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In contexts where incarceration conditions are particularly favorable, how does a custodial sentence impact the life of individuals in terms of later crime and labor market outcomes compared to a non-custodial one? We answer this question by taking advantage of a Danish reform whereby most offenders tried for a drunk-driving crime were placed on probation rather than sentenced to incarceration. Our first key finding is that stakeholders anticipated the consequences of the reform: around the time of the reform, the number of cases tried dropped and the nature of the cases changed significantly. To measure the relative impact of incarceration, we therefore resort to a novel instrumental variable approach exploiting quasi-exogenous variation in the probability of being tried after the reform, and therefore incarcerated, based on the crime date. We find that incarcerated offenders later commit more crimes and have weaker ties to the labor market than those placed on probation. Our results suggest that the criminogenic effect of incarceration is driven by its negative impact on offenders' labor market attachment, which itself appears to be particularly affected by the stigma attached to having a criminal record.

JEL Classification: K14, K42, J24

Keywords: crime, employment, incarceration, Recidivism

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Abstract

In contexts where incarceration conditions are particularly favorable, how does a custodial sentence impact the life of individuals in terms of later crime and labor market outcomes compared to a non-custodial one? We answer this question by taking advantage of a Danish reform whereby most offenders tried for a drunk-driving crime were placed on probation rather than sentenced to incarceration. Our first key finding is that stakeholders anticipated the consequences of the reform: around the time of the reform, the number of cases tried dropped and the nature of the cases changed significantly. To measure the relative impact of incarceration, we therefore resort to a novel instrumental variable approach exploiting quasi-exogenous variation in the probability of being tried after the reform, and therefore incarcerated, based on the crime date. We find that incarcerated offenders later commit more crimes and have weaker ties to the labor market than those placed on probation. Our results suggest that the criminogenic effect of incarceration is driven by its negative impact on offenders' labor market attachment, which itself appears to be particularly affected by the stigma attached to having a criminal record.

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1. Introduction

Although the world's prison population has grown steadily over the past four decades to reach more than 10.74 million people (Walmsley, 2018), the effectiveness of incarceration is increasingly being questioned. In particular, while one of its main objectives is to deter criminals from reoffending (the so-called *specific* deterrent effect), available evidence suggests that custodial sentences are less effective in preventing recidivism than non-custodial sentences (see Nagin et al. (2009) and Chalfin and McCrary (2017) for reviews). This is particularly the case for probation and electronic monitoring, which have been increasingly used as an alternative to incarceration in the OECD since the early 2000s. However, most of the existing literature is based on the US and robust evidence remains very limited in settings where the prison population is smaller, prison conditions are better, and rehabilitation programs play a greater role in prisons.[1]

Our paper contributes to filling this gap by providing robust evidence on the relative impact of custodial and non-custodial sentences in Denmark, a Scandinavian country where incarceration conditions are particularly good (Lappi-Seppälä, 2007; Pratt, 2008; Pratt and Eriksson, 2011; and Ward et al., 2013). To do so, we take advantage of a large-scale reform of the Danish legislation implemented in 2000, whereby incarceration was replaced by a probation period for drunk-driving crimes.[2] The probation period was combined with a fine and community service or mandatory participation in a rehabilitation program. While drunk driving may be committed by a specific type of offender, it is a very common offense (accounting for more than one million arrests each year in the US) and, in countries where it is liable to imprisonment, drunk driving is often one of the most frequent charges resulting in custodial sentences. In Denmark, for example, it accounted for a quarter of all custodial sentences promulgated in 1999. In addition, it is interesting to note that the framework of our study allows us to measure the impact of incarceration on offenders who did not have a strong alcohol abuse problem, thus reducing strong external validity concerns arising from the specificities

[1] The US is a very special case within the OECD. For instance, its prison population rate is the highest in the world, with 622 per 100,000 (the average world prison population rate is around 145 per 100,000). By contrast, it is much lower in European countries, with rates at 100 in metropolitan France, 75 in Germany, 63 in Norway, 63 in Denmark, and 59 in Sweden (Walmsley, 2018).

[2] Drunk driving is an important public health issue in most countries. For instance, throughout the world, drunk driving is believed to account for more than 273,000 deaths every year (Vissers, 2017). In the European Union and United States, alcohol is estimated to have caused 25 to 30% of all road fatalities in 2015 (European Commission, 2015; NHTSA, 2017) – representing around 6,400 and 10,265 fatalities respectively. In consequence, drunk driving also represents a significant cost for most countries. For instance, in the United States, the economic cost of all alcohol-impaired accidents was estimated at 44 billion dollars for the sole year of 2010 (NHTSA, 2017).

of drunk drivers. *In fine*, the results of our study can be interpreted as evidence of the relative impact of custodial versus non-custodial sentences on a large group of relatively mild offenders.

Analyzing how the reform was implemented, we show that stakeholders (defendants and courts of justice) anticipated the reform and modified their behavior in the weeks *preceding* its entering into force. As a consequence, significant selection occurred in the nature of the offenders tried before and after the reform. In practice, these anticipations materialized through a sharp drop in the number of cases tried from the moment the law was signed (but before it actually entered into force), in line with stakeholders' incentives to postpone drunk-driving cases until after the reform: defendants to avoid incarceration and courts of justice to reduce the number of cases that might have to be retried. Furthermore, we show that individuals who had their case postponed until after the reform were not selected at random, but had rather specific characteristics. In particular, wealthier defendants were more likely to have their trial delayed, thus avoiding prison. Consequently, our identification strategy does not rely on the comparison of individuals tried just before and after the reform.[3]

Instead, we propose a novel instrumental variable approach which consists in comparing offenders arrested at different dates *before* the reform was signed. Indeed, we show that the time lapse between offenders' date of crime and the date when the reform entered into force generated exogenous variation in the probability of their being incarcerated. Our approach is based on the following observations. First, variation in the date of crime quite logically generated variation in the date of trial, which on average took place 6 months later: the closer to the reform a crime was committed, the more likely the defendant was to be tried after the reform. Second, a key feature of the Danish legislation guarantees that individuals tried after a reform for a crime committed prior to it must be tried under the most lenient of the two laws. Hence, defendants tried after the reform benefitted from the new law and were therefore placed on probation. As a consequence, the closer to the reform a crime was committed, the more likely a defendant was to be tried after the reform under the new law and placed on probation instead of being incarcerated.[4]

[3] In contrast, earlier studies of this reform (Andersen, 2015; Andersen, 2016; Wildeman and Andersen, 2017) have relied on comparisons between offenders tried before and after the reform. As a result, some of our point estimates differ significantly from theirs in terms of sign and size.

[4] Our approach is loosely related to the one used in Drago et al. (2009). In their study, they use the Collective Clemency Bill passed by the Italian Parliament in July 2006 to measure the impact of suspended sentence length on recidivism. This reform reduced the length of the prison sentence of all inmates who had committed a crime before May 2, 2006. As a consequence, about 40 percent of the Italian prison population were released from prison on August 1, 2006 under the condition that they would have to serve the remaining of their sentence if they were to commit another crime in the 5-year period following their release. In this setting, the length of offenders' suspended sentence varied depending on their prison entry date, which the researchers argued is exogenous and which they used to measure the impact of suspended sentence length on recidivism.

We find that, relative to non-custodial sentences, custodial ones significantly increase offenders' involvement in subsequent criminal activities. While we do not find any impact on the number of subsequent drunk-driving crimes, incarceration significantly increases the average number of other crimes. After 8 years, incarceration increases the average number of other convictions by 0.630 – representing a 40.3% increase at the sample mean. However, we do not observe any significant effect on the probability of an offender committing another crime after being released, whether it be a drunk-driving crime or any other crime. This indicates that although incarceration does not increase the number of reoffenders, it intensifies their subsequent criminal activities. Overall, these results complement previous papers revealing the criminogenic effects of custodial sentences on other types of offenders (Cullen et al., 2011; Aizer and Doyle, 2015). These effects may therefore apply to a broader range of offenders, including those who exhibit a relatively low proclivity for criminal behaviors, such as drunk drivers.

We also find that custodial sentences increase the difficulties faced by offenders in the labor market as compared to non-custodial ones. First, the increase in the number of convictions is driven by a rise in the number of economically motivated crimes. Second, our subsequent analysis of offenders' post-sentencing labor market attachment indicates that incarceration significantly reduces offenders' probability of having a job, increases their reliance on unemployment-related benefits, and, *in fine*, reduces their income. After 10 years, incarceration represents a cumulative loss of 368,056 kroner – corresponding to a 15.2% decrease at the mean, or one and a half years' worth of income. A close examination of the timing of the effects on labor market outcomes supports the idea that individuals with a criminal record are subject to stigma that hinders their employment opportunities after release – in line with evidence found in Pager (2003), Raphael (2014), Agan and Starr (2018), and Mueller-Smith and Schnepel (2020). All in all, the evidence suggests that labor market difficulties play a particularly important role in explaining the observed increase in criminal activity.

Our findings contribute to the literature studying the impact of incarceration (see Nagin et al. (2009) and Chalfin and McCrary (2017) for reviews).[5] In particular, it complements existing studies in the Scandinavian context, which in recent years have relied on the random assignment of criminal cases to judges of varying degrees of severity to measure the causal impact of incarceration, with very

[5] A separate but related part of the literature focuses on whether increasing the length of incarceration has positive effects on offenders' post-release outcomes (*e.g.* by allowing them to take better advantage of rehabilitation programs) or negative ones (*e.g.* by worsening some of the effects of incarceration on their education, labor-market participation, health, etc.). This group of studies has yielded somewhat more mixed results (see Chalfin and McCrary (2017) for a review on the subject).

contrasting results (Dobbie et al., 2018; Bhuller et al., 2020; Michel et al., 2020).[6] While these studies identify its impact on very specific subsamples of defendants at the margin of being incarcerated, our paper focuses on a broader range of offender profiles arrested for a very common crime, *i.e.* drunk driving. Our results also stress that, even in a country like Denmark, incarceration can foster crime and have highly detrimental effects on offenders' subsequent labor market attachment. Moreover, we show that the cost of incarceration can extend beyond the period of incarceration and the first few years following release from prison. Finally, with results very close to those found in the US, our study questions the beneficial impact of incarceration in societies offering more advantageous incarceration conditions, highlighted by studies such as Bhuller et al. (2020). In this respect, our study also complements the literature on the impact of prison conditions, which has contrasting results (Katz et al., 2003; Bedard and Helland, 2004; Chen and Shapiro, 2007; Drago et al., 2011).

This study also adds to the growing literature documenting sources of dysfunction in justice systems (Vidmar, 2011; Danziger et al., 2011; Abrams et al., 2012; Anwar et al., 2012; Anwar et al., 2014; Philippe and Ouss, 2018; Cohen and Yang, 2019), as we provide evidence questioning the degree of fairness with which cases can be handled in times of legislative changes. Incidentally, these findings also suggest that traditional quasi-experimental estimators should be used with caution in similar contexts where salient contextual changes (such as a legislative reform, a program scale-up, etc.) can be anticipated by their stakeholders.

The rest of the paper is organized as follows: in section 2, we provide contextual information and describe the reform under study; in section 3, we highlight the selection that occurred in the characteristics of offenders tried around the time of the reform; in section 4, we discuss our empirical strategy; in section 5, we present our results; finally, section 6 concludes.

[6] A couple of other studies comparing the effects of electronic monitoring and custodial sentences in a non-US setting are Di Tella and Schargrodsky (2013) and Henneguelle et al. (2016), which look at their relative effect in Argentina and France respectively. However, prison conditions in these countries remain considerably worse than those found in the Nordic countries.

2. The legislative change

2.1. Context prior to the reform

In the last quarter of the 20th century, legislations on drunk-driving crimes were gradually hardened throughout the world in an attempt to reduce the number of road fatalities. As a result, drunk driving has become a very common crime[7] and, in some countries, a major source of custodial sentences.

In Denmark, individuals arrested for drunk driving have been facing a prison sentence since the establishment of a first administrative blood alcohol threshold in 1976, lowered from 0.8 to 0.5g/L in 1998. These changes led to a sharp increase in the number of custodial sentences promulgated for drunk driving: in 1999, drunk-driving was the main reason for receiving a custodial sentence, accounting for 24.82% of them. As displayed in *Table 1*, 71.9% of the defendants tried for a drunk-driving crime during that year received a custodial sentence.[8] Although the severity of the sentence varied depending on the characteristics of the offense (*e.g.* driver's level of impairment and existence of aggravating circumstances) and the number of prior drunk-driving convictions, the length of the prison sentence remained relatively short: in 95.3% of cases, it remained below 60 days. Moreover, while fines and suspensions of the driving license were also frequently imposed on defendants – in 27.8% and 30.8% of trials respectively – probation and community service were rarely used. Finally, it should be noted that because drivers are charged with impaired driving based on the results of blood tests that are provided to the court, only 1.3% of those charged were acquitted.

For what follows, it is important to note that offenders suffering from an alcohol abuse problem who received a prison sentence of no more than 60 days could then ask to benefit from a *pardon scheme* described in *Appendix A.1*. As part of this scheme, their custodial sentence could be commuted to a non-custodial sentence, including a two-year probation period and mandatory participation in a yearlong rehabilitation program. This explains why the share of offenders who were actually incarcerated (37.8%) is lower than the share of those who received a custodial sentence (71.9%).[9]

Offenders who served their prison sentences benefited from particularly advantageous conditions of incarceration, which according to the principles of the Danish prison system are meant to support the

[7] For instance, more than 1 million drivers were arrested for driving under the influence of alcohol or narcotics in the US in 2019 as per the Federal Bureau of Investigation's Uniform Crime Reporting Program.
[8] In Denmark, all individuals facing a prison sentence are tried in a court of justice.
[9] Our variable indicating whether or not an individual was incarcerated is a dummy variable capturing whether an individual has spent at least 10 days in prison – 10 days being the minimum duration of prison sentences requested for a drunk-driving crime.

principle of *normalization*, officially introduced in the early 1970s: life in prison should reflect life on the outside as much as possible so as to prepare prisoners for their release and reintegration into society. Among other things, this meant that offenders had the right to leave prison grounds during the day to go to work. To avoid detrimental association, drunk drivers (as well as other minor offenders) were kept separate from more serious criminals.

