see discussion of the reweighting procedure in sec. VI.A.

\*  $p < .1$ .  
\*\*  $p < .05$ .  
\*\*\*  $p < .01$ .

defendants experience a 30 percentage point increase in employment if incarcerated. This employment boost grows further to a nearly 40 percentage point increase within 5 years. Panel b of figure B7 decomposes the IV estimates into the potential employment rates of the compliers. This decomposition reveals that only 12% of the previously nonemployed compliers would have been employed if not incarcerated. By comparison, these compliers would experience a steady increase in employment if incarcerated, with more than 50% being employed by month 60. In panel A of figure 8, a different story emerges for the previously employed. They experience an immediate 25 percentage point drop in employment due to incarceration, and this effect continues out to 5 years.

We complement the employment results by examining the effects of incarceration on cumulative hours of work and earnings. Starting with the previously nonemployed defendants, in panel D of figure 8 we see a steady increase in the number of hours worked due to incarceration. The IV estimate increases modestly for the first 2 years and then starts

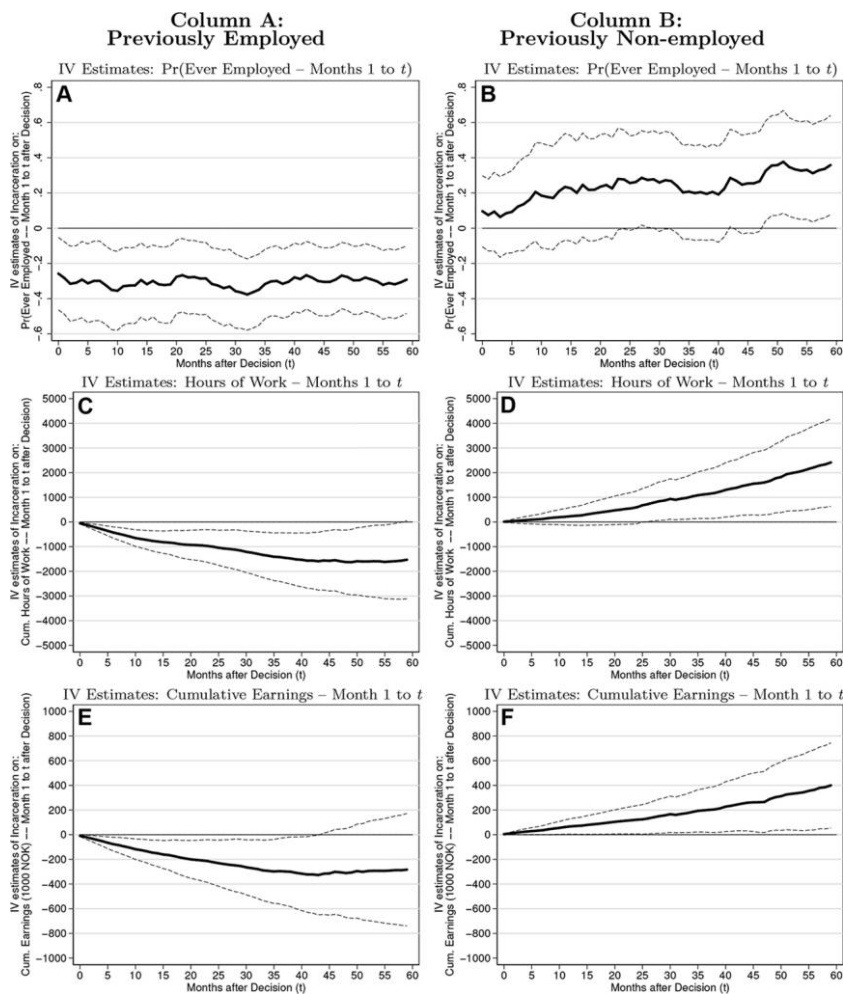


FIG. 8.—Effect of incarceration on employment by previous labor market attachment. Shown is the baseline sample consisting of nonconfession criminal cases processed in 2005–9 ( $N = 33,548$  at time of decision and  $N = 31,428$  in month 60 after decision). Dashed lines show 90% confidence intervals.

to increase at a faster rate. By month 60, incarceration increases labor supply by 2,700 hours per individual, translating into more than 550 additional hours per year on average. The decomposition by potential outcomes in panels d and f of figure B7 helps explain what is happening. If not incarcerated, few of the previously nonemployed compliers would have gotten a job. As a result of incarceration, they get a job and continue to accumulate hours over time.