2.2. Details of the new law

In 2000, a reform was passed introducing cheaper, more lenient sentences against drunk drivers. As part of it, custodial sentences of no more than 60 days were replaced by a two-year probation period and a fine, combined with either community service or participation in a yearlong rehabilitation program (identical in every way to the one offered as part of the pardon scheme just mentioned above and described in *Appendix A.1*).[10] The reform systematically applied to all offenders except those who had already been placed on probation for a drunk-driving crime more than once or were on probation at the time of the crime for an alcohol-related crime. Following the reform, the average cost per offender decreased from 15.800 DKK (the cost of a custodial sentence) to 8.300 DKK (the cost of a non-custodial sentence) (Nielsen and Kyvsgaard, 2007).[11]

The choice between community service or participation in a rehabilitation program was left to the judges based on whether or not the offender suffered from an alcohol abuse problem, the rehabilitation program being reserved for offenders exhibiting such a problem.[12] As part of this program, offenders had to take a drug causing acute sensitivity to ethanol and to participate in an alcohol treatment program.[13] Offenders were monitored throughout the duration of the treatment and the rest of the probation period.[14] Probation officers were in charge of ensuring that the terms of the probation were being respected and, in particular, of controlling offenders' drug intake and participation in the alcohol treatment program during the first phase of the scheme. Community service was to be requested against offenders who did not exhibit such an alcohol abuse problem and

[10] Generally speaking, offenders placed on probation see their prison sentence suspended on the condition that they do not reoffend and that they comply with any conditions that may be imposed. In case of mild violation(s) of the probation terms, the Prison and Probation Service decides whether or not to enforce the custodial sentence. In case of more serious violation(s), judges are responsible for making the most appropriate decision.

[11] The cost of a non-custodial sentence includes the costs associated with offender supervision and the rehabilitation program.

[12] The only difference with the rehabilitation program implemented after the reform is that, until the 2000 reform, drunk drivers had to apply to the Danish Prison and Probation Service to benefit from the pardon scheme. After the 2000 reform, it was left to the judge to decide whether or not an offender should enroll in the rehabilitation program.

[13] In practice, this program could take a variety of forms (ranging from group sessions at a clinic to individual meetings with general practitioners) and could vary in intensity depending on the individual's location, needs, and motivation (Nielsen and Kyvsgaard, 2007).

[14] During the first two months of the two-year program, offenders would usually meet with their probation officers every 2 weeks, but only once a month thereafter – unless arguments in favor of a more intensive monitoring prevailed.

was substituted to the former sentences at the following rate: 30 hours for 10 to 14 days of imprisonment, 40 hours for 20-30 days, and 60 hours for 40 to 50 days.

As displayed in *Table 1*, the share of offenders who received a custodial sentence dropped significantly after the reform, as intended: it fell from 71.9% in 1999 to 14.2% in 2001. Similarly, the share of offenders who were actually incarcerated decreased from 37.8% to 13.8%. In contrast, the share of offenders who were placed on probation rose from 0.7% to 59.0%. As the reform did not change the sanction incurred by offenders facing no prison sentence, or by those facing more than 60 days of imprisonment (who kept on serving their prison sentence after the reform), the overall share of offenders who received a prison sentence (whether it be a conditional or an unconditional one) and the share of acquitted individuals remained similar before and after the reform. As expected, community work and fines were also imposed on a greater share of offenders after the reform. The use of driving license suspension was not impacted by the reform and is similar before and after it.

As documented in the next section, the reform was perceived by offenders as a softening of the legislation – which is important to note for the interpretation of the results. Indeed, while the incarceration conditions in Scandinavian prisons are considered to be quite exceptional by American and European standards (Lappi-Seppälä, 2007; Pratt, 2008; Pratt and Eriksson, 2011; Ward et al., 2013), it is worth stressing that inmates remain subject to important freedom restrictions and other usual discomforts associated with imprisonment. Also, probation sentences remain on an individual's criminal record for 3 years from the date of conviction, while custodial sentences remain on the criminal record for 5 years from the date of release from prison.

As is often the case with important reforms that require a certain level of preparation, a few months elapsed between the moment the law was signed and the moment it entered into force. While the law was signed by Parliament on April 4th, 2000, it only entered into force on July 1st, 2000 (referred to as the *date of the reform* hereafter).

3. Implementation of the reform

In order to determine how to measure the impact of the reform, let us start by studying its implementation.

3.1. Anticipation

First, we show that the way in which drunk-driving cases were handled in district courts changed drastically in the months preceding the entering into force of the reform, providing evidence that stakeholders (defendants, courts of justice, or both) reacted in anticipation of the reform.

To show this, we use administrative data containing information on the universe of drunk-driving crimes committed and tried around the time of the reform to describe how the reform was implemented. *Figure 1* shows the evolution of the following four indicators between 1999 and 2001: a) the number of alleged drunk-driving crimes resulting in a trial committed every week; b) the number of drunk-driving cases tried every week in district courts; c) the share of defendants tried for drunk driving who received a custodial sentence by week of trial; d) the share of defendants tried for drunk driving who were actually incarcerated by week of trial. For each year, two dotted vertical lines are drawn to mark week 14 (the week when the law was signed in 2000) and week 26 (the week when it entered into force in 2000). The only reform implemented during these three years is the one studied in this paper, which occurred in 2000. For the years 1999 and 2001, vertical lines are only drawn for comparison purposes.[15]

Strikingly, the evolution of these indicators reveals that the way in which drunk-driving cases were handled in district courts changed drastically in the three-month transition period following the signature of the reform and preceding its entering into force. Indeed, the number of cases tried each week dropped significantly from 91.7 cases on average in the three weeks preceding the signing of the law to 28.0 cases on average during the transition period – representing a 69.5% decrease (*Figure 1.b*).[16] This is the case despite the fact that there was no similar variation in the number of alleged crimes resulting in a trial committed in the preceding months (see figure in *Appendix A.3*) or in the number of cases tried during the same period in adjacent years, 1999 and 2001 (*Figure 1.a*).[17] This suggests that stakeholders (courts of justice and/or defendants) anticipated the change in legislation

[15] For data confidentiality reasons, indicators c) and d) displayed in *Figure 1* are calculated as moving averages. For each week *w*, the value of these indicators is calculated as the average value of the indicators over weeks *w-1*, *w*, and *w+1*.
[16] The number of cases tried in the week following July 1st is low for all three years. This is a result of judges' summer vacation period, during which the number of cases tried in district courts goes down substantially.
[17] In *Appendix A.3*, we also show that the reform did not have any impact either on the number of individuals charged for a drunk-driving crime, which remained relatively constant prior to the reform, increased right after the signing of the reform, and progressively returned to its pre-reform level.

and that, as a consequence, a large share of trials were postponed until after the reform. Essentially, this means that a group of offenders who should have been tried before the reform was tried after.[18]

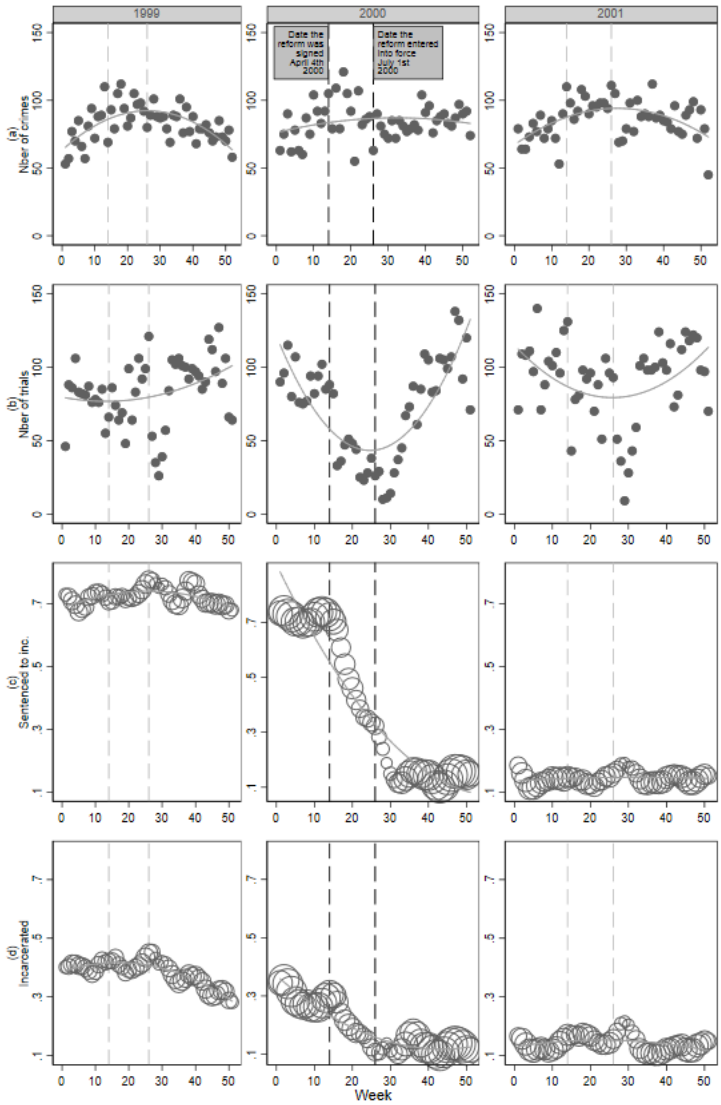

Fig. 1: Implementation of the drunk-driving legislation reform: The consequences of the reform are depicted here through the evolution of the following four indicators around the time of the change in legislation: a) the number of drunk-driving crimes resulting in a trial committed every week; b) the number of drunk-driving cases tried every week in district courts; c) the share of defendants tried for drunk driving who received a custodial sentence by week of trial; d) the share of defendants tried for drunk driving who were actually incarcerated by week of trial. For each year, the first dotted vertical line marks the week when the law was signed (week 14) and the second one marks the week when it came into force (week 26).

[18] In total, assuming that the same number of drunk-driving cases were tried between weeks 14 and 26 in 1999, 2000, and 2001, we estimate that roughly 48.1% of the drunk-driving cases which should have been tried during the transition period were in fact postponed until after the reform. In order to reach this figure, we assume that in the absence of the reform, the number of drunk-driving cases tried in 2000 would have been equal to the average number of such cases tried in the same weeks in 1999 (1,071) and 2001 (1,123) – 1,097. However, only 569 drunk-driving cases were tried during the transition period in 2000, suggesting that around 528 were postponed – which represents 48.1% of what would have been the total number of drunk-driving cases tried during that period.

The share of drunk drivers who received a custodial sentence (*Figure 1.c*) and the share of those who were actually incarcerated (*Figure 1.d*) also decreased substantially from the moment the bill was signed. This time, the decline did not take the form of a sharp discontinuity but rather of a linear decrease, suggesting that judges gradually began to implement the reform before it came into effect.[19] This also suggests some inequalities between defendants based on their trial date, since the likelihood of receiving a custodial sentence decreases progressively as the trial date gets closer to the date the reform entered into force. Overall, the share of defendants sentenced to custody decreased gradually from around 73.6% on average in the three weeks preceding the signing of the law to 34.5% on average in the three weeks preceding the date of the reform – representing a 53.1% drop. The evolution of the share of offenders actually incarcerated exhibits a similar pattern.[20]

While it is not possible to pin down the exact underlying mechanisms at play here, a closer look at the stakeholders' incentives suggests that both defendants and judges had good reasons for wanting drunk-driving cases to be postponed until after the reform: the former to avoid prison, the latter to reduce the number of cases which might have to be retried. Indeed, an important feature of Danish legislation guarantees that defendants tried after a reform for a crime committed prior to it must be tried under the more lenient of the two laws. In the context of the reform at hand, this means that individuals tried for a crime committed prior to the reform faced the risk of being incarcerated if tried before the reform, while they merely faced the risk of being placed on probation if tried after. The same feature also guarantees that defendants tried prior to the passing of a law lowering the sanction for the crime they were convicted of may request that their case be retried if they are still in prison when the reform enters into force.[21]

[19] If the decrease in the share of defendants sentenced to incarceration simply reflected a compositional effect related to the decrease in the number of cases observed in *Figure 1.b*, then the decrease would have followed a sharp decline similar to that shown in *Figure 1.b* rather than the linear decrease actually observed.

[20] This is the case despite the fact that the Danish Parliament had adopted a special regime to limit the number of offenders who would begin serving prison sentences for drunk driving during the transition period. With this special regime, the parliament hoped to avoid unequal treatment of offenders. However, *Figure 1* clearly shows that some offenders were incarcerated during the transition period for a drunk driving offence while others were not. More information on this special regime can be found here (in Danish): https://www.retsinformation.dk/eli/mt/2000/133 (accessed in November 2020).

It is also interesting to note that this share started to decrease a year before the date of the reform. A possible explanation lies in the waiting list system adopted in Denmark after a sharp rise in the number of individuals who received a custodial sentence. As a consequence, not all offenders served their prison sentence immediately after their trial.

[21] The two mechanisms may have reinforced each other. Firstly, judges may have been more willing to accede to requests from defendants to postpone their trial during the transition period. Secondly, by internalizing the judges' incentives, defendants may have been more inclined to request a postponement of their trial if initially scheduled during the transition period.

3.2. Selection

Going further, we investigate the characteristics of the defendants whose trial did take place during the transition period, and find evidence suggesting that the identity of the individuals who had their case postponed was not random.

To show this, we compare changes in the characteristics of the defendants tried in each quarter between 1999 and 2000. More specifically, focusing on individuals tried between January 1st, 1999 and December 31st, 2000, we regress different variables indicative of their criminal priors and labor market attachment (y_i) on a constant, a year dummy indicating whether a case was tried in 2000 (Y_i^{2000}), quarter fixed effects (Q_i^j), the interactions between the year dummy and the quarter fixed effects, and a time trend (T_i):

$$y_i = \alpha_i + \nu_1 Y_i^{2000} + \sum_2^4 \nu_j Q_i^j + \sum_2^4 \theta_j (Y_i^{2000} * Q_i^j) + \mu T_i + \varepsilon_i \quad \textbf{(1)}$$

In this equation, the coefficient ν_1 associated with Y_i^{2000} captures differences in the characteristics of the defendants tried in the first quarter of 1999 and 2000, while the coefficients associated with the three interaction terms, θ_j, capture differential changes in the characteristics of the defendants tried in the first quarter and those tried in the 2nd, 3rd, and 4th quarters respectively. In particular, since the transition period exactly corresponds to the 2nd quarter, θ_2 captures differential changes occurring during the transition period (starting the week when the reform was signed and ending the week when it entered into force).

The corresponding estimates are reported in *Table 2*, along with the associated standard errors, clustered at the district court and individual levels. In *Panel A*, our sample includes all defendants tried during the period. In *Panel B*, we exclude from the sample those who did not receive a prison sentence, whether it be a conditional or an unconditional one, and, as such, were not affected by the reform. This allows us to focus on those defendants who were most likely to have something to gain by having their trial postponed until after the reform.

While we do not find evidence of any change in the nature of the cases tried in the first and fourth quarters of 1999 and 2000 (columns 1 and 4), we find strong evidence of such a change occurring during the 2nd quarter of the year 2000 (column 2). Indeed, compared to those tried in the same quarter in 1999, we observe that defendants tried during the transition period, especially those who received a prison sentence, had weaker ties to the labor market: they had lower income and were more likely

to receive benefits, particularly unemployment-related benefits. While the effects are diluted when looking at the entire sample of defendants tried during the period (*Panel A*), the magnitude of the differences is particularly important and significant for the restricted subset of defendants who received a prison sentence and had something to gain from having their trial postponed (*Panel B*). For instance, at the sample mean, the income of the defendants who received a prison sentence dropped by 8.6 percentage points during the transition period. Overall, this suggests that wealthier individuals were more often able to have their case postponed until after the reform than other defendants.

In this context, an important question is whether or not this selection merely reflects an attempt by the courts to focus on offenders whose trial outcome did not depend on the timing of the trial during the transition period. We find no such evidence. If this was the case, offenders who were not eligible to the reform would represent a greater share of the individuals tried during the transition period.[22] However, we do not observe any change in the average number of drunk-driving crimes committed by offenders tried during the transition period. While defendants tried during the transition period had been convicted and incarcerated a greater number of times for crimes other than drunk driving, these characteristics did not in themselves constitute grounds for ineligibility. Hence, although these patterns may reflect the fact that the courts are biased against more serious criminals, we find no evidence that the observed selection was the result of sorting between eligible and ineligible offenders.

Finally, we do observe some compositional changes for the third quarter (column 3), but they merely reflect the fact that the number of cases remained lower than usual in the aftermath of the reform – as displayed in *Figure 1.b*.

Overall, these findings question the degree of consistency with which drunk-driving cases were handled in district courts around the time of the reform, as well as the level of fairness with which defendants were treated by the justice system during the transition period. From a methodological point of view, our results suggest that the way drunk-driving cases were handled in district courts during the transition period generated differences in the nature of defendants tried before and after the reform. Importantly, the nature of the selection is most likely relevant for the analysis of the impact of the reform on subsequent crime and labor market outcomes, raising important concerns for

[22] Again, the reform did not systematically apply to offenders who had already been placed on probation for a *drunk-driving* crime more than once or to those who were on probation at the time of the crime for an *alcohol-related* crime.

identification. More generally, this also raises questions with respect to the performance of traditional quasi-experimental estimators when applied to the study of significant contextual changes (such as legislative reform, program expansion, etc.) with consequences that can be anticipated by their stakeholders.[23]

4. Empirical strategy

4.1. Intuition behind the instrument

In order to measure the causal impact of the reform and to bypass the selection problem documented above, we use a novel instrumental variable approach. This approach relies on two features of the justice system which, when combined together, create exogenous variation in the probability for offenders to receive a custodial sentence. The first of these features is the fact that, as already mentioned above, Danish legislation guarantees that defendants tried after a reform for a crime committed prior to it must be tried under the more lenient of the two laws. This means that individuals tried after July 1st, 2000 for a drunk-driving crime committed before that date were tried under the new law. The second of these two features is the significant time gap between the moment a crime is committed and the moment the corresponding decision of justice is rendered by a district court – as further documented below. Together, these features ensure that the closer to the reform a crime was committed, the more likely the offender was to be tried after the reform under the new law, and therefore to avoid prison.

Figure 2 provides evidence of the strength of this approach. In order to do so, the data is organized based on the week *when the crime was committed* (hereafter referred to as "*week of crime*"), instead of the week when the sentence was rendered, and the following indicators are represented: a) the average time gap between the moment an alleged crime was committed and the moment the decision of justice was rendered by a district court by *week of crime*; b) the share of cases tried after July 1st,

[23] Traditional quasi-experimental estimators raise additional selection problems which, although not discussed in detail here, remain essential. In particular, one concern is that the coming into force of the new law might have been accompanied (at least for a time) by more frequent police controls to compensate for the reduction in the expected cost of the punishment by increasing the probability of being caught drunk driving. Moreover, another concern is that potential offenders might have modified their behavior around the time of the reform. For instance, they might have anticipated the above-mentioned increase in road traffic controls and behaved more carefully in the weeks following the entering into force of the reform, thus reducing the overall number of drunk-driving crimes. Furthermore, conditional on individuals internalizing changes in the legislation, the reform should also have induced a modification in the characteristics of the individuals arrested for a drunk-driving crime after the law was passed. Indeed, the lowering of the cost associated with drunk-driving crimes should mechanically have led a new range of individuals to commit drunk-driving crimes (those reaping fewer benefits from committing a crime and/or incurring higher costs if caught), thereby increasing the overall number of drunk-driving crimes.

2000 (the date when the reform officially entered into force) by *week of crime*; c) the share of defendants tried for an alleged drunk-driving crime who received a custodial sentence by *week of crime*; d) the share of defendants tried for an alleged drunk-driving crime who were actually incarcerated by *week of crime*.[24]

The evolution of the first two indicators provides graphical support for our approach. Around the time of the reform, the time lapse between the moment when a prosecutor would press charges against an alleged drunk-driver and the moment when the district court rendered its decision was substantial. On average, the time gap was of 6 months for drunk-driving crimes committed in 1999. It can be noted that this time gap tends to increase, albeit marginally, as crimes are committed closer to the reform and that it peaks for crimes committed in the weeks preceding the signature of the law (*Figure 2.a*). Possible implications for our design are discussed in *Section 4.4* below. This time gap was almost entirely driven by the case processing time in district courts. As a consequence, as individuals' arrest date got closer to the reform within the 12-month period preceding it, an increasingly large share of them was tried after, under the new law (*Figure 2.b*).

As for the last two indicators, their evolution confirms that there was significant variation in the probability of receiving a custodial sentence among individuals tried for a drunk-driving crime committed in the 12-month period *preceding* the reform, based on the date of their crime. Indeed, the share of defendants who received a custodial sentence by week of crime started going down from July 1999 from slightly less than 80% to less than 20% right after the reform (*Figure 2.c*). The same pattern is observed for the share of defendants who were actually incarcerated following their trial (*Figure 2.d*).

[24] For data confidentiality reasons, indicators b), c), and d) displayed in *Figure 2* are calculated as moving averages. For each week *w*, the value of these indicators is calculated as the average value of the indicators over weeks *w-1*, *w*, and *w+1*. Furthermore, for any given week, the share of cases tried after the reform is normalized to 0 if the actual number of cases tried *after* is equal to or lower than 3 (in total, this normalization was carried out for 16 weeks), and the share of cases tried after the reform is normalized to 1 if the actual number of cases tried *before* is equal to or lower than 3 (in total, this normalization was carried out for 4 weeks).

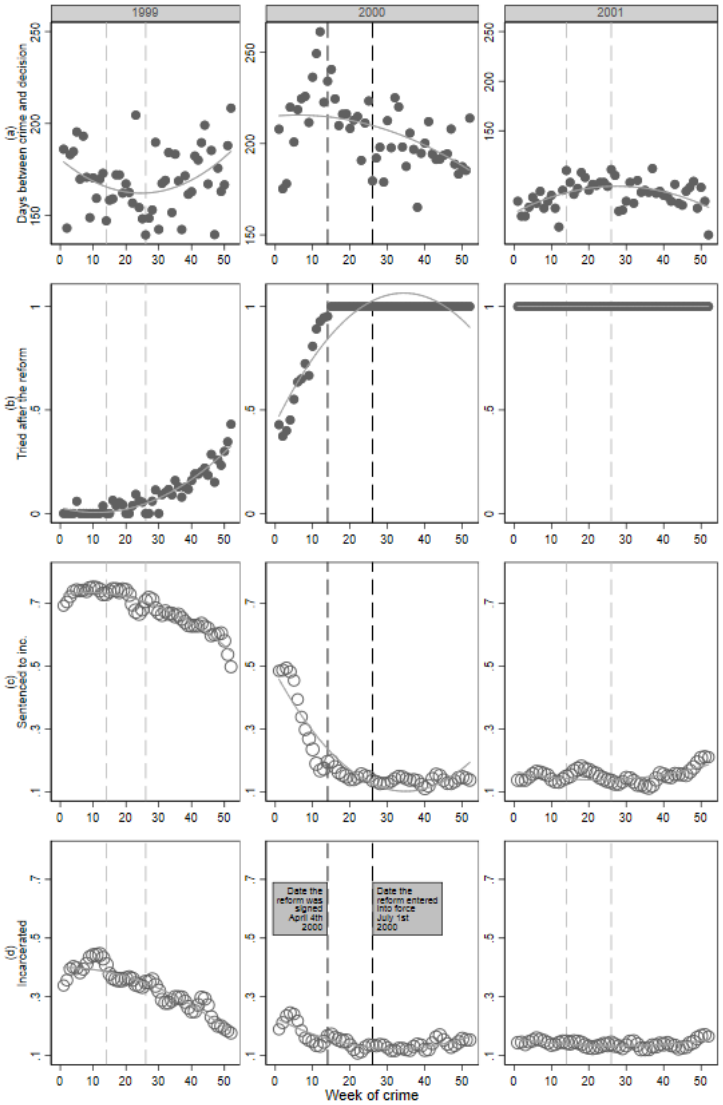

Fig. 2: Motivation for the instrumental variable approach: This figure depicts the evolution of the following four indicators around the time of the reform: a) the average time gap between the moment a crime is committed and the moment the decision of justice is rendered by a district court by *week of crime*; b) the share of cases tried after July 1st, 2000 (the date when the reform officially entered into force) by *week of crime*; c) the share of defendants who received a custodial sentence by *week of crime*; d) the share of defendants who were actually incarcerated by *week of crime*. For each year, the first dotted vertical line marks the week when the law was signed (week 14) and the second one marks the week when it came into force (week 26).

4.2. Sampling strategy

Our approach consists in comparing individuals who committed their drunk-driving crime at different dates *prior* to the signature of the reform and therefore faced varying probabilities of incarceration (as just discussed in the previous section). We focus on all individuals charged for a drunk-driving crime committed in the 24-month period preceding the signing of the law (between week 15 of 1998 and week 14 of 2000).[25] Two characteristics of our sampling strategy should be highlighted. First, by focusing on crimes committed before the reform was passed, we avoid any selection problems that could arise due to an announcement effect. Second, while *Figure 2* shows that our instrument exhibits no variation among individuals charged for a drunk-driving crime committed 13 to 24 months before the signing of the reform (between week 15 of 1998 and week 14 of 1999), including these individuals in our sample allows us to control for seasonal variations using a time trend as well as year and month fixed effects. Restricting our sample to defendants *tried* in the country at that time, we obtain a sample of 8,353 cases, corresponding to 7,959 distinct defendants.[26]

Table 3 describes the characteristics of the defendants included in our sample. They are predominantly males in their late thirties. While close to 63.7% of them held some type of job at the end of the year preceding the date of their crime, 72.4% received social benefits in the 12-month period preceding their crime. On average, defendants received transfers for 22.7 weeks, with unemployment-related benefits alone accounting for 13.2 weeks. Strikingly, 34.3% of the defendants had already had at least one conviction for a drunk-driving crime in the previous 5 years. Few of them were in a relationship (28.6%), and defendants born abroad and descendants of immigrants represented 4.9% and 0.4% of the sample respectively – slightly less than their actual share in the overall population in 2000, which was 5.4% and 1.4% respectively.

4.3. Econometric specifications

RF and IV approaches

In order to report on the impact of the reform, we show the reduced form estimates (RF) obtained by the estimation of the following equation:

[25] More information on the administrative datasets used as part of this study can be found in *Appendix A.2*.

[26] In the few cases where an individual had allegedly committed more than one drunk-driving crime throughout the study period, keeping only the case associated with the first alleged drunk-driving crime yields results similar to those displayed below (results are available upon request).

$$y_{i,t} = \delta^{RF}(\overbrace{P_i * T_i}^{I_i}) + \mu_1 T_i + \mu_2 P_i + X_i\beta + \mu_m + \mu_c + \varepsilon_i \quad \textbf{(2)}$$

where $y_{i,t}$ is the outcome of interest for individual *i* measured at time *t*. Outcomes related to subsequent criminal behavior are studied in *Section 5.1* and labor market attachment outcomes in *Section 5.2*. T_i is a trend, increasing with time, which captures the time gap between the moment when the crime was committed and the date when the law was signed (the unit for this variable is 100 days);[27] P_i is a period dummy taking the value 1 if individual *i*'s crime was committed in the 12-month period preceding the signing of the law and 0 if it was committed earlier; and X_i is a vector including all variables in the conditioning set detailed in *Appendix A.2*. More specifically, we control for various trial characteristics, such as whether the defendant was a juvenile at the time of the crime and the nature of the main charge (using a detailed 7-digit drunk-driving charge code). We also include defendants' background information, such as their gender, age at the time of the trial, immigration status (as per Statistics Denmark's typology: "immigrants", "descendants of immigrants", or "rest of the population"), their past criminal activity (the number of convictions for other drunk-driving crimes, other road traffic crimes, and non-road traffic crimes in the 5-year period preceding their crime), marital status, highest educational achievement, type of job held, and annual income. Unless specified otherwise, all baseline background characteristics included in the conditioning set were measured at the end of the year preceding the crime and are available for the vast majority of the offenders in our sample (the variables included in the conditioning set are all available from 1986). Finally, μ_m and μ_c are fixed effects indicating the month when individual *i* committed their crime and the district court where they were tried (there are 84 of them).

Our instrument, $I_i = (P_i * T_i)$, thus captures the differential effect of the trend variable T_i for crimes committed in the 12-month period preceding the day the reform was signed, compared to crimes committed in the 13 to 24 months before the signature. The parameter of interest is δ^{RF}, which should be different from 0 if the nature of the sanctions imposed on offenders before and after the reform has an impact on $y_{i,t}$, as the probability of receiving a custodial sentence is positively correlated with the time gap between the date of the crime and the signing of the reform in the 12-month period

[27] T_i is a time trend, rather than the time gap between the moment when the crime was committed and the date of the reform, to avoid violating the monotonicity assumption – which will be discussed below. Hence, T_i is constructed in such a way that the greater its value is, the closer to the reform individual *i* committed their crime.

preceding it. In contrast, *Figure 2* suggests that there is no particular reason to expect μ_1 to be statistically different from 0.

The estimates we focus on most closely are our IV estimates, which are obtained by instrumenting a dummy variable indicating whether individual *i* received a custodial sentence by I_i using a Two-Stage-Least-Squares (2SLS) estimation procedure. The resulting coefficients measure the impact of receiving a custodial sentence (as opposed to a non-custodial one) on the *compliers*, *i.e.* the subset of defendants whose date of crime within the 12-month period preceding the signature of the reform had an impact on their being sentenced to 1 to 60 days of incarceration.