Looking at previously employed individuals in panel A of figure 8, we see a different pattern. Incarceration has a negative effect on hours worked, consistent with the drop in employment observed for this group. Interestingly, the potential employment rate of the previously employed compliers is fairly similar to that of the previously nonemployed compliers if they are incarcerated (compare panels c and d in fig. B7). This suggests that incarceration can take an individual who previously had almost no attachment to the labor market and make them look like someone who also served prison time but was previously employed.

Panels E and F of figure 8 repeat the same exercise but this time for cumulative earnings. The general patterns found for employment and hours of work are mirrored in these figures, as are the decompositions based on potential outcomes in figure B6. Last, table 6 shows that differences in observable characteristics other than prior employment are not driving the contrasting labor market effects for previously employed versus previously nonemployed defendants.

One question is whether the positive cumulative effects found for the previously nonemployed reflects initial boosts in employment and hours associated with participation in a reentry program after release from prison or whether the employment effects are more long-lasting. To assess this, in panel A of table B13, we estimate year-by-year effects for employment outcomes in a given year. We find sizable and lasting effects of incarceration on future employment for the previously nonemployed. The table documents statistically significant increases in hours of work in years 2–5. If anything, the effect grows larger with each passing year, with estimates increasing from 115 more hours of work in year 1 to 734 more hours in year 5. Similar results are found for both ever employed and earnings outcomes, although the ever employed estimates are more imprecise. This suggests that we are estimating not simply the cumulative impact of a short-term effect but rather a persistent employment effect. These findings mirror what we see when looking at the intensive margins of the cumulative number of hours and earnings over time in panels D and F of figure 8.

A similar analysis is done for the previously employed in panel B of table B13. The year-by-year estimates for this group are generally negative, with the largest effect in the first year (consistent with them losing their job while in prison) and smaller effects by year 5 (suggesting that they start to recover by the end). The cumulative effects on hours and earnings in panels C and E of figure 7 are another way to illustrate these negative effects.

### *C. The Role of Job Loss and Job Training Programs*

The differences in labor market effects depending on prior employment are striking. For the previously employed group, the negative effects are

TABLE 6  
EFFECT OF INCARCERATION ON FUTURE EMPLOYMENT BY PREVIOUS  
LABOR MARKET ATTACHMENT

	SUBSAMPLE			
	Previously Employed ( <i>N</i> = 16,547)		Previously Nonemployed ( <i>N</i> = 14,881)	
	(1)	(2)	(3)	(4)
A. Dependent Variable: Pr(Ever Employed)				
Months 1–60 after decision	Baseline	Reweightd	Baseline	Reweightd
RF: judge stringency, all controls	–.155*** (.058)	–.176*** (.063)	.151** (.064)	.210*** (.070)
IV: incarcerated, all controls	–.292** (.115)	–.327** (.127)	.358** (.168)	.490** (.199)
Dependent mean	.70	.72	.43	.44
Complier mean if not incarcerated	.82	.75	.13	.13
B. Dependent Variable: Cumulative Hours of Work				
Months 1–60 after decision	Baseline	Reweightd	Baseline	Reweightd
RF: judge stringency, all controls	–815.9 (507.3)	–1,075.8* (554.1)	1,019.1*** (364.9)	1,385.9*** (415.6)
IV: incarcerated, all controls	–1,531.7 (948.1)	–1,998.2* (1,048.1)	2,413.1** (1,060.7)	3,230.0** (1,289.9)
Dependent mean	3,804.2	4,063.7	1,514.3	1,613.8
Complier mean if not incarcerated	4,410.7	3,838.3	51.4	47.8
C. Dependent Variable: Cumulative Earnings				
Months 1–60 after decision	Baseline	Reweightd	Baseline	Reweightd
RF: judge stringency, all controls	–151.0 (146.0)	–206.1 (172.0)	168.9** (73.7)	254.2** (92.6)
IV: incarcerated, all controls	–283.5 (272.6)	–382.9 (319.9)	399.9* (206.1)	592.4** (272.7)
Dependent mean	834.3	920.4	255.7	279.0
Complier mean if not incarcerated	914.2	788.3	9.95	9.70