Standard OLS approach

For comparison purposes, we also show the standard Ordinary-Least-Squares estimates (OLS) obtained when estimating the following linear model:

$$y_{i,t} = \delta^{OLS} cust_i + X_i\beta + \mu_1 T_i + \mu_2 P_i + \mu_m + \mu_c + \varepsilon_i \quad \textbf{(3)}$$

In this equation, $cust_i$ is a dummy variable indicating whether individual *i* received a custodial sentence as part of their trial and δ^{OLS} is the parameter of interest. However, for a number of reasons, the $cust_i$ variable is likely to be endogenous in this specification. Indeed, as displayed in *Table 3*, offenders who receive a custodial sentence differ significantly from those who receive a non-custodial one and, unless all differences across these two groups are controlled for (which seems unlikely to occur), these OLS estimators are likely to yield biased estimates.

4.4. Instrument validity

First-stage estimates and compliers' characteristics

First-stage estimates are displayed in *Table 4*, where we estimate the impact of having committed a drunk-driving crime closer to the signing of the law on the probability of a defendant receiving a custodial sentence (*Panel A*). To do so, we regress our outcome variable on our instrument and an increasingly exhaustive set of control variables. From column 1 to column 4, we enrich the set of control variables by adding the following covariates successively and incrementally: a time trend, period, month-of-crime and district court fixed effects (column 1), dummy variables indicative of the nature of the drunk-driving charge (column 2), information about the criminal case (column 3), and defendant characteristics (column 4).

As expected, we find that having committed a crime closer to the moment when the law was signed substantially reduces the probability of receiving a custodial sentence for a crime committed in the 12-month period preceding the signing of the reform (*Panel A*). Indeed, within that period, delaying their drunk-driving crime by 100 days would have reduced defendants' probability of receiving a custodial sentence by 14.5 percentage points. Furthermore, both the magnitude and significance level of these estimates are robust to the inclusion of covariates in the regression, suggesting that, in the 12-month period preceding the signing of the law, the instrument is independent of both defendant and case characteristics.

As a robustness check, we also calculate our first-stage estimates taking the probability for a defendant to actually be incarcerated as our dependent variable (*Panel B*). However, whether or not an offender was actually incarcerated and the length of their incarceration spell is only imprecisely observed. As a result, we construct a proxy for whether or not an individual was actually incarcerated, which captures whether an individual seems to have spent at least 10 days in prison – the minimum duration of prison sentences for a drunk-driving crime.[28] Although point estimates are lower, results are consistent with those displayed above: delaying their drunk-driving crime by 100 days would have reduced defendants' probability of being incarcerated by 7.0 percentage points in the 12-month period before the reform came into force. As already discussed above, the difference in the magnitude of the first-stage estimates displayed in *Panels A* and *B* can be explained by the implementation of the pardon scheme prior to the reform.

In *Appendix A.4*, we describe the characteristics of the compliers, *i.e.* offenders whose date of crime had an impact on whether or not they received a custodial sentence. To do so, we use the methodology followed by Pinotti (2017), which consists in eliciting compliers' characteristics by the 2SLS regression of the product of the individual characteristics and the endogenous variable on the endogenous variable using *I* as an instrument. We find that the characteristics of the first group are similar to those of the overall sample. The only difference is that compliers are slightly more likely to have a previous drunk-driving conviction – a difference that disappears when compliers are compared to the subsample of offenders who received a prison sentence (*Table 2, Panel B*). This suggests that the selection described in *Section 3* does not sufficiently affect the characteristics of the

[28] When measuring the relative effect of custodial and non-custodial sentences, we obtain similar results as those displayed below when using our proxy for incarceration as our variable of interest.

compliers to constitute a threat to the external validity of our results – most likely because the selection concerns only a small subset of our sample.

Independence, exclusion, and monotonicity

However, for this instrument to be valid, it also has to meet the following standard conditions: independence, exclusion, and monotonicity.

The independence assumption implies that the instrument is independent of defendants' background characteristics and potential outcomes (once a time trend, period, month-of-crime and district court fixed effects are controlled for). In order to investigate the validity of this assumption, we study whether defendants' pre-crime characteristics are correlated with the instrument. We do so by regressing each of the background variables displayed in the left column of *Table 3* on the instrument, the time trend, as well as period, month-of-crime and district court fixed effects. For each regression, the coefficient and standard error associated with the instrument are reported in *Table 3*. We find that the coefficients associated with the instrument are systematically small and largely insignificant, indicating that the independence assumption is likely to be met.[29] This also suggests that the reform was not anticipated by potential offenders prior to the date of its signature.

The exclusion restriction implies that the timing of the crime *itself* does not have any direct impact on our outcome variables (defendants' crime and labor outcomes up to ten years after the completion of their trial). One concern is that the risk of recidivism and/or prospects of employment might vary across defendants based on the timing of their crime or the date of their sanction. However, the inclusion in our sample of individuals tried for a drunk-driving crime committed 13 to 24 months before the signing of the reform allows us to mitigate the consequences of this potential problem by controlling for trend and seasonality effects. Another concern lies in the fact that, as displayed in *Figure 1*, the waiting time for trials increased as crimes were committed closer to the date of the reform, which could be an issue since longer trial waiting times may theoretically foster subsequent crime outcomes. Given that offenders who received a custodial sentence did not have to wait as long as those who received a non-custodial sentence (because the latter were tried closer to the date of the reform), it is possible that our results (showing that custodial sentences foster crime compared to non-custodial ones) underestimate the true negative impact of incarceration. However, two elements allow us to minimize the extent of this concern. First, the percentage increase in trial waiting time in the 12

[29] In what follows, we also show that the IV estimates are very similar irrespective of whether or not the conditioning set is included in the estimated equation. This brings additional evidence that the independence assumption holds.

months prior to the signing of the legislation remains limited. It only increases sharply in the couple of weeks preceding the signature. Second, while theoretical arguments suggest that punishment celerity should deter recidivism, criminologists now tend to agree that it has no impact (see Pratt and Turanovic (2018) for a review of the evidence).[30]

Finally, the monotonicity assumption implies that the probability of receiving a custodial sentence decreased for *all* offenders as their crime was committed closer to the reform in the 12 months preceding its signature. While nothing in the implementation of the reform leads us to suspect otherwise, we investigate the validity of this assumption by estimating the first-stage equation for various subgroups of the sample: males, females, individuals aged below 30, individuals aged above 30, individuals with prior drunk-driving convictions, individuals without any prior drunk-driving conviction, etc. The coefficients and standard errors associated with each of the subgroups are reported in *Appendix A.5*. We find that the coefficients are all positive and statistically significant (as well as very similar in magnitude). This suggests that issues of non-monotonicity are probably limited as well.

5. Main Results

We measure the relative impact of custodial and non-custodial sentences on offenders' subsequent criminal behavior and labor market attachment using the instrumental variable strategy described in the previous section.

5.1. Impact on crime

We start by measuring the relative impact of custodial and non-custodial sentences on drunk drivers' post-sentencing involvement in criminal activities.[31] The relative impacts of incarceration on convictions for drunk-driving crimes and for any other crime are studied separately. In *Figure 3*, we report on the differential effect of the two types of sentences as measured by our IV estimates on

[30] In Pratt and Turanovic's own words: *"While exceptions exist […] the general pattern revealed in this body of work is that celerity effects of punishment are nonexistent, and that even when present it can be difficult to disentangle such effects from other potentially confounding influences, like the perceived (or actual) certainty or severity of punishment. So again, the pattern is pretty clear that faster punishments appear to have little to no consistent, independent effect on one's future criminal behavior*.". While a few studies on the impact of the Hawaii Opportunity Probation with Enforcement (HOPE) project fostering swift-and-certain punishments found significant positive effects (Hawken and Kleiman, 2009; Kilmer et al., 2013), replication studies carried out in other states found no impact (see, for example, the experimental studies by Lattimore et al. (2016) and O'Connell et al. (2016)). *Criminology & Public Policy* devoted an issue to this topic, see for example Nagin (2016) and Cullen et al. (2016) in addition to the articles just cited.

[31] In order to measure the *net* impact of incarceration, we exclude from the calculation of these outcomes any crime registered under the same case ID or related to any other crime committed prior to the decision of justice considered in this study.

drunk-driving crimes (left panel) and other crimes (right panel). To do so, the following two *cumulative* outcomes are computed every 3 months for both types of crime from the date when the drunk-driving case was settled in court: the probability of being convicted of a crime by time *t* (the *extensive* margin), displayed in the top panel; and the number of convictions by time *t* (the *intensive* margin), displayed in the bottom panel. A subset of the coefficients is displayed in *Table 5.A.* for drunk-driving crimes and in *Table 5.B.* for other crimes.

We find no differential impact on subsequent drunk-driving convictions, suggesting that custodial and non-custodial sentences have similar effects on this type of crime. Indeed, the two sanctions appear to be equally effective in preventing offenders from being reconvicted for a drunk-driving crime (*Figure 3.a*), and their impact on the average number of subsequent convictions for drunk driving is also similar (*Figure 3.c*). In both cases, point estimates are relatively small in magnitude and systematically fail to be statistically significant at the 5% level. This is so despite the fact that descriptive statistics show that there is significant room for improvement in terms of recidivism: as documented in *Tables 5*, the average number of subsequent convictions for a drunk-driving crime among individuals included in our sample is 0.6 after 10 years. At first glance, this result is somewhat surprising given that some of the offenders who received non-custodial sentences were requested to participate in an alcohol rehabilitation program. However, one should keep in mind that, among the compliers, the number of defendants with a serious problem of alcohol dependence is probably limited because of the pardon scheme to which they were entitled before the reform. Hence, it is possible that this result can be explained by the specificity of our sample rather than by the ineffectiveness of the rehabilitation program offered to offenders who received a non-custodial sentence.

In contrast, we find that, compared to non-custodial sentences, custodial ones increase the average *number* of convictions for crimes other than drunk driving (*Figure 3.d*). At its peak, the magnitude of this effect is large: our results indicate that custodial sentences increase the average number of convictions by 0.630 after 8 years – representing a 40.3% increase at the sample mean. Because custodial and non-custodial sentences appear to be equally effective in preventing individuals from being convicted of a crime other than drunk driving (*Figure 3.b*), the effect on the intensive margin is in fact mechanically larger (almost twice as large) on offenders who were convicted at least once. This suggests that the criminogenic effects custodial sentences were found to have on other offenders

(Cullen et al., 2011; Aizer and Doyle, 2015) can also affect offenders exhibiting relatively low proclivity for criminal behavior, such as drunk drivers.

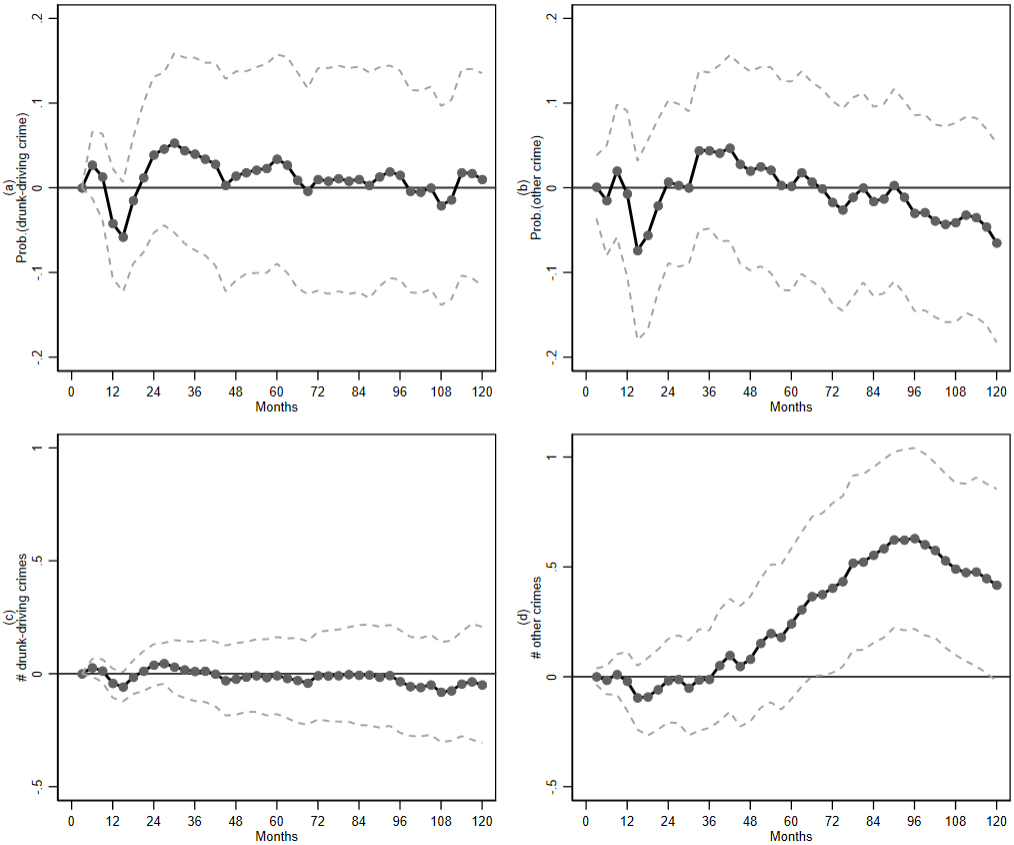

Fig. 3: Impact of custodial vs. non-custodial sentences on drunk-driving and other crimes: This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a drunk-driving crime; b) the probability of being convicted of any other crime; c) the number of convictions for a drunk-driving crime; and d) the number of convictions for any other crime. Crime outcomes are measured every 3 months from the date when the drunk-driving case was settled in court. Dashed lines represent the 95% confidence interval.

The timing of the effects is very revealing in a number of ways. First of all, it is interesting to note that point estimates experience a brief and sudden drop 12 to 15 months after the trial, which is consistent with an incapacitation effect. Although not statistically significant, this suggests that, as expected, as long as offenders are kept behind bars, custodial sentences are more effective than non-custodial ones in preventing criminal behavior.[32] Then, once the incapacitation effect is no longer at play, the negative impact of incarceration after release becomes more and more pronounced over time, up to 8 years after the initial decision of justice. This negative effect then seems to diminish

[32] The reason why this incapacitation effect does not materialize immediately after the trial at time 0 lies in the fact that we focus on convictions (and not crimes) and that there was a significant time lag between the time of the alleged crime and the time of the verdict. This can also be explained by the waiting list system in place at the time, which could delay the incarceration of offenders for up to several months after the verdict was announced.

slightly, suggesting that it may not be irremediable. This turning point will be discussed further in *Section 5.3* below.

While all estimates (standard OLS approach, RF, and IV) reach similar conclusions on drunk-driving outcomes, the naive OLS approach yields very different results than those just discussed for crimes other than drunk driving. As displayed in *Tables 5.A* and *5.B*, the standard OLS estimates suggest that custodial sentences decrease both the probability for a drunk-driver to be subsequently convicted of any other crime, and the number of such crimes they commit. For instance, these estimates suggest that custodial sentences decrease the probability of an offender being convicted of any other crime within the next 10 years by 6.2 percentage points (representing a 10.7% decrease at the sample mean) and reduce the number of such convictions by 0.296 crime (representing a 16.3% decrease at the sample mean). Both results are statistically significant at the 1% level. These differences between standard OLS and IV results suggest that the selection induced by stakeholders' anticipation of the reform may bias standard quasi-experimental estimators, which should be used with caution in situations such as this reform.

A closer look at the nature of the other crimes committed by drunk-driving offenders highlights the importance of economically motivated crimes. In *Figure 4*, we report on the relative impact of custodial and non-custodial sentences on subsequent convictions for violent (left panel), property (central panel), and other crimes (right panel) taken separately, leaving drunk-driving crimes out of the analysis. A subset of coefficients is displayed in *Tables 6*. We observe a strong and particularly significant increase in the *number* of convictions for property crimes. In contrast, we find no statistically significant effect on the *number* of convictions for violent crimes, for which the associated coefficients remain close to 0. While incarceration seems to increase the number of convictions for other crimes, point estimates fail to be statistically significant at the 5% level, making it harder to draw more definitive conclusions. Again, extensive margin coefficients fail to be statistically significant at the 5% level for all three outcomes.

Overall, our results provide evidence that non-custodial sentences can be more effective than custodial ones to prevent subsequent crime, especially property crimes.

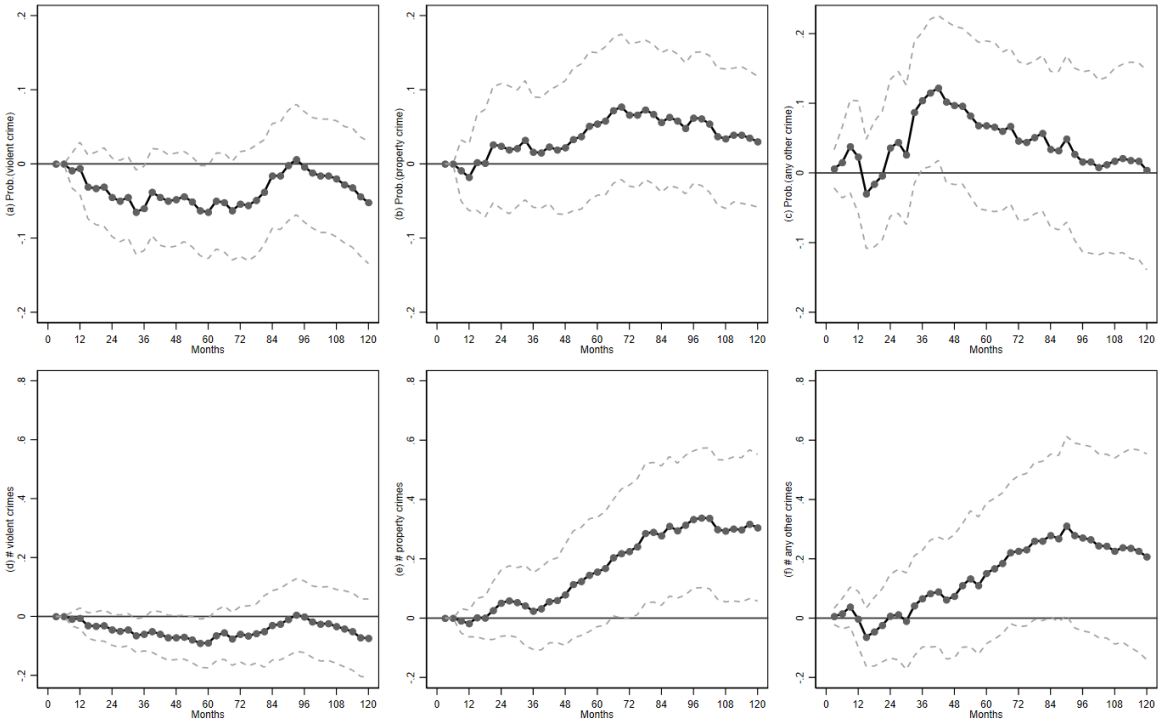

Fig. 4: Impact of custodial vs. non-custodial sentences on other crimes (breakdown by crime type): This figure depicts the cumulative impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of being convicted of a crime; and b) the number of convictions. These two outcomes are measured every 3 months from the date when the drunk-driving case was settled in court for each of the following crime types: 1) violent crimes; 2) property crimes; and 3) other non-drunk-driving crimes. Dashed lines represent the 95% confidence interval.

5.2. Impact on labor market attachment

We now turn to the relative impact of custodial and non-custodial sentences on offenders' labor market outcomes, an important dimension of offenders' lives after release. To do so, we compute the following outcomes every year from the date when the drunk-driving case was settled in court: a) the probability of having a job in year *t*;[33] b) the cumulative number of weeks during which offenders received unemployment-related benefits by time *t*; c) the cumulative income by time *t*. IV estimates measuring the differential effect of the two sentences on these various labor market outcomes are reported in *Figure 5*. A subset of coefficients is also displayed in *Table 7*.

[33] Measured by Statistics Denmark at the end of the month of November.

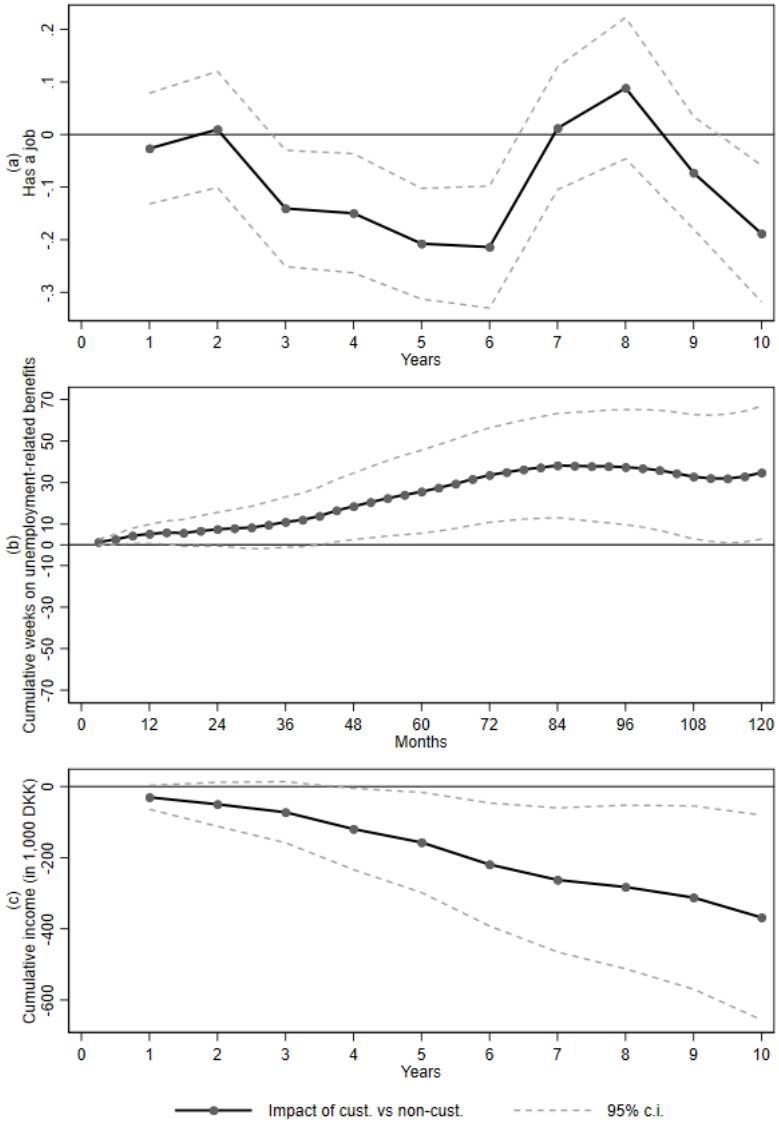

Fig. 5: Impact of custodial vs. non-custodial sentences on labor market attachment: This figure depicts the impact of custodial sentences (as measured by our IV estimates) on the following outcomes: a) the probability of having a job at the end of each year following the decision of justice; b) the cumulative number of weeks during which offenders received unemployment-related benefits; c) the cumulative income (in 1,000 DKK). We measure outcomes a) and b) every year and outcome c) every 3 months from the date when the drunk-driving case was settled in court. Dashed lines represent the 95% confidence interval.

Overall, our results suggest that, compared to non-custodial sentences, custodial ones significantly weaken individuals' labor market attachment, and that they do so on a long-term basis. Indeed, despite the limited length of the incarceration spells, offenders sentenced to custody are less likely to be employed after receiving their sentence than those who only receive a non-custodial sentence (top panel of *Figure 5*). As a consequence, they are more likely to rely on unemployment-related benefits (middle panel of *Figure 5*). This result holds for almost every year following the decision of justice – except for a couple of years towards the end of the period (we comment further on this turning point below). In both cases, the magnitude of the effects is very large. For instance, 10 years after the initial

decision of justice, the probability of having a job is reduced by 18.8 percentage points for individuals who received a custodial sentence compared to those who received a non-custodial one – representing a 54.3% decrease at the sample mean. Even in a generous welfare state like Denmark where social transfers are important, custodial sentences reduce offenders' income as compared to non-custodial ones (bottom panel of *Figure 5*). After 10 years, incarceration thus represents a cumulative loss of 368,056 kroner – corresponding to a 15.2% decrease at the sample mean, or one and a half years' worth of income.

Once again, the timing of these effects is particularly telling and crucial for the interpretation of our results. First of all, the impact of the two sanctions on the probability of having a job (*Figure 5.a*) is remarkably similar during the two years following the trial, with differences remaining small in magnitude and non-statistically significant. It is only from the 3rd year after the decision of justice that the negative effect of incarceration really begins to materialize. This may be so for a number of reasons, including the length of the probation period associated with non-custodial sentences (two years). It is also interesting to note that this turning point coincides with the moment when convictions cease to appear on the criminal record of those placed on probation (three years after the trial), while they remain on the criminal record of individuals sentenced to incarceration, whose record is only cleared five years after the date of their release. This suggests that there may be considerable stigma associated with having a criminal record, which weakens offenders' post-release labor market attachment, in line with previous findings (Pager, 2003; Raphael, 2014; Agan and Starr, 2018; Mueller-Smith and Schnepel, 2020). After six years, the difference between the two types of sanctions starts decreasing, echoing the pattern of results observed on crime outcomes. It is interesting to note that this coincides with the time when the conviction is removed from the criminal record of offenders sentenced to incarceration.[34] Towards the end of the period, the difference in the probability of having a job becomes more pronounced again. This coincides with the rise in unemployment from 2009 onwards in the wake of the 2008 economic crisis and suggests that incarceration and the difficulties it creates for offenders' more immediate labor market attachment can make individuals more vulnerable to economic downturns, possibly as a result of their more erratic professional history. Overall, these observations suggest that the relative impact of custodial and non-custodial sentences can be heavily influenced by certain features of the sanction (*e.g.* number of years a conviction

[34] As observed in *Section 5.1*, the incapacitation effect materializes about a year after conviction, meaning that the criminal record of incarcerated offenders is wiped clean five years later, *i.e.* six years after conviction.

remains on the criminal record), as well as by the state of the economy, and in particular local labor market conditions, as recently shown by Yang (2017), Schnepel (2018) and Galbiati et al. (2020).

5.3. Understanding the criminogenic effect of incarceration through the lens of labor market precariousness

So far, our results indicate that, compared to non-custodial sentences, custodial ones increase the average number of subsequent crimes committed by drunk-driving offenders. Although consistent with a growing number of empirical studies, these results are at odds with standard theoretical predictions that harsher sanctions should reduce crime.

The analysis of the relative impact of custodial and non-custodial sentences on labor market participation suggests the existence of an important mechanism at play regarding the criminogenic effect of incarceration: upon release, incarcerated offenders may use crime as a means of subsistence to compensate for the increased difficulties they encounter in the labor market because of the time they spent in prison. This explanation is supported by several arguments. First, as discussed in *Section 5.2*, custodial sentences considerably weaken offenders' labor market attachment compared to non-custodial ones. Indeed, incarceration significantly reduces offenders' probability of having a job, increases their reliance on unemployment-related benefits, and, *in fine*, reduces their income. This could explain the need to rely on criminal activities to earn money. Second, the increase in crime is mainly due to a surge in property crimes, which are essentially economically motivated crimes: burglary, fraud, handling of stolen goods, theft, robbery, shoplifting, etc. Third, the plot of the curve representing the relative impact of custodial and non-custodial sentences on property crimes (*Figure 4.e*) shows similarities with that of the curve representing the effect on labor market attachment (*Figure 5.a*). In particular, they show similar inflection points both in the short and in the longer term. Moreover, a close look at these estimates suggests that the deterioration of offenders' labor market attachment slightly precedes the increase in the number of crimes committed.[35] Indeed, the impact on labor market attachment starts to materialize from the 3rd year (around the time when the criminal record of those who received a non-custodial sentence is expunged), while the effect on crime appears from the 4th year. Similarly, the detrimental impact of prison on labor market attachment begins to dissipate from the 6th year (around the time when the criminal record of those who received a custodial

[35] One should keep in mind that our variables of interest are convictions, and that these occur with a certain delay in relation to crimes.

sentence is expunged) while the increase in the number of crimes committed comes to a halt from the 8th year.

Other traditional mediators can also explain part of the criminogenic effect of custodial relative to non-custodial sentences, but are less convincing in explaining why the cumulative effects reach a plateau after 8 years. For instance, the detrimental effect of custodial sentences on offenders' mental health or human capital accumulation may help explain part of the increase in the number of crimes committed by incarcerated offenders, but not why the effect declines in the longer run. This is also the case for the mechanism whereby incarceration facilitates the formation of a criminal network. Moreover, in the particular setting of this paper, drunk-driving offenders were only incarcerated with minor offenders (precisely to avoid prison acting as a school of crime),[36] which limits the scope of this mechanism.[37]

6. Conclusion

In this article, we study the long-term crime- and labor-market-related effects of incarceration relative to probation using a large-scale reform of the Danish legislation, whereby a custodial sentence (jail time) was replaced by a non-custodial one for most drunk-driving crimes. The study reaches several conclusions.