NOTE.—Shown is the baseline sample of nonconfession criminal cases processed in 2005–9. Controls include all variables listed in table 1 plus controls for court  $\times$  court entry year fixed effects. Standard errors are two-way clustered at the judge and defendant level. In cols. 2 and 4, we use propensity score reweighting to adjust for differences in observable characteristics across subsamples; see discussion of reweighting in sec. VI.A. Cumulative earnings are reported in NOK 1,000.

\*  $p < .1$ .  
 \*\*  $p < .05$ .  
 \*\*\*  $p < .01$ .

perhaps not unexpected, as these individuals had an actual job to lose by going to prison. To test whether job loss is the explanation, we take advantage of the fact that we can link firms to workers in our data. In particular, we follow the previously employed defendants from years 2–5



after their court case (after virtually all incarcerated individuals should be out of prison) and track whether their first employment, if any, during this period was with the same firm as they worked in before the court case. We then run two new IV regressions, quantifying the effect of incarceration on (1) the probability of being employed at a new firm and (2) the chance of being employed at the previous firm. Column 1 in table B14 shows the overall employment effect in any firm, which is a 29 percentage point drop due to incarceration. As shown in columns 2 and 3, the drop in employment is almost entirely due to a reduction in the likelihood of employment at the previous firm, whereas there is only a small and statistically insignificant effect of incarceration on the probability of employment at a new firm.

Individuals who were not working prior to incarceration had no job to lose. However, serving time in prison could give access to educational and job training programs both while in prison and immediately after. We collected individual-level data on participation in a variety of job training and classroom training programs. The most common job training program is on-the-job training in a regular or sheltered workplace, where the employer receives a temporary subsidy (normally up to 1 year) to train the individual and expose them to different jobs. Job training is specifically targeted to those who need work experience in order to find employment. It is often paired with job finding assistance, where a personal counselor helps the individual find a suitable workplace and negotiate wages and employment conditions. The classroom training programs include short skill-focused courses, vocational training, and ordinary education. Classroom training is limited to 10 months for skill courses, 2 years for vocational training, and 3 years for ordinary education. Thirty-three percent of the previously nonemployed participate in job training, and 25% participate in classroom training. In comparison, among the previously employed, 25% participate in job training and 25% in classroom training.

Table 7 reports IV estimates for both types of training using our judge stringency instrument. We focus on the first 2 years after the court decision so as to capture the training while in prison and immediately after. For the previously employed group, there are hints that participation in both job and classroom training programs increases because of incarceration but nothing that is statistically significant. For the previously nonemployed group, there is likewise no statistically significant evidence for an increase in classroom training, although the estimate is positive. Instead, what changes significantly as a result of incarceration is the probability that previously nonemployed defendants participate in job training programs. We estimate that being incarcerated makes these individuals 35 percentage points more likely to attend a job training program. By comparison, few if any of the previously nonemployed compliers would have participated in job training programs if not incarcerated.