First, we find evidence suggesting that salient contextual changes (such as a legislative reform, a program scale-up, etc.) can be anticipated by their stakeholders, who in turn can modify their behavior in line with their best interest. In the case of the change in the drunk-driving legislation studied here, we show that the way drunk-driving cases were handled in district courts changed drastically in the months preceding the entering into force of the reform (from the moment the law was signed) and that a large share of the cases which should have been tried before the reform was actually tried after. This means that a group of offenders who should have been tried before the reform was tried after and, as a consequence, avoided prison. Furthermore, we show that the identity of the individuals who had their case postponed was not random: in particular, wealthier defendants were more likely to have their trial put off until after the reform. From a policy perspective, this important result suggests that

[36] Drunk-driving offenders benefited from a special regime reserved for minor offenders, called “hæfte”, which stated that minor offenders were not allowed to be incarcerated with more serious criminals.
[37] In addition, we also compare the relative impact of custodial and non-custodial sentences on the likelihood that a drunk driver will subsequently be convicted of a crime committed with one or more other criminal(s), as well as the number of such crimes committed, and find no evidence to support the idea that prison expands offenders’ criminal network (results are available upon request).

it would be advisable to synchronize the passing and entering into force of a new law or, whenever possible, to more closely monitor how cases are handled at such times so as not to introduce any avoidable source of inequities in the justice system. From a methodological perspective, our findings also suggest that traditional quasi-experimental estimators should be used with caution in similar contexts.

Second, we show that, compared to non-custodial sentences, custodial ones do not necessarily have an impact on the number of reoffenders but can increase the average number of crimes they commit. We find evidence that, in the context of the reform under study, this increase was not driven by drunk-driving crimes but by other crimes, especially property crimes. While it is not possible to pin down the exact mechanisms at play, part of the explanation for this increase in offenders' criminal activities can probably be found in their greater precariousness. In particular, we find that, compared to non-custodial sentences, custodial ones significantly weaken offenders' labor market attachment as they decrease both their probability of having a job and their total income. It is important to note that we provide at least a partial explanation for the weakening of incarcerated offenders' attachment to the labor market. Indeed, a close examination of the timing of the effects on labor market outcomes supports the notion that individuals with a criminal record are subject to stigma that hinders their employment opportunities after release – in line with evidence found in Pager (2003), Raphael (2014), Agan and Starr (2018), and Mueller-Smith and Schnepel (2020). Indeed, not only did we find that, compared to probation, incarceration starts having an effect around the time when the criminal record of individuals sentenced to probation is cleared, but we also observed that the relative effect of incarceration starts to decrease around the time when individuals sentenced to incarceration have their records cleared.

Overall, our results are surprisingly similar to those found by other studies in the US where incarceration conditions are unarguably harsher than in Nordic countries. As such, they suggest that incarceration can remain a harmful experience, even in the context of relatively soft incarceration conditions as in Danish open prisons. The fact that few people go to prison in Scandinavia may actually reinforce its stigmatizing character. Importantly, our results suggest that, in the Danish context, an important difference between incarceration and probation is the length of time during which the criminal record remains active. As a consequence, the stigma of having a criminal record lasts longer for individuals sentenced to incarceration (five years post-release) than for those sentenced to probation (three years post-conviction).

Our paper thus contributes to understanding the role of incarceration on post-release outcomes and complements the existing literature on the topic in a Scandinavian context, such as the Norwegian study by Bhuller et al. (2020) which suggests positive effects of incarceration in Nordic countries. Understanding what drives the differences between the two studies is beyond the scope of this paper but, given the significant heterogeneity in the results found in both studies, we hypothesize that part of the explanation may lie in differences in methodologies and compliers' characteristics. While we focus on a broad range of mild offenders arrested for a single (yet widespread) crime, Bhuller et al. (2020) conversely focus on a specific type of defendants (on the margin of being incarcerated) convicted of a broader range of crimes. In any case, our results provide additional evidence that custodial sentences can have a negative impact on offenders spanning way beyond their period of incarceration. Our findings also indicate that accompanying post-release measures should be implemented to mitigate these costs for offenders and for society. In this regard, reducing the time period during which a conviction remains on the criminal record may be particularly effective.

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Tables

Table 1: Drunk-driving trial outcomes in cases tried in 1999, 2000 (year of the reform), and 2001

	Drunk-driving crimes tried in 1999			Drunk-driving crimes tried in 2000			Drunk-driving crimes tried in 2001		
	#Obs.	Mean	S.d.	#Obs.	Mean	S.d.	#Obs.	Mean	S.d.
No sanction	4,249	0.012	0.107	3,645	0.012	0.110	4,643	0.008	0.090
Prison sentences	4,249	0.726	0.446	3,645	0.689	0.463	4,643	0.742	0.438
Prison, cond.	4,249	0.007	0.085	3,645	0.289	0.454	4,643	0.590	0.492
Prison, cond. (length in days)	4,249	0.199	3.169	3,645	6.350	12.529	4,643	12.562	14.827
Prison, uncond.	4,249	0.719	0.449	3,645	0.390	0.488	4,643	0.142	0.349
Prison, uncond. (length in days)	4,249	18.129	25.402	3,645	11.296	25.178	4,643	6.652	25.932
Between 1 and 60 days	4,249	0.685	0.465	3,645	0.362	0.481	4,643	0.113	0.316
Over 60 days	4,249	0.034	0.182	3,645	0.028	0.164	4,643	0.029	0.169
Imprisoned	4,249	0.378	0.485	3,645	0.196	0.397	4,643	0.138	0.345
Other sentences	4,249	0.260	0.439	3,645	0.307	0.461	4,643	0.258	0.437
Community work	4,249	0.000	0.000	3,645	0.158	0.365	4,643	0.309	0.462
Fine	4,249	0.278	0.448	3,645	0.599	0.490	4,643	0.860	0.347
Fine amount (in DKK)	4,249	1630.560	3131.592	3,645	3432.208	3789.178	4,643	5103.518	3749.283
Driv. lic. suspended	4,249	0.308	0.462	3,645	0.300	0.458	4,643	0.314	0.464
Driv. lic. suspended (length in months)	4,249	1.851	2.774	3,645	1.801	2.750	4,643	1.885	2.784
Appeal	4,249	0.017	0.128	3,645	0.022	0.148	4,643	0.021	0.143

Notes: In this table, we describe the outcome of the drunk-driving trials in 1999, 2000, and 2001.

Table 2: Selection in the nature of the cases tried before, during, and after the transition period

	Whole sample			Weeks 1-14			Weeks 14-26			Weeks 27-39			Weeks 40-52	
				ΔT1			ΔT2-ΔT1			ΔT3-ΔT1			ΔT4-ΔT1	
Variables	#Obs.	Mean	S.d.	Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.
Panel A: all offenders														
Has a job	7,894	0.641	0.480	0.012	0.081		-0.030	0.031		-0.016	0.036		-0.033	0.027
Income	7,894	257.517	141.691	10.673	20.903		-14.598	8.254	*	-20.496	9.965	**	-4.304	8.999
Weeks of social transfers	7,894	22.669	21.538	-1.395	4.038		1.396	1.280		0.828	1.645		0.676	1.240
Weeks of labour market-related transfers	7,894	13.192	18.444	2.748	3.530		1.380	1.122		1.036	1.327		-0.301	1.082
Weeks of health-related transfers	7,894	3.500	9.184	1.044	1.669		-0.706	0.511		0.090	0.535		0.824	0.638
Weeks of pension-related transfers	7,894	5.237	15.503	-5.115	2.863	*	0.310	1.029		-0.592	1.156		-0.101	0.963
Nber of crime in past 5 years	7,894	1.688	2.089	-0.287	0.400		0.293	0.168	*	0.085	0.131		-0.062	0.132
Nber of DD crime in past 5 years	7,894	0.401	0.607	0.005	0.093		-0.037	0.040		-0.051	0.046		0.021	0.031
Nber of inc. spell in past 5 years	7,894	0.418	0.873	-0.023	0.135		0.065	0.057		-0.054	0.049		-0.011	0.045
Panel B: offenders who received a prison sentence (whether it be a conditional or an unconditional one)														
Has a job	5,597	0.638	0.481	0.102	0.099		-0.050	0.040		-0.010	0.041		0.009	0.031
Income	5,597	263.711	137.741	41.765	27.756		-22.584	11.019	**	-23.982	12.147	**	0.824	9.721
Weeks of social transfers	5,597	23.206	21.570	-3.685	4.719		3.299	1.748	*	1.785	1.628		0.346	1.335
Weeks of labour market-related transfers	5,597	13.352	18.440	0.513	4.260		2.701	1.591	*	0.860	1.430		-1.345	1.301
Weeks of health-related transfers	5,597	3.701	9.427	-0.033	1.854		-0.388	0.709		-0.045	0.693		1.080	0.728
Weeks of self-supporting transfers	5,597	0.670	4.900	0.274	0.879		0.266	0.360		0.221	0.329		0.559	0.435
Weeks of pension-related transfers	5,597	5.483	15.824	-4.439	3.077		0.720	1.295		0.748	1.413		0.052	1.201
Nber of crime in past 5 years	5,597	1.710	2.055	-0.618	0.417		0.531	0.207	**	0.032	0.159		0.084	0.138
Nber of DD crime in past 5 years	5,597	0.520	0.648	-0.027	0.122		0.059	0.050		0.041	0.049		0.011	0.034
Nber of other crime in past 5 years	5,597	1.176	1.862	-0.667	0.408		0.482	0.190	**	-0.034	0.155		0.095	0.137
Nber of inc. spell in past 5 years	5,597	0.504	0.938	-0.033	0.167		0.262	0.085	***	0.031	0.062		0.028	0.055

Notes: In this table, we describe the characteristics (mean and standard deviation) of the defendants tried between January 1st, 1999 and December 31st 2000. We also investigate whether or not the characteristics of the offenders tried in each quarter of the year 2000 remained stable. In order to do so, we regressed each of the variables displayed in the left column of this table on a constant, a time trend, a dummy variable indicating when a defendant was tried in 2000 (as opposed to 1999), three dummy variables indicating when a defendant was tried in the 2nd quarter (weeks 14 to 26), the third (weeks 27 to 39), or the fourth (weeks 40 to 52) of either 1999 or 2000, as well as the interaction of the year and quarter dummies. We report the coefficient and standard error associated with the year dummy and the interaction variables. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Sample description and balance checks

	Whole sample			Instrument		
Variables	#Obs.	Mean	S.d.	Coeff.	s.e.	
Female	8,353	0.087	0.281	0.002	0.007	
Juvenile, when the crime is committed	8,353	0.004	0.066	0.000	0.002	
Age, when the decision is rendered	8,353	38.312	11.049	-0.081	0.234	
Immigration status						
Immigrant	8,353	0.049	0.215	-0.004	0.004	
Descendant	8,353	0.004	0.061	-0.001	0.001	
Rest of the population	8,353	0.947	0.223	0.004	0.005	
Family status						
Single	8,353	0.491	0.500	0.004	0.012	
In a relationship	8,353	0.286	0.452	0.001	0.008	
Separated	8,353	0.197	0.398	-0.009	0.008	
Widow	8,353	0.014	0.119	-0.001	0.002	
Unknown	8,353	0.012	0.107	0.004	0.002	*
Education status						
Primary education	8,353	0.525	0.499	0.012	0.012	
Secondary education	8,353	0.378	0.485	-0.007	0.011	
Higher education	8,353	0.062	0.241	0.000	0.005	
Unknown	8,353	0.035	0.183	-0.006	0.003	*
Attachment to the labor market						
Has a job	8,353	0.637	0.481	-0.013	0.009	
Income	8,353	257.462	140.893	-4.041	3.019	
Any social transfers	8,353	0.724	0.447	-0.014	0.009	
Weeks of social transfers	8,353	22.695	21.523	0.149	0.419	
Weeks of labour market-related transfers	8,353	13.182	18.417	0.209	0.355	
Weeks of health-related transfers	8,353	3.409	9.029	-0.044	0.168	
Weeks of self-supporting transfers	8,353	0.680	4.844	-0.054	0.090	
Weeks of pension-related transfers	8,353	5.425	15.733	0.038	0.290	
Criminal priors						
Any crime in past 5 years	8,353	0.650	0.477	-0.022	0.010	**
Nber of crimes in past 5 years	8,353	1.693	2.085	-0.034	0.040	
Any DD crime in past 5 years	8,353	0.343	0.475	-0.013	0.009	
Nber of DD crimes in past 5 years	8,353	0.408	0.610	-0.007	0.011	
Any other crime in past 5 years	8,353	0.515	0.500	-0.020	0.011	*
Nber of other crimes in past 5 years	8,353	1.276	1.939	-0.035	0.038	

Notes: In this table, we describe the characteristics (mean and standard deviation) of the set of defendants included in our sample and report how defendants' characteristics are correlated with the instrument. The estimates describing the differential characteristics are calculated by regressing the variables in the left column of the table on a constant, the instrument, the time-to-reform variable, a dummy variable taking the value 1 when a crime was committed in the 12 month period preceding the entering into force of the reform, and district court fixed effects. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: First-stage

	(1)	(2)	(3)	(4)
Panel A: Probability to receive a custodial sentence				
Instrument	-0.148***	-0.146***	-0.145***	-0.145***
	(0.010)	(0.009)	(0.009)	(0.009)
Mean	0.645	0.645	0.645	0.645
Observations	8,353	8,353	8,353	8,353
R-squared	0.130	0.153	0.207	0.222
Panel B: Probability to be incarcerated				
Instrument	-0.071***	-0.070***	-0.070***	-0.070***
	(0.009)	(0.009)	(0.009)	(0.009)
Mean	0.335	0.335	0.335	0.335
Observations	8,353	8,353	8,353	8,353
R-squared	0.053	0.062	0.063	0.072
Trend	YES	YES	YES	YES
Period FE	YES	YES	YES	YES
Month of crime FE	YES	YES	YES	YES
District court FE	YES	YES	YES	YES
Charge FE	NO	YES	YES	YES
Case charact.	NO	NO	YES	YES
Def. charact.	NO	NO	NO	YES

Notes: In this table, we measure impact of my instrument (the number of days between the moment an individual committed their crime and the time the reform entered into force, in 100 days) on the probability for them to receive a custodial sentence (*Panel A*) and to be actually incarcerated (*Panel B*). For each of these outcomes, we regress the dependent variable on our instrument and an increasingly exhaustive set of covariates: in column (1), a time trend, a dummy variable indicative of whether or not the crime was committed in the 12 month period preceding the entering into force of the reform, and month of crime and district court fixed effects are added to the regression; in column (2) dummy variables indicative of the nature of the charge are added as well; in column (3), we add information on the criminal case; in column (4), we add defendant characteristics. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 5.A: Impact on drunk-driving crimes

	Whole sample			OLS (1)		RF (2)		IV (3)		IV (4)	
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
Panel A: Probability of being convicted again for drink-driving											
Within 2 years	8,353	0.106	0.308	0.005	0.009	-0.006	0.007	0.036	0.045	0.041	0.046
Within 4 years	8,353	0.205	0.404	-0.013	0.011	-0.002	0.009	0.010	0.063	0.014	0.063
Within 6 years	8,353	0.278	0.448	-0.016	0.013	-0.002	0.010	0.011	0.067	0.010	0.067
Within 8 years	8,353	0.323	0.468	-0.009	0.013	-0.002	0.009	0.017	0.064	0.015	0.064
Within 10 years	8,353	0.357	0.479	0.002	0.013	-0.001	0.010	0.010	0.064	0.008	0.064
Panel B: Number of drink-driving convictions											
Within 2 years	8,353	0.106	0.308	0.005	0.009	-0.006	0.007	0.036	0.045	0.041	0.046
Within 4 years	8,353	0.247	0.520	-0.009	0.014	0.003	0.012	-0.029	0.081	-0.023	0.081
Within 6 years	8,353	0.376	0.688	0.001	0.021	0.001	0.015	-0.013	0.100	-0.010	0.099
Within 8 years	8,353	0.463	0.776	0.012	0.022	0.005	0.017	-0.034	0.117	-0.034	0.116
Within 10 years	8,353	0.550	0.899	0.035	0.026	0.008	0.019	-0.052	0.132	-0.052	0.131
Trend				YES		YES		YES		YES	
Period FE				YES		YES		YES		YES	
Month-of-crime FE				YES		YES		YES		YES	
District court FE				YES		YES		YES		YES	
Additional Cov.				YES		YES		NO		YES	
F-test										242.339	

Notes: In this table, we report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2); finally, we report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual *i* received a custodial sentence by my instrument. For each category of estimates, we report estimates obtained when a trend, a dummy variable indicative of whether or not the crime was committed in the 12 month-period preceding the entering into force of the reform, month of crime and district court fixed effects, and the whole conditioning set are added to the estimated equation (columns 1, 2, and 4). For IV estimates, we also report the results obtained when the conditioning set is not added to the estimated equation (column 3). Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 5.B: Impact on other crimes

	Whole sample			OLS (1)			RF (2)			IV (3)			IV (4)		
	N	Mean	S.d.	Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.	
Panel A: Probability of being convicted again for any crime other than drink-driving															
Within 2 years	8,353	0.273	0.446	-0.043	0.011	***	-0.001	0.007		0.015	0.050		0.007	0.049	
Within 4 years	8,353	0.419	0.493	-0.058	0.011	***	-0.003	0.009		0.027	0.062		0.020	0.060	
Within 6 years	8,353	0.500	0.500	-0.062	0.011	***	0.003	0.009		-0.009	0.065		-0.017	0.061	
Within 8 years	8,353	0.548	0.498	-0.061	0.010	***	0.004	0.009		-0.023	0.062		-0.030	0.059	
Within 10 years	8,353	0.579	0.494	-0.062	0.010	***	0.010	0.009		-0.058	0.062		-0.065	0.060	
Panel B: Number of convictions other than drink-driving convictions															
Within 2 years	8,353	0.405	0.787	-0.075	0.020	***	0.002	0.014		0.012	0.103		-0.017	0.097	
Within 4 years	8,353	0.839	1.366	-0.143	0.032	***	-0.012	0.021		0.119	0.169		0.081	0.145	
Within 6 years	8,353	1.248	1.921	-0.191	0.042	***	-0.058	0.029	**	0.451	0.247	*	0.399	0.197	**
Within 8 years	8,353	1.563	2.327	-0.224	0.046	***	-0.092	0.031	***	0.673	0.274	**	0.630	0.210	***
Within 10 years	8,353	1.817	2.671	-0.296	0.055	***	-0.061	0.033	*	0.482	0.300		0.418	0.223	*
Trend				YES			YES			YES			YES		
Period FE				YES			YES			YES			YES		
Month-of-crime FE				YES			YES			YES			YES		
District court FE				YES			YES			YES			YES		
Additional Cov.				YES			YES			NO			YES		
F-test													242.339		

Notes: In this table, we report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2); finally, we report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual *i* received a custodial sentence by my instrument. For each category of estimates, we report estimates obtained when a trend, a dummy variable indicative of whether or not the crime was committed in the 12 month-period preceding the entering into force of the reform, month of crime and district court fixed effects, and the whole conditioning set are added to the estimated equation (columns 1, 2, and 4). For IV estimates, we also report the results obtained when the conditioning set is not added to the estimated equation (column 3). Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 6.A: Impact on other crimes, decomposition (extensive margin)

	Whole sample			OLS (1)		RF (2)		IV (3)		IV (4)	
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
Panel A: Probability of being convicted again for a crime violent											
Within 2 years	8,353	0.042	0.201	-0.006	0.005	0.007	0.004 *	-0.043	0.026 *	-0.045	0.027 *
Within 4 years	8,353	0.081	0.273	-0.006	0.007	0.007	0.005	-0.043	0.030	-0.048	0.032
Within 6 years	8,353	0.107	0.310	0.002	0.008	0.008	0.005	-0.055	0.038	-0.058	0.036
Within 8 years	8,353	0.129	0.335	0.007	0.009	0.001	0.006	-0.005	0.041	-0.004	0.038
Within 10 years	8,353	0.142	0.349	0.003	0.009	0.008	0.006	-0.051	0.045	-0.052	0.042
Panel B: Probability of being convicted again for a property crime											
Within 2 years	8,353	0.086	0.281	-0.001	0.006	-0.004	0.006	0.033	0.044	0.024	0.043
Within 4 years	8,353	0.148	0.355	0.002	0.008	-0.003	0.007	0.028	0.050	0.022	0.046
Within 6 years	8,353	0.192	0.394	-0.008	0.010	-0.009	0.007	0.070	0.053	0.065	0.049
Within 8 years	8,353	0.217	0.412	-0.009	0.011	-0.009	0.007	0.062	0.051	0.062	0.045
Within 10 years	8,353	0.234	0.423	-0.016	0.011	-0.004	0.007	0.031	0.052	0.030	0.045
Panel C: Probability of being convicted again for any other crime (excluding drunk driving)											
Within 2 years	8,353	0.194	0.395	-0.047	0.012 ***	-0.005	0.007	0.044	0.050	0.036	0.050
Within 4 years	8,353	0.317	0.465	-0.064	0.011 ***	-0.014	0.009	0.104	0.061 *	0.097	0.058 *
Within 6 years	8,353	0.397	0.489	-0.070	0.011 ***	-0.007	0.009	0.055	0.061	0.046	0.058
Within 8 years	8,353	0.450	0.498	-0.075	0.011 ***	-0.002	0.010	0.026	0.068	0.016	0.066
Within 10 years	8,353	0.484	0.500	-0.077	0.011 ***	-0.001	0.011	0.014	0.074	0.004	0.073
Trend				YES		YES		YES		YES	
Period FE				YES		YES		YES		YES	
Month-of-crime FE				YES		YES		YES		YES	
District court FE				YES		YES		YES		YES	
Additional Cov.				YES		YES		NO		YES	
F-test										235.082	

Notes: In this table, we report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2); finally, we report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual *i* received a custodial sentence by our instrument. For each category of estimates, we report estimates obtained when a trend, a dummy variable indicative of whether or not the crime was committed in the 12 month-period preceding the entering into force of the reform, month of crime and district court fixed effects, and the whole conditioning set are added to the estimated equation (columns 1, 2, and 4). For IV estimates, we also report the results obtained when the conditioning set is not added to the estimated equation (column 3). Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 6.B: Impact on other crimes, decomposition (intensive margin)

	Whole sample			OLS (1)		RF (2)		IV (3)		IV (4)	
	N	Mean	S.d.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
Panel A: Number of violent crimes											
Within 2 years	8,353	0.042	0.201	-0.006	0.005	0.007	0.004 *	-0.043	0.026 *	-0.045	0.027 *
Within 4 years	8,353	0.099	0.355	-0.008	0.010	0.010	0.006 *	-0.067	0.038 *	-0.072	0.038 *
Within 6 years	8,353	0.139	0.428	0.000	0.011	0.009	0.007	-0.059	0.051	-0.064	0.048
Within 8 years	8,353	0.189	0.558	0.008	0.015	0.000	0.009	-0.003	0.066	-0.001	0.062
Within 10 years	8,353	0.213	0.595	0.003	0.017	0.011	0.010	-0.074	0.074	-0.075	0.068
Panel B: Number of property crimes											
Within 2 years	8,353	0.106	0.366	0.001	0.007	-0.008	0.008	0.061	0.059	0.051	0.057
Within 4 years	8,353	0.235	0.665	0.003	0.014	-0.012	0.013	0.094	0.098	0.079	0.087
Within 6 years	8,353	0.340	0.874	0.001	0.020	-0.033	0.017 *	0.242	0.125 *	0.224	0.114 **
Within 8 years	8,353	0.414	1.026	0.012	0.023	-0.048	0.018 ***	0.346	0.132 ***	0.333	0.118 ***
Within 10 years	8,353	0.474	1.159	-0.002	0.025	-0.044	0.019 **	0.325	0.141 **	0.305	0.126 **
Panel C: Number of other crimes (excluding drunk driving)											
Within 2 years	8,353	0.248	0.564	-0.068	0.018 ***	-0.001	0.011	0.023	0.073	0.007	0.072
Within 4 years	8,353	0.492	0.871	-0.139	0.026 ***	-0.011	0.016	0.098	0.110	0.074	0.106
Within 6 years	8,353	0.739	1.193	-0.190	0.030 ***	-0.033	0.019 *	0.261	0.150 *	0.224	0.130 *
Within 8 years	8,353	0.932	1.415	-0.247	0.033 ***	-0.039	0.024 *	0.307	0.186 *	0.271	0.160 *
Within 10 years	8,353	1.092	1.601	-0.295	0.037 ***	-0.030	0.026	0.253	0.208	0.209	0.178
Trend				YES		YES		YES		YES	
Period FE				YES		YES		YES		YES	
Month-of-crime FE				YES		YES		YES		YES	
District court FE				YES		YES		YES		YES	
Additional Cov.				YES		YES		NO		YES	
F-test										235.082	

Notes: In this table, we report: Ordinary Least Squares (OLS) estimates derived from the regression of our outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); Reduced-Form (RF) estimates derived from the regression of our outcome variable on our instrument (equation 2); finally, we report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual *i* received a custodial sentence by our instrument. For each category of estimates, we report estimates obtained when a trend, a dummy variable indicative of whether or not the crime was committed in the 12 month-period preceding the entering into force of the reform, month of crime and district court fixed effects, and the whole conditioning set are added to the estimated equation (columns 1, 2, and 4). For IV estimates, we also report the results obtained when the conditioning set is not added to the estimated equation (column 3). Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Impact on labor market attachment

	Whole sample			OLS (1)			RF (2)			IV (3)			IV (4)		
	N	Mean	S.d.	Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.		Coeff.	s.e.	
Panel A: Has a job															
At 2 years	8,353	0.529	0.499	0.000	0.010		-0.001	0.008		0.039	0.056		0.010	0.056	
At 4 years	8,353	0.465	0.499	-0.007	0.010		0.022	0.009	**	-0.122	0.061	**	-0.149	0.058	***
At 6 years	8,353	0.464	0.499	-0.016	0.011		0.031	0.009	***	-0.188	0.061	***	-0.214	0.059	***
At 8 years	8,353	0.441	0.497	0.006	0.010		-0.013	0.010		0.103	0.067		0.089	0.069	
At 10 years	8,353	0.346	0.476	-0.013	0.010		0.027	0.010	***	-0.176	0.061	***	-0.188	0.066	***
Panel B: Cumulative number of weeks spent on unemployment-related benefits															
Within 2 years	8,353	30.552	36.891	3.468	0.757	***	-1.037	0.613	*	6.397	4.400		7.141	4.110	*
Within 4 years	8,353	59.938	68.065	6.355	1.432	***	-2.629	1.229	**	16.643	8.814	*	18.100	8.212	**
Within 6 years	8,353	86.932	97.115	10.055	1.985	***	-4.919	1.742	***	31.557	12.338	**	33.862	11.669	***
Within 8 years	8,353	106.459	121.188	13.500	2.442	***	-5.454	2.122	**	34.501	15.078	**	37.544	14.223	***
Within 10 years	8,353	123.173	141.833	15.670	2.917	***	-5.017	2.466	**	31.224	17.624	*	34.540	16.467	**
Panel C: Cumulative income (in 1,000 DKK)															
Within 2 years	8,353	489.691	268.664	-6.366	4.023		7.208	4.615		-19.407	35.505		-49.621	31.575	
Within 4 years	8,353	972.591	513.354	-19.739	8.480	**	17.320	8.493	**	-62.965	68.484		-119.233	58.312	**
Within 6 years	8,353	1,457.498	775.080	-38.991	14.531	***	31.814	12.789	**	-137.809	102.916		-219.018	88.260	**
Within 8 years	8,353	1,949.740	1,049.469	-49.054	20.315	**	41.015	17.076	**	-179.384	135.053		-282.364	117.438	**
Within 10 years	8,353	2,417.555	1,323.124	-66.671	25.431	***	53.463	21.308	**	-248.397	166.250		-368.056	146.951	**
Strata FE				YES			YES			YES			YES		
Additional Cov.				YES			YES			NO			YES		

Notes: In this table, we report: Ordinary Least Squares (OLS) estimates derived from the regression of the outcome variable on a dummy variable indicating whether or not individual *i* received a custodial sentence (equation 3); Reduced-Form (RF) estimates derived from the regression of my outcome variable on my instrument, a trend, a period fixed effect, and month of crime and district court fixed effects (equation 2); finally, we report the Instrumental Variables (IV) estimates obtained by instrumenting the dummy variable indicating whether or not individual *i* received a custodial sentence by our instrument. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Appendix A.1. Pardon scheme for drunk drivers with "a strong need for rehabilitation" offered prior to the 2000 reform

Between 1990 and 1994, drunk-driving offenders with an alcohol abuse problem who were sentenced to no more than 40 days could benefit from a *pardon scheme.* In this case, their custodial sentence could be commuted to a two-year probation period and a mandatory participation in a yearlong rehabilitation program (identical to the one implemented after the reform and described in section 2.2). They would eventually be granted a pardon upon successful completion of the probation period. In order to benefit from this scheme, eligible offenders had to apply to the Danish Prison and Probation Service, which would decide whether or not to grant it to them. The sanction came together with a fine and could also be combined with either a suspension or a revocation of offender's driving license. This sanction is identical to the one which would be generalized in 2000.

In 1994, the pardon scheme was extended to drunk-driving offenders sentenced to no more than 60 days of incarceration but remained applicable to offenders exhibiting a strong need for rehabilitation only. Towards the end of the 1990s, between 75 and 80% of offenders enrolled in a rehabilitation program as part of this pardon scheme were eventually granted a pardon.

According to the Prison and Probation service, only around 750 offenders were pardoned each year under the pardon scheme (Kriminalforsorgens årsberetning, 1998 and 1999). Put differently, this figure suggests that around 70% of drunk-driving offenders who received a custodial sentence were incarcerated. Using a different data source, Clausen (2007) estimated that 58.2% of all drunk-driving offenders sentenced to no more than 60 days did not benefit from the pardon scheme in the 18-month period preceding the 2000 reform. Taken together, these figures suggest that a large share of offenders did not benefit from the pardon scheme prior to the change in the legislation in 2000.