TABLE 7  
EFFECT OF INCARCERATION ON PARTICIPATION IN JOB TRAINING PROGRAMS AND CLASSROOM  
TRAINING PROGRAMS (Months 1–24 after Decision)

	SUBSAMPLE			
	Previously Employed (N = 16,547)		Previously Nonemployed (N = 14,881)	
	(1)	(2)	(3)	(4)
Dependent variable	Pr(participated in job training programs)	Pr(participated in classroom training programs)	Pr(participated in job training programs)	Pr(participated in classroom training programs)
RF: judge stringency, all controls	.056 (.063)	.073 (.065)	.147** (.063)	.054 (.067)
IV: incarcerated, all controls	.106 (.118)	.138 (.122)	.348** (.168)	.127 (.164)
Dependent mean	.17	.19	.22	.17
Complier mean if not incarcerated	.16	.18	.00	.04

NOTE.—Shown is the baseline sample of nonconfession criminal cases processed in 2005–9. Control variables include all variables listed in table 1 plus controls for court × court entry year fixed effects. Standard errors are two-way clustered at the judge and defendant level.

\*\*  $p < .05$ .

D. *Putting the Pieces Together*

So far, we have demonstrated that the decline in crime from incarceration is driven by individuals who were not working prior to incarceration. Among these individuals, imprisonment increases participation in programs directed at improving employability and reducing recidivism and ultimately raises employment and earnings while discouraging further criminal behavior. A natural question is whether the people who, because of incarceration, commit fewer crimes are the same individuals as those who become more likely to participate in job training programs and work more. Or does the decline in crime occur independently of the increase in program participation and employment? We investigate this question in table B15. In columns 2 and 3, we first break up the probability of reoffending into the probability of reoffending and employed plus the probability of reoffending and not employed. Using the IV model, we report estimates for how each of these joint probabilities are affected by incarceration. As shown in column 2, there is little change in the joint probability of reoffending and employment due to incarceration. Instead, the entire drop in recidivism appears to be driven by a reduction in the joint probability of reoffending and not employed. The only conclusion

consistent with all our estimates is that individuals who are induced to start working are the same individuals who stop committing crimes.<sup>41</sup>

Going a step further, in columns 4 and 5, we estimate the joint probability of reoffending, employment, and job training. We find that the entire drop in recidivism reported in column 1 is due to a reduction in the joint probability of being charged, not employed, and not participating in a job training program. We therefore conclude that the drop in crime we find for the previously nonemployed is driven by the same individuals who, because of incarceration, participate in job training and become gainfully employed.

## VII. Implications for Cost-Benefit Calculations

A natural question is whether the positive effects from imprisonment found in Norway pass a cost-benefit test. It is difficult to estimate the benefits of crime reduction and the costs of imprisonment, with researchers making strong assumptions and extrapolations to do so (see McCollister, French, and Fang 2010; Garcia et al. 2017). With this caveat in mind, we attempt a simple cost-benefit comparison. Our rough calculations suggest the high rehabilitation expenditures in Norway are more than offset by the corresponding benefits to society.

To calculate the costs of incarceration reported in table B16, we first compute the direct daily cost per prisoner of incarceration. To do this, we take the total prison spending reported by the Norwegian Correctional Services divided by the total number of prison days served across all prisoners in 2013. This gives a direct prison cost of \$323.50 per day. We then create an outcome variable that multiplies the number of days spent in prison for an individual's current court case by \$323.50. The IV estimate that uses this outcome measure yields a cost of \$60,515 per incarceration sentence. We note that this measure captures the average cost of incarceration, even though ideally one would like to use the marginal cost of incarceration.