Appendix A.2. Data sources[38]

GENERAL DESCRIPTION

In order to document the impact of the reform, we use Denmark's rich administrative datasets, which contain individual-level information on all of the country's residents since 1980. These datasets include a wide range of information collected annually, which can be merged using unique individual identifiers. We use these datasets to identify alleged drunk-driving crimes committed and tried around the time of the reform, to compute our outcome variables, and to create the set of control variables we use as covariates.

A. Administrative datasets

Information on crime, charges, and sanctions

Danish administrative data include detailed information on individuals' involvement in criminal activities since 1981. In particular, they include information on all crimes reported to the police, including information about the identity of the alleged perpetrators, as well as the date and nature of the *main* crime (in cases where several crimes were reported concomitantly against a single person). They also include information on all charges pressed by a prosecutor, including the identity of the individuals against which the charges were pressed, and the date and nature of the *main* charge (in cases where several charges were pressed concomitantly against a single person).[39] Finally, they also contain information about the outcome of every criminal case settled by the police, a prosecutor, or a judge. For each decision of justice, information is available on the identity of the defendants, the entity responsible for issuing the sanction, the date when the sanction was issued, as well as the nature of the sanction (a prison sentence, a fine, a withdrawal, an acquittal, etc.) and its severity (fine amounts, probation and custodial sentence length, etc.). Finally, drivers' blood alcohol content at the time of their arrest is not available in the datasets.

[38] The administrative registers used as part of this project are the following ones: BEF, DREAM, FAM, IDAN, IEPE, INDH, KRAN, KRIN, KRSI, and UDDA. Descriptions of the different registers can be found on the following webpage: http://www.dst.dk/da/TilSalg/Forskningsservice/Data/Register_Variabeloversigter (accessed in March 2019).
We provide further information on each of these datasets in *Table A.2* (placed in the appendix).

[39] These crime and charge codes are recorded by the police using a detailed 7-digit hierarchical code (1,161 codes) – the last three digits often indicating the severity of the crime. For instance, 60 different codes can be used to categorize drunk-driving crimes and charges (29 of which were effectively encountered during the study period).

Information on labor market attachment

Danish administrative data also include information on all residents' labor market attachment, which is measured and collected every year in November by Statistics Denmark.

B. Outcome variables

Using these registers, we compute the two groups of outcome variables on which we focus as part of this study: crime-related and labor-related outcomes.

First, we use these administrative records to compute two outcomes indicative of offenders' post-sentencing criminal activity. we start by assessing the relative impact of custodial and non-custodial sentences on offenders' involvement in subsequent drunk-driving crimes. In order to do so, we calculate whether or not individuals were convicted again of another drunk-driving crime and, if they did, the number of such crimes they committed. We also compute an outcome indicating whether or not individuals were convicted of any other crimes and, if they did, the number of such crimes they committed. We measure the impact of the reform on these outcomes from 3 months to 10 years, from the date when the drunk-driving case was settled in court.

Second, we also use these administrative registers to compute outcomes indicative of individuals' attachment to the labor market at different time horizons following the ruling. More specifically, we focus on the relative impact of custodial and non-custodial sentences on the annual number of days worked, as well as on annual income (which is inflated to 2015 prices using Statistics Denmark's Consumer Price Index). We measure the impact of the reform on these outcomes at different time horizons, from 1 to 10 years, from the date when the drunk-driving case was settled in court.

C. Control variables

Finally, we use these registers to compute the set of control variables that we include in the regressions. This conditioning set provides two types of information on individuals' pre-crime characteristics. First, we use variables indicative of the characteristics of the trial, such as whether the defendant was a juvenile at the time of the crime and the nature of the main charge (using a detailed 7-digit drunk-driving charge code). Second, we also include defendants' background information, such as their gender, age at the time of the trial, immigration status (as per Statistics Denmark's typology: "immigrants", "descendant of immigrants", or "rest of the population"), their past criminal activity (the number of convictions in the 5-year period preceding their crime for other drunk-driving

crimes and other crimes), marital status, highest educational achievement, type of job held, and annual income.

Unless specified otherwise, all baseline background characteristics included in the conditioning set were measured at the end of the year preceding the crime and are available for the vast majority of the offenders in our sample.

VARIABLES DESCRIPTION

A. Conditioning set

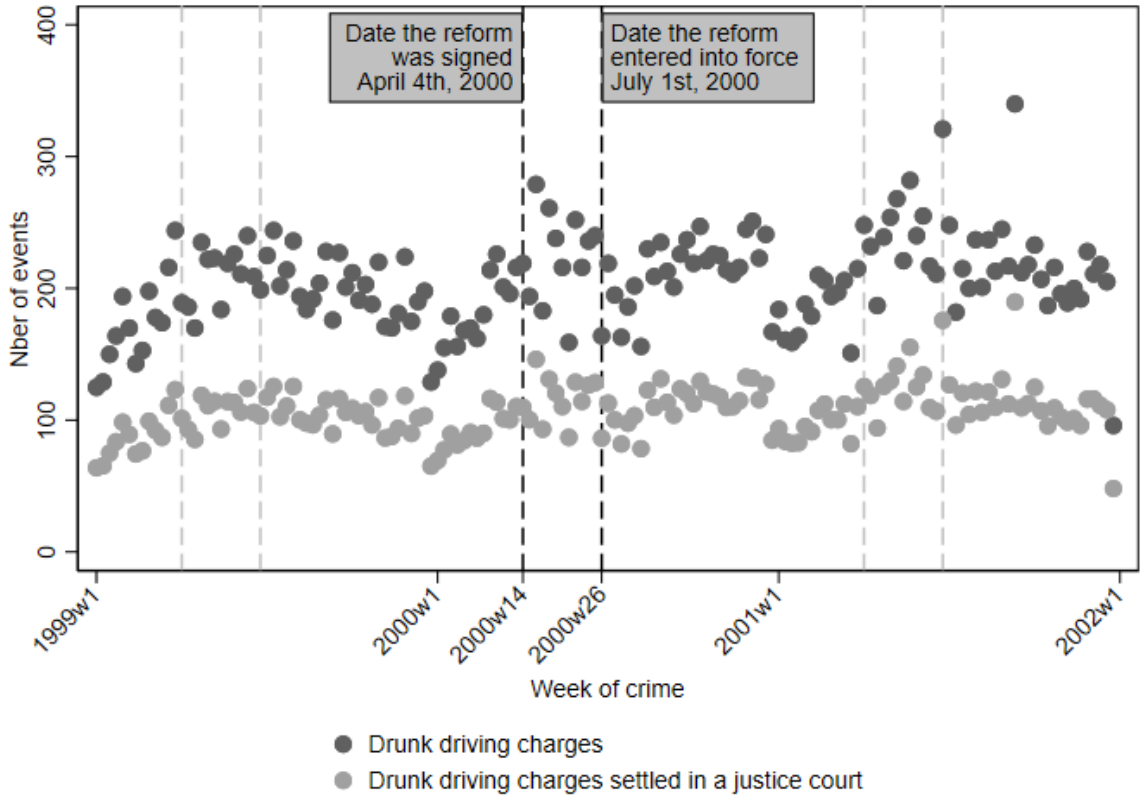
Variables	**Description** *N.B.: Names in italics refer to variables made available by Statistics Denmark. More information can be found on each of these variables on their website: https://www.dst.dk*
Offender's number of drunk-driving convictions in the 5 years preceding the crime	Computed from the date of crime. Top-coded at the 99th percentile. Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the *SIG_GER1DTO* variable.
Offender's number of other convictions in the 5 years preceding the crime	Computed from the date of crime. Top-coded at the 99th percentile. Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of the crime was retrieved from the *SIG_GER1DTO* variable.
Whether or not the offender was a juvenile at the time of the crime	Computed using information on the date of birth of the offender, as well as on the date of the crime. Variable source: information on the offenders' date of birth was retrieved from the *FOED_DAG* variable and information on the date of the crime from the *SIG_GER1DTO* variable.
Offender's gender	Variable source: information on the gender of the offender was retrieved from the *AFG_KOEN* variable.
Offender's age at the time of the trial	Variable source: information on the gender of the offender was retrieved from the *AFG_AFGALD* variable.
Offender's immigration status	Dummy variables indicative of the following four groups of individuals: - Immigrants - Descendants of immigrants - Unknown status - Rest of the population Variable source: information on the immigration status of the offender was retrieved from the *IE_TYPE* variable.
Offender's marital status	Dummy variables indicative of the following five groups of individuals:

	- Single - In a partnership - Separated - Widow - Unknown status Measured at the end of the year preceding the offender's crime. Variable source: information on the martial status of the offender was retrieved from the *CIVST* variable.
Offender's highest educational achievement	Dummy variables indicative of the following four groups of individuals: - Primary education - Secondary education - Higher education - Unknown highest educational achievements Measured at the end of the year preceding the offender's crime. Variable source: information on the education status of the offender was retrieved from the *HFFSP2* variable.
Offender's income	Annual income. Top-coded each year at the 99th percentile. Missing values were given the value 0. Measured at the end of the year preceding the offender's crime. Variable source: information on the offender's income was retrieved from the *SAMLINK_NY* variable.
Offender's job status	Dummy variables indicative of whether or not an individual has a job. Missing values were given the value 0. Measured at the end of the year preceding the offender's crime. Variable source: information on the employment status of the offender was retrieved from the *PSTILL* variable.

B. Outcome variables

Outcome variables	**Description** *N.B.: Names in italics refer to variables made available by Statistics Denmark. More information can be found on each of these variables on their website: https://www.dst.dk*
Offender's number of convictions following their trial	Computed from the date of trial. We exclude from the computation of this variable convictions related to crimes committed prior to the trial of interest or convictions registered under the same criminal case identifier. Top-coded at the 99th percentile. Variable source: we calculated this information ourselves using the exhaustiveness of the registers. Information on the date of each crime was retrieved from the *SIG_GER1DTO* variable.
Whether or not an offender was convicted following their trial	Computed from the above variable.
Offender's income (cumulative)	Annual income. Top-coded each year at the 99th percentile. Missing values were given the value 0. Measured at the end of each year, following the offender's trial. Variable source: information on the offender's income was retrieved from the *SAMLINK_NY* variable.
Offender's reliance of unemployment-related transfers (cumulative)	Annual number of weeks during which an individual received unemployment-related transfers Measured on a weekly-basis. Variable source: information on the offender's social transfers was retrieved from the *DREAM* database.
Offender's job status	Dummy variables indicative of whether or not an individual has a job. Missing values were given the value 0. Measured at the end of the year preceding the offender's crime. Variable source: information on the employment status of the offender was retrieved from the *PSTILL* variable.

Appendix A.3. Evolution of the number of charges and trials



Appendix A.4. Compliers' characteristics

	Whole sample			Compliers (sent.)
Variables	#Obs.	Mean	S.d.	Coeff.
Age, when the decision is rendered	8,353	38.312	11.049	39.097
Education status				
Primary education	8,353	0.525	0.499	0.516
Secondary education	8,353	0.378	0.485	0.401
Higher education	8,353	0.062	0.241	0.074
Attachment to the labor market				
Has a job	8,353	0.637	0.481	0.658
Earnings	8,353	257.462	140.893	270.949
Any social transfers	8,353	0.724	0.447	0.781
Weeks of social transfers	8,353	22.695	21.523	21.589
Weeks of labour market-related transfers	8,353	13.182	18.417	13.236
Weeks of health-related transfers	8,353	3.409	9.029	3.881
Weeks of self-supporting transfers	8,353	0.680	4.844	1.231
Weeks of pension-related transfers	8,353	5.425	15.733	3.241
Criminal priors				
Any crime in past 5 years	8,353	0.650	0.477	0.756
Nber of crimes in past 5 years	8,353	1.693	2.085	1.561
Any DD crime in past 5 years	8,353	0.343	0.475	0.509
Nber of DD crimes in past 5 years	8,353	0.408	0.610	0.538
Any other crime in past 5 years	8,353	0.515	0.500	0.523
Nber of other crimes in past 5 years	8,353	1.276	1.939	1.062

Notes: In this table, we describe the characteristics (mean and standard deviation) of the set of defendants included in our sample, as well as those of the compliers. To do the latter, we follow the methodology described in Pinotti (2005).

Appendix A.5. First-stage by subgroups

First-stage Analysis			*Probability of receiving a custodial sentence*		*Probability of being incarcerated*	
Subgroups		Number of observations	Mean	Instrument	Mean	Instrument
Gender	*Female*	723	0.714	-0.172***	0.274	0.064*
				(0.033)		(0.037)
	Male	7,630	0.639	-0.143***	0.341	-0.081***
				(0.011)		(0.010)
Age	*Below 30*	2,211	0.559	-0.126***	0.308	-0.064***
				(0.018)		(0.018)
	Between 30 and 40	2,776	0.671	-0.139***	0.359	-0.082***
				(0.015)		(0.017)
	Above 40	3,366	0.680	-0.162***	0.333	-0.064***
				(0.014)		(0.017)
Origin	*Immigrant*	407	0.509	-0.115**	0.280	-0.097
				(0.052)		(0.063)
	Descendant of immigrant(s)	438	0.505	-0.111**	0.281	-0.067
				(0.047)		(0.058)
	Other	7,914	0.653	-0.147***	0.338	-0.070***
				(0.011)		(0.010)
Education	*Lower education*	4,385	0.629	-0.134***	0.342	-0.069***
				(0.013)		(0.013)
	Higher education	3,678	0.666	-0.162***	0.329	-0.070***
				(0.015)		(0.017)
Employment status	*Has a job*	5,323	0.644	-0.143***	0.326	-0.081***
				(0.011)		(0.012)
	Does not have a job	3,030	0.648	-0.144***	0.352	-0.052***
				(0.015)		(0.016)
Prior drunk driving	*No prior drunk driving*	3,413	0.527	-0.099***	0.299	-0.052***
				(0.016)		(0.015)
	Prior drunk driving(s)	3,721	0.706	-0.158***	0.360	-0.083***
				(0.014)		(0.012)
Prior incarceration	*No prior incarceration spell*	4,632	0.596	-0.136***	0.296	-0.068***
				(0.012)		(0.011)
	Prior incarceration spell(s)	3,721	0.706	-0.158***	0.384	-0.078***
				(0.014)		(0.014)

Notes: In this table, we estimate the first stage equation for various subgroups of the sample. More specifically, a dummy variable indicative of whether or not a defendant received a custodial sentence is regressed on our instrument, a trend, a dummy variable indicating whether the crime was committed in the 12 month period preceding the entering into force of the reform, month of crime and district court fixed effects,as well as the whole conditioning set. Standard errors are clustered at the district court and individual levels. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.