On the benefit side, there are three broad categories. First, there is a reduction in criminal justice system expenditures due to fewer crimes being committed. Following the approach that McCollister, French, and

<sup>41</sup> To see this, let  $C$  denote crime,  $E$  denote employment, and  $I$  denote incarceration. By definition,  $P(C) = P(C \cap E) + P(C \cap \text{not } E)$ . We estimate that  $dP(C)/dI < 0$  is driven by  $dP(C \cap \text{not } E)/dI < 0$ , since  $dP(C \cap E)/dI \approx 0$ . Notice that  $dP(C \cap \text{not } E)/dI < 0$  means that some individuals with  $C = 1, E = 0$  if  $I = 0$  change behavior if  $I = 1$ . There are three possibilities for change: (1)  $C = 0, E = 0$ ; (2)  $C = 1, E = 1$ ; and (3)  $C = 0, E = 1$ . However, type 1 is inconsistent with  $dP(E)/dI > 0$ , and type 2 is inconsistent with  $dP(C)/dI < 0$ . Only type 3 is consistent with  $dP(E)/dI > 0$ ,  $dP(C)/dI < 0$ , and  $dP(C \cap \text{not } E)/dI < 0$ . Note that this is an argument about net effects; while there may be some of types 1 and 2, they would have to be offset by even more of type 3.

Fang (2010) used for the United States, we calculate police savings per crime avoided as total operating costs reported by the Norwegian Police Service divided by the total number of reported crimes. Police savings are computed to be \$3,670 per reported crime. Likewise, we calculate court savings as total operating costs reported by the Norwegian Courts, scaled by the fraction of criminal cases in the courts, and then divided by the total number of criminal cases processed in 2013. Court savings are computed to be \$2,533 per court case. We then create an outcome variable that takes the total number of crimes committed by an individual multiplied by \$3,670 plus the total number of future court cases for an individual multiplied by \$2,533. Using this combined criminal justice cost as the outcome variable and our IV setup, we estimate a savings of \$71,225 per incarceration sentence.

The second category of benefits is due to increased employment, which results in higher taxes paid and lower transfer payments. We estimate the increase in taxes minus transfers to be \$67,086 per incarceration sentence using IV, although we note that this estimate is noisy. Net transfers include all cash transfers received minus all income taxes paid over the 5-year period following the court decision. Either of these first two benefit categories would justify the direct costs of prisons. Note that our calculations cover only the 5 years after the court decision; any benefits in the future would further add to the benefits (see Garcia et al. 2017).

The third benefit category is the reduction in victimization costs due to fewer crimes being committed in the future. Victimization costs are notoriously difficult to estimate, so we instead simply note that this category would make the comparison of benefits versus costs even more favorable. Of course, the importance of this category depends on whether the avoided crimes are serious from a welfare perspective. We lack the power to precisely estimate the decrease in the number of crimes for all crime types. With this caveat in mind, we find that roughly 40% of the overall reduction is due to drops in property crime, with 4.3 fewer property crimes (standard error is 2.1), and roughly 20% is due to fewer traffic violations (estimate is  $-2.4$ , standard error is 1.2). The remaining decrease is spread across other crime types, such as violent crimes, drug crimes, and drunk driving, but these estimates are not statistically significant.

### VIII. Concluding Remarks

A pivotal point for prison policy was the 1974 Martinson report, which concluded that nothing works in rehabilitating prisoners. Around this time, incarceration rates started to rise dramatically, especially in the United States, where they more than tripled as an increasing emphasis was placed on punishment and incapacitation. In recent years, researchers and policy

makers have questioned whether incarceration is necessarily criminogenic or whether it can instead be preventive. Our study serves as a proof of concept demonstrating that time spent in prison with a focus on rehabilitation can indeed be preventive. The Norwegian prison system is successful in increasing participation in job training programs, encouraging employment, and discouraging crime largely as a result of changes in the behavior of individuals who were not working prior to incarceration.

While this paper establishes an important proof of concept, several important questions remain for future research. Our results do not imply that prison is necessarily cost effective or preventative in all settings. Evidence from other countries and populations would be useful to assess the generalizability of our findings. Moreover, while we provide some evidence that job training and employment are part of the story, it would be interesting to quantify their effects more precisely as well as to analyze other possible mechanisms, such as sentence lengths, prison conditions, drug treatment programs, and postrelease support. Additional research along these lines will aid policy makers as they tackle the challenging task of prison reform.

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